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The Hunt Institute for Botanical Documentation, a research division of Carnegie Mellon University, specializes in the history of botany and all aspects of plant science and serves the international scientific community through research and documentation. To this end, the Institute acquires and maintains authoritative collections of books, plant images, manuscripts, portraits and data files, and provides publications and other modes of information service. The Institute meets the reference needs of botanists, biologists, historians, conservationists, librarians, bibliographers and the public at large, especially those concerned with any aspect of the North American flora.

Hunt Institute was dedicated in 1961 as the Rachel McMasters Miller Hunt Botanical Library, an international center for bibliographical research and service in the interests of botany and horticulture, as well as a center for the study of all aspects of the history of the plant sciences. By 1971 the Library's activities had so diversified that the name was changed to Hunt Institute for Botanical Documentation. Growth in collections and research projects led to the establishment of four programmatic departments: Archives, Art, Bibliography and the Library.

To: w.chaloner@rhbnc.ac.uk  
From: Alfred Traverse <traverse@ems.psu.edu>  
Subject: blurb  
Cc: jskog@gmu.edu  
Bcc:  
Attached:

Hi, Bill!

The little MS that you sent to my home address with the gorgeous stamps has been read. I found it to be a basically very clear and accurate statement. (I could fill you in on how Jim got to his point of view—he held stubbornly to the idea that every name for a fossil plant part was potentially the name of a PLANT. With the new rules we are putting away that idea.) I have found a few typos, which I have marked in red and am sending the pages off to you by airmail.

In addition, I penned across the bottom that I don't see how you can totally avoid the question of illustration of microfossils by illustrations. The idea has a lot of support, though not enough on the present Committee, and it won't go away.

Happy holidays. Al.

To: Skog@gmu.edu, Traverse@ems.psu.edu  
Subject: Fossils in ICBN ex Missouri

Dear Judy and Al, In response to Mike Boulter's urgent call for contributions to the last IOP Newsletter of the Millenium to-day I have just run off a page or so on the changes initiated in the ICBN at St.Louis dealing with Morphotaxa. I have tried to "attach" it to this e-mail (which is rather steamish, operating by land line to my College in Egham) and have failed. I will airmail you each a copy tomorrow. If you can see any real horrors in it - and it is pretty brief, and does not set out the whole of the amendments, but just the briefest outline of what they aim at - I would be very grateful for corrections, ideally by e-mail ! Best wishes to you both  
Bill.

*Chaloner*

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4 November, 1998

Professor W. G. Chaloner  
Department of Biology  
Royal Holloway & Bedford New College  
Egham Hill, Egham  
Surrey TW20 OEX, England

Dear Bill:

It was kind of you to send me your MS, "Taxonomic and Nomenclatural Alternatives" for review. You don't say where it is to be published—I gather from some hints in the MS perhaps a chapter in some book. It would be interesting to know more about that. Actually, it is not clear whether perhaps the title is supposed to be just "Taxonomic Alternatives," because the "...and Nomenclatural" is pencilled in, very lightly, with a question mark. In a way, I find that symbolic or sacramental of the whole essay. It freely mixes up taxonomic and nomenclatural considerations in a way that in my view is not helpful to the average reader who is trying to understand the nomenclatural matters.

You know, of course, that I do not agree with you that the techniques for naming, and the taxonomy of the things being named are thoroughly and inexorably intertwined. It is therefore interesting that the essay has largely to do with nomenclature and codes thereof but, originally at least, carried a title mentioning only taxonomy!

You are one of my friends of longest standing in science. I therefore hope that you will forgive my being very frank about this essay. I think that it will be found confusing to most of the readers you hope to reach. This is especially true, in that you have a paper in press (Chaloner, Greuter, Nicolson and Traverse) in Taxon in which much of what you say in this essay is in effect contradicted. Readers will wonder which Chaloner to believe. The problem is, of course, that the Chaloner et al. article was actually written by Greuter, who has a less cluttered view of the matters under consideration.

As you are aware, I have for over forty years recognized that the organ-taxa and/or form-taxa business about fossil plants in ICBN, carried over from the old paleobotanical appendix of said Code confused nearly everybody (WGC excluded, he claims) who was interested, and served no useful purpose in the naming of fossil plant fragments that I have ever discerned. I worked hard to get rid of it all, and at the Leningrad Congress finally succeeded, only to have the Editorial Committee, poorly advised by the ailing Jim Schopf, restore the bit about "referable or not-referable to a family, etc." business that had to do with the distinction between form- and organ- taxa that the Fossil Plant Committee and later the whole Congress had expressly voted out. Shortly thereafter you became chairman of the Committee, and I remained as Secretary. As you were an enthusiastic fan for the form- vs. organ- taxa bit, I saw that there was no chance to completing the job of wiping its relics out of the Code. I had to gulp and be content with hoping I could persuade you not to restore the whole business.

As noted above, you are now first author of a paper in-press in Taxon, in which the concept of parataxon is

introduced for fossil plant taxa that would have been termed organ taxa and form taxa in the old Codes. The ideas in the paper are quite acceptable to me, as witness the fact that I agreed to be listed as co-author of it. The whole business of which you are enamored, about organ and form taxa, and the mystical distinction between them based on family referability, is swept away. We are left with ordinary genera such as Magnolia and fossil para-genera such as Neuropteris. Both can have fossil species, both have types, both can be referred to families, though for many fossil para-genera this would be a quixotic exercise and probably won't happen often. Everybody would understand that a family based on Neuropteris is a para-family and conceptually different from a family such as the Magnoliaceae.

The essay that is in front of me is quite at odds with the Chaloner et al. in press paper. It is confusing that you refer rather warmly to Spicer's case for "opting out of conventional nomenclature" altogether, which you understand as a "plea for a wider use of parataxa." But in Chaloner et al. we make para-taxa very much a part of "conventional nomenclature." You also use "parataxon" for the Hughesian units, among others. In my view you would be on record as making "parataxon" so broad a concept that it could hardly be understood by readers of Chaloner et al. to have the restricted paleobotanical definition proposed in Chaloner et al.

The botanical literature is currently full of articles calling for "phylogenetic taxonomy," based on the latest DNA readings and cladistic plottings of same. This is o. k., of course. But there are also linked calls for "phylogenetically based nomenclature," which in some cases would abolish the conventional ranks and would reintroduce after a hiatus of 320 years polynomial and monomial "names." Such nomenclature is unlikely to aid the day to day work of systematists whose results are important to practical applications. We should be very careful about allowing the phylogeneticists too easily to modify the working names of plants. For example, practical concerns may prompt us to hang onto the Aceraceae for a while, even if the phylogeneticists have proven by DNA studies that they should be merged in the Sapindaceae.


Likewise, I fear that your present essay does not help clarify the practical nomenclatural situation in paleobotany, and instead allows interesting taxonomic speculations much too much room to influence it. Schemes such as those of Hughes and suggestions such as those of Spicer are useful taxonomic speculations and as such are worth more investigation and experimental use. This should not impinge on formal nomenclature until decades of such use have gone by, if then.

In short, I feel that this proposed essay (which you say is almost in press) muddies the water and confuses much more than it enlightens, as to direct impact of taxonomic speculations on formal nomenclature. I think you should seriously consider withdrawing the essay, and go with your implied position in Chaloner et al., in press.

I return the MS herewith, with red-pencil corrections of certain errors, and a few comments marked directly on the MS.

I am sending copies of this letter to Werner, Rob and Judy because it seems to me the matter is of concern to them too.

Best personal greetings.

  
Alfred Traverse  
Professor Emeritus of Palynology

Page 2 of 3

Juno e-mail printed Thu, 22 Oct 1998 13:47:23 , page 1

From: Bill Chaloner <W.Chaloner@Vms.Rhbnc.Ac.Uk>  
Return-path: <W.Chaloner@Vms.Rhbnc.Ac.Uk>  
To: bettyandal@juno.com  
Date: Wed, 21 Oct 1998 06:36:08 -0700  
Subject: Nomenclature  
Message-ID: <362DE348.58C1@vms.rhbnc.ac.uk>  
X-Status: Read  
X-Mailer: Mozilla 2.0 (Win16; I)

Dear Al., I have mean to thank you for earlier message about the Greuter et al MS and the copy message to Fensome for some days. Somehow didnt get round to it. I am mailing you a copy of a MS chapter for the book shortly to be published here on "Fossil Plants and Spores: modern techniques", instigated by our Geol. Soc. of London. It is on "Taxonomic and Nomenclatural Alternatives". I try to do in a few pages what you did more effectively (and with more space) in the Palynology: Principles and Applications" volume. If you get a chance to look at it in your busy commuting life, you will see that we still have rather basic differences about why fossil plant genera are not just like other genera. But you may have had enough about all that !

I will agree not to repeat your message back to you, so as to save your printer any strain (though it is the easy, lazy way to reply !) if you will take that "free e-mail from Juno " commercial off the bottom of your reply. How's that for a deal ?  
Best wishes to you both - Bill

From: Bill Chaloner <W.Chaloner@Vms.Rhbnc.Ac.Uk>  
Return-path: <W.Chaloner@Vms.Rhbnc.Ac.Uk>  
To: traverse@ems.psu.edu  
Date: Tue, 29 Sep 1998 13:30:54 -0700  
Subject: Greuter MS  
Message-ID: <3611437E.4E30@vms.rhbnc.ac.uk>  
X-Status: Read  
X-Mailer: Mozilla 2.0 (Win16; I)

Dear Al - sorry you had to wait so long for an answer, (I have no valid excuse) but at least this should come to you without your message being attached to it ! I also hope by now that you have had my comments on the Greuter MS. It was indeed entirely his; I had actually both talked to him

and exchanged correspondence over the term "parataxa" and its use by palaeozoologists but that was a small item in the whole. I have no objection to his wading into the fossil nomenclature arena (or ? quagmire) if he can come up with a construction which is acceptable to a wide group of users. None of what is proposed here (or in the earlier Fensome and Skogg proposals) will actually change anything which the majority of us would be disposed to do.

Regarding Scofflaws, he was no (known) relative of mine, but then there are at least fifty Chaloners (with one "I") in Britain at the moment, of whom only half a dozen are relatives - and far more across the Atlantic .....so ..... !

I hope you are now securely bedded into you home office well away from University Park, and that you can receive this and other messages in the "comfort of your own home" ! Good to be in touch with you again, and I hope we can keep in contact on nomenclature and other matters by this means. Best wishes, Bill

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12 June, 1998

Professor William G. Chaloner  
Royal Holloway  
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Egham, Surrey TW20 0EX  
UK

Dear Bill:

Hey, you old notorious scofflaw, you never answered mine of 13 March. No matter. I have bad news. The dept. head just kicked me out--nothing personal; in fact, he's a nice kid. One of the new faculty members got a huge grant and needs a headquarters. I'm elected. And furthermore I have just two weeks to clear out. So lots of projects I hoped to work on are being chucked, etc. We have built a small addition to our home which will now be my palynological headquarters, but it will never equal what I have had here. I do get to keep the wet lab that you once designed, at least for the time being. And I will have a cubicle with a desk where I can hang my hat when I <sup>am</sup> here to process samples or whatever.

A few weeks ago I got back a review of a little squib I wrote for a book--about palynology and ecology. One reviewer's handwriting sure looked like that of WGC. If it was, I wanted to argue about one statement. I had commented that the pollen in peats is overwhelmingly (more than 99%) local in origin. The reviewer denied this. As peats are autochthonous deposits, and as pollen in the AIR travels 99.9%+ only a few hundred meters from the producing plant, my statement is intuitively obvious. (Some pollen gets up into the atmosphere and travels a long distance, but it is a very tiny percentage of the total pollen produced.) Besides, it has been measured by various people, such as Straka et al. This is one reason why typical peats (and thus most coals) do not give a good regional reflection of vegetation in their palynofloras. But perchance you are not the reviewer with the chalonesque script.

