Hunt Institute for Botanical Documentation
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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.

UNIVERSITY OF LONDON
ROYAL HOLLOWAY AND BEDFORD
NEW COLLEGE
Egham Hill, Egham, Surrey TW20 0EX Tel Egham (0784) 435551

Department of Biology, Huntersdale

file Chaloner

Prof D L Hawksworth
Director, CMI, Kew

15th August 1990

Dear David

I visited Al Traverse in Penn State last month and was able to talk through some of the problems of reconciling our Fossil Plant Committee of IAPT, with the Registration Committees and the N.C.U. business. As you know, he is secretary of the fossil plant committee in addition to his interest in Registration as a palynologist.

He would like to attend the Kew "Improving Stability Meeting" Feb 21 1991, and would be a very useful counterweight to Norman Hughes! Could you possibly generate a "formal invitation" to him to participate? (Even if only as a discussant?). He could probably get funds at his end with an invitation. He will stay with us, in London. There is no question of seeking funding or any other obligation at your end! Merely an invitation to participate ("without liability") so that he might seek local funding. He would be a valuable participant, if this were possible.

Yours sincerely

WG C

Professor W G Chaloner
Head of Life Sciences

cc: Dr A Traverse

Telex: 935504 Fax: 0784-437520

Hunt Institute for Botanical Documentation

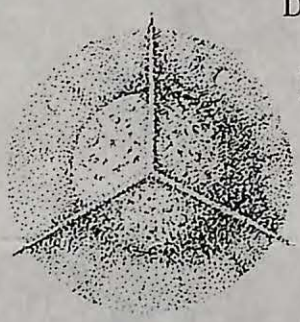
UNIVERSITY OF LONDON
ROYAL HOLLOWAY AND BEDFORD
NEW COLLEGE Egham Hill, Egham, Surrey TW20 0EX

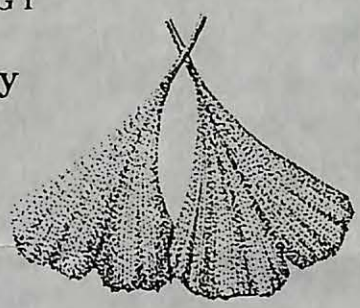
DEPARTMENTS OF BIOLOGY AND GEOLOGY

Palaeopalynology and Palaeobotany
Research Group

Prof. W. G. Chaloner, FRS (Biology)

Dr. A. C. Scott (Geology)





about 15-VIII-90

Dear Al,

I use our local letterhead to give you a smile!

Thanks for letter re-fossil plant Cities archives. Also earlier letter re. the Jansonius book. I am very loath to take on any commitments for a year or so. I have backed-up promises all over my desk, + don't want to add to them! I have every confidence that you'll do an excellent job on your part of the book — would gladly read an MS. Re archives, I agree strongly with deposition in Hunt Library. I could add a few of Chairman's papers too! Sincerely

Bill

Biology Fax; 0784 71739: Tel; 0784 435551
Geology Fax; 0784 37520: Tel; 0784 434455

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES
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phone: 814-863-3419; fax: 814-865-3191

6 August, 1990

Professor W. G. Chaloner, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham, Surrey, UK
TW20 0EX

Dear Bill:

Thanks for the fax confirming your prompt action on my Fulbright application! I appreciate your help very much. You've always been a great friend!

Should have discussed another matter when you were here, because you could have looked at the stuff, but I didn't think of it. When I became Sec. of our Committee in 1969, Serge Mamay (mostly) and Jim Schopf (a few items) sent me a bunch of archival stuff belonging to the Committee--mostly, it's reports, publications (old versions of the Code, etc.), some ancient correspondence. Goes back to Hamshaw Thomas in part, mostly later. This is housed in two large filing cases here. When Jim S. died, Esther sent me quite a bit more, which I incorporated.

Would you agree with me that this stuff should go to Hunt Botanical Library to be permantly archived, with the successive secretaries and chairpeople sending in the materials from their reigns as the years go by? I believe it would be idiotic to crate up what there now is, plus the sizable boxes that my period in office has generated, and send the whole thing off to California, Germany, or wherever? Hunt already is permanent repository for the IFPS, AASP, Botanical Society of America, IAPT itself (you've probably noticed pictures in Taxon crediting Hunt).

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

PENNSTATE

(814) 865-6711

Department of Geosciences

503 Deike Building
The Pennsylvania State University
University Park, PA 16802

10 July, 1990

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill faxed to: 0784437520
Virginia Water, Surrey, UK
GU25 4LN

Dear Bill:

Am off to Baton Rouge to work with George Hart on his MSS for the sed. book. (P. Holmes is also writing for this.) Yours of 21-VI-90 just opened.

I will be back here on 16 July. I would think we could handily spend some hours together after th O_2 converence. Lets hope we can also have at least dinner together, so that Betty can see you, too. If you'd like to stay at "Alphabet," after the conference, that also would be welcomed. We'll hold the evenings of the 18th-19th open, and you'll contact us when you get here.

Best wishes for a good trip. Congrats on the FAS, etc.

Yours very truly,

Al / Betty

Alfred Traverse

(dictated by AT; signed in his absence--hi, Bill!)

AT/et

An Equal Opportunity University

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fax: 814-865-3191

25 May, 1990

Dr. William G. Chaloner
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham, Surrey, U.K.
TW20 0EX

Dear Bill:

So soon after the last inquiry, I have another: re the 21-23 Feb. 91 symposium ("IAPT-IUBS: Improving the Stability of Names....") at Kew. Should I pull out some stops in order to be present?

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

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fax: 814-865-3191

21 May, 1990

Dr. William G. Chaloner
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham, Surrey, U.K.
TW20 OEX

Dear Bill:

You've been awfully quiet, as my cousins on the Gaspé would say.

Just got from NZ an ad for a privately published deal, "A reformed code of botanical nomenclature," by one P.G. Parkinson. Do you know anything about it? Any other nomenclatural news?

Hope you're o.k. My sister and I just had to drop everything and go to Michigan to get our mother into a nursing home. Took more than a week, and now we have to get her house emptied and sold.

All the best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

UNIVERSITY OF LONDON

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NEW COLLEGE

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PROFESSOR W. G. CHALONER, FRS
Biology Department, Huntersdale
Royal Holloway & Bedford New College
Egham Hill, Egham
Surrey TW20 0EX

Department of Biology, Huntersdale

Prof. D.L. Hawksworth,
CAB IMI,
Ferry Lane, Kew.

Feb 20 1990.

Dear David,

Improving the stability of plant names.

Many thanks for the invitation to give a 20 minute presentation to your joint meeting Feb 21 - 23, 1991. I have talked to Michael Boulter about this, and we are keen to do it jointly. I am happy with the general drift of your helpful offered title, but we might want to modify it slightly to emphasise NCU rather than IOP in this particular context. We are both currently involved in the IOP Plant Fossil Record enterprise, although my role is as a sleeping partner ! As you know, this project links strongly with NCU, but the two strands are indeed separate. I hope we can explain this relationship in Feb., and that we can use that opportunity to talk about nomenclature problems (including analogies between the fossil and fungal names) rather than "organisational" ones, during those brief 20 minutes. Mike Boulter and I will sort this out during the intervening year.

With best wishes, sincerely,

Bill

W.G.Chaloner.

Copy: Prof. M. Boulter.
Prof. A. Traverse.

Telex: 935504 Fax: 0784-437520

Hunt Institute for Botanical Documentation

PROFESSOR W. G. CHALONER, FRS
Biology Department, Huntersdale
Royal Holloway & Bedford New College
Egham Hill, Egham
Surrey TW20 0EX

Nov 21

Dear JTL –

It seemed simpler to send the Tdr. Glad you saw the Linnean item. Hope I might ~~visit~~ make a side trip to Penn State (with Judy?) when in Amherst next April. You period will help

Sincerely
Bill.

ROYAL BOTANIC GARDENS KEW

The Palm House,
Designed by Decimus Burton
in association with Richard Turner,
built 1844-48, restored 1984-88.
Engraving from *The Illustrated London News*, August 1859.

Printed by P J Graphics Limited W3 8DH.

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Chaloner

Hunt Institute for Botanical Documentation

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21 September, 1989

Prof. W. G. Chaloner FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham, Surrey
GU25 4LN, U.K.

Dear Bill;

Here's a semi-popular article by Nixon about his Trigobalanus work we've corresponded about. The Cornell quarterly in which it appeared is free, and I get a lot out of it.

Best.

Yours very truly,

Alfred Traverse

AT/et
encl

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
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1 September, 1989

Prof. W. G. Chaloner, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham,
Surrey TW20 0EX, U.K.

Dear Bill:

Digby McLaren called yesterday, and I was able to tell him that my Interlaken paper would go out today--I just sent it to Ken Hsü in Zürich. I am having Eric Barron read the final form now to be sure there are no really major changes. As you may know, he is an ex-paleobotanist of sorts (coccoliths) and a fellow-traveller. He is very helpful and I may even have to revise my parody of "I never met a geochemist I really liked."

I did my revisions from your letter before I noticed the MS pages with corrections suggested. Funny--what I wrote from your letter was almost exactly what you penned onto the pages. Some folk may favor weedy trees for the ur-angiosperms, but Leo Hickey's "riparian weeds" were not trees. You apparently haven't visited the Smithsonian in recent years. Leo got the pond-lily hypothesis permanently installed in a beautiful exhibit in the Hall of Life (or whatever it's called). In any event, I changed to "comparatively" short life cycles.

On p. 17 you asked regarding ~~OC~~ CO_2 cycles and ratio about: 1) planktonic photosynthesis--current opinion is that it is minor compared to that of land vegetation; 2) oceanic sink of dissolved CO_2, etc.--that is a very good but very ~~different~~ difficult question, and according to Jenny R., my authority on such matters (currently she's "on location," as your daughter would say, in Brazil), nobody really knows much.

Thanks very much and all the best.

Yours very truly,

Alfred Traverse

AT/et

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1 September, 1989

Prof. W. G. Chaloner, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham,
Surrey TW20 0EX, U.K.

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Yours very truly,

Alfred Traverse

AT/et

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THE PENNSYLVANIA STATE UNIVERSITY
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24 August, 1989

Prof. W. G. Chaloner, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham,
Surrey TW20 0EX, U.K.

Dear Bill:

This is re your red-penned letter on my letter of 7 Aug. re the "succession." Incidentally, the last previous item of that sort has been translated and passed to Jenny. You should be hearing from them soon. If you come, that would provide a good opportunity to talk about IAPT.CFP and perhaps do something.

Friedemann would make an excellent chairman. I gather, though I've never asked directly, that he's about ten years younger than we are. For chairman that's o.k.

The secretary probably should be a younger person and should continue to be in North America for practical reasons--do you agree? What is your opinion of Larry Matten and Gar Rothwell? They are the two in this country who have displayed the most interest, aside from Judy S. (she is younger). One of them might be considered for secretary? I really would like to find a successor and work with him/her between now and Tokyo. However, I will not resign until/unless I have a suitable successor lined up.

Best.

Yours very truly,

Alfred Traverse

[see next pg. for addenda]

Hunt Institute for Botanical Documentation

P.S. It's an entirely different matter, but I hope you were able to retrieve my illustrations from the Interlaken MS and have sent them back. I apologize profusely for the problem, but I just did what Digby said to do.

P.P.S. Odd coincidence: Judy S. just called to say she <u>is</u> interested and wanted details so that she could take it up with her administration. If she'll do it, she'll be <u>good</u>.

AT/et

Hunt Institute for Botanical Documentation

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22 August, 1989

Prof. W. G. Chaloner, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham,
Surrey TW20 OEX, U.K.

Dear Bill:

Xerox of Jansonius letter enclosed. Fact is that endlessly fine-tuning the Code has not worked, not by any reasonable standard of measurement. Presumably you saw the Hawksworth-Greuter "draft" for Biol. International (dated 2-IV-89). I liked it. The bacteriologists' abolition of 19,000 names overnight was very interesting! I note on p. 10 that we will discuss our problem in April, 1989. Well, we did, a little!

Best.

Yours very truly,

Alfred Traverse

AT/et
encl: copy of Jansonius letter

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
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21 August, 1989

Prof. W. G. Chaloner, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham,
Surrey TW20 0EX, U.K.

Dear Bill:

As you have a US$ account, I am wondering if you could convert the enclosed into a cheque for £5 and forward it plus the order form enclosed to Bernard O.? I have enclosed an addressed envelope. The extra pence can be for postage.

Because you have a US account I can't reciprocate in that direction, but if you ever need a Canadian$ or Australian$ check, I could help.

Best.

Yours very truly,

Alfred Traverse

AT/et
encl: US$ check

Hunt Institute for Botanical Documentation

UNIVERSITY OF LONDON
ROYAL HOLLOWAY AND BEDFORD
NEW COLLEGE

PROFESSOR W. G. CHALONER, FRS
Biology Department, Huntersdale
Royal Holloway & Bedford New College
Egham Hill, Egham
Surrey TW20 0EX

Egham Hill, Egham, Surrey TW20 0EX Tel Egham (0784) 34455

Apr 15 '89

Dear Al,

Sorry about this — figures (and both) enclosed. Pity I couldn't fax them back, but that would hardly help! Your fax received Apr 13, but couldn't get hold of your figs. till to-day.

Sincerely,

Bill.

Chaloner

Telex: 935504 Fax: 0784-37520

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
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14 August, 1989

Prof. W. G. Chaloner, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham,
Surrey TW20 0EX, U.K.

faxed to: 0784-37520

Dear Bill:

There's a problem about the MS situation. The figures you got are a sort of xerox, but they're a special, doctored xerox, and the copies I made from them are inferior--in other words the copies you have are the so-called "originals". Could you airmail them back to me?

Yours very truly,

Alfred Traverse

AT/et
cc: D. McLaren

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
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14 August, 1989

Prof. W. G. Chaloner, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham,
Surrey TW20 0EX, U.K.

faxed to: 0784-37520

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Yours very truly,

Alfred Traverse

AT/et
cc: D. McLaren

Hunt Institute for Botanical Documentation

UNIVERSITY OF LONDON

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Egham Hill, Egham, Surrey TW20 0EX Tel Egham (0784) 34455

Dr. A. Traverse,
Dept. of Geosciences,
Penn. State University.

July 25 1989.

Dear Al,

Many thanks for your MS for the Interlaken meeting volume, which arrived yesterday. Just to avoid assailing the mountain of other much longer-delayed jobs, (and because I thought it would be interesting anyway !) I have read through it and offer a few comments. Digby McLaren had indeed asked me to review this and another MS from the meeting. But your suggestion that I should "mail it direct to Elsevier" having just "corrected minor booboos" shows an unwarranted faith both in my good intentions and my infallibility. The least I can do is to send you back xeroxes of the eight pages on which I have made annotations; I suggest that you then adjust them - or ignore them, as is I assume your prerogative - and mail the revised MS to Elsevier ! Obviously you will hold a copy, and the figures would seem in all cases to be themselves photocopies, so it is not on my conscience that I hold "copy no. 1"

My suggested changes are mostly trivial, and I hope both legible and self explanatory. I attach some MS notes in explanation where they may not be.

Broader based criticism goes outside the realm of a referee's brief. I like your general theme of "it will be alright (for plants) on the night", but I think you exagerate it. You have heard my views on that on our morning drive through Pennsylvania last spring. You play down the impact of global change on diminished size of complex plant communities. But the vulnerability of insect pollinated plants (which constitute the mainstay of very widely dispersed constituents of rainforest) to global change (especially climatic) and anthropogenic disturbance could cause major biological change. Not necessarily life-(of Man) threatening, but certainly of diversity. Wind pollinated trees are perhaps less climatically sensistive, in this respect, but they too are vulnerable to diminished territory. The survival of Ginkgo of which you make much is essentially an "inspite-of" rather than a "because of" survival.

Thanks for letting me see the MS. I hope this changed routing for it doesn't cause more than a week's delay.

It makes a change to correspond on science (of a kind) rather than adminstrative and committee matters ! No doubt more of that in due course. With best wishes to you both, Sincerely,

Bill

W.G. Chaloner.

Copy: Dr. Digby McLaren, Ottawa.

Telex: 935504 Fax: 0784-37520

Hunt Institute for Botanical Documentation

Notes on Traverse MS July 25 '89.

p. 4. A grey spring world surely have no-one there to be ravished? The prospect is what is ravishing?

p. 7. I am not sure I follow the concept of Trigonobalanus as a complex including "living fossil" ancestors. Is my interpretation correct? If not amend and if possible clarify.

p. 8. I don't like either spores (gameto-phytes) or seeds being called "sexual elements" even in a lay sense.

p. 10. General consensus is early angiosperms were "weedy" trees – short life cycle as trees go (cf Birch) but surely not the "very brief life cycle" as in e.g. some Senecio, Cupis etc. ggg.

p. 14. Don't follow the "could or should ...?" idea – we've been manipulating plant propagation, disease, irrigation, nutrition etc since agriculture started ...?

W R Chaloner

July '89

Hunt Institute for Botanical Documentation

NATURAL ENVIRONMENT RESEARCH COUNCIL

Office Use

Application No. 08-BP

File ref GR

PROFESSOR A. TRAVERSE
PALYNOLOGY LABORATORY,
DEPARTMENT OF GEOSCIENCES,
PENNSYLVANIA STATE UNIVERSITY,
UNIVERSITY PARK,
PA 16802,
USA

Alfred Traverse
6-IX-89

Name of applicant

Institution

Committee

Jill Chaloner

1. Suggestions for other referees who should see this application (please include address if possible):

2. Overall grade of application: please circle one of the following:

ALPHA 5	ALPHA 4	ALPHA 3	ALPHA 2	ALPHA 1	BETA	REJECT

3. Comments IN CONFIDENCE to the Committee

Professor Chaloner is well known to me as a keen, resourceful, innovative researcher. He always has a group of talented young people around him, and his facilities are the best. The project is very good--looking at fossil sporopollenin with NMR offers a great chance to zero in on the elusive structure of the mysterious compound. I therefore have nothing to add in section 4.

Continued overleaf

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES
PALYNOLOGICAL LABORATORIES
435 Deike Building
University Park, PA 16802

28 July, 1989

Prof. W. G. Chaloner, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham
Surrey TW20 OEX, U.K.

Dear Bill:

Yours of 13 July in hand (took two weeks!--we really should fax everything).

I hasten to add that there are at least two more reasons other than "not interested" for lack of response to my circular-letter:

1. My letter was poorly drawn.
2. The membership is terribly busy with other things.

Hang in there.

Yours very truly,

Alfred Traverse
Sec. IAPT.CFP

P.S. Jenny Robinson just came by to say "they" are planning an oxygen meeting for Summer, 1990. She wanted to know if WGC might be coming to MA and could also come here to take part in this session.

AT/et

Hunt Institute for Botanical Documentation

UNIVERSITY OF LONDON

ROYAL HOLLOWAY AND BEDFORD
NEW COLLEGE

Egham Hill, Egham, Surrey TW20 0EX Tel Egham (0784) 435551

Department of Biology, Huntersdale

Professor A. Traverse,
Department of Geosciences,
Palynological Laboratories,
Pennsylvania State University,
435 Deike Building,
University Park,
PA 16802,
USA.

13th July, 1989

Dear Al,

Many thanks for your letter of 3rd July. As you are aware from correspondence with Mike Boulter, he is very busy in seeking funding for his massive Fossil Plant Record enterprise. I have encouraged him to see the NCU exercise as a first step towards the "total record" and he is already in touch with Ellen Farr in order to get the ING fossil plants on to his PC facility so that different elements (systematic, geographic, stratigraphic) could be run off and sent to different centres.

I am very sorry to learn that our colleagues on the Fossil Plant Committee are not interested in participating. I suppose there are two possible reasons. The first and best, would be the misgivings that Zijlstra raised in her letter and which I share, on the lines that palaeobotany may actually cherish ill-defined names of dubious typification as a kind of reserve (such names are harmless, and may one day come to have a value for a particular preservation state, for example). The other reason, which I fear is the likely one, is that they are just not interested.

I think Boulter's plans will, with good reason, go ahead anyway and this is not in conflict with inactivity by the Fossil Plant Committee. Meanwhile I will relay this letter and a copy of yours to David Hawksworth as record of our (in)-activity, by way of a report on "progress with NCU for fossil plants"!

I will obviously need to keep in touch on this matter in its various aspects.

With best wishes,

Yours sincerely,

Bill

W.G. Chaloner

Copies to: Prof. M. Boulter
Prof. D. Hawksworth

Telex: 935504 Fax: 0784-437520

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES
PALYNOLOGICAL LABORATORIES
435 Deike Building
University Park, PA 16802

3 July, 1989

Prof. W. G. Chaloner, FRS
School of Life Sciences
Dept. of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham
Surrey TW20 OEX, U.K.

Dear Bill:

Thanks for yours of 22 June. Assuming that you have got the copy of my letter to Mike Boulter of a couple of days ago you will already know that there has been zero response to the form letter from IAPT-CFP of a while back.

Gosh, though, I wish I were in a position to fly to the Canaries to make a report!

As I have said before, for our fraternity the problem is funding. Only a very few of us (e.g. Tom Taylor) have enough built-in funding to tackle this sort of project. Unfortunately the few places that could do it without more $ are probably not interested. I suppose I am typical--our lab could do all microfossils, and I would even enjoy it. But I must seek funding that will support graduate students, buy HF, etc. If I could get my hands on, say, $20,000 to fund one assistant, plus PC costs for him/her for a year it would go.

Culling the ING list for me would actually involve completely redoing it, as my microfossil contribution to ING was a last-minute, shirttail job.

I await your comments.

Yours very truly,

Alfred Traverse
Sec., IAPT.CFP

(See next page for P.S.)

Hunt Institute for Botanical Documentation

P.S. A package arrived yesterday from Paul Strother. I was expecting some samples or what ever and got....the old jacket I lent you and had forgotten about. It has a palynological history--it was acquired for the Scottish field trip after IPC in Cambridge in 1980. However, the weather turned out to be very cold and wet for the congress-proper, so I wore the jacket every day to keep from freezing. The cartoonist who did the "dropping-of-the-pilot" cartoon of me for APLF Newsletter featured it.

AT/et

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PROFESSOR W. G. CHALONER, FRS
Biology Department, Huntersdale
Royal Holloway & Bedford New College
Egham Hill, Egham
Surrey TW20 0EX

Dr. A. Traverse,
Department of Geosciences,
Palynological Laboratories,
Pennsylvania State University,
435 Deike Building,
University Park,
PA 16802,
USA.

22nd June, 1989

Dear Al,

Thanks for your letter of 7th June with your kindly and enthusiastic response to the video tape. The best part is the Trumpet Voluntary by the Professor of Music at the start. Also, O'Brien's comments on his recollection of me as a student are good for a giggle!

The enclosed letter from David Hawksworth calling for summaries of progress with NCU reminds me that this is moving pretty slowly. Please, have we had any comment from colleagues on the Committee? Are we any nearer to being able to involve them in culling the ING list?

With best wishes,

Yours sincerely,

WGC

W.G. Chaloner

Telex: 935504 Fax: 0784 ~~37520~~ 71739

Hunt Institute for Botanical Documentation

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WR12A

19 June 1989

To: All Members Subcommittee A

Dear Colleagues,

LISTS OF NAMES IN CURRENT USE

Further to my letter of 20 April 1989, and subsequent discussions with Dr F A Bisby, it seems that the morning of Sunday 12 November 1989 will be the best time for those of us participating in the Taxonomic Databases Working Group meeting in the Canary Islands to have a discussion on progress. When you receive further details of the TDWG meeting please bear in mind the above when booking your flights!

If you will not be present, it would be useful to have a summary of progress from you to present to that meeting.

With kind regards, and thanks for your continuing co-operation.

Yours sincerely,

M B Rambow

pp Professor D L Hawksworth
Director

Director: Professor D L Hawksworth, PhD, DSc, FLS, CBiol, FIBiol.
Division Heads: Identification and Taxonomic Services: B C Sutton, PhD, DSc.
Culture Collection and Industrial Services: D Allsopp, PhD, CBiol, FIBiol, MIInfSc.

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World services for identification, information, training, and consultancy in all aspects of pure and applied mycology.

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(814)863-3419

7 June, 1989

Professor W. G. Chaloner, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham, Surrey TW20 0EX

Dear Bill:

Just walked in from the European venture. The marvellous video tape was in the great pile of mail waiting for me. What a splendid gift! We'll organize a session for it a little later, and I would imagine it will also find a place in the paleobotany course, etc. Thanks so much!

The trip was mostly successful--I even chaired a session of APP in Krefeld! But my photos of the plant evolution exhibit at Brooklyn Botanical Garden and of countless fossils at Naturhistorisches Museum in Wien, etc., all went in a robbery in New York City at the corner of 32nd and Broadway, as we were proceeding to Penn Station to catch a train for the last leg of our trip home.

Thanks again.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

UNIVERSITY OF LONDON
ROYAL HOLLOWAY AND BEDFORD
NEW COLLEGE

PROFESSOR W. G. CHALONER, FRS
Biology Department, Huntersdale
Royal Holloway & Bedford New College
Egham Hill, Egham
Surrey TW20 0EX

Egham Hill, Egham, Surrey TW20 0EX Tel Egham (0784) 34455

May 12 '89

Dear Al,

This video tape may amuse you — a letter, some of your students! It is a group of three lectures (Margulis, Chalmers, Peter Raven) on "3½ billion years of life on earth" at U Mass October 1988. It was presented to me by James Walker, but it is incompatible with our videos; I have had it 'translated' onto a tape which we can use (quite successfully). Rather than return the original, which we cannot use, I am mailing it to you! Best wishes

Bill Chaloner

Telex: 935504 Fax: 0784-37520

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES
PALYNOLOGICAL LABORATORIES
435 Deike Building
University Park, PA 16802

9 April, 1989

Dr. W. G. Chaloner
c/o Walker
Department of Botany
University of Massachusetts
Amherst, MA 01002

Dear Bill:

The enclosed things from Mike Boulter were in the folder labelled "Chaloner visit" I thrust at you at one point during our hurried few hours together. Mike had wanted you and me to consider these matters during your visit. Alas, it was not possible (for that matter, we didn't do any of our subjects justice--it was absurd to have so little time). But at least I've (I hope) got the things in your hands.

All the best. Betty and I greatly enjoyed seeing you!

Yours very truly,

Alfred Traverse

P.S. Enclosed also your lens cap. It was in the office.

AT/et
cc: Boulter

Hunt Institute for Botanical Documentation

ESSC Seminar

FOSSIL FIRE

William G. Chaloner, FRS
Biology Department
Royal Holloway and Bedford New College

Friday
April 7, 1989
1:30 p.m.
22 Deike Building

Refreshments at 1:15 p.m.

Hunt Institute for Botanical Documentation

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BIOLOGY DEPARTMENT
Huntersdale.

Tel: Egham (0784) 35551

6th March, 1989

Prof. A. Traverse,
Department of Geosciences,
Palynological Laboratories,
Pennsylvania State University,
435 Deike Building,
University Park,
PA 16802,
USA.

Dear Al,

Many thanks for your letter concerning my Penn State visit. My travel plans now are to get the Allegheny Commuter flight out of Washington D.C. April 6th, leaving at 19.35 and arriving in State College at 21.40. I am sorry it is so late. I have no other choice, leaving Fort Myers at 7.40 a.m. that morning, with a rather long stopover in Washington.

Obviously I would be delighted to stop with you and Betty, but in view of my late arrival, if it would be easier I could stop the night in State College. I am happy to give the fire lecture any time to anyone!

I will phone from Florida in the week March 23-30th, when I will be with my mother-in-law Mrs. Jean Bair (at 813-489-4204).

Looking forward to seeing you both - and State College - again.

Yours sincerely,

W.G. Chaloner

PROFESSOR W. G. CHALONER, FRS
Biology Department, Huntersdale
Royal Holloway & Bedford New College
Egham Hill, Egham
Surrey TW20 0EX

Telex: 935504 Fax: 0784-37520

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1 March, 1989

Dr. W. G. Chaloner
Department of Botany
Royal Holloway and Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey GU25 4LN, U.K.

Dear Bill:

(I alternate the Egham and Virginia Water addresses, since you seem to do the same. Please advise as to the preferred one.)

Your plan for 6-8 April sounds good. On Sat., 8 April, I will accompany you into the field, and I still think we can do three localities and yet have you end the day bound for Boston. Since "your time" the trip to "Beantown" has become much easier--all Interstates: from here E on I-80, then N on I-81, then E on I-84 to New England. When I have done what you propose, I usually stay overnight in Wilkes Barre (good motels and even an accessible, good Chinese restaurant). Paul Strother may be back from UK in time to meet us at one of the locations.

Please do plan to give your "fossil-fire" talk! Earth Systems Science Center will almost certainly sponsor it (director out of town for rest of this week, but I feel quite confident). They have a post-doc (Jenny Robinson) working on the question. Andy Knoll at Harvard now has as graduate student, a former undergrad of mine (Nan Arens), who also talks of Ph.D. thesis on fossil fire. The no-honorarium offer will be stunning news to ESSC director, Eric Barron!

I am working on the mailout to IAPT.CFP. Dan N. has named you and me (in parentheses?!) as the fossil part of NCU team. More later on that.

Yours very truly,

Alfred Traverse

AT/et
cc: Eric Barron (ESSC)

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UNIVERSITY OF LONDON

ROYAL HOLLOWAY AND BEDFORD
NEW COLLEGE

Egham Hill, Egham, Surrey TW20 0EX Tel Egham (0784) 34455

Prof. A. Traverse,
Department of Geosciences,
Pennsylvania State University,
Palynological Laboratories,
435 Deike Building,
University Park,
PA 16802,
USA.

20th February, 1989

Dear Al,

Many thanks for your letter concerning my visit to State College. At present I am scheduled to leave Fort Meyers Florida, and fly to Baltimore on Thursday 6th April and then to drive from there to reach State College that evening. However, on your recommendation I will make new plans to aim to fly someway or other into State College and not drive. If Peter Crane is prepared to drive from Amherst I will then not hire a car and will ride together with him back to Amherst.

I would be very happy to give a lecture in your Department on "Fossil Fire", if you think this would be acceptable - it deals with the history of the O_2 and CO_2 in the atmosphere, fossil charcoal and the like. (I had not thought of any fee. Indeed it would be quite inappropriate.) If such a lecture does not fit in with your time schedule, that is fine and we can just forget it.

I would hope on the Saturday (8th April) to see, with Pete Crane, and yourself if available, 1 or 2 of your Tuscarora localities and stop the night somewhere east of State College, driving on the following day to Amherst.

Perhaps we could talk about "Names in Current Use" during the Friday (7th).

Could you let me know as soon as possible if the above timetable is fine for you and Pete so that I might book my flight. (Our College fax no. is: 0784-37520 if that is of any use to you.)

With best wishes,

Yours sincerely,

Bill

W.G. Chaloner

Copy to: Dr. P. Crane

Telex: 935504 Fax: 0784-37520

Hunt Institute for Botanical Documentation

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2 February, 1989

Prof. W.G. Chaloner, Head
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham, Surrey TW20 0EX, U.K.

Dear Bill;

I am answering yours about a possible PSU visit without reference to the nomenclatural business. I can hardly believe the 10 Jan. date on your letter! It surely hasn't been here very long.

First of all, you are wrong (out of date) about flying to State College. Mid-State (= Black Moshannon) Airport was abandoned to the deer long ago. The new State College/University Park Airport is right here, is fully electronic, has a nice, modern terminal, and there are now about 20 flights a day. There are direct flights from DC a couple of times a day, but if these don't suit, you can fly to Pittsburgh or Philadelphia and then here. Our airport is served by Allegheny Commuter, a part of USAir, which flies reliable 30-passenger planes. Betty and I use them about once a month in the course of making trips to visit our mothers. I would think it would be less hassle to fly here than to drive. We'll meet you and you are invited to stay with us at "ΑΒ" of course. The students will enjoy meeting you. The "old colleagues" are mostly gone, I fear.

Now, as to the Tuscarora Fm., the best locality is the one on which Norma Johnson did her MS at Mill Hall (Rev. Pal. Pal. 45 (1985), 307-360. The next best one is at Millerstown, which the nasty person in Oregon ("W^{J}") is trying to steal. I just talked to Strother, who thinks you'd probably get the best "tubes" at Mann Narrows, however (see Strother & Traverse (Palynology 3 (1979), 1-21). It seems to me that if one got a very early start, one could do all three in one day. I would guide you. Then, if Peter comes, he could take you to Amherst with him. You could start after the field excursion, or put up here that night and start the next day. We have room for both of you at home. If Peter isn't here, you could fly to Holyoke/Springfield from here (via Harrisburg-Boston). Paul Strother will unfortunately be at the British Museum, after a Silurian conference somewhere in U.K. He is disappointed.

Hunt Institute for Botanical Documentation

Must get this on its way. Hope it works and you do come.

Yours very truly,

Alfred Traverse

P.S. Did you want to lecture here? It could be easily arranged, but you won't make enough (ca. $100) for it to be worth it for that reason.

AT/et

Hunt Institute for Botanical Documentation

University of London

ROYAL HOLLOWAY AND BEDFORD NEW COLLEGE

SCHOOL OF LIFE SCIENCES
DEPARTMENT OF BIOLOGY
Egham, Surrey TW20 0EX

Prof. W.G. Chaloner, FRS, Head of School
and Hildred Carlisle Professor of Botany
Prof. R.P. Dales, Professor of Zoology and
Head of Department
Prof. J.D. Dodge, Professor of Botany
Prof. C.T. Lewis, Professor of Zoology

Tel: (0784) 35551
35553
34455

Dr. A. Traverse,
Earth Sciences, Penn State U.

Jan 10 1989

Dear Al,

This will I hope follow close on the heels of my letter of a couple of days ago dealing with matters nomenclatural; the topic now is different, and as it also concerns Peter Crane, I decided to make it a separate letter, and am copying it to him.

It now looks as though I shall be in U.Mass April 9 - 15, and the best chance to stop by at State College (perforce, without Judy) would be in the second half of the preceding week; that is, en route to Amherst from Florida. I would aim to fly from Miami to Washington on April 4 or 5, and drive from there to State College (I do not intend to attempt to fly there !) arriving either April 5 or 6. That would give me a clear 24 hours in State College to meet with you and students, catch up on NCU and other fossil plant committee matters, and maybe catch a glimpse of surviving old colleagues in Geology ?

A further part of the scenario is that Peter Crane might drive across from Amherst, also to arrive in State College probably April 6 or 7. We would then both drive back to Amherst (in one or two cars !), and have a half-developed plan to visit some of the Strother/Traverse localities in the Tuscarora Fmn., with an interest to chase any evidence to confirm or otherwise the Niklas/Smocovitis 1983 material of "plant tissue" with ringed tubes in situ, in the Lower Silurian. It remains to be seen if Peter can get away in time that week - and, more to the point, whether that week would be convenient anyway at your end. So this is a rather tentative first enquiry; if it would be possible from your side, I will then get back to Peter Crane, and we will try to arrange things. Let me know you feelings.

With best wishes to you and Betty,
Sincerely,

Bill

W.G. Chaloner.

c: Peter Crane

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7 December, 1988

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey
GU25 4LN, U.K.

Dear Bill:

Many, many thanks for the Linnaean Society gift! However, I hope you will call on me one day to reciprocate in some manner!

Betty and I would be thrilled if you could come (with Judy would be prima) to visit us--any time. However, I will be in Europe from about 20 April-20 May, 1989. Try to plan something before or after!

No word at all from you about my "draft," etc., re the list-bit, etc. (Did you get it?--can never figure out whether to write you at Virginia Water or Egham--which is correct, or more correct?) Also, I wonder where you stand on the author business (Dick Brummitt's baby).

Perhaps if you do come in April I ought to invite Judy Skog here too--assuming you empathize with my plan to "groom" her in case something happens to me, and for my eventual retirement in any event. We could all go over the secretarial archives, etc.

Happy holidays.

Yours very truly,

Alfred Traverse

AT/et

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22 November, 1988

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey
GU25 4LN, U.K.

Dear Bill:

Many thanks for the British paleobot. report, from which I have gleaned much. "The Inevitable Seed" was fun, and I will use a couple of the diagrams for teaching slides. You should try again with Fig. 4, though--(as complicated as the proverbial hooker's nightmare). It makes sense, but takes a lot of puzzling! The plate position paper is an interesting contribution, especially as a caution!

All the best, and thanks again.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

ALFRED TRAVERSE
RD 2, Box 390
Huntingdon, PA 16652

7 November, 1988

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey
GU25 4LN, U.K.

Dear Bill:

Just heard from Dick Brummitt re the "Working Party on Author Names", with the news that Clifford has withdrawn his offer to work up the "fossil authors." What, if anything, do you think we should do?

My own gut reaction is that only the names of the most prominent coiners of fossil names should be abbreviated--Gothan, Bertrand, Potonié, Berry, etc.

I also am wondering when and what I'll hear about our "registration" efforts per my last packet.

Best.

Yours very truly,

Alfred Traverse
Secretary, IAPT.CFP

P.S. I was very impressed in Brisbane by Phil Holmes.

AT/et
cc: J.E. Skog

Hunt Institute for Botanical Documentation

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12 September, 1988

Dr. W. G. Chaloner
Department of Botany
Royal Holloway and Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey U.K. GU25 4LN

Dear Bill:

I am writing on the plane to Australia*, where I just read about "Report on British Palaeobotany and Palynology, 1986-87." If I haven't already ordered and paid, I'd like a copy. Check enclosed.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

P.S. I noted recently that you use "macrofossil" where I would use "megafossil" (in nomenclatural stuff)--are you right?

*This letter just surfaced in the back-from-Australia pile.

AT/et
encl: check for equivalent of ~~£6.~~ £2

Hunt Institute for Botanical Documentation

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8 September, 1988

Dr. W. G. Chaloner
Department of Botany
Royal Holloway and Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey, U.K. GU25 4LN

Dear Bill:

I'll always cherish the "bowling team" card with the nice compliment about Paleopalynology. There's nobody whose approval I seek more. Re the cover: it was not my idea to select a chitinozoan and, furthermore, the design was presented to me at the eleventh hour, but at least I got the art editor to agree aginst her best judgment that it should go with the oral part up--they had it down in the proof, and she insisted it was esthetically more pleasing that way! In the project as a whole you were a big help, of course. Important query: did you get a free copy automatically? I requested such for you.

We are back from Australia and, as of today, after 5 days, our lost and grossly mismanaged luggage is even here too. The trip was a lot of fun. I was especially moved by the weird alpine flora of Tasmania.

It is interesting how one thing leads to another. The lecture in Berlin last year, for which the invitation came from you, led to the (somewhat altered) repeat of it in Brisbane, and that has led (through Paul Martin) to an invitation to prepare a "position paper" for a big evolution (Dahlem-type) meeting in Interlachen, Switzerland, next April--I may need more help from you! Funny. I thought I was a flop in Brisbane because such a topic can't be handled in 20 mins.

Now, about the nomenclature business--don't give up on the Secretary of IAPT.CFP! I will turn to that after a little while. MSS to Proceedings Vol. for IPC is on front burner at the moment.

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

Pity you had to put a chitting zoom on the cover too! —

Dear Al spores are pretties!

I write in July 31 '88

unashamed admiration (+envy!) at your achievement in the publication of Palaeopalynology. It really is a splendid piece of work — warm congratulations, & I hope you both enjoy Australia. Be very sorry I'll not be there. Best wishes Bill.

