

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.

December 12, 1968

Dr. Irena Grabowska
Warszawa 12
Rakowiecka 4
Instytut Geologiczny
Pracownia Paleobotaniczna
Poland

Dear Dr. Grabowska:

Thank you very much for the interesting papers on Tertiary pollen of Poland, which arrived recently. Tertiary pollen were my "first love" in palynology and, if I could get time from other duties, I would still like to work in that area. I do occasionally get time for a little Tertiary research, and I certainly appreciate being kept abreast of developments. Under separate cover I am sending you some reprints which may be of interest.

Best wishes for a prosperous 1969.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

AT:kwc

Hunt Institute for Botanical Documentation

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

16 June, 1989

Dr. F. M. Gradstein
Atlantic Geoscience Centre
Bedford Institute of Oceanography
Dartmouth, N.S., Canada
B2Y 4A2

Dear Dr. Gradstein:

Just got the form letter re C.Q.S. meeting in DC in July. I'm just back from two months in Europe, and I know I can't make it to IGC. However, please don't strike me from C.Q.S. mailing list! I want to remain in. All the best.

Yours very truly,

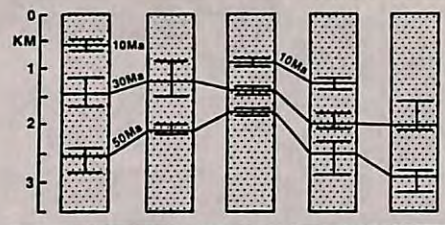
Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

COMMITTEE ON QUANTITATIVE STRATIGRAPHY (CQS)

under International Commission on Stratigraphy (ICS)



Chairman: Dr. F.M. Gradstein, Atlantic Geoscience Centre, Bedford Institute of Oceanography, Dartmouth, N.S., Canada, B2Y 4A2. Telephone: 902-426-4870 (or 2740), telex: 019-31552.

Secretary: Dr. C.M. Griffiths, Continental Shelf Institute (IKU), P.O. Boks 1883, 7001 Trondheim, Norway. Telephone: 47-7-920611, telex: 55434iku n.

Announcement:

Dear Colleague,

The Committee on Quantitative Stratigraphy (C.Q.S.) will hold a business meeting on Monday evening, 17 July, 1989 at 8:00 p.m. in Rooms 4 and 5 of the Convention Centre, 28th International Geological Congress, Washington, D.C. You are cordially invited to attend and discuss the C.Q.S. course and plans for future activities. Agenda items (submitted so far) are:

1. Introduction and Overview of 1988/89 activities.
2. Review of meetings, workshops etc. planned for 1990/91.
3. Newsletter and Publication of Bibliographies on Quantitative Bio- and Lithostratigraphy.
4. Software circulation and reviews.
5. Creation and support of "Stratigraphic (PC-based) workstation" under DOS and UNIX.
6. Round table.
7. Closing.

Other items of business that you like to see tabled should be communicated to Cedric Griffiths, or presented under Item 6 of the Agenda.

In October 1988, a subcommittee of C.Q.S. composed of Dan Merriam and Frits Agterberg reviewed activities and composition of the committee. Since "...C.Q.S. only formed in 1986 and is proceeding well...", the Subcommittee recommended that the present Committee Chairman and Secretary stay in office until the I.G.C. 29th in 1992. The Chairman and Secretary concurred, upon which this recommendation was forwarded to the I.C.S. (Dr. I. Cowie, Chairman) for official confirmation.

The 28th I.G.C. and C.Q.S. sponsored workshop on Quantitative stratigraphy on Saturday, July 15, 1989 in Washington is canceled, as requested by I.G.C. The 28th I.G.C. conference invited C.Q.S. to organize this event, but because of financial constraints (low subscription - probably the result of poor advertising in the I.G.C. program circular and awkward date in the weekend) asked us to cancel it. Instead we plan one or possibly two workshops next year, including one in Nottingham, U.K. on September 1, 1990 and a possible short-course in the U.S.A. in April 1990.

This summer, we will prepare Newsletter No. 3. Any news items should be sent to the undersigned. We particularly welcome details on new stratigraphic methods, programs and applications. An updated version of the very well received "Bibliography of Quantitative Biostratigraphy" by F. Thomas *et al.* is being prepared. Corrections, additions are invited. The companion volume on lithostratigraphy is making good progress (compiler, Cedrec Griffiths *et al.*).

Please make your views known, attend our business meeting and workshops and forward items you deem newsworthy or want to communicate.

Felix Gradstein

Felix Gradstein
Chairman, C.Q.S.

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)865-6711

23 February, 1989

Dr. F. M. Gradstein
Geological Survey of Canada
Atlantic Geoscience Center
P.O. Box 1006
Dartmouth, Nova Scotia, Canada B2Y 4A2

Dear Colleague:

Many thanks for "Recent developments in quantitative stratigraphy," "Bibliography and index of quantitative biostratigraphy," and "Newsletter CQS No. 2." All of these will be useful to a couple of my graduate students in our efforts to apply quantitative stratigraphic methods to our palynological work.

Perhaps I could go on the list for CQS Newsletter?

Best regards, and thanks again.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

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DEPARTMENT OF GEOSCIENCES
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(814)863-3419 or (814)865-6711

5 January, 1989

Dr. F. M. Gradstein (Chairman, CQS)
Geological Survey of Canada
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia, Canada B2Y 4A2

Dear Dr. Gradstein:

Just got around to reading AASP Newsletter (Oct.) and the news in it that I could obtain from you copies of "The Bibliography and Index of Quantitative Biostratigraphy" and the "Newsletter of the Committee on Quantitative Stratigraphy."

It would really help my probably losing battle to stay abreast if I could have these. I am astonished but grateful that this if free.

Best to you for a fine 1989.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

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

faxed to: 001-814-863-4627
(pg. 1 of 3)

5 May, 1992

file

Barbara Shannon, Associate Dean
The Graduate School
114 Kern
University Park, PA 16802 USA

Dear Friend:

I hereby fax my ballot to you.

However, after it is presumably adopted, the statement should be editorially revised for readability as I have shown.

Yours very truly,

Alfred Traverse

Alfred Traverse
Professor of Palynology

AT/et

NR.	:	002
EMPFÄNGER	:	-/0018148634627
SENDER	:	SNG KUHWALDSTRASSE
DATUM	:	05 MAI'92 10:42
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SEIT.	:	01
ERGEBNIS	:	OK

NR.	:	003
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DATUM	:	05 MAI'92 10:54
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ERGEBNIS	:	OK

NR.	:	004
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DAUER	:	01'38
MODUS	:	STAND
SEIT.	:	01
ERGEBNIS	:	OK

Hunt Institute for Botanical Documentation

EXISTING ARTICLE I OF THE ARTICLES OF AUTHORITY OF THE GRADUATE COUNCIL

The Graduate Faculty of The Pennsylvania State University shall consist of its present Senior and Associate Members and such members of the University community as hereafter shall be designated, on the basis of their academic credentials, scholarly achievements, and abilities in graduate education and research, to be Senior and Associate Members.

PROPOSED STRUCTURE

THE GRADUATE FACULTY

Graduate Faculty members ordinarily are University faculty members with the most advanced degrees in their fields who are assigned responsibilities in graduate education and research.

Criteria and Responsibilities

Those nominated for Graduate Faculty membership should be qualified to teach graduate courses, advise graduate students at masters and doctoral levels, and conduct research and scholarly activity leading to refereed or other professionally competitive publications or to comparable artistic, clinical, literary, or technical achievements appropriate to particular fields. Graduate Faculty members may direct theses, chair or serve otherwise on doctoral committees, teach 500-level courses and perform other such academic duties as may be authorized by the dean of the Graduate School.

ARTICLE I AS IT WOULD READ IF AMENDED

The Graduate Faculty of The Pennsylvania State University shall consist of its present Members and such members of the University community as hereafter shall be designated, on the basis of their academic credentials, scholarly achievements, and abilities in graduate education and research, to be Members.

2
3
as
1

This badly needs editorial revision — The "to be members" at the end is so separated from the sense as to be confusing — it should read:

...shall be designated as Members, ...

P.S.: This is p. 2 of 3-page letter

Hunt Institute for Botanical Documentation

BALLOT

PLEASE PLACE A CHECK IN THE BOX BY THE STATEMENT THAT INDICATES YOUR POSITION ON AMENDING ARTICLE I AND ADOPTING THE PROPOSED STRUCTURE CHANGE.

☒ I **FAVOR** amending Article I as specified above and adopting the proposed Graduate Faculty structure. *

☐ I am **OPPOSED** to amending Article I and adopting the proposed Graduate Faculty structure.

PLACE IN THE ENCLOSED SELF-ADDRESSED ENVELOPE, SEAL THE ENVELOPE, SIGN ACROSS THE SEAL, AND RETURN BY MAY 15, 1992.

Alfred Traverse

Alfred Traverse

* However, the statement should be editorially revised (doesn't change the meaning at all) as shown on following page.

N.B. ↘ This is p. 3 of a 3-page letter

Hunt Institute for Botanical Documentation

PENNSTATE

Barbara Shannon
Associate Dean
The Graduate School

(814) 865-2516
FAX: (814) 863-4627

The Pennsylvania State University
114 Kern Graduate Building
University Park, PA 16802-3396

DATE: April 22, 1992

FROM: Barbara Shannon, Associate Dean, The Graduate School

TO: Members of the Graduate Faculty

Call for Vote on Proposed Change in the Graduate Faculty Structure

The Graduate Council approved, on April 15, 1992, a proposal to change the structure of the Graduate Faculty. The proposed change would create a **single membership system to replace the current "two-tier" system, i.e., associate and senior memberships.** This proposal necessitates amending Article I of the Articles of Authority of the Graduate Council which specifies that the Graduate Faculty shall consist of Senior and Associate Members. Amendments of the Articles of Authority require approval of two-thirds of all Graduate Faculty members voting in a mail ballot sent to all members of the Graduate Faculty. The present structure of the Graduate Faculty and existing Article I of the Articles of Authority are attached together with the proposed structure and Article I as it would read if amended. Please read these carefully and then mark the ballot to indicate whether you do or do not favor amending the Articles of Authority and adopting the proposed structure. **If the proposed structure is adopted there will be only one level of membership on the Graduate Faculty with appointment to that carrying all rights and responsibilities of membership.**

Place the ballot in the enclosed, self-addressed envelope, seal it, sign across the seal, and return it no later than **May 15, 1992.**

Enclosures

An Equal Opportunity University

Hunt Institute for Botanical Documentation

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

11.I.93

Herrn Dr. Kurt Graf
Geographisches Institut
Universität Zürich-Irchel
Winterthurerstrasse 190
8057 Zürich, Switzerland

Sehr geehrter Herr Graf,

Haben Sie vielen Dank für die Übersendung Ihres interessanten und wichtigen Buches "Pollendiagramme aus den Anden." Es wird ein nützlicher Beitrag meiner Bibliothek sein. Es ist auch ein sehr angenehme Erinnerung an unsere Freundschaft 1980-81 in Zürich!

Ich wünsche Ihnen alles Gute zum neuen Jahr.

mit besten Grüßen

Ihr

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

Herrn
Dr. Kurt Graf
Geographisches Institut
Universität Zürich--Irchel
Winterthurerstrasse 190
8057 Zürich, Switzerland

den 21. Dez. 1983

Sehr geehrter Herrn Dr. Graf!

Ich schreibe Deutsch, weil ich gelegentlich diese Sprache üben möchte! Das nächste Mal würde ich meine Vorlesungen in Zürich auf Deutsch geben.

Vielen Dank für die Übersendung Ihrer Publikation über südamerikanischen Quartär, die ich mit grossem Interesse gelesen habe. Es tut mir leid, dass ich Ihnen mit Publikationen nicht helfen kann. Ich habe nämlich niemals mit südamerikanischen Sedimenten gearbeitet. Ich möchte empfehlen, dass Sie durch Ihre Bibliothek Zeitschriften von anderen sogar von amerikanischen Bibliotheken ausleihen könnten. Marine Geology haben Sie jedoch im Haus.

Mit besten Grüssen, auch an unsere gemeinsame Freunde in Zürich.

Ihr

Alfred Traverse
Professor für Palynologie

AT/et

Hunt Institute for Botanical Documentation

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

Department of Geosciences

Palynological Laboratories

Area Code 814
~~865-2342~~
863-3419

Herrn Dr. Kurt Graf
Geogr. Inst. Univ.
Postfach
8033 Zürich, Schweiz

16. Dez. 1981

Sehr geehrter Herr Dr. Graf!

Obwohl ich gebe freilich zu, dass Sie besser Englisch als ich Deutsch schreiben, möchte ich mein Deutsch üben! Ich habe schon viel verlernt. Ich danke Ihnen herzlich für die Übersendung Ihrer Veröffentlichung über südamerikanische Torfpalynologie, die ich gelesen habe. Ihre Resultate für Podocarpus-pollen hat mich besonders interessiert.

Ich wünsche Ihnen fröhliche Festtage und ein glückliches neues Jahr!

mit freundlichen Grüssen,

Ihr,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

4 August, 1980

A. Traverse

Buzz Graham

I need to confer with you re support picture for Litwin and Choi for Fall (80), Winter (81), Spring (81) terms. Am hoping you can fit me in for Monday, 11 August. Linda says you should be able to arrange it--I'll wait for time assignment.

Hunt Institute for Botanical Documentation

UNIVERSITY OF WISCONSIN-MADISON

DEPARTMENT OF BOTANY
139 Birge Hall
430 Lincoln Drive
Madison, Wisconsin 53706
Telephone: 608/262-1057

September 27, 1983

Professor Alfred Traverse
Palynological Laboratories
Pennsylvania State University
435 Deike Building
University Park, Pennsylvania 16802

Dear Professor Traverse:

Thanks for your note about Stebbins & Hill's paper and the Coleochaete/land plant origins controversy! I don't agree with Stebbins and Hill that plants originated on land, from unicellular algae, for two main reasons. First, given their hypothesis, one would expect to find a few remnant, intermediate, terrestrial charophytes, but there aren't any. Secondly, as noted in my abstract, charophytes, and Coleochaete in particular, appear to have features that could have served as preadaptation (preaptations) in the evolution of land plants. I have spent much time skin diving in the littoral zone, observing that environment and its selective pressures, and feel sure that the most significant of Coleochaete's putative preaptations occurred in response to life in the littoral. I cannot visualize an equally satisfying terrestrial scenario for the series of evolutionary steps that must have been involved in the origin of land plant gametangia and life cycle, without postulating "hopeful monsters". Thus, I feel the evidence available at present suggests that movement of "preadapted" multicellular charophytes onto land is a more reasonable hypothesis than that of Stebbins and Hill.

Thanks again for your interest. It is always enjoyable to think about these things.

Sincerely,

Linda Graham

Linda Graham
Associate Professor

LG:kjs

Hunt Institute for Botanical Documentation

UNIVERSITY OF WISCONSIN-MADISON

DEPARTMENT OF BOTANY

139 Birge Hall
430 Lincoln Drive
Madison, Wisconsin 53706
Telephone: 608/262-1057

September 16, 1983

Professor Alfred Traverse
Palynological Laboratories
435 Deike Building
University Park, Pennsylvania 16802

Dear Dr. Traverse:

In reply to your request for publications relating to my most recent abstract concerning Coleochaete, I have enclosed a reprint. Other related work is still under review or in manuscript and I will try to send that material on to you as it becomes available in reprint form.

Sincerely,

Linda E. Graham

Linda E. Graham
Associate Professor

LEG:kjs

Hunt Institute for Botanical Documentation

7 September, 1983

Ms. L. E. Graham
Department of Botany
University of Wisconsin
Madison, WI 53706

Dear Ms. Graham:

Your abstract re Colochaete greatly interested me--if a more extended version is available or ever is available, I'd like to have a copy.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

3 November, 1981

Mr. Yngue Grahn
Geological Survey of Sweden
Box 670, S-751
23 Uppsala, Sweden

Dear Mr. Grahn:

Am finally back from about a year's absence and want to thank you for sending me the marvellous Chitinozoan Newsletter, which has become much more than its name implies! It is really an early Paleozoic palynomorph newsletter and VERY WELCOME. Please keep it coming. I, ~~66~~rone, would be delighted to pay a fee for it.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

30 May, 1980

Dr. Yngve Grahn
Geological Survey of Sweden
Box 670
S-751 28 Uppsala, Sweden

Dear Dr. Grahn:

Just got yours of 21 May, and the very interesting Newsletter on Chitinozoans, with the great "logo"! I am not sure I understand whether I should write each time to Bernard Owens, but somehow I want to keep getting the publication--looks invaluable.

Thanks very much!

Yours very truly,

Alfred Traverse
Professor of Palynology
President, ICP

AT/et

Hunt Institute for Botanical Documentation

SGU SVERIGES
GEOLOGISKA UNDERSÖKNING

Prof. Alfred Traverse
Palynological Laboratories: Deike 435
Pennsylvania State University
University Park, PA 16802
U.S.A.

Uppsala, 21 May, 1980

Dear Prof. Traverse,

In CIMP Subcommission on Chitinozoa we sent the Newsletter free to all active workers on Chitinozoa. All CIMP members not actively working on Chitinozoa will get it free from the secretary of CIMP (Dr. Bernard Owens). In this case there has probably been a mistake in the distribution. I enclose an ex of the Newsletter with this letter.

Sincerely yours,

Yngve Grahn

Yngve Grahn
Geological Survey of Sweden
Box 670
S-751 28 Uppsala, Sweden

Box 670
751 28 UPPSALA
Tel 018/15 52 80

Telegramadress
Geosurvey
Postgiro
1 56 75 - 2

Box 801
951 28 LULEÅ
Tel 0920 - 976 00

Filialkontor:
Karl Gustavsgatan 12 B
411 25 GÖTEBORG
Tel 031 - 17 68 80

Tunavägen 35
223 63 LUND
Tel 046 - 14 01 05

Virvelvindsvägen 4 K
222 27 LUND
Tel 046 - 14 84 20

Box 118
930 70 MALÅ
Tel 0953 - 107 10

Plant Institute for Botanical Documentation

2 May, 1980

Dr. Yngve Grahn
Geological Survey of Sweden
Box 670, S-751
28 Uppsala, Sweden

Dear Dr. Grahn:

I have just heard of the Chitinozoan Newsletter. While I do not mainly work on Chitinozoans, it is necessary for me to keep abreast of developments in the field for teaching purposes. I would very much like to be on the list for distribution. I am a CIMP member.

Why not make a small charge for the Newsletter, so that marginal folks such as I don't have to feel guilty for asking?

Yours very truly,

Alfred Traverse
Professor of Palynology
President, ICP

AT/et

Hunt Institute for Botanical Documentation

February 18, 1970

Dr. L.J. Crambast
Institut Geologique
Fac. Sic.
Place Bataillon
34 Montpellier, France

Dear Louis:

I hear from Dr. Stafleu that you have not responded to his formal request that you state your willingness to serve on the Committee for Fossil Plant Nomenclature of the IAPT. I feel sure this is because the letter from Stafleu to you miscarried or for some other unexplained reason, because I am certain that you want to be on the Committee. Would you please write Stafleu immediately stating your formal willingness to serve? If possible, I would like a copy of the letter for our records here.

I do hope that all is well with you and that you are pushing ahead with your charophyte work. Looking forward to seeing you again one of these years, I am

Yours very truly,

Alfred Traverse
Associate Professor of Geology
and Biology

AT:kc

Hunt Institute for Botanical Documentation

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

9 August, 1991

Universitetsforlaget
Norwegian University Press
P. O. Box 2959 Toyens
N-0608 Oslo 6, Norway

Dear Friends:

Regarding the bill of which a xerox copy is enclosed, I am slightly confused. I paid IAA (International Aerobiological Association) $54 on 7 Feb. 91 by check on my paper corporation's account. This was for "IAA dues, plus Grana and pollen flora." Presumably I am already paid up for Grana. Could you explain?

Best regards.

Yours very truly,

A. Traverse

Alfred Traverse

Hunt Institute for Botanical Documentation

11 August, 1986

Grana
Almqvist & Wiksell
Box 45150
S-104 30 Stockholm
Sweden

Dear Friends:

Unfortunately I cannot any longer justify the personal expense of my Grana subscription and must depend in the future on the library.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: bill

Hunt Institute for Botanical Documentation

Grana

5 January, 1984

The Almqvist & Wiksell Periodical Company
Box 62, S-101 20 Stockholm,
Sweden

Dear Sirs:

Regarding the enclosed invoice: although I am a "charter subscriber", with a complete run back to Grana's predecessor journal, as an individual professor I can't afford $90.00 per year and must therefore cancel.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

The Almqvist & Wiksell Periodical Company
Box 62, S-101 20 Stockholm, Sweden
Phone 08-23 79 90, Cables: Almqvistbook

PROFORMA INVOICE

Our reference 144-0502-1

PLEASE ALWAYS QUOTE THIS REFERENCE

928458-9

Date 83-11-16

DR ALFRED TRAVERSE
PENNSYLVANIA STATE UNIV
435 DEIKE BLDG
UNIVERSITY PARK PA 16802
U S A

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Postal Account: 929250-9 Bank: Transfer Account 373-7707

SUBSCRIPTION WILL BE ENTERED AFTER RECEIPT OF PAYMENT

Plant Institute for Botanical Documentation

Publishers to the Royal Swedish Academy of Science and to the Universities of Stockholm, Uppsala and Gothenburg, etc.

December 7, 1972

Grana
c/o Almquist & Wiksell
Gamåa Brogatan 26, P.O. Box 62
S-10120 Stockholm 1, Sweden

Dear Sirs:

I recently received a subscription form for Grana. I was under the impression that I had a standing order for Grana--in other words that you just automatically sent it to me and billed me each year. Is this not true?

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

23 March, 1984

Dr. Paul Grant
Department of Geology
Royal School of Mines
Prince Consort Road
London SW7 2BP, U.K.

Dear Dr. Grant:

Per xerox enclosed, my records seem to hhow that I ordered one of the Spore Colour Charts--or at least intended to do so. Did I fail to send it in? Did I fail to send money? (I note letter doesn't say "enclosure"). At any rate, I never got the diapositive or card--I guess I would now really prefer the card, despite previous order. Could you let me know~~ii~~n what respect this fell through? I do want one of the colour charts somehow or other.

Looking forward to hearing.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: copy of previous letter

Hunt Institute for Botanical Documentation

27 February, 1985

Dr. Richard E. Grant
E-205 Natural History Building
Smithsonian Institution
DC 20560

Dear Dr. Grant:

May I have a copy of the new international directory of paleontologists? Professor Walliser tells me it's available from you for $7.00. Is that right, and to whom is the check to be made out?

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
phone: 814-863-3419
fax: 814-865-3191

14 August, 1990

J.D. Watkins, Production Director
Graticules, Ltd.
Morley Rd., Tonbridge
England TN9 1RN, U.K.

Dear Mr. Watkins:

Thanks for yours of 2 August re difficulties with the England Finder. I am sharing your letter with a colleague who brought up the problem I mentioned earlier.

However, I feel that perhaps I did not fully explain the problem. Following the suggestion in my textbook, and because others had earlier proposed the same thing, England Finder locations for illustrated specimens are beginning to appear in publications about fossil spores, pollen, etc. When somebody borrows such an illustrated slide from the author and is using a microscope that inverts the field, or even that has a different mounting system from that used by the original author, there are reports of difficulties.

For that matter, in our own lab when we go from the Olympus microscopes in the teaching lab to the Leitz microscopes in the research lab, we note a regular slight difference about two letter-fields laterally. I have had no difficulty making the appropriate corrections.

If you have time to comment, it would be appreciated.

Best regards.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Directors: L. RHEINBERG, G. RHEINBERG, P. E. RHEINBERG, J. D. WATKINS, W. D. HOGBEN, B.Sc.

GRATICULES LIMITED

Morley Road · Tonbridge · TN9 1RN · England

Telephone: 0732 359061 Cables: RHEINBERG TONBRIDGE

Telex 95311 Fax: (0732) 770271

OUR REF:

YOUR REF:

DATE:
2 August 1990

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

For the Attn of Alfred Traverse, Professor of Palynology

Dear Professor Traverse,

Ref England Finder (EF)

Thank you for your letter dated 19th July, referring to a colleagues problem in using our England Finder Slide.

All E.F.slides are the same so the reference patterns are the same in relation to the three edges identified by the arrows.

It is important that the location edges used when producing the reference co-ordinates on a subject slide; by substitution of a E.F., are clearly indicated as reference datum edges, so the recipient of a subject slide uses the same edges of a E.F. slide and subject slide when seeking to identify the area of interest.

Whether the microscope has an image correcting optical system, or a straight forward reversing optical system should not affect the alignment; the information on the E.F. slide is laterally reversed, so that it reads correctly when viewed though a monocular microscope. If a microscope is used with an image correcting prism it will necessitate developing a technique of reading upside down information. Please note if the E.F. is reversed the sample on the subject slide will be reversed.

The key to using the England Finder is to make sure the correct datum edges are used when mounting slides into a mechanical stage.

I hope this information provides a solution to the problem.

Yours Sincerely,

J Watkins

For Graticules Ltd
J. D. Watkins
Production Director

Hunt Institute for Botanical Documentation

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

13 June, 1989

Graticules Limited
Morley Road
Tonbridge TN9 1RN
Kent, England, U.K.

Dear Friends:

We were distressed to receive your recent letter saying that you had still not received payment for 2 England Finders ordered and received long ago by us (see enclosure for invoice #, etc.).

As you can see by this enclosure, Penn State sent payment in early March. Please check your records and let us know immediately if there is still a problem. We use England Finders all the time in our work and certainly want to remain on good terms with your company!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: xerox of payment order

Hunt Institute for Botanical Documentation

Linda
This is our proof
of payment. Hope
this will clear up your
problem. ☺
Erma.

Mellon Bank N.A.
PITTSBURGH, PENNSYLVANIA 15230

PAYMENT ORDER NO. 184263

DATE MARCH 9, 1989

PAY TO: GRATICULES LIMITED
MORLEY ROAD
TONBRIDGE TN9 1RN
KENT, ENGLAND

AMOUNT	RATE	DOLLAR EQUIVALENT
GBP-62.50-	1.7403	USD-108.77-
	COMMISSION	
	CHARGES	
	TOTAL	USD-108.77-

FOR: RE: INVOICE NO. 60 36808 DATED 25-1-89 ORDER NO. LQ
463348 TERMS NEDD/MA

WE ACKNOWLEDGE RECEIPT OF YOUR INSTRUCTIONS TO TRANSFER THE ABOVE AMOUNT

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308 RIDER BLDG
120 S. BURROWES STREET
UNIVERSITY PARK, PA 16801

IS: NATIONAL WESTMINSTER BANK LTD
OVERSEAS BRANCH
53 THREADNEEDLE STREET
LONDON E C 2 ENGLAND

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STATEMENT

A/c PSU 02 As At 31-MAY-89

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Telephone: 0732 359061
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To:

PENNSYLVANIA STATE UNIVERSITY
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435 DEIKE BLDG
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USA

Document			Debit	Credit	Outstanding
Type	Reference	Date			
Invoice	SO 36808	25-JAN-89	62.50	copy invoice sent May 1989.	£62.50
				VERY OVERDUE cheque by return please!	
Aged Debt		MAY	0.00		
		APRIL	0.00		
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		FEBRUARY +	62.50		

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TERMS: STRICTLY NETT MONTHLY

TOTAL NOW DUE	£ 62.50

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From	Account	Date	Now Due
PENNSYLVANIA STATE UNIVERSITY	PSU 02	31-MAY-89	£ 62.50

Hunt Institute for Botanical Documentation

7 September, 1981

Mr. L. Rheinberg
Managing Director
Graticules Ltd.
Morley Road
Tombridge, Kent, U.K. TN9 1RN

Dear Mr. Rheinberg:

Thanks for yours of 14 August. I still intend to write a small note to Micropaleontology one of these days, explaining that further publications on conversion of microscope coördinates are superfluous because of the wonderful England Finder. I am hoping I can explain what your "official response" is to the problem that came up in Switzerland while I was there. I asked them to get a Finder for instruction purposes--and then discovered that it didn't work perfectly because some of the scopes are Wild, some Leitz, and the Wild stage is arranged in the opposite sense from the Leitz, so that the slide snugs down on the opposite side. We solved the problem by posting a card for the students, that it is necessary (as I recall) to add 2 letters to the reading from the Wild scope when moving to the Leitz. I just wanted you to be aware that the Finder is not absolutely isodiametric with reference to the grid.

"Palynology" is international--it is defined in most dictionaries, e.g. the "Webster's New Collegiate" I have in front of me. The word was coined in 1944 in England (!) by Hyde and Williams. The best known British practitioner is probably Dr. Norman F. Hughes, Sedgwick Museum, Cambridge, though there are perhaps 100 or so palynologists in U.K. Very briefly, palynology is the study of pollen, spores, and other microscopic chemically similar organic bodies.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
P.S. What is a "Maltwood Finder"? Do you have a little history of the England Finder?

Hunt Institute for Botanical Documentation

Directors: L. RHEINBERG, G. RHEINBERG, P. E. RHEINBERG, J. D. WATKINS, W. D. HOGBEN, B.Sc.

GRATICULES LIMITED

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Telephone: 0732 359061 Cables: RHEINBERG TONBRIDGE

Telex 95311

OUR REF: LR/IR YOUR REF:

14th August 1981

Mr. Alfred Traverse,
The Pennsylvania State University,
College of Earth & Mineral Sciences,
Department of Geosciences,
435 Deike Building,
University Park,
Pennsylvania 16802,
U.S.A.

Dear Mr. Traverse,

Thankyou for your letter of 30th July about the England Finder.

As I remember, when Mr. England first suggested the finder to me as an improvement on the Maltwood Finder, he proposed that only the two sides adjacent to the name card should be used for location purposes and indeed over the last 20 to 30 years of manufacture, we have always used these two edges for location so that on these two edges any England Finder should give the same readings. The insistence on using just these edges depends not only on the accuracy of the other edges of the finder, but also on the similar accuracy of the specimen slide, which at that time was extremely unlikely. The method offered accurate reading without the cost of expensive glass working either of Finder or of slides.

Since that time both microscope design and the accuracy to be expected from normal specimen slides has varied considerably so that many years ago we included the third edge with arrows, using for the Finder glass accurate in length to within .003". Nevertheless the original instructions just to use the two edges adjacent to the nameplate will yield the best results. Most microscopes have stages using the best two sides and I was under the impression that only one earlier model used the reverse sides - which I had assumed was why we hear so little of this problem today.

For interest sake I have just checked round in our factory to confirm that our manufacture and inspection uses still the original plates made for checking up.

Also for interest sake, may I ask you to explain the word "Palynology". I suspect that in England we use a different word for this science.

Wishing you every success with your paper, I am

Yours sincerely,

L. Rheinberg,
Managing Director.

Hunt Institute for Botanical Documentation

UNIVERSITÉ LOUIS PASTEUR

STRASBOURG

INSTITUT DE GÉOLOGIE

Strasbourg, 6 mai 1981

Professor Dr. A. TRAVERSE
Geologisches Institut
ETH-Zentrum
CH-8092 ZÜRICH

Dear Professor Traverse,

I thank you very much for your kind letter. I am glad to see that you much enjoyed the APP-APLF meeting, especially my father's contribution as well as mine.

Also for me it was a great and unexpected pleasure to make your acquaintance. What a pity that the languages restricted a little our conversations !

Indeed, the visit of the museum in Ringendorf was too short and thus a little frustrating, especially for those who were interested in a special subject. If you feel so and if you want, you can come again without any problem. Give me only notice of it beforehand.

Yours sincerely

Léa GRAUVOGEL-STAMM

LABORATOIRE DE GÉOLOGIE ET DE PALÉONTOLOGIE
SERVICE DE LA CARTE GÉOLOGIQUE D'ALSACE ET DE LORRAINE
CENTRE DE SÉDIMENTOLOGIE ET DE GÉOCHIMIE DE LA SURFACE DU C. N. R. S.
1, RUE BLESSIG - 67 - STRASBOURG - TÉLÉPHONE: (88) 35.66.03

Hunt Institute for Botanical Documentation

10 April, 1981

Dr. Lea Grauvogel-Stamm
Institut de Géologie de L'Université Louis Pasteur
1 Rue Blessig
F-67084 Strasbourg Cedex, France

Dear Dr. Grauvogel-Stamm:

I guess it makes no sense to write you in anything but English!--I can't speak or write French (though I can read it), and you would just as soon have English as German!

I did want you to know how very much I enjoyed the meeting as a whole, but especially your contributions to it! Last fall I read your book to my wife and student as we drove from Pennsylvania to Arizona--not having any idea who you were, or that I would someday meet you! It is a marvellous, interesting piece of work. What a thrill to meet the author and to see some of the specimens. It was actually a little frustrating to have only a few minutes in your family-museum with your collections. Some day, I hope I can visit again--for longer! In the meantime, please also tell your father how much I appreciated his generosity in opening the museum for the APP-APLF "horde"!

Best wishes to you in your research!

Yours very truly,

Alfred Traverse
Visiting Professor

AT/et

Hunt Institute for Botanical Documentation

Received: from donald.uoregon.edu by pangaea.ems.psu.edu
(4.1/PSU_ESSC/GEOSC-2.02) id AA21124; Wed, 12 Jul 95 12:28:04 EDT
Received: from OREGON.UOREGON.EDU by OREGON.UOREGON.EDU (PMDF V4.3-9 #7713)
id <01HSS065QUME8WXFU1@OREGON.UOREGON.EDU>; Wed,
12 Jul 1995 09:27:39 -0700 (PDT)
Date: Wed, 12 Jul 1995 09:27:39 -0700 (PDT)
From: Jane Gray <LADYJANE@oregon.uoregon.edu>
Subject: reprint
To: traverse@ems.psu.edu
Message-Id: <01HSS065QVK88WXFU1@OREGON.UOREGON.EDU>
X-Envelope-To: traverse@ems.psu.edu
X-Vms-To: IN%"traverse@ems.psu.edu", LADYJANE
Mime-Version: 1.0
Content-Transfer-Encoding: 7BIT

July 12

Science reprint will be in the mail today. Suppose it should go to your Penn State address-- if not, advise when you get this. Suppose that some would not find tct useful-- the problem was that i was dealing with samples from megafossil-bearing rocks, that had representatives of all three families, so it seemed reasonable that all were represented by pollen. One could, of course, spell out the names all the time. Why did the individual berate the useage-- was there a specific reason? Its kinda like the cheno-am designation. does this individual not like that either?