Hope you and Judy are aok. Please come and visit us if you ever have half a chance. We'll show you a good time.

Yours very truly,

Alfred Traverse, Palynologist-without-portfolio

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13 March, 1998

Professor William G. Chaloner  
Royal Holloway  
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UK

Dear Bill:

Honestly, I didn't wait until Friday the 13th on purpose.

Betty and I left here just before Xmas and went by stages to Clearwater, FL, where we spent Jan.-Feb. with a raft of my Canadian cousins. We both acquired bicycles and rode like mad. We also travelled around central Florida quite a bit, and did a lot of visiting with my relatives, some of whom I hadn't seen for more than half a century.

We also both worked on projects, for which we took a truckload of literature and our computer. Betty is presenting a paper at the big medieval congress later this year (her book on Neidhart was published in Germany late last year). I was working on a Devonian paper, since submitted, and on a proposal for modification of the Code, ditto.

We returned here via a mess of botanical gardens and arboreta, and the Big Ten Womens Basketball Tournament in Indianapolis, on 6 March.

Your letter of 9 Feb. arrived here too late to be forwarded to FL--our daughter, Martha, was taking care of such matters, and she had a deadline of 18 Feb.--we left the place we had rented on 25 Feb. So, it was just now opened.

I was interested in your incidental comment on e-mail. It is too existential. I found this out the hard way with one of our kids. With regular snail mail I sometimes ripped up a letter without sending it to him, with e-mail it is too darned easy to hit "send." He is still mad at me for things I am supposed to have said.

Regarding main points of your letter (using the same numerical system as your letter):

1. I think the parataxon idea is a good one. I suppose the

idea is rather like Mike Boulter's "fossil taxon" of some years ago.

2. This statement of yours helps to confirm me in the opinion that the whole organ-form taxon concept(s) was so arcane that even quite bright people couldn't handle it. I saw that about 1951 and have been convinced ever since that the whole thing should be abolished. I don't think it ever did much good. Perhaps the "parataxon" idea will present a very useful new start.

3. You are here explaining why you think *Ginkgoites* is an "organ genus," etc., but as you know I don't think this nomenclaturally helpful at all. I say forget organ and form genera and get on with paleobotany.

4. No comment--I need to have seen the background to this matter.

5. What you say is surely correct.

6. Indeed, as late as the last time I looked, zoologists have no special formal categories for fossils at all.

I also plan to be in St. Louis (my sister lives there, which will simplify the housing problem a little). I love the place. The MBG is rivalled only by Kew in my experience. But the zoo is also great. The art museum (housed in one of the buildings of the "meet me in St. Louis, Looney" fair) is very nice. Cahokia Mounds is the most important Indian site in the whole northern part of North America. The museum at the arch is well worth a visit. Cruises for lunch on the Mississippi are fun. Etc.

And, oh yes, I will be promoting and defending my proposal.

Random thought before closing. I have urged in various letters to Greuter that "whole plant taxa" in paleobotany should be kept informal, as they most always have been. However, might it not be useful for somebody to publish a guide to the matter? I think the idea of a *Lepidodendron* plant and a *Calamites* plant and an *Archaeopteris* plant has considerable utility. I don't think they should be governed by rules (what a mess they would be!). But shouldn't there be some (non-legal) guidelines?

Nice to hear from you.

Yours very truly,

Alfred Traverse  
Professor Emeritus of Palynology

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- Professor W G Chaloner FRS
- Royal Holloway  
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Surrey TW20 0EX
- Telephone 0784 443581
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- TeleFax 0784 471780

Professor Werner Greuter,  
Berlin.

Feb. 9 1998

Dear Werner,

### FOSSILS IN THE ICBN & THE BIOCODE

I return to the fray after a busy spell of teaching through the autumn and into January; I had meant to reply to your helpful e-mail of Dec.14, "by the end of the year" as requested, but too many other things intervened !

Over the years, so many articles have been written, so many proposals made and rejected, in the fascinating game of fossil plant nomenclature! As I generate this note, my desk is littered with a small selection of them. I will not embark here on a lengthy diatribe, but just offer a few thoughts on your proposals and related matters. I will send a brief e-mail just to say that this is on the way, but my e-mail goes out from my pc via our mainframe at Egham. This means I cannot compose and edit matter onto a disk and then throw it into e-mail, but any e-mail message must be typed in "live", and goes on line by line without the chance to retract or revise as one goes along. Hence, a letter rather than e-mail. I will send a copy to Rob, Judy and Al Traverse as maybe-interested parties.

1. On the whole I like your suggestion of having parataxa (parataxon) replace the "revised" version of form-genera as Rob and Judy wish to use the term, even if only because their procedure will cause considerable confusion between people using it in the classic sense, and those who would try to follow such an emendation if it were adopted. There are plenty of other reasons.

2. I have never understood why it was believed that the taxonomic decision which preceded putting a given fossil genus into the category of form-genus or organ-genus somehow took the matter out of the court of the ICBN, as Rob and Judy suggest (Taxon Aug. 97, p. 558 et seq.). Of course that meant that it was a given author's decision as to whether a particular name was an organ genus or a form genus, a taxonomic decision, but when that was made, the Code explained the consequences.



There are many such overlaps between taxonomy and nomenclature in the working of the code ! Of course, deciding which starting date applies to a given name involves a taxonomic decision; once you have made it, the Code then tells you what to do.

As Al Traverse points out,(e-mail to me, Feb 1997 !) the idea that once a fossil genus is put in a family it became a organ genus, not a form genus, meant that it was up to anyone to simply make a family based on the genus in order to upgrade it to organ genus. He cites an imaginary "Rhabdosporitaceae" as illustrating this idea. Of course this is true; however, if such a family were made, on those grounds, it would simply not be used by the majority of workers. It has always been the case that what was an organ genus for one author was a form genus for another. That is inevitable with what is in truth a taxonomic decision !

3. The difficulty of teasing apart the taxonomic element from the "pure nomenclature" of the Code is illustrated where we meet fossils which can at the whim of the author be assigned either to a "non-fossil genus" or a "fossil genus" (as defined in the footnote to the Tokyo Code, p.3). The Jurassic leaf which has been assigned to *Ginkgo huttoni* by Harris and other palaeobotanists has been designated by as many others as *Ginkgoites huttoni*. That latter genus is based on a Triassic fossil leaf species, and it is used by those palaeobotanists who are not happy to put a Jurassic leaf into a living ("non-fossil") genus. Anybody is entitled to put *huttoni* either into the living *Ginkgo* or the fossil taxon *Ginkgoites*. It is a taxonomic decision, with nomenclatural (and taxonomic !) consequences. Palaeobotany is stuck with the untidiness of that kind of taxonomic/nomenclatural interface.

Incidentally, for me *Ginkgoites* is an organ genus, in that I believe for a number of good reasons that it can reasonably be attributed to the Ginkgoaceae. But for others it may be regarded as a form genus, which should not be attributed to any family because we cannot in any adequate sense show that it produced fructifications which would justify that assignment. So for some, *Ginkgoites* is a form genus, for me it is an organ genus. It is a taxonomic decision of any author, as to the category in which he considers that genus belongs.

4. I like the clarification of the awkwardness of fossil taxa in being limited in application to the preservation state and the type of organ on which they are based, spelt out in the Fensome Skog article (*Taxon* Aug. 97) and in your own preamble. More especially, the concept that you explain in the phrase "left to the practising palaeobotanist to answer on a case by case basis". I also like the principal in your own proposal to acknowledge the (broad) analogy that exists between the nomenclature of taxa based

on parts and preservation states of fossils to those based on parts of the life cycle of fungi. As you well know, that was at one phase included in the Code, and was (to the regret of some of us) dropped in later years.

5. I have no informed opinion on the application of the ICBN to either Dinoflagellates or Diatoms. I cannot believe that the *Spiniferites/Gonyaulax* and *Tasmanites/Pachysphaera* problems do not have counterparts in the plant macrofossil context, as in Ginkgo/Ginkgoites above, and many others of a like nature.

6. Finally, you might be interested to see how some of the Zoologists dealing with parataxa (in my interpretation) perceive that term. I enclose a couple of pages photocopied from Bromley's book on trace fossils. He says of trace fossils "nor are they parataxa in the sense of names applied to parts of animal fossils". What a pity - I thought the analogy was rather strong and helpful, and indeed extended to plant "form taxa" ....!

W.G.  
only

Anyway, I look forward to the debate in St Louis, and hope/intend to be there. I have a General Symposium accepted by the programme Committee on the "Evolution of the Carbon Cycle" (which has absolutely nothing to do with nomenclature !) which will I hope, among other things, help get me to St. Louis. But I also hope to be there in time for the nomenclature session too!

With best wishes,

Sincerely,

Bill

W.G.Chaloner.

Copies: Fensome, Skog, Traverse.

At - it's too  
long since we  
exchanged ideas on  
all this! This reply  
refers to your  
e-mail of a year  
ago - oh, oh...

Bill

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

Dear Bill:

Finally I have a few free moments to respond to copies of letters (to Judy Skog & Rob Fensome, of 6 Dec. 96; to David Hawksworth of 26 Dec. 96), kindly sent to me about the end of last year when I was out of town.

Rather than getting into a discussion at this time of your ideas re modification of ICBN and creation of a Biocode, I would like to state some general impressions.

One such impression is that your ideas would complicate the Code for philosophical, not pragmatic reasons. Part of this may stem from what I perceive as the fact that you are not on any regular basis working in the trenches of practical nomenclature, as Jansonius and I are. I view the Code as a document intended to assist one in the practical matter of application of names to biological-paleobiological entities. I think that anything that complicates our procedures should be assumed to be deleterious to our goals, unless clearly proven useful.

The fact is that, except for dinocysts and other unicellular algae, all plant fossil taxa are what W. Greuter suggests calling "parataxa." Even if parataxon *Rhabdosporites* sp. is found in the sporangia of parataxon *Tetraxylopteris*, this does not mean the union of the two parataxa. Any workable code will have to recognize this situation for fossil plants, and it may be a reason why there cannot be a "biocode," as the situation is different for animal fossils. Mosaic evolution is common, almost a rule, in plant evolution.

I would have no trouble with "parataxon," per Werner's suggestion, but not if it is simply added to a pantheon of such terms, along with the unnecessary "form taxon" and "organ taxon." In my opinion, all the stuff you philosophize about as justifying, for example, "organ genera," is irrelevant to a handbook of nomenclatural instructional rules. With the exception of suprageneric categories, I see, as I have said before, no difference in naming of fossil spores and naming living mosses. I admit to drawing some satisfaction in support of this notion from the fact that the granddaddy of all nomenclaturalists found no difficulty in applying the same general ideas to naming rocks and minerals.

For that matter, what harm would come from somebody took it into his/her head to have a family "Rhabdosporitaceae?" I'd rather handle such matters informally, as with Potonie's turmal names, but I can't see that it would cause any real problem if somebody made up "families" to replace the subturmae in Potonie's system. Who would care? People who didn't want to use such categories could simply ignore them. In the trenches that I mentioned above the problems that we deal with are typification, proper description, effective publication--of generic and specific names.

Whom does it or did it benefit to have a distinction between "form taxa" and "organ taxa" as opposed to other (real?) taxa? Is there "case law?"

Greuter's parataxon suggestion, by the way, does have the advantage of being a little vague. Spores, pollen, acritarchs and dinoflagellate cysts are NOT organs--that you can't dispute. So, how could they be "organ genera?"

As I have written Werner with a copy for you (will come by snail mail because I made a mistake in the CC address), I think that all "whole plant" nomenclature in paleobotany should remain, as it has all these decades, informal. It should remain utterly outside the Code, just as colloquial names and potoniean turmal names are. There is no need to formalize and codify the procedure for naming whole plant concepts.