PROFESSOR W. G. CHALONER, FRS
Biology Department, Huntersdale
Royal Holloway & Bedford New College
Egham Hill, Egham
Surrey TW20 0EX

JOYCE A O'SHEA
The Bowling Team
Oil (6¾" x 10")
© Joyce A O'Shea, 1988
Summer Exhibition
Royal Academy of Arts 14 May – 7 August 1988
Printed in England by The Beacon Press, Lewes, Sussex

Chaloner

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

July 25, 1988

Dr. William G. Chaloner
School of Life Sciences-Biology
Royal Holloway and Bedford New College
University of London
Egham, Surrey, England TW20 OEX

Dear Bill:

Thanks for your very interesting letter of 12 July, just in (10 days?!).

I'm in a mess - two weeks before departure for Australia, Betty in the hospital (emergency hysterectomy-plus), my mother's nursing team in Michigan in rebellion, to mention a few problems. I can't get at the letters to the committee until September. Can it wait until then?

There's one aspect of the situation that troubles me. Botany has always shied away from a "nomenclature commission." Such a commission would, as in Zoology, act in a more or less studied, orderly fashion on problems submitted. Botany now is perhaps taking a far more radical path than a commission! It's rather startling -as if the USA abolished the stock market, etc., and established a five-year plan.

All the best.

Yours very truly,

Alfred Traverse

/jb

Hunt Institute for Botanical Documentation

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23 June, 1988

Dr. W. G. Chaloner
Department of Botany
Royal Holloway and Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey, U.K. GU25 4LN

Dear Bill:

Mike B. writes cryptically: "Bill tells me of the Kew Revolution for ICBN."

I take it that the Wm. referred to is WGC. What does he mean--I have a very vague recollection of something but would appreciate a succinct word or two if you could oblige.

The "authorities" asked me to "respond" in a somewhat light-hearted vein to NFH's address at the 7IPC banquet. Any suggestions? I wish I could see his biorecord book now in press.

All the best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

UNIVERSITY OF MASSACHUSETTS
AT AMHERST

Botany Department

Morrill Science Center
Amherst, MA 01003
(413) 545-2235

18 November 1987

Dr. Alfred Traverse
435 Deike Bldg.
Penn. State University
University Park, PA 16802

file

Dear Al:

I want to thank you very much for your evaluation of Professor William Chaloner's professional standing for our endeavor to bring him to the University of Massachusetts as a Barrett Professor. Your letter will be a great help in my effort to obtain this prestigious appointment for the Botany Department. I know that requests for letters of reference are a very time-consuming task, and I want to thank you personally for taking the time to help us with this matter. One's greatest reward in undertaking a request such as this is probably knowing that you have helped further the cause of botanical science, and in this instance the field of organismal and evolutionary biology in particular. For this I am most grateful.

Sincerely,

James W. Walker
Professor of Botany
and
Head of Department

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17 November, 1987

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey GU25 4LN, U.K.

Dear Bill:

Thanks for yours of 10 November--I had the book on my desk, but hadn't somehow made the "Chapman" connection. I'm having a slide made.

The "slide" must be your first slide, and if you could spare a copy for teaching purposes, great. (I could use it next week for evolution, but that won't happen.)

My "Berlin paper" is nearly ready to send in to Historical Biology.

Best.

Yours very truly,

Alfred Traverse

P.S. I just wrote a smashing letter of rec. for you which, as I understand it, is supposed to help pay for your annual trip to Maine. Ha! Gotcha!

AT/et

Hunt Institute for Botanical Documentation

Geological Factors and the Evolution of Plants

TRANSCRIPT

Edited by Bruce H. Tiffney

PROFESSOR W. G. CHALONER, FRS
School of Life Sciences
Department of Biology
Royal Holloway & Bedford New College
Egham Hill, Egham
Surrey TW20 0EX

NOV 10

Yale University Press
New Haven and London

Al – quick answer to your question of Oct 26.

This was some of Chapman's phylogenetic diag.

Was your "slide 1" one that I used as "earth history condensed to a year"? If you want that (my first slide) I'll mail it. Bill

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9 November, 1987

Dr. James W. Walker, Head
Department of Botany
University of Massachusetts
Amherst, MA 01002

Dear Jim:

I am happy to respond to yours of 28 October re the consideration of Professor William D. Chaloner for the Wilmer D. Barrett Professorship at UMass.

Dr. Chaloner has been a close professional friend of mine since I met him at the University of Michigan in 1953, when we were both in our twenties. "Bill" Chaloner was and is a most stimulating, entertaining and interesting companion in the field, at the microscope, or at a formal dinner. He is always full of ideas and of ways to express them. His research is by now, of course, justly world famous and ranges over practically the whole of paleobotany/palynology. He has been almost incredibly productive of books, articles, symposia, and has also had a very considerable impact on our branch of science through his many graduate students and his activities in international scientific organizations and meetings.

Professor Chaloner is a marvellous lecturer--for example, he was selected by the recent International Botanical Congress in Berlin as one of the all-Congress lecturers. He spoke on recent advances in our knowledge of the early land plants. It was a superb lecture. He has done the same sort of things for all sorts of audiences, lay as well as scientific. Whenever he is a featured speaker, as at the International Palynological Congress in Cambridge a few years ago, the event is immediately assumed to be the main "extra-curricular" occasion of the meeting, and everybody is there. He has a knack for novel ways of explaining things that appeals to listeners of all levels.

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In short, Chaloner seems to be an ideal choice for the sort of association with UMass that you propose.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

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UNIVERSITY OF MASSACHUSETTS
AT AMHERST

Morrill Science Center
Amherst, MA 01003
(413) 545-2235

Botany Department

28 October 1987

Dr. Alfred Traverse
435 Deike Bldg.
Penn. State University
University Park, PA 16802

Dear Al:

Dr. William G. Chaloner, Hildred Carlisle Professor of Botany and Head of the School of Life Sciences, Royal Holloway and Bedford New College, University of London, Fellow of the Royal Society, and President of the Linnean Society of London, is being considered for appointment as a Wilmer D. Barrett Professor in the Department of Botany of the University of Massachusetts, Amherst. Barrett professors are required to be in residence at the University of Massachusetts for at least one week each semester for a period of three years, during which time they must give a lecture at a university-wide convocation once each year, as well as a series of lectures in the Department in which they hold their appointment. Barrett professors receive a sizable stipend each year plus funds to cover all their traveling and housing expenses.

If you are able to do so, I would be most grateful if you could write a letter of recommendation for the file that I am currently preparing that will be sent to the Board of Trustees of the University of Massachusetts for final approval of an offer to Dr. Chaloner of a Barrett Professorship in the Department of Botany. Although I would expect most of your comments to pertain to Dr. Chaloner's professional standing, any comments you wish to make concerning his teaching and/or lecturing abilities will be most welcome.

I would be most appreciative of your assistance in this matter. If, however, for any reason you feel that you cannot provide us with an evaluation of Dr. Chaloner, please either write or call me at (413) 545-2235 as soon as possible so that I will have time to obtain the necessary number of references that we need in order to go forward with this important personnel action. I thank you very much for your consideration of this request.

Sincerely,

James W. Walker
Professor of Botany
and
Head of Department

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26 October, 1987

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey GU25 4LN, U.K.

Dear Bill:

I'm digesting some of my cryptic notes-to-self from Berlin. One item says, "ask WGC about Chapman's diagram--showed charophyte evolution." Can you elucidate with a reference or whatever? I believe that the note stemmed from your big public lecture because on the same card it says to request a copy of a slide for teaching purposes. I believe that reference is to a sort of evolutionary tree. I also have a note to "request a copy of slide 1 from GL16." I am not sure whether that's the same slide or a different one!

Enough trouble for now.

Yours very truly,

Alfred Traverse

AT/et

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1 October, 1987

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey GU25 4LN
U.K.

Dear Bill:

Fellow named Bill Friedman, University of Georgia, gave a seminar here last week about his research on Ginkgo male gametophytes. Marvellous photos of the multi-branched pollen tube. In the question period I asked about cycad pollen, with which he also works--he said he never heard of any proximal opening to release spermatozoids. Now there's the article in American Scientist Vol. 75, 270 ff., re cycad pollination, which seems to say the same thing--sperms travel in the pollen tube (cf. esp. Fig. 3). Comments?

As ever.

Yours very truly,

Alfred Traverse

AT/et

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23 August, 1987

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey GU25 4LN, U.K.

Dear Bill:

I am finally back from the Calgary Devonian session (very educational but not half as much as the Tyrrell Museum in Drumheller, which I played hookey and visited, along with two of my students). So, except for one day after the Europe caper, this is the first day here since early July!

Yours of 11 August was in the pile of mail. We too have fond recollections of you at Berlin (I have pics of you and Al at Botanical Garden--remember? Betty), tho' we still feel somewhat guilty not to have shared your moment of real glory at the big dinner. I did greatly enjoy hearing your general lecture re plant evolution. Of course, D. Edwards sat to my left and made a lot of editorial comments, but I'm a little deaf in that ear and didn't get much of it. That reminds me that at the Calgary meeting there was a raft of plant material displayed, including Australian specimens of Baragwanathia. Pat Gensel, John Richardson and I agreed that we're not really convinced those specimens represented land plants, however the graptolite dating turns out. (The label said, "though Baragwanathia is an undoubted land plant, specimens of this sort are always found in marine sedimentary rock." Hmmm.)

Per your paragraph one--you surely would be welcome here if you ever get a chance to come, and with notice I could arrange a seminar and the usual small honorarium that goes with such.

I will write the new members (including Judy, so she has something in writing), with copies to you.

I will also write the IOP blurb--will send you a draft for comment. It may take a week or so to get this job done. Speaking of IOP.NL, I suppose someone will write about Mike Cichan, Arizona State--it was his little girl who was the only survivor of the Northwest crash in Detroit last week.

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Your suggestion re publication of (suitably altered) my Berlin paper is most welcome. I'll get at that ASAP and send off the MS to Tony Hallam. You could perhaps apprise him of my intention? I must get my paper for David Batten's session in Halifax (7 October) ready first, however. The evolution paper will <u>then</u> be "an der Reihe:.

That seems to cover the essentials. By the way, don't do anything rash about your ME property--real estate is a good inflation hedge. I wonder if you have a good agent-advisor there, as it sounded to me as if your rent is far too low. A <u>good</u> agent is expensive, but pays in the end.

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

University of London

ROYAL HOLLOWAY AND BEDFORD NEW COLLEGE

DEPARTMENT OF BOTANY

Professor W G Chaloner, PhD, FRS
Professor J D Dodge, DSc

Huntersdale
Callow Hill
Virginia Water
Surrey GU25 4LN

Tel: Egham (0784) 35551

11th August, 1987.

Dear Al,

I had hoped to say a fond farewell to you and Betty, but somehow I never saw you both again after the Friday evening. I enjoyed our re-encounter and hope we don't need to wait for Tokyo for the next. (Maybe State College, or Australia?)

Left over items are:-

1) I trust you have a copy of the re-elected (and new) Members of our Committee. I don't seem to have the official version issued at the end of the "Nomenclature Section." The important aspect was the newcomers - Judy Skog, Muriel Fairon-Demaret (address as for Street) and Venkatachala. The latter two do not know of their nominations and require tactful letters to this effect from our Secretary! (I can't believe either will refuse?)

2) Would you be prepared to write a brief report on those items in Nomenclature which concern palaeobotanists, for the IOP Newsletter? You did a similar item after Sydney - very useful, and it reminds palaeobotanists that our Committee exists, and what it does. The only serious items were the defeat of the Latin - or - English proposal, and the appointment of the "Committee for Registration" which is to recommend ways out of the journal/book approval versus Name Registration maze, in time for Tokyo.

If you don't wish to tackle this, I will understand, and will have a go at it myself but (need I say?) am confident that you would do it better.

3) Finally, you talked of publishing your admirable paper on "plant evolution dances to a different beat". It was one of the only two papers given that day that really should be published. Would you think of putting it in the new journal Historical Biology - Tony Hallam is one of the editors. This is a new European venture, and will have a wide geo- and bio- audience. As you may surmise, I have a vested interest, as a member of the editorial board. I am enclosing appropriate blurb. Do think about it. It would be great to have something on the plant side with the breadth of your theme in the first number.

Continued.....

Telex: 935504 Fax: 0784-37520

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Well I can't think of any more homework for you. I hope they get your book out soon. Take it easy - and my love to Betty.

Sincerely,

Bill

W. G. Chaloner

Dr. A. Traverse,
Department of Geosciences,
Pennsylvanian State University,
University Park,
PA 16802,
U.S.A.

Hunt Institute for Botanical Documentation

HISTORICAL BIOLOGY

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An International Journal of Paleobiology

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3. the affiliation and address of each author;
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Tables should be presented in a clear and precise form to help both typesetter and reader. Number tables consecutively with arabic numerals and give a clear descriptive caption at the top. The data should be arranged such that columns of like material read vertically, not horizontally. Draw horizontal rules below the table caption, below the column headings and at the foot of the table. Avoid the use of vertical rules to separate columns. Explanatory table footnotes should be designated by superior lower-case letters and typed below the table.

MEASUREMENTS AND UNITS

Measurements and units should be expressed with their exact abbreviations in metric SI units. Examples of the six basic SI units are:

Length: meter (m)	Time: second (s)
Mass: kilogram (kg)	Temperature: Celsius scale ^{o}C
Electric current: ampere (A)	Luminous intensity: candela (cd)

There are no periods or plurals when the official abbreviations are used. Note also the abbreviations for minute (min) and hour (h), and especially microgram (μg) and microliter (μl). It is preferable to avoid non-metric units, but if essential they may be used in parentheses after the proper SI units.

For further details consult BSI 5775 Quantities, Units and Symbols, available from the British Standards Institution, Linford Wood, Milton Keynes, MK14 6LE, England, UK.

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Abbreviations for measurements and units should be expressed as mentioned above. Common abbreviations (e.g. US for United States) are written without periods and without any spaces between the letters. Other abbreviations should be spelled out completely the first time they are used, as should acronyms.

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Biochemistry: Biochemical terminology shuld follow the recommendations set out in the International Union of Biochemistry's Biochemical Nomenclature and Related Documents 1978 (reprinted for the Biochemical Society, London, England).

REFERENCES

Harvard style

References and notes are indicated in the text by the name and date system: either "Recent work (Smith, 1984)..." or "Recently Smith (1984)...". If more than three authors are listed, cite the reference as "Smith et al. (1984)...". However, in the end reference list, the first six authors should be given, then "et al." used. When citing more than one reference at a time, list them in chronological order, then in alphabetical order if there is more than one in the same year (e.g. "Smith, 1980; Black, 1981; Green, 1981"). If there is more than one publication by the same author(s) in the same year, distinguish by adding a, b, c etc. to both the text citation and the list of references (e.g. "Smith, 1980a").

References should be collected and typed at the end of the paper in alphabetical order according to the first author. Listed references should be complete in all details, including article titles and journal titles in full. Examples:

Mackenzie, F.T. and Pigott, J.D. (1981) Tectonic controls of Phanerozoic sedimentary rock cycling. Journal of the Geological Society of London, 138, 183-196

Perch-Nielson, K. (1985) Mesozoic calcareous nannofossils. In Plankton Stratigraphy, edited by H.M. Bolli, pp. 329-426. Cambridge: Cambridge University Press

Ramsay, A.T.S. (1977) Editor. Oceanic Micropalaeontology, 2 vols. London: Academic Press

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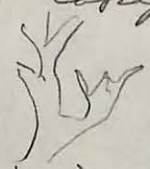
Hunt Institute for Botanical Documentation

Aug. '87 Int. Botanical Congress, Berlin 1987

Chloro general lecture I

"Chapman" Region — ask WAC re [illegible] ref.
this — dried Chlorophyte solution

Tufford Book — request copy of slide



Homologous, isomorphic alternation — Fowke favor!

H.C.

Antithetic alternation — Fowke favor!

Calocheate

OVER

Rhynia [illegible] like homologous archegonia in plants like Ps. apparently does!

put with correspondence as of WAC

Gen. Committee IAPT

23-VII-87

Sm. C. votes only on proposals that are accepted — no binding "rejections" come from S.C.

Submitted Bird Chlorophyte to Voge member of S.C.

SC votes to override Sp. Comm.'s decision not to ask for conservation of Lycopersicon esculentum.

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15 June, 1987

Dr. W. G. Chaloner
Department of Botany
Royal Holloway and Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey, GU25 4LN

Dear Bill:

Thanks for yours of 4 June. I hope the changed title went in for printing in the programme, but I can't really tell if that happened or not. I just whipped off a letter to Greuter about it, but I suppose if it hadn't happened until now, it won't. What a horrible job poor Werner undertook!

See you.

Yours as ever,

Alfred Traverse

AT/et

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26 May, 1987

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey, U.K. GU 25 4LN

Dear Bill:

I waited for days by the phone for a call from you--ha! I hope you had fun in North America.

Enclosed is a copy of my abstract for Berlin. The original went to headquarters, of course. Please note that I've changed the title, better to reflect my ideas.

It was humbling to discover, in reading various combos of Niklas, Tiffney, Knoll and others within the last few days, that most of my "original ideas" already were in some other paper. Most, but not all.

See you. Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl

Hunt Institute for Botanical Documentation

Regarding the article, "Oldest land-creature fossils found in PA," in the Feb. 10 *Collegian*, I have a few comments.

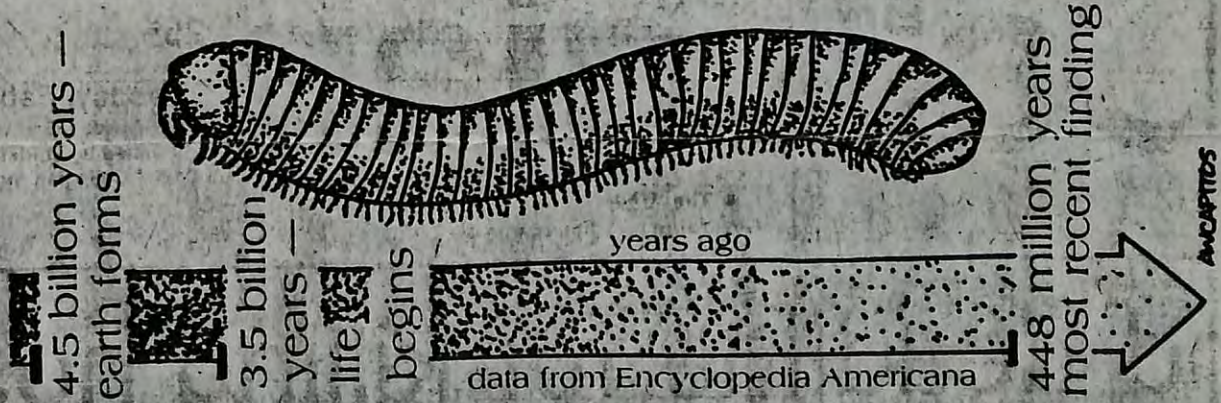
For years my students and I have been working on evidence of land-life in the rocks along Route 322 near Potters Mills, just above and therefore just a bit younger than those involved in the Harvard-Oregan work reported in your news release. Many local colleagues, who know about our work have asked for a clarification of it in connection with the Harvard-Oregon work. (Why the Harvard-Oregon paper did not mention our related research, I have no idea).

Our investigations have been concerned with the fossils of land dwelling *plants* about 440 million years old, and several former students and I have produced the only systematic studies of such plant fossils yet published. One of these students (Paul K. Strother) is now a professor at Boston University, and he and now *his* graduate students, have also been working on the US 322 roadcuts. It is clear that Strother and I need lessons on how to get headlines!

Alfred Traverse
professor of palynology

Oldest land-creature fossils found in Pa.

By MARYANN LIDDY
Collegian Staff Writer



Two University of Oregon geologists working in Pennsylvania recently discovered the oldest fossil traces of land-dwelling creatures, which predate existing fossils by about 30 million years.

Gregory Retallack, a geology professor, and Carolyn Feakes, then a graduate student, found new evidence of land creatures along Route 322 in the mountain gap south of Potter's Mills.

The findings, published in the Jan. 2 issue of *Science* magazine, described a series of burrows, one to two centimeters in diameter with W-shaped grooves, that suggest the creatures had hard-shelled segmented bodies, much like modern millipedes.

"No actual skeletons (of the creature) were found," Feakes said. "We discovered trace fossils of the animal, which are patterns left in the soil from its environment."

The earliest accepted age of non-marine life was 414 million years old, Feakes said.

The soils along Route 322 where Retallack and Feakes found the burrows are estimated to be as old as 448 million years old, she said.

John Crosiar, assistant director of the University of Oregon's news bureau, said the discovery is significant because "it is one more piece of evidence of how land creatures evolved."

"The fact that we found animals in the soil shows the presence of very early land-dwelling creatures," Feakes said.

Evidence of fecal pellets showed the creatures ate solid food, according to *Science*, which also indicates that the soil must have supported some type of plant life to feed the land-dwellers.

Feakes said an earlier study of the same Appalachian soils had revealed evidence of life, but it was believed to be marine rather than land-dwelling because of the way researchers had interpreted the environment.

The geologists did not expect to find any actual preserved body fossils since, Feakes said, any remains of the creature's body would have been destroyed over time from oxidation and changes in the rocks.

"There's a possibility of continuing the research," Feakes said, "but probably not in the immediate future."

Retallack is working on a project in Kenya while Feakes is conducting research at Harvard University, Feakes said.

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University of London

ROYAL HOLLOWAY AND BEDFORD NEW COLLEGE

DEPARTMENT OF BOTANY

Professor W G Chaloner, PhD, FRS
Professor J D Dodge, DSc

Huntersdale
Callow Hill
Virginia Water
Surrey GU25 4LN

Tel: Egham (0784) 35551

22nd May, 1987

Prof. Dr. Werner Greuter,
XIV International Botanical Congress Berlin eV,
Konigin-Luise Strasse 6-8,
D-1000 Berlin (West) 33,
West Germany.

Dear Werner,

In reply to your letter of 19.4.87, here is the programme for our Congress Symposium on Saturday July 25th, 1987.

SESSION 5-31. **EVOLUTION AND THE FOSSIL RECORD**

1st Session. **Chairman: W.G. CHALONER**

08.30 - 9.30 a.m.	ANDERSON, J.M. & ANDERSON, H.M.	"Evolutionary patterns in Permo-Triassic plant life"
09.00 - 09.30 a.m.	BOULTER, M.C.	"Events and evolution: Tertiary Plant succession in off-shore drilling, NW Europe"
09.30 - 10.00 a.m.	TRAVERSE, A.	"Megaevents, but no punctuation" ⊕

Preferred coffee break: 10.00 - 10.30 a.m.

2nd Session. **Chairman: A. TRAVERSE**

10.30 - 11.00 a.m.	CRANE, P.R.	"Rates of diversificiation in flowering plants"
11.00 - 11.30 a.m.	TIFFNEY, B.	"Habit, habitat and rate of species appearance in fossil Angiosperms"
11.30 - 12.00 noon	EYDE, R.H.	"Fossil record and evolution of Cornus sensu lato"

NOTE 1: I understand that my co-organise, H. Walther is unlikely to get to Berlin; hence Traverse as second chairman.

NOTE 2: To the best of my knowledge, J. Anderson and B. Tiffney are definitely coming to Berlin.

Yours sincerely,

W.G. Chaloner

⊕ More changes of title shown on Abstract!

Copy to: H. Walther

Telex: 935504 Fax: 0784-37520

Hunt Institute for Botanical Documentation

University of London

ROYAL HOLLOWAY AND BEDFORD NEW COLLEGE

DEPARTMENT OF BOTANY

Professor W G Chaloner, PhD, FRS
Professor J D Dodge, DSc

Huntersdale
Callow Hill
Virginia Water
Surrey GU25 4LN

Tel: Egham (0784) 35551

4th June, 1987

Dear Colleagues,

Berlin Botanical Congress

Berlin and our session 5-31 will soon be upon us! I am enclosing the final programme for that, now scheduled for Saturday, July 25th in the morning.

As we shall not be receiving the abstracts of papers until we arrive in Berlin, I am anxious to get copies to you of all of our abstracts as soon as possible. It would obviously be a help for you to have seen one another's abstracts before preparing the final version of your presentation. So far I have received 3 abstracts only (for which I thank you) and these are enclosed with this letter. Could the remaining three contributors (J. & H. Anderson, B. Tiffney and R.H. Eyde) please let me have copies of their abstracts as soon as possible.

It is important for the success of our symposium that we deal with the theme in a concerted way and obviously awareness of the contents of the several contributions is an important part of the game.

I look forward to seeing you in Berlin at 8.15 a.m. on July 25th, if not before!

With best wishes,

Yours sincerely,

Bill

W.G. Chaloner

Copies to: Drs. J. & H. Anderson
Dr. M. Boulter
Dr. P. Crane
Dr. R. Eyde
Dr. B. Tiffney
Dr. A. Traverse
Dr. H. Walther

Telex: 935504 Fax: 0784-37520

Hunt Institute for Botanical Documentation

SOME EVENTS INFLUENCING PLANT EVOLUTION DURING THE TERTIARY OF NORTHERN EUROPE

M.C. Boulter (Palynology Research Unit, North East London Polytechnic, Romford Road, London E15 4LZ).

Fossil plants are abundant at all localities of the Brito-Arctic Igneous Province (BIP): Rockall Plateau, Voring Plateau, east Greenland, Faeroes, Spitsbergen, Mull and Antrim. Dinoflagellate cysts date the intra-basaltic sediments at NP10, near the Palaeocene/Eocene boundary. The immature soils of the Province supported pioneer vegetation comprising taxa unfamiliar in the more diverse warmer Tethyan floras to the south east and served as a barrier to some migrations. The evolution of some of the plants in the BIP Flora will be mentioned, particularly the Arcto-Tertiary representatives of the Osmundaceae, Onocleaceae, Taxodiaceae, Platanaceae, Juglandaceae and the Betulaceae. Younger sediments in the by then off-shore region show distinct yet different pollen assemblages in the Palaeogene and the Neogene. This evolutionary stability contrasts with more active processes in the cyclically warmer climates to the south. The first and last appearances of some plant taxa in the two regions will be compared.

PLANT EVOLUTION DANCES TO A DIFFERENT BEAT

Alfred Traverse (Department of Geosciences, Pennsylvania State University, University Park, PA 16802 USA).

Land plants have produced new forms at rates comparable to animal groups. The driving forces for plants are different. Floral upheavals, such as emergence in the Permian of cycadophytes-ginkgophytes-coniferophytes, and Cretaceous angiosperms were due to gradual replacement resulting from introduction of new forms and piecemeal extinction. Most transitions of plant groups in time are gradational--thus the preference in palynostratigraphy for "palynozones". Plant "geoevents" have not been synchronous with similarly epochal animal originations/extinctions and hence cannot be directly attributed to the same causes. Initiation of plant forms probably results primarily from intrinsic genetic factors. Plant genera and families are much less subject to extrinsic evolutionary factors than are similar animal groups. Plasticity of plants, in producing related forms adapted to very different environments is partly responsible; hybridity and polyploidy produce variants with different characteristics in some organs, but retaining basic features of the original. Vegetative reproduction, or persistence, and potential long dormancy of propagules are also significant.

THE ABOMINABLE MYSTERY REVISITED: THE RADIATION OF ANGIOSPERMS IN FOSSIL RECORD

P. R. Crane & S. Lidgard (Department of Geology, Field Museum of Natural History, Chicago, Illinois 60605, USA).

Large scale diversity patterns in the fossil record provide insights into the dynamics of major floristic and vegetational changes complementary to those derived from studies of local paleoecology. Quantitative analyses of over 200 floras from the late Mesozoic and early Tertiary at both the specific and generic levels indicate that the initial major angiosperm radiation occurred earlier and more rapidly than previously recognized. The radiation of angiosperms and the correlated decline in the diversity of cycadophytes and pteridophytes resulted in a dramatic change in the systematic composition of terrestrial vegetation during the Albian. Attempts to quantify changing patterns of diversity confront numerous difficulties, including biases in the fossil record, the nature of the geological time-scale and poorly standardized systematics, but the major trends during the mid-Cretaceous are robust and statistically significant. The consistency of these trends both locally and 'globally', raises interesting ecological and evolutionary questions to be addressed through further analysis of the paleobotanical record.

Hunt Institute for Botanical Documentation

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

21 April, 1987

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill
Virginia Water, Surrey, U.K.
GU25 4LN

Dear Bill:

Nomenclatural query from the nether regions: Verrutriletes litchi (Harris) Potonié came originally from Tom Harris' Greenland work where he gives no hint that I can find about the origin of "litchi". Are you privy to any sources about this? If it's a person it should be litchii, but I can't determine.

Thanks in advance.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

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

25 February, 1987

Dr. W. G. Chaloner
Dept. of Botany, R.H.B. College
University of London
Huntersdale, Callow Hill
Virginia Water, Surrey
GU25 4LN, U.K.

Dear Bill:

Re PALEOPALYNOLOGY: Because I never throw anything away, you are in luck. I had a bundle labelled "Chaloner review, Chaps 11+", which I guess is what you are talking about. In a way, I hate to part with it because it was my record of what you had said. However, Betty wanted me to chuck all that stuff months ago. I guess it's o.k.! It's on its way.

Best.

Yours very truly,

Alfred Traverse,
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

18 November, 1986

Dr. W. G. Chaloner
Dept. of Botany, R.H.B. Colleges
University of London
Huntersdale, Callow Hill
Virginia Water, Surrey, U.K.
GU25 4LN

Dear Bill:

At AASP a couple of weeks ago I lunched with Doug Nichols and explained IAPT.CFP with him. He is very enthusiastic about it and will be a big asset. I told him that you and I were agreed that he should be coopted, but I emphasized that it was your decision and should not be assumed or announced until you write him. The co-option should also be announced to IAPT. I can do that, if you let me know by a cc of letter to DJN that it's happened.

From a pragmatic standpoint, Doug needs to know asap, in order to start politicking for a ride to Berlin.

Schaarschmidt is driving me bonkers by not responding to my letters about his "Begutachtung." I was writing his boss, Will Ziegler, today about something else and enquired if Fried. were ill or what. I can't send out what needs to be sent until I hear. (I suppose I'll have to--yeeks--phone?) I will put DJN on the list of recipients unofficially as soon as you o.k. it.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

ATet

Hunt Institute for Botanical Documentation

University of London

ROYAL HOLLOWAY AND BEDFORD NEW COLLEGE

DEPARTMENT OF BOTANY

Professor W G Chaloner, PhD, FRS
Professor J D Dodge, DSc

Huntersdale
Callow Hill
Virginia Water
Surrey GU25 4LN

Tel: Egham (0784) 35551

14th November, 1986

Dr. D. Nichols,
U.S. Geological Survey,
Mail Stop 919, Box 25046,
Denver,
Colorado 80225,
USA.

Dear Doug,

We have a vacancy on our "Committee for Fossil Plants" of the IAPT. (Our constitutional position is set out in Division III of the I.C.B.N.). We have lost one member, sadly, in the death of Jan Muller, and another by retirement. As you may have feared, I am writing to ask whether you would be prepared to help us in this way? If you accept, we would in fact be co-opting you now and wouls nominate you for formal election at Berlin.

Our main work (which is not onerous!) will be between now and I.B.C., Berlin, and particularly at Berlin. I hope that this will perhaps help you in getting funding to get there; but obviously your membership is independent of your availability for Berlin.

Our Secretary, Al Traverse, was for some reason (surely not some misplaced modesty?) shy of formally writing to invite "his first Ph.D. person" to join us! So the pleasure falls to me; I do hope you will feel prepared to do so, and that I can look foward to working with you at Berlin and beyond.

With best wishes,

Yours sincerely,

Bill

W.G. Chaloner

c.c. Dr. A. Traverse

Telex : 935504 Fax : 0784-37520

Hunt Institute for Botanical Documentation

Cover.

PROFESSOR W. G. CHALONER, FRS
Department of Botany
Royal Holloway & Bedford New College
Huntersdale, Callow Hill
Virginia Water
Surrey GU25 4LN

Dear Al,

Many thanks for telling me how the SEPM field trip [illegible] Maine coastal processes. Makes me worry for our cottage over winter! How do matters [illegible] nomenclature?

Frohe Weinachten — Frohe Kongress!

Bill

P.8. Richmond Park, Surrey.
Red Deer Stags grazing.
A mile from our Kent!

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Hunt Institute for Botanical Documentation

24 October, 1986

Dr. W. G. Chaloner
Dept. of Botany, R.H.B. Colleges
Univ. of London, Huntersdale, Callow Hill
Virginia Water, Surrey, U.K. Gu25 4LN

Dear Bill:

Re yours of 12 October, 1986--I still haven't heard back from FS (see enclosed letter-copy). As soon as I do, we're ready to go with the ballot, etc.

Now, re Doug Nichols--I'm delighted the suggestion pleased you. He's really good and very interested in nomenclature. He was my first Ph.D. person, and I thought you might interpret the nomination as "Traverse always going on about his student." Will you write him? If he agrees, I will (unless you'd prefer to do it) inform authorities in IAPT. Let me hear asap. (You have the power to co-opt.)

All for now.

Yours very truly,

Alfred Traverse
Professor of Palynology
Secretary, IAPT.CFP

AT/et
encl: copy of letter fo Schaarschmidt

Hunt Institute for Botanical Documentation

24 October, 1986

Dr. W. G. Chaloner
Dept. of Botany, R.H.B. Colleges
University of London, Huntersdale, Callow Hill
Virginia Water, Surrey, U.K. GU25 4LN

Dear Bill:

Just to acknowledge that I did get your circular-letter of 4 August to the Berlin symposium participants, and to say that I have posted it to remind me about the variouwsexhortations.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

file Chaloner

University of London

ROYAL HOLLOWAY AND BEDFORD NEW COLLEGE

Department of Botany

Professor W G Chaloner PhD FRS
Professor J D Dodge DSc

Huntersdale
Callow Hill
Virginia Water
Surrey GU25 4LN

Tel:Egham(0784)35551

19th September, 1986

Professor A. Traverse,
Palynological Laboratories,
Department of Geosciences,
College of Earth & Mineral Sciences,
Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802,
USA.

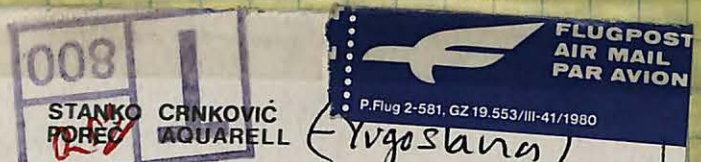
Dear Professor Traverse,

Thank you very much for your several letters. Professor Chaloner is away on holiday on the continent at present and then is going straight on to a conference at Montpellier. I will give him your letters as soon as he returns to College at the end of the month.

Yours sincerely,

Mrs. L. Jackson (Secretary)

Hunt Institute for Botanical Documentation



(Yugoslavia)

Austrian Tyrol: Sept 15 '86

Dear Al,

This is the kind of holiday when the post-cards arrive inevitably from the last stop! Judy - I are camping around Europe for 3 weeks en route to a meeting at Montpellier celebrating 100 yrs of Embryer. (+ don't say 'who is he'!). Travel via Lake Garda, Yugoslavia, Budapest + Vienna (Y. camp sites are good!). Your long Berlin nomenclature letter arrived the day we left as I was struggling to complete my paper for Montpellier. Many thanks - will reply on return Oct! Best wishes B. M. C.

Dr. A. Traverse,
Palynology Lab.,
Earth Sciences
~~Penn. State Univ.~~
State College,
Pennsylvania,
U.S.A.

Chelona

Hunt Institute for Botanical Documentation

University of London

ROYAL HOLLOWAY AND BEDFORD NEW COLLEGE

DEPARTMENT OF BOTANY

Professor W G Chaloner, PhD, FRS
Professor J D Dodge, DSc

Huntersdale
Callow Hill
Virginia Water
Surrey GU25 4LN

Tel: Egham (0784) 35551

4th August, 1986.

Dear Colleagues,

XIV International Botanical Congress, Berlin: Evolution and the fossil plant Record (Symposium 5-31).

Time is moving fast towards July 1987, and we need to take action in preparation of this symposium. We now have six potential contributions. Richard Eyde's paper has been "offered" to us and (subject to his agreement) I believe it will be a useful extension of our brief. We had quite a large number of other papers "offered" to our meeting, but they were really just general palaeobotanical papers not dealing with the observability or the progress of evolution, and have been excluded.

In that each symposium will have three hours available, I suggest that each speaker work in terms of a half hour slot - let us say twenty minutes of talk, 5-10 minutes discussion. I hope speakers will deliberately promote discussion in their talks. (You don't need reminding that discussion is what makes the difference between a published paper and one presented at a meeting!)

May I respectfully - and apologetically - urge you all to re-read my letter of 24th June 1985 before preparing your talk? This was intended not as a directive as to content, but really to remind speakers that our theme is the evolutionary process. The audience is entitled to expect that whatever kind of fossils are being talked about they will be made intelligible to a general botanical/evolutionary audience, and not just to an "in-group" of palaeobotanists/palynologists.

Our papers (see note attached) range from macrofossils to palynology, Permo-Trias to Tertiary, fossil to Recent. The greatest hazard of overlap is in Crane and Tiffney's contributions. I hope that they can let me have abstracts early (I suggest end of March 1987), so that I can adjudicate on any disputed territory. Please all note that abstracts are neded in Berlin by May 31, 1987. I will ensure that as soon as they become available to me, these will be circulated to all contributors to our symposium, unless this is done from Berlin.

Funding from Berlin is very limited (see attached sheet headed '3'). All of you will receive an official invitation to speak, from the Congress Secretariat some time betwen September and the end of this year. If I have not head from to the contrary by September 15th 1986, I will assume that you (all six named speakers below) have formally agreed to participate (obviously, subject to having adequate funds!) and I will notify the Secretariat (by their deadline of September 30th) to issue formal invitations. I hope these will help you to get funds locally, but if you are likely to have serious difficulty on travel funds, please let

Continued.....

Telex: 935504 Fax: 0784-37520

Hunt Institute for Botanical Documentation

me know now and I will put in a bid on your behalf to Berlin. (Note I have to do this before September 15th 1986).

Please get in touch if you have any problems. I greatly look forward to an interesting, contentious meeting!.

With best wishes,

Yours sincerely,

Bill Chaloner

W. G. Chaloner

Copies: Andersons)
Boulter)
Crane)
Tiffney) Speakers
Traverse)
Eyde)

Walter)
Schaarschmidt) for information
Greuter)

Hunt Institute for Botanical Documentation

31 JUL 86

5-31 Evolution and the fossil plant record (Chaloner,W.G.; Walther, H.)

+ Anderson, J.H. (& Anderson, H.M.) 30.09.86 (ZA) 5-31 15 A
Evolutionary patterns in Permo-Triassic plant life

+ Boulter, M.C. 30.09.85 (GB) 5-31 15 A
Events and evolution: Tertiary plant succession in off-shore drilling, NW Europe

+ Crane, P. 30.09.85 (USA) 5-31 15 A
Rates of diversification in flowering plants

+ Tiffney B. 30.09.85 (USA) 5-31 15 A
Habit, habitat and rate of species appearance in fossil Agiosperms

+ Traverse, A. 30.09.85 (USA) 5-31 15 A
Megaevents, but no punctuation

---

o Eyde, R.H. (USA) 5-31
Fossil record and evolution of Cornus sensu lato

.....

Hunt Institute for Botanical Documentation

Funding and grants: Allocation of funds to Congress participants will be decided at the end of September. The Programme Committee will make the allocation on the basis of demands received from the Symposium Organizers. In order to have any of your speakers sponsored, your statement must be received at the Secretariat by **September 15. This is a stringent deadline!**

N.B.

If authors have already indicated that they request support, or at least that they may need it, this is shown by the letter "f" in front of their name, on the "title list". "(GYB)" following the name means that an application for one of the "Young Botanists Grants" has been submitted. A capital "F" before the name, and a sum following it, indicates grants that have (in a few exceptional cases) already been confirmed.