Congratulations to Betty. Why german lit?

i'm afraid if i took a nap that it would last all afternoon (or morning), but just drink coffee instead. its generally at night that it is hardest (especially after a glass of wine!). Have to stay up all hours to medicate an old (ancient really) cat that needs several kinds of pills at different hours.

all the best, jg

Hunt Institute for Botanical Documentation

Date: Tue, 11 Jul 1995 14:20:18 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: ladyjane@oregon.uoregon.edu
Cc:
BCc:
Subject: tct
X-NUPop-Charset: IBM 8-Bit

Dear Jane:

Good to get further info on TCT. No, I don't have a copy of the Science paper with Paul Martin. If you still have copies, by all means send me one. If you don't, give me the exact lit. ref., and I can xerox it in the library. I want to get it right. I must admit that you were not on my list of suspects, and were it not for Norm Frederiksen I would never have thought of you. One person I wrote said it was probably "somebody in the West." She then went on to berate anybody who would use such an expression as TCT. Actually, I find it very useful.

We're fine. Betty got her Ph. D. at the May Commencement (in German lit.). We just returned from a little trip to celebrate our 44th anniv. and my becoming emeritus (against my will), both on the same day, 30 June. We went to Portland, Seattle and Vancouver and vicinities. Oregon, which I had never visited before, greatly impressed me--for example, by the absence of sales tax--in Vancouver it was 12%! I doubt that I'll go to the Houston meeting. I never have liked meetings, and I don't "need" it any more. I might change my mind, in which case I will look forward to seeing you.

Part of your "tiring easily" is just the passage of years! I'm in great shape, and I also tire easily. Naps are the answer.

Best. Al T.

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Received: from oregon.uoregon.edu by pangaea.ems.psu.edu
(4.1/PSU_ESSC/GEOSC-2.02) id AA05732; Thu, 26 Jan 95 12:48:10 EST
Received: from OREGON.UOREGON.EDU by OREGON.UOREGON.EDU (PMDF V4.3-9 #7713)
id <01HMAQH5706C8WXYHI@OREGON.UOREGON.EDU>; Thu,
26 Jan 1995 09:48:02 -0800 (PST)
Date: Thu, 26 Jan 1995 09:48:02 -0800 (PST)
From: Jane Gray <LADYJANE@oregon.uoregon.edu>
Subject: recent letter
To: traverse@ems.psu.edu
Message-Id: <01HMAQH5706E8WXYHI@OREGON.UOREGON.EDU>
X-Envelope-To: traverse@ems.psu.edu
X-Vms-To: IN%"traverse@ems.psu.edu", LADYJANE
Mime-Version: 1.0
Content-Transfer-Encoding: 7BIT

Thanks for yours of Jan. 9. Thought I would try this method of communication for just a brief response and correction. First, I remember well that you were my instructor in paleobotany lab @ Harvard. I've always been grateful for that course and your part in it. I'm also delighted to hear that your book is doing so well-- I have seen it cited often. I'm also sorry that my letters appear to upset you so!

My diabetes is the insulin-dependent type-- obesity is not a factor-- I've dropped about 30 lbs since we last saw each other, so i'm a virtual walking skeleton. Ask your physician son to explain the differences between diabetes I (insulin dependent) and diabetes II (the obese type)! One is a metabolic disorder with none of the hormone produced-- the other is inability to use the hormone that is produced etc., etc. In any case, I'm doing really well at control, but it takes a certain care in scheduling, note keeping etc. Its a ready made disease for a scientist!

CORRECTION. You have an incorrect impression re Richardson. R. did NOT suggest bryophytic or hepatic affinities for the SPORE TETRADS. He did suggest the possibility of such affinities for late Silurian/Devonian SINGLE, TRILETE SPORES such as Streelispora and Aneurospora (see Phil. Trans R. Soc. Lond B309, 201). In the Richardson and Ioannides paper there is a passing reference to the possibility of bryophyte and lichen affinities for some spores, but this is labelled as "pure speculation" and again he is not referring specifically to spore tetrads nor made any specific comparisons between modern and fossil taxa.

I indeed was the first to suggest such affinities for OBLIGATE SPORE TETRADS. Incidently, I just reviewed a paper for Nature (Edwards, Duckett, Richardson) that reports an in situ tetrad occurrence in an hepaticlike plant. There was not one word that R. had suggested such affinities for dispersed spore tetrads. Surely there would have been had he so suggested. Sorry about that.

All of the best, jane

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES
PALYNOLOGICAL LABORATORIES
435 DEIKE BUILDING
UNIVERSITY PARK, PA 16802, USA
Phone: (814)863-3419; Fax: 814-863-7823
E-Mail: traverse@ems.psu.edu

9 January, 1995

Dr. Jane Gray
Department of Biology
1210 University of Oregon
Eugene, Oregon 97403-1210

Dear Jane:

Yours of 29 December is being read this afternoon between episodes of getting ready for the paleobotany course for the last time--and just when I was getting really good at it! Spackman taught it until he retired about eight years ago. (Well, actually, he offered it, but it only was given perhaps three times in the whole period that we were both here--lack of enrollment. Since I took it over the enrollment has been increasing, and it has been given every year but one, beginning in 1988.) I'm always reminded of you when I give the course, because you were once one of my students in the paleobot lab--I still use some of the ideas from that Harvard lab in my course here.

I am so sorry to hear that you are a severe diabetic patient. (In central PA in the countryside they would say that you have "bad sugar.") Our physician son with whom we are in daily contact informs me that the techniques in treating folks like you are now pretty sophisticated. As for me, I am indeed long in the tooth, but I seem to be in very good shape. My mother, whose family I most resemble, lived to 98, and her brother, my uncle, is still alive at nearly 100. We visited him in Ontario in July, and he was celebrating his 50th wedding anniversary to his <u>second</u> wife. He was married to the first wife over 20 years.

It is not misinformation that I was critical of some proposals of yours (or was it just one?) many, many years ago. But to characterize that as enmity is to imply that my criticism was personally motivated and/or not scientifically based. That I deny. It is true that I would have copies of the reviews, but I certainly don't have time to go to my archives (kept in cases on a landing here) to try to find them now, and I don't in the least care if you believe me or not.

I believe from my years of study in psychology (in seminary) that most well-informed readers of much of page one of your letter

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would see marked traces of paranoia. In fact, it is the opinion of even some of your best friends in science that you have been arrogant and overbearing at times. Personally, I forgive you for this, but I think you are not being completely honest in self-analysis if you fail to recognize this. Introspection is not one of your hallmarks.

Regarding the Potonié-Remy-Gothan, etc., "-phytic" business and its interpretation, I enclose some pages from my book that I think would convince any fair minded person that to be au courant with the literature you had to at least mention this other stab in your paper. I probably would never have thought about it, but another person called me saying he was mystified that you didn't seem to know. It's not that your boundaries and mine are the same, but that someone before you in the fairly recent past had had a go at the business that needed to be mentioned. It's not a question of "credit" but merely one of quoting the relevant literature. Your statement that it is you who cleared up the origin of the -phytic notion is baloney. It is/was common knowledge of all those who are familiar with the literature and have copies of the older German textbooks on their shelves. It doesn't need any more to be cited that one has to quote Darwin every time one talks about natural selection. Furthermore, the basic idea comes from the German literature of well back into the 19th century. I may have a go at expanding my coverage of this theme in the second edition of my textbook, which is in prep.

As far as I know, John Richardson is the first person to note similarity of the Ord./Sil. tetrads to liverworts (hepatics, as he put it).

If using Lady Jane as your e-mail address isn't egocentric, I don't understand the word. It would be like me using "Big Al". Egocentric is a synonym for vain, and whether you have vanity plates, or a car for that matter is irrelevant. Your e-mail address is exactly the same idea as vanity plates. Period. But I don't care, as I said before. It's even rather fun.

I am not especially a football fan, for the record. In thirty years at PSU, this was my third game. The first was in 1978 when I went as a priest to Columbus, Ohio, to conduct the funeral of Jim Schopf, and Esther (Jim's wife, Bill and Tom's mother) insisted that Betty and I use their tickets, as Penn State was in town. The second was a few years ago when Betty's Ph. D. professor (in the German Dept.) gave us her tickets for the Cincinnati game. The Tournament of Roses is much more than a football game, and I confess that I do love parades. A former student of mine who now works at U. of O. came by just before we left for CA to give me a bottle of "Ducks and Roses" ale, that he brought for me on the plane from Eugene. He says he knows you.

If you were in the same room with CM she can put you on her life list and claim to have met you, believe me. If you never met

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her, how come you asked to be remembered to her (though you couldn't spell her name)?

Well, that seems to cover the points raised. Incidentally, in case it's of any interest, I am no longer functioning as a cleric (since 1981). In fact, I have no connection with any religious body. I'm sure you're not interested, but as I brought up that connection several times above I thought I'd better complete the story.

Take care of yourself. God, it couldn't be that obesity got this started. My recollection of the last time I was with you, on the streets of NYC, some years ago, is that you were very trim and fit looking. Of course, as you hint, regardless of genetic background, etc., when one gets to be 70 (or thereabouts) one has one foot on a banana peel all the time. I must say that I would have almost no regrets if it all terminated tomorrow. I have had a fulfilling life, and it is already long. I have accomplished quite a bit in many different directions. (I am a little embarrassed that I even published a few squibs of theology!) I don't owe anybody or any institution in the world a red cent nor an apology. What more could I ask?

Good luck in 1995.

Yours very truly,

Alfred Traverse

enclosures:xeroxes

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES
PALYNOLOGICAL LABORATORIES
435 DEIKE BUILDING
UNIVERSITY PARK, PA 16802, USA
Phone: (814)863-3419; Fax: 814-863-7823
E-Mail: traverse@ems.psu.edu

21 December, 1994

Dr. Jane Gray
Department of Biology
1210 University of Oregon
Eugene, Oregon 97403

Dear Jane:

Yours of--it says--honest--18 September--just arrived. Of course, it also gives the return address as Boucot. Hmmm. Postmark is 7 Dec., a day that will live in infamy, as I recall. I'm such a straightforward guy that I don't understand such oddball things very well.

As far as helping or not helping you is concerned, you surely can't be talking about reciprocity, as I don't remember your having done me any favors. I have never made any secret of the fact that I haven't cared for certain aspects of your way of behaving. Just to take a recent example, you published a paper not long ago in which you had a go at the Paleophytic/Mesophytic/Cenophytic business, without mentioning even in passing that in 1988 (in Paleopalynology) I had an exposition of my ideas about it, with a classification that in some respects resembles yours. Editors and reviewers don't let me get away with that sort of thing.

Now, as to reviews. I keep a complete file of all such stuff I have ever written, and I think my reviews of proposals of yours were honest appraisals and would stand up in court as such. The most recent review of a proposal of yours was a number of years ago, and I gave you a very positive, almost a rave review. Believe it or not. For about two years I have been declining all requests to review proposals, as I am no longer submitting any and am therefore "out of the loop." I think that folks in your and my generation should not be submitting proposals. I don't need another grant and am content to have them awarded to folks who will be able to parlay them into promotions, etc. Maybe you need one so that OU will let you mail letters out under your own name.....hmmm. You once told me you had done very well with some motel stock, but times are hard all over. I recall once having to loan you money when you arrived in the Big Apple penniless.

Hunt Institute for Botanical Documentation

Regarding the lipid business you raise, apparently (but I am just guessing) this is in response to somebody's review (not mine) of a proposal of yours. I had nothing to do with it and only marginally understand what you are getting at.

If you wish to correspond with Carmen Moy (not Moe), you have her address. I see her very seldom. She collects palynologists, the way ornithologists collect bird sightings. You are now apparently on Carmen's life list. Congratulations.

I use email all the time, and was tempted to try the address you sent, but I was not 100% sure in view of the first paragraph of your letter that the address wasn't a joke, so I decided to go with snail mail instead. If the address is for real it's a bit egocentric, but no worse than a prestige plate on your car. Rather fun.

Betty and I are flying out to LA on the 28th, in order to be present when our football team demolishes your "Ducks" on 2 Jan. 95. Heavens, even Harvard beat them in the Rose Bowl a few (well, 74) years ago.

Have a good 1995. It was nice to hear from you.

Yours very truly,

Alfred Traverse

Hunt Institute for Botanical Documentation

Department of Biology
1210 University of Oregon
Eugene, Oregon 97403- 1210
December 29, 1994

Dear Al:

I have no idea what happened with regard to the letter that was so delayed. But I am genuinely glad to hear from you, and would like to answer some of the issues raised in your letter.

It will be good to clear the air, as both of us are getting "long in the tooth" as they say and moreover, I am seriously ill (I'm an insulin dependent diabetic). So let me respond point by point. I hope you will do the same.

1) Re grants. A former director of the NSF earth science panel (now deceased) and a former panel member (also now deceased) let it be known to me that you (A. Traverse) were my (jg) "worst enemy." While this might be construed as a breach of confidence, they thought that I "should know." That was all that I was told-- no specifics. Now if you say that this is false, you can send me a copy of what you wrote (as you claim to keep all this data) and that will certainly set the record straight. If I was provided with misinformation, I want to know about it.

This information was provided me many years ago now (20+ years ago) when I first was involved in the pre-Devonian spore work and having a great deal of antagonism from my colleagues about the work ,and its significance. At that time every attempt was made to stop grant support and to see that the papers were not published (if you would like to see any of the reviews from this time period, I will be glad to send you copies). Fortunately, I had a number of people who believed in me and in the significance of the spore work. In any case this intense opposition and antagonism has left a few permanent scars, and made me leary of trusting almost anyone. I might add that some people are still at it, as evidenced by recent reviews, exacerbated by the attention my work gets from ecologists and evolutionists.

As for the issue raised above (item 1), I was obviously hamstrung in addressing it with you. I do not believe in anonymity in reviews of any kind-- because it leads to innuendo, various kinds of viciousness, and hard feelings. People should be prepared to say face-to-face what they are thinking. If not, they are be hiding behind the cloak of anonymity. In any case, presumably one could sue and get such information under the freedom of information act. Why bother. If these two individuals "lied" to me, or misconstrued anything that you wrote, it is indeed time to clear the air.

2) Sorry that you feel I did not credit you for the ideas published in my recent P-3 paper. I don't see that your concept in similar to mine in any way and can only conclude that you failed to read the paper closely. This is the way I see the differences. Again, if I have misinterpreted anything you wrote, please don't hesitate to set the record straight.

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a. The basic concept of Paleophytic, Mesophytic and Cenophytic, of course originated with neither of us, as I showed in my paper. Neither of us can be credited with anything to do with that concept.

b. You propose two marine based "eras," Archeophytic and Proterophytic which run from origin of earth to the first trilete spores (apparently some time in the Lower Silurian as nearly as I can read your Fig. 6.1). These include the standard events revealed by the Precambrian fossil record-- evidence for procaryotes, eucaryotes, etc. etc.

I propose one land based "era" the Epeirophytic that runs from the Late? Precambrian to pre-Llanvirn, mid-Ordovician. I indicate this to be speculative and suggest possible land organisms for this unit: fungi, bacteria, cyanobacteria. I discuss none of the standard Precambrian evidences at all since I am basically not concerned with the Precambrian.

Within the Epeirophytic, I propose one Epoch, Phycomycophytic, which I indicate to be continent based and a counterpart for Krassilov's Thallassophytic.

The boundaries and the concepts of your units and my units are entirely different. How have I not credited you with anything here?

c. Your Paleophytic extends from the Lower Silurian to some time in the Permian. You propose no subdivisions of this unit. You use trilete spores as the basis for the lower limit.

I propose a Paleophytic that begins with the post mid-Ordovician and whose upper boundary is fixed in the traditional sense in the Permian just as yours is. I use spore tetrads as the basis for the lower limit. The upper boundary limit is not your idea and not mine; it was proposed by others well before us as I demonstrate in the paper.

I propose three subdivisions of my revised Paleophytic that I refer to as epochs: Eoembryophytic, Eotracheophytic and Eutracheophytic. I do not see that you propose any subdivisions here.

I was not interested in the Mesophytic or Cenophytic and said so.

If I've seriously overlooked something here-- please enlighten me. I did not and do not see that there is any necessity to "credit" you with anything here.

3) As long as we are addressing the issue of "credit" you certainly were niggardly in citing any of my papers in your book and elsewhere. Even the one paper you cite you make light of-- suggesting that it is all wrong (for some rather strange reasons I might add). In addition to casting aspersion on the paper in a prominent way (can't say that you did this with anyone else) you cite nothing else with regard to the Early Paleozoic work-- thereby implying that nothing else had been published by me. Even when noting the possibility that the tetrads might be liverworts (I believe that I am the first to make this suggestion) there is not one word about my work.

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4) Yes the email address that I provided you is correct. As I indicated in my letter it is from my namesake Lady Jane Grey (I was named after her) and for years some people have used this as my nickname. How is this egocentric? I have a good friend and we refer to one another as Lady J and Princess Di (so I guess that we are both guilty of egocentricism!). The email address jgray was already taken on the UO campus. I do not now and have never believed in customized licence plates.

5) With regard to Carmen M we have never met-- we have exchanged one letter each. If she is an ardent collector, she obviously is not interested in my species!

6) Hope you had a good time at the game. I do recall that you are a football fan. Fortunately, I have no interest in sports whatsoever, and would agree that I would like to see the DUCKs beaten all to hell!

All the best to you and Betty,

Jane

Jane Gray

Hunt Institute for Botanical Documentation

Department of Biology
1210 University of Oregon
Eugene, Oregon 97403-1210
September 18, 1994

Dr. Alfred Traverse
Palynological Laboratories
435 Deike Bldg
Pennsylvania State University
University Park PA 16802

Dear Al:

I have been thinking about you lately in connection with the many years that we go back and with regard to all the tremendous support that you have provided my research over the years-- also going back now many years to the tenure of John Lance at the NSF. I don't believe that I have ever thanked you-- without friends like you I would never have been able to accomplish the little bit that I have!

I also wanted to encourage you to send me copies of your papers-- I continue to teach courses in pollen analysis and nonmarine paleoecology. I will, of course reciprocate with anything that you might like to have. I have an e-mail address so you can respond in that way if you would like [ladyjane@oregon.uoregon.edu]. The "ladyjane" is in reference to my namesake, Lady Jane Grey erstwhile Queen of England for about 8 days, and not to ladyjane in Lady Chatterley's Lover (but then I don't suppose that you read books like that!)

By the way, if you were one of the reviewers of my recent American Chemical Society proposal and wrote the following: "I will not address the 'practical' side of the proposed project except to comment that since it is my understanding that petroleum hydrocarbons are derived from lipids found in ancient marine phytoplankton (italics mine) looking for terrestrially derived POC as a petroleum source would require some theoretical justification," I would like to call the following to your attention so you can update your information in this regards.

1) Lacustrine Basin Exploration ed by B.J. Katz, AAPG Mem. 50, 340 pp., 1990

2) Lacustrine petroleum source rocks, ed. by Fleet, Kelts, Talbot, Geo. Society of London, Special Publication 40, 391 pp., 1988

3) Coal and Coal-bearing strata as oil-prone source rocks, Scott and Fleet, eds., GSA Special Publication No. 77, 1994

There is now a great, great deal of interest in lacustrine basins that include land derived materials as hydrocarbon (petroleum) sources.

Finally, give my regards to Carman Moe-- she wrote me recently after attending my talk at the GSA meeting, saying how much she enjoyed the presentation.

All the best,

Jane

Jane Graf

Hunt Institute for Botanical Documentation

Denver Research Center

Marathon Oil Company

PO Box 269
Littleton Colorado 80120
Telephone 303/794 2601

April 26, 1982

Dr. Alfred Traverse
The Pennsylvania State University
Deike Bldg.
University Park, PA 16802

Dear Al:

Thanks for the note re Jane Gray's manuscript. I'm pissed. I feel used. I don't know what to do about it.

On the one hand I believe in the right of a scientist to make an ass of him- or herself and on the other I want controls imposed (by GSA, for example) to protect reviewers and incorporate their critiques and/or opinions in a mss.

Why don't I simply enclose a copy of a letter I wrote Jane a year ago?

I'm tempted to write GSA. Why have a peer review system if it isn't being used? In other words, I spend my time and energy to improve her manuscript and she simply ignores everything I've done. The GSA (editors) doesn't know what I said, what my suggestions, questions, doubts were. She then cites me as a reviewer, which somehow gives the impression of approval, of collaboration.

'Nough about that topic.

I'm working on getting Penn State (your lab, specifically) into the Marathon grant hopper. The Marathon Foundation offers all kinds of grants, some with no strings attached.

Re my change of residence. True. At age 60 we decided to tackle luxury living. Had written off Hawaii as a retirment haven, then decided against building on (family room, etc.) in the old neighborhood, then looked at condominium living --the sensible modus vivandi for two 60-year olds.

I'm simply not ready for condominium-apartment-type living. So we might as well go first-class, which is what we did. Got into debt up to my ears, but the money now goes to the banks, rather than the IRS. New challenges, new ideas about comfortable living, new landscaping, etc., etc. We're having a ball. The house, environment, view of the front range and the whole Platte Valley, peace and quiet all combine into a dream-come-true. Why not?

Come and see us, anytime.

Best regards,

PS. May take early retirement to paint, read, write, and think, if the U.S. Steel deal doesn't work out to my liking.

Hunt Institute for Botanical Documentation

MARATHON OIL COMPANY DENVER RESEARCH CENTER

LITTLETON, COLORADO 80120

August 17, 1981

Dr. Jane Gray
Biology Department
University of Oregon
Eugene, OR 97403

Dear Jane:

I've read your manuscript, as you can see. I couldn't help but edit, wherever and whenever clarity was needed.

I'll leave it up to you. I'd just as soon not be mentioned as a critical reader. If you use my name, you'd be obligated to accept some of my criticism and suggestions, and you may not wish to do so.

As I mentioned to you over the phone, I am very much interested in the advent of land plants. In fact, I have some stuff that looks suspiciously like wood, from Middle Ordovician rocks. I'm convinced that land plants didn't burst on the scene; that it took a long time to reach the diversity and complexity displayed by Devonian land plants. I thus sympathize with you and am rooting for you.

It seems to me that the whole problem comes down to this: Are tetrads and trilete spores, and cuticle, restricted to land plants? If so, then whenever tetrads and trilete spores and/or cuticle are found you must have had land plants to produce these organs and tissues. If the rock in question is dated as Caradocian in age and you then have tetrads and trilete spores and cuticle in it -- and it is not contamination -- then land plants must have existed in Caradocian time. It's that simple.

I don't know what flak you have been getting or who your critics are, but I doubt that it is anything personal or vindictive. I suspect that you are being criticized for not providing enough information and/or documentation. The first question that comes to mind when the first occurrence of something or other is shoved back in time is the veracity of the age of the rock it was found in. What is the evidence for a Caradocian age, for example? By what means and by whom was the rock dated? Such evidence is essential. If that evidence is lacking, the rest is suspect.

I buy a general discussion article, such as the one previously published in GEOLOGY (1978, vol. 9, no. 8), but the evidence, the fossil material, etc., has to have been shown and described and documented previously. Making claims without supportive evidence is neither convincing nor scientific.

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For example, you may be right in arguing that the mode of spore dispersal changed from tetrad to single (dispersed) spores with time, but where is the proof? What are the tetrad to single spore ratios from older to younger rocks? That you think it may have happened or that you would like it to have happened is irrelevant. Show me the data, the evidence, and then I'll believe you. Or, maybe I won't.

Documentation and description of the samples ought to be included. Locations and well data are needed.

The vertical and geographical positions of the samples ought to be documented. A simple biostratigraphic column is essential. Not every geologist is working on early Paleozoic rocks, and besides, the geologic column is continuously being revised. You have to check out the latest modifications and accepted usage. For example, the 1975 Elsevier chart (van Eysinga) shows the Downtonian as earliest Devonian, not latest Silurian as shown on the 1972 chart, and the Pridolian as a "local" term and correlative with the upper part of the Ludlovian.

	Late	Ludlovian
Silurian		Wenlockian
	Early	Llandoverian = Albion
		Valentian
		Ashgillian
	Late	
		Caradocian
		Llandelian
Ordovician		Llanvirnian
	Early	
		Arenigian/Skiddavian
		Tremadocian
		Shidertinian = Dresbachian
Cambrian	Late	
		Tuorian

If American nomenclature is used, correlate with the European terms to help the reader (e.g., Dresbachian above).

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Now to the actual material, the evidence. How many samples? What about lithology, environment of deposition? How many tetrads, how many single spores? How many pieces of cuticle? Their measurements. Don't you want to describe and name your material, or if identical with previously described stuff, refer to it by name? How big are the spores? What is the size range, based on x-number of specimens? Is there exine ornamentation? What about some SEM pix?

What about the cuticle? Did you find only one piece? How many? How big are the cell imprints? Don't you want to describe the stuff, name it? Is there enough cuticular material to have some chemistry done?

Another philosophical-scientific issue is the time span that you are trying to delineate for land-plant emergence. The fact that you found no tetrads in some Dresbachian rocks doesn't mean that all Dresbachian lithologies are devoid of tetrads, that there were no land plants in Dresbachian time. How much of the Dresbachian did you sample? The absence of a given fossil doesn't prove that it never existed. If absence doesn't prove anything, then a time span during which land plants might have made their first appearance can't be established. You simply don't have a marker at the early end of the span.

I have no quarrel with your argument that spores can be the only evidence, the only remnants of certain plant species. The chance of some spores to survive -- when millions are produced annually by a single plant -- is much greater than the chance of vegetative parts. Microscopic size and resistant exines give spores additional advantages. You make a pretty good case.

One more criticism: Don't impose on the reader. Don't tease him. Don't expect me, the reader, to go somewhere else for information that's pertinent to your paper. An alleged manuscript, unless actually in press and being processed for publication by some specific journal, isn't available to me. It has no label and thus can't be identified, and thus doesn't exist. Don't tease me with a tidbit and then tell me to go somewhere else to find out about it.

Jane, please take the above comments as pure and simple criticism of a manuscript, an article addressed to me, the reader. Whether Jane Gray wrote it, or Joe Blow, is irrelevant. You asked me to read the manuscript, critically, and that's what I did. I spent considerable time on it. Mentioning my name in the paper doesn't mean anything to me; it isn't going to enhance my reputation. Don't feel obligated. If you disagree with any or all of my comments and suggestions and/or that you feel they do not improve your paper, fine with me. I respect your right to ignore my opinions. In fact, I sincerely believe in a scientist's right to make an ass of himself. I do not entirely endorse the procedure of peer reviews, because it can be a form of censorship. I believe in saying what you think in print and letting the reader be the judge.

Best Regards

G. K. (Joe) Guennel

GKG/bh
cc: R. J. Ebens

Hunt Institute for Botanical Documentation

Dr. Travers,

I recently went to a lecture at C.M.S.U. given by a prof. of auto safety from Penn State. It brought back memories of the many lectures I enjoyed that were given by my favorite Penn State prof. May you have a very Merry Christmas and a prosperous New Year.

MERRY CHRISTMAS

Susan Gray
107 Ming
Warrensburg, MO
64093

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES
PALYNOLOGICAL LABORATORIES
435 DEIKE BUILDING
UNIVERSITY PARK, PA 16802, USA
Phone: (814)863-3419; Fax: 814-863-7823
E-Mail: traverse@ems.psu.edu

12 September, 1996

Director
Gray Herbarium
Biological Laboratories
Harvard University
Cambridge, MA 02138

Dear Colleague:

A half-century ago when I was a doctoral student at Harvard I collected at the Gray floral snippets for my working collection of modern pollen. To give you an idea of how long ago this was, the herbarium was then located on Garden St.

One species I collected was: Hippocratea volubilis (Hippocrateaceae). The material was collected in Panama in 1937 by a collector named Allen, whose number for this collection was #319.

Material I recently collected from the Smithsonian Herbarium, also labelled Hippocratea volubilis, produces pollen that agrees with various atlases of modern pollen, whereas the material I processed 50 yrs. ago at Harvard did not.

Could you have somebody reexamine that sheet in your herbarium to see what it really is? Perhaps I made a mistake of some sort in labelling my packet, though if I did it will be a first.

Thanks very much.

Yours very truly,

Alfred Traverse

Hunt Institute for Botanical Documentation

26 October, 1966

Gray Manufacturing Co.
16 A[redacted] St.
Hartford, Connecticut

Dear Sirs:

I recently inherited an Audograph and the listening unitythat goes with it (serial nos. 136156 and 138998, respecttively). Both seem to be in good running order, despite their advanced apparent age.

I am in need of dictating equipment in my position here, and it has occurred to me that I could perhaps acquire some new accessories for these units (microphones, foot controls, etc.) and use them instead of buying new machines.

The question is--do you still manufacture accessories for these units? Where is your nearest representative located, whom I could see about this?

Yours very truly,

Alfred Traverse

Alfred Traverse
Associate Professor

Hunt Institute for Botanical Documentation

9 December, 1983

Mr. Guy Graybill
RD 3, Box 234
Middleburg, PA 17842

Dear Mr. Graybill:

Your letter of 4 December was referred to me for answer. The specimen illustrated in your excellent photo (returned herewith) is of a nice specimen of Arthrophycus sp. In Pennsylvania such specimens are almost always from the Tuscarora Formation (Lowest Silurian), and this specimen looks like a typical Tuscarora example. These are very common in Tuscarora outcrops, for example along Hgwy. 322 in the Seven Mountains area. Originally they were thought to be algae, hence the name. Now they are recognized as trace-fossils, probably made as a result of feeding of large worms. (Trilobite trails have also been suggested.)

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: D. P. Gold
encl: 2x2 transparency

Hunt Institute for Botanical Documentation

Amoco Production Company
4502 East 41st Street
Post Office Box 591
Tulsa, Oklahoma 74102
Research Center

August 3, 1983

83215ART0069

Dear Colleague:

Having successfully reached the age of 55, I have elected to take early retirement from Amoco, effective October 1, 1983. After that, my address will be:

4149 South Sandusky
Tulsa, OK 74135
(918) 627-1516

In my case, "retirement" simply means a change of activities. I intend to work as a private consultant. With more than 30 years of experience in palynology and organic petrology (visual analysis of kerogen, vitrinite reflectance, palynomorph translucency, and microspectrofluorescence), I want to apply that knowledge to the practical problems of petroleum exploration.

If you are aware of the need for a consultant in any of these areas, I would appreciate it if you would urge the interested party to get in contact with me.

Sincerely yours,

John F. Grayson

J. F. Grayson

JFG:pt

Hunt Institute for Botanical Documentation

December 27, 1967

Dr. John F. Grayson
Pan American Petroleum Corporation
Research Center
P.O. Box 591
Tulsa, Oklahoma 74103

Dear John:

The Executive Council of the newly formed AASP directed me to write to you to extend the thanks of the organization to you for the fine way in which you arranged the hospitality for all of us and for the plenary sessions of the new group. The smooth way in which everything ran should be a considerable source of satisfaction to you. Speaking of smooth things, I suppose nearly everybody would agree that the affair Friday night at the Harvard Club was a (the?) high point of the session. Thanks again for the Association, and for me personally.

Yours very truly,

Alfred Traverse
Secretary-Treasurer
American Association of Stratigraphic
Palynologists

AT:kwc

cc: Dr. Paul Nygreen

Hunt Institute for Botanical Documentation

December 15, 1967

Dr. John F. Grayson
Research Center
Pan American Petroleum Corporation
P.O. Box 591
Tulsa, Oklahoma 74103

Dear John:

Just got home a few hours ago--I spent a day with Jim Schopf enroute here--and found your very nice letter of 6 December waiting for me. You must have found it odd that I didn't acknowledge it while in Tulsa!

I don't know whether it is ethical to discuss such matters, but I would like to say that I greatly appreciated your nominating me for the various things for which you did--the courtesy was not lost on me. I wanted to reciprocate, but wasn't really sure what your pleasure was in the matter. I know that you did not want it to seem that you were spearheading the whole thing. It was really difficult for me to understand your failure to be elected to the council. You certainly will be needed within the inner circle of the outfit if it is going to succeed.

You had asked me to phone you when I got to Tulsa Thursday, but after driving from Indianapolis that day I felt only like taking a bath and retiring. I hope you understadd. It was a disappointment to miss seeing your wife again.

Dunay, Kremer and Nichols reciprocate your good wishes.

Happy holidays.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

Hunt Institute for Botanical Documentation

FORM 950 4-64

PAN AMERICAN PETROLEUM CORPORATION

RESEARCH CENTER

4502 EAST 41ST STREET P. O. BOX 591

TULSA, OKLAHOMA—74102

December 6, 1967

Dr. Alfred Traverse
Department of Geology
Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

It was a real pleasure for me to spend a day with you last week and I want to thank you for your wonderful hospitality. I was quite impressed with the setup of palynology at Penn State and I know the credit belongs entirely to you. If things keep progressing the way you have them started, there is no doubt in my mind that Penn State will become the outstanding center of palynology here in North America. I certainly hope so and if there is anything I can do to help it along, please don't hesitate to ask.

