

Hunt Institute for Botanical Documentation
5th Floor, Hunt Library
Carnegie Mellon University
4909 Frew Street
Pittsburgh, PA 15213-3890
Telephone: 412-268-2434
Email: huntinst@andrew.cmu.edu
Web site: www.huntbotanical.org

The Hunt Institute is committed to making its collections accessible for research. We are pleased to offer this digitized item.

*Usage guidelines*
We have provided this low-resolution, digitized version for research purposes. To inquire about publishing any images from this item, please contact the Institute.

*Statement on harmful and offensive content*
The Hunt Institute Archives contains hundreds of thousands of pages of historical content, writing and images, created by thousands of individuals connected to the botanical sciences. Due to the wide range of time and social context in which these materials were created, some of the collections contain material that reflect outdated, biased, offensive and possibly violent views, opinions and actions. The Hunt Institute for Botanical Documentation does not endorse the views expressed in these materials, which are inconsistent with our dedication to creating an inclusive, accessible and anti-discriminatory research environment. Archival records are historical documents, and the Hunt Institute keeps such records unaltered to maintain their integrity and to foster accountability for the actions and views of the collections' creators.

Many of the historical collections in the Hunt Institute Archives contain personal correspondence, notes, recollections and opinions, which may contain language, ideas or stereotypes that are offensive or harmful to others. These collections are maintained as records of the individuals involved and do not reflect the views or values of the Hunt Institute for Botanical Documentation or those of Carnegie Mellon University.

*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.

X-Sender: marnell@etna.iirg.pi.cnr.it
Date: Wed, 27 May 1998 09:49:57 +0200
To: traverse@ems.psu.edu
From: "Marnell Dickson" <marnell@iirg.pi.cnr.it>
Subject: Martin et al paper/Geothermics

Dear Professor Traverse,

Thank you very much indeed for your fast and exhaustive review of the revised version of the Martin et al. paper. I have forwarded your comments and those of the other two reviewers, and requested the authors to address all the recommendations.

Many thanks once again, on behalf of our editors, for your kind collaboration,

Best regards

Marnell Dickson

"Geothermics" and "International School of Geothermics"
IIRG-CNR
Piazza Solferino 2
Pisa 56126 - Italy

Fax: + 39 50 47055
Phone: + 39 - 50 - 46069/41503/41327
E-mail: marnell@iirg.pi.cnr.it



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

23 January, 1991

Dr. R. V. Dietrich
Department of Geology
Central Michigan University
Mt. Pleasant, MI 48859

Dear Dr. Dietrich:

Thanks for the "last draft." I think this is a very useful thing, but it should be updated every 2-3 years. I am sure that when others see it they will also come up with additions, and, of course, new ones are always being invented.

Best regards.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

CENTRAL MICHIGAN UNIVERSITY
*DEPARTMENT OF GEOLOGY*
314 Brooks Hall
Mt. Pleasant, Michigan 48859
517-774-3179

October 18, 1990

Alfred Traverse, Professor
Pennsylvania State University
Department of Geosciences
435 Deike Building
University Park, PA 16802

Dear Professor Traverse:

Thank you for your letter of September 18 which was on my desk upon my return from an extended trip. We shall certainly consider your comments and suggestions for additions in preparing the final copy of our list of acronyms.

Regards,

R. V. Dietrich

RVD/mb

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

18 September, 1990

Dr. R. V. Dietrich
Department of Geology
Central Michigan University
Mount Pleasant, MI 48859

Dear Dr. Dietrich:

Someone gave me a copy of the recent acronym list. Several additions you should consider:

TSOP: The Society for Organic Petrology (always referred to as "Tee-Sop");

L-O (Analysis) = Lux-Obscura: in paleopalynology, a microscopic technique in very common use;

AP: Arborescent pollen (you already have NAP).

Also please note misspelling of impetus (p. 1, para. 2, last line, of your "Acronyms and abbreviations used in Geology").

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Date: Sat, 07 Dec 1996 16:18:27 -0500
From: Bill Dimichele <DIMICHELE.BILL@nmnh.si.edu>
To: traverse@ems.psu.edu
Subject: book -Reply

Dear Al:

Bob Gastaldo is here visiting and he says that he is contributing a chapter. I think I will also. They are not asking for too much, only a few printed pages for each article (maybe 10-12 ms pages), so I think it cannot hurt to contribute. For the amount of effort required, I think its better to be part of this than not.

I agree that the mix is kind of weird, and not necessarily the same people I would have chosen for all the contributions.
Not only are spores not plant parts, they are sometimes whole plants! What must it be like to be totipotent or be "whole" even when you are at the same time a part, given the developmental complexities of plants. I guess this problem is just one of a zoocentric world view.

There you have it.

Cheers,
Bill DiMichele



Hunt Institute for Botanical Documentation

Date: Sat, 07 Dec 1996 09:06:22 -0500
From: Bill Dimichele <DIMICHELE.BILL@nmnh.si.edu>
To: traverse@ems.psu.edu
Subject: proposed journal -Reply

Al:
Thanks for signing up.
The intent of this is mostly to explore the possibility. I imagine that doing any more than being on the original memo is totally voluntary.

Happy holidays to you, and happy new year.

Bill DiMichele



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

8 July, 1991

Dr. W. A. DiMichele
Dept. of Paleobiology, MRC 121
National Museum of Natural History
Smithsonian Institution
Washington, DC 20560

Dear Bill:

The set of 33 film reels was sent to you under separate cover. I just re-checked reel #1. It says "Paleobotanical species catalog. U.S. Geological Survey, Paleontology & Stratigraphy Branch." That rings a bell, as I believe Jim Schopf showed me that file in DC in July, 1948. I was then a grad student. Jim was working at the Smithsonian, though a USGS employee. Spackman came here in 1949 and by 1951 was already out of paleobotany proper, so this will have been acquired in 1950.

Someone should write a proposal for $$ to get this into a database. Then it would be very useful, and the updates could build on it.

Best regards.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

1 October, 1986

Dr. D. L. Dinelly
Department of Geology
University of Bristol
Bristol. U.K. B58 1UG

Dear Dr. Dinelly:

Bill Oliver asks for news to be sent to you. I am not sure whether our small time efforts qualify, but you can easily put in the (as we say) waste paper basket.

A student (Andrew Schuyler) and I are actively studying the palynomorphs from a wide variety of rocks ranging in age from Givetian to Famennian, in New York and Pennsylvania, mostly in the Catskill Magnafacies and related sedimentary sequences. This work is in collaboration with John S. Bridge at SUNY-Binghamton and with Rudy Slingerland of the Pennsylvania State University, both of whom are interested in the sedimentology of these rocks. Schuyler's and my goals are to attempt to set the palynomorph sequences in stratigraphic order and in correlation with European and other North American schemes, both spore/acritarch-based and conodont-based.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

To: dino@cenpes.petrobas.gov.br
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: illustration
Cc:
Bcc:
X-Attachments:

Dear Dino:

Don't worry about the extra messages. Did no harm. Glad to hear about your work on the cover suggestion for the book. I think I'll run it by the publisher right away. I'll also look forward to getting your draft of what you propose. At worst I'll have a neat item for next year's Christmas card, or whatever! Thanks so much for your interest.

Best. Al.



Hunt Institute for Botanical Documentation

X-Sender: Dino@correio
Date: Wed, 6 Nov 1996 08:50:32 -0300
To: traverse@ems.psu.edu
From: Dino@cenpes.petrobras.gov.br (Rodolfo Dino)
Subject: cover ilustration

Dear Dr. Traverse

Good to get your e-mail note.
Sorry, concerning my e-mail address. The problem is that the e-mail to Rio can sometimes be
erratic so (I don't know why) some messages return.
I, and my computer are having a bad period. I attempted to send some messages to everyone and my
feedback said it had been lost in transmission. You may have received as many as three e-mails with
the same message (+ the fax). My apologies.
I agree with you that an altered version of the illustration will be an original one. So, is not necessary to write to the Committee.
I already start to work in it, and I'm selecting some really good forms to add at the new "image".
After I finished the draft of this new one I'll send to you for your appreciation, comments, suggestions etc..
(Of course I get a picture that you need to obtain the ok of the publisher).

Again, apologies for all the same messages,

With very best wishes

Dino

==========
##########
# Rodolfo Dino #
# Petrobras/Cenpes/Divex/Sebipe #
# Cid. Universit., Q. 7, Ilha do Fundao #
# 21949-900 Rio de Janeiro, RJ, Brazil #
# Email: Dino@cenpes.petrobras.gov.br #
# Fax: +55 21 598 6828 #
# Phone: +55 21 598 6452 #
##########
==========



Hunt Institute for Botanical Documentation

To: dino@cenpes.petrobras.gov.br
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: figure
Cc:
Bcc:
X-Attachments:

Dear Dino:

This a. m. I received an e-mail from you that seems to duplicate what I've received previously and to which I have responded by "snail mail" when e-mail was returned for some reason or other.

The gist of what I said was that I appreciate your kind offer to work with me on creating a figure that would include a greater variety of palynomorphs than are to be found on the journal cover. I also emphasized that it is a little early to write the Committee--I'll do that later. However, if you come up with an altered version it would be regarded as essentially original, and I would need mostly your permission.

All the best. Al.



Hunt Institute for Botanical Documentation

X-Sender: Dino@correio
Date: Fri, 18 Oct 1996 10:00:12 -0300
To: traverse@ems.psu.edu
From: Dino@cenpes.petrobras.gov.br (Rodolfo Dino)
Subject: Front cover figure

Dear Dr. Traverse

Thank you so much for your letter of 22-August, and for the reprints enclosed. I realy appreciated it.
Excuse me for not replied immediately. It has been particulary busy here at Petrobras. In addition I went
to 39 Brazilian Geological Congress (01/07- September) and there was a lot of work piled up on my
desk when I returned - hence the delay.
With regards the utilization of the figure ilustrated on the front cover of our bulletin, there is no problem.
It will be an honour for me.
After consulting the Editor Committee of the bulletin, they (also agree with me) would like that you send
(to the committee) a letter asking for the use (just to formalize).
Meanwhile, I was thinking that like your book covers all aspects of palynology, it would be more appropriate
if the front cover presents all types of palynomorphs. If you are interested I could make another figure (like
that of the bulletin), but changing some forms (as you can see, all of them in the bulletin are cretaceous species), including other types grains (acritarchs, scolecodonts, quitinozoans, etc..) and also of different ages.
What are your thoughts?

Please give me your oppinion and/or comments about

Again a big thank you

Warmest regards

Dino



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

6 December, 1993

Dr. William L. Diver
Department of Geological Sciences
University of Plymouth
Drake Circus, Plymouth
Devon PL4 8AA, UK

Dear Dr. Diver:

It was so nice of you to come up to me at the Chaloner celebration and let me know who you are! As my wife said, we are very familiar with the name!

Next time we have a rent car in UK we'd like to visit Devon and Cornwall. I hope you'll just happen to be around to greet us in Plymouth. In the meantime, don't forget that you are most welcome here.

All the best for the holidays and for the New Year!

Yours very truly,

Alfred Traverse

Hunt Institute for Botanical Documentation

26 September, 1980

Dr. W. L. Diver
Department of Geology
Royal School of Mines
Prince Consort Road
London SW7 2BP, England

Dear Dr. Diver:

Is there any chance you might still have a reprint available of "On the interpretation and classification of Precambrian organic-walled microfossils", Geology vol. 7, 1979? I could certainly use it in my teaching and research. At the moment I only have a xerox copy of dubious quality. I'm especially interested in the nascent systematics for the Cryptarcha.

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: 814-863-7823

15 April, 1993

Ms. Chris Dobias
Global Medical Communications
41 Madison Ave., 30th Fl.
NYC 10010

Dear Friend:

Your fax of 13 April in hand, and I of course remember your phone call.

It occurred to me that your needs might be served by the enclosed--at least, it's a place to start. Fig. 13 on the photo plate is equivalent to the figure on p. 9 of Research Penn State, which is a SEM picture. The SEMs on p. 10 are equivalent to the photos Figs. 2 and 7. I have original photos for the SEMs somewhere, if they are satisfactory. I am sure there is no copyright problem. The earliest pollen grains are much older--300 million years. They, in turn, are descended (really, of course, it's the producing plants that are involved-pollen can't descend from pollen) from spores. The earliest spores are at least 450 million years old. This batch has the advantage also of giving the size (50 micrometers is 1/20 of a millimeter because 1 micrometer is 1/1000 mm.). The smallest dot one can see with the unaided eye is 100 micrometers--but one can only then tell that it's a speck, nothing about the speck. If this appeals to you (I have relatively few pairs, with a SEM and a photo of the same thing), let me know quickly and I'll get on to rounding up the negatives in order to make good prints of all six items.

Yours very truly,

Alfred Traverse
Professor of Palynology

Hunt Institute for Botanical Documentation

GLOBAL
MEDICAL COMMUNICATIONS LTD.
41 MADISON AVENUE NEW YORK, NEW YORK 10010

FAX
2 pages

TO: Alfred Traverse Ph.D
Pennsylvania State University
Deike 435
University Park, PA 16802

FROM: Chris Dobias
Global Medical Communications
41 Madison Avenue, 30th Floor
New York, NY 10010
Phone: 212-889-6060
FAX: 212-889-6543

DATE: April 13, 1993

SUBJECT: Pictures of Fossil Pollen

Dear Dr. Traverse:

It was a pleasure speaking with you this afternoon. I will make sure that the word "pollens" does not appear in our book. Also, our librarian in borrowing a copy *Paleopollenology*.

As I mentioned over the phone I am gathering pictures for a book titled *Ancestors of Allergy: Portraits of the Pioneers* . The book is designed to be an illustrated history. I would like to include pictures of fossil pollen in the book and hope you can help me. SEM pictures are best because of the wonderful detail. Both group shots of several pollen grains or photos of single grains will be just fine. Please do not be afraid to send too many photos to review or hesitate to call me if you have any questions.

If you do not wish to send original photographs, photocopies of SEMs are generally good enough to give me an idea of what the pollen grain looks like.

**continued/next page**



Hunt Institute for Botanical Documentation

— Fax Transmittal Cover Sheet —

**Global Medical Communications Ltd.**
41 Madison Avenue, 30th Floor
New York, NY 10010
Phone: (212) 889-6060
Fax:(212) 889-6543

To: DR ALFRED TRAVERSE
Deike 435
Fax #: 814-863-7823
From: Chris Dobins
Date: April 13 '93
Time:

No. of pages (including cover sheet): 3

Message:

Please call Chris Dobins at (212) 889-6060 if you do not receive all pages or if some pages are illegible.

Hunt Institute for Botanical Documentation

*Ancestors of Allergy: Portraits of the Pioneers* is being published with an unrestricted educational grant from Marion Merrell Dow by Global Medical Communications. We are planning on a print run of 25,000. *Ancestors of Allergy* will be given as a gift to physicians in the US, Canada, Europe, Australia, New Zealand, and Japan.

Thank you so much for taking the time to help me. I look forward to hearing from you soon.

Very Best Regards,

Chris Dobias

CC: D. Moccio
L. Lafler
M. Rodriquez
L. Farrell

Hunt Institute for Botanical Documentation

Department of Geology-Geophysics
Pennsylvania State University
University Park, Pennsylvania 16802

18 August, 1966

Mr. Sterling N. Dobie
Whitsett, Texas

Dear Sterling (I can't remember whether I was permitted such familiarity—
but at this distance it can't get me in much trouble!)?

The package of specimens arrived today, and I am very glad to have them. The fern stem is one of the nicest I have ever seen--that's why I took a picture of it last summer, and I am very glad to have same.

The other stem is an opalized fern stem of, I am sure, a different species (and probably genus). I hope I can collect some more like it some one of these years.

Regarding Mr. Henke's specimen, it is a piece (apparently--I haven't made thin sections and all that, but we are agreed that there isn't much doubt) of fractured chert, the fractures ("veins") being filled with some form of quartz, probably mostly chalcedony. No wonder you remember this sort of thing from Mason County--I wonder, really, if this specimen didn't somehow get transported from up in that area. Chert is pretty uncommon in your area as native rock, unless my memory is faulty. Does Mr. Henke want the specimen back? You didn't say, which I take to mean he doesn't. But I will keep it around for a while and would be glad to send it to him if he wants it. All he needs to do is send me a postcard.

I have certainly enjoyed knowing you, and as I said earlier, I hope that we can get together again from time to time.

Regards to Mrs. Dobie and the others.

Yours very truly,

Alfred Traverse

Hunt Institute for Botanical Documentation

Department of Geology-Geophysics
Pennsylvania State University
University Park, Pennsylvania 16802

S. V. Dobie
Whitsett, Texas 78075

3 August, 1966

Dear Mr. Dobie:

I have thought of you many times this past year, and it was a great pleasure to hear from you again the other day.

As you can see from the above, my arrangement at the University of Texas turned out to be a one-year deal only, to my great disappointment. I loved Texas and wanted to stay, but I had the offer of a good position here and could seem to make no deal for myself in the Lone Star State, so facts had to be faced. For some time, SMU was considering taking me on, but that fizzled. The irony of the situation was that I was awarded a National Science Foundation grant to study the fossil plants of Texas in several different areas--a largish sum of money for equipment and field work. It seems odd to be quartered in Pennsylvania to do it!

Nevertheless, here I am. My wife and children are still in Austin trying to sell our house there and otherwise wind up our personal affairs in Texas.

I have an excellent position here, with considerable freedom to do my own research, along with quite a bit of teaching. I am, I think, a good teacher. In a sort of twisted kind of logic, that fact was actually used against me at the University of Texas. The fact that I was an effective lecturer and was popular with the students was taken as evidence that I must not be much good as a scientist--even tho' the national officials who decide such things thought enough of me to give me one of the largest grants ever dished out for study of fossil plants. I heard by grapevine that it "bugged" some of the other faculty members that I was a Christian clergyman in my spare moments, tho' nobody even suggested that I neglected my academic duties.

I intend to keep up my professional interests down your way despite my remoteness. Indeed, the NSF grant will make possible about a month in Texas both this coming year and again the following year. Whitsett is most certainly on the agenda. If I let you know plenty in advance, perhaps an assistant and I could occupy the cabin on your place for a day or two. Right now I don't know for sure when this would be, but my best guess is next spring.

I take it that you sent me (by parcel post) the section of fern stem. I am delighted! I made good use of the photos in some of my lectures

Hunt Institute for Botanical Documentation

last year. The package has not yet come, and I am going to get off a letter to UT immediately to be sure that I get it!

Sue Ellen is a charming girl, if I am remembering the right daughter—the one who was assisting Mrs. D. in the store? But if she is your daughter I am sure that she would be charming—I was much impressed by the sort of family life you have provided for your kids, and it is small wonder that the grandkids look forward to visiting so much.

Wasn't that a frightful thing at UT day before yesterday? I notice in the pictures that many of the victims fell in spots that I walked on or near practically every day. There is much talk of trying to stop that sort of thing by banning sale of firearms. But a fellow off his rocker could steal a gun, couldn't he? Besides, the people who are getting so excited about such a law are forgetting that the guy in Chicago last month didn't use a gun, for the most part. Are they going to ban the sale of kitchen knives and clothesline, too? No, the problem is quite different. The problem is that we have one way or another brutalized our society. And I fear that our constant waging of war—however true it may be that we have "had" to—plays some sort of role in the thing. It frightens me how much our culture in some ways begins to remind one of Germany's of some decades ago—same prevalence of murder and suicide, same looking toward military solutions for all problems with enemy countries. A resurgence of some of the Christian virtues would do a lot more good than gun laws.

Let's do keep in touch. Be assured of my continued interest in the natural history of Texas. I'll be in Whitsett to see you sometime during the coming year.

Best regards.

Yours very truly,

Al (I would like it if you'd call me that)

Alfred Traverse

(Associate Professor of Geology)

Hunt Institute for Botanical Documentation

Three Rivers, Texas
July 25, 1966

Dr. Alfred Traverse
Austin, Texas

Dear Dr. Traverse,

Our mutual friend Mr. Eugene (Pat) Henke visited me this week. He brought a dark-colored rock with oddly shaped markings and requested that I pass it on to you for possible identification. It is quite unusual for this section of Texas, though I seem to remember having picked up similar specimens when we lived in Mason County.

With his rock I enclose for you a fern stem which you "snapped" last September at the store. I intended to give it to you at the time, but failed to do so. Also included is a stem fragment with opalized exterior, which comes from McMullen County.

Sue Ellen attended the University Orientation session early this week, and came home full of enthusiasm. She is thrilled at the prospect of enrolling there in September.

I hope you can come back for another rock hunt this fall. We have gone out very seldom during this year.

With kindest personal regards,

Sincerely yours,

Sterling Dobie

Sterling Dobie

Hunt Institute for Botanical Documentation

August 19, 1965

Mr. S. N. Dobie
Whitsett, Texas

Dear Mr. Dobie:

I am sorry that it has taken so long to answer your nice letter of 26 July, regarding the fossil ferns about which you and Miss Girard have previously corresponded. Several field trips have intervened since I got your letter -- hence the delay. How would Saturday, 4 September, be for my visit with you? Friday, 3 September, would be equally good, if more desirable for you. I am looking forward to meeting you.

Yours truly,

ALFRED TRAVERSE
Geologist

AT/cjg

Hunt Institute for Botanical Documentation

Dear Dr. Traverse:

Shall I expect you Friday A.M. Suggest you come to Karnes City via Luling (#183 and 80), or by San Marcos (#123). From Karnes City take Farm Market #99 to Whitsett. Crooked, but much less traffic that way.

I have a copy of Dr. Arnold's treatise, but would appreciate seeing anything else that has been published relating to the fossil ferns.

We have a camp cabin on our farm 6 miles South of Whitsett, if you decide to "rough it" Friday night. Better wear wide-brimmed hats – this sun is hot!

Be seeing you SW Dodie

Hunt Institute for Botanical Documentation

THE UNIVERSITY OF TEXAS

BUREAU OF ECONOMIC GEOLOGY
THE LITTLE CAMPUS
18TH AND RED RIVER STREETS
UNIVERSITY STATION, BOX X
AUSTIN 78712

27 August, 1965

Mr. S. N. Dobie
Whitsett, Texas

Dear Mr. Dobie:

Friday, 3 September, would be fine. I should arrive about nine a. m., if I leave here at 6:00, as is my plan. I propose to bring my daughter, aged eleven, who is strong and helpful and interested in geology. She will help me carry sample bags and whatnot. I believe we will bring a couple of folding cots, in case we work until late on Friday, so that we could sleep on a porch (or someplace) in Whitsett that night, rather than driving back to Austin in the dark. I hate night driving!

Although I thought I had a complete run of Professor Arnold's papers (he is an old friend), I do not seem to have a copy of the one you mention. I can get one if it seems important to do so. I shall bring along some other publications that may be helpful to us.

Incidentally, I would be interested in other sorts of plant fossils and in outcrops of coal (lignite) which may be known to you.

I am looking forward to meeting you, and I hope that this expedition will not cost you too much time from your business.

Yours very truly,

Alfred T. Haverso

Hunt Institute for Botanical Documentation

Whitsett,

August 21.

Dr Alfred Traverse,

Austin, Texas.

Dear Sir:

If either of the dates mentioned in your letter of yesterday suits you, I think that Friday, the third would probably be best. My wife and I operate a country store in Whitsett, and Saturday is generally our busiest day. I have lined up one place where you might find the Stems and hope to have others available.

Do you know of Dr. C. A. Arnold's treatise on what he calls "Cyathodendron Texanum"? It is in "Papers of the Michigan Academy of Science, Arts and Letters"-- Vol. XXX, 1945. An interesting discussion of the fossils, and the only published materil I have read concerning them.

Feel free to come on Saturday if for any reason Friday proves unsuitable.

Yours sincerely,

Dr Dobie

Hunt Institute for Botanical Documentation

Whitsett Texas
July 26, '65

Dr. Alfred Traverse
Austin. Texas.
Dear Sir:

Miss Roselle Girard, Geological Assistant at the University, writes me that you are interested in the fossil fern stems found here. I have collected them for several years, and shall be glad to show you my specimens and help in every way possible anyone who has a scientific interest in them.

If possible, advise me several days before your visit. I shall try to get a permit from one of the owners of large ranch properties, where the fossils have not been hunted extensively.

I would suggest that you come down before September 25, when the dove season opens - certainly before the opening of the deer season (Nov. 1 in McMullen Co.; Nov. 16 in most others) There are too many reckless hunters in the woods after those dates!

Yours very truly
S. Whitbie

Hunt Institute for Botanical Documentation

THE UNIVERSITY OF TEXAS

BUREAU OF ECONOMIC GEOLOGY

July 23, 1965

Mr. S.N. Dobie
Whitsett, Texas

Dear Mr. Dobie:

Perhaps you will recall our 1963 correspondence concerning the fossil fern stems that you obtained from Live Oak and adjoining counties.

Last month a paleobotanist, Dr. Alfred Traverse, joined our summer staff (this fall he will teach at the University's Department of Geology here in Austin). When I told him of your plant fossils, he expressed great interest in them. He hopes to be able to visit your area sometime this summer or fall and attempt to collect some specimens in place. May he stop by and talk to you about this? Or perhaps you would wish to write to him at the above address.

Yours truly,

ROSELLE M. GIRARD
Geological Assistant

RMG:eh

cc: Dr. Alfred Traverse

Hunt Institute for Botanical Documentation

NATIONAL SCIENCE FOUNDATION

# PROPOSAL EVALUATION FORM

NSF Form 1B (9-81)
*Supersedes All Previous Editions*

| PROPOSAL NO. | INSTITUTION | PLEASE RETURN BY |
|---|---|---|
| EAR-8318943 | MERCER UNIVERSITY | |

| PRINCIPAL INVESTIGATOR | NSF PROGRAM |
|---|---|
| DOD BRUCE D | STRATIGRAPHY & PALEONTOLO |

TITLE
THE PALYNOSTRATIGRAPHY OF THE RICO FORMATION, COLORADO
PROBLEM

COMMENTS (QUALITY OF THE PROPOSED RESEARCH, RECENT RESEARCH ACHIEVEMENTS OF THE PRINCIPAL INVESTIGATOR(S), ETC.) CONTINUE ON ADDITIONAL SHEET(S) AS NECESSARY.

It is not pleasant to render a mostly negative judgment about anybody's proposal, especially when it is apparent that the proposer is obviously trying hard to get a program going where nothing now exists. However, in all honesty I must point out that the proposer has almost no experience in paleopalynology, and his "laboratory" lacks nearly all of the requisite equipment. I am not convinced by the proposal that either he or the undergraduate assistant who won science-fair awards for palynology in high school will be able to establish such a project ex nihilo. The stratigraphic research outlined is the sort of thing students do in our laboratory for term-papers--it ties in only slightly with bigger problems and with other laboratories. The lengthy, breathless description of more or less outmoded laboratory techniques and some naive things said about sampling do nothing to dispell my misgivings about the competence of the proposer.

In brief, if NSF has special funds for helping a place like Mercer get started, well and good--Dod should be encouraged to submit an equipment proposal to the appropriate division and try to learn palynology. But the very limited paleontological-stratigraphic funds of NSF should not be used to support this very weak proposal.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☐ GOOD ☐ FAIR ☒ POOR

Verbatim but anonymous copies of reviews will be sent only to the principal investigator/project director. Subject to this NSF policy and applicable laws, including the Freedom of Information Act, 5 USC 552 and formal requests from Chairpersons of Congressional committees having responsibility for NSF, reviewers' comments will be given maximum protection from disclosure.

REVIEWER'S SIGNATURE
Alfred Traverse

REVIEWER'S NAME (TYPED)
Dr. Alfred Traverse

OTHER SUGGESTED REVIEWERS (OPTIONAL)

REVIEWER'S COPY

Hunt Institute for Botanical Documentation

RUN DATE: 09/02/83

THE UNIVERSITY OF UTAH
SALT LAKE CITY 84112

UTAH GEOLOGICAL
AND MINERALOGICAL SURVEY
103 UTAH GEOLOGICAL SURVEY BUILDING

May 1, 1972

Dr. Alfred Traverse
Professor of Geology & Biology
Deike Building
Pennsylvania State University
University Park, PA 16802

Dear Dr. Traverse:

We would be delighted to have you continue studies on the Mississippian coal in the Manning Canyon. I suspect the shales should be equally prolific. However, the location is not Millard County but Summit County, on the north flank of the Uinta Mountains. There is coal in the Manning Canyon, and in Uintah County, south flank of the Uintas as well.

The location would be section 4, T. 2 N., R. 16 E., in Summit County near a place called Hole-in-the-Rock.

Cordially,

H. H. Doelling

H. H. Doelling
Economic Geologist

cl

Hunt Institute for Botanical Documentation

April 24, 1972

Dr. Helmut Doelling
Utah Geological Survey Building
University of Utah
Salt Lake City, Utah 84113

Dear Dr. Doelling:

Fred May, who recently completed requirements for an M.S. in geology under my direction here gave me a sample a while back of some "Manning Canyon Shale" which I have processed palynologically. It contained a beautiful flora which a student and I have looked at and have judged on a preliminary basis to be Chester in age. I believe Fred had said that it was suspected of being Mississippian, so the result should not be too surprising. The flora is really gorgeous and very interesting. I am wondering how you all would feel about the student and I going on with the study of the locality as time and funds permit? Could you provide a more precise location than Fred gave me, if you are agreeable? Fred's location is "coal prospect on Coal Mine Hill, East Millard County".

It is truly astonishing to me to find a Mississippian flora in Utah with so little (practically no) evidence of metamorphism!

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

17 September, 1984

Dr. Graham Dolby
Robertson Research Canada, Ltd.
604 1st St., S.W.
Calgary, Alberta, Canada T2P 1M7

Dear Graham:

Just a note to say thanks for lugging me home from the doctor in Calgary. Unfortunately I seem to have had a viral infection, and I needed not antibiotic but a couple of days' rest. That doesn't seem to be possible pre-retirement, so I'm still coughing but am much better. Thanks again.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

18 March, 1985

Dr. Jancis Dolby
Nova Husky
1411-25th Ave. N.E.
Calgary, Alberta, Canada T2E 7L6

Dear Friend:

Thanks very much for the reprint of your paper with John R. and Frances Parker (a 3-continent paper, wow!). As we are working in the local Devonian pretty regularly, I like to keep as informed as possible! Thanks for thinking of me.

I take it that Jancis Dolby is a junior synonym of Jancis Ford--I believe I recall that. Wish I could have talked you into keeping the original name for scientific purposes. Oh, well.

Have been (knock on wood) as healthy as a horse since Calgary laid me low in August!

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Date: Mon, 25 Sep 1995 14:16:09 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: ucfbjpd@ucl.ac.uk
Cc:
BCc:
Subject: refs
X-NUPop-Charset: IBM 8-Bit

Phil Dorling

Dear Phil:

It's fine to call me Alfred, and I like being called that. In North America male persons are only allowed one syllable, though female persons can have more--even four. So, I can be Al or Fred or Butch, or whatever, but not Alfred.

Lame excuse that you couldn't find an e-mail add. for me. Snail mail from UK here only takes three days (or so).

Anyhow, here is my first order response to the questions:

1. Goeppert, H. R., 1838. De Floribus in statu fossili. Commentatio Botanica. Vratislaviae, Typis Grassii, Barthii et Sociorum.

2. Ehrenberg, C. G., 1836. Ueber das Massenverhaeltniss der jetzt lebenden Kieselfusorien und ueber ein neues Infusorien-Conglomerat als Polirschiefer von Jastrabao in Ungarn. Abh. k. Akad. Wiss. Berlin.

3. de Jersey, 1965: this IS in the biblio of my book, under J, because de Jersey does not capitalize the D and until he does, it must be so alphabetized according to the rules I follow. Noel himself wants it carried under D, and he should go ahead and start capitalizing it.

4 & 5: "Muller 1773 and 1786." Could you help us with these? We have the entire text on WP 6.0 now, and we searched to no avail. Where do these refs. appear? Please send me info.

There is also a problem re Ehrenberg. He was very productive, and it is possible that the refs. you want are in E. 1837, of which separates were already available in 1836: Ehrenberg, C. G.. 1836/1837. Zusaetze zur Kenntnis grosser organischer Ausbildung in den kleinsten thierischen Organismen. Abhandlungen der k. Akademie der Wissenschaften (1835), Berlin, S. 151-180.

I am in the course of producing the second edition of the book, and will try to take care of problems such as these and I therefore appreciate your help.

Best wishes. Alfred.

Hunt Institute for Botanical Documentation

Received: from mail-a.bcc.ac.uk (mail-a.bcc.ac.uk [144.82.100.21]) by pangaea.em
Received: from ucl.ac.uk by mail-a.bcc.ac.uk with SMTP (PP)
          id <19968-0@mail-a.bcc.ac.uk>; Wed, 20 Sep 1995 18:54:50 +0100
Message-Id: <199509201755.NAA07836@pangaea.ems.psu.edu>
X-Sender: ucfbjpd@pop-server.bcc.ac.uk
Mime-Version: 1.0
Date: Wed, 20 Sep 1995 18:54:49 +0100
To: traverse@ems.PSU.EDU
From: ucfbjpd@ucl.ac.uk (Phil Dolding)
Subject: Re: refs
X-Mailer: <PC Eudora Version 1.4>
Content-Type: text/plain; charset="us-ascii"

>Several people have forwarded your request for refs. from my book. I guess
>that you assumed I am deceased and not available. I am now preparing for
>two palynology lectures later today. However, at the first possible moment
>I'll send you the info you want.
>
>Best. Alfred Traverse

Dear Alfred (If I may call you that)

I know you are still alive and kicking!
I wrote to you about 4 years ago, about possible
postgraduate studies in the US, but I managed to get
a studentship here in London.

I mailed the bulletin board, as I could not
find an e-mail address in any recent AASP literature.

Thanks for the message,

best wishes

Phil Dolding

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

30 December, 1990

Mr. Philip Dolding
192 Ravens Way, Lee Green
London SE12 8HD, England, U.K.

Dear Mr. Dolding:

Yours of 26 November was a long while getting here, and arrived just as our semester was coming to an end, followed by the holidays (still with us--I'm the only one in the building!).

I am not sure I understand fully what you have in mind. I am assuming what you're referring to is a doctoral program in palynology under my direction. I am asking the responsible person here, Linda Gruhn, to send you the usual materials about application. Admission is competitive. You will need to take the Graduate Record Examination ("GRE"), which Sheffield University probably administers.

At the moment I have only two doctoral students, one working on chitinozoans of Saudi Arabia, another on the K/T boundary in Texas. In the past I've had students work on everything from Ordovician to present (archeological palynology!). My own research tends to be in Devonian, Triassic or Paleogene.

So, that's it. If you want to come and if the admission committee decides they want you, I'd be glad to work with you, and you'd find Penn State a stimulating place. In UK, Professors Boulter and Chaloner in the London area know about our set-up intimately from visiting here.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: L. Gruhn

Hunt Institute for Botanical Documentation

Thank a [illegible]
Time to get here!
arrived 13-XII-90

192 Ravens Way,
Lee Green,
London,
SE12 8HD,
ENGLAND.

26th November 1990.

Dear Dr. Traverse,

I am writing to you after seeing the reference to your institution in the July issue of the AASP Newsletter. I would be very grateful if you could send me details of any postgraduate palynology courses you may have to offer.

I am currently about to present my dissertation, in completion of my M.Sc. in palynology at Sheffield University. I have been working on Upper Cretaceous (Upper Campanian - ?Lower Maastrichtian) sediments from the Northern Antarctic Peninsula, and recorded over one hundred marine and terrestrial taxa. I have found the work most enjoyable and rewarding and would welcome an opportunity to continue studying in this area of research.

I could send you a copy of my curriculum vitae and a more detailed synopsis of my dissertation, if desired.

Yours sincerely,

(Philip Dolding).

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEIKE BUILDING
UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
863-3419

July 22, 1988

Dr. Gary E. Dolph
Department of Botany
Indiana University at Kokomo
Kokomo, Indiana 46902

Dear Gary:

Many thanks for the PSBSA 87-Biblio, and for your reprints on leaf form, etc., enclosed therewith (or the other way around!). Obviously your students have fun studying botany with you!

I often think of you as I speed through Kokomo. My mother lives in St. Joseph, MI, and my mother-in-law is hospitalized in the Indianapolis area. So I do the US 31 bit several times a year. Occasionally, I go off into downtown Kokomo to eat a picnic meal at the city park along the stream.

Yours very truly,

Alfred Traverse

/jb

Hunt Institute for Botanical Documentation

19 November, 1984

Dr. Gary E. Dolph
Indiana University at Kokomo
2300 S. Washington St.
Kokomo, IN 46902

Dear Gary:

Thanks for the packet of reprints. I especially liked the very interesting article about leaf margins vs. climate. Reminded me of my courses with I.W. Bailey in the long-ago!

The computer articles make me feel nervous--I know we desperately need to be using such--for example for my thousands of card-recorded pieces of data, etc. But NSF has turned down every request, all proposals to oil companies have been declined, and Penn State will not provide same. I can raise $20,000 for a new microscope but not $6000 for the computer setup we need. "Computer illiteracy" is one thing, but how about "computer poverty"?

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

25 September, 1981

Dr. Gary E. Dolph
Indiana University @ Kokomo
2300 S. Washington St.
Kokomo, IN 46901

Dear Gary:

Just got back from Australia (XIII-IBC)--there was even a meeting of IAAP (Hickey, Christophel et al.)! When I returned to my office, the IAAP Bibliography was here--it's a beautiful job! But I sometimes feel overwhelmed by the job of staying afloat in the sea of new info.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

14 July, 1982

Alfred Traverse

Phil Dolsen

A "paleobotanical, coal petrological and palynological" open house' has been scheduled in connection with the AIBS August meeting at PSU to take place in Deike Building on Monday, 9 August, at 4:15 p.m. (to last about an hour). A check with Karen indicated that you would probably be here then and would be the logical person to coordinate any exhibits in connection with Coal Section work. We think that the same material you had out for EMEX would be fine, if that can be arranged. This note is just to alert you to the fact that there will be an open house. We will arrange palynological exhibits in the paly. lab and in 418. There will be refreshments in the hall outside my office in connection with the open house. Please pass the word among any interested Coal Section people. Everyone invited. I'll check with you sometime later this week or next about this.

AT/et
cc: William Spackman

Hunt Institute for Botanical Documentation

Phil Jolsen, C.P. 21 July. 1970

- Dear Phil —

You will be glad to hear that check from Longyearbyen. I have debited a check for $40.40 to the Coal Section for use of the vehicle, at 10¢ per mile. (Please tell Jr. Specimen)

Enclosed is a check to your account for £183.18, for:

£30.00 aluminum tubes
£3.18 plastic tapes
£150.00 your fee
£183.18

Will you acknowledge receipt (postcard fine)?

Best wishes,

AE

Hunt Institute for Botanical Documentation

2-VII-70

Phil, when I get check from Stein,
I will disburse to you:

$30.00 for alum. tube (you should
effect acct. out of $45.87
= $15.87)

3.15 for plastic tapes
$100.00 for your fee
$183.15

ok?

I also should pay to the section
$ 40.40, for the "car"

AT

Hunt Institute for Botanical Documentation

Pay Phil ± Section

TO: Mr. Martin E. Cusich

FROM: A. Traverse

FOR: drilling project at Stewart Bog

| | |
|---|---|
| Expenses per attached account | $ 172.69 |
| Charge for 2 days time, Alfred Traverse | 300.00 |
| Charge for 3 days time, Research Assistant (includes preparations for trip, plus clean up) | 150.00 |
| Total charge, payable to A. Traverse | $ 622.69 |

Hunt Institute for Botanical Documentation

# TRAVEL EXPENSE ACCOUNT

Name Alfred Traverse

Budget

Department of Geology
Organic Sediments Laboratories

Amount of Cash Advance

Period from June 24, 1970 to June 26, 1970

Purpose of Trip Obtain core of peat from Stewart Bog, Sharon, Pennsylvania.

If you have paid expenses of others
show names and explain here All expenses are for self and C.P. Dolsen, Research Assistant.

| Items | Sunday | Monday | Tuesday | 7/24 Wednesday | 7/25 Thursday | 7/26 Friday | Saturday | TOTAL |
|---|---|---|---|---|---|---|---|---|
| Taxi or Airport Limo. | | | | | | | | |
| Bus Fare | | | | | | | | |
| Airplane Fare | | | | | | | | |
| Railroad Fare | | | | | | | | |
| Pullman car (receipt) | | | | | | | | |
| Aluminum tubing for cores* | | | $ 30.00 ✓ | | | | | $ 30.00 |
| Lodging (receipt | | | | $ 22.32* | $ 22.32* | | | 44.64 |
| Breakfast, including tips | | | | | 4.22* | $ 4.37* | | 8.59 |
| Lunch, including tips | | | | 5.50 | 3.60 | 3.50 | | 12.60 |
| Dinner, including tips | | | | 10.95* | 9.00 | | | 19.95 |
| Field supplies: ice, etc. | | | | 9.75 | | | | 9.75 |
| Bridge[illegible] Plastic Tape | | | 3.18* | | | | | 3.18 |
| Telephone or Telegram | | | | .20 | | | | .20 |
| Registration Fee (receipt) | | | | | | | | |
| Sample jars | | | | 3.38 | | | | 3.38 |
| TOTAL | | | $ 33.18 | $ 52.10 | $ 39.14 | $ 7.87 | | $ 132.29 |

Privately-Owned Auto - Total Number of Miles 404 at $ .10 cents per mile 40.40

Total Expenses (Including Privately-Owned Auto) $ 172.69

| Itinerary | Sunday | Monday | Tuesday | Wednesday | Thursday | Friday | Saturday |
|---|---|---|---|---|---|---|---|
| From | | | | State College | | Sharon | |
| To or At | | | | Sharon | Sharon | State College | |
| To or At | | | | | | | |
| To or At | | | | | | | |
| Automileage | | | | | | | |

* receipts
✓ charged for tubes used only

Hunt Institute for Botanical Documentation

[illegible] 317 DRIKE ST
[illegible] PA

[illegible] 4081 N 6TH ST HBG PA

PRO. NO. 06-525202

SHIPPER'S B/L NO.

EQUIPMENT NO.

TRANS TO:

COLL

MFR SHARE 0 C/L SHARE 0

C/L CODE

CUST. CODE

| DESCRIPTION | WEIGHT | RATE | CHARGES |
|---|---|---|---|
| [illegible] 4 PC ALUM TUBING 3" IN DIA | 60 CWT COD PPR | | $ [illegible] |

$ 38.92 C.O.D.

Paid
CASH

C.O.D.

MOTOR FREIGHT EXPRESS
P. O. BOX 1009
YORK, PENNA. 17405

FOR ADDRESS AND PHONE NUMBER OF TERMINAL NEAREST YOU SEE YOUR YELLOW PAGES

Hunt Institute for Botanical Documentation

**O. W. HOUTS & SON, Inc.**
120 N. BUCKHOUT STREET
STATE COLLEGE, PA.

PHONE 238-6701

Date 6-23 19 70

Mr. & Mrs.

Address Cash

POSITIVELY NO REFUNDS OR RETURNS AFTER 10 DAYS

| QUAN. | | PRICE | AMOUNT |
|---|---|---|---|
| 4 | Roll Plastic tape @ 75¢ | | 3 00 |
| | tax | | 18 |
| | | | 3 18 |

Paid

64367

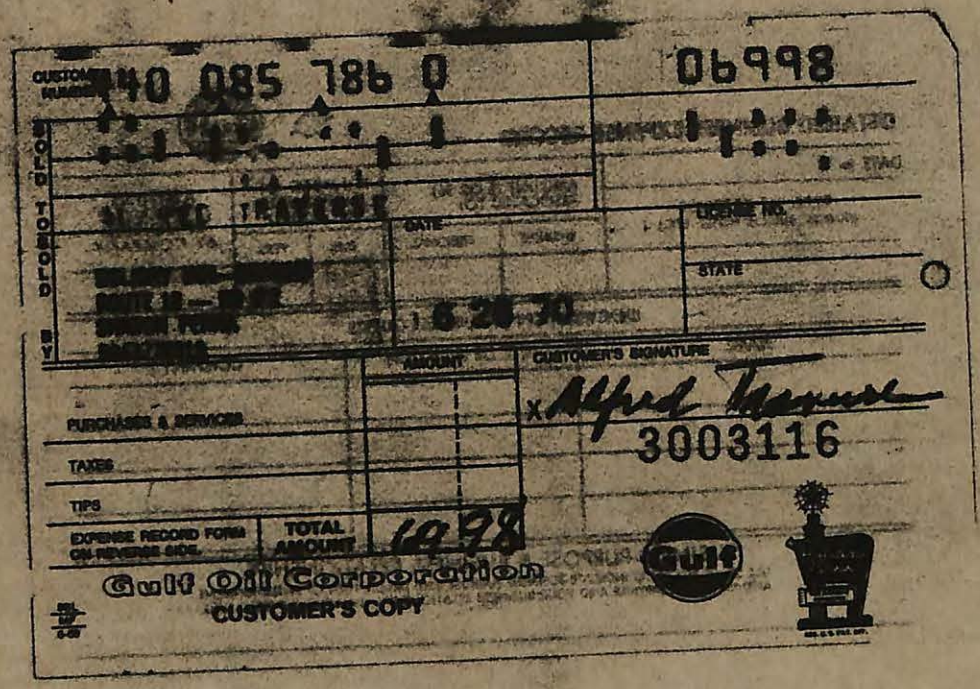
SOLD BY RECEIVED BY

TAX

TOTAL

ALL claims and returned goods MUST be accompanied by this bill.

