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

To: fensome@agc.bio.ns.ca
From: Alfred Traverse <traverse@ems.psu.edu>
Subject: poster doc.
Cc:
Bcc:
Attached:

Hi, Rob!

The documentation stuff re the poster just came. Many thanks. I will post it on my propaganda board. I just noticed that palynomorph is misspelled "palymorph" in the caption to the illustration of the Fownes Beach loc. Whoops! And I wasn't 100% happy with the placement of some of the ? with the names. For example, in the Camerosporites case, the ? should surely be at the end, not at the beginning. But I can't swear that I wasn't to blame, either.

Have a great 1999. Best. Al.

check to be sure this went out

Date: Wed, 01 Apr 1998 13:37:25 -0800
From: Rob Fensome <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: traverse@ems.psu.edu
CC: jansonij@cadvision.com, hartkopf-froeder@mail.gla.nrw.de
Subject: Als proposal

Hi Al, Jan, Chris

My apologies for lack of response. I'm swamped with overcommitment right now - finishing the dino index, organizing a symposium at DINO6 in June and preparing 9 presentations (none on my own, but still far too many), and serving as a principal compiler/editor of a popular book on the geology of the Maritimes. As you may gather, all of this precludes my "day job", so after DINO6 I have to take a course on learning to say "no" - certainly there will be a moratorium on conference attendance for a few years!

I'm flattered (in a sense) by Greuter's notion that I have "forced my purist" approach on paleobotanists - that I should be so persuasive - WOW! To the extent that I pretend to be a student of people, I find that those living in glass houses are always the keenest to cast stones.

I too am floored by Greuter's problem with the meaning of "specimen" He made a similar comment in reviewing the paper that Judy, Jan and I just finished. What we did was simply put in the following sentences:

"To us and to palaeobotanists in general the term "specimen" refers to an individual fossil or organism, not to the entire residue on a microscope slide or palaeobotanical sample (e.g. an entire coal ball). We recommend that palaeobotanical systematists assume that this is the meaning of the word "specimen" in the Code, otherwise unnecessary and unproductive confusion will arise."

Why waste time on other people's problems - at least if they don't affect any of us.

If you decide to propose modifying Article 8.4, Al, you should at least reread our proposal - If you can't find the earlier copy that I sent you, I'll fax you another. It will be out in the May Taxon.

I'm still very uneasy about going the whole hog and allowing the type of a fossil name (sic) to be an illustration. The quality of illustrations in palynology at least very often leaves much to be desired and I fear a widespread lowering of standards should your proposal come to be accepted. Moreover, selective focus can give a very misleading idea of

actual morphology. And regardless of how you load your argument with the numbers game, it is a fact that recourse to types has solved or clarified a number of taxonomic problems in recent years (as Jan knows well with his restudy of Stan Pocock's material).

However, I also see your argument with respect to lost or inaccessable types. Your argument that most palynological holotypes are unavailable - at least not readily available - is unassailable. Could there be a compromise - a holotype must be designated but if lost or damaged, the illustration should serve as type (Judy came up with this idea). This would serve to maintain standards, please both the specimen and illustration schools of thought, and effectively formalise what mostly happens now anyway. What do you think, all.

Back to the swamp.

Best wishes to all, Rob

From: bettyandal
Full-Name: Alfred or Elizabeth I. Traverse
To: fensome@agc.bio.ns.ca
Cc: jjansonij@cadvision.com
Subject: the usual
X-Status: Unsent

Dear Rob:

My Devonian paper has gone off to some other advisers, and I now turn my attention to getting my proposal out of here.

Your comments per 21 Jan. are very useful. I have put in the numbers you have provided and am trying to see if there are places the paper could be chopped without emasculating it. One thing that came out of your message that has bothered me a lot, was the question of nannofossils and diatoms. They are CaCO3 and SiO2 respectively, and thus not subject to the disintegrative processes that attack sporopollenin and chitin, etc., but the difficulty of storage, and refinding of both preparations and the specimens in them, is shared I would suppose with palynomorphs, though I have no direct experience. I am debating how to handle this. At first I was inclined to make the definition " organic-walled plant microfossil" in order to make it clear that I am talking about palynomorphs exclusively. Now I am thinking I'll leave it general, so that diatoms and discoasters, etc., are included if somebody wants to include them. So, thanks. But (sorry!) I'll have to add at least a sentence at the appropriate place to note that such microfossils are not subject to the same sorts of disintegration as palynomorphs (though they can be dissolved).

In addtion to the Devonian work that was more pressing, I have delayed sending off the proposal paper because I had sent a copy of the MS also to Hartkopf-Froeder over two weeks ago, via International Priority Mail,, after getting his agreement to look at it, per e-mail. Since then it has been absolute silence from Krefeld. No e-mail to say that the package came, didn't come, etc. Nothing. I guess it must have shocked him badly. Hmmm. I guess now I'll just have to go ahead without hearing from him, which makes me unhappy.

Cheers. Al.

From: Rob Fensome <fensome@agc.bio.ns.ca>
Return-path: <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: "Alfred or Elizabeth I. Traverse" <bettyandal@juno.com>
Cc: jjansonij@cadvision.com
Date: Wed, 21 Jan 1998 16:34:13 -0800
Subject: Re: response to sugg.
Message-ID: <34C69405.11CD@agc.bio.ns.ca>
References: <19980121.105859.7567.1.bettyandal@juno.com>
X-Status: Read
X-Mailer: Mozilla 3.01 (Win95; I; 16bit)

Hi Al

I've just surfaced from a horrible bureaucratic all-day meeting yesterday and a review of a long monograph that has taken me a few days to do. (While you've been picking grapefruit, I've been picking figurative nits, as is my wont.) I took a look at your revised ms and found it much more readable than the first draft. Please excuse me from going through it with a fine tooth comb again, but I agree with Jan that it could be shortened - I think the editor will urge you to slim it down.

Suggested revision of 1st para, 2nd sentence, p.2: "There are something in the neighborhood of [if you must] 20,000 validly published species names for fossil spores and pollen, as well as approaching 10,000 species names for organic-walled algal microfossils; these 30,000 specific names are assignable to about 5,000 generic names. Such microfossils ...."

How strongly do macrofossil people feel about all this (keeping in mind that some microfossil people have yet to be convinced)? It would be very simple to drop the wording "... of non-fossil plants ..." from Article 8.3, and thus avoid going into contortions over defining "microfossils". The simpler the changes, the easier it might be to get them accepted, as long as those simpler changes do the job of course. I wonder, too, what the diatom and nannofossil people would think - maybe they wouldn't care. Article 8.4 would have to be modified too.

Anyway, these are ramblings. My feelings are that you should submit your ms and see where the chips fall.

Cheers Rob

From: Rob Fensome <fensome@agc.bio.ns.ca>
Return-path: <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: "Elizabeth I. Traverse" <bettyandal@juno.com>
Date: Mon, 19 Jan 1998 18:07:35 -0800
Subject: Re: update
Message-ID: <34C406E7.53C@agc.bio.ns.ca>
References: <19980119.155916.7927.0.bettyandal@juno.com>
X-Status: Read
X-Mailer: Mozilla 3.01 (Win95; I; 16bit)

Hi Al

The ms arrived and I'll try to take a look in the next couple of days.
I'm a bit swamped right now with a huge ms to review, a presentation to management to prepare for next week, and a horrendous bureaucratic all day workshop (on "broad-banded job descriptions", whatever they are) that, as a supervisor of two technicians, I have to attend tommorrow. (Re the latter, why can't one catch the flu on such days; I usually get the flu in the periods that I've mapped out for uninterupted research.) Glad you mostly liked the mss. They were slightly changed and I think the sentence on illutrations as types was stated more neutrally.

All the best Rob

Elizabeth I. Traverse wrote:
>
> Dear Rob:
>
> Well, my huge pile of "brought with" reading and writing projects IS
> decreasing in size! Yesterday I spent the whole day trying to convert a
> Chinese Triassic sporomorph paper into accurate, understandable English.
> Paul Strother is editing the Proceedings Volume for the Houston ICP-AASP
> meeting and asked me to be a reviewer for the paper. There are a number
> of new names. One thing I noticed is that the authors say nothing about
> repository. The slides all have the prefix "SK" but unlike the BSIP
> situation Jan describes somewhere, this doen't automatically translate
> into anything obvious (S could stand for Sinica, but what about K?) Of
> course, you, Jan and I all know that for palynomorphs it wouldn't really
> be particularly helpful, even if we did know that SK stands for Academia
> Sinica, and that the slides are therefore somehow associated with that
> institution (with branches in Beijing, Nanjing.....) Repository in
> palynology means almost nothing in terms of actual accessibility and
> findability. Heck, I even have a friend who confessed to me a while back
> that he carries holotype slides around (they presumably had a depository
> designation) , along with his car keys and whatnot.
>
> This all leads up to saying that I now have carefully read your e-mail to
> me of 14 Nov. (Subj. Re MS and others). You sent me excerpts from a

> couple of manuscripts "for my information," and I appreciate that. I
> found 99.5% of what you et al. said made great sense. However, on p. 3
> there is the statement: " ...it may not be an illustration. (It is
> necessary for the types of some extant plant taxa [you should have
> said--ahem--names] to be illustrations because of their delicate,
> unpreservable nature; however, preservability is not considered to be
> such a problem for fossil plants.)" As you know, I would argue that
> permanent preservability and accessibility, which is part of what is of
> concern here, are definitely a problem for plant microfossils.
> Palyno-fossils are indeed hard to preserve, hard to conserve, and
because
> of both size and chemistry are very easy to lose or permanently
misplace,
> even if people didn't carry them around in their coat pockets.
>
> I'm wondering how the "International Express Mail" worked. Of course,
> OUR post offices are all closed today, and there is no mail delivery,
so
> it's a good thing there's e-mail, eh?
>
> Best. Al.
>
> P. S. Thanks very much for the guess on acritarch numbers. I'll go
> along with that. You are too modest. I was talking about the world's
> authority on the number of taxa and names out there, and as the indexer
> thereof, you had better be such a.

From: Rob Fensome <fensome@agc.bio.ns.ca>
Return-path: <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: bettyandal@juno.com
Cc: jjansoni@nrcan.gc.ca
Date: Fri, 16 Jan 1998 14:19:46 -0800
Subject: Counts
Message-ID: <34BFDD02.52A2@agc.bio.ns.ca>
X-Status: Read
X-Mailer: Mozilla 3.01 (Win95; I; 16bit)

Hi Al

Thanks for your messages. I would hardly count myself as an expert on acritarchs. I just did a lot of Indexing grunt work. But thanks for the flattery anyway. Re numbers, just from the fact that the 1990 acritarch index is about the same size as the 1993 dino index, I would say that the numbers of acritarchs and dinos are more or less equivalent, but the acritarch index also included prasinophytes and nebulous Precambrian thingies as well, and it is difficult to draw a firm line between acritarchs and, respectively, prasinos and thingies. Can you make any better sense Jan?

One pedantic point: you should probably make clear that your counts refer to names, not taxa. Apologies if this is a repeat comment.

I'd be happy to take a quick look at your ms again - maybe I'll be convinced this time.

Hope you are enjoying your cycling. I was dreaming yesterday about cycling in the English countryside - my particular idea of heaven, though I haven't been able to do it for years.

All the best Rob

From: Rob Fensome <fensome@agc.bio.ns.ca>
Return-path: <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: traverse@ems.psu.edu
Date: Tue, 23 Dec 1997 14:48:42 -0800
Subject: MS etc
Message-ID: <34A03FCA.1464@agc2.bio.ns.ca>
X-Status: Read
X-Mailer: Mozilla 3.01 (Win95; I; 16bit)

Hi Al

I owe you a very sincere apology for not getting back to you sooner - indeed as promised. December has been a pretty horrid month in way way and another and I've not had the extra hours that I rely on to keep up with extra-curricular activities (anything that doesn't involve direct biostratigraphy of offshore eastern Canada). That's an explanation, not an excuse - I have no excuse!

1) I would say that there about 575 names for genera of fossil dinoflagellates and approaching 4,000 species names. We have to do an accurate count for the new "Lentin and Williams" Index, and I may have that for you in a week or two. The other groups I'm not sure about.

2) I would say that - at a total guess about 10% of these may have findable types. (Which is a problem I grant you - but is not the crux of my objections, as discussed below.)

4) Sorry to preach about "algae". I'm slightly touchy on the subject since I was horrified (at the same time as being honored - sort of)recently to have been asked to write up an entry for a (the?) paleontological encyclopedia on "skeletized algae", including dinos.

With regard to writing style, I tried to look at it from the point of view of someone that you would have to convince to gain your case - I guess I don't respond as well to polemics as I do to straighter styles. Anyway, I trust you took what, if anything, you could from my comments and discarded the rest.

I'm still open to being convinced by your proposal. Both you and Jan are very strong proponents, so I may well be missing something. My main objection is that it would seem to encourage even more sloppiness in curating types without actually gaining us any real advantages - after all, we do refer mostly to illustrations now as our "sense" of types. The latter (i.e. the de facto day to day procedures in using types) wouldn't change, but people would be effectively released from the responsibility of making any effort to preserve any material.

Anyway, I hope that you and Betty have a marvellous holiday season.
Very best wishes Rob

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: MS, etc.

Dear Rob:

I will probably bother you for clarifications a few more times, but for the moment:

1. Can you come up with an estimate of the number of FOSSIL dinoflagellate genera, and of species? Can you do the same for acritarchs? Charophytes? Tasmanitids? The Pediastrum bunch? Other such "plant microfossils" that I am forgetting? I would think that as a cataloger, you should be able to do this, and I'd like to cite it as a piece of actual data (pers. comm.). From the CFPS and Jans. cards I am comfortable with about 12,000 species in 4000 genera for sporomorphs.

2. Then, would you in some way take a guess at what % of the types for these things are actually in existence and findable? For example, if the author gave only stage coordinates, I can tell you right now that they are NOT findable. If they were mounted in glycerin gelatine, they are almost certainly not any longer studiable. If they were designated in the former USSR or in any central European country pre-1950 they are almost certainly not in existence. (I remember in Leningrad asking Samoilovich if I could see Malyavkina's type specimens. This was in Sam.'s lab, and there was much merriment among the Russians when my question was translated by the watcher from the Praesidium who accompanied me to the lab.) One way to do this would be to pick, say twenty species at random (first one on every 20th page in a list, for example), then ask yourself honestly if the types probably exist and could be found and visited or borrowed. This sort of estimate is obviously a guess, and I wouldn't cite that as coming from you unless you are comfortable with the result--but it would be instructive to have such data. I plan to do it for sporomorphs.

3. I am, among other things, a herbarium botanist, and have spent a lot of time in herbaria. The people who wrote ICBN were mostly herbarium botanists. They have a concept of type specimens being clearly tagged herbarium sheets, usually housed in a special cabinet for types, available for loan under certain conditions. The situation for palynological types is UTTERLY different from that.

4. I am just two years away from having taught palynology, paleobotany and plant evolution. You can count on it, that I know about the multi-kingdom proposals, and about the Protista, etc. However, it is a question of definition, and the concept "Algae" can definitely be defined to include dinoflagellates, whether the result is concordant with modern biological concepts or not. Indeed, all the algology texts on my shelf do exactly that. It depends on the rules of the game that one sets up before discussing the subject. The Acritarcha, of course, have unknown relationship by definition, but those with sporopollenin walls are surely not fungal or animal, so what is left? Presumably algae, or photosynthetic protists (which can be included under "Algae," if that is the rule of the game that one is playing).

There is no question that we have different writing styles, but what I am doing is clearly a form

of polemics, and in my view requires polemical style. So, I may not accept all your suggestions on style, though I REALLY DO appreciate your approach, and will seriously consider all the proposed changes. In some instances, the use of a single word or telegraphic expression changes the meaning. E. g.: "about" is clearly NOT the same as "in the neighborhood of". If I get some actual numbers from you that I can combine with my number for sporomorphs, then I can say "about".

Thanks for reading. Al

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: various
Cc:
Bcc:
X-Attachments:

Dear Rob:

Martin Head says that the postal strike won't last long.....hmmmm. He says that dozens of postal workers are proven thieves as a result of a police sting operation and that this limits public support for the workers. I should hope so.

Just checked, and indeed your fax came through. As far as I can tell in a quick glance, it's aok. Thanks very much.

What I mean by quixotic, of course, is that some persons for some reason would prefer a theoretical type specimen, which probably has disintegrated, or is on a strew slide with thousands of other specimens and no location given other than stage coordinates, or.... to having a designated holotype illustration plus epitypic illustrations. Incidentally, I was assuming but will now make it clearer that the designated illustrations would almost always be PUBLISHED illustrations, and thus available all over the world. Wouldn't that be great? Or someone could publish them altogether in a series of volumes like the JH cards.

I confess to knowing little about dinos, but I suspect that if you really looked into it that, just as I KNOW to be true for pollen & spores, most of the designated holotype specimens are impossible to locate or have disintegrated. I suspect that even the holotype for Saeptidinium hansonianum, which I designated a half century ago and deposited at Harvard is either not findable or disintegrated.

Enough for now.

Best. Al.

Date: Fri, 21 Nov 1997 14:58:33 -0800
From: Rob Fensome <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: Alfred Traverse <traverse@ems.psu.edu>
Subject: Re: MS, etc.
References: <199711211836.NAA13074@pangaea.ems.psu.edu>

Hi Al

Sent it by fax anyway. Hope you can read the pencil marks on the ms.
If necessary I'll send the original after the strike. Yes we have a
postal strike and no mail is being accepted. Anyway, let me know if you
have problems reading anything.

All the best Rob

Alfred Traverse wrote:
>
> Dear Rob:
>
> As you can see, I sometimes take a vacation from reading this stuff for a
> couple of days, hence delay.
>
> I'd say go ahead and MAIL the thing and let the devil take the hindmost.
>
> But please make a xerox of your comments and corrections so that we could do
> something more creative if necessary.
>
> I don't have phone privileges for outgoing calls any more. I can do it if I
> arrange a prepaid card, etc., but it is so much trouble that I don't do it.
>
> If you want to send the thing by fax, the number is 814-863-7823. The
> secreteary who minds the machine always gets the messages to me promptly.
>
> I will be interested to hear what your reactions were, to say the least!
>
> Best wishes. Al.
> Alfred Traverse
> Palynological Laboratories
> Department of Geosciences
> 435 Deike Building, PSU
> University Park, PA 16802
> Ph.: 814-863-3419
> Fax: 814-863-7823

Natural Resources Canada | Ressources Naturelles Canada

Geological Survey of Canada | Commission geologique du Canada

19 November 1997

Alfred TRAVERSE
Department of Geosciences
Pennsylvania State University
UNIVERSITY PARK, Pennsylvania
U.S.A. 16802

Dear Al:

I've just read your manuscript and found it very interesting. Its funny how we all have such different takes on similar landscapes - in this case the "landscape" of botanical nomenclature. I have to say that, although I've had problems with types, I've never had a problem with the fact that, for fossils, types are specimens, not illustrations. I guess that makes me incomprehensible and quixotic. (Which shouldn't be a prelude to thinking that I necessarily disagree with you, just that I will need quite a bit of coaxing to believe that your proposals, in general, will not cause more problems than they solve.)

Putting my nomenclature related views aside for now, my overall sense is that you will have to tighten up the text considerably and remove or modify the more emotive sections. Taxon likes proposals to be short and to the point and, although your ms can't be accused of having such qualities at present, there is ample scope for reduction and sharpened focus. (OK, lets call a spade a spade - I feel its long-winded.) Also, the editors will probably strong arm you into putting your proposals at the beginning. Specific points relating to the text are as follows:

1) Please take care to use the words "legitimately" and "validly" judiciously in Code related contexts.

