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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.

From: Mail Delivery Subsystem <MAILER-DAEMON@ems.psu.edu>
Return-path: <>
To: <traverse@ems.psu.edu>
Date: Tue, 23 Dec 1997 11:35:38 -0500 (EST)
Subject: Returned mail: User unknown
Message-ID: <199712231635.LAA04117@pangaea.ems.psu.edu>
X-Status: Read

This is a MIME-encapsulated message

--LAA04117.882894938/pangaea.ems.psu.edu

The original message was received at Tue, 23 Dec 1997 11:35:28 -0500 (EST)
from traver.geosc.psu.edu [128.118.174.64]

---- The following addresses had permanent fatal errors ----
<nrederic@usgs.gov>

---- Transcript of session follows ----
... while talking to igsrsparc1.er.usgs.gov.:
>>> RCPT To:<nrederic@usgs.gov>
<<< 550 <nrederic@usgs.gov>... User unknown
550 <nrederic@usgs.gov>... User unknown

--LAA04117.882894938/pangaea.ems.psu.edu
Content-Type: message/delivery-status

Reporting-MTA: dns; pangaea.ems.psu.edu
Received-From-MTA: DNS; traver.geosc.psu.edu
Arrival-Date: Tue, 23 Dec 1997 11:35:28 -0500 (EST)

Final-Recipient: RFC822; nrederic@usgs.gov
Action: failed
Status: 5.1.1
Remote-MTA: DNS; igsrsparc1.er.usgs.gov
Diagnostic-Code: SMTP; 550 <nrederic@usgs.gov>... User unknown
Last-Attempt-Date: Tue, 23 Dec 1997 11:35:38 -0500 (EST)

--LAA04117.882894938/pangaea.ems.psu.edu
Content-Type: message/rfc822

Return-Path: <traverse@ems.psu.edu>
Received: from traver.geosc.psu.edu (traver.geosc.psu.edu
[128.118.174.64]) by pangaea.ems.psu.edu (8.8.8/8.8-psu-ems) with SMTP id
LAA04115 for <nrederic@usgs.gov>; Tue, 23 Dec 1997 11:35:28 -0500 (EST)
Message-Id: <199712231635.LAA04115@pangaea.ems.psu.edu>
X-Sender: traverse@ems.psu.edu (Unverified)
X-Mailer: Windows Eudora Version 1.4.3
Mime-Version: 1.0
Content-Type: text/plain; charset="iso-8859-1"
Content-Transfer-Encoding: quoted-printable
Date: Tue, 23 Dec 1997 11:35:54 -0500
To: nrederic@usgs.gov
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: rec.

Dear Norm:

Of course I would write a letter of rec. for you.

We are leaving in a few hours for Florida, where we will spend Jan. and

N. D. Frederickson
USGS Nat. Ctr. Stop 970
12201 Sunrise Valley Dr.
Reston, VA 22092

Feb.
(in Clearwater). However, all snail-mail and e-mail are being forwarded to
the FL address, and I would take care of the matter from down there.

Wish I could dream up another project for Germany! Betty and I were there
in Oct. and Nov. to mend some fences, and to take a look at places in the new Bundesl=E4nder we hadn't seen. Betty found much of interest for her medieval studies. (A book based on her dissertation was published in Stuttgart last year.) I especially enjoyed Mecklenburg-Vorpommern, where were the Schaarschmidts' guests. I also liked Potsdam and Weimar very much.

Happy holidays and a great 1998 to you and yours.

Best. Al.

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

--LAA04117.882894938/pangaea.ems.psu.edu--

United States Department of the Interior

U.S. GEOLOGICAL SURVEY
Reston, Virginia 20192

Mail stop 926A
Fax 703-648-6953
nfrederi@usgs.gov
December 15, 1997

Dr. Alfred Traverse
Dept. of Geosciences
Pennsylvania State University
University Park, PA 16802
Fax 814-863-7823

Dear Al:

I am applying for a Humboldt fellowship (technically, a Humboldt Research Prize) to work at the University of Bonn, and I was wondering whether you could write a letter of recommendation for me. I don't think there will be a great hurry about it, probably some time this winter or next spring, and I can provide some details about the nature of the research if you can do it.

Merry Christmas!

Cheers,

Norman

Norman Frederiksen

To: nrederic@usgs.gov
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: rec.
Cc:
Bcc:
X-Attachments:

Dear Norm:

Of course I would write a letter of rec. for you.

We are leaving in a few hours for Florida, where we will spend Jan. and Feb. (in Clearwater). However, all snail-mail and e-mail are being forwarded to the FL address, and I would take care of the matter from down there.

Wish I could dream up another project for Germany! Betty and I were there in Oct. and Nov. to mend some fences, and to take a look at places in the new Bundesländer we hadn't seen. Betty found much of interest for her medieval studies. (A book based on her dissertation was published in Stuttgart last year.) I especially enjoyed Mecklenburg-Vorpommern, where we were the Schaarschmidts' guests. I also liked Potsdam and Weimar very much.

Happy holidays and a great 1998 to you and yours.

Best. Al.

To: nfrederi@usgs.gov
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: aff., cf.
Cc:
Bcc:
X-Attachments:

Dear Norm:

Big apologies for neglecting to answer yours of 8 Jan. re the above subject within a reasonable time. Problem is mostly that we were out of town until 20 Jan., since 26 Dec., and you know how that is.

I have looked in all the books that seemed promising to me, but aff. and cf. aren't even mentioned in all but one: Stearn's Botanical Latin. He defines both and discusses their origin, but doesn't cf. (=compare) them one to one.

I get the impression that the nomenclatural crowd think use of these terms is beneath their dignity. Either you identify, or you don't!

So, I can only tell you that to me, cf. (=confer=compare) suggests a closer relationship than aff. (affinity). cf. implies that you think your item is very close to the named entity, but are a little reluctant to come right out and make the identification. Aff. means a general relationship is noted--looks somewhat like Sapotaceae, that sort of thing.

So, your usage certainly differs from my understanding, but there are no rules on it that I know of. (And I hope to goodness that I haven't said or done something to the contrary somewhere.)

Best wishes. Al.

From: "Norman Frederiksen" <nfrederi@usgs.gov>
Date: Wed, 8 Jan 1997 15:28:44 -0500
To: traverse@ems.psu.edu

Hi, Al. Hope you had a nice Christmas and New Years.

For 30 years I have been using aff. to mean that the specimen might be the same species, and cf. to mean that it probably isn't the same as a given species. Now somebody has pointed out that the Dictionary of Geological Terms says that they have the opposite meaning. I looked in a number of taxonomy books, mainly botanical, including the ICBN, and I can't find these abbreviations defined anywhere. Have I been wrong all these years?

Cheers, Norm

From: nfrederi@gccmail.cr.usgs.gov
Date: Wed, 09 Oct 96 09:15:28 MST
To: traverse@ems.psu.edu

Frederiksen

Dear Al:

Thanks for the info. I was going to change the ending of one of Ray Christopher's species, so now I don't have to.

Cheers, Norm

To: nfrederi@gccmail.cr.usgs.gov
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: contact
Cc:
Bcc:
X-Attachments:

Dear Norm:

Yeah, your message of 7 Oct. was on the machine when we came in today. The time was listed in MST. You were in Denver when you phoned the other day?

A longish answer to your query went out by snail mail this a. m. As emeritus, I get e-mail and snail mail free, but I have to pay for telephone calls, and besides I have to do it with a credit card, which requires dialing (I am not making this up) 28 numbers. I don't phone much. We have e-mail everywhere (carry a laptop and modem when travelling) and four different e-mail addresses for different purposes. (At home, for example, it's Alnbet@aol.com.)

The gist of my answer is that the Tokyo Code has nothing to say about generic names ending in -is. According to the Code, you should follow the gender assignment of the original author of the generic name. I agree with JJ that all -pollis and -sporis names "should" have the same gender, but the only way to bring that about would be to amend the Code at the next Congress. -sporites and -pollenites names are taken care of in the Code--they have to be masculine.

Come see us again. Best, Al.

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E-Mail: traverse@ems.psu.edu

7 October, 1996

Dr. Norman O. Frederiksen
USGS National Ctr., Stop 970
12201 Sunrise Valley Drive
Reston, VA 22092

Dear Norm:

Am replying to your interesting note of 26 September by snail-mail because I still don't have your e-mail address, and in my emeritus status I have to pay personally for all phone calls, but e-mail and snail mail are still gratis.

I have re-read Jan's blurb from AASP Newsletter of Oct., 1989. Ye gods, it read as if I never heard of it before. Hope that's not a trend! Actually, reading it afresh leaves me somewhat confused, even though I note that Jan acknowledges my assistance in his deliberations. The current, Tokyo Code offers no relevant info in the pertinent Article, now bearing the number 62, "gender of generic names." I mean that -is names are not specifically dealt with. -ites names are mentioned--they must be masculine. (Even if, as in Carpites, they are for a feminine organ.) Jan's dependence on "botanical tradition" I believe is not pertinent, as the Code is clearly not talking about such matters as the gender of pollen, but things such as that traditionally trees are feminine (I suppose because arbor is feminine) and the like. (I note that JJ says in the article that "most names of trees....are feminine," but as far as I know, all tree names are feminine.)

I believe that names ending in -sporis or -pollis, according to the present Code, should be given the gender that the original authors gave them. That's what the Code says to do. Personally if I were making up a name with those endings I would treat them as feminines (even though I haven't found either one in my three Latin dictionaries), just because they resemble in form a bunch of feminine Latin nouns, and because practically all botanical generic names ending in -is are treated as feminine.

In any case, to deal with your specific question, you should give endings to new trivial names that accord with the usage of the original authors of the generic names unless you buy the package that there is botanical tradition one way or the other. I agree with Jan that it would be desirable for all names ending with these two "words" to have the same gender, but in my opinion the

only way to bring this about would be for one of us to offer a proposal to the next nomenclatural session (St. Louis, 2000, I think) to amend the Code by adding the statement that such names are always feminine, or masculine--I don't think it would be helpful to have any provision that they are "masculine if microspores or pollen....etc." Confusing. Make them all the same gender, willy-nilly.

Enclosed are a few reprints that your card says you don't yet have.

All the best.

Yours very truly,

Alfred Traverse

encl.:reprints
c:JJ

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E-Mail: traverse@ems.psu.edu

6 August, 1996

Dr. Norman O. Frederiksen
U. S. Geological Survey
MS 926 A
Reston, VA 22092

Dear Norm:

E-mail has so taken over my correspondence that it's actually nice to have an occasional letter that must be sent out snail mail! Do you all still not have e-mail connection?

This is to thank you for the copy of "Palynomorph biostratigraphy....North Slope of Alaska." Read with interest. Small question: could you let me know what you mean, exactly, by the "soapy water" treatment under methods? I suppose it does what my Darvan treatment does, and that's of interest because the stuff is no longer available, and I should put something in its place in the redo of "Paleopalynology," as I occasionally get a letter from somebody asking about what to do.

All the best.

Yours very truly,

Alfred Traverse

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E-Mail: traverse@ems.psu.edu

10 July, 1995

Dr. N. O. Frederiksen
MS 970, National Center
U. S. Geological Survey
Reston, VA 22092

Dear Norm:

Betty and I are just back from our trip out west. I find yours sent out on 22 June in the pile. Good work! I'll contact "Lady Jane...." (that's actually her e-mail address) for verification that she was the inventor of TCT. I also contacted others, for example Linda Heusser, who nominated others, but you are the only one to come up with a reference. JG's response will be memorable, that's for sure.

Speaking of e-mail, don't you have such capability in Reston? It sure is handy. I've been converted to both it and microwave, but am still holding out on VCR--what's the use of recording things you don't have time to watch anyway?

Yours very truly,

Alfred Traverse

P. S. Betty enjoyed the congrats

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5 February, 1995

Dr. N. O. Frederiksen
USGS National Center
MS 970
Reston, VA 22092

Dear Norm:

Thanks for the reprint of your thorough paper on Paleocene of Pakistan. I had read the paper in Palynology, and had entered the two new genera in my generic index, but it is nice to have a reprint, for example to loan to a student. My palynology course is slated to continue here, despite my technical retirement in July of this year.