Hunt Institute for Botanical Documentation




Hunt Institute for Botanical Documentation

333 Traveler's | 11/19 07067
DEPARTED 6/26 | DATE | FOLIO NO.
ARRIVED 6/24
FROM

| | | | |
|---|---|---|---|
| | | | 11.00 ROOM |
| | | | 00.66 TAX |
| | 524F320 | | 11.66 |
| | | | 11.66 |
| | | | 03.60 REST |
| | | LOCL | 00.20 PHON |
| | 524F420 | | 15.46 |
| | | | 15.46 |
| | | | 10.95 REST |
| | 524F425 | | 26.41 |
| | | | 26.41 |
| | | | 04.22 REST |
| | 525F571 | | 30.63 |
| | | | 30.63 |
| | | | 11.00 ROOM |
| | | | 00.66 TAX |
| | 525F708 | | 42.29 |
| | | | 04.37 REST |
| | 526F 17 | | 46.66 |




*Thank You - Call Again*

**Holiday Inn® of SHARON**

Telephone 981-1530, A.C. 412
3000 Hermitage Road
SHARON, PENNSYLVANIA 16146

Hunt Institute for Botanical Documentation

ROOM 337 Nelson

RATE 11.00 FOLIO NO. 07068

OUT DATE 6/26

ARRIV. 6/24

CLERK

FROM

TO

| | | |
|---|---|---|
| | | 11.00 ROOM |
| | | 00.66 TAX |
| | 324-321 | 11.66 CR |
| | | 11.66 CR |
| | | 11.00 ROOM |
| | | 00.66 TAX |
| | 325-710 | 23.32 CR |

Thank You - Call Again




**Holiday Inn® of SHARON**

Telephone 981-1530, A.C. 412
3000 Hermitage Road
SHARON, PENNSYLVANIA 16146

C22327D

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
UNIVERSITY PARK • PENNSYLVANIA

College of Mineral Industries
Department of Geology

July 20, 1962

Mr. Alfred Traverse
3737 Bellaire Boulevard
Houston 25, Texas

Dear Mr. Traverse:

Volume 15 of the Catalog of Fossil Spores and Pollen is back from the printer and your copy has been mailed, under separate cover, to you for review. I have also included a notice which will be sent to all of our subscribers explaining the increase in price for institutions and industry which the Advisory Committee recommended at their meeting last November.

I am pleased to inform you that the National Science Foundation has approved, for a two year period, our request for a renewal of the grant to assist in the publication of the Catalog. Contributions from the petroleum industry for 1962, however, have amounted to only $12,000; so our total funds will not allow the publication of the seven volumes we had hoped to produce this year. We anticipate the production of five volumes this year and perhaps more for 1963 if sales income, due to the new subscription rates, is substantially increased.

I hope your impressions of Volume 15 are favorable, but I will appreciate your comments in any case.

Very truly yours,

Phil Dolsen

C. Philip Dolsen
Business Manager

CPD:jes

Hunt Institute for Botanical Documentation

December 1, 1961

Mr. C. P. Dolsen
Business Manager
Catalog of Fossil Spores and Pollen
Organic Sediments Laboratory
Pennsylvania State University
University Park, Pennsylvania

Dear Phil:

All seems in order with Volume 14, and it is very good to see it in print. Keep up the good work!

I might point out a misuse of terminology in the Compiler's Preface. Circumscription is not the same as description. Circumscription is a somewhat philosophical notion of the boundaries of a taxon. Compiler meant, or should have meant, description in that first sentence.

Perhaps I would be allowed the somewhat bilious comment that the compiler should be more careful with split infinitives, which he seems to love: "to more adequately treat;" "to readily distinguish."

Best wishes to all of you. I certainly hated to miss the meeting of the Advisory Committee. I am looking forward to hearing what was accomplished.

Very truly yours,

Alfred Traverse

AT:pjh

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
UNIVERSITY PARK • PENNSYLVANIA

College of Mineral Industries
Department of Geology
Palynological Laboratories

November 8, 1961

Dr. Alfred Traverse
3737 Bellaire Boulevard
Houston 25, Texas

Dear Dr. Traverse:

Your copy of Translations of the Catalog of Fossil Spores and Pollen, Volume 1, has been mailed to you under separate cover. Volume 14 of the Catalog will be back from the printer this week and your copy mailed by early next week. We also hope to have a copy of the Cumulative Index of the first ten volumes of the Catalog in your hands well in advance of the Advisory Committee meeting so that you will have time to review all three volumes.

Sincerely,

C. Philip Dolsen

C. Philip Dolsen
Business Manager

CPD:ncs

Hunt Institute for Botanical Documentation

File Catalog 1 F.S.R.

# THE PENNSYLVANIA STATE UNIVERSITY

UNIVERSITY PARK • PENNSYLVANIA

College of Mineral Industries
Coal Research Section

October 13, 1961

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

Dear Dr. Traverse,

Thank you for your letter of October 6, 1961, containing your comments on volume 13 of the Catalog. I wish all the members of the advisory committee were as prompt with their comments.

Some of the errors, such as the misspelling of adjacent, are the result of our photocopying of the original texts. This is designed to reduce the chance of error on our part, but it carries through any errors from the original. Undoubtable, we have also made a few ourselves.

As for the missing pages, my face is red. Our usual method of spot checking for missing pages has worked reasonably well in the past but we have a new printer for this volume who obviously isn't so reliable. We will send you those pages and be sure to check all the rest of the volumes before we send any more out. Please accept my apologies.

The other matters you mentioned are beyond my knowledge and you will have to take them up with Dr. Spackman or Tate, perhaps at the Advisory Committee meeting.

Thank you again for your prompt action in reviewing volume 13.

Best regards,

Phil Dolsen

C. Philip Dolsen

CPD:ms

Hunt Institute for Botanical Documentation

October 6, 1961

Mr. C. Philip Dolsen
Palynological Laboratories
Pennsylvania State University
University Park, Pennsylvania

Dear Phil:

Got your letter of September 28, 1961, and also received the copy of Vol. 13 of the Catalog. It seems a creditable job, and I have found only a few things on which to comment -- not counting minor typographical errors such as the misspelling of adjacent on page 13 and 42.

I am a bit puzzled by the way you have handled Bhardwaj's publication of Cyclobaculisporites (13-42 and others). I don't have the original publications available here, but the situation looks a bit fishy. From what is presented in the Catalog, it would appear that the proper citation should be Cyclobaculisporites ovimammus Bhardwaj, as it would appear that Imgrund did not validly publish his thesis and that it has, therefore, no bearing on the nomenclature. Dr. Schopf, I am sure, would know the exact status of this deal.

Regarding the index: I do not see any purpose in listing specific epithets separately, e.g., eremus, Florinites. This is never done in any work on systematic botany (cf. Gray's Manual), and it just wastes space in the Catalog. I cannot imagine an instance where one would want to look up a specific epithet without knowing the genus to which it belonged.

Please note: the following pages were missing from the volume sent me, and I would appreciate your sending them to me - 13-41, 13-111, 13-153, 13-155, 13-157, 13-159, 13-161, 13-163, 13-165.

Best regards to Jane, to Tate and all.

Very truly yours,

Alfred Traverse

AT:pjh

Shell Development Company
Exploration and Production Research Division

bc: Dr. Traverse
Dr. Higgs

Hunt Institute for Botanical Documentation

November 8, 1960

Mr. Philip Dolsen
Organic Sediments Laboratory
Pennsylvania State University
University Park, Pennsylvania

Dear Phil:

With regard to sample 60-T3, peat from nine feet below surface of bottom, mouth of Harney River, Florida, which I obtained from you in September, the radiocarbon age is 3670 ± 150 yrs. You will note that this does not differ significantly from the age for 59-T3 (3250 ± 160 yrs) that we ran last year.

I am retaining one-half of the sample for pollen analysis, as previously agreed.

You presumably realize that I am sending you this data subject to the usual restriction on publication. Should these dates be published credit is to be given as follows: "Shell Development Company (A Division of Shell Oil Company) Exploration and Production Research Division, Houston, Texas).

Very truly yours,

Alfred Traverse

AT:mpd

Shell Development Company
Exploration and Production Research Division

bc. Technical Files

bcc. Information Copies
Dr. J. T. Smith
Dr. D. V. Higgs
Dr. A. Traverse

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
UNIVERSITY PARK • PENNSYLVANIA

College of Mineral Industries
Department of Geology

September 23, 1960

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

Dear Dr. Traverse:

Under separate cover I am sending you two cans of peat representing the lowermost three (3) inches of peat at Site T3, 1/2 mile offshore near the Harney River. They are labelled 60-T3 but are equivalent to the 59-T3, 9'depth, samples you had analyzed for us last Spring and that gave the anomalous age. If there is enough material in each can, it would be interesting to run them as separate samples, but they may be combined into one sample if more material is needed to make the analysis.

As Dr. Spackman may have told you, the National Park people are interested in some of our data, particularly the Carbon 14 dates, to be used to make museum exhibits on the interpretive history of the Everglades area. If this may be considered publishing, perhaps you had better send us the information for proper expression of credits.

Hope you have no trouble in convincing the proper people of the high value of the Carbon 14 ages obtained from these samples.

Sincerely,

C. Philip Dolsen

C. Philip Dolsen

CPD:jpd

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
UNIVERSITY PARK, PENNSYLVANIA

COLLEGE OF MINERAL INDUSTRIES
DEPARTMENT OF GEOLOGY

February 18, 1960

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

Dear Dr. Traverse:

I am sending you, under separate cover, six samples of Florida peat for Carbon 14 dating. These samples are from three different cores we obtained near the Harney River, just north of Shark River in southwest Florida. One core, 57-T1, was obtained just back from the shoreline while the other two were obtained offshore, one-half mile out (59-T3) and one mile out (59-T6). The relationship between these cores and the samples from each is shown in the accompanying diagram. Several of these samples are small but, I hope, large enough for the analyses. If not, I may be able to steal additional material from the sections earmarked for pollen analyses.

If this is more samples than you can readily handle for Carbon 14 analysis, please omit the top 3-inch sample from 59-T1, as this one is somewhat analagous to one of the surface samples you had analyzed for us last year. If two samples must be omitted, then exclude the 75"-78" 59-T1 sample also.

Perhaps I am assuming too great a familiarity on your part, with the southwest Florida coast. If so, I shall be happy to send you more precise locations on a map, as I did with the samples you so kindly had analyzed for us last year.

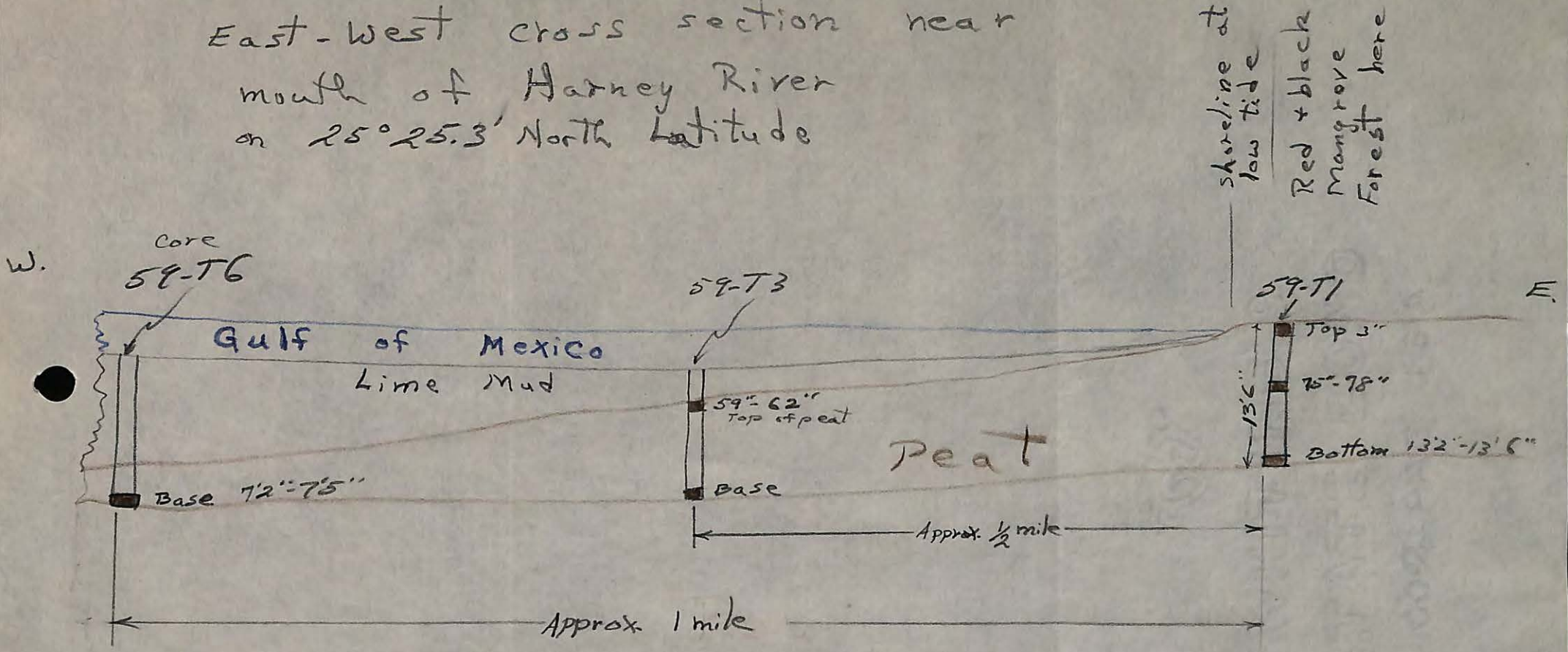
Sincerely yours,

C. Philip Dolsen

CPD:jd

Hunt Institute for Botanical Documentation

East-West cross section near mouth of Harney River on 25°25.3' North Latitude




59-T1 - 13.5 feet of sediment - all peat.

59-T3 - Depth varies within very small area - about 9 ft of sediment - 4½-5 ft of lime mud - the rest peat.

59-T6 - Depth varies again - about 7.5 ft. of sediment, only last 3 ft is peat.

The peat disappears entirely about ¼ mile out beyond 59-T6 site.

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
UNIVERSITY PARK, PENNSYLVANIA

COLLEGE OF MINERAL INDUSTRIES
DEPARTMENT OF GEOLOGY

November 3, 1958

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

Dear Dr. Traverse:

I am enclosing a copy of part of the U.S. Coast and Geodetic Survey Chart #599 with the locations of the two peat samples I sent you marked in red. The area around sample 58-C1 is dominated by Avicennia nitida while the area around 58-C2 is dominated by Rhizophora mangle.

Both samples are surface samples, going not more than 6 inches deep. When we first started our work in Florida we assumed that peat was still being formed under the mangrove forests. A study of the surface peats would thus enable us to recognize mangrove peats when they were encountered at some depth below the surface. Present environments could also be projected back. However, a more critical look at the area this past summer has caused us to doubt the assumption that peat is still being formed. There is very little surface litter to be seen. Also, the surface is completely awash at high tide and there is enough current in most of the area to sweep away new material that might form peat, despite the maze of roots. The peat that presently forms the surface seems to be too compact and altered to have just been formed. We are hoping that the carbon dating will settle the question.

I hope this is the information you need in order to submit our samples for analysis. If there is anything else I shall be happy to supply it at your request.

Sincerely,

C. Philip Dolsen

C.Philip Dolsen

CPD:jpd

Hunt Institute for Botanical Documentation

14 F
14 February, 1981

Dr. Howard V. Donahoe, Jr.
Regional Geologist
Nova Scotia Department of Mines and Energy
P.O. Box 1087
Halifax, NS, Canada B3J 2X1

Dear Dr. Donahoe:

The long delay is because your letter of 22.I.81 didn't reach me until yesterday. I am here (until June) on sabbatical.

My fame as a finder of Fundy Group palynofloras is mostly legend. True, we have tried very hard, but so far no success in Nova Scotia at all--only some palynofloras from the New Brunswick side, between Fundy Nat. Park and St. John. There several of my students and I have worked--see Ph.D. thesis of Bruce Cornet (1977;,borrowable from Penn State University). I must get this described and published as a top priority business when I return to PA. OOn preliminary opinion so far is Carnian. I have heard that Paul Olsen (vertebrate paleontologist, Yale) has got very poorly preserved pollen/spores from below the North Mountain Basalt at one locality in the Annapolis Valley vicinity. You might write him.

Nova Scotia remains the only extensive Triassic sedimentary basin I have ever worked in, in which my efforts have been more or less 100% frustrated--red beds are notoriously bad, of course, and the one sort of section that could (and did) contain abundant palynomorphs is cooked by the damned basalt. Do you know if there are any cores from the Bay of Fundy? My bet is they would go, and I'd love to have a crack at it.

Best wishes.

Yours very truly,

Alfred Traverse
Visiting Professor

AT/et

Hunt Institute for Botanical Documentation

NOVA SCOTIA
DEPARTMENT OF MINES AND ENERGY
P.O. BOX 1087 · HALIFAX · NOVA SCOTIA · B3J 2X1

January 22, 1981

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

Dear Dr. Traverse:

I am writing on the Stratigraphy and structure of the Triassic-Jurassic Succession (Fundy Group) on the north shore of the Minas Basin (west of Truro). Jon Bujak of Bedford Institute suggested that I write to you.

I understand that you and your students have sampled many strata in the Acadian Triassic for palynomorphs. I am writing to refine the biostratigraphic framework of Triassic-Jurassic rocks along the Parrsboro-Five Islands shore. Have you any dates from the Strata in this area? In particular any strata of pre-Carnian age? post-Hettangian age? Any information you provide will be fully acknowledged.

Sincerely,

Howard Donohoe

Howard V. Donohoe, Jr. Ph.D
Regional Geologist

HVD/vlj

Hunt Institute for Botanical Documentation

**West Virginia University** | MORGANTOWN, WEST VIRGINIA 26506

College of Arts and Sciences
Department of Geology and Geography

July 6, 1971

Professor Alfred Traverse
Deike Building
Penn State University
University Park, Pennsylvania 16802

Dear Professor Traverse:

I want to acknowledge and thank you for your letter of June 24 concerning the interest of Dr. Richard W. Hedlund to return to academic life from industry. We have filled all of our positions for the next academic year but will be hiring several men for the following years starting with the fall of 1972.

We are anxious to develop our coal geology program and probably will do it by joint appointments of faculty by the Geology and Geography Department as well as by the West Virginia Geoligical Survey. This arrangement would ensure a balance between teaching and research, and research which is both low and high risk. The Department is expanding and geology associated with coal, oil and gas will be a growth area. If Dr. Hedlund is interested in our program and wishes to be considered for a future position as a palynologist, then he should send me a letter accompanied by a resume including 3 references we can contact.

Thank you for thinking of us, I remain,

Sincerely yours,

Alan C. Donaldson

Alan C. Donaldson, Chairman
Department of Geology and Geography

ACD:meq

Hunt Institute for Botanical Documentation

June 24, 1971

Dr. Allan Donaldson
Department of Geology
University of West Virginia
Morgantown, West Virginia 26505

Dear Dr. Donaldson:

I have heard by the sometimes inefficient grapevine that you are contemplating adding a palynologist to your staff. If this is true, and if you want a man who is extremely well versed in all branches of geology as well, with considerable industrial experience, I would highly recommend your considering Dr. Richard W. Hedlund, of the Atlantic Richfield Company in Dallas, Texas. I know that Dick is a capable teacher as well as a first class research man, and I am also aware that he would like to make reentry into the academic profession.

With best wishes, I am

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

UNITED STATES
DEPARTMENT OF THE INTERIOR
BUREAU OF MINES

September 24, 1953

The question was submitted, "Is the iron disulfide present in the Zap column pyrite, or marcasite?"

These two forms of the compound can be readily distinguished as one or the other by means of x-ray diffraction analysis. Even though the characteristic series of "d-spacings" are not too dissimilar, the orders of relative intensity are quite distinct. In the A. S.T.M. index of diffraction data there are four patterns listed for both pyrite and marcasite. H owever, there is no wide disagreement among the different investigators.

I rechecked my data on the twelve samples of mineral matter in the Zap column. On all twelve patterns, each, of course, consisting of the superposed patterns of the several constituents, I was able to detect the lines of pyrite. Only on sample "J" is there a possibility that there is a mixture of marcasite and pyrite. Whether marcasite is actually present, or whether the lines suggesting its presence are merely the result of a fortuitous agreement is a problem that will be investigated further.

Also, I checked the diffraction data in R. Schellumbaum's thesis, wherein he reported finding pyrite in samples of North Dakota lignite. His data definitely indicate that the patterns are those of pyrite, and there isn't the slightest suggestion of any marcasite being present.

I do not have copies of his complete data. It would be interesting to check it against the A.S.T.M. index (not available when he performed the research.)

A. Donovan

Hunt Institute for Botanical Documentation

## $FeS_2$ in North Dakota Lignite

As pointed out by Sprunk and O'Donnell 1/ iron disulfide ($FeS_2$) is a very common mineral constituent of coal. It occurs in two mineral varieties, pyrite and marcasite, for both of which the general term iron pyrites can be used without signifying that the $FeS_2$ meant is known to be one ~~or the other~~ variety. Iron pyrites is a very common but not abundant constituent of North Dakota lignite.

Traverse 2/ has reported a petrographic analysis of a column sample of North Dakota lignite. 188 thin sections covering 92.2% of the 12-foot Beulah-Zap lignite bed, Dakota Collieries Mine, Zap, Mercer County, North Dakota, were analyzed and gave a trace percentage of recognizable $FeS_2$ as 0.3%. From the optical work on this column sample, there is no evidence to show whether the $FeS_2$ is present in the form of marcasite or pyrite. However, Donovan 3/ has used the x-ray diffraction technique on twelve sub-samples of the column sample used by Traverse for the microscopic petrographic analysis. Each sub-sample represented approximately one foot of the 12-foot vertical extent of the column sample. Small representative samples of the twelve sub-samples were ground in a mortar and put in $CCl_4$ for 4 - 24 hours. The sink fraction was used for x-ray diffraction study. $FeS_2$ was found in each of the twelve sub-samples. In eleven of the sub-samples the $FeS_2$ was certainly identified as pyrite, in one sub-sample there is a possibility that marcasite and pyrite are both present. Whether marcasite is present in this one sub-sample is being further investigated by the x-ray technique. It is evident that most, and perhaps all, of the iron disulfide in the Zap column is pyrite.

Wild and Schellenbaum 4/ report investigations of raw North Dakota lignite by the x-ray diffraction technique. They found $FeS_2$, which they identified as pyrite from the diffraction pattern. The exact origin of the lignite used was not given in the report. The authors also used the x-ray diffraction technique on lignite char briquets and on lignite ash, in neither of which $FeS_2$ was found.

Hunt Institute for Botanical Documentation

1/ Sprunk, George C. and O'Donnell, H. J. , Mineral Matter in Coal. U. S. Bureau of Mines, Technical Paper 648, pp. 19-31, 1942

2/ Traverse, A., A Method for the Petrographic Analysis of Commercially Delivered Lignite. Economic Geology, in press.

3/ Donovan, A. unpublished work, 1953. See Quarterly Report of Chemistry and Physics Section, Lignite Branch, Fuels Technology Division, Region V, U. S. Bureau of Mines. Report for July 1 - July 31, 1953, Subproject 192A.

4/ Wild, R. and Schellenbaum, R. X-ray Diffraction Studies of Mineral Matter in North Dakota Lignite. Proc. North Dakota Acad. Science. Vol. 5, pp. 40-42, 1951

Hunt Institute for Botanical Documentation

26 January, 1981

Dr. Jeffrey B. Doran
Provincial Museum
12845 102 Ave.
Edmonton, Alberta, Canada T5N 0M6

Dear Dr. Doran:

Just got your paper on Psilophyton. That is one of the most spectacularly illustrated paleobotanical papers in history! Congratulations! The locality is well known to me, as the Traverses come from Shigawake, Quebec (Gaspé), and I frequently visit up there (we use the ferry at Dalhousie).

The paleobotanist here, Dr. René Hantke, shewed my enthusiasm for your paper. Could you possibly spare a reprint for him also? (same address as shown here).

Yours very truly,

Alfred Traverse
Visiting Professor

AT/et

Hunt Institute for Botanical Documentation

February 10, 1971

Professor Erling Dorf
Department of Geological and Geophysical Sciences
Guyot Hall
Princeton University
Princeton, New Jersey 08540

Dear Erling:

Many thanks for the group of reprints which arrived recently. All of it is interesting. I suppose you probably guessed that I would get the most good out of "Paleobotanical evidence of Mesozoic and Cenozoic climatic changes". I have been reading it with considerable interest and know that I will be able to use the information in my teaching. Particularly, the illustrations will make very fine overhead transparencies, presuming in the absence of other information that you don't object!

Everything very hectic here with multiple committee assignments and teaching duties, but I am managing to get a little work done here and there on the surface sediments of the Black Sea.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/mdw

Hunt Institute for Botanical Documentation

Dear Al -

Many thanks for your note. I wrote Elso immediately. Tragic loss.

Your Houston snow-storm is "unusual" to say the least. Up to then, the heaviest snow-fall was less than 6". According to Visher's "Climatic Atlas of U.S.", subtropical climate in U.S. can expect up to 10" of snow per annum, and up to 20" of snow as its heaviest per annum. Heaviest 24-hour snowfall may go up to about 12". Keep trying!

Best of luck + regards. Sorry my visit to Houston was such a very short one. Sincerely yours,

Billy Doyle

Hunt Institute for Botanical Documentation

PRINCETON UNIVERSITY
PRINCETON, NEW JERSEY

*DEPARTMENT OF GEOLOGY*
CABLE ADDRESS: GUYOT, PRINCETON, N.J.

June 5, 1959

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

Dear Al:

Thanks for your letter and comments. I shall welcome all kinds of criticism, and hope you will offer more when I see you in Montreal.

About the use of "subtropical" for the region of southeastern U. S. I first ran into the problem of the difference between warm temperate and subtropical in the 30's when I was working on the Late Cretaceous floras. After much consultation and perusal of the literature I felt that I had to follow the terminology of the climatologists and geographers, nine out of ten of whom refer to the area of southeastern U.S., including dear old Houston, as "humid subtropical"; they call "tropical" the tip of Florida, etc., which you would refer to subtropical".

As to the middle initial and the Jr. - above my desk I have several lists of names and addresses, including the one in the Report of the Committee on Paleobotany, March 1954. When I made my final check of names, I referred to this list in which (p. 23) you are listed as Alfred F. Traverse, Jr. I have now crossed out the F., and the Jr. The membership list of the Geological Society of America, June 1958, has your name as Alfred F. Traverse.

Looking forward to seeing you in Montreal in August.

Sincerely,

Erling

Erling Dorf

Hunt Institute for Botanical Documentation

May 19, 1959

Dr. Erling Dorf
Geology Department
Princeton University
Princeton, New Jersey

Dear Erling:

Your paper, "Climatic Changes of the Past and Present," is interesting reading and is going to be helpful to me in pulling together climatic data on the Tertiary that is pretty much scattered heretofore. There are a few things that I would probably argue with you about. For example, in Map 7, I would, on the basis of several years botanizing in this general area, have strong doubts about the propriety of considering our climate really subtropical. It seems to me that the floristic subtropics ought to include those areas where some of the hardier tropical species are able to make a go of it, though obviously marginal, due to occasional cold weather, or too low average temperature, and other factors, such as intensity of insolation. If you use this criterion, you would have to pull your upper limit of subtropical in Map 7 much further south, to include only southern Florida, a narrow fringe of the Gulf Coast, where Avicennia, for instance, has a precarious hold, and extreme southern Texas, where, despite occasional frosts, Malpighia glabra and other tropical things are pretty well established. Because of the severity of the winters, there is nothing much subtropical about the vegetation in Houston, not to mention the northern parts of Texas that are included on your map.

As a minor gripe, may I complain mildly about the use of my middle initial and "Jr." You didn't get that from the publication in question. I have scrupulously stuck to the name printed below, because of confusion likely to result in later years. But occasionally someone does a little sleuthing and crosses me up.

Best wishes. Hope to see you in Montreal.

Very truly yours,

Alfred Traverse

AT:pjd

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

Oct. 15-5

Hi Al:

Sorry to have missed the excellent meeting in Indiana.

I was at that time enroute to Labrador where I collected about 20 species of fossil plants from red argillite beds associated with the Labrador iron ore deposits. The plants include conifers, ferns and dicots (mostly the latter) and appear at first glance to be Late Cretaceous. I hope to give a paper on this occurrence at Montreal next August.

Best of luck and kindest regards

Sincerely

Delevoryas

Dept

Hunt Institute for Botanical Documentation

May 12, 1958

Dr. Erling Dorf
Department of Geology
Princeton University
Princeton, New Jersey

Dear Erling:

Just opened the package of reprints you sent some time ago. They were awaiting my return from a recent field trip to Florida. Another factor in delaying their getting into my hands is that you sent them to my old North Dakota address. Fortunately, they were forwarded, but I should appreciate your amending my address on your list.

It was especially good of you to send the guidebook for the New Jersey field trip, which I had been disappointed not to be able to attend. Perhaps one of these years I'll be able to repeat the trip on my own.

Very best wishes.

Very truly yours,

Alfred Traverse

AT:mpd

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

August 1, 1957

Dr. Erling Dorf
Camp Roosevelt
Yellowstone National Park
Wyoming

Dear Dr. Dorf:

As he may have already told you, Professor Chaney has had a sudden change of plans that will prevent his being with us in Palo Alto for the meetings. In particular, he'll be unable to preside at the all day "symposium". He has suggested, and I strongly second the motion, that you preside in his place. As Dr. Chaney says, you will be able to give the program much the same slant as he would give it. I earnestly hope that you will agree to take over the job. Dr. Chaney says that he will "brief" you.

Really, the only thing this requires is your physical presence. Since your paper is scheduled for mid-morning on the 26th, I presume you would be on hand anyway. The idea was for the affair to be quite informal ("symposium" was probably a bad name for the sort of thing we had in mind).

I am sending a copy of this to your permanent address, so that it would be forwarded elsewhere, if necessary.

Very truly yours,

Alfred Traverse, Secretary
Paleobotanical Section, B.S.A.

AT:hmp

cc. Addressee - Princeton University

Hunt Institute for Botanical Documentation

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

January 20, 1954

Dr. Erling Dorf
Geology Department
Princeton University
Princeton, New Jersey

Dear Dr. Dorf:

If there are still copies available, would you send me a copy of "Analysis of Cretaceous Stratigraphy and Paleobotany of Atlantic Coastal Plain", Bull. Amer. Ass. Petrol. Geologists, vol. 36, No. 11, 1936?

Very truly yours,

AT

Alfred Traverse
Coal Technologist

ATraverse:dm

cc: Region V
Lankford
Traverse
File 083.3
C File

Hunt Institute for Botanical Documentation

January 19, 1956

Dr. Erling Dorf
Department of Geology
Princeton University
Princeton, New Jersey

Dear Dr. Dorf:

Thanks for the copy of "Plants and the Geologic Time Scale," which came recently. Apart from what is, I suppose, self-conscious annoyance at having plant megafossils made equivalent to fossil plants, I have thoroughly enjoyed reading the paper.

Best wishes.

Very truly yours,

A. Traverse

AT:djh

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

19 September, 1984

Dr. Ken J. Dorning
Pallab Research
58 Robertson Research
Sheffield S6 5DX,
U.K.

Dear Dr. Dorning:

I was very interested in your poster on palynomorph coloration-maturation at the 6 IPC, but never managed to get by when you were about to discuss it. Do you have a copy of the graph you could spare? Where would you put fungal spores on your chart?

Best wishes, and hope you can find time to respond.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

8 March, 1977

DearHenri Dorra
950 Estrella Drive
Santa Barbara, CA 93110

Dear Henri:

What excitement!--we are really delighted to hear that you are coming to Central Pennsylvania. There is a good chance that on March 24 you will find the weather all too reminiscent of your days in Boston. A month later would have been better planning! I have been in touch with the art department about your visit and have talked with both Dr. Hager and his secretary, Marcha Magno, (who seems to be therperson actually in charge of the arrangements).

The truth is, as you perhaps already have discovered, that it is very difficult to get to State College (University Park is just the campus post office and is actually surrounded by State College) by public transportation. Indeed, in March, air travel to our local terminal is so uncertain as to be not worth the trouble, in my opinion. If I were you I would either rent a car in Philadelphia (the best idea) or come by Amtrak train. The secretary seems to be under the impression that you are not only going to Philadelphia but also coming here from Philadelphia. It is about a five-hour drive and approximately the same time by bus. The train trip from Philadelphia to Huntingdon (where we live) is about four hours. There is, however, only one train a day in each direction.

I get the impression that I probably ought not to count on having you socially any time in the evening of the 24th--they will have things planned and are taking your lecture very seriously. Betty and I will certainly be there, but it will not provide an opportunity to visit with our old friend, H.D. We'll try to work something out for Wednesday or Friday before you leave. We would certainly like to have you stay with us at home, but we do not live near Penn State (a 35-minute commute probably seems near by California standards), so it might not be practical for the 24th--I'll leave it up to you.

Hunt Institute for Botanical Documentation

We are mindful of the fact that your real reason for coming is your obligation to the art people here, and we don't want to interfere with that in any way, but we are determined to see as much of you as possible. Consider us at your disposal--we'll try our best to fit into whatever plans you have.

Looking forward to getting more information, so we'llknow when to expect you. We are so happy to hear you are coming, though sorry that Mary will not be with you.

Best wishes as always.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: Marcia Magno

Hunt Institute for Botanical Documentation

950 Estrella Drive
Santa Barbara, California 93110
805- 682-3124

February 15. 77

Dear Betty and Al.

I was invited to give a lecture at Penn State on March 24; this would be a marvelous occasion to see you (assuming that you live fairly near the University.) The head of the Art Department is Hellmut Hager knows more than I do about the arrangements; I am sure he will give you all the information you may need & invite you to the lecture in case you want to come. I'll leave the 25th for Philadelphia.

Hager
229 Arts Bldg
5-6326

Asc. Marcia Magro

Hunt Institute for Botanical Documentation

Mary will not be with me. Unfortunately; someone has to stay with the children.

I do hope that we will establish contact.

Best to you both

Sincerely

Henri Dorn

Hunt Institute for Botanical Documentation

File Dörhöfer

XXXXXX

15 August, 1977

Dr. Brian Norford
Geological Survey of Canada
3303 33rd St. NW
Calgary, Alberta T2L 2A7

Dear Dr. Norford:

This is with regard to Gunter Dörhöfer's application to join your staff. I have come to know Gunter reasonably well from visits with him at meetings and, more recently, in his lab at the University of Toronto. My wife and I met socially as well with Gunter and his charming wife while in Toronto a few weeks ago. I know Gunter's work is good, as apparently you do as well. As to his personal qualities, I can say that I find him charming, friendly and possessed of unusual poise--a winning smile and good ability to relate with other people. While I have not worked with him, I can't imagine that I would be unable to do so. I certainly would be delighted to have him as a co-worker here at Penn State. What more can I say? Dörhöfer is really a first class guy and only the usual visa troubles have kept him from being snapped up by one of the oil companies months ago--I know that from conversation with some of my colleagues.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

26 May, 1978

Dr. Gunter Dörhöfer
Hindemithstrasse 1
D-3008 Garbsen 6
Fed. Rep. of Germany

Dear Gunter:

Thanks so much for "Palynologie...Bückeberg-Formation...usw." (Als ist Ihnen bekannt, lese ich ganz leicht deutsch. Die Übung war mir sehr hilfbar!)

I am amazed as usual by your productivity. It is good to know you accept our interpretation of the Corollina vs. Classopollis question. Rhetoric aside, there is no reasonable doubt it is the correct approach. Your paper will be very useful in our lab.

It is still a great disappointment to me that you couldn't see a way clear to stay on this side of the Atlantic. On the other hand, you may be glad ultimately. Things are not going at all well in North America.

All the best to you and your lovely wife. If I can do anything for you, please ask.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

28 April, 1977

Dr. Gunter Dorhofer
Department of Geology
University of Toronto
Toronto, Ontario, Canada M5W 1A1

Dear Gunter:

This is in response to your letter of (believe it or not) 26 February! I note that in the letter you seem to request a reprint of my paper from Marine Geology on Palynology of the Great Bahama Bank. I am sending it to you under separate cover.

I'm sorry to hear that as of the writing of your letter you had not had the success with your job seeking that you had hoped. Certainly if recommendation from me would have done any good, you would have the position, as I have a very high opinion of you personally and the quality of your work.

Hoping to see you again one of these days, and with very best wishes, I am

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

University of Toronto

DEPARTMENT OF GEOLOGY · TORONTO · CANADA · M5S 1A1

Dörhöfer, Gunter

February 26, 1977

Dear Al,

Thanks for your letter.

No, I did not receive any reprints from you, however, I am just now very interested in one. I mean the paper you did with Ginsburg on the Bahama palynology.

These Canadian winters have the advantage (the only?) of being long and cold. As there is not much you could do else, I spend presently most of the time writing an MS on the topic of my Halifax paper.

I think A has settled now long enough.

Regarding the job situation. I have applied with several oil companies, however, I haven't received an offer so far and, I guess, I have to wait a bit further.

Best wishes

Gun Fo

Sesquicentennial

1827-1977

Hunt Institute for Botanical Documentation

15 February, 1977

Dr. Gunter Dörhofer
University of Toronto
Department of Geology
Toronto, Ontario, Canada M5S 1A1

Dear Gunter:

Would you believe I just opened your letter of 6 January? It's a complicated story, and I wondt bore you with it. So much time has gone by that I am a little unclear about what I did regarding your request. In your first paragraph you mentioned my "unfortunate loss" which I take to be the loss of my notebook at Halifax. That's never happened to me before, and it did embarrass me because people made requests which I wrote down and then of course had no record of. Did I ever send you the reprints that you mentioned?

Regarding the opening at Penn State, What I was talking about was that the department of geology here is appointing people to temporary teaching posts at various ranks, mostly assistant professor, with the understanding between Penn State and the candidate that it is unlikely that the job will lead to a tenured position. Not imposible, but unlikely. I believe they advertised these positions at the GSA meeting, and in any case you could obtain more information by writing to the chairman of the department of geosciences at Penn State, Dr. C. W. Burnham. It would sure be fun for me if you were interested, but I can't really highly recommend the jobs under the circumstances. It would be a stop-gap measure.

Thanks for writing. Do let me know about the reprints (a postcard would be sufficient). I also look forward to seeing you in Tulsa if not before.

Yours very truly,

AT
pp

Hunt Institute for Botanical Documentation

**University of Toronto**

GUNTER DORHOFER, DEPARTMENT OF GEOLOGY · TORONTO · CANADA · M5S 1A1

Jan. 6/77

Dear H,

Let me express my sincere regret for your unfortunate bad. In fact, I saw you scrabbling down that you intended to send me some of your reprints.
Shortly before I left Halifax you told me, that there might be an opening at Penn State. What is it all about?
I very much enjoyed meeting you in Halifax and hope to see you again in Tulsa this year

Gunter

Sesquicentennial
1827-1977

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

Dr. Harry Döring
Institut und Museum für Geologie
und Paläontologie
Goldschmidtstr. 3
3400 Göttingen

den 2. Juni 1992

Sehr geehrter Dr. Döring,

es hat mir wirklich gefallen, Sie in Göttingen endlich kennenzulernen! Ihr Name und Ihre Arbeit sind mir seit Jahren bekannt, und ich hatte erwartet Sie in Berlin während dieses Aufenthalts in Deutschland zu besuchen.

Falls Sie Zeit hätten, möchte ich von Ihnen so viel wie möglich von der Geschichte des heutigen Stands und von der Zukunft von Paläobotanik/Palynologie in der Invalidenstraße (Museum und Anstalt, 43 und 44) zu hören. Es ist eine sehr interessante Geschichte und potentielle Tragödie.

Es ist offensichtlich zu spät, die Bibliothek in No. 44 zu retten, und das ist für die allgemeine Situation tragisch. Was könnte man tun, mindestens einen Rest von der Paläobotanik in Berlin zu bewahren?

mit besten Grüßen

Ihr

Alfred Traverse

AT/et

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

20 February, 1995

Dr. Ken Dorning
Pallab Research
58 Robertson Rd.
Sheffield S6 5DX
UK (0742) 337183

Dear Ken:

I note in CIMP Newsletter 47 (14-XII-94) that I'm supposed to get am abstract vol. for the CIMP 1994 Sheffield meeting. As far as I know, I haven't. Am I on the list?

Should you have e-mail capability you could respond that way, and it would kill a second bird with the same stone--namely giving me your e-mail address.

All the best. A few reprints enclosed. I find no evidence I've ever sent you any. I never get around to general mailouts, but always try to respond in kind.

Yours very truly,

Alfred Traverse

encl.: reprints

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

12 November, 1990

Dr. Ken J. Dorning
PALLAB Research
58 Robertson Road
Sheffield S6 5DX, England, U.K.

Dear Ken:

Saw your AASP abstract. Can you send a little more info re "Rivelin finder"? That's news to me. Your whole technique sounds interesting, but would be hard to apply with just the abstract.

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

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

9 May, 1988

Dr. Kenneth J. Dorning
Pallab Research
58 Robertson Road
Sheffield, England S6 5DX, U.K.

Dear Ken:

As you know from a recent letter, I am still waiting to get from you or Mary Dettmann the abstract for your most interesting title for Symposium 12, IPC-Brisbane.

You will recall that in the original invitation I mentioned the desirability of getting the platform MS ready for publication in the Proceedings volume (Elizabeth Truswell, editor). Such a paper would perforce be very brief.

However, I have been encouraged by Columbia University Press to get up a volume on palynomorph sedimentation based on some of the Brisbane papers (and a few others who won't be partipating at Brisbane, such as David Batten). A chapter by Dorning enlarging on the ideas from your Brisbane talk would be marvellous. Would you write one? At this point what I need (asap) is a yes/no, so that I can assess the feasibility of this project.

All the best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

30 January, 1985

Dr. Kenneth J. Dorning
Pallab Research
58 Robertson Rd.
Sheffield S6y5DX, U.K.

Dear Ken:

Many thanks for yours of 4 December, with more discussion of your fascinating curves, and assurance that (with credit, of course!) I can use them in the little textbook. The explanation of the "high start" for 2-3 is certainly reasonable, and I should have thought of it!

When I get the version drafted that I plan to use, I'll send a copy to you pre-publication, (If my erratic memory functions, that is!).

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

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

January 20, 1954

Mr. Edward Doucette
Division of Fuel Technology
Pennsylvania State University
State College, Pennsylvania

Dear Mr. Doucette:

The coal samples that I promised to send you are in the attached package. As per your request, each sample is 50 grams, or more. All of the coal has been crushed with a Williams swing-hammer laboratory mill to minus -1/8-inch size, and has been sent you in tightly stoppered 4-ounce jars. (Moisture loss seriously affects the characteristics of lignite and subbituminous coal.)

All of the samples, subbituminous and lignite, are from the Tongue River member of the Fort Union formation, and therefore represent a rather short time interval. The original material was presumably pretty much the same. The difference in rank must, therefore, be due to geochemical factors. Indeed, where differences in rank occur in coal of the same age, this is usually true.

We have available proximate, ultimate and B.t.u. analyses for all of these coals. If you will let us know as your work develops what data you would like for certain of these samples, we can send you the information. You will realize that we are sending you the samples in the expectation that your results will be available to us and may be of interest to us in our work.