Please consider the following when formulating your wishes:

For the total scientific programme of our Programme Division (i.e., for 54 symposia plus c. 30 poster sessions) our programme committee has a sum of 61000 DM at its disposal. To this you may add 8000 DM for the "Grants for Young Botanists" scheme (to which 5000 DM will hopefully be added that await confirmation) and 2500 DM for the same purpose that are earmarked for young members of the "Deutsche Botanische Gesellschaft". Make your average - but keep in mind that these funds will not by necessity be equally distributed, but according to actual needs and considering the cost-to-effect ratio.

We shall apply for special funds for invited speakers (and organizers) from the countries of Eastern Europe (including Yugoslavia) and the German Democratic Republic. These, if granted, will be in addition to the above-mentioned sums.

For the "Young Botanists" grants at least, poster authors are also eligible. Please advise on the scientific quality of the applicants in your field, irrespective of whether or not you select them as speakers.

Grants from the "general pot" will have to serve as seed money, and can in no case cover more that a small portion of the total expenditure of Congress participation. Recipients will be informed of the grant allocation in October, and will be asked whether they accept the grant and confirm their participation on that basis. Some negative replies are to be expected: therefore, it is good to have reserve candidates at hand.

Please send the names of those you want to have sponsored in sequence of priority, with a short statement of the reasons of your support and an indication of the sum of money that is required as a minimum.

I look forward to hearing from you soon. Do not hesitate to contact me if there should be any problem or question.

With best regards, Yours sincerely

Prof. Dr. W. Greuter
Secretary of the Congress

Hunt Institute for Botanical Documentation

7 July, 1986

Dr. W.G. Chaloner
Dept. of Botany, R.H.B. Colleges
University of London
Huntersdalle, Callow Hill
Virginia Water, Surrey, U.K. GU25 4LN

Dear Bill:

Having spent the last week or more "with" you (your review of Chaps. I-X of my text), I am really more in a mood to discuss some of that than the Berlin Congress, but I did want you to know that I have yours of 27 June and will get on it as soon as possible. That means in a couple of weeks. (You did a fine job on Chaps. I-X, and I am very grateful!)

This has been a diffficult year indeed. We had the more-or-less surprise wedding in Louisiana of our youngest on 3 June, a trip to the Adirondacks to see to a serious bunch of problems that relate to renting our lake property (Betty and I poured 4400 lbs. of concrete on 24 June, having first handled the dry cement twice to get it to our lakefront). We are now getting ready to make yet another trip to Indianapolis and Michigan re our parents. My Mother still lives alone in the house she was occupying when I met you and CA in Ann Arbor and went to Grand Ledge with you. She is 93 and o.k., but my sister is saying "you must do smoething about Mother!" Etc. In the interstices I have had three Ph.D. students finish up in May-June, and a few other amusements.

All the best for now.

Yours very truly,

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

16 January, 1986

Dr. W. G. Chaloner
Department of Botany
R.H.B. Colleges, Univ. of London
Huntersdale, Callow Hill,
Virginia Water, Surrey
U.K. GU25 4LN

Dear Bill:

I note in recent IOP Newsletter 28, p. 6, that Snigrevskaya has "forwarded to the nomenclature committee for fossil plants" a proposal (?suggestion?) for (required?) indication of fossil or "living" (extant?) types. What is the status of this deal?

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

16 August, 1985

Dr. W. G. Chaloner
Department of Botany
Royal Holloway & Bedford Colleges
University of London
Huntersdale, Callow Hill
Virginia Water, Surrey, U.K. GU25 4LN

Dear Bill:

Yours of 29 July went with us to our little place in the Adirondacks--our "Magic Casements". We now are 100% responsible for it and, despite a plethora of things to do at PSU and the time lost to other family matters this year, we had to go there for a week to mend fences (literally and figuratively).

I'm glad you didn't chuck my poor little suggested paper in what you all call the "dust-bin". I believe there are germs of a good idea or two in the thing, but others sound so erudite on the subject, that I'm scared to death I'll (as my grandfather would have said) "make a hare of myself".

As you say, we should get together one of these years in N. Amer., tho' I realize that such fraternization is understandably not something you seek in Maine. For that matter, maybe Betty and I can get you and Judy to motor over to the Adirondacks to visit us too--it's just right ~~right~~ for a paleontologist's retreat: pink Precambrian granite overlain by glacial outwash sand and erratics. Our lake is rather pretty.

I was interested in your use of an airmail stamp @ 44¢. There is no longer such a thing as domestic airmail postage in the U.S.--(all mail is treated the same--badly). In other words you supposedly wasted 22¢. However, the letter is postmarked 30 July, and we left State College on 2 August, so maybe it helped? Hmm.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Magic Casements, Surry, Me
July 29
85

Dear Al,

Your letters of July 9 (with check – thanks) and July 11 were forwarded here, just received. I like your suggestion for Berlin very much – it's high time palynologists acknowledged that evolution is part of their brief. I'll write you more positively from London. I'm back there Aug 5. One day we must meet up in Maine! Sincerely Bill Chaloner

C35782

U.S.A.

THUNDER HOLE, ACADIA NATIONAL PARK, BAR HARBOR, MAINE

This is one of the most spectacular spots along Ocean Drive. The name "Thunder Hole" derives from the pounding surf.

ME1418D-Color Photo: Paul A. Knaut, Jr. ©

MIKE ROBERTS COLOR PRODUCTIONS OAKLAND MASS

Published by Bromley & Company, Inc., Boston, Mass. 02210

Hunt Institute for Botanical Documentation

11 July, 1985

Dr. W.G. Chaloner
Department of Botany
Royal Holloway & Bedford Colleges
University of London
Huntersdale, Callow Hill
Virginia Water, Surrey, U.K. GU25 4LN

Dear Bill:

Yours of 24 June re Berlin-87 and U.36 was very flattering. I'm not sure I belong in such fast company.

Further, the nature of the contribution I can imagine offering probably will overlap with Mike Boulter's. I enclose a resumé--too long (±500 words), but I couldn't seem to cram it into shorter space. Maybe you could comment. If you want me to work on such a presentation, let me know.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: blurb

Hunt Institute for Botanical Documentation

9 July, 1985

Dr. W. G. Chaloner
Dept. of Botany, Royal Holloway & Bedford Colleges
University of London
Huntersdale, Callow Hill
Virginia Water, Surrey, U.K. GU25 4LN

Dear Bill:

Pal. Ass. 5 & 19 just came. Per my recollection of what I should do, I enclose a check for $10.00 (£3.50 + £ 5.25) x 1.30 = $8.78 + $1.22 for postage and unforeseen exigencies such as an unlikely surge in value of £. Thanks for your help.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

encl: check for $10.00

Hunt Institute for Botanical Documentation

July 85

ROYAL HOLLOWAY AND BEDFORD COLLEGES
University of London

Department of Botany
Huntersdale,
Callow Hill,
Virginia Water,
Surrey GU25 4LN

With Compliments of Professor Chaloner.

These have just arrived from the Pal. Assoc. No. 5 was £1.50 and No. 19 was £5.25.

Hope they are what you wanted.

Hunt Institute for Botanical Documentation

Royal Holloway and Bedford Colleges
UNIVERSITY OF LONDON

Department of Botany

Huntersdale
Callow Hill
Virginia Water
Surrey GU25 4LN

Professor W G Chaloner, PhD, FRS
Professor J D Dodge, DSc

Tel: Egham (0784) 35551

10th May, 1985

Professor A. Traverse,
Department of Geosciences,
Palynological Laboratories,
College of Earth and Mineral Sciences,
Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802,
USA.

Dear Al,

Thanks for your several letters around 2nd May. I appreciate the kindly sentiment in your letter to Roger Jones. Your letter to McNeill, and the suggestion that I comment is hard to follow as I have not received the circular on lectotypification. Nor your comment on the illustration matter. Were these in Taxon? - I haven't seen them.

With best wishes,

Yours sincerely,

Bill

W.G. Chaloner

Hunt Institute for Botanical Documentation

ROYAL HOLLOWAY & BEDFORD COLLEGES
(University of London)

Department of Botany

Huntersdale
Callow Hill
Virginia Water
Surrey
GU25 4LN

Professor W G Chaloner PhD FRS
Professor J D Dodge DSc

Tel:Egham(0784)35551

1st March, 1985

Professor A. Traverse,
Dept. of Geosciences,
College of Earth & Mineral Sciences,
Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802,
USA.

Dear Al,

My secretary has somehow mislaid your letter to me concerning the Pal. Ass. Publication Sales, and I am afraid I cannot remember the exact details of the copies you wanted.

Please can you send me a xerox of your letter and I will deal with it as promptly as I can. I enclose for your further information a xerox of the most recent Pal. Ass. circular dealing with Publication Sales. As you will see, some copies are already Sold Out.

With many apologies,

Yours sincerely,

Bill

W.G. Chaloner

Al - note limit to own copy! (Forget "own use" clause!)

Hunt Institute for Botanical Documentation

27 Februayyy 1985

Dr. W. G. Chaloner
Dept. of Botany, Univ. of London
Royal Holloway & Bedford Colleges
Huntersdale, Callow Hill
Virginia Water, Surrey, U.K.
GU25 4LN

Dear Bill:

Thanks for the copies of Snigerefakaya correspondence. I have a little trouble with understanding, as there may be a missing previous letter, or something. Anyway, I would oppose addition of letters as suggested--anti-Linnean. We could end up back with Tournefort if we're not careful. Further, I oppose all recommendations, as you may recall. I got that way when years ago a certain prominent botanist picked through a Rec. with a passionate speech--I asked why he didn't put it in as an article. He replied, "Oh, it would never go as an article, but if we get it in as a recommendation, it will gain respectability and eventually may go as an article." Slippery business.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

ROYAL HOLLOWAY & BEDFORD COLLEGES
(University of London)

Department of Botany

Huntersdale
Callow Hill
Virginia Water
Surrey
GU25 4LN

Professor W G Chaloner PhD FRS
Professor J D Dodge DSc

Tel:Egham(0784)35551

25th January, 1985

Professor A. Traverse,
College of Earth and Mineral Sciences,
Palynological Laboratories,
Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802,
USA.

Dear Al,

This is Heywood's receipt for your $20.00. I indicated its source but he acknowledged it back to me. As you see I paid £18 for those $20 - we are into sad times!

Glad to see your book advertised for publication in November 1985. I can't wait!

With best wishes,

Yours sincerely,

Bill

W.G. Chaloner

Hunt Institute for Botanical Documentation

The University of Reading

Professor W. F. Chaloner

Dear Bill

Many thanks for the £18 cheque for the Tom Harris Fund. It builds up slowly but steadily.

Yours

With the compliments of
Professor V.H. Heywood

Department of Botany
Plant Science Laboratories
University of Reading
Whiteknights, Reading RG6 2AS

Telephone: (0734) 85123
Ext: 7911
Telex: 847813

Hunt Institute for Botanical Documentation

24 Januayyy 1985

Dr. W. G. Chaloner
Dept. of Botany, Royal Holloway & Bedford Colleges
University of London
Huntersdale, Callow Hill,
Virginia Water, Surrey U.K. GU25 4LN

Dear Bill:

Just read that the Palaeontological Association has for sale Special Papers Nos. 1, 5 & 19. The notice here says the sale closes on 31 December, 1984. That's problem no. 1 Problem no. 2 is that the State College banks charge $15.00 for a check in pounds of any amount. Ridkculous!

Request--could you pick up the phone and order me two copies of Smith & Butterworth (no. 1) @£4 and one each of nos. 5 (Jenkins) and 19 (Kemp & Harris) @£2.50?

If you're successful, despite problem no. 1, drop me a line to that effect. I'll send you a $ check for the amount, plus some for postage, etc. I deduce from other evidence that you have an American bank account.

By the way, if it would ever be helpful, I maintain a Canadian account and could provide a cost-free check in Canadian funds, on the long chance you dver need one.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Dec. 84

I trust your voice returned swiftly, when you got back from Calgary! Whatever your feelings, it made me feel better about my drop-out in Sydney.

Original poster by Tom Browne
for "Cinderella", Theatre Royal, Drury Lane, 1904.

From The Raymond Mander and Joe Mitchenson
Theatre Collection.

Hope to see you before long in London / Maine / Berlin etc.

Best wishes for a
Merry Christmas and
a Happy New Year

Bill & Judy
chaloner

Hunt Institute for Botanical Documentation

19 November, 1984

Dr. W. G. Chaloner
Dept. of Botany
Bedford College, Univ. of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Re your lecture at DC-AASP--could I borrow the slide of "grasshopper eating spores", so that I can have a copy made? I'd have two copies made and return original and one copy immediately. Would be good for teaching purposes.

Good to see you, albeit briefly, in DC.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Life as correspondence

Dr. W. G. Chaloner
~~Botany Department~~
~~University College~~
~~Gower Street~~
~~London WC1, England~~

Summer add.:
c/o Beir
Magic Casements
RFD 3
Ellsworth, ME.
USA

Dept. of Botany
Birkbeck College
Univ. London
Malet St.
London, WC1E 7HX
England

Dept. of Botany
Bedford College, Univ. of London
Regent's Park, London
NW1 4NS

1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 14, 15, 17, 18, 20, 23, 24, 25, 26, 27, 28,
Paleozoic spores 29, 30, 31, 32, 33, 34, 35, 36, 37, 39

Hunt Institute for Botanical Documentation

14 May, 1984

Dr. W. G. Chaloner
Department of Botany, Bedford College
University of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Thanks for yours of 24 April, which took ages to get here. Glad you're not offended by arrangements for C. Norman also seems satisfied. I noted with apprehension that in his letter, he capitalized as follows: "...the Code I propose to outline...." Maybe it doesn't mean any more than the "...for you and I...." that crept into your letter. And from a member of the Royal Society, too!

See you in C. You're right--we should consult beforehand. More later.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: D. J. Nichols

Hunt Institute for Botanical Documentation

Bedford College
(University of London)
DEPARTMENT OF BOTANY,
REGENT'S PARK LONDON NW1 4NS
Telephone: 01-486 4400
Telegrams: Edforcoll London NW1

24th April, 1984

Professor A. Traverse,
Department of Geosciences,
College of Earth and Mineral Sciences,
Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802,
USA.

Dear Al,

Many thanks for your two letters of 9th April. The programme you outline for the ICBN debate sounds fine and I will do my best to fit in with it.

It only remains for you and I to consult briefly beforehand to avoid unnecessary repetition in defending the Code.

With best wishes,

Yours sincerely,

Bill

Professor W.G. Chaloner

copy to: D. Nichols

Hunt Institute for Botanical Documentation

9 April, 1984

Dr. W. G. Chaloner
Dept. of Botany, Bedford College
University of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

The other letter in this envelope covers matters mentioned in yours of 27 March, just received. Several people pointed out that Bill E. is too irenic and therefore not aa effective chairman when the going gets hot.

You say in yours that you enclose a copy of a letter fo NFH. You don't, but a letter-copy came previously in a separate envelope--it seems to be the one you refer to.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

9 April, 1984

Dr. W. G. Chaloner
Dept. of Botany, Bedford College
University of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Have just been on the phone a couple of times with Judi Lentin and once with Doug Nichols. The much-discussed evening session will occur, same day as the abbreviated (two-paper) morning session at which Norman will present his "new code". After considerable consultation with Judi, I agreed that Doug Nichols would make a good chairman. He is a skilled chairman, is not renowned as anti-Norman, and is very interested in nomenclature (indeed, when there is a North Amerrican "opening" I would nominate him for this Committee).

I would propose that you, Norman, Jan J., Bill Sarjeant (if interested) and I make (15 min.?) more or less formal presentations, followed then by an open forum. There is still time to add others or change the format, because the printed program will only have the title: "ICBN vs. Hughes code vs. ????!? A colloquium and open forum on paleopalynological nomenclature. D.J. Nichols, U.S.G.S., Chairman". I hope you are agreed to all this.

I suppose from here on out, Doug Nichols should get copies of any letters about this.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: Lentin, Nichols, Hughes, Sarjeant, Jansonius

Hunt Institute for Botanical Documentation

BEDFORD COLLEGE

(UNIVERSITY OF LONDON)

REGENT'S PARK, N.W.1

With Compliments

of Professor Chaloner's secretary.

I think I forgot to include this xerox in Prof.'s last letter to you. My apologies.

Len Daeth

Hunt Institute for Botanical Documentation

DEPARTMENT OF BOTANY,

12th March, 1984

Dr. N.F. Hughes,
Department of Earth Sciences,
University of Cambridge,
Downing Street,
Cambridge CB2 3EQ.

Dear Norman,

Many thanks for your letter.

I think it could be interesting if you would summarise your alternatives to present usage of binomial nomenclature and we could get a debate at Calgary. I imagine Bill Sargeant would be prepared to join you (though obviously not necessarily on the same ground as that you have adopted) and I assume Al Traverse and Jan Jansonius (and if thought suitable, myself) could 'defend' current practise - though we might need to shift ground in that! I agree it should not be a purely ICBN polarised exercise, attacking and defending the Code.

I hope this won't become a triangular correspondence with much letter-crossing along each side! I'll keep quiet after this letter, and look to see you and Al try to find a slot in the Calgary Calendar.

Incidently, many thanks for passing on news of the rather underpublicised visit of Gould to Clare College, to Carol Thomas.

With best wishes,

Yours sincerely,

W.G. Chaloner

Hunt Institute for Botanical Documentation

10 February, 1984

Dr. W. G. Chaloner
Department of Botany
BedforddCollege, University of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Once again, thanks for your efforts re the famous Linn. Soc. figure, per your nice note of 23 January, 1984.

I'm very glad you brought up the question of the "debate in Calgary". I'm told that only 2-3 formal papers have been offered for that session. What do you suggest we do? Suggest transfer of those papers to a general contributed-paper session and convening of some sort of (evening?) open forum on the subject? (I'm afraid that would fall flat.) Invite Norman to take the floor? I'm somewhat perplexed, as you can tell.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

REGENT'S PARK LONDON NW1 4NS
Telephone: 01-486 4400
Telegrams: Edforcoll London NW1

Jan 23 1984

Dear Al,

Follow-up on your letter of Jan 9 regarding payment for reproducing Lmin. for. figure; I've raised this with the Treasurer — it will be waived. Forget the invoice, and he will confirm waiver of payment to you, direct.

What are your plans for the debate in Calgary? Best wishes, Sincerely, B.M.

Hunt Institute for Botanical Documentation

20 January, 1984

Dr. W. G. Chaloner
Department of Botany
Bedford College, Univ. of London
Regent's Park
London NW1 4NS, U.K.

Baar Bill:

Many thanks for the little paper from "Evolution today"--my dumb little diagrams can now be suitably improved! "R-strategy" and "K-strategy" is really marvellous in-talk, and I am indebted to you for passing it along. The "little paper", joking aside, is very stimulating.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

9 January, 1984

Dr. W. G. Chaloner
Department of Botany
Bedford College, Univ. of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Thanks for the little paper for me to use in improving my teaching slide re your "size and form" ideas. I scribbled furiously in Sydney and apparently didn't do too badly.

A new problem, with which you may be able to help: I wanted to use Fig. 1, p. 56 of your article (1976) in the Ferguson and Muller, Eds., volume in my nascent text. Sent the usual form, as I have successfully to all sorts of places (even Elsevier!). From the Linnaean Society, however, I got a bill, per enclosure!

Would a phone call from you possibly get me out of this? If not, I'll just get our draftsman to flange up a different version embracing the same facts, which are, I suppose, public property. But I wanted to use yours!

So far I've been doing right well at avoiding spending a fortune on this project (only because I don't pay EIT!).

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
enclosure

P.S. I suppose you heard that Rick Pierce, Mobil palynologist, was murdered in connection with a steet robbery in Dallas last month? Also, you may be interested that my post-doc, Carmen Moy (prev. Tavera), had a stroke in Dec. while in Strasbourg working with Monique Shuler et al. Only brain surgery saved her, but she is now more or less normal, thank God. You met her in Montreal.

Hunt Institute for Botanical Documentation

Prof. A. Traverse,
Palynology Lab.
(Excluding Scolecodonts)
Earth Sciences Building
Penn. State University
State College, Pennsylvania, USA

Bedford College, London: Jan 6'84

BY AIR MAIL

TEPCO PRODUCT

POST CARD

Dear Al,

The Indians do make some decent post-cards (especially of the "erotic carvings") but this isn't one of them. Catching up on reading over the Christmas break, I enjoyed your sporomorph / palynomorph item in AASP Newsletter 1 and while I only found myself agreeing with most of it, I liked particularly your point that there are indeed spore genera; I too agreed with Jim at Reading on this. For him there was a Lycospora Free which had an almost metaphysical relationship with Lepidodendron. We'll return to this in Calgary, will we not? Cheers - Bill

मान मन्दिर घाट - वाराणसी
Man Mandir Ghat - Varanasi
(INDIA)

Chaloner

Hunt Institute for Botanical Documentation

Palm House, Royal Botanic Gardens, Kew

Hunt Institute for Botanical Documentation

To the Trustees -

Greetings and Best Wishes

for Christmas and 1984.

Hope to see you in Calgary?
(Are we debating together??).

Bill + Judy Graham

7 December, 1983

Dr. W. G. Chaloner
Department of Botany
Bedford College, University of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

See enclosed xerox--my best effort to make an evolution "handout" from my notes on your lecture in Sydney--did I get it more or less right? (Haven't used it yet.)

Hope you all have a great holiday season. We expect one grandchild during it.

All the best to you and Judy.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl

Hunt Institute for Botanical Documentation

From: Dr R.W.J. Keay, C.B.E.

The Royal Society
6 Carlton House Terrace, London. SW1Y 5AG
Telephone 01-839 5561 Ext. 246 Telex 917876

14 October 1983

Our ref: NB/3/1/LUM

Dear Dr Youmès,

The British National Committee for Biology has recently noted the contents of the provisional annotated agenda for the IUBS Executive Committee meeting, to be held on 28-29 October 1983.

Item 5.2.1 of the provisional agenda concerns the proposal to establish the International Association for Botany & Mycology as a group. The British National Committee would have no objection, in principle, to the establishment of such a group but, in recalling that a prime aim of the dissolution of the divisional structure of IUBS at the 21st General Assembly was to encourage the formation of ad hoc interdisciplinary groups to study specific problems as they arose, it is the view of the British National Committee that the granting of any formal status with IUBS to the International Association for Botany & Mycology should be discouraged as simply perpetuating in another form the divisional structure previously deemed to have been unsatisfactory.

Professor Kelly has been requested to express this viewpoint at the Executive Committee meeting on behalf of the British National Committee for Biology.

Yours sincerely,

Executive Secretary

Dr T. Youmès
Executive Secretary
International Union of Biological Sciences
51 Boulevard de Montmorency
75016 Paris
FRANCE

cc Professor P.J. Kelly

Hunt Institute for Botanical Documentation

3rd October, 1983

Dr. R.W.J. Keay,
Executive Secretary,
The Royal Society,
6 Carlton House Terrace,
London SW1Y 5AG.

Dear Keay,

IUBS: Formation of Botany & Mycology Group

Following our brief conversation last week I wonder if I might express my mild concern at the move to form this resurrected version of the old "Division of Botany" within IUBS. I believed that IUBS had decided, rightly, at the 21st General Assembly in 1982 to abolish the old Divisional structure, leaving the sections to operate within the overall structure. Grobbelaar circulated all section Chairman/Secretaries a while ago, on the formation of this "proposed" "group". The Secretary of the Palaeobotany Section, Dr. M. Boulter, and I (as its President) believed that this was a retrograde move simply backtracking to perpetrate the old botany/zoology structure. We expressed our disinclination to support this proposal to Grobbelaar but I understand that the majority of sections favoured.it.

I am not clear what representation the British Committee for Biology will have at the meeting of the IUBS executive in Paris on October 28-29th, but I hope they will press Grobbelaar for some justification for a step which seems to go counter to the new and more forward-looking structure that IUBS has adopted.

Yours sincerely,

W.G. Chaloner

copy to Dr. M. Boulter

Hunt Institute for Botanical Documentation

20 April, 1983

Dr. W. G. Chaloner
Department of Botany
Bedford College, Univ. of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Thanks for yours of 6 April. We really must promote a typewriter for you, and a short course in its operation. This time I only could decipher about 87%, a new low. However, that still permits me to understand what I need, I think.

The "galleys" were very interesting and seem to show that all is in good shape. The "stratigraphic relations" bit for definition of "fossil" is not satisfying to me. There must be a more felicitous way of defining fossil. Art. 10.1 has an interesting implication:"...the species name... is...the full equivalent of its type." Is Ginkgo huttoni fully equivalent to its type? What of Ginkoites huttoni? Are they both "fully equivalent to the type"? If so, they are indeed fully equivalent to each other. Art. 11's "separate names" isn't really specific enough to make clear what you're tyying to establish with your example. I'm afraid another sentence or two is necessary.

I hope you approve of my most recent deal in AASP Newsletter.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

REGENT'S PARK LONDON NW1 4NS
Telephone: 01-486 4400
Telegrams: Edforcoll London NW1

DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

April 12 1983

Dr. Werner Greuter,
Botanischer Garten,
Berlin-Dahlem

Dear Werner,

Many thanks for your very full reply (23.3.83) to my letter and xerox excerpts relating to Rotularia. I accppt your view on the item of "no need to change the cited type of R. marsiliaefolia" (- I leave the finer points of spelling adjustment to your judgement). But I remain perplexed that R.m. has never been figured (even though I know that this is not required for fossils pre- 1912, from Art. 38.1; but that leaves this species as more than a little enigmatic, since I don't believe that it has ever been figured).

I enclose 17 pp. of the galley/page proofs with some trivial corrections. I imagine that they have already been picked up, but I send them to you just to show willing. I am a little ashamed not to have found more errors - I imagine that you and colleagues have done so already. Some of these raise matters of editorial taste (p. 6, p. 8, p.24) while others are straightforward errors.

Congratulations on completing this incredible task on behalf of us all. You certainly deserve your "holiday" of a spell of field work - I hope that it is productive.

With best wishes,

Yours sincerely,

Bill

Copy; Al Traverse.

W.G. Chaloner.

Typed at home to save time - if not mistakes!

(just received)

Many thanks for New Yorker of Feb. 21 re the Penobscot saga. We do take the New Yorker (just to keep in touch!) but I missed this. Will now read it with new dedication - Bill

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

REGENT'S PARK LONDON NW1 4NS
Telephone: 01-486 4400
Telegrams: Edforcoll London NW1

April 6 1983

Dr. A. Traverse
Pennsylvania, USA

Dear Al,

Thanks for yours of March 25. I am absolutely satisfied that Ginkgo huttoni and Ginkgoites huttoni are the same species. They are based on the same type! I would simply like to have it acknowledged (and not merely be implicit) in the Code that these are two different but legitimate names. The use of each has different implications (although it might be hard to get palaeobotanists to agree what those implications are!).

I am still checking through

Hunt Institute for Botanical Documentation

the galley-cum-page proof of the Sydney Code. I enclose some excerpts for you to get a glimpse of how achan taken at Sydney emerged from the editorial operation. I've marked relevant items in red! As I am sure you understand these are "for information" (unless you see some rip-roaring typo. error — in which case over to Greuter!), not for revision. That must wait for Berlin.

With best wishes

Yours

Bill.

Encl: ICBN Sydney "Galley" pp. 9, 10, 14-16, 51, 52.

Copy: Werner Greuter.

chalcone

Hunt Institute for Botanical Documentation

25 March, 1983

Dr. W. G. Chaloner
Department of Botany
Bedford College, Univ. of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Thanks for yours of 2 March. I really do hope the Calgary thing will be profitable, but am somewhat dour on the subject. Perhaps it could clear the air a bit--can we ever make Bill S. and Norman H. realize that the ICBN can never be a framework for genusboxes, or whatever? It is as if the rule book for baseball were having amendments proposed from the U.K. Cricket Union (or whatever). In fact, that would be too reasonable. Amendments from the South African Field Hockey Union would be a better analogy.

In your paragraph 4 I only have problems with one phrase...."the same species". Philosophically, that bothers me a little. Are they really "the same species"? Interesting!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

REGENT'S PARK LONDON NW1 4NS
Telephone: 01-486 4400
Telegrams: Edforcoll London NW1

DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

2nd March, 1983

Professor A. Traverse,
Palynological Laboratories,
Department of Geosciences,
Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802,
U.S.A.

Dear Al,

Many thanks for your letter, and copy to Werner. I am happy to copy all that sort of silly chat to him - he is quite right that we should copy all letters concerning the Fossil Plant Committee to him, and I will do that in future. (As this letter does just touch on the matter, I will send him a copy.)

I will gladly participate in an open forum on nomenclature at Calgary. As you say, many people are sick of this but an open debate (with pro- and con- speakers, 2 of each) just might ginger up interest.

I am less sure about offering a paper on the "examples" approach to a classification of ICBN. What I have in mind is to fly a kite on some (probably only macro-examples) in IOP newsletter fairly soon to try to ginger reaction there to such a proposal of putting fossil examples in the Code.

I had in mind to offer for inclusion in the Code that, for example, Ginkgo huttoni and Ginkgoites huttoni are both legitimate names for the same species (the one recognising its inclusion in the living, non-fossil genus, the other not). Similarly, Pecopteris unitus is a legitimate name for the fern leaf which may equally be called Ptychocarpus unitus on the basis of synangia borne on it. No doubt other "tighter" examples could be sought.

However, I think Norman wants the debate to track over less conventional grounds, c.f. his proposals for Sydney. That's fine by me. I am chasing the two "reject" citations of Werner. Both look complicated - I suspect one is valid, one not. More shortly.

With best wishes,

Yours sincerely,

Bill.

W.G. Chaloner

copy to Dr. W. Greuter

Hunt Institute for Botanical Documentation

9 February, 1983

Dr. William G. Chaloner
Botany Dept., Bedford College
Univ. of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Yours of 28 December, 1982, arrived here soon after our departure for Germany, where we spent nearly all of January. (Lectures and conferences in Kiel and Göttingen and conferences in Frankfurt.)

Your letter is full of interesting things, nearly all of which probably require specific comment--but in view of the long delay and the pileup at the typewriter, I'll confine myself to a couple:

Your idea of examples to help with interpretation of the Code is very good. I will keep it in mind when the Sydney book comes, and you and I can work on a very preliminary list. S. Meyen would be one interesting guy to bounce 'em off--also Schaarschmidt, Faegri and Jansonius. (By the way, do keep a keen eye out for potential younger members of the Committee.)

Jansonius and Judi Lentin have already set up a proposed session at Calgary re the nomenclatural matters and asked me to be its chairperson. I accepted. I warned Judi that it might die for lack of interest, but presumably Norman at least would prevent that? It would help if you translated your interest in the matter into some sort of concrete public contribution--perhaps it would be a good place to run up the flag on the proposed "example-approach", with heavy emphasis on palynomorphs. I can imagine such a practical lesson in applied nomenclature answering many of the Hughes-Sarjeant criticisms.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: Jansonius

Hunt Institute for Botanical Documentation

11 June, 1982

Dr. W. G. Chaloner
Department of Botany, Bedford College
University of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

The thick packet of stuff re the Code arrived and has been read with pleasure, profit, perplexity and amusement. It looks as though you have done a good job for "us". As ever, I feel that the Editorial Committee takes too much upon itself—and if this packet is representative, the tendency is getting worse. I believe the EC should stick pretty strictly to the minutes of the previous meeting, word for word, except only where changes adopted at the Congress require blending, or where obvious grammatical or other errors exist.

Specifically:

3.2. You are so right about "not form genera". Just "genera" is cofrect. I do hope you won that. "Non-fossil" doesn't bother me. It's redundant but does imply the importance of paleobotany, after all!

7.17. That was a real clanger! Persevere on that. Hope it is not typical of what the EC unleashed.

11.1. The old language was good and should have been left alone. "Separate names" is to my mind ambiguous.

13.3. I never really thought about it until I read the (unnecessary?) revision here, but persons could use 13.3 to support the contention that a "non-fossil" genus cannot have a fossil species, depending on how one construes "name of a taxon"? The present editorial revision makes this sharper. Is it contra 13.3 for a genus (say Quercus) to have a foot in each room?

20.2. Ex 4. The words "under this Article" should be omitted if it's still possible. (I am a little bothered by parallelism here. The other words mentioned are nouns as "technical terms". Lanceolatus is not a technical term as a noun. Hmmm.) To agree with style of Ex. 3, this should read "....Chapman 1952, coincide with technical terms and are therefore not validly published."

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20.2. Ex 4 (continued). By the way, I believe it is not best form to put the dates for a generic name in parentheses when there are no literature citations or other words. Thus, it should be "Lanceolatus Plumstead 1952" and "Lobata v. G. Chapman 1952" (cf. Rec. 50E examples.)

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

q

AT/et

P.S. Perhaps you saw my book review in Science 2-3 wks. ago in which I panned "Geobotany II". One of my complaints was that a new generic name was buried in the book--not mentioned even in the author's abstract. The author is really sore--but you would agree, nicht wahr? "Geobotany II" is not the best place for a new name to appear?

Hunt Institute for Botanical Documentation

BEDFORD COLLEGE
(UNIVERSITY OF LONDON)
Department of Botany
REGENT'S PARK, NW1 4NS

With the Compliments

of

Professor W. G. Chaloner

Hunt Institute for Botanical Documentation

9th June 1982.

Dr. N. O. Frederiksen,
USGS National Ctr.,
Stop 970
12201 Sunrise Valley Drive,
Reston,
Virginia 22092.
U.S.A.

Dear Norm,

Would you please consider the following as an item for the next AASP Newsletter Forum? (I only received Vol.15, No.2 yesterday by surface mail!).

Bill Sarjeant's reply to A. Traverse stings me to answer his accusations against the "Committee for Fossil Plants" of the IAPT, of which I am the Chairman. This is one of the nine permanent nomenclature committees set up by the International Botanical Congress; its brief is to comment and advise the Nomenclature Section on any palaeobotanical proposals. Bill suggests that we did "a lousy job of acquainting ..... other palynologists about proposed changes, and seeking our opinions." This smacks of someone who does not read the newspaper or watch TV and complains "why doesn't someone tell me what is going on?" (In this instance, the counterparts to the newspapers and TV are perhaps Taxon and the IOP Newsletter!). The former is the "official" journal for public notification of proposed changes to the ICBN. If Bill "does not have access to Taxon", I am sorry, but that is hardly our fault. (Surely he could get sight of a copy somewhere at some time; or borrow one from a fellow Canadian?). North America and palynology are both well represented on the Fossil Plant Committee. An excellent review of its working methods was given by Traverse in the IOP Newsletter No.9 of May 1979, pages 7-10. Over the last five years, that periodical carried numerous letters and notes on matters relating to the nomenclature of fossil ~~spores~~ plants. Jan Jansonius, who is a countryman of Bill's and a member of the Fossil Plant Committee, wrote a full review on the application of conventional binomial nomenclature to spores in Taxon 30 (2), 438-48 in May 1981.

Bill Sarjeant writes of "the threatened deletion of the form-genus (from the Code) - has it happened by now?" Did he read Traverse, in AASP Newsletter 15 (1)? (To save him checking, the form genus is still alive and well, and will figure, as before, in the "Sydney Code".

One final point, on Traverse's very fair and full commentary. He says that the Editorial Committee of IAPT is "writing the Sydney Code." In the interests of precision, I must record that we are simply editing it - there is a difference! The Nomenclature Section at Sydney "re-wrote" the Code (in small degree, only). Our job is simply to reconcile these changes with one another and with the totality of the Code. We do not have licence to "write it" with the freedom that that implies.

Over .. .. ..

Hunt Institute for Botanical Documentation

Incidentally, Norm, if you get a further letter from Al replying on similar grounds, don't hesitate to ignore this - or use your admirable judgement to except any parts you wish.

With all good wishes,

Yours sincerely,

Bill

W. G. Chaloner.

Copy to Prof. A. Traverse.

I congratulations on yours contribution!

Hunt Institute for Botanical Documentation

4 June, 1982

Dr. W. G. Chaloner
Dept. of Botany, Bedford College
Univ. of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

It was good to hear (yours of 26-V) that a copy of the Rhynie booklet is on the way. I will of course forward the other copy as you request. However, the barter payment you suggest will not be possible until later--I am not going to Montreal. Can't--AIBS here will take time, and I am also going (with Betty) to Evitt's dino short course this summer (late June-early July). That's all I can do in one summer. Sorry you aren't coming here--probably similar reasoning for you. A post-doc of mine (Carmen Tavera, "the Venezuelan bombshell") will be there and will doubtless meet you if you give her 0.5 chance.

Best wishes as ever.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

REGENT'S PARK LONDON NW1 4NS
Telephone: 01-486 4400
Telegrams: Edforcoll London NW1

May 26 82

Dear Al,

I am to-day mailing two ⊕ copies of my little booklet on the Rhynie flora — by surface post. Don't worry about the £ — till later payment in kind in Scotch when we next meet — maybe in Montreal this summer? Alas I am not now going to the meeting at State College.

Life keeps very busy — meetings exams, committee etc — little research. Werner Greuter keeps us all busy too at ICBN.

Bill. Chaloner

⊕ Can you pl. mail one to Steven Schindler - address in encl.

Hunt Institute for Botanical Documentation

5 May, 1982

Dr. W. G. Chaloner
Department of Botany
Bedford College, Univ. of London
Regent's Park
London NW1 4NS U.K.

Dear Bill:

Any chance of a copy of your "Plants invade the land"? I am quite prepared to pay and thought only to avoid the charge ($) for a check in sterling, because I know you have a $ account. Just let me know how many $ for book and postage.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

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DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

12th October 1981

Professor A. Traverse,
Department of Geosciences,
Pennsylvania State University,
College of Earth and Mineral Sciences,
University Park,
Pennsylvania 16802
U.S.A.

Dear Al,

Many thanks for your letters of the 7th and 29th September and the copy of your letter to Ed Voss. I am floundering around trying to catch up after seven weeks absence throughout the summer - and I am appalled to realise that I have been out of College for four out of eight months. China was indeed an exciting experience - socially and palaeobotanically speaking. They have got Carboniferous and Permian coalballs and are well on their way to tackling them. Palaeobotany and palynology are certainly flourishing there!

I am most grateful for your notes on the meeting in Sydney and I assume that our report going to Taxon will simply be that of the Committee dated 19th August 1981. Thank you also for the Nicholson and Reymanowna letters, which help to keep me in touch with what is happening.

I hope to write a brief note for the I.O.P Newsletter reviewing what happened to nomenclature matters at Sydney; if there is time I will let you see a draft (but if you would prefer it, why don't you write such a note and mail it to me and I will relay it to Mike Boulter?) If I do not receive this within the month I will do something off my own bat, but I doubt if there is time for us to shunt a draft version too and fro. Anyway, it is not all that important.

Kind regards,

Yours sincerely,

Bill

W. G. Chaloner.

Hunt Institute for Botanical Documentation

xxxxx
new! 863-3419

Dr. W. G. Chaloner
Department of Botany
Bedford College, Univ. of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Yours of 12 October took the usual eight days to get here. Glad to hear that you are home safely. You don't comment on what Judy thought of China.

Regarding the proposed note for IOP Newsletter. I started writing same and called Dan Nicolson at the Smithsonian just now to check some of my facts. He was very helpful, but he also said that I would have the new Taxon, with Grenter's complete summary of results by 5 November, and that really should be used to check our story. (You probably won't get yours for a few more weeks.) May I urge therefore that you give me a little more time to be able to use the Geruter article? As soon as I get it, I'll finish writing and send the blurb off to you. You can change it as you like (within reason!) and then give it directly to Mike as a joint offering (I'd like that).

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
P.S. Bruce Cornet was here last week. Doug Nichols this week, and Dave Dilcher next week--exciting! Norrie Robbins passed her thesis defense last week.

Hunt Institute for Botanical Documentation

29 September, 1981

Dr. W. G. Chaloner
Department of Botany
Bedford College, Unkv. of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

I'm not sure it is of any use to you--you probably remember more--but herewith my typed-up notes of IAPT committee sessions in Sydney. Can't do any harm. Some of it is ±cryptic.