Best wishes, as ever.

Yours very truly,

Alfred Traverse

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E-Mail: traverse@ems.psu.edu

31 January, 1995

Dr. N. O. Frederiksen
USGS National Ctr., Stop 970
12201 Sunrise Valley Drive
Reston, VA 22092

Dear Norm:

Thanks for Open-File Report 94-653, which I just read. Imagine vegegation like that at 70 degrees N.!

All the best.

Yours very truly,

Alfred Traverse

THE PENNSYLVANIA STATE UNIVERSITY
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E-Mail: traverse@ems.psu.edu
14 November, 1994

Dr. Norman O. Frederiksen
USGS National Center, Stop 970
Reston, VA 22092

Dear Norm:

Just read the preprint of your paper, "Paleocene floral diversities...models," which you sent a while back. Thought-provoking. I am instinctively suspicious of applying <u>any</u> model based on animal studies to plants (ref. is to Barry, of course), but you make some convincing arguments. Thanks for sending the paper.

Also, in case I didn't do so before, thanks for the xerox of the Visscher and Brugman paper a while back.

Best wishes.

Yours very truly,

Alfred Traverse

encl.:reprint

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E-Mail: traverse@ems.psu.edu
10 October, 1994

Dr. Norman O. Frederiksen
U. S. Geological Survey
National Center, MS 970
Reston, VA 22092

Dear Norm:

Thanks for passing on the information about Norman Hughes. That was a shock. I heard from him not long ago, and there was no mention of serious illness. Betty and I were good friends of Norman and Pamela, and we'll write immediately.

Heavens, we just got over the news about Gerhard Kremp!

By now, you will have another communication from me to answer.

All the best.

Yours very truly,

Alfred Traverse

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3 October, 1994

Dr. Norman O. Frederiksen
USGS National Center, Stop 970
12201 Sunrise Valley Drive
Reston, VA 22092

Dear Norm:

Thanks for "Paleocene floral diversities..." from RPP, which I read this morning. Interesting ideas, to say the least.

Just checked your card and can hardly believe it's right about what I've sent you. Just in case it is, I enclose a few more reprints, which may or may not be of interest.

In the USGS "New Publications" blurb for June, 1994, I just noticed that you put out a contribution that I definitely should read. Hi-lited xerox enclosed. Does your record show that you already sent it? I have a box of reprints at home waiting to be read, and it could be there.

All the best. We just returned from Spain, where I gave a keynote address at the Spanish-speaking palynology conference. Naturally, not in Spanish!

Yours very truly,

Alfred Traverse

encl.:reprints

United States Department of the Interior

GEOLOGICAL SURVEY

RESTON, VA 22092

TAKE PRIDE IN AMERICA

Mail stop 970
August 26, 1993

Dr. Alfred Traverse
Dept. of Geosciences
435 Deike Building
Pennsylvania State University
University Park, PA 16802

Dear Al --

Thanks for the photos of Mom. spackmanianus. One, they show more annulus than the holotype, and two, so-called endoplicae are sometimes present, or triradiate folds may be present. In short, the same sort of variation as Mom.? annulatus. Enclosed is what I wrote for my paper on Eocene Juglandaceae after talking with you about Mom. spackmanianus. However, in light of the photos, I think it is safe to say the two species are conspecific. I think it would be good for you to include one or two photos of these triradiate specimens in your paper. If you wish to place the two species into synonymy, by all means do so. If not, I will do it. As to whether they should be placed into Plicatopollis or Momipites, I have no firm opinion. Certainly in the sense of Nichols' 1973 Momipites paper, they would belong to Momipites.

Cheers,

Norm

Norman Frederiksen

Genus Plicatopollis Krutzsch 1962
emend. Frederiksen & Christopher 1978

Plicatopollis? spackmanianus group

This complex contains two species:

Plicatopollis? spackmanianus (Traverse) n. comb. (basionym: Engelhardtia spackmaniana Traverse, 1955, p. 44-45, fig. 9[27]).

Plicatopollis? annulatus (Frederiksen and Christopher) n. comb. (basionym: Momipites? annulatus Frederiksen and Christopher, 1978, p. 130-131, pl. 1, figs. 17-21). Additional specimens were illustrated by Frederiksen (1981, fig. 16-4[18-19]).

Remarks: A. Traverse (oral commun., 1993) reexamined the protolog specimens of Plicatopollis? spackmanianus (from the upper Oligocene or lower Miocene of Vermont) and found that about 25 percent of them have at least a faint triradiate endoplicate or plicate mark. About 1/3 of the protolog specimens of Plicatopollis? annulatus (from the upper Oligocene of South Carolina) have triradiate endoplicae or plicae; round or arcuate pseudocolpi ("thin spots" of Frederiksen and Christopher, 1978) are uncommon in this species. Nevertheless, I would like to emphasize the transitional morphological relationship of this species group to typical species of Plicatopollis (in the sense of Frederiksen and Christopher, 1978) by transferring the group's species questionably to Plicatopollis. Whether P.? spackmanianus and P.? annulatus are conspecific is uncertain; P.? annulatus seems to be a very variable species, perhaps more so than P.? spackmanianus.

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24 August, 1993

Dr. Norman O. Frederiksen
U. S. Geological Survey National Center
MS 970
Reston, VA 22092

Dear Norm:

Thanks for the Manum thing and for the useful hint about Zhou. I've written to Wiggins. Potonié's mistake in the ref. is amusing, isn't it? Island means Iceland in German, of course, so it's understandable.

All the best.

Yours very truly,

Alfred Traverse

United States Department of the Interior

GEOLOGICAL SURVEY

RESTON, VA 22092

TAKE PRIDE IN AMERICA

Mail stop 970
August 3, 1993

Dr. Bruce Tiffney
Department of Geological Sciences
University of California
Santa Barbara, CA 93106

Dear Bruce:

Just received your letter of July 30. Yesterday I sent off reviews of your short and long papers. As I explained, since Al is doing all that is necessary pollen-wise for these papers, which is absolutely fine with me, there is no purpose in my being a co-author but am happy to be a reviewer to whatever extent you two would like.

As far as places of publication are concerned, I think the short paper should go to Northeastern Geology, because those readers are the ones who will have seen the original Stockwell and Washington paper. As far as the long paper is concerned, there are various possibilities, RP&P, for example, although I have a prejudice against commercial journals because they are ruining the finances of libraries (says he, who has papers in press in RP&P and Historical Geology, but that is because both are part of conference proceedings).

Well, I am delighted that the Brandon botanical work is being brought up-to-date. Good show!

Cheers,

Norman

Norman Frederiksen

cc: Traverse

COPY

Paleocarpological Laboratory

Department of Geological Sciences
University of California
Santa Barbara, California
93106

Office: 805-893-2959 Fax: 805-893-2314
E-mail: Tiffney@magic.ucsb.edu

July 30th, 1993

Dr. Norman Frederiksen
Paleontology & Stratigraphy
MS 970
US Geological Survey
Reston, VA 22092

Dear Norm:

I just received Al's package with his contribution and his comments on portions of the two papers that I drafted. I wanted to make certain that one thing was "perfectly clear", inasmuch as I am not certain I explained myself in earlier missives.

The shorter of my two drafts " *The Brandon Lignite (Vermont) is of Tertiary, not Cretaceous, Age*" is fairly straightforward. It is designed for Northeastern Geology, and serves only to call the reader's attention to the failure of logic in the earlier Stockwell & Washington paper. To be unkind, I put no real data into it, as I fear Northeastern Geology has a limited circulation.

When all three of us initially considered the second paper (my draft titled "*The Age of the Brandon Lignite, Vermont*"), it was suggested that it be a brief paper for Geology. There is nothing wrong with this idea, but when I sat down to write my "macrofossil" share of it I attempted to provide ALL the information that I could. I wanted to use this as an exercise to explore all the macrofossil contributions to an age determination. The result was far too long for Geology, as Al promptly noticed.

This raises the following question - should (a) I re-draft my portion to fit Geology standards, or (b) should we pick a new place of publication for a longer publication?

This question is mildly influenced by another piece of data. Al informs me that Bill Spackman would like to publish the woods he described from Brandon in his PhD thesis. He enquired if that might be done in the context of a Brandon review paper, including pollen and fruits and seeds. IF this latter paper were to become reality, then there would be a "long" paper in which I could place my lengthy thoughts, favoring a "Geology" venue for the "Age" paper. By the way, this larger paper would be a floristic venture, not a stratigraphic one.

Again, the question the three of us must resolve is where the second paper should be submitted, and therefore, how long it should be. If the consensus is for Geology, then I need to trim and recast my verbiage. If, on the other hand, you find my approach and the data I present to be (in your mind) necessary to support my conclusions, then should we go for a publication that would allow more space? If so, what?

Best wishes,

cc: A. T.

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23 July, 1993

Dr. Norman Frederiksen
U. S. Geological Survey
National Center MS 970
Reston, VA 22092

Dear Norm:

Yours of 19 July just in. Actually, I was about to send you a draft of what I've written, for you to critique it. What I've written is an extensive, free-standing paper, dealing with both palynostratigraphy and systematics. I will appreciate your comments about content, etc., including where I should send it. I was thinking of Review PP, but I suppose they might not take it. Hmmm. You should be getting the thing in a few days.

I owe to you and Bruce galvanizing me into doing this. Actually, I had been planning an even more extensive redo for the 50th anniversary of Traverse 1951.

I am not sure where this leaves us re Bruce's papers. I will get at that also in a few days, and I'll suggest some possibilities.

Thanks for the additional biblio things. Yes, I had received a copy of Groot's Calvert paper. Indeed, I played a prominent role in producing it, although I don't seem to be acknowledged. I believe we processed all the samples. (For which Delaware Survey paid us at cost.) I was struck by how much similarity to the Brandon palynoflora there was.

By odd coincidence I got a letter from D. Mai in the same mail. I enclose a copy. He sent a reprint of several, but not all of the papers to which you have called my attention. I'll write to him for them immediately.

That's it for now. All the best. See you in August, I suppose?

Yours very truly,

Alfred Traverse

c:Tiffney
encl.:letter copy

United States Department of the Interior

GEOLOGICAL SURVEY

RESTON, VA 22092

TAKE PRIDE IN AMERICA

Mail stop 970
July 19, 1993

Dr. Alfred Traverse
Dept. of Geosciences
435 Deike Building
Pennsylvania State University
University Park, PA 16802

Dear Al -- Just got back from a long vacation, so I'm far behind on many things. Bruce sent me copies of both his manuscripts on the Brandon flora. I read both of them, but I haven't added any new material because I didn't know what you had done in the meantime. It isn't clear what you would like to do (except update the Brandon pollen list in terms of modern taxa), so I will wait til I hear from you before attempting any analysis of the pollen. I suppose my contribution is a chronostratigraphic range chart for the pollen taxa you list? But if you want to make a range chart - there aren't that many papers on Oligocene and Neogene pollen from the eastern U.S., and you probably have them all (I enclose a reference by Groot - did you have that one already?) - and do all the pollen data and interpretations for these two papers, that is fine with me; I really have no overriding interest in the Brandon, and the pollen work after all *was* done by you.

With regard to the two papers by Mai, the USGS library has both of them, and maybe your library can get them on loan. I enclose the contents of one of them, on subtropical elements - the reference that Mai himself gave for his own paper was wrong. I have searched his subsequent papers for additional subtropical papers (parts II etc.) but have found none.

As for the 1967 Florenzonen paper, that is probably pretty much out of date by now. I enclose some additional Mai references which I have xerox copies of, which are considerably more recent.

Cheers,

Norm

Norman Frederiksen

cc Tiffney

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

22 June, 1993

Dr. Norman O. Frederiksen
USGS National Center, Stop 970
12201 Sunrise Valley Drive
Reston, VA 22092

Dear Norm:

In the course of present furious Neogene/Paleogene boundary reading catchup I have become aware that neither my personal reprint collection, nor the PSU library, has Mai's various publications, for example the two marked on the enclosure.