2) I'm not crazy about use of the word algal here, though I agree it is hard to find an alternative. Dinos are half zooplankton, half phytoplankton that just happen by concensus to be governed by the ICBN. In modern biological terms they are protists. And we don't know what percentage of the acritarchs are "algae", but its a pretty sure bet that many aren't. "Plant" microfossils may be best, with plant in quotes.

3) I hate to harp on dino-upmanship, but a considerable number of dino types are available and some of us travel with types so that colleagues can see them directly. I'm sympathetic to many of the problems that you list here and elsewhere re. types, but for dinos it is not true that only a "very tiny minority" of types are available. Tone down your statements and I wouldn't disagree.

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Canada

4) You're right to highlight these problems, but again, I can't relate to the draconium scenarios of never finding any specimens, as you seem to be invoking. Again, I have examined literally dozens of dino types.

5) Taxon will make you use British spellings I'm afraid.

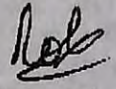
6) Why would a photograph be any more accessible than a specimen? Or, in some cases, preservable for that matter. Color photos will probably not last as long as a well-curated holotype in Canada Balsam.

My general thoughts on all this are not clear, at least yet. I can't help hearing alarm bells. In practice we mostly refer to illustrations for our impressions of holotypes anyway (which I realise can be construed as supporting your case). In my view, many paleobotanical systematists are too casual with regard to taxonomic and nomenclatural practice as it is, resulting in far too many fuzzy blobs as illustrations of holotypes. If researchers were given the "freedom" not to have to anchor their concepts with nomenclatural types that are actual specimens, I fear (perhaps irationally) that fuzzy blobs will become the norm. Epitypes seem to me to be a license for instability. Anyone who publishes on a taxon, it seems, can designate an epitype (once the idea catches on). I'm not sure how that helps us retain the original concepts; but it may well encourage a lot of competing subsequent concepts.

There's my reaction for what its worth. I'm open to argument, and certainly am not as strongly con as you are pro. But I do feel strongly that, for the sake of your own cause, you need to far more tightly constrain the ms before submitting it.

With regard to Ordovician dinos, you were right to be skeptical. Molecular phylogenetic and biogeochemical evidence indicates that dinos were around at that time, but the characteristic dino form that we see in the Mesozoic to present resulted from a major radiation in the Triassic and Jurassic. Prior to that, dinos didn't necessarily look like - well - dinos. The only Paleozoic "dino" that is at all convincing is *Arpylorus*, and even that is nothing like any post Paleozoic dino except that its wall splits into plates. The spiny things like *Palaeohystrichosphaeridium* are really unconvincing. However, recognition of dinosterane associated with particular acritarchs may change this story in coming years.

Best wishes



Robert A. Fensome
Marine Resources Geoscience Subdivision

RAF/nk

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Phone: (814)863-3419; Fax: 814-863-7823
E-Mail: traverse@ems.psu.edu

13 Nov., 1997

Dr. R. A. Fensome
Geological Survey of Canada
Atlantic Geoscience Centre
Box 1006
Dartmouth, Nova Scotia
Canada

Dear Rob:

Enclosed is an MS in preliminary form, that I intend to submit to Taxon. If you read my chapter in the J & M volume, you know that the subject is one about which I feel strongly. I hope you agree with me, but whether you do or not, I would appreciate your reading the thing. Please return it to me with your red pencil comments on particular things, and perhaps a letter with general reactions.

Hope all is going well. We are just back from the trip to Germany, which was a lot of fun and went very well. In Freiburg I wished I had you with me. I was cornered by a professor and student who think they have Ordovician dinoflagellates--the plates and archeopyles seem pretty iffy, and the "plates" are limited in number, like a modern dino. Hmmm. I didn't want to be impolite (they were showing us such a good time!), but I did say not to be too surprised if Nature turns it down.

All the best.

Yours very truly,

Alfred Traverse
Professor Emeritus of Palynology

encl.:MS

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: MS, etc.
Cc:
Bcc:
X-Attachments:

Dear Rob:

As you can see, I sometimes take a vacation from reading this stuff for a couple of days, hence delay.

I'd say go ahead and MAIL the thing and let the devil take the hindmost.

But please make a xerox of your comments and corrections so that we could do something more creative if necessary.

I don't have phone privileges for outgoing calls any more. I can do it if I arrange a prepaid card, etc., but it is so much trouble that I don't do it.

If you want to send the thing by fax, the number is 814-863-7823. The secretary who minds the machine always gets the messages to me promptly.

I will be interested to hear what your reactions were, to say the least!

Best wishes. Al.

Date: Wed, 19 Nov 1997 12:51:42 -0800
From: Rob Fensome <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: traverse@ems.psu.edu
Subject: Your ms

Hi Al

I've been through your ms and recorded my reaction in a letter and annotations on the ms itself. Since we are (still) on the verge of a postal strike here I will fax the letter and ms to you. Since the ms and letter together are 12 pages long, you may wish to be there as I fax it in order to field it. If so, call me (902-426-2732) and we'll try to choreograph the process. I'll be here (bar restroom and copier sorties) this afternoon till about 3.40 yours, and tommorrow in the afternoon (I have a meeting in the morning). If you don't need fielding practice, let me know by e-mail (along with the appropriate fax number) and I'll fire it off to you promptly.

All the best Rob

Date: Fri, 14 Nov 1997 13:47:47 -0800
From: Rob Fensome <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: traverse@ems.psu.edu
Subject: Re MS and others

Hi Al

I'd be happy to look at your ms. A potential problem is that we are on the verge of a mail strike here, so you may wish to send me the ms electronically. If it is in Word Perfect, you can just attach the file and, as long as the suffix is ".wpd" I should be able to read it. Alternately, you could send me the file in ASCII format or, if you have Windows, do a "cut and paste job into the body of the message.

I'm attaching a couple of manuscripts for information. The proposal has gone, but the discussion ms is still in its last preparatory stages, so comments welcome (on both, but the proposal is out of our hands for now anyway). There is no, or only minimal overlap I think with what you are proposing, but clearly there is a relationship.

We're supposed to have our first (thankfully wet) snow tonight, though if much accumulates it will get washed away by rain on Sunday. I hope the weather is not too inclement because tommorrow is my annual day off from work and family responsibilities, when I get to go and visit all the second hand bookstores in town and spend my guilt-free birthday money.

All the best Rob

Typification and the names of fossil plants - a review and discussion

Robert A. Fensome1, Jan Jansonius2 & Judith E. Skog3

Introduction

Subtle ambiguities and loopholes in the tangle of International Code of Botanical Nomenclature (Code) rules surrounding valid publication and the designation of types, as well as valid publication and the provision of illustrations, have caused problems in recent years for fossil plant nomenclature. These problems have resulted in inconsistencies in interpretation and the invalidation of names of numerous otherwise well-conceived and carefully described fossil species on the basis of what appear to many systematists to be arcane technicalities. Moreover, some of the problems have derived from articles that have only recently been embedded in

the
Code. Below we present a review and discussion of some of the more significant concerns surrounding typification and fossil plant names.

Typification of plant fossil names at specific rank

The specimen designated as the nomenclatural type of a species (fossil or extant) in the protologue is a "holotype" (Art. 9.1). If no holotype was originally designated (prior to 1958) or if it has since been lost or destroyed, a "lectotype" must be designated (Arts. 9.2., 9.9). Traditionally, a lectotype is selected from the original material (1988 Code, Art. 7.5); however, subtle but significant editorial changes to the present Code have removed direct mention of the
original material in reference to the lectotype, leaving only an indirect reference in Art. 9.6, which states that a "neotype" should be designated as the nomenclatural type if all of the original
material is missing. Thus, a holotype is the originally designated type, a lectotype is a specimen
designated from the original material if a holotype is missing, and a neotype is a specimen designated from outside the original material if the holotype is missing and no potential lectotype
is available.

The term "isotype" has been used in some palynological works (e.g. Drugg 1970). According to Art. 9.3, "An isotype is any duplicate of the holotype...." A footnote makes it clear that the term "duplicate" is "... given its usual meaning in herbarium curatorial practice. It is part
of a single gathering of a single species ... made ... at one time." This implies that an isotype should be from the same genetic clone or from the same very restricted original locality: hence,
designation of an isotype is not appropriate with respect to dispersed fossil species. Specimens of
the same palynomorph species brought together somewhat fortuitously in the same preparation
and specifically cited in the protologue are more appropriately considered as paratypes (Art. 9.5).
The same argument can be made for megafossil specimens brought together fortuitously (form
the biological perspective) in, for example, coal balls.

When all material associated with a name is ambiguous, an epitype may be selected to serve as an interpretative type, as long as the type that it helps to illustrate is clearly indicated (Art. 9.7). Although not much used by palaeobotanists and palynologists, this concept might be
of value in cases where the original fossil material is not well preserved or does not show the diagnostic features sufficiently clearly. However, such considerations are verging on taxonomic
rather than nomenclatural realms (see Fensome & Skog, 1997) and, if practiced incautiously,

designation of epitypes might cause more problems than solutions.

Most palaeobotanists have no problem with the establishment of a type when a name is first applied to a new taxon (Art. 7). Less straightforward are other situations (Arts. 8-10), such as where the original type is lost or has been reduced or damaged during analysis. Problems can also arise in cases where a type was not originally designated (since 1958, resulting in an invalid name). The nomenclatural type of a fossil plant taxon is "one whole specimen" (Art. 8.5) and, in contrast to the situation for extant plants (cf. Arts. 8.1, 8.3), it may not be an illustration. (It is necessary for the types of some extant plant taxa to be illustrations because of their delicate, unpreservable nature; however, preservability is not considered to be such a problem for fossil plants.) If the specimen designated as type has been sectioned, or if it comprises a "part" and "counterpart" split, for example, along a bedding plane, all fragments of the originally intact specimen together constitute the type, even if these are now distributed among several repositories. In contrast, it is conceivable that a plant fossil, originally considered to be a single specimen and designated as a holotype, is found on subsequent re-investigation to comprise two or more specimens. Art. 9.2 indicates that one of these specimens must be chosen as lectotype, and Art. 9.10 and Rec. 9A.5 further address this issue.

Since 1990, according to Art. 37.4, indication of the type "... must include one of the words typus' or holotypus', or its abbreviation, or its equivalent in a modern language." Art. 37.3 stipulates, in part, that "Citation of the collector's name and/or collecting number and/or reference to any other detail of the type specimen or illustration is required [for valid publication]." The rambling vagueness of this Article makes it practically impossible to apply, and consequently it has been largely ignored in palaeobotanical and palynological works. Although a collector's name is commonly not specified, usually some descriptor of the type specimen is given by authors of new names of taxa. It would clearly be overly pedantic to invoke this Article, and we feel that the presence of such a poorly constrained rule devalues the entire Code, as it leaves open the question "Why should other articles be respected if this one is not?"

The conjunction of articles in the Code treating fossils, types and illustrations have caused some confusion. Taken together, Arts. 8.4 and 38.1 essentially indicate that, in order to be validly published, the name of a fossil species (or taxon of lower rank) must be accompanied by at least one illustration "showing the essential characters", and one of the illustrations must be of the type. However, nowhere does the Code actually specify that the type has to be identified with a specific illustrated specimen. This has caused problems in correctly interpreting the status

of
some names; Fensome & al. (in press) discussed these problems in more detail and presented
formal proposals to remedy the situation.

Typification of plant fossil names at generic rank

Art. 10 governs the designation of types for generic names. Since 1983 Art. 10.1 has specified that "The type of a name of a genus or of any subdivision of a genus is the type of a name of a species ...." Thus the type of a genus now is a single specimen, whereas in codes prior
to 1983 it was defined as a species, the "type species". The current wording of Art. 10.1 leaves
some ambiguity in cases where a species containing the genus type is considered a taxonomic
junior synonym of another species - does the type of the genus change, becoming the type of the
senior synonym? Art. 7.2 clearly prompts a negative answer to this question, stating that "A nomenclatural type ... is that element to which the name of a taxon is permanently attached ...."
For example, the type of the dinoflagellate generic name Korystocysta is fixed as the holotype of
Korystocysta kettonensis, even though this species has been considered by some authors to be a
taxonomic junior synonym of Korystocysta gochtii. The Code no longer contains the term "type species", as it no longer has a formal meaning; however, in our opinion it is still useful to employ
this term informally as a shorthand for "the name of the species to which the nomenclatural type
of the genus was originally assigned".

A problematic spin-off of this relatively recent change (from species to specimen as type of a genus) involves generic names published before 1958 whose "type species" lack a designated type. These generic names are validly published, since designation of a type was not
mandatory prior to 1958 but, strictly, lack a type in the sense of Art. 10.1 of the current Code, even though the author may have designated a "type species". In such cases, a lectotype may be
designated, but in many cases this may not be practical or desirable (for example due to lack of
access to the type material or its inadequate illustration in the literature). Hence, in such cases,
we recommend that authors continue to cite the type of the genus as the "type species".

Repository citation

In a newsletter article, Skog & Fensome (1995) discussed the requirement (Art. 37.5) that, as of 1990, a new name at species rank or lower needs to be accompanied by indication of the type's repository - i.e. the institution or herbarium in which the type is lodged must be specified. This requirement has had significant impact since, as Skog & Fensome pointed out, many specific names of fossils published since 1990 were not valid solely because of this technical requirement, in spite of the fact that most are excellently described and otherwise fulfill all requirements of the Code.

In correspondence with one of us [JJ] about this issue, Chair of the Code Editorial Committee, Werner Greuter, considered that "... the intent of [Art. 37.5] is not to make deposition of types in a public herbarium mandatory, but to force authors to make the whereabouts of their types publically known. Greuter continued that "Article 37.5 [specifies] herbarium', not public herbarium' (nor unfortunately, collection, which is what is meant) ...."

Rec. 7A is also relevent in this context. This states that "It is strongly recommended that ... [type material], especially the holotype, be deposited in a public herbarium or other public collection ...." However, since this is a Recommendation and not an Article, it is not mandatory under the Code for a repository (herbarium or other collection) to be public.

We endorse Skog & Fensome's conclusion that, as long as an author fully specifies a location, public or private, for her or his types, Art. 37.5 is satisfied. However, in contrast to the opinion of Skog & Fensome (1995) and in the more liberal spirit of Rec. 9A.4, we suggest that if an author does not directly cite the repository but does give specimen numbers including abbreviations that clearly refer to a particular institute or collection, this is acceptable under Art. 37.5. This appears to agree with the spirit of Art. 37.5, Note 1: "Specification of the herbarium or institution may be made in an abbreviated form ...." An example would be new names proposed by authors working at the Birbal Sahni Institute of Palaeobotany who provide numbers including the letters "BS" or "BSIP".

However, we also support Skog & Fensome's cogent urging of authors of new taxa to lodge or bequeath their types in a responsible public institution and to clearly specify the lodgement in their publications.

Acknowledgements

Literature cited

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Fensome, R. A., Jansonius, J. & Skog, J.E. In press. Proposals to amend provisions regarding
typification of the names of fossil plants. Taxon.
Lentin, J. K. & Williams, G. L. 1993. Fossil dinoflagellates: index to genera and species. 1993
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Sarjeant, W. A. S. 1976. Dinoflagellate cysts and acritarchs from the Great Oolite Limestone (Jurassic: Bathonian) of Lincolnshire, England. Geobios, Lyon, no.9, p.1-43, pl.1-6.
Skog, J. E. & Fensome, R. A. 1995. What's in a date? Palynos,18, no. 1: 8-9.

1 Geological Survey of Canada (Atlantic), Bedford Institute of Oceanography, P.O. Box 1006, Dartmouth, Nova Scotia, B2Y 4A2, Canada.
2 Geological Survey of Canada (Calgary), 3303 33rd St. N.W., Calgary, Alberta, T2L 2A7, Canada.
3 Department of Biology, George Mason University, Fairfax, Virginia, 22030-4444, U.S.A.(00-00) Proposals to amend provisions regarding typification of the names of fossil plants.

Robert A. Fensome1, Jan Jansonius2 & Judith E. Skog3

The following proposals are made in an attempt to clarify an ambiguous situation surrounding the designation of holotypes for the names of fossil taxa at specific and infraspecific
rank, as is fully discussed below. The proposed changes are conservative in that they will support the validity of a group of names whose status is currently in doubt due to a lack of clarity
in the existing rules and will set out unambiguous provisions for future cases of a similar kind. No names that are at present unambiguously validly published are threatened by the present proposals.

(00) Modify Art. 8.4 to read :
"The type of the name of a taxon of fossil plants of the rank of species or below is the specimen
whose figure is, as of 1st January 2002, identified as being of the type and either accompanies or
is cited in the valid publication of the name (see Art. 38)." (Proposed modification not in italics.)

(00) Add a new paragraph to Art. 8:
"8.x. In cases where, prior to 1st January, 2002, the type of a name of a taxon of fossil plants of

the rank of species or below is designated but not identified among the illustrations, the name is validly published. However, one of the specimens illustrated in the protologue must be chosen as lectotype. This choice will be superseded if it is later demonstrated that the author of the name clearly intended another specimen illustrated in the protologue to be type."

With reference to fossil plants, Article 38.1 stipulates that since 1912 "... a name of a new taxon of fossil plants of specific or lower rank ... must be accompanied by an illustration or figure showing the essential characters ..., or by a reference to a previously and effectively published illustration or figure." Note that this Article does not specify that the illustration must be of the type - indeed, the figure could be a theoretical line drawing showing "the essential characteristics", not an illustration of an actual specimen. However, Article 8.4 states that "The type of the name of a taxon of fossil plants of the rank of species or below is the specimen whose figure either accompanies or is cited in the valid publication of the name .... If figures of more than one specimen were given or cited when the name was validly published, one of those specimens must be chosen as the type."

The conjunction of Articles 38.1 and 8.4 introduces some confusion into the requirements for valid publication; however, essentially, in order to be validly published, the name of a fossil species (or taxon of lower rank) must be accompanied by at least one illustration "showing the essential characters", and one of the illustrations must be of the type specimen (holotype).

This confusion was initially brought to the attention of one of us (Fensome & al., 1990) during the preparation an index of fossil prasinophytes and acritarchs ("single-celled" organic-walled fossils of uncertain - probably mostly algal - affinity). Fensome & al. (1990) noted many cases in which a new species was adequately illustrated, at least for nomenclatural purposes, and a holotype was designated by citation of a number (presumably a microscope slide/specimen number), but in which it was not indicated which of the illustrated specimens, if any, represented the holotype. An example of this is Moyeria uticaensis Thusu 1973, the type species of the generic name Moyeria Thusu 1973. Thus, since no type was identified with an illustration, Fensome & al. did not accept the species name as validly published; and consequently they also did not accept Moyeria as a validly published genus name. Fensome & al., and we here, have felt unhappy about this rejection of adequately described and documented new taxa; indeed, the Code does not presently specify that the illustration of the type must be identified as such, and hence Fensome & al. may have been incorrect in their assessment of the validity of the names proposed by Thusu. That author may have satisfied the Code and included the holotype among his

illustrated specimens, but there is no way of ascertaining this from a reading of his publication.

The present failure of the Code to provide an explicit requirement that the illustration of a type be identified as such is in contravention of the spirit of responsible nomenclatural practice
and hinders communication and the stability of taxonomic concepts. This problem can be eliminated, without threatening the status of existing names, by the modifications to the Code proposed above. Pending formal consideration of these proposals, and assuming that all other
conditions of the Code are met, we recommend that authors accept as validly published all names
for which a type is designated and illustrations provided, but for which the illustration of the holotype is not identified as such.