I enjoyed meeting and talking with Doug Nichols again and also the opportunity to meet your two new students, Bob Dunay and Peter Cramer. The evening at your house was an exceedingly pleasant one and I just hope that everyone there enjoyed it as much as I did. Please express my gratitude to Betty for her hospitality during my visit. If she is still fretting about her birthday, you may remind her that it is really true that "life begins at 40".

I am looking forward to seeing you in Tulsa this coming weekend. Thanks again for all your kindness during my visit.

Sincerely yours,

John

John F. Grayson
Research Group Supervisor

JFG:mjp

Hunt Institute for Botanical Documentation

FORM 950 4-64

PAN AMERICAN PETROLEUM CORPORATION

RESEARCH CENTER
4502 EAST 41ST STREET P. O. BOX 591
TULSA, OKLAHOMA—74102

November 14, 1967

Dr. Alfred Traverse
Department of Geology
Pennsylvania State University
University Park, Pennsylvania 16802

Dear Alfred:

It was a pleasure talking to you yesterday and I am looking forward to our visit at Penn State toward the end of the month. My plans are to arrive at State College at 3:13 p.m. Tuesday, November 28 on Alleghany flight 604. Thank you for making a reservation for me for that night. If anything should happen that would force a change in plans, a delayed arrival of the flight, or anything like that, I will be in contact with you by telephone.

As yet, I have not been able to put together a talk, but I will give it the old college try before I come. Best wishes for an enjoyable meeting at the GSA in New Orleans.

Sincerely yours,

John

John F. Grayson
Research Group Supervisor

JFG:mjp

FOX RIVER BOND
25% COTTON

Hunt Institute for Botanical Documentation

bcc

October 30, 1967

To: Drs. Barghoorn, Cross, Grayson, Guennel, Hoffmeister, Kosanke, Loeblich, Schopf, Stover, Wilson

Dr. John F. Grayson
Pan American Petroleum Corporation
Research Center
Tulsa, Oklahoma 74103

Dear John:

Beginning with Volume 28, we are planning to discontinue the Advisory Committee of the Catalog of Fossil Spores and Pollen. The Advisory Committee has served the Catalog faithfully and well, and those of us on the regular staff of the Catalog wish to express our thanks to all of you for a job well done. "CFSP" is now established on a fairly firm, permanent footing at Penn State, and we feel that it is no longer necessary to bother so many people with the various problems connected with publication of the Catalog. We hope that we can still continue to call on you less formally when advice is necessary.

Hoping to see each of you in person before long, I am

Yours very truly,

Alfred Traverse
Editor
Catalog of Fossil Spores and Pollen

AT:kwc

Hunt Institute for Botanical Documentation

October 16, 1967

Dr. John F. Grayson
Research Group Supervisor
Pan American Petroleum Corporation
4502 East 41st Street
Tulsa, Oklahoma 74102

Dear John:

By all means--a visit from you would be delightful! I would greatly enjoy it, and I am sure that we have much to talk about that would be of mutual interest and value.

Doug Nichols is the only student I have who is really well into his program in palynology. I do have twoother students who are getting their feet wet, but they could hardly expect to show you much except the latest results of their efforts to pass the language requirements, etc. Considering that I have still been here only a few weeks more than a year, I feel not too bad about the progress we've made toward getting a program in palynology in high gear.

When you can, let us know the date more exactly so that there won't be conflicting things. Since I won't get back from G.S.A. until 24 November, I suppose that the early December alternative would be slightly better, but it doesn't really matter much.

Best regards as ever.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

AT:kwc

Hunt Institute for Botanical Documentation

FORM 950 4-64

PAN AMERICAN PETROLEUM CORPORATION

RESEARCH CENTER

4502 EAST 41ST STREET P. O. BOX 591

TULSA, OKLAHOMA—74102

October 12, 1967

Dr. Alfred Traverse
Department of Geology
The Pennsylvania State University
University Park, Pennsylvania 16802

Dear Alfred:

Sometime during the last week of November or the first week of December, I will probably be up in the northern part of the country on a recruiting trip. If it is convenient, I would like to plan on spending a day with you at Penn State. Not only would I like to renew my acquaintance with Doug Nichols and to meet any other students that you are inspiring with palynology, but I would also like to spend some time with you discussing problems of mutual interest.

If you feel such a visit would be feasible, please let me hear from you. My very best wishes.

Sincerely yours,

John

John F. Grayson
Research Group Supervisor

JFG:mjp

Hunt Institute for Botanical Documentation

FORM 950 4-64

PAN AMERICAN PETROLEUM CORPORATION

RESEARCH CENTER
4502 EAST 41ST STREET P. O. BOX 591
TULSA, OKLAHOMA—74102

July 31, 1967

Dr. Alfred Traverse
Catalog of Fossil Spores and Pollen
Deike Building
The Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

Thank you for your letter of July 26. It was my pleasure having you and Doug Nichols visit here in Tulsa. I still feel sorry that your stop here had to be on the one day that was unavailable for visiting. If you could have spent a day with us, I know both you and Doug would have enjoyed not only meeting the palynologists here, but also seeing the spectrum of palynologic activity in which we are engaged. I do hope you will have another opportunity to stop by this way. Nevertheless, I thoroughly enjoyed the evening we had together.

I am glad to hear that your collecting in Texas was successful and that you were able to take back some of the "Auld Sod" with you. If you run out of samples to macerate, please let me know and I will be happy to supply you with all the rock you desire. Best wishes.

Sincerely yours,

John F. Grayson
Research Group Supervisor

JFG:mjp

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

July 26, 1967

Dr. John F. Grayson
Pan American Petroleum Corporation
Research Center
P.O. Box 591
Tulsa, Oklahoma 74103

Dear John:

It was good to see you in Tulsa and to have the opportunity of viewing the Pan Am. palynological operations first hand. It seems to me that you and your co-workers have about as nice a set-up as I can possibly imagine. I do hope there will be opportunities in the future for us to compare notes on our various projects quite often.

It was very kind of you to take Doug Nichols and me out to dinner. We both appreciated the experience of visiting with you very much.

We had fairly good collecting success in Texas and should have no trouble finding things to macerate in months to come.

Please give my regards to the other members of your staff--especially to Art LeBlanc, whom I had not seen or heard from in years.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

AT:kaw

Hunt Institute for Botanical Documentation

June 15, 1967

Dr. John F. Grayson
Pan American Petroleum Corporation
Research Center
P.O. Box 591
Tulsa, Oklahoma 74102

Dear John:

Thank you so much for your kind words about the article, contained in your letter of 5 June.

A graduate student of mine, Doug Nichols, and I will be passing through Tulsa enroute to Texas for a spot of field work in a couple of weeks. I would guess that we would arrive in Tulsa on the evening or late afternoon of Wednesday, the 28th of June. Nichols has never seen a palynological setup outside of a university, and I wondered if we might be permitted to visit you for a couple of hours before we press on to Texas?

Hoping to see you in a couple of weeks, I am

Yours very truly,

AT

Alfred Traverse
Associate Professor of Geology

AT:kaw

Hunt Institute for Botanical Documentation

FORM 950 4-64

PAN AMERICAN PETROLEUM CORPORATION

RESEARCH CENTER
4502 EAST 41ST STREET P. O. BOX 591
TULSA, OKLAHOMA—74102

June 5, 1967

and well arrive
25 June p.m.

Dr. Alfred Traverse
Department of Geology
Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

Thank you very much for sending me a reprint of your paper, "Palynology of the Surface Sediments of Great Bahama Bank, as Related to Water Movement and Sedimentation". Congratulations on an exceptionally fine study. The conclusion that pollen distribution is controlled primarily by the sedimentation pattern rather than the source is certainly quite an important departure from much of the theory now in the literature. The fact that pine pollen appears to stay suspended in the water for long periods of time is also an interesting concept.

The fact that this is such a well documented study plus the fact that it is a first on recent carbonate sediments practically insures that it is bound to be a classic in years to come. I am very happy to have a copy of this available for immediate reference. My best wishes.

Sincerely yours,

John

John F. Grayson
Research Group Supervisor

JFG/nm

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
ORGANIC SEDIMENTS LABORATORY
UNIVERSITY PARK, PENNSYLVANIA 16802

College of Mineral Industries
Department of Geology and Geophysics

Area Code 814
865-6545
865-6543

October 21, 1966

Dr. John F. Grayson
Member, Advisory Committee, CFSP
Pan American Petroleum Corporation
Research Center
Tulsa, Oklahoma

Dear John:

Missed you at Utrecht, but it was good of you to send Sullivan--a generous gesture it seemed to me (don't disillusion me).

I have proposed elimination of Option C (punch-cards) of the Catalog effective with Volume 26 (almost ready) as a step toward expedition of the operations here. The subscribers have responded slowly but mostly favorably to a questionnaire. Few (if any) people use the punch feature anyway. Any comments?

Yours very truly,

Alfred Traverse, Editor
Catalog of Fossil Spores and Pollen

AT:le

Hunt Institute for Botanical Documentation

Socony Mobil Oil Company, Inc.
P. O. BOX 900, DALLAS 21, TEXAS

December 11, 1961

RESEARCH DEPARTMENT
FIELD RESEARCH LABORATORY

Dear Al,

Thank you for your recent letter and for the recent reprint on pollen in drilling muds. I'm happy to hear that you will be writing a chapter on modern pollen processing techniques for Jane.

I would be delighted to help you in any way that I could. Unfortunately, I suspect you are the victim of unfounded rumors (which most rumors are) that I have some "ultra refined methods" in that area. Enclosed find a processing schedule that I have used for the past seven years. You will quickly recognize that there is nothing original in it. (At least I don't think so. It's been so long since I outlined it that I can no longer be sure when each specific step came from.) This simple process has served so well that I have never felt the need for doing any improving upon it.

If there is any way in which I can be of help, please don't hesitate to ask. Meanwhile, my very best wishes to you and your wife for a very happy holiday season.

Sincerely yours,

John Grayson

P.S. We missed you at Penn State.

Hunt Institute for Botanical Documentation

File: Correspondence, Grayson, J. F. Dec. 1961

MODERN POLLEN SCHEDULE

(Use distilled water only)

1. Place sample in 50 ml. centrifuge tube and half-fill tube with 10% KOH.
2. Add small amount of Pinus sylvestris pollen. (Used only with non-saccate samples)
3. Place tube in 110°C oil bath for 10 minutes.
4. Wash sample through fine mesh sieve with jet stream of water. Centrifuge and decant.
5. Wash sample into 15 ml. centrifuge tube, centrifuge and decant.
6. Wash sample with acetic acid, centrifuge and decant. Repeat the acetic acid wash.
7. Acetolyze* sample for 3 minutes at 100°C, centrifuge and decant.
8. Wash sample with acetic acid, centrifuge and decant.
9. Decant off sample leaving mineral matter.
10. Divide sample into two parts**.
11. One part is washed twice with water and then stained with one drop of saturated aqueous Safranin O for 5 minutes.
12. This part of sample is then washed twice with water.
13. It is then washed twice with Cellosolve, centrifuged, decanted, diaphane added and slides made.
14. The other part of the sample is chlorinated*** for approximately one minute (time varies), centrifuged and decanted.
15. This part is then washed twice with water and twice with Cellosolve. After decanting the Cellosolve, diaphane is added and slides are made.
16. Each slide bears two cover slips - one with unstained chlorinated material beneath and one with stained acetolyzed material beneath.

*Acetalysis Solution = 9 parts acetic anhydride and 1 part concentrated H_2SO_4.

**If sample is very small, do not divide but go through steps 11, 12, and 13.

***Chlorination Solution = 100 parts acetic acid, 1 part concentrated H Cl and 1 part Na ClO_3.

Hunt Institute for Botanical Documentation

Hunt Institute for Botanical Documentation

July 26, 1960

Dear Al,

Thank you for the copy of your Report to the S. E. P. M on the IX International Botanical Congress. I am happy to have a copy of this report for my files.

I guess I must have been asleep most of the time in Montreal. I didn't even realize there was a movement toward anarchy afoot. Anyway, I'm happy to hear that the "Treason" was detected and stamped out and the radicals thoroughly routed. Hope to see you in Stillwater.

John Grayson

November 13, 1957
Dallas, Texas

Dear Colleague:

Thank you for your cooperation on the questionnaire. Since no further response has been received during the past several months, I feel the poll has been completed.

Of the 61 names listed on the original letter, all but eleven replied. Some of you sent in names of people working in palynology that were unknown to me at the time the original letter was written. Every name given to me was sent a questionnaire. From this group, 12 more palynologists have answered. Therefore, I feel that this poll represents at least 84 per cent of those actively engaged in palynological work. I suspect that many of those who did not answer are no longer actively interested in palynology and, therefore, the actual percentage represented here is probably higher than 84 per cent.

The enclosed sheet gives the results. A few words of explanation are necessary. In some cases, more than one choice was checked --therefore, the discrepancy between the number of answers and the number of choices. Some answered by means of letters. In all cases, the letters were very explicit so that interpretation, for statistical purposes, was very easy.

Personally, I am very much in favor of a separate palynological organization. Palynology is rapidly becoming a mature field of science and maturity brings responsibility. As palynologists, I feel we have the responsibility to provide a means of association and exchange of information among those who are interested in different facets of the field but have palynology as a common interest. I believe, a palynological organization would encourage research and the publishing of the results of research; promote interest in the study of polospores; facilitate the exchange of ideas among those interested in palynology; stimulate the building of modern polospore reference collections and the exchange of material among palynologists; and aid in the establishment of standard terminology and nomenclature in the field. These things are necessary, if palynology, as a field of science, is to mature.

Thank you again for your cooperation.

Sincerely yours,

John F. Grayson

John F. Grayson

JFG:mb
enc.

Greetings!

Hunt Institute for Botanical Documentation

QUESTIONNAIRE RESULTS

August 1, 1957

	TEACHING AND/OR RESEARCH	GOVERN-MENTAL RESEARCH	INDUS-TRIAL RESEARCH	STUDENT	TOTAL
The initiation of a separate palynological organization:					
1. In favor	21	6	17	4	48
2. Not in favor	4	5	2	0	11
3. No answer	0	0	2	1	3
If such an organization were formed, it should be:					
1. Sect. of indep. Paleobot. Soc.	2	0	2	1	5
2. Sect. of Bot. Soc. Am.	6	4	2	0	12
3. Soc. affiliated with AIBS	1	0	1	0	2
4. Soc. affiliated with AAAS	3	0	2	0	5
5. Affiliated with GSA	2	5	1	2	10
6. Soc. co-associated with botanical and geological organizations.	13	2	8	1	24
7. Independent Paly. Soc.	5	1	7	1	14
Other Suggestions:					
S.E.P.M.	0	0	4	1	5
No Choice Indicated	2	2	2	0	6
Interested in helping to initiate a palynological organization:					
1. Would be interested	18	5	15	3	41
2. Would not be interested	6	4	5	0	15
3. No answer	1	2	1	2	6

Hunt Institute for Botanical Documentation

July 17, 1957

Dr. John F. Grayson
Magnolia Petroleum Company
P. O. Box 900
Dallas 21, Texas

Dear John:

Thanks for your nice letter of July 9th, which I have shown to Dr. Rittenhouse.

Forgot in my weariness the night before you left to ask you whether you would consider being chairman of the palynological committee of the P. S., should such a thing be created. What's your reaction?

Very truly yours,

Alfred Traverse

AT:hmp

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

MAGNOLIA PETROLEUM COMPANY

A SOCONY MOBIL COMPANY

FIELD RESEARCH LABORATORY

HENRY C. CORTES
VICE-PRESIDENT
C. I. ALEXANDER
MANAGER
P. P. REICHERTZ
ASSISTANT MANAGER
SUPERVISOR PRODUCTION RESEARCH

P. O. BOX 900
DALLAS 21, TEXAS

N. P. STEVENS
SUPERVISOR EXPLORATION RESEARCH
E. S. WILHELM
ASSISTANT TO MANAGER
SUPERVISOR GENERAL SERVICES
T. A. POLLARD
ASSISTANT TO MANAGER

July 9, 1957

Dr. Alfred F. Traverse
Shell Development Company
3737 Bellaire Boulevard
Houston, 5, Texas

Dear Al:

I would like to thank you for the wonderful visit I had at the Shell Laboratories. Needless to say, I was very impressed with the facilities, the personnel and the work that you are doing.

Louise and I would also like to thank you and Betty for your excellent hospitality. It was a trip that we will remember for years to come. The shoe arrived in perfect condition and has been reunited with its mate.

Your lignite samples are now being processed. I hope we have as much success as you did.

Thank you again for the enjoyable visit and stimulating discussions. I'll be looking forward to seeing you in California on August 20th.

Sincerely yours,

John

John F. Grayson

JFG:mb

Hunt Institute for Botanical Documentation

MAGNOLIA PETROLEUM COMPANY

A SOCONY MOBIL COMPANY

FIELD RESEARCH LABORATORY

HENRY C. CORTES
VICE-PRESIDENT
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ASSISTANT MANAGER
SUPERVISOR PRODUCTION RESEARCH

P. O. BOX 900
DALLAS 21, TEXAS

N. P. STEVENS
SUPERVISOR EXPLORATION RESEARCH
E. S. WILHELM
ASSISTANT TO MANAGER
SUPERVISOR GENERAL SERVICES
T. A. POLLARD
ASSISTANT TO MANAGER

June 10, 1957

Dear Al,

Thank you very much for all the arrangements. Both Louise and I are looking forward to this Houston visit. With the fine map you drew, I should have no difficulty in locating the motel or your residence.

We will plan on leaving Dallas early on the morning of the 26th. Since this is a Wednesday, traffic should be light and we should make good time. I plan to be in Houston between 3 and 4 p. m. provided, of course, no difficulties are encountered. Any arrangements you wish to make are fine with us. We will be happy with any baby sitting arrangements you make. There is nothing Louise likes better than to get away for an evening.

As soon as we get settled at the motel, I'll call you. Thank you again for all the plans.

Sincerely,

John

Grayson

Hunt Institute for Botanical Documentation

MAGNOLIA PETROLEUM COMPANY

A SOCONY MOBIL COMPANY

FIELD RESEARCH LABORATORY

HENRY C. CORTES
VICE-PRESIDENT
C. I. ALEXANDER
MANAGER
P. P. REICHERTZ
ASSISTANT MANAGER
SUPERVISOR PRODUCTION RESEARCH

P. O. BOX 900
DALLAS 21, TEXAS

N. P. STEVENS
SUPERVISOR EXPLORATION RESEARCH
E. S. WILHELM
ASSISTANT TO MANAGER
SUPERVISOR GENERAL SERVICES
T. A. POLLARD
ASSISTANT TO MANAGER

June 3, 1957

Dr. Alfred F. Traverse
Shell Development Company
3737 Bellaire Boulevard
Houston 5, Texas

Dear Al:

By the time you receive this letter, you will have received a carbon copy of my acceptance to Dr. Noyes Smith. Therefore, everything is very official. Thank you for all the arrangements.

I would appreciate it if you would make motel reservations for us for the nights of the 26th and 27th. I will plan on driving to Houston on the 26th. I would also appreciate it if you would send a sketch map of how to get to the motel and laboratory via U.S. 75 from the north.

Sincerely yours,

John F. Grayson

John F. Grayson

JFG:mb

Hunt Institute for Botanical Documentation

Dear Al,

Official approval has been received to use a rent car for the Palealtamiral Field Trip, so you can count on me. Thanks for the cc of Schopf's letter. The "administrators" are kicking around your invitation for a visit. Will let you know officially as soon as possible. Thanks.

John

Grayson

Hunt Institute for Botanical Documentation

May 20, 1957

Dr. John F. Grayson
Magnolia Petroleum Company
Field Research Laboratory
P. O. Box 900
Dallas 21, Texas

Dear Dr. Grayson:

We understand from Dr. Alfred Traverse that you are interested in seeing his laboratory and discussing problems of mutual interest with him.

We should be pleased to have you visit us here in Houston. Dr. Traverse has indicated that June 27 would be most convenient.

Very truly yours,

NOYES D. SMITH, JR.

Noyes D. Smith, Jr.
Vice President

hmp

bc. Dr. Traverse
Dr. Rittenhouse
D file

Hunt Institute for Botanical Documentation

MAGNOLIA PETROLEUM COMPANY

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N. P. STEVENS
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E. S. WILHELM
ASSISTANT TO MANAGER
SUPERVISOR GENERAL SERVICES
T. A. POLLARD
ASSISTANT TO MANAGER

May 13, 1957

Dear Al,

Just a short note to tell you that I'm still interested in visiting your laboratory if you can arrange an invitation. The best date I can think of at the moment would be June 20th or June 27th. See what you can do.

John

Dr. John F. Grayson

Dr. Magruder
Dallas

Hunt Institute for Botanical Documentation

Dallas, Texas
May 8, 1957

Dear Colleague:

In conversations with several palynologists during the past few months, the idea of an independent palynological organization has arisen repeatedly. The purpose of this letter is to obtain the opinions of the people who are engaged in palynological work. In view of the objective of contacting a large number of people individually, I hope that you will excuse my use of a form letter.

Below is a list of the people to whom this letter is being sent. They are the people known to me to be engaged in palynological work. If you know of others who are studying, teaching or practicing some aspect of the palynological field, please send me their names and addresses so that I may obtain their opinions also.

Ames, T. H.
Arnold, C. A.
Barghoorn, E. S.
Benninghoff, W.
Brown, C. A.
Browning, J.
Buell, M. F.
Cain, S. A.
Clisby, K. H.
Courtemanche, A.
Cross, A. T.
Dahl, A. O.
Deevey, E. S.
Denton, G.
Durham, O.
Evitt, W.
Felix, C. J.
Fisher, J. C.
Frey, D. G.
Funkhouser, J.
Gerhard, J.
Gray, J.
Gunnel, G. K.
Hansen, H. P.
Heusser, C. J.
Hoffmeister, W. S.
Jeffords, R. M.
Kersting, C. C.
Kosanke, R. M.
Kremp, G. O. W.
Kurtz, E. B.
LeBlanc, A. E.
Leffingwell, H.
Leopold, E. B.
Livingstone, D.
McLaughlin, R. E.
Morgan, J. L.
Niering, W. A.
Norem, L. W.
Pierce, R. L.
Radforth, N. W.
Reinertsen, D.
Rouse, G. E.
Rowley, J. R.
Schemel, M. P.
Schopf, J. M.
Sears, P. B.
Smith, L. C.
Stanley, E.
Staplin, F.
Stover, L.
Traverse, A. F.
Trotter, C. L.
Tschudy, R. H.
Tynan, E.
Upshaw, C.
Waloweek, W.
Wilson, L. R.
Winslow, M.
Wodehouse, R. P.
Wolfe, M. K.

I will sincerely appreciate your cooperation on this questionnaire. I hope the results will be available for discussion at the next palynological conference.

Sincerely yours,

John F. Grayson

John F. Grayson

JFG:mb

enc.

Hunt Institute for Botanical Documentation

NATIONAL SCIENCE FOUNDATION
WASHINGTON 25, D. C.

February 19, 1957

Dr. Alfred Traverse
Exploration and Production
Research Division
Shell Development Company
3737 Bellaire Boulevard
Houston 25, Texas

Dear Doctor Traverse:

Thank you very much for your letter of February 7 containing your comments on the proposa; for support of Dr. John F. Grayson's publication, The Postglacial History of Vegetation and Climate in the Labrador-Quebec Region.

They will be extremely helpful to us in our evaluation of this proposal.

Sincerely yours,

T. O. Jones

T. O. Jones
Assistant to the Head
Office of Scientific
Information

Hunt Institute for Botanical Documentation

February 7, 1957

Dr. John F. Grayson
Magnolia Petroleum Company
Field Research Laboratory
P. O. Box 900
Dallas 21, Texas

Dear John:

The copy of your thesis that you kindly loaned me is on its way back to you via registered mail. We have appreciated having it. I have had photostats made of the strictly palynological parts.

Thanks also for the mimeographed version of your mathematical expression for density of pollen and spores in sediments.

Your ballot and appended comment came the other day, and reminded me of the Pollen Conference and related matters. I still think it would be a mistake for the pollen folks to be completely independent of other paleobotanists with whom they really have much in common. I wonder if we shouldn't have a paleobotanical society (independent of the B.S.A.) with a palynological section? I am sure that the practical difficulties are great (especially because of the small number of people), but the idea is perhaps worthy of consideration.

With best wishes.

Very truly yours,

Alfred Traverse

AT:hmp

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

file under John F. Grayson

NATIONAL SCIENCE FOUNDATION
WASHINGTON 25, D. C.

February 1, 1957

Dr. Alfred Traverse
Shell Development Company
3737 Bellaire Boulevard
Houston 25, Texas

Dear Doctor Traverse:

We have received a request from Dr. John F. Grayson, who is at present working with the Magnolia Petroleum Company, for support of the publication of his thesis, The Postglacial History of Vegetation and Climate in the Labrador-Quebec Region as Determined by Palynology.

The Foundation has normally not been in the habit of supporting the publication of doctoral theses. We do not, however, have a strict rule against this and would perhaps, if a thesis were extremely outstanding, support its publication as a monograph. We understand that you could give us some preliminary advice as to whether we should consider supporting this particular thesis.

Sincerely yours,

T. O. Jones

T. O. Jones
Assistant to the Head
Office of Scientific
Information

Hunt Institute for Botanical Documentation

THE POSTGLACIAL HISTORY OF VEGETATION AND CLIMATE IN THE LABRADOR-QUEBEC REGION AS DETERMINED BY PALYNOLOGY

by

John F. Grayson

The objectives of this investigation were:

1) to reconstruct the vegetational history of the northern Quebec-Labrador region from the time it was deglaciated to the present,

2) to determine the successional stages that occurred in the development of the present vegetation,

3) to use the evidence obtained from this investigation to deduce the climatic changes that have occurred,

4) to date the retreat of the last glacial ice sheet that covered this region, and

5) to obtain further evidence concerning two theories that have developed from previous palynological work. They are the Xerothermic Theory and the Final Period Revertance Theory.

The sediments of four carefully selected bogs, extending over a distance of about 400 miles along a north-south axis through the interior of this region, were analyzed at six inch intervals for their pollen and spore content. The vegetational associations at the various stations were analyzed by the quadrat method. Local successional trends were studied in the bog zonation at the various stations and were compared with the regional vegetational types. The methods used for palynological and vegetational analyses are presented in detail. Calculations for the construction of

Hunt Institute for Botanical Documentation

palynological profiles are based upon the sum of the total pollen and spores rather than the conventional method of using the sum of the arboreal pollen. For each bog, general profiles were constructed for the following groups: arboreal, shrub, bog indicator, and miscellaneous. Individual profiles for selected taxa were also constructed. Radiocarbon age determinations are presented for three of the four bogs.

The following conclusions may be drawn from this investigation.

1. Palynological evidence indicates three regional periods. Tundra vegetation dominated during the first period. A Betula-Alnus association succeeded the tundra and was dominant during the second period. The boreal woodland succeeded the Betula-Alnus association and has been dominant for the past 4000 to 5000 years. Superimposed upon these regional periods, local succession in the vicinity of each station apparently developed in the same order as the regional succession.

2. As for climatic change, only a continued warming trend, with pulsations, from the time of deglaciation of this region to the present is substantiated.

3. Much of this region was deglaciated about 8000 years ago and practically all of it about 6000 years ago.

4. A warm period, which reached its maximum about 3500 years ago, is indicated. It is questionable whether this period was warmer than the present climate of this region.

5. No evidence was found to support the Final Period Revertance Theory in this region.

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P. O. BOX 900
DALLAS 21, TEXAS

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SUPERVISOR EXPLORATION RESEARCH
E. S. WILHELM
ASSISTANT TO MANAGER
SUPERVISOR GENERAL SERVICES
T. A. POLLARD
ASSISTANT TO MANAGER

December 28, 1956

Dr. Alfred Traverse
Shell Development Company
3737 Bellaire Blvd.
Houston 25, Texas

Dear Al:

Thank you for your recent letter and card. My delay in answering is due to the fact that I just returned from my field work.

No thanks are necessary on your part since it was a pleasure having you and van Raadshooven here. My chief regret is that your visit could not have been longer.

By all means, photostat as much of my thesis as you wish. I would appreciate its return as soon as you are finished with it. Thank you for Couper's address. I will write him for his Mesozoic and Cenozoic paper.

The reference to the Sittler paper is:

Sittler, C., 1955. Methodes et Techniques de Preparation des Sediments en vue de leur Analyse Pollinique. Rev. I. F. P. X No. 2, Febrier. 1955.

My best wishes to you and van Raadshooven for a happy holiday season.

Sincerely yours,

John

John F. Grayson

JFG/ms

Hunt Institute for Botanical Documentation

December 17, 1956

Dr. John F. Grayson
Field Research Laboratory
Magnolia Petroleum Company
P. O. Box 900
Dallas 21, Texas

Dear John:

Thanks for a very nice and instructive visit. Your invitation was very kind, and I hope that the visit was stimulating for you to an extent commensurate with the energy expended by you. We also remember fondly the evening at your home and the evening at the Southern Kitchen, a place that was quite as advertised by you.

Couper's address, per your request, is:

Dr. R. A. Couper, Palaeobotanist
Geological Survey Office
Department of Scientific and Industrial Research
93 Boulcott Street
Wellington C. 1, New Zealand

Certainly appreciated your comments on the various matters relating to the Pollen and Spore Catalog and the Paleobotanical Section. I am looking forward to receiving notification that you are one of us.

Looking forward to seeing you again, I am,

Yours very truly,

Alfred Traverse

AT:rlg

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

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HENRY C. CORTES
VICE-PRESIDENT
C. I. ALEXANDER
MANAGER
P. P. REICHERTZ
ASSISTANT MANAGER
SUPERVISOR PRODUCTION RESEARCH

P. O. BOX 900
DALLAS 21, TEXAS

November 19, 1956

N. P. STEVENS
SUPERVISOR EXPLORATION RESEARCH
E. S. WILHELM
ASSISTANT TO MANAGER
SUPERVISOR GENERAL SERVICES
T. A. POLLARD
ASSISTANT TO MANAGER

Dr. Alfred Traverse
Shell Development Company
3737 Bellaire Boulevard
Houston 25, Texas

Dear Al:

Thank you for your recent letter. The dates you suggest are fine with me. By all means, bring Van Raadshooven with you. I will be interested not only in meeting him but in learning how to pronounce his name.

I will be expecting you then on December 11th. My home phone number is FRanklin 4-5715. The Laboratory's phone number is FEderal 1-6531. If you should have any difficulty in finding your way around, please don't hesitate to call.

Sincerely yours,

John

John F. Grayson

JFG/vc

Hunt Institute for Botanical Documentation

November 9, 1956

Dr. John F. Grayson
Magnolia Petroleum Company
Field Research Laboratory
P. O. Box 900
Dallas 21, Texas

Dear John:

Thanks for your nice letter of Halloween. I would indeed like to take you up on the invitation. One of our Dutch palynologists from the the Maracaibo, Venezuela, laboratory is visiting with us this year, and I would like to bring him along, if that is acceptable. His name is Raadshooven, and he has worked with Kuyl and Muller, of whom you have no doubt heard.

We would like to plan to be with you on Tuesday, December 11, and part of Wednesday, December 12, if we do not finish our discussions on the 11th.

Would you let me know if the suggested dates are satisfactory?

Yours very truly,

Alfred Traverse

AT:rlg

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

Dr. John F. Grayson
Magnolia Petroleum Company
Field Research Laboratory
P. O. Box 900
Dallas 21, Texas

Dear John:

Thanks for your nice letter of Halloween. I would indeed like to take you up on the invitation. One of our Dutch palynologists from the Maracaibo, Venezuela, laboratory is visiting with us this year, and I would like to bring him along, if that is acceptable. His name is Van Raadshooven, and he has worked with Kuyl and Muller, of whom you have no doubt heard.

We would like to plan to be with you on Tuesday, 11 December, and part of Wednesday, 12 December, if we do not finish our discussions on the 11th.

Would you let me know if the suggested dates are satisfactory?

Yrs,

Alfred Traverse

Muriel: Would you make a copy of this letter for Dr. Rittenhouse and ask him for his approval of this visit before the letter is returned to me for my signature?

OK
I presume AT has checked with Van R.
GR

Hunt Institute for Botanical Documentation

MAGNOLIA PETROLEUM COMPANY

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October 31, 1956

Dr. Alfred Traverse
Shell Development Company
3737 Bellaire Boulevard
Houston 25, Texas

Dear Al:

Several months ago at the Oberlin Conference, we discussed the possibility of a visit to our Laboratory. Since we saw so little of each other, we never had the opportunity of getting down to a concrete date.

If you are still interested in seeing our facilities, I would like to extend an invitation to you to visit us. From all indications, things should have a semblance of normality for the next few months. So, if it is convenient for you, I assure you that you will be welcome.

Sincerely yours,

John

John F. Grayson

JFG/vc

Hunt Institute for Botanical Documentation

March 9, 1956

Dr. John F. Grayson
P. O. Box 900
Dallas 21, Texas

Dear John:

Thanks for the nice card of February 21. I share your desire for an early reunion. I am in the throes of getting settled here and will not have much to show you for some time. I look forward to getting a look at your setup whenever it would be convenient, preferably after the first of April. Or, if you are going to the Pollen Conference at Oberlin in mid-May, perhaps we can compare notes there, with the idea of an exchange of visits thereafter.

Best wishes as ever.

Yours very truly,

AT

Alfred Traverse

AT:lbd

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

April 9, 1956

Dr. John F. Grayson
Magnolia Petroleum Company
Field Research Laboratories
P. O. Box 900
Dallas 1, Texas

Dear John:

Thanks for the copy of your recent and very significant paper on the fluoride replacement technique. As you doubtless know, some people have been aware that some such reaction was going on, but a formal presentation of the technique, with the X-ray evidence of what occurs was badly needed, and you are to be congratulated for getting this valuable information into print.