To repeat: the Code(s) should avoid all complexities that are not helpful in the naming procedure. As I have hinted above, this may argue against the appealing sounding concept of a Biocode--because when too many sorts of entities are dealt with there will have to be too many exceptions taken care of--connection proves identity and synonymy for animal fossils, not for plant fossils, etc. Perhaps it is better to have separate codes for different sorts of organisms. I tend to think so--and even would prefer it if the fungal specialists would withdraw from the ICBN--it is interesting, for example, how the special needs of naming these funny things have tended to dominate the nomenclatural sessions at the botanical congresses--where I would be prepared to argue they have no business being anyway. Fungi are more like animals than plants.

Enough for now.

Good to hear that you are having a break from winter weather. We are not. It has been solid winter since November. 19 F. this a. m. Complete snow cover. I was chain-sawing fallen trees in my woods last Sat. with a helper, and it was very cold (10 F.) but brilliant sunshine with very clear air, and it was exhilarating. I would miss winter--some days!

All the best. Al.

To: w.chaloner@rhbnc.ac.uk  
From: traverse@ems.psu.edu (Alfred Traverse)  
Subject: hello  
Cc:  
Bcc:  
X-Attachments:

Dear Bill:

Whoa! the "from" line of your recent missive says: UHBA003@vms.rhbnc.ac.uk, rather than the address I am using for this message, which is certainly mnemonically superior. What gives?

I am somewhat bemused by your more or less welcoming me to Internet. I am reasonably sure that I have been into e-mail far longer than you. Betty and I have four e-mail addresses between us, and we use them at our three residences and this office, and we use our laptop on long trips by plugging into the phone lines in motels for e-mail. I have not (yet?) got into the Web to any significant extent because it looks to me to be a terrific time-waster. I'd rather look in the microscope. But e-mail is terrific. A bit too existential, perhaps. One of our sons is currently really angry with me because of alleged "insults" that he found in my messages to him. I thought I was just being frank. Hmmm.

Best regards and please pass along best wishes to Judy. Al.

Chaloner

**UHBA003@vms.rhbnc.a, 08:03 AM 2/17/97, RE: no Latin!**

**1**

From: UHBA003@vms.rhbnc.ac.uk  
Date: Mon, 17 Feb 1997 8:03:41 GMT  
To: traverse@ems.psu.edu  
Subject: RE: no Latin!

Dear Al, it has been too long since we communicated on paper !  
Delighted to see you are now listening in (?viewing) life on the  
e-mail strand of the Net ! Good to see your note on the Latin story,  
and kindly attribution of the bilingual provision. That Feudian  
slip on the last line is just one of those interesting oddities of  
e-mail - cant be bothered to cancel and re-write the whole thing.  
Hope the snow has melted in State College. It is pretending to  
be Spring in London. Best wishes, Bill.

**Printed for traverse@ems.psu.edu (Alfred Traverse)**

**1**

Of course Meyen in his textbook (Fundamentals of Palaeobotany,

a lot more besides ; You have heard it all before, I know.  
Living genus of gymnosperm plants which of course have seeds, but  
based on seeds. In that respect it is entirely different from a  
Lagenostoma is a genus for seeds; for me it is an organ-genus,  
be told, we do not know the rest of most of those plants,  
other genera of seeds into that family, but as you don't need to  
on a seed. We can make a family, Lagenostomaceae, for it and put  
palaeobotanists, Lagenostoma is still an organ genus, based only  
way in a code of nomenclature. To me and many other  
genus, and so did not need to be singled out in any particular  
treatment of an organ genus was precisely that of an extant plant  
understanding of the removal was simply that the nomenclatural  
Congress. Jim Schopf scolded me roundly for this ; However, my  
and I could not get away for it, although I attended the  
Leningrad Congress; the meeting was held during our term-time,  
ICBN, I was unfortunately not at the nomenclature section of the  
Regarding the original (1975) removal of the organ genus from the

First, thank you both very much for letting me see your proposal,  
and even more for inviting me to join you in authorship. It is  
a very generous offer, especially as it certainly doesn't need  
my support and will probably gain the endorsement of your  
committee without it ; I have to say that although I agree with  
your sentiments very largely, there are details that I do not  
like, and would not support your present version. I wish we could  
get round a table and talk about it, but as we can't let me offer  
you these thoughts as a first shot.

I should also add - though I said it to Judy in the summer - that  
your joint poster on the workings of fossil plant nomenclature  
was really excellent, and I wish you were publishing that instead  
of your revision of the Code ; I agreed with 90% of it, though  
would fret somewhat over your interpretation of form-genera (see  
below ;)

I am directing this to Judy, hoping that she might fax it on to  
Rob, since I have only her fax number at my elbow, from the Santa  
Barbara listing. I really also owe her a reply to her letter to  
me of May 3 (I blush to say), although I apologised in person,  
and happily talked it through in Santa Barbara in July.

Dear Judy and Rob,

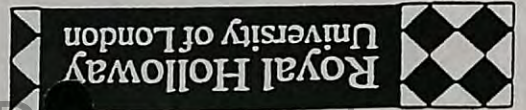
PROPOSAL TO EMEND THE ICBN

FAX 001 703 993 1046

Dec. 6 1996

VA, USA  
George Mason University,  
Dept. of Biology,  
Dr. Judy Skog,

Chaloner



• Department of Geology  
• Professor W G Chaloner FRS  
• Royal Holloway  
• University of London  
Egham  
Surrey TW20 0EX  
• Telephone 0784 443581  
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• Telefax 0784 471780

As you will gather, I like the present Art. 3.3 and am sorry that it has been so misunderstood. Perhaps it could be tidied up, but I am not really happy with your proposal to abolish it. On the other hand I would be perfectly happy to abolish Art. 3.4; this has always seemed an absolutely fatuous item; the previous

The account in your article, p. 2, of the status of dinoflagellate genera seems to me to exactly parallel the angiosperm situation with leaves and fruit. It becomes a quibble of no great importance that Fensholt et al regarded fossil dinoflagellate genera as "genera" rather than "organ genera", though I will keep out of that area!

I do not know how you read the code to claim that "arguably ... if one names a fragmentary fossil then it **must** be called a form of the kind: Art. 3 refers to the species of **SOME fossil plants** of which it says "the genera to which they are assigned are not assignable to a family". If it had meant that all fossil plants based on parts ("fragmentary") cannot be assigned to a family it would have said that! An enormous number of species of Tertiary angiosperms (leaves, most of the fruit and seeds in the London Clay) are based on "fragmentary specimens" in the sense of Art. 3. But they are none the less assignable to either living genera ("non-fossil genera" in the usage of the code) and hence to a family, or are assigned to organ genera (like *Wetherellia*, from the London Clay, of which only the fruit is known) which is placed in the Euphorbiaceae. I believe that this is the only reasonable reading of Art. 3.3.

Returning to your article, you will note that when the editorial committee changed "may be unassignable to a family" to "are not assignable to a family", Schopf was a member of that committee and I (happily) was not! However, I totally agree with the edited version, and on that particular score, with him. The distinction was intended to be precisely on the concept that form genera were "unnatural" (i.e. to be excluded from Meyen's category of N-genera, that is, like living ones). They were peculiar of course in that some of them were so ill-defined - like *Taeniopteris*, for example, that they could not be placed in a family. Many, including that, but others such as *Dadoxylon* were evidently including material which probably belonged to more than one family - in this case, Araucariaceae and Cordaitaceae (Cordaitanthaceae of Meyen) and Rufforiaceae (see Meyen p. 189). This of course has no counterpart in any living genus! Your citation of the current use of *Cladophlebis* makes the same point.

1987, where he naturally cites his joint paper with Traverse of 1979) sets out with typical clarity the five "main distinctions between the taxonomy (and nomenclature) of living and fossil plants" on p. 11. They are valid, and require that we do not pretend (as Al Traverse suggests in his May 27, 1996 letter to me, copies to you both) "that exactly the same rules can be used for naming fossils as for naming liverworts". Meyen was well aware that this does not wash, and indeed categorises all fossil genera as "N" or "F", the precise equivalent of organ ("Natural") or form genera!

article (3.3) has just explained what form-genera are, so that it seems totally redundant to explain that the Code does not prevent you from using them ! Examples of form-genera have been given in the previous few lines under Art 3.3 - how could anyone ever have thought that the Code "precluded the use of form-genera of fossils" ??

So, if you have had patience enough to read this far, you will see that I am not entirely happy with your proposals ! However, I am sure that the gap in our thinking is much smaller than it may seem. (I am not sure whether you will be pleased or distressed with that analysis !). I offer this as a first thought - you deserved it some days ago, having faxed your article, but alas other activities intervened.

Do please submit your proposals if you wish, but you will understand that I do not wholly support them. However, I am sure that should not, and I hope will not, deter you. We can have a lively debate in St. Louis perhaps ? But I fear the Nomenclature Section might not have the stomach for it.

I hope we may continue this serious consideration of how to sharpen up the Code, and indeed the new BioCode derived from it.

With best wishes,

Yours sincerely,

*W.G. Chaloner*  
W.G. Chaloner.

Copies (mailed):

Fensome  
→ Traverse

Dear Al - this is really a belated reply (in part) to yours of May 27 - esp. concerning the abolishing of the organ genus! (Have you seen the Draft Biocode's use of organ genus - interesting that its ed. cttee, including Werner and others well versed in the ICBN did not think it was "abolished"!

PROFESSOR W. G. CHALONER, FRS  
Geology Department  
Royal Holloway University of London  
Egham Hill, Egham  
Surrey TW20 0EX

**COPY**

Prof. David Hawksworth,  
Chairman, International Committee  
on Bionomenclature,  
IMI, Egham, Surrey, UK.

Dec 26 1996

Dear David,

**SUGGESTED EMENDATIONS OF ITEMS DEALING WITH FOSSILS IN  
THE DRAFT BIOCODE OF AUGUST 1996.**

I hasten to offer you some emendations to the Draft Biocode, before your deadline of Dec 31 1996 runs out ! I apologise for leaving it this late, but my comments really come from re-thinking some of the finer points of the matter as a result of reading an article in the IOP Newsletter 59 (December 1996) by Judy Skog and Rob Fensome. In that article they state their intention to propose some changes to the ICBN (of which they had kindly faxed me a copy earlier) and remark that "the draft BioCode is unacceptable in its present state, and that the Committee for Fossil Plants suggestions on the first draft have not yet been incorporated into the Draft Biocode".

This leaves us with the rather untidy situation of having parallel changes being suggested (formally) to the ICBN, to catch the St. Louis Congress, coupled with revision of the Draft Biocode running alongside. I guess it will be the job of your committee to receive and act on suggestions from Rob Fensome's committee, and perhaps glance sideways at my comments, dealing with related matters, which I set out below ! As you will see, I am copying this letter to the several people who might be interested, including Werner and John McNeill as the two botanical members of your committee.

**NOTES ON DRAFT BIOCODE: PREAMBLE, ARTS. 3, 11, 19 and 36.**

I am warmly in favour of the **Preamble** and **Art. 3** in their bringing together the nomenclatural awkwardness shared by pleomorphic fungi, fragmentary plant fossils and ichnotaxa. They are helpful, in that they contain an indirect invitation to argue by analogy from one to the other in cases where ambiguity may seem to exist. (As you well know, earlier editions of the ICBN from Leningrad ('78) on back had an explicit linking of fossil form-taxa and the fungal pleomorphic problem in the (then-) Art. 59. But palaeobotanists, and especially R. Potonie who tried to apply the concept, found great difficulty in deciding exactly what the equivalent of the "perfect state" was in fossil vascular

plant nomenclature !)