Best. Hope you survived all that wining and dining in PRC and that you can now wash your mouth plus or minus effitiently and adequately.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: notes

Hunt Institute for Botanical Documentation

28 May, 1981

Dr. W. G. Chaloner
Botany Dept., Bedford College
University of London
Regent's Park
London NW1 4NS, England, U.K.

Dear Bill:

Thanks for yours of 13-V. Enclosure will bring you partly up to date on my personal situation--see also my other letter of 28 May enclosed in this mailing.

I am mortified about Dr. Schaarschmidt's first name//he had alreadz pointed it out, plus some other errors in my letter. I like him, as I've said before.

What's the latest on Galtier?

You are right about the prelim mail vote//just a straw vote. Has no weight at all, and you personally are in no way--not even morally--obligated by your vote. I won't have voted at all, I suppose.*

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology
Secretary, IAPT-CFP

AT/et
encl: AT health bulletin

*note from "et": The Feb. Taxon with the mail vote enclosure has just come to light in the mighty stacks of mail I am sorting at the office. Perhaps Al will get his vote in, after all.

Hunt Institute for Botanical Documentation

28 May, 1981

Dr. W. G. Chaloner
Botany Dept., Bedford College
University of London
Regent's Park
London NW1 4NS, England, U.K.

Dear Bill:

Enclosed copy of letter fo Snigirevskaya is self/explanatory. I also enclose a xerox of her card to me. At Leningrad Zhilin impressed me as a sort of combination gadfly/nut, but he could be interesting. On the other hand, he might make trouble for Sergei, who is (meiner Meinung nach) far better. It's your decision, of course.

We arrived in PA on 17th May. On the 18th, iI was violently ill, and on 19th early I was operated on--appendectomy (burst appendix, etc.) I had a rough time, but am better and still intend to fly back to Zurich on 9 June, if the surgeon gives me the green light. I'll be at ETH until 15 July.*

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology
Secretary, IAPT-CFP

AT/et
encl: copies of letter to and card from Snigirevskaya

*Don't correspond with me there. Betty will be in the office at PSU to receive mail and will notify me of any urgent business.

Hunt Institute for Botanical Documentation

25 April, 1981

Dr. W. G. Chaloner
Bedford College, Univ. of London
Department of Botany
Regent's Park
London NW1 4NS, England, U.K.

Dear Bill:

Thanks for yours of 22 April. Sorry that my letter crossed the Atlantic--I should have marked it, "Don't forward!"

It is a temptation to send you corrections of your German, but never mind. I have always said that in use of foreign languages, bravery is more important than ability. I remember in Leningrad that though I was reasonably sure I knew more Russian than you, you dared to use the language in public and I didn't. Besides, as you can see from the enclosed xerox, Schaarschmidt was not greatly impressed withä my German letter--he answered in English! He has now, as you see, formally accepted, and I suppose a brief note from you would be in order. He is not only competent, he is a very nice person--I was very favorably impressed with him and Mrs. S.

Greenland is politically a part of Denmark, but is it not a part of the N.A. Plate?

I am stunned to hear that Taxon for Feb. is (apparently) still not out. I have seen nothing of it in this geologically oriented institute, of course! Doesn't matter, I guess--I strongly believe the starting-point deal can't be considered until Berlin.

We leave for Turkey in a couple of days--will give lectures in Izmir and Ankara--perhaps also in Istanbul. We will also see Cappodocia, Ephesus, etc. Will be back in PA on 18 May for about three weeks. Then back here to middle July. Then back to PA until Sydney.

All ~~abb~~ best.

Yours very truly,

Alfred Traverse
Visiting Professor

AT/et
encl: coppy of Schaarschmidt letter

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

REGENT'S PARK LONDON NW1 4NS
Telephone: 01-486 4400
Telegrams: Edforcoll London NW1

April 22 1981

Dear Al,

Your 'welcome back to Europe' letter was forwarded to Florida. Just to get the spirit of it, I brought it back with me to London where I had to put it at the bottom of a pile awaiting more pressing answers. Hence my writing only now ...

I am impressed that your German has become so fluent. Dann müssen wir alle unsere Erörterung in der deutsche Sprache machen im 1981. In dieser Weise werden wir gleichmassige Verwendung der drei Sprache des ICBN geben! (And don't send me a corrected version of this! Concentrate on correcting my English!).

Thanks for going ahead and inviting Schaarschmidt. That is fine. I will follow this with an invitation to Jean Galleé with a copy to you. (That will await the return of my secretary - and typing - tomorrow).

I think the Boersma and Kemp

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can await the Berlin meeting. We have enough on the agenda for Sydney.
I am still awaiting the Feb. number of Taxon. What is happening?

Of course Greenland is part of Europe. Did you think it was part of Massachusetts?

See you in Sydney

Bill.

Hunt Institute for Botanical Documentation

21 January, 1981

Dr. W. G. Chaloner
Botany Department
Bedford College, Univ. of London
Regent's Park
London NW1 4NS England, U.K.

Dear Bill:

Yours of 12 January only took a week, from your desk to mine! Letters from USA have been taking as long as three weeks! I am somewhat "verstreut" today--Reagan inaugurated, hostages free, and the dreadful news that Jim Urban and family killed in an airplane accident in Wyoming (happened in October, but somehow I missed it in the post-AASP session field work, and rushed departure for here).

I note that you're only "good" for Sydney, while I'm "certain". Hmmm. I'm the only "certain"--at least add, "God willing"! Scary. I also note that I'm "0-10". Couldn't I be "0.5-9.5" on the basis of my cordiatalean work and coal petrology, albeit from the long ago? I have published megafossil names. I mean you gave Meyen a 2 for palynology, after all. Hasn't Hari Maheshwari published palynology--I'm sure he has. Delevoryas has done spores. Etc. Also "Hol." is for Muller and Jansonius like "Eng." for you. Should have been "Neth." Ten lashes with a pickled eel!

Agreed re Stooch proposal and I hope you and I get our way. We need a new starting point like a hole-in-the-head! But that is not enough of an argument to satisfy Sergei and perhaps others.

Re the new members for the Committee: I recall from correspondence a long time ago with Stafleu, that there is no fixed number of members. Especially as we have too many deadwood members who seem utterly disinclined to resign, I believe you would be absolutely justified in "coöpting" two new members (clear with Voss first). Now, as to your list, I know only two personally. Schaarschmidt would probably be good (Pfefferkorn would be better, but as he works in U.S., you would probably not recognize him as German, though he's 100% deutsch?) Retallack is very talented, but if he has any interest in nomenclature, it's news to me. Gould would probably be a better choice--why not ring up Geoff Playford at Cambridge and discuss with him and/or Mary? Of course, either of them (Playford, especially Mary) would be good members too, but for this pesky scale business! I agree we need a French-speaker. Perhaps you should join me at the APLF-APP session in Strabbourg in March--and we'll size up the each, Fr. and de.

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Now for a comment re your comments on Art. 3.2. I really don't understand your point about "Turmae" and all that. Form-genera were specifically excluded from family-assignment, pre-Leningrad, right? So why can't the Code say that "genera" may be regarded as not belonging to a family if desired--in other words to make reference to a family optional, not required. Then some clown can have his Densosporitaceae if it tickles him, but I can say I don't make such a family reference, though I would put Densosporites in the Lepidodendrales, or Lycopsida, or whatever.

Guess that about covers it. Hang in there.

Yours very truly,

Alfred Traverse
Visiting Professor

AT/et

Hunt Institute for Botanical Documentation

23 December, 1980

Dr. W. G. Chaloner
Botany Department, Bedford College
University of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Thanks for yours of 15 December. Betty loved your sage counsel on Heimweh. It cheered her up some from having lost her watch in Basel Sunday (we were visiting Dr. Leuschner).

I also liked your statement on the Storch proposal. But precisely the question is whether there is an overwhelming case. I intend to research a bit, but at the moment I'm underwhelmed.

The Jonker letter was great.

All the best to you and Judy for the holiday season and always. Have a good time in USA.

Yours very truly,

Alfred Traverse
Visiting Professor

AT/et

P.S. We thought you were going to be at Amherst (Bett's father's and brother's alma mater). Now it turns out it's only U.Mass. Remember us to Jim and Audrey W.

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

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DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

WGC/DMH

15th December 1980.

Professor A. Traverse,
Eid. Technische Hochschule Zurich,
Geologisches Institut
CH-8006 Zurich
Sonneggstrasse 5
Switzerland.

Dear Al,

Sorry about the Heimweh - you have probably got over it by now. At least you have the most important element of Heim (in your Ehefrau) with you. I will be in Amherst for a month from 18th February to 20th March before Judy joins me for a week with her family in Florida (I'll need it by then!)

I have your letters of the 4th and 24th November to answer. Regarding the first, there is no remote chance of my researching the starting point in depth at Harvard whilst giving three lectures a week in Amherst! Why don't you research it in the libraries of Central Europe? What better place?

Seriously, I do not think this proposal is such a "must". The whole purpose of an agreed starting date is surely to avoid endless searching and recrimination. My view is that once agreed (like choice of a lectotype) a starting date should be left alone unless an overwhelming case to the contrary can be presented. I do not believe it has. "Justice" to a worthy but departed palaeobotanist, and national considerations seem unimportant to me besides the need for stability.

You will have Voss' letter of the 1st December (I trust forwarded to you?). I will send you my brief reply. I enclose a copy of my letter to Jonker which is, I believe, self-explanatory.

With best wishes to you both for Christmas and 1981.

Yours sincerely,

Bill

W. G. Chaloner.

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

REGENT'S PARK LONDON NW1 4NS
Telephone: 01-486 4400
Telegrams: Edforcoll London NW1

2nd December 1980.

W. G. CHALONER: Lecturing Visit to U.S.A. 1981.

I am going as Visiting Professor to the University of Massachusetts from the 19th February to 21st March 1981. I shall be travelling in the U.S.A. after that date, and I plan to be back in Bedford College by the 6th April.

If you need to get into touch with me during my stay in Massachusetts, my address will be:-

c/o The Department of Botany,
University of Massachusetts,
Amherst 01003,
Massachusetts, U.S.A.

W. G. CHALONER.

Hunt Institute for Botanical Documentation

WGC/DMH

15th December 1980.

Dr. F. P. Jonker,
Brugstraat 2,
4147 AX Asperen
Netherlands.

Dear Fritz,

I am very sorry to hear that you plan to resign from the IAPT fossil plant committee, but well understand your situation. Do I understand that you plan formally to resign after Sydney (August 1981)?

I am very anxious that we should have as full a complement of members as possible at Sydney to sort out our nomenclature tangles. If you will definitely not be coming, how would you feel about resigning with immediate effect, so that I might try to replace you with someone who will be present at Sydney? Please do not feel that I am hustling you along - for obviously you will continue to make your thoughts known to the Committee by publication and correspondence as in the past. Could you let me know fairly soon how you feel about this?

With best wishes for Christmas and 1981 to you and your wife.

Yours sincerely,

Bill

W. G. Chaloner.

Copy to Professor A. Traverse.

Hunt Institute for Botanical Documentation

24 November, 1980

Dr. W. G. Chaloner
Department of Botany
Bedford College, Univ. of London
Regent's Park
London NW1 4NS, U.K.

Dear Bill:

Yours of 12-XI was on my desk at ETH when I arrived on the 20th--don't know when it had come, but we may be looking at one week instead of three!

I agree with you heartily re Jonker--as you probably guessed, I was trying to be diplomatic and "supportive"--it seemed to me at Cambridge that he had lots of regrets and is a little grumpy about it. But I believe we should avoid any real suggestion of self-selection of successors. I don't have my files, obviously, but I don't believe I made any promises to J. did I? I am sure I made it clear that you have the appointment. You're right that we have enough Dutch influence, and I agree that participation is all-important. Therefore I would counsel no Eastern Bloc persons unless we know they'll be as active as possible--at least return all ballots and questionnaires. I don't know Galtier--any of the others sound good to me, though it's a surprise to hear of Pedersen's interest in the subject. I suppose you should give consideration to the question of subject-matter balance--are we too deep in palynologists? (Mary D. would suit me to a "T" otherwise, and I especially like the idea of Australia, and a potentially active woman--we have none at present.) But to summarize, I think you should make the decision, make the appointment and inform Jonker. I doubt he'll give you any trouble--why should he care? When the decision is made, your final formal letter to the appointee should show copies to me and to Ed Voss. If the letter doesn't make it obvious, a separate note should explain to Ed that this is a replacement for Jonker. (I should get copies of all your IAPTCFP correspondence just to keep me informed.)

Greatly relieved that you didn't mind me poking M.&S. up--though it may have done no good. Also relieved that you found no great problem with the report--I greatly regretted that there was no time to get your comments ahead of time--it would have helped. Fortunately, the Taxon editor is Dan Nicolson, and he phoned me as soon as he got the MS (several days after deadline, but I had arranged that). He picked up the "official opinion" blooper too, and we decided on "review" instead. Of course, I had to depend on DN for this correction--no time to exchange the MS in the mail. I do hope that everybody understands that the "opinion" or "review" is not at all binding. This seems obvious from the fact we vote, not always in accord with the referee.

Hunt Institute for Botanical Documentation

By now you will have seen the copy of the Voss letter I sent you. He is going to be hard to deal with--very acid and disagreeable (over the phone). I believe he's pretty defensive about Art. 3.2, and any criticism of the Editorial Committee. He says that rather than challenge the legality of the "Leningrad Code", the correct procedure would be to accept that Code as the agenda, then later propose the language we want, whatever that has become by the end of the Nomenclature Section. However that comes out, the question of how the Editorial Committee can be kept under control is a big question. My ("terse"?) article in IOP Newsletter hints darkly at the great power of the E.C. ~~in~~ "Refer it to the E.C." should be resisted strongly at the Congresses.

What are your Amherst plans? Dates, etc. I have it in the files, but I don't have the files.

All the best.

Yours very truly,

Alfred Traverse
Visiting Professor
Secretary, IAPTCFP

AT-et

P.S. Hello, Bill. Am using a German Typewriter and can't get used to the fact that y and z are reversed--sorry.

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

REGENT'S PARK LONDON NW1 4NS
Telephone: 01-486 4400
Telegrams: Edforcoll London NW1

WGC/DMH

12th November 1980.

Professor A. Traverse,
c/o Dr. K. J. Hsü
Goeologisches Institut
E.T.H.-Zentrum
Sonneggstrasse 5
Ch-8092 Zürich, Switzerland.

Dear Al,

Welcome to Europe! Glad to know letters will now only take two weeks instead of three!

In reply to your correspondence with Jonker, I can see no point in postponing his replacement until after Sydney. On the contrary, we badly need participation there. Surely, if he is not going to the Congress anyway on his own statement, he would not object to our replacing him forthwith? Would you care to write to him to this effect (assuming you agree with this sentiment) and seek his suggestions regarding Dutch candidates. (However, let us note that we already have one Dutch and one ?ex-Dutch, palynologist on our committee!)

I would, pragmatically, rate highly the value of any candidate who is likely to be present at Sydney. Better any palaeobotanist (who has at least published a name) present at the committee meetings, than some paragon of nomenclatural inspiration who stays at home. Do quiz Jonker on expectation of anyone he names going to Sydney.

On further reflection, I do think having another Dutch member as well as Muller and Jansonius would be excessive! Especially as we have neither French, German nor Danish or Norwegian membership. I would (again) suggest Dr. Jean Galtier at Montpellier; or Dr. Friedeman Schaarschmidt, Frankfurt; or Kay Pedersen ⊕ of Aarhus (who combines macro and palynology with good sense). If one went all out for probability of attending in Australia, then how about Gould or one of the Playfords (too palynological, but very sound)? I would appreciate your thoughts.

Many thanks for handling the report so admirably, and incidentally for chasing our two inactive Russian colleagues. I agree with it fully, though I am hesitant about your varied use of "official referee" (with which we need not agree) and elsewhere the "official opinion ..... obtained for the committee" which sounds rather different, and somehow more binding on the committee. I regard all our referees' opinions as just that, for our guidance but in no sense of greater weight than any other member's opinion.

I do hope Ed. Voss will note and comment on the unresolved matter you refer to in the last paragraph. I am most concerned, as Chairman, to have the constitutional position on what

⊕ I hear he is definitely going to Sydney.

Hunt Institute for Botanical Documentation

happened at and after Leningrad, regarding 3.2, cleared up. If the view of our committee can simply be "edited out" why bother to meet?

Again, many thanks for all your hard work. Have a good sabbatical!

Yours sincerely,

Bill

W. G. Chaloner.

Hunt Institute for Botanical Documentation

8 September, 1980

Dr. W. G. Chaloner
Department of Botany
Bedford College, Univ. of London
Regent's Park
London NW1 4NS, England

Dear Bill:

Thanks for yours of 27-VIII-80!

The references were very welcome and have been duly filed. I guess you are right about "generalities of palynology"--I suppose there are less than a dozen "generalists" in paleopalynology world-wide, and I only need a Pre-Cambrian acritarch paper to solidify my claim to greatest stratigraphic range of publications!

By now you know that my reaction to Meyen's review of Storch is identical to yours. I feel bad about your improvement of Meyen's English. But it was understandable before, and we just gave the original, as you know. (Sergei's English is astonishing.) What should I do with your "gloss"? (I guess when I write my report for Taxon, I'll use it? As you may recall, in that report I only give highlights.)

Whether I can make Voss's(?) deadline for Taxon depends on how the votes roll in--please get yours in! I hope to squeeze in just in the nick of time!

Your "three weeks in Maine" probably overlapped with our few days in the Adirondacks at Betty's parents' place@ One of these years we should get together....

Best regards.

Yours cordially,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Bedford College
(University of London)

DEPARTMENT OF BOTANY
Head of Department:
Professor W. G. Chaloner, Ph.D., F.R.S.

REGENT'S PARK LONDON NW1 4NS
Telephone: 01-486 4400
Telegrams: Edforcoll London NW1

WGC/DMH

27th August 1980.

Professor A. Traverse,
Department of Geosciences,
The Pennsylvania State University,
University Park,
Pennsylvania 16802
U.S.A.

Dear Al,

Many thanks for your letter of the 6th August. The sources are:-

Mesofolles

(1) Gould, R.E. and Delevoryas, T. 1977. The biology of Glossopteris: evidence from petrified seed-bearing and pollen-bearing organs. Alcheringa 1, pp. 387-399, 1977.

(2) Clement-Westerhof, J.A. 1974. In situ pollen from gymnospermous cones from the Upper Permian of the Italian Alps - a preliminary account. Rev. Palaeobot. Palynol. 17, pp. 63-73. QE901-R49

(3) Nidpuria (enigmatic male fructification) Triassic, of India, with Striatites pollen: Pant and Basu, 1979: Palaeontographica B, 168, pp. 129-146.

I am gratified to be asked, partly because I would never dare publish it (the lecture) but also because I fear you keep up with the generalities of palynology better than most, nowadays!

Many thanks for copies of letters relating to nomenclature proposals. I returned from Vancouver and Maine a week ago to find Meyen's comments awaiting me. I gather you already have a copy. As he invited me to "correct his English" I have done this, with some licence, and attach the revised version (which perhaps you will use in place of the original for onward transmission).

I note with mild concern his statement that he "recommended to Storch that he should make ... (this) ... proposal." This should really have disqualified him from acting as referee on it. If I had known, this would have been my view. Anyway, we have his recommendation now, and I think it is fair.

Many thanks, too, for H. H. Thomas' notes for the stockholm Agenda. They make interesting reading. Also for Jim's various offprints, etc. and his copy of the 1972 edition of the Code.

I hope you whip in referee's recommendations in time for the pre-Sydney report.

Over .. .. ..

Hunt Institute for Botanical Documentation

6 August, 1980

Dr. W. G. Chaloner
Department of Botany
Bedford College, University of London
Regent's Park
London NW1 4NS, England

Dear Bill:

I look forward (±urgently) to hearing from Archangelsky and Meyen re their evaluations for IAPTCFP.

However, this isn't about that. On the back of a scrap of paper from Cambridge, I have notes on your splendid, entertaining evening talk. Question: could you please provide literature reference for your statments re

1. Arberiella-Glossopteris-striate bisaccates link-up

2. Lunatisporites produced by conifers

3. "Something-starting-with-M" (I can't read my notes) produces striates (therefore 2-3 "parent groups" for striates).

My colleague, Roger Cuffey, the paleobryozoologist here who doubles as our official-debater-with-fundamentalist-anti-evolution-people, is very upset by your "no intermediate forms" dictum, as best I could recall it. Hmmm.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

11 June, 1980

Dr. W. G. Chaloner
Department of Botany
Bedford College, University of London
Regent's Park
Londdon NW1 4NS, England, UK

Dear Bill:

Under separate cover (two packages), I am shipping you some reprints that are duplicates from my nomenclatural bequest from the estate of JMS. If you have, perhaps a student could use them?

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

9 April, 1980

Dr. W. G. Chaloner
Department of Botany
Bedford College, Univ. of London
Regent's Park
London NW1 4NS, England

Dear Bill:

Is the above a new and permanent address for you? I suppose that is why you attached the little slip of paper with the address. I want to be sure. Unfortunately we have sent you out as of Birkbeck as recently as the last list sent to members of the IAPTCFP.

Yours very truly,

Alfred Traverse
Professor of Palynology
Sec'y., IAPTCFP

AT/et

Hunt Institute for Botanical Documentation

6 February, 1980

Dr. W. G. Chaloner
Dept. of Botany, Birkbeck College
University of London
Malet St.
London, WC1E 7HX, England

Dear Bill:

Re yours of 21 January--first, I could send you a xerox of my letter to JMS of a couple of years ago, asking whether he really meant I should put together a proposal, as he had suggested. I suggested it would be improper, but he insisted no. I tend to agree with you that I was right in the first place.

Second, when we circulate the opinions for a vote a little later, there is no reason why you can't send me the opinion on Meyen/Traverse anonymously, that is, with name removed. As I have previously said, I intend to give all the opinions anonymously anyway--in the latter with the "ballot".

Late-breaking news here--I have a sabbatical leave for Nov., 1980, to May, 1981. Will be in Zürich, Switzerland. Naturally, I am eager to keep this from interfering with IAPTCFP affairs! I guess the folders involved are few enough that I can take the essentials with me and operate more or less normally. But I thought you should know.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology
Sec., IAPTCFP

AT/et

Hunt Institute for Botanical Documentation

Birkbeck College London - Sept 18

9—528
Ц. 6 к.

Dear Al -

Its nice to see that the Russians still allow this small glimpse of Fantasy! Thanks for your letter. I was surprised not to see Traverse/Meyer in Taxon 28(4) Aug 79, just received. Only Hughes' brief reply. Don't agree about a Commission (though I like Phyla). You must snap out of the Transatlantic idioms about Bogart and Bacall before you minute the 1981 meeting at which I will or cannot put your proposals to members when they appear a Cliff-hanger you'll have everyone confused. Best wishes Bill Chaloner

Советский художник. Москва. 1968

А. Н. Бенуа (1870—1960)
Фантазия на версальскую тему. 1906
Государственная Третьяковская галерея
A. N. Benois (1870—1960)
Fantasia on a Versaille theme. 1906
State Tretyakov Gallery

Напечатано в СССР

Hunt Institute for Botanical Documentation

XXXXX

5 September, 1979

Dr. W. C. Chaloner
Department of Botany
Birkbeck College
Univ. of London, Malet St.
London WC1E 7HX, England

Dear Bill:

Thanks for yours of 22-VIII. When I drove through your "area" of Maine in July I wondered if you were there--no matter, I was trying to get through the place without running out of gas and to get to PA on an "Even day" and couldn't stop in any case (but you wouldn't understand....?).

OK re Hughes' proposals. I shall write Jan immediately with a copy to you.

Re Meyen-Traverse proposal, your suggestion of Faegri to referee is fine. This is obviously a proposal for which I should do nothing. Therefore, when and if it appears in Taxon (I haven't even had an acknowledgment of receipt), you should please write to whomever. (Actually, I debated with JMS at length in '78 whether it would be kosher for me to submit a proposal at all. He insisted it would be o.k.)

Don't worry about the Commission--botanists are traditionally as much against that as they are opposed to "phylum", or anything else zoölogical. (However, phylum has gradually won supporters and may go through and into ICBN soon.) I can only say that the zoologists do very well with their Commission and that botanical laissez faire is often maddening (e.g., if Hystrichosporites and Ancyrospora are synonyms, which has priority? ICBN says that the first person who makes a selection must be followed. For years, everybody thinks that Bogart 1969 is first to have made a selection, of H. Then, it is discovered that Bacall in the Antarctic Whaling Journal picked A. in 1968. Stupid. The commission could have made a selection, only theirs would count, and it would be in their book, properly indexed.)

Yours very truly,

Alfred Traverse
Professor of Palynology

encl: cc of letter to J.J.
AT/et

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, F.R.S.
01-580 6622

Malet Street
London, WC1E 7HX

22 August 1979

Dear Al,

I write in very belated acknowledgement of letters of 27 April and 13 June and various helpful correspondence of which I received copies between those dates. Many thanks for handling our elections so peacefully! We now have a good gang, even if the two Great Powers predominate, and we have no Chinese! (Should we invite one of the Russians to withdraw so that we could elect a Chinese member?).

To answer outstanding points:-

(1) I personally am not in favour of a powerful executive nomenclature commission (on the model of the zoologists?) to legislate on matters of nomenclature. Above all, NOT to issue edicts on synonymy. But I will hold my peace on this so that I can at least try to be a disinterested chairman when you put up the proposal.

(2) I believe that we should seek a second opinion on Hughes' proposals, even if only to have a living member on our committee well briefed. I think to offer them to Meyen would really be throwing a Christian to a lion. I would like to suggest Jansonius, both because he has a very real interest in the finer points of nomenclature, and because he is a palynologist - and Hughes' thinking is much oriented in that direction. Also as a relatively new recruit (on the penultimate intake!) he might be less inclined to gibe at such a lengthy assignment!

(3) Many thanks for helpful notes and photocopies on the 1820 problem. I mailed these - somewhat delayed - to Storch. Personally, I hope he'll let that particular sleeping dog lie.

(4) As I haven't yet seen the Meyen-Traverse proposal in Taxon (has it appeared yet?)I still feel amiably disposed towards you both. But this reminds me again of the need for a Chinese representative to review such a Russo-American proposal. But, more seriously, it will need a referee; perhaps Faegri would tackle it? Or should I even ask you if there is anyone you don't want to have referee it?

- over -

Hunt Institute for Botanical Documentation

With best wishes, and greetings to State College colleagues,

Yours sincerely,

Bill

W.G. Chaloner

Professor A. Traverse,
Professor of Palynology,
The Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802,
USA

Hunt Institute for Botanical Documentation

12 March, 1969

Dr. W. G. Chaloner
Department of Botany
Birkbeck College
University of London
Malet St.
London, WC1E 7HX, England

Dear Bill:

Thanks for your ballot. The list of candidates would have been longer had the Soviet nominations arrived in time. I was very embarrassed that nominations sent in by Krassilov, which seemed to be quite thoughtful, had to be ruled too tardy, as they didn't arrive until after the ballot had been sent out. (I had delayed sending it for several weeks after the announced deadline and had decided that no more nominations would come in.) I just today received Val's votes along with a little note plaintively lamenting that his nominees were not on the ballot.

Perhaps our reasoning was faulty, but we contemplated that allowing votes to be concentrated on one or two candidates would increase the likelihood of a clear decision. If that doesn't happen, however, I guess we can all live with a second ballot. To tell the truth, I've never seen this sort of thing get through without a second ballot.

I find your paragraph about Norman's proposals most encouraging. I feared in my marrow that you might be sympathetic to them. Truth is I am even more extremely in the direction of Jim than was dear Jim. I think we took a step in the right direction at Leningrad but didn't go far enough. The organ-and form-genus concept will bedevil us until we simply remove it root and branch from directions for nomenclature. Although the ideas behind both concepts are perfectly (if you'll pardon the expression) valid, they are not really helpful in deciding what is the correct name to apply to a particular taxon. It is apparent to me even when attempting to discuss this question in person with Norman that he simply does not see that application of binomial nomenclature need be no strait-jacket for whatever sort of biostratigraphic arrangements one may choose to make.

Hunt Institute for Botanical Documentation

Another nomenclatural matter I would like to discuss with you in great length one of these days when we get together personally is the possibility of a botanical commission parallel to that of the zoological commission. I am well aware that botanists have prided themselves for almost a century on not having such a thing, but I am growing more and more convinced that this is simply (in theological terms) pride. How nice it would be if a botanical commission existed that could rule once and for all whether Hystrichosporites and Ancyrospora are synonyms or not, and if they are which one is to be chosen. It would be so much neater than our present practice of waiting until somebody makes a choice and then hoping that you haven't missed a prior choice in some obscure journal. Or how about Corallina-Classopollis? Until such a decision is made (which means under present conditions, never) there will be the Corallina school and the Classopollis school, and endless discussions of the matter will go on. The fact is everything has already been said, and it is just a question of opinion. Both sides should have a chance to state their case in court, and then a ruling should be made and have done with it. I won't quote you, but what do you think?

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, F.R.S.
01-580 6622

Malet Street
London, WC1E 7HX

Feb 12 '79

Dear Al,

I enclose my ballot paper — I do not regard it as a secret ballot!

I was disconcerted to see the length of the list of candidates. I suggested four votes on the assumption we would have about eight nominations. I fear we may not get a clear outcome — except for those candidates for whom random casts all four of their votes. (I had not anticipated your ruling that one could place more than one vote on one candidate!). Anyway, lets see how it goes. As you say we may need a run-off vote.

I was disconcerted by Norman's proposals in Taxon, & find myself to my surprise, much aligned with

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the views of our late colleague Jim. I hope we won't need to thrash around over them too much at the Congress; indeed I think they contain in themselves the worst condemnation of any attempt to use the Code to influence what are really not nomenclatural matters.

Many thanks for handling our ballot. Hope it is decisive!

Best wishes

Sincerely

Bill.

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, F.R.S.
01-580 6622

Malet Street
London, WC1E 7HX

Aug 22 '78

Dear Al,

Many thanks for your letter. Glad to hear your ancestors came from Gaspé — that explains that hybrid French/Scottish nit-picking streak of yours! My phrase was "on the south side of the Gaspé peninsula". I used that phrase because many British palaeobotanists have a vague idea where Gaspé is. They have (mostly) not heard of the Restigouche River — where I was collecting on the south shore around Campbellton, New Brunswick. Hence my adding the provincial citation for anyone

Hunt Institute for Botanical Documentation

who cared. So I defend both "South side of the Gaspé peninsula" and "New Brunswick" and protest that there is no contradiction — at least not at the level of a danger!

I'm sorry to hear from Bill Speidel man than the Catalog is to be deprived of your talents at Danger Spotting! I've just had the proofs of vol. 41, two months late by surface mail. I have much of the disquiet of Jan Janssonius re-editorial policy generally, & have said so in a letter to Bill Speidel man. Someday I must hear your version of the story!

Some time we must meet up in Maine on the south side of New Brunswick!

Best wishes Bill Chaloner

Hunt Institute for Botanical Documentation

9 August, 1978

Dr. W. G. Chaloner
Department of Botany, Birkbeck College
University of London
Malet St.
London, WC1E 7HX, England

Dear Bill:

Thanks for the copy of the British palaeobotanical bibliography just arrived--interesting thing and very useful. It was interesting to note that you collected fossil plants on "the southern shore of the Gaspe peninsula". This is where my paternal ancestors came from, and we just spent a few days up there. One clanger in your report though--no part of the Gaspe peninsula whatsoever is in the province of New Brunswick. You must not have noticed the Quebec boundary as you crossed over! Best wishes and thanks again.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

16 January, 1978

Dr. William Chaloner
Dept. of Botany
Birkbeck College, University of London
Malet Street
London WC1E 7HX, England

Dear Bill:

I guess that the copies of enclosed letters to Schopf and Voss tell it like it is. I thought that what we had done at Leningrad was pretty clear, that we had eliminated organ-genus altogether, and had substituted form-genus for what had previously been form-genus and organ-genus. The draft page of the Code which Jim has just sent me does something quite different from that. It was my understanding that in the list of examples all of them were to be regarded as formgenera in the new sense, and that this had symmetry and comprehensibility to it. I guess that perhaps we should go back to the drawing-board, use the term "fossil-genus", or something of the sort, and forget about both organ- and form-genus. At any rate, I am now convinced from Jim's letter, the previous letter from Meyen, and my own recollection of the proceedings in Leningrad that we each must have been drinking a different brand of vodka! Knut Faegri has total recall, I guess, and it might be interesting to hear what he thinks happened. Certainly what is in this draft page of the Code is not what I had in mind. I agree with you that we have merely spawned another batch of proposals, and this could go on forever!

Yours very truly,

Alfred Traverse
Professor of Palynology
Secretary, IAPT Committee
for Fossil Plants

AT/et
encl: copies of letters to Schopf and Voss

Hunt Institute for Botanical Documentation

9 Jan, 1978

to Chaloner, with copies to Schopf (Jim?) and Meyen & M. Boulter

Dear Bill:

I don't understand how it took so long, but your letter dated 15 November didn't reach me until the holiday madness began in December. I have finally got around to meditating on your letter and had framed an answer in my mind when I noticed on my desk also a copy of a letter to you from Sergei Meyen. It makes me feel that what I had intended to say to you is redundant and probably unnecessary because Sergei has said essentially what I would have said. Let me tuck his letter away however and try to say what I would originally have written.

What we intended to do at Leningrad was to abolish altogether the unworkable distinction that had previously existed between form- and organ-genera. Instead, it was our intention to emphasize that all taxa consisting entirely of fossil material are different from those based on at least partly living material. That difference can be a very large one or a not so significant one, depending on age of the material and the nature of the fossils. However, it is clear that in general there is such a distinction. It would be unthinkable to have a genus of extant plants based only on the pollen--I can't imagine that any taxonomist in his right mind would allow such a thing to occur although it isn't strictly speaking illegal under the Code. However, such taxa are the rule with fossil material. It was our intention in abolishing the organ-genus to stress instead of the unrealistic distinction between form- and organ-genera, simply the distinction between fossil genera, and genera based on at least partly extant materials. Because the word is already in the language of the Code, we settled on "form-genus". Perhaps it would have been better to have used just the word "fossil-genus", but that had possible semantic difficulties as no. well.

Therefore, the problem you raise in paragraph two of your letter is not to me a real problem. The distinction between a form-genus and a "genus" is that the former consists exclusively of fossil material.

The list of things given under examples, which you mention in your third paragraph, does not come as far as I am aware, include any which are not fossil genera. They are therefore all form genera within the meaning of that term as we construed it. Possibly we should aim to get a definition of form genus into the glossary, or perhaps even into the body of the Code. The problem we inadvertantly created by the language of Article 3 was formally corrected by the Editorial Committee under Ed Voss, probably with Jim Schopf's help. They changed para. 3 to read: "A form-genus may be unassignable to a family" (emphasis mine). We now mean by "form-genus" exactly what we formerly called a fossil organ-genus.

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I don't agree with you about the "awful mess". I think it is much simpler this way. I think any effort to restore different categories of fossil taxa would be a retrograde step. As to the question you raised in the last paragraph, I expect that Jim Schopf, who has seemingly more time for letter-writing and gets to it faster, has already told you that the draft revised Code is already prepared, and you probably could get a xerox copy of the pages you wish from Ed Voss at University of Michigan (Dr. Edward G. Voss, North University Building, Univ. of Michigan Herbarium, Ann Arbor, MI 48104).

Regarding the P.S. you penned on the letter, I should tell you that I have had nothing to do with the Catalog of Fossil Spores and Pollen for over a year. I will discuss the matter with you at length privately when I next see you. Hoping that this was helpful,and with best wishes for 1978, I am

Yours very truly,

AT pp

Dr. William Chaloner, Dept. of Botany, Birkbeck College, Univ. of London, Malet St. London, WC1E 7HX

Dr. James M. Schopf, OSU, Dept. of Geol. & Mineralogy, 107 zMendenhall Lab., 125 S. Oval Mall, Columbus, OH 43210

Dr. S. V. Meyen, Geological Institute, Academy of Science, USSr, Pyzhevskii Pereulok 7, Moscow 209017

also to Mike Boulter

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, F.R.S.
01-580 6622

Malet Street
London, WC1E 7HX

15 November 1977

Prof. A. Traverse,
The Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802,
USA.

Dear Al,

I am writing to you in your capacity as Secretary of the Committee for Fossil Plants, having finally got round to trying to see the implications of what happened at Leningrad in 1975.

In Taxon 24 p. 690 you say that "a form genus may be unassignable to a family" in replacement of the old definition "a form-genus is a genus unassignable to a family". Since the organ-genus is now totally abolished (alas) I now seek to see a boundary between "form-genus" and "genus" (these being our only remaining taxa at generic level). That "may be unassignable" makes it clear that the assignability at family level is not the definitive distinction. But I don't know what is!

I don't understand your emendment in Taxon under "Examples", since you cited "Lepidocarpon ..... to ..... Dordoxylon" etc. but "delete all reference to form- and organ-genera". The code will therefore cite these seven genera (three given family assignments) but without saying which is a "form-genus" and which just a "genus"; one cannot guess which, since the definition makes it clear that family assignment is not relevant. As "examples" to help one grasp the form- genus concept, these seven hardly help!

I really feel that we are in an awful mess. The present confusion is worse than the ill-defined situation of Art. 3 in the 1972 Code. I plan to write a provocative letter to the IOP Newsletter under the title "Bring back the Organ Genus", to try to stimulate new proposals for 1980, and to show that we are really no better off than we were before.

Can you clarify your intention in the Taxon 1975 note, and thereby dissuade me from such rash letter writing? And can you tell me whether a revision of the Code is in press, (and if so, in whose hands) and whether a draft emended Art. 3 now exists? I am sending copies of this to "interested parties" below.

With best wishes,

Yours sincerely,

Bill

W.G. Chaloner.

P.S. - How is IAC doing?

c.c. J.M. Schopf

Hunt Institute for Botanical Documentation

15 February, 1977

Dr. William Chaloner
Botany Department
Birkbeck College
Malet Street
London, W.C., England

Dear Bill:

Thanks for your card with the congratulatory message. I also enjoyed thinking of Nevsky Prospekt again. I was mortified not to be able to attend the meeting in Lucknow, but it seems to have been providential that I wasn't there from all accounts. You are right about the trip to England in 1981 (the meeting just passed was as of 1976, as I understand it!). I had really hoped that the meeting would be in Calgary, but Betty seems to be delighted that it's set for U.K.--she's never been, and this will be the chance!

Best wishes to Judy.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

20 PARKGATE BARNES LONDON SW13

II 76

Happy memories of shopping in УНИВЕРМАГ — Many (belated) thanks for family news and your new address. Hope it brought you nearer to the liquor store. Congratulations on your new ICP office! The only good thing you may get out of it is a trip to England in 1982(?). Hope you are not snowed in. Love from the Gradoners.

СССР. Ленинград. Невский проспект
UdSSR. Leningrad. Newski-Prospekt
U.R.S.S. Léningrad. La Perspective Nevski
Nevsky Prospekt, Leningrad. U.S.S.R.
URSS. Leningrado. Avenida Nevski

Цена 3 коп.

Hunt Institute for Botanical Documentation

February 26, 1976

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London W.C.1E 7HX
England, U.K.