Data on collection of these samples is appended.

Very truly yours,

Alfred Traverse
Coal Technologist

ATraverse:dm
Enclosure
cc: Region V
Lankford
Traverse
File 625.1
C File

Hunt Institute for Botanical Documentation

| Sample No. | Date of Collection | Nature of Sample and Mine | Coal Bed | Location From Which Sample was Taken | Comments |
|---|---|---|---|---|---|
| 1 | 6-12-53 | Subbituminous B coal, Colstrip mine (Northern Pacific Railway), Colstrip, Rosebud County, Montana | Rosebud | S. E. ¼ of S. W. ¼ of Sec. 12 - T. 1 N. - R. 41 E | Taken from a fresh face about 10 feet from the bottom of a 25-foot bed |
| 2 | 6-11-53 | Subbituminous B or C (probably B) coal, Big Horn Coal Co. mine, near Sheridan, Sheridan County, Wyoming | Local name: "Master vein" | S. E. ¼ of Sec. 1 - T. 52 N. - R. 85 W. | Collected from a fresh face: saw cuttings from a chain saw preparing "upper bed" for mining. Bed is about 5 feet thick |
| 3 | 6-11-53 | Subbituminous C coal, Wyodak mine, near Gillette, Campbell County, Wyoming | Smith-Roland | 600 feet S. and 700 feet W. of N. E. corner of Sec. 28 - T. 50 N. - R. 71 W | Collected from a fresh face about 15 feet from bottom of the 80 foot bed being mined |
| 4 | 6-16-53 | Lignite, Knife River Coal Mining Co., Peerless mine, Gascoyne, Bowman County, North Dakota | Harmon (also called Scranton) | Sec. 34 - T. 131 N. - R. 99 W. | Collected from a fresh face about 6 feet from the bottom of the upper bench of the bed ("upper bed"). The upper bench is about 10 feet thick and is separated from the lower bench by about 2 feet of clay |

Hunt Institute for Botanical Documentation

| Sample No. | Date of Collection | Nature of Sample and Mine | Coal Bed | Location From Which Sample was Taken | Comments |
|---|---|---|---|---|---|
| 5 | 6-17-53 | Lignite, Truax-Traer Dakota Star mine, near Hazen, Mercer County, North Dakota | Beulah-Zap | S. E. ¼ of Sec. 18 - T. 145 N. - R. 86 W. | Collected from a fresh face, about 6 feet from the bottom of the bed, which was about 15 feet thick. |
| 6 | Aug. - Sept. 1951 | Lignite, Baukol-Noonan mine, Divide County, North Dakota | Noonan | T. 162 N. - R. 95 W. (approximate) | From representative samples taken by periodic sampling of commercially delivered stoker-size coal used in gasification "run 17" at the Grand Forks station. The representative samples consisted of several 55-gal. drums of coal each. Representative samples of a few pounds were taken from these, using a large sample divider. This coal was crushed to minus -1/8-inch size and divided again in a small sample divider to produce the samples sent you. |
| 7 | Same as No. 6 | Lignite, Dakota Briquets and Tar Products, Inc., mine, near Dickinson, Stark County, North Dakota | Probably "E" or "F" | Sec. 17 - T. 139 N. - R. 95 W. (approximate) | |
| 8 | Same as No. 6 | Lignite, Western Dominion Mines, Ltd., Bienfait mine, Bienfait, Saskatchewan, Canada | - | - | |

Hunt Institute for Botanical Documentation

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

November 27, 1953

Mr. Edward Doucette
Division of Fuel Technology
School of Mineral Industries
State College, Pennsylvania

Dear Mr. Doucette:

Your letter of November 13 has been brought to my attention. It is as yet not possible to select a single coal seam in the Fort Union coal region (North and South Dakota, Montana, Wyoming and Saskatchewan) that is known to be lignitic in rank in one part of its range and subbituminous in another part. Samples of varying rank from seams that are very nearly the same age can, however, be obtained.

Would you let me know how many samples you would need and how much coal would be necessary? We have only a very limited amount of coal that would be suitable for your purposes. If we can provide samples that would be adequate, it would be interesting to us to acquire such information about molecular structure of the coal as you may learn.

You will perhaps be interested to know that the late Charles R. Robertson, for whom this laboratory was named, was the congressman from North Dakota who introduced the bill for establishment of the laboratory.

Very truly yours,

RP

Alfred Traverse
Coal Technologist

ATraverse:dm

cc: Region V
Lankford
Traverse
File 625.1
C. File

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE COLLEGE
SCHOOL OF MINERAL INDUSTRIES
STATE COLLEGE, PENNSYLVANIA

DEPARTMENT OF MINERAL TECHNOLOGY
FUEL TECHNOLOGY
METALLURGY
CERAMICS

RECEIVED
DEC 4 - 1
U. S. Bureau of Mines
Grand Forks, North Dakota

December 1, 1953

DOCUMENT BOND
CONTENT USA

Mr. Alfred Traverse
Coal Technologist
Lignite Research Laboratory
U. S. Bureau of Mines, Region V
Box LL, University Station
Grand Forks, North Dakota

Dear Mr. Traverse:

Thank you for your letter of November 27 and the offer to provide me with samples from the Fort Union Region.

I am disappointed to learn that samples are not available from one single seam of varying rank. My basis for this impression stems from a conversation with a geologist who studied the Powder River Basin in some detail. However, I am sure that coals of very nearly the same age would suffice for my study, providing, of course, that they have been exposed to the same geological conditions as have the Fort Union Coals.

The size of sample is actually not too important. I would like samples of about 50 grams, though 10 grams should suffice.

The number of samples is entirely dependent upon their selection. I am making this study on the basis of structural changes and need, for this reason, samples that are closely related in all respects. For this reason, the Dakota lignites were selected. If only a few samples are available, I would prefer to have them over a limited range with little but noticeable difference in rank. Any increase in the number of samples should serve only to expand this range of study. I would also appreciate any additional analytical information which you may have concerning the samples, such as Btus, etc.

I wish to thank you for your cooperation in this matter and to assure you that any information found will be made available to you.

Very truly yours,

Edward Doucette
Division of Fuel Technology

ED:bm

Hunt Institute for Botanical Documentation

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

January 15, 1954

Air Mail

Mr. Edward Doucette
Division of Fuel Technology
Pennsylvania State University
State College, Pennsylvania

Dear Mr. Doucette:

Since your letter of December 1 came, I have been busy preparing a paper for the A.A.A.S. meetings and have been away from Grand Forks two weeks. I have also been waiting for a letter from Dr. William Spackman about the nature of your work. This explains the long delay in sending the samples to you. We are getting the material ready today. The samples, plus an explanatory letter about them, will follow.

Very truly yours,

Alfred Traverse
Coal Technologist

ATraverse:dm

cc: Region V
Lankford
Traverse
File 625.1
C File

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

Dr. John G. Douglas
Geological Survey of Victoria
140 Bourke St.
Melbourne 3000, Australia

Dear Jack:

How nice to hear from you! You surely sound busy.

Your son, Ross, is most welcome here. We live in the country about 25 miles from State College, which is the closest place he would get to by Greyhound bus. (University Park is a part of State College.) It's on the main road, Chicago to New York (I-80). Ross might well also consider AMTRAK, the national railway passenger system. If you don't get a sleeper, its cost is comparable to bus. Our home is near a station (Huntingdon, PA) on the Chicago-New York run. There are two trains a day in each direction (New York-Philadelphia-Harrisburg-Huntingdon-Pittsburgh). Office address and phone above. Our home address is: RD 2, Box 390, Huntingdon, PA 16652; phone 814-643-1958. Ross should contact us ahead by phone or letter, as we travel a good deal, largely because of our scattered family and could possibly be out of town.

All the best to you and Anne.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

Department
of Industry
and Office
of Economic
Planning

In reply
please quote
Reference No.

JD:CG19 D.70

25 July 1990

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

Dear Al

At the moment I am frantically trying to finish off the IOP 1988 Proceedings consisting of 23 papers (one or two of very moderate quality) before I go off to two IGCP 245 Conferences; the first at Bucharest, the second at Alma Ata, USSR.

My boy Ross (24) is going to the Conferences with me, and is returning via USA. I believe he will travel by Greyhound, and may call in, as is the manner with the young, looking for a free feed and a roof for the night.

If he does contact you it will probably be some time in November, after he has had his look at this and that in Europe. He has been a bit of an idler (although vigorous enough to get a good job, and save for his trip) and I have encouraged him to travel. I intend to retire in November, and have arrangements in hand to get some space in Earth Sciences at Monash University, only a short bike ride away from home. I certainly would not finish in the next two or three years everything I have in hand, but with 2 or 3 days work a week should achieve something.

Hoping you are both well.

Regards

JACK DOUGLAS
Principal Research Geologist

Correspondence
PO Box 173
East Melbourne
Victoria 3002
Australia

Head Office
228 Victoria Parade
East Melbourne
Victoria 3002
Tel (03) 412 8000
Fax (03) 419 0770

Energy
151 Flinders Street
Melbourne
Victoria 3000
Tel (03) 412 8000
Fax (03) 650 9525

Minerals
115 Victoria Parade
Fitzroy
Victoria 3065
Tel (03) 412 8000
Fax (03) 412 7988

Overseas Offices
Frankfurt London
Los Angeles Tokyo

Other Offices
Bairnsdale
Ballarat
Bendigo
Dandenong
Doncaster
Footscray
Geelong
Mildura
Shepparton
Traralgon
Wangaratta

An equal opportunity employer

Victoria GROWING TOGETHER

4816(F1)

Hunt Institute for Botanical Documentation

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

6 July, 1989

Dr. John G. Douglas
Geological Survey of Victoria
140 Bourke St.
Melbourne 3000 Australia

Dear Jack:

Betty and I were stunned by the news about dear Enid. We had no idea that she was near the end of her life. We'll write Noel, of course.

We are fine. Had a great trip to Europe this spring.

All the best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

DEPARTMENT OF INDUSTRY,
TECHNOLOGY AND RESOURCES

All correspondence
to be addressed to —
Director-General, P.O. Box 173,
East Melbourne, 3002, Victoria

*Your Reference:*

*Our Reference:*

16/6/89

Dear Al & Elizabeth

Noel Schlieper may not have informed you that Enid died last February. (Cancer of the liver)

I did not know myself for two months, and have intended to notify you for some time, but keep forgetting.

Hoping that you are both well

Regards.

Jack Douglas

Contact:

228 Victoria Pde., East Melbourne,
3002, Victoria, Australia
Tel. (03) 418 8200, Telex AA33832
FAX (61-3) 4190770

Contact:

151 Flinders Street, Melbourne, 3000,
Victoria, Australia
Telephone (03) 653 9200
Telex AA36595 FAX 639525

Contact:

140 Bourke Street, Melbourne, 3000,
Victoria, Australia
Telephone (03) 665 0555
Telex AA36595 FAX 639525

Other Offices:
Dandenong
Footscray
Ballarat
Bendigo
Mildura
Shepparton
Traralgon
Wangaratta
London
Tokyo
Frankfurt
Los Angeles

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

5 January, 1989

Dr. John G. Douglas
Geological Survey of Victoria
140 Bourke St.
Melbourne 3000, Australia

Dear Jack:

Many thanks for the packet of reprints that came a while back and were a very pleasant reminder of you and your work. I was especially interested in the Sil.-Dev. thallophyte stuff, as geologists are always bringing me scraps of stuff from rocks of that age wanting me to decide: roots, algae, burrows? It's good to have pictures of some examples of what the thallophyte ones ought to look like!

Thanks also for the "unsolicited testimonial." You are a good friend.

All the best to you and Anne for 1989 from us both.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

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

12 September, 1988

Dr. John G. Douglas
Geological Survey of Victoria
140 Bourke St.
Melbourne 3000, Victoria
Australia

Dear Jack:

Congrats once again to you for a super meeting. You and Ann clearly knocked yourselves out over the thing, but now you can bask in the reflected glory!

The Tasmanian trip was a winner, not least for meeting the Schliegers from Melbourne. They are really nice, interesting people. Betty enjoyed the trips around Melbourne and nearby places--especially the sheep-shearing trip.

In Brisbane, at Noel de Jersey's place, we saw lots of wallabies and kookaburras. All because our accommodations were messed up by Uniquest, and poor Noel had to put us up for a day (silver lining!).

Regards to Ann. Include us on your next USA trip.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

DEPARTMENT OF INDUSTRY,
TECHNOLOGY AND RESOURCES

All correspondence
to be addressed to —
Director-General, P.O. Box 173,
East Melbourne, 3002, Victoria

*Your Reference:*

*Our Reference:*

17 March 1987

Prof. Alfred Traverse
Dept. of Geosciences
Palynological Labs
435 Deike Building
University Park PA 16802
USA

Dear Al

Thanks for your reply about my Berlin paper-you may be right about over-reacting, but I like to do things properly, and facilities are so overstrained here that I'd have had to do everything myself, which wasn't the situation a year or so back.

However the issue is hypothetical now anyway. Three weeks or so ago I got a letter from Berlin saying I was selected for oral presentation in Symp. 5-32, without any prompting from me.

I don't know if that's good or bad, but that's the situation now, and I'm speaking on "Southern Hemisphere Fossil Flora".

Thanks for the encouragement. See you over there all being well.

Regards

John Douglas
PRINCIPAL RESEARCH GEOLOGIST

Contact:

228 Victoria Pde., East Melbourne,
3002, Victoria, Australia
Tel. (03) 418 8200, Telex AA33832
FAX (61-3) 4190770

Contact:

151 Flinders Street, Melbourne, 3000,
Victoria, Australia
Telephone (03) 653 9200
Telex AA151681 FAX 639525

Contact:

140 Bourke Street, Melbourne, 3000,
Victoria, Australia
Telephone (03) 665 0555
Telex AA36595 FAX 6636325

Other Offices:
Dandenong
Footscray
Ballarat
Bendigo
Mildura
Shepparton
Traralgon
Wangaratta
London
Tokyo
Frankfurt
Los Angeles

ITR IS AN EQUAL OPPORTUNITY EMPLOYER.

Hunt Institute for Botanical Documentation

26 March, 1986

Dr. J. G. Douglas
Geological Survey of Victoria
ITR, GPO Box 173
East Melbourne 3002, Australia

Dear Jack:

I'd like a copy of 1st Circular re 3rd IOP Conference.

Thanks.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Northern Arizona University · FLAGSTAFF, ARIZONA 86011

GEOLOGY DEPARTMENT BOX 6030

January 15, 1984

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

Dear Dr. Traverse:

The Kayenta flora has remained an enigma for years. Graduate students as well as professional palynologists have spent months sampling the beds. The mammal quarry contains only ghosts of one or two forms. I am currently running two localities I consider "last resort" samples.

With best wishes,

Very truly yours,

William R. Downs

P.S. I do not hold a Ph.D.

Hunt Institute for Botanical Documentation

21 December, 1983

Dr. W. R. Downs
Department of Geology
Museum of Northern Arizona
Flagstaff, AZ 86001

Dear Dr. Downs:

Just read with considerable interest the article by you and others in Science. Some of my students and I have for quite a long time been working on the palynostratigraphy of Triassic-Jurassic rocks of Arizona and elsewhere. How are the chances of a chunk of the siltstone associated with the vertebrate remains for us to have a go at the spores/pollen? All we need is, say 100-200 grams. It would beinteresting for us and would provide an additional fix on the age.

Hoping to hear, and with best wishes, I am

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

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

12 March, 1987

Dr. James A. Doyle
Department of Botany
University of California
Davis, CA 95616

Dear Jim:

Thanks for the copy of Bruce's letter to you of 28 Jan., accompanied by your interesting handwritten note of 2 Feb.

I was reminded that I (probably) hadn't responded, by a phone call the other day from Peter Hochuli (once a colleague at Zürich, now at Bordeaux, France). He has some "angiospermid" pollen from the Ladinian (Lower Triassic or Mid-Triassic if you use one) and wanted to know if Bruce's stuff was ever published. I related the whole tale of woe.

Bruce's letter to you seems to indicate that my previous suggestion that he might be talked into a redo by you is realistic. For Bruce, in the mood he was in, that was a very conciliatory letter. Of course, I speak as one who dealt with BC on a regular, daily basis for years. (Can you believe it?)

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Dear Al -

More ESP - I got your letter just as I was about to mail off the enclosed answer to this blast from Bruce. Many would say that it's a waste of time to respond to something that could serve as such a beautiful example in a psychology textbook, but in the past Bruce has gone through more lucid phases, and if I catch him in one of them my remarks might have some effect.

-Jim (Feb. 2 1987)

Prof. James A. Doyle
Dept. of Botany
Univ. of California
Davis, CA 95616

January 28, 1987

Dear Jim,

(file Doyle)

Enclosed please find a reprint of my latest paper dealing with evidence for pre-Cretaceous angiosperms. You probably have heard already about it, and may even have read it. It is unfortunate that this work could not appear in a journal with better plates, but the manner of publication may well turn out to be an historic commentary on the current hostile environment to unpopular ideas on angiosperm evolution. I have been very thorough and careful with my descriptions and interpretations. I believe that they accurately reflect the morphology and anatomy of this plant. Additional material collected last year adds substantially to our knowledge of Sanmiguelia, and convinces me, at least, that I am dead on target. S. lewisii is an angiosperm, albeit a primitive one, based on carpel and anther morphology and ontogeny. Yet it supports neither the magnoliid theory of angiosperm evolution nor the pteridosperm theory of angiosperm ancestry. There is evidence now for a transmission tissue that was attached to one side of the ovary wall, plus a 1 mm long silicified anatropous ovule (from a partly silicified carpel) that may well answer questions about the gametophyte and fertilization. Additional mummified examples of Synangispadixis prove that the microsporophylls were biloculate and paired, and that the pollen sac wall and septum between the two sacs broke down during ontogeny. Never before has a fossil seed plant provided so much morphological, anatomical, and ontogenetic information that relates directly to early angiosperm evolution.

I am unsure about how you will treat this paper, since your latest views seem to be inclined toward a Triassic origin of angiosperms. You may recall the Magnolia-like fruits from the Richmond basin reported in the paper, which I had described to you in old letters. They seem to fit in very well with what Sanmiguelia (Axelrodia) is telling us, despite any discrepancies with the conduplicate theory of carpel evolution. No matter how well documented or convincing a theory on angiosperm evolution based on the study of living angiosperms, the fossil record will be the final judge of our abilities as scientists. Regardless of how you treat my paper on Sanmiguelia, the facts remain; the goose is cooked; the cat's out of the bag. All that remains to be done is further documentation and clarification of what has already been revealed.

Turning to the subject of my paper on Triassic and Jurassic angiospermid pollen evolution, I am very disappointed in your abuse of reviewer authority to reject my paper. We are passing through a dark period in the history of American paleobotany and

Hunt Institute for Botanical Documentation

palynology when reviewers will censor papers mainly because they disagree with the interpretations of an author. You and I both know the reasons behind your rejection. It is quite commonplace these days for the authorities on a subject to bias the literature in favor of their preferred opinions and theories. If you felt that I was incorrect in some of my interpretations and viewpoints, you should have requested that I present both sides of the issue. I may have treated published statements of yours somewhat sternly, but they were wrong. Perhaps I did accomplish my purpose by shooting straight at the heart of the problem so that you might refrain from making absolute statements about when the angiosperms didn't exist or about what fossil angiosperms can be called early angiosperms. There is more than enough room for differing and contrasting viewpoints in paleobotany and palynology, but why, as Burger has said, should there be a winning viewpoint that excludes alternative viewpoints? I have something to contribute other than sterile systematic descriptions, and you have purposely ignored my interpretations in your publications, referencing only my 1979 abstract. That, Jim, in my opinion is very poor science!

In am not sure when my pollen paper will be published. It is no longer a problem of trying to prove the existence of Triassic angiosperms based only on pollen. Now I can concentrate on what the various Triassic pollen types are telling us, at least about pollen evolution. Don't expect systematic papers on my pollen to appear anytime soon; I have decided to dig in and remain entrenched in my position that the paper you reviewed or an updated version of it will have to appear in print before I devote any time to systematic descriptions. How long can you wait? Do you want just the descriptions without interpretations, or are you afraid that my ideas may turn out to be correct? Keep in mind, that the longer it takes for that paper to be published, the closer it will appear in time to the revolution in thinking about angiosperm evolution that is destined to occur (it may well have already started). How you could think that the absence of well documented (convincing) pre-Cretaceous angiosperms meant that they did not exist is a fascinating problem that has driven me to blind side you with the Sanmiguelia paper. And if you say that you thought all along that angiosperms probably existed earlier, but that they had to be adequately documented first before your writings would reflect a change in opinion, your motives will appear more political than scientific. The rejection of papers that question your ideas and those of Hickey, Hughes, Brenner, Dilcher, Walker, Wolfe, Muller, Niklas, Tiffney, Scott, Leopold, and Barghoorn about a Late Jurassic-Early Cretaceous origin may well appear in historical perspective as attempts to preserve and maintain the reputations of those in positions of authority.

Your latest papers on cladistics show that you are groping for the missing pieces to the puzzle. It is fascinating how your search is bringing you back to the Triassic. I am now working on a new discovery, also from the Sanmiguelia locality in Texas,

Hunt Institute for Botanical Documentation

that may very well provide some critical pieces to the angiosperm puzzle. If you think that my speculation in the paper about carpel evolution is folly, think again! I may have the inflorescence to Pelourdea, an herbaceous plant with lanceolate Welwitschia-like leaves. Preservation is excellent, and the implications so profound and far reaching that my publication on this plant may add significantly to our understanding of how the angiosperm carpel evolved. It is a missing link that combines features thought to be characteristic of the Bennettitales, Gnetales, and Angiospermae. It provides evidence for the origin of angiosperm anthers, and also Synangispadixis. Ironically, it provides little support to the conduplicate carpel theory, but gives clues to the origin of the ventral suture, glandular stigma, and transmission tissue. It does not support any relationship between angiosperm ancestry and Caytonia.

You have the advantage in publication and your establishment as an authority in angiosperm evolution. I have the advantage in possessing the evidence for pre-Cretaceous angiosperm evolution and possible origin. I am on the rise while you are trying to maintain your position and recognition as an authority. Sooner or later our paths will cross again, and I may have to reflect on all the past injustices that have been done to science and myself through prejudice, bias, and abuse of authority.

I offer a possible solution to our differences as a way for us to both benefit. I suggest that you write to the editors of one or more good journals, suggesting that they invite me to publish a paper on pre-Cretaceous angiosperm pollen evolution. I, in return, will rewrite my pollen paper, omitting all references to past mistakes and undersight, and give an objective presentation that will contrast my interpretations with yours, as well as show where we agree. I will also provide a small section briefly describing (but not naming) the taxa in systematic fashion. The main purpose of the paper will be to present possible trends in angiosperm pollen evolution through the Mesozoic, and to set the groundwork for more detailed systematic papers on each (stratigraphically and chronologically separate) pollen assemblage. Too much work has gone into the original paper for me to abandon it now, and I am too upset with the attitudes of those in authority to surrender to their demands. If I must wait until I have enough money to publish the paper on my own, it will contain an acid review of historical blunders, narrow-mindedness, prejudice, and botanical politics. The choice is yours: Help me to get the evidence out now in an objective presentation, or risk what may come out later.

Sincerely yours,

Bruce

Bruce Cornet, M.S.,Ph.D.
14222 Kimberly Ln. #411
Houston, TX 77079

Hunt Institute for Botanical Documentation

UNIVERSITY OF CALIFORNIA, DAVIS

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF BOTANY DAVIS, CALIFORNIA 95616

February 2, 1987

Dr. Bruce Cornet
14222 Kimberly Ln. #411
Houston, Tx 77079

Dear Bruce:

Thanks for the reprint of your very interesting paper on **Sanmiguelia**; I had seen Dan Axelrod's copy, but I had hoped to get one of my own. I'm not completely convinced that it's an angiosperm, but it certainly looks more likely to have something to do with the origin of angiosperms -- whether as an angiosperm or as some member of the "anthophyte" group between full-fledged angiosperms and their closest previously known gymnosperm relatives -- than anything ever reported. I think you may have overinterpreted certain morphological details, including some important ones (e.g., gynoecial structure), and there are a few features that may be obstacles to considering it a link between Gnetales and angiosperms. However, this is no condemnation: if everything about your reconstruction and systematic placement of this plant turns out to be correct on this first run-through, it will be unique in the history of paleobotany.

Concerning your pollen paper, I really don't think that I abused my reviewer authority. The whole purpose of the review system to decide whether papers are published based on colleagues' judgement of the scientific validity of their data **and their interpretations.** I assure you that the arguments in my review are the true reasons for my judgement; I wrote such a detailed review so that it would be clear to any third party that I wasn't rejecting your ideas just because they disagreed with my own. I think my arguments stand on their own merits (as apparently the editor did too), and I don't see you contesting them, only my motives. If my real motive was fear that your paper would damage my reputation or status as an "authority," I would have to be a fool; surely I would have to realize that the truth about Triassic angiosperm pollen would eventually come out (already other people are starting to publish pictures of similar things). Furthermore, if you can bring yourself to read my review over carefully, you will see that I stressed the positive aspects of your work as well as the negative ones. Believe it or not, there have been times when I've defended your discoveries as the best evidence of Triassic angiosperms to date against people who wanted dismiss all your work out of hand. It's ironic that I've made more effort to treat your work seriously than anyone I know* (did anyone else ever write such long replies to your letters, or such a detailed review of your pollen paper?), and for this I end up catching the most flak.

At a more philosophical level, people who know me and have read my papers carefully know that I have been one of the Cretaceous workers least hung up on refuting pre-Cretaceous angiosperms. Since my first paper I have never made "absolute statements about when the angiosperms didn't exist," but have instead tried to emphasize what was going on in the Cretaceous and what it

*I'm sure you're an exception - sorry for the oversight!

Hunt Institute for Botanical Documentation

said about trends and relationships within the angiosperms. All I've ever claimed about the age question is that the Cretaceous record indicates that any earlier angiosperms must have been much more primitive and undiversified than was once thought, a conclusion you seem to accept. (Other people have taken my work as evidence against pre-Cretaceous angiosperms, but that's their problem.) As for rejecting specific pre-Cretaceous angiosperms, there's a big difference between applying high standards for proof and being dogmatic, or between saying that something hasn't been shown to be an angiosperm and that it isn't one. With so many cases where people have been embarrassed by pre-Cretaceous angiosperms that were either not angiosperms or not pre-Cretaceous, history suggests this is an area where it's better to err on the side of caution.

The reason that I have not referenced more of your ideas in my papers is very simple. In my experience, more people get upset about being "scooped" by others citing their unpublished ideas than the reverse, especially when the person doing the citing goes on to criticize them. Furthermore, when I suggest that you should publish a descriptive account of your work first, I don't mean "sterile systematic descriptions"; I mean photographs and presentation of your most incontestable morphological observations (I think you may be confusing my philosophy with that of your former mentor). I think your **Sanmiguelia** paper is a lot better in this regard. Speaking as one who has had ideas that were once unorthodox, it is usually prudent to publish one's most solid results first and then go into the speculations later.

Finally, there's no need for any deal of the sort you offer (which incidentally would be highly unorthodox in science). If you wrote a paper that **really** dealt with my criticisms (which might mean reducing your MS by only a third), I don't think you'd have any trouble getting it published without any intervention by me. Also, I don't think I'd have any trouble recommending it for publication if I got it to review, even if it was full of references to my past errors (assuming of course they were errors I had actually made). Conversely, I'd not hesitate to make the same criticisms again if I thought they were still valid, even under threats of exposure of my alleged crimes against science. If you published a paper with even a fraction of the acidity of your letter, you would damage your reputation much more than mine. On the other hand, it couldn't hurt to take my criticisms as suggestions of ways to get your work taken seriously by others in the field, whatever you think were my motives in making them.

Sincerely,

James A. Doyle

Hunt Institute for Botanical Documentation

Al -

Nov. 7, 1986

After hearing [illegible] second hand things that Bruce is saying about this, I thought I'd like you to have a copy, to judge for yourself. -Jim Doyle

free concept

Review of
"Triassic and Jurassic Pollen with Angiospermous Affinities" by Bruce Cornet

This paper reports some of the most interesting observations related to the origin of angiosperms to be made in recent years. Some (though not all) of the pollen types illustrated show the most angiosperm-like features known in any pre-Cretaceous fossils, and as such they constitute the strongest candidates to date for representatives of the angiosperm line before its well-documented "explosion" in the Cretaceous. Whether or not they are angiosperms, they certainly render obsolete the statement that distinctively angiosperm-like pollen has not been found before the Cretaceous. Furthermore, as Cornet recognizes, the combination of angiosperm- and Gnetales-like characters in several of these types takes on new significance in light of recent cladistic analyses indicating that angiosperms are more closely related to Gnetales than has been widely assumed, although as sister group of Gnetales (Crane, 1985) or of Bennettitales plus Gnetales (Doyle and Donoghue, in press), rather than by polyphyletic origin of angiosperms from early Gnetales, as proposed by Cornet. The presence of columellae and tectal perforations in these grains will be evolutionarily interesting even if it turns out that they are gnetalian (which is especially likely for **Equisetosporites**), since it would mean that the potential for these characters is still restricted to groups that are close to the angiosperms. However, because of severe problems in practical interpretation of the observations, misinterpretation of arguments and views of other workers, selective magnification of some pieces of evidence while ignoring others, neglect of alternative hypotheses, and other basic errors in scientific reasoning, I cannot recommend publication of the paper in anything like its present form. In the following review I will not deal with the many stylistic and organizational problems, but will focus instead on the scientific issues; in any case, many of these problems (e.g., the intermixture of descriptive data and phylogenetic speculation) are only symptomatic of underlying problems of logic. I stress that the list of problems treated here is by no means exhaustive; I have indicated others on the manuscript.

At a basic observational level, the presence of columellae and tectal perforations and the variations in aperture condition in many of these forms are undeniably correctly interpreted, but many other details of exine structure, especially at the TEM level, are not at all adequately documented and do not take into account limitations in use of exine characters in fossil pollen. Most importantly, the value of a non-laminated endexine (used repeatedly by Cornet as support for angiosperm affinities) has come under much doubt and has been widely abandoned, not only because of questions on its systematic distribution in fossils, but also because laminations and chemical differentiation are often invisible in fossils even when they were probably originally present, except in cases of very good preservation. This is all the more a problem at low magnifications, as in many of the sections illustrated. For example, laminations are known in a few cases in medullosan pollen but are generally not detectable, and they are not visible in existing TEMGs of pollen of Bennettitales and Mesozoic seed ferns (**Pteruchus**: Taylor et al., 1984, RPP). This has led most authors to conclude that although in principle endexine structure may have some value in seed plant systematics, the problems of preservation make it almost useless as a practical means of distinguishing angiosperms and gymnosperms. When grains are well enough preserved that the exine is still low in electron density, the presence of a

Hunt Institute for Botanical Documentation

thick, chemically differentiated nexine (whether called an endexine or interpreted as homologous with the footlayer of angiosperms, as done by Zavada) may be suggestive of gymnosperm affinities even when laminations are not visible. However, the exine in most of the Cornet's grains is so dark due to preservational effects (judging not only from the poor xeroxes provided but also from original TEMGs by the Walkers that I have seen) that no contrast in electron density could be seen even if it were originally present. Interestingly, the Chinle **Equisetosporites** is a partial exception, and in sections of a monocotyledonoid grain from the same formation that I have seen the nexine is darker than the columellae and tectum, suggesting that it is an endexine. Laminations are visible in one of the JB2 specimens; Cornet dismisses these as due to poor preservation, but one might equally well claim that this is the only specimen to reveal the true nexine structure. Similarly, in some cases (e.g., Monosulcate sp. C, **Praetricolpipollenites**), exine structure is described as "granular-columellate," but examination of the figures shows little reason to call it anything but granular. Ironically, Cornet himself implicitly acknowledges this blurring of exine criteria when he notes that the "basic sexine structure of angiosperm pollen" is seen in modern **Ephedra** (p. 79).

As examples of misinterpretations of other authors, Cornet cites Walker as saying that both monosulcate and tricolpate pollen occurs within Nymphaeales, Piperales, Laurales, Winterales, and Magnoliales (p. 39), but actually Walker reported tricolpate pollen only in Nymphaeales and Winterales (this may be a result of confusing "more than one aperture" with tricolpate). He implies that Le Thomas supports the notion of stepwise reduction from several apertures to none in magnoliids (p. 39), but she proposed only a trend from one aperture to none, not from more than one. Walker & Walker are cited as arguing on basis of pollen that the first magnoliids were anemophilous (p. 69), but in fact they argued that they were entomophilous, and this was based on floral and other macromorphological features, not pollen; what they did suggest was that the first prominent Cretaceous monosulcates, particularly **Clavatipollenites**, were the among the first angiosperms to revert to anemophily, and this was based on supposedly related Chloranthaceae, not on the morphology of **Clavatipollenites** itself. Doyle and Hughes are cited as representing the view that angiosperms arose in the Cretaceous from cycadopsid ancestors (p. 72), but Doyle, also noting the analogy of mammals, has repeatedly stressed that the Cretaceous record does not rule out a pre-Cretaceous origin of angiosperms, but only puts limits on the amount of pre-Cretaceous diversification (e.g., Doyle, 1969, p. 22); and although Hughes more unequivocally favors a Cretaceous origin, he considers angiosperms polyphyletic (his position is closer to that of Krassilov). The fact that glossopterids are the Paleozoic group closest to Gnetales in Crane's cladogram is cited as support for origin of the polyplicate pollen of Gnetales from striate bisaccates (p. 73), but this is highly misleading, since Crane interpolates **Caytonia**, corystosperms, and Bennettitales plus **Pentoxylon**, none of which have striate pollen, between glossopterids and Gnetales, negating any homology between the two characters. Niklas et al. are cited in support of the idea that the Cretaceous diversification of angiosperms is an artifact of increasing numbers of fossiliferous deposits (pp. 76-77), but although they do state this hypothesis, they reject it as inadequate. Actually, the number of suitable deposits decreases through the English Lower Cretaceous and remains about the same through the Potomac, yet the increase in morphological diversity can be seen in both sections.

Hunt Institute for Botanical Documentation

An equally serious problem is Cornet's tendency to magnify one or two selected similarities between his fossils and modern angiosperms while ignoring major differences in other characters ("one-character phylogeny"), and to overlook evidence for alternative relationships. Thus he compares **Equisetosporites** with the spiraperturate pollen of some Berberidaceae and **Thunbergia** (p. 52), but he does not mention that their exine structure is quite different, nor does he note that other Berberidaceae s. lat. and Acanthaceae have tricolpate or tricolporate pollen (more on this below). He cites the presence of a chromosome number of n=9 in **Calectasia** as support for its primitive status (p. 28), but he does not mention that n=7 (which he recognizes is more primitive) occurs in other monocots. In comparing Triassic trisulcates with **Calectasia**, he is forced to equate the endexine of the former with the intine of the latter (p. 29). Within the Triassic angiospermid complex, he compares the VB4 spiraperturate type with **Equisetosporites**, although in exine structure it is closer to the "monocotyledonoid" complex. Similarly, in discussing JB2 zonasulculates he does not consider the possibility that they are offshoots of the **Corollina** group, which they resemble in possession of a zonasulculus, differentiation of an ulcus-like area in one hemisphere, and even exine ultrastructure (e.g., columellae showing globular tendencies), although he does mention the similarities obliquely; instead, he compares them with angiospermous zonasulculates such as **Dichastopollenites**, which differs in its much coarser sculpture, details of aperture structure, and lack of an ulcus. Similarly, he minimizes the morphological gaps between some types, while ignoring or rejecting alternative connections that seem no worse. Thus he relates the Carnian "monocotyledonoid" complex to **Steevesipollenites**, which requires loss of auriculae and several changes in exine structure (transformation of the clavae into a smooth tectum underlain by regular columellae), whereas they could be derived by as few or fewer steps from **Eucommiidites** or monosulcate Bennettitales with granular exine structure (origin of perforations, enlargement of granules into columellae).

Even if all of Cornet's interpretations of exine structure are correct, his extreme one-character phylogeny and neglect of alternatives invalidate the main thesis of the paper: that similarities between various Triassic angiospermid pollen types and modern groups such as **Calectasia** and **Sanchezia** support a polyphyletic origin of angiosperms from gnetalian ancestors. A couple of decades ago, when phylogeny was generally considered a matter of armchair speculation with no clear methodological principles, it might have been possible to maintain that all phylogenetic hypotheses are equally speculative and deserve equal time, but in these days of critical evaluation and formulation of principles of phylogenetic analysis, this is no longer possible. The only way to evaluate Cornet's hypothesis (or any other) is in terms of its implications -- how well it explains (accords with) the totality of characters of the groups involved -- relative to alternative hypotheses (i.e., its relative parsimony). The obvious alternative in the present case is that the resemblances between the trisulcates and **Calectasia** pollen on the one hand and **Equisetosporites** and **Sanchezia** pollen on the other are simply accidental convergences within advanced angiosperm groups. This does not deny the possibility that the Triassic forms are related to angiosperms (which seems strongest for the Carnian monocotyledonoid types and some of the Rhaeto-Liassic reticulate types), or even that the presence of gnetalian features such as accessory furrows supports a direct sister group relationship between angiosperms and Gnetales, as proposed by Crane. Under the hypothesis that the

Hunt Institute for Botanical Documentation

modern groups retain primitive pollen types from gnetalian ancestors, one would expect that they would show primitive or gnetalian features in other aspects of their morphology. With some imagination, Xanthorrhoeaceae might be considered fairly primitive monocots, but they do have some features that are hard to interpret as anything but advances (vessels with simple perforations, a cambium producing masses of vascular bundles, syncarpy), and about their only gnetalian feature is xeromorphy. Furthermore, although there is plenty of controversy over angiosperm phylogeny, all authors that I know agree that Acanthaceae are among the most advanced dicots, and Cornet provides no evidence, other than a couple of rhetorical questions, to the contrary.

To restate the alternatives in more specific terms, to claim that some angiosperms were derived from the **Equisetosporites** line (which is associated with megafossils that show good evidence of gnetalian affinities and no obvious angiosperm features) and others from the **Steevesipollenites** line would imply that in each line most gnetalian features were lost, and all the common advances of angiosperms were gained independently (six such angiosperm autapomorphies are listed by Crane, nine by Doyle & Donoghue, ten by Dahlgren & Bremer, 1985, **Cladistics**). To this must be added the fact that **Sanchezia** is related in other characters to other Acanthaceae and asterid dicots that have tricolporate pollen, and that there is massive evidence from both comparative morphology and the Cretaceous record that most if not all tricolporate pollen is derived from tricolpate. One must suppose either (1) that Acanthaceae are unrelated to other tricolporate groups, in which case not only the numerous angiosperm autapomorphies just mentioned but also tricolporate pollen and all the other shared advances used to place the Acanthaceae in the higher dicots must have arisen separately (e.g., vessels with simple perforations, diacytic stomates, tubular corollas, epipetalous stamens, bilocular syncarpous ovaries, zygomorphy, unitegmic tenuinucellate ovules, to name a few); or (2) that putatively more primitive dicots with tricolpate and tricolporate pollen are derived from Sanchezia-like ancestors, which would mean that all the characters of Acanthaceae just listed must have reverted to their opposite states, most of which seem more primitive on any concept of gymnosperm outgroups (and in some cases on Cretaceous fossil evidence); or (3) some equally unparsimonious combination of these hypotheses, involving Acanthaceae and some number of other asterid families. Similar considerations apply to the apparent advances of Xanthorrhoeaceae. When this is compared with the hypothesis that a couple of pollen features arose convergently in advanced angiosperms and Triassic gymnosperms, what possible basis can there be, other than blind faith in the infallibility of pollen characters and the worthlessness of all others, for preferring Cornet's hypothesis? The idea that the similarity between **Equisetosporites** and **Sanchezia** pollen is due to convergence becomes still more plausible when it is noted that Acanthaceae are notorious for having some of the most diverse and bizarre pollen types of any angiosperm family; for this reason, pollen has been used to define taxonomic subdivisions in the group for nearly a century. If there is any family where one might go fishing for pollen types to compare with unrelated fossils, it would be Acanthaceae. The same is nearly as true for the Xanthorrhoeaceae, which are also remarkable for their palynological diversity.

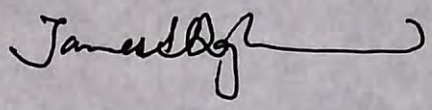
Cornet seems to be vaguely aware of these problems when he suggests that characters such as auriculae and polyplicate morphology may not be directly retained from the gnetalian ancestors of angiosperms but rather the result of re-expression of suppressed genetic information. However, admitting this

Hunt Institute for Botanical Documentation

possibility undercuts all the evidence for relationships of the groups concerned: it implies that the occurrence of these characters in angiosperms is a type of homoplasy, namely reversal, and it becomes equally parsimonious to assume that they arose by another type of homoplasy, pure convergence. The idea of reversal might be preferred if there were independent evidence for interrelationships of the groups concerned, but in fact the only evidence is the very pollen characters in question.

Another problem is that Cornet frequently asserts that one particular direction of evolution (e.g., from spiraperturate to radial symmetry) is easy to imagine on morphogenetic grounds while the opposite direction is not. Not only are these suggestions vague and totally speculative (almost nothing is known about morphogenetic control of aperture position), but the use of such arguments in general as criteria for polarity has come under severe criticism recently, on the grounds that polarity assessment should be based primarily on the distribution of characters rather than on "scenario-level" explanations for their existence, whose plausibility is often a function of the story-telling ability of the worker.

Lest this evaluation seem overly negative, I reiterate that I consider the pollen types reported in this paper extremely interesting and potentially significant. Cornet also deserves great credit both for recognizing their importance where others would have (and have) passed over them and for his persistence in amassing material that most would dismiss as too rare to be worth studying. Hence I urge him to publish a descriptive account of the most unequivocal and solid of his observations, after weeding out of the overinterpretations of ultrastructural features and the poorly supported phylogenetic speculations. What would remain would be exciting enough in its own right, and it would enhance rather than harm the credibility of its author.



Hunt Institute for Botanical Documentation

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

27 January, 91987

Dr. James A. Doyle
Department of Botany
University of California
Davis, CA 95616

Dear Jim:

A while back you (very helpfully) sent me a copy of your review of Bruce's paper on the "Triassic angiosperm pollen." I never saw the paper, as he knows what I would say and what I would urge him to do. I found your critique to be fair and correct, and is exactly what I have repeatedly said to him about all this. It is poignant, however, that at the NYC AASP session you quoted Bruce's work in support of one of your theses, though you were obliged to prevent the publication of the research! It is tragic that nobody is any longer able to guide BC as I did when he was here. He is in many ways the most interesting guy I've ever worked with. (Henry Andrews, who supervised his MSc thesis will second the motion--I contributed to that project by helping to edit it and keeping BC calmed down through the publication process.) He should not be working alone, and if we're not careful he'll be like Wilhelm Bock, Marcus Jones, and others who eventually bought their own printing presses. What would happen if you offered to help BC by co-authoring a revised version of the paper? Worth trying? I no longer can influence B.C. He advises ME!

I enjoyed reading the review, by the way, for its intrinsic value to my understanding.

All the best to you for '87.