Acknowledgements

We are grateful to R.A. MacRae and P. Mudie for helpful suggestions. This is Geological Survey of Canada Contribution No. XXXXXXX.

Literature cited

Fensome, R. A., Williams, G. L., Barss, M. S., Freeman, J. M. & Hill, J. M. 1990. Acritarchs and fossil prasinophytes: an index to genera, species and infraspecific taxa. American Association of Stratigraphic Palynologists, Contributions Series, no.25, 771 p.

Thusu, B. 1973 Acritarchs of the Middle Silurian Rochester Formation of southern Ontario. Palaeontology 16: 799-826.

1 Geological Survey of Canada (Atlantic), Bedford Institute of Oceanography, P.O. Box 1006, Dartmouth, Nova Scotia, B2Y 4A2, Canada.

2 Geological Survey of Canada (Calgary), 3303 33rd St. N.W., Calgary, Alberta, T2L 2A7, Canada.

3 Department of Biology, George Mason University, Fairfax, Virginia, 22030-4444, U.S.A.

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: dias, etc.
Cc:
Bcc:
X-Attachments:

Dear Rob:

Whoa! We just got back from the Adirondacks--annual fall trip, with an old friend from Texas. We have a place at Big Moose Lake. Now we are getting packed to go to Utah. We will pick up Sid Ash in Ogden with a rent car and go with him to Oregon, where we have another "marginal palynology" project--he is doing the megafossil plants, of course. When we get back it will THEN be time to go to Germany--on 16 October. We'll be back on 5 Nov. We are going to spend Jan. and Feb. with Canadian relatives (from NB) in a trailer park in Florida.

Making the images bigger with the 35 mm. format is impossible--the cameras don't have bellows. However, with my big sheet film camera (Aristophot) I can make the image whatever size I want with the bellows. When I make slides for a presentation I use polaroid color film, make up a plate from the prints and then have that used to make the 35 mm. slides for presentation. Sarah Fowell used a scanner at Lamont-Doherty to produce some smashing enlargements of color versions of the pics in our Nova Scotia paper.

Thanks for the news about the meeting. I guess I'll have to attend again one of these days. I don't think I was treated very well by some prominent people in AASP, and I stay away partly not to be reminded of those matters. Betty has submitted an abstract for a big convention in HER field next Spring, and I am looking forward to being an "accompanying spouse!"

And thanks for the invitation to visit you. I want to do that, to see how you operate in your own laboratory. I repeat, however, that you are welcome to visit here whenever you like. At the moment we are having an addition put on the house in the country, to provide workspace for me when PSU takes the rest of what I have here away (50% of my space has been reclaimed so far).

All the best. Al.

Date: Tue, 23 Sep 1997 18:58:12 -0700
From: Rob Fensome <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: traverse@ems.psu.edu
Subject: Transparencies

Hi Al

Hope you had a good trip to Germany. Thanks for your note - glad you got the "dias" back. They were fine, as you will see when we finally manage to get a version of the poster to you. The only thing I would suggest would be to make the images a bit bigger - but you may be limited by your microscope system of course.

I have fond memories of my visit too, so I would very much enjoy a return visit in the not too distant future. Alternatively maybe you folks feel like a Nova Scotia holiday one of these days.

The Wood's Hole meeting was excellent - small (59) but intimate, friendly and scientifically exciting - and not just about phytoplankton. The gurus of the various databases (PALYNODATA, DINOSYS and others) are beginning to talk about ways of collaborating - an exciting prospect. I saw my first digital presentation - "slides" "projected" from a "computer" rather than a slide projector.

Cheers Rob

X-Authentication-Warning: turbot: Host agcux.bio.ns.ca claimed to be agcux
Date: Wed, 03 Sep 1997 14:55:06 -0700
From: Rob Fensome <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: traverse@ems.psu.edu
Subject: slides and poster

Hi Al

Hope your summer has been less hectic than mine - visitors, inlaws moving, deadlines, etc. Things would be calming down about now except that Wood's Hole organizers persuaded me to give a major talk on dino evolution, so I'm bustling right now to get slides prepared and put together and connecting with co-authors (I am but a front man in this enterprise) - all for 40 minutes of "glory".

This message is really just to let you know that your Fundy Basin slides are on their way back to you - with thanks again. I will send you a copy of the poster in due course when we have finished slides for the current job and we can tease the computers to work with such a large file - otherwise it will come in bits. We plan to present it at the Atlantic Geoscience Society's annual meeting at Acadia (Wolfville) in February. I will keep you posted.

Also thanks for copying nomenclature items to me. I will digest them after AASP.

All the best to Betty and yourself Rob

X-Authentication-Warning: turbot: Host agcux.bio.ns.ca claimed to be agcux
Date: Fri, 23 May 1997 14:07:06 -0700
From: Rob Fensome <fensome@agc.bio.ns.ca>
Reply-To: fensome@agc.bio.ns.ca
To: traverse@ems.psu.edu
Subject: pics

Hi Al

Sorry I didn't get back to you - very remiss of me. The pics are fine and they have been incorporated into the poster. The images were mostly on the small side, but we were able to enlarge them and sharpen them up a bit electronically, as well as remove the background debris and play around with the background color. (Its actually quite scary what you can do electronically.) The poster also contains a lot of other photographs, so it is a huge file and "they" are having trouble printing it right now - it may have to be split into two pieces. Anyway, you should expect to see a copy arrive on your doorstep in late June or early July.

Hope you and Betty have a good weekend. Cheers Rob

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: pics
Cc:
Bcc:
X-Attachments:

Dear Rob:

This is also "scary"--I am answering your rejoinder before you sent it, using local time for both ends.

Further technical reflections: the pics were taken with a 54x oil Leitz fluorite objective. Better def. than with 40x. 100x oil, although it yields about twice as big images, has very shallow depth of focus. With the Leica 35 mm. the 54x oil does yield a very small image, and there isn't a thing you can do about it. With the Aristophot camera which I use with Polaroid, I can use the 54x to get maximum quality of picture, and then use the bellows of the camera to go to 1500x or 2000x to fill the frame (1000x yields images in the 5-10 cm. range for most forms) if desired.

I have been wondering if I shouldn't try to go to a digital camera. I wonder if there is a way to do it without winning the state lottery.

Best. Al.

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: pics, again
Cc:
Bcc:
X-Attachments:

Dear Rob:

Don't worry about the time I've spent on this project. It's been very educational. When I last was into making slides with the scope for my lectures, I used Kodak Photmicrographic Film--it was marvellous, but has apparently been discontinued. As I told you earlier, for lectures of all kinds it is much better to make up a plate with 10 or so specimens, and have a slide made from that--for that I use Polacolor, and it has the advantage that control of the color with the filter set I have is simplicity itself because it is a question of one picture at a time.. For this project I consulted with my photo guru down the street, who advised me to use Fiji Sensia 100, as it is fine grain and has the same speed as TMax 100, which is what I use for B&W. Trouble is that he didn't remind me that it is adjusted to outdoor light, and one needs to add blue. I found that out when I had to toss the first 36 slides for wretched yellowness. For the second roll I used a light blue filter. Still too yellow. For the third roll I went to medium blue--much better. Next time (if there is one) I'd use stronger blue yet. However, I now have selected the best of the lot for you and am ready to send them off. Each item has a number (sometimes 2 different intensities, sometimes 3, sometimes only one), in case you need to write me about it. For example, I have mostly provided only generic assignment, but I could give you species too with a little digging around.
If there were time to shoot another 36 exposures, I'm sure I could be prouder of the results, and if you deem it suffiently important, I could even do that after you get these and overnight the new ones to you.

So, Betty will be getting these off to you later today by some sort of express mail.

Best. Al.

X-Authentication-Warning: turbot: Host agcux.bio.ns.ca claimed to be agcux
Date: Fri, 02 May 1997 13:38:17 -0700
From: Rob Fensome <fensome@agc.bio.ns.ca>
To: traverse@ems.psu.edu
Subject: pics

Hi Al

Thanks for the message. Sorry that you are having some difficulty. When the images are digitized the yellow hue can be changed, so that's not a problem - however resolution would be, although even that can be improved I believe. Color negatinves would be fine if you find them a lot easier to manage. Thanks again for all the trouble you are going to.

Lorraine's group did Elijah a while back - but I'm sure they didn't pay $20,000 for the soloist - can anyone sing $20,000 well I have to ask?

Cheers Rob

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: pics
Cc:
Bcc:
X-Attachments:

Dear Rob:

Thanks for yours of 28 Apr., just read.

Day before yesterday I took my 36 exposure roll of photos of nice Triassic/Jurassic sporomorphs from Fundy Basin to my local photo gurus for processing. I just picked them up. I am disappointed in the results and now see why I gave up using 35 mm. for color (though I use it for BW all the time). Color is very tricky in the microscope. These are all too yellow. If I had been using Polacolor I'd have noticed this in the first shot and adjusted with pale blue filters. I'm also not satisfied with the definition. Another problem, of course, is that the 35 mm. pics are relatively tiny, whereas I always take Polacolor at 1000 x, so that the average sporomorph is 40-50 mm. across. For teaching and scientific papers I made up plates of ten or so such and had them photographed by the PSU photo services people to produce 35 mm. slides.

So, now I have another roll of film and will try a pale blue filter, keeping everything else the same. We'll see. A few of the present ones might even be acceptable.

Interesting to hear about Lorraine's singing group. Maybe I mentioned that Betty's large choral society just did Mendelssohn's Elijah, with a professional orchestra and a soloist from the Metropolitan Opera whom they paid $20,000 for the one performance. The whole thing was a tour de force for sure.

Best. Al.

X-Authentication-Warning: turbot: Host agcux.bio.ns.ca claimed to be agcux
Date: Mon, 28 Apr 1997 14:17:24 -0700
From: Rob Fensome <fensome@agc.bio.ns.ca>
To: traverse@ems.psu.edu
Subject: pics

Hi Al

Thanks for your message. Several exposures of each would be good. We send stuff out to be scanned and color negs or dias are equally good in terms of quality - yes slides can be directly scanned. However, in the deal we have worked out with the supplier, we are charged CA$1.00 per slide and CA$1.65 per negative - the difference being in the ease of handling.

Winter seems to be over here too, though we won't see much in the way of leaves until mid, or even late May. Lorraine in the midst of a heavy music season right now, which makes me chief cook, bottlewasher and parent at home. This coming weekend, Lorraine's choir (Halifax Camerata Singers - the only "professional" choir in the Maritimes, professional in the sense that the members are vocational singers, though there is no payment per se) is cutting a CD featuring Maritime folk songs.

Good luck with your trees and "hi" to Betty.

Cheers Rob

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: pics
Cc:
Bcc:
X-Attachments:

Dear Rob:

No, I have no preference as between transparencies and prints. In my uninformed way I thought when you said you would scan the pics in, that prints would be better. When Sarah and I were getting stuff ready for a poster of hers she used prints for scanning and got fabulous results as to definition. I will use several exposures for each pic so that you can choose whichever density is best.

For my own info I'd like comments on the prints vs. dias (as the Germans say for transp.) question. Do you scan in directly from a dia, or do you have to make the required intermediate negative and print first? That seems so unlikely that my guess is that you use the dia directly

You are one of several of my correspondents who use the trick of answering a missive by using all caps inserted into the original message. The problem with this technique is that when one prints out the message it always puts it into extra pages. I can imagine if two people who use this technique were corresponding, it would soon get entirely out of hand.

Winter seems finally over. Yesterday I planted a flowering dogwood, an ornamental pear, and a fir (Abies). And noticed that a damned male deer last October girdled one of my favorite trees--Amelanchier canadensis. I planted it because there was a dandy on my grandfather's farm in New Brunswick, and I loved the berries. This is a regular occurrence--the bucks use small trees to clean their antlers--and I usually protect all trees with plastic tubes until they are about six inches in diamer--at that point they are too big to be useful to our gigantic vermin. But this "billberry", as we inaccurately called it in NB, got missed. This antler-honing, plus the incredibly destructive browsing, are nocturnal habits.

Best. Al.

X-Authentication-Warning: turbot: Host agcux.bio.ns.ca claimed to be agcux
Date: Thu, 24 Apr 1997 16:52:55 -0700
From: Rob Fensome <fensome@agc.bio.ns.ca>
To: traverse@ems.psu.edu, fensome@agcux
Subject: Re: photos
References: <199704241737.NAA00393@pangaea.ems.psu.edu>

Hi Al

Many thanks for your message. I've made the appropriate inserts below in the body of your message
below - I hope it is readable. This is the first time I've done this, so I'll send a copy to myself too, to check that it transmits properly.

Cheers Rob

Alfred Traverse wrote:
>
> Dear Rob:
>
> Gotcha note about the pics for the poster. What I propose to do is to get a
> roll of the appropriate color film and shoot a roll of pictures from my
> slides. That will take most of a day but is worth it. But before I begin,
> I have a couple of questions: 1. do you have a preference as to film? I
> will use a Fuji color film for prints of the least grainy type unless I hear
> to the contrary. Ektachrome Kodak is another possibility. SLIDES WOULD BE EASIER TO CONVERT TO DIGITAL FORMAT, BUT IF YOU TRULY PREFER TO USE PRINT FILM, PLEASE GO AHEAD - WE WILL NEED TO BORROW
THE NEGATIVES THOUGH. 2. May I have
> permission to shoot the pictures from Fundy Group samples with a view to
> getting the best pictures - ABSOLUTELY-or do the pictures have to be from the slides I
> have on loan from you all? NOT AT ALL - ANY FUNDY MATERIAL WOULD BE FINE If the latter, it will take longer .
>
> The $142 package from Stuttgart just came in the door--with a slip saying,
> "Bitte entschuldigen Sie den Packfehler." So, now I gues I am up to date,
> with both Vol. 3 and Vol. 4. VOLUME 5 (A SMALL ONE PROBABLY COMPRISING AN INSERTABLE PACKET OF SPECIES) ON SPANISH LANGUAGE TAXA, WILL GO TO PRESS THIS YEAR. i'LL TRY TO REMEMBER TO KEEP YOU
POSTED.
>
> Best. Al.
> Alfred Traverse
> Palynological Laboratories
> Department of Geosciences
> 435 Deike Building, PSU

> University Park, PA 16802
> Ph.: 814-863-3419
> Fax: 814-863-7823

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: photos
Cc:
Bcc:
X-Attachments:

Dear Rob:

Gotcha note about the pics for the poster. What I propose to do is to get a roll of the appropriate color film and shoot a roll of pictures from my slides. That will take most of a day but is worth it. But before I begin, I have a couple of questions: 1. do you have a preference as to film? I will use a Fuji color film for prints of the least grainy type unless I hear to the contrary. Ektachrome Kodak is another possibility. 2. May I have permission to shoot the pictures from Fundy Group samples with a view to getting the best pictures--or do the pictures have to be from the slides I have on loan from you all? If the latter, it will take longer .

The $142 package from Stuttgart just came in the door--with a slip saying, "Bitte entschuldigen Sie den Packfehler." So, now I gues I am up to date, with both Vol. 3 and Vol. 4.

Best. Al.

X-Authentication-Warning: turbot: Host agcux.bio.ns.ca claimed to be agcux
Date: Wed, 23 Apr 1997 13:47:15 -0700
From: Rob Fensome <fensome@agc.bio.ns.ca>
To: traverse@ems.psu.edu
Subject: Triassic palynomorph images

Hi Al

I recall that you picked up that alternate address from somewhere and mentioned it to me - it was
the first time I knew that I had a second e-mail address. And until I changed to using my Netscape
e-mail the difference between the two addresses was invisible. I certainly have always promoted my
original address - "bio" for Bedford Institute of Oceanography sounds far nicer than NRCan (for
Natural Resources Canada).

John Wade is preparing a poster under the Fundy paper authorship (thus including your good self) in
which he would like to include some palynomorph images. However, I have a problem in providing
said images because you have all the good slides; moreover, my selection of photographs of that
material is not great. I wonder therefore if you would either let me borrow back the slides (you

can borrow them back again) or, alternatively, loan me some photographic images of the key taxa.
The images should be in the form of negatives (preferably color) or transparencies, in order that
we can scan them and then place them in the poster in electronic format.

The taxa we should illustrate are those mentioned in the paper - Ovalipollis spp., members of the
Patinasporites complex, Alisporites spp., Triadispora spp., Corollina, and any others that you would consider appropriate. My preference would actually be for you to send the images rather than
the microscope slides, since you know your way around that material far better than I do.

The poster is actually being compiled by my technician, Bill MacMillan (John is now emeritus and
has no facilities other than his office space and a computer), and it is looking very attractive, with cliff shots, colorful maps, etc. A few palynomorphs will fit in very nicely. You will of course receive a complimentary copy of the poster, laminated if you wish.

The poster has to be ready for early June, so time is at a premium - sorry for not giving you

more
notice. Your assistance in this would be very much appreciated Al - please let me know as soon as possible whether you will be able to help.

All the best Rob

From: fensome@agc.bio.ns.ca
Date: Mon, 21 Apr 97 18:45:27 -0300
To: traverse@ems.psu.edu
Subject: re: reprints
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al

I hope you had a good trip, if that's appropriate. The reprints are without charge to you - we're happy to have had you on board. The price for the Eisenack volume is steep, but sounds about right. Consider that you are supporting (in part anyway) a good cause - royalties are plowed back into the project.

Judy and I are just finishing the last minutiae of the "form-genus" paper, which has been accepted for publication after a relatively painless review process - thanks I'm sure to pre-submission feedback from yourself and others. (I haven't had a response yet from Bill, by the way.)

All for now, except would you please use my other e-mail address - fensome@agc.bio.ns.ca
I'm using Netscape now for e-mail and the above address doesn't come in on that line (or whatever the appropriate jargon is). No big deal - just a bit more convenient for me if you use the "bio" address.

Cheers Rob
Cheers Rob

****

Reply to: fensome@agc.bio.ns.ca

Rob Fensome
GSC Atlantic
Geological Survey of Canada
P.O. Box 1006
Dartmouth, NS B2Y 4A2
Canada
Tel. 902-426-2732
Fax. 902-426-4465

From: fensome@agc.bio.ns.ca
Date: Tue, 25 Mar 97 16:24:01 GMT (Original AST)
To: jjansonius@gsc.nrcan.gc.ca, traverse@ems.psu.edu
Subject: Nomenclatural issues
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Jan and Al

Life finally seems to be coming under control - I'm even making progress at the microscope - so I'm opening this file to make comments on some of the nomenclatural issues that have been whizzing by me in the last few months. I'm sorry they are so tardy - apologies especially to Jan - if you are in the final throes of putting together the genera file, or have even finished it, the last thing you need is input at this stage. However I don't think there is much of great import in the following that will affect (or could have affected) your endeavour.

With regard to the exchange between Gea Zijlstra and Jan, I would comment that I personally could live with either of two scenarios - either no corrections from the original author's spelling (except correction of the ending if necessary), or corrections based on clear (strict?) guidelines. (Sorry Jan but I'm not sure whether you would consider the latter to constitute a form of straightjacket - but aren't all rules and guidelines straightjackets of sorts.) In the latter case, I think there needs to be consistency. E.g.1: it could be stipulated that we should correct names/epithets based on personal names only - i.e. the misspelled epithet "dettmaniae" would become "dettmanniae". E.g.2: it could be specified that we correct names/epithets based on personal names and places - i.e. in addition to the above example, the misspelled epithet "hacknesense" would become "hacknessense". E.g.3: it could be specified that all names/epithets based on all proper nouns - i.e in addition to the above examples, Anonaceaephyllum would become Annonaceaephyllum. Such clearcut guidelines would allow fussy systematists like myself to worry less about what the correct spelling should be and get on with more important things.