Dear Bill:

We have finished the alterations on Volume 40. You and Jansonius get the prize for finding the most errors, and I really do think that between the two of you, you caught all the real bloppers. We have been training a new secretary, and I subpect that some of the errors are a result of that plus the transitional period when we had no regular person. It is not really possible to comment in detail, as you can appreciate, but I can only tell you that we have made all of the corrections which you suggested with a relatively few exceptions where we didn't agree. Many of the things that you found were also found by Jansonius, but in some instances he took a slightly different view, and the comparison of the two was very helpful in achieving the best possible result. Some of the spelling mistakes were very embarrassing and hard on the morale of an old purist like me. Thesmisspelling of a word standing right next to the word misspelled was especially embarrassing, and I am glad that it at least furnished you with a slight amount of merriment! (By the way, in this country spelt is spelled spelled!) Regarding the efforts we have made to clean up Malyavkina's infraspecific units, we have not changed formae to varieties unless there was at least one variety already in the species. You are, of course, correct that Malyavkina didn't have much notion what she was doing at the infraspecific level. (I also would like to make you aware that in my opinion there is no place for infraspecific taxa in palynology! It has therefore been extremely hard on my blood pressure to spend so many hours trying to straighten out things which are not worth straightening out.

On p. 40:36, we have decided to leave things as they are, despite your recommendation. Figure 18 was apparently stuck in by Malyavkina to show what the spore looked like with a perisporium. Its illustration did not affect the nomenclature, in our opinion.

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On p. 40:165, you have brought up a very interesting point which I have run past Stafleu at another time, and am still not certain what the truth of the matter is. The question is, can a forma be validly published under a new combination which was not validly published? I originally thought that if it was perfectly clear to what species it was referred it could be validly published, but Stafleu has decreed that I am wrong.

The preferred spelling of specter is specter in the U.S.

I have agreed with you on p. 40:140 regarding notations for the plate numbers, practically over Ames' dead body.

On p. 40:146, *Cycadaceae-lagenella* is said to represent pollen from a complex of modern cycads. Hence, the case is different from that presented on p. 40:146, in which a single genus is mentioned.

On p. 40:136, we have corrected the business about the spelling of *Encephalartos*. The problem is that Malyavkina spelled it two different ways on the same page. So we have just gone to using quotation marks and her spelling. If you use quotation marks, you must use the author's spelling.

On p. 40:65, you are quite right about the rank designation not being explained in the Compiler's Preface to Volume 40. This is an unfortunate thing, and I regret that we didn't work those footnotes differently. It would now be a herculean task, however, to redo them all. The only defense I can offer is that ~~in~~ the Compiler's Preface to this volume does explain where one goes to get the full information.

On p. 40:29, you bring up the specter of the organ genus. We have added a page to the Compiler's Preface in which we explain that, despite the findings at Leningrad, we do not propose to change this volume of the Catalog. Indeed, as I understand it, the Code from t Leningrad will not be effective until it is published, just as the Seattle Code is the 1972 Code, not 1969 Code.

Regarding the spelling of fern sub-family for *Dicksonia*--your spelling is not the one that should be used either, but the real problem is that Malyavkina's name is not a sub-family, but a tribal name. We have therefore decided to spell it exactly the way she did, with quotation marks, as being the only feasible approach.

You are quite right about the hard to understand paragraph on p. 40:66, and we have rewitten it.

Many thanks for your assistance with this volume despite your negative feelings. Like Everest, Malyavkina's work was there and had to be dealthwith!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, Ph.D.
01-580 6622

Malet Street
London, WC1E 7HX

Jan 6 '76

Dear Al,

I enclose the corrected 'proofs' of the Catalog. May this be the end of the Malpighiaceae contributions! Your efforts to reconcile the activities of such an addlepated systematist with the orderly scheme of the Code deserve a medal. I just hope it was worth it!

I have cut out the offending pieces to reduce weight — all are identified by inclusion of the page number. The range from trivial uncertainties of

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meaning to typographic errors.
Good luck with them.

Best wishes to yourself,
Bill, Tate et al -

Bill.
Chalmers

Hunt Institute for Botanical Documentation

December 2, 1975

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London, W.C.1E 7HX
England, U.K.

Dear Bill:

Your proof copy of Volume 40 of CFSP is on its way under separate cover. As in the past, kindly return the pages on which corrections must be made, with your red-pencil notations. Let's set 10 January as the deadline for return. Needless to say, we greatly appreciate your careful editorial help.

Happy holidays.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Hunt Institute for Botanical Documentation

October 24, 1975

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London, England
W.C.1E 7HX

Dear Bill:

Thanks for your services as a "nomenclator"--I could sure use you on Sunday mornings. I offended Ms. Watson by chiding her (only semi-seriously) for ripping off American paleobotanical problems. Of course, with Srivastava dabbling in Mull lignite, what can we expect?

Kay Pederson was the Dane all right--I liked him too. You weren't paying enough attention to her, though--she definitely told me she works (amonggother things?) on Cenozoic seeds.

Off to Houston for AASP--am taking Betty. I have been offered visiting professorship at E.T.H., Zürich, 1976-77, and am entitled to sabbatical--would you do it? They will pay me the 1/2 salary I would lose here, but I hear horrendous tales about cost of living. E.T.H. itself quoted me $1000/mo. for a 2-BR apartment in downtown Z!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, Ph.D.
01-580 6622

Malet Street
London, WC1E 7HX

15 October 1975

Prof. A. Traverse,
College of Earth & Mineral Sciences,
The Pennsylvania State University,
Deike Building,
University Park,
PENNSYLVANIA 16802,
USA.

Dear Al,

The Oxford English Dictionary defines a nomenclator as a "client in ancient Rome charged with naming persons met". (This, in contradistinction to nomenclature). I am flattered that you think I'm capable of functioning in this way (as well as in the sphere of nomenclature), as I have always thought how helpful it would be for me to have a nomenclator myself, especially at congresses! (This is, incidentally, a good reason for using the English "nomenclatural" rather than the American "nomenclatorial").

But to come to the point - the photographic young lady was indeed from Manchester. I imagine she had contrived to be in the photo. Her name is (Dr./Mrs.) Joan Watson; ex-Harris student, author of a Brit. Mus. Bull. (Geol.) paper on our Wealden flora (her thesis work). Address: Geology Department, The University, Manchester.

The two Danes are, I imagine, the "couple" from Aarhus University. He (with a beard) is Dr. Kay Pedersen (Geology Dept., Aarhus); Pre-Cambrian of Greenland and not, I believe, Tertiary at all. She is a plant physiologist; I don't remember her name. I think they enjoyed one another's company for purposes other than talking palaeobotany. (They had a car in Leningrad, which may help identify them). I presume this is the couple. I like Kay very much - a most convivial fellow, and well-informed palaeobotanically.

Don't worry about your age and forgetting girls' names. It is when you don't even notice them that you want to start worrying.

Thanks for your letter of 7 October with news and comments.

With best wishes,

Yours sincerely,

W.G. Chaloner.

Hunt Institute for Botanical Documentation

October 7, 1975

Dr. William Chaloner
Botany Department
Birkbeck College
Malet Street
London, England
W.C.1E 7HX U.K.

Dear Bill:

Thanks for yours of 16 September, which awaited my return from field work in North Dakota.

Since you wrote, I have heard from Stafleu, stating that both Krassilov and Mchedlishvili have been put on the committee, based on the action of the nominating committee at Leningrad. As you say, I suppose it doubtful that either one of them will make it to Australia, but one can always hope. Of course, there were those underprivileged persons from backward countries that did not make it to our deliberations in Leningrad either....

I am troubled by your comment on the report adopted at Leningrad, though I agree that we must monitor the editorial revisions carefully. As far as I can recall, the report meant just what it says. No clear distinction between form-genera and "other genera" was to be drawn. The examples were to be listed as is, without comment, as genera of fossil plants.

I gather that I do have your consent to sending out the little report to the committee at this point. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

cc: Jim Schopf

Hunt Institute for Botanical Documentation

October 3, 1975

Dr. William Chaloner
Botany Department
Birkbeck College
Malet Street,
London, W.C.1E 7HX
England, U.K.

Dear Bill:

I just got a color picture of one of the meetings in Leningrad, along with an identifying overlay. It apparently came from a female paleobotanist from England--I think Manchester--whom I met at the sessions. I seem to recall her last name was Watson. Unfortunatly, she did not sign the photograph or the overlay, and I am unable to respond without her full name and address. I probably should remember and usually do when the subject is a pretty girl, but perhaps my age is beginning to show. Could you provide the lady's name and address so that I can respond to her? Also, do you remember the names of the two paleobotanists from Denmark who were much in evidence? I remember them very well but not their names. One was a man and the other a woman, both fairly young. I seem to recall that they worked on late Cenozoic material--seeds?

Hoping to hear from you about these minor problems and with best wishes as always, I am

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Hunt Institute for Botanical Documentation

December 30, 1974

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London, W.C.1E 7HX
England, U.K.

Dear Bill:

I must write this letterwwhile I am still in the glow of gratitude for the smashing job you did on the review of Volume 39. I really wish I were in a position to send you a case of something or something. I don't know what we would do without you. We have naturally accepted almost all of your suggestions because there really was no way out of it! Comments in detail:

1. On p. 117 we note that an item was not validly published in Malyavkina, 1949, and it is nevertheless treated, and you raise the question of why this should be done. We explain the situation in a footnote on a different page, and, besides, our policy is to cover everything that has been effectively published.

2. We seem to have an honest difference of opinion on the use of the word "unlikely" in one sentence on p. 56. It seems to me that when you look at the whole structure of the sentence that unlikely is okay. Inappropriate would also be okay. It is true that, as you say, it had already been done to include them within the same genus, but I put the word "should" inthe sentence for just that reason. In other words, this is one of the few cases where I decided to stick with what I had.

3. On p. 79 the problem is that we did not make sufficiently clear to you, I guess, what we were doing. The practice with the infraspecific units was to change the forma typica to variety typica in all cases where the rest of the infraspecific units were varieties, and that is the case in almost all instances. In the case you quote on p. 74, however, Malyavkina for some reason had them all as forms, so we kept her ranks.

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4. On p. 99 you have noticed something that hadn't occurred to me nor to any of the other reviewers. While you are certainly correct in your observation, and it is embarrassing, I don't believe we will cut the whole volume apart to correct the situation. It's one of those historical things that I am afraid ~~that~~ even Sherlock Holmes would never have figured out after the fact. When we first did this volume and Volumes 38 and 40, we were carrying Malyavkina's pesky Greek prefix letters as part of the infraspecific names, viz. alpha, beta, etc. Then the names were alphabetized according to the English alphabet, inasmuch as we had transliterated (or whatever you would call it) the Greek letter alpha into the expression alpha hyphenated to the infraspecific name. At that point somebody at the Smithsonian caught me at this and pointed out that the alphas, etc., were not in any sense part of the name according to long standing botanical tradition (although Malyavkina obviously thought that they were!). It was urged at that point that these Greek letter equivalents should be wiped out, which we did on the existing formats. We did not realize at the time and had not realized right up to the present moment that this of course left the infraspecific units in a rather perplexing order. Since they are all properly labelled, and there are seldom more than four of them, I don't think it will cause any real problems. I guess what they really should be is re-alphabetized because there are two orders in which they appear in Malyavkina--one in the captions and another in her text!

5. You and Kosanke have both jumped me on the letter a appended to the date 1960 for Synopsis III. This is a very difficult problem. People really should avoid publishing more than one paper a year! After long soul-searching, I decided a year or two ago to use the suffix letters for all Catalog purposes. I have a standard file of cards which are used for producing all the bibliographies for the Catalog, so there is no confusion at least within the Catalog. I suppose it could be argued that one should use the suffixes only when more than one paper from the same year happens to be quoted, but that creates problems as you can imagine. I think I am just going to stick with what I am doing and let the devil take the hindmost.

As far as all the rest of the changes are concerned, I can only repeat how ecstatic I am that you found these silly mistakes which had passed through approximately eight people without being noticed. We all got a big laugh out of the page which had "the" species instead of "type species"!

You really are doing an outstanding job, and I hope the future will provide an opportunity for me to acknowledge this in a more concrete way somehow or other.

Best wishes of what remains of the holiday season when you get this!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

October 7, 1974

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London W.C.1E 7HX, England

Dear Bill:

Under separate cover we are sending you a "proof" copy of Volume 39 of the Catalog of Fossil Spores and Pollen. Will you follow previous procedures by sending us ~~back~~ pages which you feel need corrections, along with an explanatory letter? We would very much like to have your critique by mid-November.

You may have been wondering what has been happening with Volume 38. It came back from the printers about 6ix months ago, but we have been unable to obtain the binders from the General Binding Corporation despite pleas, threats, and prayers. The current lie is that they will be delivered on 9 October. Once we get this order filled, we are taking our business elsewhere.

Looking forward to hearing from you about Volume 39 as soon as possible, and with best wishes I am

Yours very truly,

Alfred Traverse
Professor of Palynology
Editor, CFSP

AT/vsi

Hunt Institute for Botanical Documentation

January 18, 1974

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London, W.C.1E 7HX
England, U.K.

Dear Bill:

Thanks for yours of 9 January regarding your comments on Volume 38 and other matters. We'll need all the luck and good wishes we can get on the Supplementary Volumes. Almost by definition, the items which we will treat are things which Kremp et al. passed by the first time around because they were regarded as "too difficult".

We have been waiting with special interest to hear from you regarding Pollenites. I must say that the rest of the committee has not shed any new light. Your position is interesting, but I can't decide what to do about it. It seems to me that the critical matter on Pollenites is the question of description. Everything that you say about the way the genus was used in the '30's is absolutely true, but I dontt see how that helps. There just doesn't seem to be any description, and unfortunately it never was monotypic. Itss only sort of a side issue anyway, but Henry Andrews tells me that he feels that his disclaimer in the introduction to the Generic Index was sufficient so that his designation of a "lectotype" should not be regarded as validly published. In any case, typification is not the problem because such was not required pre-1958. Have you any further gems to offer vis a vis the description issue?

Thanks for the letter-copy re Jeffrey's book, of which we are ordering a couple of copies. Your reflections on typification should probably be aired at Leningrad--can you think of a proposal that would have the effect of getting the matter on the agenda?

Hunt Institute for Botanical Documentation

Re your point on "emend."--I wrote Stafleu, and his answer still confuses me, so I am writing again. I find it hard to believe that use of "emend." is actually wrong except where type is excluded. I understand Art. 47 to mean that use of "emend." is not required except in such a case.

Thanks again for your continuing help. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, Ph.D.
01-580 6622

Malet Street
London, WC1E 7HX

9th January, 1974

Professor A. Traverse,
Professor of Geology and Biology,
College of Earth and Mineral Scineces,
Department of Geosciences,
The Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802.

Dear Al,

Thank you for your very full commentary on my corrections to the Catalog. It is nice to know that my dessicated academic jibes are appreciated in the heart of Pennsylvania. It all makes this tedious business of proof reading worthwhile.

I took due note of your national ruling on numerical matters which I will take to heart.

I am delighted to hear from your letter of 13 December that you are at last launching out into some of the more contentious (and hence interesting) areas of palynology. I wish your Supplementary Volumes all success!

Regarding Pollenites, I would have felt that it is quite as valid as is the case with Triletes. The status of the two genera is in some respects similar. In each case, they were used in the 30's in combination with a range of species which were later transferred to more narrowly-defined genera. I think each of those genera was 'valid enough' at that time, that the species in question can be regarded as legitimate names (which is what you seem to be calling to question with Pollenites).

I see from Potonié 1960 (Synopsis III p.18) that P. iliacus Potonié 1931 has been proposed by Andrews (1955) as a lectotype species for the genus. (Incidently, Andrews dropped spore genera from the later, 1970, edition of the Generic Index, so it only appears in that 1955 edition.) My view is that in the 1930's, Pollenites was a legitimate portmanteau name with P. iliacus an an acceptable type. Potonié (loc. cit.) seems worried that P. iliacus was made the type of Illexpollenites in 1937; this seems to offer problems for this latter genus rather than for Pollenites. Potonie is also worried that Andrews' choice of P. iliacus as type was obviously arbitrary (and so contrary to Art.8). But one must accept that Art.8 merely allows an arbitrary lectotype to be superceded - i.e. anyone not liking it may propose a better one. It does not

...2

Hunt Institute for Botanical Documentation

suggest that an arbitrary - and bad - choice of lectotype invalidates the genus. I agree with Faegri (quoted in Potonié 1960) that Pollenites has 'no definite meaning to-day' - but that, too, is no reason for regarding species used in combination with it in the 30's, 40's or even 50's as thereby illegitimate.

I hope this is helpful. Have you sought views of Potonié and Schopf - which would obviously be more so?

With best wishes,

Yours sincerely,

Bill

W.G. Chaloner

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, Ph.D.
01-580 6622

Malet Street
London, WC1E 7HX

9th January, 1974

Dr. C. Jeffrey,
Royal Botanic Gardens,
Kew,
Richmond,
Surrey.

Paperback: £1. W.C.

Dear Dr. Jeffrey,

I have just been reading, with great pleasure, your splendid book on 'Biological Nomenclature' just published by Edward Arnold. It is most valuable for all of us involved in this field and I was particularly interested to learn for the first time some of the divergences of treatment between the three codes.

I wish I could afford to send a copy to all the members of the Special Committee for fossil plants, of which I am a member! I am doing the next best thing in drawing it to the attention of at least the chairman (Jim Schopf) and secretary (Al Traverse). I was also pleased to see the book referred to in the Minute of the IUBS executive at Sofia (11.3) in the context of setting up a committee to study the relationship of the several codes.

I would like to comment on two things that still puzzle me a little. You emphasise, rightly, that the code uses the word 'type' for two quite different things - either tangible (as in the holotype of a species) or abstract, as in a genus representing the type of a family. Yet under your discussion of 'kinds of types' (p.19) you maintain the generality by speaking of an 'element' and yet I get the impression that both you and the code intend this 'type terminology' to be applied only at the specific level. Your definition (and of course the Code's) of 'holotype' would allow it to be used either for a specimen (as in the holotype of a species) or for a species (as in the type species of a genus or 'Generitype' of some authors). But I have never seen it so used, and I have often wondered whether the compilers intended this. Usually palaeobotanists speak either of the type species, or (rarely) Generitype, or commonly and quite incorrectly as the Genotype. This latter horror (in view of its prior use in genetics) is regrettably common among palynologists. I think there is still room for the Code to spell out our 'types' terminology rather more fully.

One point I was particularly glad to see included was your paragraph 7.8. But inspite of this relevant Article (47) many palaeobotanists and palynologists ignore this and claim to 'emend' a genus (and enthusiastically cite an emending author's name) whenever possible.

.../2

Hunt Institute for Botanical Documentation

In effect, any new species brought into a genus - especially if fossil - will expand the generic concept, and this expansion can be spelled out as an 'emendation'. Palynologists, especially, will then add this 'emending author's' name after that of the original author in a generic heading. It makes our author citations very cumbersome and I wish the Code would cite an example to encourage people to regard it as unnecessary.

As you will see, I am being lazy and am using this letter at the same time as an excuse to comment on the matter to colleagues on the fossil plant committee.

With best wishes,

Yours sincerely,

Bill Chaloner

W.G. Chaloner

c.c. Professor J.M. Schopf
Professor A. Traverse

Hunt Institute for Botanical Documentation

December 13, 1973

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London W.C. 1E 7HX, England

Dear Bill:

Tate and I are currently working on a series of "Supplementary Volumes" which will provide new combinations, new names, and other tads of nomenclatural information from the papers covered in volumes 1-30--before we begin covering such data.

There is one very thorny problem we have already encountered on which we badly need your advice before we proceed: many genera have been based on species which were originally attributed by Potonié to the genus Pollenites. Tate and I have so far been unable to discover any proof that Pollenites was ever validly published. If it was invalidly published, under the rules none of the species attributed to it were validly published either. As you can imagine, this plays hob with many generic names published by Thomson & Pflug and others.

Will you please let us have the benefit of your counsel as soon as possible so we know how to proceed. Do you have information that Pollenites was validly published? If so, by whom and where?

Looking forward to getting your advice, I am

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

December 11, 1973

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London W.C. 1E 7HX
England

Dear Bill:

Yours of St. Andrew's Day came while Tate and I were going through your previous batch of comments. I must say that you do us more good than anybody else on the committee in terms of finding embarrassing errors which are kept from appearing in the light of day.

Let's take the 30 November squib first. I agree with your point about the inelegant language and have changed it more or less in the direction you suggest.

Now as to your general point about Malyavkina's names. The difficulty may be that our philosophy has not been sufficiently clarified in discussions between you and me. We have the opinion that all effectively published palynological names should be covered in the Catalog, regardless of how useful the names may be. We obviously, therefore, must cover effectively published names which may well be invalidly published or illegitimate. The question of how good Malyavkina's names are is of course subjective. Regarding the possibility of type specimens, I don't think this is really a hopeless matter because "topotype" material does exist. I have pictures of some such stuff for Corollina. I think it might be worthwhile to attempt to come up with a bunch of such things or, you may be right in concluding that the best thing to do is to let sleeping dogs lie. I do feel, however, that we had to put Malyavkina's names into the Catalog.

I did know about Venkatachala and Goczan and used it in the paper for Anaheim.

Hunt Institute for Botanical Documentation

I guess I must back down on "Corrugate" which I thought was a nice new name for what previously was called Dicatricose. Unfortunately, Corrugate is used in another sense by the Russians, per the Encyclopedia.

Regarding the listing of genera treated in volume 38, I believe we will not add Caytodipterella, although Tate and I discussed the matter in some length. Our feeling is that such a thing is not a primary coverage, and it would be picked up when we do Malyavkina '53.

Regarding p. 38-42, you sure did find a lot of mistakes there, for which we are deeply grateful. What happened was that the footnote reference no. [3] which should have gone after Pflug's name somehow or another got put at the end of the bracketed material, and the footnote showing the true origin of the bracketed stuff was dropped altogether. We have taken care of that, along with the other typographical errors on the same page.

We have taken care of the "section-section" business on p. 45.

We have changed the tense of "designate" on p. 73, ~~dually~~ duly recognizing that Malyavkina is unfortunately among the deceased.

We have decided to reject your suggestion to write the word one on p. 78 as a digit 1. We do this in deference to the U.S. Government bibliographic style manual, which demands writing out all numbers from one through nine, and using numerals for numbers ten and above.

After much discussion, we have decided to go along with you on *nomen nudum*. This is, after all, what the Code does.

We are going to skip adding a footnote reference to the misspelling on p. 85, although we usually do include such a note. It doesn't seem to be worth the dismembering of the page which would be required.

Your note on p. 91 has thrown me into a complete tail spin. I never for a moment thought that the provisions of Article 47 precluded using the notation "emend." unless the type is excluded by the emender. If you are right in your interpretation, there are hardly any real emendations. I refer you to Article 51 which gives as its example (Myosotis) a case where the type is specifically said not to have been excluded. I think the problem is the word "warrant". What is meant is "demand". I have written Dr. Stafleu about this important point and hope that he goes along with me. If he does not, we are in for a big lot of trouble on the whole subject of emendations as used in palynological systematics.

Hunt Institute for Botanical Documentation

On p. 97 we're glad you caught the error in an incorrectly placed ampersand in the figure numbers. On the same page, however, we do not agree with you on the subject of "emend.", as just explained.

On p. 102, we are glad that you picked up the mistake in the spelling of Apiculatisporis.

On p. 165, we appreciate the error you caught in the spelling of heterogeneous. Similarly, on p. 173 the mistake in Aliferina.

On p. 182, we have decided not to make any change in the statement "and the specimen illustrating this variety is identified as specific holotype", even though you are right that this was not added in exactly comparable cases. We do not see that adding a little extra information in some of the footnotes does any harm, although we do try to keep them pretty much the same if possible.

On p. 205, the reason why wennoted the disuse of the name Jurea is that it seemed to be more neglected than any Malyavkina name! We went on to explain that even Potonié does not cover it. There is a good deal of justice in your comment, but we have decided to leave things as they are.

In the bibliography, you have raised a point that other members of the committee also raise regarding the letters on the dates of publication of several of the papers. I have gone round and round about this, and have decided that there is no way to keep these multiple publications within one year (nobody should publishemore than one paper in a year!) except to use letter suffixes. I have put the suffixes on the working cards that we have in the Catalog's permanent files, and they will always be used consistently within our bibliographies. I suppose if I wanted to be consistent about this, I should do it also where the dates appear within the text of the Catalog, in footnotes, etc. What do you think? Thanks for the boo-boos which you discovered in the index.

Have a good holiday season with your family, whom I remember with great fondness and thanks again for all your help.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

XII-1973

To the Travers -

Best wishes for
Christmas and
the New Year

from Bill & Judy
Chaloner

Sphenopteris nummularia

Annularia radiata

Upper Carboniferous, Kent.

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, Ph.D.
01-580 6622

Malet Street
London, WC1E 7HX
30th November, 1973

Professor A. Traverse,
Dept. of Geology and Biology,
Deike Building,
The Pennsylvania State University,
University Park,
Pennsylvania 16802,
U.S.A.

Dear Al,.

I am afraid the enclosed page from Vol.38 somehow missed the post with the last batch and I am forwarding it to you with my apologies. It is not a vital point but might, I think, nonetheless be of interest to you. I hope I have not missed the bus.

With best wishes,

Yours sincerely,

BM

W.G. Chaloner

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, Ph.D.
01-580 6622

Malet Street
London, WC1E 7HX

13th November, 1973

Professor A. Traverse,
Dept. of Geosciences,
Palynological Laboratories,
The Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802,
U.S.A.

ДОРОГИ КОЛЛЕГИ !

So we have got onto trying to exhume Malyavkina! I hope it is worth it, but somehow I doubt it. I have done my best with the proofs, and enclose the usual assortment of offered corrections. Some of them may be a little contentious, some are matters of style which are barely my business (like "nude names") but some are genuine errors.

There is one general point that I feel is worth making. I am very doubtful whether many of her genera are clearly enough defined (even with your carefully pieced-together bits of her key) for anyone ever to feel confident in assigning anything to them. A few very distinctive ones (such as Corollina) may be. Since her drawings are so hopeless, the matter of whether the type specimens themselves really exist is particularly important. The only way in which many of the genera could ever compete with later possible synonyms would be for her types to be refigured. Do you have any direct evidence from Samoilovich or others that they do still exist? As a token demonstration of their reality, it would be nice if slide numbers could be cited.

To take a single example, which you rightly consider at some length, I don't believe that authors ever will (or should) adopt Eresina against Acanthotriletes because we can always 'shelter' behind the inexplicit quality of the figures and the manifestly woolly species concept which she adopts. We can always argue that on the basis of the figure alone - even together with the description - we have no confidence that Eresina is the same thing as Acanthotriletes. Your statement on p.38-164 that Acanthotriletes could be maintained against Eresina only by conservation does not mean that Acanthotriletes will have to be abandoned as you imply; we can all go on using Acanthotriletes because we do not accept the synonymy as being proven (or even provable). In other words, palynologists can take the taxonomic

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../2

decision not to regard the two names as synonyms, and so by-pass the nomenclatural question as to which is the legitimate name if there were synonymy. In the same way, we can go on using Classopollis, because it can be argued that we are not really clear what Malyavkina meant by Corollina. But then I did not hear your paper at Anaheim, and if I had I might have been persuaded.

Incidently, regarding your discussion of Circulina - Corollina you make no mention of Venkatachala and Goczan 1964. Have you seen this paper? (Acta Geologica 8 (1-4), 203.) They at least attempt to separate Classopollis and Corollina as two viable genera. yes.

I will look forward to seeing the next installment.

With best wishes,

Yours sincerely,

Bill Chaloner

W.G. Chaloner

P.S. Thanks for your letter re - the nomenclature committee. That's fine!

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Species name: *Cardiolina trisecta* Malyavkina 1949

[**0.** Споры гладкие; экзина не смятая (упругая), средней толщины — 13.

13. Щель разверзания короткая, длина лучей ее у нормально развитых экземпляров составляет обычно $^1/_2$—$^2/_3$ радиуса; очертание спор правильно- или неправильно-треугольное с округленными (не выступающими) углами; со слабо вдавленными, реже — с несколько выпуклыми, боковыми сторонами—

Род *Cardiolina* gen. n., VI.

Юра — нижний мел (стр. 37, табл. I).][3]

Author: Malyavkina, V.S., 1949, p. 37-38, pl. 1, figs. 14 & 22

Reference: Index of spores and pollen; Jurassic--Cretaceous: Trudy VNIGRI, N.S., no. 33, 138 pp., 51 pls., 3 text figs., 5 tabs.

Type locality: USSR

Type horizon: Jurassic, Middle

Preparation: Not indicated

Repository: Collection of VNIGRI, Leningrad

Holotype: Not determinable

[1][*Cardiolina* Malyavkina 1949 would appear to be treated as a synonym of *Deltoidospora* Miner 1935 emend. Potonié 1956, per Potonié, 1960, p. 28. Although such consideration would imply recognition of valid publication of the genus in Malyavkina, 1949, this was probably not Potonié's intention.]

[2][Descriptions of five infraspecific taxa and an illustration of one subform are included under this combination. A protolog attributed to *Cardiolina trisecta* f. *typica* is treated as that of a variety and its infraspecific epithet is altered to read *C. trisecta* var. *trisecta* per the discussion in the Compilers' Preface. The subform *C. trisecta* f. *typica* subf. *punctata* should probably be treated as a nude name because the only protolog text attributable to it is that given in the caption to pl. 1, fig. 20. The caption states that punctate specimens are recognized as *C. trisecta* f. *typica* sf. *punctata*. If accepted as valid, the name of this taxon should read *C. trisecta* var. *trisecta* f. *punctata* which would require that the name of the taxon including the specific holotype be recognized as *C. trisecta* var. *trisecta* f. *trisecta*.]

[3][Bracketed text reproduced from generic description, Malyavkina, 1949, p. 31.]

TYPE SPECIES OF THE GENERIC NAME *Cardiolina*
(Monotypic in Malyavkina, 1949)

Did Potonié really emend Deltoidospora in the sense of the Code Art 47.

why not just "... treated as a synonym of Deltoidospora Miner 1935 by Potonié (1960, p. 28)..." ?

Hunt Institute for Botanical Documentation

November 2, 1973

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London, W.C. 1, England

Dear Bill:

Thanks for yours of 15 October, which was awaiting my return from the AASP meeting in Anaheim (which I thought went very well). I had indeed received your earlier letter of September 17, but did not mention it in connection with the "Catalog letter" because it seemed to me to be a matter of less priority (!) and could await my completion of the many things I had to do for the Anaheim meeting: 3 papers, 2 lectures to be given afterwards in Utah, etc.

Actually, I had anticipated you by a month--in August I got to worrying that surely I should be doing something, and wrote Jim Schopf (the chairman) to that effect. His answer is enclosed per xerox and still seems (in White House-ese) "operative". I think what I will now do is write Stafleu about this--so your letter did do some good!

Best wishes, old friend. We look forward to hearing about Vol. 38!

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi
Enclosure: xerox of letter

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, Ph.D.
01-580 6622

Malet Street
London, WC1E 7HX

15th October, 1973

Professor A. Traverse,
Dept. of Geosciences,
The Pennsylvania State University,
Deike Building,
University Park,
Pennsylvania 16802,
U.S.A.

Dear Al,

Thank you for your letter of October 3rd and for the Catolog proofs which arrived at the same time. I will certainly let you have emended pages by November 20th.

I sent you a brief letter on September 17th and as you did not mention it in your letter I am taking the rash step of sending you a copy. I do this apologetically, not to press for a quick reply but just because it seems, in the circumstances, that it might have gone astray.

With best wishes to you and you colleagues,

Yours sincerely,

Bill

W.G. Chaloner

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

October 3, 1973

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London, W. C. 1, England

Dear Bill:

Under separate cover we are sending your "proof copy" of Volume 38 of the Catalog of Fossil Spores and Pollen. Please use the usual technique for reviewing the volume--tear out the pages on which you have comments and indicate the changes in red thereon and return the pages to us. Alternatively, if there are only a few changes, you could simply let us know what page and line are concerned in your responding letter. I am hoping that your copy of the "proof" will be in your hands by 15 October, and I am therefore asking that we have your review back by 20 November.

We of course continue to be exceedingly grateful to you for your help in this way, and we ernestly request a thorough and critical review.

Best wishes from all of us at CFSP.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology
Editor, CFSP

AT/vsi

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
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01-580 6622

26th February, 1973

Professor A. Traverse,
Deike Building,
The Pennsylvania State
University,
University Park,
Pennsylvania 16802,
U.S.A

Dear Al,

I am sorry to have been so long in replying to your letter. It's been even worse than a proofs session on the 'Catalog'! I can only plead a heavy bout of teaching which has just ended.

There was really no cause for me to ponder the matter anyway, as it seems completely clear cut. I cannot see any conceivable case for regarding Malyavkina's Greek letters as part of a nomenclatural designation - bi- or tri-nomial! I accept Stafleu's view unreservedly.

With best wishes to you and colleagues in Earth Sciences,

Yours sincerely,

Bill Chaloner

W.G. Chaloner

Hunt Institute for Botanical

BIRKBECK COLLEGE
(UNIVERSITY OF LONDON)
MALET STREET
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01-580 6622

17 January 1973

Dear Al,

Thanks for your detailed critique of my scrawl written on the Catalog proofs. I have no comeback on Vittatina. I fear that your analysis is valid.

Best wishes to you all.

Sincerely,

Bill

W.G. Chaloner

Professor Alfred Traverse,
Department of Geology and Biology,
Deike Building,
The Pennsylvania State University,
University Park,
Pennsylvania 16802,
U.S.A.

Hunt Institute for Botanical Documentation

January 4, 1973

Dr. William G. Chaloner
Botany Department
Birkbeck College
Malet Street
London, W.C. 1, England

Dear Bill:

Thank you very much for your letter of 17 November regarding the mistakes in Volume 37. We continue to appreciate your fine work for us and the assistance that you give us in preventing us from making fools of ourselves in public. I do hope that we can count on your assistance for a long time to come. Comments on your comments in detail are as follows:

Title page: You are absolutely right, and we have changed the title to Paleozoic Megaspores and Miospores.

Compiler's Preface: We had some trouble in deciding what to do about the problem you raised on the first page of the Preface but have decided to go all the way and list seven American states even though the samples which produced the new species, I am not sure that we shouldn't have confined it to just Indiana, but it's not inaccurate to list Michigan too, as you point out.

Spelling of palaeocristatus has been corrected.

p. 10. You are right about the volume and page numbers being missing. We allowed this volume to be xeroxed before pagination had taken place. It was an unfortunate mistake, but it seemed not worth the investment of money to do the job over again since the numbers could for the most part be clearly made out in the penciled version in the upper right hand corner.

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We decided to leave the footnote about the Dunkard Gp. as is. It is quite true that the Dunkard has been regarded as Permian for a very long time--at least 65 or 70 years.

p. 183. We had previously noted the error in the spelling of Potonieisporites, but it is the sort of mistake that I much appreciate your looking for.

p. 188. We have corrected the spelling of Trolard in our bibliography (and, more importantly, on the card which will generate future references to the same paper in other bibliographies).

p. 98. Regarding your comment on our providing unnecessary information under preparation--opaque megaspores mounted on pasteboard slides.arOur feeling here was that we tried to provide the entire preparation technique, not just the portions which apply to the particular slides from which the specimens mentioned were obtained. This particular example is just one of the more obvious cases, and I do suppose it looks a little silly, but we have decided to go with providing the whole picture.

p. 116. The question you have raised about our reference to the Code as the 1964 Code is one that has been brought up also by several of our readers, and I have referred the question to Stafleu for an opinion. He agrees with you and others that it should be referred to as the 1966 Code, and I am going to make the change beginning with the next volume. I must confess, however, that I am stubborn about my "error". I still think the Code should be referred to as of the time of its adoption, since it was really in effect from that time. The publication of the little blue book seems to me to be not the same thing as the adoption of the Code. However, I am a good loser, and I realize that I am in a tiny minority (of one?) on this question. I have also decided not to do anything about a literature reference to the ICBN in this volume, but perhaps I should do so in the future.

p. 151. You were right quite right about the mistake of figure number in Peppers' paper.

p. 196. You are quite right about our language regarding the effect of a transfer. I have consistently tried to avoid language which seems to imply that the most recent paper published is the one which must be followed! I just let that particular footnote slip by. We have changed it to read "the effect of the transfer would be to reduce the genus..."

p. 205. Same comment as above. We have changed the language to read "which, if accepted, reduces Trochosporites...."

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p. 211. We were not convinced by the force of your arguments on Vittatina because we are talking about "Wilson's Vittatina". Clarke & Chaloner apparently didn't recognize that the genus Vittatina was not in the literature. Vittatina Samoilovich, according to our understanding, does not exist. Perhaps you would like to comment more on this subject for our edification.

Bibliography: All of your corrections in the bibliography have been made. You will be interested to hear that your colleague, Kosanke, found over 20 mistakes in the bibliography, enough to make me drink an extra martini before dinner.

Once again, thanks for your help. Please keep up the good work and have a good 1973. Best wishes to Judy.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
University of London

Department of Botany
Professor W. G. Chaloner, Ph.D.
01-580 6622

Malet Street
London, WC1E 7HX

17 November 1972

Dear Al,

Thanks for your letter of November 9. Regarding my move from University College, I hope this letterhead explains my motives; but we are in fact close to University College here, and I still keep close contact with them.

I am sorry that you couldn't visit us in Maine. We shall be there in many future years, as her parents cottage near Ellsworth will pass eventually to Judy. I am enclosing (a record?) sixteen pages from volume 37 which I have just finished. I don't like "miospores and other pollen" incidentally - it contains the same fallacy as the original. Why not "Paleozoic megaspores and miospores"?

Warm regards to Bill, Tate and to the Scholtens.

Sincerely,

Bill

W.G. Chaloner

Professor Alfred Traverse,
Deike Building,
The Pennsylvania State University,
University Park,
Pennsylvania 16802,
U.S.A.

Enclosure

Hunt Institute for Botanical Documentation

November 9, 1972

Dr. William G. Chaloner
Department of Botany
Birkbeck College
University of London
Malet Street
London, W.C.1E 7HX, England

Dear Bill:

Thanks for the information about Paclt's pene-proposal. You took the words right out of my mouth--my opinion would definitely be that the fossil names should not be conserved. Maybe Lepidodendron, maybe Calamites, bur definitely not Hymenotheca, etc. I remember hearing a paper at a Paleobotanical Section meeting in the mid-50's in which one of our members gave a detailed list of at least 25 names of genera in the paleobotanical literature which are homonyms. In time, I suppose it will all get straightened out.

Of course the most intriguing part about your letter was the casually dropped information that you are now associated with "Birkbeck College". I suppose in due time we will hear what the significance of this move is. On the surface it sounds as if you got run out of your nice spot on Gower Street and had to make up this fictional college to cover your traces. Well, I must say it has a fancy sounding ring to it. You always did have a lot of class.

One of my big disappointments in the last couple of months is that we were unable to pop in on you in Maine on our way back from our vacation to Gaspe Peninsula. I really had intended to try to do it, but we spent too much time with my relatives in Quebec and New Brunswick and just didn't have enough time. One of these years, though....

You will be interested to hear that the most recent AASP meeting in Rhode Island worked out pretty well, with 125 registrants. Most of us had been in fear and trembling that by having a meeting in such an out of the way place, with only one resident palynologist, we were asking for trouble. In terms of the physical arrangements, if

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not the quality of the papers, it was probably the best meeting we ever had. I enjoyed it thoroughly. Marjorie Muir put in an appearance for one meeting of the session to 'field'* complaints from our side regarding the ICP and its undemocratic, non-representative, procedures. I believe it can be stated with fairness that Mrjorie got an earful.

Looking forward to hearing from yourabout Volume 37 one of these days, I am with very best wishes

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi
P.S. I just opened yours of 23 October, 1972. You are certainly right about miospores including pollen. How would "Pollen and other Miospores" suit? In any case, we certainly do have to change that. Embarrassing that it slipped by.