Very truly yours,

Alfred Traverse

AT:hmp

Shell Development Company
Exploration and Production Research

Hunt Institute for Botanical Documentation

Feb. 21, 1956

Dear Al,

Thanks you very much for the information as to how to obtain a copy of your work on the Brandon Lignite. I have received my copy and will make good use of it. I already have the microfilm copy of your thesis that I obtained. I hope you are enjoying your new position. I hope that I will be able to visit you sometime in the near future.

Sincerely,

John T. Grayson

Hunt Institute for Botanical Documentation

MAGNOLIA PETROLEUM COMPANY

A SOCONY MOBIL COMPANY

FIELD RESEARCH LABORATORIES

P. O. BOX 900
DALLAS 21, TEXAS

DUNCANVILLE ROAD
BETWEEN LEDBETTER
DRIVE AND FIVE MILE ROAD

D. H. CLEWELL
DIRECTOR

March 27, 1956

Dr. Alfred Traverse
Shell Development Company
3737 Bellaire Boulevard
Houston 25, Texas

Dear Al:

Thank you for your recent letter. I will be looking forward to meeting you at the Palynology Conference at Oberlin. I hope that we will have time together to discuss an exchange of visits.

I hope that you are settled now and are enjoying your new position.

Sincerely yours,

John

JFG/ms

John F. Grayson

Hunt Institute for Botanical Documentation

UNIVERSITY OF MICHIGAN
ANN ARBOR
DEPARTMENT OF BOTANY

January 3, 1954

Dr. Alfred Traverse
U.S. Bureau of Mines
Grand Forks, North Dakota

Dear Dr. Traverse:

Last week in Boston I heard your paper on the application of palynology techniques to coal investigations and thought it was one of the most interesting papers given. I wanted very much to meet you and to be able to talk with you, but I didn't get the chance. After the session, you were engrossed in conversation and I was unable to wait long since I had an appointment. I was sincerely hoping to see you at the Biologist's Smoker, but I was disappointed not to see you.

My own paper was on Pleistocene palynology - the field in which I am working at present. The work that I am doing on a series of bogs from Ungava I hope to eventually present as my doctoral thesis. But my interests are rapidly shifting to Pre-Pleistocene palynology. Since doing some Paeleobotanical work with Dr. Arnold here, I have become interested in the Tertiary and Mesozoic floras and vegetation.

For the past few months I have been searching the literature for pictures and descriptions of pollens and spores, for methods of treating various sediments, etc. And as yet I have had no success. The first lead I heard of was your mention of a publication on Tertiary Pollen by Thompson & Fluke? - (how distorted did I get it?). You can see some of the reasons that I would have wanted to meet you and discuss some of methods and tools you are using.

I wonder if you would be so kind as to give me some assistance by letter? Could you give me a lead as to where to find the proper literature? Where to look for methods of extracting and concentrating pollen and spores from various types of sediments such as limestones, shales, lignites, etc? And where to find pictures or descriptions of pollens and spores that have already been studied? I would sincerely appreciate any information that you might be able to give me. If you have any papers of your own concerning this work, I would very much like to have reprints.

I do hope that I am not asking too much of you Dr. Traverse, but I will be deeply grateful to you for whatever help you might be able to give me. I wish you success in your work and hope that I may have the opportunity of meeting you soon.

Sincerely yours,

John F. Grayson

John F. Grayson

Hunt Institute for Botanical Documentation

UNIVERSITY OF MICHIGAN
ANN ARBOR
DEPARTMENT OF BOTANY

February 18, 1954

Dr. Alfred Traverse
Coal Technologist
U.S. Bureau of Mines
Grand Forks, North Dakota

Dear Dr. Traverse:

Thank you very much for your detailed letter of January 12, and also the reprint of your paper. Please forgive the delay in answering but I have been undergoing an intense period of studying for my Doctoral Prelims. Having successfully completed them yesterday, I can now resume my normal routine. One of the first things on the agenda is some intensive research on the methods of extracting pollen from various types of sediments.

Thank you for your references. At the present time I am trying to trace down every one of them - trying to get a copy of each for myself. I don't like reading German, but I will if it is necessary. I already have a number of preparations in the mill now - using Arnold's slow technique and Kosanke's faster technique. But my aim will be to find better and better techniques.

However, I am running into some difficulty that you may be able to help me with. Would it be possible for you to send me some sediments to work on? The sediments that I am working on now are those of Dr. Arnold's, but he seems to think that they are rather sterile and he has suggested that you might have more fertile material available. I am not particular in the material that I work on - in fact, the more varied the better - shales, limestone, lignite, sandstone, coals, etc. Is there any chance that you might send me a little bit of North Dakota?

Your suggestion of a visit to Grand Forks sounds very tempting to me. Undoubtedly I could learn alot about techniques and methods of study of these "critters". If I would be able to make a trip to Grand Forks - say this summer - would it be possible for you to spare enough time to give me some help? I will be very interested to find out how susceptible you would be to a visit like this.

Thank you again for your assistance. My best wishes to you.

Sincerely yours,

John F. Grayson

John F. Grayson

Hunt Institute for Botanical Documentation

Region V
Box LL, University Station
Grand Forks, North Dakota

January 12, 1954

Mr. John F. Grayson
Department of Botany
University of Michigan
Ann Arbor, Michigan

Dear Mr. Grayson:

Thank you for your very nice letter of January 3. Sorry I missed seeing you after the sessions in Boston. It is good to hear of your interest in Tertiary palynology, because this is indeed one of the widest open fields in American science, and we can stand plenty of additional investigators.

Your problems about getting started in the literature evoke sympathy from me. It is not very long ago that I was in the same boat. First of all, about techniques: be prepared to read considerable German.

1. Richard Kräusel, Die paläobotanischen Untersuchungsmethoden. Jena, Verlag von Gustav Fischer, 1950. This has a brief outline of nearly all the maceration techniques.

2. F. Thiergart, Die Mikropaläontologie als Pollenanalyse im Dienst der Braunkohlenforschung. Verlag von Ferdinand Enke in Stuttgart, 1940. This has something on technique (pp. 7-8).

3. Erdtman, G., An Introduction to Pollen Analysis. This book has a little information on technique.

4. Bertsch, K., Lehrbuch der Pollenanalyse. Verlag Ferdinand Enke, Stuttgart, 1942. This is crammed with information on technique. See especially pp. 24 ff. for technique to be used with various sediments, from chalk to coal. This book probably is the closest thing there is to a laboratory manual on this subject.

The maceration techniques for coal depend very much on the nature of the particular coal being investigated. For example, a technique used with lowest rank lignitic coal may not work with

Hunt Institute for Botanical Documentation

higher rank coals. The Brandon lignite, about which I spoke in Boston, can be extensively macerated with dilute KOH. This has little effect with normal North Dakota lignite unless it has first been oxidized with a strong oxidizing agent. Similarly with various other sedimentary rocks - it depends on the character of the particular rock.

Professor Arnold and Mr. Chaloner over at the Museum have both had experience with macerating bituminous coals for preparation of spores. You might like to talk to them. Professor Arnold often uses the concentrated nitric acid technique, which is simple and adequate but very slow. (If your interest is primarily with Tertiary material, it would be an excellent idea for you to visit Grand Forks sometime.)

Now, as to illustrations of Tertiary pollen: start with the paper of Thiergart's mentioned above. Then see: Zur Geologie der rheinischen Braunkohle, 1-6. Geol. Jarb. Band 65:35-144, 1950. This is a collection of papers on the German brown coal, on which the great bulk of Tertiary palynological work has been done. Thomson's paper has some illustrations of pollen forms, and the other papers contain interesting information re Tertiary palynology.

The work I mentioned in Boston is: Thomson, P. W. and Pflug, H., Pollen und Sporen des Mitteleuropäischen Tertiärs. Palaeontographica, Band 94, Abt. B., Liefg. 1-4, 1953. This has plenty of illustrations but is rather weak taxonomically.

Illustrations of modern pollen: see Erdtman's book mentioned above, also Erdtman's more recent: Pollen Morphology and Plant Taxonomy. Chronica Botanica, Waltham, Mass., 1952. See also: Selling, O. H., Studies in Hawaiian Pollen Statistics, parts I and II. Bernice P. Bishop Museum Special Publications 37 and 38, 1946. Remember, however, that there is no substitute for a reference collection.

Perhaps this will be sufficient as a starter. I have said nothing about Mesozoic material because my experience is very limited there. The methods of preparation are the same, but the taxonomic work is different with pre-Tertiary material (this is true for Paleocene sediments to some extent).

The enclosed paper is unfortunately my only publication so far on this subject. I believe that Professor Arnold has available most or all of the books and papers I have quoted above.

Best wishes to you in your work.

Very truly yours,

AT

Alfred Traverse
Coal Technologist

ATraverse:dm
Enclosure

cc: Region V, Lankford, Traverse, File 625.1, C File

Hunt Institute for Botanical Documentation

Region V
Box LL, University Station
Grand Forks, North Dakota

March 10, 1954

Mr. John F. Grayson
Department of Botary
University of Michigan
Ann Arbor, Michigan

Dear Mr. Grayson:

In the attached package are two small samples of North Dakota lignite and three of the Brandon, Vermont, lignite. (Good preparations can be made of very small amounts of lignite.) You will find the pollen content much higher in the Vermont stuff, and the maceration easier. This is because the North Dakota material has a much higher content of wood and other large plant parts, serving to "dilute" and obscure the pollen. The notations "gritty", "amorphous", and "fine grain" lignite on the jars of Vermont material are terms used by Barghoorn, Spackman and me for distinguishing different parts of the deposit and have no broader significance.

For the Vermont lignite, I recommend successive treatments of (1) hydrofluoric acid, (2) 10% KOH, (3) acidified 5% $NaClO_2$, and (4) a 1:9 mixture of acetic anhydride and concentrated H_2SO_4 ("acetolysis" - this is done with heat - all other steps in the cold). The same schedule can be used with the higher rank North Dakota lignite, if an additional step is added after demineralization with HF: oxidization with $KClO_3$, in a 20% solution of HNO_3 (Schulze's reagent). This oxidation is necessary to permit extraction of the humic material with the KOH. Of course, you will need more detailed information than the bare outline above provides. But this would be a very long letter were I to give an adequate schedule for you to follow. You might find it helpful to borrow my Ph. D. thesis from the Harvard library. It contains detailed schedules. Mr. Charles Trotter of Penn. State College had a problem similar to yours last year and was able to borrow the thesis for reference. (Although in the course of preparation, it is as yet unpublished.) Have your library undertake the job of borrowing it. The reference: Traverse, Alfred. The Pollen and Spores of the Brandon Lignite, a Coal in Vermont of Lower Tertiary Age. Ph. D. Thesis, Department of Biology, Harvard University, Cambridge, Massachusetts, 1951.

Hunt Institute for Botanical Documentation

If you could arrange to visit Grand Forks this summer, as you suggest, I think you might find it interesting and profitable, especially if you are intending to do your doctoral research on Tertiary sediments. Please let me know at least a month ahead of time when you will be here. I plan to be in Grand Forks all summer, except for perhaps two collecting trips.

Very truly yours,

Alfred Traverse
Coal Technologist

ATraverse:dm

cc: Region V
Lankford
Traverse
File 625.1
C File

Hunt Institute for Botanical Documentation

UNIVERSITY OF MICHIGAN
ANN ARBOR
DEPARTMENT OF BOTANY

March 25, 1954

Dr. Alfred Traverse
Bureau of Mines
Grand Forks, North Dakota

Dear Dr. Traverse:

Just a few days ago I received your package and very kind letter. Thank you so very much for both of them. I am very grateful to you for these samples of lignite. These are the first lignites I have been able to get ahold of. So far this semester I have been working on various coals, sandstones, limestones, shales, slates, volcanic ash, and several non-descript unidentified rocks. Already I have two of your samples undergoing treatment. Also I have put in a request for an interlibrary loan of your thesis from Harvard. In addition to the method, I think I might be very interested in reading the whole thesis. The time is approaching when I'm beginning to think about writing such a thing myself.

Thank you, however, for as detailed an account as you did give me in your letter. It certainly is enough to point out the right direction.

Thank you for your invitation to Grand Forks this summer. I would very much like to make the trip and to spend some time working with you. My interest in Tertiary sediments is actually a post-doctoral interest. As for my doctoral research, that seems pretty well set at the moment. Last summer I made a personal expedition into the interior of Labrador and Northern Quebec for the purpose of sampling a series of bogs. It is the palynology of these bogs that I am now working on which I hope will turn out to be my thesis. However, as soon as I finish this Pleistocene work, I want to start working in Pre-Pleistocene sediments.

I wish I could be certain of a visit to Grand Forks this summer, but there are certain complications which may make it difficult if not impossible. The only outstanding one is that my wife is expecting a baby towards the end of the summer - and I'm positive that she would want to come to Grand Forks also. But if things turn out favorably, I certainly would welcome a visit with you. However, you may rest assured that I will let you know in plenty of time.

Thank you again for all of your kindness. I will keep you informed of the progress that I make on your samples. With best wishes, I am,

Sincerely yours,

John F. Grayson

John F. Grayson

Hunt Institute for Botanical Documentation

UNIVERSITY OF MICHIGAN
ANN ARBOR
DEPARTMENT OF BOTANY

RECEIVED
MAY 14 19[illegible]
U. S. Bureau of Mines
Grand Forks, North Dakota

May 11, 1954

Dr. Alfred Traverse
Coal Technologist
U.S. Bureau of Mines
Grand Forks, North Dakota

Dear Dr. Traverse:

Since my last letter to you, I have been highly successful in extracting pollen from the samples that you sent to me. Also I have had your thesis borrowed from the Harvard Library and enjoyed it very much. You certainly did a tremendous amount of work on that thesis - and really produced a wonderful paper. In fact I like it so much that I would like very much to have a copy of it myself for reference - especially those fine photos of the pollen grains.

The University Library said that they would be glad to microfilm it for me if you would give your permission. Therefore I wonder if you would be kind enough to give me your permission. I realize that it has not been published yet and therefore you may be hesitant about granting this permission. Let me assure you that I would use your thesis only as a personal reference - as an aid to methods, identification, and morphology of the pollen that I encounter in your lignite deposits. Most of my work is concentrated on Pleistocene deposits of Quebec and Labrador - on which I'm doing my thesis. Therefore it will be years before I will be in a position to do any publishing on Pre-Pleistocene material.

However if you feel that there would be any risk involved, then by all means please withold your permission. Otherwise please send me a letter with your permission so that I may have the thesis microfilmed. Thank you.

With best wishes, I am,

Sincerely yours,

John F. Grayson

John F. Grayson

Hunt Institute for Botanical Documentation

Region V
Box LL, University Station
Grand Forks, North Dakota

May 18, 1954

Mr. John F. Grayson
Department of Botany
University of Michigan
Ann Arbor, Michigan

Dear Mr. Grayson:

Thank you for your very nice letter of May 11. I am very glad that you found my thesis helpful, and I am happy to give you permission to have it microfilmed.

Very truly yours,

AT

Alfred Traverse
Coal Technologist

ATraverse:dm

cc: Region V
Lankford
Traverse
File 625.1
C File

Hunt Institute for Botanical Documentation

29 October, 1976

Dr. Hilde Grebe
Geol. L. A. Postfach 1080
D-4150 Krefeld
W. Germany

Dear Hilde:

Many thanks for the book sent by you and Frau Potonie (I am writing her also). I really appreciate your kindness.

I have also received Synopsis Vol. VI, and your good letters of explanation.

Saw many of our mutual friends at the recent AASP-CIMP session in Halifax--for example, Norm Frederiksen, and I had a pleasant evening with him. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Krefeld d. 10. 10. 1976

Dear Al,

Thank you very much for your note from October 4, and the check for $ 16.00. As you rightly assumed vol. 7 is a gift and the $ 16,- do cover the expenses for vol. 6. Volume 6 and the receipted bill are on the way. I hope you have got them by now.

Best wishes
yours sincerely

Erika
Erebe

Hunt Institute for Botanical Documentation

Dear Al,

Included you find the receipted bill for vol 6 of Pobumi's „Synopsis ---". The book is on its way to you and I hope it will get there allright in due time. I also hope you have received by now vol. 7 (=25 of the „Fortschritte ---")

Kind regards

Sincerely yours

Willi Greve

Hunt Institute for Botanical Documentation

Dr. Hilde Grebe
Geologisches Landesamt
Postfach 1080
4150 Krefeld
West Germany

October 4, 1976

Dear Hilde:

Many thanks for the kind and helpful letter of 22 September. I look forward to getting the copies of vols. 6 & 7 of the Synopsis. I guess vol. 7 is a gift, and, I assure you, a most welcome one! As for vol. 6, it was very good of you to order it, and I also look forward to having it. My check for $16.00 is enclosed. Let me know if it turns out to be more.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

enclosure: check for $16.00

AT: mk

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

September 3, 1976

Dr. Hilde Grebe
Geologisches Landesamt
Nordrhein-Westfallen
415 Krefeld, de Greiffstr. 195
West Germany

Dear Hilde:

Could you tell me how I could go about ordering copies of volumes 6 and 7 of the Synopsis? Volume 6 is apparently for the most part a reprint of Potonié and Kremp, 1954, but I would like to have it for my files, and of course volume 7 is something I must also have for my palynological library. If possible, perhaps it could be sent directly to me with a bill?

Looking forward to hearing from you about this and with best wishes in the meantime,

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:kc

Hunt Institute for Botanical Documentation

Dr. Hilde Grebe
Geologisches Landesamt
Postfach 1080

4150 K[illegible]feld, den 22. 9. 1976

Prof. Dr. A. T r a v e r s e

Palynological Laboratories
Dept. of Geosciences

THE PENNSYLVANIA STATE UNIVERSITY
Deike Building

UNIVERSITY PARK, Pennsylvania 16802

U. S. A.

Dear Al,

In answer to your letter from September 3rd concerning parts 6 an 7 of Potonié's Synopsis, I have got on its way to you (by Sea Mail) a copy of part 7 (=vol. 25 of the "Fortschritte in der Geologie in Rheinland u. Westfalen"). Mrs.Potonié and I had a few free copies of the volume, and we decided to sent them to people who asked for them, because we did not have enough for all the collegues we would have liked to supply with one. You are fortunate, you get the last but one! - As for "Synopsis" part 6, I have ordered one for you, and I will sent it to you as soon as possible. I think it is easier I pay for it here, and you pay me back at your convenience either by taking it as payment on account for my next volume of the Catalogue or by money order or any other way you like. The price should be DM 40,00.(ca. 16,00 $).

With best wishes
sincerely yours

Hilde

Hunt Institute for Botanical Documentation

Dr. Hilde Grebe
Geologisches Landesamt

D-415 [illegible]feld, den 26. 9. 1974
De Greiff Str. 195, Postfach 1080
W.-Germany

Dr. Jan J a n s o n i u s

IMPERIAL OIL LIMITED
Production Research and
Technical Sevice Laboratory

339.50 Avenue South East
C A L G A R Y, Alberta T2G 2B3

C a n a d a

Dear Jan,

Just a short note before I am off to Brazil on a 6 weeks visit. The Dr. thesis of U. Horst's seems to have acquired some interest these days. I don't know what has happened to our (very poor print) copy of this paper. Part of it seems to have disappeared. Also, the film is not at hand just now. But some time ago I sent a copy of the film (negativ) to Tate A m e s (Palynological Laboratories, 529 Deike Building, University Park, Pennsylvania 16802). So perhaps you could borrow the film from him?

H o r s t 's adress is, or rather was: D-3 Hannover - Buchholz, Postfach 54, Niedersächsisches Landesamt für Bodenforschung. He is retired now but I am reasonably sure that a letter to this adress will find him.

As for joining the AASP I am not sure at the moment. May I decide on that after I am back from Porto Alegre?

Now I hope you will be able to get a copy of Horst's Dr.-Thesis.

With best wishes
yours

Dr. Hilde
Grebe

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

June 17, 1974

Dr. Hilde Grebe
Geologisches Landesamt
D-415 Krefeld
De Greiff Str. 195
Germany

Dear Hilde:

Thank you so much for your nice letter of 12 June with the disappointing but understandable news that you cannot prepare an obituary for Professor Potonié for Taxon. I have turned immediately to my second choice, Gerhard Kremp, and hope that he can oblige. I have sent him along a xerox copy of the very interesting information which you provided. If Gerhard can't do it, I may ask Bill Chaloner. Do you have any other suggestions? From the data which you sent and my memories of the Grand Old Man, I think I could almost do it myself.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi

Hunt Institute for Botanical Documentation

Dr. Hilde Grebe
Geologisches Landesamt

D-415 Krefeld, den 12. Juni 1974
De Greiff Str. 195
Postfach 1080

Professor Alfred T r a v e r s e
The Pennsylvania State University
Deike Building
Dept. of Geosciences
Palynological Laboratories
U N I V E R S I T Y P A R K , Pennsylvania 16802
U. S. A.

Dear Al,

I am very sorry to have to disappoint you. I do appreciate your kindness in asking me to write the orbituary for Professor Potonié for "Taxon". Unfortunately I must recline the flattering offer. At request by Dr. Punt I have already written the "In Memoriam Robert Potonié" for the "Review of Palaeobotany and Palynology" and the manuscript has been accepted for publication and is in press now. You will understand that I cannot write two orbituaries for the same person, not even for Prof. Potonié, with all the sincere admiration and affection I had for him. I am sure you will find someone else to do the orbituary for Dr. Potonié in Taxon. I include a copy of the Curriculum Vitae of Prof. Potonié's written by himself. I had it from Mrs. Potonié and found it a great help for my "In Memoriam".

Yours very truly

Hilde

Hunt Institute for Botanical Documentation

Lebenslauf ROBERT POTONIE

(Abschrift)

Ich, Robert Henri P o t o n i é, ev. getauft, deutscher Staatsangehörigkeit, bin geboren am 2.12.1889 zu Berlin als Sohn des Geh. Bergrats Professor Dr. Henry P o t o n i é.

Ich besuchte das Falkrealgymnasium zu Berlin und die Oberrealschulen zu Lichterfelde und Potsdam.

Ab 1909 studierte ich an der Berliner Universität Geologie und andere Naturwissenschaften, insbesondere Chemie und Botanik, und zwar bei den Professoren Branca, Penck, Pompeckj, Schwendener, Haberlandt, Thoms, Diels usw.. 1913, während der Krankheit und nach dem Tode meines Vaters, leitete ich kommissarisch die von diesem gegründete "Naturwissenschaftliche Wochenschrift".

Bis Ende 1918 nahm ich am ersten Weltkrieg teil.

Am 27.3.1920 bestand ich in Berlin die Doktorprüfung mit einer Arbeit "Der mikrochemische Nachweis fossiler kutinisierter und verholzter Zellwäande sowie fossiler Zellulose und seine Bedeutung für die Geologie der Kohle".

Im selben Jahr wurde ich Volontärassistent an der Preußischen Geologischen Landesanstalt zu Berlin.

Am 18.3.1922 habilitierte ich mich an der Technischen Hochschule Berlin mit einer Arbeit über die Ligninabstammung der Kohle vom paläontologischen Standpunkt aus betrachtet.

1923 wurde ich planmäßiger Assistent an der Preußischen Geologischen Landesanstalt zu Berlin.

Am 13. 6. 1923 erhielt ich an der Technischen Hochschule einen Lehrauftrag für Kohle, Erdöl und die anderen brennbaren Gesteine.

Am 2.2.1926 bestand ich an der Preußischen Geologischen Landesanstalt die zweite geologische Staatsprüfung.

Am 6.7.1926 erhielt ich an der Technischen Hochschule Berlin einen weiteren Lehrauftrag als Nachfolger von Konrad Keilhack über die Geologie Norddeutschlands.

Am 14.9.1929 erfolgte die Ernennung zum außerordentlichen Professor an der Technischen Hochschule Berlin.

Am 29.6. 1934 wurde ich Bezirks-Geologe am Reichsamt für Bodenforschung

Hunt Institute for Botanical Documentation

Ab 1937 war ich Herausgeber des Geologischen Teils des Geologischen Zentralblattes.

Am 20.1.1940 Ernennung zum Beamteten außerordentlichen Brofessor an der Technischen Hochschule Berlin.

1940 in einem Brigadestab der Nachtjägerdivision Kammhuber.

Von 1941-1944 arbeitete ich an der Reichsanstalt der Luftwaffe, wo ich u.a. die geologischen Belange zu vertreten hatte, zuletzt in Rumänien.

1942 Ernennung zum Ehrenmitglied des Volksbundes Naturschutz.

Seit dem 24.8.1944 befand ich mich zuerst in rumänischer, dann in russischer Kriegsgefangenschaft, aus der ich am 11.5.1948 zurückgekehrt bin. Die während meiner Gefangenschaft verfaßten geologischen Arbeiten sind mir am 1.5.1948 von einem russischen Kommissar in Brest-Litowsk abgenommen worden.

31.3.1950 Ernennung zum Honorarprofessor an der Universität Bonn und zum Leiter der Kohlenpetrographischen Abteilung des Mineralogischen Instituts.

1951 Wahl zum Präsidenten der Internationalen Kommission für Kohlenpetrologie.

1954 Ernennung zum Abteilungsdirektor am Amt für Bodenforschung in Krefeld.

Wahl zum Mitglied der Internationalen Kommission für palaeobotanische Nomenklatur.

Nov. 1957 - April 1958 Lehr- und Forschungstätigkeit (als Visiting scientist) am Birbal Sahni Institut (für Palaeobotanik) in Lucknow, Indien.

Sept. 1958 Wahl zum Präsidenten der Internationalen Kommission für die Mikroflora des Paläozoikums.

1961 Ernennung zum Ehrenmitglied der Indischen Palaeobotanischen Gesellschaft

30.6.1961 Verleihung des Großen Verdienstkreuzes des Verdienstordens der Bundesrepublik Deutschland.

6.8.1964 Wahl zum Ehrenmitglied der Internationalen Organisation für Palaeobotanik.

10.9.1964 Wahl zum Ehrenpräsidenten der Internationalen Kommission für Kohlenpetrologie . - Verleihung der Reinhardt-Thiessen-Medaille. -

16.6.1966 Wahl zum Korrespondierenden Mitglied der "Socoété Royale des Sciences de Liège".

16.4.1969 Verleihung der Médaille de l'Université Liège

1971 Verleihung der Silver Jubilee Medaille des Birbal Sahni Institutes für Palaeobotany Lucknow/Indien.

Hunt Institute for Botanical Documentation

June 3, 1974

Dr. Hilda Grebe
Geologisches Landesamt
Krefeld, Westwall 124
Germany

Dear Hilda:

Several of us on the International Committee for Fossil Plant Nomenclature have been corresponding about what should be done regarding an obituary for Dr. Potonié in Taxon. It would normally be our responsibility since he was a member of our committee. I have advised several people that you would be the logical one to write such an obituary, and I am most ernestly urging you to do so. Will you do it?

Yours very truly,

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

AT/vsi
cc F. Stafleu
J.M. Schopf

Hunt Institute for Botanical Documentation

Department of Geology-Geophysics
Pennsylvania State University
University Park, Pennsylvania 16802
U. S. A.

5 August, 1966

Dr. Hilde Grebe
Geologisches Landesamt
Westwall 124
Krefeld 415
Germany

Dear Dr. Grebe

This letter is intended to serve two purposes--to let you know that I am now here in association with Dr. Spackman, and to comment on the chapter "Terminology of Spores", for the CIMP publication.

I have read the chapter quite carefully and find it to be admirably concise and useful--along the lines of Faegri-Iversen. Erdtman would doubtless regard it as far too simple, with his appreciation for elegance in terminology, and the definition of terms. The truth is that there are far more of us for whom simplicity is a help to understanding than there are those for whom elegance in language is helpful.

Is there a chance that you would have an extra copy of the mimeographed "Vorschläge u.s.w."? If so, I would very much like to have one for my records.

Yours very truly,

Alfred Traverse

Alfred Traverse
Associate Professor of Geology

Regards to my friends in Krefeld--see you at Utrecht?

Hunt Institute for Botanical Documentation

Faxed to:

001-814-863-7823

Dr. Roy J. Greenfield
Dike 441-PSU
UPR-PA 16802-USA

NR.	:	001
EMPFÄNGER	:	DEPT OF GEOSCIENCES
SENDER	:	SNG KUHWALDSTRASSE
DATUM	:	24 JUN'92 9:41
DAUER	:	00'34
MODUS	:	STAND
SEIT.	:	01
ERGEBNIS	:	OK

Dear Ray,

Your nice letter of 1 June just in. I have nearly moved to Canton and you! Restaurants here are a disaster from a health standpoint — too much fat, too much meat, almost no vegetables.

I get back on 1 July. How about 10 July for the first trip to our „Stammtisch" in Calder Alley?

Betty leaves tomorrow. I go to Stuttgart for a bit of last-minute field work with a colleague there.

All the best — and greetings to Dixie

Al Traverse

Hunt Institute for Botanical Documentation

441 Deike Building
University Park, PA 6802
June 1, 1992
(814) 865-5723

Dear Al and Betty,

I am enclosing an article on the woman's Olympic basketball team. I is interesting that none of the present crop of players was selected for the team. I wonder what the players who are out of college for several years have been doing to keep in form.

Hope you have been getting things done and enjoying yourselves on sabbatical. I think I remember that you have about another month in Germany. I am looking forward to our next trip to the Canton. Do you want to set a date now.

Nothing of major import is happening at school. We are in the process of trying to hire a stratigrfer and a geophysicist. Budgets are bad for the next couple of years as usual.

Presidential politics are in the usual state of confusion. Most of the population is coming to the assessment of Bush that we have had for a while. Bill Clinton is not much liked although few people know much about him. Ross Pirou, the independent candidate is popular for the moment. It looks like the election year will be interesting.

Diane and I are going to her 30th class reunion next week then we'll go to Vermont for a few days.

Look forward to seeing you.

Sincerely,

Roy

Hunt Institute for Botanical Documentation

Ca 5-14-92

From: A. Traverse
Beethovenstr. 36, Nr. 25
6000 Frankfurt a.M. 1
Germany (Ph. 798-8555)

Off.: Senckenberg Mus.,
Abt. Paläobot.
Senckenberganlage 25
6000 Frankfurt a.M. 1
Germany (Ph. 7940004-50)

Fax No: 746.238
(not Frankf. nos have 69 as prefix)

Dr. Roy Greenfield
Deike 4th Fl., PSU
Univ. Park, PA 16802, USA
Fax No. 814-863-7823

Dear Roy!

Until this week all has gone super with the computer. Now there is some glitsch, and it hangs up solid in WP 5.1 (usually, not always!) — after 4-5 letter-keys are entered. The local "gurus" have used all diagnostic tools and are of the opinion that there is some minor erasure in either DOS or WP 5.1. Could you send us copies (not originals) of our program disks? Betty says they're in her desk, bottom left drawer. Local gurus think we should "reinstall." Please send air mail to our "home" address.

Just in case, could you also find out if Zeos Int. has a representative in Germany? (Sorry)! Greetings

Al

Hunt Institute for Botanical Documentation

441 Deike Building
Geosciences Dept.
Penn. State Univ.
Univ. Park, PA 16802
(814) 865-5723
FAX (814) 863-7833
E-Mail roy@geosc.psu.edu
30 March, 1992

Dear Al and Betty,

I got your fax. Between the handwriting and the low resolution of the fax I took a while to decipher. I am not sure this will solve your problem, but I am sending a copy of the 3.5 inch copies of WP 5.1 I found in Bettys desk. Try installing this word perfect first.

I could not find the Zeos Dos disk so I made a system disk up. Do you know where the Zeos Dos disk might be. I do not know what Dos is on the Zeos. The version I am including is V4.01. Be careful in using this disk. If by some chance you file COMMAND.COM is bad you could copy this file with a DOS Copy command to your disk. However be very careful when you do this. It can mess up the disk to mix versions of DOS. You best have one of the local Experts help with this. Only do it as a last resort.

Make sure the config.sys file is ok. before doing anything with the DOS disk I sent.

On other fronts nothing major is happening around the dept. We have had to return $75,000 to the central administration so things are not too flush.

All three of my boys have been in town. We enjoyed seeing them. We went to the regional girls basketball tournament game here, where PSU won by about 20 points. I also listened to the next game where MISS beat PSU in the last 30 seconds.

I miss the lunches have you found good restaurants there? It looks like you will be able to make it back to the US while the recession is still going.

Regards,

Roy

Greenfield

Hunt Institute for Botanical Documentation

441 Deike, Jan. 23, 1992

Dear Betty and Al,

Your letter was the longest hand written letter I have gotten in a long time. In sounds like things are going well in most aspects. How is the housing working out.

It finally got cold here last weekend so skiing has been good for a few days. I have been having a lot of trouble with my PC disk at school. I will have to get a tape backup unit and then reformat the whole thing.

Diane is getting on well and really enjoying her job.

Enclosed is a disk and the following instructions.

By the way. You may very well be able to contact me on electronic mail on Bitnet. Most University and scientific establisments can do this. Ask some of the computer people around if this is posible. My address is roy@geosc.psu.edu . This would give at worst a few hours delivery time.

Reguards

Roy

INSTRUCTIONS FOR GETTING THE OK182 PRINTER INTO WORD PERFECT

1. COPY THE ENCLOSED FILE WPDM6.ALL INTO YOUR WORD PERFECT DIRECTORY

NOTE: THIS IS PROBABLY THE DIRECTORY WHERE WP LOOKS FOR THE PRINTER SPECIFICATION FILES. IF THERE IS ANY QUESTIONS USE SHIFT-F1 FOR SETUP AND LOOK AT CHOICE 6 TO SEE WHERE PRINTER FILES ARE LOCATED

2. BACK IN REGULAR TYPING SCREEN GO TO PRINTER SHIFT-F7. THE HIT ADDITIONAL PRINTERS (2 OR A). YOU WILL HAVE A CHANCE TO SELECT THE OK182. DO SO. MAKE SURE THE * IS ON THE OK182 CHOICE ON THE SELECT PRINTER SCREEN. THIS MAKES IT THE DEFAULT. IF NOT MOVE THE CURSOR OVER IT AND HIT CARR.-RET. THEN ON THE SELECT SCREEN HIT UPDATE (7 OR U) THIS SHOULD SET YOU UP

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY

INTER-OFFICE CORRESPONDENCE

Date: July 10, 1967

From: L. P. Greenhill

To: Mr. Alfred Traverse
Associate Professor of Geology

Thank you very much for your memorandum of June 20 expressing appreciation for the assistance provided by Paul Stevenson and his staff in preparing instructional illustrations for your lectures in Geological Sciences 21.

