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To: jjansoni@nrcan.gc.ca
From: Alfred Traverse <traverse@ems.psu.edu>
Subject: fungal spores;IBC, etc.
Cc:
Bcc:
Attached:

Hi, Jan:

Betty is even more improved today, but it's almost two weeks since the initial onset of the bug, when she was taking care of Celia's home, kids, and pets, in Camp Hill. Today she is madly working on the annual budget for our condominium complex, of which she is council member and treasurer. Council monthly meeting is today at noon. The toilet in our unit has sprung a leak, and she is also waiting for the plumber over there, with various notes posted. We stayed over there last night, but she slept in the guest place because she was concerned that her coughing might keep me awake. Actually, compared with the all night St. Patrick's Day party at the fraternity next door, her coughing was nothing.

During the night I happened to think that if you really are not going to take part in the IBC at all, you should save yourself a bundle by not registering for it. The Bureau of Nomenclature is really totally independent of the Congress-Proper, or at least was at past congresses and probably will be again. To take part in the debates, and vote, etc., at the nomenclatural sessions it is necessary to be an IAPT member, at least in theory, but I don't think one is often if ever challenged even on that. If I were you, with your intentions, I would show up at MBG and sign in for the Bureau of Nomenclature but not register for the Congress, and use the moneys saved to pay other expenses. The Congress-Proper meets at the Center of America downtown, and one registers there, I presume. The sign up for the Bureau of Nomenclature will presumably be at the MBG.

It seems to me from para. 2 of yours of 16-III that you have understood correctly what I wrote about my further examination of the spore body we have been discussing. The slides have now been duly sent to Carnegie, after I received a letter from the current reigning curator of fossil plants, who seemed very marginally to understand what she would be receiving, but she did know about the fact that the Carnegie is "official" repository for AASP. Of course, you know my attitude: these things are actively disintegrating preparations, and the whole exercise is delusional. The item figured in the paper in J. of P. is only slightly more the same item that now exists on the slide just sent to Carnegie than the skeleton in A. Lincoln's tomb in Springfield, IL, really is Old Abe. The picture on the $5 bill is more like him.

Comments on your para 4. Use of a "glass rod" for applying a drop of glyc. jelly + residue to a slide or coverslip is not good technique. To get a representative mixture of what the residue contains, one must use a glass TUBE.
Years ago I proved this with experiments--probably only in a Shell report.

Beyond my first description of the AYAF method, after I developed it, others (e. g., the USGS lab in Denver) took it up, but I seldom used it--glycerin jelly has so many advantages (ease of handling, storage, and optical qualities, primarily). So, I never mentioned it again in print. As such items go, it was a winner, because of the index of refraction.

Sure, you may use those polaroid photos any way you like. By the way, the ones in the publication were polaroid also. When I am studying a bunch of slides, I use 35 mm. Kodak TMax100 and take hundreds of photos. The magnification is erratic, unless I pay for "custom" processing, which is something like 5x the normal charge. Then I make a mockup of the plate using the Kodak prints and then make the prints for the final plate with Polapan 100 (664), because with the bellows of the Aristophot camera I can adjust the size to get exactly 1000x or whatever magnification I want to use (almost always 1000x). Using a millimeter scale you can determine the exact size of the sporebody in question by using the figure in the J. of P. article.

I am taking a printout of your message home to check whether I have Kalgutlar pubs. and will let you know. Where would I have met him?

I am now entering the Stanley Place address in my various address files. It's getting complicated, that business. We have three residences, and I have this little office at PSU, for a total of four places. It's even worse than that, as I have two offices at home, the personal one and the new palynological one. They are on different floors, etc., but that's not so bad--the exercise afforded by the running up and down is a good idea!

OK, back to checking the e-mail. Ye gods, that thing is an unrelenting taskmaster, and there are people out there urging me to get even deeper into computers! When would I care for my plants?

Best. Al.

Delivered-To: traverse@ems.psu.edu
From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: 'Al' <alandbetty@juno.com>
Cc: 'Traverse' <traverse@ems.psu.edu>
Subject: Dicellaesporites serrulatus and other
Date: Tue, 16 Mar 1999 18:03:43 -0500
X-Mailer: Internet Mail Service (5.5.2448.0)

Dear Al,

Thank you for your letter of 5 March, accompanying the new reprint and containing an exegesis of the annotated fig. 11.