(Yes, I agree of course that there are more important things, but in my opinion it is important - not least for external perception of our science - that plant names should be consistently spelled; otherwise we are allowing anarchy to have a foot in the door, and anarchy can spread so easily, as the present "big wigs" in charge of botanical nomenclature are doing their darndest to demonstrate.)

Just a point from your letter of 12th December relating to the form-genus proposals, Al. You wrote (point 6) "Practically all fossils are in some sense or other `fragmentary'. I cannot agree that therefore all fossil

plants are, by definition of the present ICBN form-genera!" Judy and I were just pointing out that the ICBN literally seems to say that all fragmentary fossils (and hence, as you imply practically all fossil plants) are form-genera, and hence not assignable to a family. This is of course an absurdity, as I think you are trying to point out in your comment - however (as I think we essentially agree now), the absurdity is in the Code, not in Judy's and my article or proposal. Incidentally I haven't had any feedback from Bill yet.

I've now read your exchange regarding the van der Hammen genera through to about early January and, although there may be some more in subsequent e-mails, I want to get something down in blue and white (my screen colors) before I get too lost. Are van der Hammen's names (some anyway) illegitimate? Jan's argument seems to be that since vdH designated as the holotype, e.g., of Retitricolpites (as a subgenus) "... a recent pollen grain of Neea macrophylla ...", Retitricolpites is illegimate following Article 52.1. However, Article 52.2 states that "For the purpose of Art.52.1, definite inclusion of the type of a name is effected by ... citation of the name itself, unless the type is at the same time excluded either explicitly or by implication." Hence, it could be argued (as I think we did in a parallel case last year, the specifics of which I can't remember offhand) that vdH did not perceive Retitricolpites and Neea as synonymous and that he didn't intend the holotype of Neea to be (illegitimately) included in Retitricolpites. Hence, it could be argued that Retitricolpites was both valid and legitimate in vdH. (Note added later: I realise from one of Al's subsequent notes that there are yet more specific complications in the Retitricolpites situation, but please take my point here as a general one.)

Trouble is, if one argues that Retitricolpites is valid and legitimate, one is stuck with what was intended to be a fossil form-genus (sensu Fensome and Skog - "alternate genus" if you wish) with an extant type. Retitricolpites should then perhaps be treated as a taxonomic (albeit validly published and legitimate) junior synonym of Neea. Then we are free to use another form-genus for pollen grains, unless Retitricolpites vdH is conserved with a different type. Retitricolpites Potonie (ex Traverse in Farr et al??) (type R. ovalis) would be an illegitimate junior homonym of Retitricopites vdH (type Neea macrophylla).

With regard to Karina Hoorn's reading of the ING entry for Psilatricolpites, I think that she misread the (admittedly muddy) statement "The combination in the genus was not validly published by van der Hammen and Wymstra ...." This I think refers to the species name Tricolpites operculatus, not the generic name Psilatricolpites, which they indicate to have been validly published in vdH & W.

(By the way, the ING is very messy for fossil dino genera, and shows no indication that any fossil dino workers were consulted.)

I now read on ....

With regard to the names that Ross (1949) put in quotes, he indicated that these are "working names" and hence provisional. Article 34.1 states that "A name is not validly published ... when it is merely proposed in anticipation of the group concerned ... (so-called provisional name) ...." Ross's working names were proposed in anticipation of their non-acceptance, which seems to me be a step even more provisional. I don't have strong feelings, except that we need to be consistent in interpretation. Is it worth a change of mind at this stage and all the problems that might create? (Note added later: I see that Al said essentially the same thing on 7th February. I would agree with you, Jan, that unless Weyland and Krieger specifically refer to Ross's specimen as type, then as validating authors, their specimen automatically becomes the type. This is the way I interpreted many such cases in the "acritarch index".)

With regard to your last question of January 15th, Jan, as validating author I think that you would have to specify the lodgement. But if Greuter wants to play games with quixotic rule changes, maybe we systematists in the trenches can play games too. Perhaps you could specify that "presumably the type is in the collection of XXX Institute" (wherever the authors were) or even "... the collection of XXX palynologist (being the originating author). The rules require a repository to be named - it doesn't say that you can't bend the truth. (Naughty perhaps, but if the Code is causing us unreasonable headaches but not prescribing remendies, what do the "establishment" people expect.)

I'm now on Al's message of 5th February. With regard to the vdH names, if you need some species lists from Palynodata, I think we could work something out - even if it means that I'm a junior junior author. (I'm not angling for another publication, just trying to figure out the best way to legitimately maximize the info available to you, Jan.)

With regard to Jan's note of 10th February, I'm essentially in agreement with Jan over continued use of the term "type species". In doing the "dino index" Graham and I have found that, in discussions, we have to use the term "type species" as a shorthand for "the species to which the nomenclatural type of the genus was originally (and usually still is) assigned." There is just no practical(!) way around it that we can see. (It certainly wouldn't be practical to cite that phrase on a regular basis.) However, I'm firmly in favor of the Code specifying that the type of a genus is a specimen, and I think in our indexes and catalogues we have to be clear that we are using the term "type species" in an informal sense. To this end I'm happy to note,

Jan, that you are listing "type" under each genus and not "type species".

This would seem to get me caught up on everything but BioCode matters (yikh!), which may take a few more months for me to get around to. More pressing is the writing of a proposal to clarify the Code on the designation of fossil types. Again, Judy and I promise not to be Greuterian; we intend only to do housekeeping that will make life easier for paleobotanists. I'll send you both a draft, and if you like it enough perhaps you might be inclined to be co-authors. I also have to write up a cluch of dino-related conservation proposals (Protoperidiniaceae in favor of Congruentidiaceae and Rhaetogonyaulacaceae in favor of Shublikodiniaceae).

Have a great Easter. Sorry that this has been so long (indeed I wonder if either of you are still with me at this point).

Cheers Rob

****

Reply to: fensome@agc.bio.ns.ca

Rob Fensome
GSC Atlantic
Geological Survey of Canada
P.O. Box 1006
Dartmouth, NS B2Y 4A2
Canada
Tel. 902-426-2732
Fax. 902-426-4465

From: fensome@agc.bio.ns.ca
Date: Mon, 10 Mar 97 19:52:58 GMT (Original AST)
To: traverse@ems.psu.edu
Subject: re: WGC letter
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al

Thanks for your message. Sincerest apologies for misrepresenting your views. I know that you have talked about mosaic evolution before - and I agree with everything you say on that score. However, I also know that you are opposed to special pleading in the Code, so I interpreted that as extending to alternate names for fossils. Your message explains all, and if the opportunity presents itself in a future letter, I will rectify the offending phrase. I will certainly remove the comment from any version that I circulate.

Your comment that Bill is a theoretician is an interesting one. I absolutely and emphatically agree that it is unfortunate that theoretical notions should muddy the waters of what should be a straightforward manual. My "impression" is that the editorial committee is full of theoreticians and my worry is that it really doesn't matter how strong a case we make and how much support we have from the trenches, Judy and my proposals on form genera will be shot down at the editorial stage. Like you, I like and respect Bill, but I have to admit that the letter was written largely with this aspect in mind. However, from what I hear of your experiences, he probably won't be very amenable to our views. I'm all for simpler approaches, if you have any suggestions.

The BioCode seems to be another example of theoreticians run mad. I foresee chaos - perhaps we paleobotanists (sensu lato) should have our own Code - viz a (judiciously) cleaned up ICBN? How's that for a heretical idea to end this message on. No, I'm not serious - yet.

All the best Rob

****

Reply to: fensome@agc.bio.ns.ca

Rob Fensome
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Geological Survey of Canada
P.O. Box 1006
Dartmouth, NS B2Y 4A2

To: rfensome@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: WGC letter
Cc: jjansonius@gsc.gc.ca, jskog@gmu.edu
Bcc:
X-Attachments:

Dear Rob:

Unfortunately, I asked the wrong question. I knew how much they charged for Vol. 3. Trouble was they sent Vol. 4. So, I just sent them a letter explaining the situation, ordering Vol. 3 and asking for appropriate adjustment of price.

Your letter copy of missive to Chaloner is in. I will not respond in depth here, but I will say that reading your letter, say with no prior knowledge of the whole affair, would convince most practically minded people that the whole business of loading up the Code, a manual for NAMING, with line after line of theoretical gobbledygook that has little to do with the practical matter of naming, is hogwash.

I don't know where you get the idea that I am against having a strong statement that alternate names for fossil plants are acceptable and desirable--in all of the stuff I've sent you lately I have emphasized that I am FOR such usage. Mosaic evolution in plants makes the situation different from that in animals. Florinites sp. may be the pollen grain genus for a whole range of gymnosperm fossil plants. Geminospora may even be both a progymnosperm microspore and a lycopsid spore. Finding Florinites in a pollen sac should have no bearing on the name for the plant bearing that sac. Etc.

It slowly has dawned on me that Bill is, in fact, a theoretician who seldom (if ever) works in the trenches of fossil plant naming. You, Jansonius and I have more nomenclatural experience in our left toes than dear Bill has in his whole body. I very much like and respect Bill, but I really think it is unfortunate to let theoretical notions come much into play in the framing of what is a handbook for doing a certain task.

I believe that the Code should provide for alternate name authorization, but I am against all efforts to pin down the matters about form and organ genera, their alleged relationship to each other, and to alleged higher categories, and all of the various clones of those concepts.

Whole plant names (Lepidodendron, etc.) should be left informal as they now are and should not be provided for in the Code at all. They are concepts that are best left fluid, like common names for extant plants.

As I noted above, all a person has to do is read your letter to be sure that this whole business needs treatment in a strong solvent. If two successive chairmen of the fossil plant nomenclatural committee have so much trouble making sense about something, wouldn't a logical conclusion be that making the average bloke in the trenches understand it is a waste of time and perhaps a will o' the wisp in the bargain? Perhaps even that a simpler approach is

called for?

All the best. Al

From: fensome@agc.bio.ns.ca
Date: Mon, 3 Mar 97 17:41:47 GMT (Original AST)
To: traverse@ems.psu.edu
Subject: re: catalog
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al

There are 4 volumes of the Eisenack Catalog New Series out. Volume 3 costs US$160. Hope this info helps you decide what to do. We plan to put out a small supplement on species originally described in Spanish later this year, but after that it will probably be a couple of years before we do the next volume. This is because I'm going to be heavily involved in the next "Lentin and Williams" index of dinoflagellates and I'm a co-author on a volume on dinoflagellates in the Treatise of Invertebrate Paleontology series. Its a good opportunity to review the genera.

A copy of a letter from me to Bill Chaloner on the form-genus issue should land on your doorstep soon.

All the best for now. Cheers Rob

****

Reply to: fensome@agc.bio.ns.ca

Rob Fensome
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Natural Resources Canada
Ressources Naturelles Canada

Geological Survey of Canada
Commission geologique du Canada

21 February 1997

Thanks for the copy of your note to Werner, Al. I'll be writing soon in answer to your comments of our MS. However, I think you, Judy and I are ± on the same wavelength. Not so Bill, I think - hence this weighty (?) response.
all the best - Rob

Prof. W.G. Chaloner
Department of Geology
Royal Holloway
University of London
EGHAM, Surrey, England
TW20 0EX

Dear Bill:

Many thanks for your comments on the proposal by Judy and I and apologies for not responding sooner. The subject requires space for thought and I don't seem to have had much of that recently - 1997 has been a "fire-fighting" year so far.

You are right in believing that the gap between our thinking is not substantive. Your letter gave us the opportunity to think over our proposal and make some modifications, but I think at core our original ideas remain. For discussion purposes the subject can be broken down into a number of separate questions/issues:

1) The ambiguity (or non-ambiguity) of the wording of Article 3.3.
2) The desirability (or non-desirability) of allowing for alternate names for fossils.
3) The location of the appropriate legislation within the Code, if alternate names are deemed desirable.
4) The nature of this legislation and whether it should involve the terms "form" or "organ" genera.
5) Whether such legislation should/needs to say anything about family assignment.

I'll address each of these issues separately in an attempt to clarify our case and perhaps to provide a basis for identifying areas where we still disagree.

1) With regard to Article 3.3, you state:

> "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-genus and cannot be assigned to a family'. The Code says nothing of the kind: Article 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.'" (Your emphasis and double emphasis.)

I think what you are trying to point out is that Article 3.3 is only intended to refer to fossils that are so fragmentary that assignment to a family is not possible.

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Canada

Intended originally maybe, but the first part of the Article states: "Because of the fragmentary nature of the specimens on which the species of some fossil plants are based ...." O.K., so the implication is that SOME fossil plants (I'm repeating your emphasis) are based on fragments (for the sake of argument, twigs, seeds, leaves, pollen, cysts, etc.) and some aren't. The further implication surely is (reading literally, without special knowledge of the history of the Article) that Article 3.3 is discussing these "some fossil plants" that are fragmentary. The phrase that you emphasize reads "some fossil plants", not "some of the fossil plants that are based on fragments".

The Article then continues "..., the genera to which they [i.e. in my reading - the species of fossil plants that are based on fragments] are not assignable to a family". Ergo, all genera based on fragments are not assignable to a family. (You may respond that the Code doesn't say "all", but surely in lieu of any other qualification, "all" must be assumed.) I'm not being difficult (at least not intentionally) - this is the way that I read the Article. It is also the way others read it. For example, I showed it to two experienced palynological colleagues here and, without my biasing their response, they came out with the same interpretation. Perhaps it has something to do with the strange workings of dinophycologists' brains.

However, in defense of dinophycologists, when I showed an early draft of this letter to Judy (who is a paleobotanist sensu stricto of course), she commented that "While my original interpretation was a bit closer to that of Bill, I still thought that almost all fossil plants HAD to be assigned to form-genera because they were all by nature fragmentary." (I should add here that this letter is written with Judy's help and endorsement.)

I notice that your letter also is somewhat ambiguous about Article 3.3. In the passage quoted above, your interpretation is clearly that not all fossil plant fragments are obligatorily assigned to form-genera. To me this seems to imply that no definition of form-genus is involved in 3.3. (Unless it is the suspiciously circular definition that form-genera are genera incorporating fragmental fossils not assignable to a family.) However, two paragraphs down you state that Article 3.3 "... has just explained what form-genera are ...."

The point here is not so much my interpretation versus your interpretation, but rather, is the Article ambiguous? Given that I am only one among many who seem to have "mis"interpreted it, as we also demonstrate in the manuscript, I would contend that Article 3.3 is ambiguous at best and, at worst, directly misleading.

2) I believe there is broad agreement that alternate names need to be allowed for fossil plants, because of the fragmentary nature of the fossil record. To Judy and I, this is the crux of the matter, not the assignability of genera to families.

One possible solution might be to delete all special pleading for fossils in this regard, as I believe Al Traverse would wish. However, this would lead to uncertainty and much profitless debate about whether, for example, *Tiliapollenites* really is just a junior synonym of *Tilia* and should be disallowed. Allowing alternate names under the Code permits the debate about *Tilia* and *Tiliapollenites* to proceed in a taxonomic context, without nomenclatural rules obtruding.

3) According to general interpretation, including that of Ed Voss in a letter to Judy, Article 3 deals with ranks of taxa. It is largely a descriptive Article laying out the names and sequence of ranks. To us, Article 3.3 is a "red herring": why is it necessary to state that some fossil genera are not assignable to families? Article 3.1 specifies that each genus is assignable to a family - it doesn't say that each genus must be assigned to a family (and indeed, not all genera are assigned to families). So whats the big deal about fossil genera that aren't thus assignable. If, as we would argue, the crux of the matter is about allowing alternate names for fossils, this is related to priority matters and is better dealt with in Article 11.

I agree with you that Article 3.4 is rather useless. Judy and I didn't touch on it in our proposal since it is innocuous as it stands, and we didn't want to give the impression that we were on a "slash and burn" sortie.

4) This somewhat overlaps previous points but, as noted above, because we feel that the crux of the matter is the allowance of alternate names for fossils, we think that the appropriate legislation should be under Article 11. We believe that the legislation, in essence, needs to allow for alternate names, which we, in the proposal, term form-genera. We could equally have called them alternate names, but this term is used elsewhere in the Code. We are not necessarily stuck on the term "form-genus", but we certainly feel that the term "organ-genus" has too much associated baggage to be harmlessly resuscitated. (Note that moving the legislation as we propose wouldn't affect existing practice.)

5) Surely family assignment is a taxonomic issue. Article 3.3 comes across as irritatingly dogmatic when it stipulates that form-genera (i.e. in my reading, all fragmental fossil plants) "... are not assignable to a family ...." But why are we then given more rope at higher ranks - "... although they [form-genera] may be referable to a taxon of higher rank." Since I know the history of the Article, I realise that originally the concept of form-genus was intended to be the counterfoil to the concept of organ-genus. As I understand it, if a fossil plant fragment showed enough diagnostic features to be assignable to a (natural) family, then it was treated in an organ-genus; if not, then it was treated in a form-genus. These are the equivalents of the "N" (Natural) and "F" (Form-) genera of Meyen, which you discuss as if dealing with an on-off toggle device in a computer program. Surely there are all gradations between N and F genera, and hence between organ-genera and form-genera. In any case, and critically, such distinctions are based on taxonomic decisions and, in our view, have no business being treated in any way in a nomenclatural code.

Back at Article 3.3, the removal of organ-genus left the wording ambiguous. What is currently intended, I think, is an expression that fossil plant fragments are often insufficiently characteristic to be assignable to a natural family. However, Judy and I consider that this is not what Article 3.3 literally says. Moreover, we also argue that, as it stands, the Article is partly inappropriate for inclusion in a code of nomenclature and partly misplaced within the Code. Since the Code has to be clear, accessible and nontaxonomic, Judy and I felt that we had to make some proposals.

Well, there is our argument again. I'm not optimistic about persuading you over to our way of thinking, but perhaps the above discussion may serve to locate where we still disagree. Thanks Bill for your patience. I am copying this letter to Al and Judy. With your permission I would also like

to copy at least this letter to Werner Greuter, Dan Nicolson, David Hawksworth and John McNeil, who are also involved in the broader context of this debate.

Its too late to wish you a Happy New Year, but perhaps best wishes for a good Spring and Summer is a brighter sentiment anyway. My mother tells me that you need rain too, so I'll wish some of that, but only at night.

Best wishes,

(Rob).

Robert A. Fensome
Marine Resources Geoscience Subdivision

RAF/nk

c.c. J. Skog
A. Traverse

From: fensome@agc.bio.ns.ca
Date: Mon, 17 Feb 97 18:48:52 GMT (Original AST)
To: traverse@ems.psu.edu
Subject: re: nomenclature
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al and Jan

Thanks for keeping me informed. My silence is not due to lack of interest but to lack of time. Actually, I did spend today answering (finally) Bill Chaloner's letter regarding the form-genus manuscript of Judy and myself. I will send you a copy of course. (Jan, due to a sort of oversight - I was going to suggest you as an official reviewer, so I didn't want to involve you too early - you haven't seen a copy of this yet. I will remedy the situation soon.)

Yesterday we said goodbye to a visiting palynologist, Raquel Guerstein, from Argentina, who has been here for 2 years working with myself and Graham. She brought her family - like me she has two young daughters - and as well as being a very productive two years scientifically, our two families became very close. It was sad to see them go, but a relief in the sense that my schedule may become a little freer. We've been spending much of the last few months wrapping up our co-operative efforts.