We try to make every effort to assist the faculty by providing such services and it is gratifying to know that these services are appreciated.

Hunt Institute for Botanical Documentation

June 20, 1967

Alfred Traverse

Mr. L. P. Greenhill
Director of the Division of Instructional Services
103 Carnegie
Campus

Dear Mr. Greenhill:

This is just a brief note to acknowledge with gratitude the very fine cooperation I received in preparing instructional illustrations for my lectures in Geological Sciences 21 during this past term. Mr. Paul Stevenson and his staff spared no effort to provide me with charts and overhead transparencies of the highest quality.

I appreciated this very much.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

AT:kaw

Hunt Institute for Botanical Documentation

5 July, 1984

Greer Laboratories, Inc.
Lenoir, NC 28645

Dear Friends:

Colleagues at the Smithsonian tell me that we might be able to purchase from you pure samples of pollen of various species. We need this for experiments we are currently undertaking.

Could you send a list of what you have and how we could order same?

Thanks for your help.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

May 3, 1956

Mr. D. C. Greer
Chief Engineer
Highway Department
Highway Building
11th and Brazos
Austin, Texas

Dear Mr. Greer:

As part of our research program, Dr. A. F. Traverse, Jr., of our staff is studying the morphology and distribution of spores and pollen of present day plants. We believe that such studies will lead to a better understanding of the spores and pollen that occur in the rocks of Texas and other areas and eventually will be helpful in finding new oil and gas fields.

For his studies Dr. Traverse needs small samples of plant materials, many of which grow on highway rights of way. The samples needed usually consist of several blossoms, stems or small branches of each kind of plant but occasionally may consist of an entire small plant. Dr. Traverse is a trained botanist who received his Ph. D. from Harvard University and worked with the U. S. Bureau of Mines for four years before joining our staff. He appreciates the importance of preserving plant life and his collecting will not damage plant colonies or otherwise deface the property from which collections are made. He has already been granted permission to collect from all parks and public properties in the city of Houston.

It would greatly facilitate Dr. Traverse's work if he could collect plant specimens from highway rights of way and we should appreciate receiving permission for him to do so on such rights of way in all parts of Texas.

Very truly yours,

NOYES D. SMITH, JR.

Noyes D. Smith, Jr.

AFT:rlw

bc - Dr. Traverse
Dr. Rittenhouse
D file

Hunt Institute for Botanical Documentation

November 19, 1973

Alfred Traverse

Dr. Ilene Fife
225 Sparks Bldg.

file

Dear Dr. Fife:

Ms. Frances Gregory has asked me to write a letter in support of her application to do graduate work. I have known "Fran" ever since I came to Penn State eight years ago and think that I am reasonably conversant with her intellectual ability. Although Ms. Gregory has never obtained an advanced degree, it is clear to me that she obviously has the native ability to do so and that the main reason she has never got a Master's or Doctor's degree is mostly a product of personal circumstances, not lack of ability. She is perceptive, has a good memory, and organizes information well. I would expect that in a graduate program she would handle herself as well as anybody in her age group and probably better than most, for the reason that she has remained close to the academic scene since getting her Bachelor's degree, even though not for the most part as a graduate student. I have had some experience, through working with "Fran" in our department when she was here, with her ability to organize difficult material and comprehend the relationship between concepts and data, and I feel sure that if she is in a position to devote time to graduate work she will do it with some distinction.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

CONFIDENTIAL REFERENCE

From:

The Pennsylvania State University
UNIVERSITY PLACEMENT SERVICE
Teacher Placement Division
Grange Building
University Park, Pennsylvania 16802

To the person addressed:

Your name has been given to us as a reference by the person named below.

Mary Frances Gregory

We appreciate your appraisal of his qualifications. Your statement will be part of his permanent record. It is confidential and made available only to employing officials.

To:

Prof. Alfred Traverse

Since this form will be photographically reproduced, it is necessary that you exercise great care in filling it in. What you write will be photographed exactly and sent out to prospective employers.

GENERAL EVALUATION: Please write something specific about the strength and weakness and the abilities and qualifications of the registrant.

(PLEASE TYPE OR USE BLACK INK)

I have come to know Miss Gregory quite well in the six years I have been here, as her position in the Geology-Geophysics Department (now merged in the new Geosciences Department) has very frequently made her the person to whom I have had to turn for information, advice and assistance.

"Fran" has a very quick and retentive mind and always seems to be on top of the situation. She has an unusual ability to assess the best way to tackle a problem when it develops and seems to be able to suggest the best way to achieve solution.

She makes friends easily and obviously likes people (witness the hordes of students who leapt to her defense when the dissolution of her position here was announced). I know of many people who run ideas past her desk as an indication of how the wind will blow when more people get into the picture. She is always accessible and never seems too busy to spend a little time with anybody who needs her help, and yet she seems to get her work done. During peak periods especially, that has been no mean accomplishment. I regard her as very efficient.

Fran is also a most attractive person--always well dressed, always with a pleasant smile. This is certainly an attribute in any office situation. She practically exudes readiness to be helpful.

In brief, I think this woman is extremely well qualified in every way to do executive - administrative work in practically any office situation. I only regret that a way can't be found to employ her within this college.

Signed Alfred Traverse

Position Professor of Geology & Biology

Address ~~Deike 529, Univ. Park,~~ Pa. 16802

Date 6-III-72

Hunt Institute for Botanical Documentation

(Do not use reverse side)

Form 1101.17

April 22, 1969

A. Traverse

F. Gregory

Dear Fran:

Since I shall be out of town when you get back*, I thought I'd better get something in writing to you before I get back:

1. Did we send materials to Miss May Wong?

2. Dan Habib of Queens Collgge called the other day to say that the boy he had expected to apply got scared and joined the army, or something (shades of Harvey Zeiss).

   However, he is sending two students to look the department and me over in the near future. Unfortunately, they can only come on a weekend, but I did want you to be aware that they are coming. Names: Martine Dreyfuss (a girl) and a Mr. Jacobson. Habib says both are good students. Seniors. Dates: Sat.-Sun 3-4 May. (Have to come on weekend because of comprehensive exams and the like during the week.)

Alfred Traverse

AT:kwc

* Do the titles of my lectures ever get into that list of faculty lectures in the slick-paper thing?

Hunt Institute for Botanical Documentation

UNIVERSITY OF CALIFORNIA

DEPARTMENT OF PALEONTOLOGY
BERKELEY 4, CALIFORNIA

October 10, 1961

Dr. Alfred Traverse
P. O. Box 481
Houston 1, Texas

Dear Dr. Traverse:

I am looking forward to meeting you next Monday morning. We are requesting that you be issued a temporary campus parking permit; if you will give your name to the guard at the west gate (head University Avenue) when you arrive, he should have the authorization.

Your lecture on "Palynology and the origin of the Angiosperms" will be given Tuesday afternoon, October 17, at 4:00 p.m.; the seminar on "Microscopy of the Low-Rank Coals" Wednesday morning at 9:00 a.m.

Dr. Arnold and some of the other micropaleontologists are quite interested in discussing microforams with you; if you care to do so, time for an informal presentation of the microforam problem in palynology can be arranged.

Tuesday noon you will be a guest of the department at luncheon. Tuesday evening Mrs. Gregory and I would like to have you dine with us in Lafayette. I expect the rest of your time will be filled in rather quickly when you arrive.

With kindest regards,

Sincerely yours,

Joseph T. Gregory

Joseph T. Gregory
Chairman

JTG:MP

Hunt Institute for Botanical Documentation

UNIVERSITY OF CALIFORNIA

DEPARTMENT OF PALEONTOLOGY
BERKELEY 4, CALIFORNIA

August 15, 1961

Dr. Alfred Traverse
Shell Development Company
Exploration and Production
Research Division
P. O. Box 481
Houston 1, Texas

Dear Dr. Traverse:

We are indeed pleased that you will be able to lecture to this department on palynology next month, and also are most grateful to your company for its generosity in defraying the expense of the trip. You indicated that the latter part of October would be most convenient to you; we should prefer sometime during the week of October 16-20 to the following week if you can arrange it. Our general seminars have commonly been held on Tuesdays but can be arranged for almost any day of the week.

"Palynology and the origin of the Flowering Plants" is an excellent topic in which both the paleontologists and botanists of the University will be greatly interested. However some of our staff have misgivings about the suitability of a seminar on "Trends in paleobotanical systematics" for the rather general audience which you will face. (The seminar will by no means be confined to advanced students in paleontology botany). It seems to us that either another evolutionary topic or else a discussion of some of the problems of botanical associations of fossil spores and pollen might be more interesting. We leave final selection to you, however.

Please let me know the dates on which to expect you, and after you have made travel reservations the time of your arrival. We shall try to meet you at the airport.

Most sincerely,

Joseph T. Gregory

Joseph T. Gregory

JTG:MP

Hunt Institute for Botanical Documentation

17 August, 1961

Dr. Joseph T. Gregory
Department of Paleontology
University of California
Berkeley 4, California

Dear Dr. Gregory:

Thanks for your nice letter of 15 August, which seems to tie up most of the loose ends of the proposed visit to Berkeley in October. I am certainly looking forward to the trip. I fell in love with Northern California in 1957 at the Stanford paleobotanical meetings, which I chaired, and have been trying to promote a re-visit for quite a while. I had been planning for us to take a family vacation in the area, but each year something else came up in the clan to make it impossible.

I shall plan to arrive in Berkeley the morning of 16 October. You will probably be astonished at my backwardness, but I customarily travel by train, using the air lines only for emergencies. Since in this instance I have made no alternative arrangements with the University of Havana, I believe I'll stick to the rails. Unless the schedules have been changed since '57, the train stops in Berkeley. Shell will arrange for a hotel and a rent car for me, so the best thing would be for me to contact you Monday after I get all of that ~~bahongodre~~ of. I may plan to arrive Sunday, in order to be able to visit some of the botanical sights I missed last time. I much appreciate the offer to meet me, but I think it would inconvenience you without cause.

Regarding the subject for the seminar--I fully understand your reaction, and I am sure you also understand my problem: there are plenty of things I would greatly enjoy talking on but which are taboo for me. In fact, anything that I have been working on is in that category. Yet, I want to talk about something on which I can speak with authority. Two other possibilities come to mind:

Hunt Institute for Botanical Documentation

1. The microforam problem in palynology

2. "Non-pollen" forms encountered in Tertiary palynology

I think I can present an interesting talk on either of these subjects. The so-called microforams have been of considerable interest to me. Though one could make an argument that discussion of them is beyond the paleobotanical pale, the truth is that they do occur in many palynological preparations, and their interpretation is a matter of concern for palynologists. This goes for "non-pollen" as well (mostly algae of various sorts), but presentation of #2 would be more of a survey sort of thing than an explanation of a research problem.

Would you let me know which of these two topics would be the more desirable?

I could, of course, do either a warm-over on the Brandon lignite problem, or even a bit on the petrography of lignitic coals, both paleobotanical problems of some interest on which I am reasonably well informed but which seem like yesterday's meal loaf to me for a group that is presumably pretty well r ad.

Thanks for your great patience.

Yours very truly,

Alfred Traverse

Hunt Institute for Botanical Documentation

UNIVERSITY OF CALIFORNIA

DEPARTMENT OF PALEONTOLOGY
BERKELEY 4, CALIFORNIA

August 1, 1961

Dr. Alfred Traverse
1428 Adkins Road
Houston 24, Texas

Dear Dr. Traverse:

We should like to have you visit Berkeley early in the Fall semester to give a public lecture on some phase of Palynology and meet the department seminar in paleobotany. The University will pay you an honorarium which will cover air-coach fare and other travel expense, and we shall be glad to provide for your entertainment while here. I hope that you can arrange to spend at least three full days on the campus, so that we may profit by leisurely discussions.

Your talks may be on any research topics which you believe would interest a group of paleontologists; the seminar of course can be more technical than the public lecture.

If you are able to do this, would you please let me know one or more dates during October that would be possible for you to come here. I will then proceed with arrangements.

With kindest regards,

Joseph T. Gregory

Joseph T. Gregory

JTG:MM

Airmail

Hunt Institute for Botanical Documentation

INSTITUTUM BOTANICUM
UNIVERSITATIS
Szeged, Hungaria

Mr. Alfred Traverse
Retiring Secretary
Paleobotanical Section, B.S.A.
P.O.Box 481
Houston 1.
Texas

Szeged, ~~1959~~ 27.1o.1959.

Dear Sir!

Thank you very much the name list of the persons participating ân the luncheon of the paleobotanical section in Montreal. This will be a real pleasant reminiscent of the fine days of Canada. Connected with this I may ask a favour. Please be so kind and send a photo making of the paleobotanists to my address. This photo was made at 26 August, after the lecture, in front of the Physical Institute. I paid in the price of it at that time, but since I left Montreal earlier, I did n't get the photo, however it would be very kind reminiscent too, because the persons were present on my lecture also.

I beg your pardon for my request.

Thanking again your cordiality

yours sincerely

P. Gregus

/Prof.P.Gregus/

Hunt Institute for Botanical Documentation

Greuter

To: zi@zedat.fu-berlin.de
From: Alfred Traverse <traverse@ems.psu.edu>
Subject: MS
Cc:
Bcc:
Attached:

Liebe Frau Dr. Zimmer, lieber Herr Dr. Greuter,

das Manuskript von Philippe et al. ist wahrscheinlich vor zwei Wochen angekommen, aber ich bin auf einer Reise gewesen und habe es erst heute geöffnet.

Ich habe nicht seit Jahrzehnten mit fossilem Holz gearbeitet. Es ist ein hochspezialisiertes Fachgebiet. Ich könnte natürlich die sprachlichen und nomenklatorischen Sachen gut kontrollieren, aber Sie brauchen unbedingt jemanden aus dem englischen Sprachraum der/die auch mit den holzanatomischen Sachen vertraut ist. Chaloner oder andere Menschen an der Univ. London (Andrew Scott, Margaret Collinson) könnte jemanden empfehlen.

Es tut mir wirklich leid für Sie ein Problemkind zu sein. Ich schicke sofort das Ms Ihnen zurück.

Mit besten Grüßen

Ihr Alfred Traverse

Hunt Institute for Botanical Documentation

From: "PROF. DR. W. GREUTER" <wg@zedat.fu-berlin.de>
Return-path: <WG@mail.zedat.fu-berlin.de>
In-Reply-To: <C72C9282075AD111AF6300805FFE1C8F1FB0D9@s5-cal-r1.gscc.nrcan.gc.ca>
To: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
Cc: fensome@agc.bio.ns.ca, traverse@ems.psu.edu, alandbetty@juno.com
Date: Wed, 23 Sep 1998 15:17:18 +0000
Subject: Re: FW: homonyms CORRECTION
Message-ID: <m0zLozn-00NGqVC@mail.zedat.fu-berlin.de>
X-Status: Read
X-Mailer: Pegasus Mail for Windows (v2.54)

Dear Dr Jansonius,

No major problem as I see it. My reply is intercalated below. I am copying my answer directly to Rob and Al.

> From: Jansonius, Jan
> Sent: Tuesday, September 22, 1998 1:34 PM
> To: 'wg@zedat.fu-berlin.de'
> Cc: Fensome, Rob; 'Al'
> Subject: homonyms
>
> Dear Dr. Greuter,
>
> I would appreciate if you could help me figure out how to deal with a
> problem in how to deal with some synonyms.
>
> I am involved in a major review of fossil fungal remains, esp. fungal
> spores. We (Ram Kalgutkar and I) want to clean up and deconfuse both
> nomenclature, as well as taxonomy, of all validly published names. A
> number of authors have poor access to th eexisting literature, and have
> not been too critical as to which genus a new species ought to be best
> assigned to.
>
> There are:
> 1 Multicellaesporites elsikii Kar & Saxena 1976
> 2 Multicellaesporites elsikii Ediger & Alisan 1989
> 3 Multicellaesporites elsikii (Ram. & Sris.) Kumar 1990
> [= Staphlosporonites elsikii Ramanujam & Srisailam 1980]

(2) and (3) are illegitimate under Art. 53. Neither can compete for priority at any rank.

> In our first go around, we coined the new name (4) "M. edigeri" for name
> (2). This name is, of course, still a MS name. [How would it affect the
> reasoning in the following if in the meantime this new combination had
> been published?]

Perfectly good intent (but no action yet).

> In our second go-around, we decided that the species with name (2) should
> be transferred to the genus Pluricellaesporites.
> My quandary is: do I then name that species (5) P. edigeri nom. nov. comb.
> nov.; OR, do I just transfer it as (6) P. elsikii (Ediger & Alisan) comb.
> nov. [non: M. elsikii Kar & Sax.; non: M. elsikii (Ram. & Sris.) Kumar]?

The choice of epithet is yours, as the species has [I assume] no

Hunt Institute for Botanical Documentation

legitimate name to go with it. However, your (6) would not be a comb. nov. but the name of a new species (requiring all respective validation paraphernalia) or a nomen novum (based on the illegitimate (2)), and would in either case have no parenthetical authorship citation. The situation is covered in Art. 58, in a much too complicated language (I have a proposed complete rewording in print, to be published in November Taxon); Art. 58 Ex. 2 is exactly to the point.

> Name (3) is redundant, because we want to maintain it in
> Staphlosporonites. However, should it still receive a new epithet to
> counter the homonymy? Or, is it enough to mention that the combination
M.
> elsikii (Ram. & Sris.) Kumar is illegitimate, and the transfer not
> accepted by us?

It is enough to mention it as an (illegitimate) synonym. Actually you cannot validated a new name (with a new epithet) unless you accept it – which you obviously don't (see Art. 34.1). It would certainly go too far if, just to regularise the homonymy situation, you would feign in print to accept a name (and generic placement of a species) which in fact you disapprove.

> Name (5) was never effectively published, so it cannot very well be
both
> 'comb. nov.' AND 'nom. nov.' Can the specific epithet 'elsikii', as
used
> in the illegitimate combination of name (2), be maintained as the
> legitimate epithet in the new combination (6) P. elsikii (Ediger &
> Alisan)?

See above.

> To me the latter course feels wrong - possibly because there might be a
> future worker who wants to maintain the species in Multicellaesporites.
> Also, this epithet "elsikii" was directly combined with
> Multicellaesporites as an (illegimate) primary binomial.
> The third example of the homonymous combination was a secondary
transfer,
> and therefore can be rejected (as was the transfer) as an illegitimate
> name, without affecting the legitimacy of the basionym.
I LATER THIS AFTERNOON FOUND THAT, IF I WANT TO TRANSFER THE SPECIES TO PLURICELLAESPORITES, THAT P. ELSIKII ALREADY EXISTS, AND SO I WILL HAVE
TO
FIND A NEW NAME FOR THE SPECIES UNDER THAT GENUS. AND WHICH OTHER NAME
THAN
P. EDIGERI COMES TO MIND? IT ALMOST IS RIDICULOUS. NEVERTHELESS -- THE PRINCIPLE BEHIND MY QUESTIONS HAS NOT CHANGED. WHAT SHOULD I HAVE DONE IF THE NEW COMBINATIOIN P. ELSIKII WAS NOT PREOCCUPIED?

As matters stand, option (5) is out, and (6) is available either as a nom. nov. or sp. nov.; of course you are free to choose any epithet not yet combined under P. (and not tautonymous).

> I think that an example of this eventuality would be useful, following
> that of Art. 53. Ex. 4. Of course, the rules of the ICBN should be
> clearly and precisely worded, but in practice it is easier to follow a
> concrete correct example, than work out an unexpected eventuality, when
> the elements needed for correct interpretation of the code are
scattered
> over several disjointed articles.

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Recall: Art. 58 Ex. 2 is to the point.

> In Art. 53, it is clear that a junior homonym is illegitimate. This
> article is in the section dealing with rejection. Is therefore this
> illegitimate junior homonym to be rejected? And if so, is it then not
> incumbent on the one doing the rejection to propose a new name for the
> orphaned taxon? Or, is there a deeper meaning for not including the
word
> 'rejected' in Art, 53, although it is included in the text of Art.
52.1?
> In Art. 58.1 there is the suggestion that the name to which Art. 53
> applies is to be rejected -- or is it just 'unavailable'? However, the
> latter word is not in Art. 53. And: what does it mean?
> Anyway, Art. 58 goes on to imply that, unless another name that has
> priority exists, a new name MUST be chosen. Under 58.1(c), "a new
> combination, based on the name in the other rank, may be published."
Does
> that indicate that my name (6) is correct -- yet, that is a straight
> lateral transfer, not one in 'another rank'?

There is some old vocabulary still used in those articles, including "rejected" and "must" that you quote, which I dislike. ("Rejected" also appears in Art. 53 Note 1, by the way.) What "rejected" means is: "not available for use". It is not an individual action (requiring publication etc.) but is the intrinsic status of an illegitimate name. This is clear, basically, from Art. 11 concerning priority and choice between competing names, where only legitimate name are stated to compete. Again, the "must" in Art. 58 is not an order to anyone "rejecting" an illegitimate name, that he/she "must make" a new name instead, but is the statement of fact, that if you need a name for a taxon you recognise, and there is no legitimate name available, you'll have to make a new name -- else you cannot properly refer to your taxon.

> I would like to do the right thing, because that is the purpose of our
> fungal spore compilation -- to set an example -- but I am unsure about
> what the Code tries to tell me about this case. (And there are a number
of
> very similar situations we have to deal with.)

I hope this answers all your questions.

> I am conscious of the fact that you must be very busy -- with things
that
> appear to be more significant -- but if you have no time to explain the
> Code, or where I went off course, at least an indication as to which of
> the numbered names are correct would be of great help to me. In that
case:
> ignore the cumulation of question marks.
>
> I am sending a copy to Fensome and Traverse, for their comment if they
> have the time, or for their education. I may send them your reply.
>
> Looking forward to your enlightning comments,
> with best regards, your
> Jan Jansonius.

Kind regards, Werner Greuter

Professor Werner Greuter
Botanischer Garten & Botanisches Museum Berlin-Dahlem
D-14191 Berlin

Hunt Institute for Botanical Documentation

wg@zedat.fu-berlin.de
http://www.bgbm.fu-berlin.de/bgbm/staff/wiss/Greuter+W/default.htm

Hunt Institute for Botanical Documentation

Comments: Authenticated sender is <WG@mail.zedat.fu-berlin.de>
From: "PROF. DR. W. GREUTER" <wg@zedat.fu-berlin.de>
To: traverse@ems.psu.edu (Alfred Traverse)
Date: Wed, 18 Mar 1998 11:12:37 +0000
Subject: Re: proposal
CC: nicolson.dan@nmnh.si.edu
Priority: normal

Copy sent to Jansonius

Dear Al,

I have read your further thoughts on the matter with great interest. Let me start by saying that the proposal is now basically with Dan in Washington, and that he is the one who will look with you into the details. I am copying this reply to him so as to keep him informed.

> Let me state very concisely what I want to accomplish. It is very obvious
> that AVAILABLE types for practical purposes mostly don't exist for
> paleopalynology. To deal with this situation, I advocate accepting
> illustrations as types. It is not my intention to threaten the status of
> ANY names in the literature, but to provide a technique for designating
> lectotypes, neotypes and epitypes from among existing illustrations, as the
> need is perceived, and to encourage palynologists henceforth to designate
> illustrations as types of new names. Therefore the wording of 8.3 in the
> present Code: "...the type MAY be an illustration." is for me just
> right--permissive, not obligatory. I also note in my MS "...typification by
> illustration of plant microfossils SHOULD BE PERMITTED...."

> I must confess that if we can get this into the Code I anticipate that
> typification by illustration would be seen as making so much sense that it
> would be the norm. It may be that some palynologist of the future will
> invent an easy technique for making single grain preparations in a truly
> permanent mounting medium (i. e., one that doesn't now exist), and that
> there could be at some time in the future a truly international clearing
> house for the loan of such specimens (I am horrified to contemplate for the
> first time, from your letter, that a "specimen" is for us usually a slide
> with 5,000 "Exemplare" of fossil spores, perhaps representing 75 different
> taxa). I do not want to make it illegitimate to designate an actual
> Exemplar. I just want to make it possible to do the much more sensible
> thing (under present conditions, which I do not expect will change any time
> soon) of designating illustrations as types.

> I state all of the above, just in case it is not obvious from my MS.

My feeling is that, when one reads your text, the above message comes through clearly enough. At least, it is what I understood. And if it is what micropalaeontologists want, so be it.

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The specimen definition being a matter of its own (though by no means an unimportant one) it is still my advice to steer at large of it in the present context. There is a special committee working on it, and they will (or should) be interested in how your discipline feels about it. Dan being the convener of that committee, he will doubtless pass on the message. If you agree to just avoid the term in your proposal (in the way I had suggested by annotating, or in some similar way), this would take care of the problem for the time being.

> Perhaps it is worth saying also that I find acceptance of the idea very
> strong among people such as Hartkopf-Froeder and Jansonius, who, like me,
> have been faced with the necessity of trying to track down types, and
> opposition comes from (some very famous) persons for whom nomenclature is a
> formal game, who philosophically like the idea of having a physical type,
> even if it is admittedly an inacessible will-o'-the-wisp.
>
> Now, to your points:
>
> 1. Your para. 2. How will what I propose "impinge...on the validity of
> published names."? That I don't understand at all. If a name needs a
> lectotype or even a neotype , the validity of its publication is not thereby
> challenged, is it?

Post-1957 names require no lectotype since their validity depends on designation of the (holo)type upon publication. In some cases an illustration has been designated as the type of a taxon of extant plants, although it was not technically impossible to preserve a specimen. Such names are (and have usually, at least in higher plants, been treated as) not validly published under the requirements of Art. 37. Addition of the words "not practical" in Art. 8.3 would therefore open a can of worms by making many presently invalid names arguably valid.

> 2. 8.3 vs. 8.4. This suggestion of yours is probably very helpful, if I
> can modify it some. As I originally wrote my proposal, it dealt ONLY with
> 8.3, as this seemed to me to be the article that most urgently needed
> alteration. This is the article that most clearly permits typification by
> illustration, but it specifically excludes fossil plants. If I follow your
> suggestion to "...leave Art. 8.3 alone." what do I do about this situation?
> How about my proposing to delete "non-fossil", and that alone, in 8.3? It
> clearly is never impossible to preserve megafossil plant specimens
> anyway--the rock of ages! But if I can't at least get that "non-fossil"
> wording out of there it seems to me that it guts what I want to accomplish.

Why would this gut your idea? At present, for the purpose of what a

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type may be, Art. 8.3 deals with non-fossil plants and Art. 8.4 with the fossil ones. Since your problem is with fossils exclusively, what cane there be more logical than to deal with it in the 8.4 context, leaving the (spiny!) questions implicit in 8.3 to the "non-fossil botanists" to deal with?

> 3. I am not aware of any names that would be retroactively validated, but I
> would like to run that by Jansonius--I think everybody has always designated
> specimens, not illustrations. The exceptions are cases where no type at all
> was designated. Would it be your view that if the author in such a case
> provided an illustration without designating it as anything other than an
> illustration, that my change would retroactively validate that name because
> the illustration accidentally typified it? My view of designating a type is
> that it requires overt action. The word type must be used.

If no one in your field, after 1957, wrote "Type: Fig. XY", you have no problem. If someone did and his/her name was nevertheless accepted as if it were valid, again you have no problem. The only possible destabilization (assuming that what you propose is indeed limited to the special case of microfossils) would result if names published with a stated "iconotype" had on that account been dismissed as not being validly published. If Jansonius can have a quick check and reassure the Section (and the "Fossil Committee") that no negative consequences are to be feared (or that they are outweighed by the positive ones), this will greatly enhance the chances of your proposal to pass.

> 4. As noted above, I am fascinated to discover that "specimen" for us
> really means the slide, not the spore/pollen Exemplar. That is a really
> strong argument for my position that, if we must have physical types, not
> illustrations, we should require that they be single grain preparations.

> 5. I am not 100% sure that I understand your reference in your para.8 to
> Art. 8.1. "...type may consist of more than one individual....." Even the
> preparation of a single grain mount is enough of a challenge. I doubt that
> any palynologist will contemplate the tour de force of making a slide that
> contains, say, ten specimens of a certain taxon. Won't happen. What we
> normally have is a slide with hundreds or thousands of "specimens",
> typically representing dozens of taxa. ONE of the microfossils is
> designated a holotype for a name. Often a single slide will have three or
> four microfossils designated as types for different taxa.

> Furthermore, in your suggestion as to how I could propose to modify 8.4 you
> write: "type...of a plant microfossil may be either a specimen or an
> illustration." "Specimen" in such a context makes no sense unless it refers
> to the microfossil Exemplar!

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Perhaps I should explain. Even a flowering plant type may have mould attached to it that belongs to a quite different species, or perhaps be affected by Cuscuta. There may be some circular reasoning in this, but what Art. 8.1 MEANS (and is thereby supported by 9.9) is that only those individuals or individual fragments belonging to the taxon described belong to the type proper. Diatom specialist among others must, I suspect, have the same kind of situation as micropalaeontologists. I gather that they try (more or less successfully, and facing the same kind of difficulties as you do) to locate an individual on a "type slide" to pin down their type concept more accurately -- but they are not forced to do so.

But the crux of the problem is not there. Even now botanists are free to do exactly what you feel should be done. However, if you make this mandatory then in many cases (perhaps mostly or exclusively outside micropalaeontology, at least since Rob Fensome has forced his "purist" approach upon that field) post-1957 names that are generally accepted as valid but for which a (numbered or otherwise identified) slide was stated to be the type would turn out to be invalid and in need of validation (again as by Art. 37).

As I said, this problem (which I accept to be a real one) extends much beyond what you are proposing in your text. So again my advice: keep it dissociated. If you can think of a good solution to your question that does not affect stability, propose it separately (and either limit what you propose to the group you are working in, or get in touch with people working in other groups that may be affected).

What I would like to see in the Code is splitting of the Code's type concept into two categories, say, an "assemblage type" and an "atomic type", when mention of the assemblage type is sufficient for purposes of Art. 37 (at least until some date in the future that might have to vary depending on the group concerned) but within it an atomic type may be designated by a (second-level) lectotypification if required.

> 6. Your para. 9: can you think of a way out of the "specimen" dilemma?
> Another word? I must say, your usage doesn't agree well with English
> dictionaries. For example, current Webster's Collegiate: "specimen...an
> item or part typical of a group or whole...." I have scurried to my Roget's
> without much luck. We need a word to designate the actual individual which
> we designate. Perhaps "individual microfossil?" I will have enough trouble
> with this proposal without crossing you and others on "specimen," however!
> I will reluctantly avoid the word somehow. But afterwards we
> paleopalynologists will all slip because it makes so much sense.

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>
> I will have a go at changing my actual proposal along the lines you suggest,
> when I hear from you what you think about deleting "non-fossil" from 8.3. I
> will be very interested in what you think of my new proposal and will e-mail
> it to you. The changes in the proposal-proper will require some rewriting
> in the supporting paper, but not a lot of it.

I'll be glad to look at your re-worded proposal and let you have my opinion (but copy anything you write to Dan as well). As to other editing minutiae (including the question of whether and how to avoid the "specimen" discussion), I'd suggest that you look at the annotated (by me) ms. copy that Dan will let you have, together with his own comments, and make the best of it.

> Alfred Traverse
> Palynological Laboratories
> Department of Geosciences
> 435 Deike Building, PSU
> University Park, PA 16802
> Ph.: 814-863-3419
> Fax: 814-863-7823

Best regards, Werner
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Hunt Institute for Botanical Documentation

To: wg@zedat.fu-berlin.de
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: proposal
Cc:
Bcc:
X-Attachments:

Dear Werner:

Your e-mail of 13 March is very helpful, but will take some digestion.

Let me state very concisely what I want to accomplish. It is very obvious that AVAILABLE types for practical purposes mostly don't exist for paleopalynology. To deal with this situation, I advocate accepting illustrations as types. It is not my intention to threaten the status of ANY names in the literature, but to provide a technique for designating lectotypes, neotypes and epitypes from among existing illustrations, as the need is perceived, and to encourage palynologists henceforth to designate illustrations as types of new names. Therefore the wording of 8.3 in the present Code: "...the type MAY be an illustration." is for me just right--permissive, not obligatory. I also note in my MS "...typification by illustration of plant microfossils SHOULD BE PERMITTED...."

I must confess that if we can get this into the Code I anticipate that typification by illustration would be seen as making so much sense that it would be the norm. It may be that some palynologist of the future will invent an easy technique for making single grain preparations in a truly permanent mounting medium (i. e., one that doesn't now exist), and that there could be at some time in the future a truly international clearing house for the loan of such specimens (I am horrified to contemplate for the first time, from your letter, that a "specimen" is for us usually a slide with 5,000 "Exemplare" of fossil spores, perhaps representing 75 different taxa). I do not want to make it illegitimate to designate an actual Exemplar. I just want to make it possible to do the much more sensible thing (under present conditions, which I do not expect will change any time soon) of designating illustrations as types.

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I will have a go at changing my actual proposal along the lines you suggest, when I hear from you what you think about deleting "non-fossil" from 8.3. I will be very interested in what you

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think of my new proposal and will e-mail it to you. The changes in the proposal-proper will require some rewriting in the supporting paper, but not a lot of it.