Could you loan me for xeroxing these, or other more recent ones that you think would be more helpful to me? If, as Bruce says, you have discussed the BL with him, I should have a look at his various zonations for central Europe. As you know, I treat publications lent for this purpose as first order priority.

I'm really kicking myself around the block, because I met him in Göttingen last year and had a short talk, but missed a perfect opportunity to beg publications.

Thanks for reading, and I hope you can help.

Yours very truly,

Alfred Traverse

encl

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

27 May, 1993

Dr. Norman O. Frederiksen
USGS National Ctr., Stop 970
12201 Sunrise Valley Drive
Reston, VA 22092

Dear Norm:

Under separate cover we are returning the microfilm of the Goldstein thesis. I copied many of the pages, using the antediluvian machine in the library. There were a number of interesting things about it--first, that that thesis would scarcely have qualified for a master's degree here. If that was a doctoral diss., the master's thesis of NOF at PSU should certainly have been worth a doctorate! Second, that the shape of the total flora gives an obvious impression of younger than Brandon--common grass pollen, chenopod pollen, lacking sapotaceae, etc. There are no photos, so I can't judge whether there really were Picea grains or not (he uses the pine/spruce ratio heavily). There certainly was no spruce in the Brandon deposit.

Thanks very much. All the best.

Yours very truly,

Alfred Traverse

Paleocarpological Laboratory

Department of Geological Sciences
University of California
Santa Barbara, California
93106

Office: 805-893-2959 Fax: 805-893- 2314
E-mail: Tiffney@magic.ucsb.edu

March 1, 1993

Dr. Norman Frederiksen
US Geological Survey
MS 970
National Center
Reston, VA
22092

Dear Norm:

A range of things to deal with.

1) Your Abstract.

You conclude it with the observation of the absence of Nypa on the eastern US coast. Guess what... I think Nypa was there. I have a single specimen sent to me by an amateur which is not very good (it lacks cellular detail) but it is very much like Nypa in form and size. I have refrained from weaving a paper around it in the hopes that the amateur might find more specimens in time. If I can find what I did with them, I will send you a slide of the goods. Of course, I am assuming that you are basing your statement on the absence of Nypa pollen,m which (if indeed absent) would be worrisome to my interpretation of this megafossil.

2) Boreotropical routes of connexion.

I am not surprised that the "goods" are missing in your North Slope samples, as it is my understanding that Alaska was at a higher latitude in the early Tertiary than was the North Atlantic land bridge. I can only agree with you that what is needed is higher latitude eastern North American links to check this inference out. I have had my eye out for higher latitude east Asian megafossil reports of early Tertiary age, and have not seen very many - and the few I have actually tracked down and distilled generally look cool temperate. I am certain that there is much that I have missed, but I tend to favor an Atlantic route for warm temperate and evergreen forms, and both an Atlantic and Bering route for cool-temperate deciduous ones. This makes me lean towards an Atlantic route for many of my favorite taxa (e.g. Magnolia, Symplocos, etc.).

By the way, I also would assume (ad hoccing my way out of your question) that the pollen samples you are now getting from the Brito-Arctic province come from the most resistant remnants of what was a more substantial land connection (with more environments!!) in the early Tertiary than what is now left to sample. Hence, I would find your assumption of the islands being "unfriendly" was unlikely. But needless to say, this is all conjecture. By the way again, I REALLY like your plot of directions of migration. Sorry I did not think of it first, but it really is better suited to palynological use, as you have more data points than us megafossil types.

Could you possibly get some pollen samples from some of Wolfe's Paleogene localities further south in Alaska to see how many (and what kinds of) things were going on on Alaska's southern margin?

Now, on to the age of the Brandon. I take your response to mean that you are interested in writing an "Age of the Brandon" paper. So, here we go...

3) Responses to the NE Geology.

I agree that a pithy response should be sent to NE Geology, and a longer paper elsewhere. I do not want to waste any ink in a rag with as limited readership as NE Geology on a matter of wider importance. You speak of the longer paper as "a couple of manuscript pages", but I honestly think that it could reach 8-10 manuscript pages plus references & illustrations without threat of being padded. I would vote that it be configured to Geology and submitted there first. (Unless it grows beyond their size constraints).

As to Al Traverse, I called and spoke with him at length. He would be delighted to join this venture in both of its parts. He is willing to consider an Early Miocene age for the Brandon, but definitely would like to (a) think about the data and (b) play intellectual football with both of us before reaching any final decisions.

All this said, it is time to think about the actual order of business....

4) Order of business.

I will draft an outline of a "response letter" to NE Geology in the next few weeks and send it to you and Al for your collective additions and 2 cents. It will largely focus on debunking the dreadful science and assumptions of the Stockwell and Washington paper, NOT on establishing the age of the Brandon in our views.

I will also draft an outline of uncertain complexity to guide us in preparing a longer (hah) paper. The major portions of this paper will include
(a) A review previous age estimates and pertinent literature.
(b) A discussion of estimates of the age arising from the megafossils both in terms of biostratigraphic context and the paleoclimatic context.
(c) A similar section where you palynologists discuss the age of the Brandon in its biostratigraphic and paleoclimatic context.
(d) A summary/discussion where we pull the information together for a joint conclusion.,

I am not certain when this paragon of logic will hit the computer printer - days or weeks in the future, but it will be forthcoming after the quarter ends here (mid-March). After I have generated the rough drafts, it will be in your court to take the next steps!

Best wishes,

Bruce

Bruce

PS - Do you have an e-mail address? Mine is as above. Al does not have one yet, but is thinking about it. They are useful for manuscript exchanges!

United States Department of the Interior

GEOLOGICAL SURVEY
RESTON, VA 22092

Mail stop 970
June 1, 1992

Dr. Alfred Traverse
c/o Dr. Friedemann Schaarschmidt
Forschungsinstitut Naturmuseum Senckenberg
Senckenberganlage 25
6000 Frankfurt a.M., Germany
Fax 49 69 794 004 37

Dear Al:

Thanks for your reply to my message. I can see that you are going to be really busy for the next several weeks finishing up your work, and even more swamped when you get home. So I will find somebody else to do the review. Poor Schaarschmidt, how awful! Please give him my regards and hopes für eine gute Besserung.

Yours,

Norm

Norman Frederiksen
Fax 703-648-5420

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

FAXED TO; 001-703-648-5420

31 May, 1992

Dr. Norman Frederiksen
U. S. Geological Survey, MS 970
Reston, VA 22092

Dear Norm:

Your fax came while I was at the APP meeting in Göttingen. I would be very pleased to review your paper. There is a small problem about where to send it. I am really here for only about three weeks more, as I go to Stuttgart for four days at the very end of my stay and fly home on 1 July.

There's another problem about which you may have by now already heard. Friedemann Schaarschmidt was operated on for cancer on the day before Good Friday and is now getting daily radiation. He is out of the picture here for quite a while. I do talk to him by phone every day, but I don't think he can realistically be expected to review the paper.

On balance, I suppose it would be best to send the paper to PSU, if you can wait until July. If not, you better get somebody else.

It's very disappointing to learn that you were right here, and we missed you. Of course, we don't hang around one place very long waiting for people to turn up.* We've missed other folks too. Betty's studies (Althochdeutsch) are going well. We reciprocate all the greetings!

Yours very truly,

Alfred Traverse

* Dissr. will be on early medieval subject — she has been going all over getting literature — esp. also interested in Frau Welt stances (Worms + Nürnberg

EMPFÄNGER : 7036485420
SENDER : SNG KUHWALDSTRASSE
DATUM : 01 JUN'92 11:43
DAUER : 00'40
MODUS : STAND
SEIT. : 01
ERGEBNIS : OK

United States Department of the Interior

GEOLOGICAL SURVEY
RESTON, VA 22092

Mail stop 970
May 28, 1992

Dr. Alfred Traverse
c/o Dr. Friedemann Schaarschmidt
Forschungsinstitut Naturmuseum Senckenberg
Senckenberganlage 25
6000 Frankfurt a.M., Germany
Fax 49 69 794 004 37

Dear Al:

I am finishing a first draft of a paper comparing the Eocene floral histories of eastern North America and Western Europe, which developed from a paper I am giving at the North American Paleo Convention in Chicago in June (abstract enclosed). They want to publish the papers in Historical Biology. Would you be willing to review the paper for me? It's only 21 pages long including references. I would also be pleased if Dr. Schaarschmidt could look at it too.

I was in Frankfurt early in January and tried to reach you but wasn't successful. Hope you are having fun. Best regards to your wife.

Cheers,

Norman

Norman Frederiksen
Fax 703-648-5420

DIFFERING HISTORIES OF EOCENE ANGIOSPERM DIVERSITY IN EASTERN NORTH AMERICA AND WESTERN EUROPE: DEPENDENCE ON PALEOGEOGRAPHY

FREDERIKSEN, Norman O., U.S. Geological Survey, MS 970, Reston, VA 22092 U.S.A.

Studies of Eocene angiosperm pollen floras in eastern North America (my work, especially in the eastern Gulf Coast) and western Europe (Boulter, Krutzsch) have shown significant differences in floral diversities between the two regions: in western Europe, maximum diversity was in the early Eocene and it decreased thereafter; in eastern North America, maximum diversity was in the middle part of the middle Eocene. The hypothesis presented here is that paleogeography was an important control on the diversity histories in the two regions: eastern North America was part of a large terrestrial landmass, whereas the terrestrial depositional basins of western Europe were on islands or peninsulas surrounded by the sea. Migrations between eastern and western North America were relatively easy, but migrations within what is now western Europe involved island-hopping, which explains distinct diachroneity of some angiosperm first appearances among different basins there. Western European basins were in contact with a large land mass during late Paleocene time but became isolated and smaller during the middle to late Eocene marine transgression. These changes resulted in decreased genetic exchange and increased probabilities of extinction due to (1) greater competition among species because of a reduced number of niches and (2) presence of small, isolated species populations, leading to local variations in extinctions, which probably explain the observed diachronism of taxon last appearances in different areas of Europe. Terrestrial climatic cooling in western Europe may be linked to decreasing contact between the NW European Tertiary Basin and the warm Tethys Seaway during the middle and late Eocene. In short, some combination of low environmental heterogeneity, geographic isolation, and long-term climatic deterioration probably caused the decrease in angiosperm diversity during the middle and late Eocene in western Europe.

Several factors encouraged increasing or stable diversity in eastern North America but were far less effective in western Europe: (1) Eastern North America underwent greater climatic fluctuations during the Eocene (thus, immigration of taxa with different climatic preferences took place at different times), whereas the islands and peninsulas of western Europe had more uniform, maritime climates. (2) Evolution and immigration of r-selected taxa in eastern North America were favored by distinct dry seasons at certain times during the Eocene and by repeated marine transgressions and regressions that created opportunities for evolution and immigration of r-selected plants on and to freshly exposed coastal plain. In contrast, the predominantly maritime climates of western Europe in the early and middle Eocene favored K-selected plants, which had fewer possibilities for evolution and which had greater difficulty in migrating because island-hopping taxa are mainly r-selected. (3) "Arcto-Tertiary" taxa adapted to cooler climates lived and evolved in the uplands of the Appalachian Mountains, whereas western Europe was relatively flat in the Eocene -- another example of its relative lack of environmental heterogeneity.

Dale Beeson
713 W. Beaver Ave.
State College, PA 16801

7 November 1989

Dr. Norman Fredericksen
U.S. Geological Survey
National Ctr., Stop 970
12201 Sunrise Valley Dr.
Reston, VA 22092

Dear Norm,

I want to let you know how helpful you were in sending me relevant materials last Spring which proved useful in my preparation for the PhD Candidacy Exam at Penn State. One outcome of that exam was to fuel my interest in pursuing palynologic research on paleotropical K-T sections. Alfred Traverse and I would like to study detailed floral transitions across the extinction (event) boundary from such sections (ie. possibly from Venezuela, Central Africa ?).

I would propose to make this study the subject of a doctoral thesis. I believe the potential for some interesting science is there given the lack of such work within about 20 degrees north and south of the paleoequator for that part of the geologic section.