Yours very truly,

Alfred Traverse
Professor of Palynology

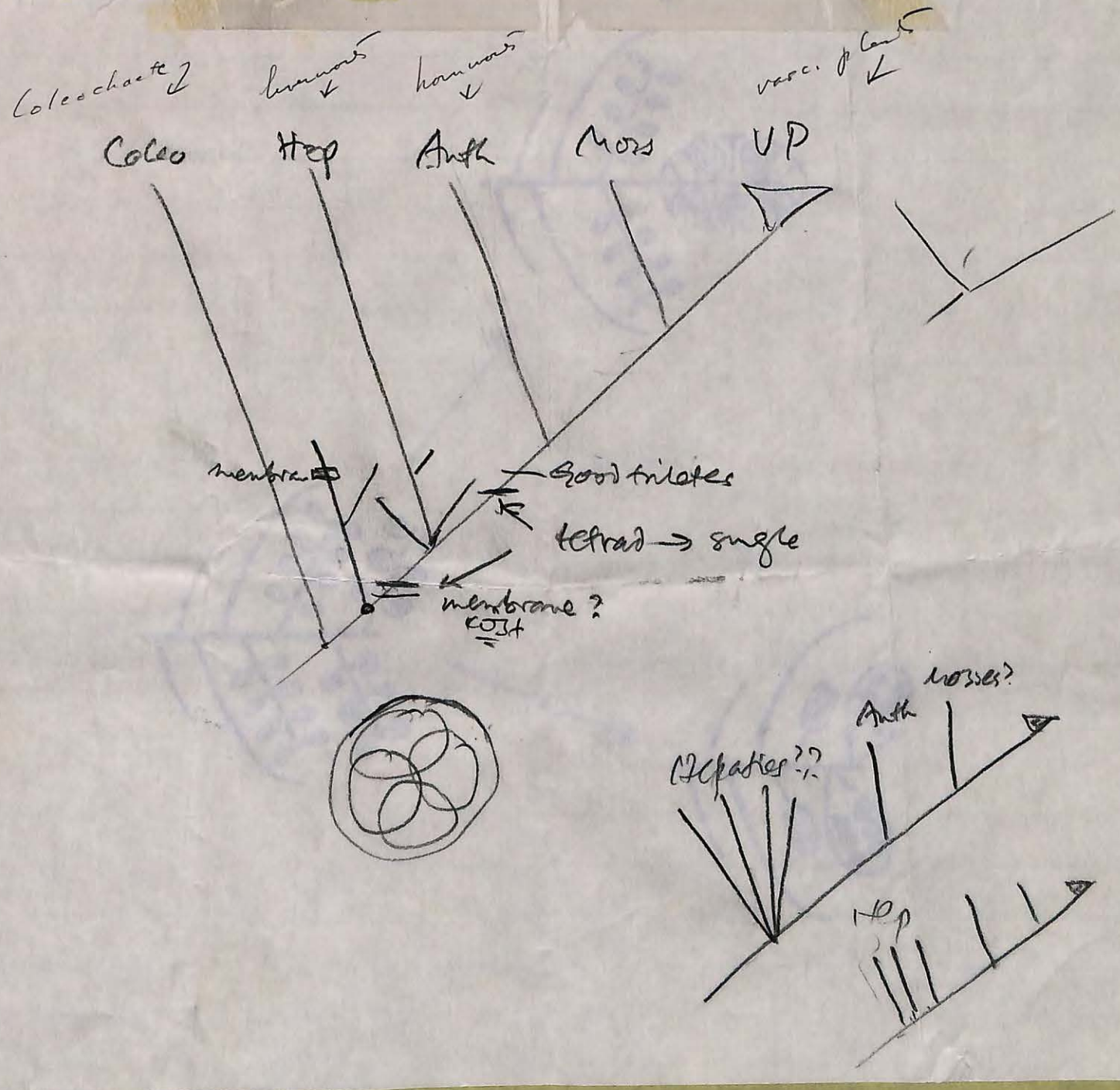
AT/et

Hunt Institute for Botanical Documentation

as concep.

J. Joyce at Harv. Oct. 82

his cladistic analysis of land plant evol.




Hunt Institute for Botanical Documentation

A.

Mr. James Doyle
~~Biological Laboratories~~
~~Harvard University~~
~~Cambridge, Mass. 02138~~
~~Dept. of Botany~~ & Museum of ~~Paleontology~~
Univ. of Michigan
Ann Arbor, MI 48104

~~313-763-5094~~

Univ. of Calif
Dept. Botany
Davis, CA
95616

Off: 916-752-7591
Dept: 916-752-0617

~~36~~, 32, 33, 34, 35, ~~36~~, 37, 39, 41, 42

Hunt Institute for Botanical Documentation

NATIONAL SCIENCE FOUNDATION

# PROPOSAL EVALUATION FORM

NSF Form 1 (4/84)
*Supersedes All Previous Editions*

| PROPOSAL NO. | INSTITUTION | PLEASE RETURN BY |
|---|---|---|
| BSR-8415772 | Univ of California-Davis | 09/08/84 |

| PRINCIPAL INVESTIGATOR | NSF PROGRAM |
|---|---|
| James A. Doyle | SYSTEMATIC BIOLOGY PROGRM |

TITLE: Morphology and Evolution of Early Cretaceous Angiosperm Pollen

Please evaluate this proposal using the criteria presented on the back of this review form. Continue on additional sheet(s) as necessary.

Dr. Doyle is justly famous for his pursuit of angiosperm evolutionary pathways via pollen morphology. With various noted co-workers he has plumbed this subject just about as thoroughly as it could be, for fifteen years. Doyle has kept himself very focussed on this very important matter. He is hard-working to a fault. There is no question he will make contributions from the proposed work. However, I am inclined to think that the best is probably already behind us in this area, and I do not expect many "bombshells" on the scale of Doyle's previous work to come out of the proposed research. What we need now is probably more megafossil work in the levels concerned--easier said than done! Doyle's summary of the current situation in early angiosperm fossil pollen studies is great, though I think he is setting up a straw man by saying that pollen studies broke up a previously held view of very early angiosperm origin, per Axelrod. That view was never the majority point of view.

The budget is remarkable--very modest general funds, 0% salary for Doyle (few universities encourage that-some don't allow it), but over $20,000 for a post-doc and graduate student. Can one read between the lines that Doyle has neither of these persons already?--I guess yes is the answer. I believe it would be great to support grad students to work with Dr. Doyle, but I really do oppose NSF funding in advance for an un-named post-doc who (+ associated overhead) is slated to get the lion's share of the money. Un-named grad students, o.k., un-named post-docs, no! Dr. Doyle has written a good summary of the status of the sort of work he has pioneered, and has outlined some interesting things to look into now, but only a small proportion of the funding is for things I want to support--Doyle's research and that of graduate students (he has to date had few), but I don't like funding a pig-in-a-poke.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

REVIEWER'S SIGNATURE

REVIEWER'S NAME (TYPED)
Alfred Traverse

OTHER SUGGESTED REVIEWERS (OPTIONAL)

Douglas J. Nichols
U.S.G.S., MS 919
Federal Center, Box 25046
Denver, CO 80225

Dr. Alfred Traverse
Palynological Laboratories
Pennsylvania State University

REVIEWER'S COPY

Hunt Institute for Botanical Documentation

20 April, 1983

Dr. James Doyle
University of California
Department of Botany
Davis, CA 95616

Dear Jim:

Hope all is well with you. I have a begging request--our teaching and reference collection is very poor for lower Cretaceous. (I discovered to my horror long ago when I first came here that Brenner left no duplicate residues or rocks samples from his thesis work.) Could you spare me say 4-5 (or whatever--even one would help) pieces of shale of known to be palynomorph-rich, from several different Berriasian to Albian levels? I would use them primarily in the palynology course. If you could do this I'd be very grateful.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

UNIVERSITY OF CALIFORNIA, DAVIS

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF BOTANY DAVIS, CALIFORNIA 95616

April 3, 1980

Prof Alfred Traverse
Department of Geosciences
Pennsylvania State University
University Park, Pa 16802

Dear Al,

Thanks for the copies of your letter to Rossovsky and his reply. I guess it was right not to take Elso's worries too seriously. It happened that a couple of weeks after we talked Otto Solbrig was through here, and, without any prompting from me whatever, started to talk about how Elso had got the idea that his position wasn't going to be renewed, and how he (Otto) couldn't imagine what he (Elso) was worried about. However, I must say that Rossovsky's letter is very cleverly hedged and ambiguous. After all, nobody claimed that the supposed demise of paleobotany at Harvard was due to lack of funds, but rather to the department's reputedly antipaleobotanical priorities.

Please give my best wishes to Nonie – why doesn't she ever call me on the Gov't. tie-line any more??

With best regards,

Jim

J. A. Doyle

Hunt Institute for Botanical Documentation

3 March, 1980

Dr. James Doyle
Department of Botany
University of California
Davis, CA 95616

Dear Jim:

Enclosed xerox from the famous H. Rosovsky is self-explanatory. The next to last sentence is very open-ended!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: copies of letter to and letter from Rosovsky

Hunt Institute for Botanical Documentation

xxxxxx

April 28, 1986

Dr. James A. Doyle
Museum of Paleontology
University of Michigan
Ann Arbor, MI 48109

Dear Jim:

Bruce Cornet has shared with me from time to time and after insistence on my part, a bit of his correspondence with you during the last couple of months. I suspect that you are thanking your lucky stars that your office is approximately 500 miles instead of only 50 yards away from that of BC! Bruce is one of the best field men and one of the very best lab men I have ever known, and when he sticks to stratigraphic interpretation and morphological interpretation, he does extremely good work. I am a great deal less impressed by his ability to generalize and theorize and, in fact, have occasionally refused to indulge him by listening to some of the wilder things. A great deal of what has been going on regarding "angiospermoid" features of fossil pollen is in the blue-sky wildly speculative ballpark. The truth is that Bruce doesn't even know very much about the angiosperms, much less about their origin. What I am really worried about is that he has been chewing us such vast amounts of time, materials, etc., in connection with his various speculations that he is in considerable danger of not completing his doctorate before the time which I long ago told him would be his terminus here--August, 1976. If this really does happen, it would be rather sad, because he has xxxxxx a great deal of very interesting and important information. I knew about this tendency to get involved in not too well-grounded speculation before Bruce came here, as it apparently was well developed and obvious when he was with Andrews at Storrs, but I have been relatively powerless to keep him on the track. Well, I suppose there is a chance that this is intensifies an impression of narrowness in my attitude, but I have had a specific project for us to accomplish, we have been extremely close to fruition, and it is frustrating that it may be down the drain. I do not seem to have had much luck in convincing Bruce that this is a real danger.

Hunt Institute for Botanical Documentation

Regarding our mutaal friend, Ken Brown--I enjoyed his visit here very much, and thought he was an interesting, but an exceedingly quiet individual with apparently a great deal of latent ability. I am afraid, however, that he is not up to the standards which this department maintains for admission of graduate students, and, unless I prove to be a poor ppophet, I don't think he is going to be admitted.

Looking forward to having some chance to talk with you some day--as opposed to just brushing shoulders in the hall some place, and with regadds,

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Hunt Institute for Botanical Documentation

The University of Michigan

MUSEUM OF PALEONTOLOGY

ANN ARBOR, MICHIGAN 48109

(313) 764-0489

April 13, 1976

W. Bruce Cornet
Department of Geosciences
Deike Building
Pennsylvania State University
University Park, Pa. 16802

Dear Bruce,

Here are a few remarks in response to your last letter--things are getting rushed here with the end of the term coming up.

In regard to taxonomic treatment of your material, I really think it would be premature and misleading to "assign" it to anything but incertae sedis: there is plenty of opportunity to talk about possible affinities in the text. This goes even for monosulcates from the Lower Cretaceous: I think that is very unfortunate that people feel a compulsion to "classify" every fossil as far as they can under a neat hierarchical scheme, often based more on tradition than anything else (I often see Liliacidites and even Clavatipollenites assigned to "Class Monocotyledonae, Family Liliaceae" even though many of these forms don't even have any characters that would indicate monocots vs. dicots). No, I don't think laminated vs. nonlaminated endexine is the ultimate distinction between gymnosperm and angiosperm pollen---the ultimate distinction is whether they were produced by plants phylogenetically related to those we recognize as gymnosperms or angiosperms, and the ways these are defined (better, recognized as natural groups) have nothing to do with pollen. The idea that endexine structure might be another distinction correlated with these was a tentative suggestion on the part of Van Campo, Lugardon, and myself, based on what we knew about modern angiosperms and gymnosperms (the latter, pitiful remnants of their Mesozoic glory). The fact that our proposed pollen criteria (plus others we might have suggested, such as tectal perforations) are now known in pollen two geological periods before there is any other evidence of an angiospermous level of organization in other organs means at the very least that these criteria should be placed under suspicion. In other words, yes, I do think our knowledge is too imperfect to exclude exceptions in both groups. That's why we recommended studies of in situ gymnosperm pollen. Combinations such as alveolar ectexine and nonlaminated endexine would cast further doubt on the universal validity of the criteria, and tell us once again we need more independent information.

Hunt Institute for Botanical Documentation

THE UNIVERSITY OF MICHIGAN
MUSEUM OF PALEONTOLOGY
ANN ARBOR 48104

If you find columellar ectexine plus nonlaminated endexine, it would certainly be justified to call a pollen grain angiospermoid, if you point out carefully and clearly that you mean by this that it has characters previously known only in angiosperms and go on to discuss the possible implications as completely and objectively as possible. However, consider that there are psychological reasons for avoiding such a term---people change "oid" to "ous" all too quickly.

I gather from your discussion that we are now in agreement that the large OL dots in type U are lumina and the smaller LO dots at lower focal levels (indicated in your latest photo, and called nexinal pseudoreticulum in the older one) are infratectal structure elements, whether columellae or not. Please don't be too sensitive about my questioning your interpretations---these things are a bitch to interpret, and it's no sin to change your mind. I just got done doing light and SEM of some of the single grains of "Retimonocolpites" which you gave me (I'll send prints when I have them), and I strongly suspect that it and type U represent basically the same system, differing in the size of the lumina. Sometimes there are definitely elements under the tectal muri as seen by light, but they seem to be short and oddly shaped; in SEM, the muri lie almost flush with the nexine, as in Potomac R. peroreticulatus and reticulatus. In others, I'm not sure I can see elements at all with light. Considering also the alveolar-columellar transitions you mention, maybe "columelloid" would be more accurate than "columellar," but in any case what is important is to discuss all observations and variations objectively, pointing out both similarities with and differences from previously described structure types.

As an aside on your remark that we would "surely not" classify such pollen with a nonlaminated endexine as gymnospermous*---do you really mean this? Surely you can't rule out its having been produced by a gymnosperm, any more than you can rule out that it was produced by an angiosperm---consider all we've discussed. As another aside, why do you think pollen is "conservative"? Usually people use this word to mean "good" taxonomically, i.e. not subject to too many reversals and convergences; others (myself included) criticize on general grounds the practice of a priori labeling a particular set of characters as conservative or not. It depends on the group of organisms you're working on.

I very much appreciate your proposal that I be in with Jim Walker and you on the preliminary paper on these pollen types, and if he is willing I would like to accept it. I thought briefly that I might be getting a free ride, but I don't think I would so long

*vs. gymnospermoid!

Hunt Institute for Botanical Documentation

THE UNIVERSITY OF MICHIGAN
MUSEUM OF PALEONTOLOGY
ANN ARBOR 48104

as I continue to spend as much time as I have thrashing out various issues with you, and I would like to contribute more anyway. There are other considerations related to your remarks about fearing that your results might reflect negatively on my credibility, etc. I would argue that rationally speaking they shouldn't really reflect on my judgement or honesty in interpreting the Cretaceous record: even if your pollen grains are angiosperms, they essentially extend back patterns seen in the earliest Cretaceous angiosperm record rather than directly contradicting my trends (the way Triassic tricolpates would!); if they are not angiosperms, they contradict the criteria Van Campo, Lugardon and I proposed, but I think we were making honest generalizations from the existing data and made it clear that was what we were trying to do, rather than putting them forth as dogma. However, in view of the not always rational and often destructively competitive and backbiting climate that seems for some reason to be prevalent in American paleobotany, I'm sure others besides yourself would see your results as damaging to me, so I would be grateful for the opportunity to be among the first to criticize my own pronouncements. Actually, if you think about it, the existence of semitectate columellar pollen in the Triassic is as damaging to Jim Walker's atectate→tectate columellar→semitectate trend as it is to anything I've said, unless you're willing to assume that the angiosperms had radiated to the level of more advanced monosulcate magnoliids by the end of the Triassic, or that these grains have nothing to do with angiosperms, even as distant ancestors. Maybe a mass public confession is in order. I'll be seeing Jim around the first of May between a meeting in Massachusetts and going out in the field for a week--if you've written to him by then, we could talk over some things then. Then I could drop by to see you on my way back to Michigan.

Some last comments on the Cretaceous leaf record: of course, it is always possible that bilobed leaves existed alongside Ficophyllum, Celastrophyllum, etc. for several stages and for some reason didn't get preserved; the question is, which hypothesis do the data we actually do have tend to support, that bilobed leaves are primitive, or that they are advanced? My general scientific philosophy is that one should base conclusions and arguments on the positive data that exist, not on possibilities which cannot be denied but for which there is no specific positive evidence. In the case at hand, I can't see any specific reason to propose selective preservation of bilobed leaves--they certainly preserve well enough from the Cenomanian on--except to save the hypothesis that they are primitive. Bear in mind that practically any theory can be maintained if you add enough extra hypotheses (think of epicycles in Ptolomaic astronomy): it's only relative to alternative theories

Hunt Institute for Botanical Documentation

THE UNIVERSITY OF MICHIGAN
MUSEUM OF PALEONTOLOGY
ANN ARBOR 48104

that the merits of a theory can be judged. To get back to the case of selective preservation, I think the fact that people spend too much time denigrating the fossil record and not enough time looking at it to see whether it might not have something to say after all is one of the diseases of contemporary paleobotany. Of course the fossil record is incomplete, but are we justified in assuming from the start that it is diabolically misleading? With that assumption, no wonder progress is so slow. Actually, I think the Cretaceous stratigraphic record is a lot more reliable than people have thought, particularly now that we have sampling from a variety of facies and geographic areas from the same time intervals and keep coming up with more or less the same things. I don't think the Everglades and Okefenokee analogies are quite to the point: you can talk about how good or bad the leaf record is only by comparison with places where leaves are accumulating, not peats! I can cite other studies, in particularly a very recent one by Bob Spicer in England, which showed that all the woody species (plus some aquatics) got preserved to some degree or another in at least the environment where he was working. Likewise, how many of the 85 species not seen in the Everglades pollen record are common, how many are rare? I would suspect that most of them are relatively rare, with exceptions such as Lauraceae. I could go on, but I think you must see the general point of my argument; basically, if you find Barremian bilobed leaves after all, the probability of your hypothesis will rise (though not to certainty, considering that there are other Barremian leaf types), but so long as they are known only from later stages of the angiosperm record, it has no more to be said for it than any of a variety of other hypotheses based on late Cretaceous leaves do, and less than hypotheses based on early Cretaceous leaves.

Best regards,

James A. Doyle

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

25 March 1976

Dr. James A. Doyle
Museum of Paleontology
The University of Michigan
Ann Arbor, MI 48104

Dear Jim,

Thank you very much for your helpful letter and restraint from an emotional rebuttal. I am truly sorry if some of my comments hit you between the eyes, although you have shaken me up on occasion with provocative comments during our written correspondence. Your objective approach is clear now, and I commend it. I would not be corresponding with you as I have done had I not initially had great respect for your comprehensive treatment of and critical approach to early angiosperm evolution. Frankly, I was worried that my discoveries might unjustly hurt your published conclusions and interpretations, and ultimately reflect on your credibility (however slightly).

We seem to be in basic agreement on the taxonomic treatment of my material, but I have a few questions, your answers to which will clarify several points.

1. Is the presence or absence of a laminated endexine the ultimate distinction between gymnosperous and angiospermous pollen, since angiosperm pollen possesses a wide range of structural types (with the exception of alveolar)? Or is our knowledge too imperfect to exclude exceptions in both groups?

2. Would a monosulcate grain with an intrareticulum (sensu Cornet) and non-laminated endexine exclude gymnospermous affinity, and should we assign such a grain to incertae sedis?

3. How would we treat a fossil monosulcate grain with truly alveolar structure and non-laminated endexine, considering that assignment to the Cycadales might be as unjustified as assignment to the angiospermae?

4. If all of the basic requirements of distinctive angiosperm exine structure are met by one of my anasulcates, ie. tectal reticulum, rod-shaped columellae, non-laminated endexine or lack of endexine, are we justified in calling such a pollen angiospermoid (not angiospermous), particularly when one considers the conservative nature of pollen and the distinctiveness of angiosperm pollen in general?

Hunt Institute for Botanical Documentation

* on page 1

angiospermoid

What conditional statement would you make (if any) were such an adjective used? Your answer to the first question* is basic to the rest of the questions, and may very well be basic to recognition of pre-Cretaceous angiosperm pollen.

After relating your advise about interpretation of columellae vs. lumina to recent experience with type U, I have some critical observations to report. Several specimens of type U appear to have a very short "columellar" zone, so short that I can only observe bright lumina of the tectal reticulum. Other specimens have a wider "columellar" zone (in optical section). In these specimens the bright dots of lumina seem to dominate the field, but with careful focusing, I quickly realized that there were two sets of "bright" dots. As I focused through the tectal reticulum, a pattern of less-bright dots appeared under muri while the lumina were still black. Then just a slight movement of the fine adjustment to a lower focal level produced a different pattern of bright dots corresponding to every lumina of the tectal reticulum (at this focal level the first set of dots became fuzzy dark dots or they were obscured by the thick reticulum). Am I not correct in interpreting the first set of less-bright dots as structural elements? The same double pattern of bright dots occurs with types Y and W. In each case the muri of the tectal reticulum seem to break up into round elements. Check the enclosed photo of type U, which shows the reticulum breaking up into columella-like elements (circled area).

The main problem in recognizing such structural elements is that they are short, and may even be lacking in some specimens. The range in variation of thickness of the "columellar" zone is significant enough to produce specimens with obvious structure and specimens with no apparent structure in the same species.

To further illustrate my point, I will discuss ~~another~~ specimen of type Y, which appears to lack a "columellar" zone. The nexine seems to be separated from the tectal reticulum by only a thin space. Careful focusing shows no structure in the "columellar" zone. Indeed, the tectal reticulum may be resting (almost) directly on the nexine as in your TEMG of Retimonocolpites cf. reticulatus (Pl. 6, fig. 1, Doyle et al., 1975). Careful study of the photos of type Y sent to you earlier should raise serious doubt about the lack of columellae in that specimen, particularly in areas where the reticulum, black lumina, and bright dots under muri of the reticulum are all "in focus" at the same time. Other larger bright dots are probably lumina as you suggest. The specimen has enough depth to exclude the interpretation that such "columellar" dots are the lumina of the far wall (which cannot even be brought into clear focus). Optical section of the wall shows a much wider "columellar" zone than in the previous specimen of type Y, and also reveals short "pegs" (obscured to a large degree by the thick tectal reticulum).

Other hopefully objective observations show a considerable variation in the complexity of the "columellar" zone in some species. I could pick a specimen with a dominance of rod-like columellae and a few scattered short "walls" joining some columellae to represent the structure of types C, N, R, and X, but that would be misleading. Other specimens show a dominance of "walls" with rod-like swellings in the "walls". The overall structural pattern in these specimens is more like a maze of dead ends, some compartments, and some free rod-like columellae. Such structure is not completely alveolar (sensu Doyle), but rather a mixture of alveolar-like and columellar-like elements. Possibly this structural pattern is

Hunt Institute for Botanical Documentation

derived from an intrareticulum, and has reached a truly columellar condition at one extreme of variation. How would we taxonomically classify pollen containing such structure if a non-laminated endexine were present? - surely not gymnospermous.

Without specimens at hand (excluding specimens from the Norian sample M-4) I can understand how you might assume that I am misinterpreting structure as a result of enthusiasm and quick interpretation. Indeed, the implication (and it is only an implication) of taxonomic affinity is too great for me to be careless. That is why I will wait for tangential TEMGs before going from "possible and worth considering seriously" to "probable" taxonomic affinity. Even if we can find no structural or morphological reason for excluding angiosperm affinity, it would be very risky to call such pollen angiospermous without other reproductive structures, as you have said. This has always been my position, even though pollen is supposedly conservative, and semitectate structure (Retimonocolpites) relatively advanced for primitive angiosperm pollen (unless tectate-imperforate, tectate-granular, and "atectate" are derived).

Although I am committed to publish with Walker on my "angiospermoid" pollen, I would very much like you to contribute views and interpretations on at least some of the more problematical species. If Walker is willing, perhaps all three of us could put out a preliminary report, to be followed by a formal description and naming of species by Walker and me. If you are willing, I will ask Walker to include you in a preliminary paper. In that way your support of whatever interpretation we jointly arrive at will help all of us. In that way you will get samples of all palynoflorules to study, before publishing, as well as additional single-grain mounts.

There appears to be one major flaw in your reasoning that the angiosperm leaf record is so accurate that the first stratigraphic appearance of any particular leaf type is the point at which that leaf type evolved, or that it must have evolved from some leaf type in slightly older rocks (up to three stages). With a basic understanding of the limited ways in which leaves are transported and preserved, how little of the existing flora leaves represent, how environmentally controlled such leaf assemblages really are, and the limitations to collecting and adequate sampling, it is very risky (to say the least) to assume that some leaf type could not have existed before its "first" appearance. I am not condoning such irresponsible speculation that "angiosperms had differentiated to the family level before they appeared in the Early Cretaceous", etc., but I am questioning our ability to recognize holdovers from pre-Cenomanian rocks that are either preangiosperms or more primitive than Ficophyllum, Proteaephyllum, or Rogersia, etc., or that have evolved independently of known predecessors.

Consider the potential fossil record for the Everglades or Okefenokee Swamp, the vegetation and organic sediments of which are being intensely studied by several graduate students in the Organic Sediments Dept. on my floor. Of the 1647 species of pteridophytes, gymnosperms, and angiosperms in the Everglades (Long and Lakela, 1971), very few leaves, if any, are being preserved in the peats or organic sediments. In the Okefenokee Swamp most leaf litter is accumulating as peat, but turn that peat into a lignite, and it would be very difficult to extract whole leaves. Little

Hunt Institute for Botanical Documentation

clastic deposition in the Okefenokee would seriously limit the number of shale plant-bearing units in the geologic future were the Okefenokee deposits to be burried and preserved.

The pollen record is almost as bad. In the Everglades only about 140 species of spores and pollen have been recognized in the sediments, and in the Okefenokee only about 55 species. Although the number of parent plants represented by the pollen spectrum is much greater due to similar pollen produced by two or more species of plant, one can hardly expect half of the overall vegetation to be accounted for by palynomorphs or retrievable leaves in the sediment. Now apply those statistics to Cretaceous environments and tell me that Liriophyllum or a similar bilobed predecessor could not have existed in the Aptian! All the paleobotanists, palynologists and neobotanists that I have discussed this problem with are very skeptical of "representative" fossil floras.

Sincerely yours,

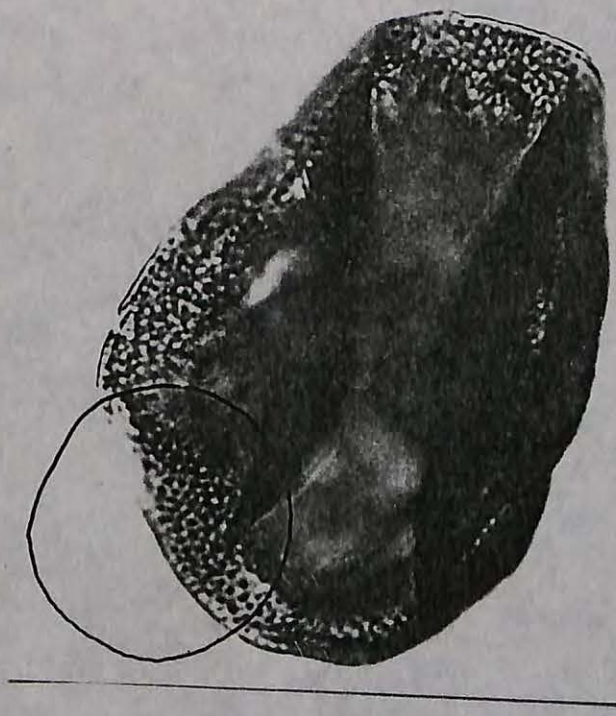
Bruce

Bruce Cornet

Hunt Institute for Botanical Documentation

clastic deposition in the Okefenokee would seriously limit the number of shale plant-bearing units in the geologic future were the Okefenokee deposits to be burried and preserved.

The pollen record is almost as bad. In the Everglades only about 140 species of spores and pollen have been recognized in the sediments, and in the Okefenokee only about 55 species. Although the number of parent plants represented by the pollen spectrum is much greater due to similar pollen produced by two or more species of plant, one can hardly expect half of the overall vegetation to be accounted for by palynomorphs or retrievable leaves in the sediment. Now apply those statistics to Cretaceous environments and tell me that Liriophyllum or a similar bilobed predecessor could not have existed in the Aptian! All the paleobotanists, palynologists and neobotanists that I have discussed this problem with are very skeptical of "representative" fossil floras.



Hunt Institute for Botanical Documentation

The University of Michigan
MUSEUM OF PALEONTOLOGY
ANN ARBOR, MICHIGAN 48109
(313) 764-0489

Doyle

March 20, 1976

Bruce Cornet
Department of Geosciences
Deike Building
The Pennsylvania State University
University Park, Pennsylvania 16802

Dear Bruce,

Thanks for your latest letter. Let me clarify a few things about Furcula and "picture mismatch," since I feel sometimes that you're accusing me of dogmatism by association, holding back new ideas, etc. Never would I say, nor have I said or implied that Furcula (or various other fossils with some angiosperm characters and some non-angiosperm characters) can have nothing to do with angiosperms and should be forgotten--I would have thought that the phrase "although it is possible that Furcula represents some gymnospermous sister group of the angiosperms" left the impression in the reader's mind that it was still a very interesting fossil indeed, far from a dead issue (we say "sister group" rather than "ancestor" since it's probably very rare to see the direct ancestor of anything, but "ancestor" can't be ruled out). Try also to keep in perspective your role in "opening up acceptance of alternative ideas for angiosperm evolution" by considering that Jack Wolfe and I wrote that before any of your work came up, and I would say almost the same thing today, maybe with a little more elaboration (cf. below). Also, please don't automatically lump me with Hughes (though in defense of him you should note that he says all Mesozoic gymnosperm groups should be considered as potential angiosperm ancestors; I wouldn't go quite so far myself). Frankly, I see no way in which your work or any other has changed the status of Furcula as a pre-angiosperm or not one. The fact that you have found Triassic pollen with exine characters which are found only in angiosperms today and which have hence been proposed (as a working hypothesis only!) as a guide to angiosperm affinities in dispersed pollen is hardly new evidence on the affinities of Furcula, almost halfway around the world; the concept that it has any bearing on Furcula is purely an intuitive guess, no better or worse than dozens of others one might make, which may pay off someday but which hasn't yet. To suggest that it does is biogeopoetry, an art form of some value which I engage in myself quite frequently (though you may not be aware of this, since you usually encounter me in my critical mode), but which should not be confused with hard science. What

Hunt Institute for Botanical Documentation

you have done in a positive way about Furcula is simply to reemphasize its never-denied similarities to angiosperms; what we were saying (and still do) is that it also has undeniable differences from all modern angiosperms with which it might be compared. The only conclusions which I feel are warranted are: (1) it is not a full-fledged angiosperm, contrary to what statements such as "if it had been found in the Tertiary, no one would have hesitated to call it Sterculia" would imply; it is either (2a) a pre-angiosperm or angiosperm relative which hadn't made it to the level of modern angios in the one part of the plant of which we have objective knowledge, or (2b) a member of some gymnosperm group which attained certain "angiosperm" characters but which had nothing at all to do with the angiosperms phylogenetically. From what you say, I think you would accept (1); as a believer in the method of multiple working hypotheses, I hope you mean what you say about presenting alternative interpretations enough to avoid excluding either (2a) or (2b). I think where I admittedly get upset is when you go too fast from thinking something is "not impossible" to "possible and worth considering seriously" to "probable"--i.e., from intuition to working hypothesis to conclusion; if someone draws conclusions too quickly, he'd better be nimble, but people would take him more seriously if he used his nimbleness at the pre-conclusion stage.

I would never insist that a plant must have a set of pollen or leaf characters that are completely angiospermous in order to be considered in discussions of angiosperm origins; however, the characters of other organs had better be reasonably modifiable into the situation in angiosperms. The more drastic the modifications you have to postulate, the more difficult it is to maintain the plausibility of the initial hypothesis, however attractive it might have appeared in the first place. Actually, I don't think we're anywhere near this situation with any of these Triassic fossils yet--we haven't found even one in association with another organ. Think over what you say about picture mismatch in the light of these considerations. Surely neobotanists (and others) should be more forgiving of errors in the direction of caution (if you can call it an error to shelve something as incertae sedis!) than of errors made in prematurely announcing positive conclusions.

I am a little hurt by your remarks on scraping away barnacles to look for weaknesses in the hull of (my) theory (though I understand the barb, since your letters provoke an initial adrenaline reaction in me, too). I do not have any particular "theory" about pre-Barremian angiosperm evolution to "defend" (Hughes may have said earlier angiosperms did not exist, but I never have), though I do think that the Barremian and younger record does render some theories more or less likely, and I think I am totally justified

Hunt Institute for Botanical Documentation

in criticizing arguments or interpretations that I think are incorr or less conclusive than their authors make them out to be. As examples of theories I'd put in the "unlikely" category, I mean the idea that the angiosperms had differentiated to the family level before they appeared in the Early Cretaceous, or that the Amentiferae are primitive angiosperms. About the only one of your speculations I might put in that category is the concept that bilobed Cretaceous leaves represent a direct primitive holdover from a bilobed condition in Furcula-like pre-angiosperms: from their stratigraphic position in the framework of the early angiosperm radiation (Liriophyllum appears in the Cenomanian, after angiosperms with non-bilobed leaves have already been diversifying for three stages; Magothy Fontaineas are much younger still), the best they could represent is some sort of atavistic throwback, and this is such a speculative possibility (almost impossible to tell from chance secondary convergence even in modern forms) that it should not be considered seriously unless there is strong subsidia evidence for it (e.g., proving that Furcula is a reasonable pre-angiosperm in its other organs, rather than just saying that you can't prove it isn't). Most of my criticisms of you have concerne arguments, interpretation, and methodology, not challenges to my own or others' dogma.

Before discussing type U, I'd like to try to clear the semant air a little. Who ever said alveolae have to have slanted as well as vertical walls, or that alveolar structure means several tiers of alveolae? The structure in bisaccates usually has slanted walls delimiting tiers of small alveolae, but not always: sometime it's a simple honeycomb. In contrast, the structure in cycads is usually if not always a simple honeycomb; there are however a few pictures which suggest some species have more than one tier of alveolae, as in medullosans. Since all these structures have something in common (cells delimited by walls of sporopollenin) and since all occur in gymnosperms but never (so far as we know) in angiosperms, it is useful for several reasons to use some common term for them and to distinguish them from the different situation in most (nongranular) angiosperms (spaces delimited by radial rods of sporopollenin). This is clearly how and why Van Campo defined the term alveolar, both in 1971 (you might read that paper) and again in Doyle, Van Campo & Lugardon (1975)--I can't imagine where you got the idea that several tiers of alveolae were required, except maybe from Walker & Skvarla's (1975) sketch, which incidentally doesn't look a like any real structure I've seen. If you wish, you can redefine the term "alveolar" so that your pollen doesn't fall into that category, but be sure to stress that this is "alveolar sensu Cornet non Doyle, Van Campo & Lugardon," and don't accuse others of throwi up a guise of semantics and definitions. Furthermore, even if you

Hunt Institute for Botanical Documentation

do all this, don't confuse the uninitiated by saying "it (type A) is as columellar as it is alveolar"; rather, say "it is no more alveolar (sensu Cornet) than it is columellar."

As for my difficulty in distinguishing alveolar structure and an infrareticulum (=intrareticulum: nobody seems to have agreed on the best Latin prefix), I urge you again to read Van Campo (1971) or the elaborated version in Doyle, Van Campo & Lugardon (1975). Alveolar and infrareticulate are not contradictory terms. A major point of the above papers is that there are several types of exine structure that appear "reticulate," including not only the open reticulate type (as in your type N), but also at least two where the "reticulum" is below the tectum, hence infrareticulate. An infrareticulum can be formed of columellae arranged in a reticulum, or by the walls of alveolae (as defined by Van Campo, Lugardon and me).

This leaves the question of type U and type N. I don't recall the series of double-takes I did when I first saw type U that leads you to accuse me of seeing what I wanted to see, since as I look at it now and as I remember thinking after the initial shock wore off, this is certainly the most angiospermous-looking of any of your pollen types, except for type N. However, the labels on your photos surprise me, since we seem to be seeing different things: check your LO analysis carefully! In your pictures, the bright points you label "columellae" correspond to dark points surrounded by light muri when seen at higher focal levels. I had thought you were talking about the darker points around the light ones seen at lower focal levels as the columellae, but you label these as "nexinal pseudoreticulum." These latter points are in fact quite suggestive of columellae to me (though they are too vague to rule out perforated alveolar walls or large granules without TEM or SEM of broken surfaces), while the bright dots look like tectal perforations located over the lumina of the infrareticulum. This is very similar to the LO pattern you get in some Annonaceae and Magnoliaceae with small columellae and small tectal perforations; I remember well that Jim Walker had similar trouble in telling such tectal perforations from columellae when he was just starting to look at Annonaceae, so you could be in worse company! Look at his light and SEM pictures of Cremastosperma, Malmea, etc. (Walker, 1971, Pl. 2-3, 57:3,4), try to get SEMGs of broken surfaces to find out what's really going on, and please be careful! I think the same thing goes for type Y: the optical section and the LO suggest the bright points are tectal perforations rather than columellae. In this type, there doesn't seem to be any readily visible infratectal structure beneath the broad muri.

Hunt Institute for Botanical Documentation

Concerning the large "Retimonocolpites," keep it in the same genus with the Cretaceous forms if you like, but if you do I think you should point out for the neobotanists and others not familiar with the subtleties of parataxonomy that this is a form genus and no affinity with the Cretaceous forms is necessarily implied; in other words, that your taxonomy (from which the nomenclature follows by mechanical rules) is strictly morphological and doesn't pretend to reflect concepts of phylogenetic relationships. By suggesting that you keep or provide another name for it, I'm saying that it may be wise from a scientific point of view to postpone rigorous application of existing morphological taxonomic concepts when there are reasonable doubts about genetic relationships, namely a gap in the stratigraphic record of the taxon of a whole period. Incidentally, the definition of Retimonocolpites doesn't say anything about columellae, although the type species, R. dividuus, does have them.

One last point of a more philosophical nature. I think it's very constructive that you are inviting other palynologists to criticize your interpretations; however, I hope that you're taking their criticisms as more than just a way to work the bugs out of your personal Theory, namely as a chance to learn more about the scientific method and the advantages of communication and to gain needed perspective from the experience of others, even if it means abandoning cherished assumptions at times. We are not all just solitary searchers for the truth sailing into the unknown; we're also part of a scientific community and products of its history, and we ignore our debts to it at our own risk. Frankly, with reference to things you say sometimes, I think it takes either bravado or a certain lack of perspective on the way scientific discoveries are made to go into the Triassic with an expressed goal of finding pro-angiosperms. Even if you think of the more spectacular discoveries of paleobotany, such as Beck's discovery of the connection of Archaeopteris/Callixylon, Archaeopteris happens to have been the most common Late Devonian plant, and there was an established body of information on it and its relatives (not all of it correctly interpreted, of course, but done with sufficient care to be readily reinterpreted) without which recognition of the progymnosperms would have taken far longer. It's all too easy to think that Dawson's discovery of Psilophyton went unnoticed for 50 years because of the narrow-mindedness of paleobotanists; it's more difficult but more correct to realize that more solid information and new theory were needed before its discovery could be appreciated. After all, Dawson also described Prototaxites as a conifer at about the same time! I would think by now that your several false starts and reappraisals would be practical enough proof that it's time to temper enthusiasm with a more critical and comprehensive evaluation of data and just how much (and how little!) they say about sets of

Hunt Institute for Botanical Documentation

alternative hypotheses, and to lower your sights to more realistic goals if you want to avoid major frustration. I wouldn't go as far as Scheuring, who admonished me for toying with evolutionary interpretations of exine structure and told me that what we should be doing is compiling an objective, comprehensive catalog of exine structure types in the Mesozoic before even thinking about evolution, but his is a viewpoint that should be considered very seriously (and I don't mean just in order to think up the right rebuttal to it!). From what you say in your latest letter about presenting alternative interpretations, etc., I think you realize at an intellectual level that attacking and defending pet interpretations like lawyers in court or "selling" particular theories isn't the best way to arrive at the truth, but I suspect this hasn't quite sunk in at the emotional level. That's only human, but it's also human to keep working on it.

With best wishes,

Jim

James A. Doyle

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

file

15 March 1976

Dr. James A. Doyle
Museum of Paleontology
The University of Michigan
Ann Arbor, MI 48104

Dear Jim,

Thank you very much for your lengthy and critical letter. I appreciate your candor about my tendency to jump to conclusions. Yet unlike some anonymous paleontologists, I am equally nimble at changing my conclusions when contrary evidence emerges, rather than throw up a guise of semantics and definitions. What you may not realize is that I have been studying Newark "Retimonocolpites"-types for a long time, and have given the subject much thought. At least in this context, my interpretations are hardly impetuous. I can hardly be expected to be right all of the time. My desire to let other palynologists attack my interpretations of my discoveries before publication is an attempt to work out the bugs, so to speak, in my arguments, etc. I am learning a lot about human behavior through constructive argument, and how to deal with barriers composed of arbitrary definitions. Palynological nomenclature has been a good training ground.

I must be making some progress in opening up acceptance of alternative ideas for angiosperm evolution, because Furcula seems to have been a dead issue prior to my observations and discoveries. Now at least you are willing to consider it as a potential ancestral grade. The divided apex may not be a normal dicot pattern, but nor is the divided apex of a number of putative Cretaceous angiosperm leaves (eg. Liriophyllum). Are we unwilling to recognize evolutionary grades of morphology no longer (?) possessed by extant angiosperms? What is an angiosperm? Can a plant with a "normal" magnoliid flower be called an angiosperm if it possesses Furcula- or Nilssonia-like leaves? Let's not get hung up on comparisons with extant dicots as you justifiably admonished me not to do. Liriophyllum appears to be possessed by a plant bearing an acceptable angiospermous flower (Dilcher, Crepet, Beeker, and Reynolds, 1976). What about those yellow secretory? bodies, also seemingly possessed by Furcula?!

Your comments on insect vectors are well taken. Once again we fall back on negative evidence. Perhaps the Bennettitales and Cycadales never did evolve tectate-perforate exines in their pollen, which (if a valid option) would suggest that the angiosperm-type of pollen, with intraexinous cavities (for sporophytic incompatibility proteins?) and

Hunt Institute for Botanical Documentation

outer access through perforations, may have been possessed by the ancestral group to the angiosperms. You seem to be demanding a set of pollen or leaf characteristics which is completely angiospermous. Yet, were I to discover unquestionable angiosperm pollen or leaves in the Late Triassic, such a discovery would provide nothing about the ancestral or proangiospermous condition, and that is what I am looking for. To be able to relate a preCretaceous fossil to angiosperm phylogeny seems to be an impossible task without the entire plant.

Please do not be too surprised that I am still referring to type U* as possessing columellae. I am not changing the rules while still playing the game, as it were, but I am reasonably sure that I see true rods in type U. I could be wrong, but consider this: The putative x-section of broken wall of type U may not belong to that species. The stub on which type U is (was) mounted also contains type R (which has an imperfect intrareticulum). The specimens were badly damaged when a clumsy technician put his finger on top of the stub before coating. Although the stub was divided into four quadrants, the digital disaster may have resulted in displacement of fragments of crushed grains. Because the presumed fragment of type U is not at its original location in its quadrant (which is to be expected), that fragment could belong to type R.

There are other problems, such as magnification. The photograph indicates that the bar is 4 mu long. The thickness of type U exine does not exceed 2 mu, but the bar indicates an exine thickness of over 8 mu, which does not correspond with any of my exine measurements of any type! I initially assumed that the SEM was malfunctioning, and that is why the photo is so good. All other attempts to photograph at "similar" magnification have turned out badly. Therefore, throw out that photo. I do not know if it can be trusted. If it is an x-section of type R, you have every right to say that I imagined columellae. Some vertical structures in the photo appear rod-like (to me), while others "appear" wall-like, an interpretation that agrees with LO-analysis of type R. Additional specimens of type U reinforce my interpretation that the majority of (if not all) structural elements are rod-like in that species. Don't believe me until I have tangential TEMG's. This is one "bug" I am glad to remove.

You have concluded that type A has an "obvious" alveolar structure. Although I have conceded that this type is not columellar, you seem to have difficulty in distinguishing a true alveolus from an intrareticulum (or is that wishful thinking on your part?). I define alveolar structure as a honeycomb pattern of walls, some vertical, some slanted. The "cells" are usually variable in shape, with occasional tangential walls dividing the "cells" into "tiers". When the structure has only vertical walls and only one layer of more or less uniformly sized "cells", the term, alveolar, is not completely accurate. In fact, it is misleading by implying taxonomic affinity (of sorts), particularly when such structure is classified as gymnospermous. The intrareticular structure of type A is as columellar as it is alveolar. Just as it lacks rods, so too does it lack slanted walls (as seen in optical cross section). This is my definition and opinion, which explains why I did not initially interpret

* Type U in Feb. 4 letter changed to type V.