Thanks for your kindness.

Al

Hunt Institute for Botanical Documentation

Comments: Authenticated sender is <WG@mail.zedat.fu-berlin.de>
From: "PROF. DR. W. GREUTER" <wg@zedat.fu-berlin.de>
To: traverse@ems.psu.edu
Date: Fri, 13 Mar 1998 14:43:15 +0000
Subject: microfossil proposal
CC: nicolson.dan@nmnh.si.edu
Priority: normal
X-MIME-Autoconverted: from Quoted-printable to 8bit by pangaea.ems.psu.edu id IAA21500

Dear Al,

I have started pre-editing your proposal(s) to amend Arrt. 8.3-4; before sending the draft on to Dan (who may have qualms of his own, but has the reputation of being a much kinder editor than I am), I would like to raise a number of essential points directly with you.

The first batch of questions relates to what you are concretely proposing. It is obvious from your comments that you realise that your proposal will concern two quite distinct aspects: it will impinge (retroactively so!) on the validity of published names, and it will widen the option of lecto-/neotypification of published names. These two aspects, and their consequences, are not fully considered in their (partly divergent) impact, it seems to me.

Dismembering what you propose into its elements (as I will have to do editorially if they stay, since its parts have very different impact), I can only say that the proposed addition of "or not practical" in Art. 8.3 would concern all plants, fossil and non-fossil alike, and would retroactively affect the validity of many names. No one has tried to assess whether this would be for the better or worse (the answer will presumably differ according to the plant group concerned), but as Rapporteur I would have to recommend strongly against such a change in view of its uncertain and probably far-reaching consequences.

To make this perfectly clear: you may propose whatever you wish, and it will be published; however, if you want to succeed it may be useful to take my advice into consideration. In the present case, the advice is: leave Art. 8.3 alone. It is a can of worms anyway, and there may be proposals from "non-fossil botanists" to amend it. Bringing in fossils can only exacerbate the present problems, and is definitely not a good idea.

My advice: stick to Art. 8.4, to which the fossils are presently confined; (1) in line 1, after "fossil plants", add "(microfossils excepted)"; (2) add a §, to read: "8.4bis. The type of a name of a

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plant microfossil may be either a specimen or an illustration." This is straightforward and simple, and as far as I can see completely covers your point. It will retroactively validate those microfossil names for which, after 1957, the type has been cited as being an illustration; this, however, is a calculated risk, and you are presumably aware of the consequences. You might as well mention whether (and to what extent) cases exist in which names are at presently technically invalid but would become valid by acceptance of what you propose; and whether such names have up till now, as a rule, been accepted as if they were valid or, rather, dismissed as being invalid.

The second question is not directly relevant to the issue at stake but reflects on the wording used throughout your rationale. I am aware of the fact that micropalaeontologists use the term "specimen" in a sense that is not consistent with the letter and spirit of the Code, i.e., for the individual microfossil rather than for the preparation (slide) as a whole. The dual meaning of "specimen" is not a problem limited to your discipline but pervades the nomenclatural discussion in many groups, and can even claim its roots in the Code itself which is ambiguous in its wording.

In the nomenclature of non-fossil plants, interpreting "specimen" in the sense of the curatorial unit, not of the individual plant or plant portion, is an essential pre-requisite for stability. Taxon has therefore followed a consistent policy of not allowing use of the term in the second sense. In the pre-edited draft I am forwarding to Dan I will suggest changing "specimen" throughout your text to either "microfossil", or "type", or "holotype", or "specimen type" -- as appropriate.

I do not know whether you have strong feelings on the terminology question, although I expect that, in matters of principle, you and other micropalaeontologists are not prepared to admit the option offered by the last portion of Art. 8.1 in their domain (your whole argumentation shows that you do not consider this as an even theoretical possibility).

If you feel strongly about wanting to use the term "specimen" in your sense, you may have it, but only on the condition that you insert a paragraph explaining the situation and defining your use of it. Personally, and since the issue is not central to what you propose, I feel that to avoid using the term is the better solution.Pressing that side-issue would rather weaken your argument.

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In parallel with a copy of this message, I am sending the annotated version of your text to Dan, and take it that you will sort out details directly with him. Feel free, however, to let me see copies of you exchange and, if appropriate, elicit my comments on controversial or unclear matters.

With best regards,

Werner Greuter

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27 April, 1998

Dr. Werner Greuter
Botanischer Garten und Botanisches Museum
Königin-Luise-Straße 6-8
D-14191 Berlin, Deutschland

Dear Werner:

I have been taken up lately with the illustrations-as-types-for-microfossils proposal and by the very late discovery a couple of weeks ago that I am a member of the Orthography Committee, and the subsequent necessity for me to catch up with their work. (I still don't understand why other members of that Committee are convinced that the Tokyo 60.11 and its linkage to Rec. 60C.1 are more restrictive and compelling of "corrections" than was the Sydney 73.10 and its corresponding linkage--to me they seem identical in effect, and both have to do only with terminations. But others are trying to convince me that epithets such as backhousii now have to be corrected to backhousei because of the Tokyo Code, whereas they could remain under the Sydney Code.

However, I am clearing my desk this a. m., and I come across the printout of the attached file from you regarding your "parataxon" ideas. I have a couple of comments for your consideration.

First, the prefix "para-". This prefix in English has a slightly deprecatory or even pejorative cast. Thus, a "paralegal" is a person who is able to do some of the work of a lawyer, but is not a licensed or "real" lawyer and is therefore restricted in his/her functions. A "paramedical" or "paramedic" has a similar position in a physician's office. In both cases the connotation is that the "para-" is of lesser, subordinate standing. Parapsychology has to do with things such as extrasensory perception--i. e., not "real" psychology. I have several papers in which I use the term "parapalynology" in a semi-humorous fashion--palynology done with very poorly preserved and very sparse palynomorphs. On the other hand, some students of fossil dinoflagellate cysts use the term "paratabulation" for the tabulation pattern found in the fossil cysts, corresponding to the tabulation (real tabulation?) characteristic of the thecal stage of extant, living dinoflagellates. They also speak of paraplates, parasutures, etc. Here the meaning is not exactly pejorative, but definitely implies supplementary or secondary. None of this means that you should necessarily change the term, though I am bothered by the implication of "not quite up to par."

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I have studied a thesaurus and wonder if you would consider "analog (=analogue) taxa?"

I also am not happy about the linkage of the paleobotanical form taxa concepts with the form taxa ideas associated with fungi having a pleomorphic life cycle. In fungi there is a biological basis for the separate morphs or phases, whereas in paleobotany the situation has to do with accidents of preservation and the fragmentary nature of practically all plant fossils. The cases are so different that the lumping of them together seems unsatisfactory to me, though you seem enamored of it.

With fossil plants, the separate form taxa names can be used even when different parts with different form taxa names are found connected. There is no equivalent in fossil plants for the fungal teleomorph and anamorph, and the holomorph is represented in paleobotany only in an analogous way with the informally named "whole plants." The naming of these whole plants are and should remain informal in paleobotany.

Thus, I do not see it as helpful to link the two situations together other than by a cross reference. Your good idea of "para-taxa" requires for fossil plants a completely new article in my opinion, not a joint one with fungi (which are not plants and may one day have their own code). And why not? When I first got into the nomenclatural sessions in the 50s, fossil plants had a whole separate annex in the Code. (Getting rid of it was one of my principal contributions to nomenclature, but there are those out there who think I did a bad thing.)

Enough for now. Thanks for reading.

Yours very truly,

Alfred Traverse
Professor Emeritus of Palynology

cc:R.Fensome, J.Skog

Hunt Institute for Botanical Documentation

Alfred Traverse
Embassy Park 509
16416 US.19.N
Clearwater, FL 33764
USA

e-mail: bettyandal@juno.com

12 Feb., 1998

Dr. Werner Greuter
Taxon
Botanischer Garten und Botanisches Museum
Königin Luise Straße
D-14191 Berlin, Germany

Dear Werner:

Here is the final version of the proposal manuscript, as described in our earlier correspondence.

I am not sure whether this should not have gone directly to Nicolson, but Judy Skog, my successor as secretary of the fossil group, assures me that it should go to you, so here it is.

All the best from sunny Florida, which we are leaving for home in about two weeks.

Very truly yours,

Alfred Traverse
Professor Emeritus of Palynology

enclosure: MS and two copies

Hunt Institute for Botanical Documentation

To: wg@zedat.fu-berlin.de
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: prop.foss
Cc:
Bcc:
X-Attachments:

Dear Werner:

Actually, the whole effort to recover that MS was very educational. Night before last I regaled an old friend, a geophysicist, and his wife, at dinner about my computer adventure of the day. The guy took it as a challenge and worked some yesterday, using my and two other computers, and some more today, finally succeeding in converting the MS to English in Word Perfect, but he could only print it by printing out the pages one at a time with "print screen." Print commands in Word Perfect refuse to work.

I am taking all of this stuff to Florida with me when we leave a week from yesterday and will be back in touch sometime in January. All our mail and e-mail is being forwarded.

It was saddening to hear about Stafleu, though I never got to know him well. He once got me into an awful mess. When the generic index project was nearly complete, I mean a matter of weeks away from completion, somebody startled him by telling him that there were generic names for fossil spores, pollen, etc. He contacted me, and I agreed to drop everything and work like mad for a month to get perhaps 85% of what there then was into the system, but it was not a job I could be proud of, and I am still catching flak from people who run into some of my decisions and find out that I was responsible. I should have refused to do it. Afterwards Frans was always rather cool toward me, and I found that hard to accept--it seemed to me to be a case like a rape, where the victim is blamed for wearing a short skirt and too much makeup.

All the best. Al.

Hunt Institute for Botanical Documentation

To: wg@zedat.fu-berlin.de
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: fossil proposals
Cc:
Bcc:
X-Attachments:

Dear Werner:

Yesterday I got two e-mail messages from you.

The first started with a one-page message from you under subject: "fossil proposals," beginning, "the following draft set of proposals was...." Then followed a five-page "(000-000) Proposals regarding the nomenclature of fossil plants (Draft, 14.12.97)

The rules governing the nomenclature....."

The second communication from you also had the subject "fossil proposals" and started "Content-type: text/plain; charset-US-ASCII....This message contains a file prepared for transmission using the MIME BASE64 transfer encoding scheme...please ask your system administrator for help...."

The said administrators and I have just misspent an hour at this computer trying to get this attachment to work for us, but we have all failed. It could be that the problem has to do with the fact that we do not have MS Word in the computer (only Word Perfect). We can get the pages of encoded stuff, but that is all.

Is there a chance that these two MSS are really the same thing anyway?

If not, I would like to see the whole thing and will indeed send you my comments.

By the way, with the counsel of Jansonius and Fensome I am putting together a proposal to amend the Code vis-a-vis typification for microfossils and will soon submit same to Taxon.

Glückliche Feiertage und einen guten Rutsch ins Neue Jahr wünsche ich Ihnen

Ihr Alfred Traverse.

Hunt Institute for Botanical Documentation

Comments: Authenticated sender is <WG@mail.zedat.fu-berlin.de>
From: "PROF. DR. W. GREUTER" <wg@zedat.fu-berlin.de>
To: traverse@ems.psu.edu (Alfred Traverse)
Date: Tue, 16 Dec 1997 17:49:23 +0000
Subject: Re: fossil proposals
Priority: normal

Dear Al,

sorry you misspent a whole hour trying to read the separate message (MIME-encoded attachment). I should have put my warning, that this is not really necessary, higher up in my introductory letter to the (non-encoded) text. You'll find that warning in the last-but-one letter paragraph.

So, you don't miss anything except the nice formatting when you use the message that you can read. It's all there.

The problem may be that you use a WordPerfect version that is not newest brand and cannot yet convert Word 6.0. Times change so rapidly! To me, 6.0 is already the "old" version that most people can cope with, whereas the version I now use, Word97, is known to cause problems.

I look forward to receiving your proposal on microfossil typification. From the little I know, I should expect that it is completely independent from the proposal in my message. When drafting the latter, I searched in vain your earlier letter in which you convinced me of the spuriousness (for nomenclatural purposes at least) of the concept of "whole organism biological taxa" in palaeobotany. You had put this into much nicer and more convincing words than I could have done. Perhaps you could contribute some sentences along these lines to our (proposedly joint) text -- that is, if you can get yourself to like it.

Best regards, Werner

PS.: I have to add sad news: Frans Stafleu passed away last night. Botany and nomenclature have lost a great man, and I, a very good friend.

Professor Werner Greuter
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http://www.bgbm.fu-berlin.de/bgbm/staff/wiss/Greuter+W/default.htm

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(Draft, 18.12.97)

The rules governing the nomenclature of fossil plants have long been unsatisfactory in many respects. Partly due to semantic disagreement, partly owing to the problems inherent in the fragmentary nature of fossils in general, and partly perhaps as a consequence of different traditions developing in various branches of palaeobotany, it has so far been impossible to produce a set of provisions that would reconcile past and current nomenclatural practices of palaeobotanists with the tenets of the nomenclatural *Code*. Recently, Fensome & Skog (in Taxon 46: 557-562. 1997) have produced two proposal that aim at rectifying this situation (but see comments below). In parallel, the botanical representatives in the International Committee of Bionomenclature tried to reformulate the rules on plant fossil nomenclature in the frame of the successive *BioCode* drafts. The results of their efforts, in which they were encouraged by expert feedback from several palaeobotanists, is of course of relevance primarily for the botanical *Code* rather than for the *BioCode* (the zoological rules, surprisingly, do not single out animal fossils by giving them any kind of special treatment). It is therefore logical that we present our conclusions here, in a purely botanical context.

We completely agree with Fensome & Skog (l.c.) in the inappropriateness of the present definition of "form-genera" in Art. 3.3, which introduced an inappropriate and unnecessary element of taxonomic judgement into fossil nomenclature. Deletion of Art. 3.3, as proposed by Fensome & Skog, is also a part of our own set of proposals. We must however comment on some of the implications of their second proposal, Prop. (05), with which we partly disagree.

- Prop. (05) would threat the definition of "form-genera" and the ways in which their names compete with other names under a single paragraph, within Art. 11. In spite of obvious similarities, they would thus become dissociated from the provisions dealing with other form-taxa (of fungi with a pleomorphic life cycle), when to our mind that link should, on the contrary, be strengthened. What underlies the present concept of "form-taxa" in the *Code* is not related to the way in which priority operates between competing names (which is what Art. 11 is about). "Form-taxa" as intrinsically understood under the *Code* differ in essence from "normal" biological taxa, and from other "form-taxa" of a different quality as well. The fact that the names of "form-taxa" and "normal" taxa, or names of qualitatively different "form-taxa", do not compete with each other has nothing to do with (limitation of) priority, but is simply due to the fact that they do not, by definition (and irrespective of the known or inferred biological context), represent the same taxon. We therefore believe that the best placement for the definition of "form-taxa" is right at the beginning of the *Code*, in Art. 1, where the notion of taxon is defined. The special provisions for the nomenclature of form taxa are appropriately placed in a single article, Art. 59, which already deals with the names of fungal "form-taxa".

- We find the term "form-taxa" (or "form-genera") less than ideal, both semantically and in view of its divergent usage in the past. While agreeing in general terms with Fensome & Skog (l.c.) when they use a new concept of "form-genera" that "equate with both form-genera and organ-genera of previous usage", we find it preferable to introduce a new term for that concept. We are here proposing "parataxa", which we found to be both euphonious and linguistically appropriate, so that it may hopefully gain general acceptance.

- We have theoretical and practical problems with limiting the concept of fossil parataxa to the single rank of genus, as Fensome & Skog (l.c.) propose. We are unconvinced by the reasons they give for that limitation. When the type of a name of a genus is the type of a name of one of its species, we cannot see how the genus can be viewed as a parataxon if the species is not. The parataxon concept is intrinsically rank-independent, even though there is no conceptual difficulty with placing fossil (or fungal) parataxa in a "normal" taxon of higher rank, nor any rule in the *Code* against doing so.

- Proposal (05), contrary to our own approach, provides no clear definition of fossil "form-genera". The implication is that "form-genera" are those to which fossil specimens can be assigned. This is no definition, however, since fossil specimens can also be assigned to extant plant genera which, of course, are no "form-genera". What is lacking is a clear statement that non-algal [or perhaps better, non-diatom; see below] fossil taxa (those whose name is based on a fossil type) are by necessity parataxa. While it is legitimate (and often necessary) to think of plants of the past as belonging to "normal" biological taxa, such taxa will always remain conjectural due to the fragmentary nature of the material at hand. There is nothing wrong in the informal use of scientific names for a theoretical "whole-taxon" concept as long as no ambiguity is thereby introduced, it being nevertheless clear that in a formal context that same name designates a parataxon of the category to which its type belongs. Even though the name *Sigillaria* Brongn. pertains to a parataxon of bark fragments, one may justifiably speak of *Sigillaria* woods when referring to the stands of trees from which these fragments are believed to stem, and when one forms the notion of an evolutionary clade to which these trees belong one may refer to it as *Sigillariaceae* even though that name, too, formally pertains to a bark-fragment parafamily.

- One aspect is left purposely vague, both in Prop. (05) and in our own proposals: the definition of the limits of parataxon categories. This question must be left to the practising palaeobotanist to answer on a case-by-case basis, bearing in mind the intent of the original author and, perhaps most importantly, the tradition established in each concrete case. There may be names referring only to a certain preservation state (petrifaction or compression) of leaf fossils, but others that are applied to leaf fossils irrespective of their preservation state, and the difference may not always be evident from the type material alone. There is no sensible way, and probably no need, to regulate on exact parataxon categories. Flexibility is required, and is being provided.

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Our ideas have been incorporated in a set of four proposals, of which (1) addresses an independent issue. Prop. (2) and (3) are closely correlated, and (4) presents an alternative to both but can stand independently (in which case it will only affect Art. 11.7).

(1) *Substitute the term* "parataxa" (*singular:* "parataxon") *for* "form-taxa" *(of pleomorphic fungi) and* "form-genera" *(of fossil plants), throughout the* Code.

Note. – The following proposals do not in substance depend on this change in terminology, although their present wording assumes that proposal (1) is carried. The terms to be replaced appear in the *Tokyo Code* in Art. 3.3-4 plus Note 1 and Ex. 1-2 (but see Prop. 2), Art. 7.9, Art. 11.1, Art. 25.1, Art. 59.3 & 5 and Ex. 3-4.

(2) *Delete Art. 3.3, 3.4, Art. 3 Note 1, both examples, and the qualifying clause in the second sentence of Art. 3.1; and add a paragraph to Art. 1, as follows:*

"*1.2.* Taxa normally consist of whole organisms in all their life stages, irrespective of the nature of the corresponding name-bearing types. In some categories of plants, taxa that correspond only to parts of organisms or to definite stages of their life history may , for practical reasons, be recognised and named. Such taxa are termed parataxa. This *Code* provides, in Art. 59, for names of parataxa in the following categories: fossil non-algal taxa, and some fungi with a pleomorphic life history."

Note. – This proposal includes Prop. (04) by Fensome & Skog (l.c.: 557) to delete Art. 3.3 and the correlated clause in Art. 3.1. It suggests to the Editorial Committee to treat parataxa of fungi and fossil plants in parallel, within Art. 59, rather than dealing with the latter separately under Art. 11, as favoured by Fensome & Skog (l.c.) in their Prop. (05). The latter question is, however, editorial and does not affect the substance of our proposal.

(3) *Change the title of Division II, Chapter VI, to read* "Names of parataxa", *and add the following two paragraphs and two Notes at the beginning of Art. 59:*

"*59.0.* Except as specified below, names based on any part of an organism or portion of its life history are treated as applicable to the whole organism and compete for priority as provided for in Art. 11.

"*59.0bis.* Fossil non-algal taxa are parataxa (see Art. 1.2), which for nomenclatural purposes comprise only those parts, life-history stages, or preservation states of organisms that are represented by the corresponding nomenclatural types, and whose names for purposes of priority compete only with names based on a fossil type representing that same part, life-history stage, or preservation state.

"*Note 0.* This provision does not prevent assigning fossil parataxa of a rank lower than genus to a non-fossil genus.

"*Note 0bis.* This provision does not prevent the informal use of names of fossil parataxa to refer to the theoretical concept of whole extinct taxa."

Note. – If this proposal is adopted, Art. 11.7 can be editorially changed into a Note, with cross-reference to Art. 59.0bis, and might be reworded as follows: "Names of non-algal taxa based on a fossil type do not compete for priority with names of correlated taxa based on a non-fossil type".

(4) *Make the same changes and additions as by Prop. (3), but in Art. 59.0bis, instead of* "Fossil non-algal taxa", *write* "Fossil taxa, excepting diatoms *(Bacillariophyceae)*,"; *make parallel changes in the proposed Art. 1.2 and in the Note corresponding to present Art. 11.7 (see above).*

Note. – This proposal is made with a view of disallowing names of fossil algae to displace junior names believed to belong to the same biological taxon. Fensome & Skog (l.c.: 559-560) point out that the current situation is destabilising in, e.g., the *Dinophyceae,* where fossil and living taxa "have largely received two sets of names, the equivalencies of which are becoming increasingly well known". They discuss the example of *Spiniferites* (fossil) and *Gonyaulax* (extant). A parallel case, with a similarly destabilising result, was brought up by Boalch & Guy-Ohlson (in Taxon 41: 529-531. 1992) who convincingly argued that fossil *Tasmanites* and extant *Pachysphaera (Prasinophyceae)* may be regarded as congeneric, the former having priority. *Tasmanites* was originally described from Permian rocks, and its species span the earth's whole history, from Precambrian to Pliocene (Fensome & al. in Contr. Ser. Amer. Assoc. Stratigr. Palynologists 25: 471-476. 1990). Apparently, the one group in which fossil and extant forms have traditionally been treated in a common context, and in which both generic and specific names based on fossil types are in widespread use for extant genera and species, are the diatoms. The *Bacillariophyceae* provided the only examples (10 in all) of generic names based on fossil types that are in current use for extant taxa (Greuter & al. in Regnum Veg. 129: xvi. 1993). Subject to positive advice from the Committee for Algae, narrowing the algal exception to the sole diatoms would therefore appear to be a positive step.

[Acknowledgements to follow.]

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(000-000) Proposals regarding the nomenclature of fossil plants

(Draft, 18.12.97)

The rules governing the nomenclature of fossil plants have long been unsatisfactory in many respects. Partly due to semantic disagreement, partly owing to the problems inherent in the fragmentary nature of fossils in general, and partly perhaps as a consequence of different traditions developing in various branches of palaeobotany, it has so far been impossible to produce a set of provisions that would reconcile past and current nomenclatural practices of palaeobotanists with the tenets of the nomenclatural *Code*. Recently, Fensome & Skog (in Taxon 46: 557-562. 1997) have produced two proposal that aim at rectifying this situation (but see comments below). In parallel, the botanical representatives in the International Committee of Bionomenclature tried to reformulate the rules on plant fossil nomenclature in the frame of the successive *BioCode* drafts. The results of their efforts, in which they were encouraged by expert feedback from several palaeobotanists, is of course of relevance primarily for the botanical *Code* rather than for the *BioCode* (the zoological rules, surprisingly, do not single out animal fossils by giving them any kind of special treatment). It is therefore logical that we present our conclusions here, in a purely botanical context.

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(4) *Make the same changes and additions as by Prop. (3), but in Art. 59.0bis, instead of* "Fossil non-algal taxa", *write* "Fossil taxa, excepting diatoms *(Bacillariophyceae),*"; *make parallel changes in the proposed Art. 1.2 and in the Note corresponding to present Art. 11.7 (see above).*

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[Acknowledgements to follow.]

Hunt Institute for Botanical Documentation

Comments: Authenticated sender is <WG@mail.zedat.fu-berlin.de>
From: "PROF. DR. W. GREUTER" <wg@zedat.fu-berlin.de>
To: w.chaloner@rhbnc.ac.uk, d.hawksworth@cabi.org, johnm@rom.on.ca,
nicolson.dan@nmnh.si.edu, silva@ucjeps.herb.berkeley.edu,
traverse@ems.psu.edu, piers@indhort.demon.co.uk
Date: Sun, 14 Dec 1997 20:23:01 +0000
Subject: fossil proposals
Return-receipt-to: "PROF. DR. W. GREUTER" <wg@zedat.fu-berlin.de>
Priority: normal

Dear friends,

the following draft set of proposals was foreshadowed in earlier correspondence and has been taken into account in the current version of the Draft BioCode. It is intended for publication in Taxon, presumably in the May issue.

I would like to ask for your critical comments and suggestions, and also, additionally, whether I may assume that you are prepared to endorse and co-author these proposals (subject of course to your advice being followed).

Not only do I hope that my approximate handling of the English (British) language will be improved by your contribution. It is quite clear that, being neither a palaeobotanist nor a phycologist, I need the advice of the specialists among you on the appropriateness of (a) the proposals themselves and (b) the arguments presented in their support. Of course they all do rest on what I had as feedback from you in the past, but whether I interpreted your comments correctly and drew the right conclusions from them remains to be seen.

Since I do not know how well your e-mail system copes with the kind of formatted attachments I use to send, I will first of all append the whole text directly hereunder, as an unformatted ASCII file. If you can crack the attachment, you will find it formatted in Word 6.0 which is usually converted without problems to other up-to-date text editing systems. If you cannot, don't despair and use the unformatted version instead.

May I hope to hear from all of you by Xmas? or by the end of the year? This would be great!

With season's greetings, yours

Werner

Hunt Institute for Botanical Documentation

(000-000) Proposals regarding the nomenclature of fossil plants

(Draft, 14.12.97)

The rules governing the nomenclature of fossil plants have long been unsatisfactory in many respects. Partly due to semantic disagreement, partly owing to the problems inherent in the fragmentary nature of fossils in general, and partly perhaps as a consequence of different traditions developing in various branches of palaeobotany, it has so far been impossible to produce a set of provisions that would reconcile past and current nomenclatural practices of palaeobotanists with the tenets of the nomenclatural Code. Recently, Fensome & Skog (in Taxon 46: 557-562. 1997) have produced two proposal that aim at rectifying this situation (but see comments below). In parallel, the botanical representatives in the International Committee of Bionomenclature tried to reformulate the rules on plant fossil nomenclature in the frame of the successive BioCode drafts. The results of their efforts, in which they were encouraged by expert feedback from several palaeobotanists, is of course of relevance primarily for the botanical Code rather than for the BioCode (the zoological rules, surprisingly, do not single out animal fossils by giving them any kind of special treatment). It is therefore logical that we present our conclusions here, in a purely botanical context.

We completely agree with Fensome & Skog (l.c.) in the inappropriateness of the present definition of "form-genera" in Art. 3.3, which introduced an inappropriate and unnecessary element of taxonomic judgement into fossil nomenclature. Deletion of Art. 3.3, as proposed by Fensome & Skog, is also a part of our own set of proposals. We must however comment on some of the implications of their second proposal, Prop. (05), with which we partly disagree.

- Prop. (05) would threat the definition of "form-genera" and the ways in which their names compete with other names under a single paragraph, within Art. 11. In spite of obvious similarities, they would thus become dissociated from the provisions dealing with other form-taxa (of fungi with a pleomorphic life cycle), when to our mind that link should, on the contrary, be strengthened. What underlies the present concept of "form-taxa" in the Code is not related to the way in which priority operates between competing names (which is what Art. 11 is about). "Form-taxa" as intrinsically understood under the Code differ in essence from "normal" biological taxa, and from other "form-taxa" of a different quality as well. The fact that the names of "form-taxa" and "normal" taxa, or names of qualitatively different

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"form-taxa", do not compete with each other has nothing to do with (limitation of) priority, but is simply due to the fact that they do not, by definition (and irrespective of the known or inferred biological context), represent the same taxon. We therefore believe that the best placement for the definition of "form-taxa" is right at the beginning of the Code, in Art. 1, where the notion of taxon is defined. The special provisions for the nomenclature of form taxa are appropriately placed in a single article, Art. 59, which already deals with the names of fungal "form-taxa".

- We find the term "form-taxa" (or "form-genera") less than ideal, both semantically and in view of its divergent usage in the past. While agreeing in general terms with Fensome & Skog (l.c.) when they use a new concept of "form-genera" that "equate with both form-genera and organ-genera of previous usage", we find it preferable to introduce a new term for that concept. We are here proposing "parataxa", which we found to be both euphonious and linguistically appropriate, so that it may hopefully gain general acceptance.

- We have theoretical and practical problems with limiting the concept of fossil parataxa to the single rank of genus, as Fensome & Skog (l.c.) propose. We are unconvinced by the reasons they give for that limitation. When the type of a name of a genus is the type of a name of one of its species, we cannot see how the genus can be viewed as a parataxon if the species is not. The parataxon concept is intrinsically rank-independent, even though there is no conceptual difficulty with placing fossil (or fungal) parataxa in a "normal" taxon of higher rank, nor any rule in the Code against doing so.

- Proposal (05), contrary to our own approach, provides no clear definition of fossil "form-genera". The implication is that "form-genera" are those to which fossil specimens can be assigned. This is no definition, however, since fossil specimens can also be assigned to extant plant genera which, of course, are no "form-genera". What is lacking is a clear statement that non-algal [or perhaps better, non-diatom; see below] fossil taxa (those whose name is based on a fossil type) are by necessity parataxa. While it is legitimate (and often necessary) to think of plants of the past as belonging to "normal" biological taxa, such taxa will always remain conjectural due to the fragmentary nature of the material at hand. There is nothing wrong in the informal use of scientific names for a theoretical "whole-taxon" concept as long as no ambiguity is thereby introduced, it being nevertheless clear that in a formal context that same name designates a parataxon of the category to which its type belongs. Even though the name Sigillaria Brongn. pertains to a parataxon of bark fragments, one may justifiably speak of Sigillaria

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woods when referring to the stands of trees from which these fragments are believed to stem, and when one forms the notion of an evolutionary clade to which these trees belong one may refer to it as Sigillariaceae even though that name, too, formally pertains to a bark-fragment parafamily.

- One aspect is left purposely vague, both in Prop. (05) and in our own proposals: the definition of the limits of parataxon categories. This question must be left to the practising palaeobotanist to answer on a case-by-case basis, bearing in mind the intent of the original author and, perhaps most importantly, the tradition established in each concrete case. There may be names referring only to a certain preservation state (petrifaction or compression) of leaf fossils, but others that are applied to leaf fossils irrespective of their preservation state, and the difference may not always be evident from the type material alone. There is no sensible way, and probably no need, to regulate on exact parataxon categories. Flexibility is required, and is being provided.

Our ideas have been incorporated in a set of four proposals, of which (1) addresses an independent issue. Prop. (2) and (3) are closely correlated, and (4) presents an alternative to both but can stand independently (in which case it will only affect Art. 11.7).

(1) Substitute the term "parataxa" (singular: "parataxon") for "form-taxa" (of pleomorphic fungi) and "form-genera" (of fossil plants), throughout the Code.

Note. - The following proposals do not in substance depend on this change in terminology, although their present wording assumes that proposal (1) is carried. The terms to be replaced appear in the Tokyo Code in Art. 3.3-4 plus Note 1 and Ex. 1-2 (but see Prop. 2), Art. 7.9, Art. 11.1, Art. 25.1, Art. 59.3 & 5 and Ex. 3-4.

(2) Delete Art. 3.3, 3.4, Art. 3 Note 1, both examples, and the qualifying clause in the second sentence of Art. 3.1; and add a paragraph to Art. 1, as follows:

"1.2. Taxa normally consist of whole organisms in all their life stages, irrespective of the nature of the corresponding name-bearing types. In some categories of plants, taxa that correspond only to parts of organisms or to definite stages of their life history may , for practical reasons, be recognised and named. Such taxa are termed parataxa. This Code provides, in Art. 59, for names of parataxa in the following categories: fossil non-algal taxa, and some fungi with a pleomorphic life history."

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Note. - This proposal includes Prop. (04) by Fensome & Skog (l.c.: 557) to delete Art. 3.3 and the correlated clause in Art. 3.1. It suggests to the Editorial Committee to treat parataxa of fungi and fossil plants in parallel, within Art. 59, rather than dealing with the latter separately under Art. 11, as favoured by Fensome & Skog (l.c.) in their Prop. (05). The latter question is, however, editorial and does not affect the substance of our proposal.

(3) Change the title of Division II, Chapter VI, to read "Names of parataxa", and add the following two paragraphs and two Notes at the beginning of Art. 59:

"59.0. Except as specified below, names based on any part of an organism or portion of its life history are treated as applicable to the whole organism and compete for priority as provided for in Art. 11.

"59.0bis. Fossil non-algal taxa are parataxa (see Art. 1.2), which for nomenclatural purposes comprise only those parts, life-history stages, or preservation states of organisms that are represented by the corresponding nomenclatural types, and whose names for purposes of priority compete only with names based on a fossil type representing that same part, life-history stage, or preservation state.

"Note 0. This provision does not prevent assigning fossil parataxa of a rank lower than genus to a non-fossil genus.

"Note 0bis. This provision does not prevent the informal use of names of fossil parataxa to refer to the theoretical concept of whole extinct taxa."

Note. - If this proposal is adopted, Art. 11.7 can be editorially changed into a Note, with cross-reference to Art. 59.0bis, and might be reworded as follows: "Names of non-algal taxa based on a fossil type do not compete for priority with names of correlated taxa based on a non-fossil type".

(4) Make the same changes and additions as by Prop. (3), but in Art. 59.0bis, instead of "Fossil non-algal taxa", write "Fossil taxa, excepting diatoms (Bacillariophyceae),"; make parallel changes in the proposed Art. 1.2 and in the Note corresponding to present Art. 11.7 (see above).