Since you are involved with the upcoming Penrose Conference on "Correlation of Nonmarine Cretaceous Strata," I thought that you may know of people having access to material from K-T tropical sections. Someone who might have and be willing to part with a small amount of sample material closely spaced (centimeter scale) across the boundary interval would be ideal. Of course, all information developed from such research would be made available to the contributor.

I Will be looking forward to hearing from you and hope that all is going well for you and your research.

Yours truly,

Dale Beeson

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

28 July, 1989

Dr. Norman O. Frederiksen
U.S. Geological Survey
National Center, MS 970
Reston, VA 22092

Dear Norm:

Thanks for the reprint, and for the nice acknowledgment in it. Thought of you at APP meeting in Krefeld in May. I was even asked to chair one of the sessions, though my German is far short of yours.

Best.

Yours very truly,

Alfred Traverse

AT/et

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

23 May, 1988

Dr. Norman O. Frederiksen
U.S.G.S.
National Center, MS 970
Reston, VA 22092

Dear Norm:

The form (with enclosed note from BT), and your letter returning the Brandon slide, were in "the pile", as suspected. I have signed the form and return it herewith along with the slide, which as I explained in another note, was a gift. Maybe you can use it again.

Best.

Yours very truly,

Alfred Traverse

AT/et
encl

United States Department of the Interior
GEOLOGICAL SURVEY
RESTON, VA 22092

970 National Center
May 10, 1988

Dr. Alfred Traverse
Dept. of Geosciences
Pennsylvania State Univ.
435 Deike Bldg.
University Park, PA 16802

Dear Al:

Thank you very much for your review of my New England paper. I appreciate your wishing a thumbnail sketch of each assemblage, but I think that would be hard to do because one could only list some of the main groups and some species that wouldn't mean much to the readers of Rhodora. But your comment does point up the fact, which I tend to take for granted but should be noted in the paper, that most work, especially palynological, has been for biostratigraphic purposes, and therefore, especially for the older floras, little attention has been paid to spores and gymnosperms.

It was interesting to me that the questions asked by the audience after my talk had to do with the concept of reworking and that you could get pollen of several ages all in one sample - they were intrigued by that, but it means I have to explain in the paper a little about what reworking is and what it means.

Thanks also for the slide, which I return. It made a nice addition to the maps and charts I showed.

Cheers,

Norm

Norman Frederiksen

United States Department of the Interior
GEOLOGICAL SURVEY
RESTON, VA 22092

970 National Center
April 29, 1988

Dr. Alfred Traverse
Dept. of Geosciences
Pennsylvania State Univ.
435 Deike Bldg.
University Park, PA 16802

Dear Al:

I received your review of my manuscript. Thank you very much for your comments and for finishing it so promptly. I also received the slide of you and Barghoorn burrowing into the lignite - thanks, it will make a nice slide for the talk. I'll return it in a couple of weeks, after the talk. I must say you don't look any different than in 1949 - amazing!

See you in Brisbane.

Cheers,

Norm

Norman Frederiksen

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

25 April, 1988

Dr. Norman Frederiksen
U.S. Geological Survey
National Center, MS 970
Reston, VA 22092

Dear Norm:

I have found a spare hour or so of a Sun. p.m. to "do" your very interesting New England paper. I find only a few things I'd like you to consider. I wrote 'em in the MS.

However, I think you need an additional table listing:

1. all of the localities, with a precise literature reference, from which the places can be found;
2. the age for each deposit;
3. (optional, but I'd like it) a very brief (10 words or less) caricature of the flora at each locality. This would be a little work, but you are in a position to do it, and it would be very helpful. (Fig. B is too general for this purpose; I think it would even be helped by my suggested new table.)

I wonder if you shouldn't include at least a passing reference to the Pond Bank lignite of PA? (Tschudy, 1965). "New England" is a political accident, and central PA is hardly any farther from central Vermont than is Nantucket Island.

The MS is enclosed.

Yours very truly,

Alfred Traverse

P.S. The sign-off sheet from BT hasn't come, but I'll take care of it when it does.

AT/et

United States Department of the Interior

GEOLOGICAL SURVEY
RESTON, VA. 22092

970 National Center
April 6, 1988

Dr. Alfred Traverse
Dept. of Geosciences
Pennsylvania State Univ.
435 Deike Bldg.
University Park, PA 16802

Dear Al:

Enclosed is my New England manuscript for review. Better to get the criticisms now than later! Feel free to scribble on the manuscript to your heart's content. You will find that the figures and tables have been given arbitrary letter designators for the time being.

I sent the sign-off sheet for reviewers to Bruce Tiffney; he will send it on to you, and I would appreciate it if you would return it to me. Thanks a lot.

Elke says to thank Betty very much for the Grillparzer paper. She hasn't had a chance to look at it in detail, but it will be useful when she gets back to Grillparzer - right now, she is working on the combination of Heine and Rahel Varnhagen.

Cheers,

Norm

Norman Frederiksen

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

4 April, 1988

Dr. Norman O. Frederiksen
United States Geological Survey
970 National Center
Reston, VA 22092

Dear Norm:

Here's the promised slide--perhaps it's not what you wanted--but if you coupled it with a slide made from the sketch in my USBM-RI5151 (1955), viewers could easily visualize the situation.

Best.

Yours very truly,

Alfred Traverse

AT/et
encl: slide

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

30 March, 1988

Dr. Norman O. Frederiksen
U.S. Geological Survey
National Center, MS 970
Reston, VA 22092

Dear Norm:

The letter of 2 March re NSF-sponsored funding to Australia was very good news! You are to be congratulated on your good offices in politicking for the $. I will remember the luncheon conference with Bill Orr in NYC.

See you in Brisbane if not before.

Yours very truly,

Alfred Traverse

AT/et

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

28 March, 1988

Dr. Norman O. Frederiksen
U.S. Geological Survey
National Center, M.S. 970
Reston, VA 22092

Dear Norm:

Slide follows shortly--from 1949, if that's o.k.

Would be glad to review.

Betty wonders if you ever forwarded her Grillparzer paper to your Ehefrau, as she didn't hear? Only concern was that it might repose in an AT-like stack of unread correspondence.

Best.

Yours very truly,

Alfred Traverse

AT/et

Dear Al - I'm giving a talk on the floral history of New England at Harvard on May 7. Could you lend me a 35 mm. Kodachrome of the Brandon pit, please? Thanks a lot. 3/17/88

Cheers, Norm Fredericksen

P.S. I'm supposed to send the paper to Rhodora. Would you like to be a reviewer?

24 March, 1986

Dr. Norman O. Frederiksen
U.S. Geological Survey
970 National Center
Reston, VA 22092

Dear Norm:

Thought the enclosed journal on womens' writing in Canada might titillate your wife, who probably doesn't subscribe. Thwow out when finished.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: journal

P.S. Speaking of women writers, I wrote a paper on Catharina Regina von Greiffenberg for my baroque German lit class last semester, and found her fascinating.

United States Department of the Interior

GEOLOGICAL SURVEY
RESTON, VA. 22092

970 National Center
November 27, 1985

Dr. Alfred Traverse
Geology Dept.
Pennsylvania State University
University Park, PA 16802

Dear Al:

In reply to your letter of 20 November:

1. I just returned from Denver where Doug, Tschudy, and I had somewhat of a go-round on the Proteacidites thing. The Denver people are somewhat provincial in their outlook, because they rarely see Proteacidites in their samples above the Cretaceous, so when they see an assemblage with Prot. they more or less automatically call it Cretaceous. In California, on the other hand, Prot. routinely occurs at least to the top of the Paleocene, and I very much doubt that it is reworked in these samples because whereas the Upper Cretaceous contains several distinct species of Prot., in the Paleocene you only see one. I suppose they are now more or less convinced that Prot. occurs in the Paleocene in California (more convincing than my word on it, no doubt, is the fact that Warren Drugg in his published dissertation found Prot. in scattered samples in the Paleocene of the Moreno Formation).

2. Doug and I haven't discussed the problem of Ulmipollenites undulosus vs. U. krempii; I suppose we will have to one of these days because we are putting together a joint paper on the geographic distribution of sporomorph species on either side of the K/T boundary in North America.

3. I have no particular axe to grind as regards whether it should be mcgregorii or macgregorii, and cheerfully accept your interpretation of the Code in this regard as having settled the matter.

It's good to see the book coming down the home stretch, down a smooth rather than bumpy road, hopefully.

Cheers,

Norm

Norman Frederiksen

Correspondence

PS Form 3811, July 1983

SENDER: Complete items 1, 2, 3 and 4.

Put your address in the "RETURN TO" space on the reverse side. Failure to do this will prevent this card from being returned to you. The return receipt fee will provide you the name of the person delivered to and the date of delivery. For additional fees the following services are available. Consult postmaster for fees and check box(es) for service(s) requested.

1. ☐ Show to whom, date and address of delivery.
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N.O. Frederiksen
USGS National Ctr., Stop 900
Reston, VA 22092

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5. Signature – Addressee
X N. Frederiksen

6. Signature – Agent
X

7. Date of Delivery
6/25/85

8. Addressee's Address *(ONLY if requested and fee paid)*

DOMESTIC RETURN RECEIPT

20 June, 1985

Dr. Norman O. Frederiksen
USGS National Center, Stop 970
Reston, VA 22092

Dear Norm:

Many thanks for your help--I'll probably ask you to look at the final plate after a little while.

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: negatives

* * * SPECIAL GEOLOGY SEMINAR * * *

"Quantitative Biostratigraphy"

Dr. Norman Frederiksen
USGS National Center
Reston, Virginia

Monday
April 15, 1985
4:00 P.M.
341 Deike Building

26 March, 1985

Dr. Norman O. Frederiksen
USGS National Center
MS 970
Reston, VA 22092

Dear Norm:

Just to confirm that you will speak to a special departmental seminar, at 4:00 p.m., Monday, 15 April, on "Quantitative Biostratigraphy", in Deike 341.

Betty and I will expect your arrival at "Alphabet" on Sunday, 14 April, about 7:00 p;m. Map Enclosed. (Do not let Norrie talk you into a different route!--this is the best, unless there's snow, in which case you'd stay on I-81 to Harrisburg and take US 322 to Lewistown, US 22 to Huntingdon, etc.)

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl

7 December, 1984

Dr. Norman O. Frederiksen
U.S. Geological Survey
970 National Center
Reston, VA 22092

Dear Norm:

Your letter dated 19 November (!!?) just arrived on my desk. Even Calgary to here does better than that!

As I said over the phone, I'm more than happy to cooperate with NF. Only reservation is that I'm a guy who is saddled with 5 grad students and a post-doc, plus NSF projects, plus 2 courses per semester to teach (with labs, etc.), so I obviously don't have too much spare time! I have to be careful not to promise what I can't possibly deliver.

You ask for a recommendation of a basin with good spores for you to concentrate on. I believe the answer is obviously the Newark-Gettysburg-Culpepper Basin, and it has the advantage of being entirely within 4 hours drive from you, mostly much less. The Hartford Basin would work too, and those are the only two that would make the slightest sense, given your directives!

I think, yes, a visit here would be productive--in the Spring Semester. Maybe I could con you into giving a talk to assembled palynologists on something or other--whatever you like? That would be helpful to the program here.

If you like, you are most welcome to stay with us, but if your travel funds are ±unlimited, you might enjoy being on campus, at the Nittany Lion Inn. Up to you. We can negotiate the date over the phone.

Best wishes, especially for the season.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

United States Department of the Interior

GEOLOGICAL SURVEY
RESTON, VA. 22092

970 National Center
November 19, 1984

Dr. Alfred Traverse
Dept. of Geosciences
Pennsylvania State University
435 Deike Bldg.
University Park, PA 16802

Dear Al:

Hope you made your class on time!

As I mentioned on the phone, I have been assigned to work half-time this year on the Eastern Early Mesozoic Basins Project. However, since you and your students are doing the biostrat., and I am not at all anxious to learn a whole new set of pollen grains, what I would like to concentrate on is TAI's. Parenthetically, I might add that Al Froehlich, who is chief of the EEMB project here, is dying to have me do sporomorph biostratigraphy. Maybe if you talk with him sometime about your work and mine you can mention that doing Triassic-Jurassic palynology is very difficult and specialized and I probably couldn't make much of a contribution biostrat-wise very soon, therefore I'm better off doing TAI's etc. etc.