**Art 11.1 and 19.7** are derived, helpfully, from the existing IBCN.

**Art. 36.2.** I have problems with this in that it seems to say that organ-genera are form genera ("Names of organ-taxa ....are names of form taxa"). Since it is later explained that organ-taxa can be assigned fully to higher taxa, they are not then in my understanding "form taxa". (See the definition of organ-genus, and organ taxa in your Draft Glossary, with which I am very happy !) Instead of the present form of that Art., why not omit the comment that they are form taxa, and simply lead on :

" Names referring to specific organs of fossil botanical taxa ..... pleomorphic life history, are applicable only to the organ or morph represented by their type. They cannot be applied to the whole fossil **plant** or to the fungus in all etc. etc. ..."

**Art 36.3.** There are problems here in referring to "the whole fossil organism". There are precious few "Whole organism" fossil vascular plants; what we have to deal with is a leaf connected to a stem, or a sporangium to a leaf, or a seed to a sporophyll. The present Draft's phraseology is in that sense inappropriate. (Of course we all accept that many names based on fossil organs, rightly or wrongly, have been referred to whole organism *living* genera but that is another matter; those are not, for me, "whole organism fossil genera" since they are based on extant, "living", "non-fossil" types even when fossil species are placed in them!).

I suggest modifying the present version to read (lines 3 - 4) ... " even through the holomorph, or fossil specimens linking that organ with other organs may be known, and they bear different names." OR something to that effect. Reference to "Whole fossil organisms" introduces an air of unreality to the BioCode !

Continuing with that article, the attribution of a given name to the category of organ-genus or form-genus is inevitably subjective. The suggestion that the author can decree whether it shall be a form genus or organ genus for all time is misleading. All that can be said is that " The name of a fossil botanical etc. ....is essential in establishing whether he regarded it as an organ-genus or form genus." It is up to other authors to follow him in that concept, or treat his taxon otherwise !

The last sentence must be deleted ! You cannot transfer an epithet based on a seed or a pollen grain to "a genus of whole organism fossils" (even if such exists, which as you gather, I doubt !). To do so would be in total conflict with Art. 36.2 ("These names are applicable only to the organ... represented by their type, **not to the whole fossil...**") If you want to have a name for a whole organism, you cannot expand the concept of the species based on a single organ to encompass the whole; you must make a new name for the whole, and leave the original epithet with its original, limited meaning.

That is not to say that you cannot assign a fossil species based

on e.g. leaves or fruit alone to a **living** genus (e.g. *Ginkgo huttonii*, leaves of a Jurassic Ginkgo, or *Nypa burtini*, fossil *Nypa* fruit; but that is not as I read it in conflict with Art. 36.2 if emended as above).

Finally, I like **Art. 36.6**, coming from the Zoological Code, but relevant to palaeobotanists in dealing with the nomenclature of plant-feeding ichnofossils (bites out of leaves !).

If you wanted to talk about any of this I am around for the next few weeks in London, more often at 0181 748 3863 than my College number above, but daily on e-mail (w.chaloner@rhbnc.ac.uk)

Best wishes for 1997 !

Yours sincerely,

William G. Chaloner.

Copies: Rob Fensome  
Werner Greuter,  
John McNeill,  
Dan Nicolson,  
Judy Skog,  
Al Traverse,

THE PENNSYLVANIA STATE UNIVERSITY  
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E-Mail: traverse@ems.psu.edu

27 May, 1996

Professor W. G. Chaloner  
Geology Department  
Royal Holloway, Univ. London  
Egham, Surrey TW20 OEX, UK

Dear Bill:

Yours to Dan Nicolson, dated 26 April, 1996, was in the mailbox when I returned this a. m. to the office after spending most of May in Korea and Japan. I suppose it is now a little late to comment, but, what the heck, why not?

To my mind, you are at least as confused as others you cite regarding form and organ genera, etc. To say that the organ genus was not "...in some way abolished..." is absurd, when the context is ICBN. Of course, it was not abolished from history or from the minds of those who liked it, etc., etc. But it most certainly WAS abolished from official nomenclatural usage.

The Code provides a means of efficiently handling nomenclature, and it is and always has been my position that exactly the same rules can be used for naming fossils as for naming liverworts (I seem to recall that Linnaeus even named rocks and minerals the same way). All efforts to get into "referability to family," "forms," "organs," etc. were misguided efforts to introduce taxonomic concerns into naming of fossil plants. I gather that you would even like to stick in stuff about "whole plants." This is not to say that such matters are wrong or of no concern, but (in my opinion) they don't belong in ICBN, which should be a set of instructions about handling nomenclature, PERIOD. God knows the business is complicated enough without muddying the water with arcane matters about organs, forms, whole plants, and such. They belong in textbooks about paleobotany but not in a code of nomenclature. I don't even like the suggestion that names of fossil plants be designated per se as "fossil" (maybe with a required scarlet letter "F" ?). Fungal taxa aren't labelled "myco-genera," etc., and they aren't even plants, for crying out loud.

Yours very truly,

Alfred Traverse

c: J. Skog, R. Fensome, D. Nicolson

Dr. Dan Nicolson,  
Department of Botany,  
Smithsonian Institution,  
Washington DC  
**FAX 001-202 786 2563**

April 26 1996

Dear Dan,

**General Committee Post-Tokyo Circ. 3**

My scored ballot follows on the next page, shortened by removal of the offending re-ballot to save fax pages !  
I am unhappy about some of the comments coming from the fossil plant committee - Judy Skog's interpretation of the organ-genus/form-genus useage in particular. To say "form genus is the accepted category" and that "the organ genus has not been missed" just confuses the issue ! The organ genus was not in some way abolished when the organ genus was dropped from the Sydney Code. It was simply accepted that organ genera would be treated in every way nomenclaturally as other genera are, and therefore no special recognition of their status was needed in the Code. Accordingly in Art 3 we enumerated as "not form-genera" several names of fossils (*Mazocarpon*, *Lepidocarpon*), which are patently not genera of whole plants, but are of course organ-genera, based on single organs, and which have been assigned to families. When Judy says "there is really not a good reason why (a) form genus could not be assigned to a family if possible" this is just a riddle. Of course if an author decides to assign a given form-genus to a family, he is then treating it as an organ-genus - that is, a genus in the useage of the Code, and so **not a form genus**. (This is absolutely implicit in what is stated in Art 3.3) The statement that "for the moment, form genus is the accepted category" (as though that had somehow displaced organ genus) is equally unacceptable; for if by that she means that this is the designation for all fossil plant genera, then none of them can be assignable to families !  
The suggestion that she suggests as a compromise, of using the term "fossil genus" to somehow improve on either form genus or organ genus just confuses the issue. Both are fossil genera, that is to say they are based on plants which are fossils. (There are also plenty of fossils which have of course been placed in non-fossil genera, that is genera based on extant plants !)  
Really none of these issues is particularly relevant to the matter of a single unified code. It is a pity that that has simply given the excuse the raise some of them again. In any event, Judy may well perceive these remarks as just the sour grapes of the guy who has handed over as chairman of the Fossil Plant Committee, and perhaps she would be right !

I will copy this page alone to Judy Skog, Rob Fensome and Al Traverse, for obvious reasons.

*W. G. Chaloner*  
Bill Chaloner

Dr. Dan Nicolson,  
Botany Dept., MRC-166  
Smithsonian Institution,  
Washington.

FAX 001-202-786 2563

Nov. 8 1995

Dear Dan,

CG Circular 2 - Ballot.

I am (as usual) saddened and ashamed by my inability to keep to Gen. Com. deadlines ! There always seem to be other items in my working life which intrude with larger more threatening penalties for non-compliance with deadlines - or things like refereeing papers where I know the authors are waiting indignantly for some response. And so on.

I phoned yesterday to try to offer verbal apologies, and then failing to get you, decided to fax instead.

I was in touch with David earlier about the Glossary, and there are still a few items there which I would like to see modified (notably "artificial") , and this I will deal with directly to him. I assume the relevant deadline there is in time for the Commission in March 1996 ?

I return to your plea for comments on Bionomenclature which were I know needed "before the end of Sept." I naturally look to things with a fossil angle to them, but realise that the serious response on this front should come from the Special Committee for Fossil Plants, of which I am no longer a member anyway ! I assume from Circular 2 that the Draft was put to them, - I guess by Judy Skogg - and I hope they may have responded?

In the Draft I am concerned for my own part only with the phraseology relating to form taxa in Art. 3. I believe that it should not read: -

" Taxa that do not **consist of whole organisms** but of particular parts of organisms" but rather -

"Taxa that are not **based on whole organisms** but on particular parts of etc. etc."

As I construe it, these form taxa (whether fungal or fossil) do not **consist** of part of an organism, but rather the taxon is **based on** part of an organism. We have a tangible thing, the organism (or part of it), the taxon which is a man-made abstraction based on the tangible, and the name of the taxon, which is also an abstraction. I think this needs putting right.

However, I like this Art., which goes further than ICBN Art. 3.3 and 3.4 in explicitly bringing together fossil plants and fungi, which becomes an invitation (which is not explicit, nor

/continued



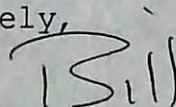
should it be) to argue by analogy from the one to the other ! As you know, some earlier codes had that implication, which was later taken out.

I also like the parts of Art. 35 which face several fossil issues head-on in a useful way. But I am doubtful about Art. 35.3 in the phrase "even though the whole fossil organism may be known and have been named". The point is that virtually no whole fossil organisms are known; the reality is that we have a name based on a fern leaf, and another based on the structure of the sorus or sporangia (i.e. classically on "vegetative" and "fertile" states respectively - a concept relevant to the fungal situation) and the intention is that even **when reproductive parts are found linked to vegetative parts, both names can still be used** for their respective parts. This has not by any means involved a "whole fossil organism". I am not making a very helpful suggestion here; it would need to be something like "even though separately named parts were found in connexion with one another", but it perhaps needs further thought.

These are just a couple of small items - there will surely be more forthcoming when other palaeobotanists think through the significance of these phrases. In the meantime, I will send a copy of this to Al Traverse (as ex-Sec., Fossil plants) and Judy Skog as current Sec. hoping that their committee is deep in contemplation of these matters !

Again, many apologies for the lateness of this response.

Yours sincerely,



William G. Chaloner.

Copies: By mail:-  
Al Traverse ~  
Judy Skog

THE PENNSYLVANIA STATE UNIVERSITY  
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435 DEIKE BUILDING  
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29 July, 1994

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

Dear Bill:

Thanks for the reprint of the Beerling & WGC contribution re stomatal density---fascinating and important.

In case you don't see PSB, I enclose a tearout I wanted you to have. Marvellous attack on the required Latin diagnosis.

Best to you and Judy.

Yours very truly,

Alfred Traverse

encl.: Tearout

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30 May, 1994

Dr. W. G. Chaloner  
School of Life Sciences  
Dept. of Biology  
Royal Holloway, Egham Hill  
Egham, Surrey TW 20 OEX  
U.K.

Dear Bill:

Just read your Yokohama "Speech" (and attendant statements) on pp. 164-5 of Englera. Congratulations again! Paleobotany paradoxically is the only up-to-date group. Some of the pro-Latin crowd continue to make asses of themselves in print--but at least dear A.C. was not there to thunder that the Latin requirement is great because it "disadvantages us all equally." (It wasn't true anyway, as .e.g., Romanians can handle Latin more easily than can Turks.)

All the best.

Yours very truly,

Alfred Traverse

AT/et

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13 May, 1994

Dr. W. G. Chaloner  
School of Life Sciences  
Dept. of Biology  
Royal Holloway, Egham Hill  
Egham, Surrey TW 20 0EX  
U.K.

Dear Bill:

Be sure to send a reprint of the Beeling & Chaloner paper on stomatal density (RPP 81: 11-17). I use this info for one of my paleobotanical lectures. One of these days I will get out a general mailing of my most recent stuff--never seem to have the time!