* baseball term

Hunt Institute for Botanical Documentation

BIRKBECK COLLEGE
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LONDON WC1E 7HX · ENGLAND
01-580 6622

23 October 1972

Dear Al,

Thank you for your letter of 6 October and the proof of Volume 37 of the "Catalog". I certainly hope to get any troublesome pages returned to you by 1st December. Naturally the content is of more interest to me than usual and I have had trouble on the first page in your use of "miospores and pollen". For me, miospores include pollen! I will raise this and other items with you in due course.

Best wishes to you and colleagues.

Sincerely,

Bill

W.G. Chaloner

Professor Alfred Traverse,
Deike Building,
The Pennsylvania State University,
University Park,
Pennsylvania 16802,
U.S.A.

Hunt Institute for Botanical Documentation

University College London
Gower Street London WC1E 6BT England

File

Prof. A. Traverse,
Catalog of Fossil Spores and Pollen,
Deike Building,
The Pennsylvania State University,
University Park,
PENNSYLVANIA 16802

11th August, 1972

Dear Professor Traverse,

[redacted]

Dr. Chaloner has just arrived and, as you were hoping for a reply by return of post, I thought I would just let you know that Dr. Chaloner has already left for America. You probably have his address there but, if not, it is as follows:

c/o Beir,
Magic Casements,
RFD3 ELLSWORTH,
Maine, USA.

Yours sincerely,

Secretary,
Dept of Botany.

Hunt Institute for Botanical Documentation

August 4, 1972

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

Thanks for yours of 11 July. We have now completed the corrective surgery on Volume 36 and much appreciate your comments as usual. We elected to pass on the misspelled German word (lung for lang), decided that the Triletes Trilites contradiction is sufficiently explained by our existing footnote, noted the lack of capitalization in the genus name Sphagnites (all other reviewers also picked it up!), decided that we would not cover the further new combination of Cingulatisporites problematicus until we work up the Playford and Dettmann paper, corrected the spelling of Palaeozoic (and reflected on what difficulties this slight differences in American and British spelling cause editors like me).

I certainly am interested in your going to Ireland in search of fossil plants--especially Tertiary. I hope to hear more about that one of these days.

I am not planning to attend the Montreal congress for the reason that the preliminary programs made the paleontological pickings sound too thin, except (of course!) for your as always sterling contribution.

However, a few members of my family and I do plan camping trip to eastern Quebec and New Brunswick in late August, and we will be returning through Maine. It would be great fun to see you, and perhaps we could plan to camp for a day or two in a suitable place

Hunt Institute for Botanical Documentation

Fidelity Onion Skin
100% COTTON

near wherever it is you stay. Hoping to hear from you by more or less return mail about this, I am

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

Department of Botany and Microbiology
UNIVERSITY COLLEGE LONDON
GOWER STREET LONDON WCIE 6BT

TELEPHONE 01-387 7050

July 11 1972

Dear Al,

I write in characteristic haste on the eve of a field trip to (Southern!) Ireland, chasing Devonian + Tertiary plants.

This number of the Catalog was dull. I could hardly find a thing. Only 2 bona fide items and three possibles. I enclose the five pages. I hope they are not too late. ~~The~~

We shall be in Maine all through ~~July~~ August. Any chance of your stopping by en route to the Montreal congress? Do let me know if there is.

Best wishes

Bill Chaloner.

Hunt Institute for Botanical Documentation

University College London
Gower Street London WC1E 6BT England

May 22 72

Dear Al,

Draft of
Vol. 36 of the Catalog
arrived To-day, with
your letter. I will
send you any
ensing pages I can
find as soon as
possible.

Best wishes

Sincerely

Bill Chaloner

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

May 17, 1972

Tel:

Under separate cover we have shipped you proofs of Volume 36 of the Catalog of Fossil Spores and Pollen. We would appreciate it if you could return your review to us by 15 July. As usual, we recommend that the best way to do this is to return to us the pages on which you have comments, with the comments noted thereon.

Once again, let me reiterate how extremely valuable to our editorial process your help in reviewing is to us. We have been saved many costly errors, eliminated the necessity of reprinting correction pages, etc. CFSP is thereby much improved.

You may be interested to know that Volume 35 is in the printing process.

Best wishes for a nice summer.

Yours very truly,

Alfred Traverse
Editor

AT/vsi

Hunt Institute for Botanical Documentation

March 1, 1972

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

Your comments on volume 35 were absolutely invaluable! It is, of course, humiliating to all of us that things can slip through the way they do. You found more mistakes than anybody, but it will be of interest to you that Kosanke, Cross, Kremp, and Schopf, all found silly little things that even you missed. I have sent each of them a xerox of your page about Podocrapites. It is quite comparable to a mistake in Hart's own book in which in one of his references to his own name there is an unfortunate substitution of "f" for "h".

Comments in detail on your corrections as follows:

After a long debate, we decided to go with re at least for the present volume. According to our American dictionary this is not actually incorrect, but I believe we will convert to "on" in the future. You were right about the boo-boo on Article 51 in the Nomenclatural Comment--it was a misprint for Article 57, and the appropriate change has been made.

With regard to the final paragraph of the Nomenclatural Comment, you suggested that we delete the final sentence, but we decided instead to tone it down somewhat. The problem we were faced with was that you thought that we were too positive, and Jim Schopf thought we weren't positive enough. We therefore decided that we must have inadvertently taken up approximately the correct position. I do hope you will agree with the way we have rewritten the Comment. We have made the corrections of typographical errors in the bibliography and very much appreciate your having caught them.

Hunt Institute for Botanical Documentation

The same goes for sundry typographical errors in the body of the volume, such as the incredible "HN_4OH" (it still mystifies me how a thing like that can get by five or six readers--and you were the only one of the committee who picked it up!). I am similarly embarrassed about Frankfort. We greatly appreciate your picking up pages on which the footnote reference numbers were missing, the misspelling of Kimberley and other typographical errors, all of which have been corrected. We defer to you regarding the Hilton Plant Bed although we must have picked up the world Series somewhere for it. We have rewritten footnote 2 on page 112 to make it clear that what we should be saying is that the combination is a manuscript name, not that *Verrucosósporites* is a manuscript name.

I really can't overestimate how extremely useful you are being to us, and how much I hope you will continue to serve in this capacity as long as you are able to.

You should be getting volume 36 proofs before too long--I would estimate about a month to six weeks.

Best wishes as always.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

February 3, 1972

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

Thanks a lot for your review of Volume 35. You needn't have apologized for tardiness--in fact, yours was the third review received. I hope to get the others in a couple of days. Volume 34 is taking an interminable length of time to finish the printing process (the printers ran out of paper, of all things!). You should get yours before too long.

It's frustrating to learn that you are so often in the fairly near neighborhood, and yet we never see you. I guess it can't be helped. It is interesting to know that you are giving a paper on Devonian spores from our own back yard. You are quite right in assessing the paleontology part of the Montreal program as disappointing--I wrote a couple of hot letters to the organizers earlier in the game and got nowhere. I doubt that I will even bother to show up.

Everything is about as usual around here. Thanks for your continued helpfulness with the Catalog.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

Feeler :

W. Chaloner to

AT

Ca. 1-II-72

Hunt Institute for Botanical Documentation

CFSP Nomenclatural Comment re *Taeniaesporites*, *Lunatisporites*, and *Lueckisporites*

Published opinions concerning the characterization and nomenclatural status of the principal genera of broad-striped bisaccate pollen (*Lueckisporites* Potonié & Klaus 1954, *Taeniaesporites* Leschik 1955 & *Lunatisporites* Leschik 1955) are varied and confusing. The generic description of *Taeniaesporites* excludes the holotype of the type species (Grebe, 1957, p. 61), and the generic protolog of *Lunatisporites* is unclear. Various authors have indicated that they consider *Taeniaesporites kraeuseli* Leschik and *Lunatisporites acutus* Leschik, the type species of their respective genera, to be conspecific, which requires suppression of one generic name in favor of the other. *Lueckisporites* is generally accepted as comprising forms with a single longitudinal cleft.

Bharadwaj, 1962, p. 93, appears to be the first to make *Taeniaesporites* and *Lunatisporites* formally synonymous, and on the basis of "page priority" recognizes *Lunatisporites* as the name of the taxon. Just two months later, Jansonius, 1962, also published the opinion that *Taeniaesporites* and *Lunatisporites* are synonyms but selected *Taeniaesporites* as the name because the generic characters are better documented. Art. 51 of the 1964 Int. Code Bot. Nomencl. stipulates that the first author who makes a selection in such a case must be followed.

Taeniaesporites and *Lunatisporites* also have been made synonymous with a number of other genera, for example *Taeniaesporites* with *Striatites* Pant, per Schaarschmitt, 1963, and *Lunatisporites* with *Protohaploxypinus* Samoilovich emend. Hart, per Hart 1964. Other names associated with elements of this complex include *Taeniaepollenites* Visscher 1966, a name proposed for *Taeniaesporites* as emended by Klaus 1963, namely for those members of the original *Taeniaesporites* complex which are trilete. General reviews of the problem are to be found in the paper by Visscher, 1966, mentioned above, and Scheuring, 1970. Scheuring emends *Lunatisporites* and merges *Taeniaesporites* with it to include those forms with multiple taeniae divided into two subsets by a central longitudinal furrow.

There have been several efforts to preserve the name *Taeniaesporites*. Leschik in 1959 emended the genus under the heading "*Taeniaesporites* Leschik 1955 nov. sens." (see CFSP 35-202--203). Jansonius, 1962, p. 61, and Klaus, 1963, have also attempted to preserve the name through emendation of the generic description. Other authors (Balme, 1970; Clarke, 1965; Hart, 1965; Jain, 1968) have accepted either the emendation of Jansonius or that of Klaus with or without reservations. As stated above, however, Bharadwaj's selection of *Lunatisporites* seems definitely to have priority.

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Species name: *Anguisporites minutus* Singh (1964)

Fig. 33. *Anguisporites minutus* sp. nov., proximal face; V.44252.

Description:

Diagnosis. Size 28–35 μ (15 specimens), central body subtriangular surrounded by an equatorially attached cingulum, Y-mark as long as the radius not extending beyond the central body, exine granulose on both the faces.

Description. Size 28–35 μ (15 specimens). Holotype 30 μ, subtriangular to almost circular with a subtriangular central body, measuring 22 μ in the holotype, surrounded by an equatorially attached cingulum, uniformly about 4 μ broad. Trilete mark prominent, Y-rays as long as the radius, not extending beyond the central body, labra thick and undulating. Exine of the central body granulose proximally as well as distally. Cingulum coarsely granulose. Surface and margin of the spore rough.

Comparison. A. anguinus[1] Potonié and Klaus is just double the size of *A. minutus*. Grebe (1957, pl. 1, fig. 10) has figured cf. *A. anguinus* from the Zechstein, Germany, which is not a very clear specimen. However, it falls within the size range of *A. minutus*. *A. intonsus* Wilson also differs from *A. minutus* in being larger (48–55 μ). *A. contortus* Wilson has contorted ornamentation of the body exine.

Holotype. Plate 44, fig. 33; slide V.44252.

Author: Singh, H. P., 1964, p. 250, pl. 44 fig. 33

Reference: A miospore assemblage from the Permian of Iraq: Palaeontology, vol. 7, pt. 2, pp. 240-265, pls. 44-46, 4 text figs., 2 tabs.

Type locality: Iraq (Northern), vicinity of Mosul, Atshan well, ca. 12 mi. W of Mosul

Type horizon: Permian, probably Upper (per this study), Chia Zairi fm.

Preparation: Cold HF (24 hrs.), warm 50% HNO_3 (5 mins.), dil. HN_4OH, acetone wash, gravity separation in bromoform-acetone (1/6 by volume), acetone wash, glycerin jelly (assemblage and single grain mounts)

Repository: Palaeont. Dept., British Museum (Nat. Hist.), London, England
Holotype: Pl. 44, fig. 33, slide V. 44252

[1] [See *Anguisporites anguinus*, CFSP 13-117.]

see also pp. 216, 218 etc.

NH_4OH seems more likely. Tell Tate to take Chemistry 001 over again!

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Ähnlich gestaltet ist außerdem *Trizonaesporites grandis*[1] (LESCHIK 1956: 129, Taf. 20 Fig. 10, 11) und es wurde bei diesem bereits darauf hingewiesen, daß die Stränge im Zentrum der Spore Reste solcher „wall cells" sein könnten.

Wenn R. POTONIÉ (1958: 41) allerdings an der Berechtigung von *Trizonaesporites* zweifelt — sie soll auf zufällige Besonderheiten der Erhaltung begründet sein —, so sei dazu bemerkt, daß man ohne Kenntnis des Materials darüber kein Urteil fällen darf. Ich habe in diesem Falle lediglich die Ähnlichkeit der isolierten Sporen mit solchen aus Fruktifikationen von Cordaiten zu begründen versucht. Der Vergleich mit den von FLORIN beschriebenen Sporen ist durchaus naheliegend. Ich habe außerdem inzwischen noch mehrere Formen mit gleichen Merkmalen gefunden. Es kann sich daher nicht um zufällige Bildungen handeln.

Auch in den Gondwana-Gebieten kommen solche Formen vor, z. B. VIRKKI's Spore Nr. 52 (1946: 118, Abb. 30). Bei dieser sieht man auf dem Zentralkörper zwar verschiedene Falten, die bei unseren Exemplaren fehlen; aber abgesehen davon herrscht gute Übereinstimmung. Wenn VIRKKI auch keine ausführliche Beschreibung gibt, so genügen die Angaben und Abbildungen doch als Nachweis dafür, daß Sporen vom Bau derjenigen von Norronaub im australischen und indischen Perm vorhanden sind, dagegen auf der Nordhalbkugel fehlen. Ähnlichkeiten mit unserer Form hat auch *Florinites* SCHOPF & WILSON & BENTALL 1944, neigt aber stärker zur Zweisack-Bildung; auch ist oft eine Y-Marke zu beobachten. Von den Accinctisporiten von Neuewelt (LESCHIK 1955: 48-50) unterscheiden sich die Sporen von Norronaub durch ihre erhebliche Größe und den nicht so klar abgesetzten Randsaum. Es liegt also eine neue Art vor.

Holotypus: Taf. 3 Fig. 21. — SM.B. 7421.

Author: Leschik, G., 1959, p. 62, pl. 3, figs. 21-22

Reference: Sporen aus den „Karru-Sandsteinen" von Norronaub (Südwest-Afrika): Senckenbergiana Lethaea, vol. 40, no. 1/2, pp. 51-95, pls. 1-5

Type locality: Southwest Africa, region around Norronaub (Norronaub lies 20 mi. E of the Keetmanshop-Windhuk railroad yard at Gibeon.)

Type horizon: Lower Permian, Karroo sandstone, coal lens

Preparation: 1-2 g. crushed sample, HF (48 hrs.), 10% hot HCl, 10% HNO_3, 2% KOH, glycerin jelly, single grain mounts

Repository: Paleobot. coll., Senckenberg-Museum, Frankfort a.M., Germany

Holotype: Pl. 3, fig. 21, SM.B. 7421, coord. 67.8 x 3.3

(Coordinates determined by mechanical stage on microscope 346038, Realgymnasium, Fulda. The 1st and 2nd numbers indicate respectively the horizontal and vertical coordinates, which are referenced to an India ink dot on holotype slide.)

[1] [See *Trizonaesporites grandis*, CFSP 13-95.]

NOT to be confused with Frankfort Kentucky, Michigan, + Indiana.

See also p. 35-185, -188, -190, -192

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Species name: *Faunipollenites varius* Bharadwaj (1962)

Description:

Diagnosis — Central body apparently subcircular, 6-8 horizontal striations, distal channel wide, ill-defined. Sacci hemispherical.

Description — Holotype 106 μ long with central body 64 μ high; bilateral bisaccate grains. Central body apparently subcircular, the outline being hazy and apparent due to the striations ends or the laterally bladder-free margin. Striations normally simple, unforked. Distally sacci inclined but not much, the zones of saccus attachment ill-defined. Sacci hemispherical, coarsely intrareticulate.

Locus Typicus — Samla Seam, Samla-Kendra Colliery, East Raniganj Coalfield, India.

Holotype — Pl. 18, Fig. 230.

[*Generic Diagnosis* — Disaccate, bilateral, haploxylonoid pollengrains. Central body outline ill-defined, proximally exine intramicroreticulate and bearing a number of horizontal, simple or forked striations, rarely with vertical, connecting striations also; distally a uniformly wide to biconvex area free from saccus, where the exine is thin and sparsely granulose. Distal zones of saccus attachment ill-defined.

Generic Description — Bilateral grains with the bladders almost as much in height as the central body. Bladders distally inclined and laterally either separated from each other widely or narrowly. Central body with ill-defined outline and laterally having broad ends. Body exine thin and proximally intramicroreticulate with ± 8-12 horizontal striations some of which may be forked. Distally body free from the saccus in a biconvex or uniformly broad, ill-defined channel, exine faintly granulose. Sacci ± hemispherical, coarsely intrareticulate.

Reconstruction — See Text-figs. 12 A, B.

Comparison — *Striatopodocrapites* compares closely with *Faunipollenites* but for the fact that the central body in the former is well-defined and it consists of mostly diploxylonoid grains. Bolkovitina (1956) has referred organizationally similar pollengrains but lacking striations to *Protopinus* Bolkh., from the Upper Mesozoic of U.S.S.R. *Lunatisporites* Lesch., possesses smooth to microverrucose, indistinctly structured exine on the proximal, saccus-free face.

Genotype — *Faunipollenites varius* sp. nov.] [2]

Presumably in error for Striatopodocarpites — I don't believe StriatopodoCRAPites ever existed tho' it would have been a good name for some spores in the literature

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Dimensions. (Twenty-five specimens.) Spore-body length 30(36)42μ, spore-body width 30(37)41μ, overall length 48(57)63μ.

Comparison. P. minor (Klaus 1963) comb. nov. is most similar to the present species, but differs in the shape of the sacci and the absence of the distal attachment thickenings.

Holotype. Plate 42, fig. 4. Slide PF2210. Sample H 5, Hilton Plant Bed, Westmorland; Upper Permian.

I think you must follow the local usage + not create this designation, which is not in fact used.

see also p. 35-148

Author: Clarke, R. F. A., 1965, p. 337, pl. 42, figs. 3-5, text fig. 11

Reference: British Permian saccate and monosulcate miospores: Palaeontology, vol. 8, pt. 2, pp. 322-354, pls. 40-44, 16 text figs.

Type locality: Great Britain, Westmorland, 3 mi. ENE of Appleby, Hilton Beck, sample H 5

Type horizon: Upper Permian (Zechstein), Hilton Plant Series Bed

Preparation: 20 g. crushed sediment (-1 mm.). If calcareous, boiled in 20% HCl; 40% cold HF (24 hrs.-4 days); centrifuge 5 min. at 2,500 r.p.m. If highly carbonaceous, conc. HNO_3 (12-15 hrs.); Na_2CO_3 sol.; bromoform-acetone flotation (1/5 acetone by vol.); glycerin jelly mounts, mostly single grain and unstained.

Repository: Geological Survey and Museum, London, England
Holotype: Pl. 42, fig. 4, slide PF2210

also Index p. 35-305

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V. is not in the normal sense a "MS name" – it was widely used, but in a confused sense, + was replaced by Thymospora. This note might better read "V. was presumably used here in error for Thymospora"

Author: Bharadwaj, D.C., & S.K. Salujha, 1964, p. 197, pl. 3, figs. 66-69; Bharadwaj, 1962, pl. 5, fig. 80

Reference: Sporological study of Seam VIII in Raniganj coalfield, Bihar (India) - Part I. Description of sporae dispersae: Palaeobotanist, vol. 12, no. 2, pp. 181-215, 12 pls., 6 text figs., 4 tabs.

Type locality: India, Bihar, eastern part Raniganj coal field

Type horizon: Permian, lower Gondwana, Damuda series, Raniganj stage

Preparation: 40 g. of 2-5 mm. coal fragments, wash, fuming HNO_3, daily addition of HNO_3 (4-6 days), divide sample. Smaller portion: 10% KOH (H_2O bath at 90°C), glycerin jelly mounts. Larger portion: sieved (Müller sieves, 0.1 x .06): 10% KOH (H_2O bath at 90°C), agitation in watch glass to concentrate organics, glycerin jelly mounts.

Repository: Not indicated

Holotype: Bharadwaj, 1962, pl. 5, fig. 80

[1] [Fig. 80 is reproduced from Bharadwaj, 1962, pl. 5.]

[2] [*Verrucososporites* is probably a manuscript name and should read *Thymospora*.]

[3] [See *Verrucososporites leopardus* (CFSP 13-17), and *V. pseudogranulatus* (CFSP 13-67).]

Hunt Institute for Botanical Documentation

Department of Botany and Microbiology
UNIVERSITY COLLEGE LONDON
GOWER STREET LONDON WC1

TELEPHONE 01-387 7050

Jan 30 1972

Dear Al,

I am enclosing a few pages from the Catalog volume 35. I am sorry it has taken me so long to go through it, and I hope that the draft has not, so to speak, gone to press. Few of the items are vital - indeed some are merely matters of taste or opinion. But a few might be deemed worth taking action on.

I still haven't received the published version of vol. 34, but it may well not have been mailed yet.

I hope you had fun at the AASP meeting - one of these years I must linger in the States long enough to get to another meeting. This summer we shall be in Maine through most of August, and I hope to take a bus up to Montreal for the Geological Congress - or rather for a couple of days, during which I hope to give a paper on some spores from Maine (Devonian). But I get the impression that palaeontology as a whole (never mind palynology) is not scheduled as a major item on their programme.

Best wishes to you, your family,
Bill, Tate - and to the Scholtens.

Sincerely,

W.G. Chaloner.

Hunt Institute for Botanical Documentation

University College London
Gower Street London WC1E 6BT England

Department of Botany and Microbiology

11 January 1972

Dear Al,

Thanks for proofs of volume 35. I will mail corrected pages shortly. I still hold three copies of proofs of vol. 34, but still await the finished version! I presume it is still in the press.

Best wishes,

BY AIRMAIL
AIR LETTER
PAR AVION
AEROGRAMME

GREAT BRITAIN
W.C.1.
11 1 72
LONDON

Hunt Institute for Botanical Documentation

December 31, 1971

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

Under separate cover we have sent you the usual "proof" copy of Volume 35. Please review our work and let us have your comments as soon as convenient, at least by 15 February. Probably the best way to give us your comments is to remove the pages that need comment and scribble your corrections, etc., on them in red pencil and return the pages. Do not return the whole volume, however.

Since we have been using this proof copy method of review, we have greatly reduced the amount of trouble in the printing process and at the same time have been able to accept corrections without fear of cost of reprinting pages. Do let us have your comments at your early convenience.

Happy 1972 from all of us.

Yours very truly,

AT

Alfred Traverse
Editor

AT/vsi

Hunt Institute for Botanical Documentation

July 30, 1971

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

Your letter regarding the "proof" copy of Volume 34 was most useful and your apologies were unnecessary. We have corrected the Compilers' Preface in view of Martin's paper, and I have prepared an English translation of the Iversen & Troels-Smith business about pore and colpus morphology for inclusion in the introduction of Volume 34. At this stage of the game, that was really the best we could do. I think it's probably superior to a footnote anyway. Your third comment was regarding orthographic errors in one of the papers. The error is in the original material which we are republishing. We do not change that in any way, nor footnote it unless it is important.

Thank you again for your help. It really is appreciated. Best wishes for a good summer and for a happy return from Novosibirsk.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

From 20 Parke Road, Barnes, London, S.W.13.
[redacted]8 4702

July 18

This is the nearest I have to the Rev. Traverse notehead. I am writing in usual haste on the eve of my trip to Novosib. I have managed only 160 pp. of the Catalog 34 but couldn't find a single misprint. I enclose 3 pages with comments — the age of Simpsai material is important as of course it is repeated throughout. You might look at Martin's paper. I think the code

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POST CARD

THE ADDRESS TO BE WRITTEN ON THIS SIDE

used by van der H + Wijnstra
Cal etc. might be spelled out from
a footnote on first use (p. 34-37).
Sorry I couldn't find anything
more exciting! Incidentally, I now
have not only mine, but Schopf's
copy of the proofs! Best wishes
Bill Crandall

Hunt Institute for Botanical Documentation

1968: MARTIN, A.R.H. (Palaeontology 11 (4) pp 549-53) reviews this, and synonymises Taxo- and Aquila- in part. There is no good reason to believe in reworking, and indeed Martin concludes that "it is believed that the age (or the lignites) ... may be pre-Eocene." I agree with him. W.A.C. 9 Jul. 71

Aquilapollenites Rouse. Presence of specimens of this genus probably indicates Tertiary recycling of older material in the Isle of Mull sediments studied by Simpson.

Nomenclatural Comments such as that on *Retitricolporites* (34-63) are presented on separate pages, rather than as footnotes, partly because of their length and partly because they are comments of a more expository sort than is natural for footnotes.

We encountered two problems in Simpson, 1961, which require comment. Simpson published many photomicrographs and descriptions of extant pollen for direct comparison with his fossil palynomorphs. This information is ancillary to the protologs, and we have therefore presented it in our treatment. Also, Simpson in many instances did not designate holotypes for his species but instead indicated a series of forms as typifying his taxa. These he refers to as "cotypes". The 1964 International Code of Botanical Nomenclature makes clear that a holotype should be indicated. Furthermore, the Code provides (Art. 7) that where an author uses joint types, they are either syntypes or isotypes. Syntype is probably the more appropriate designation for dispersed spore specimens, and we have used this term rather than cotype in presenting Simpson's data.

The 22 new generic names are:

Clavainaperturites van der Hammen & Wymstra 1964

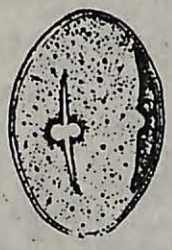
Clavastephanocolpites van der Hammen & Wymstra 1964

Ctenolophonidites van Hoeken-Klinkenberg 1966

Desmidiaceaesporites Hunger 1953 ex Potonié 1960

Hunt Institute for Botanical Documentation

Species name: *Foveotricolporites florschutzi* (van der Hammen 1954) emend. van der Hammen et Wymstra (1964)



florschutzi A [1]

9

Foveotricolporites florschütziі (1300 ×)

Explain
Was
convenha
in a
footnote?

Description:

Pollen grain tricolporate, furrow Cα1, Cβ2, Cγ1, pore Pαc, Pβ2, Pγ1, sculpture type foveolate with diameter of the lumina of < 1 μ, sometimes the grain is reticulate, semitectate, with endexine thinner than the ectexine, and very indistinct columellae. The size of the grain is 18 μ. Variation of size is unknown, index pollinis 1.3, index polaris 0.16.

This species was described originally as *Tricolporites florschützi* from the Maestrichtian of Colombia (Van der Hammen 1954b). We give here a more complete description, according to the grain from slide R.H. 4429—39 no. 1, loc. 105.2 × 32.5 (micr. Po 40), that seems to correspond to this species, although it is somewhat smaller than the holotype.

[EJEMPLAR TIPICO, PLACA, etc.	TIPO	ESCULTURA (Y ESTRUCT.)	TAMAÑO (en micras)	DETALLES (medidas en micras)
Suesca Cogontá, Capa VII Nº 3 Pl. FI 27 F. Guaduas.	C_3P_3	sca-fov ret.	26.5	Cost. tr.] [1]

Author: Hammen, T. van der, and T.A. Wymstra, 1964, p. 238, pl. 2, fig. 9

Reference: A palynological study on the Tertiary and Upper Cretaceous of British Guiana: Leidse Geol. Mededel., vol. 30, pp. 184-241, 3 pls., 26 text figs., 4 diagrams

Type locality: [Columbia, Cundinamarca, Suesca-Cogontá]

Type horizon: [Cretaceous, Maestrichtian, Guaduas fm.]

Preparation: [Not indicated]

Repository: Coll. of the Palynological Section, Natl. Geol. Service of Columbia, Bogata

Holotype: [Capa VII, no. 3, pl. FI27]

[1] [Illustration and other elements of the specific protolog of *Tricolporites florschutzi* reproduced from van der Hammen, 1954, p. 95, pl. 9, fig. designated "*florschutzi* A".]

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Natural relationship: The type-specimen is a recent pollengrain of *Bartsia santalinaefolia* (HBK) BENTH. (Scrophulariaceae).

Age and locality: Recent: Colombia.

Type: Slide R I 17, Col. I.G.N.C.; Fig. 22.

Pollengrains tricolpate, exine psilate.

Subgenotype: *Tricolpites (Psilatricolpites) incomptus* nov. fsp.][5]

[DIAGNOSIS: Psilate, tricolpate pollen. TYPE FOR GENUS: *Psilatricolpites incomptus* V. d. Ham. 1956a, Pl. VII, fig. 22.][6]

Author: Hammen, T. van der, and T.A. Wymstra, 1964, p. 235, pl. 2, fig. 2

Reference: A palynological study on the Tertiary and Upper Cretaceous of British Guiana: Leidse Geol. Mededel., vol. 30, pp. 184-241, 3 pls., 26 text figs., 4 diagrams

Type locality: [Columbia, Santa Rosita, Puente Nariña]

Type horizon: [Cretaceous, Maestrichtian, Guaduas fm.]

Preparation: [Not indicated]

Repository: Coll. of the Palynological Section, Natl. Geol. Service of Columbia, Bogata

Holotype: [Capa I, no. 8, pl. CI4]

[1][See Nomenclatural Comments on *Psilatricolpites*, CFSP 34-41.]

[2][The specific epithet was spelled *clarisimus* in van der Hammen, 1954, p. 94.]

[3][Illustration of figure designated "*clarisimus*" is reproduced from van der Hammen, 1954, pl.8. Tabular presentation of the specific description is from van der Hammen, 1954, p. 94]

[4][Form generic description of *Psilatricolpites* van der Hammen 1956 as given in van der Hammen and Wymstra, 1964, p. 234.]

[5][Form specific description of *Psilatricolpites incomptus* and form subgeneric description of *Psilatricolpites* van der Hammen, 1956, p. 88.]

[6][Form generic description of *Psilatricolpites* (van der Hammen 1956) Pierce, 1961, p. 49.]

THIS SPECIES DESIGNATED "LECTOGENOTYPE"
OF THE NAME *PSILATRICOLPITES* van der Hammen 1956
in van der Hammen and Wymstra, 1964, p. 234,
to replace the type species
Psilatricolpites incomptus van der Hammen 1956

Hunt Institute for Botanical Documentation

July 19, 1971

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

Thanks for yours of 9 July. It looks as if you were right--the proof copy was sent surface mail. I intend to take steps to assure that this does not happen in the future. We look forward to receiving your comments. You will presumably also receive the copy from Schopf's lab which was sent you as a form of insurance. I would think that it would be unnecessary to return it to Jim since he will get the printed version in due course. However, I suppose that might be made a matter of correspondence between the two of you. I really can't see why he would want it.

Have a good trip to Novosibirsk and brace yourself for Volume 35, proofs which you should receive in fairly short order!

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

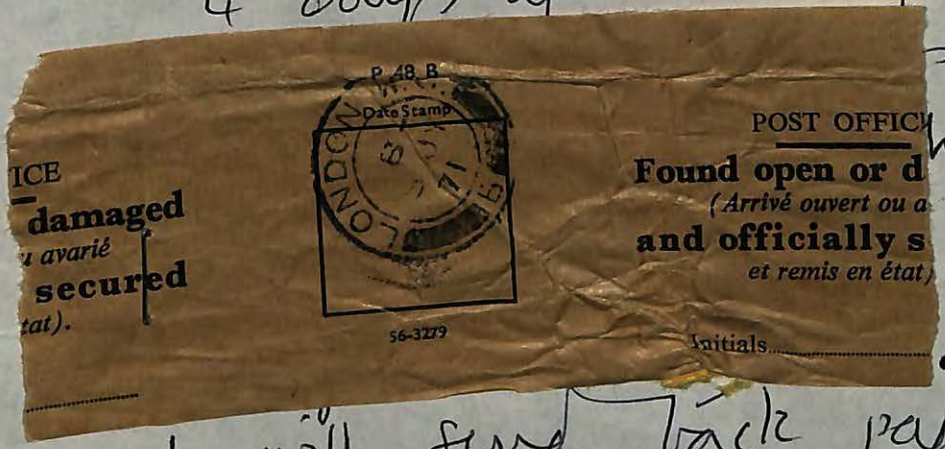
AT/vsi

Hunt Institute for Botanical Documentation

Department [illegible] Microbiology

Dear Al,

Catalog 'proof' 34 arrived today one month 4 days after mailing.



I will send back pages with comments in a couple of days.

Best wishes

Bill Graham

Hunt Institute for Botanical Documentation

USGS COAL GEOLOGY LABORATORY
P & S BRANCH

Orton Hall, Ohio State University
155 S. Oval Dr., Columbus, Ohio 43210

July 7, 1971

Dr. W. G. Chaloner
Department of Botany
University College
Gower St.,
London, W.C. 1, England

Dear Dr. Chaloner:

Dr. Schopf is participating in the Gordon Conferences at New Hampton, New Hampshire, so I received Dr. Traverse' call today asking that Dr. Schopf's proof copy of volume 34 of the Catalog of Fossil Spores and Pollen be sent to you because your copy had gone astray in the mails, and, so, the volume is enclosed. I suppose that Dr. Schopf would like to have it returned when it has served your purpose, but I suggest that you wait a couple of weeks before mailing it in case he wishes to send other instructions.

Sincerely,

Phoebe E. Reigle
Clerk-Stenographer
FOR: DR. JAMES M. SCHOPF

Encl.
VIA AIRMAIL,REGISTERED,RETURN RECEIPT REQUESTED

cc: Dr. Alfred Traverse

Hunt Institute for Botanical Documentation

July 2, 1971

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

Bob Dunay returned to our midst the other day with the chilling news that the "proof" copy of volume 34 of CFSP had not arrived by the time he visited you. I do hope that it has arrived in the meantime. According to the best information I can develop here, it was sent air mail. If, when it does arrive, there is evidence that this is not the case, I would appreciate hearing about it so that I can take appropriate measures to prevent this happening in the future. By the way, I greatly appreciate your willingness to spend some time with Dunay. He seems to have profited not only from visiting with you but especially from the days he was with Fisher in Wales.

Do you by any chance have reprints of "Lower Devonian spores from South Wales", by yourself and Maurice Streel, 1966, published in Argumenta Palaeobotanica? I don't seem to have a copy of this either under your name or under Streel's. As you may have heard from me already, I have a student, Jim Warg, who is working on a Devonian problem in this general area, and we could use a copy of the paper for our files of related information.

Have a good trip to Novosibirsk. I'll be watching the newspapers for stories of palynologists detained in Siberia.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

ATsi

Hunt Institute for Botanical Documentation

University College London
Gower Street London WC1E 6BT England

July 1 1971

Dear Al,

In a hasty reply to your letter -

1). The proofs of Vol. 34 have STILL not arrived. I think they must have gone surface mail in error. I will write as soon as they arrive.

2). Spent a very pleasant half day with Dunay.

3). Rob Scholten stopped by en route to the Continent so got up to date on State College matters.

4). Sorry I won't be able to hear you give your paper по-русски at Новосибирск. I'm going but I'm still not sure why!

Best wishes

Sincerely

Bill Graham

Hunt Institute for Botanical Documentation

June 24, 1971

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, ENGLAND

Dear Bill:

Thanks for yours of 18 June. I am sure that your "proof" of Volume 34 should have arrived by now--we sent it air mail. Let us know.

I have passed along your comments on Tate's query. From what I have heard of the situation, it seems to me that you are right about the ineffective and hence invalid publication of Imgrund's thesis. We just heard from Hilda Grabe that Dr. Imgrund is now deceased, so we are not likely to have an opinion from him!

I have decided to skip the meeting in Novosibirsk for various reasons, mostly personal rather than professional. Probably I am making a mistake some ways, but you just can't do everything.

Looking forward to hearing from you about your review of Volume 34, I am

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

Department of Botany and Microbiology
UNIVERSITY COLLEGE LONDON
GOWER STREET LONDON WC1

TELEPHONE 01-387 7050

June 18 '71

Dear Al,

Thanks for your letter of May 26. The proofs of the Catalog 34 have not yet arrived so I thought I should just scrawl this note to let you know. If they came surface mail, I suppose this is reasonable, (or rather, to be expected) but I will at least

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confirm their arrival when they do come.

May I hope to see you at Novosibirsk? I shall be there.

Best wishes

Sincerely

BW

Chaloner

P.S. Apologies for delay on Tate's query – I am taking legal advice (from Ross, I hope!) but I feel Ingrundi's thesis was not effective pub.

WGC

Hunt Institute for Botanical Documentation

May 26, 1971

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

Under separate cover is your "proof" copy of Volume 34 of the Catalog of Fossil Spores and Pollen. As in previous volumes, we would appreciate a thorough review of this volume so that we can then submit it for printing. As I believe I have also previously said, please do not bother to return the volume to us but either tear out the pages on which you wish to make comments and return just those pages, or send your comments referring to particular pages and lines.

I am sure you know how much we all appreciate the effort that you put into doing this work for us, and we realize that your complimentary copies of the Catalog are only a token compensation. In order to keep to our publication schedule, we would very much appreciate having your comments by 1 July.

With best wishes as ever.

Yours very truly,

Alfred Traverse
Editor

AT/vsi
P.S. A number of minor changes, including substitution of "syntypes" for "cotypes" in Simpson's papers, have been made on the formats, but the changes were made after the "proof" copies were xeroxed.

Hunt Institute for Botanical Documentation

April 12, 1971

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1
ENGLAND

Dear Bill:

Here is a copy of AASP Proceedings Vol. I. to make up for the one you did not receive. I note from my philatelic magazines that there were innumerable local outfits delivering the mail, and also issuing "stamps(?)" during the "Great Mail Strike". Perhaps your first copy of Proceedings Volume I will be delivered within the decade. Seriously, if the other copy does eventually show up, please donate it to one of your students or to the U. S. London Geology Library with the compliments of AASP.

Dues response has been excellent, 160/290+, considering that only one month has passed since that notice was sent. Members (1970-71) residing in foreign countries now number 47 (!) and this does not include the 30 Canadian members. The Association is becoming worldwide in scope, not just U.S.

Sorry to hear that you will be unable to attend the Tucson meeting, but Novosibirsk should be an exciting adventure.

Finally, coping with the awkwardness of your long-distance membership is really no problem at all because we (The Royal "we" for the Association) value your interest and membership in AASP.

Cheers.

Sincerely,

RTC:yy

Robert T. Clarke
Secretary-Treasurer
AASP

bc: Alfred Traverse

Hunt Institute for Botanical Documentation

January 6, 1971

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

Many thanks for your fine review of volume 33. We have incorporated practically all of your suggestions in one way or another. Naturally, I am embarraased by the mistakes in the Compiler's Preface which crept in I know not how. I am so glad that you caught them. Also, we have added a footnote on the genera which you indicated required special mention as to Skarby's "lumping". For my part, I must confess that she went too far, but I think that to have a footnote calling attention to her work is an excellent addition to the usefulness of that section of the Catalog. I also expanded the bibliography of volume 33 to include reference to her paper. There were, in addition to the genera which you noted, also six or seven others from the paper by Krutsch which were also "lumped" by Skarby. Presumably, you did not feel that we should use a footnote in those cases because Skarby prefaced reference to them with "cf."

We were startled to learn that our proof-reading had not caught the incorrect citation of zinc bromide and have taken care of that failing. Also, we were delighted that you caught the mistake where one extra line of Góczán appeared in our coverage. That sort of thing is extremely annoying, as is such a gremlin as *Maestrichtian* in italics where it should be Roman! As I say, we have incorporated practically all of the suggestions which you made. The only one which I can think of offhand which we did not use was your suggestion that we signify Plate 2 on page 29. On reflection, Tate and I decided that this was obvious enough from the reference material provided as to be unnecessary. I hope you will agree.