With regard to the spelling of English words, one of the great advantages of being Canadian is that one can choose which spelling one deems the best (depending on the ultimate location of the text of course) - and I have to say that I don't always find the American spelling an improvement over the British. (There's an unbiased statement if ever there was one.) There are some really odd quirks in this American versus British English debate - note that on the one hand it is the National Geographic and the Geographical Magazine, but the Royal Botanic Gardens at Kew, in contrast to the name of most such American instititions, which I understand are called "botanical gardens". (You're probably going to tell me that the latter is wrong Al and that the whole situation is just one of British inconsistency.)

Enough. I still intend to peruse the various problems that you have been discussing. I also have to write up some proposals on tidying up the type situation for fossils - nothing drastic or Greuterian though.

Cheers to you both.

Rob

****

Reply to: fensome@agc.bio.ns.ca

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Geological Survey of Canada
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Date: Thu, 16 Jan 1997 12:41:56 -0500 (EST)
Resent-From: fensome@agc.bio.ns.ca
Resent-Date: Thu, 16 Jan 97 12:59:32 GMT (Original AST)
Resent-Message-ID: <EPX7+uSarma@agcban1.bio.ns.ca>
From: fensome@agc.bio.ns.ca
To: traverse@ems.psu.edu
Subject: re:
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Forwarded to: smtp[traverse@ems.psu.edu]
cc:
Comments by: Rob Fensome@BAS@GSC HALIFAX
Comments:

Hi Al

I intended to copy the attached to you directly, but forgot. Thanks for your input on the form-genus proposal. I will reply to you in more detail in due course, but I think we are pretty much in agreement - even where you tried to make out that we weren't (the message being that we needed to clarify our text).

Cheers Rob

---------- [Original Message] ----------
Hi Jan

Thanks for "copying" me. I will take a look at the van der Hammen problem in due course, but I'm somewhat swamped right now - largely beacause my New Year's resolution is to spend 3 days per week working at the microscope (after all that is what I'm paid for, sort of). Everything else has to fit into the other two days plus the odd evening and weekend. Not sure how long I'll hold out.

The question about type goofs is related to the situation that we were discussing earlier about types that were technically designated but not directly related to a specific illustration. Did the author actually give a specimen number (I'm aware of all the pitfalls that that may involve)?

Judy and I plan to come to grips with the type situation for fossils in terms of a proposal which will make it clearer what to do in situations like the one you have found and the myriads of examplles that we found in doing the "acritarch index". At this stage my provisional thoughts would be to accept the original as validly published IF the original author attempted to designate a single specimen - i.e. gave a specimen number and/or listed a

single illustration (even though two or more might appear on the plate). If no such attempt was made, I fear that the name ought to be considered not validly published.

I hope this helps.

Cheers Rob

****

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From: fensome@agc.bio.ns.ca
Date: Tue, 7 Jan 97 13:44:25 GMT (Original AST)
To: w.chaloner@rhbnc.ac.uk, wg@fub46.zedat.fu-berlin.de, d.hawksworth@cabi.org, johnm@rom.on.ca, jskog@gmu.edu, traverse@ems.psu.edu
Subject: Cascade of messages
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

This is just to wave a flag (a white flag?) to signify that I'm here and receiving the cascade of Code related e-mails. Its nice to know that Judy and I (Bill and Al too) seem at last to have stirred some interest in the fossil situation. I don't have the time at present to digest and comment on the problems as they deserve. I will devote a day to nomenclatural matters soon however and give some feedback.

I fully endorse Judy's note of this morning, especially with regard to the need to avoid the taxonomic concept of organ taxa in a nomenclatural code and, in general, the necessity to keep taxonomic matters out of nomenclatural codes of whatever stripe.

Cheers Rob

****

Reply to: fensome@agc.bio.ns.ca

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From: fensome@agc.bio.ns.ca
Date: Tue, 26 Nov 96 11:24:37 GMT (Original AST)
To: traverse@ems.psu.edu
Subject: re: mountants
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al

A package with mountant and recipe is on its way to you. The reason that I haven't expressed an opinion re Clearcol is that I don't have one - I'm not aware that I've ever used the stuff. I've asked our lab tech here and he says that it was used as an experiment in the early days, long before I arrived here, but didn't prove satisfactory.

I look forward to your thoughts on our form-genus proposal. I think that we are both motivated by the need to keep taxonomy out of nomenclatural rules. I believe that one can only do this for fossils by allowing (not prescribing) alternative ("form") names for fragments or life-cycle stages in fossils. Otherwise the rules, in theory, would force people to synonymize, say, Gonyaulax and Spiniferites. If alternate names are allowed for, then the decision about synonymizing these genera becomes a purely taxonomic issue, unshackled by nomenclatural rules (intended or not; clear or, as at present, fuzzy).

Anyway, as I say, Judy and I look forward to your comments.

All the best Rob

****

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Fensome, Rob

No Recipient, No Subject ± 11/25/96 1

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: MS

Dear Rob:

This is Thanksgiving week, and we are very took up, but I WILL get at the MS ere long. If your main thrust is alternate names approval, you won't get static from me, but I hate long sets of directions for actual application of names that apply only (in this case) to (certain kinds of) fossils.

Thanks for clarification on mounting media.

By the way, the custom of having T. D. on the second Monday in October, to which I was born, makes a hell of lot more sense than having it three weeks before Xmas.

Best. Al.

To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: mountants
Cc:
Bcc:
X-Attachments:

Dear Rob:

No, no, not glycerine, glycerin jelly. Glycerine is, of course, a liquid and doesn't have a perfect RI either. Gj, on the other hand, is a thermoplastic solid and has a sensational RI. Its permanence is a very strange business. I have slides I made 50 yrs. ago that are fine, and some I made ten years ago that are hopeless.

I look forward to getting the sample and the recipe. I repeat, however, your opinion on Clearcol, etc.?

The form genus thing came through o. k. I'll get to it soon, probably over the weekend. As you know, I have always felt that the rules for naming are applicable to practically anything, even non-living things (and that was Linnaeus' opinion too). I therefore think that special provisions for naming of this and that kind of thing are unnecessary. I would surely keep it to an absolute minimum. The old form genus versus organ genus thing was a bunch of crap. Some thought should be given to the fact that one of these days there will be a single Code for everything. Will the governors thereof want stuff about "form genera?"

All the best. Al.

From: fensome@agc.bio.ns.ca
Date: Mon, 18 Nov 96 20:05:31 GMT (Original AST)
To: traverse@ems.psu.edu
Subject: ...no subject...
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al

Sorry for the delay in answering your mountant questions in any detail. However, I really don't have a lot of detail to add - mountants have not been a passion of mine and I've been (complacently?) happy with the stuff we use. I've never really worked with glycerine - its reputed impermanence (which I'm sure you will gainsay) has put me off. I'm writing this in the evening in my usual Monday night curatorial/correspondence binge, and hence I can't lay my hands on the supplies right now. But tommorrow I will ask our lab tech to package up some of the mountant and send it down to you. I will also ask him to reveal the old family recipe.

I'm looking forward to hearing your thoughts on our form genus item. Did it transmit OK from Judy's machine. If not, I can try from here.

All the best to you and Betty Rob

****

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To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: ref. index
Cc:
Bcc:
X-Attachments:

Dear Rob:

Now it comes out. It has been so many decades since I did RI measurements that I remember little about it. I'm pretty sure, however, that one should start with the raw material, not a sandwich of glass, the film one wants to measure, and another mounting material, in other words a three-layered sandwich. I no longer have the piece of equipment needed either, but I could borrow that. If you can send me a small vial of the stuff, I'll have a go at it.

However, I was also hoping to get from you your comments on Clearcol, etc. Eventually I would also like to know the exact nature of your "homemade" mountant. The recipe, that is. I need to get that stuff into the new ed. of Paleopalynology.

Best. Al.

From: fensome@agc.bio.ns.ca
Date: Tue, 5 Nov 96 14:41:43 GMT (Original AST)
To: traverse@ems.psu.edu
Subject: re: mountants, etc.
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al

Interesting! I'm on the run right now - I leave tommorrow for Virginia to spend a few days discussing nomenclature with Judy, among other things trying to come up with a note/proposal on form taxa. I'll keep you posted. It occurs to me that since you have some of our slides, you could determine the RI sooner than I could get to it. If not, I'll try to address your questions next week. I look forward to hearing about some of your and Sarah's results.

Cheers Rob

****

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From: fensome@agc.bio.ns.ca
Date: Fri, 20 Sep 96 19:11:29 -0300
To: jjansonius@gsc.nrcan.gc.ca, traverse@ems.psu.edu
Subject: ...no subject...
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al and Jan (Jan, please note the last paragraph)

I hope you had a good trip Al. Now that the illegitimate/not legitimate terminology is sorted out I have no problems understanding your meaning and I sympathise with your opinion re. validity and legitimacy. The more jargon there is, the more confusion is likely to be caused.

No of course there is no obligation to use the "ex" device - my point was that if all invalid names were illegitimate sensu stricto (as your words were saying, Al), then no one would have cause to invoke an ex. I erred in using the word "need" instead of "cause". (Though "need" can be argued for in a communication sense - which, after all, is the name of the game, isn't it?)

Jan, I will be in Calgary from Saturday until Wednesday (28th Sept to 2nd Oct). I'll be there with a team of people from GSC Atlantic doing a PR tour of oil companies and we have a fairly punishing schedule. However I do seem to be off the hook on Monday morning and plan to visit ISPG - I'll be sending a parallel message to James. Maybe that's the best chance to get together - over nothing specific but I'm sure we can discuss nomenclature if nothing else.

All the best Rob

****

Reply to: fensome@agc.bio.ns.ca

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To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: legitimacy
Cc: jjansonius@gsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Rob:

Let's start with the last sentence of your message of earlier today.
Indeed, there is no "need" for the "ex device," as Art. 46 makes very clear. See the examples, e. g., ex. 13: ....may be cited as...Nutt. ex Seem. or...Seem. The use of the ex device is absolutely not a rule, every mention of it being qualified with the permissive "may." Use it if you like, or just don't mention the Author who did not validly publish the name.

However, you are right that in my previous message I should have used the expression "not legitimate" instead of "illegitimate," to avoid confusion, as linguistically illogical as this seems. I accept that as an editorial correction, but the rest of what I said is still my opinion. The Code would be better off to stick to legitimate and not legitimate and eliminate the category of valid publication as if it were something different. It is simply a subset of legitimate, as I previously said.

Further comment. From the point of view of the Code, a name that has not been validly published has no standing. Thus, a "homonym" that was not validly published is not a homonym. To put it in philosophical terms, valid publication is a necessary but not sufficient condition for legitimacy. Thus, as I understand it, all of the "illegitimate" cases you are talking about have indeed been validly published but are not legitimate for other reasons.

This sort of discussion is very good for making one think about things and check the BOOK. However, as Jan has pointed out, the present book has such large doses of editorial comment that a century of fine tuning has been seriously compromised.

Best. Al.

From: fensome@agc.bio.ns.ca
Date: Tue, 17 Sep 96 12:04:44 -0300
To: jjansonius@gsc.nrcan.gc.ca, traverse@ems.psu.edu
Subject: Legitimacy
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al

Thanks for yor message. May I respectfully submit that I think you are misreading Article 6.4 and getting "illegitimate" confused with "not legitimate". The term "illegitimate" has a very specific meaning, spelled out in Article 6.4. That Article specifies effectively that "A name which .. was illegitimate when published cannot become legitimate later ...." In your interpretation of the term, a name that is published, but not validly published, would not be legitimizable. This is not the case or we wouldn't have all the "ex" authorsips that we do; indeed there wouldn't be any need for the "ex" device.

Cheers Rob

****

Reply to: fensome@agc.bio.ns.ca

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To: fensome@agc.bio.ns.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: valid publication etc.
Cc: jjansonius@gsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Rob:

I don't see why you are confused, but here is another go at it. "Valid publication" in ICBN is a subset of legitimacy. It's like "all reeds are weeds but not all weeds are reeds."

For a name to be a correct name (Principle IV) it must be fully legitimate, in accordance with ALL of the rules that apply (Art. 6.3).

One relatively small subset of this blanket requirement for correctness is valid publication, which is agreement with Arts. 32-45, as you say. A subset of this subset is, for example, effective publication.

Thus a name can be effectively published but not validly published. OBVIOUSLY, such a name is illegitimate.

A name can be validly published, which means, among other things, that it was effectively published, and yet still be illegitimate and thus not a correct name for anything, because it is not in accord with some rule that has nothing to do with valid publication--for example priority (Art. 11).

Now, in my previous message to Jan about this subject, I think that this is hard to understand, apparently, and I see no purpose in it really. It would be more useful to have the concepts of legitimate and illegitimate, period, and give up valid publication as such altogether. Then, legitimacy would simply subsume all the articles that are required for "valid publication"--which it in fact now does. I see no advantage in separately labelling Arts. 32-45 as required for "valid publication." For example, Art. 32 could begin: "A name of a taxon...must ....." without introducing "valid publication at all." How does it help understanding to have a section of the rules required for legitimacy labelled "....valid publication...."?

Do you get it now?

Best. Al.

From: fensome@agc.bio.ns.ca
Date: Mon, 16 Sep 96 12:25:43 -0300
To: jjansonius@gsc.nrcan.gc.ca, traverse@ems.psu.edu
Subject: Various nomenclatural meanderings
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al and Jan

In reference to Jan's note of 20th August related to identification of the type, I'll mention for Al's benefit some of the points that Jan and I discussed over the phone. The experience that Jan and Ram had over holotypes that are designated by a specimen number only and not related by the author to any specific illustration is familiar to me. It exactly parallels many (dozens) of situations that we came across when compiling the "acritarch index". In such cases, and where the author illustrated more than one specimen, we considered that the name was not validly published since, effectively (we argued), no holotype had been designated. I would now soften this stand and argue that technically a holotype has been designated and an illustration provided - hence the name is validly published in strict accordance with the Code even if not in the spirit of the Code. The Code definitely needs tidying up in this area.

In your response to this topic, Al, dated 12th September, I'm very confused about your concepts of validity and legitimacy. As I understand it, a validly published name is one that is accordance with articles (32-45) dealing with valid publication - i.e. a description is required, a holotype must be designated, it must be effectively published, etc.

A legitimate name is one that is fully in accordance with the rules (Article 6.3) - i.e it must be validly published and, essentially, not a junior homonym or a nomenclatural junior synonym. If a name is not validly published, it is simply not (yet) legitimate; to be deemed illegitimate a name must be validly published and be a junior homonym or nomenclatural junior synonym (a few other rare situations may also apply) (Article 6.4).

Once illegitimate, a name is disqualified (save for conservation), whereas if a name is simply not legitimate, it can be legitimized. The 4th paragraph of your note of the 12th, Al, makes no sense to me in my above understanding of validity and legitimacy.

Jan, you know that I am largely sympathetic with your problems with the present nomenclatural establishment - there seems to be an awful lot of "work" for the sake of it (or perhaps for the sake of vanity) and this has resulted in a lot of "unecessary meddling" with things that used to work quite well thank you. However, there are some areas of the Code that are

very poorly written and badly thought through (by theorists I suspect rather than those of us to whom the Code is a working tool). These need some good cold common sense poured on them. (I realise that I am perhaps sounding a bit self-serving, but not too blatently I hope.)

To the point! I feel that the changes regarding the typification of genera are good and remove a lot of confusion. The type of a genus is now a specimen (effectively I would argue that in the best interpretations it always was) and thus is not subject to the vagaries of taxonomic synonymies at species rank. One just has to look at the confusion that Lentin and Williams got into over the type of Korystocysta to illustrate the problems of the "bad old days": the type (species) of the genus would change depending on which synonymy was in vogue at the time.

Having said this, the use of the term "type species" unofficially, perhaps in quotes, is a good shorthand way of discussing the "species to which the type of the genus is assigned". I think that you could do this in a repetetive catalog-style publication as long as you do identify the actual type of the genus and you somewhere clearly discuss the way in which you are using the term.

This is enough for now I think. Incidentally, Judy Skog and I are getting together in early November to try to draft some reccommendations to clarify some of these issues. I will keep you both informed of progress and I'm sure you will both endeavour in turn to keep Judy and I honest and avoid over-meddling. If it works we won't try to fix it -- but if it doesn't .....

Either e-mail address works for me Al. Where did you find the new one?

All the best to you both. Rob

****

Reply to: fensome@agc.bio.ns.ca

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From: fensome@agc.bio.ns.ca
Date: Fri, 26 Jul 96 8:52:47 -0300
To: jjansonius@gsc.nrcan.gc.ca, traverse@ems.psu.edu
Subject: illegitimate name?
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Jan and Al

I arrived back from vacation a couple of days ago and found your e-mails awaiting me. I feel a bit like the net at Wimbledon, although in this case the net is going to jump up and grab the ball.

The case of Lacrimasporonites is quite similar to the situation surrounding the generic name Eupoikilofusa. (Jan, you may remember that Judy and I had a panel on Eupoikilofusa in our IPC poster.) Cramer (1970) proposed the new generic name Eupoikilofusa, in typical Cramerian obfuscating fashion, he listed the older name Dactylofusa as a synonym - clearly and not specifically excluding the type of Dactylofusa. Hence, in the acritach index (Fensome et al., 1990), we invoked Article 63.1 (now 52.1) and considered Eupoikilofusa to be illegitimate.

Since 1990 I guess that I have become more conservative (definately with a small "c", certainly not with a big one), and it is becoming fashionable (in some ways) not to change things adversely for purely nomenclatural reasons if it can be at all helped - note for example the apparently more liberal attitute towards conservation of names.

Given the above and the fact that most acritarch workers were merrily continuing to use the name Eupoikilofusa, I have recently taken another look at the Eupoikilofusa problem and noted that Article 52.2 (1994 Code) states that "For the purpose of Art. 52.1, definite inclusion of the type of a name is effected ... (d) by citation of the name itself, unless the type is at the same time excluded either explicitly or BY IMPLICATION. In a paper in preparation with Merrell Miller trying to sort out the situation, I plan to argue that, since Cramer elsewhere in his 1970 paper clearly used the genus Dactylofusa, this is implying that he did not really consider the two genera to be synonyms.

It occurs to me that you can argue that, since Ediger and Alisan actually created a new species, L. traversii, they were implying that they did not consider the two species to be synonyms, even if their actual wording is misleading and misguided. In this way, we avoid appearing to throw our nomenclatural heavy weight around.

What do you both think?

Cheers Rob

****

Reply to: fensome@agc.bio.ns.ca

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Received: from agcban1.bio.ns.ca (agcban1.BIO.ns.ca [142.2.43.1]) by pangaea.ems
Received: by agcban1.bio.ns.ca; Thu, 2 May 96 13:28:36 -0300
From: fensome@agc.bio.ns.ca
Date: Wed, 1 May 96 16:29:59 -0300
Message-ID: <EPX7+K3wVla@agcban1.bio.ns.ca>
To: traverse@ems.psu.edu
Subject: Loan extension
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO
Content-Type: text

Hi Al

This is just to extend the slide loan for one more year, with an option of further extensions if required by you and as long as the slides are not needed here. It is unlikely that they will be needed, but I have to say that; I'm just happy that you and Sarah are working on the material.

Hence, the loan to you of the following Geological Survey of Canada slides is extended to 1st May, 1997:

P19276-02, P19276-05 (both from Melvin Beach)
P31683-01 (from Five Islands)
P31686-01 (from Carrs Brook)
P31687-01, P31687-07, P31689-01, P31689-07, P31691-01 (all from Martin Head - the locality, not the person)
P31692-01, P31692-07, P31693-01 (all Fownes Head)
P32736-01 (Waterside)

Would you check please to make sure that the slide numbers correspond with the ones that you actually have. Also I'd appreciate it if you would acknowledge this message.