Hunt Institute for Botanical Documentation

the structure as alveolar, and have been reluctant to do so.

One theme that you have been repeating is fear of "uncritical and misleading 'picture matching'", as though "uncritical" and "misleading" refer to any paleobotanist who presents any evidence of angiosperm ancestry. You seem to be making the definition of "angiospermous" so restrictive that de novo evolution of the angiosperms is the only acceptable solution, ie. divine intervention. I am beginning to wonder if "overcritical" and "misleading" can be used to describe attempts to reject potential pre-Cretaceous evidence on the basis of "picture mismatch", ie. some minor detail (eg. an incomplete intrareticulum) that overshadows significant evolutionary developments (eg. tectal perforations and intraexinous cavities for sporophytic incompatibility mechanism?). The "lack" of peripheral loops in the secondary venation of Furcula is an overcritical undersight, because the divided ends of secondary veins are clearly joined ~~either~~ by the intervening reticulate venation or they possess branches that form weak loops in several cases (Harris, 1932, Pl. I, figs 1 and 5). Obviously I am stressing positive evidence for incipient marginal loops. The reason I mentioned Asimina pygmaea at the Geobotany Conference is that in some of its narrow leaves the peripheral loops are weak or break down completely. Thus I can build an equally strong argument for Furcula being angiospermoid as you and Jack can for rejecting Furcula. Even if Furcula were as advanced as "unquestionable" fossil angiosperm leaves, we still could not be certain that it is angiospermous without reproductive structures. Somehow that aspect of the problem has been minimized.

Thus, there are two sides to every coin: By rejecting Furcula as angiospermous, you, Wolfe, Page, and Hughes may have damaged acceptance of this leaf as angiospermous even if carpellate reproductive structures are found associated! What does that do for neobotanists' opinion of paleobotanists? The point I am trying to make is locked doors are hard to open (sometimes even with a key), and an objective approach to Furcula would be to present alternative interpretations, pro and con, independent of personal opinion (which doesn't seem to be worth much in the long run). The same approach holds true for my pollen.

Pollen of some woody "Ranales" (Praglowski, 1974) has structure consisting of rods and short muri, but does any tendency towards alveolar structure reject angiospermous affinity in fossil forms? You jumped to conclusions when you first saw SEMG's of my "Retimonocolpites", and you saw what you wanted to see in the light photos of type U. I now have discovered two additional reticulate species in JB2 with short "bright" columellae under muri. The list of potential, tectate-columellate perforate anasulcates is becoming longer and longer. Each type must be critically examined, and it only takes one species that has reached angiospermous complexity of exine structure to poke a hole in current theory. The ship won't sink without Late Triassic, angiospermous reproductive structures (excluding pollen), but it will take on water. Perhaps you should scrape away barnacles to look for weaknesses in the hull of your theory. That point raises the question of nomenclature.

Hunt Institute for Botanical Documentation

You have inadvertently emended the definition of Retimonocolpites to include species with and without columellae supporting a reticulate ektexine. By doing so you have added one characteristic with no close analog in extant angiosperm pollen - good show. But you have also brought such columella-less species closer to the definition of the Late Triassic Retisulcites. Perhaps Retimonocolpites peroreticulatus and R. cf. reticulatus (Doyle et al., 1975) are proangiospermous. Nevertheless, some species of similar grains from my sample M-4 appear to possess good rod-like columellae, which by definition excludes them from Retisulcites, while other species from M-4 appear to lack columellae. If your statement about not using the name, Retimonocolpites, does not reflect taxonomic evaluation of exine structure, what other reason do you have for not using the same morphological name for species as close as those of M-4 and Cretaceous Retimonocolpites spp.? Perhaps I can justify a different name if my grains possess a laminated endexine, if all species of Retimonocolpites have smaller lumina at their apices, or if all Cretaceous species possess distinct sculpture. I am well aware of implied taxonomic affinity by using the same name, but nomenclature has nothing to do with taxonomy.

Concerning your astute criticism about angiosperms being forced "underground" by the radiation of Corollina producers in the Jurassic, JB2 contains a large percentage and diversity of Corollina spp. (true for the entire Rhaetian). It is not until the palynoflora is "smothered" with Corollina pollen (95+%) that my "angiospermoid" pollen disappears. In Europe there are "islands" where Corollina never does become dominant (and may be completely absent in some palynoflorules) in the Early Jurassic. There Chasmatosporites Nilsson (=types N and R?) and Cycadopites reticulatus sensu Nilsson (=type U?) flourish. Perhaps the plants which produced my pollen became restricted to "temperate" areas where Corollina essentially drops out of the record. There they "cooled off" and didn't resume their "rapid" evolution until they could filter down through Corollina - dominated territory to the tropics as environmental conditions became more suitable as in the Rhaetian.

Keep smiling. Our letters are serving a constructive purpose.

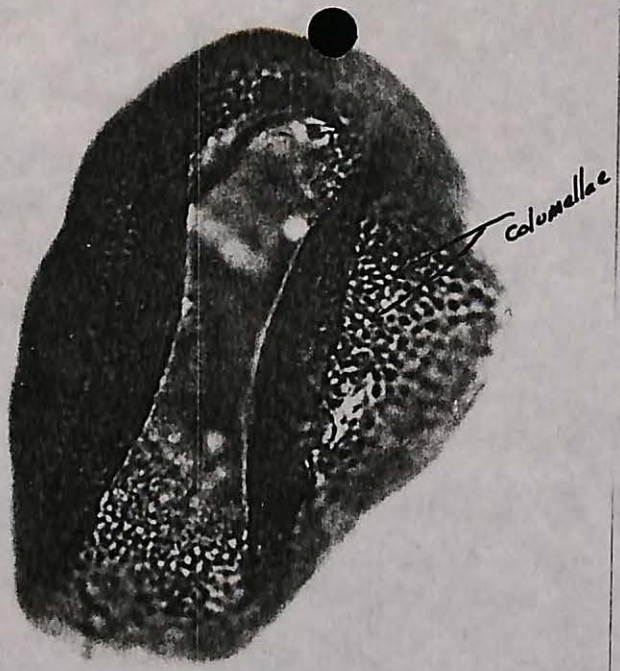
Yours truly,

Bruce

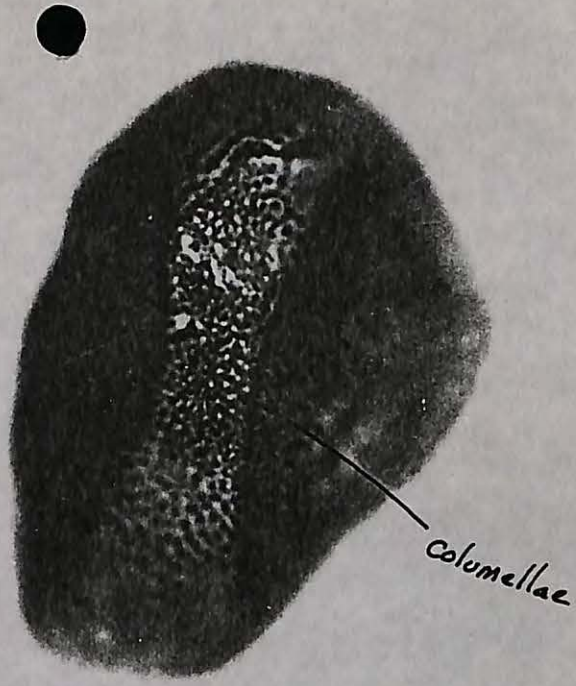
Bruce Cornet

Seven photographs of types U, W, and Y enclosed.

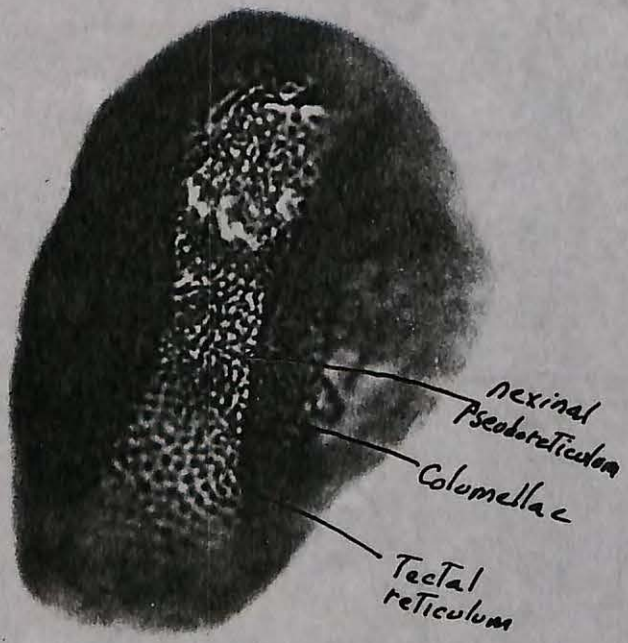
Hunt Institute for Botanical Documentation




JB2 Type U




JB2 Type U




JB2 Type U

Hunt Institute for Botanical Documentation

JB2 Type W






JB2 Type W




Hunt Institute for Botanical Documentation

March 12, 1976

Dr. James A. Doyle
Museum of Paleontology
University of Michigan
Ann Arbor, MI 48104

Thank you so much for the thick reprint, "Observations on exine structure...pollen". In addition to the generosity this represents in your remembering me at all, I am aware of the dollar-and-cents expense of reprints of this size. The paper is, of course, of great interest to me, and I am very glad to have.it. I have long been teaching in my palynology course that a closer look at exine structures in the Mesozoic would eventually make a contribution to the question of the origin of the angiosperms, and I greatly admire your approach to the problem. Until we find more of the pesky things in organic connection, however, we will never know for sure whether we are proving the existence of angiospermous pollen exine structure in the Jurassic or whether we are showing that some Jurassic gymnosperms had angiospermous-like pollen. It is this sort of consideration which I have been urging upon my student, Cornet, in trying to get him to be a little more conservative in his interpretation of admittedly very interesting structure from ~~you~~ Triassic-Jurassic palynofloras. Incidentally, your techniques for studying single grain mounts by both light and electronmicroscopy seem to work--Cornet applied them with considerable success to some of our stuff (it is unfortunate that we do not have more ready access to better electronmicroscopy here).

I have been trying off and on for several weeks to reach you on the telephone to discuss a graduate student at Michigan who has expressed at least a mild interest in doing work here under my direction. I'll keep trying.*

Best wishes and thanks again for the very interesting reprint of a quite important piece of work (~~where~~ those the pictures you were handing out in Houston?!)

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

*In the meantime, he has been here, and I have had your letter.

Hunt Institute for Botanical Documentation

The University of Michigan
MUSEUM OF PALEONTOLOGY
ANN ARBOR, MICHIGAN 48109
(313) 764-0489

March 11, 1976

File:

Doyle

W. Bruce Cornet
Department of Geosciences
Deike Building
Pennsylvania State University
University Park, Pennsylvania 16802

Dear Bruce,

Thanks for the xerox of Harris's description of Furcula. Even though Furcula is occasionally simple, I'd argue that the statement that it "strongly resembles some leaves of Asimina parvifolia (Michx.) Dunal" is far too close to the sort of uncritical and misleading "picture matching" methodology which has so blackened the reputation of paleobotany as a science. First of all, even if some Furcula leaves may be impossible to tell from a pinnately veined dicot leaf (or some ferns such as Tectaria species, or Gnetum) when held at arms length, the fact that they are usually forked is a striking departure from the normal dicot pattern. Patterns of variation, after all, are often just as important as morphological features or structures taken in isolation. Second, although the presence of more than one order of reticulate venation and freely ending veinlets is highly significant as a reason to keep Furcula in the running as a candidate for an angiosperm ancestor or relative, the secondary venation, as Jack Wolfe points out in the Wolfe, Doyle & Page paper of which I've sent you a reprint, is still basically dichotomous---hence, although I'd be the last to rule it out as an angiosperm relative, it's certainly not an angiosperm yet. Third, any comparison with modern species, however well you may mean it just to show that similar characters exist among modern angiosperms (not quite true, as I've pointed out), can only lead to disasterous misinterpretation by neobotanists thirsting for familiar names. You could have cited hundreds of other species that are as similar to (and as different from!) Furcula as Asimina parvifolia is. I strongly urge that you read and take to heart Jack's discussions (1973. Fossil forms of Amentiferae. Brittonia 25(4): 334-355; Wolfe, Doyle & Page, 802-4) of the methodological pitfalls of angiosperm paleobotany. We late 20th century paleobotanists have a long and hard uphill fight to mend the unpardonable mistakes in both theory and practice made by angiosperm paleobotanists around the turn of the century (what Leo Hickey calls the "charlatan phase of American paleobotany") and the bad reputation it's given us since then; you may not be

Hunt Institute for Botanical Documentation

so conscious of this, but having travelled in both neo- and paleobotanical circles I can assure you this isn't just paranoid imagination on my part. Just consider how little space is devoted to discussion of the fossil record in most introductions to evolution and morphology of angiosperms (e.g., Foster & Gifford) and you may get a hint of what I mean.

Since I've been rushing to finish up another paper and to send off slides showing Lower Cretaceous angiosperms of Africa to Jardiné for a talk this very month, I haven't had as much time to look at the sandwiches you gave me as I would have liked. However, I'm still thinking about them, and this week or next I plan to embed some and send them off to Lugardon for sectioning. From looking at the "Retimonocolpites" with light (what has it been called? Retisulcites? It might be prudent to keep a separate name if there is one), I agree there are radial elements between nexine and tectum, but I still see something a little unusual about their shape as revealed by LO analysis. I'm looking forward to seeing TEMGs.

I really do think you should seriously re-evaluate the exine structure of the species with small perforations of which you gave me a photo of a broken surface. I bring this up since I was a little surprised to hear from Ken Brown that you were still interpreting the radial structures as columellae, despite our discussion at Bowling Green. Today I showed the picture separately to two SEM specialists (not palynologists) and asked them to interpret the structure between the two layers. I was very careful not to lead them on at all by saying how either of us had interpreted them or by telling them about exine structure concepts. Both saw essentially the same thing. One described the structure as consisting of vertical sheets running perpendicularly between the two layers, of which we were seeing the broken edges; he wasn't sure if the spaces between the sheets were cavities or tunnels, but he thought they were definitely concave. The other said she saw "cups" delimited by "membranes", "cells" or "compartments" (about as nice a description of alveolar structure as you could ask for). Again, she saw the tapering vertical elements as the illuminated broken edges of walls, not columns; think of the illuminated area on a wall as the sun shines in a tall window almost from the vertical. This prompts me to say something "heavy" which I ask you to please take as a constructive criticism and/or friendly warning that some people might be too falsely considerate to give you: you are getting a rather widespread reputation as someone who sees too quickly what he wants to see, and this case is all too good an example of why (think also of the still more



Hunt Institute for Botanical Documentation

obviously alveolar forms you initially thought were columellar). I have had to defend you on several occasions--I think so much of your work is valid and important that it would be a shame if people began to dismiss it because you haven't worked hard enough to correct this tendency to interpret things too quickly and in terms that a little more reflection would suggest might not be appropriate, or at least risky.

Here are a few points at random raised by your letters--I don't think I'll get a chance to deal with them in detail unless I stop at Penn State later this spring or summer (a definite possibility). On the Cabomba-type sculpture, think a little about what I said about Asimina and the thirst for modern names; within the Nymphaeaceae, only Cabomba has this particular sculpture, and if you're not claiming this is Cabomba, does the comparison mean anything? Read what Jack and I say on Onoana (p. 803-4).

On North Carolina Triassic insect faunas with beetles and the first Diptera: this matches what I learned in my fossil insect course about Upper Triassic faunas elsewhere. No doubt angiosperms wouldn't have had any problems getting pollinated in the Late Triassic if they had existed then, and Bennettitales undoubtedly exploited the beetle resource (why didn't they develop sculpture and perforations--or did they?). On the other hand, the first Lepidoptera, higher Diptera, and higher Hymenoptera are known from the Cretaceous, thanks to recent Russian work (summarized in Rodendorf & Zherikhin, 1974. Paleontologiya i okhrana prirody. Priroda 1974(5): 82-91).

To discuss your leaf, anatomy, and floral ideas would take too long: for leaves, check Leo Hickey and Jack Wolfe's paper in the Ann. Missouri Bot. Garden symposium and Doyle & Hickey on Potomac leaves in the Columbia book edited by Beck (I'll send you a copy). In general, it's important to avoid picking out examples here and there from modern angiosperms to argue a point without considering as much other evidence as possible on the relationships and advancement of the plants involved. There are so many weird angiosperms that you can find one to prove practically anything if you look long enough. E.g., with much straining some case might be made for the relevance of Berberidaceae (Rüffle has done this, I don't know the exact reference), though I doubt it in the light of the Cretaceous leaf record; but Ipomoea is a highly specialized angiosperm no matter how you look at it.

On the pre-angiosperms being forced underground by the radiation of Corollina producers: if so, why then did the Cretaceous ones

Hunt Institute for Botanical Documentation

first rise to dominance in the one part of the world (Africa-South America) where Corollina was still dominant?--unless they had changed basically in their ecology (and presumably morphology) while they were underground in the Jurassic. The situation must be different from the dinosaur-mammal one: Corollina doesn't first become extinct and the angiosperms then radiate; angiosperms start to radiate while Corollina is still going strong where they radiate most vigorously.

Your ideas on anasulcate to tricolpate may very well apply for Eucommiidites, but watch out before suggesting that this says anything about the origin of Aptian-Albian angiospermous tricolpates, except purely as an analogy*. Even if Eucommiidites has something to do with angiosperms (an idea for which I would have thought my paper with Van Campo and Lugardon had removed the last positive evidence), you'd still have to explain somehow the origin of angiospermous monosulcates which lack a laminated endexine, as well as the fact that early Cretaceous tricolpates are reticulate-columellar, like contemporaneous "advanced" monosulcates, not tectate-granular, like Jim Walker's "atectates" or Eucommiidites. (I assume you would now revise totally the first paragraph in "From anasulcate to tricolpate--a theory!") Incidentally, Nymphaea and Nelumbo may have something to say about how the "tricolpate" condition arose in Nelumbo, but it would be quite a job to show that Nelumbo was on the line to the rest of the tricolpate dicots.

Concerning dibenzofuran derivatives in Rhaeto-Liassic coals, the fact that they can be found in both lichens and (advanced!) angiosperms suggests more that they are more widespread in the plant kingdom now (and then) than we know, than that they indicate the existence of either lichens or angiosperms.

Well, I'm starting to run down. Please keep me informed as to developments; of course, I'll let you know immediately if or when Lugardon gets any results on the "Retimonocolpites."

With best regards,

Jim

James A. Doyle

*as I said at Bowling Green, I think this analogy is just as good as the trichotomosulcate or de novo hypotheses.

Hunt Institute for Botanical Documentation

The University of Michigan
MUSEUM OF PALEONTOLOGY
ANN ARBOR, MICHIGAN 48109
(313) 764-0489

January 28, 1976

Bruce Cornet
Department of Geosciences
Deike Building
Pennsylvania State University
University Park, Pa. 16802

Dear Bruce,

Thanks for your voluminous and stimulating letter and photos of January 26. The reason I didn't stop at State College was that I decided not to go east at all--too many papers to write. I'll answer your letter in more detail later (especially on leaf evolution), but there are a couple of things I wanted to say now about the pollen which I regard as more immediately important since they involve interpretation of direct observations rather than theory.

Your monosulcates are indeed beautiful and exciting in their excellent preservation and potential as the best sampling I've seen ever of all those poorly described sporadic Mesozoic monosulcates which I believe we need to known about if we're ever going to identify ancestors or early representatives of the angiosperms. However, I see no evidence of angiospermous exine structure features in these forms (i.e., features found today in angiosperms only, and unknown in gymnosperms). As is clear in all the cases where you have SEM, the "reticulum" is infratectal, and what is more important, all your photos are consitent with its being made up of a cycad-type alveolar "honeycomb" rather than true columellae (I say "consistent with" rather than "indicative of" since the details are at the verge of resolution by light; type Q is the most problematical, but I would be quite sure types A, B, and P are alveolar). In this respect, your sketches in Table 2, showing the reticulum on the outside, repeat the same mistake that some of the best people made early in the history of palynology (e.g., Bailey's pictures of Annonaceae with free-standing columellae, corrected by Walker; Van Campo, 1945ish, showing the reticulum of pine bladders on the outside). We tend to associate such structure with Cycadales, but similar structure occurs on the central body of winged conifers, and who knows what other groups of Triassic gymnosperms may have had. I'd look at some modern cycads, other gymnosperms, and angiosperms for comparison (bearing in mind that when one of the latter has exine structure like that of some gymnosperm you couldn't tell what it was if you found it in the fossil record), and I'd try to fracture some of the fossils and look at them under SEM. Hence

Hunt Institute for Botanical Documentation

I'd agree that these fossils are something to get excited about, but not something to be "disturbed" about. I think caution is in order at this point, not because of any weight of accumulated dogma (which should be taken lightly), but rather because the exine structure criteria we think can be used to tell angiosperms and gymnosperms are so delicate, and because in our present ignorance of exines in Mesozoic gymnosperms it would be unwise to apply them too quickly.

One subsidiary point: it's true one might get the impression from the literature that the angiosperm radiation largely begins at the tricolpate level, but this is not quite correct. First, many palynologists have tended to equate the first tricolpates with the first angiosperms (they may know there are monosulcate angiosperms, but they tend to think of these as monocots); second, even if you know there should be angiospermous monosulcates, they are often very subordinate numerically and hard to recognize as you scan for reconnaissance purposes. My paper with Van Campo and Lugardon should help clarify the situation, as should recent unpublished work by Hughes and Laing on the Wealden, which shows a nice suite of monosulcate angiosperms with much the range of total variation as in the basal Potomac. See also the enclosed abstract for this year's Bot. Soc. meeting on the African Lower Cretaceous. Actually, there's no reason to assume that the monosulcates had to undergo their radiation into "monosulcate-derived" types before the tricolpate radiation. Although some of the weird types are seen in the Lower Cretaceous (zonasulculates, trichotomosulcates), a lot of them are found today only within certain families (e.g., Annonaceae), and there is no reason not to believe that many of these families differentiated as such at about the same time as many triaperturate alliances (late Cretaceous, even early Tertiary). Notice, too, that some modern tricolpate groups have very primitive features (Illicium, Schisandra, various Ranunculales, vesselless Trochodendron and Tetracentron), suggesting that they could have branched off near the beginning of the radiation, rather than representing a secondary radiation two periods after the first. Again, I'm not saying this to argue for some particular dogma, but just to emphasize the rather wide margins of uncertainty that both modern and fossil evidence permit.

Just a few points on leaves (I'll develop them later when I'm not so rushed): who uses marginal veins as a general criterion for or against angiosperm affinities? Does the finer venation of Scoresbya look particularly angiospermous (remember, reticulation itself is not enough!)? Why don't you see angiosperm leaves reminiscent of Scoresbya until the Upper Cretaceous, whereas there are lots of "normal" pinnate leaves earlier?

Best regards,

Jim

Hunt Institute for Botanical Documentation

For Botanical Society of Africa meeting, 1976

JAMES A. DOYLE, SERGE JARDINÉ & ANTOINE DOERENKAMP. Museum of Paleontology, University of Michigan, Ann Arbor, Mich. 48109, and Elf R.E., Boussens, France.--Evolution of Angiosperm Pollen in the Lower Cretaceous of Equatorial Africa.

The Cocobeach Series of Gabon and equivalents in the Congo and Brazil consist of fluvial-lacustrine rift valley sediments, overlain by salts and uppermost Aptian-Albian carbonates marking the first entry of marine waters into the South Atlantic. As in North America and Europe (Laurasia), the oldest recognizably angiospermous pollen grains from the middle Cocobeach (Elf palynozones V and VI) are columellar monosulcates; in Zone VI, these show much the same range of sculpture variation as in the basal Potomac Group of the U.S.A. and the Barremian of England (tectate, finely to coarsely reticulate, crotonoid). Reticulate operculate forms, first seen in dated Aptian rocks of Algeria, and the first reticulate tricolpates appear in Zone VII; by Zone IX (including the salt deposits), tricolpates are relatively diverse (reticulate, verrucate, striate). These Aptian occurrences of tricolpates in Africa (as in Brazil and Israel) antedate their first definite occurrences (Albian) in Laurasia; however, except for the presence of polyporates (lower Albian on) and tricolpodiorates (upper Albian), the timing of later appearances is roughly the same as in Laurasia: tricolporates in the middle or upper Albian, triporates in the Cenomanian. These data suggest origin of some major angiosperm types (particularly tricolpates) in Africa-South America, and more favorable conditions there for early angiosperm diversification (semi-arid, judging from associated "ephedroid" and Classopollis-producing gymnosperms and salt deposits), but lag in migration into Laurasia was not great enough to distort the underlying evolutionary sequence.

Hunt Institute for Botanical Documentation

February 11, 1974

Dr. James A. Doyle
Department of Botany & Museumoof
Paleontology
University of Michigan
Ann Arbor, Michigan 48104

Dear Jim:

Perhaps it's a question of the right hand not knowing what the left hand is doing. I'm sitting here before a roaring fire reading a big batch of past mail and came across a reprint of the monocot paper which I recently requested. I can't read the date on the post mark, and am not sure whether this represents a response to my recent letter or whether you had sent the paper previously. In any case, it is fascinating reading and speaks to a question about which I have long pondered--the perplexing business of the ~~innova~~-derivation ~~tion~~ of the monocots from the dicots. The traditional theories never made me very happy.

Thanks for sending this paper, and I expect I will be meditating about it for some time and would rather not comment in detail at this point.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/vsi
P.S. How can a man as busy as you are find time to even address his own mailing labels? Admittedly, your script is a wonder to behold, but somebody around your outfit should take care of that sort of thing for you.

Hunt Institute for Botanical Documentation

January 17, 1974

Dr. James Doyle
Department of Botany
University of Michigan
Ann Arbor, Michigan 48104

Dear Jim:

My student, Bruce Cornet, has shown me a reprint of your monocot paper which you sent him. I had not seen it and am much impressed and very interested. I do hope you will be able to send me a reprint also. This incident prompted me to look through my card file to see what I had sent you recently and note to my amazement that I still carry you as a graduate student at Harvard and have sent you absolutely nothing. This is pure oversight ~~on~~ my secretary's and my fault, and I hasten to send off to you a batch of what I happen to have at the moment, mostly of little interest to you probably, but you can always pitch what you don't want. In the meantime, I hope you will send me a copy of the monocot thing and we will keep in better touch in the future!

Best wishes for 1974.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

November 13, 1969

Mr. James Doyle
Biological Laboratories
Harvard University
Cambridge, Massachusetts 02138

Dear Jim:

I just have been digging out from underneath the Second Annual Convention of the AASP (why don't you join?), and have finally had a chance to read my copy of "Cretaceous angiosperm pollen of Atlantic Coastal Plain and its evolutionary significance". It was good of you to send it to me, and I have found it extremely interesting reading. You seem to have done a very thorough job of reviewing the literature, in addition to your own work on the floras of the coastal plain. Had I stayed in Texas I had every intention of getting into the Cretaceous field--in fact, I had a series of sediments near Austin that I had already begun to work on before I left. Now I suppose it's unlikely that I will do more than read papers like yours and cheer from the sidelines, both because of other research interests being more pressing and because I really seem to becoming sort of a research director instead of (drat it!) doing much myself. The reason is purely economic. If you have to spend most of your time promoting things to raise money to pay your salary there isn't too much time left for other activities.

Why don't you plan to drop in at Penn State on one of your itineraries? It would be good to get to know you a little better.

Yours very truly,

Alfred Traverse
Associate Professor of Geology
andBBiology

AT:vsi

Hunt Institute for Botanical Documentation

16 November, 1983

Dr. Robert G. Doyle
Delaware Geological Survey
University of Delaware
Newark, DE 19711

Dear Dr. Doyle:

Thanks for yours of 26 October.

Re Newark Supergroup sediment ages: all Basins contain sediments dated palynologically as being as old as Carnian, except the Hartford Basin, which seems to yield no sediment older than Norian. The southern Basins in fact so far have yielded no sediment of ages other than Carnian. Newark-Gettysburg-Culpeper Basin goes Carnian to Pleinsbachian; the Hartford-Deerfield Basin, Norian to Bajocian; the Fundy Basin, Carnian to Pleinsbachian (Possibly Toarcian).

In answer to Dr. Benson, our sampling concentration does not as yet allow us to be helpful, as far as fill pattern is concerned. Interesting question!

I read your abstract with great interest--thanks.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

State of Delaware
DELAWARE GEOLOGICAL SURVEY
UNIVERSITY OF DELAWARE
Newark, Delaware
19711

ROBERT R. JORDAN, STATE GEOLOGIST
101 PENNY HALL
PHONE: 302-738-2833

October 26, 1983

Dr. Albert Traverse
Department of Geological Sciences
Penn State University
University Station,
Pennsylvania 16802

Dear Dr. Traverse:

Thank you for the time you were able to spend with me on the telephone Tuesday afternoon. I can appreciate your busy schedule. Let me reintroduce myself; I am working on a project with another scientist, Dr. Richard Benson. Our work is concerned with a study of the buried rift basins of the inner margin of the Atlantic continental shelf. I have finished up a draft of a paper which I hope to give at the Northeast Section of GSA next March. It will re-examine the tectonic history of the rifted margin of the Mid-Atlantic Region. A copy of the abstract is enclosed.

One of the particular concerns that we have is the oldest age of the sediments in the basins. During our phone conversation you said that all of the rifted basins examined in any detail contained material which gave at least a Carnian age, with the possible exception of the Hartford basin. I believe you also said that the basins from central Virginia on south contained no sediments (sampled) of Jurassic age. Could you confirm both of these statements for me? The only other question relative to age would relate to the youngest age date that you have recovered from any of the rift basins in the Atlantic Region. We would appreciate your ideas on that subject.

Dick Benson, my partner in the effort, is a biostratigrapher. He wishes to ask if you have some idea about the depositional history of the beds from which you have taken samples. Depending on the way these basins were filled, the location of the earliest fill could be in several parts of an individual basin. Do you feel that the extent of your sampling has allowed you to cover any possibility regarding the depositional and fill pattern, and therefore, the location of the oldest or nearly oldest sediments. Could you comment on this thought?

Hunt Institute for Botanical Documentation

I appreciate any time that you can take to respond to these few questions. Your experience in this area is large and the ideas you have would be very important to us. Thank you very kindly for your attention.

Yours very truly,

Robert G. Doyle
Research Associate III

RGD:dcw

Hunt Institute for Botanical Documentation

# 1984 GSA ABSTRACT FORM

USE THIS FORM FOR ALL 1984 GSA MEETINGS (SECTION & ANNUAL MEETINGS)

**PLEASE COMPLETE ALL SECTIONS BELOW, 1 THROUGH 8**

**① TYPE YOUR ABSTRACT IN THE SPACE BELOW,** using fresh black carbon ribbon. Follow the exact format shown on this year's instruction sheet. Blue lines below show absolute limits for your abstract; do not exceed them. Do not fold abstract when completed, as folding causes carbon to flake off paper. Retyping may be required.

№ 44056

GEOLOGIC FRAMEWORK FOR THE DEVELOPMENT OF THE U. S. MID-ATLANTIC CONTINENTAL MARGIN

DOYLE, Robert G. and BENSON, Richard N., Delaware Geological Survey, University of Delaware, Newark, DE 19716

Studies of multichannel seismic-reflection profiles were used to construct a geologic framework for the development of the U. S. Mid-Atlantic Continental margin. Mesozoic extension of the margin was a tectonic continuum marked by three recognizable stages. Stage A, the rift stage, had two sub-stages: A-1, with horizontal extension, initiated by normal faults that formed large grabens, in Late Triassic time, along reactivated Alleghenian fracture trends; A-2, with diffuse rifting, that opened NNE-trending, listric (?) fault-bounded basins. A-2 tensional stresses were intensified by mantle plume-generated continental uplift of 1-3km along the present Atlantic margin in a 200km-wide belt.

Stage B, the break-up stage, began when two rift arms from each triple junction linked with rifts of adjacent triple junctions to form a connected series of basins. The third, unlinked (failed) arms became sediment-filled grabens, one of which was the precursor of the northern part of the Baltimore Canyon trough. Subsidence along the two active arms in the Mid-Atlantic, the SW-trending (Delmarva) and ENE-trending (Long Island) arms, occurred by block faulting accompanied by vulcanism in Early Jurassic time to form a transition crust. Coeval erosion of the adjacent highlands and filling of the Stage B basins resulted in a planation surface.

Stage C, the spreading stage, began as a crustal rupture along the east side of the Stage B basins above the initial Mid-Atlantic spreading center in Early Jurassic time. The locus of rupture defines the boundary between transition crust and new oceanic crust, now marked in places by the East Coast magnetic anomaly. Spreading was accompanied by major subsidence of the planation surface formed in Stage B which was onlapped by the sedimentary fill of the Baltimore Canyon trough.

**② REVIEW CLASSIFICATION**

For review purposes, you must specify only one of the following categories. Pick the one in which reviewers can be expected to evaluate your abstract properly. If you are unsure, check category 31. The category checked here does not necessarily determine the technical session in which your abstract will be included, if accepted.

- ☐ 1 archaeological geology
- ☐ 2 coal geology
- ☐ 3 economic geology
- ☐ 4 engineering geology
- ☐ 5 environmental geology
- ☐ 6 general geology
- ☐ 7 geochemistry
- ☐ 8 geology education
- ☐ 9 geomorphology
- ☐ 10 geophysics
- ☐ 11 geoscience information
- ☐ 12 history of geology
- ☐ 13 hydrogeology
- ☐ 14 marine geology
- ☐ 15 mathematical geology
- ☐ 16 micropaleontology
- ☐ 17 mineralogy/crystallography
- ☐ 18 paleontology/paleobotany
- ☐ 19 petrology, experimental
- ☐ 20 petrology, igneous
- ☐ 21 petrology, metamorphic
- ☐ 22 petrology, sedimentary
- ☐ 23 planetary geology
- ☐ 24 Precambrian geology
- ☐ 25 Quaternary geology
- ☐ 26 sedimentology
- ☐ 27 stratigraphy
- ☐ 28 structural geology
- ☐ 29 tectonics
- ☐ 30 volcanology
- ☐ 31 other—describe below

_____

_____

**③ TYPE OF SESSION PREFERRED:**

See instructions for information on each session type.

☒ Oral ☐ Poster ☐ Either

☒ Symposia Geophysics of the Coastal Plain and Continental Margin of the U. S.

(Title of symposia for which your abstract was invited)

**④ CAN YOU BE A SESSION CHAIRMAN?**

If so, please complete:

Topic _____

Your name _____

Telephone (late June/early July) _____

**⑤ OF THE AUTHORS ON THIS PAPER, HOW MANY ARE:**

GSA Mbr/Fellow _____ Non-GSA Professional _____

GSA Student Assoc. _____ Non-GSA Student _____

**⑥ SPEAKER'S IDENTITY AND CONTACT ADDRESS:**

Speaker's name Robert G. Doyle

Organization Delaware Geological Survey

Address University of Delaware

City Newark State DE Zip 19716

Office Telephone: ( 302 ) 738-2833

Home Telephone: ( ) _____

Dates I can be reached: _____

**⑦ PERCENTAGE OF THIS PAPER PREVIOUSLY PRESENTED** 0 **WHERE AND WHEN PRESENTED**

_____

_____

**⑧ SEND ORIGINAL + FIVE COPIES OF ABSTRACT TO APPROPRIATE OFFICE. SEE ATTACHED INSTRUCTIONS FOR ADDRESSES AND DEADLINES.**

THE GEOLOGICAL SOCIETY OF AMERICA
(303) 447-8850

Hunt Institute for Botanical Documentation

22 September, 1982

Dr. Avery A. Drake, Jr.
Assistant Chief Geologist, Eastern Region
U.S. Geological Survey
Reston, VA 22092

Dear Dr. Drake:

Lauren Wright has kindly referred to me yours to him of 31 August, re the planned multifaceted study of the Triassic-Jurassic basins of eastern North America.

I have had a (mostly paleopalynological-paleobotanical) project here on this subject for many years. It is currently funded by NSF grant # EAR-8204410, "Palynological biostratigraphy of continental Upper Triassic-Lower Jurassic rocks of North America". Doctoral theses produced have been: (list includes related papers).

Dunay, Robert E., 1972 (Ph.D thesis): "The palynology of the Triassic Dockum Group of Texas, and its application to stratigraphic problems of the Dockum Group." Pennsylvania State University.

(also):
Dunay, R.E. & A. Traverse, 1971. Preliminary report on Triassic spores and pollen of the Dockum Group, Texas Panhandle: Geoscience & Man 3:65-68.

Dunay, R.E. & M. J. Fisher, 1974. Late Triassic palynofloras of North America and their European correlatives: Rev. Palaeobot. Palynol. 17:179-186.

Cornet, Bruce, 1977. (Ph.D thesis): "The palynostratigraphy and age of the Newark Supergroup". Pennsylvania State University.

(also):
Cornet, Bruce & A. Traverse, 1975. Palynological contributions to the chronology and stratigraphy of the Hartford Basin in Connecticut and Massachusetts: Geoscience & Man 11:1-33.

Cornet, Bruce, A. Traverse & N.G. McDonald, 1973. Fossil spores, pollen, and fishes from Connecticut indicate Early Jurassic age for part of the Newark Group: Sci. 182:4118:1243-1247.

Hunt Institute for Botanical Documentation

Robbins, E.K., 1981 (Ph.D thesis): "Fossil Lake Danville: the paleoecology of a late Triassic ecosystem on the North Carolina-Virginia border". Pennsylvania State University.

(also):
Robbins, E.I. & A. Traverse, 1980. Degraded palynomorphs fro m the Dan River (North Carolina) - Danville (Virginia) Basin: Carolina Geol. Soc. Field Trip Guidebook B:x:1-11.

Robbins, E.I. & A. Traverse, 1982 (paper presented at annual mtg. of AASP in Dublin, 14 September 1982):"Processes of natural resource accumulation in ancient lake sediments in the Newark Rift System (eastern North America).

At present, Ronald J. Litwin is working on the western USA Triassic for a M.Sc. and later Ph.D. Dr. Carmen Tavera is a post-doc with me, working on the Eagle Mills Fm. of Texas. Dr. Sydney R. Ash of Weber State (Utah) is working on the megafossil plants in the Newark Supergroup Basins, in collaboration with my own palynological investigations. I expect to continue such research as long as I am at Penn State.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: Lauren Wright

Hunt Institute for Botanical Documentation

Transferse

# United States Department of the Interior

GEOLOGICAL SURVEY
RESTON, VA. 22092

In Reply Refer To:
Mail Stop 953

August 31, 1982

Professor Lauren A. Wright
Department of Geosciences
Pennsylvania State Univeristy, University Park
University Park, Pennsylvania 16802

Dear Lauren:

The U.S. Geological Survey plans to carry out a multifaceted study of the Early Mesozoic basins (Triassic-Jurassic) in Eastern North America over the next few years. To further that objective, we solicit your help in the form of information, advice and assistance. Al Froelich of the Branch of Eastern Regional Geology (Mail Stop 926, National Center, Reston, Virginia 22092; 703 860-6421) is assembling and assessing the available geological, hydrological, and geophysical data pertaining to these basins and their surrounding areas. He would like your help in obtaining the following materials:

1. A list or copies of Graduate Theses, or other references on Early Mesozoic basins.

2. A list or copies of any maps and their scales that cover the exposed Mesozoic rocks.

3. A summary of geophysical data.

4. Geologic and electrical logs of deep drill holes, core holes, water wells, or oil wells, especially depth and locations of any that penetrate to pre-Mesozoic "Basement" and whether cores or cuttings are available for study.

We would also like to know of your faculty's current geologic and geophysical studies of Mesozoic rocks, the names of interested faculty members, and your plans for future studies.

Thank you for your cooperation.

Sincerely,

Avery A. Drake, Jr.
Assistant Chief Geologist,
Eastern Region

Hunt Institute for Botanical Documentation

Dr. Alfred Traverse
527 Deike Building
Pennsylvania State University
University Park, Pa. 16802

1 August, 1975

Mr. Jeff Dravis
Inst. of Marine Sciences
1 Rickenbacker Causeway
Miami, FL 33149

Dear Mr. Dravis:

Yours of 25 June came during a prolonged absence of mine in Eur_ope. I only today returned to the office (to find my secretary has resigned--pardon the typing.).

Regarding my work in the Bahamas: As Dr. Ginsburg probably remembers, my work in the B. was done remote control. I mean, I have never been there but worked on samples and data provided by Dr. G.

Therefore, I do not have more geographical or geological information on the samples--only what came from his notebooks at the xtime. It follows that if he doesnxt have information, I canxt be of much help.

I am puzzled and disturbed by the "discrepancies" you report. I thought that Dr. G. had checked the figures from the report and the publication pretty thoroughly. I enclose a xerox of the location chart from the original company report (EPR Report 591, 1960--perhaps Dr. G. has a copy). Does it agree with the figures in the Marine Geology paper?--I don't have a copy here. If not, the figures in the co. report would take priority! Please let me know.

Sorry not to be more helpful.

Yours very truly,

Alfred Traverse

Alfred Traverse
Professor of Palynology

Enclosure: xerox of page from EPR Rept 591

Hunt Institute for Botanical Documentation

# UNIVERSITY OF MIAMI

INSTITUTE OF MARINE SCIENCES
1 RICKENBACKER CAUSEWAY
MIAMI, FLORIDA 33149

Cable: IMSUM
Phone: (305) 350-7211

June 25, 1975

Dr. Alfred Traverse
Dept. of Geology and Geophysics
Pennsylvania State University
University Park, Pa. 18602

Dear Dr. Traverse,

I am graduate student here at the Marine Lab, and my thesis research has been the investigation, in detail, of Holocene depositional environments on Eleuthera Bank, leeward of Eleuthera Island, Bahamas.

Your 1966 paper, co-authored with Dr. Ginsburg (in Marine Geology, vol. 4, p. 417-459), and dealing with the palynology and sedimentation in the Bahamas, lists about ten sample locations from the Eleuthera Bank area.

I approached Dr. Ginsburg about this and it turned out that he had most of these samples in storage, from which I was able to obtain splits. He also had a map showing the locations of these sample stations; the only problem is that there are discrepancies from his "field map", compared to what was published in Marine Geology.

He suggested that I write in hopes that you might be able to clarify the locations of the sample locations. Also, would you have any additional information about each of the stations, such as water depth, sediment thickness, benthic communities, and sediment analyses?

I would greatly appreciate any help you can give me as these samples would be beneficial to my work. However, before I can use them I must be positive about the locations, since one of the purposes of the study is to derive a map of the surface sediments on Eleuthera Bank.

The stations in question are: 6275, 6180, 6181, 6182, 6183, 6184, 6185, 6186, 6187.

Thank you for your time and assistance in this matter.

Sincerely,

Jeff Dravis

Jeff Dravis
Dept. of Marine Geology

Hunt Institute for Botanical Documentation

QUEENS COLLEGE
of THE CITY UNIVERSITY OF NEW YORK
FLUSHING • NEW YORK 11367

DEPARTMENT OF GEOLOGY

TELEPHONE: 212-445-7500

Dr. Traverse

May 15, 1970

Dear Dr. Traverse,

Thank you for your kind letter. I would like to tell you that my decision to study at Columbia instead of at Penn State is purely a personal one and that it does not reflect any deficiency on the part of the Geology Department at Penn State. There is no doubt that Penn State has the most adequate facilities for palynological investigation. In fact, the only criticism that I can make at the moment is that Penn State is so "land-locked." In the last few semesters I have become more and more interested in marine geology and oceanography and in the use of palynology as a tool to study these fields. I think that Lamont-Doherty Geological Observatory which is associated with Columbia offers me a good opportunity to pursue my interest in the deep-sea environment.

I am no longer studying pollen

Hunt Institute for Botanical Documentation

QUEENS COLLEGE
of THE CITY UNIVERSITY OF NEW YORK
FLUSHING • NEW YORK 11367

DEPARTMENT OF GEOLOGY TELEPHONE: 212-445-7500

sedimentation off the western coast of Puerto Rico since I was unable to obtain, on time, sufficient samples from the Puerto Rico Nuclear Center in Mayaguez. For the past semester I have turned my concentration from spores and pollen to dinoflagellates and I am now studying the Upper Cretaceous – Lower Tertiary dinoflagellate stratigraphy of a long core from the New Jersey Coastal Plain. I hope to be finished over the summer and to defend my thesis in September.