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Best wishes to you, Judy and the rest of your family!

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

As from: Dept of Botany & Microbiology
University College London
Dec 1 1970.

Dear AT,

I eagerly accept your offer to be lazy, & tear out corrected pages & return them to you — they are enclosed. I have been reading the Catalog proofs in odd moments (mainly evenings) while attending an SEM operators course — hence delay in the letter.

I think it is worth noting that GOCZAN's "new" genera are regarded as synonyms by Skvarla (with enormous justification) — a enclose a xerox of the synonymy of Extratripora (though I imagine you have her paper — I cited the reference at the top of the xerox). I respect her work greatly as an antidote to Pflug / Krutzsch / Goczan genus mongering.

The legality of Leigh / Porcle is a mess isn't it? The names are not valid — as you indicate — and hence are illegitimate. I am not clear why you grace them with spurious respectability by housing them in the catalogue. I can think of more

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deserving illegitimate taxa, starting with Reinsch's (his figures are better, & the work harder to obtain).

Tell Tate that he has my full, unqualified & enthusiastic permission to publish all or any part of Chaloner 1954, and other publications of mine dealing with spores for the Catalog. I will write him on the matter very shortly; I would like inclusion along side it a paper by Spinner & one by Alvin each describing allied Mississipian megaspores — both in Palaeontology of the last few years (I have no references with me).

Best wishes to you & colleagues

Bill Chaloner

2 v

Chaloner

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somewhat bent towards one of the poles. Amb in normal cases fundamentally triangular with a composite equatorial or subequatorial aperture at each corner. Two wall layers, sexine and nexine, visible in light microscope. Sexine of aperture regions more or less intumescent with meridionally oblong or, rarely, almost circular pores penetrating the outer layer and the enclosed intumescence fillings. Nexinous ora of varying shape, size, and orientation.

TYPE SPECIES.— *E. fractus* PFLUG, Wehmingen, Germany; Danian (?) to Paleocene (THOMSON and PFLUG, p. 69, pl. 6, fig. 2).

STRATIGRAPHIC RANGE. — Dispersed pollen grains referable to *Extratriporopollenites* (PFLUG) emend. are reported from widespread deposits of late Cretaceous or early Tertiary age (Turonian to Eocene), most of them located in the northern hemisphere. The greatest abundance and variety of forms occur in the uppermost Cretaceous and the Paleocene.

REMARKS. — The author's reasons for uniting the genera listed in the synonymy are given below. The abbreviation "cf." indicates morphological similarity, and expresses the writer's belief that direct comparison with adequate material would prove generic identity with *Extratriporopollenites* (PFLUG) emend.

TRIATRIOPOLLENITES PFLUG (THOMSON and PFLUG, 1953, pp. 36—37, 76—82). — The genus *Triatriopollenites* PFLUG comprises a number of Tertiary pollen forms allegedly differentiated from the forms here referred to *Extratriporopollenites* by the presence of an atrium in the apertures and by the absence of "interloculum", "solutionsmeridium", "oculus", "praevestibulum", and "endanulus". As mentioned above (pp. 8—10), the present writer does not recognize these characters as diagnostically significant. It is difficult to identify the *Triatriopollenites* species in the slides on the basis of the original descriptions and pictures. There is, however, no doubt that some of the forms assigned by PFLUG to this genus show typical *Extratriporopollenites* characters, such as composite apertures with slit-like pores and distinct intumescences, and a wall consisting of two layers visible in the light microscope. It would, therefore, seem more appropriate to transfer at least the following forms to *Extratriporopellenites*: *T. excelsus* subsp. *typicus* (THOMSON and PFLUG, 1953, p. 77, pl. 7, figs. 27—31; PFLUG, 1953, pl. 25, figs. 7, 8), *T. excelsus* subsp. *turgidus* (THOMSON and PFLUG, 1953, p. 77, pl. 7, figs. 36—46; PFLUG, 1953, pl. 25, figs. 4, 6, 10), *T. excelsus* subsp. *microturgidus* (THOMSON and PFLUG, 1953, p. 78, pl. 7, figs. 59—63), *T. excelsus* subsp. *semiturgidus* (THOMSON and PFLUG, 1953, pp. 77—78, pl. 7, figs. 47—58; PFLUG, 1953, pl. 25, figs. 5, 11, 12), *T. pseudosporites* (THOMSON and PFLUG, p. 78, pl. 7, figs. 32—35; described by PFLUG, 1953, as *Sporopollis pseudosporites* (PFLUG) n. comb., p. 97, pl. 25, figs. 1—3), *T. roboratus* (THOMSON and PFLUG, 1953, p. 78, pl. 7, figs. 71—74; PFLUG, 1953, pl. 21, figs. 10—13), *T. pseudovestibulum* (THOMSON and PFLUG, 1953, p. 78, pl. 7, fig. 75), *T. concavus* (THOM-

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Acta Universitatis Stockholmensis = Stockholm Contrib. Geol. 16 1968 pp 1-60.

SYSTEMATIC DESCRIPTION

Form genus ***Extratriporopollenites*** (PFLUG) emend.

1953 *Extratriporopollenites* n. gen. (PF.) — THOMSON and PFLUG, pp. 69—76 (pro parte).
1953 *Triatriopollenites* n. gen. (PF.) — THOMSON and PFLUG, pp. 36—37, 76—82 (pro parte).
1953 *Sporopollis* n. gen. — PFLUG, pp. 96—97 (pro parte).
1953 *Plicapollis* n. gen. — PFLUG, pp. 97—98.
1953 *Trudopollis* n. gen. — PFLUG, pp. 98—103.
1953 *Vacuopollis* n. gen. — PFLUG, pp. 103—105.
1953 *Conclavipollis* n. gen. — PFLUG, p. 105.
1953 *Extratriporopollenites* PFLUG 1953 — PFLUG, pp. 106—108.
1953 *Nudopollis* n. gen. — PFLUG, pp. 108—109.
1953 *Basopollis* n. gen. — PFLUG, pp. 109—110.
1953 *Oculopollis* n. gen. — PFLUG, pp. 110—111.
1953 *Papillopollis* n. gen. — PFLUG, p. 111.
1953 *Tetrapollis* n. gen. — PFLUG, pp. 112—113 (pro parte).
1953 *Pentapollis* n. gen. — PFLUG, p. 113.
1953 *Triatrio-pollenites* PFLUG 1953 — PFLUG, pp. 114—115 (pro parte).
1959 cf. *Latipollis* n. fgen. — KRUTZSCH, pp. 128—131.
1959 cf. *Monstruosipollis* n. fgen. — KRUTZSCH, pp. 131—133.
1959 cf. *Turonipollis* n. fgen. — KRUTZSCH, pp. 133—134.
1959 cf. *Complexiopollis* n. fgen. — KRUTZSCH, pp. 134—136.
1959 cf. *Pflugipollis* n. fgen. — KRUTZSCH, pp. 136—137.
1959 cf. *Emscheripollis* n. fgen. — KRUTZSCH, pp. 137—139.
1959 cf. *Minorpollis* n. fgen. — KRUTZSCH, pp. 139—141.
1959 *Quedlinburgipollis* n. fgen. — KRUTZSCH, pp. 141—142.
1963 Формальный род *Sporopollis* PFLUG, 1953 — ZAKLINSKAJA, pp. 154—163 (pro parte).
1963 Формальный род *Oculopollis* PFLUG (1953) — ZAKLINSKAJA, pp. 164—180.
1963 Формальный род *Papillopollis* PFLUG, 1953 — ZAKLINSKAJA, pp. 180—183 (pro parte).
1963 Формальный род *Basopollis* PFLUG, 1953 — ZAKLINSKAJA, pp. 183—185.
1963 Формальный род *Nudopollis* PFLUG — ZAKLINSKAJA, pp. 185—193.
1963 Формальный род *Extratriporo-pollenites* PFLUG, 1953 — ZAKLINSKAJA, pp. 193—203.
1963 Формальный род *Trudopollis* PFLUG — ZAKLINSKAJA, pp. 204—224.
1964 *Hungaropollis* nov. gen. — GÓCZÁN, pp. 253—254.
1964 *Sümegipollis* nov. gen. — GÓCZÁN, pp. 256—258.
1966 *Choanopollenites* STOVER, n. gen. — STOVER, ELSIK, and FAIRCHILD, p. 8.

EMENDED DIAGNOSIS. — Pollen grains of small, medium, or large size, peroblate or peroblatoid to almost spheroidal, isopolar or subisopolar with the apertures

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Species name: *Cyatheacidites tectifera*
Archangelsky & Gamerro (1965)

2[1] 3 1

4 5 6

7 8 9

Pl. 2

Cyatheacidites tectifera n. sp.: Fig. 1, cara proximal. P. m. 264, c. 42.8/107.1 (x 700). — Figs. 2 - 3, cara proximal, corte óptico y cara distal del holotipo. P. m. 247 (x 700). — Figs. 4, 5, 6 (x 1200), detalle L. O. de la exina del paratipo de las figuras 7 y 8. — Fig. 4, enfoque superior (perforaciones aparecen como puntos negros). — Fig. 5, enfoque medio (perforaciones aparecen como puntos claros; comienzan a notarse las columelas). — Fig. 6, enfoque inferior (se ven claramente las bases de las columelas). — Figs. 7 - 8, corte óptico y cara distal de un paratipo. P. m. 248 (x 700). — Fig. 9, vista ecuatorial. P. m. 265, c. 49.1/104 (x 700).

No Pl. 1 to avoid confusion with Fig 2 on page 33-30

Feature of this copy only?

Author: Góczán, F., 1964, p. 260, pl. 9, figs. 17a-c

Reference: Stratigraphic palynology of the Hungarian Upper Cretaceous: Acta Geol., Acad. Sci. Hungary, vol. 8, nos. 1-4, pp. 229-264, 9 pls., 2 tabs.

Type locality: Hungary, southern Bakony Mts., Sümeg, borehole no. Sp-1, 185.75-191.30 m.

Type horizon: Upper Cretaceous, upper Santonian, marl

Preparation: Varied, controlled by lithologic character of samples (coals: HNO_3; marls: HCl + HNO_3 and/or HF), heavy liquid gravity separation in $ZnCl_2$ or CdI_2; gelatin-carbolic acid mounts

Repository: Not indicated

Holotype: Pl. 9, figs. 17a-c, slide no. Sp-1-6, pollen no. 35.2-110

[1] [*Laudaypollis simplex* appears to be a manuscript combination, which should read *Laudaypollis clarus*. Holographic alteration to this effect appears in separate sent to A. Traverse.]

[2] [See *Diporina neuburgae*, CFSP 8-146.]

[3] [Should read "pollen no."]

[4] [Bracketed information derived from generic description.]

TYPE SPECIES OF THE GENERIC NAME
(Monotypic in Góczán, 1964, p. 260)

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Note there a elsewhere - the genus is regarded by Skarby 1968, p. 24 as a synonym of Extratriporopollenites Pflug

Author: Góczán, F., 1964, p. 258, pl. 9, figs. 10a-c, 11, 12

Reference: Stratigraphic palynology of the Hungarian Upper Cretaceous: Acta Geol., Acad. Sci. Hungary, vol. 8, nos. 1-4, pp. 229-264, 9 pls., 2 tabs.

Type locality: Hungary, southern Bakony Mts., Sümeg, borehole no. Sp-1, 185.75-191.30 m.

Type horizon: Upper Cretaceous, upper Santonian, marl

Preparation: Varied, controlled by lithologic character of samples (coals: HNO_3; marls: HCl + HNO_3 and/or HF), heavy liquid gravity separation in $ZnCl_2$ or CdI_2; gelatin-carbolic acid mounts

Repository: Not indicated

Holotype: Pl. 9, figs. 10a-c, slide no. Sp-1-4, pollen no. 31.4-88.8

[1] [Should read *triangularis*.]

[2] [Bracketed information derived from generic description.]

*[Generic name spelled *Sümegipollis* by Góczán. Orthography corrected per Art. 73, para. 3, 1964 Int. Code Bot. Nomencl.]

TYPE SPECIES OF THE GENERIC NAME
(Monotypic in Góczán, 1964, p. 257)

Species name: *Trudopollis praesubhercynicus* Góczán (1964)

3 4 5

3–5. Trudopollis praesubhercynicus nov. sp. (Bakonypölöske, borehole N° Bp-1, Upper Maestrichtian)

Description:

Diagnosis: Pollen grains globular, triporate. Pores of slightly subequatorial arrangement. Monoanulate. Postvestibulum, interloculum, cuneus and a field of dissolution in polar view available. Latter trending towards the cuneus. Endexine not yet differentiated into three independent parts, a reason why platea is lacking. Pore canal index below 0.2.

Remarks: Trudopollis subhercynicus[1] nov. sp. shows the greatest affinity to the species Trudopollis subhercynicus Krutzsch, from which it differs in the lack of plateae, anuli and atria. The problem of "Trudopollis subhercynicus" versus "Pollenites pompeckji"[2] has been discussed by KRUTZSCH (1954), POTONIÉ (1960), and KRUTZSCH (1961).

Trudopollis praesubhercynicus nov. sp. varies in size and in the pattern of the polar field of dissolution. Its size range is, as a rule, 16–20 μ. The holotype measures 16 μ. It is the most characteristic pollen of the Maestrichtian. Its predominance is confined to zone "H".

Locus typicus: Bakonypölöske, borehole N° Bp-1.
Stratum typicum: depth range 80 m, grey clayey marls, Upper Maestrichtian

Holotypus: slide N° Bp-1–80, pollen N° 26.0–89.2. (Pl. IX, fig. 3.)
Derivatio nominis: after the pre-stage of the exine structure of "subhercynicus".

Author: Góczán, F., 1964, p. 255, pl. 9, figs. 3-5

Reference: Stratigraphic palynology of the Hungarian Upper Cretaceous: Acta Geol., Acad. Sci. Hungary, vol. 8, nos. 1-4, pp. 229-264, 9 pls., 2 tabs.

Type locality: Hungary, Bakony Mts., Bakonypölöske, borehole no. Bp-1, 80 m.

Type horizon: Upper Cretaceous, upper *Maestrichtian*, marl

Preparation: Varied, controlled by lithologic character of samples (coals: HNO_3; marls: HCl + HNO_3 and/or HF), heavy liquid gravity separation in $ZnCl_2$ or CdI_2; gelatin-carbolic acid mounts

Repository: Not indicated

Holotype: Pl. 9, fig. 3, slide no. Bp-1-80, pollen no. 26.0-89.2

[1] [Should read *Trudopollis praesubhercynicus*.]

[2] [See *Pollenites pompeckji*, CFSP 2-68 and 14-102.]

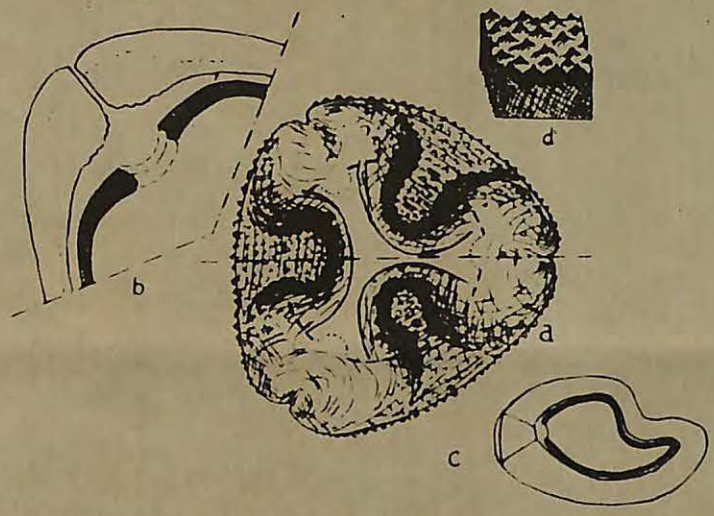

Abb. 8. *Emscheripollis inflatius* [8 a: V = 1000 ×]. 8 a = Skizze zur Polansicht (proximal), b = äquatorialer Schnitt, c = vertikaler Schnitt, d = Skulpturschema.

Square brackets of generic diagnosis should open here

Description:

Diagnose: Eine fsp. der *Emscheripollis* mit bis zu 10 μ starker äußerer Wandschicht, mit 5 bis 10 μ starken, plicenartigen Tori auf der Proximalseite. Oberfläche dicht mit zugespitzten punctaten oder größeren Skulpturelementen besetzt (Abb. 8); dies gleichmäßig auf der ganzen Ober- und Unterseite. Inneres Lamellenpaket der äußeren Wandschicht nur undeutlich definierbar (vgl. Versuch Abb. 8). Innere Wandschicht starker als bei *E. subsudeticus*.[1] Endoporus bildet in Aufsicht nur einen flachen, konkaven Bogen.

Bemerkungen: Unterschiede zur fsp. *E. subsudeticus* liegen in flacherem Endoporus, größerer Wandstärke erheblich stärkerer Skulptur (bei „*subsudeticus*" nur schwach extrapunctat).

Typenangaben: Holotypus: Taf. 33, Fig. 1—8; Exemplar entstammt Präparat Weißwasser-Rietschen 19 (1955) — 10,4 *(19,8/98,1)*. (Gleiches Exemplar ist schon 1958 a [= Synonymie-Zitat oben] abgebildet worden.)

[Die sonstigen Typusangaben stimmen mit dem bei der fsp. *Pflugipollis lusaticus*[1] (S. 137) Vermerkten überein.]

Derivatio nominis: Nach der Gesamterscheinung benannt; *inflatius* = angeschwollen, gewölbt.

Synonymie (zugleich zum Teil für die folgende fsp. berechnet, da bisher nur summarische Gruppen- bzw. Formgenusangaben veröffentlicht worden sind):

1956 c Gruppe 19: Große, dickwandige Form mit Endoplicen auf einer Seite (intern als *Emscheripollis* bezeichnet, noch unbeschrieben). — Krutzsch: Tabelle I, Spalte 19.

1957 Gleiches Zitat wie voriges. — Krutzsch: 126, 128, Taf. (= Tabelle) XIX, Spalte 19.

1958 a Gruppe 46: *Emscheripollis* Krutzsch 1958 b fsp. — Krutzsch: 517, Tabelle II, Spalte 46, Taf. 6, Fig. 32—34 (Abbildungen beziehen sich nur auf die vorliegende fsp.). [Zitat 1958 a = informatorische Angabe.]

(I) Diagnose: Größere Pollen (⌀ etwa 40 bis 45 μ) aus dem Normapolles-Stamm mit proximal gelegenen, kräftigen, plicenartigen Tori, die beide Wandteile umfassen. Äußere Wandschicht an den Germinalien bis zu 10 μ stark, in der Seitenmitte bis zu 3 μ absinkend; Dickenzunahme erfolgt allmählich, d. h. tumescensartig. Äußere Wandschicht wahrscheinlich in zwei größere Lamellenpakete, die dicht zusammenhängen und auch nicht immer unterscheidbar sind, unterteilt. Das innere Paket bildet eine Art Anulus aus, während das äußere annähernd gleich stark bleibt. Ein Prävestibulum konnte bisher nicht mit Sicherheit festgestellt werden. Das Exogerminal ist in Form eines schmalen, vertikalen Colpus,

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Species name: *Hungaropollis ajkanus* Góczán (1964)

6a

6b

6c

6a—c. Hungaropollis ajkanus nov. sp. (Ajka, Kossuth-shaft, Lower Campanian)

Description:

Diagnosis: Pollen grains triporate with the pores situated in the equatorial plane. Exine thickened, sides concave; endexine thick exhibiting a circular pattern; vestibulum primitive, narrow; interloculum complete. The lamella complex "a" of the endexine extends up to the primitive vestibulum; complex "b" up to the endopore; its edge facing the vestibulum is slightly split up. A narrow canal opens into the exopore towards the centre and grows wider, forming an oval cavity in the inner complex "c". No endanulus. Pore canal index above 0.4. Surface chagrenate.

Remarks: It differs from Hungaropollis krutzschi[1] nov. sp. in having a concave inner and a circular outer outline. Variable both in size and in the pattern of the endexine. Holotype 50 μ. Predominance confined to zone "D" of the Lower Campanian. Although scarce, it is a typical species of this zone.

Genus Trudopollis Pflug 1953.

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Species name: *Quedlinburgipollis altenburgensis* Krutzsch (1959)

Note here as elsewhere - This genus name is regarded by Skarby 1968 p. 24 as a synonym of Extratriporopollenites Pflug.

Author; Krutzsch, W., 1959, p. 142, pl. 33, figs. 13-33, text fig. 11

Reference: Einige neue Formgattungen und -Arten von Sporen und Pollen aus der mitteleuropäischen Oberkreide und dem Tertiär: Palaeontographica Abt. B, vol. 105, nos. 5-6, pp. 125-157, pls. 31-35, 18 text figs.

Type locality: East Germany, Harz district, Altenburg near Quedlinburg

Type horizon: Upper Cretaceous, Senonian, Santonian, leaf clay in association with Heidelberg beds, brackish-terrestrial clay

Preparation: Not indicated

Repository: Paleontological spore collection, staatliche Geologische Kommission, Berlin

Holotype: Pl. 33, figs. 13-17, prep. Quedlinberg-1/27, coord. 19.3/102.5

[1][Bracketed information derived from generic description.]

TYPE SPECIES OF THE GENERIC NAME
(Monotypic in Krutzsch, 1959, p. 141)

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Locus typicus: Ajka, Kossuth-shaft.
Stratum typicum: lower seam group, bright brown coal, Lower Campanian.
Holotypus: slide N° A-K-28-55/2, pollen N° 38.3—92.0. (Pl. VIII, fig. 5a—b.)
Derivatio nominis: in honour of W. KRUTZSCH, palynologist, Berlin, Germany.

[Diagnosis: Pollen grains triangular, triporate, with thickened exine and rounded angles. Exine thickness measured along the sides equals 1/5 of the whole diameter. Exine polyanulate. No atrium, oculus and polpapillus. Interloculum complete, vestibulum primitive. Endexine consisting of three complexes of layers, each of which terminates at different distance along the pore region. The narrow pore canal enters an oval cavity within the inner complex of lamellae and terminates in a narrow aperture towards the centre. Germinal apparatus developed on the proximal side. Pore canal index above 0.3.

Remarks: Hungaropollis nov. gen. is closely related to Papillopollis Pflug 1953, on the one hand, and to Extratriporopollenites Thomson et Pflug 1953, on the other. From the former it is differentiated by the lack of polpapillus and by the size of the anulus, from the latter by the patterns of the endexine and the pore region. It has been known so far from the Senonian deposits of Hungary only. Index fossil of the Campanian substage.

Generotypus: Hungaropollis krutzschi nov. gen. et sp.

Derivatio nominis: after its occurrence in Hungary.][1]

Author: Góczán, F., 1964, p. 254, pl. 8, figs. 4a-b, 5a-b
Reference: Stratigraphic palynology of the Hungarian Upper Cretaceous: Acta Geol., Acad. Sci. Hungary, vol. 8, nos. 1-4, pp. 229-264, 9 pls., 2 tabs.
Type locality: Hungary, southern Bakony Mts., Ajka, Kossuth-shaft
Type horizon: Upper Cretaceous, lower Campanian, lower seam gp., brown coal
Preparation: Varied, controlled by lithologic character of samples (coals: HNO_3; marls: HCl + HNO_3 and/or HF), heavy liquid gravity separation in $ZnCl_2$ or CdI_2; gelatin-carbolic acid mounts
Repository: Not indicated
Holotype: Pl. 8, figs. 5a-b, slide no. A-K-28-55/2, pollen no. 38.3-92.0

[1] [Bracketed information derived from the generic description.]

*[Specific epithet spelled *krutzschi* in Góczán, 1964. This orthographic error is corrected here per Art. 73, Note 3, 1964 Int. Code Bot. Nomencl.]

TYPE SPECIES OF THE GENERIC NAME

Note here as where appropriate — this genus is regarded by Skaarup 1968 p. 26 as a synonym of Extratriporopollenites Pflug

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CFSP
Reference

[*Heliosporites saevus*
= *Antulsporites saevus* (Balme 1957)
Archangelsky and Gamerro 1966, p. 369.]

Hymenozonotriletes pseudoalveolatus (Couper 1958)
Singh 1964, p. 83
basionym: *Cingutriletisporites pseudoalveolatus*
Couper 1958 15-123

Phyllocladidites mawsonii (Cookson 1947)
Couper 1953 forma *verrucosus* (Cookson
1957) Singh 1964, p. 113
basionym: *Dacrydium mawsonii* (Cookson
1947) Cookson 1957 forma
verrucosus Cookson 1957 15-58

[Although this is listed as a new combination by Singh 1964, the combination is not attributable to Singh. The species *mawsonii*, originally assigned to *Disaccites* by Cookson, 1947, was reassigned to *Phyllocladidites* by Couper, 1953. As the forma *verrucosus* was described by Cookson, 1957, for the original species, *mawsonii*, albeit in the genus *Dacrydiumites*, if *mawsonii* is assigned to *Phyllocladidites*, all the infraspecific entities are included. The correct citation would be: *Phyllocladidites mawsonii* (Cookson 1957) Couper 1953 forma *verrucosus* Cookson 1957.]

Pityosporites alatipollenites (Rouse 1959)
Singh 1964, p. 123
basionym: *Pinus alatipollenites*
Rouse 1959 24-95

Plicatella erdtmanii (Pocock in Singh 1964 ex
Pocock 1964) Amerom 1965, p. 112
basionym: *Appendicisporites erdtmanii*
Pocock in Singh 1964
ex Pocock 1964

33-233

33-233

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REFERENCES FROM

PUBLICATIONS TREATED IN VOLUME 33

Amerom, H.W.J. van, 1965. Upper-Cretaceous pollen and spores assemblages from the so-called "Wealden" of the province of Leon (Northern Spain): Pollen et Spores 7:93-133. [CFSP Vol. 33]

Archangelsky, S., 1963. Notas sobre la flora fósil de la Zona de Ticó, provincia de Santa Cruz. Introduccion y 1. *Cladophlebis tripinnata* n. sp.: Ameghiniana 3:57-63.

_____, 1965. Notas sobre la flora fósil de la zona de Ticó Provincia de Santa Cruz: Ameghiniana 4:52-56. [CFSP Vol. 33]

_____ & J.C. Gamerro, 1965. Estudio palinológico de la formación Baqueró (Cretácico), Provincia de Santa Cruz-I: Ameghiniana 4:159-170. [CFSP Vol. 33]

_____, 1966. Estudio palinológico de la formación Baqueró (Cretácico), Provincia de Santa Cruz-II: Ameghiniana 4:201-207. [CFSP Vol. 33]

_____, 1966. Estudio palinológico de la formación Baqueró (Cretácico), Provincia de Santa Cruz-IV: Ameghiniana 4:363-372. [CFSP Vol. 33]

Balme, B.E., 1957. Spores and pollen grains from the Mesozoic of Western Australia: C.S.I.R.O. Fuel Research, Physical and Chemical Survey of the National Coal Resources, Ref. T.C. 25, Chatswood, N.S. Wales, 48 pp. [CFSP Vol. 16]

Baranov, V.I., V.K. Nemkova & G.K. Kondpatev, 1957. Leaf impressions and spore-pollen assemblage of plant-bearing horizon from the Mikhailovska formation of the river Kem: Publication of Western Kazakhstan Institute vol. 117.

Belsky, C.Y. & E. Boltenhagen, 1963. Sporomorphes de position taxonomique incertaine du Crétacé Supérieur du Gabon: Grana Palyn. 4:262-270.

_____ & R. Potonié, 1965. Sporae dispersae der Oberen Kreide von Gabun, Äquatoriales Afrika: Paläont. Zs. 39:72-83. [CFSP Vol. 33]

Bolkhovitina, N.A., 1953. Spore-pollen character of Cretaceous deposits of the central provinces of the USSR: Trans. Inst. Geol. Sci., Acad. Sci. USSR 145 (Geol. Ser. 61):1-184 (in Russian). [CFSP Vol. 8]

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COMPILERS' PREFACE

Volume 33 of the *Catalog of Fossil Spores and Pollen* treats the new taxa, emended specific and generic descriptions, new combinations and new names from seven publications on Cretaceous spores and pollen. The microfossils are from North and South America, Africa, and Europe.

It will be noted that we are continuing expanded coverage begun in Volume 32 by presenting emended specific and generic descriptions which appeared in the papers covered. In addition, in Volume 33 we also are treating new combinations and new names. Where this supplementary information was associated, when originally published, with a new species, the CFSP coverage is in the main body of the volume. Otherwise, it is given in the form of an appendix, at the end of the volume (see Table of Contents). Volume 33 covers 25 new genera, two new subgenera, 111 new species. Four emended generic descriptions are presented, of which two are in the appendix; the appendix also contains an emended description for one species. Sixteen new combinations, and four new names, are treated in the appendix.

The 25 new genera treated are:

Andreisporites Belsky et al. 1965

Classoidites Ameron 1965

Complexipollis Krutzsch 1959

Constantinisporis Belsky et al. 1965

Hungaropollis Góczán 1964

Latipollis Krutzsch 1959

Duplopollis Krutzsch 1959

Emscheripollis Krutzsch 1959

Favoisporis Krutzsch 1959

Heliosporites Archangelsky & Gamerro 1966 [= *Antulsporites* Archangelsky & Gamerro 1966]

Pflugipollis Krutzsch 1959

Psittacopollis Ameron 1965

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wide at the apices; appendices very thick in a vertical section and usually split into two to three branches depending upon the degree of inflation of the spore, ektexine on the proximal side ornamented by about fifteen sets of ribs, each about 2 microns wide and spaced about 0.5 microns apart, and parallel to each side; the ribs closer to the centre of the spore merging at their extremities with the thickened margo bordering the laesurae, only the outer ribs reaching the apices; ektexine on the distal side ornamented by the same number of ribs with similar width and spacing; the ribs closer to the outer margin converging at the base of the appendices, but each rib at the distal pole fusing with the one just outside it and so on, the last one reaching to the apex; a triangular area about 15 microns in diameter cut off at the distal pole with complex ribbing pattern, having ribs parallel to only two sides; ribs commonly branched.

Size range: Equatorial diameter (including appendices) 85 to 95 microns.

Equatorial diameter of the holotype (including appendices) 94 microns.

Distribution: Middle Albian. Rare in the shale member of the Clearwater Formation and in the Grand Rapids Formation.

Type locality: Fort Augustus No. 1 well

Holotype: Pl. 5, figs. 1, 2, 4, 5. Depth 2842 to 2851 feet; Clearwater Formation, shale member.

Imperial Willingdon No. 1 well

Pl. 5, fig. 3. Depth 1911 to 1921 feet; Grand Rapids Formation.

Holotype: Pl. 5, figs. 1, 2, 4, 5. Slide No. 203; co-ord. 11.5/40.9.
Pl. 5, fig. 3. Slide No. 153; co-ord. 12.5/38.

Remarks: The specific name is derived from "*bifurcatus*" (divided, branched into two) and refers to the appendices which are usually split.

Since $ZnCl_2$ etc for Goczan

ALSO
PP
173
175
177
178
180
182
184
etc
to 219

Author: Singh, C., 1964, p. 54, pl. 5, figs. 1-5

Reference: Microflora of the Lower Cretaceous Mannville group, east-central Alberta: Res. Council Alberta, Bull. 15, 239 pp., 29 pls., 13 text figs.

Type locality: Canada, Alberta, Lsd. 7, Sec. 29, T. 55, R. 21, W 4 M., Anglo-Home-C. and E., Fort Augustus No. 1 well, 2842-51 ft. [The Provincial Survey System is used to define localities. Terms are provided in the following order: Legal subdivision (Lsd.), Section (Sec.), Township (T.), Range (R.), Direction and Meridian (M.).]

Type horizon: Lower Cretaceous, Mannville gp., Clearwater fm., shale memb.

Preparation: 10-15 g. sample, washed, crushed (Calcareous samples: 85% orthophosphoric acid diluted to 50% with dist. H_2O), 48% HF; Schulze's sol., 15-20 mins. (coaly samples 3-4 hrs.); 5% K_2CO_3, 3-5 mins.; wet sieved for megaspores (100-mesh screen), ZnBr separation of fines (sp. gr. 2), corn syrup or polyvinyl alcohol (Elvanol grade 51-05) mounts, generally single grain mounts

$ZnBr_2$

Repository: Paleobotanical collection, Research Council of Alberta, Edmonton, Canada

Holotype: Pl. 5, figs. 1-2, 4-5, slide no. 203, coord. 11.5 x 40.9

Hunt Institute for Botanical Documentation

CHALONER

University College London
Gower Street London W.C.1. England

Department of Botany & Microbiology

20 November 1970

Dear Al,

Many thanks for proof of vol. 33. I will do my best to get comments etc. to you by 7 December.

Best wishes,

Sincerely,

BW

W.G. Chaloner

Professor A. Traverse,
Catalog of Fossil Spores & Pollen,
Deike Building,
The Pennsylvania State University,
University Park,
Pennsylvania 16802,
U.S.A.

Hunt Institute for Botanical Documentation

CATALOG OF FOSSIL SPORES AND POLLEN

Chaloner

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

THE
INTERNATIONAL COMPENDIUM
OF
PALYNOLOGICAL SYSTEMATICS

November 13, 1970

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

In the attached package is your "proof" copy of Volume 33 of the Catalog of Fossil Spores and Pollen. Please return your review to me by 7 December, so that we can get the thing published before the first of the new year.

As I have previously explained, it is not necessary to return the volume to us. Just remove the pages on which you wish to make comments and return those, or (if the comments are relatively simple) merely mention the page number and line in your review.

You will note that this volume is some pages shorter than the last two volumes. The reason is that Volumes 31-32 were nearly a hundred pages too long, which strains both the ring-binders and the CFSP budget.

Looking forward to receiving your review and with assurances of the great value of these reviews to our editorial process, I am

Yours very truly,

Alfred Traverse
Editor

AT/vsi

Hunt Institute for Botanical Documentation

October 20, 1970

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill,

Many thanks for the reprint of 'The Rise of the First Land Plants". Believe it or not, I am able to use it the first day of it's arrival - must give a lecture in evolution of the embryophytic plants. Naturally, I am also very pleased to have copies of the other papers, especially 'Palaeobotany and phytochemical phylogeny". Your productivity continues to astound me.

AASP just had its third annual convention in Toronto, and it went very well--110 registrants, if I remember correctly. Mostoof the papers were very good, but we need to do something to elevate the standards of illustration. Some of the slides were really abominable! Geoff Norris did a splendid job of organizing and running the meeting, and it reflects considerable credit on him.

I was able to have dinner one night with Michael Fisher, and found it a most interesting experience. I told him about the fact that I have a student working with me on several Triassic problems, mostly in Texas, and I'm hoping that the student will be able to meet Fisher, and perhaps you may be willing to see him too on his way back to this country from Austria where he is at the moment. His name is Bob Dunay. I believe you met him at Baton Rouge, but it's doubtful that you would remember it.

Best wishes as always.

Yours very truly,

Alfred Traverse
Professor of Geology and Biology

AT/vsl

Hunt Institute for Botanical Documentation

October 6, 1970

Dr. W. G. Chaloner
Department of Botany
and Microbiology
University College London
Gower Street
London W.C. 1, England

Dear Bill,

Thanks for yours of 28th of September, and I have sent off for more information about the book. We should have it, obviously. Glad you got your copy of Volume 32, and I hope that you also got the letter which was sent to subscribers (that might have been omitted in your case) regarding the damnable business of missing pages. It is entirely possible that your copy is missing one or more pages. Please check and let us know. We are printing additional copies so that we can send pages to the people who are missing them. The printers had a fire in the midst of the printing of Volume 32, and that was what caused the difficulty. It has given us no end of trouble. Volume 33 has been unconscionably delayed for reasons mostly beyond our control, but you <u>really</u> should be getting your review copy before too long. One of the fall-out effects of the long delay is that Volume 34 will follow rapidly on its heels! Must be getting back to getting ready for the A.A.S.P. meeting!

By the way, thanks for the note on the name change of the *Cosmosporites*. The only reason that it was not footnoted in Volume 32 is that we didn't pick it up! With you on the editorial committee, such things won't happen in the future. Best wishes to as always.

Yours very truly,

Alfred Traverse
Professor of Geology
and Biology

AT/kkw

Hunt Institute for Botanical Documentation

Department of Botany and Microbiology
UNIVERSITY COLLEGE LONDON
GOWER STREET LONDON WC1

TELEPHONE 01-387 7050

28 September 1970

Dear Al,

Thanks for your letter, and for Vol. 32 of the Catalog. I didn't expect any advance compensation for my efforts. Incidentally, I note that in that volume, Cosmosporites Richardson is used without any citation to his later name change (Richardson, 1962, Palaeontology 5, p. 192) of that genus to Calyptosporites. I think that despite the sanctity of an original description, an actual nomenclatural change (which carries no taxonomic element) should perhaps be noted, even if just as a footnote, in the catalogue treatment. — with a thought for future volumes?

Regarding Studies in Fossil Plants, I am, for obvious reasons, delighted that you enquire, and enclose the appropriate brochure! ⊗

With best wishes,

Sincerely,

T Bill

W.G. Chaloner

Professor A. Traverse,
Catalog of Fossil Spores & Pollen,
Deike Building,
Pennsylvania State University,
University Park,
Pennsylvania 16802,
U.S.A.

⊗ Sorry: none left! 'Delighted', because Alvin, Barnard and I co-edited the book. It is published (as a bound volume) by Academic Press — Berkley Square House Berkley Square, W. 1. (London).

Hunt Institute for Botanical Documentation

September 14, 1970

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

When you get around to it, would you let me know how to go about ordering "Studies on fossil plants," edited by Alvin and published by the Linnaean Society of London in 1968? I suppose all I need is an address for the Linnaean Society. Thanks in advance.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT:kc

Hunt Institute for Botanical Documentation

University College London
Gower Street London W.C.1. England

Department of Botany & Microbiology

14 May 1970

Dear Al,

Thank you for your letter of March and the various papers which followed it. It did not seem to need any special reply, so it coasted along until now.

I well understand the position regarding Nichols, and of course I would have advised him on the same lines as you did. An academic post, so specifically tailored and equipped for palynology is a rare find. I am sorry that he won't be here, but obviously the decision is the right one.

I look forward eagerly to seeing the first draft of the next Catalog number withwhich I am to be involved. Also, incidentally (although your involvement here is very indirect), to the proceedings of the first AASP meeting! I see in your Bulletin that "all the MSS have been refereed" - I am delighted, but can the authors not see the referees' suggested changes? I realise full well that this is not your province, but if you get near the wheels of the editorial members of AASP, see if you can get them turning.

We have a conference on sporopollenin scheduled in London in September, and immediately following it a meeting of C.I.M.P. I shall be in Maine with the family in August, and will mail two years AASP sub. from there. Sorry I shan't be able to stay on for the Toronto meeting, which I would greatly have enjoyed attending.

Best wishes to you, your family, and Penn State Colleagues,

Sincerely,

Bill

W.G. Chaloner

Dr A. Traverse,
Department of Geology & Geophysics,
Pennsylvania State University,
University Park,
Pennsylvania 16802, U.S.A.

Hunt Institute for Botanical Documentation

March 11, 1970

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

Please excuse me for mixing AASP and Penn State business in one letter. I am enclosing the copy of the abstracts of papers presented at the "recent" convention. Also, I am sending a copy of the new brochure on the Catalog which you requested (naturally, you may have as many of these as you wish by merely asking!). So much for the AASP.

re NSF deal

I don't know whether Doug Nichols has written you from his temporary headquarters in Tempe, Arizona, or not. I visited him briefly there last week but forgot to discuss the matter with him in the press of other business. Th Spackman's and my surprise, Doug has been offered a really very attractive position at one of the new campuses at the State University of New York. They are offering to equip a beautiful palynological setup for him with the works, including even a full set of the Catalog! Under the circumstances, we are advising Doug that it would probably be unwise (in view of the tight job situation in palynology over here) for him to accept the NSF deal to work with you in England, even if it is awarded. I realize the danger that this may offend you and hurt our chances of getting somebody in the future to spend the year with you, but it seems to us that for a man who is a little older than the average new Ph.D. and has a family, this is sound advice. I only hope that you will think so too. Of course, Nichols, when and if he gets the NSF award, may decide to overrule us!