Note. - This proposal is made with a view of disallowing names of fossil algae to displace junior names believed to belong to the same

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biological taxon. Fensome & Skog (l.c.: 559-560) point out that the current situation is destabilising in, e.g., the Dinophyceae, where fossil and living taxa "have largely received two sets of names, the equivalencies of which are becoming increasingly well known". They discuss the example of Spiniferites (fossil) and Gonyaulax (extant). A parallel case, with a similarly destabilising result, was brought up by Boalch & Guy-Ohlson (in Taxon 41: 529-531. 1992) who convincingly argued that fossil Tasmanites and extant Pachysphaera (Prasinophyceae) may be regarded as congeneric, the former having priority. Tasmanites was originally described from Permian rocks, and its species span the earth's whole history, from Precambrian to Pliocene (Fensome & al. in Contr. Ser. Amer. Assoc. Stratigr. Palynologists 25: 471-476. 1990). Apparently, the one group in which fossil and extant forms have traditionally been treated in a common context, and in which both generic and specific names based on fossil types are in widespread use for extant genera and species, are the diatoms. The Bacillariophyceae provided the only examples (10 in all) of generic names based on fossil types that are in current use for extant taxa (Greuter & al. in Regnum Veg. 129: xvi. 1993). Subject to positive advice from the Committee for Algae, narrowing the algal exception to the sole diatoms would therefore appear to be a positive step.

[Acknowledgements to follow.]

Professor Werner Greuter
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http://www.bgbm.fu-berlin.de/bgbm/staff/wiss/Greuter+W/default.htm

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Greuter

wg@fub46.zedat.fu-b, 01:39 PM 2/16/97, fossil plants, etc. 1

To: wg@fub46.zedat.fu-berlin.de
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: fossil plants, etc.
Cc: w.chaloner@rhbnc.ac.uk jjansonius@gsc.nrcan.gc.ca fensome@agc.bio.ns.ca
Bcc:
X-Attachments:

Dear Werner:

This has taken too long. We were out of town most of January, and then this had to get in line (in the queue, as WGC would say).

Your urging of the present officers of the CFP to keep in touch had a familiar ring to it. I hope you got on the mailing list for IOP Newsletter, and that you have paid the modest fee for it. Ha.

Re "form taxa," "organ taxa," etc., my position still is that the naming of fossils does not per se demand that all categories of items named be designated as such-and-such category of fossil in the manual for manipulation of names we call ICBN. I do not see this as a necessary part of the naming process, and I think the Code(s) should contain no unnessary material. For example, Bill Chaloner is a strong advocate of "organ-genera," but dinoflagellate cysts are not organs and it is weird to designate them as organs. Should there also be "unicell-genera" to take care of this little untidiness? Spores and pollen are also not "organs"--an organ is a composite of tissues. Do we need to designate them as "monad propagule taxa," "tetrad propagule taxa," etc.?

Re your no. 1. I am in agreement that the Code and coming codes should provide clearly that plant fossil taxa (unlike paleozoological taxa) are not necessarily to be united because of connections discovered between them. The Code should underline that alternate names for different parts of plants continue in use, even after such connections are proven. This is necessary because of mosaic evolution in plants. (However, I think it very strange that Tom Taylor recently published a paper on "Rhabdosporites spores" but in the abstract and text explains that the spores are from sporangia of the plant fossil, Tetraxylopteris. Surely in such a case the article should be entitled, "Spores of Tetraxylopteris..." with a textual explanation that such spores, if found dispersed, would be identified as Rhabdosporites. In fact, such spores are found also in sporangia of several other plants, not all closely related.)

Your no. 2. Whole organism naming in paleobotany should. in my view, remain informal, as it has been for a very long time. The entities so named shouldn't even really be referred to as "taxa." They are concepts. Cordaites is a leaf genus. It's ok to follow a century of convention and informally (subject to NO rules) call the concept of the whole tree/shrub "Cordaites." It won't be helpful to formalize procedures for doing this under rules. The informal approach has worked fine. Why rock the boat? Can anybody cite an example where the informal method hasn't worked o. k.?

Your no. 3. This seems to cover the same ground as no. 2, as to whole-plant names.

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Sigillaria and Lepidodendron are bark names, but it is helpful also to use these names informally in text books, etc., as names for whole plant concepts. When you say, "Speaking of them as whole plants...would be incorrect...." seems to me non-sensical, since I view Sigillaria as a whole plant concept as informal, like Potonie's turmal names for dispersed spore classification, not governed by any rules. Perhaps a better analogy would be so-called "common " or "colloquial" names for plants (which in the circles I frequent are neither common or colloquial). These are not regulated and shouldn't be. It's not "incorrect" to call Trillium erectum "wake robin" instead of "stinking Benjamin"--it's just a different game. I am also against efforts to formalize the naming of whole plant concepts. Much better to leave the matter totally unregulated and subject to "market" dynamics.

I realize I am repeating myself a bit when I say that provisions should make it even more clear than it is in ICBN that -pollenites and -dendron names will continue to be applied (only) to the specified parts, even if they have somewhere been found to be connected. Mosaic evolution of plants dictates this, among other considerations. However, when spores are found in a sporangium attached to a megafossil plant part with well established morphology and anatomy, and named so-and-so, the name applied to the spores, if they had been found dispersed, should not be used except parenthetically in designating them.

Your no. 4. In general, you are right, but there shouldn't be a blanket rule against fossil species of extant genera because in the Pleistocene it is an expectable situation. I think this should be left to taxonomic opinion, with no rules. I know of no case in which this has caused noticeable mischief.
There should be no rules put in place because of philosophical considerations. They should all be pragmatically determined.

Your no. 5. Here I differ from you rather markedly. Means of preservation is a quite legitimate basis for creating a fossil taxon. Artisia is a pith cast of a special type for cordaiteans--from several different genera. The name can and should apply only to these casts, that mode of preservation. The same is true of Knorria, representing a particular sort of preservation of the inside of the same bark, the outside of which would be a Lepidodendron. Knorria is a useful concept for material where one can probably never learn what Lepidodendron it came from.

I also have a copy of your missive of 7 Jan 97, which like the other one, has been fascinating reading. I really should give "eutaxa" and "parataxa" more thought, and indeed, if they would replace organ taxa and form taxa without a trace of the latter, I might think they are a good idea. I have a fear that organ taxa and form taxa would become subsets under parataxa, and we would have a lot more business in the Code muddying the nomenclatural water.

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Thanks for reading. Ich bitte um Entschuldigung. Al.

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Date: Sun, 16 Feb 1997 13:39:01 -0500
From: Mail Delivery Subsystem <MAILER-DAEMON@agc.bio.ns.ca>
Subject: Returned mail: Host unknown
To: <traverse@ems.psu.edu>

Content-Type: text/plain; charset="us-ascii"
Content-Description: Session Transcript

550 gsc.nrcan.gc.ca.fensome (tcp)... Host unknown
550 <w.chaloner@rhbnc.ac.uk.jjansonius@gsc.nrcan.gc.ca.fensome@agc.bio.ns.ca>... Host unknown
Received: from pangaea.ems.psu.edu by agcux with ESMTP
(1.40.112.8/16.2) id AA168118467; Sun, 16 Feb 1997 14:41:07 -0400
Return-Path: <traverse@ems.psu.edu>
Received: from traver.geosc.psu.edu (traver.geosc.psu.edu [128.118.175.109]) by pangaea.ems.psu.edu (8.8.3/8.8-psu-ems) with SMTP id NAA03887; Sun, 16 Feb 1997 13:37:21 -0500 (EST)
Message-Id: <199702161837.NAA03887@pangaea.ems.psu.edu>
X-Sender: traverse@ems.psu.edu (Unverified)
X-Mailer: Windows Eudora Version 1.4.3
Mime-Version: 1.0
Content-Type: text/plain; charset="us-ascii"
Date: Sun, 16 Feb 1997 13:39:01 -0500
To: wg@fub46.zedat.fu-berlin.de
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: fossil plants, etc.
Cc: w.chaloner%rhbnc.ac.uk.jjansonius@gsc.nrcan.gc.ca.fensome

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Alfred Traverse
Palynological Laboratories

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Comments: Authenticated sender is <WG@fub46.zedat.fu-berlin.de>
From: "PROF. DR. W. GREUTER" <wg@fub46.zedat.fu-berlin.de>
To: jskog@gmu.edu, fensome@agc.bio.ns.ca, traverse@ems.psu.edu,
w.chaloner@rhbnc.ac.uk, johnm@rom.on.ca, d.hawksworth@cabi.org
Date: Tue, 7 Jan 1997 18:10:56 +0000
Subject: fossils in the BioCode
Priority: normal

Dear all,

yesterday I had some exchange with Judy (copy to John & Rob) over old questions and reciprocal updating. I think I got quite some new insight into the problems concerned. On the way home (past midnight as usual, and freezing cold it was) I had an idea that I thought might work and make everyone content. I elaborated on it earlier today, and can come up with a completely new possible solution. (I acknowledge having been inspired by Al Traverse's comment reported by Judy in her letter to Dan, September 1995, yet I am far from convinced that he will accept co-paternity!)

You may like it or scrap it, or improve it if it is worth while.
Sorry, Judy, not to have felt able to avoid organ-taxa (it's exactly the term required, is it not?; but as you say, at this stage terminology should be of secondary importance).

The basics of the new concept can be summarized as follows:

Shift 3.2 to 2.2, and make it a definition of "normal" taxa as opposed to "special" taxa, with an additional Note.

Leave 36.2 to fossil plants alone, completely rethought and reworded.

Scrap the present 36.3 (the first sentence is trivial, at most worth a Note if not completely redundant; the remainder, rightly judged inacceptable by Bill, falls in view of new 36.2). New 36.3 can accommodate the fungal aspects of former 36.2.

The new concept avoids (for the time being) any special, official lists, but leaves ample scope for conservation whenever the effects of applying it are undesirable. Acceptability of the concept may depend on the number of cases in which conservation is required. Anyhow, I have a feeling that the concept is both flexible and clear -- and might even appeal to palaeozoologists in the long run.

Here we go:

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2.2. Taxa are normally, and ideally, thought of as groups of organisms linked through common descent (eutaxa). For practical reasons, taxa are sometimes recognized that do not meet this criterion (parataxa).

Note 1. Parataxa cannot as a rule be named separately from eutaxa (Art. 36.1). However, Art. 36 provides for separate sets of names for three special categories of parataxa: organ-taxa of fossil plants (Art. 36.2), fungal anamorphs (Art. 36.3-5), and ichnotaxa (Art. 36.6).

36.2. A name typified by a particular fossilized plant organ may be used to designate a parataxon (Art. 2.2) consisting only of fossil organs of the same category (organ-taxon). A name of an organ-taxon is not applicable to fossil organs of any other category. It -- or its epithet in the required combination -- may however, when desired, be used to designate a hypothetical group of organisms (eutaxon; Art. 2.2) to which its type is believed to belong. When used to designate eutaxa, and only then, names typified by a fossil plant organ compete for precedence with names that are typified by an organ of a different category believed to pertain to the same taxon.

Up to you!

Best regards, Werner

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Comments: Authenticated sender is <WG@fub46.zedat.fu-berlin.de>
From: "PROF. DR. W. GREUTER" <wg@fub46.zedat.fu-berlin.de>
To: traverse@ems.psu.edu, fensome@agc.bio.ns.ca
Date: Mon, 6 Jan 1997 19:05:28 +0000
Subject: (Fwd) biocode and fossils
Priority: normal

Dear Rob and Al,

just got your email addresses from Judy, so here we go!

------- Forwarded Message Follows -------
From: Self <FS_BGBM1/WG>
To: chaloner,skog,hawksworth,McNeill,nicolson
Subject: biocode and fossils
Date: Mon, 6 Jan 1997 16:02:04

Dear all,

this is in reply to Bill Chaloner's most helpful letter to David (& al.) of 26 December.

First: I got Judy's email address from the IOP Web site, but could not find that of Rob Fensome (not an IOP member?!), nor Al Traverse's. Can anyone help, please?

Second: Many things appear to go on in the Fossil Plant Committee of which I have no glint. Could those in charge (Judy?) make sure that Dan and I are on the mailing list for all Committee correspondence? As Officers of the General Committee we are ex officio (non-voting) members of all such committees.

Third: I am embarrassed but not only did I never receive the IOP Newsletter 59 to which Bill refers, but worse I find that the Newsletter is not in the B library at all. Whom should I best address to remedy this? Surely it would be of mutual benefit if the IOP Newsletter were available at IAPT headquarters? But perhaps it is a confidential document, since the Web page under which it is supposed to be available is obviously void...

So my present reaction must unfortunately limit itself to those points that are explicit in Bill's letter referred to above.

1. Form-taxa: if this term is objectionable or confusing for palaeobotanists it can go (which would also imply changing "form taxa" to "taxa" in Art. 3.2, second sentence). However, would it not

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be nice to have a common term for taxa subject to the special regulations of Art. 36? Mycologists at present call them "form-taxa" anyway (ICBN Art. 59.3), which is why we thought this to be a good designation generally. Would "parataxa" be preferable? or is that term preoccupied?

2. Whole-organism taxa: I like Bill's suggestion, and much of his wording. Perhaps one should refer to fungi and fossils in separate phrases. In Art. 36.2, line 5, one might replace "not to the whole fossil, or" by "not to different organs or combinations of organs, nor". [This would also ensure that a name typified by a seed or spore need not and cannot be used to designate fossil leaves or wood.] Art. 36.3 might then state "even though fossil specimens may exist linking that organ with other organs for which earlier names are available".

3. The last three sentences of Art. 36.3 merely tried to foreshadow what palaeobotanists might want and need, and may indeed be completely off the mark, being worded by a complete layman. Please consider, however, that the following points will have to be covered:

If generic names for fossils exist that are used for whole plants, or for various of their organs, then they must be exempted from the provisions of Art. 36.2. Otherwise Sigillaria and Lepidodendron might (I guess) have to be restricted in their use to fossil wood samples, and speaking of them as whole plants, forming forests, would be incorrect.

Conversely, you cannot exempt supraspecific names from the limitations of Art. 36.2 without risking to have, for priority reasons, to apply some "-pollenites" or "-xylon" to fossil leaves.

Genera may originally contain species with names typified by different organs, yet their ultimate type will as a rule belong to a single organ category.

Perhaps, then, the only viable solution is to state that the application of generic names of fossils is determinbed by the nature of their ultimate type, except for names placed on "official" lists for which a wider use is authorized. Would there be many such names?

As to the last sentence, that Bill wants deleted, he may be quite correct; yet something is needed there. For new taxa: can the same rule as 36.4 for fungi be applied? What happens as a rule, in palaeobotany, when an organ species is transferred to a non-fossil genus (say, as Ginkgo huttonii or Nypa burtinii)? Are they then meant to designate different, associated organs, or would they still

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function as organ-species names? In the latter case, my gut feeling is that there should at least be a strong recommendation against creating and using such combinations, and for using instead combinations under, say, "Ginkgophyllum" and "Nypocarpon".

Clearly, much more thought is needed on these issues.

4. As to Art. 36.6, I am glad of Bill's support; but perhaps would he go one step further and accept the extension: "... not to the organism reponsible for or victim of the work"?

5. A last point appears to be quite new. Should it not be made clear somewhere (preferably perhaps in Art. 14, with a cross-reference under Art. 36) that for the purposes of the nomenclature of fossil organisms (I think this equally applies to animals) the cast is considered as equivalent to the organ responsible for it? Otherwise, some might come and pretend that, when the type is a cast, the name can apply only to casts. Or do I miss something?

That's it for today. Best greetings to all!

Werner Greuter

Hunt Institute for Botanical Documentation

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23 December, 1993

Prof. Dr. Werner Greuter
Botanischer Garten und Botanisches Museum
Königin-Luise-Straße 6-8
D-14191 Berlin, Deutschland

Dear Werner:

Just got the reprints of the Committee for Fossil Plants report. Makes me think I may have screwed up--could you check? I intended also to order 100 reprints of the Traverse and Strother paper on Tetrahedraletes. I may have thought the payment had already been taken care of because of the check sent for the CFP report. I don't want the Tetradraletes reprint order to be cancelled because of my error.

Happy 1994 to everybody in your group.

Yours very truly,

Alfred Traverse

Hunt Institute for Botanical Documentation

International Association for Plant Taxonomy
International Bureau for
Plant Taxonomy and Nomenclature

IAPT – BERLIN

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Dr. Alfred Traverse
Dept. of Geosciences
Pennsylvania State University
University Park, PA 16802
U.S.A.

23.12.93

Dear Al,

Thanks for the corrected proofs. Just to avoid a misunderstanding, the bill of 2 November 1993 for which you sent a cheque concerned your paper in the November Taxon (I hope you have meanwhile received the ordered 100 reprints), and so do not get surprised at receiving another invoice for your paper in the Februar 1994 issue of Taxon.

With kind regards and seasons' greetings, also from Brigitte (who has left for a short holiday),

Yours sincerely,

Werner Greuter

Hunt Institute for Botanical Documentation

International Association for Plant Taxonomy
International Bureau for
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Prof. Al Traverse
Secretary, Committee for Fossil Plants
Pennsylvania State University
University Park

2 Oct 1993

Dear Al,

I am sending you today a document by Dan Nicolson and myself relating to the "new nomenclature" inaugurated at Yokohama, and to its likely repercussions on the work and functioning of Permanent Nomenclature Committees. Please give it your full attention, and feel free to send copies of it, with your own comments, to the members of your Committee.

Dan and I would appreciate knowing your personal views on this, and those of others as well.

Dan and I would welcome your comments.

With kind regards, yours sincerely Werner Greuter

Hunt Institute for Botanical Documentation

International Association for Plant Taxonomy
International Bureau for Plant Taxonomy and Nomenclature

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1 Oct 1993

To the Secretaries of all Permanent Nomenclature Committees

from Werner Greuter, Chairman, and Dan H. Nicolson, Secretary of the General Committee

Those present at the recent nomenclature sessions will know that there was a new wind blowing at Yokohama. Although NCU protection was not (not yet) allowed, and although registration, accepted as a principle, will not become mandatory before the year 2000 and conditional to the confirmation of the XVI IBC, the Section was aware to an unprecedented degree of the justified needs and demands of the users of scientific plant names. Almost incredibly – if you think of violent debates at past Congresses – unrestricted conservation of species names, and rejection of any name (irrespective of rank) that would cause a disadvantageous nomenclatural change, were both admitted by an overwhelming majority on a simple show of hands!

The Nomenclature Section was fully aware of the potentialities of what had been decided. Recognizing the need for Permanent Committees to co-operate if the new options introduced into the Code were to be put to practice, the Section passed a resolution explicitly addressing them: "The Section urges the General Committee and through it all Permanent Committees to make full use of the options that the Code now provides in order to ensure nomenclatural clarity and stability."

This request is not as commonplace as one might think at a first glance. Committees in the past have not always made use of the available options, deciding on the adoption or otherwise of conservation proposals on the basis of criteria that are now (and and often were then) foreign to the Code. In particular, the importance of the named taxon in terms of economic use, scientific interest, number of subordinate taxa and geographical range has often been put onto the balance, although the only criteria given by the Code (except formerly for species names) are "avoidance of disadvantageous changes" and "retention of those names which best serve stability of nomenclature".

The Section also moved what was to become part of an All-Congress Resolution: "While work continues to find further ways to reduce changes of well-established names, such names should not be displaced for purely nomenclatural reasons, whether by change in their application, or by resurrection of long-forgotten names." This includes an implicit exhortation not to indulge in any avoidable name change in the future but to submit conservation or rejection proposals instead. The "wide open floodgates" referred to at past Congresses are to stay ajar. Certainly, we for Taxon will not any more accept papers that include futile name changes but will instead urge authors to submit proposals as appropriate. Other scientific editors will hopefully follow suit, and authors will rapidly learn.

Some thoughts are needed at this stage, perhaps including new guidelines and procedures, unless the system be bogged down. We intend to publish appropriate guidance for authors of proposals in order to make economy of printed space. We also intend to reopen discussion on Committee procedures in the General Committee. Before doing either we would like to have your opinion and advice, and be sure of your co-operation.

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We envisage limiting the length of published proposals to, say, one printed page per name (more complex arguments might be submitted as additional papers subject to peer review). [A similar constraint was in the past applied to proposals to amend the Code.] Consideration of "the case both for and against conservation", required by the Code, would then have to be limited to the legally relevant aspects: avoidance of change and service to stability. Presumably the usage in the literature of different names for the same taxon, or of the same name for different taxa, will have to be documented – but this might be done somewhat more globally ("50 titles, including ...") and with abbreviated citations in the text rather than by reference to a "Literature cited" section. Before going into any more detail we would like to know from you who have got the experience, (a) what kind of information is essential from a judging Committee's point of view [particularly items often wanting], and (b) what kind of information is basically superfluous though often included.

Committee work will, in the future, have to be even more expedient than it has been – but should remain as careful as it used to be. Our present feeling is that this can be achieved by defining more clearly and in more narrow adherence to the Code the criteria for judgement, perhaps limiting them to three: (1) is a proposal necessary at all (many are not); (2) is it justified under the stability/avoidance of change criterion (bearing in mind possible side effects); and (3) is it the most appropriate answer. If the answer to (1) or (2) is negative the proposal should be rejected and the reasons for rejection stated accordingly, unless salvation can be found under (3); if the first two answers are positive but the third negative, the Committee should feel free to recommend its own, new or modified version of the proposal (rather than defeating it and inviting for a new one to be submitted, as was sometimes done). Our next question (c) is whether you agree with these considerations, and whether they deviate considerably from past practice in your Committee.

Under the Section's new mandate, the General Committee will certainly in the future have to carefully consider proposals not recommended by the pertinent Permanent Committee, taking its rationale into account. [In the past, only proposals recommended for adoption were acted upon by the General Committee.] We will now have to ask ourselves whether the present adoption requirements (decided by the General Committee but not based on anything in the Code) are still appropriate. We can break up the question as follows: (d) is the requirement of a two-thirds majority for acceptance really appropriate, or should we shift to 60 % as the Nomenclature Section is used to (this would make no difference for Committees with 9 or 12 members), or even to 50 %, or to the majority of votes cast? (e) Is a proposal not achieving a qualified majority rejected, or is a similar majority required for rejection, as would seem fair? (f) Should the last option become true, what would happen to proposals that were neither accepted nor rejected? Would Committee discussion continue (as is presently the case in the Committee for Fungi), or would they go to the General Committee, with full arguments for and against, for a final decision?

We would like to have your personal view on the lettered questions, and on all other aspects that may be relevant and on which you may care to comment. Feel free to inform your Committee (they have to know!) but do not feel obliged to ask for their opinion and await the answer. We would much appreciate your reply within, say, three weeks, and no later than by the end of October. Use fax or e-mail if this is convenient.

With our best regards

Werner Greuter

Fax:
(+49 30) 830 06 218

E-mail:
wg@biologie.fu-berlin.dbp.de

Dan H. Nicolson

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Hunt Institute for Botanical Documentation

**Journal of the
International Association for Plant Taxonomy**

TAXON

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**Prof. A. Traverse
Palynological Laboratories
Department of Geosciences
Pennsylvania State University
University Park, PA 16802
U.S.A.**

20.Apr.1993

Dear Al,

We acknowledge receipt of your paper "Traverse, A.: On the current nomenclatural status of *Tetrahedraletes*", and have forwarded it to the nomenclature editor, Dr. Nicolson. You will hear from him in due course.

When reading the updated "Instructions to authors" you may have already noticed that page charges are billed to authors for each printed page in excess of 6, published under their authorship in a single issue of *Taxon*. The rate is US $ 30 per page for members of IAPT and US $ 50 for non-members.

With kind regards,
Yours sincerely,

Brigitte Zimmer

Werner Greuter

Copy: Nicolson

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16.April.1993

Herrn Dr. Werner Greuter
Botanischer Garten und Botanisches Museum
Königin Luise Straße 6-8
1000 Berlin 33, Deutschland

Sehr geehrter Dr. Greuter,

Herr Schaarschmidt hat endlich seinen Stimmzettel eingeschickt. Es ändert die Situation kaum, aber um ganz korrekt zu sein, könnten Sie bitte die beigelegten Seiten meines Berichts für die alten Versionen davon austauschen? Haben Sie vielen Dank!

mit besten Grüßen, auch an Frau Zimmer, verbleibe ich

Ihr

Alfred Traverse

Anlagen

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13 April, 1993

Dr. Werner Greuter
Botanischer Garten und Botanisches Museum
Königin Luise Straße 6-8
D-1000 Berlin 33, Deutschland

Dear Werner:

Here's the report of the Committee for Fossil Plants. I am also sending a copy to Bill Chaloner with the request that he get it xeroxed in x copies, either in UK or Japan, so that a little pile of them can be available at the nomenclatural session. Later, if a version for Taxon is desired, as I suppose is likely, I can produce an expurgated version short enough to be published. Of course, the authors of the opinions would probably be delighted to see the whole thing in print.

I was startled by the various votes--a real swing since '87. I am very pleased. (I am not as hidebound as you probably think.)

Yours very truly,

Alfred Traverse

encl: report
c: W.G. Chaloner

Hunt Institute for Botanical Documentation

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7 April, 1993

Prof. Dr. Werner Greuter
Botanischer Garten und Botanisches Museum
Königin Luise Straße 6-8
D-1000 Berlin 33, Germany

Dear Werner:

Enclosed are original and two copies of a nomenclatural note for Taxon. It is also available on a Word Perfect 5.1 diskette. Dr. Strother and I have been involved with the subject names from the beginning and believe this note would help prevent a small bit of chaos from happening.

I also have ready for you the report of the Committee for Fossil Plants, having finally received the last ballot I can reasonably hope to get. Practically 100%. I believe you will be pleased by the results. (Inserted before printing: Bill Chaloner just called from Florida and says "hi". He will be in Japan, but I won't--no funding.)

Best regards, also to Ms. Zimmer, please.

Yours very truly,

Alfred Traverse

encl: manuscripts

Hunt Institute for Botanical Documentation

Frankfurt a.M.
28 März, 1992

Hr. Werner Greuter
IAPT - Berlin usw

Sehr geehrter, lieber Werner,

vorsicht! Es kommt noch ein Primarschulebrief aus Frankfurt!

Ich kann weder hier noch in meinem Arbeitszimmer ein Ferngespräch machen 1) die Kabinen in dieser Gegend sind alle 1) gefährlich und 2) meistens kaputt. Es ist 1 Km nach einem Postamt. Tatsächlich können wir ein Telefon nur für Frankfurt Anrufe benutzen! Ich muß daher schreiben!

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Dr. Krutzsch wird nicht während meines Besuchs in Berlin sein. Ich werde jedoch seinen Mitarbeiter Dr. Rüffle und hoffentlich (falls er nicht zu krank ist) auch Dr. Schulz am 6. April um 14⁰⁰ im Museum besuchen.

Meinen Notizen zufolge, werde ich Sie am Freitag 7. April um 14⁰⁰ im Bot. Gart. besuchen. Der Brief von Dr. Rüffle sagt, daß Sie mich auch am 8. April erwarten. Wir könnten das am 7. IV besprechen.

Unser Hotel ist „Excelsior" – Handelsbergstr. 14. Wir werden dort ab 2. IV. sein.

Mit besten Grüßen

Ihr Alfred Tiemann

Hunt Institute for Botanical Documentation

Hunt Institute for Botanical Documentation

Senckenberg-Anl. 36, Nr. 25
6000 Frankfurt a.M. 1

21 März 1992

Dr. Werner Greuter
usw.

Sehr geehrter Dr. Greuter,

Ihren guten Brief vom 18. III habe ich gestern bekommen. Ich habe zu schwer gearbeitet, Deutsch zu lernen um es mindestens in Deutschland nicht zu üben! Wir werden die Nomenklatur in Berlin wahrscheinlich nur auf Englisch besprechen, aber ich muß versuchen mit deutschen „Beamten" die deutsche Sprache zu benutzen! (Meine Frau und ich wissen nicht, ob "for the sake of politeness I shall respond in English" eine Beleidigung war oder nicht!)

Entweder 7. IV oder 8. IV Nachmittag scheinen gut zu sein. Ich würde den 7. vorschlagen. Also, bis den 7. April um „2 00 p.m." = 1400!

Mit besten Grüßen

Ihr Alfred Traverse

= „Al"

P.S. Ich berücksichtige, wenn es möglich wäre, Dr. Kiritzach und andere im Paläontologischen Museum an 8. IV zu besuchen. Sie könnten eventuell Sie anrufen.

International Association for Plant Taxonomy
International Bureau for
Plant Taxonomy and Nomenclature

IAPT – BERLIN

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Dr. Alfred Traverse
Beethovenstr. 36, Nr. 25

W-6000 Frankfurt/M. 1

18.03.92

Dear Al,

Thank you for your letter, written in a remarkably scholarly German, to which for the sake of politeness I shall respond in English.

I am most pleased to have an opportunity to meet you here, and to discuss matters of common interest with you. I am particularly anxious to raise some points related to the Names in Current Use project, since my worries concerning fossil names in this field persist.

I will be out of town up to and including the 6th of April, but shall delighted to meet you in the afternoon of either the 7th or 8th. I suggest that you may drop in on either day at, say, 2 p.m. Perhaps you could drop a line, or send a fax or phone message, when you know which date suits you best.

Looking forward to our meeting, and with kind regards,

Yours sincerely,

Werner

Werner Greuter

Hunt Institute for Botanical Documentation

Bockenheim. 36, 1/11.25
6000 Frankfurt a.M. 1
16. März. 1992

Sehr geehrter Dr. Greuter,

Ich bin dieses Semester
am Senckenbergischen Museum
als Fulbrightprofessor tätig.
Wir bleiben bis 1. Juli, und
es ist schon eine wunderbare
Erfahrung gewesen.

Die Fulbrightkommission hat
alle Fulbrightprofessoren gebeten,
eine Tagung 2-10 April in
Berlin zu besuchen. Es scheint
mir sinnvoll, wenn möglich sie
während dieser wahrscheinlich
langweiligen Tagung zu besuchen.

Sie haben uns bis jetzt kein
festes Programm geschickt, obwohl
ein paar Sachen schon abgemacht
sind. 2 April und 10 April
sind die „an" und „ab" Tage.
5 April ist für verschiedene
Exkursionen geplant. (Im Bot.
Garten steht nicht auf der Liste!)
Die anderen Vorträge, Sitzungen
usw. werden für Vormittags
geplant (mit Ausnahmen"). Ich
vermute, daß ich Sie und den
Garten besuchen könnte, wann (? wenn!)
es Ihnen gefallen würde. Könnten
Sie einen Termin für einen Vormittag
6-9 April wählen? Sie könnten mich
hier schreiben, oder eine Notiz an
unser Hotel (Excelsior, Hardenbergstr. 14)

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für spät Donnerstag 2. April
für mich zurücklassen.

Ich schreibe im Augenblick ein
Kapitel über die Nomenklatur
für Jansonius' palynologisches
Buch, und eine Stunde mit
Ihnen könnte für diese
Arbeit sehr wichtig sein.

mit besten Grüßen

Ihr Alfred Traverse

"Al"

Hunt Institute for Botanical Documentation

**Botanischer Garten
und Botanisches Museum
Berlin-Dahlem**

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

10.9.1992

Dear Al,

I was busy the whole last week with IAPT matters and with drafting the comments for the synopsis of nomenclature proposals for the Tokyo Congress. Only today can I deal with your fax request of last Tuesday.

You will find the answer to your question on page 267 of the Zoological Code, under the definition of a "nominal taxon". For zoological nomenclatural purposes, a "nominal species" is not what we would call a (taxonomic) species, but a concept that, for all practical purposes, is limited to the name and its nomenclatural type.

The "nominal taxon" concept is a very practical and artful device of zoologists, for which we botanists might well envy them. For zoologists, the simple and clear statement "a type of a nominal genus is a nominal species" suffices to express what we, botanists, have to circumscribe with a phrase such as: "a type of a name of a genus is the type of a name of a species, when for citation and designation purposes, the name of the species alone is considered as the full equivalent of its type." This is, in a nutshell, what the new article 10 is all about.

Is it possible that you have been confused by the similarity between "nominal taxa" and "nominotypical taxa", which are quite different and entirely independent concepts?

With kind regards

Yours sincerely

Werner Greuter

Hunt Institute for Botanical Documentation

Dear Al, Even though late I prepared a "MS" → AT in a hurry. Your points were well taken. These copies will keep you up-to-date in the Greuter Saga.

Dr. Greuter
Botanisches Museum
Konigin Louise Strasse
W-1000 Berlin 33

Yes, my old MS could be arranged by morphology. I threw the originals out a long time ago (the pixtures are now on the type script).

Dear Dr. Greuter, 92 08 24 Best regards, —

Thank you for your extensive letter of 13 08 92; I will try to answer it (at least in part) before I leave for the 8.IPC in Aix-en-Province. I am sorry to have missed the boat with my proposals - I had not received the copy of Taxon in which the date was announced, and you had not specified it either in your earlier letter.

If, as you say, a name always has a type, for the purposes of the Code, I do not understand why Art. 37.1 requires that a type has to be specified after 1957, for publication to be valid. As I read that Art.: if on or after 1 Jan. 1958 a name is published without indication of its holotype (which, for a generic name, is the holotype specimen of a species; see the direct reference to Art. 10), the publication was not valid. When *Acaciapollenites* was proposed in 1972, there was no holotype of the species that was cited as "type species". That is why I say that the Sydney Code made the publication of that name invalid; the Berlin Code still says that it was invalid, because in 1972 the requirements of Art. 37.1 were not satisfied. [For that matter: where could I find in the Code that "For the purposes of the Code, a name always has a type (and is considered to have it from the moment of its valid publication) irrespective of the fact that the type may have been designated later, or remains to be designated in the future"?] Preamble 10 states that the latest edition of the Code supersedes all previous editions. Principle VI states that the Rules of nomenclature are retroactive unless expressly limited. With those two caveats, Art. 37.1 sees *Acaciapollenites* as not validly published (although its publication satisfied all requirements of the Code that was in force in 1972).