Alternatively, or in addition, it occurs to me to ask, since you have a lot of experience with the Triassic-Jurassic, whether I could learn to identify maybe 20 species of spores and pollen that are the main guide fossils and thus could give at least quick and dirty age picks for some of Freohlich's samples. What I do not want to get involved with is learning 100 species; I simply don't have time for that, especially since I don't know how long I will be on this project - possibly only for this one year.

What I am most interested in doing on the EEMB Project is to begin with one basin where there is pretty good biostrat. control and where I can get a set of samples that will give a survey of the lateral variation across the basin, as much as possible in rocks of the same age, and also a traverse or section from oldest to youngest rocks within the basin to see what happens stratigraphically. Also, of course, it has to be a basin in which the baking is not too bad; I don't mind reasonable browns, but I gather that in some basins (like Norrie's) the pollen is mostly dark brown to black.

So what I am asking is, which basin do you think is the most suitable for such an analysis? I am not worried about going out

collecting samples, but of course if you have some I could get a cut of, that would be dandy. Anyway, why don't you think about it (no hurry, I also have to look at some samples that people here have collected and on which vitrinite reflectances have been run). As I say, I don't know whether I will be on this project more than a year, but I would like to get some results to keep them at bay for the present year.

Good to see you in Arlington. I hope your work goes well, and it would be fun to work with you on some samples from these rocks. I have some travel money for this project, so I could come up there for a few days if you think it's worthwhile.

Cheers,

Norman

Norman Frederiksen

5 November, 1984

Dr. Norman O. Frederiksen
USGS National Ctr., Stop 970
12201 Sunrise Valley Drive
Reston, VA 22092

Dear Norm:

I finally solved the problem of who it was who has a place on the Gaspé--Matt Klare--but he was not on the field trip. I must have met him elsewhere during the meeting. But thanks for the list anyhow!

Best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Nov. 28, 1983

Dear Al -- Thank you for the price quote for Duck's dissertation. We are authorized to pay it, so we will be glad to get it when you can get it done. Reason another USGS type could speak at Penn State and I couldn't is that some branches are flush, and ours is broke. Hope we do get in some money during the year, or it's going to be a long year.

Cheers,

Norm Frederiksen

16 November, 1983

Dr. Norman O. Frederiksen
USGS National Center
Stop 970
Reston, VA 22092

Dear Norm:

Duck's thesis is about 500 pages, so xeroxing would be about $25.00, and the plates could be printed by us at cost (45 plates, an additional $25.00). So, the bottom line is, it'd be expensive: about $50.00. There's another catch--Duck apparently took the plate negatives with him to Korea. I have an urgent message out to him to return them. I expect he'll oblige me on this. So, for $50.00, you could order a copy from us, and have your very own, with original-quality plates.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Nov. 2, 1983

Dear Al:

Just a note to remind you to send us an estimate of xeroxing and plate reproduction charges for Duck Choi's dissertation.

Sorry my coming didn't work out for November. Hope something can be arranged for the Spring; I would enjoy meeting your students.

Cheers,

Norm Frederiksen

10 October, 1983

Alfred Traverse, 435DDeike

Science Seminars, 210 Whitmore Lab

For week of November 14 - 18, 1983

Thursday, November 17

Palynology Dr. Norman O. Frederiksen (USGS National Center, Reston, VA), "Cenozoic palynological studies in southern California". 11:15 a.m., 337 Deike Building. Dr. Traverse, 3-3419.

10 October, 1983

Dr. Norman O Frederiksen
USGS National Center
MS 970
Reston, VA 22092

Dear Norm:

Just a confirming line that we are expecting you for the evening of 16 November at our home, to give a lecture on 17 November in 337 Deike, on "Cenozoic palynological studies in southern California".

I have sent the title in to the weekly "Science Seminars" newsletter for PSU, so you may well get a few hearers in addition to the 12 in Geosc (Biol) 423--perhaps 20-25 or so. It is to be hoped you can visit with graduate students informally during the 17th. We would expect that you'd stay at our place again that night and return to DC-area on the 18th. We may have some sort of informal party somewhere along the line.

To get to our place, take I-270 to I-70 (Frederick area); I-70 to I-81 (Hagerstown);I-81 to PA 16 (near Greencastle); PA 16 to PA 522 (McConnellsburg); PA 522 to US 22(Mt. Union); US 22 to PA 26 (Huntingdon); then follow the enclosed map. Note that it's exactly 7.0 miles from the place you pick up PA 26, near the courthouse in Huntingdon, to our place.

Betty and I, and all the students greatly look forward to this. Isn't it great that it happens just by luck oofall in the slot for Paleogene in my lecture schedule!?

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: 1)course outline, 2)map

9 September, 1983

Dr. Norman O. Frederiksen'
U.S. Geological Survey
National Center, Stop 970
Reston, VA 22092

Dear Norm:

Here's my "P.S."

P.S. I just read Doug Nichols' statement in the July Newsletter that the definition of "sporomorph" in the AGI Glossary is wrong. He appeals to original sources. The meaning of common terms depends, however, on usage. Nichols himself uses "palynology" in the broader, now accepted sense, not per Hyde & Williams 1944. Whether a definition of a common term agrees with an Erdtman publication of 40 years ago is of interest but is not compelling.

Let me know if the Choi thesis loan didn't work out.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

6 September, 1983

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Per recent phone conversation, I'd greatly appreciate a suite (whatever you could spare) of good glossy prints of Cenozoic sporomorphs (ahem) for my textbook. I would need size and/or magnification and geographic-stratigraphic position. I could use and return negatives if you'd prefer. Prints I could keep would be great.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

AMERICAN ASSOCIATION OF STRATIGRAPHIC PALYNOLOGISTS, INC.

970 National Center
U.S. Geological Survey, MS 970
Reston, VA 22092
July 28, 1983

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

Dear Al:

Thank you for your Comment for the Newsletter. Unfortunately, as you will see when you get the July issue, your comment didn't get into it. What happened was that the whole issue got fouled up. Vaughn said he would print and postage this issue, to save money (those damned tightwads on the Board of Directors again!). But he was going to leave College Station the middle of July, so the pasted-up Newsletter had to be there before then. But the end of June I had to go into the hospital, so the only thing that could be sent him was what already happened to have been typed up by then. So the upshot is that the July issue is skimpy and a lot of good stuff got left out, including your Comment. Sorry, but it will be in the October issue for sure.

Cheers,

Norm

Norman Frederiksen

for "comment" section of AASP Newsletter

Despite my frequent appearance in this column lately, perhaps I could be allowed a few lines more, in order to comment about "sporomorph" and/or "palynomorph". As the writer of the definitions of these two words in the 1972 AGI Glossary (kept unchanged in the present edition, in which the palynological definitions are credited to Kosanke), I have thought quite a bit about these terms. It seems to me that clearly both have a place and that neither should be merged in the other. We need a less bulky term for "spores and pollen", and as John Grayson's "polospores" never caught on, "sporomorph" does very nicely for this; (fossil pollen grains are, after all, microspore-walls). On the other hand, we also need an all-inclusive term for spores/pollen, chitinozoans, dinoflagellate cysts, the works. "Palynomorph" suits here, and that's the way most palynologists use it. I once had a letter from E. D. Zaklinskaya, protesting the use of "palynology" for anything but spores/pollen. Literally interpreted, she's quite right--παλυνοσ in the word "palynology" refers to pollen. I presume that a similarly purist view could object to "palynomorph" when applied to dinoflagellate cysts, but paleopalynology is now almost universally understood to mean study of all of the acid-resistant, organic-walled microscopic entities (±5-500μm) that turn up in macerations of sedimentary rock; (that excludes nannofossils and diatoms but includes most megaspores). All sporomorphs are palynomorphs, but not all palynomorphs are sporomorphs!

Alfred Traverse
Professor of Palynology
Pennsylvania State University
Department of Geosciences
University Park, PA 16802

AMERICAN ASSOCIATION OF STRATIGRAPHIC PALYNOLOGISTS, INC.

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MANAGING EDITOR: Vaughn M. Bryant, Jr.
Anthropology Department
Texas A&M University
College Station, Texas 77843

June 13, 1983
970 National Center
U.S. Geological Survey
Reston, VA 22092

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

Dear Al:

Thank you for your comment about sporomorph vs. palynomorph. However, I wonder if you would also like to comment about what my original remark was about in the April Newsletter, that is, the question whether sporomorph (and, by implication, palynomorph) refers to a specimen or to a taxon. Particularly if you wrote the definitions of these terms in the AGI Glossary, in which these terms are defined to mean only a specimen, I would be interested in why the terms can't also mean a taxon, as Doug thought. (I sent the remark to Judi to see if she wanted to comment, which she did, which shows up in the April Newsletter. But she commented on whether one should use sporomorph or palynomorph from the point of view of the what kinds of critters each contains, rather than answer the question whether the terms mean a specimen or a taxon, or both.)

Cheers,

Norm

Norman Frederiksen

for "comment" section of AASP Newsletter:

Despite my frequent appearance in this column lately, perhaps I could be allowed a few lines more, in order to comment about "sporomorph" and "palynomorph". As the writer of the definitions of these two words in the 1972 AGI Glossary (kept unchanged in the present edition, for which the palynological editor is R. M. Kosanke), I have thought quite a bit about these terms. It seems to me that clearly both have a place and that neither should be merged in the other. We need a less bulky term for "spores and pollen". John Grayson's "polospores" never caught on. Joe Guennel's "miospores" has two drawbacks: 1. the 200 micron limit often doesn't work well, and I don't like to exclude megaspores anyway.

2. Miospore is a later homonym of the biologically important and well established "meiospore". "Sporomorph" does very nicely. Fossil pollen grains are, after all, microspore walls. On the other hand, we also need an all-inclusive term for spores/pollen, chitinozoans, dinoflagellate cysts, the works. "Palynomorph" is handy here, and that's the way most palynologists use it. I once had a letter from E. D. Zaklinskaya, protesting the use of "palynology" for anything but, as I would say, sporomorphs. Literally, she was quite right: the Greek word behind "palynology" refers to pollen, not scolecodonts. I presume that a similarly purist view would object to "palynomorph" when applied to dinoflagellate cysts, but paleopalynology is now almost universally understood to mean study of all of the acid-resistant, organic -walled microscopic entities (ca. 5-500 microns) that turn up in macerations of sedimentary rock. That excludes nannofossils and diatoms but includes most megaspores. All sporomorphs are palynomorphs, but not all palynomorphs are sporomorphs!

Dr. Frederiksen has asked me to comment also on a question debated intramurally at USGS:

whether the terms "sporomorph" and "palynomorph" should be used for taxa, or for specimens, or for both. I never questioned when writing the abovementioned definitions for AGI that I was defining a term for specimens. Thus, one could speak quite properly of "sporomorph taxa" because a sporomorph is a specimen. However, "sporomorph taxon" or for that matter "sporomorph" used (incorrectly, in my opinion) as a synonym for "sporomorph taxon" gets one into a knotty philosophical problem that I used to debate by the hour with J. M. Schopf. Schopf vehemently asserted that every so-called spore or pollen (=sporomorph) taxon really is a taxon of whole plants: the spore or pollen grain is merely a typifying element for the whole plant. Thus, Jim urged that there is no such thing as a spore or pollen taxon, and he would surely feel the same way about sporomorph used to mean such a taxon. I always viewed this opinion of Schopf's as quixotic, but he was a taxonomist's taxonomist, and there are others who share this idea. I mention the matter only to emphasize how complicated this seemingly simple question is. In any event, palynologists sometimes do speak loosely and informally of sporomorph as equivalent to spore or pollen form-taxon: "Sporomorphs such as Densosporites and Corollina...." However, I believe it is better to avoid this and refer to Densosporites very explicitly as a form-genus. Sporomorph should be used for less precise reference to specimens: "Many sporomorphs were encountered on the slide...." Of course, it is unreasonable to expect to be rigorous about this in practice, and the subject is now even more complicated: Under the new (Sydney) ICBN, the type of Densosporites will be a specimen, that is a single spore! This means that even the form-generic concept for Densosporites will be tied to a single specimen, not a population (the type species) as before. Obviously, even the concept of the form-genus is somewhat affected by this big change in typification. The nasty problem of neotypification will come up certainly--where is the type specimen of the type species of Densosporites? But to get back to the main point--I'd like to retain both "sporomorph" and "palynomorph" and to use each for specimens, free of the various complications inherent in the taxon concept.