Yours very truly,

Alfred Traverse
Secretary-Treasurer

AT:kc

Enclosures: abstracts, brochure re Catalog

Hunt Institute for Botanical Documentation

University College London
Gower Street London W.C.1. England

Jan 15 1970

AASP.

Dear Al,

May I continue my life-long policy of battening on American generosity whenever offered, and request the Abstracts of the papers presented at the Penn. State AASP convention? Also, please, the 1969 Brochure on the Catalog mainly for its complete bibliography & just to keep me up to date generally.

Best wishes

Sincerely

Bill Chaloner.

Hunt Institute for Botanical Documentation

University College London
Gower Street London W.C.1. England

Department of Botany and Microbiology

30 December 1969

Dear Al,

The candidate sounds too good to be true - even his two failings commend him to me. I sincerely hope he gets his award; I won't write him further, having simply given an enthusiastic but perforce qualified 'yes' to the proposal from this end. As soon as there is a glimmer of positive information re his N.S.F. application, would you have him let me know, and I will get wheels turning here.

Best wishes to you all for 1970,

Sincerely,

Bill

(W.G. Chaloner)

Professor A. Traverse,
International Compendium of
Palynological Systematics,
Deike Building,
The Pennsylvania State University,
University Park,
Pennsylvania 16802,
U.S.A.

Hunt Institute for Botanical Documentation

December 12, 1969

Dr. W.G. Chaloner
Botany Department
University College
Gower Street
London WC1, England

Dear Bill:

As I'm sure you guessed, I was aware that Doug Nichols was considering applying for an NSF grant to support a year's study in collaboration with you. In fact, it was I who suggested this as a possibility. Nichols is really a very fine student. He presented the preliminary results of his thesis work at the recent AASP convention, and it was the consensus of most people that he did a better job than most of the professors on the program!

I have been working in close association with Nichols for over three years now, and I have found very little about his work which is other than first-class. He has a truly remarkable general grasp of geology--much better than mine. On the biological side, he is better prepared than most palynologists of my acquaintance, though he is not primarily a botanist and would not claim to be. He is on the whole, very good company and a desirable person to have around. I can only think of a couple of negative things to mention in order to make this a fully frank appraisal. Doug is not an aggressive individual, and to some Americans this is regarded as unfortunate, but I remember when I was a student at Kings in Cambridge being criticized by some of my English colleagues for being too aggressive. What I mean to suggest is that Doug can sometime give the impression of being diffident, though this is more of an impression that is created rather than a true evaluation of his qualities. He can also be maddeningly eclectic to the point that one begins to wonder if his interests are those of a dilettante and yet when the chips are down he seems to come up with pretty good ideas. He is not the hardest working man I have ever known, partly because he insists (who knows?--he may be right) on spending large blocks of time each week with his family. Around American high-pressure research outfits such as this one, that is regarded as heretical. As an overall rating, I would say Doug is a very capable man, one of the two or three best of his age group.* My own guess is that he will not turn out to be another Krutzsch as far as productivity is concerned, but I expect that he will publish some first-class papers before he is through.

* in 70's

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I can't really assess what Doug's probabilities of getting the grant are, but I would suppose he might have a pretty good chance. His record is really first-class, and I expect he will be recommended by the various people whom he asks. If you want still more information and would care to be more specific in your questions, don't hesitate to write me again.

Best wishes to you, Judy, and the kids for the holiday season.

Yours sincerely,

Alfred Traverse
Associate Professor of Geology & Biology

AT:kc

Hunt Institute for Botanical Documentation

PERSONAL AND CONFIDENTIAL

University College London
Gower Street London W.C.1. England

Department of Botany and Microbiology

3 December 1969

Dear Al,

Now you can help me with a problem! Can you briefly and informally comment on your student Nichols? His paper qualifications look most impressive, and I am very interested in his area of research; we would be happy to have him here and there is plenty of scope for Tertiary palynology with an ecological slant in Britain - assuming he can get an N.S.F. grant. But before I put up a formal (provisional) request to have him accommodated here, I feel I must approach at least one of his referees!

With best wishes,

Yours sincerely,

Bill

W.G. Chaloner

Professor A. Traverse,
Department of Geology and Geophysics,
The Pennsylvania State University,
University Park,
Pennsylvania 16802,
U.S.A.

Hunt Institute for Botanical Documentation

November 5, 1969

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London W.C. 1, England

Dear Bill:

I hope I won't become too much of a pest, but can't resist using a good source of information when I have discovered one. We are currently wrestling with a difficult problem which you may be able to help us with.

The enclosed photocopies are of pages from a 1964 paper of Singh. Singh apparently had access to a manuscript of Pocock, which was eventually published in late 1964, or early 1965 (we are still trying to run that down), but in any case after the Singh paper.

The question is--what is the status of the names Singh took over from Pocock (probably without permission and certainly--according to a letter--without intention of valid publication)?

Consider Appendicisporites erdtmanii, p. 48, for example. A description is provided, and only one specimen is figured or mentioned. I would interpret that as a holotype even though the word holotype is not used. Does this constitute valid publication? If so, should it be cited ....Singh or Pocock in Singh, or Pocock ex Singh? Does the statement "in press" have the effect of invalidation?--it wouldn't seem so to me.

Appendicisporites crickmayii, p. 47, seems a different case because there are several illustrated specimens mentioned. There

Hunt Institute for Botanical Documentation

is, therefore, no type. A. crickmayii would clearly seem to me to be a nomen nudum. Right?

Appendicisporites cooksonii (Balme) Pocock, p. 49, is a new combination. What appears on p. 49 seems obviously enough to satisfy all requirements, unless the statement "in press" has some sort of force that I don't find in the code. Should this be cited A. cooksonii (Balme) Pocock in Singh, or ....Pocock ex Singh, or ....Singh?

This is an excellent illustration of the reasons why I urge not using manuscript names ...ever, at all.

Thanks for your help.

Yours very truly,

Alfred Traverse

AT:vsi
Enclosure: pp. from Singh paper

Hunt Institute for Botanical Documentation

University College London
Gower Street London W.C.1. England

Department of Botany and Microbiology

WGC/GG

2nd October, 1969

Dear Al,

Thank you for the flattering invitation to join the Editorial Committee of the Catalog, which I gladly accept. The free copy and the kudos of being on the committee will be very adequate reward; but of course my real motive is that I enjoy seeking (and especially finding) fault with your work (and Tate's!). Many thanks, and I will look forward to my participation, which I trust will be prompt enough not to delay things.

Best wishes,

Sincerely,

Bill

W. G. Chaloner

Dr. A. Traverse, Editor,
Catalog of Fossil Spores and Pollen,
Pennsylvania State University,
Deike Building, University Park,
Penn. 16802, U.S.A.

Hunt Institute for Botanical Documentation

September 25, 1969

Dr. W.G. Chaloner
Botany Department
University College
Gower Street
London WC 1, England

Dear Bill:

How would you feel about joining the Editorial Committee of the Catalog of Fossil Spores and Pollen? There are numerous reasons why I would find this very desirable and appropriate. One is to give a more international flavor to the committee. Another, and more important, is to get more "feed back" from the committee. I have been having a little trouble with one or two of the present members in that they do not scour the proof copies of the Catalog sufficiently--what Tate and I need is a really thorough going review of each volume.

The mechanics of the review are as follows: after we have completed our work on the formats, which are cards with the material mounted on them, we make a photocopy of the formats and send these bound copies to the Editorial Committee members. They tear out pages which seem to requires change, make comments in red pencil, and return them to me along with a letter explaining whatever alterations they recommend. We allow something like six weeks for the review, and in your case we would send the proof volume air mail. All you get in return for all this work is one free copy of each volume--and the glory of being listed on the Editorial Committee!

Let me know how you feel about this. Best wishes as always.

Yours very truly,

Alfred Traverse
Editor
Catalog of Fossil Spores and Pollen

AT:kc

cc: Dr. J.M. Schopf

Hunt Institute for Botanical Documentation

Eaton
Fidelity Onion Skin
100% COTTON

April 16, 1969

Dr. W.G. Chaloner
Botany Department
University College
Gower Street
London WC 1, England

Dear Bill:

Well, I know when I'm licked! I have been working a couple of hours a week on Russian, but you aren't playing fair with these trips to Russia. I derive only slight solace from the fact that you made two grammatical and four spelling errors in the Russian you wrote on my card. (Perhaps there is hope for me after all.) It was good of you to remember me with a greeting.

Bob Sanders and family were over at our house the other day, and I enjoyed showing pics of the Chaloners at home. Bob is doing a first class job with this information thing. There is a great deal of political pulling and hauling going on about the thing, with the Oklahoma people trying to capture the whole field for themselves. We are resisting this bid for "cooperation", as they (in Machiavellian style) call it, and I think that we will win the day.

Best wishes as always.

Yours very truly,

Alfred Traverse
Associate Professor of Geology
and Biology

AT:kwc

Hunt Institute for Botanical Documentation

Москва. Вид на набережную имени Тараса Шевченко
Moscow. A view of Taras Shevchenko Embankment
Moscou. Vue sur le quai Taras Shevtchenko
Moskau. Blick auf die Taras-Schewtschenko-Uferstrasse
Moscú. Vistal del malecón Taras Shevchenko

ПОЧТА

Geol. Moscow Арш

ДОРОГИИ КОЛЛЕГИ! МОЙ ЛЕКЦИИ ПО-РУССКИ БИЛ ОЧЕН ХОРОШО! I'm getting all wind up for Novo Sibirsk 1971: how are those verbs coming along Al? There are 500 palynologists listed in the national directory here (there's a circulation for the catalogue!) Ed. Stanley is here - you can't escape those Penn State Alumni. Best wishes, Bill Chaloner.

СОВЕТСКИЙ ХУДОЖНИ

Фото Т. Бикмана и А. Наталиной

АМЕРИКА - U.S.A.
Prof. A. Traverse et al.
Earth Sciences,
Coll. Mineral Industries
Penn State University
University Park,
Pa. U.S.A.

Hunt Institute for Botanical Documentation

Pile Chaloner

C.I.M.P. Group No. 21 : The Classification of Palaeozoic Spores.

QUESTIONNAIRE

If you have any interest in the attainment of a single widely accepted classification of fossil spores, may I urge you to complete this questionnaire, and return it to me? If your available time is severely limited, please at least simply answer the questions by ticking or numbering the boxes as required. Beyond entering your name and institution, no further writing is needed and the whole thing can be completed in fifteen minutes; it can then be mailed to me to-day, or (if you prefer to procrastinate over such things) at least by the end of February, please. The resulting collation of the views of palynologists on the purpose and processes of spore classification will then be reported to the C.I.M.P. meeting to be held in Liège, April 1969, and will be available to you and others in the form of the report of that meeting.

If your time is slightly less pressing, you might also care to read the post-script to this questionnaire (p. 7) setting out briefly the background to this group and its objectives. Please do not be misled by the occasionally flippant tone of this questionnaire; its purpose is entirely serious, and the collective opinion of palynologists on the matter of spore classification is here being sought in a spirit of serious-minded enquiry.

W. G. Chaloner.
(Secretary, C.I.M.P. Group No. 21)
Botany Department,
University College,
London, W.C.1., England.

31st January, 1969.

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NOTE: These questions are to be answered with either a tick ☑ in one or more of several boxes, or a number indicating an order of preference: [1] the best; [2] the next best; and so on. You will be told in each question which is required. If you consider that any question is unfairly phrased, or that in your opinion the question has no meaning, or that it cannot be answered in the form suggested, or that for any reason you cannot or do not wish to answer it, then tick the box opposite "Don't know or etc.", which is intended for these and similar circumstances.

NAME: (Capital letters) . . ALFRED . TRAVERSE

INSTITUTION, address: . . . . . .

DR. ALFRED TRAVERSE
Dept. of Geology & Geophysics
Pennsylvania State University
University Park, Pa. 16802

Section A: Interest and Experience of Worker:

1. I have particularly studied, or am otherwise interested in, material of the following age(s): (tick one or more) -

Devonian and/or earlier:	☒
Carboniferous:	☐
Permian:	☐
Mesozoic:	☐
Tertiary:	☒
Quaternary:	☒
~~None~~ interest in palynology general:	☒

2. Have you published any papers dealing with, or touching on, palynology?

Yes ☑ No ☐

3. Have you needed at any time, for publication or for other record purposes, to arrange records of fossil spores or pollen according to some system?

Yes ☑ No ☐ Don't know or etc. ☐

Section B: Objectives and Motivation of Worker: Palynopsychology (the study of mental processes of those studying palynology).

4. Which of the following approximates most closely to your interests and aims (tick one box only):-

a) Are you interested in fossil spores mainly as a basis for effecting biostratigraphic correlation of rocks in an applied context (e.g. petroleum exploration)? ☐

b) Are you interested in palynology with biostratigraphy as the main aim in order to attain correlation for its own sake (i.e. a stratigraphic palynologist s.s.)? ☐

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c) Is your interest in palynology partly for biostratigraphy but at least equally for other purposes viz. palaeoclimatology, other aspects of palaeoecology, etc? ✓

d) Are you interested in palynology as a special branch of palaeobotany, being interested in the fossil spores per se as a primary objective? ☐

e) Don't know, etc. ☐

5. For the purposes of your own work in palynology, do you consider the most useful arrangement of fossil spore genera to be (tick one box only):-

a) An attempt at a biological classification, hence with implications of a "natural" or "evolutionary" or "phylogenetic" structure (cf. Couper, Palaeontographica B 103, 1958) ☐

b) A more or less morphographic system (cf. Potonié 1960 et seq. - see also other comparable systems, page 5.. ✓

c) A series of species and genera, with the genera unassigned (or regarded as incertae sedis) at the suprageneric level, (cf. Schopf, Wilson and Bentall, 1944) and so arranged alphabetically, or etc. ☐

d) Some other system (you need not specify; or indicate briefly if you wish) ☐

e) Don't know etc. ☐

6. Which of the following statements best approximates to what you regard as the purpose of a morphographic classification (tick one box only):-

a) It is a purely arbitrary way of arranging taxa of spores for convenience of reference in data retrieval; expediency is the only criterion for judging the definition of its constituent taxa without reference to what is known of Recent or fossil parent plant (in situ) relationship. ✓

b) It should be primarily of an arbitrary and expedient nature, as outlined above, but this spirit should be modified by a preparedness to accept categories which are thought to be biologically significant (e.g. "perinate", "saccate") even though in fossils the distinction between these categories may be clear in most cases but not all. ☐

c) It should be as expedient as possible, but major biological concepts such as the pollen/spore distinction may also be incorporated. ☐

d) Don't know, etc. ☐

7. Which of the two following statements best approximates to your views on the limitations of a given classification (tick one box only):-

a) Any satisfactory morphographic system should be at least potentially applicable to spores of all types and ages ✓

b) A system based, say, primarily on Devonian spores could never be satisfactorily applied to Mesozoic or Tertiary spores (even within the same range of types - e.g. the trilete spores of

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view as to the basis on which fossil spore genera are to be founded (tick one box only):-

a) A genus based on fossil spores must be founded solely on its morphography (morphology); that is to say, the characters observable in the spore itself (regardless of its age or supposed relationship) determines its generic assignment. [✓]

b) The genus may be based on a combination of age and morphology (i.e. it is reasonable to place a Carboniferous spore in the genus Leiotriletes and to assign a Mesozoic spore of basically very similar morphology to Deltoidospora, for the very reason of this difference in age) [ ]

c) Don't know, etc. [ ]

9. Which of the following statements best approximates to your view of some form of numerical handling of palynological data (as for example the allocation of an arbitrary number to each spore taxon, a number to each feature, and the handling of the data by means of punch cards or computer, or analagous method) without the construction of a classification (tick one box only):-

a) Such a method would serve only as a basis for data recovery, which has little or no bearing on the need or otherwise for a spore classification . [ ]

b) Such a method might be expected eventually to render a spore classification unnecessary. [ ]

c) Such a method might contribute to an assessment of the value of different criteria in a morphographic classification, but this will, at most, only complement a spore classification and will never be a substitute for it. [✓]

d) Don't know or etc. [ ]

10. Arrange in the order that you regard as that of their importance, the following three attributes of any proposed spore classification (putting 1 in the box of the most important, 2 in the second, etc.):

a) That its constituent subdivisions (however based) are clearly defined on criteria that are evident to all workers and are as unambiguous as possible [1]

b) That the classification conforms to what you regard as the most satisfactory basis for such a classification, even if you concede that some of the concepts may be hard to apply and may be misconstrued by other workers. [3]

c) That the classification - whatever its inherent shortcomings or merits - has been widely used and/or is likely to be widely adopted in future work. [2]

d) Don't know. [ ]

Section C: The structure of a morphographic classification.

11. In your view, should the ideal morphographic classification (tick one box only):

a) have a single criterion to define each level in the heirarchy (e.g. all turmae to be defined on aperture characters only, all subturmae on equatorial features, etc. cf. Dettmann) [ ]

b) have different criteria employed at the same level in the heirarchy, within different divisions (e.g. that the subturma Azonotriletes may be subdivided on the character of its sculpture, while the subturma Saccites may be divided on the number of sacci (cf. Potonié; Smith and Butterworth) [✓]

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12. Whatever your answer to the previous question, in what order do you consider the following features should be used in the heirarchy of a morphographic classification? Indicate your view by entering a number in each box, putting a 1 in the box for the feature which you believe should be used at the highest taxonomic level, 2 for the next and so on down to 7 for the feature to be rated at the lowest level. If you think any feature should not be used at all, put a dash - ; (please put a number or dash in each box):-

a) Exine stratification (or separation of exine layers) [7]

b) The sporites/pollenites distinction [2]

c) Spore size (e.g. megaspores/microspores separated at say 200 μ) [1]

d) Exine sculpture [6]

e) Character of aperture (triradiate, monolete, colpus, etc.) [3]

f) Equatorial feature [4]

g) Number of apertures [5]

h) Other(s) (you need not specify; or state here) [8]

i) Don't know or etc. [—]

13. Solely in terms of the clarity (unambiguity) of the term as you understand it, arrange the following in sequence. Note that this is merely stating your view on how easy it is to decide whether any spore has this feature, or not; it is not a statement about the importance of each character in a classification. Put 1 in the box opposite the clearest, 2 in the next, and so on: (put a number in each box):-

Saccate [1]

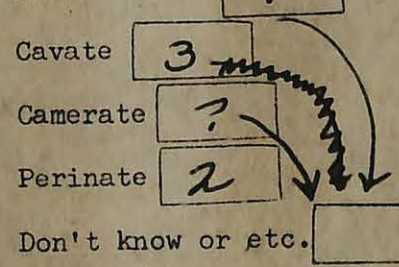
Pseudosaccate [?]

Cavate [3]

Camerate [?]

Perinate [2]

Don't know or etc. [ ]



Section D: Relative merits of systems in current use:

14. Please indicate with a number in the appropriate box below the extent to which you have knowledge of the various systems of classifications, on the following scheme:
If you have never heard of the work, write 1

If you have previously seen the work, and have a superficial acquaintance with it, write 2

If you have read it, but not studied it critically, write 3

If you have studied the system carefully and in detail, write 4

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b) Corsin et al 1962 [1.5]

c) Dettmann 1963 [3]

d) Hart 1965 [1]

e) Neves and Owens 1966 [1.5]

f) Richardson 1967 [2]

g) Smith and Butterworth 1967 [2]

h) Potonié "in lit. 1967" (an outline scheme sent in an open letter to Akpern, and circulated to CIMP members in the C.R. of the Paris meeting of February 1967 [1]

15. Which one of these systems do you believe to be the most satisfactory <u>in general</u> (since they clearly do not all have the same scope, you may tick one or more boxes):-

a) Potonié and Kremp et. seq. (1960, etc.) [✓]

b) Corsin et al. (1962) [ ]

c) Dettmann (1963) [✓]

d) Hart (1965) [ ]

e) Neves and Owens (1966) [ ]

f) Richardson (1967) [ ]

g) Smith & Butterworth (1967) [✓]

h) Potonié "in lit. 1967" [ ]

Finally, please feel free, if you have time, to amplify your answers on a further sheet. Any comments received will be carefully read and presented (in abbreviated form if necessary) to the Liège meeting.

N.B. — I have answered this "across the board", <u>not just for Belgium</u>

5/ I feel that purely morphographic description is the only way in the Jurassic and purely "botanical" the only way for Pliocene — I am ambivalent to varying degrees in the Cretaceous - Eocene interval.

9/ "a" + "c" seem to me virtually the same answer — by checking "c", I did not mean to denigrate data recovery.

12/ The trouble with the question is that my answers would differ quite a bit if the genera were broken down to "species and "forms".

13/ Please get Smith & Butterworth + Kremp, etc. to

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February, 1967 under the secretaryship of Dr. B. Owens, to consider the problems of conflicting Palaeozoic spore classifications, and if possible to reach some compromise on a generally acceptable form of classification which could be used by the CIMP.

2. A questionnaire was accordingly sent out by Dr. Owens prior to the Sheffield CIMP meeting (1967) seeking the views of palynologists on certain aspects of the matter.

3. The questionnaire elicited disappointingly few replies. These and the many still unresolved differences in aims and methods were further discussed at the Sheffield CIMP meeting (September 7, 1967) and W.G. Chaloner succeeded B. Owens as secretary of the Group.

4. Two meetings of the London Palynology Group (augmented by CIMP members from Sheffield and elsewhere) reviewed the matter informally, in October 1967 (at Kings College, London) and again in November, 1967 (at Imperial College).

5. An informal meeting of the Group (and any other interested palynologists) is planned at University College, London for mid-March, 1969. This questionnaire is designed to elicit views on the present position. Despite its cumbersome form, this questionnaire has been drawn up in such a way that the effort and time required to answer it will, it is hoped, be minimal.

6. While the aim of this CIMP group is Palaeozoic spore classification, some effort has been made to avoid begging the question of whether this is, or is not, in conflict with classifications appropriate to spores of other ages. Accordingly, replies are invited from any palynologist.

W.G.C.

14. "1.5" = the limit of it but not on it.

P.S. The names are mine, but the ideas are the product of the group by AMOS, SANDERS, TRAVERSE & VISHER

Lloyd Traverse

Hunt Institute for Botanical Documentation

October 30, 1968

Dr. W.G. Chaloner
Botany Department
University College
Gower Street
London WC1, England

Dear Bill:

The packet of reprints you mentioned at the AASP meetings--Palaeoecology of fossil spores" and others--was waiting here when I got back from the field trip that followed the meetings (on which we picked up some very interesting Triassic material in North Texas). Thank you for keeping me abreast of your really fantastic productivity (but of course you don't have the Catalog and its chattels around your neck!). If we could shut all of you completely off for about 50 years, I think the rest of us might get caught up.

I also have your letter re AASP membership and the check from Marcia, which came in a delighfful flowery envelope (I am so glad that we had the sense to elect her to the Executive Committee!). It would probably be more systematic to write to you about all that when I take a day soon and bring AASP business up to date. Naturally, there will be no problem--we are honored to have you aboard. Your presentation at the meeting was the highlight of the affair, and I suppose that you also stood LSU on its head the next week.

Best wishes to your wonderful family.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

AT:kwc

Hunt Institute for Botanical Documentation

October 11, 1968

Dr. W.G. Chaloner
Botany Department
University College
Gower Street
London WC 1, England

Dear Bill:

This is ridiculous, answering yours of 14 June, when I'll be seeing you in person in about a week. But it seems unsystematic not to answer letters, even after three months, and (who knows?)--I could miss you in the crush in Baton Rouge.

The money for the data thing came, and I hired R.B. Sanders, a fairly recent Wilson Ph.D., who (as you doubtless know) was at Nottingham last year replacing W.A.S. Serjeant. The project has money for at least one other professional type, in theory, but NSF has put a lid on expenditures. It's all very complicated, but the gist from our point of view is that Sanders and his secretary will be the whole staff for a while. I am sorry that I didn't let you know about this more promptly, but there just hasn't been time, and negative news for your man wouldn't have helped much anyway. Should we have a burst of available cash, I'll contact you again for suggestions.

I note that you say you were to be in USA in August, in "N.Y. and Maine". We have a summer home in upstate N.Y. If you make more such forays in the future, do let us know, so that a rendezvous at our place, or wherever, might be contemplated.

Best wishes.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

AT:kwc

Hunt Institute for Botanical Documentation

Department of Botany
UNIVERSITY COLLEGE LONDON
GOWER STREET LONDON WC1

Quain Professor of Botany and Head of Department:
D. LEWIS, D.Sc., F.R.S.
Professor of Plant Chemistry:
L. FOWDEN, Ph.D., F.R.S.

Telephone: EUSton 7050

June 14 1968

Dear Al,

I have just returned from a field trip chasing fossil plants and spores (Devonian) round Ireland. I hasten to reply to your letter, and am inflicting my typing on you to try to speed things up.

Your project sounds very exciting - I am a little sorry that I didn't get your letter about ten years ago, when I would have have eagerly applied myself ! There is one student taking our MSc. course this year who is very interested; but I fear he may be too junior in standing for what you want. He graduated in geology last year, and will have done a year of palynology and other micropaleo- this month. Just to give you some idea of the character of our course here, I enclose the theory exam paper which they are sitting at this very moment. Do let me know if a man at this level would be in the running. If not, can you lay down any further specifications ?

We'll be in the US through August, but alas only in N.Y. and Maine.

Forgive a scrappy letter - the exam season is here at full blast.

Best wishes. Bill. Chaloner

Hunt Institute for Botanical Documentation

UNIVERSITY COLLEGE LONDON

UNIVERSITY OF LONDON

M.Sc. in the Faculty of Science in Micropalaeontology

THEORY III - PALYNOLOGY

FRIDAY 14 JUNE 1968 Morning 10 - 1

Answer FOUR questions.

N.B. In the following, the term spore is used in its broad sense, including spores s.s. and pollen.

1. Fossil spores have been used successfully for "fine stratigraphy" in both the Carboniferous and the Quaternary. Discuss how far the method, and the principles behind the method, differ in these two cases.

2. In some instances the change in character of the fossil spores within one geological period is greater than that occurring between that period and the ensuing one. Discuss this with reference to either the Cretaceous or the Devonian.

3. Write notes, illustrated where appropriate, on four of the following:- Saccate; Intratriporopollenites; Cystosporites; Prepollen; Monolete; Priority in nomenclature; Cingulum; Classopollis.

4. Give a brief account of the range of form of angiosperm pollen, and discuss the problem of discriminating between angiosperm pollen and that of gymnospermous plants, when found in the dispersed state.

5. The morphographic system of spore classification, widely used for some years, has recently been subject to criticism. Give an account of this system, indicate the basis of the criticisms, and discuss how far objections to the system can be overcome.

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.../over

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6. What is meant by heterospory, and how far can the existance of a life cycle of this type be discerned in fossil dispersed spores? Give a brief account of the main types of fossil megaspores through the geological record.

7. How may palynological systematic and stratigraphic data be recorded and processed in purely numerical form? What are the advantages and disadvantages of such procedure?

Hunt Institute for Botanical Documentation

May 29, 1968

AIR MAIL

Dr. William G. Chaloner
Botany Department
University College
Gower Street
London WC 1, England

Dear Bill:

There seems to be a good chance that the National Science Foundation will soon fund a rather large projcet under my supervision for study of systems for information storage and retrieval in palynology. If so, do you know of anybody with a degree in palynology who would like to come here for two years to work with me as Research Associate at a salary in the $10,000-plus range? Naturally, the man could indulge his own interests too, using our now rather good facilities, as long as he produced the work which I am expected to develop for NSF. What do you say?

Regards to your charming family.

Yours very truly,

Alfred Traverse
Associate Professor of Geology
Editor
Catalog of Fossil Spores and Pollen

AT:kwc

Hunt Institute for Botanical Documentation

15 October, 1966

Dr. W. G. Chaloner
Department of Botany
University College
And all that

Dear Bill:

Doggone it, it turns out that various other obligations are going to keep me from being present for the Peabody Museum thing in New Haven after all. The only reason that I care a bit is that it will mean I won't see you again.

Needless to say (I hope), if you could arrange to visit these hills after you finish at New Haven (or before, for that matter), we would be delighted to have you do so and stay with us.

[redacted] voyage.

Yours very truly,

Alfred Traverse

Hunt Institute for Botanical Documentation

23 January, 1963

Dr. W. Chaloner
Department of Botany
University College
Gower Street WC1
London, England

Dear Bill:

Regarding yours of 26 November, which has been too long in the answering--may I first belatedly wish you all the best for 1963.

You will note my change of address. By now the grapevine that you mentioned in your letter may have clarified the message a bit. The gist of it is that I have resigned from Shell and am here studying theology, with the idea of eventually combining palynological with ecclesiastical interests. It remains to be seen just what this will mean.

Regarding the Congress and the program: if I could get Shell to agree, and if I could get my ecclesiastical authorities to let me go, I think I could make a very interesting contribution to the B symposium, with a paper on my recent sediment work. It should help considerably with some of the problems involved in megafossil-microfossil relationships.

I am writing the appropriate authorities at Shell to open the question of clearance and to my bishop regarding the possibility of my getting sprung for a week or two from the clinical training I would normally be taking at the time of the Congress. There would also be the problem of finances, but I have no doubt that with an invitation from you, I should be able to promote money from the National Science Foundation, or what-have-you.

Yours very truly and with all best wishes,

Hunt Institute for Botanical Documentation

UNIVERSITY COLLEGE LONDON · GOWER STREET WCI

DEPARTMENT OF BOTANY

HEAD OF DEPARTMENT:
Professor D. LEWIS, F.R.S.

Telephone EUSTON 7050

26th November 1962

Dear Al,

Conflicting reports drift along the branches of the grapevine, regarding your future plans. I sincerely hope you will be "available" to palynology for 1964, and will give the attached letter your earnest consideration. I hope you will not feel that this is tailoring the field of discussion too closely; it is at least a part of spore work in which you have played a major role, and we hope you can make it!

Sincerely,

Bill Chaloner

Dr. A. Traverse,
Shell Development Company,
3737 Bellaire Boulevard,
Houston,
TEXAS, U.S.A.

Enc.

Hunt Institute for Botanical Documentation

Palaeobotany Section Committee

Xth International Botanical Congress

Dear

We are making provisional plans on behalf of the International Botanical Congress Organising Committee, Palaeobotany Section, to convene speakers for the Palynology symposium (Number 5, p. 6 of the printed programme already circulated). As you may know it is the feeling of the organising committee of the whole Congress that the maximum amount of time should be given to certain selected symposia rather than to a miscellany of contributed papers. One day has been allocated to symposia dealing with pre-Quaternary palynology; two themes are to be considered:

A. (Probably morning.) Spore Morphology and Phylogeny: it is intended that among other things this should pursue the question of how far the record of living genera (and the use of names based on living plants) can be extended backwards in time by palynological records.

B. (Probably afternoon.) The Relationship Between Spore Assemblages and Contemporaneous Macrofossil Floras (including quantitative aspects).

The committee are contemplating inviting two or three main speakers (to be allotted half an hour each) for each symposium, to be followed by several contributors of invited discussion; it is hoped that such an arrangement might make for more fruitful debate than usually arises spontaneously.

While we are not yet in a position to make formal invitations, we would be grateful to know:

(1) Do you intend to come to the Edinburgh conference?

(2) Regardless of your answer to (1), would you be interested to contribute to symposium A, either as a main speaker or with an invited contribution to discussion?

(3) Do you wish to suggest any modification in title or addition to the content of the symposia?

(4) We do not regard the distribution list of this letter as complete. Do you know of any colleagues or others who are currently working along the lines of either of these topics (other than the addressees given below) whom you would wish to suggest as contributors?

Yours sincerely,

W.G. CHALONER
N.F. HUGHES

Copies to:

P.E. Balme
N.A. Bolkhovitina
G. von der Brelie
R.A. Couper
C.J. Felix
J. Gray
J.F. Grayson
E.M. Knox
A.R.H. Martin
J. Muller
W. Remy
G.E. Rouse
R.A. Scott
F. Staplin
A. Traverse
R.H. Tschudy

Hunt Institute for Botanical Documentation

University College London · Gower Street WC1
BOTANY DEPT. Oct. 6 1961

Dear Al,

Many thanks for your letter, and apologies for the delay in acknowledging it. It will be circulated around the palaeobotany section cttee., and discussed, with other proposals, at our next meeting. These are just the sort of comments and suggestions that we wanted - many thanks.

Yes, indeed we will be running field trips, but perhaps wrongly, we did not seek the views of "foreigners" as to what they should cover ! Believe me, we have enough divergent opinion here as to where and how they should be run ! There is plenty of advice available from locals in all the important areas as to where we should or should not visit. The outcome seems likely to cover a range from Scottish L. Devonian, through L. and U. Carb., Jurassic (of Yorks), Tertiary of the South of England, to Pleistocene. You will surely get the details later.

I shall be coming to the States as you rightly heard, after Christmas, to "visit" Penn State for six months giving a course in palynology mainly. I hope to get to Tucson, of course, and will look forward to seeing you there !

With best wishes,

sincerely,

Bill

W.G. Chaloner.

Dr. A. Traverse,
Shell Development Company,
3737 Bellaire Boulevard,
P.O. Box 481,
Houston 1 Texas.

Hunt Institute for Botanical Documentation

Botany Dept.
University College London · Gower Street WC1
Oct.1, 1959

Dear Al,

Many thanks for your letter, with the references concerning Continental Drift and similar matters. I will certainly turn them up when I next go to the British Museum. I have sent under seaparate cover a copy of the New Biology containing the Continental Drift article. I hope it may interest and/or amuse you !

I wrote to Bill Spackman Sept. 1, along the lines on which we had spoken, but have so far had no reply. Maybe his field work in the South has delayed his replying, although of course it is possible that the letter went astray, particularly if it was forwarded. Maybe I should write again. Many thanks again for your help and advice in this direction, anyway. I still live in hopes !

With best wishes, very sincerely,

Bill Chaloner

W.G. Chaloner.

Hunt Institute for Botanical Documentation

September 14, 1959

W. Chaloner, etc.

Dear Bill:

The papers for which I promised to send references are:

1. Krames, K. Neue Methoden zur Feststellung von Pol und Krustenbewegungen der Erde. Der Aufschluss, Jg., H. 6, pp. 109-117 (no date available).

2. _____ Geologischer Kompass. Geofisica Pura e Applicata - Milano, Vol. 22, Fasc. 1 - 2, 1952.

3. _____ Stubberuntersuchungen im Braunkohlentagebau der Grube Berrenrath. Braunkohle, Wärme und Energie, Bd. 8, Heft 17/18, pp. 329-336, 1956.

I think you'll find these interesting reading. I'd be interested in your comments some day. I have met Krames in Krefeld and found him quite an interesting chap.

Good to renew acquaintances with you in Montreal.

Best wishes.

Very truly yours,

Alfred Traverse

AT:jlb

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

September 12, 1958

Dr. W. G. Chaloner
University College
Gower Street
London, WC1, England

Dear Bill:

Thanks for the reprints. As might have been expected, you are doing very interesting and important work. I appreciate being kept abreast of your progress and having the papers for my files.

The Paleobotanical Section of the Botanical Society of America just had its annual meeting. I am secretary and played some part in organizing the affair. We had a week of it -- a field trip in southern Indiana (which 45 persons attended), followed by the technical sessions in Bloomington (four days of it, papers by Schopf, Wilson, Schemel, Delevoryas, Kremp, etc., etc.) Your name came up from time to time, and I remember wishing you could have been with us!

Yours very truly,

Alfred Traverse

AT:pte

Exploration and Production Research Division
Shell Development Company

Hunt Institute for Botanical Documentation

October 29, 1957

Dr. W. G. Chaloner
Botany Department
University College
Gower Street
London W. C. 1, England

Dear Bill:

Was delighted to hear from you again and especially to know that you are now in full production again. I occasionally hear about you by the rumor route, but it was good to get in direct communication once more. Professor Arnold and I have enjoyed a good laugh this year in reminiscing about the key episode in the Grand Ledge mine. He was with us on the paleobotanical field trip in California this year.

I am enclosing a card that may be used to obtain a copy of my Brandon monograph. You will have to replace the portrait of Franklin with a more appropriate feminine likeness but otherwise I think it will be o.k. Please let me know whether this produces results. If not, I will get you a copy by another technique. You certainly should have one. Under separate cover I am sending a packet of other reprints. Some of them you may already have, but I have no record of having sent you any.

I am indeed trying to keep up my interest in Paleozoic things and would like to be kept supplied with your papers.

I join you in hoping we can get together again one of these years. In the meantime, all best wishes for you and your work.

Very truly yours,

Alfred Traverse

AT:ml

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

Botany Department.

University College London · Gower Street WC1

Oct. 23 1957

Dear Al,

I wonder whether you could possibly spare me separates of two of your papers - namely that of 1955 on the pollen analysis of the Brandon Lignite, and your more recent Micropaleontology paper. In addition to my interest in Palaeozoic spores, I am having to apply myself to younger stuff to supervise a student working on the Oligocene lignites of the Isle of Wight.

I don't know whether you have time to interest yourself in Palaeozoic material at all, but I am sending some off-prints of mine by surface mail, in the hope that they may be of interest to you.

I am at present teaching here, and I am lucky in getting plenty of time for my own research. By being palaeo-botanists in the Department here, I am treading (however unworthily !) in the footsteps of Scott, Oliver, Salisbury and Marie Stopes!

I hope that your own work goes well, and that we may meet again some time soon in the world of palaeobotany.

With best wishes, sincerely,

Bill Chaloner

W.G. Chaloner.

Dr. A. Traverse,
Shell Development Co.,
Houston, Texas, U.S.A.

Hunt Institute for Botanical Documentation

Region V
Box LL, University Station
Grand Forks, North Dakota

October 20, 1953

Mr. W. G. Chaloner
The Museum of Paleontology
University of Michigan
Ann Arbor, Michigan

Dear Bill:

Thanks for the letter of October 14. As you will probably have heard from Professor Arnold by now, I have had to change my plans and shall not be going to State College before the meetings. I am going to the meetings by train, via Boston. It would have been fun to collect in the Pennsylvanian mines with Dr. Spackman, and I regret missing the opportunity.

If you plan a western tour as part of your stay here, I hope you'll plan to visit Grand Forks.

Very truly yours,

RP

Alfred Traverse
Coal Technologist

ATraverse:dm

cc: Region V
Lankford
Traverse
File 051
C. File

Hunt Institute for Botanical Documentation

The Museum of Paleontology,
University of Michigan,
Ann Arbor, Mich.

October 14th. 1953

Dear Al,

Have you by any chance been able to make more concrete plans for your projected visit to the Pennsylvanian coal-field en route to the Toronto conference ? If there is still any chance of its coming off, and I could work in with it in some way (with of course splitting the cost of gas etc.) I am still very interested in the possibility.

It is still a bit questionable whether I would be able to go on into Canada, but I should certainly like the opportunity to do some collecting in Pennsylvania.

I tried to macerate some of the winged seeds from the Grand Ledge shale, but without very much success. It seems that although they looked so promising in coming away from the matrix very readily, there is in fact little of the cuticle actually preserved. I am at present trying to work on some Mississipian megaspores from the Michigan coalfield, and also on some Cretaceous ones from Alaska.

I hope that your own work is going well:

with best wishes,

Yours sincerely,

Bill Chaloner

W.G. Chaloner.

Hunt Institute for Botanical Documentation