You wrote (Art. 10.1): "the species name ... is considered as the full equivalent of its type". However, your quote is incomplete, because it is preceeded by the limiting caveat "For purposes of designation or citation of a type..." A designation or citation of the species name, then, is acceptable as an 'indirect indication'; but if we accept this to mean that we do not really need a holotype specimen as nomenclatural type of a genus name (after 1957), this second sentence of Art. 37.1 would directly contradict the first sentence. An 'indirect reference' cannot refer to a holotype specimen if none was ever existing.

Of course, if *Acaciapollenites* had been proposed before 1958, I would agree with you that the name would be still valid as of its first valid publication; but I wrestle with this 1958 date in Art. 37.1, which originally, in the same Art., pertained to different conditions.

I do not understand your reference to "Art. 33 of the 1956 Paris Code": that article does not deal with typification; I cannot follow your argument here. However, in the Paris Code, Art. 9 specifies that 'the type of a genus ... is a species'. Art. 10 then specifies that 'the nomenclatural type of a species is a single specimen'. I cannot see how

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you justify the notion that the Paris Code meant that "type" always stood for "holotype": clearly, a species was acceptable as a nomenclatural type in that 1956 Code.

I have no time, now, to engage in further discussion on some of the other points. However, I have read through most of your letter (and I will reread it in the coming months!), and am pleased at how we are coming to a better mutual understanding. All I want for the Code is to be fair, equitable, easy to understand, and unambiguous. If I misunderstand the wording of the Code, I am sure there are many more who understand it even less (and then there are those who publish new names, but who never saw a copy of the Code). You, as Chairman of the Editorial Committee, can learn from the wrong conclusions that I am making on the basis of my reading of the present wording of the Code. I have no ambition to become a member of the Editorial Committee, but I would be happy to read through any new text to see if it is 'fool-proof'.

For now, with my best regards,

yours sincerely,

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

Hunt Institute for Botanical Documentation

PENNSTATE

The Pennsylvania State University
Department of Geosciences
503 Deike Building
(814) 863-7823 [FAX]
Date: 4-VIII-92

☐ Marked here if urgent reply is requested.

Fax Transmission to:

Name: Prof. Dr. Werner Greuter, Director
Company/Institution: Botanical Garden - Dahlem, Berlin
Department:
Fax Number: 49 30 830 06 218
Phone Number: 49 30 831 60 10

Transmission from:

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

Total Number of Pages (Including Cover Page): 1

Message:

Dear Werner, In rewriting my chapter I've done o.k. with your notes, except for your statement "... typification under ICZN ... is exactly the same as ... post-Sydney ICBN." I just can't get that to check out! The underlined portion to the left means to me that the situation is like pre-Sydney ICBN. I find no evidence in ICZN of specimen typification of genera (except for "nominotypical taxa", which are autonyms). Comment?

Al

CHAPTER XIII. THE TYPE CONCEPT IN NOMENCLATURE

Article 61. Principle of Name-bearing Types.—

(a) **Statement of the Principle of Name-bearing Types.**—Each nominal taxon has, actually or potentially, its name-bearing type. Thus, the name-bearing type of a nominal family-group taxon is a nominal genus, that of a nominal genus-group taxon is a nominal species, and that of a nominal species-group taxon is either a holotype, lectotype, neotype, or syntype series [Art. 72a (ii)]. The name-bearing type of any nominal taxon, once fixed in conformity with the provisions of the Code, is not subject to change except by use of the plenary power of the Commission [Art. 79], or, in the case of nominal species-group taxa, as provided in Articles 74 and 75. The name-bearing type provides the objective standard of reference by which the application of the name it bears is determined, no matter how the boundaries of the taxon may change.

(b) **Name-bearing types of nominotypical taxa.**—The name-bearing type of a nominal taxon is also the name-bearing type

115

↑ ICZN

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

Hunt Institute for Botanical Documentation

International Association for Plant Taxonomy
International Bureau for
Plant Taxonomy and Nomenclature

IAPT – BERLIN

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Prof. Alfred Traverse
The Pennsylvania State University
Dept.of Geosciences,Palyn.Laboratories
University Park, PA 16802

20.07.92

Dear Al,

First, you are most welcome of course to incorporate any of the suggestions I have made, either literally or in modified form. I will be only too pleased if you do.

Second, this may indeed be quibbling, but I am always somewhat alarmed when people think that permanent nomenclatural committees are working under the authority of IAPT. The only body that has authority over the permanent committees is the General Committee, and both are elected at and by International Botanical Congresses. IAPT has by tradition volunteered to assist all these committees in their work, mainly by offering them Taxon as a forum for publication, and will continue to do so in the future even in the absence of a formal link or commitment.

Third, I believe that we both know what palaeontologists are doing when applying names to their taxa. My only point is that this policy is not at present covered by the letter of the Code. I take it that the main problem is that palaeontologists fail to agree as to how this letter should be changed.

Fourth, thank you for enlightening me on the definition of a palynomorph. I trust that this definition will be given somewhere in the book, but still believe it would be useful to cross-reference to it in your chapter.

Fifth, there is no definition of "herbarium" in the Code, and I would say that, since the Code covers the names of all organisms treated as plants, herbaria should by analogy include collections of all such organisms. Anything else would, obviously, be most unfair. Incidentally, Art. 37.5 just asks for specification of the herbarium or institution in which the type is conserved, not by which it is owned. As long as a worker does not keep the types in his or her private bedroom, but (at least for the time being) at a working place in an institution, there should be no difficulty (not even under a narrow concept of "herbarium").

I am writing these comments in some haste, since I am busily preparing the ground for the forthcoming meeting with John McNeill to prepare the "Synopsis of proposals". So excuse my brevity. I hope I have nevertheless written to the point.

With kind regards,

Yours sincerely,

Werner Greuter

Hunt Institute for Botanical Documentation

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17 July, 1992
faxed to: 49 30 830 06 218

Dr. Werner Greuter
Botanischer Garten u. Botanisches Museum
Berlin-Dahlem

Dear Werner:

So I'm finally back, and first order business is to revise the chapter on palynological nomenclature. There were five reviews: yours (unofficial but probably the most helpful), Fensome, Dan Nicolson, Colin McGregor (these 3, "official"), and Jansonius (he's the editor). Tough job, for example, because the reviews conflict in places (you liked my style, as did Fensome, but the other three thought it was offensively light-hearted). If I omitted everything that everybody objected to for one reason or another, the paper would fit on one side of a 10-mark note.

Now I need comments on a number of points:

1. In 90% of the places where your sharpened up the accuracy, I'd like to use your language, practically unchanged. Is that o.k., with general acknowledgment?
2. In a few places, it seems to me you are close to quibbling. For example, it really puzzles me that every time I wrote that "CFP" is IAPT-associated you marked it out, usually with an exclamation point. It may be that you are technically correct, but the committees all operate as if they were part of IAPT; they all use Taxon as their house organ (!), etc. Furthermore, one W. Greuter, in the Preface to the current ICBN (p. xiii), says "The various permanent...committees...operate under the auspices of the International Association for Plant Taxonomy...." "Operating under the auspices" and "operating as part of" are rather obscurely different in this case?
3. I know of no case where a fossil spore or pollen name has ever been used for any other organ, let alone for a reconstructed whole plant. It seems to me very quixotic not to face the fact that fossil sporomorph names really are used as if independent of the names for other categories of plant fossils, although the ICBN is applied to their nomenclature in the same ways. Because of mosaic evolution, heterochrony and convergent evolution, "taxa" of pollen/spore forms do not correspond nicely to whole-plant taxa, yet differentiation of sporomorph forms is possible and important (e.g., for stratigraphy), and these forms can be named using ICBN-governed methods. Comment?
4. The current definition of what a palynomorph is comes

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from my textbook, Paleopalynology. I based that definition on current practice of palynologists: a palynomorph is defined by two qualities: (1) size (I set this at 5-500μm) and (2) chemistry (resistant to natural decay, and to attack by hydrochloric and hydrofluoric acids in the laboratory). Diatom frustules are o.k. sizewise, but they are siliceous and do not normally appear in palynological preparations. Lepidopteran scales are (apparently) chitinous and can occur (rarely).

5. "Herbarium" was understood to mean "public herbarium" when the wording was introduced into ICBN (of that I'm practically certain). But, in any case, "herbarium" doesn't apply to fossil palynomorphs, does it?

Well, I especially need a faxed answer to item 1. I'd like comments on the other points, but at your convenience, per post, would be fine.

I really greatly appreciate your help with this MS and hope you'll be willing to spar with me again on such matters. All the best, and a greeting to Ms. Zimmer, who has given the name "bag lady" new respectability.

Yours very truly,

AL

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

Genève 5. VI. 92

Jean Werner

Thanks for yours of 21. May. 92.

The nomenclatural matters are complex and will require much thought and correspondence — we should have had a conference - place set up with you from London!

However, I must respond immediately about the Naturhundemuseum question. I reported absolutely accurately what I was told. However, Kuntzsch was not involved (he was out of town during my two visits to the Museum); Jaberg heard the whole thing and did not offer any comments - explanations. The urgency of the matter was stressed, by ~~Boeckeler~~ Jr. Schulz & Dr Riddle, and indeed I had had letters from them since my return. I gather that there may be an internal dispute struggle of some sort ~~the~~ involved, as S. & R. were quite subdued, and little perturbed, in Jaberg's presence. But the bird sold by the Naturhundemuseum did not belong to the museum & I came ~~to~~ and in fact the cause of the difficulty! According to Schulz, however, it did ~~have~~ the paleobotany belong & one of the type collections, & there are a danger of being deprived to

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(according to Schulz, the paleobotanical library ended up in a small library in Brandenburg.)

other addresses. I must emphasize that I trust Eberhard Schulz absolutely. He is a straight-shooter and he intended for it ~~to~~ I believe it will let it go at that.

See the best. Greetings to Mr. Timmer from the "big-man."

Yours

Ad

Pp

Bobbie.

c/ Chalmers

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2 June, 1992

Prof. Dr. Werner Greuter
Botanischer Garten
Königin-Luise-Str. 6-8
W-1000 Berlin 33

Dear Werner:

You are a busy man and don't need so many letters from me, but consider it as a momentary flurry that will soon clear up, may be rather interesting, and will not become a real blizzard!

I just returned from the annual APP meeting in Göttingen. There I had lunch with Krutzsch, who seems to have come largely to see me, and again talked to Schulz, as well as with H. Döring, who is now at the Geol.-Paläont. Inst. in Göttingen after years at Außenstelle-Bernau of the Central Geol. Inst. (= 44 Invalidenstr. = now Bundesanstalt für Geowissenschaft & Rohstoffe). I also had several talks with Jaqueline Strahl, a young palynologist who now works at the just-named Bundesanstalt (= former Central Geol. Inst. = former Preussische Geologische Landesanstalt).

More about the library problem--it's already too late. The trucks are there moving the collections out. It's true that the library in question was not a part of the Museum, but of the abovementioned Bundesanstalt. It was, however, as Schulz says, the backbone of the library facilities for paleobotanists who worked at <u>both</u> the Museum <u>and</u> the "Bundesanstalt." It's a disaster for paleobotany that this has happened, but it has. The building where it was housed <u>was</u> the building that the Treuhand now owns, and it is obvious that all of these matters are interlinked. (The paleobotany literature collections are to be mostly in a library somewhere in Brandenburg.)

Regarding the Treuhand building--it's loss to the total scientific complex in Invalidenstraße will be very unfortunate for the long run. Schulz and Krutzsch both now say that the sale is for the moment "on hold," and they don't know why. Who knows, the fuss we've made may have caused some questions to be raised! The potential loss of the building and the (now) actual loss of the library are part of a general malaise.

Now for the overall situation--this is the worst. Krutzsch says that the reason he can't get interested in the whole matter is partly his impending retirement and partly because "mit der Paläobotanik in Berlin ist es aus, aus...." Among other things, he says that the paleozoologists <u>want</u>

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31 May, 1992

Prof. Dr. Werner Greuter
Botanischer Garten
Königin-Luise-Str. 6-8
W-1000 Berlin 33

Dear Werner:

Thanks for the copy of the correspondence with Jansonius. I had seen a draft of the JJ paper and, indeed, you may recall that I discussed aspects of it with you in Berlin. However, Jan is not right in implying that I agreed with his whole package. I did agree with him that altering the means of typification for generic names was unfortunate. (But I can live with it, and I know "when the jig is up.") For example, I strongly disagreed with JJ about changing the date for required typification and used practically the same language as you did.

I hope you will agree, from looking at my MS for the chapter in the Jansonius palynology book, that my understanding of the current status of the issues was clarified by discussions with you! I look forward to comments on that thing. Of course, Jansonius also has to accept the chapter.

Thanks for reading.

Yours very truly,

Al

Alfred Traverse

Hunt Institute for Botanical Documentation

International Association for Plant Taxonomy
International Bureau for
Plant Taxonomy and Nomenclature

IAPT – BERLIN

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Professor Alfred Traverse
c/o Paläobotanische Sektion
Naturmuseum Senckenberg
Frankfurt/M.

28.5.1992

Dear Al,

It was kind of you to let me have a look at your nomenclatural overwiew, and careless to ask that I should comment. You will hardly like all I have jotted down in form of annotations to the manuscript, but (to quote you), my facts are at least more nearly straight.

I do like the style you use, which successfully endeavours to make a very though steak chewable (if not really juicy). But I also feel that in several places your wording is misleading, and sometimes the facts are wrong. I am not quite sure how extensive changes you had in mind when stating that some could still be incorporated; that you will have to decide on your own.

Of course some points on which we may disagree are a matter of judgement, but in many cases this judgement may have been influenced by inaccurate information. There is one particular point that puzzles me, though: your claim that special rules for names of fossils are a nuisance. In one of the comments I had to write up in my role as Rapporteur, I once stated that special rules required by the special features of the organisms named were in order, but that those special rules mandated by the peculiarities of specialists of a group were perhaps less so. There is perhaps no other group covered by the ICBN which has as many objective peculiarities as fossils (yet some of the special fossil rules, such as the starting-point date, certainly belong to the idiosyncratic category).

If you were to start anew (and perhaps this is not your last nomenclatural magnum opus), I would ask you to consider a slightly different approach, at least for the earlier chapters: first to describe the needs and constraints of paleontologists, to explain what is current practice (or what you would advise to become current practice), and eventually to make clear where there are inadequacies in the Code, and how the Code might usefully be changed to cover that practicep. Clearly, types of non-fossil plants too are often fragmentary (even if not to the same degree as for fossils), but then you can always hope to match them with a whole organism and find out what, say, the trunk, roots or fruits look like. This is not possible with fossils, so in a way all fossil specimens belong to "form-taxa" of some kind (to use a concept close to your own notion of "form-taxa", much more akin to the defunct "organ-taxa" than to form-taxa as currently defined in the Code). It seems to me that what you need in paleontology is, on one hand, a set of clearly defined (both mutually exclusive and hierarchically nested) categories within which names may compete if (and only if) their types are accepted to belong to that category. There

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may even be some exceptional cases where fossil and non-fossil types might be assigned to the same category: I think of the Diatoms in particular [why the heck do you exclude them from paleopalynology when you include lepidopteran scales?!], which may well be the historical root for the awkward algal exception in Art. 45.4 – a real nuisance in other respects! On the other hand you could, for those who needed them, have a different category of "ideal" fossil taxa, not found in nature but originating from reconstructions on the basis of available evidence. These too might be named (being comparable to non-fossil taxa in many respects), but their names could not be typified by single specimens. Perhaps a combination of at least two different types from as many different fossil categories would be required to typify them, and they could be applied only to designate plants (hypotheses!) believed to combine the respective features. These names would designate taxa that make sense to evolutionists, biogeographers and paleoecologists, not to stratigraphers of course; but I believe that there is scope for both kinds of names, and that much of the present uncertainty stems from the fact that extant names are used in both senses without anyone making this basic distinction clear.

I am pretty sure Jim Schopf was technically right in his interpretation of what the Code actually says (after all, he is responsible for some of it); but I grant you that your own point of view is much closer to what the Code should say.

This letter is not intended to duplicate what I have jotted down in handwriting, which you may hopefully manage to decipher (I have no problems in reading it myself, but some do). Note that I made my notes sequentially as I progressed with reading, so that some of my qualms are taken care of by what you are writing further down in the text. But then, perhaps at least a cross-reference to the later explanation might be a good idea, to avoid readers scratching their head in wonder.

The one thing I have not tried to note or eliminate are duplications, of which there are a good number. But perhaps it was your considered intent to hammer in some points through repetition...

With kind regards, yours sincerely



Werner Greuter

Hunt Institute for Botanical Documentation

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19 May, 1992

Prof. Dr. Werner Greuter
Botanischer Garten
Königin Luise Str. 6-8
W-1000 Berlin 33

Dear Werner:

Enclosed is a copy of my manuscript for the massive Jansonius volume. It profited greatly from discussions with you--you may not agree with me on some points, but the facts are at least more nearly straight! If you have corrections or suggestions, they would be appreciated, and appropriate changes could still be incorporated. The book is a general treatment of paleopalynology, with individual chapters about everything imaginable.

Best.

Yours very truly,

Alfred Traverse

It would be OK to write directly or res or. mail, if you like.

AT/et
encl: copy of MS

Hunt Institute for Botanical Documentation

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19 May, 1992

Prof. Dr. Werner Greuter
Botanischer Garten
Königin Luise Str. 6-8
W-1000 Berlin 33

Dear Werner:

Just returned from conference in London with W.G. Chaloner and M.C. Boulter regarding IAPT.CFP business, its future, the current status of our NCU lists, and related matters.

In the as yet unexplained mysteries department: Chaloner received a copy of the fossil plant list from your office last November. Is that the one you thought you'd sent me in Pennsylvania? The matter is only of historical importance!

I discovered gradually in London that this massive list is not at all a "draft" or proposed NCU list, a fact that your P.S. to "Sixth Mailing" does not seem to recognize. This fossils list is a print-out of generic names, and the covering data for them from Mike Boulter's PFR (Plant Fossil Record) database. This was obvious to me from the fact that illegitimate names I know I recommended dropping (I was the original palynomorph compiler for ING and was also one of the participating editors for the PFR compilation) nevertheless appear on the "MFPL" (Massive Fossil Plant List). I am even wondering where the "11%" exclusion figure you cite came from--I forgot to ask Mike about that.

I wanted to report on the above information, and to note that all three of us believe it will require a lot of work to produce an NCU list from the PFR printout. Such work obviously won't be complete for several years (it probably can't even start until the Secretary of IAPT.CFP is back home).

Bill and I also would like to report that we discussed at length the future of CFP. We agree that he will step down as chairman ater the Tokyo Congress. I will remain on as Secretary for a while, to assist the new Chairman in the transition, but a new Secretary will probably also take over fairly soon.

All the best, and greatings to Ms. Zimmer.

Yours very truly,

Alfred Traverse

Hunt Institute for Botanical Documentation

P.S. On an entirely different matter: WGC, MCB and I also discussed the "situation on Invalidenstraße." In his office in IOP (Int. Org. Palaeobotany), Mike is organizing an appropriate letter to Senator Erhardt. A couple of weeks ago I was requested by Dr. Rüffle to write an encomium for them to ust ("to whom it may concern"), and I did.

AT

AT/et
c: Chaloner, Boulter

Hunt Institute for Botanical Documentation

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13 April, 1992

Prof. Dr. Werner Greuter
Botanischer Garten und Botanisches Museum Berlin-Dahlem
Königin-Luise-Str. 6-8
W-1000 Berlin 33, Germany

Dear Werner:

Thanks for spending so much time with me last week. On the whole I am not too upset by what I learned from you, except for aspects of typification of generic names. When that package was pushed through at Sydney I was very upset, but various people (including you?) assured me in the hall it wouldn't really make any difference. It turns out that it does. In my opinion it is unfortunate that the type of a generic name and the holotype of the type species can be different. I had understood that, if the author of a new generic name selected a junior synonym as type species for the generic name, the "correction" of this error to the senior synonym would include "correction" of the type to that of the senior synonym--which "should" have been selected in the first place. This did not mean to me changing the type of the generic name but correcting what amounts to an error. However, I now understand what the facts are, directly from the mouth of the "perpetrator of the crime."

Chaloner is putting on pressure for me to join him in London in May for a pre-Tokyo NCU-IAPT.CFP meeting. Hard to justify in terms of my work assignment here. Hmmm. It was a shock to learn that I never got that copy of the NCU list, nor the instructions as Sec. of IAPT.CFP to do something about it--maybe another reason Chaloner and I do need to get together with Boulter and Galtier (new IOP president). Perhaps Galtier is the right guy to work up the letter to the Berlin-Senat that we discussed?

Thanks again for a very productive two afternoons. Greetings to Frau Zimmer. Thanks also for the two new satchels (lifetime supply?) and the great story about their origin from you and her.

Best wishes.

Yours very truly,

Alfred Traverse

AT/et
cc: Chaloner, Jansonius

Hunt Institute for Botanical Documentation

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Herrn
Dr. Werner Greuter
Botanischer Garten und Botanisches Museum
Königin-Luise-Straße 6-8
D-1000 Berlin 33, Germany

Sehr geehrter Herr Dr. Greuter,

ich nehme an, daß die in letzter Zeit angekommene Kopie der "Library Edition" der XIV.IBC-Zusammenfassungen von Ihnen geschickt worden ist. Weil meine eigene Kopie in der Post verloren gegangen ist, bin ich Ihnen natürlich sehr verbunden! Ich möchte mich herzlich bedanken.

Ich wünsche Ihnen frohe Festtage und mit der Redaktion der "Berlin Code" viel Glück!

Alles Gute.

Ihr

Alfred Traverse
Professor für Palynologie

AT/et

Hunt Institute for Botanical Documentation

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15 June, 1987

Prof. Werner Greuter
XIV International Botanical Congress
Königin-Luise-Str. 6-8
D-1000 Berlin (West) 33, Germany

Dear Werner:

Thanks for news (your note of 4-VI-87) about possibility of my sending materials to Berlin c/o your address.

Thanks also for further news re sessions 5-164 and 5-165, per yours of 1-VI-87. Despite the 1 August deadline, a poster author could still agree, on an individual basis, to reappear by a poster some time during the afternoon of 31 July. I have seen that done at other such sessions, and it provides a time for a few people who couldn't come at the scheduled time to visit the author of a particular poster for information. However, I'll be careful to keep this under control.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

P.S. I changed the title of my talk for Session 5-31 to "Plant evolution dances to a different beat", and the abstract with the changed title was in to Chaloner in plenty of time. However, I can't tell if the changed title will be in the programm. I hope so.

AT/et

Hunt Institute for Botanical Documentation

return

XIV International Botanical Congress Berlin

XIV International Botanical Congress Berlin e.V.
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Phone (030) 831 60 10

Prof. A. Traverse
Dept. of Geosciences
College of Earth and Mineral Sciences
Palynological Labs., University
Deike Building
University Park, P. A. 16802
USA

30.09.85

Dear Prof. Traverse,

You are doubtless aware of the fact that the XIV International Botanical Congress, to be held in Berlin (West) from July 24th to August 1st, 1987, gives opportunity to botanists from all over the world, and from all disciplines of the pure and applied plant sciences, to meet, to discuss and to exchange their views and experiences.

At the request of the Programme Committee 5 and of the Symposium Organizers concerned, and on behalf of the Organizing Committee of the Congress, I take pleasure to hereby invite you to present a paper in the frame of the Symposium "Evolution and the fossil plant record".

The Symposium Organizers have suggested the following title for your paper:

"Megaevents, but no punctuation".

You may consider this as a working title, which may be modified further on, if need should be, subject to the Symposium Organizers' agreement. The preliminary plan of the Symposium allots a time of 15 Min. (including discussion) to your paper.

Please confirm your acceptance in writing to me at your earliest convenience, with copy to the Symposium Organizers (you will find their addresses on the attached "Symposium Information Sheet"). Insofar as you have not yet replied to the First Congress Circular (of which copies are still available on request), your positive reply will be considered as equivalent to a preliminary registration - which means that your name will be placed on the mailing list for the Second Congress Circular.

When accepting this invitation, please consider that owing to the expected large number of participants each member of the Congress - as with previous congresses - has to be limited to a single presentation (symposium paper or poster).

I am sorry to state what you may already know, that funds at our disposal are very limited. Although we are making every effort to raise further funds in order to subsidize invited speakers, we cannot at this stage commit ourselves to provide you support. Funding regulations also prevent us from waiving or lowering the congress fee for invited speakers. I therefore recommend that you approach sources within your own country, pointing out that this is customary not only for International Botanical Congresses but for big international congresses in general. I hope and trust that you will succeed.

Looking forward to your reply, and with kind regards,

Yours sincerely

Prof. Dr. Werner Greuter
Secretary of the Congress

Hunt Institute for Botanical Documentation

14 February, 1983

Dr. Werner Greuter
Botanischer Garten und Botanisches Museum
Königin-Luise Strasse 6-8
D-1000 Berlin 33, Germany

Dear Werner:

Thanks for yours of 30 Jnauary, which just today arrived!

There has been some correspondence of IAPT-CFP since Sydney, though no "official newsletters". I have now noted that you should receive copies of the former, as well as of the latter. As we approach the Congress, things will pick up!

I don't know who 'did' the megafossil plant names for INC. (I did the sporomorph names, but Stafleu only thought of them at the last minute, and it wasn't possible to get it really complete--it's approximately 85% complete. Since then, Jansonius has come out with his generic card file. I have kept up my own list, which differs from Jansonius' here and there--he includes things that don't belong, in my opinion, and we differ on whether certain publications, e.g. Malyavkina 1949, are acceptable or not.) Anyway, Andrews' Index of Generic Names of Fossil Plants 1820-1965 does list Rotularia Sternberg 1825 and Asterophyllites Brongniart 1822, per enclosed xeroxes. I suspect that whoever 'did' INC's list simply omitted some suppressed names or missed them altogether. It is puzzling, as one would expect that Andrews' list would have been a primaryhsource. I am primarily a paleopalynologist and do not have access to the original publications needed here, so if there's something more esoteric about these two names, Chaloner will have to take care of it.

As far as I can see, what you've done is o.k. and has my approval. Who was so clever as to pick up that Schlotheimia is also a moss name?

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology
Sec., IAPT-CFP

AT/et
cc: Chaloner
enclosure: xeroxes

Hunt Institute for Botanical Documentation

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und Botanisches Museum
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Prof. Dr. Alfred Traverse
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to: Bill Chaloner
Al Traverse

Berlin, den 30.1.1983

Dear Bill, dear Al,

yesterday the second half of the Code ms., comprising the Appendices and indexes, was sent off to Utrecht. A big relief I must say, and a lot of last-minute effort to have the Appendices standardized in conformity to what we had decided.

The fossils, in App. III, take not even two pages, but were about the worst bit. Lots of minor but tedious changes had to be made: sequence of synonyms, abbreviations of titles, type citations. ING was a marvellous help.

I am enclosing photostats of the two pages as they will be printed, asking you to give me your formal approval on behalf of the 'Fossil Committee'. No need (and no time!) to ask the whole committee, there is nothing controversial as far as I can judge. You will note the absence of Metasequoia, now transferred to Spermatophyta in conformity with the non-fossil nature of the type of the conserved name. You will also note the absence of the nom. rej. Schlotheimia, under Asterophyllites: being a later homonym of Schlotheimia Bridel 1812 (a moss), it does not threaten the nom. cons. and need not be listed.

I have noted that two of the rejected names are not taken up in ING: Rotularia Sternb. (sub Sphenophyllum) and Asterophyllites Brongn. 1822 (sub Asterophyllites Brongn. 1828). In view of the reliability and completeness of ING, this cast some doubt on the validly published status of these rejected names. Could one of you check? It would be disturbing to list invalid names, and it would just be soon enough to delete the entries on the galley proofs.

Unless Bill volunteers for proofreading of App. III (I have not put him on the provisional mailing list for that, but he knows that I am glad to have this changed), any instructions for changes should be with me in Berlin by March 31. This is a stringent deadline! Count 8-10 days for the mail: it takes long to 'swim', these days, whether it be from the U.K. or USA.

I shall assume that no comment means approval - but shall nevertheless appreciate explicit approval, by or on behalf of Al as the Committee Secretary.

I've received no copy of correspondence of the 'Fossil Committee' since Sydney, but assume that this reflects its pacific mood and lack of business rather than failure to put my name on the mailing list (as earlier requested by Ed).

Best wishes Yours sincerely

Werner Greuter

cc/ Voss, McNeill, Nicolson

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MEDULLOSACEAE

Dolerotheca Halle, Kongl. Svenska Vetenskapsakad. ser. 3. 12: 42. 1933.
T.: *D. fertilis* (Renault) Halle (*Dolerophyllum fertile* Renault).

(=) *Discostachys* Grand'Eury, Géol. Paléontol. Bassin Houiller ... Gard t. 8, fig. 2. 1890.
T.: D. cebennensis Grand'Eury.

MEGALOPTERIDACEAE

Megalopteris (Dawson) E. B. Andrews, Geol. Surv. Ohio 2(2): 415. 1875.
T.: *M. dawsonii* (Hartt) E. B. Andrews (*Neuropteris dawsonii* Hartt).

(=) *Cannophyllites* A. T. Brongniart, Prodr. Hist. Vég. Foss. 130. 1828.
T.: C. virletii A. T. Brongniart.

GLOSSOPTERIDACEAE

Glossopteris A. T. Brongniart, Prodr. 54. 1828 (Hist. Vég. Foss. 1: 222. ... 1831).
T.: *G. browniana* Brongniart

(H) *Glossopteris* Rafinesque, Anal. Nat. Tabl. Univ. 205. 1815 [PTERIDOPH.].
≡ Phyllitis J. Hill 1757.

GINKGOACEAE

Baiera C. F. W. Braun, Beitr. Petrefacten-Kunde 6: 20. 1843.
T.: *B. muensteriana* (Presl) Heer (*Sphaerococcites muensterianus* Presl).

(H) *Bajera* Sternberg, Versuch (Tentamen): 28. 1825 [FOSS.: CALAMAR.].
T.: *B. scanica* Sternberg.

CORDAITALES

Cardiocarpus A. T. Brongniart, Rech. Graines Foss. Silic. 20. t. A 1–2. 1880.
T.: *C. drupaceus* Brongniart.

(H) *Cardiocarpus* Reinwardt, Syll. Pl. Nov. 2: 14. 1826 [SIMAROUB.].
T.: *C. amarus* Reinwardt.

Cordaianthus Grand'Eury, Mém. Acad. Roy. Sci. Inst. France 24: 227. t. 26. f. 4–12. 1877.
T.: *C. gemmifer* Grand'Eury.

(=) *Botryoconus* Göppert, Paleont. 12: 152. t. 21. f. 1. 1864.
T.: *B. goldenbergii* Göppert.

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9592 **Taraxacum** Wiggers, Prim. Fl. Hols. 56. 1780.
T.: *T. officinale* Wiggers (*Leontodon taraxacum* Linnaeus) (*typ. cons.*).

(H) *Taraxacum* Zinn, Catal. 425. 1757. ≡ *Leontodon* Linnaeus 1753.

9604 **Pyrrhopappus** A. P. de Candolle, Prodr. 7: 144. 1838.
T.: *P. carolinianus* (Walter) A. P. de Candolle (*Leontodon carolinianum* Walter) (*typ. cons.*).

9604a **Thorelia** Gagnepain, Not. Syst. (Paris) 4: 18. 1920.
T.: *T. montana* Gagnepain.

(H) *Thorelia* Hance, J. Bot. 15: 268. 1877 [SPERMATOPH.].
T.: *T. deglupta* Hance.

HETEROPYXIDACEAE

9712 **Heteropyxis** Harvey, Thes. Cap. 2: 18. 1863.
T.: *H. natalensis* Harvey.

(H) *Heteropyxis* Griffith, Not. Pl. Asiat. 4: 524. 1854 [BOMBAC.].
T.: *Boschia griffithii* Masters.

XIII. FOSSIL PLANTS

SPHENOPHYLLACEAE

Sphenophyllum A. T. Brongniart, Prodr. 68. 1828.
T.: *S. emarginatum* (Brongniart) Brongniart (*Sphenophyllites emarginatus* Brongniart).

(=) *Rotularia* Sternberg, Fl. Vorwelt 1(2): 33. 1822.
T.: *R. marsileifolia* Sternberg.
(≡) *Sphenophyllites* A. T. Brongniart, Mém. Mus. Hist. Nat. 8: 209 ('*Sphoenophyllites*'). 234. t. 13[2]. f. 8. 1822.

CALAMARIACEAE

Asterophyllites A. T. Brongniart, Prodr. Hist. Vég. Foss. 159. 1828.
T.: *A. equisetiformis* (Sternberg) Brongniart (*Bornia equisetiformis* Sternberg).

(H) *Asterophyllites* Brongniart, Mém. Mus. Hist. Nat. 8: 210. 1822.
T.: *A. radiatus* Brongniart.

(≡) Bornia Sternberg, Versuch Fl. Vorwelt 1 (Tentamen): xxiii. 1825.
(=) Bechera Sternberg, Versuch Fl. Vorwelt 1 (Tentamen): xxx. 1825.
T.: B. ceratophylloides Sternberg.
(=) Brukmannia Sternberg, Versuch Fl. Vorwelt 1 (Tentamen): xxix. t. 19. f. 1-2, t. 45. f. 2, t. 58. f. 1. 1825.
T.: B. tenuifolia Sternberg.

Calamites A. T. Brongniart, Hist. 1: 121. 1828.
T.: *C. radiatus* Brongniart.

(H) *Calamitis* Sternberg, Fl. Vorwelt 1 (2): 22. 1820.
T.: *C. pseudobambusia* Sternberg.

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