I would like to thank-you again for your kind interest and for the warm hospitality that I received at Penn State. I hope that I have not caused you any inconvenience.

Sincerely yours.

Marlene M. Dreyfus

Please find enclosed a check for $5.00 covering my dues for this year in AASP, and a check for the same amount from Steve Jackson. ~~Thank you~~.

Hunt Institute for Botanical Documentation

April 23, 1970

Miss Martine M. Dreyfus
Department of Geology
Queens College
Flushing, New York 11367

Dear Martine:

Thank you for your brief note of 12 April, which came today. I'm of course sorry to hear that you have decided against working on your doctorate here, and I am hoping that whatever alternate arrangements you have made will prove satisfactory. I wonder if you could take time at your convenience to tell me quite frankly whether there is something wrong with our "recruiting policy" which prompted you to change your mind. I have been concerned for some time about whether we are presenting the obviously excellent facilities of Penn State in palynology in the best possible light, and your comments would be quite useful to me.

Best wishes to you.

Yours very truly,

Alfred Traverse
Associate Professor of Geology
and Biology

AT:kc

Faculty Dining Room
150 SEATS

Hunt Institute for Botanical Documentation

QUEENS COLLEGE
*of* THE CITY UNIVERSITY OF NEW YORK
FLUSHING • NEW YORK 11367

DEPARTMENT OF GEOLOGY

TELEPHONE: 212-445-7500

April 12, 1970

Dr. A. Traverse
Department of Geology
The Pennsylvania State Univ.
State College, Pennsylvania

Dear Dr. Traverse,

I will not be able to attend the Pennsylvania State University in September, 1970 as a Ph. D. candidate. However, I would like to thank you for your kind interest.

Respectfully yours,

Martine M. Dreyfus

Hunt Institute for Botanical Documentation

MISSOURI BOTANICAL GARDEN
"SHAW'S GARDEN"
2315 TOWER GROVE AVENUE
ST. LOUIS 10, MISSOURI
PROSPECT 6-5567

July 15, 1958

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

Dear Al:

I am sending you the reprint under separate cover. If I were writing it over again there would be some changes and additions, but the conclusions would be the same, except that I would not stress vegetational continuity (complete) as a prerequisite for migration.

Went has been hired primarily as a money getter, as I understand it. He does not consider himself a physiologist, but rather more of an ecologist. He spent some time with the Buitenzorg Gardens some years back. So I am reserving judgement for the present.

Sincerely,

Bob

R. Dressler

Hunt Institute for Botanical Documentation

**Chevron Oil Field Research Company**
A Standard Oil Company of California Subsidiary
P.O. Box 446, La Habra, CA 90631

January 4, 1974

Prof. Alfred Traverse
Dept. of Geology & Geophysics
College of Earth and Mineral Sciences
The Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

Many thanks for the impressive collection of reprints. All are excellent contributions and will be of considerable use to me. There's no doubt but that palynology can at times be of assistance in Franciscan geology and is certainly worth trying. We have come across the H. lepidophytus assemblages in several wells recently from various parts of the world. It seems to be a marker worldwide but what with the Heerlen Congress etc., it's pretty hard to state categorically whether one is in the uppermost Devonian or lowermost Carboniferous. At least it puts one in the ballpark. With highest regards and all best wishes.

Sincerely,

Warren

Warren S. Drugg

Hunt Institute for Botanical Documentation

September 26, 1973

Dr. Warren S. Drugg
Chevron Oil Field Research Corp.
P.O. Box 446
La Habra, California 90631

Dear Warren:

Thanks for yours of 4 September. I still have not received the correspondence, but I suppose at this juncture it would be best to merely regard this as one of the more amusing experiences of our lifetimes and not worry about it too much. I'm only hoping that they don't expect the Newsletter article in the very near future. It obviously will have to be post-Anaheim business! I will let you know immediately when and if the CIA stuff arrives.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

P.S. What are the chances that the unfortunate use of "CIA" is what is causing the correspondence not to make its way up out of your laboratory or across the country? I am only half kidding.

Hunt Institute for Botanical Documentation

4 Sep 73

Dear Al:

The package containing all of the CIA correspondance has been mailed today. Let us hope that it arrives there intact. It includes the "missing" letters - or at least it did when left in the mail room.

What is wanted is a resumée for inclusion in our newsletter. The length and scope would be up to you.

Regards

Warren

Dregg

Hunt Institute for Botanical Documentation

August 15, 1973

Dr. Warren S. Drugg
Chevron Oil Field Research Corp.
P.O. Box 446
La Habra, California 90631

Dear Warren:

Thanks for your letter of 10 August. I had naturally hoped that the "self-explanatory letters" simply were put aside on your desk somewhere and that you would be able to recover them. Your mention in the "PS" of the "files on the correspondence etc." is very interesting. I would think that such correspondence would help me greatly in putting together what I am supposed to put together. However, I don't think that the correspondence should be sent to me in its orgginal form. Rather, I would hope that Chevron would let some little beaver make a xerox copy of the letters and send that on to me. One of these days when I get around to this matter I suppose I had better ask Harry Leffingwell to do something of the same. I do not like to trust to my memory of how things went at various meetings, many of which occurred late at night--well beyond my maximum efficiency time! The degree of intimacy with which I am supposed to know this subject has been highly exaggerated. I did not attend any of the international meetings which spawned the ICP with the exception of the one at Utrecht in 1966, at which it could not be aggued that I was in the "inner circle", having recently risen sphinx-like from the ashes of my seminary experience--or at least that's the way many people viewed it!

Now for the clincher, which is likely to send you off into a wild tantrum of some sort: the "letters" which were supposed to beplace the ones which you previously did not enclose and which were mentioned in this letter of 10 August expressly under your signature as "attach: letters", were <u>not</u> attached or enclosed. All that came in the largish envelope was your letter, and as I openedtit I was admittedly

Hunt Institute for Botanical Documentation

speculating about why good old Warren was now sending one page letters in 8x10 envelopes. I presume that you and I can laugh about this over a martini in Anaheim. In the meantime, if you want to have a try at your third strike, it would be appreciated.

Best wishes and a chuckle here and there.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

**Chevron Oil Field Research Company**
A Standard Oil Company of California Subsidiary
P.O. Box 446, La Habra, CA 90631

August 10, 1973

Dr. Alfred Traverse
Dept. of Geology and Geophysics
College of Earth and Mineral Sciences
The Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

I am rather amused that I forgot to enclose the letters which were supposed to be self-explanatory. I wonder who in the world I did send them to? In any case I will try again although there's no guarantee of success. Actually you already have the general idea and the amount of detail, length, diplomacy etc. are up to you. With all best wishes.

Yours very truly,

Warren

Warren S. Drugg

Attach: Letters

P.S. I have the files on the correspondence etc of AASP which I will be happy to send to you if you so desire.

Hunt Institute for Botanical Documentation

August 28, 1973

Dr. Warren S. Drugg
Chevron Oil Field Research Corp.
P.O. Box 446
La Habra, California 90631

Dear Warren:

Your letter of 20 August makes the mystery still deeper, doesn't it? Is there sombbody in your mail room who is opposed to the idea of enclosures in business letters!? The last one was really funny because the one little lonely letter arrived in a large manila envelope suitable for reprints and whatnot. Your second paragraph stating that you are going to hold off at more attempts at failure, in your words, leaves me up in the air because I did get the impression from the first letter that there was something I was supposed to do, and the enclosures with your letter were supposed to help me understand just what it was I am supposed to do. As you say, however, the CIA files ought to go a long way toward helping me perform the task of summarizing the history of ICP vis-a-vis the AASP. As long as I wasn't supposed to have something ready by mid-October, I am all right. We have three papers for the Anaheim meeting to get ready for that.

Best wishes, and hang in there. Fire your mail room staff!

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

20 Aug 73

Dear Al:

I can't believe it! I suppose that I may have "screwed-up" the first time - but the second time I know that my letter and 4 or 5 letter were clipped together and left in our mail room for mailing. As I mentioned before they are not as informative as you might hope.

I will hold off temporarily at any more attempts at failure — because I have to work on plates for the dino forum and time is running out.

I will Xerox the CIA files within a couple of weeks ±. I believe I have all of Leffingwell's files which I think he was glad to get rid of. However, I wouldn't swear to it. I am quite sure that the Xeroxes from me will comprise the whole thing.

Warren Dingg

Hunt Institute for Botanical Documentation

July 18, 1973

Dr. Warren S. Drugg
Chevron Oil Field Research Corp.
P.O. Box 446
La Habra, California 90631

Dear Warren:

Thank you very much for yours of 5 July. In your letter you say "the enclosed letters are self-explanatory". However, there was only one letter enclosed which was an item from Geoff Norris to you. I can read between the lines, however, that what is wanted is a resume regarding the current status of ICP fro the AASP Newsletter. As the "official" US representative on ICP, I suppose I am the natural one to write the resume. However, I will seek assistance from Harry Leffingwell, who has been my "mentor" in these matters for sometime. He has the advantage on me of having attended the Novosibirsk congress. I suppose the presumed "missing letter" from you to Geoff explains more precisely what you had in mind regarding the summary, and perhaps you had better let me have that so that I'll know how long, how detailed, etc.

Looking forward to seeing you in Anaheim, I am with best wishes

Your friend,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

**Chevron Oil Field Research Company**
A Standard Oil Company of California Subsidiary
La Habra Laboratory • P.O. Box 446, La Habra, CA 90631

April 3, 1972

Dr. Alfred Traverse
Department of Geology and Geophysics
College of Earth and Mineral Sciences
The Pennsylvania State University
University Park PA 16802

Dear Al:

Many thanks for "Palynology, petrology, and depositional environments of some early Tertiary lignites in Texas", "Preliminary report on Triassic spores and pollen of the Dockum Group, Texas Panhandle", and the review of Aspects of Palynology. These are fine contributions and I'm very pleased to have them. I have already used your Dockum paper as an aid in dating some samples from another part of the world. In passing I might add that I agree with your comments on the Tschudy-Scott book. With all best wishes.

Sincerely,

Warren

Warren S. Drugg

Hunt Institute for Botanical Documentation

CHEVRON OIL FIELD RESEARCH COMPANY
A STANDARD OIL COMPANY OF CALIFORNIA SUBSIDIARY
LA HABRA LABORATORY P.O. BOX 446 LA HABRA CALIFORNIA 90631

May 25, 1971

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

Dear Al:

I have no objections whatsoever regarding the use of the dinoflagellate observations which I made some time ago. In fact I am flattered. Perhaps I have mentioned this before but in the past I have examined quite a few Franciscan samples. Some were barren but I recall that a couple of them had Proteacidites thalmanii which suggests Maastrichtian or there abouts. Another sample had Taurocusporites suggestive of Lower Cretaceous. There is no doubt but that the Franciscan includes a number of different ages. Your paper is excellent and will be of great interest to everyone dealing with the Franciscan. Through papers like yours we will eventually be able to piece together the history and details of the Franciscan.

I am looking forward to seeing it in print. With all best wishes.

Sincerely,

Warren

Warren S. Drugg

Hunt Institute for Botanical Documentation

May 18, 1971

Dr. Warren Drugg
Chevron Oil Field Research Corporation
P.O. Box 446
La Habra, California 990631

Dear Warren:

I am enclosing a xerox copy of the note on the Franciscan materials which is to be published in the AASP third proceedings volume. I am hopeful that you will not feel that I used too much information from your most kind letter of several months ago. It still would not be too late to slice part of that out, but I believe you will agree that it greatly strengthens the paper. Let me know if you feel that I should do some chopping or anything else in relation to what you said. I am of course hopeful that you will think it was all right. Looking forward to seeing you again one of these days xx months, I am

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/mdw
Enclosure: xerox copy

Hunt Institute for Botanical Documentation

January 8, 1971

Dr. Warren S. Drugg
Chevron Oil Field Research Corp.
P.O. Box 446
La Habra, California 90631

Dear Warren:

I am very grateful to you for having taken so much time with my rather pitiful slides of Franciscan material, and I am looking forward to getting the slides back for comparison with what you have provided in the letter. To date they still haven't come, but I suppose will in a few days. I am happy that your indication is that the date is most probably Upper Jurassic, though Lower Cretaceous is not excluded, because that is exactly what I had figured from the pollen information (spotty as it is!).

I would like to make some reference to the general cast of what you have said in the final paper, but I would naturally want you to see what I said for your approval before I submit it. The way things are shaping up for this term, that might be at least a few weeks! Thank you again, very much, and very best wishes for 1971.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

**CHEVRON OIL FIELD RESEARCH COMPANY**
A STANDARD OIL COMPANY OF CALIFORNIA SUBSIDIARY
LA HABRA LABORATORY P.O. BOX 446 LA HABRA CALIFORNIA 90631

December 30, 1970

Dr. Alfred Traverse
Department of Geology and Geophysics
College of Earth and Mineral Sciences
The Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

I have looked at your Franciscan slides and feel that they are probably Upper Jurassic, albeit with no great or absolute confidence. My feeling is partly based on what is there and partly based on what is not there. I have numbered your slides with pieces of tape so that you can check back on the specimens.

Slide #1 - PRC 2030

Specimen #6 - Micrhystridium fragile Deflandre. Range Bajocian to Oxfordian. I have little faith in the genus Micrhystridium for age-dating. *Also reported K - Oligocene,*

Slide #2 - PRC 2028

Specimen #1 - This specimen resembles Hystrichosphaeridiopsis erugata and H. adnata--both new species from John Warren's unpublished Ph.D. Dissertation on "Dinoflagellates and acritarchs from the Upper Jurassic and Lower Cretaceous rocks of California." Both species range from Tithonian (Portlandian) to Hauterivian. The genus has never been published. Your specimen is too poorly preserved to say much about any affinities with validly published genera.

Specimen #2 - This specimen appears to be sort of a cross between Tanyosphaeridium isocalamus (Defl. & Cookson) and "Hystrichosphaeridium" multicalamus n. sp. of Warren. The former ranges from Tithonian to ?Aptian. The latter from Berriarian to ?Aptian. Your specimen is closest to Warren's species.

Specimen #3 - Micrhystridium inconspicium Deflandre. Same comments as for the previous Micrhystridium. *U. Jur. (also reported from Cret.)*

Slide #3 - PRC 2028

Specimen #6 - Resembles Paskentasphaera plurimispinora n. gen., n. sp. of Warren. Tithonian to Hauterivian.

Specimen #7 - Resembles Scriniodiniopsis variabilis n. gen., n. sp. of Warren (Range, Tithonian-Barremian). Of published forms it is close to Scriniodinium galeritum (Deflandre) Klement (Upper Jurassic-Malm). I couldn't really see the archeopyle so I don't know which genus would be more correct. → *Endoscrinium of Vozzh.*

Specimen #8 - Probably an Oligosphaeridium--also probably long-ranging as a general type.

Hunt Institute for Botanical Documentation

Slide #4 - PRC 2028
Specimen #4 - This seems close to Polystephanophorus paracalathus Sarjeant from the Oxfordian.

Slide #5 - PRC 2028
Specimen #5 - Resembles Paskentasphaera plurimispinosa of Warren mentioned above.

As you can see, there is some spread of ranges but most forms would probably fit best in the Upper Jurassic. I have relied on Warren's work quite a bit because he deals with California Mesozoic phytoplankton. In any case, exact comparisons are difficult because of the somewhat poor preservation and the lack of numerous specimens. It goes without saying that to recover anything from the Franciscan is a triumph.

With best regards,

Warren

Warren S. Drugg

Hunt Institute for Botanical Documentation

MEMORANDUM GO-144

TO: MR. _____ 3 Dec 1962

FROM: MR. _____

SUBJECT: OUR FILE:

YOUR FILE:

Dear Al

Just a note to let you know that your letter and the slides have both arrived in good shape. I have glanced at the material and at least some of the forms look familiar.

At the moment I have a few odds and ends to finish up and will probably not get around to really looking at the slides until next week. In any case it will be fairly quickly.

Warren Jugg

Hunt Institute for Botanical Documentation

DRUGG

November 30, 1970

Dr. Warren S. Drugg
Chevron Oil Field Research Corp.
P.O. Box 446
La Habra, California 90631

Dear Warren:

Under separate cover I have sent you the slides which I indicated several days ago were on their way. With this letter I am enclosing polaroid pictures of three of the things I am especially interested in, as well as a list of these and the others. You will note that each fossil is indicated by a little paper label with the number written on it. Once you have located the fossils and have your own stage coordinates for them, you may put a little water on the cover slip and carefully soak off the stickers--I don't need them to find the fossil. They are mounted in glycerine jelly, however, so it would be best not to use warm water. The location of the fossils is also indicated in terms of Leitz Ortholux coordinates, and I have given coordinates on each slide for the "x" which I have scratched on the slide, so that you can figure out what the conversion factor is for your microscope. However, I believe that with the help of the paper labels (presuming that they stay stuck on!) you will be able to find the microfossils without difficulty. I do hope you will not judge my prowess as a lab man on the basis of these macerations. I found out toward the beginning of this study that the less maceration used on the samples, the better. Looking forward to getting your comments, I am with thanks and best wishes

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi
Enclosure: pictures

Hunt Institute for Botanical Documentation

Locations of fossils on slides sent to Warren Drugg, 25 Nov., 1970

PRC-2028.2

(Ref. x= 55.5 x 116.4)

1. 31.4 x 122.0 (Polaroid photo provided)
2. 37.8 x 119.4 (Polaroid photo provided)
3. 46.4 x 110.1 (reworked acritarch?)

PRC-2028.4

(Ref. x=63.9 x 118.5)

4. 28.7 x 113.2 (Polaroid photo provided)

PRC-2028.5

(Ref. x= 58.3 x 119.3)

5. 32.0 x 129.1

PRC-2028.b

(Ref. x= 23.2 x 117.5)

6. 28.8 x 127.7

7. 38.5 x 118.7 (dinoflagellate?)
8. 41.9 x 128.5 (once located, the sticker should be soaked off)

PRC-2028.a

(Ref. x= 15.8 x 114.9)

9. 41.8 x 120.8 (acritarch?)

Hunt Institute for Botanical Documentation

= x 2

PRC 2028-2

37.8 x 119.4

57 microns

Several different times of exposure
for the same fossil, to emphasize different
features.



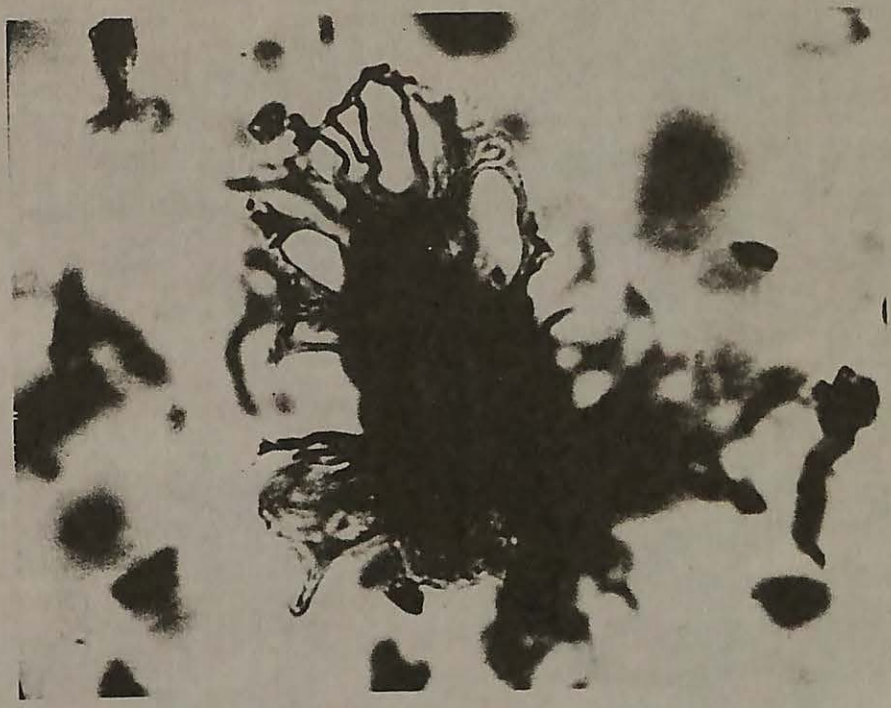
Looks distinctive enough to be identifiable

Hunt Institute for Botanical Documentation

PRC 2028-4

28.8 x 113.2

96 microns



Somewhat resembles Sarjeant's illustration of "Adnatospheridium aenulum" in Erdtman's "Handbook of Palynology", p. 172

Hunt Institute for Botanical Documentation

May 15, 1969

Dr. Warren S. Drugg
Chevron Oil Field Research Company
P.O. Box 446
La Habra Laboratory
La Habra, California 90631

Dear Warren:

Glad to have your endorsement, per 5 May. Really, had I known that palynologists would be in the majority, I would have presented a different program, but I am glad that some of the things were of interest anyway.

Thanks for the info re Varia-1. The ones on the Bahama Bank are clearly recent. The distribution on the Bank in the surface sediment is identical to that for dinoflagellate cysts, whatever that may mean. The data from Hajos is most interesting to have for my records.

Hope we'll see you here for the AASP meeting in the fall. You should find the featured paper by Dave Wall interesting.

Best wishes.

Yours very truly,

Alfred Traverse
Associate Professor of Geology & Biology

AT:kwc

Hunt Institute for Botanical Documentation

CHEVRON OIL FIELD RESEARCH COMPANY
A STANDARD OIL COMPANY OF CALIFORNIA SUBSIDIARY
LA HABRA LABORATORY P.O. BOX 446 LA HABRA CALIFORNIA 90631

May 5, 1969

Dr. Alfred Traverse
Department of Geology and Geophysics
College of Earth & Mineral Sciences
The Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

Just a note to tell you how much I enjoyed your very fine talk and the following discussions in the New Orleans. It was a real pleasure to see you again after a lapse of several years.

As I recall, I mentioned to you that your Varia-1 type resembled Mecsekia Hajós. I have looked it up and Varia-1 is very similar to M. spinosa and M. spinulosa which in turn are similar to each other. The reference is Márta Hajós, 1966, "A Meczeki Miocén diatomaföld rétegek mikroplanktonja" - A Magyar Allami Földtani Intéget Evi Jelentése Az. 1964. (Note, the type was fixed in 1966). I might add that Hajós does not know what the affinities are. I find it in the Miocene-Pliocene.

With best wishes,

Warren

Warren S. Drugg

Hunt Institute for Botanical Documentation

**CHEVRON RESEARCH COMPANY**
A STANDARD OIL COMPANY OF CALIFORNIA SUBSIDIARY
LA HABRA LABORATORY
P. O. BOX 446 LA HABRA CALIFORNIA 90633

Dr. Alfred Traverse
The Pennsylvania State University

Dear Al:

I thank you for your kind comments on the Moreno paper. In general I'm reasonably well pleased with it, but if I could do it again there are several changes I would like to make. It was in press for better than a year which gave me plenty of time to think about old mistakes and to discover new ones. In any case I'm sure that the ages are correct and that the basic information is correct. I think any future publications will be less ambitious. It's really too much to try to do a good job on spores, pollen, dinoflagellates, and acritarchs - all at the same time. I get confused rather easily so you can imagine that I literally wallowed in confusion at times. The company did pay for the reprints! My salary is in a fairly modest range so there wouldn't have been any reprints other than the 25 gratis ones if I had to pay for them myself.

I also thank you for sending me your excellent paper on surface sediment palynology of the Great Bahama Bank. This paper is of particular interest to me since

Hunt Institute for Botanical Documentation

CHEVRON RESEARCH COMPANY

we are doing some palynology on some of the wells drilled in the Bahama area.

With very best wishes

Warren

Drugg

Hunt Institute for Botanical Documentation

May 23, 1967

Dr. Warren S. Drugg
Chevron Research Corporation
3282 Beach Boulevard
La Habra, California 90633

Dear Warren:

Received your handsome reprint. Haven't really had time to read it thoroughly yet, but I can see that it is obviously well illustrated and a worthwhile study. I hope that Chevron paid for the reprints. The ones for my recent paper in Marine Geology practically pauperized me.

Regards to the Loeblichs. Best wishes to you until we meet again.

Yours very truly,

Alfred Traverse
Editor
Catalog of Fossil Spores and Pollen

AT:kaw

Hunt Institute for Botanical Documentation

10 July, 1984

Mr. Nai-zheng Du
Birbal Sahni Institute for Palaeobotany
Lucknow 226007, India

Dear Mr. Du:

Thanks for yours of 17 June, 1984. It is good to know of your work with fossil woods. I appreciate your thinking of me for post-doctoral work, but I must be negative in response, for two reasons: I work almost exclusively in palynology, and I unfortunately have no money available for post-doctoral research in our laboratory. The paleobotanist here is Dr. William Spackman, who has worked with fossil wood, and I am sending your letter on to him, in case he might be interested.

Best wishes to you in your life and your work.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: Dr. W. Spackman

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

4 July, 1997

Dr. Ailbhe M. Duane
British Antarctic Survey
NERCd
High Cross, Madingley Road
Cambridge CB3 0ET
England

Dear Dr. Duane:

Many thanks for sending me a copy of your very interesting and beautifully done paper on the Byers Group. I am currently working on a very difficult sporomorph flora from a suspect terrane in Oregon that is about the same age as your stuff, and I note some old friends, even though NOBODY thinks the terrane came from Antarctica!

Your given name is sure different. I can't even guess as to gender. Is it Gaelic or Urse or some other Celtic name?

You didn't request exchange, but I'm sending a few things anyway, just in case.

Best wishes to you in your work.

Yours very truly,

Alfred Traverse
Professor Emeritus of Palynology

encl.:reprints

Hunt Institute for Botanical Documentation

May 1, 1970

Dr. Jules DuBar
Geoscience Department
Morehead State University
Morehead, Kentucky 40351

Dear Jules:

This is just to let you know how much I enjoyed being in Morehead--after the first fifteen minutes, it was a thoroughly enjoyable experience! I especially appreciated all of your and Susan's hospitality though I thought that it was above and beyond the call of duty. Please extend my thanks to Susan for making me feel so much at home.

I would also like to be remembered to Dr. Straley, and to Messrs. Chaplin and Philley. I especially appreciated John's sacrificing some of his rare spare time to show me a bit of the local geology on Wednesday morning. Although I didn't get away until about noon, I still was home by 8 p.m.

Perhaps you could find time one of these days to write a suitably fulsome letter to my Dean, Dr. Charles L. Hosler, thanking him for making me available as an AGI visitor. Such a letter will help my situation here, obviously!

Best wishes to all of you.

Yours very truly,

Alfred Traverse
Associate Professor of
Geology and Biology

AT/mrw

Hunt Institute for Botanical Documentation

the seminar plus the talk to the stratigraphy class. This is probably the time to mention before I forget it, that I do most of my lecturing with the aid of an overhead projector and hope that you can arrange one. I also have some 2 x 2 and some 3¼ x 4 slides, so the ideal arrangement would be to have all three types of projectors available. Let me know if that poses some difficulty, and I can rearrange things but it would be best if I could have all three projectors for at least the two public lectures. (It would be easiest to do without 3¼ x 4).

I believe I will plan to drive to Morehead, and that would get me in something on the order of 10 p.m. on the 26th. As you may have heard from scuttlebutt, I function as an Episcopal priest on Sundays, and I will depart from Morehead directly after officiating at our Church. I hope the late arrival will not foul things up particularly, but I have discovered from previous experience at this time of year that if I were to plan flying, my arrival time would be even more uncertain. One's probability for departure from our airport does not reach 50% until sometime in May. I wish it were possible to bring my wife, but I guess she will once again have to stay home and tend the nest. Let me know where I will be staying and how to get there, and I can take care of myself on the 26th. I will stay through the 27th and 28th and drive home on the 29th. I am enclosing what I have by way of vita. I suppose you might want to look up what is said about me in American Men of Science. I enclose also a copy of a recent article about the Catalog of Fossil Spores and Pollen, of which I am Editor, parts of which you might also like to use. Beyond that, I leave it up to you. Some people seem to be quite fascinated with the fact that I am theologically trained and an ordained Episcopal clergyman, as well as a paleontologist, especially when they find out that I teach a course in evolution, but it seems to me that that is largely irrelevant.

Looking forward very much to meeting you. I am

Yours very truly,

Alfred Traverse
Associate Professor of
Geology and Biology

AT/mrw
Enclosures

Hunt Institute for Botanical Documentation

9 September, 1983

Mr. Russell F. Dubiel
Office of Energy and Marine Geology
Branch of Energy Minerals
U.S. Geological Survey
Box 25046, M.S. 916
Denver, CO 80225

Dear Russ:

Santa Claus in the form of the nice box of samples just arrived. I will have my graduate assistant, Ron Litwin, get hot on maceration of the samples after he completes, defends and submits for publication his M.Sc. thesis on several ferns with in situ spores from the Chinle. He has also worked up the general palynology of enclosing rock, which would perhaps be of interest oo you, as it applies to the general stratigraphic position. When the thesis is finished (Oct.?), you could borrow it through your library. If I remember, I'd be glad to send a xerox, but it would be like me to forget. Anyway, it'll be better for you to use the original.

We do plan to get into the upper Chinle problem with a vengeance as soon as possible (we already have some samples from Ash), so your help has been a great surprise and boon to us.

Best wishes and thanks very much!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: Litwin, Ash

Hunt Institute for Botanical Documentation

United States Department of the Interior
GEOLOGICAL SURVEY
BOX 25046 M.S. 916
DENVER FEDERAL CENTER
DENVER, COLORADO 80225

IN REPLY
REFER TO:

Office of Energy and Marine Geology
Branch of Energy Minerals

August 26, 1983

Alfred Traverse
Department of Geosciences
The Pennsylvania State University
University Park, Pennsylvania 16802

Dear Dr. Traverse:

Earlier this summer while doing field work in Utah and Arizona, I spent several days collecting plant fossils and discussing sedimentologic models for the Chinle Fm. with Sid Ash at the Petrified Forest National Park. I had told Sid of another black mudstone I had found and sampled from the upper part of the Chinle near the west end of White Canyon, Utah, near Lake Powell. I have a general understanding of the work you and Sid are doing on the TR/J boundary problem and, in my study of the lower Chinle, keep my eye out for black mudstones in the upper Chinle. Sid mentioned that he would be leaving for Africa early this Fall, and so as not to lose the samples in the move or the general rush of things, he suggested I write and send you the samples directly. Thus the reason for this letter and the wordy introduction.

Enclosed are samples from the black, organic-rich mudstone, collected each foot, approximately 2 ft. into the outcrop to ensure fresh material, from the 4 Aces Mine section. I've enclosed xeroxes of my field notes and a xerox of the 4 Aces Mine-East section, approximately one quarter mile to the east. The sedimentologic detail was measured at 4AM-E; the 4AM section was to be used primarily to record lateral changes in unit thickness in a panel of the outcrop, thus the lack of lithologic detail in the middle of the section. While observing the upper portion of the Chinle, we noticed the black mudstone, measured up to it, and took samples.

My own study of the Chinle is concerned with the lower depositional package of Shinarump, Monitor Butte, and Moss Back Members, so I am not normally recording detailed sections of the upper Chinle. Additionally, I am using the top of the Moss Back and it's lateral equivalents as a datum for hanging sections so I can not offer a measurement of the upper Chinle to the Wingate sandstone; if needed, this may be available in other USGS publications for the White Canyon area. I've added additional explanatory notes on the xeroxes to help you decipher some of my shorthand. When I sent Sid the samples for the Hey Joe Mine I did not realize he was forwarding them to you, and left it to Sid's familiarity with my study to decipher some of my field notes. Since you are once more removed, if you have any questions concerning any of the samples, method of collection, stratigraphic position, or whatever, please don't hesitate to drop me a line or give me a call and I'll provide any additional explanations that you may need.

Hunt Institute for Botanical Documentation

I've also enclosed a copy of my most recent Survey report to give you a general idea of what I've been doing with the Chinle. I've begun a PhD at the University of Colorado, Boulder this Fall and will be examining the lower Chinle system in a larger area than covered in the report. I am very interested in your palynomorph work on the Chinle, as a time stratigraphic framework would be very helpful in our sedimentologic studies, particularly if it can be extended to the lower Chinle. If I can be of any assistance in this regard, please let me know. Lastly, Sid sent me a copy of the P. smithii paper that you did together; do you have any other recent references that I might look at to keep abreast of your Chinle work?

Thank you very much for your time; I hope this material is useful to you.

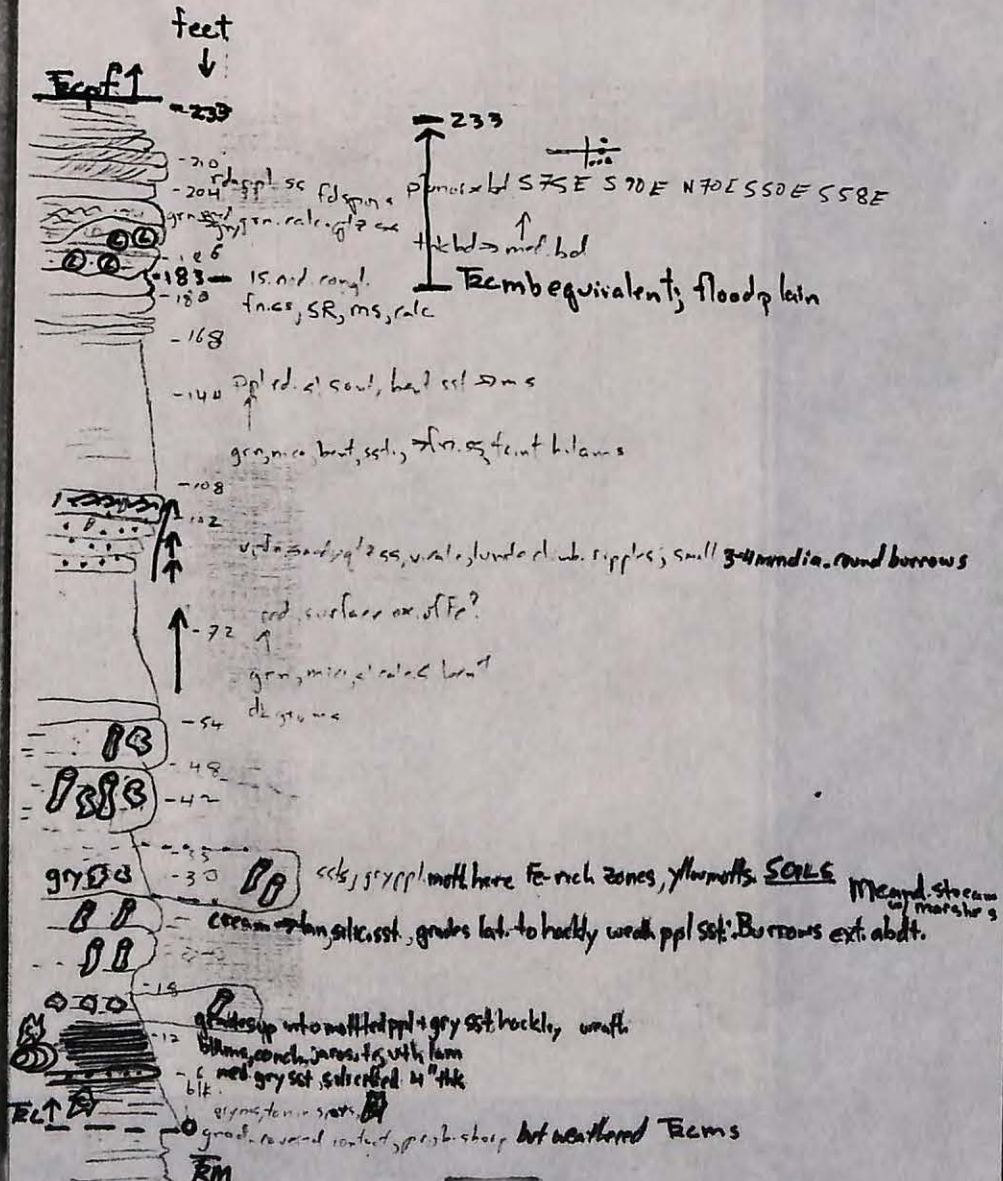
Sincerely,

Russ

Russell F. Dubiel
Geologist

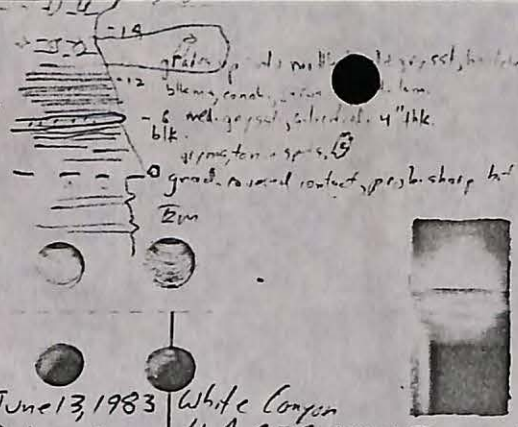
Hunt Institute for Botanical Documentation

Russ Dubiel
USGS; Book # 4 1983




June 13, 1983 | White Canyon
Paul Mike, asst. | 4ACES MINE - E. Burrow Locality
Dressed 4AM-F

Hunt Institute for Botanical Documentation

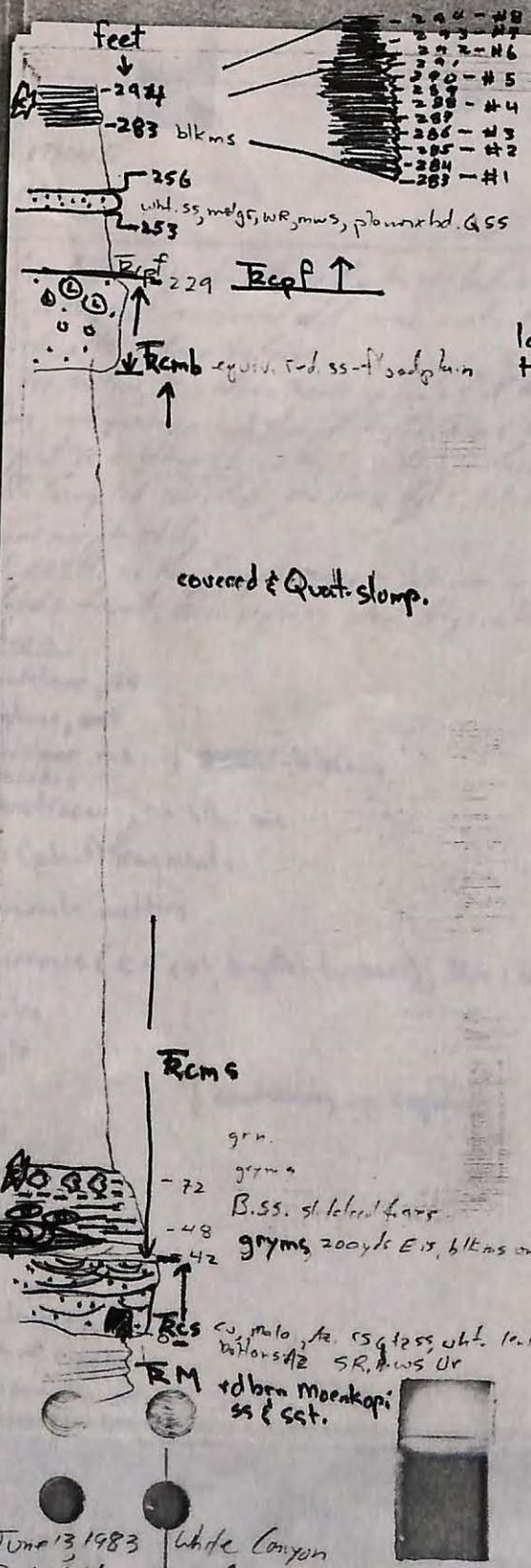


June 13, 1983 White Canyon
Paul Milde, assgt. 4 ACES MINE – E. by Borrow Locality
processed. 4AM-E

– mile post 56 on Hwy 95 = 12mi NW of Fry Canyon Store
– regional dip on Rm (strike N33E) dips 1°S 57E



Hunt Institute for Botanical Documentation




Russ Dubiel
USGS; Book #4 1983

laterally equivalent to Moss Back Ss
thasic floodplain

covered & Quat. slump.

June 13, 1983
Paul M. Ug asst.

White Canyon
4 ACES MINE
measured 4AM

Note: 4 Aces Mine located on fig. 2 pg. 4, of Open File report.

Hunt Institute for Botanical Documentation



-72 grn
-49 grn-a
-42 B.SS. sl lolenl fms
grn; 200yds E is blk ms on Tem

Rcs Cu, malo, Az. CS; t2SS, wht. lenticular, cut + fl
0 buttons A2 SR. flows Ur



June 13, 1983 White Canyon
Paul Mills, asst. 4 ACES MINE
measd. 4AM

- mass. sect. at Rcs Ur channel, w/ Cu Azur. + buttons 1cm dia, & malachite in SS; sect is covered alot; mass. mostly for comparison to other 4 Aces sections
- * at top of Rcl section, i.e., reddish Rcmb equivalent of lt. red/grey + sandy rd ms, ie lyr = 300' x 100 yds. dump of Rcpf & Rco & Rwl. Cut
- * on copper point is a blk. ms! in Rcpf; outcrops from 283 ft. to 292 ft. Sampled every foot, #s 1-8 for Sid Asb & Eric E/J polymorph study
- * blk ms at 283 ft. in Rcpf, grn → gry → blk. lam. w/ ; cross-; no microfossils found; in gry ms; plant frags in blk ms

Explanation

- sandstone, ss
- siltstone, sst
- mudstone ms ; blk ms
- ostracodes
- conchostracans, in blk. ms
- organic (plant) fragments
- purple-white mottles
- giant burrows (≈ 4" x 4", lungfish burrows); Bss = Burrowed ss
- limy nodules

ppl purple
rd red
blk black
gn green
gry gray
adit

↑ coarsening up sequence

- climb. ripples
- sym. ripples
- small 2-4 mm dia. burrows



Hunt Institute for Botanical Documentation

THE CHURCH UNION

60 Rockledge Drive
Pelham Manor, N.Y. 10803

914 PE8-2973

May 3, 1971

The Rev. Alfred Traverse
Deike Building
The Pennsylvania State University
University Park, Pennsylvania 16802

Dear Father:

Thank you for your letter of April 21st. I have tried to follow your suggestion with this month's issue of the News and I hope it will meet the problem. I have labeled it "Ascension-tide". I thought that ought to be far enough into the future to allow even for the failing service of the postal department. The mischief is, of course, that the Postmaster at Guilford, Connecticut (where the newspaper is mailed) objects when I do not put the month on the first page masthead and I cannot get the Postal officials in Washington to give me a ruling about this seasonal designation to offset the local Postmaster's objections. He does insist, as of this moment, on having the month listed on all the inside pages and here, too, I would like to follow the same practice as on the front sheet but I must await some determination from Washington.

I have all the best intentions in the world about getting the News out on time but if it is not one"roadblock" it is another. I am, of course, under the necessity of writing the copy, proofreading and pasting in between fieldwork trips and administrative duties, and these schedules sometimes get out of hand. This month I thought sure that I would catch up but the printer fooled me. His pressroom manager took his holiday and copy submitted on the 13th of April did not reach us in galley form until almost the end of the month. I do not seem to be able to win! I do hope the day will come when there can be a separate staff for the News.

Again thanking you for your letter and with all good wishes, I am

Faithfully yours,

Albert J. duBois

(The Reverend Canon) Albert J. duBois
Executive Director

Dictated by Canon duBois
Signed in his absence by

Father Francis W. Read

FWR

Hunt Institute for Botanical Documentation

29 December, 1982

Alfred Traverse, 435 Deike

Ms. Lynn DuBois, Assoc. Manager Graduate Housing, 180-A Atherton Hall

Just a note to say thanks for your very helpful extension of aid to me and to my new Turkish graduate student, Volkan Ediger. It was an awkward time for a new arrival, and you eased it considerably!

Hunt Institute for Botanical Documentation

24 October, 1986

Patricia Meaney, Registrar
University of Queensland
St.Lucia, Queensland 4067
Australia

Dear Ms. Meaney:

Yours of 7 October re the Dudgeon thesis was a shock. We sent the thesis back immediately (ca. January, 1986) after I finished reviewing it, and are hoping something bad hasn't occurred. Unfortunately I find no evidence that we insured (= "registered") the package, which would have made it traceable. That doesn't mean we didn't insure it, but it was a long time ago! At that time the Registrar with whom we corresponded was S. A. Rayner.