2 June, 1983

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Self-explanatory blurb enclosed.

Hope you are flourishing. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl

21 April, 1983

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

How embarrassing (p.3, current AASP NL)! Roger Jan du Chêne is Secretary of ICP, Caratini (who is pretty sensitive) is President. Abject apologies in French and English are in order.

Best wishes (you'll need 'em)!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

P.S. This letter is obviously not for publication!

Mar. 3, 1983

Dear Al: Thank you for your Comment for the Newsletter. One can anticipate some nice yelling and screaming in your section at the IPC!

CC

Cheers,

Norm (Fredericksen)

28 February, 1983

Dr. Norman O. Frederiksen
US Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

I just read my Oct. AASP Newsletter (have been in Germany the month of January), and would very much appreciate your including the enclosed "comment" in the next available slot. Many thanks!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl

Sept. 8, 1982

Dear Al: Thank you very much for your letter of appreciation and support. I presume that much of your enthusiasm stems from the apparent fact that you and Chaloner are clearly having the better of it in your little disagreement with Sarjeant???

Cheers,

Tom

Fredericksen

27 August, 1982

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Just a fan letter to let you know what a super job I think you're doing with the AASP Newsletter. One can hardly wait until the next number arrives! I hope you find it suffieiently rewarding that you will be able to continue it for a long time.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

21 June, 1982

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

I just got a copy of Bill Chaloner's statement (letter of 9 June) re Sarjeant. I do hope you'll publish it as nearly in full as possible.

Best as ever.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

Mar. 5, 1982

Dear Al -- Thank you for your letter of 1 March for the Newsletter. I sent a copy to Sarjeant to see if he wanted to reply. No doubt he'll have something to say!

Cheers,

Norm

1 March, 1982

Dr. Norman O. Frederiksen
Editor, AASP Newsletter
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Bill Sarjeant on p. 6 of the current (15:1) Newsletter says, "...the problems of trying to classify dinoflagellate cysts under the existing International Code of Botanical Nomenclature, and the greater problems likely to be encountered when that Code is revised by taxonomists with little appreciation of the problems either of planktonologists or of palynologists...." Although I have great respect for Bill's contributions to our science, I feel I must correct these statements. First, the Code has nothing (well, almost nothing) to do with how one "classifies" dinoflagellate cysts, fossil pollen, or anything else. The Code is only a rule book for the application of names, telling how names should be compared, which techniques of publication make names validly published, whether this or that name is legitimate, what is the type for a name, etc. The only way this impinges at all on classification is that an initial decision must be made--is a dinoflagellate cyst a plant remain? If it is so considered, then the rules about synonymy, priority, and all that, apply, if latinized, botanical-style binomial names are used for dinoflagellate cysts. That one decision, "Is a dino a plant?" is a taxonomic (classificatory) question. But aside from that, the workings of the Code are strictly about the technical matters of naming and the handling and application of (correct) names. Secondly, Bill has apparently not been aware that palynologists-planktonologists are involved in the housekeeping of the ICBN. The Chairman of the Committee for Fossil Plants of IAPT, and the Secretary thereof, are both palynologists. One member of the Committee (W.R. Evitt) is a dino specialist. The Chairman of the Committee (W.G. Chaloner) is also on the small Editorial Committee of IAPT that is writing the "Sydney Code". Both the Chairman and the Secretary played significant roles in the debates which led up to the new Code. No matter what classificatory scheme may be developed for dinoflagellate cysts, the entities in that classification will have names (A-1-65?; or whatever). If latinized binomial nomenclature is used in the botanical fashion for them, the Code tells one how to do it, but the Code has only to do with the naming, not with the classifying. Furthermore, there is no obligation at all to use the botanical sort of names--use letters, symbols, or whatever, if you like and can find others to read about it. It's only that if one does purport to use botanical-style binomial nomenclature, the Code is the accepted way to do it.

Yours very truly,

AT/et

Alfred Traverse
Professor of Palynology

xxxxxx
863-3419

21 December, 1981

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Many thanks for the kind words in yours of 14 December. I was concerned about the length of the article, but I was eager to communicate some interesting and some important matters, as you recognized.

Your letter expresses a widespread concern about the Berlin Congress: field trips. There is some wistful hoping that East Germany may eventually warm up to the idea of a Congress. But the majority opinion is that the Congress is meeting in Western Europe, and the field trips can be in Austria, Denmark, Switzerland, etc. (Both National Geographic and the New Yorker have fascinating recent articles on Berlin which whet my appetite!)

Best wishes for the holidays.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: copy of letter to M.C. Boulter

Dec. 14, 1981

Dr. Alfred Traverse
Dept. of Geosciences
Pennsylvania State University
University Park, Pa. 16802

Dear Al:

Thank you for your article on the Sydney meeting. It was longer than I anticipated; I calculate it will take up about 3½ pages of the AASP Newsletter. But as usual it is interesting, useful, and witty, and I think it is worth printing in full. Sounds like a marvellous meeting. Hope I can get to the Berlin one - what are they going to do for field trips, look at ruts that our tanks have dug up running around in the Grunewald?

Merry Christmas!

Cheers,

Norm

Norman Frederiksen

xxxxx
863-3419

11 December, 1981

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Here is the report on the Congress. I haven't counted words, but I note that it runs somewhere in the 2500-3000 word range. If that is too long, please phone me as soon as possible (note new phone number), as I would rather try to get into some other newsletter than to cut it significantly.

Best for the holidays. And a happy and prosperous 1982 to you!

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: MS

28 January, 1980

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

I am reading your two papers "recently" sent me--and enjoying them very much. The Virginia Paleogene paper will certainly be useful to many people, what with ranges being given and all. Regarding 'locations on mixed slides' , about which we both have bothered--have you tried an "England Finder"--I find it works so well that we should just adopt it and abandon all the other complexities.

About the Pollenites paper, I suppose I'll have to respond eventually because I am formally acknowledged on p. 1, but unfortunately you did not (as I think you should have) indicate how DHN, AC, and AT "voted".

There are, in my opinion, fatal flaws in your argument. So, no, old friend and delightful dinner companion, I will not agree to disagree!

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

23 January, 1979

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Thanks for yours of 11 January. Don't be put off by the opinions of Nicolson and Cronquist. I am a member of the general committee of I.A.P.T., and we frequently have to vote on whether certain generic names should be regarded as homonyms within the meaning of the Code. You would think that would be a fairly straightforward matter, but the votes are frequently on the order of 6-5 or 7-4, indicating that opinions can differ even on such seemingly concrete matters. I would be willing to argue vehicly with Nicolson and Cronquist, if necessary, that you have made a very strong series of points, especially with regard to non-acceptance. Anyhow, as I'm sure you realize, under the botanical Rules it's all a question of market acceptance. If most people agree with you, that's the way it goes. In zoology, on the other hand, the nomenclatural commission would eventually have a session and vote for or against your proposition. As an example of what I mean, I would cite the generic name, Corollina. I don't think there's a question in the world but what a commission would vote that that name should be used instead of Classopollis. However, a very substantial majority of people who work with the things persist in using Classopollis, and so in effect it is the "correct" name by virtue of market acceptance.

Best wishes to you as always, old friend.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

United States Department of the Interior

GEOLOGICAL SURVEY
RESTON, VIRGINIA 22092

January 11, 1979

Dr. Alfred Traverse
Department of Geosciences
Palynological Laboratories
Pennsylvania State University
University Park, Pa. 16802

Dear Al:

Thank you very much for your review of the paper on Pollenites. Alas, the situation is getting worse instead of better. Although the paper comes to the conclusion that Pollenites was never validly published, two of the three reviewers, Dan Nicolson and Arthur Cronquist, disagreed and said that only unimportant technicalities or misinterpretations of the Code manifested by us in the paper have allowed us to come to the conclusion of invalid publication of the genus. In short, I'm sorry I ever got involved with this mess, but I think I'll make one more try and send it to Taxon and see what they say.

Cheers,

Norm

Norman Frederiksen

3 January, 1979

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Review of your paper enclosed. I believe the points raised are important and your attention to them will help produce a better paper. You need some rewriting--especially of the priority section. I believe my suggestion that you have a figure or figures in which the Potonie squibs are provided for the readers is a very important one. Etc.

All the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: review of paper

Frederiksen, review of paper

p. 3, para. 2, l. 7:

A minuscule point--I object to the split infinitive

p. 3, para. 2, l. 13: Names are not "valid", but "validly published". You have made this error throughout.

p. 3, para. 2, end (and many other places): the question of valid publication (not "validity") is one thing, and the "usefulness" of the generic concept is another matter. Your paper is about valid publication of the name, and you should not muddy the water by getting into taxonomic or other"pragmatic"questions.

throughout: another nit-picker--you should render Potonié with the accent mark always.

pp. 3-5: It is not at all clear to the reader why you bring up the priority question. You don't explain why it is a "source of confusion". I think I know, but this section really needs reworking and expansion to make it all "scan". What you're getting at, I suppose, is that if Potonié happened to introduce Pollenites in a paper where there is only one P. species, he might accidentally have been in business vis-a-vis description via monotyping. One interpretation of the dates, as I recall, has that result!

p. 7: For me, the question of whether "pollen grain" is a permissible description is critical, though it seems not to be to others. That's because I'm a botanist and know that "pollen" is a term having to do with function not (never!) form, "morphology" or whatever. You simply can't describe a thing by saying it's a "pollen grain", if the purpose is to distinguish it from "spore". If one were dealing with fossil "spores" that's a different question (because the definition would include fossil pollen exines). The statement on your p. 7 that "...Pollenites were pollen grains and not spores...." is not "wrong" but simply meaningless from a morphographic point of view.

p. 12, l. 4: "...had validity...." same sort of comment as before. You should say, "...was validly published and had priority...."

In brief, I believe that you "have 'em" as far as non-description goes, but "non-acceptance" is perhaps stickier. Remember that what Potonié wrote in later years has no compelling standing. It is interesting and has impact, but it is not compelling. On the other hand, if 1931b is the critical

paper, non-acceptance is probably substantialist by his expression "...soll der Genitiv den Gottung vor die Worte...." (underlining mine!) So (aha!)...it is just a "word"!--you should stress that.

Presumally, I agree that it was not accepted, but I think the non-descriptive nature of the thing is even more persuasive. It is even almost certainly against the intent of Art. 20, because "pollen" is a technical term, and the "..ites" doesn't save it, in my opinion. But others will not agree.

A further recommendation to you--why not reproduce as figures the portions of the various Potonié papers that are critical and at issue? That would be very helpful to the readers. I could not have done the work represented by this critique without what you sent from Potonié papers.

United States Department of the Interior
GEOLOGICAL SURVEY
RESTON, VIRGINIA 22092
971 National Center

December 1, 1978

Dr. Alfred Traverse
Geology Dept., 529 Deike Bldg.
Pennsylvania State University
University Park, PA. 16802

Dear Al:

Under separate cover I have sent you Tate's and my manuscript on Pollenites. I would appreciate it very much if you would review it for us and return the review to me so we can make necessary changes, and I can then send it to the Director for Survey approval. Thanks a lot.