Hope everything is OK. Best wishes also to Betty.

Cheers Rob

****

Reply to: fensome@agc.bio.ns.ca

Rob Fensome
GSC Atlantic
Geological Survey of Canada
P.O. Box 1006
Dartmouth, NS B2Y 4A2
Canada
Tel. 902-426-2732
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Date: Thu, 2 May 1996 13:05:38 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: fensome@agc.bio.ns.ca
Cc:
BCc:
Subject: slides
X-NUPop-Charset: IBM 8-Bit

Dear Rob:

How nice to hear, and the extension is most welcome. I checked the slides with the list and agreement is 100%. By the way, do you use a double mounting technique--I mean, were they so made? If so, what mountants, primary and secondary. If not, what mountant?

At the moment Betty and I are in the final stages of getting ready to leave for the rest of May to Korea and Japan. I'm giving lectures, but mostly it's a pleasure visit.

All the best. Al.

Date: Tue, 27 Feb 1996 13:06:46 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: fensome@agc.bio.ns.ca
Cc:
BCc:
Subject: Fundy Basin
X-NUPop-Charset: IBM 8-Bit

Dear Rob:

I'm delighted that you will let me hang onto those slides for a while longer. I don't preclude, and I'm sure Sarah doesn't, the possibility that there might be an opportunity for you to be co-author with us of one of what I imagine will be a group of papers about the Fundy stuff.

Good suggestion--I hope to visit you one day to get help with the 2nd edition of Paleopalynology--Bill Evitt did that for me for the dino stuff for the first edition. You could give me pointers and perhaps some pictures, etc., for acritarchs as well. Exactly when this visit will occur I am not really sure, but I'll give you buckets of warning.

If I were in your shoes I'd be going to Houston too, but I don't have to, and I'll save my travel dollars to visit Halifax (among other places).

Thanks again for arranging extension of the loan.

Have fun. Al.

Received: from agcban1.bio.ns.ca (agcban1.BIO.ns.ca [142.2.43.1]) by pangaea.ems
Received: by agcban1.bio.ns.ca; Mon, 26 Feb 96 18:10:02 -0400
From: fensome@agc.bio.ns.ca
Date: Mon, 26 Feb 96 17:54:10 GMT (Original AST)
Message-ID: <EPX7+nyWAla@agcban1.bio.ns.ca>
To: traverse@ems.psu.edu
Subject: Fundy basin, etc
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO
Content-Type: text

Hi Al

Thanks for your message and the green light for the paper. I'm glad to hear that you will be doing some more work on the Fundy Basin. Why don't you and Sarah incorporate into the study the material that you have on loan from us, if useful. (We should formally extend the loan of the slides so as to keep track of them; I'll check into that tommorrow.) I certainly don't need to be involved and I'm more than happy to see the information published. Since I'm supposed to be the GSC Atlantic's long term resident expert on all things palynological in the region, I would very much like to look at the material with you sometime as a learning exercise. Perhaps that can be incorporated into an exchange of ideas and information when you visit.

I'm sorry that I won't see you in Houston. I'm not a "meeting" animal either, but I haven't been to one for a while, so I feel that I should wave the flag.

Best wishes to Betty and yourself
Rob

****

Reply to: fensome@agc.bio.ns.ca

Rob Fensome
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Date: Fri, 23 Feb 1996 13:04:11 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: fensome@agc.bio.ns.ca
Cc:
BCc:
Subject: paper
X-NUPop-Charset: IBM 8-Bit

Dear Rob:

Your fax of 21 Feb. was delivered to me this morning. Thanks very much for letting me see this. Seems o. k. to me.

Sarah Fowell revisited the N. B. localities last summer with Paul Olsen. They collected a bunch more samples, which they split with me. Sarah and I will be doing another paper one of these days, based on my long ago, never published research, plus study of these new samples. Will keep you informed. Sarah is with another former student of mine, Nan Arens, in California, at the moment. She'll be back at Lamont Doherty in a month or so. A student assistant is processing the new N. B. samples. Another look at Nadon's locality at Melvin Beach would be in order.

I do not plan to attend the Houston meeting, for a variety of reasons. For one thing we will just be back from a trip to Japan and Korea. For another I don't like meetings much and need a "carrot." Houston in June is not my idea of a carrot. However, I STILL plan to visit you in N. S. one of these days. That kind of a "meeting" I like.

Best regards. Al.

MARINE RESOURCES GEOSCIENCE SUBDIVISION

GEOLOGICAL SURVEY OF CANADA (ATLANTIC)

P.O. BOX 1006
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• 1-814-863-7823

DATE: 21- FEBRUARY, 1996 PAGES TO FOLLOW: 5

TO: DR. AL. TRAVERSE AT: DEPT OF GEOSCI., PENN. STATE

FROM: ROB FENSOME PHONE: (902) 426-2732

Original will be sent to you: YES (Courier)___ Yes (Mail)___ (NO ✓) OTHER___

MESSAGE:

Dear Al,

Long time no hear. I hope all is well. The Fundy Basin manuscript has been revised and is now ready for final submission. "Attached" are the pages with palynological information. Please check and make sure everything reads as it should. I've been through the revised ms, which is substantially shorter than originally, with a fine tooth comb. I think it is much improved.

Hope to see you in Houston? All the best to Betty and yourself.

Cheers

Rob

Klein (1962, 1963b) interpreted the Quaco Formation as a probable alluvial fan deposit sourced from the west or southwest. From paleocurrent and other sedimentological data, Nadon (1981) interpreted the Quaco Formation as a braided river deposit formed from a large, stable, axial river flowing toward the north or northeast along the northern side of the basin. The interpretation of river position fits well with the model for the evolution of the Mesozoic rift basins, where initial deposition was dominated by linear fluvial systems adjacent to the border fault. However, the concept of a northerly flow direction is more difficult to reconcile with the regional setting. It is generally agreed that the formations above and below the Quaco Formation represent alluvial fan and fluvial sediments sourced from the adjacent highlands to the north and west, and that these probably interfinger with sheet flood/playa facies to the south and southeast. Therefore, it is difficult to invoke an intervening period when the river flow was reversed and a major roundstone conglomerate was deposited in a generally conformable relationship with adjacent formations sourced from the northwest. Also, considering the broad regional setting, neither downstream (northward) facies or upstream (southerly) source area can be satisfactorily ascribed to such a system. However, it may be possible that, because of the few accessible exposures, Nadon's paleocurrent data was taken from a northerly flowing braided segment of a generally easterly flowing river.

Our field observations of the Quaco Formation and its relationship with adjacent units suggest probable rapid deposition of an already available cobble/boulder gravel supply brought about by unusual geological circumstances. These could include a sudden change in the elevation of the graben floor, resulting in the relocation or capture of rivers and rapid erosion and redeposition of an existing gravel or conglomerate unit. The Quaco Formation is considered too local, too unique and too uniform to have resulted from normal braided river processes acting over a long period of geologic time.

Echo Cove Formation

The Echo Cove Formation, which conformably and gradationally overlies the Quaco Formation (Figure 7), is made up of medium to very coarse-grained, red to red-grey sandstone and red to green shale, with numerous fanglomeratic to conglomeratic beds particularly in the upper part of the formation. The formation is more than 1 km thick. Paleocurrent data from the members of the Echo Cove Formation indicate an orderly shift in source direction from northwest to west (Nadon, 1981; Nadon and Middleton, 1985) (Figure 17).

Nadon and Middleton (1985), reporting on palynomorph assemblages from near the top of the Echo Cove Formation at Melvin Beach, indicated that the miospore assemblage is different from any in the eastern United States rift basins and, therefore, may signify a Ladinian (Mid Triassic) age. However, palynofloras recovered in the present study from the upper part of the Echo Cove Formation at

Fownes Beach are dominated by several species from the *Ovalipollis* complex, relatively uncommon members of the *Patinasporites* complex and a variety of bisaccate pollen, including *Alisporites* spp. and *Triadispora* sp.. *Corollina* is absent. This assemblage is clearly Carnian and probably Late Carnian (see also Traverse, 1983). Inasmuch as the Fownes Beach fossil occurrences are at least 2 km above the base of the Triassic sequence, they cannot be considered as indicative of its maximum age and allow speculation of earlier Middle Triassic rocks forming the base of the succession.

At Martin Head, 30 km northeast of St. Martins, about 100 m of cross-bedded yellow and pale red, fluvial and eolian Triassic sandstone beds with intercalated grey shales and red-brown shales are in fault contact with Carboniferous metasediments. Two shale horizons at Martin Head have yielded well-preserved palynofloras. One of these floras is dominated by *Patinasporites* complex forms, especially *Patinasporites densus*. Also present are *Cycadopites* and many bisaccate pollen species, including *Brachysaccus* sp. This is a Carnian assemblage, although not latest Carnian owing to the abundance of *Patinasporites*. The other Martin Head palynoflora contains abundant *Camerosporites*, as well as *Cycadopites*, *Brodispora*, *Ovalipollis*, *Samaropollenites* and other bisaccates. The abundance of *Camerosporites* and the presence of *Brodispora* make late Carnian a likely age assignment and hence correlation with the Echo Cove Formation.

Waterside section

Near Waterside, on the northwest side of Chignecto Bay (Figure 3), there are a series of outcrops of massive red sandstones; some red conglomerates; and beds containing abundant red shale rip-up clasts with green reduction rims. These beds also are in fault contact with Carboniferous sedimentary strata. The relatively fine-grained nature of the Waterside area clastics, as compared to the conglomeratic nature of much of the Echo Cove, Quaco and Honeycomb Point formations, likely indicates the main source area is the adjacent Carboniferous upland rather than the Precambrian.

Discussion

The palynomorph dating of the Echo Cove Formation allows correlation of both it and the underlying units to the Wolfville Formation. Although there were no fossils recovered from the Waterside section, its fluvial nature, together with projection from the seismic data from Chignecto Bay, would indicate that it correlates to the lower part of the Triassic section and so is equivalent to the Wolfville Formation.

In addition to palynology, other methods have been used to helo correlate the scattered early Mesozoic strata in the Bay of Fundy region. Symons et al. (1989) attempted to use magnetostratigraphy to correlate and date outcrop samples. They suggested that the Lepreau and Honeycomb Point formations equate to the lower

laterally continuous and gradational fluvial/playa/lacustrine facies throughout Fundy Basin at this time. Such facies are thought to be represented by intervals with uniform and continuous seismic reflections, within parts of the uppermost Wolfville and Blomidon sections in the Fundy Subbasin east of Grand Manan Island, and also by sequences overlying the high amplitude reflections of the Wolfville Formation in Chignecto Subbasin. Similar continuity in playa-lacustrine strata are documented in the Newark Supergroup in other basins (Olsen, 1988b, 1990; Olsen et al., 1989). As discussed above, this continuity is believed indicative of hydrological closing of drainage outflow points; a decreasing volume of sediments reaching the basin; and most importantly, fluctuating climatic conditions from humid to more arid (Schlische and Olsen, 1990; Olsen, 1990).

Powers (1916) reported purple shales beneath the North Mountain Basalt just north of Dark Harbour on the west side of Grand Manan Island. Also, Alcock (1948) reported reddish brown sandstone and conglomerate beneath the basalt in a small area near the north-south trending fault contact with early Paleozoic metasediments on the eastern side of the island. Since neither of these reports appear to have been verified by subsequent studies they were field checked for this study as possible equivalents of the Blomidon Formation.

The purple shales reported by Powers (1916) are in fact silicified argillaceous siltstones (Wade and Jansa, 1994). Viewed in the microscope, the Dark Harbour samples are light dusky red to light grey, fine-to very fine-grained, well sorted argillaceous siltstone to silty mudstone. Many samples have a fine to coarse vuggy porosity caused by leaching. Vugs may be lined with 2 or 3 types of crystals, e.g. calcite and possibly gypsum and zeolite. In outcrop, water was observed seeping from several of these leached beds. The diagenetic characteristics of these rocks bear no resemblance to any of the other Triassic/Jurassic sequences associated with the Fundy Basin and are interpreted to be due to the percolation of ground waters enriched in dissolved silica. Based on their lithologic homogeneity, traces of evaporitic minerals and structural conformity to the North Mountain Basalt, they are correlated to the Blomidon Formation (Wade and Jansa, 1994).

The clastics reported by Alcock (1948) were not located. However a nearby quarry, with recent workings below original ground level, is still in massive basalt.

The Blomidon Formation spans the Norian, Rhaetian and lowermost Hettangian stages (Mertz and Hubert, 1990). Fowell and Traverse (1995), and the present authors, found good palynomorph assemblages in grey mudstones in the uppermost metre of the Blomidon Formation at Partridge Island, on the north flank of Minas Subbasin. Fowell and Traverse noted that the palynofloras are all *Corollina* dominated, but minor constituents show that the uppermost assemblages are Hettangian whereas 30 cm lower in the section, an array of *Cycadopites* spp. and rare *Patinasporites* indicate a late Triassic (probably

Rhaetian) age. If these conclusions are correct, the Triassic/Jurassic boundary and its associated animal extinctions are circumscribed by this short section at the very top of Blomidon Formation.

Based on a Norian-Rhaetian time span of approximately 15 Ma and an estimated 3000± m of Blomidon Formation in the Fundy Subbasin, a maximum depositional rate (compacted) of 20 cm/Ka can be derived. This compares favourably with rates for time and facies-equivalent strata from other Triassic rift basins (Schlische and Olsen, 1990). However, it contrasts to the 4 cm/Ka and 1.5 cm/Ka calculated by Mertz and Hubert (1990) and Schlische and Olsen (1990) for the Blomidon Formation in outcrop due to their very low thickness estimates.

In Fundy Basin, shallow water playa facies dominated during the latest Triassic and had impressive lateral continuity (Figure 20). It is thought that, given the distribution and thickness of the Blomidon sequence, this playa facies was initially restricted to the basin margins although, by the earliest Jurassic, it probably extended throughout the basin. Evidence for evaporitic beds is present in outcrop at Rossway, Blomidon and Red Head, although their original (and current) volumetric contribution to the Blomidon strata is unknown. Classical deep water, organic-rich lacustrine shales, equivalent to the lower Blomidon, have yet to be encountered, but are interpreted from facies trends to have been modest volumetrically, and restricted to the Fundy and Chignecto depocentres.

NORTH MOUNTAIN BASALT

The North Mountain Basalt is a succession of subaerial, high-titanium, quartz-normative tholeiite flows (Stevens, 1980; Puffer, 1992) that outcrop extensively in Nova Scotia and on Grand Manan Island. It forms North Mountain which bounds the Annapolis and Cornwallis Valleys and also occurs in a series of fault blocks along the north side of Minas Basin and on Isle Haute (Figures 3, 15 and 21).

In the Digby Neck area of Nova Scotia, thick, coarse-grained, lower and upper basalt flows are separated by 7 or more thin amygdaloidal flows which are rich in zeolites (Papezik et al., 1988). The composite thickness in this area is about 400 m. To the northeast, in the Scots Bay area, up to 16 flows with a composite thickness of about 275 m are reported (Klein, 1962; Colwell, 1980). The formation was penetrated by the Chinampas and Cape Spencer wells where it is 333 m and 187 m thick respectively and by a core hole west of Morden, Nova Scotia, where it is 210 m (Colwell, 1980). The basalt covers the western two-thirds of Grand Manan Island and is well exposed on the northern and southern extremities of the island in near vertical cliffs up to 100 m high. In exposures throughout the Bay of Fundy region, columnar jointing is common.

The chemistry of the basalt is very similar to others in the eastern North America flood basalt province and indicates that it was formed during the initial period of

were originally interpreted as rollover anticlines (Brown, 1986), but our subsequent analysis indicates they cannot be formed in that manner. This is supported by a recently published analysis by Withjack et al. (1995). Neither the amount of structural inversion or the amount of sediment removed by erosion is known but both may be significant. The thickening of beds away from the anticlines and toward the Headlands Fault System indicates syndepositional movement and probable local erosion and redeposition of early Scots Bay sediments (Figure 22). At least 2 km of the formation has been eroded at the junction of the Cobequid and Headlands faults. Finally, Tanner's (1990) study of the diagenetic history of the formation in the McCoy Brook area, indicates that up to 2 km of erosion has occurred in that region; and Olsen (1993) notes that thermal history indicators for the U.S. rift basins suggests that at least 2000 m of section has been removed from these basins as well.

The age range for the Scots Bay Formation is uncertain. Based on macrofossil content, the formation is Early Jurassic (Olsen, 1981, 1988a). Palynofloras above the basalt, at about 1100 feet, in the Chinampas well indicate an Early Jurassic, probably Hettangian age (J.P. Bujak, pers comm., 1979). The variety of paleontological and radiometric analyses cited previously indicate that the underlying North Mountain Basalt is earliest Hettangian in age. Therefore, Scots Bay deposition can be dated as early Hettangian. The appearance of Late Triassic palynomorphs reported from near the base of the formation in the Chinampas well (J.P. Bujak, pers comm., 1979) suggests probable erosion and reworking of Blomidon Formation equivalents along the northern margin of the basin.

Traverse (1987) reported strata as young as Pliensbachian in cuttings samples from the Chinampas well. The Liassic age depends on very abundant *Corollina* of several species, some of which do not occur below Pliensbachian levels in the Hartford Basin of Connecticut. Olsen (1988a) indicated Pliensbachian-Toarcian strata in the Economy/Parrsboro area. However, with up to 2000 m of the formation missing due to erosion, there is no indication of the minimum age of the Scots Bay Formation or whether younger Jurassic or even Cretaceous units once occurred in the Fundy Subbasin or in overthickened areas adjacent to the Headlands Fault System (Figure 24). Early Cretaceous sediments are known from a number of locations in Nova Scotia (Wade and MacLean, 1990, figure 5.42).

One method of estimating the minimum age of the Scots Bay is through depositional rates. The basin filling models described above and illustrated in Figures 20 and 23 would indicate that, following the reduction of stream gradient and the establishment of lacustrine facies, accumulation rates were generally lowered. The ~2500 m of Scots Bay section remaining in Fundy Basin ranges in age from early Hettangian to at least Pliensbachian, a time span of about 17 ma and a depositional rate of about 15 cm/Ka. This is less than the 19-20 cm/Ka rate calculated for the Wolfville and Blomidon formations. Applying the 15 cm/Ka rate

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28 July, 1995

Dr. R. A. Fensome
Geological Survey of Canada
Atlantic Geoscience Centre
Box 1006
Dartmouth, Nova Scotia B2Y 4A2
Canada

Dear Rob:

Here are the pages of the massive Fundy MS on which I've scribbled. In lieu of contrary info I'm assuming that it's ok to save on shipping and send only the pages I've written on.

Additional comments:

1. Lots of folks only read the abstract of such things. I'm a little unhappy that the palynostratigraphy isn't worth a few words.

2. As noted on p. 28 and elsewhere, I think it's quite clear that the Eagle Mills as far west as Texas is also a rift basin deposit, albeit resulting from S. Amer. contacts instead of African. And it seems a natural extension of the Newark basins in Florida

3. I wonder where the Norian suggestion on p. 35 is coming from? I believe that late Ladinian is pretty likely for some of the NB stuff, but Norian is unlikely from what I've seen so far.

4. I think that the reader is left with the almost certainly false opinion that hydrocarbon potential in the Fundy is good. The Richmond Basin looked even better on paper, and Bruce Cornet wasted millions (of other other people's money) drilling dry holes. I've held all the oil he ever found in my hand (in a quart jar).