Best as always.

Yours very truly,

Alfred Traverse

AT/et

THE PENNSYLVANIA STATE UNIVERSITY  
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2 December, 1993

Prof. William G. Chaloner  
Biology, Royal Holloway BNC  
Egham, Surrey TW20 OEX  
UK

Dear Bill:

We found a package from Phil Holmes re his proofs here on return, so that mystery is solved. Unfortunately, his chapter is the only major problem item in the whole book, so it is very unfortunate that it has taken so long to get the matter resolved. He had sent it over here by surface mail. It was postmarked mid-October.

As we left Castle Holloway on Monday at 6:00 a. m., the security man drew us a marvellous map of how to solve the problem of getting the M25 toward Gatwick, not toward Heathrow--involves going over the Thames and then doubling back. However, he needn't have bothered, and I thought I'd relay the info, to benefit the next visitor: as a BIG exception, it was beautifully signed. Absolutely no problem.

Another matter of possible interest. We found a Chinese restaurant on the High St. in Egham, near the A30 roundabout, which was excellent. We hadn't found it on Saturday night because it wasn't yet open. (Opens at  $\frac{8}{7}$ :00 p. m., or so.)

Another piece of news: there was nobody at the Egham station to take our tickets on departure (unlike Barnes, where that Nigerian was all over us). So, whatever you put in the machine for the "right to travel" was all that British Rail got out of it. I felt guilty, but what could I do?

It was great fun to see you and Judy again, and we reiterate, daß Ihr an der Reihe seid, uns zu besuchen! (I really meant not intending to go to meetings--have to pay personally, for one thing.)

Yours very truly,

Alfred Traverse

PENNSSTATE



The Pennsylvania State University  
Department of Geosciences  
503 Deike Building  
(814) 863-7823 [FAX]  
Date: 5.XI.93

Mark here if urgent reply is requested.

**Fax Transmission to:**

Name: Prof. W. G. Chaloner  
Company/Institution: Royal Holloway, Univ. of London  
Fax Number: 44-784-471739  
Phone Number: 44-784-435551

**Transmission from:**

Name: Dr. Alfred Traverse  
Phone Number: 814-863-3419

**Total Number of Pages (Including Cover Page):** 1

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**Message:**

Dear Bill:

Phil Holmes never responded with author proofs of his book chapter for my sedimentation volume for CUP. He hasn't answered our various prods. Do you know his whereabouts? (Most recent info we have is a year old and puts him in Harrow.) The proofs were supposed to (and still should) come to me. It's pretty late, but I'd still like to see what he found.

Best.

Yours very truly,

Alfred Traverse

\*If any portion of this transmission is unclear, please call \_\_\_\_\_.

PROFESSOR W. G. CHALONER, FRS  
Biology Department  
Royal Holloway University of London  
Egham Hill, Egham  
Surrey TW20 0EX

Dr. Alfred Traverse,  
Dept. of Geosciences,  
Penn State University,  
University Park, PA, USA.

Fax: 0784 471739

Oct 25 1993

FAXED TO: 0101 814 863 7823

Dear Al,

Many thanks for your fax of Oct 21. I received it just as I was heading out of London for a meeting of our Natural Environment Research Council - an activity which now claims about one tenth of my time - and this is the first chance I have had to reply.

I am running this off at home, but when I get into College I will enter the Zhou address here. As I understand it, he succeeded H.H. Li as the principal (chief?) palaeobotanist at the Palaeontology Institute of the Chinese Academy in Nanjing, so that his address will be that of Li. In any event, I will insert it here: *PROF. ZHI YAN ZHOU, Nanjing Institute of Geology and Palaeontology, Academia Sinica, CHI-MING-SSU NANJING 210 008 P. R. CHINA.*

Regarding the production of the new edition of the Code, I was placed on the Editorial Committee again, on the whim of the two Raporteurs (Greuter and McNeill) who by tradition seem to run these things. As you know, this is my third round on that committee. One tradition seems secure and well-founded, at any rate - that is, that the members of the Committee must have been present at the Nomenclature Section, and so "heard the discussion". We are scheduled to meet in Berlin just after Christmas, fares paid by IAPT, plus rather basic accommodation in the Berlin Botanical Garden. "We" includes of course your countrymen Dan Nicolson and Paul Silva, and Rob's countryman Mc Nail. The Committee will be listed in the November Taxon, (which you will see before I do?) along with membership of the other Committees elected in Yokohama. Since your own internal revision has already replaced yourself with Judy, (as your copy Fax to Dan Nicolson shows) that might catch the press on that Nov. Taxon issue?

So the decision on Rob's involvement in editorial activity is taken out of his hands and mine! I must say that January is not my favourite time to spend a week in Berlin, but it is always an interesting place, and I have not been there since the Congress of 1987. Indeed, it will be my first post-unification trip there.

My memory is that you are still a member of the General Committee of IAPT, as it was enumerated at Yokohama, but perhaps that is ex-officio membership as Sec., Fossil Plant Committee; if so, Judy then replaces you in that capacity? You might usefully clear that up with Dan Nicolson too.

I trust our retreat from the Committee will not diminish our own personal contacts? Who knows, they might coopt us back on again? I certainly hope that we shall both be present at the St Louis Congress in 1999, even if only just to goad the activity of the new Committee and the Nomenclature Section!

*W.G. Chaloner*  
W.G. Chaloner.

Maybe you would copy to Judy and Rob if you rate it worth it?

Faxed to: 44-784-471739

THE PENNSYLVANIA STATE UNIVERSITY  
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21 October, 1993

Prof. W. G. Chaloner  
Department of Biology  
Royal Holloway & Bedford New College  
Egham, Surrey TW20 OEX, UK

Dear Bill:

Well, the Fensome-Skog-Traverse two-day conference has occurred, and went very well. Among other things, it was decided that Judy will succeed me immediately as Secretary of the the Committee for Fossil Plants. However, Rob and Judy thought that I should get one final report (swan song, or when the fat lady sings) to the Committee, announcing this fact and commenting on what happened in Yokohama, as outlined by you. (We went over, item by item, your marvellous record of the voting results.)

In connection with this, I need an address for Zhou Zhi-Han (PRC). I hope you can provide by fax or return mail.

All of this is a little difficult emotionally. Reminds me of my last Sunday in church.

Best wishes, also to Judy, please.

Yours very truly,



Alfred Traverse

c:Fensome, Skog, Nicolson

P. S. Rob, who is sitting with me, asks me to inquire which of you will be responsible for work on the new Code. He is hoping it is you, because the international travel might be difficult for him to swing.

13. J. Walker (Australia)
14. Zhuang Wen-Ying (PRC)
15. G. Redeuilh (France)

Committee for Fossils

1. Chair: R. Fensome (Canada)
2. Secr.: A Traverse (USA)
- Member
- 3. S. Archangelsky (Argentina)
- 4. K. Faegri (Norway)
- M - 5. Fairon-Demaret (Belgium)
6. J. Jansonius (Canada)
7. D. J. Nichols (USA)
8. F. Schaarschmidt, F. (Germany)
9. J. E. Skog (USA)
10. B. S. Venkatachala (India)
- 11. D. Batten (UK)
- 12. G. Playford (Australia)
- 13. R. Ravn
14. V. Wilde (Germany)
15. Zhou Zhi-Han (PRC)

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4. W. G. Chaloner (UK)
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6. P. M. Jørgensen (Norway)
7. D. Hawksworth (U. K.)
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16. R. E. G. Pichi Sermolli (Italy)
17. H. M. Burdet (Switzerland)
18. G. Smith (S. Africa)
19. Z. Iwatsuki (Japan)
20. A. Anton (Argentina)
21. P. Holmgren (NY USA)

Rapporteur Général

W. Greuter (Germany)

*Don H. Nicolson  
Chair Secretary  
Nomenclature Committee*

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23 July, 1993

Professor W. G. Chaloner  
Dept. of Biology  
Royal Holloway & BNC  
Egham Hill, Surrey TW20 OEX  
England

Dear Bill:

Your fax of earlier today just received by me (i. e., picked up out of the mailbox). I'll rush this to the P. O. personally in the hope you'll get it sooner--I think the PSU postal "system" is partly responsible for delays.

Regarding Bob Ravn: he's a pollen man. He's had a longish career with oil companies (BP--he was stationed at their research centre in Middlesex, UK; Sohio; Standard, Amoco, BP-Alaska). Recently, I can only guess as to reasons, he left BP and set up his own company in Alaska: his current address= Dr. Robert L. Ravn, Aeon Biostratigraphic Services, 1013 N. Dimond Blvd. #228, Anchorage, Alaska 99515. (Dimond is not misspelled.)

He was nominated by Nichols. I've never met him, but have been in touch for years. Long ago I helped him with a Carboniferous paper, which is quite good. Most recently I have edited two longer papers of his for Palaeontographica, on Early Cretaceous palynofloras. He is knowledgeable about nomenclature and enthusiastic about being on the committee.

Oh, yes, he's currently president of AASP.

All the best.

Yours very truly,

c:Fensome

Alfred Traverse

P. S. I faxed you day before yesterday about another matter--i. e., 21 July. Worrisome that you don't mention it today. I'll fax again just before you leave, per your instructions, which are just what I proposed in my fax. 15

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9 July, 1993

Prof. William G. Chaloner  
Department of Biology  
Royal Holloway & BNC  
Egham, Surrey TW20 OEX  
England

Dear Bill:

You're right about 1789 date for family names. However, one would have to be careful about not introducing an internal contradiction with language of current Article 13.

Enclosure for your amusement re flooding at Pattee Library.

Glad you liked story about the wood caper in the mountains. There's more. After Ms. Smith ("I'm the Wood-Smith," her business card announces.) left I was faced with the huge task of throwing the pieces of wood down the hill, then stacking them down there (two cords is a lot of wood). To keep myself amused in the pouring rain, I counted the pieces out loud in Turkish. When you get over 100 that gets a little hairy and sounds very funny (2124 is: "iki bin yüz yirmi dört"). Apparently I was overheard because my agent mentioned it in a conversation yesterday.

All the best.

Yours very truly,

Alfred Traverse

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25 April, 1993

Professor W. G. Chaloner  
Royal HBN College  
Department of Biology  
Egham Hill, Surrey  
England TW20 OEX

Dear Bill:

A fax is just in from Volker Wilde which I hasten to share with you. I would like to nominate him for the CFP. You will note that he agrees with me about Chris H-F. The latter has not only not completed his doctoral work, though he has abundant opportunity to do so, but he has more or less thumbed his nose at it. That's all very well, but doesn't sit well in D-land. Volker would be excellent.

Thanks for the commiseration card from Florida. Our weather is almost always lousy. Surely you remember that. It's always too something--cold, hot, wet, dry, windy, cloudy, to mention a few of the "too" possibilities. We had six inches of snow just three days ago (gone now), and tomorrow will be 80 perhaps. We'll put the air conditioners in next Saturday.

Hang in there.

Yours very truly,

Alfred Traverse

encl: letter xerox

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13 April, 1993

Professor W. G. Chaloner  
Biology Department, Huntersdale  
Royal HBN College  
Egham Hill, Surrey TW20 0EX  
England

Dear Bill:

Fun to talk to you from the Keys the other day and to contemplate that you were .5 as far away from here as you are when you are at home.

Thanks for the IOP blurb. I should have had an acknowledgment. However, you sounded so negative on the English alternative to Latin that I was startled to observe that you voted FOR the proposal on your ballot. Did you err?