I am very sorry for problems this has undoubtedly caused you all.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Dudgeon

TH270 RDW:DLP

**University of Queensland**
ST LUCIA, QUEENSLAND, 4067

7 October 1986

Professor Alfred Traverse
Department of Geosciences
Pennsylvania State University
435 Deicke Building
University Park, Pennsylvania 16802
UNITED STATES OF AMERICA

Dear Professor Traverse,

I refer to the thesis, detailed below, which you recently examined for this University.

You will be interested to know that the examiners' reports have now been received and it has been decided to award the degree.

If you have not already done so, would you please return your copy of the thesis (marked ATTENTION: THESES SECTION), preferably by registered surface mail, as it is now required for distribution within this University.

The University appreciates your assistance in this examination.

Yours sincerely,

Patricia Meaney
for
Registrar

Candidate: Murray James DUDGEON

Degree: Doctor of Philosophy

Title of Thesis: PALYNOLOGY OF THE YAAMBA BASIN (EOCENE), CENTRAL QUEENSLAND

Hunt Institute for Botanical Documentation

University of Queensland
ST LUCIA, QUEENSLAND, 4067

TH230 RDW:KMP

20 January 1986

Professor Alfred Traverse
Department of Geosciences
Pennsylvania State University
435 Deike Building
University Park
Pennsylvania 16802
UNITED STATES OF AMERICA

Dear Professor Traverse,

Thank you for your report on the thesis submitted by the candidate referred to below. You will be advised at a later date of the final decision concerning the award of the degree.

If you have not already done so, would you please return the thesis to me, preferably by registered surface mail, at your earliest convenience.

I have arranged for the payment of the honorarium due to you.

Yours faithfully,

(S.A. Rayner)
Registrar

Candidate: Murray James DUDGEON

Degree: Doctor of Philosophy

Title of thesis: PALYNOLOGY OF THE YAAMBA BASIN (EOCENE), CENTRAL QUEENSLAND

Hunt Institute for Botanical Documentation

TELEPHONE: (07) 377 1111
TELEX—UNIVQLD AA40315

File Dudgeon

# University of Queensland

ST. LUCIA, QUEENSLAND, AUSTRALIA, 4067

ADDRESS REPLY TO REGISTRAR
BUT IN REPLY PLEASE QUOTE:
**TH223** RDW:DLP

FOR ENQUIRIES REGARDING
THIS LETTER TELEPHONE:
**377 2638**

13 November 1985

Professor Alfred Traverse
Department of Geosciences
Pennsylvania State University
435 Deicke Building
University Park
Pennsylvania 16802
UNITED STATES OF AMERICA

Dear Professor Traverse,

Thank you for agreeing to examine the thesis listed below.

A copy has been forwarded to you today. If you have not received it within three weeks of the date of this letter please let me know immediately.

An Examiner's Report Form is enclosed for your use. If convenient, I should like to receive your report, in duplicate if possible, by 13 January 1985.

Would you kindly return the thesis to me, preferably by registered surface mail, after you have completed your examination.

Yours faithfully,

(S. A. Rayner)
Registrar.

**Candidate:** Murray James DUDGEON

**Degree:** Doctor of Philosophy

**Title of thesis:** PALYNOLOGY OF THE YAAMBA BASIN (EOCENE), CENTRAL QUEENSLAND

Hunt Institute for Botanical Documentation

TH22A

THE UNIVERSITY OF QUEENSLAND
St. Lucia. Q. 4067.

# EXAMINER'S REPORT – HIGHER DEGREE THESIS

**(To be completed after reference to information provided for the guidance of examiners)**

NAME AND ADDRESS OF EXAMINER: Professor Alfred Traverse
Department of Geosciences, Pennsylvania State University
435 Deicke Building, University Park Pennsylvania 16802, USA

NAME OF CANDIDATE: Murray James DUDGEON DEGREE: PhD

TITLE OF THESIS: PALYNOLOGY OF THE YAAMBA BASIN (EOCENE), CENTRAL QUEENSLAND

| | | Yes | No |
|---|---|---|---|
| 1. | I am willing to have my identity revealed to the supervisor.* | ☒ | ☐ |
| 2. | I am willing to have my identity revealed to the candidate.* | ☒ | ☐ |
| 3. | (Australian Examiners only) I would be available to participate in an oral examination if it were considered necessary. | ☐ | ☐ |

4. *SUMMARY RECOMMENDATION ON THESIS*

After examination of the above thesis (and the supporting papers, if any) I recommend:

Tick *one* only

(i) that the candidate be awarded the degree; ☐

(ii) that provided the listed minor textual errors (see page 1 of this report) are corrected to the satisfaction of the internal examiner and the head of department, the candidate be awarded the degree; ☐

(iii) that provided the passages referred to (see page of this report) are revised or the candidate provides a defence of his position acceptable to the head of department and the internal examiner, the candidate be awarded the degree; ☒

(iv) that, although the thesis does not meet the standard expected for the degree, the candidate be allowed to revise and resubmit it; ☐

(v) that the thesis be rejected and the degree NOT awarded. ☐

The grounds for my recommendation are set out on the following pages of this Report.

*In making a detailed report on the thesis, comments and suggestions should be full enough to enable the University to gauge the quality of the thesis. Comments on the following would be appreciated, particularly in relation to PhD theses:*

- *the extent to which the candidate has demonstrated*
  - *originality*
  - *critical insight*
  - *capacity to carry out independent research*
- *the extent of the contribution to knowledge made by the thesis, and in particular its contribution to the understanding of the subject with which it deals.*
- *the suitability of the thesis for publication.*

*Guidance for any revision or textual correction referred to in the examiner's summary recommendation should be included.*

.....
Signature of Examiner

6/I/86
Date

*See paragraph 4 *Notes for Examiners (Examiners' reports)*

Hunt Institute for Botanical Documentation

**RECOMMENDATION.** (Please type in recommendation from page 1.)

After examination of the above thesis I recommend that provided the passages referred to (see page 1 of this report) are revised or the candidate provides a defence of his position acceptable to the head of the department and the internal examiner, the candidate be awarded the degree.

**DETAILED REPORT.**

See attached sheets.

Please attach additional pages of your report securely.

Hunt Institute for Botanical Documentation

Dudgeon thesis: General Comments (Alfred Traverse)

Mr. Dudgeon's thesis would, with a few alterations, be fully acceptable to me and to our university in partial fulfillment of the requirements for a Ph.D., were he a student here. He has adequately outlined the problem. His abstract summarizes the results sufficiently, with a few exceptions (see comments). The descriptions and illustrations are excellent. The citation of the literature is quite thorough. The geological analysis is good (and very interesting), and I found his biological analyses to be stimulating and imaginative. I append my detailed comments. Most of them are intended to help the author if he publishes this work. I feel strongly that some sort of addendum to the thesis is necessary in order to stress that the author does not intend to publish the nomenclatural changes proposed in the thesis itself. None of my other proposed revisions would be conditions for accepting the thesis, if making them poses serious problems (I note that the review copy is beautifully and permanently bound!).

Dudgeon thesis: Detailed Comments (Alfred Traverse)

Abstract, para. 2, l. 7: should give technical name for sedges at least in parentheses.

A disclaimer is needed here, and preferably also in the systematic section, explaining that valid publication of the new taxa is not intended in the thesis. This is to prevent accidental valid publication, as well as interpretation by others that this has occurred. Also, I believe it would be very helpful to list the new and transferred taxa in the abstract--at the very least the new genera should be listed.

General comment: mistakes in punctuation, etc., are not numerous and have not been noted, as I take it this is not my function. Examples: p. 48, next to last line, "...that northern...."; p. 58, para. 2, l. 4 "depauperate"; p. 81, l. 12 "Caledonia"; p. 82, "immaculate" preservation--poor usage; p. 85, next to last line "...Proteaceae are...."; p. 98, l. 10 "lacustrine"; p. 100, l. 3 "...alga,...."; p. 113, para. 2, l. 7 Code.

p. 6 i: I am unsure why a pre-treatment with HCl was not used--no carbonates present at all? (carbonate is mentioned on p. 30 as present at some levels).
ii: The use of HF, con. $HNO_3$, acetolysis, KOH, and 50% $HNO_3$ probably destroyed some delicate palynomorphs--in fact, this combination of treatments is notorious for such destruction.

pp. 9-10: Growth of actinomycetes is not a problem if the glycerin jelly is made with phenol according to the usual

Hunt Institute for Botanical Documentation

recipe.

p. 11, para. 3, l. 5: Light microscopy has an even more important advantage over SEM--the latter analyzes almost exclusively superficial features.

p. 15, Table 1: Caption should relate this table to r-valus explained on p. 17, which are stated to come from this table. But the caption doesn't mention r-values.

p. 17, last line: I am sorry that "hopefully" is misused--I had thought this misuse was a North American problem! (Our college editor, recently retired, had a sign over the door, "Abandon 'hopefully' all ye who enter here"!) Also, this statement is so vague as to be out of place--more discussion of the matter would have been helpful to the reader.

p. 32, para. 1, l. 10: ...comb. nov. See earlier comment that a disclaimer should be provided to forestall the thesis itself being regarded as valid publication.

p. 33, Fig. 8: It seems unfortunate that the full generic names for the characteristic taxa of zones is not given, though I realize this is common practice.

p. 34, next to last line: Polycolpites reticulatus: a good general rule is to give the author citation for a specific name unless it has already been given on the same page.

p. 41 (and many other places) re: Stereisporites levis and Haloragacidites harrisii: see comment for p. 34.

p. 56: When "Eremean" is first introduced, it should be explained, as it is on p. 62, last para.

p. 70: I do not fully agree with the argument in para. 1, as the ecological interpretation of Cenozoic pollen can also be controlled by known requirements of related living plants, as the author also acknowledges (see Table 3, pp. 77 ff., etc.).

p. 79, last line: The author apparently means, "...only two can be identified with modern species...."

p. 80, para. 2, l. 1: What is the evidence?

p. 81-82: If Casuarina-like pollen is "extremely rare" (p. 82, l. 19) I doubt that it's worth so much discussion!

p. 83 # 1-2: Another possible explanation more likely than either presented is: long-distance wind-transport.

p. 92, para. 2, l. 19: The reader finds it jarring to have learned that Hekel is absolutely unreliable (p. 31) and then to find Hekel cited as an authority.

Hunt Institute for Botanical Documentation

p. 97, l. 4: The word "specifically" should be deleted.

p. 100 (and elsewhere), l. 1: Should say "microspores"

p. 108, para. 2, l. 2: Same caveat as previously that "sedges" should carry a technical name in parentheses, or be avoided.

p. 110 ff.: Nomenclature: This section, on a vexing matter, is the only section that gives me real problems. I hardly know where to begin.

p. 110:

1. It doesn't matter much how names for taxa are made up, and their validity and applicability is not affected by this consideration. "Ericipites" is as good a name as, say, Tetradopollenites of Pyramidosporites, but no better. Dudgeon should delete the last sentence of para. 2. Also, it should be noted that Potonié was earlier in the field of naming Cenozoic spores/pollen.

2. para. 3: The status of Erdtman's and Cookson's sporotype-sporomorph names is controversial. I am of the view that many of them were validly published.

3. Types are not required by ICBN until 1959, so Krutzsch's designation of a type for Cupanieidites is, of itself, irrelevant.

p. 112:

para. 1: Pollenites (should be capitalized) was not for Potonié a "term" but a genus--indeed, if it were a term it could not be a generic name, according to ICBN (Art. 20) and that has important implications indeed--many species depend for their date of publication on whether Pollenites was a genus or not; a species cannot be validly published except in combination with a validly published generic name.

para. 2: Thomson & Pflug's work was epochal all right, but not because it was a "system".

para. 3: The author needs to address the question of use of extant plant names for the Neogene, especially late Neogene--that's a "different ball game"--and Martin 1973a was dealing with such material.

para. 4: The definitive refutation of Hughes' proposals is Jansonius (1981: Taxon 30:438-448), though Traverse (1975) perhaps should also be quoted.

p. 113:

para. 1: One could use extant generic names plus arbitrary sub-divisions, "Quercus sp. 1", etc. I believe this is what Boulter really intended for late Neogene.

para. 2: I object to use of any "system" as Dudgeon seems to understand it. Indeed he does not use a system--he uses generic and specific names as seem appropriate to each case, just as in, say, Gray's Manual of Botany.

para. 2, l. 2: Should follow the ICBN, Sydney, (1983)! Despite Dudgeon's statement here, this work introduces a flock of new names: 3 new genera, 28 new species, 11 nov. comb.! That's o.k., so why try to sweep it under the rug?

Hunt Institute for Botanical Documentation

pp. 113-116: I am not convinced that use of the turmal system for Cenozoic material is useful. It is more complex than necessary. Why not just have headings: trilete spores, monolete spores, bisaccate pollen, etc.? Furthermore, the author states in Table 3 that he can refer the palynomorphs to extant taxa in a very large proportion of cases with apparent certainty, and in most other cases with a ? Why isn't Rubiaceae as useful a category for Rhoipites decoratus as "PTYCHOTRIPORINES Naumova 1939"? (p. 166) Further, I guess this is as good a place as any to object to (very common) use of turmal divisions as if they were formal nomenclature, with author reference and all. Perhaps the author reference could be put in brackets to underline an author's understanding that "PTYCHOTRIPORINES" is not a formal name. Dudgeon presents "BIRETISPORITES" Delcourt & Sprumont" as of equivalent status with "LAEVIGATI...."

p. 116 (under Biretisporites). Dudgeon here does something that many professional palynologists have done: uses "microspores" incorrectly. Does he know Biretisporites to be heterosporous? It is almost certainly an isospore, isn't it?
He could say (best) "miospore", or "small spore", or "isospore or microspore", but he should not say microspore. I will not enlarge on this--Dudgeon does it throughout, and there are many precedents--but the practice is nevertheless wrong! For example, on p. 121, Dudgeon describes Stereisporites levis as "microspores", and they really are!

p. 121: It would be wise (tho' not required) to list H. levis as "basionym". Also, is there no fig. no. for the Krutzsch plate? If so, it must be listed for valid transfer.

p. 150: Equisetosporites notensis nov. comb. requires citation of the holotype fig. no. in Cookson. Also here as elsewhere it would be helpful to label this citation as "basionym".

N.B. This same objection holds for some other comb. nov., e.g. p. 152. They should all be checked. This is especially important for comb. nov. which are type species of new genera (p. 188: V. valvatus), as the type specimen of that species is now the type of the genus too (decision at Sydney Congress).

p. 227: ...spinyspora: only the fig. number of the type specimen should be given (perhaps there are three figs. of it?) for valid transfer.

p. 234: Elsik and Jansonius have both shown that Paleogene fungal remains are of great biostratigraphic value!

Hunt Institute for Botanical Documentation

Examiner's name and address:

Professor A. Traverse
Department of Geosciences
Pennsylvania State University
435 Deicke Building
University Park
Pennsylvania 16802
UNITED STATES OF AMERICA

**TH22D**

The Registrar,
University of Queensland,
St. Lucia. Q. 4067.

*Examination of thesis submitted for degree of* Doctor of Philosophy

*by* Murray James DUDGEON

Dear Sir,

*EITHER*

I accept your invitation to act as examiner of the thesis and I anticipate being able to submit my report within one month of receipt of the thesis.

Yours faithfully,

Alfred Traverse 23/07/85
Signature of Examiner Date

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

---

*OR*

I regret that I am not able to accept your invitation to act as examiner of the thesis.

Yours faithfully,

..... ...../...../.....
Signature of Examiner Date

Hunt Institute for Botanical Documentation

TELEPHONE: (07) 377 1111
TELEX—UNIVQLD AA40315

ADDRESS REPLY TO REGISTRAR
BUT IN REPLY PLEASE QUOTE:
TH221 MWW:JAB

# University of Queensland

ST. LUCIA, QUEENSLAND, AUSTRALIA, 4067

FOR ENQUIRIES REGARDING
THIS LETTER TELEPHONE:
377 2638

16 October 1985

Professor A. Traverse
Department of Geosciences
Pennsylvania State University
435 Deicke Building
University Park
Pennsylvania 16802
UNITED STATES OF AMERICA

Dear Professor Traverse,

## INVITATION TO ACT AS EXTERNAL EXAMINER OF THESIS

The University of Queensland invites you to act as an external examiner of the thesis described below.

As the University wishes to ensure that the examination of theses is completed as expeditiously as possible, it would be appreciated if you could submit your report, in duplicate if possible, within two months of receipt of the thesis; if you are uncertain that you will be able to do so, could you please let me know.

Information concerning examiners' reports, with particular reference to the form of the final recommendation, is set out on the reverse of this letter. I have also enclosed a copy of the rules relating to the degree or, in the case of Ph.D. theses, an information sheet for your guidance.

The honorarium payable to external examiners in this connection is $A150.00.

I should be grateful if you would complete and return the enclosed memorandum indicating whether or not you are prepared to examine the thesis.

If you are prepared to act as an examiner I shall send you a copy of the thesis and an Examiner's Report Form, both of which should be returned to me when you have completed your examination.

Yours sincerely,

Envelope to & return this

(S.A. Rayner)
Registrar

**Candidate:** Murray James DUDGEON

**Degree:** Doctor of Philosophy

**Title of Thesis:** PALYNOLOGY OF THE YAAMBA BASIN (EOCENE), CENTRAL QUEENSLAND

Hunt Institute for Botanical Documentation

UNIVERSITY OF QUEENSLAND

# ADDITIONAL INFORMATION FOR EXAMINERS OF PH.D. THESES

The PhD degree at the University of Queensland is a research degree only. A candidate is required to pursue under prescribed supervision an approved programme of research for at least 2 years full-time or 3 years part-time (if already awarded a Masters degree), 3 years full-time or 4 years part-time (if holding a bachelor degree with Honours I or IIA). Except under special circumstances, the candidate is required to pursue study without interruption until the thesis is completed. A full-time candidate is not permitted to undertake other study or employment without the permission of the Postgraduate Studies Committee.

## Standard of Examination

The candidate's thesis should provide a substantial contribution to learning and should reveal a capacity to relate the research topic to the broader framework of knowledge in the disciplinary area in which it falls. Examiners' comments on the candidate's originality and critical insight would be particularly valuable. The thesis must be clearly, accurately and cogently written and suitably illustrated and documented.

## Appointment of Examiners

Once the format of the thesis has been approved, the Registrar submits the thesis to at least three examiners appointed by the Postgraduate Studies Committee after consultation with the Head of the Department. One of these is normally a member of staff of the University of Queensland, and at least two must be external to this University. One of the external examiners is usually from overseas. The supervisor of the thesis should not normally also be the internal examiner. On the submission of the thesis the supervisor may prepare a report on the candidate's work and on the thesis; such a report would be made available to the Head of the Department and the Chairman of the Postgraduate Studies Committee.

The candidate has the right to request that the thesis be submitted to the examiners, even if this is against the advice of the supervisor.

## Award of Master's Degree

If the Postgraduate Studies Committee is of the opinion that the candidate should not be awarded the PhD degree but that the work is of sufficient merit to justify the award of a master's degree of this University, it may, at the request of the Head of Department concerned, make a recommendation to the appropriate Faculty Board that consideration be given to the award of such a degree.

## Corrections to Thesis

Corrections inserted by the candidate after the examination should be made neatly and in such a way that they will not interfere with the legibility of the volume. Where corrections become obtrusive, retyping should be undertaken.

## Advice to candidates regarding requirements for PhD Thesis

Four copies of the thesis are to be submitted to the Registrar. for examination. Three permanently bound copies will be retained by the University after the examination and final acceptance of the thesis.

The thesis must be preceded by a short abstract not normally exceeding 700 words; this should normally be followed by the table of contents, a list of all illustrations and diagrams, a signed statement on sources, the main text, the appendices and the bibliography.

In addition to the thesis the candidate may submit any work previously published, whether or not it relates to the subject of the thesis, which should be inserted in a pocket at the back of the thesis.

Since a thesis must have continuity in format and in content, relevant published materials should normally be submitted as supporting papers with the thesis. However, such materials as a whole or in part may also be incorporated in typescript in the thesis, where they form an integral part of the thesis.

The thesis should not be unnecessarily long. There is no prescribed length for a thesis as length will vary according to the topic and the discipline. However, as a guide, the length should not greatly exceed 400 pages, including maps, and diagrams. The supervisor should be consulted on the appropriate length of the thesis.

The thesis in a scientific field should provide sufficient information to enable an external examiner to appreciate that the investigation has been concluded with a high level of technical skill, that the candidate is familiar with and has employed (if relevant) the most suitable statistical techniques, and that the procedures are clearly of doctoral standard.

The thesis should make clear which work has actually been performed by the candidate and in which cases further analysis has been made on the results obtained by another.

There should be an appropriate balance between the different parts of the thesis. In particular, the original contribution to knowledge should be clearly distinguishable from the introductory material and the survey of the literature.



Hunt Institute for Botanical Documentation

1 February, 1979

Ms. Debra Dufek
Apartment 15, 507 West Pierce
Macomb, IL 61455

Dear Ms. Dufek:

Thank you very much for your letter of 18 January. It's interesting to hear that you might want to do palynology at Penn State. I note that your master's degree will be in biology, and it makes me wonder how much geology.you have had. That I cannot evaluate from your brief letter. I am a professor on both the biology and geology departments here, but it is true that my office is in the geosciences part of the university, and it is somewhat awkward for biology graduate students to work with me--they usually are offiçed in the biology department, which makes it a little less convenient than when they are on the scene here near the palynology labs and my office. However, it would be possible to do palynology here as a biology graduate student. If you were to come here in geology, on the other hand, you would have to satisfy the basic requirements in geology. I guess what I am saying is that I need some further information from you to assess what the story would be.

If you did decide to apply here, whether in geology or biology you should write for the usual application materials to The Graduate School, Pennsylvania State University, etc., requesting such forms and other information.

With reference to my areas of research, I should tell you that I currently have underway things in the Devonian, the Silurian, the Triassic, and the Tertiary. I have always been interested in a wide range of things, and my students have worked on things as stratigraphically remote as Ordovician and Recent. Subjectmmatter-wise, I am also interested in sedimentary processes as related to palynology, and in paleoecology and paleoclimatology--for example in connection with my work with the sediments of the Black Sea (I was onboard scientist with the Glomar Challenger a few years ago in that area).

No question about it, the best thing to do in order to assess whether you would like it here or not and whether our program would be satisfactory for you or not would be for you

Hunt Institute for Botanical Documentation

to visit Penn State. In the meantime, I hope that this information has been what you wanted. Best wishes to you. Regards to Dr. Stidd.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: Graduate Admissions Committee

Hunt Institute for Botanical Documentation

Jan. 18, 1979

Dr. A. Traverse
Dept. of Geoscience
Pennsylvania State University
University Park, Pennsylvania
16802

Dear Sir:

I am in the process of applying to the Dept. of Geoscience at Penn State. I am hoping to do graduate work in palynology. I am in the process of finishing my Masters in Biology at Western Illinois University. My thesis research involves a coal ball study in the Berryville coal under Dr. Benton M. Stidd.

I am interested in obtaining information about the area of research you are presently conducting and have available for graduate students. and the possilbility of working under you beginning the fall of 1979, if accepted to graduate school and to your department.

Thank you.

Sincerely,

Debra Dufek
Apt 15
507 W. Pierce
Macomb, Il.
61455

Hunt Institute for Botanical Documentation

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

7 April, 1989

Dr. Susan Duffield
Amoco Production Company
P.O. Box 50879
New Orleans, LA 70150

Dear Susan:

How nice of you to write about the book! There <u>are</u> mistakes in it, but for teaching a course in the subject, I believe it has virtues. (You wouldn't think so from Jansonius' review!)

I loved the postcard. The red-headed is the only woodpecker we are supposed to have which never visits our feeders in winter (even two pileateds come!). It is said that our <u>mobs</u> of starlings explain this--they take all the red-headeds' nesting cavities.

All the best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES
PALYNOLOGICAL LABORATORIES
435 Deike Building
University Park, PA 16802
(814)863-3419 or (814)865-6711

20 October, 1988

Dr. Susan L. Duffield
Amoco Production Co.
P.O. Box 50879
New Orleans, LA 70150

Dear Sue:

Fan letter--that was a <u>super</u> review of S. J. Gould's book, though you were perhaps too kind to Gould, who always has an ax to grind.

Best.

Yours very truly,

Alfred Traverse

AT/et

Hunt Institute for Botanical Documentation

6 September, 1983

Dr. Susan L. Duffield
Amoco Production Co.
P.O. Box 50879
New Orleans, LA 70150

Dear Sue (if I may be so presumptuous?):

It's outrageous that it has taken 6 weeks to respond to your nice letter dated 18 July. It got here about 23 July. Almost immediately began a series of family things, including a son's wedding, etc., than the beginning (17 August) of Fall Semester, etc., etc.

Thanks for the compliment about my review. I believe your present statement is much better and explains your position fully. Those reviews are usually a completely thankless task.

My student, Norma Johnson, and I are very grateful for the chunk of Jupiter Fm. shale, which we'll process asap. Interesting to hear of J.G.'s systematic study, but I'll believe it when I see it. It's not her style to do systematic studies.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

P.S. Enclosed are xeroxes of our most recent blurbs about the "spores", etc.

Hunt Institute for Botanical Documentation

**Amoco Production Company (USA)**
New Orleans Region
Amoco Building
Post Office Box 50879
New Orleans, Louisiana 70150

18 July 1983

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

Dear Dr. Traverse:

Thank you for your review of the manuscript on the spore-like microfossils from Anticosti. I appreciate your suggestions, most of which have been incorporated into the revised manuscript.

I did not add a systematic paleontology section to the manuscript; I did, however, add a paragraph to explain why I have not placed Tetrahedraletes medinensis Strother and Traverse 1979 in synonymy with Nodospora burnhamensis Strother and Traverse 1979 (please see attached).

I have enclosed with this letter a sample from the shale member of the Jupiter Formation for your reference collection. I understand that Dr. Gray is making a more complete study than mine of the spore-like microfossils and will include material from Anticosti in her study. !

Sincerely,

Sue Duffield

Susan L. Duffield

Hunt Institute for Botanical Documentation

described by Strother and Traverse (1979) for Nodospora. However, in other orientations (Figures 3B, E, F, I) the same spore type appears as a tetrahedral tetrad and is very similar to the specimens illustrated by Strother and Traverse (1979) as Tetrahedraletes medinensis Strother and Traverse 1979. In many specimens from the Anticosti material a membranous sac encloses the tetrad (Figure 3H). Strother and Traverse (1979) stated in their description of Nodospora that a membranous sac might be present. The spore type is the same as that illustrated by Gray and Boucot (1971) and Gray et al. (1982); these authors did not apply Linnéan binomens to the spores nor did they formally describe the spores. The spore type is also the same as that referred to as Tetrahedraletes medinensis by Duffield and Legault (1981, pl. 2, fig. 9).

In addition to the reports of Strother and Traverse (1979) of Nodospora burnhamensis and Tetrahedraletes medinensis from the Tuscarora, N. burnhamensis and T. medinensis have been reported by Miller and Eames (1982) from the Medina Group of the Niagara Gorge. The determination made in the present paper that most of the Anticosti spore-like microfossils, including those previously reported as T. medinensis (Duffield and Legault, 1981, pl. 2, fig. 9), are specimens of N. burnhamensis resulted from the study of over 200 specimens in various orientations on strew slides. Because the observation of numerous specimens in various orientations is necessary, it is not possible to determine from the photographs of Strother and Traverse (1979) or Miller and Eames (1982) whether the forms identified as T. medinensis by these authors are equivalent to N. burnhamensis.

Apart from the question of whether a difference between the cross and tetrahedral configurations does exist, the important constructional feature

Hunt Institute for Botanical Documentation

Duffield

**Please check one:**

_x_ **My name may appear on review.**

___ **My name should not appear on review.**

**Please answer the following questions and return to the editors with the manuscript:**

**1. Is this paper a significant contribution to paleontology? Why?**

Yes, because it adds a further dimension to knowledge of late Ordovician/ earliest Silurian probable land-plant floras.

**2. Could this paper (text or plates) be condensed by a significant amount? If "yes", please indicate how this could be done.**

Could not be condensed. In fact, it quite clearly should be expanded to include systematic descriptions of the important fossils, especially as the author suggests misinterpretation of the taxa mentioned by other authors. (See also appended additional statement.)

**3. Please indicate revisions you believe are mandatory if this manuscript is to be published. Briefly discuss these below or on a separate page.**

Please see appended statement.

**4. Do you consider the systematic part of the paper, if it contains systematics, accurate and up-to-date? Are identifications, diagnoses, and descriptions satisfactory? Are illustrations satisfactory?**

No. But I don't think this should preclude publication as a note somewhere. It's better than no description at all. If the author wishes to merge Nodospora and Tetrahedraletes she should provide more conclusive evidence and then go ahead and do it, formally. (Please see also, appended statement.)

Hunt Institute for Botanical Documentation

5. Please rank this manuscript compared to other papers you have read recently in the subject/field on the following categories.

| | Unpublishable | Publishable with major revisions | Publishable with minor revisions | Publishable as is |
|---|---|---|---|---|
| a) Organization | | | | X |
| b) Clarity of expression | | | | X |
| c) Scientific treatment | | X (See statement - refers to systematics) | | |
| d) Significance | | | | X |
| e) Illustration | | X (needs illustrations to support assertion of synonymy) | | |

Please return to:

JOURNAL OF PALEONTOLOGY
Department of Geology/Geography
University of Massachusetts
Amherst, MA 01003

Hunt Institute for Botanical Documentation

Additional comments on Duffield's paper:

Ms. Duffield has previously orally reported (NE-SE GSA, 1982) that Nodospora et al. occurs in some of her Jupiter Fm. samples. As a further example of the sorts of things reported by Gray & Boucot, systematically described by Strother & Traverse and Miller & Eames, this is an interesting contribution and is publishable as a note somewhere. However, as emphasized already by Strother & Traverse (Comment in Geology, 1983), Banks (various places), and by D. G. Smith (Lethaia, 1981), it is time to get down to cases with these things, stop the casual illustration and references to this and that, and get on with systematic description of the forms, and systematics generally. In a recent essay in Geology, Gray et al. (1983) also assert that Strother & Traverse misinterpreted Nodospora and Tetrahedraletes, and attack the application of nomenclature to these fossils at all. What Gray & Boucot ignore is that it's not the names as such that are important, but the systematic description which accompanies formal nomenclature. Duffield now says the same as Gray et al., but again no systematic description is provided to back up her assertions. If Duffield wants to declare Nodospora and Tetrahedraletes synonymous, she should systematically do so. If she's right (she could well be), her action in selecting between them must be followed, and would be helpful. What she does in this MS is just to make an assertion of (probable?) synonymy and leave it at that. It would only require a page or two more to take care of this systematically and give us all something to shoot at. That would be good science, even though she runs a risk of being wrong.

Finally, however, Duffield's figures do not convince me. She says Figures A, D & H (which we've called Nodospora) and B, E, F, I (which we've

Hunt Institute for Botanical Documentation

called Tetrahedraletes) are all the same thing in different orientation. (I think her own illustrations support the opposite contention.) Has she tried changing the orientation of individual specimens to prove her contention? Has she tried TEM? If she's going to make a de facto assertion of synonymy, she should try to give results of her study.

For future systematic work with these things, should Duffield be mentioned as asserting synonymy or not? Her plate and captions pull no punches, but she backs off in the text.

It could certainly be argued that this sort of short report of occurrence, with illustrations, should go in, say, Science, while J. of P. should hold out for a systematic, descriptive paper. That's the J. of P.'s business, but it would be my opinion.

Additional ±minor editorial comments:

p. 3, l. 2 from bottom: "The same spore type"--well, this is what is before the house (see other comments). Is she prepared to assert identity or not? If so, she should go ahead and do it formally.

p. 4, ll. 8-9: application of "binomens"--what Duffield should say is that Gray et al. simply don't describe the fossils. The names are a minor problem, relatively.

p. 5, l. 7: "retained in open nomenclature". I guess this means "is not referred to a named taxon", or something like that. The truth is that all of the nomenclature in this paper is pretty "open", and I would suggest deleting this statement. "Open nomenclature" is not a good

Hunt Institute for Botanical Documentation

expression anyway.

plate: magnification is given as "880 x". This is undesirable. 1000x and 500x are the usual palynological standards. 1000x is best, because it permits direct conversion of micrometers to millimeters. 500x or even 600x also permit easy conversion, but 880x? Why?

Hunt Institute for Botanical Documentation

29 September, 1981

Ms. Sue Duffield
Department of Earth Sciences
University of Waterloo
Waterloo, Ontario, Canada N2L 3G1

Dear Ms. Duffield:

Thanks for yours of 13-IX-81 and the interesting reprint re the Anticosti Island sediments. Too bad about the postal strike. I also tried to phone, but only once--as I was gone almost all summer (got back finally on 1 Sept). Thought I might see you on or about the 4th, but knew in the absence of word it was unlikely. You are still welcome when you get time!

Best to you, and to Dr. Legault. I am still hoping she'll come down here to talk acritarchs with us some day! So far she's said "Maybe, and that's final".

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

Department of Earth Sciences
University of Waterloo
Waterloo, Ontario N2L 3G1
13 September 1981

Dr. Alfred Traverse
Department of Geosciences
The Pennsylvania State University
435 Deike Building
University Park, Pennsylvania 16802

Dear Dr. Traverse:

Thank you for your letter of 27 July. Unfortunately it did not arrive here until early September, and I am sorry to have missed you this summer. I would be interested in receiving reprints of any future work you might do on lower Silurian palynomorphs.

I appreciate your interest in the Anticosti project. I am enclosing with this letter a reprint with the preliminary results of the biostratigraphical study.

Sincerely,

Sue Duffield

Hunt Institute for Botanical Documentation

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

Department of Geosciences
435 Deike Building

Area Code 814
865-2342

July 27, 1981

Ms. Sue Duffield
Dept. of Earth Science
University of Waterloo
Waterloo, Ontario, Canada N2L 3G1

Dear Ms. Duffield:

Your nice letter of 16 June was here when I arrived recently from Europe. There was also a cryptic phone message from Paul Strother indicating that he will meet you here in August - if I'm around, perhaps I'll get included too! Because of the Australia trip, I am not planning to go to New Orleans - a ~~prior~~ need to stay home accordingly.

Your work sounds very interesting, and I do want to hear more.

Best regards to Dr. Legault - but will this letter ever reach you? What if Canada settles its P.O. strike and ours then begins?

Yours very truly,

Alfred Traverse

Alfred Traverse
Professor of Palynology

/jb

please excuse typist!



Hunt Institute for Botanical Documentation

University of Waterloo

Waterloo, Ontario, Canada
N2L 3G1

Faculty of Science
Department of Earth Sciences
519/885-1211

16 June 1981

answer immediately

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

Dear Dr. Traverse:

Thank you very much for your offer to show me your collecting sites for the Tuscarora. I would like to accept the invitation to come to Pennsylvania in September. Because I will be a teaching assistant in the fall term, the one weekend I would be able to be away from Waterloo is the weekend of September 5th. This is also Labour Day weekend, and it closely follows the Botanical Congress, so I do not know how convenient this would be for you. For this reason, I am also writing to Dr. Strother in case he plans to be in Pennsylvania in August. However, if the first weekend in September would suit you, I would appreciate hearing from you.

I am doing my thesis under Dr. Legault. The thesis is a biostratigraphy study of acritarchs across the Ordovician-Silurian boundary and in the lower Silurian and a paleoecology study of acritarchs and conodonts. The land plant spores were found in a distinctive shale member of the Jupiter Formation (late Llandovery); the spores occur in samples which also yield abundant typical Silurian acritarchs. Dr. Legault and I plan to present the results of the biostratigraphy study at the AASP meeting in October. If I do not meet you in September, I hope to meet you in New Orleans.

Sincerely,

Sue Duffield

Hunt Institute for Botanical Documentation

28 May, 1981

Ms. Sue Duffield
Dept. of Earth Science
University of Waterloo
Waterloo, Ontario N2L 3G1, Canada

Dear Ms. Duffield:

You don't say in yours of 27 April, but you must be a student of Dr. Legault, and therefore automatically a friend of mine. Indeed, I have discussed the Anticosti work with her.

We are, of course, ready and eager to show you to the localities where Strother and I have collected the materials on which we have published. Technically, "central Pennsylvania" is a better description of location than "eastern Pennsylvania". I can steer you to productive localities, because we have processed many Rose Hill and Tuscarora samples that are barren.

Paul Strother and I are working on new Tuscarora and other Silurian localities on which we have not yet published. I believe you should best collect from our productive and published localities//as your aim is reference.

I just got back from sabbatical leave last weekend and walked smack-dab into an emergency operation for burst appendix. If able, I go to Europe on 9 June for a month, then to athe International Botanical Congress in Australia in August, back for good about 1 September. Can you come to PA a weekend in September?

Another thought is to write Mr. Strother (Harvard Univ., Paleobotanical Laboratories, 22 Divinity Ave., Cambridge, MA 02138). He is also out of pocket much of the summer, but there are 'windows' during which he'll be in PA and could guide you. (I cannot overemphasize that we can save you days of lab work by taking you to the best places.)

You do not need a permit to collect, unless you wish to collect along an Interstate Highway, where stopping is illegal, and even there, 99.9% of the time you could talk your way out of it without a permit. I believe I'd forget it.

Regards to Dr. L.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
cc: P. Strother

Hunt Institute for Botanical Documentation

answered

# University of Waterloo

Waterloo, Ontario, Canada
N2L 3G1

Faculty of Science
Department of Earth Science
519/885-1211

27 April 1981

Dr. A. Traverse
Palynological Laboratories
Department of Geosciences
Pennsylvania State University
University Park, Pennsylvania 16802

Dear Dr. Traverse:

I will be in eastern Pennsylvania at the end of the summer, and I would be interested in obtaining from you a list of sampling localities in Pennsylvania for the Tuscarora and Rose Hill formations. My thesis at the University of Waterloo includes the study of acritarchs from the Jupiter Formation (Llandoverian), Anticosti Island, Quebec. Some of the palynomorphs you described in your paper with P. K. Strothers, specifically Tetrahedraletes medinensis and Nodospora sp., appear also in the Jupiter Formation. I would be interested in processing Tuscarora and Rose Hill samples as reference material. Any information you could give me about sampling localities would be appreciated; I would also like to know if permits are necessary for collecting along Pennsylvania highways.

Sincerely,

Sue Duffield

Hunt Institute for Botanical Documentation

December 18, 1956

Dr. Suzanne L. Duigan
Botany School
University of Melbourne
Carlton, N. 3
Melbourne, Australia

Dear Dr. Duigan:

Many thanks for the fine piece of work on Dr. Cookson's biography and bibliography. It is a pity that I did not contact you before bothering other less qualified biographers. I did not know that you were to be reached at the Melbourne Botany School, but now have put your name in our address file and am glad to have made contact with you. I am now, thanks to you, ready to prepare the ballot for corresponding membership, and I share your hope that Dr. Cookson will be elected, though I have no way of influencing this result.

Thanks again, and best wishes of the season.

Yours very truly,

Alfred Traverse

AT:rlg

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

November 27, 1956

Dr. Suzanne Duigan
Department of Botany
University of Melbourne
Carlton, N. 3
Victoria, Australia

Dear Dr. Duigan:

Some time ago the Paleobtanical Section of the Botanical Society of America was requested by the parent Society to propose one or two foreign paleobotanists for the honor of corresponding (i.e, honorary) membership. In the initial poll four persons were nominated, including Dr. Isabel Cookson. As secretary of the group it falls my duty to poll the membership on the decision as to which one or ones of this group should be honored. I must circulate with the ballot a biography and bibliography for each of the persons. About two months ago I wrote Dr. Couper in New Zealand, and more recently Dr. Balme in your country, for help in getting together the material for Miss Cookson. Although I frequently correspond with Dr. Cookson, I did not want to approach her directly about this, of course. I did not have your address. Dr. Couper has now responded that he cannot get the material together and suggests that I contact you. Would you be willing to do this for me?

The biography and bibliography should be quite concise, the biography setting forth just the important vital facts, and the bibliography listing only the most important papers. If you can do this for me, will you put it in the mail at the earliest possible moment? I must get out the ballot as soon as possible. If you can do the job, will you inform Balme that you are doing it? His address, which you presumably already have, is Mr. B. E. Balme, C.S.I.R.O. Coal Section, P. O. Box 3, Chatswood, New South Wales, Australia.

Yours very truly,

Alfred Traverse

AT:rlg

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

SVERIGES
LANTBRUKSUNIVERSITET

Datum
3.3 1981
Ert datum

Beteckning

Er beteckning

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

Dear Professor Traverse,

Many thanks for the group photo! It is a very good photo, everybody seems to be at ease and can be clearly recognized.

It took me a long time to write my thanks, but I wanted to send you something in return. I hope you will like the photo I am enclosing, anyhow the colours have come out in a recognizable way.

Best regards to your wife and to you.
Yours
Anita Dunbar
Anita Dunbar



Postadress
SLU-Fytotronen
Box 50026
104 05 STOCKHOLM 50

Besöksadress
Roslagsvägen

Telefon
08 - 15 09 20

Postgiro
1 5667-9

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
DEPARTMENT OF GEOSCIENCES

INTER-OFFICE CORRESPONDENCE

DATE: 29 January, 1980

FROM: Alfred Traverse, Professor of Palynology
435 Deike

TO:
Robert E. Dunham
Vice-President for Undergraduate Studies
117 Old Main

re: course lists for Winter and Spring, 1981

I have recently been granted a sabbatical leave for the Winter and Spring Terms, 1981.

Will you please direct the appropriate person to see to it that my courses for those terms are omitted this one time from the official course list, viz.:

Winter Term: Geosciences 423 (=Biology 423)

Spring Term: Geosciences 526 (=Biology 526)

This will save some students a lot of trouble.

Approved: _____
E. Willard Miller,
Assoc. Dean Res. Instr.

cc: Dr. Burnham, Dean Miller, Judy Bailey, Dotty Duck

Hunt Institute for Botanical Documentation

To: dunninnp@ucbeh.san.uc.edu
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: manuscript
Cc: AHC@worldnet.att.net
Bcc:
X-Attachments:

Dear Nick:

Hope this goes. The address, which I picked up from the MS, seems defective--one would expect Dunning to be spelled better!

I got the Péten MS a couple of days ago and feel that I must trouble you re a few pages therein, hoping that they can still be corrected.

First, on pp. 24-25, Dave and I would like our e-mail addresses to be included. They are as above on this message.

On p. 14, l. 6 up, a generic name is misspelled. It should be Gloeocapsa.

On the same page, ll. 14 ff., although the matter is touched on later, I think for this to be professionally correct, the sentence should be rewritten as follows: [strike the initial word] Moraceae-Urticaceae, Combretaceae (here Terminalia), and Burseraceae (Bursera) represent tropical deciduous forest trees, and Cupuliferoidae is a pollen form group, presumably representing trees and/or shrubs of tropical members of the beech order (Fagales).

On p. 17, sentence beginning with l. 2 should be rewritten as follows:

The "Cupuliferoidae" [note correction of spelling] comprise a pollen form group, probably representing trees and/or shrubs belonging to the beech order (Fagales) that have entered the region in recent times.

I'm sorry for any trouble this will cause you, but I would be embarrassed to have it go out with errors.

Hope all goes well with you. The report is very good reading!

Best. Al.



Hunt Institute for Botanical Documentation

16 July 1991

Dr. Alfred Traverse
435 Deike
University Park, PA 16802

Dear Dr. Traverse:

Enclosed (and under separate cover) are 21 additional samples from a core of Lake Petexbatun in Guatemala for palynological analysis as arranged by David Rue. Attached is a list of the depths from which each section was taken (also listed on the sample bags).

Also enclosed is another sample from the Lake Tamarandito core. Your initial analysis judged this core to be rather unproductive. The enclosed sample is from higher up in the core, from above what looks to be a desiccation event of some kind, and, therefore, may be richer in pollen. We are hoping so because it turns out this core, despite being only half as long, is five times as old as that from Lake Petexbatun.