5. Fowell and Traverse, 1995, is now out and should be fixed up in the biblio. If you all agree that referring readers to a place where they can inspect the TAI business in understandable graphic presentation, you may wish to ref. Traverse 1988.

All the best.

Alfred Traverse

Received: from agcban1.bio.ns.ca by pangaea.ems.psu.edu
        (4.1/PSU_ESSC/GEOSC-2.02) id AA11139; Fri, 23 Jun 95 10:30:43 EDT
Received: by agcban1.bio.ns.ca; Fri, 23 Jun 95 11:30:39 -0300
Date: Thu, 22 Jun 95 16:31:34 -0300
Message-Id: <EPX7+jSQuja@agcban1.bio.ns.ca>
From: fensome@agcux.bio.ns.ca
To: traverse@ems.psu.edu
Subject: re. assorted subjects
X-Incognito-Sn: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al

Thanks for your message. With regard to the ms, please feel free to decide to what depth you would like to go through it. I will go through it all in detail, but if you wish to skim the non stratigraphic parts and just critique the latter, that would be fine.

OK, please hang on to the Fundy slides for now. I'll have my technician update the loan slip. At some point I would like to have another look at them myself, but I can't see that happening next summer.

I would be happy to critique the dino chapter in your book and if that involves a visit from you, so much the better. Give me plenty of lead time though please Al, so that I can comfortably work it into the schedule.

All the best from the sunny Summit city

Rob

****

Reply to: fensome@agc.bio.ns.ca

Rob Fensome
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P.O. Box 1006
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Tel. 902-426-2732
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Date: Mon, 19 Jun 1995 16:02:35 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: fensome@agcux.bio.ns.ca
Cc:
BCc:
Subject: assorted subjects
X-NUPop-Charset: IBM 8-Bit

Dear Rob:

Thanks for yours of 5 June. Re photos for Hunt archives. It is ok to send directly there, but it might be a tad better to me, as I could explain to the archivist when I make my (more or less) annual run to Pittsburgh with such stuff. Any format is ok, and they encourage sending pictures even if a person says he/she did it before--for old guys like me they have sometimes a range of photos from the 20s to the 60s and beyond.

The huge MS from you all arrived a couple of days ago. I refer, of course, to Wade et al. I know you've told me about this before, but could you now tell me exactly what I am supposed to do? Full scale review, or what? Send to whom--you? All pages back or just those with changes? Etc.

Re the slides from you/GSC: they're in the box they came in, sitting with my NB Triassic slides, hoping for the decks to clear one day so that I can get back to work on them! I remain very interested in the whole Fundy Bay Triassic question. So, yes, I'd like to retain the slides for "a while." The department is letting me keep all of my facilities until at least July, 1998. My faculties, on the other hand, nobody on this planet can guarantee.

Glad to hear that you survived cutbacks.

Warning: I am doing a second edition of "Paleopalynology." Bill Evitt helped me by critiquing the dino part of the first edition. Will you do the same for me with the second edition? Might involve some new illustrations, etc. I'll do all leg work, of course. Might have to fly up to Halifax at some point!

All the best. Al

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Date: Mon, 5 Jun 95 19:07:36 -0300
Message-Id: <EPX7+NNsoja@agcban1.bio.ns.ca>
From: fensome@agcux.bio.ns.ca
To: traverse@ems.psu.edu
Subject: Archives, etc
X-Incognito-Sn: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al

Thanks for your note. Please continue your good work as archivist for IAPT.CFP. I for one did not submit a photograph to the Hunt. I will ask Judy to request photos of new committee members in her next mailing (or sooner if the next scheduled mailing is not for a while). Any special format (if possible) or will any reasonable representation on film do?

I have sent you a final draft of the Fundy manuscript. John said he would like feedback within a month if you can manage that. Let me know if not and I will see if I can negotiate more time. Incidentally Al, can you check that you still have some slides of ours? According to the transmittal slip, you should have 13 slides. If you would like to retain them for a while, thats fine (and I am still open to the idea of joint work on Fundy material). I'm just trying to keep track of them.

Hope all is well with Betty and yourself. All the palynologists at GSC Atlantic (formerly AGC) survived the recent round of cutbacks, so we must be doing something right.

Best Wishes

Rob

****

Reply to: fensome@agc.bio.ns.ca

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P.O. Box 1006
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Date: Wed, 31 May 1995 14:58:11 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: fensome@agc.bio.ns.ca
Cc:
BCc:
Subject: archives
X-NUPop-Charset: IBM 8-Bit

Dear Rob:

You may or may not recall that I have been unofficially the archivist for the IAPT.CFP. I got the committee and IAPT to go along with the idea of housing our archives at the Hunt Library in Pittsburgh, where IAPT, AASP, IFPS, etc. have their archives. I make a run down there about once every year or so with a carload of stuff. Will do so again on 15 June. Have one large box of stuff from various stages in the Committee's life. I'm telling you about this to be sure it continues to have "official blessing," and to give you and Judy a chance to respond. All the best. Al T. P. S. Did you and Judy and new members of the Committee ever send the Hunt photos of yourselves? They urgently request same. They have archives on practically every botanical organization in the world. I wonder if they have CAP?

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Received: by agcban1.bio.ns.ca; Wed, 22 Feb 95 12:43:30 -0400
Date: Wed, 22 Feb 95 12:37:23 GMT (Original AST)
Message-Id: <EPX7+jaqGja@agcban1.bio.ns.ca>
From: fensome@agcux.bio.ns.ca
To: traverse@ems.psu.edu
Subject: re: test
X-Incognito-Sn: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al

Got your message. I must have the address in the AASP directory changed! I just spoke with Jan. He said that you can just cite "chapter 6" or put "Fensome et al., chapter 6". I'm not sure - and forgot to ask - whether "chapter" should be rendered as "chapter", "Chapter" or just "Ch.". Perhaps you have other examples in your manuscript that you can check. I hope this helps. Regards to Betty.

Cheers

Rob

****

Reply to: fensome@agc.bio.ns.ca

Rob Fensome
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Tel. 902-426-2732
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Date: Mon, 20 Feb 1995 19:48:16 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: rob=fensome@agcban1.bio.ns.ca
Cc:
BCc:
Subject: chapter for book
X-NUPop-Charset: IBM 8-Bit

Dear Rob: Finally, a little block of time to work on the final version of the nomenclatural chapter with which you have been very helpful. I am changing all refs. to the Code to the 1994 version, etc. There were a couple of letters from Jansonius with suggestions, in part apparently stemming from a letter or letters from you to him, of which I am not sure I got copies. If I did, we can't find them. Could you phone me sometime Tues. afternoon to give me input? (One of the odd facts of electronic life is that e-mail is free, and I have to pay personally for phone calls!) One problem is literature references to your work. I want to be sure they're correct. For example, there is a parenthetical reference to "Fensome & Riding, Ch. 6." Acc. to Jan, apparently from you, it is supposed to be Fensome et al., Ch. 6. I don't remember putting that in in the first place. In any event, what Fensome et al. is it? 1993 doesn't seem to have chapters. Besides, none of the authors is a Riding. Hmmm. Thanks. Al.

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21 December, 1994

Dr. R. A. Fensome
Geological Survey of Canada
Atlantic Geoscience Centre
Box 1006
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Canada

Dear Rob:

Hi there! Happy holidays and have a great 1995, a year when I swear I'm going to get up your way.

Purpose in writing at the moment is to inquire how to obtain the publication listed below, and for information about Vol. 1 also. I have all of what I suppose is the "Old Series," but perhaps a complete listing is available somewhere.

We're very busy, as usual. The palynology course had 16 in it, and they were a marvellous class. The term papers included at least two that are publishable contributions.

Betty is now within week or two of finishing her dissertation. We are flying off to CA on 28 Dec., to attend the Rose Bowl and attendant festivities. I suppose you never heard of it?

My records indicate that I owe you some reprints. They are enclosed.

All the best.

Yours very truly,

Alfred Traverse

attachment: pub. announcement
encl.:reprints

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10 May, 1994

Dr. R. A. Fensome
Geological Survey of Canada
Atlantic Geoscience Centre
Box 1006
Dartmouth, Nova Scotia, Canada B2Y 4A2

Dear Rob:

Good to talk with you this a. m. I do hope we will have a joint project on the Triassic of Fundy Basin. I just finished trying to reach Sid Ash. He's out of the office, but his Navajo mother-in-law managed enough English to get across the idea that they are not out of town, so I'll probably hear back soon.

Re the manuscript, here's what I suggest:

p. 11, para. 4.: ....palynoflorules recovered from the upper part of the Echo Cove Formation at Fownes Head are dominated by several Ovalipollis-complex species, relatively infrequent members of the "Patinasporites complex", and a variety of bisaccate pollen, including Alisporites spp. and Triadispora sp. Corollina is totally absent. This sort of palynoflora is clearly Carnian, and late Carnian is probable, but a more exact stratigraphic fix in terms of internationally recognized ranges awaits more work with minor elements of the flora (cf. also Traverse, 1983). Nadon....

p. 12, middle of last para.: At Martin Head....two shale horizons at Martin Head have yielded well-preserved palynoflorules. One of these florules is dominated by Patinaporites densus and related members of the "Patinasporites complex." Also present are Cycadopites and many species of bisaccates, including Brachysaccus sp. This is a Carnian assemblage certainly, and the abundance of Patinasporites probably implies not the very latest Carnian. More precision will probably come from study of the minor constituents. The other Martin Head palynoflorule contains abundant Camerosporites, as well as Cycadopites, Brodispora, Ovalipollis, Samaropollenites, and a variety of other bisaccates. The abundant Camerosporites, plus Brodispora, make early Tuvalian (late Carnian) a likely age assignment. [These are my two Martin Head assemblages--I don't know if they're the same two referred

to in the MS.]

p.14, top: Fowell and Traverse (submitted for publication 1984) found good palynomorph assemblages in gray mudstones in the uppermost 1 meter of the Blomidon Fm. at Partridge Island. The palynoflorules are all Corollina-dominated, but minor constituents show that the uppermost assemblages are Hettangian, whereas 30 cm. lower in the section, an array of Cycadopites spp. and rare Patinasporites indicate late Triassic age (probably Rhaetian). If these conclusions are correct, the Triassic/Jurassic boundary associated with one of the greatest animal extinction episodes in Earth history, is included in this short section of the Blomidon Fm.

p. 19, bottom: Traverse (1987) reported strata as young as Pliensbachian in the Fundy Group. This is based on core samples from the Bay of Fundy, southwest of Point Lapreau, New Brunswick. The Liassic age depends on very abundant Corollina of several species, some of which are taxa which do not occur below Pliensbachian levels in the Hartford Basin of Connecticut. Olsen....

Literature references

Traverse, A., 1983. (Abstract) Fundy Group palynoflorules on the northern margin of the Bay of Fundy, New Brunswick, Can.: Geol. Soc. America Abstracts with Programs Vol. 15, no. 3, p. 122.

Traverse, A., 1987. Pollen and spores date origin of rift basins from Texas to Nova Scotia as early Late Triassic: Science 236:1469-1472.

Fowell, S. J., submitted 1994. Palynology and age of the upper Blomidon Formation, Fundy Basin, Nova Scotia.

++

That seems to take care of it for the moment.

All the best.

Yours very truly,

Alfred Traverse

Natural Resources Canada
Ressources Naturelles Canada

Geological Survey of Canada
Commission geologique du Canada

Rec'd about May 94

Dear Al,

Please find enclosed the latest draft of the Wade et al. article. John (Wade) tells me now that he would like to use a few fossil names, so I made a start by using Corollina. However, we could add a few more, sprinkling them in where appropriate. Rather than delay further by doing this now, I'll let you read the draft. Then we can chat. Or, if you prefer, you could make some annotations and I will call you with any questions. From my notes of the Fall trip, I see that I promised you a locality map showing Melvin Beach. Hence the other enclosure.

Hi to Betty

Best Wishes

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11 March, 1994

Dr. Rob Fensome
Geological Survey of Canada
Box 1006
Dartmouth, Nova Scotia B2Y 4A2
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Dear Rob:

Well, yours of 23 Feb. arrived 10 March. About like transatlantic before airmail.

At the moment I'm working on the Triassic project I have in Germany, but I'll get back to the Fundy before long. I have invested too much time on that not to see more results in print!

I'm not sure what you mean about obtaining a copy of the Cornet thesis. I think you mean a copy to keep. That could be difficult. Years ago Bruce was selling them--he had about 300 copies made and did a brisk trade for a while. I was talking to him just yesterday, trying to get him to lay off his UFO "research" and got nowhere. ("Al, I've seen these fellows getting out of their craft and talked to them." Damn it, I failed to ask him what language they speak.) You might write him asking him about whether he still has copies. Other than that, I would suggest getting your library to request loan of a copy from the PSU library. God knows, GSC library in Ottawa has done US lots of favors! You could then have a xerox copy made.

Bruce's address: 27 Tower Hill Ave., Red Bank, NJ 07701. Ph.908-747-9244. It might help to mention your connection with me. But, for heaven's sake don't mention the UFO business.

All the best.

Yours very truly,

Alfred Traverse

Energy, Mines and Resources Canada
Energie, Mines et Ressources Canada

Geological Survey of Canada
Commission geologique du Canada

23 February 1994

Dr. Alfred Traverse
Department of Geosciences
Pennsylvania State University
435 Deike Building
UNIVERSITY PARK, Pennsylvania 16802
U.S.A.

Dear Al:

A note to say thank you for the photograph. I too enjoyed my visit and do hope we can follow up the Triassic/Jurassic Fundy material beyond the Wade et al. paper before too long. As for the latter, I proposed some revisions related to our discussions, but John is tied up with other projects and hasn't got to it yet.

Incidentally, I am very interested in obtaining a copy of Bruce Cornet's thesis. What is the best way to go about that?

Regards to Betty.

Best wishes,
Basin Analysis Subdivision

RAF/nk

Rob Fensome

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1 February, 1994

Dr. R. A. Fensome
Geological Survey of Canada
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Box 1006
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Canada

Dear Rob:

Just a little reminder of you? visit here, which we enjoyed very much.

One of these days I still want to visit your lab, to pick your brain about techniques, for one thing, to use the computer files on some spore names, for another.

All the best.

Yours very truly,

Alfred Traverse

enclosure: photo

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28 October, 1993

Dr. R. A. Fensome
Geological Survey of Canada
Atlantic Geoscience Centre
Box 1006
Dartmouth, Nova Scotia
Canada

Dear Rob:

It's amazing what I accidentally learn from visitors! Enclosed is an article I would never have read except for our visit to Old Main and the Poor murals. PSU really did get a "land grant." They apparently frittered it away. Typical.

We are on p. 403 of the page proofs. Soon I'll be able to get to the Triassic palynomorphs again.

Once again. It was marvellous to have you here, and I hope we have a long, productive relationship.

Yours very truly,

Alfred Traverse

enclosure

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25 February, 1993

Dr. Robert A. Fensome
Atlantic Geoscience Centre
Geological Survey of Canada
Bedford Institute
Box 1006, Darmouth, Nova Scotia

Dear Rob:

Your CFP ballot material, and the promised preliminary MS just came.

What I have and am at the moment trying to work up is sections I collected in the mid-70s from Martin Head, from Fowndes Head, and from St. Martin's (Quaco Conglomerate), and the core sample preparations I got from Mobil about the same time, with permission to do whatever I liked with them. Bruce Cornet and I on several different forays failed to find productive localities on the Nova Scotia side, but Sarah Fowell and Paul Olsen succeeded just a few months ago in finding a productive zone in Nova Scotia. The palynoflora is beautifully preserved and seems to be about the same age as mine from the NB side--i. e. Carnian. However, I think that all of the Carnian samples may be from different Carnian levels. My Martin Head stuff seems to me to be younger than the Fowndes Head material, for example, and Sarah's is likewise a little different. The core contains levels that are well up into the Jurassic.

I note that although you quote me in this MS, my title isn't in the literature-cited. I believe the reference is also wrong. As far as I can recall, my only published contribution so far is from 1983 (not "1978," per the MS): Geol. Soc. America Northeastern Section, etc. I enclose a xerox copy. I don't think I ever said anything about Jurassic in the "Economy/Parrsboro area"--where is that coming from? Until Sarah's breakthrough, I knew of no claim to have found Tr/Jr palynomorphs in Nova Scotia except for Bujak's, which turned out to have stemmed from ONE Corollina grain he recalled having seen--he couldn't produce it, as far as I can recall. I am somewhat startled to discover that I never have said anything in print about the core. I can't see why I shouldn't now.

In brief, yes, if you can give me just a little more time, I would be glad to give my best guess on the age of the three sections mentioned above and in the abstract, and for palyniferous zones in the core.

I also think that with appropriate credit, Sarah wouldn't mind having her locality mentioned too. I was never convinced that Nadon and Middleton were right about pre-Karnian age of their stuff. I have no samples from what they describe as "Melvin Beach" and would really like to have a go at such! In fact, I want to keep up my interest in the Fundy Basin very much. Could J. A. Wade give me just a couple more weeks to whip up something on this for inclusion? And then, yes, if you want to have me work with you on additional projects in the FB, I'd love it.

Yours very truly,

Alfred Traverse

enclosure: xerox

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12 October, 1992

Dr. R. A. Fensome
Geological Survey of Canada
Atlantic Geoscience Centre
Box 1006
Dartmouth, Nova Scotia, Canada B2Y 4A2

Dear Rob:

In April of this year I spent two afternoons with Greuter in Berlin. He is the guy who is pushing for NCU adoption and to get at least insemination of the ovule at the 1993 Bot. Congress in Japan. He was hot under the collar that I as secretary of the (IAPT-sponsored) Committee for Fossil Plants, had not "taken action" to get "our" NCU "list" finalized. Indeed, I'd never seen it. He gave me a copy of the three-inch stack to take back to Frankfurt, where in a few hours of perusal I discovered that the list is by no means ready to be accepted. It's full of typographical and other errors, for one thing. It <u>is</u> a very useful list. Then I visited Chaloner and Boulter in London and we jawed about the whole matter. It was obvious that Boulter, under whose aegis the list was prepared, had <u>no intention</u> that the <u>list as such</u> would be an NCU list for Tokyo. Greuter was very irritated to hear this but about June was ready to accept the fact that at least the <u>fossil plant list</u> would <u>not</u> be ready for Tokyo.

That answers the first question. Regarding your second question, I can say that I know Jan J. and some others are very anti-NCU. I am not. I think the idea has many merits, but I think it should not be a hasty <u>nor</u> wholesale action. I think we should adopt rather restricted, very carefully research sub-lists <u>piecemeal</u> over at least a decade or so. Perhaps a mechanism could be set up at Tokyo for accomplishing this without working on it only at congresses. As I re-read your letter, this tends to agree with your ideas, I think (?).

I hope to meet you soon, and I hope very much you will be willing to work with the fossil plant committee.

Yours very truly,

Alfred Traverse

c: W.G. Chaloner

ATLANTIC GEOSCIENCE CENTRE

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FAX: (902) 426-4465

DATE: 8-Oct-92 **PAGES TO FOLLOW:** 2 **FAX NO:** 1-814-863-7823

TO: DR A. TRAVERSE **AT:** GEOSCIENCES, PENN STATE U.

FROM: R. A. FENSOME **PHONE:** (902) 426-2732

Original will be sent to you: ___ **YES (Courier)** ___ **YES (Mail)** ✓ **NO** ___ **OTHER**

MESSAGE:

Please see attached letter.