On closer examination of the flower stamps I discover that they are, indeed orchids. I have a senior undergrad doing a little research project on orchid pollen this semester, and he spotted the stamps. He's been raising orchids since he was 5, or so he claims. I also note that there aren't three different stamps, only two. 18 and 24 pence, on your letter. I would appreciate (cancelled would be fine) copies of other members of the set. When at your college one of the most interesting things I found out about is that A. Scott is a professional philatelist (literally). He was able to print out for me from his computer an annotated list of all stamps illustrating fossil plants. I was delighted to be able to point out an omission--the Archaeopteris on a Canadian stamp of a couple of years ago.

Warm greetings.

Yours very truly,

Alfred Traverse

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23 February, 1987

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

Dear Bill:

Who is "Clifford", whom Brummitt ~~s~~ seems to be authorizing to "do"  
the author abbreviaitions for  $\phi$  fossil plants? Is he/she o.k.?

Best. More, ad seriatum, follows.

Yours very truly,

Alfred Traverse  
Professor of Palynology

— AT/et  
encl: copy of letter to Brummitt

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1 February, 1993

Professor William G. Chaloner  
Department of Botany  
Royal Holloway & Bedford New College  
Egham Hill, Egham  
Surrey, England TW20 OEX

Dear Bill:

As you will perhaps know by now, the letter, ballot & questionnaire went out to the Committee last week.

The purpose of this is to let you know that I did send a copy to Rob Fensome. I am suggesting that he go ahead and vote, and I wonder if you should not simply co-opt him now, so that he is "officially" a member of the Committee.

All the best.

Yours very truly,

Alfred Traverse

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19 January, 1993

Dr. W. G. Chaloner  
Department of Biology  
Royal Holloway & Bedford New College  
Egham Hill, Egham  
Surrey, England TW20 OEX

Dear Bill:

As you know, if you got the package about CFP mailout, Rob Fensome has decided after much soul-searching to accept the chairmanship, if proffered. I think he would be very good. He almost certainly can't make it to Japan, but he points out that he returns "home" to UK a couple of times a year and could easily visit you to accomplish the changing of the guard.

He says he could accept Doug Nichols as Sec., but it is clear that he is lukewarm about it. He says he personally would like David Batten. I have no real objections to that, except one that has just occurred to me and which applied also to Nichols. Is it a good idea to have both officers be microfossil persons?

That brings me full circle to Judy Skog. Dan Nicolson called me the other day to forward her cause. As I told him, that was my idea in the first place, but I hadn't been impressed with Judy's enthusiasm for the job. Dan said I had misinterpreted the responses she gave me. I suppose that is not surprising, as I still misinterpret Betty's statements constantly.

Judy has the advantage, among others, of being a megafossil person. It is also true that I could effect the transfer of files ,etc., to her very easily. Fensome knows about her and says it would be o. k. with him. The decision is going to be up to you, as you clearly will be making the nominations at Tokyo.

All the best. Regards to Judy. (J. Chaloner, that is.)

Yours very truly,

Alfred Traverse

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19 November, 1992

Prof. W. G. Chaloner  
School of Life Sciences  
Dept. of Biology  
RHB New College  
Egham Hill, Egham,  
Surrey TW20 0EX, U.K.

Dear Bill:

Please communicate. Where do we stand? What did you think of my last letter--re DJN, RF, etc.

Enclosed copy of recent blurb to Hawkesworth is for your info.

Best to Judy.

Yours very truly,

Alfred Traverse

AT/et

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12 October, 1992

Prof. W. G. Chaloner  
School of Life Sciences  
Department of Biology  
RHB New College  
Egham Hill, Egham  
Surrey TW20 0EX, U.K.

Dear Bill:

Haven't heard from you yet about my recent letter re Fensome, etc. Here are copies of a recent correspondence exchange between us that might interest you. I look forward to hearing more from you about where we go from here.

Best.

Yours very truly,

Alfred Traverse

AT/et  
encl: copies of correspondence with Fensome

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1 October, 1992

Prof. W. G. Chaloner  
School of Life Sciences  
Department of Biology  
RHB New College  
Egham Hill, Egham, Surrey  
TW20 0EX, U.K.

Dear Bill:

Just got from Dan Nicolson Gen. Comm. Circ. 13, copy of p. 1 enclosed. As you will note, there is an implication of inaction by me (hi-lited). Did you ever get my letter of 20 August with a xerox of Nicolson's "1st draft printouts, etc."? I was waiting to get your opinion before I contacted Dan about our little part of this list.

Best.

Yours very truly,

Alfred Traverse

AT/et  
c: D. Nicolson

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**faxed to 44-784-437520**

24 September, 1992

Dr. W. G. Chaloner  
Dept. of Biology  
RHB New College  
Egham, Surrey, UK

(pg. 1 of 2)

Dear Bill:

Sorry about the delay in answering your fax of 14 Sept. Since receiving it I have kept my palynology course humming (17 students), and have made my Fall visit to close our place in the Adirondacks for the winter (Betty couldn't come because of other obligations--man, is it hard to get 6 mattresses into plastic mattress bags without assistance!).

I share your concern for the future of the Committee, of course. It is good you were able to confer with and about possible new faces. Of your a-d, I know Fensome only by correspondence, but he seems excellent (and even Greuter is impressed by his knowledge of the Code!). I wonder even if he should be the new chairman? As you know, I promoted the cause of Doug Nichols for chairman, and you and I were more or less agreed he was the logical choice. However, I think it is critical that the heir-apparent be at Tokyo. I have so far met only slammed doors as far as (NSF) funding for nomenclaturalists to go to Tokyo is concerned, and Doug doesn't seem able to swing it through USGS. It doesn't look encouraging. Doug was born in 1942 and is therefore 50 years old. In 1999 he would be 57 (a child!). Regarding Friedemann, he is both older (born in 1934) and in rather questionable health (two heart attacks in previous years, serious cancer surgery this year). I do not think he is a viable candidate, if for no other reason than that he will presumably be retired by 1999, if not before. He is a very good man, a valued and dear friend, etc., but that is beside the point. Judy Skog has expressed only (very) mild interest in succeeding me--I respect Judy a lot, but I'm no longer of the opinion that she's a logical candidate, though there's no doubt she could do the job, and maybe another person could convince her.

About "Hartman-Froder"--you must be referring to Hartkopf-Fröder. He is very good, and he and I have a sort of joint project going. However, he is "only" a Diplom-Geologe! He is still a Doktorand and, despite my constant proddings, doesn't seem to be able to get his Ph.D. dissertation done. I don't believe we would have any credibility in Germany if we appointed him, until and unless

he completes.!

Now to summarize--the one guy-gal on the list for whom I have great enthusiasm and for whom I know of no drawbacks (such as no travel money) is Fensome. As I've previously said to you, I think only one of us should step down at Tokyo, and one should remain to counsel the new person. Because you seemed pretty much committed to Tokyo resignation, I had suggested I stay on as secretary to counsel the new chairman, with the idea it would be Nichols (it never occurred to me that USGS wouldn't fund a chairman/elect to attend the congress!). Under these new circumstances and if Fensome's willing, maybe either:

1. You should stay on for a couple of years and work Fensome in as new secretary (I could help too), or

2. I should stay on for a couple of years, help Fensome become established as chairman, and let him find a compatible secretary (Skog?) for himself.

It is tempting to phone Fensome to discuss all this, but I won't until I get reaction from you. One thing we must know is if he'll assure us of 99% certainty of attendance at Tokyo.

I agree that we need to get rolling, though there is relatively very little of the traditional business of the Committee. (I still haven't got a 2nd Circular from the Congress itself!)

Best regards.

Yours very truly,



Alfred Traverse

AT/et



UNIVERSITY OF LONDON

**ROYAL HOLLOWAY AND BEDFORD  
NEW COLLEGE**

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

Department of Biology, Huntersdale

18/LETAT/WGC/cye

14th September, 1992

FAX to:
 Dr. A. Traverse  
 Penn State University

Dear Al,

I am writing this in an interval in the Aix conference, and I will have it typed and faxed to you as soon as I get back.

I naturally took my chance of talking to (selected!) people here and in Paris about the Fossil Plant Committee. Although you and I discussed this along with other matters, I think it would be helpful if we put our thoughts on paper - indeed, perhaps we should have taken Minutes!

When we spoke, we reviewed the need to have active nomenclaturally well-informed people to take over our roles as Secretary and Chairman of the Special Committee for Fossil Plants. Ideally, they should be available to go to Tokyo, and see how it all works, and thus to be nominated and (presumably) formally appointed/elected by the Nomenclature Section at the end of Tokyo.

I have spoken to Judy Skog and Friedemann Schaarschmidt in Paris and to Doug Nichols and Jan Jansonivs in Aix. For each of the first three named it was a private chat about them alone; with Jan I discussed the candidacy of the others. Jan and I also spoke of Bob Fensome (Nova Scotia) and Christopher Hartman-Froder (Krefeld) as possibles. Fensome was not here, but I met Chris H.-F., and I am impressed with him.

In brief summary, I would offer the following possibilities on which Jan and I agreed:

	CHAIRMAN	SECRETARY
1st choice	a. Doug Nichols	b. Bob Fensome
2nd choice	c. Schaarschmidt	d. Judy Skog

It would seem that a, c and d would all have some difficulty in getting to Tokyo, but that their prospects of doing so would be enhanced by a formal committee proposition for their nomination. (I presume we could move towards this by a Committee ballot, as soon as possible?).

Jan also spoke warmly of Hartman Froder as a possible Committee member. That is fine by me.

Continued/.....

Tel: 035504

Fax: 0784 471739

- 2 -

Dr. A. Traverse  
Penn State University

14th September, 1992

Jan is keen to remain on Committee, planning to quit after Missouri, 1999. It is my intention to resign for - or at - Tokyo, as I gather you do. Our two offices should be opened up for younger people.

In that connection, what is the age of Doug? I do not know, but wonder whether he will still be working in 1999?

If you would respond to this soon, that would perhaps set the wheels turning to get a committee view on this - as surely we must do before Tokyo, so that we can present a new "slate" for the Committee at the 1993 Congress?

The meeting here has been great on atmosphere, climate, company and papers but weak on organisation!

Your letter of the 20th August, 1992, which was on my desk for my return will receive my attention as soon as possible.

Best wishes,

Bill

W.G. CHALONER

Faxed To: 0044-784-471-739

Prof. W.G. Chaloner  
RHB - New College  
Egham Hill, Surrey, UK TW20 0EX

EMPFÄNGER : 0784 471739  
SENDER : SNG KUHWALDSTRASSE  
DATUM : 10 JUN '92 10:26  
DAUER : 00'40  
MODUS : STAND  
SEIT. : 01  
ERGEBNIS : OK

Dear Bill :

Your book about J.C. (so to speak)  
& der Spiegel is on its way to you.

Do you know anything about this guy  
(see below)? There is a chance  
of a co-project, and I wonder  
what you know. Birkbeck College caught  
my eye.

Andreas Koeppen  
Geologisches Institut  
Universität Heidelberg  
Im Neuenheimer Feld 234  
6900 Heidelberg

Sehr geehrter Herr Prof. Traverse,  
wie in unserem Telefongespräch  
Erläuterung meines Projektes

besprochen, möchte ich Ihnen eine kurze  
mitteilen, jedoch zuerst mich kurz vorstellen.  
am Ende sagte er: Ich gratuliere!  
für einen Amerikaner ist Ihr Deutsch  
relativ  
gut.