Please send the results of your analyses to both Dave and myself. The grant from U.S.A.I.D. that is paying for this work stipulates that the money must be spent by September 1991. If possible, could you please send an invoice to the project in advance of the actual work (that is, right away) so that we can spend their money without risk of red tape foul ups? Invoices should be sent to: Petexbatun Regional Archaeological Project
Dr. Arthur Demarest, Director
(Attn. Tracy Farrell)
Dept. of Anthropology
Vanderbilt University
Nashville, TN 37235

Thank you.

Best wishes,

Nicholas Dunning
Asst. Professor
Dept. of Geography
University of Cininnati
Cincinnati, OH 45221-0131



Hunt Institute for Botanical Documentation

412

Samples

1. 410-12 cm
2. 388-90 cm
3. 368-70
4. 348-50 cm
5. 328-30
6. 308-10
7. 288-90
8. 268-70
9. 248-50
10. 228-30
11. 208-10
12. 188-90
13. 168-70
14. 148-50
15. 128-30
16. 108-10
17. 88-90
18. 68-70
19. 48-50
20. 28-30
21. 0-5 cm

On #15 a dune
grass Dune Rue.
Plant
sample
discarded
on 8-VIII-95

Hunt Institute for Botanical Documentation

CABLES Y TELEGRAMAS:
"GEOLOGICO"

REPUBLICA DE COLOMBIA
MINISTERIO DE MINAS Y PETROLEOS
INSTITUTO GEOLOGICO NACIONAL
BOGOTA

APARTADO NACIONAL 2504
APARTADO AEREO 4865

SECCION: Palynology NUMERO: EM-31

December 8, 1957

Dr.
Alfred Traverse
Shell Development Company
3737 Bellaire Boulevard
HOUSTON 25, Texas, U. S. A.

Dear Sir:

I am studying Geology at the Universidad Nacional and also Palynology with Dr. Thomas Van Der Hammen. He has shown me your interesting paper "Pollen Analysis of the Brandon Lignite of Vermont". Would you be so kind to send me a reprint of it? I would appreciate it very much. I would also like to receive any other of your papers you may have.

Kind regards from Dr. Van Der Hammen.

Thanking you in advance I remain yours sincerely

EMILIO MUTIS DUPLAT

Return to:
Emilio Mutis Duplat
Apartado Nacional # 2599
Bogotá, Colombia, South America

Hunt Institute for Botanical Documentation

Dr. Traverse

January 8, 1962

Dr. J. W. Durham
Department of Paleontology
University of California
Berkeley 4, California

Dear Dr. Durham:

You probably recall having given me several samples of amber with which I proposed to experiment a bit in the hope of being able to develop a technique for macerating the stuff.

One of my children was so fascinated by your articles, pictures, and the samples, that he gave a little talk on amber to his class, using the samples as props. After that, I tackled the problem of a solvent for the material.

I suppose you knew already that amber is used in the manufacture of some very fancy varnishes, and the texts on paint chemistry mention that hot glycerin "dissolves" amber. The quotation marks are to express the idea that what really takes place is a sort of thermal cracking. I tried this with one of the samples and did succeed in getting all of the sample into solution in the glycerin after about a week of boiling. However, there were no recognizable fossils in the residue and I felt that the temperature had been too high.

We then discovered that tetrachloroethane, which boils at the more respectable temperature of 167°C, dissolves amber slowly. We refluxed one of the samples in a Soxhlet apparatus one month and the amber dissolved, leaving the residue in a cellulose thimble, which I had contemplated could easily be dissolved by acetylation. However, it appears that the amber has impregnated the fibers of the thimble so thoroughly that it cannot be acetylated. So, in a sense, we are back where we started, except that we <u>have</u> discovered that tetrachloroethane (a very toxic substance - must be refluxed in a hood) dissolves amber. I think the next step would be to substitute a glass wool thimble for the cellulose one, as I don't think that the amber would protect the glass from attack by hydrofluoric acid.

Hunt Institute for Botanical Documentation

Hoping this will be of interest, I am,

Very truly yours,

Alfred Traverse

AT:nw

bc: Technical Files

Information Copies:
Dr. Traverse
Dr. Higgs

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

October 24, 1961

Dr. J. W. Durham
Department of Paleontology
University of California
Berkeley, California

Dear Dr. Durham:

The discussion of plant fossils in amber the other day was much fun and was appreciated. I intend to look into the question of whether the stuff can be macerated or not, and am glad that you were able to let me have a few samples. Thanks also for the copy of your article on the general subject, which I read with interest on the train enroute to Houston.

I hope it will be possible for us to meet again some time in the future with a bit more time available.

Very truly yours,

Alfred Traverse

AT:nw

bc. Dr. Traverse
Dr. Higgs

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

THE AMERICAN ACADEMY OF ALLERGY

Executive Office
208 E. Wisconsin Avenue
Milwaukee 2, Wisconsin

544 Sunset Drive Angwin, California
January 5, 1959

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

Dear Doctor Traverse:

Through Dr. Bernard T. Fein of San Antonio I have received a copy of the September Shell News telling of the results of your pollen research. I did not know that the work has really come to flower in such a short time! I'm amazed at the finding of pollen so deep in the earth. My attention has always been on the sky.

Could you send me several copies of the magazine to show my friends? At the time of my retirement I was approached by one of the large oil companies about acting as botanist consultant on such a project. I think it was Magnolia. Nothing came of it.

You may be interested to know that I am still carrying on with my work as secretary of the Pollen and Mold Committee of the American Academy of Allergy. Last summer I was called in consultation on an aerobiology project at Brookhaven National Laboratory. In October my wife and I were guests of Mr. Laurance Rockefeller while setting up a field and air research project in the Virgin Islands National Park (on St. John Island). Also for a day at his Dorado Beach Hotel on Puerto Rico.

My very best wishes.

Sincerely,

O C Durham

Oren C. Durham, Secretary
Pollen and Mold Committee

OCD:alh

Hunt Institute for Botanical Documentation

December 12, 1957

Dr. Oren C. Durham
American Academy of Allergy
544 Sunset Drive
Angwin, California

Dear Dr. Durham:

It was very interesting to see your chapter in Sheldon's book, and I have enjoyed reading it. The photographs of flowering material are very good and certainly ought to help a non-botanist with identifications of flowering plants. I wish there were a really extensive handbook of such pictures!

I must say that it is too bad that the allergy palynologists have not learned to treat their pollen before examination. The cell contents obscure the structure and sculpture of the pollen wall too much and should by all means be removed, as it is only the wall in which one is interested for identification. The techniques for destruction of the contents are, after all, very simple. For example, from my preparations I find it hard to believe that oak and maple pollen could be confused, but I suppose if I were examining whole pollen, it would be a different story.

I hope you are wrong in thinking that the Manual is out of print. I am trying to get a copy, as I think we should have it available for reference. I am returning your copy under separate cover. Thanks again for your help.

Very truly yours,

Alfred Traverse

AT:ml

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

December 2, 1957

Mr. O. C. Durham
544 Sunset Drive
Angwin, California

Dear Mr. Durham:

The big packet of reprints (photostats) from the Abbott Laboratories in Chicago, came today. I certainly do appreciate this interest you have taken in helping me understand some of the background of the sort of work you have done. (It is more surprising to me in that I have never been able to get a response from friend Wodehouse, though, going back to graduate student days, I have written him several times for information. I wonder if I have his correct address.) These papers do help me in my effort to build a comprehensive pollen library. I am looking forward to future contacts with you.

I have been especially interested in "The Pollen Harvest", from Economic Botany, which I hadn't seen--it's fascinating reading.

Very truly yours,

Alfred Traverse

AT:ml

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

THE AMERICAN ACADEMY OF ALLERGY

Executive Office
208 E. Wisconsin Avenue
Milwaukee 2, Wisconsin

544 Sunset Drive, Angwin, California
November 25, 1957

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

Dear Doctor Traverse:

The Bureau of Mines sent me a copy of your Brandon monograph. I appreciate your kindness in making it available. When I see a scholarly product such as this, I am ashamed of my amateur work. But since you are so much interested in the chapter in Doctor Feinberg's book I am encouraged to send still another, Doctor Sheldon's Manual of Allergy, which is probably out of print by now.

You are well acquainted with Wodehouse's Pollen Grains. My chapter in the Sheldon book is an attempt to present allergists and their inexperienced helpers with a simple approach to pollen morphology. The Wodehouse book is far too technical for most of them to use at the start. So while you will find nothing valuable in the chapter, it will serve to give you some idea of how we have blundered along in a field that was new and without available literature when I began studying pollen dispersal more than thirty years ago.

Many thanks for your courtesies.

Sincerely,

Oren C. Durham

Oren C. Durham, Secretary
Pollen and Mold Committee

OCD:alh

Hunt Institute for Botanical Documentation

October 28, 1957

Dr. Oren C. Durham
Abbott Laboratories
544 Sunset Drive
Angwin, California

Dear Dr. Durham:

Thank you for the reprints. I have had photostats made of the two that you requested be returned. Under separate cover I am returning them, along with reprints of certain of my papers that may be of interest. (Note that my most important paper must be obtained by filling out a card that I am enclosing. Just drop the card in the mail.) As an enclosure with this letter I am returning the list of your publications, with the items checked that I would like to have. If you have reprints available or if I can get photostats as you suggest, it would be good to have them available for reference here.

I met Dr. Ogden at the paleobotanical meeting in Palo Alto in August. I suppose it was following that meeting that he visited you. Sorry you did not make it to the meeting. I would have enjoyed meeting you.

Very truly yours,

Alfred Traverse

AT:ml

Enclosure

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

Abbott
LABORATORIES
544 Sunset Drive, Angwin, California
Manufacturing Pharmaceutical Chemists · · North Chicago, Illinois

October 7, 1957

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

Dear Mr. Traverse:

The five articles in the series on Volumetric Incidence of Airborne Allergens are enclosed. Two of these reprints are my only copies here at my present home. But I can have photostatic copies sent to you from my office in North Chicago, if you wish.

Further research along these lines has been almost entirely lacking since 1947, but now Dr. Eugene Ogden of the New York State Museum and the University of the State of New York is set up for a three-year project that will take up where I left off. You will note that I did not consider my results to be the last word on the subject. Doctor Ogden has just been here to confer with me about his project.

Mark any other of my articles that you would like to have, and if no reprints are available I can have photostats made.

Yes, I should like to know about the work you are doing, so please send reprints of your most important papers.

Most sincerely,

Oren C. Durham

Oren C. Durham, Consultant
Abbott Laboratories

OCD:alh

Enclosures

Hunt Institute for Botanical Documentation

October 2, 1957

Dr. O. C. Durham
544 Sunset Drive
Angwin, California

Dear Dr. Durham:

If you have copies available, I would appreciate reprints of your papers in the Journal of Allergy, vol. 17. I am especially interested in the factors for conversion to unit value of various pollen grains, as proposed by you. If you have other papers for distribution, I would also appreciate them.

My research is in pollen analysis of modern sediments and of sedimentary rocks. If you have any interest in this field, let me know, and I'll send you reprints.

Very truly yours,

Alfred Traverse

AT:ml

Shell Development Company
Exploration and Production Research Division

Hunt Institute for Botanical Documentation

file: Dutcher

Southern Illinois
University

CARBONDALE, ILLINOIS 62901

*Department of Geology*

November 24, 1970

The Penn State University
Deike Building
University Park
Pennsylvania 16802

ATTENTION: Dr. E. G. Williams, Dr. R. Scholten, Dr. R. J. Cuffey,
Dr. A. F. Traverse and Dr. C. P. Thornton

Gentlemen,

When I was back at Penn State in October, I spent time with John Jobling relative to his Ph.D. thesis in Montana. John has finished two complete field seasons and has made excellent progress from this point of view. Many of his findings are of tremendous interest, and I feel strongly that the problem, when carried to a successful conclusion, will be a worthwhile contribution.

I began to suspect last spring, and am now convinced, that Palynology will play an important roll in John's work. I do not mean that it is a Palynological thesis; however, because of the roll which this will play in John's work, we have asked Al Traverse if he would be willing to serve on John's committee. Al has agreed to this. I do not approve of a six-man committee, but in view of my present Geographic position, I think that this is worthwhile.

I hope that this will be a functional thesis committee, and that John can feel free to come to you with questions and problems as they arise. I will be able to make some money available to John for thin-sections and base maps this fall.

Thank you very much for your help, and best regards to all.

Sincerely,

R. R. Dutcher
Professor and Chairman

RRD:ssh

Hunt Institute for Botanical Documentation

16 Nov., 1967

A. Traverse

R. Dutcher

Subject: Microscopes for teaching Geology 423 (palynology), Geology 421 (coal petrology) and Geology 420 (paleobotany)

There are at present no microscopes in the department suitable for teaching the laboratory sessions in the subject course. Last year we were able to improvise by using research microscopes. This was possible only because of the low enrollment (6) and the fact that each student had access to a research microscope.

There are in the teaching laboratory used for this course 10 B & L microscopes set up with objectives and so forth for the study of coal-petrographic preparations. It was originally the intention of Dr. Spackman that these microscopes would also be used for teaching Geology 423, but somehow the accessories needed for this purpose were never obtained. I propose that we get these accessories for these microscopes immediately so that we will be prepared to teach the laboratory sessions in this course without using research equipment for the purpose, even if it is available--which is notllikely.

What is needed is one objective (50x fluorite, B & L 42-33-61), and modifying accessories for the present light, condenser, and stage. The objectives cost $210 each, the modifications and the rest of the setup cost $107.50 each, making $317.50 per microscope, $3175.00 for ten scopes.*

Alfred Traverse

*Per microscope:

| | |
|---|---|
| O. C. White illuminator (B.& L recommended) | $ 9.50 |
| mechanical stage, graduated, B & L # 31-59-67 | 81.00 |
| exchange present condensers, 31-58-87, for B & L 31-58-74 Abbe B & L 31-58-04 sleeve B & L 31-58-28 iris, exchange price | 17.00 |
| B & L 42-33-61 fluorite 50x objective | 210.00 |
| | $317.50 |

Hunt Institute for Botanical Documentation

16 Nov., 1967

Memorandum

A. Traverse to R. Dutcher

Subject: Microscopes for teaching Geology 423 (Palynology)

There are at present no microscopes in the department suitable for teaching the laboratory sessions in the subject course. Last year we were able to improvise by using research microscopes. This was possible only because of the low enrollment (6) and the fact that each student had access to a research microscope.

There are in the teaching laboratory used for this course a dozen or so B&L microscopes set up with objectives and so forth for the study of coal-petrographic preparations. It was originally the intention of Dr. Spackman that these microscopes would also be used for teaching Geology 423, but somehow the accessories needed for this purpose were never obtained. I propose that we get these accessories for eight of the microscopes immediately so that we will be prepared to teach the laboratory sessions in this course without using research equipment for the purpose, even if it is available--which is not likely.

What is needed is one objective (50x fluorite, B&L 42-33-61), and modifying accessories for the present light, condenser, and stage. The objectives cost $210 each, the modifications to the rest of the setup cost an estimated $100 each, making about $300 per microscope, or $2400 in all.

Alfred Traverse

Hunt Institute for Botanical Documentation

D. R.R.D.

July 31 51

Dear Al

Forgive the paper please but you know how rough things are "in the field".

I have - on the off chance that you or Doug would be willing to help me a bit - mailed two samples (air mail) to you. If there is some rough quick way that they could be prepared to determine if similar material would be of value palynologically I would certainly be very appreciative. Both ~~the~~ samples are Paleocene and/or Eocene and in this area could be important in interpreting the tectonic history.

Samples are identified as follows:

W. Fork of W. Redbridge Creek - highly carbonaceous silt immediately under a massive conglomerate bed.

Gold Creek - slightly carbonaceous med. to fine grained ss. ~~but~~ best available but so far from this area.

Hunt Institute for Botanical Documentation

So my question is – can sporomorphs be extracted from there?

Also what are the chances of picking the Paleocene – Eocene boundary via the palynology route?

Many thanks & best regards

Russ Jutcher

Box 638
Red Lodge,
Montana 59068

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
~~STATE COLLEGE, PENNSYLVANIA~~
University Park, Pennsylvania

COLLEGE OF MINERAL INDUSTRIES
DEPARTMENT OF GEOLOGY

August 16, 1955

Dr. Alfred Traverse
Coal Microscopy Laboratory
U.S. Bureau of Mines
Building 20, Federal Center
Denver, Colorado

Dear Dr. Traverse:

Just a note to thank you very much for the use of Bradley's paper on the Green River formation. It was of great value in figuring out where we were in the section. One thing that impressed me is the great uniformity of the formation. The gross characteristics seem to hold true over a great lateral area.

Thanks very much for all of your time spent with Dick Neavel and myself. Please give my regards to your wife.

Yours sincerely,

Russell R. Dutcher

RRD:jpd

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
~~STATE COLLEGE, PENNSYLVANIA~~
University Park, Pennsylvania

COLLEGE OF MINERAL INDUSTRIES
DEPARTMENT OF GEOLOGY

May 30, 1955

Dr. A.F. Traverse
Fuels Microscopy Laboratory
U.S. Bureau of Mines
Building 20, Federal Center
Denver, Colorado

Dear Dr. Traverse:

Thanks very much for your letter. Plans have changed somewhat - not the possible time of arrival, but now I will be accompanied by Dr. Kremp who is anxious to meet and talk with you. I believe that either he or Professor Spackman has written to you concerning this matter.

What we wish to do is obtain samples in an attempt to set up stratigraphic control in the uraniferous lignite seams of northwestern South Dakota. We will be going far afield from the actual area in a kind of reconnaissance study. There will be also detailed sampling carried out at the sites visited for the past two or three years. In my "spare time" and on "days off" I will be looking for material for dissertation work. I wish to collect "dull" coal from as many varying coal ranks, types and places as possible. Hope to be able to characterize to some extent some of the macerals.

Many thanks again.

Yours sincerely,

Russ Dutcher

Russell R. Dutcher

RRD:jd

Hunt Institute for Botanical Documentation

May 24, 1955

Mr. Russell R. Dutcher
Mineral Sciences Bldg.
The Pennsylvania State University
University Park, Pennsylvania

Dear Russ:

Glad to hear from you about the proposed collecting trip. By all means do plan to stop here for a chat. The 18th or 20th of June, as you suggest, would be fine. There is some doubt in my mind about how much I can help you as far as the business of collection sites, etc., is concerned. I will do what I can. But you might find the visit here worthwhile anyway. For my part, it will be interesting to hear about your work and just what designs you may have on the Bighorn, Powder River and Williston basins. If you have time, jot a note for me about what you propose doing, so I can be thinking about it.

If you should call me, call BE 3-3611, ext. 6749, or ext. 478.

Best wishes to Professor Spackman and the others.

Very truly yours

AT

Alfred Traverse
Head, Fuels Microscopy Laboratory

cc J. M. East, Jr.
ATraverse/dw

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY
~~STATE COLLEGE, PENNSYLVANIA~~
University Park, Pennsylvania

COLLEGE OF MINERAL INDUSTRIES
DEPARTMENT OF GEOLOGY

May 19, 1955

Dr. Alfred Traverse, Head
Fuels Microscopy Section
United States Bureau of Mines
Building 20, Federal Center
Denver, Colorado

Dear Dr. Traverse:

I expect to be in the west this summer collecting materials for Professor Spackman. The areas concerned center about the Bighorn, Powder River and Williston Basins. The purpose of the trip is to collect palynological samples for study in connection with the U.S. Atomic Energy Commission program here at Penn State.

If at all possible, I should like to stop and briefly discuss the matter of collecting and collecting sites with you. Plans now indicate that I will be in Denver around the 18th or 20th of June. If you plan to be there, I would appreciate the opportunity to stop at your office. I would be able to telephone enroute to let you know a more definite date.

Yours sincerely,

Russell R. Dutcher

Russell R. Dutcher

jpd

Hunt Institute for Botanical Documentation

Dr. S. K. Dutta Ph. D., D. Sc., F. Pb. S., FIAP
Professor, DEPARTMENT OF APPLIED GEOLOGY

Gram : Dibrugarh University

Phone : 20468 Extn. 31

P. O. Dibrugarh University
Post Box No. 10
Dibrugarh - 786004
Assam [ INDIA ]

Date 3-3-92.

To

Prof. A. Travense,
Department of Geosciences
The Pensylvania State University
503 Deike Building,
University Park, P A 16802
U.S.A.

Dear Prof. Traverse,

Your kind letter dated December 13, 1991 received here in February 20, 1992. It took so much time because of the postal strike. Thank God that it reached me safely.

I am very glad to receive all the informations sought for. This would serve my purpose. I thank you very much for the same.

With all the best.

Yours sincerely

( S. K. Dutta ) 3/3/92

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

13 December, 1991

Dr. S. K. Dutta
Professor & Head, Applied Geology Department
Dibrugash University
Assam, India 786 004

Dear Dr. Dutta:

It was so nice to get to know you a little bit in Lucknow. I recall that you expressed an interest in the enclosed items. I hope they are what you wanted.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: biblio-bio material

Hunt Institute for Botanical Documentation

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

6 July, 1989

Dr. S. K. Dutta
Department of Applied Geology
Dibrugarh University
Dibrugarh 786004, Assam (India)

Dear Dr. Dutta:

Thanks for yours of 21 June, 1989. I am happy to oblige. A photo and a copy of my current university vita is enclosed. You will have to decide what is "significant"!

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl

Hunt Institute for Botanical Documentation

DEPARTMENT OF APPLIED GEOLOGY
Dr. S. K. Dutta Ph. D., D. Sc.
Professor and Head,

DIBRUGARH UNIVERSITY
DIBRUGARH - 786004
ASSAM ( INDIA )

Ref. No. DU/GEOL/M/4/'89

Date 21/6/89

Dear Prof. Traverse,

I would like to intimate you that the Department of Appled Geology, Dibrugarh University propose to display the photographs of eminent Earth Scientists in the Exhibition room. In that connection, I request you to send me a copy of your photograph of about 10cm X 17cm size. Also kindly give in brief the significant bio-data.

I shall look forward to your kind reply along with a photograph.

With best regard.

Yours sincerely

( S.K.Dutta ) 21/4/89

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

13 February, 1995

Dr. John A. Dutton
Dean, College of EMS
116 Deike, PSU
University Park, PA 16802

Dear John:

You probably picked up from my tone of voice on Thurs., 9 Feb. that I was somewhat startled by your request that I compare the contents of my labs with the wantlist of a person who is being offered a position here to see whether he could take over my equipment in lieu of the college purchasing new things for him.

As I told you, I was startled because when there were negotiations with Dr. Arthur a few months ago about my retirement, one of the inducements to persuade me to retire was the proposal that I could continue to use my laboratory facilities here after retirement until at least July, 1998, with extension beyond that date possible, I suppose depending on my performance. You, I believe, got a copy of Dr. Arthur's letter on this matter.

My agreement to retire was dependent primarily on this guarantee, as the financial inducements were nearly a wash with what I would get by not retiring and beginning nevertheless to draw full social security in addition to my salary in Sept., 1995. Dr. Arthur's letter specified that continued use of my labs was <u>not</u> guaranteed if I declined to retire on 1 July, 1995. My financial and legal advisers both therefore counselled me to accept the retirement package if I proposed to remain active, as I do.

Isn't life complicated?

Best wishes.

Yours very truly,

Alfred Traverse

c:Dr. Michael Arthur

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

# I N T E R O F F I C E  C O R R E S P O N D E N C E

Date: 12 December, 1989

From: Alfred Traverse, 435 Deike

To: John Dutton, 116 Deike

Re: Department Head search

Your letter of 5 December re the Department Head search is in hand. I am very sorry that Shel is "standing down." He has been very helpful to me and to the palynology program. After years of a very unfriendly head, Shel was most refreshing!

I would be willing to serve on the Search Committee. I would say that somebody from the organic side of geology must be on the committee. If Roger Cuffey or Alan Davis is so appointed, I should not be.

As to other people, I would suggest Roy Greenfield to represent geophysics, and Art Rose for geochemistry, but only if Art says he won"t be head. He would be very good as head.

AT/et

Hunt Institute for Botanical Documentation

PENNSTATE

(814) 865-6546

Office of the Dean
College of Earth and Mineral Sciences

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

December 5, 1989

Dr. Alfred Traverse
Professor of Geosciences
435 Deike Building
University Park, PA 16802

Dear Al:

As you know, Professor Shelton Alexander has announced his intention to step down as Department Head at the end of his current five–year term.

We will conduct a national search for a new Department Head. As provided in University policy, I will appoint a Search Committee to perform that task and make recommendations to me.

I am asking each member of the Geosciences faculty to write to me nominating members of the search committee. Please indicate whether you are willing to serve on the committee. If you wish to be considered as a candidate for the position of Department Head, please decline to serve.

I look forward to your cooperation in finding a new Head for the Department of Geosciences.

Sincerely,

John A. Dutton
Dean

dvs

An Equal Opportunity University

Hunt Institute for Botanical Documentation

THE PENNSYLVANIA STATE UNIVERSITY

Inter–Office Correspondence

DATE: August 10, 1989

FROM: John A. Dutton

TO: Alfred Traverse

RE: Energy and Fuels Research Center

Thank you for your memo of August 1 expressing your concern about the impending move of the Energy and Fuels Research Center to the research complex on the eastern side of the campus. As you know, adequate space for our many activities is the most serious issue facing the College, and our attempts to resolve the many contentious issues do not always satisfy all parties concerned.

The competing considerations in the case of the EFRC were the continued facilitation of collaboration with Geosciences versus enhanced collaboration of the Center with other College activities in coal research, processing, and combustion.

As you may know, we are creating a major energy research center in the Academic Projects and Academic Activities buildings and the new Research Building E. The Combustion Lab and the entire Fuel Science Section are now housed there as is the new coal–based jet fuels research institute. Moreover, we are in the process of designing a $3.4 million addition to Academic Projects and Activities that will link them and provide first–class facilities for our research on coal properties, combustion and desulfurization. We expect to start construction as soon as the design is complete and hope to occupy the new building within two years.

Although the main efforts of EFRC will be in the new facility, Dr. Davis will maintain an office and laboratory in Deike and continue to be an active and valued member of the Geosciences faculty.

We would like very much to have all of our activities in contiguous space that facilitated collaboration and enhanced our sense of collegiality, but the growing needs for office and laboratory space mitigate against that possibility. Unfortunately, we shall be spreading out in the years ahead. That is, I guess, an inevitable, although unfortunate, consequence of success.

dvs

Hunt Institute for Botanical Documentation

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

# I N T E R O F F I C E  C O R R E S P O N D E N C E

Date: 1 August, 1989

From: A. Traverse, 435 Deike

To: John A. Dutton, Dean, College of Earth & Mineral Sciences

Re: Proposed relocation of Energy and Fuels Research Center

The full impact of the abovementioned relocation has finally hit me. I am sure that the reaction of a single professor and his program carries little influence. However, I feel compelled to express the opinion that the move of the group out of the main center of the College is disastrous for all of us who work with organic matter. For my whole 23 years here, the presence of the coal research people in the same building has been constantly stimulating. The graduate students associated with Davis and Spackman have always interacted on a day-to-day basis with me and my students. The similarity of instrumentation has always been conducive to exchange of observations and ideas between palynologists and coal petrologists. Furthermore the design by Spackman of the 4th-5th floor facilities for coal and other organic matter research created a unique laboratory for such work, a factor certainly closely tied to the international reputation of energy research at Penn State, a program widely regarded as one of the best in the world. Penn State can, in my opinion, ill afford to disrupt the momentum of the research, undermine the morale of the personnel involved, and most certainly damage the fine reputation so carefully built up over the years.

I would urge that this move be reconsidered.

AT/et
cc: S. Alexander

Hunt Institute for Botanical Documentation

Department of Botany,
Tilakdhari College
JAUNPUR (U.P.) India
Sept. 16, 1957

Dr. Alfred Traverse
Shell Development Company,
3737 Bellaire Boulevard
Houston 25, TEXAS (U.S.A.)

My dear Dr. A. Traverse:

I am very much thankful for your letter & your valuable reprints (3) which I received a few days back.

Regarding the Enigmocarpon paper, I learn from my peon that it was sent to you by ordinary mail. Perhaps this is why it has not reached you so far. In case you ~~you~~ do not receive the reprint in a week's time, please let me know so that I may send another copy of it.

My another paper on "Pollengrains & Pollination in Sahnianthus flower" is in the press. In case you are interested, a ~~Copy~~ reprint will be sent to you as soon as it is available.

With regards.

Very truly yours
J. N. Dwivedi.
(J. N. DWIVEDI.)

Hunt Institute for Botanical Documentation

July 19, 1957.

Dr. A. Traverse Esqr.
Shell Development Company,
Exploration & Research Division,
3737, Bellaire Boulevard,
Houston 25, Texas (U.S.A.)

Dear Dr. Traverse:

As desired by you, I am sending a reprint of my paper, "On a new specimen of Enigmocarpon Panjai Sahni," published in the Journal of Palaeontological Society of India, Vol. I, no. 1, 1956. Please acknowledge.

I shall be extremely pleased to receive some of your valuable reprints in exchange.

Sincerely yours
J. P. Dwivedi

Department of Botany,
Tilakdhari College,
JAUNPUR (U.P.)
India.

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 25, 1967

Mr. Arthur Dymond
Atlas Chemical Industries
P.O. Box 790
Marshall, Texas 75671

Dear Mr. Dymond:

Doug Nichols and I spent a most interesting day at your mine and I am only sorry that we did not make it back into Marshall in time to tell you so personally and to see the movie, which I am sure we would have found informative. Everybody at Atlas, especially Jim Jarvis, the mine superintendent, helped us every way that we could have possibly hoped. I was greatly impressed with the efficiency of Mr. Jarvis's operations at the mine, and his willingness to help us was most welcome. After I finished at the mine, we did return to Marshall though to late to see you, and we called on the folks at the lab diagonally across the street from your office because Mr. Jarvis told us that one of the people there, a Mr. Broughten, wanted to meet me. It turned out that he had heard about some of my previous work with lignite. I was most impressed that somebody in Marshall already knows me.

Thank you again for all your help.

Yours very truly,

Alfred Traverse
Associate Professor of GEology

AT:kaw

Hunt Institute for Botanical Documentation

20 February, 1986

Dr. Gary Dyrkacz
Chemistry Division
Argonne National Lab.
9700 South Cass Ave.
Argonne, IL 60439

Dear Gary:

Many thanks for yours of 10 Feb., and for the reprints. I am still very interested in the pesky sporopollenin problem and would like to collaborate with you, if you could figure out a way I could do so. A gentle processing method for isolation of sporopollenin was to be my next effort one day, and clearly digestive enzymes seemed to be the way to go.

Please keep in touch.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Hunt Institute for Botanical Documentation

2 December, 1985

Dr. Gary Dyrkacz
Argonne National Lab.
Chemistry Division
9700 S. Cass Ave.
Argonne, IL 60439

Dear Dr. Dyrkacz:

Just a reminder about sending me a reprint of your paper re concentrating exinite, etc.

I'd also like to keep in touch with you re sporopollenin, with the idea of possible future collaboration.

Enclosed is a xerox copy of my old Shell report on sporopollenin.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl

Hunt Institute for Botanical Documentation

Bill Dziedzic
133 Vaughn St
Olyphant, Pa 18447
May 24, 1972

Dear Dr. Traverse.

Tomorrow morning along with this letter I am Sending you Several Fossil Specimens.

I have boasted that I have hundreds of Specimens, which is true, but I have been too busy harvesting Fossils that I have no time for inventory or underdification.

I have sent two Seed Ferns Pinnae, the Specimen with the larger Pinnules. I think is a real good Specimen, So is the other except that without the terminal Pinnule it is incomplete.

I think the specimen of seed Fern and Sphenophyllum is pretty interesting side by side and with good detail.

The Sphenophyllum Specimen on the red shale is a part of a large thick piece of Shale from which I first found my very best Specimens of Sphenophyllum.

The Specimen of Lepidodendron I had planned to send you not because of the Leaf Cushions, but because of the otherside Showing Corbonized wood pulp.. Thats what it looks like to me and its interesting. I have run across quite a few Specimens Showing that pattern I was just going to ask your opinion of it was a good idea to harvest such Specimens

Also I have included two Fossils which I ask the question whats it? One Specimen looks like Sphenophyllum an undeveloped form. To my naked eye the stems look similar to Sphenophyllum stems.

Hunt Institute for Botanical Documentation

I just found the (2) enclosed Specimen this morning or I should say shortly afternoon: I have found four or five [redacted]ch Specimens and discarded my very first ones. But each one looked alike and thats what troublesome.

Also there is one Specimen which I have no Idea about except that it is some type of a Fungus or Something.
I have found a few slabs of shale containing such growths (growths) But I threw them on my discard pile in the woods behind the back yard. If you think I should keep them I will bring them back unto the fold again. I have one Specimen, but it seems throwing away Specimens and bringing them back seems to be the thing I've been doing.

Heres another example, Last year I found two branch casts taken from a top of a layer of coal about two feet thick. I put them away, even gave a segment of the Smaller Branch or trunk cast to a fellow from New Jersey who was picking Fossils on the rock dump. When I gave it to him, he was very reluctant to take it. He said it was worth something because it was cast of Iron pyrite or marcasite. To make a long story short I threw that section away and last week I went over to the old stopping hole and reclaimed it. Im glad I had enough sense to put it somewhere for Just in Case.

Finally with regard to Specimens shipped to you, I've also included a Calamite fructification, A seed cone would be more a term that should be applied, at least from the laymen. This Specimen I also found this morning.

And it was one of the challenging joys of Searching

Hunt Institute for Botanical Documentation

that is, no one knows what a strike of a hammer will show upon breaking a stone or a slab of shale.

I've sent it to you, because I'm afraid I'd wreck it trying to skin it down.

Also one hour later on a large slab of shale about 3 feet by two and a half feet by about ten inches thick I saw the very best specimen of a Calamite Seed Cone ever along with a very good specimen of Lepidodendron three inches above the seed cone. (Cleavage was very good, but on the end where it counted most (at the fossil) it was not good, and as a result I lost that specimen, and I sure mumbled quite a bit about and probably out loud.

Dr. Traverse this is my second Spring I'm collecting I should know better. My goal should always be, preserve the fossil at all cost, for I may never find another. And I probably will not not like that specimen I let get away. I'm a hammer and chisel man. Breaking rocks into sand, and shale into dust would sure come easy with me.

Finally I would like to tell you about my Geological find of last fourth of July. I found something which looks like a beetle. That was my first decision, then I thought maybe it's just a seed germinating. I forgot all about till I started to take inventory and clean up my fossils the past weekend. After looking at it again I'm convinced I'm right and if it's not a beetle it's something similar. Detail very good. If true that would be something to be happy about and make collecting fossils even more exciting.

Hunt Institute for Botanical Documentation

I hope you can read my writing without too much trouble. I have a problem with my index finger. It's dislocated at the knuckle, and the doctors say I have an arthritis problem at the next joint.

I want you to know I appreciate your help and interest very much. I know you must be very busy with your work. But if and when time permits that you can find time to indentify specimens sent to you by me, I'll be very thankful. and they are yours.

Dr. Travers there is one thing about collecting fossils And that there is never a shortage of space for Personal or other good fossil Specimens. One always manages to find a place or give them away to some one who will appreciate them.

My letter has turned out to be a short story interesting I hope. I will remember you and if I come across some real good seed ferns Specimens As I'm sure I will one day. I will send them along and with anything else that I think might be very interesting

Sincerely Yours
Bill Dziedzic

My last name is a Humdinger.
It is Spelled DZIEDZIC

Hunt Institute for Botanical Documentation

Bill Dziedzic
13[redacted]aughn St
Olyphant, Penna 18447
June 17, 1972

Dear Dr. Traverse,

Thank you kindly for your letter of June 2, and for your suggestions for Coating and Marking Specimens, and for Identifying Fusain and Cordaianthus.

Your interest in the Specimen which I thought was some kind of Fungus, hastened me to my discard pile of Specimens, and I returned a few pieces back to the hold category.

In the beginning when I first started collecting Fossils, I would inscribe the date, and general location right on the Specimen. Somehow I got away from doing that And Since I was collecting certain kind of Fossils in certain places, I felt I would know the Source. But I have already found out my memory isn't as good as I thought it is And it would be better Marking the Specimens.

Hunt Institute for Botanical Documentation

With regards to the Specimens I sent you. I thought the Penna on the fine shale was nice, also the Specimen with Seed Fern Penna and Sphenophyllum.
The others I sent along to show what kind of Fossils Plant I have Collected.

For your records. The Seed Fern (the better one) is from Throop, Pa 1971. The Seed Fern & Sphenophyllum (1971) I also found in Throop. Its not from the original source I found it on on old railroad Trackbed where they dumped Mined rock to build up the railroad bed.

Also the Specimen of Sphenophyllum and the Specimen which I thought was some kind of fungus was found on a Mine dump, Some area, Source Olyphant, Pa., MAY 1971.

The "Fusain" Cordaianthus, and Seed Cone was found Some area. The area is called Underwood, but whether its is located in the Town of Throop, or Olyphant I dont know. So I Just mark UN on My Specimens. If you would mark yours I would Suggest (Underwood) Olyphant PA MAY 1972.

In the post, Two weeks ago exactly, I found some nice Specimens of Seed Fern. The Pinnas are long

Hunt Institute for Botanical Documentation

and tapered. The Pinna shows up (show) real good but the veins do not up well.

I had planned on sending you a specimen last week with a Pinna and what looks like a seed print. I found a few specimens showing prints as described above. I was excited about it, but since it was so near vacation time I didn't want to impose on you. You've been very kind and helpful and I'll always remember that your interest and suggestions encouraged me very much.

I hope your vacation will be a healthy and happy one; And when autumn comes I may be able to send you a beautiful Specimen or two

Very Truly Yours
Bill Dziedzic

Hunt Institute for Botanical Documentation

May 15, 1972

Mr. Bill Dzwodzic
133 Vaughn Street
Olyphant, Pennsylvania 18447

Dear Bill:

Thank you for your kind and interesting letter of 6 May regarding your hobby which you share with me. Keep up the good work.

Regarding your offer of a gift of fossils I should probably say in all honesty that our biggest problem here at Penn State in attempting to do paleontology is that nobody realizes the necessity of extensive collections in this field of geologicaendeavor. Therefore we have almost no storage space, and I must keep my collections down to the relatively few specimens I really need for my own research. However, if you should want to send me one or two well preserved examples of seed ferns which you have collected I would find this interesting and, also, it would give me a souvenir of our correspondence.

Best wishes to you again.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

Bill Dziedzic
133 [illegible]ughnott
Olyphant, PA 18447
May 6, 1972

Dear Mr Traversa,

Many, Many thanks for your reply to my inquiry and for your most helpful information.

Since there are no minerals and Gemstones to speak of in this area, Fossil Collecting is about the only hobby available. And I often wonder why I waited so long to get involved myself.

After forty six years of life (Just yesterday) I awakening to my environment and I find Fossil Collecting as a joy and a pleasure Sweet to the soul and (good for the health. Even though (I must be the donkey) and I do carry heavy loads a mile more or less. I dont ask for help unless I really have to.

Hunt Institute for Botanical Documentation

Members of my family have given me a hard time and some of the neighbors think there's something wrong with me; Nevertheless I keep picking and collecting and enjoying it

I've used a strong solution of vinegar and hot water to remove the "white deposits" from the fossils. Most of my Dinnularia Stellata Leafwhorls were coated. After seeing them cleaned up, probably not the way an expert does it, but it sure makes it all worthwhile

I have hundreds, maybe thousands of fossils unsorted, Lepidodendron, Annularia, Sphenophyllum, Seedferns. As yet I can't break them down to Genera or even family

It would give me great pleasure if you would accept a selection of fossils as a token of my appreciation for your great help. If you have a preference on fits available. I'll gladly send it along

Thanking you again I remain

Sincerely Yours

Bill Zadzie

Hunt Institute for Botanical Documentation

April 28, 1972

Mr. Bill Dzredzic
133 Vaughn Street
Olyphant, Pennsylvania 18447

Dear Mr. Dzredzic:

Thank you for your letter of 19 April. I am glad to hear that you enjoy working with fossils--I have long wondered why people don't find it as much fun as some other much more popular hobbies.

The fossil which you sent me is known as "Cyclopteris". The name doesn't mean too much, however, because this particular sort of pinnule (small basal leaf) is formed on the fronds of a number of unrelated ferns. Nevertheless, the name Cyclopteris is the way to identify a pinnule such is the one you sent me.

Regarding your "white deposits"--it depends of what they consist. In some cases they will be calcite, and strong vinegar or dilute hydrochloric acid will get rid of the deposit for you. In other cases the deposit will be due to clay minerals, and their removal is going to be somewhat more difficult. Soaking in water to which a little detergent has been added, and followed by careful brushing will often get rid of such a layer. Sometimes the soaking would have to be allowed to proceed for several days for it to do very much good.

Unfortunately, your question about a book is a difficult one to answer. The problem is that nobody has got around to writing such a book because of the relative lack of interest in fossil plants. There are things which are useful--including the German book I used for work on your Cyclopteris. However, they are hard to obtain, and would scarcely be of very much use to you. Actually, Professor Arnold's book is about as good as there is. Sometime when you feel in the mood to do some good for the science you might write a letter to the Paleobotanical Section of the Smithsonian Institution in Washington recommending that they produce such a

Hunt Institute for Botanical Documentation

book to help lay people with the identification of fossil plants. Doubtless they will respond on the first time around that there are such and such books available, but I am sure that they will turn out to be either too simple to be of much use or no better than Arnold.

Well, I guess that's about all I can help you with at the moment. Best wishes to you and your paleobotanical efforts.

Yours very truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

June 2, 1972

Mr. Bill Dziedzic
133 Vaughn Street
Olyphant, Pennsylvania 18447

Dear Bill:

Thank you so much for your letter of 24 May and the specimens which you sent me. They will serve as an interesting reminder of our correspondence. The specimens are quite nice, and I have sprayed them with spray plastic for inclusion in my collection of such things. You might want to try that with some of yours if you haven't already discovered it. After getting the specimens as clean as you can in whatever way you aim to do so simply stand the specimens up in someeplace where you don't have to breathe the fumes such as in a well ventilated garage and spray them with a clear plastic. I use a kind which I get at a drafting and artists supply store called Grumbacher "Tuffilm", but I believe that other products indended for spraying art work and so on would be equally satisfactory. They protect the specimens and besides that make the leafes, etc., stand out better than when they are unsprayed.

Regarding the "carbonized wood pulp"--that is usually called "Fusain". It is quite common and may or may not be of interest depending on how well preserved the woody structure is within it, something which you cannot tell without making thin sections and doing considerable microscopic study.

The specimen which you thought was an undeveloped Sphenophyllum seem to me to be very probably instead a cone of Cordaianthus. I doubt that there is very much structure in it which would show up in a thin section. Regarding the specimen which you describe as "some type of a fungus"--I have puzzled over this a good deal and am convinced that the thing which looks like a fungus is probably a sedimentary structure resulting from the hardening of the shale--perhaps as a result of biological activity but not, strictly speaking, a fossil. There are, however, on the same slab some more or less circular laminated bodies about the size of a dime or a little smaller which are possibly invertebrate animal remains of some sort.

Hunt Institute for Botanical Documentation

I intend to show them to one of my paleontological friends one of these days for an opinion. They would not be of any great interest, however, because they are not well enough preserved. It was interesting to hear about your find of a fossil insect--they do occur in rocks of this age but are not too common, and your find would be well worth preserving.

One final comment--your collections would be of much more importance to somebody who might later on have an interest in them. If you would keep very thorough notes in a good notebook regarding the exact date of collection and the exact location from which each of the fossils which you collect comes. The best technique is to number every speciman and list it separately. They can be numbered by painting a small section of a fossil with yellow paint, printing the number with india ink, and covering with colorless fingernail polish.

Best wishes to you.

Yoursxvery truly,

Alfred Traverse
Professor of Geology & Biology

AT/vsi

Hunt Institute for Botanical Documentation

Bill Dziedzic
133 Vau[illegible] St
Olyphant, Penna 18447
April 19, 1972

Dear Sir,

Im a handicapped person who enjoys picking Fossils.

If and when time permits, will you please indentify the enclosed Fossil.

Also I would like to know if its possible to eliminate the Milky. White deposits on Fossils.

Can you recommend any books that would help in identifying Fossil plants. I have "Paleobotany" by C.A. Arnold and Copies of a few pages of "Fossils" by Fenton.. Both of which are two steep for this person, who never finished grade School.

The enclosed Fossil is yours and I thank you Kindly for your help Sincerely.

Stamped, addressed Envelop Enclosed. Bill Dziedzic

Hunt Institute for Botanical Documentation