Cheers,

Norm

Norman Frederiksen

NOF:gc

United States Department of the Interior
GEOLOGICAL SURVEY
RESTON, VIRGINIA 22092
971 National Center

June 15, 1978

Dr. Alfred Traverse
Dept. of Geosciences
Palynological Laboratories
Pennsylvania State University
University Park, Pa. 16802

Dear Al:

Thank you very much for the two slides of Brandon lignite pollen. It certainly is lovely material. I haven't looked through the slides in detail for Momipites yet, but the few specimens I saw look very much like M. annulatus Fred. & Christ., and I imagine the two species are conspecific. However, the few specimens of M. spackmanianus that I saw did not have the Plicatopollis-like triradiate mark that so struck us about M. annulatus. Perhaps it will be useful, for research purposes, to maintain the two species and only use M. annulatus for specimens, like the type, that have the triradiate mark, and see whether there is any pattern to the ratio between the two species.

Your scuttlebutt that I am unfriendly toward ICP is incorrect. I was among those on the Executive Committee who voted in Tulsa to support it with AASP funds. At the midyear meeting of the Executive Committee this spring, the subject of ICP came up because several vocal members of AASP had squawked that we were supporting a boondoggle. So I made a motion that we should send out a letter to the membership, explaining in as much detail as feasible the history of ICP and of AASP's involvement in it, noting that the Executive Committee was in favor of ICP, and asking the members whether they approved of our support or not. The motion died for lack of a second. Then somebody (I forget who, might have been Graham Williams) said that it was our job as the elected E.C. to decide such things rather than ask the membership about everything that comes up, whereupon the E.C. voted unanimously that we at least favored the concept of the ICP if not all the details of it. So I expect complaints to continue to come in as long as AASP supports ICP, but I also expect that the membership will generally be in favor of it, especially as they become more informed, and that support will continue to be forthcoming.

Cheers,

Norm

Norman Frederiksen

7 June, 1978

Dr. Norman Frederiksen
United States Department of the Interior
Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Thanks for yours of 16 May regarding slides of the Brandon lignite. I am happy to present you with a "couple of slides". The only slides I have ever sold are of modern pollen. I think I explained that to you before, but in brief the reason I sold them is that the usual deal is "exchange", and that simply doesn't help pay the hourly wages of the person or persons who have to manufacture the slidew. I am without any assistance secretarial or other, except my wife, and I just couldn't get into the business of manufacturing hundreds of slides for people unless there was some way of paying a student or whomever to help us. I guess if I were getting frequent requests for Brandon lignite slides I might have to have a similar policy, but for an old friend I certainly am happy that you have them if you can use them. I will look forward to hearing about what you think of E. spackmaniana!

I heard from scuttlebutt that you are not very friendly toward the ICP. All of this has come as a dreadful shock to me since it was the AASP that got me into that thing in the first place by appointing me as their representative on the Council. I am probably somewhat paranoid in reaction of feeling betrayed at this point for the vitriolic mail I have been getting on the subject. Wish you were next door so we could chat about it. I really did enjoy talking to you in Halifax. Very best wishes to you, old friend.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

United States Department of the Interior

GEOLOGICAL SURVEY

971 National Center
Reston, Va. 22092

5/16/78

Dr. Alfred Traverse
Geology Dept.
Pennsylvania State Univ.
University Park, Pa. 16802

Dear Al:

I would like to buy a couple of slides of Brandon lignite pollen. The reason is that I am coming out with a new species, Momipites annulatus, in vol. 2 of Palynology, which I now suspect is a synonym of Engelhardtia spackmaniana, but I need to look at a number of specimens of the latter to be sure. Would it be possible for you to sell me a couple of slides with specimens of this species on them? I could just as well borrow them, but I seem to recall that you prefer to sell rather than lend slides.

Hope you have a good summer.

Cheers,

Norman

Norman Frederiksen

If we have, let's make a couple of slides and I'll have a check against to see if E. spack. or not)

United States Department of the Interior

GEOLOGICAL SURVEY

National Center stop 971
Reston, Va. 22092

Apr1.11, 1977

File Frederiksen

Dr. Robert Tschudy
U. S. Geological Survey
Federal Center
Denver, Colo. 80225

Dear Bob:

Thanks very much for looking at the range chart and making comments. I just finished a short E & R for Bill Olive, on one sample each from the Forest Hill, Bucutunna and Chickasawhay of Miss. and Ala. These were the first samples I had seen of middle and upper Oligocene from the Gulf Coast. I also found Parsonsidites conspicuus in the Chickasawhay. The problem of Momipites is more complicated. In my work on the upper Eocene I split this genus into 2 species, M. coryloides and M. microfoveolatus, though the latter is probably pretty broad. But in the South Carolina material a quite different species appeared, which Christopher and I are naming M.? annulatus. This species has a distinct annulus and is very similar to Traverse's Engelhardtia spackmaniana except that his species is larger. Since then I have seen M.? annulatus in late Eocene lignites of Texas, and also in all Oligocene samples, where it becomes a larger and larger part of total Momipites up-section, and is the dominant species of the genus in my sample of Chickasawhay from Alabama but the only species of the genus from this formation of South Carolina. However, it may be that the species itself changes up-section, so I'm not sure yet exactly what its morphological limits are. But I would like very much to see a couple of Brandon samples to see the variation within E. spackmaniana. Do you have slides of Brandon that you could loan me?

The absolute ages on the Clubhouse Crossroads core are from the forthcoming Prof. Paper on the core by Hazel et al., which we all contributed to.

As to the feminine endings, I note from Traverse's letter that he goes by Recommend. 75A(3), according to which the original gender is what counts. I go by Recommend. 75A(1), according to which essentially all endings of -is are feminine, and which appears to recommend that where the genus ending is -is, the endings of its species should be made feminine. However, the whole thing seems to be somewhat ambiguous.

Thanks again.

Cheers,

Norman

Norman Frederiksen

Copy to Traverse

United States Department of the Interior

GEOLOGICAL SURVEY
Mail stop 971
National Center
Reston, Virginia 22092

September 21, 1976

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

Dear Al:

Thank you very much for your very useful comments. By the way, I have found that once you have a common point on a slide (say, the middle point of a standard size slide), it is easy to convert from one stage scale to another, if the numbers increase to the right on one scope and to the left on the other. You add the coordinates for the two scopes together, and then subtract to get the new coordinates for your scope. For instance, if the X coordinate on the original (author's) scope for the slide midpoint is 15.3, and on your scope it is 33.4, then you add the two together, which equals 48.7. The author says a certain specimen has an X coordinate of 18.1 on his scope; that means it will be 48.7-18.1=30.6 on your scope. Of course, the original author must state which way the numbers increase on his scope, which I did.

Cheers, and see you in Halifax

Norm

Norman Frederiksen

xxxxxx
865-2342

26 October, 1977

Dr. Norman O. Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

As I mentioned to you previously, I am not in a position to give a really thorough, word-by-word review of "Middle Eocene to early Oligocene plant communities...". However, I have looked at it pretty thoroughly and offer the following:

1. I didn't come across many typographical boo-boos, and that's not what you wanted anyway. However, I did note that on page 1, line 15, Ephedra should be italicized.

2. I feel that somewhat more explanation of the rationale for the cluster analysis and other mathematical manipulations is desirable if not absolutely necessary. What were you aiming to find out, etc.? Without such explanation, it seems to be sort of accidental.

3. There should be more discussion at appropriate places, with a summary and preferably some sort of illustration, of the ecological findings--this would be a large part of an explanation to the reader of why the study was undertaken and why it is worth reporting.

4. The abstract should also make clear what came out of the study. I know that there is a little of this in the abstract, but it needs to be sharpened up and pulled together, in my opinion.

5. Apparently systematic treatment will come later, but I think something should be said about it. I certainly also think that some form of illustration would be helpful for the sort of reader you might expect, if this is to be in a professional paper or whatever. How about at least caricatures of the major pollen types in connection with the range charts, along the lines of the work of Döring

in East Germany, and certain other German papers? I would like that, and I confess that I would also like actual photomicrographs of the most critical forms. It would only be a plate or two.

Well, that seems to be about what I have time for at the moment. I think that the provision of the sorts of things I am suggesting would greatly strengthen the paper. Beyond that, I don't really find much wrong with what you presented. It's more a question of a feeling of incompleteness than a feeling of lack of quality. Good to see you in Tulsa, and I much enjoyed your oral presentation. I wish that I could give my paper again. I was totally flabergasted by the self-projection bit, and I will never let that happen to me again! Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

XXXX
865-2342

14 October, 1977

Dr. Norman Frederiksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Thanks for your letter of October 4th and the manuscript which has in the meantime arrived. I hope you will be willing to accept a rather informal review which I will give you in your hand in Tulsa along with the manuscript. I will read it in the car as Betty and I drive along to Oklahoma.

See you down there. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

United States Department of the Interior

GEOLOGICAL SURVEY
971 National Center
Reston, Va. 22092

Oct. 4, 1977

Dr. Alfred Traverse
Dept. of Geosciences
529 Deike Bldg.
Penn. State University
University Park, Pa. 16802

Dear Al:

Under separate cover I have sent you a manuscript on middle Eocene to early Oligocene plant communities of the Gulf Coast, based on pollen and spores. I would appreciate it very much if you would review it for me, a necessary step before I can submit it for Director's approval. Jane Gray said she would include it in her symposium volume for the Paleo Convention, although it was not given at the convention, assuming enough people submit manuscripts to make a volume possible. If you feel you don't have time to work on it, don't hesitate to send it back, but I thought you might be interested in it, and anyway you would seem to be a logical reviewer.

Thanks a lot. See you in Tulsa.

Cheers,

Norman

Norman Frederiksen

THE PENNSYLVANIA STATE UNIVERSITY
DEIKE BUILDING
UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences
Department of Geosciences
Palynological Laboratories

Area Code 814
~~863-0543~~
865-2342

15 September, 1976

Dr. Norman Frederiksen
USGS National Center, Stop 971
12201 Sunrise Valley Drive
Reston, Virginia

Dear Norm:

I have done the best that circumstances permit with your ms. It will be returned in a few days. I have spent a number of hours with it, and I have had Tate Ames and my grad student, John Bebout, also have a go at it. I have not conferred with them re their opinions, as I thought you'd profit most from independent approaches.

My comments are appended, on separate sheets.

The biggest single comment is that more descriptive info is needed to accompany the pictures--even size measurements are few and far between.

Don't get me wrong--the paper will be very valuable and interesting when it's finished. Glück Auf!

Yours very truly,

Alfred Traverse

Alfred Traverse
Professor of Palynology

COMMENTS ON PAPER BY N. FREDERIKSEN

pp. 1-2: Jim Schopf would have a fit!--he would insist that these things of yours are species of plants. I don't go as far as he does, but I admit that a more felicitous expression would be "...species of sporomorph form-genera...." At any rate, "species of sporomorphs" is not good. Even "pollen species" comes across better than "species of pollen".

p. 41. The hundreds of coördinate readings for fossil locations are not as useful as they might be--from many experiences I now realize that when the mechanical stage-readings run in the opposite direction from those on the stage originally used, it's a mess. The best solution is so many mm. and tenths thereof to the right and above the lower left corner of the coverslip--or use an "England Finder" (they really work) or use a slide-marker (probably least equivocal of all)--the outfit costs about $35.

p. 69. Would question use of formal name for a single specimen.

General comment: I object to the almost total lack of measurements for the fossils. Of course, you say the illustrations are "1000X", and one can, at least in theory, work back, but it makes it impossible to be sure--especially since some of the ones that can be checked seem not to be right (cf. Pl. 7, fig. 6--p. 129 says this item averages 30 mu, but the photo measures 40 mm.). Size is taxonomically so important that I think you must give the data for each species illustrated. Also, your other descriptive information is so minimal that it comes down to picture-matching. Especially where you are doing systematics with some species, you really must provide some description beyond what is now before me!

p. 97. If Pl. 4, fig. 15 is Sphagnum, why isn't Pl. 4, fig. 1?

p. 107. Citation incomplete--should include: "...non P. andiniformis (sp.?--I don't have ms. at moment) Bolkhovitina, etc." Check the Code for format. Also suggest use of the more conventional "nomen novum".

Further: the type should be explicitly stated--this is a fault throughout the paper.