Ich habe an den Universitäten Würzburg (Vordiplom), Aachen (Diplom) und  
am Imperial College, London (Imperial College International Diploma) studiert.  
Danach schloss sich eine 1-jährige Tätigkeit als Assistent am Birkbeck College,  
London an. Seit Februar 92 bin ich in Heidelberg als Wissenschaftlicher  
Mitarbeiter über DFG-Finanzierung zur Durchführung meines Projektes  
angestellt. Bisherige Arbeitsschwerpunkte waren:  
- Auskartierung von Faziesseinheiten im Oberkarbon (Aachen);  
- Untersuchung rezenter Karbonatsedimentation (Imperial College);  
- Well Logs und deren digitale Verarbeitung, Ground Penetrating Radar und 3-D  
modellieren (Birkbeck College);

Best, Al

Fax: 069-794-004-37



UNIVERSITY OF LONDON

ROYAL HOLLOWAY AND BEDFORD

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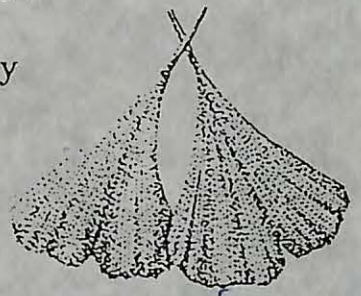
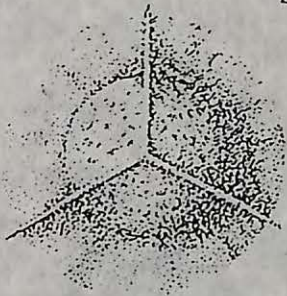
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DEPARTMENTS OF BIOLOGY AND GEOLOGY

Palaeopalynology and Palaeobotany  
Research Group

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

Dr. A. C. Scott (Geology)



May 26 92

Dear Al,

Herewith Raistrick papers.  
 Please note for legality's sake that  
 I am lending you these while  
 you write your review. I am  
 bound by Geol. Soc. copyright not  
 to "supply them to another person."  
 (I hope we can keep our conscience  
 clear!). Great to see you again  
 & have (some) time to talk. Very sorry  
 to hear of Take's death ~~while you~~  
~~received just after you left.~~  
 Keep in touch! Best wishes  
 Bill

Chaloner

Biology Fax: 0784 471739 Tel: 0784 435551  
 Geology Fax: 0784 437520 Tel: 0784 434455

Chalover

Duplicate of letter  
Royal etc. (one Scott)

Dear Bill,

20-V-92

Enclosed copy of letter to WB <sup>only</sup> (2/3 of WAC!)  
is self-explanatory and I hope o.k. I also sent  
a copy to Scott's note. Which reminds me —  
shouldn't he be on the IAPT-CEP? When  
you step down there, <sup>would</sup> ~~there~~ be no UK members.

I enclosed also a copy of a fax to J.S. Nichols,  
which I hope is o.k., and a copy of a letter to Peter,  
FYI.

I hope to get ~~see~~ your comment on the Jansonian  
chapter. I was interested to learn that ACS also  
wrote one.

Many thanks to Chris for all of her help  
during my visit. And to you for making  
it so much fun — the new "Abstecker" was  
especially nice. I was greatly impressed by  
the whole Royal HBNC setup, both intellectually  
and aesthetically.

Enclosed is a xerox for Ingeborg. Will encourage her to learn a  
little German — it's about pyritized wood structures, and I got  
it as a result of getting her a call about pyritization on  
wood: c. of letter to Nichols & Grewer, Scott <sup>product</sup>  
<sub>plate</sub>

xerox for Ingeborg

See the lab, as always.

Yours as always  
AT

P.S. You will be amused to learn that I found on  
my desk has a request from Paul Lyons to  
review 2 articles for the journal volume. My son  
told that if I'd write I could review a different  
article. My friend a list - I too believe see  
request IV 4 4

Frankfurt a.m.

18. May. 92

Charles

Dear Judy & Bill -

Just to say what a great  
time I had with you. I  
know it was a busy time, and  
you didn't really need a  
house guest. Nevertheless,  
it was a super visit, and  
I enjoyed it all.

London impressed me. So much  
of it is like a gigantic village.  
On Sunday morning at Clavis I  
was interested that there was a  
neighbourhood store where we could  
buy a paper, milk, bread, etc.  
On Sunday in Frankfurt there is exactly  
one (1) place open - the Hauptbahnhof.

My only negative impression of  
London is the insane obsession  
with automobile transportation.

By contrast, there is 50% more  
use of the U-Bahn, S-Bahn, Trams  
& buses in Berlin now than in 1977.  
Way to go! (Obviously I am  
speaking here as a "German", not  
as an American! - in USA we have  
no local public transportation  
except in a few places. Huntington,  
Pa. doesn't even have trolleys.

I will be looking for news about  
the new grandchild. I, really  
enjoyed getting to know Sarah.

See the best, and

Thanks again

Al Travençolo



UNIVERSITY OF LONDON

ROYAL HOLLOWAY AND BEDFORD  
NEW COLLEGE

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

as resp. CHALONER

(6-V-92)

001-607-798-8146

p.272

Department of Biology, Huntersdale

10/LETAT/WGC/cye

1st May, 1992

FAX: 010 49 69 794 004 37A. Traverse,  
Forschungsinstitute und Naturmuseum Senckenberg  
Frankfurt.DRAFT SCHEDULE OF VISIT OF DR. A. TRAVERSE TO LONDON:MAY (13th?) - 14th - 15th (16th?)

May 13th p.m.: Arrival in London - Heathrow - WGC will collect.

Take to Chaloner's at 20 Parke Road, Barnes, London SW13  
9NG, Tel: 081 748 3863.

May 14th a.m.: Drive to Egham.

10.00 a.m. Conference with Boulter - to discuss NCU, PFR, IOP, Paris  
and Aix 'Species' debates, proposals before Fossil Plant  
Committee, future officers of FPC. Action re Tokyo.  
(Boulter available all day).

Lunch With colleagues in W.G. Chaloner's room.

p.m. Continue discussions; meet research students in Biology,  
RHBNC (Esp. Hemsley re Sporopollenin project).

Dine, overnight Chaloners.

May 15th a.m.: Meet with palaeobotanists/palynologists in Geology, RHBNC ?

Lunch Senior Common Room, Egham.

p.m. Further discussions WGC/A.F.T. Visit Kew, meet  
palynologists in Herbarium, see Kew Gardens (or other  
arrangements? Meet Hawksworth re NCU? Others at Kew?).

(Natural History Museum? Meet Richardson?).

Dine, overnight Chaloners.

May 16th: Leave London for Frankfurt?

PROFESSOR W G CHALONER, FRSc.c. Prof. M. Boulter - PEL  
Dr. A. Scott - Geology  
Dr. A. Hemsley - Biology

No! AT!

= yes!

no - too  
much for  
one day

May 17th

London → Frankfurt

No → to Clow Earle for  
conferences with him & Roger  
Jones re publication with  
U. London Press

ROYAL HOLLOWAY & BEDFORD NEW COLLEGE

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

Facsimilie Telephone: 0784 471739

To Speak to the facsimilie operator, telephone 0784 435551.

TO ..... Forschungsinstitute und Naturmuseum Senckenberg, Frankfurt .....

Dr. A. Traverse  
ATTENTION OF .....

010 49 69 794 004 37  
FACSIMILIE TEL NO .....

NO OF PAGES ..... 2 ..... THIS PLUS ..... 1 .....

FROM ..... Prof. W.G. Chaloner .....

DATE ..... 1st May, 1992 .....

-----  
MESSAGE:

FORSCHUNGSINSTITUT UND NATURMUSEUM SENCKENBERG  
PALAEBOTANISCHE SEKTION  
SENCKENBERGANLAGE 25

6000 FRANKFURT A.M. 1, DEUTSCHLAND (Germany)

fax: ~~069-746-238~~; phone: 069-794-004-50

*New number: 069-794-004-37*  
28 April, 1992

faxed to 0784-437520

Dr. W. G. Chaloner  
Dept. of Biology, RH & B New College  
Egham Hill, Egham, Surrey, U.K.

Dear Bill:

Have probably found source of funds for a ticket--now we need to set a date for the WGC-AT-MCB-TG meeting.

I guess I've already told you, FS has been operated on for cancer and now is undergoing radiation therapy in the hospital in Hoechst. It looks as if the thing hadn't metastasized. Herbert Straka's, wife, a very good friend of ours, just died of cancer in Kiel, but we can't go to the funeral because of the rail strike. If the strike ends in time, we want to visit poor Herbert, and we've picked the weekend of 8-11 May. I plan only to attend one day (28 May) of APP under these new circumstances (i.e., no excursion afterwards to Friedemann's home diggings (Chemnitz, etc.), etc. I hope you can avoid those two dates in May.

Best.

Yours very truly,



Alfred Traverse

SNG KUHWARDSTRASSE

TEL: +49-69-79400437

28 APR '92

9:16

SENDEBESTÄTIGUNG

NR. : 001  
EMPFANGER : 0784 437520  
SENDER : SNG KUHWARDSTRASSE  
DATUM : 28 APR '92 9:16  
DAUER : 00'48  
MODUS : STAND  
SEIT. : 01  
ERGEBNIS : OK

FORSCHUNGSINSTITUT UND NATURMUSEUM SENCKENBERG  
PALAEBOTANISCHE SEKTION  
SENCKENBERGANLAGE 25  
6000 FRANKFURT A.M. 1, DEUTSCHLAND (Germany)

13 April, 1992

Professor W. G. Chaloner  
Department of Biology  
Royal Holloway and Bedford New College  
~~Callow Hill, Virginia Water, Surrey~~  
GU24 4LN, U.K.

Dear Bill:

Yours of 30 March, 1992, was here when we returned from a week in Berlin--it was a compulsory Fulbright conference and very interesting (even 87-yr.-old Sen. Fulbright and the U.S. ambassador to Germany came). I played hockey three afternoons and divided them between Greuter at the Bot. Garden and Daber, Schulz et al. at the old Prussian Nat. Hist. Museum on Invalidenstraße. The conversations with Greuter were very productive, though I don't agree with him about lots of things (see enclosure).

Before I forget, the fax number here is 069-746-238. Phone is 069-794-004-50 (private line to my office).

There are big problems about a visit to U.K.--mostly that I have insufficient time to make progress on the projects here. The \$ are, of course, also a concern. I'll ask Fulbright, but I doubt they'll fund it. Staying with you would sure help. If I flew, I guess it could be done in 3 days total. I would like to see you, Galtier and Boulter about something quite apart from nomenclature. The fossil plant collections (Schlotheim, Ehrenberg, H. Potonié, etc., etc.) and library at 43 Invalidenstraße are in serious danger because of funding problems in the new unified city of Berlin and the accidental fact that one of the buildings belonged to the (DDR) state and has hence been taken over and sold by the Treuhand Anstalt to a company which now wants the museum to get out of its building. We need to organize a letter from IOP to the Berlin Senat calling attention to the world-class nature of the paleobotanical collections and library and their importance to Germany and the world.

When exactly do you propose to have this meeting? APP etc. takes me away 28 May-3 June. I am committed to a trip to Stuttgart 26-30 June. I fly back to USA on 1 July.

Yours very truly,

Alfred Traverse

encl: c. of letter to Greuter  
c: Greuter



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Department of Biology, Huntersdale

10/LETAT/WGC/cye

30th March, 1992

Dr. A. Traverse  
Senckenberganlage 25,  
D-6000 Frankfurt am Main 1  
Germany.

Dear Al,

Sorry to see the note on your letter of March 24th to Jansonius to the effect that it is not possible for me to visit Frankfurt on either of the suggested dates.

I had a meeting with Mike Boulter last week and we discussed important issues to resolve before Tokyo 1993. When I saw him I believed I would be able to talk with you in Frankfurt. As that now seems impossible is there any chance of your coming to Britain for a meeting involving both the Fossil Plant Committee and the N.C.U. project? Jean Galtier will be here in May and as new President of IOP might add to the justification for your coming. Would your Fulbright people possibly fund your fare to London? You would be most welcome to stay with us if you could get here. I will gladly put forward a formal proposal of an IOP meeting (relevant to N.C.U.) with Mike Boulter and Jean Galtier as the prime convenors and for a Fossil Plant Committee meeting based on Chaloner, Traverse and Galtier (if this would help strengthen your case for coming).