If you do not receive all pages please call: Nelly Koziel at (902) 426-2740

or ROB **at (902) 426-** 2732

Energy, Mines and Resources Canada
Energie, Mines et Ressources Canada

Geological Survey of Canada
Commission geologique du Canada

8 October 1992

Dr. Alfred Traverse
Department of Geosciences
435 Deike Building
Pennsylvania State University
UNIVERSITY PARK, Pennsylvania 16802
U.S.A.

Dear Al:

On a recent visit to Toronto (primarily to see Geoff Norris and his cohorts), I had the good furtune to spend an hour with John McNeil (of *ICBN* fame). John and I discussed various points of nomenclature, but our meeting was dominated by the "Names in Current Use" project. Later in the same week I progressed to Ottawa (there to visit Colin McGregor and Dave Jarzen, among others) and managed to locate "the Canadian copy" of the preliminary list of paleobotanical generic names, dated Spring 1992. I understand, from Dr. McNeil, that the "entrenching" of the principle of such lists in the *ICBN* will be proposed in Japan next year, but, at that time, probably no actual lists would be entrenched. I have made a copy of the paleobotanical list, which I can compare against our large palynological and dinoflagellate (extant and fossil) data bases. As it stands, I think our lists contain far more generic names than are in the NCU lists (even allowing exclusion of invalid, illegitimate or incorrect names).

I wonder if you could update me on the current status of the paleobotanical NCU list. Who is looking after it? Is Jan Jansonius' abhorrence of the whole idea shared by the paleobotanical subcommittee? If so, is there not going to be a final paleobotanical list? If there is, are there plans to broadly circulate a preliminary list to test its completeness?

As I've indicated above, I'm very much aware of Jan's objections to the NCU list, especially for paleobotanical names. I am to some extent in sympathy with Jan's opinion: we don't seem to be doing so badly without such a list, and its preparation would involve (has involved?) a lot of additional, arguably non-vital work. However, it strikes me that if we are forced to produce an NCU list, but the resultant list is only half complete and produced by a small coterie of workers, the results would be disastrous.

I guess this letter boils down to two questions. Could you update me on what's going on with regard to the NCU project as related to paleobotany? And what do you and other paleobotanists with nomenclatural influence feel about it?

.../2

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Canada

On other matters, I hope you had no further trouble interpreting my review of your chapter for the Jansonius-McGregor book. My own chapter (with Jim Riding) on the dinos is currently being reviewed; I'll have to be efficient if I'm to beat Jan's deadlines. Our big tome on dino evolution and classification is currently at the galley proof stage, so thats nearly out of the way. Please say hello to Carmen Moy if she is still working in your lab.

I look forward to hearing from you.

Best wishes,

Robert A. Fensome
Basin Analysis Subdivision

RAF/nk

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FAX: (902) 426-4465

DATE: 16-JULY-1992 **PAGES TO FOLLOW:** 0 **FAX NO:** 1-814-863-7823

TO: DR A. TRAVERSE **AT:** GEOSCIENCES, PENN. STATE U.

FROM: ROB FENSOME **PHONE:** (902) 426-2732

Original will be sent to you: ___ **YES (Courier)** ___ **YES (Mail)** ✓ **NO** ___ **OTHER**

MESSAGE:

2nd fl - E pattee Life Sci. ref.

Dear Al,

I'm genuinely pleased that you find my review helpful. The references that you requested are as follows:

PE1580.B7 1956

BROWN, R.W., 1954. Composition of Scientific Words. Privately published. (But nevertheless widely distributed - if you have trouble obtaining a library copy, I could make a copy of the introductory part for you.) 882p.

FARR, E.R., LEUSSINK, J.A. & STAFLEU, F.A. (eds), 1979. Index Nominum Genericorum (Plantarum). Regnum Vegetabile, volumes 100-102; 1896p (in 3 vols)

FENSOME, R.A., TAYLOR, F.J.R., NORRIS, G., SARJEANT, W.A.S., WHARTON, D.I and WILLIAMS, G.L., 1992. A classification of living and fossil dinoflagellates. Special Papers in Micropaleontology. (In press)

Canada FENSOME ET AL. (1991) = AASP C.S. 26 — Yes.

Best wishes
Rob.

If you do not receive all pages please call: Nelly Koziel at (902) 426-2740

or ROB at (902) 426-2732

PENNSTATE

The Pennsylvania State University
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503 Deike Building
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Date: 16-VII-92

☐ Marked here if urgent reply is requested.

Fax Transmission to:

Name: Dr. Robert A. Fensome
Company/Institution: Atlantic Geoscience Centre, GSC
Department: Dartmouth, Nova Scotia
Fax Number: 902-426-4465
Phone Number: 426-8513

Transmission from:

Name: A. TRAVERSE
Phone Number: 814-863-3419

Total Number of Pages (Including Cover Page): 1

Message:

Dear Rob—

Finally home and working on the nomenclature chapter. Probably will need to contact you again, but I wanted to say right off that your review is

FABULOUSLY HELPFUL!

P.S.:
Your note "(54)": "Brown (1954)" — more complete ref. poss.?
(55): "Fensome et al." — "
(19) & (55): "Fensome et al. 1992..." "
(41): "Fensome et al. 1991" (= AASP C.S. 26?)

Best Al

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

Energy, Mines and Resources Canada
Energie, Mines et Ressources Canada

Geological Survey of Canada
Commission geologique du Canada

July 26, 1991

Dr. Alfred Traverse
Department of Geosciences
435 Deike Building
Pennsylvania State University
University Park, Pennsylvania 16802
U.S.A.

Dear Al:

I am writing in search of nomenclatural advice. I am currently finishing a monograph on the classification and evolution of fossil and living dinoflagellates (with Max Taylor, Geoff Norris, Bill Sarjeant, David Wharton and Graham Williams). In the process of this project, which has been in progress for several years, we have met with serious problems regarding homonymy of genera. Such problems arise because dinoflagellate workers consider themselves individually as phycologists, protistologists, protozoologists and micropaleontologists (palynologists). Hence, dinoflagellate workers as a group use both botanical and zoological nomenclature. Nomenclatural tangles such as those described in the attached manuscript excerpt, as well as in Taylor et al. (1986, Taxon, v.35, no.4, p.890-896) and Taylor et al. (1987, Systematic Zoology, v.36, no.1, p.79-85), are relatively common. The principle proposal of Taylor et al. was to rule as illegitimate those names of phytoplankton which have a senior homonym in either botanical or zoologic nomenclature. As you may know, the proposals of Taylor et al. (1986) were rejected.

The question I pose to yourself and other nomenclaturally oriented colleagues of influence is: how should we best proceed from here to solve our dilemma? The matter constitutes a very real working-level problem for dinoflagellate taxonomists and we were somewhat disappointed that the Committee did not provide any constructive guidance when rejecting the proposal. One apparent problem was that phytoplankton couldn't be defined, although we did circumscribe the group; focussing on dinoflagellates might solve that issue, but then I'm sure that the criticism would arise that special provisions cannot be made for such a "small group". I also read that dinoflagellate workers "should get together and use a single code". This does of course make sense, but attitudes of both sides of this nomenclatural issue are so deeply entrenched that such a solution will not occur in the foreseeable future. Should we revise our earlier proposal, amassing as many dinoflagellate authors as possible; or should we try the conservation route for such names as Dissodinium? Or is there a better route to follow?

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Your thoughts and advice on all this would be much appreciated, especially since we would like to prepare something for the November Taxon.

Yours sincerely,

Robert A. Fensome
Basin Analysis Subdivision

RAF/nk

c.c. W.A.S. Sarjeant
F.J.R. Taylor
G.L. Williams

Encl.

Excerpt from Fensome, Taylor, Norris, Sarjeant, Wharton and Williams, in preparation.

At present, the nomenclature of dinoflagellates can be formally governed either by the *International Code of Botanical Nomenclature* (*I.C.B.N.*; Greuter *et al.*, 1988), or by the *International Code of Zoological Nomenclature* (*I.C.Z.N.*; Ride *et al.*, 1985), depending upon whether these organisms are considered as algae (plants) or as protozoans (animals). Following the proposal of Downie *et al.* (1961), paleontologists have, by consensus, chosen to treat dinoflagellates as plants; however, no similar consensus has emerged amongst biologists. We view dinoflagellates as neither animals nor plants, preferring instead to consider them as protists (in the sense of Taylor, 1978). Since, however, there is no formal code for the latter, even though a Kingdom Protista has been recognized for more than a century (see Corliss, 1984), and since the majority of specialists on modern and fossil dinoflagellates follow the *I.C.B.N.*, we endorse the use of that code.

The most serious and perplexing nomenclatural issue facing dinoflagellate workers concerns homonyms and was addressed by Taylor *et al.* (1986, 1987). Neither the *I.C.B.N.* nor the *I.C.Z.N.* recognizes as homonyms those names proposed under one code but preoccupied under the other; and each code obliges the use of names which are, in some cases, banned under the other. This patently absurd situation leads to a number of nomenclatural tangled webs such as the examples outlined in the following paragraphs.

Using zoological nomenclature, Stein (1883) proposed the generic name *Goniodoma* for a genus of dinoflagellates, with "*Peridinium*" *polyedricum* Pouchet 1883 as type. However, as a junior homonym of the butterfly genus *Goniodoma* Zeller [date to be checked, but earlier than Stein], the name *Goniodoma* Stein is "illegitimate" in terms of the *I.C.Z.N.* Lemmermann (1900, p. 368) placed *Goniodoma* Stein under the botanical family name Peridiniaceae, thus rendering *Goniodoma* an apparently validly published and legitimate botanical name, since the generic name *Goniodoma* is not botanically preoccupied. Dodge (1981) believed *Goniodoma* Stein (i.e. *Goniodoma* Stein 1883 *ex* Lemmermann 1900) to be an illegitimate homonym under the *I.C.B.N.* and proposed a new name, *Triadinium*, to replace it. However, since *Goniodoma* Stein *ex* Lemmermann is a legitimate botanical generic name dating from 1900, and since

Triadinium Dodge is a validly published botanical generic name dating from 1981 with the same type as *Goniodoma* Stein *ex* Lemmermann, *Triadinium* Dodge must be considered a superfluous nomenclatural synonym of *Goniodoma* Stein *ex* Lemmermann (Article 63.1), and as such banned from all future use under the *I.C.B.N.* Furthermore, *Triadinium* cannot be "legitimized" under the *I.C.Z.N.* since it would become a junior homonym of the ciliate genus *Triadinium* Fiorentini 1890. Thus, currently, use of the name *Goniodoma* is obligatory under the *I.C.B.N.*, whereas workers using the *I.C.Z.N.* are officially obliged not to use *Goniodoma*, but have no "legitimate" alternative.

Klebs (1912) validly proposed the generic name *Diplodinium* under the botanical family Gymnodiniaceae. The name *Diplodinium* was not preoccupied prior to 1912 in botanical nomenclature, but was preoccupied by *Diplodinium* Schuberg 1888 and *Diplodinium* Fiorentini 1890 [check] in zoological nomenclature. Thus, believing *Diplodinium* Klebs to be a preoccupied name, Klebs *in* Pascher (1916a) proposed a new name, *Dissodinium*, to replace *Diplodinium* Klebs in a botanical context. Hence, in the very act of trying to rectify this situation involving homonyms, Klebs *in* Pascher (1916a) actually created an illegitimate superfluous botanical name - *Dissodinium*. Nevertheless, *Dissodinium* Klebs *in* Pascher has come to be the universally "accepted" name amongst dinoflagellate workers. Similar examples were cited by Taylor *et al.* (1986, 1987) and in the systematic section of the present work.

good candidate for conservation

Taylor *et al.* (1986, p. 893), amongst other suggestions, proposed that Article 65 of the *I.C.Z.N.* be augmented with the following new clause: "The name of a phytoflagellate is illegitimate and is to be rejected if it is a later (junior) homonym of the name of a taxon of the same rank validly published under the *International Code of Zoological Nomenclature* or the *International Code for the Nomenclature of Bacteria*". Unfortunately, this proposal was rejected (see Greuter *et al.*, 1989), apparently because the term phytoplankton could not be well-defined. Hence, the very real problems surrounding the treatment of dinoflagellate homonyms remains unresolved.

Energy, Mines and Resources Canada
Energie, Mines et Ressources Canada

Geological Survey of Canada
Commission geologique du Canada

20 November 1991

Dr. Alfred Traverse
The Pennsylvania State University
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435 Deike Building
UNIVERSITY PARK, Pennsylvania 16802
U.S.A.

Dear Al:

Thank you very much for your letter of 14th August. I'm sorry that it has taken me so long to respond, but I've been finishing up the big dinoflagellate monograph that I mentioned in my previous letter to you, and have been doing my best to ignore/delay other pursuits such as correspondence. Now that the manuscript is in the hands of the reviewers I can return to Earth.

With regard to the homonymy situation, to some extent I am sympathetic with your hardnosed approach towards using a single code. However, since we have to live with our dinoflagellate zoological colleagues, we feel tht some tact is required. An additional problem is that some dinoflagellate names (mostly ICBN names) are homonyms of ciliate names (all ICZN names). Molecular studies are now confirming that ciliates and dinoflagellates are close relatives. It seems absurd that we should have to reject a dinoflagellate name because it is a homonym of the name of an orchid (Isabelia for example), but accept one even though it is the junior homonym of a ciliate (for example Sporodinium, for which the name Gyrodinium is universally, if illegitimately, used).

Based on correspondence from yourself and other nomenclatural experts (I enclose a particularly informative letter from Werner Greuter for your information), I think that my co-authors and I should not be precipitous. Perhaps for the next round of proposals (for the 1996? congress/deadline) we should prepare some proposals for conservation/rejection of selected genera. I think that we can make some good cases, but if we are turned down, at least we can then pass on the blame for our future use of unpopular names. Like Jan Jansonius, I feel uncomfortable about sanctioned names. But such a procedure might work, depending upon how it was applied, and how comprehensive the lists are.

Please feel free to use my examples in your chapter; indeed I would be flattered. (Incidentally, with regard to one of the examples, I now agree with you regarding Goniodoma). You may also wish to use examples from AASP Contribution 25, the "Acritarch Index"; we went to some length over nomenclature and the ICBN in the Introduction to that work.

Anyway, thanks again for your response to my earlier letter; it was much appreciated. With best regards,

Yours sincerely,

Robert A. Fensome
Basin Analysis Subdivision

RAF/nk

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30 December, 1990

David Fensom
c/o Biology Department
Mount Allison University
Sackville, New Brunswick, Canada
E0A 3C0

Dear David:

Yours of 6 November--the folder with delightful painting of Hong Kong--was most welcome and interesting. I am not sure I've acknowledged it before, as things have been rather confused for a couple of months. My Mother died after a fall and hip surgery in mid-October. At 97, she couldn't expect to go on much longer, but I wish it hadn't happened that way.

It is sad to learn of your dear wife's condition. It is only, I believe, three years since I visited you both in Sackville, and she seemed great. Surely this illness developed fast. The diagnosis you mention is the same one my mother got on admission to the nursing home in May, 1990, but Mother was much older! Are you still living in your home near the center of Sackville?

All the best.

Yours very truly,

Alfred Traverse

AT/et

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14 August, 1991

Dr. Robert A. Fensome
Atlantic Geoscience Centre
Geological Survey of Canada
Bedford Institute of Oceanography
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Dear Rob:

Yours of 26 July just came in. Wow! Your new monograph sounds like a terrific contribution.

The problem you raise about the Codes is a very interesting one. As you may know, I am writing the chapter on nomenclature in the book Jansonius is putting together on palynology. If you don't object too strongly I would like to use your stated dilemma as one example in what I put together.

I am not a Linnean scholar, though I have read a little from the old master. I imagine that he would be perplexed that binomial nomenclature applied to organisms can't be handled by ONE code. Since we do have multiple codes, it would seem that logically there should be separate ones at least for the KINGDOMS--but fungi are now handled under the botanical code, indeed the mycologists are perhaps the most influential (vocal) people at the sessions at botanical congresses where the ICBN is revised! It would seem that perhaps logically the protists, including your group, should have a separate code. I mention all this only to express my opinion that we should really be out on the hustings trying to get everybody to agree on a single code!

Having said all that, and recognizing that you have a problem in the here and now, I would say that for all that dinoflagellates are protists, they are CLEARLY more nearly related to the Plantae than to the Animalia. For example, the cysts consist (at least partly) of sporopollenin. What animal includes that compound in its inventory? My advice therefore would be to INSIST that dinos are plants from the point of view of the various codes at least, and let the devil take the hindmost. Such a decision may be unpopular with a few specialists, but it certainly makes life easier! Names published for dinoflagellate cysts as if they were animals would be validly published (ICBN 45.4) as plants, but homonymy would be governed only by conflict with plant names. (See ICBN 65). If one takes this hardnosed and logical approach,

the problems you outline in the excerpt from Fensome et al. are very easy problems. I certainly don't need to explain that in detail, but for example, *Goniodoma* is certainly the correct name for the entity you discuss in para. 3. However, it looks to me from your data that (per ICBN 45.4) it was " validly published" in 1883 (despite the fact that it was zoologically a homonym--that seems to me to be irrelevant.)

Another matter that is in the wings but which cannot be ignored is that there is a strong movement underway to establish lists of "sanctioned names"--the mycologists have already done that with extensive lists. This is really just an extension of the idea of conservation. Jansonius hates the whole idea, but it has a big constituency, and I sometimes think it would be the way to go to get some troublesome names settled, once and for all.

Hope that this is helpful, and that you won't object to my using your letter as a "case".

Yours very truly,

Alfred Traverse

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10 May, 1991

Dr. Robert A. Fensome
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Atlantic Geoscience Centre
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Dartmouth, Nova Scotia, Canada B2Y 4A2

Dear Bob:

Actually, I'm not sure I'm permitted such familiarity! But I really did appreciate the neat gift of AASP Contrib. Ser. 25 & 26! I will remember your kindness. I enclose some reprints you may not have.

Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
encl: reprints

Energy, Mines and Resources Canada
Geological Survey of Canada

Energie, Mines et Ressources Canada
Commission geologique du Canada

Dear Colleague

Please accept the enclosed reprints with the authors' compliments. The publications are:

Acritarchs and fossil prasinophytes: an index to genera, species and infraspecific taxa. American Association of Stratigraphic Palynologists Contributions Series #25

Alphabetical listing of acritarch and fossil prasinophyte species. American Association of Stratigraphic Palynologists Contributions Series #26

As we intend to produce an updated and corrected second edition of the "Acritarch Index" in a few years time, I would be grateful if you would bring to my attention omissions and mistakes of any kind.

I would also appreciate copies of any reprints you might have on acritarchs and fossil prasinophytes. To those of you who are more involved in research on dinoflagellates and Mesozoic - Cenozoic miospores, please keep me on your reprint lists. I retain a significant interest in these palynological groups.

Best regards,

Robert A. Fensome

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Canada

22 March, 1984

Dr. R. A. Fensome
Department of Geology
University of Toronto
Toronto, ON, Canada M5S 1A1

Dear Dr. Fensome:

I note in recent CAP Newsletter that you are now Sec.-Treas. I tremendously enjoy the Newsletter (best in the world) and want to be sure I keep getting it. I understand that despite my Canadian birth and connections I am not eligible for membership, but can get the Newsletter for $1/annum. I enclose check for $4. I believe that certainly brings me up to date. If there is surplus, please record it as part of Geoff Norris' dues, so that CAP will be spared the embarrassment of dropping the President for non-payment (per a recent NL).

Best. Regards to GN.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

encl: check