Further: Bisaccates are the devil, but I certainly am far from convinced that Pl. 4, figs. 16-18, are Podocarpus--why not Cedrus, for example? This is, by the way, a further illustration of the fact that more descriptive info is necessary--especially where names are proposed, new combinations made, etc.

p. 110. Pl. 5, fig. 2 looks a lot like Keteleeria .

p. 113. P. labdacus was not validly published in 1931--because Ollenites had not been val. published. The epithet labdacus was, I believe, validated by Raatz in 1937 (I don't have my references here).

p. 128. I wonder if fig. 5 is really Ephedra? It looks more like a reworked Mesozoic EQuisetosporites, or whatever.

p. 136. Pl. 7, figs. 12-14 represents three different things. Fig. 14 seems even to be a different genus.

p. 143. Pl. 7, figs. 18-19 is more along the alnoid line than like Corylus. It is certainly not Carpinus.

p. 146. Pl. 7, fig. 7 seems very unlikely to be Myrica--certainly not close enough to use the modern generic name.

p. 147. Pl. 7, figs. 24-27 don't remind me at all of Casuarina, for whatever that's worth! (Casuarinidites is misspelled on p. 324, fourth line from the bottom.)

General: Basionyms should be cited for all species that have been transferred and are being cited ~~cited~~ formally. In instances where you are making the nov. comb. this is required for valid publication. For example, even where only one species is cited, and it obviously is the basionym, it is still better to be explicit. On p. 222, you should say "basionym: Pollenites...."

General: re plates and figures: It would be much easier to use the paper if the figure captions included the page number of related text.

p. 235 and general: You'll probably find it hard to believe, but I am nervous these days about referring Paleogene species to recent genera! Not that I'd never do it, but Plate 12, figs. 28-29, certainly could be other things in the Nyssaceae-Cornaceae alliance.

Incidentally--there is no reason to assume that all Paleogene forms are referable to extant taxa--therefore, "unknown" is quite an acceptable statement for affinity--in a larger percentage of cases than you have so opted.

p. 291: Plate 16, figs. 15-18, does not look ericaceous to me--I am not even sure the form follows the correct "rule" as to aperture placement. It resembles more my Tetradopollenites laxus (Traverse, 1955), which Krutzsch (1970) made the basis of his "Laxipollis".

August 5, 1976

Dr. Norman Frederiksen
U.S. Geological Survey
National Center 971
Reston, VA 22092

Dear Norm:

Thanks for yours of 2 August--I will do what I can with the paper when it comes.

However, in case I should forget later to comment, I wanted to take care of the "P.S."! By all means, if it can be established that the form of the generic names to which you refer is definitely feminine, the endings of the specific epithets should be changed to the correct ending. As you may know, at the recent Leningrad Congress we ruled that the "-ites" endings are masculine, and therefore all *-sporites* and *-pollenites* generic names must carry epithets with masculine form endings. By the same token, the generic names to which you refer would require the same sort of change--if you are right about the form of the words in question, which I am not in a position to check here. The Article which has effect as far as correcting the endings is concerned is, I believe, Article 72. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

United States Department of the Interior

GEOLOGICAL SURVEY
971 National Center
Reston, Virginia 22092

August 2, 1976

Dr. Alfred Traverse
Department of Geosciences
529 Deike Building
Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

Under separate cover I am sending you two packages (a box and a tube) containing the manuscript of my thesis, which is supposed to appear as a USGS Prof. Paper. I would appreciate it very much if you would review it for me. Spend as little or as much time on it as you like, and show it to anybody you like. I am particularly interested in your opinion as to the botanical affinities of the species, as I am working on a couple of ecological papers based on these botanical data.

Thanks a lot.

Yours truly,

Norman Frederiksen

P.S. I have changed a lot of specific epithets to feminine forms, particularly for genera ending in -is, because the Code (1966, Recommendation 75A) says this ending is feminine in botany. Or should I leave the epithet endings masculine if the author who first assigned the species to an -is genus treated it as masculine (e.g., Thomsonipollis magnificus (Pflug) Krutzsch)?

809 Cornwall Drive
State College, PA 16801
April 28, 1976

Dr. Norman Frederiksen
U.S. Geological Survey
National Center 971
Reston, VA 22092

Dear Norm:

Under separate cover I am sending you a slide of my TR2-1984, being a preparation of *Dorstenia contrajerva* L., per the information on the data card that accompanies the slide. As before, the pollen was prepared by standard acetolysis and mounted in glycerin jelly. A "bill" for the slides (this one and the previous two, per my letter of 22 December 1975) is also enclosed. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb
Enclosure: bill

December 22, 1975

Dr. Norman Frederiksen
U.S. Geological Survey
National Center 971
Reston, VA 22092

Dear Norman:

Under separate cover I am sending you a slide of my TR-536 (*Parthenocissus quinquefolia* f. *hirsuta*; *Vitaceae*) and my TR2-1300 (*Thrinax argentea; Palmae*). Each species include a data card with all the information. The pollen was prepared by standard acetolysis and mounted in glycerin jelly.

I have still not been successful in getting decent material of *Dorstenia*, but don't give up! In the meantime, let's hold off on the "bill". I will take care of that all in one shot when I get the *Dorstenia* slide to you.

Best wishes of the season,

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

United States Department of the Interior

GEOLOGICAL SURVEY

National Center 971
Reston, Va. 22092

28 Oct. 75

Dear Al:

Thanks for your letter. I would like to order one slide each of Parthenocissus, Thrinax and Dorstenia. Please send along a bill so I can turn it in to get you paid.

Yours truly,

Norman

Norman Frederiksen

P. S. Our bookkeeper said the bill should be on University letterhead.

TR 536
Parthenocissus quinquefolia f. hirsuta (Vitaceae)
TR 500
Thrinax parviflora (Palmae)

United States Department of the Interior

GEOLOGICAL SURVEY

National Center 971
Reston, Va. 22092

28 Oct. 75

Dear Al:

Thanks for your letter. I would like to order one slide each of Parthenocissus, Thrinax and Dorstenia. Please send along a bill so I can turn it in to get you paid.

Yours truly,

Norman Frederiksen

P. S. Our bookkeeper said the bill should be on University letterhead.

October 24, 1975

Dr. Norman Fredriksen
U.S. Geological Survey
971 National Center
Reston, VA 22092

Dear Norm:

Thanks for yours of 7 October. I am not in the practice of lending my reference slides, but I can offer another alternative. I sell slides at the cost of their preparation, charging only for the labor invloved. The price for such a small order would be $3.00 per slide. I have excellent material of *Parthenocissus* and of *Thrinax*, I do not at the moment have *Dorstenia*, but if you want it I would have the girl run up some herbaring material.

I think your suggestion about *Nothofagus* in the northern hemisphere is a quite cogent possibility. Why didn't I think of that? Looking forward to seeing you in Houston.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

P.S. Gutzler is, indeed, a very fine student and a credit to all of you who helped train him.

United States Department of the Interior

GEOLOGICAL SURVEY

971 National Center
Reston, Va. 22092

Oct. 7, 1975

Dear Al -- I am working on a paper on the late Eocene flora of S.E. North America, and I wonder if you have slides of the following genera: Dorstenia (Moraceae), Parthenocissus (Vitidaceae), and Thrinax. Do you have them, and if so, could you lend them to me, please?
By the way, I got some slides of Celtis spp. from Tschudy, and checked them against Elsik's so-called Nothofagus, and I'm sure that's what he has. What do you think?

See you in Houston.

Yours,

Norm Frederiksen

P. S. Please say hello to Bob Gutzler from me; hope he's as good a student there as he was in San Diego.

San Diego State College
San Diego, California 92115

Geology Department
June 19, 1970

Dear Dr. Traverse:

Enclosed is the abstract of a paper I would like to give at the AASP meeting in Toronto. I sent the original to Dr. McGregor.

Yours truly,

Norman Frederiksen

Norman Frederiksen

August 4, 1969

Dr. Norman O. Frederickson
Geology Department
SAn Diego State College
San Diego, California 92115

Dear Norm:

Enjoyed your recent post card, bbut I still somewhat adamantly maintain that it is better to avoid formal names in anything other than a formal publication. I would argue that sendingoout copies of material such as you sent could be "effectivelpublication" within the meaning of such per the code--it depends how many copies and how wide spread the coverage was. Your are on more firm ground regarding the "full reference", though I believe even in that instance one could argue that for persons who have only published a paper or two a reference to the author himself might be a sufficiently full reference.

Anyway, the main point is that formal nomenclature should definitely be avoided in theses and other informal treatments.

Looking forward to seeing you again one of these years, I am

Yours very truly,

Alfred Traverse
Associate Professor of Geology
and Biology

AT:kwc

Dear Dr. Traverse: As you say, there was no intention to publish new combinations formally by sending out the thesis plates. Furthermore, even if there had been, the new combinations would not have been valid because (a) the plates were not effectively published merely by sending them to people and (b) new combinations require a full reference to the original place of publication, which was certainly not given on the plate captions I sent out. I'm about ready to type up the pollen and spore taxonomy and submit it to Rev. Paleobot. Palynol., so they should be out in a year or so.

Yours truly,

Hans Tralau

July 22, 1969

Dr. Norman O. Frederiksen
Geology Department
SAn Diego State College
San Diego, California 92115

Dear Norm:

Thank you for the "preprint" (or whatever it would be called) of part of your thesis material. As you say, it was of considerable interest to have a look at same. We are working on Eocene materials from the Gulf Coast too. Also glad to hear that you are making a "reentry" into the academic milieu. It has its disappointments, but on the whole is clearly betoer than industry for the average academically oriented person.

Regarding the "preprint", I do have one caveat. Since you used formal nomenclature (that would have better waited until the formal publication of the whole monograph), a question may come up about whether the "preprint" itself constitutes valid publication. For specific transfers, where no descriptions are required, but only reference to previously published ones, it could be that the "preprint" is valid. I suppose you would agree that this is undesirable and presumably unintentional. If it does constitute valid publication--what date should it bear? I have worked my copy: "mailed out 16 May, 1969."

Best wishes, as ever.

Yours very truly,

Alfred Traverse
Editor
Catalog of Fossil Spores and Pollen
Associate Professor of Geology & Biology

AT:kwc

Field Research Laboratory
Mobil Research and Development Corp.
P. O. Box 900
Dallas, Texas 75224

Dear Colleague:

Several weeks ago I sent you the abstract and plates from my dissertation, which I just finished at the University of Wisconsin. I expect to publish the work, but it will be at least a year or two before it comes out, and in the meantime I thought you might be interested in having at least the plates. According to Foraminifera, the material represents the entire Upper Eocene and probably in part also the uppermost Middle Eocene.

I am leaving Mobil Oil Corporation, where I have worked since 1961, and am joining the faculty of San Diego State College. My address beginning this summer will be:

Geology Department
San Diego State College
San Diego, California 92115

I will be teaching and doing research in palynology at San Diego State, and I would very much appreciate it if you would send me reprints of your papers.

Yours truly,

Norman Frederiksen

Norman O. Frederiksen

1968

Comments

1. I think it would be a mistake for you to get involved in making taxonomic judgments like Wilson wants to do in his system. If you think certain species are synonymous, publish these opinions separately.

2. As I understand it, you wanted to begin by putting the data from the Catalog on tape. I think this is a waste of time; the only useful information will be gotten by starting from scratch, one paper at a time, and recording all the species found by the author, not just the new ones.

3. I do not think the enormous time necessary to classify each species into a morphologic key system is worth the effort. It is rare that one can solve a problem knowing only the morphologic types present, except maybe to period level, and one hardly needs a computer to solve a problem of this kind.

4. We have found that many problems of synonymy can be worked out by a printout listing the species in alphbetical order, followed by the author and date of the species, followed by the genus assigned to the species by the author of the paper you're looking at.

   Species epithet) Orig. author) date) genus) paper) other information

5. One essential item of information for making accurate range charts is to know the method the author used for dating the rocks he looked at. Some dates that have been given for assemblages are quite inaccurate or in some cases circular reasoning was used to arrive at the date. It would be useful to have a measure of the certainty of the age of the palynomorphs, although this would probably get you involved in subjective judgments.

N. Tedenbrien

(Robb)

P. S. Please send me the results of your survey.

Name Author