It would be a great pity if you return to the USA without being able to discuss the really important issues which will come to a head in 1993.

With best wishes,

*C. Y. Edwards*

Dictated by  
BILL CHALONER  
and signed in his absence.

*= Professor W. G. Chaloner, FRS*

c.c. Prof. M. Boulter - Polytechnic of East London.

471739

Telex: 935504 Fax: 0784-437520



UNIVERSITY OF LONDON

ROYAL HOLLOWAY AND BEDFORD  
NEW COLLEGE

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

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

Department of Biology, Huntersdale  
10/LETFS/WGC/cye

18th March, 1992

Dr. F. Schaarschmidt,  
Sektion Palaobotanik,  
Forsch/-Institut Senckenberg  
Senckenberganlage 25  
D-600 Frankfurt am Main  
Germany

Dear Friedemann

As you may know we have been working on Quaternary Fossil Leaves for the last year and a half counting the stomatal density to follow changes in response to atmospheric carbon dioxide change. I attach an offprint on this work, although we have taken the research much further now and have some interesting results.

I would like to try and explore whether we can extend this approach back into the Tertiary for as far as it is possible to identify the leaves to extant genera. I know that much of Kräusel's material is housed in your collection and I would like to spend a couple of days in the Senckenberg museum to see whether this is a feasible extension to our Quaternary work. Our research grant for the Stomatal research will make this visit possible, particularly if I could use your "academic visitors" accommodation, if that is acceptable.

I would plan to combine that research visit with the opportunity of discussing Fossil Plant Committee matters with Al Traverse and if possible yourself in connection with the IBC in Tokyo. I have in mind two possible dates for such a visit, these are:

May 26th - May 29th, 1992 - inclusive  
June 1st - June 4th, 1992 - inclusive.

*rein, nicht  
annehmbare*

I would be most grateful to learn whether either (or both?) dates are acceptable to you and Al Traverse.

I believe there is an Arbeitskreiss meeting in Göttingen around that time and so wondered if either of these dates may not be convenient for you. If need be I could visit Frankfurt in your absence, but of course I hope very much to be able to see you whilst I am there.

With best wishes,

Yours sincerely

*Bill*

BILL CHALONER

c.c. Dr. A. Traverse - Frankfurt.  
Enc.

T. A. R. 20  
Geothorenstr. 36, Nr. 25  
6000 Frankfurt a. M. 1

28. I - 72

Chaloner

Dear Bill,

I got yours of 21 Jan. yesterday - 6 da.  
I believe it's really better to use the  
above "home address" - mail seems to  
wander around in Sauerbrunn a while!

Yours right, and I said it too - in  
brief, the winds of change have sapped  
motivation for work in our Committee.  
You. was never "fun," but was viewed  
as necessary.

Friedemann is "in Valant" - because it's  
Rose's 50<sup>th</sup> birthday today. He took the  
week. I am sure he can arrange for  
you to stay in the Museum, as Betty and  
I did several years ago. Spartan! (Our bedroom-  
only accom. reminds us of camping.)

April 9<sup>th</sup> will be in Berlin - obligatory  
meeting for all Fulbright profs. I hate  
to lose the work day. So we must decline

the nice invitation. As you say, he just  
to get that damned book out of here!  
(It was maddening to have to slip the  
whole project here — not as planned!)

Will take care of Kemp invite.

see the head,

AL

encl. xerox of Kemp letter

FORSCHUNGSINSTITUT UND NATURMUSEUM FRANKFURT  
PALÄOBOTANISCHE SEKTION  
SENCKENBERGANLAGE 25  
6000 FRANKFURT A.M. 1, DEUTSCHLAND (Germany)

8 January, 1992

Professor W. G. Chaloner  
Department of Biology, Huntersdale  
Royal Holloway and Bedford New College  
Egham Hill, Egham, Surrey TW20 OEX  
UK (Grossbritannien)

Dear Bill:

First of all, this is just to report in. Betty and I are pretty well settled. We have brought our laptop (386 microprocessor, the works) and have bought a printer here. With FS' help we are gradually working through the problems, but we still can't get umlauts and a few other things.

My first job here is to get the finishing touches on the sed book completed. I am getting very near the end of it, but it is a pity that we couldn't finish before we came here. We had to ship a large suitcase of MSS as extra baggage.

Friedemann and I have been eating lunch together and, among other things, discussing nomenclatural matters. We are agreed that the recent business about sanctioned names, NCU lists, etc., has sapped interest of people who would otherwise be following our affairs. For that reason, you and I are going to have a hell of a time finding satisfactory successors. It's analogous to Gorb. trying, say six weeks ago, to find a suitable young communist interested in taking over the party. Judy S. as much as told me a while back, "With the possibility that we might be blind-sided by books of sanctioned names, would my department head favor me taking on the secretarial job?"

Fried agrees that the Lycopodites conservation idea is a good one.

He suggests that Kerp (Netherlander now teaching in Germany) would be a good reviewer for the proposal. When a decision is made on that, should I send out the invitation, etc.?

Now we'll see if we can make the printer work! All the best.

Yours very truly,

Alfred Traverse

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

12 December, 1991

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

Dear Bill:

Yours of 3 Dec. 91 just in. I'm back from India about two weeks (your name was often mentioned, including by me), and we leave for the sabbatical in two weeks. Incidentally, I got the Fulbright professorship quite promptly after applying, about a year ago. (You apparently thought there was a problem?)

In Frankfurt I have a very serious research purpose, and I must complete it. Unfortunately, a Fulbright prof is expected to give a few lectures, but I'm going to keep that to a minimum.

It really hurts, but I had long ago decided that I couldn't go to ICP-IOP in '92. Our Fall Semester begins in mid-August, when I will have been back from Germany only a month. I just can't haul off and leave the country again, in the middle of the semester. I am still working on sporopollenin and related matters, and would have loved to be part of your symposium--but I can't do it.

I agree--the proposal to conserve looks good. How about Bill DiMichele or Hermann Pfefferkorn as a referee? I believe you can also write one, and you are certainly qualified to do so, but I believe we should then have an "outside" referee too. I will get on this as soon as I hear (you can write me c/o Friedemann).

Thanks for copy of letter to Greuter. However, I don't recall having seen his letter (or whatever) in which he apparently deals with Aequitriradisporites (misspelled in your letter).

All the best.

Yours very truly,

Alfred Traverse

AT/et



UNIVERSITY OF LONDON

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Department of Biology, Huntersdale

7/LETAT/WGC/cye

3rd December, 1991

Dr. A. Traverse  
The Pennsylvania State University  
Department of Geosciences  
Palynological Laboratories  
435 Deike Building  
University Park  
PA 16802  
U.S.A.

Dr. A. Traverse  
c/o Dr. F. Schaarschmidt,  
Sektion Palaobotanik,  
Forsch.-Institut Senckenberg,  
Senckenberganlage 25,  
D-6000 Frankfurt am Main  
Germany.

Dear Al,

The enclosed copy of a very belated letter to Werner "says it all" - or nearly.

I am delighted to hear that you were (finally(?)) successful in getting funding to go to Frankfurt. I hope while you are there, you may get to flit across to London? The bad news in that would seem to be that you are scheduled to go home well before the IOP and IPC meetings in France. I hope - indeed assume - that you will stop on (or return?) for them?

In that connection, I am convening a meeting on "Sporopollenin: composition, biogenesis and evolution". We have already four secure speakers, but knowing your interest in this, I wondered if you would care to revamp and present your "Sporopollenin is a condensed carbohydrate" thesis? Anyway, a contribution there would be much appreciated. I hope you might agree to do this!

Finally, on the "fossilizing" Fossil Plant Committee matter - we have a proposal in front of us for a conservation-of-type (typ. cons. prop.). For once I like it! Can we act? (I would be prepared to review it, but perhaps the chairman should not? I enclose two pages of the Traité which perhaps suggests I should be disqualified!).

Look forward to seeing you during 1992. Happy Christmas - and New Year!

Sincerely,

BILL CHALONER

Enc

THE PENNSYLVANIA STATE UNIVERSITY  
DEPARTMENT OF GEOSCIENCES  
PALYNOLOGICAL LABORATORIES  
435 Deike Building  
University Park, PA 16802  
phone: 814-863-3419  
fax: 814-865-3191

27 August, 1990

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

Dear Bill:

Thanks for yours of (probably) about 15-VIII-90.

Good to know you don't object re archives. Hold your own presidential archives--I would think that (as IFPS does) it might be well for the new Chairman and Secretary to get only the last previous files--which then would go to the archives before the next change.

Re the nomenclatural chapter--well, ok--but couldn't you give me benefit of a critique of the outline I faxed you? You did get it?

Thanks for the effort to get a formal invite from Hawksworth. That would be fun.

Best regards.

Yours very truly,

Alfred Traverse

AT/et

THE PENNSYLVANIA STATE UNIVERSITY  
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PALYNOLOGICAL LABORATORIES  
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3 July, 1990

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

faxed to: 0784-437520

Dear Bill:

Here's my very tentative first stab at an outline for the chapter I discussed with you a couple of weeks ago. I've sent a copy off to Jansonius. As I told you, I'd very much welcome you as co-author, if it seems appropriate. At the moment I'd like your suggestions about the outline--do you see big areas that I've left out? Would you suggest a different sequence of topics? Etc. I look forward to hearing. The response can be by ordinary mail. I thought I'd fax this because it is already late in the procedure.

All the best. Hope you got the deal about my Fulbright application for Frankfurt, 1992 (I have, I'd guess, about a 30% chance--they favor people who haven't previously travelled, and my age is probably against me). The recommendation was due (literally) day before yesterday!

Yours very truly,

Alfred Traverse

AT/et

encl: copy of outline for chapter for Jansonius-edited book

Outline for AT chapter-JJ et al. palynology book

I. Introduction

- A. The purpose of nomenclature generally and in paleopalynology specifically
- B. Nomenclature with relation to taxonomy and systematics

II. Classificatory schemes and their links to nomenclature

- A. "Artificial" schemes vs. "natural" schemes and related nomenclatural problems
- B. The turmal system and other classifications-cum-nomenclature: Potonié-Ibrahim, etc., Elsik, etc.
- C. The Biorecord system of Hughes and its potential links to nomenclature

III. Informal vs. formal systems of nomenclature

- A. Raistrick's alphanumeric system and other, later, similar schemes (Shell system, etc.)
- B. The rationale for application of Linnaean-style nomenclature to fossil palynomorphs--advantages and disadvantages

IV. The basic mechanics of the use of Linnean nomenclature for palynomorphs, with comments on the fact that palynomorphs occur in 4 of the 5 kingdoms of organisms, and the problem that palynomorphs are often parts of a life cycle or parts of an organism that includes other named entities

V. Differences between the formal codes of nomenclature as these impinge on palynomorph systematics, with special emphasis on

the botanical and zoological codes

- A. The special peculiarities of the rules as they apply to fungal spores, dinoflagellate cysts, chitinozoans and scolecodonts, with additional comments on special situations such as lepidopteran scales and foraminiferal linings
- B. Conflicts between the codes, e.g., as to illustration and specimen-conservation requirements

VI. Special nomenclatural problems for palynomorphs

- A. Typification, lectotypification and neotypification
- B. What constitutes an "illustration," a "description," a "diagnosis," "adequate conservation of type specimens," etc.
- C. Form-genera and "organ-genera" vs. "genera."
- D. Higher taxa--are they needed for palynomorphs? How about sub-genera, sub-species, varieties, etc.

VII. "Registration" of names and journals, "sanctioned names" and other matters possibly on the horizon

- A. Would it better serve palynology to have a commission decide what names and what types are "correct," making a "bible" of sanctioned names?, or
- B. Should palynology depend on the existing Codes, the endless modification of them, and occasional conservation of names?

VIII. Summary.