If I understand your related observations properly, it seems that there are no distinct pores at either end, but a weak annulus ("anulus"!) marking the thin septum between (counting from the 'equator') the third [what you referred to as 'terminal cell'] and fourth cell. The final cells in a number of conidia are thinner walled, and are separated by a thinner septum, because, in this group, each cell made its own endoderm (for which reason each septum is double walled) except the terminal cell, the outer wall of which is not coated by an endoderm, and the basal septum of which thus has only a single layer. This last (fourth) cell, then, really does have a continuous covering ("membrane" you call it), and not a preformed opening. Of course, if other spores now assigned to Diporicellaesporites were given as secure a scrutiny as you now gave D. serratulus, it might be found that several of them would have termini closed by a thin membrane. We now, I think, will want to transfer this species to Multicellites gen nov., which differs from Diporicellaesporites in having closed (non-porate) terminal poles.
I thank you for the trouble to look at those details, because I had a suspicion that that might be the case, but could not convince myself from the photographs.

Before I forget: yes, we still live on 11 Stanley Place SW, Calgary, AB, Canada T2S 1B2 -- and we hope to live there for a few years longer, we really love our place.

On another topic, the g.j. slides. No, I hadn't looked in your book at what you said on that score. But I did react to the statement in yours of 16 February, in which you wrote:
I WANT THE ENTIRE COVERSLIP AREA TO BE FULL OF MATERIAL,
AND MY SLIDES NORMALLY ARE.
Now, in your letter, you say that you do leave a ring around the periphery and seal that with some material that ought to do the job. (We hope for longer than we will be around to observe.) Hey, what can I ask more for? However, on re-reading your letter I notice that the ring is not a ring between the circumference of the coverslip and the mounted residue, but between the edge of the turned-and-mounted coverslip and the edges of the carrier slide. This appears structurally less desirable, because the sealing agent is outside the preparation, and apt to get knocked or mechanically abused. If you can have a bead of wax under the coverslip, along its periphery, by not spreading your residue all the way to the edge, you get a permanent seal that is very efficient, and difficult to dislodge or damage. I just read your text on p. 451-452, and without any implied criticism, I only want to relate the method followed at Imperial Oil: there, glyc. jelly slides were made on the warm hot plate, on the coverslip lying on its labeled glass slide, by first dropping a small drop, or a solid bit, of the g.j. onto the coverslip (with a glass rod), then a drop of the washed organic residue (again with a rod), whereupon the two drops would be stirred and mixed, and spread on the coverslip with the last rod. We would ensure that the residue mix in g.j. would stay clear of the edge of the round coverslip by about 3 mm. After spreading it, the tray with prepared slips would be left on the tray for another 30-45 minutes, to drive off the surplus water from the residue, then they would be turned over and pressed onto the glass slide with a piece of pencil erasor to ensure that there were no lumps sticking up, or annoying air bubbles. Then, we would drop a bit of melted wax under the edge, which would readily spread around the full circumference. If there was too much wax, it would get cleaned up with some

solvent. Our residues were stored in water with a drop of fungicide. {RATS RATS -- this is the very method you describe for making permanent single-grain type slides in fig. A-8 (p. 480). Except, we used a larger "window". Al, have you looked at any of these single grain slides, lately? Have the specimens in them really deteriorated a lot?}
This worked very well for us.
I shall not return to this topic.

I didn't find an entry to AYAF in your index -- I'm intrigued.

Well, as you can see, this e-mail was interrupted by jumping back and forth between yoyr letter and your book.

Would it be ok with you if we use two of your polaroid photos in a small publication in which we want to publish the photos of your and other's material (Clarke, Rouse, Salard-Cheboldaeff) that show some of the critical details better than in the original publications?

O, yes, I don't think you have met Ram Kalgutkar. He came from India a long time ago, did a MSc and PhD with Len Hills in Calgary (the latter on lichens), but couldn't find a real job on the strength of that. He went into business for himself (ran a depanneur in central Calgary), then got a job at the GSC as processor in the palynology lab. But, with the typical liberty within the GSC, where people can switch back and forth from administration to research, he was given permission to work part time on professional subjects. Thus, he has several good publications on fossil fungal spores. If you don't have any of his reprints, let me know and we shall rectify that. He knows an real big heap on fungi, but doesn't communicate very easily. I now have a bit better idea on how to edit the package. We're a bit like Jack Sprat and his wife, rather opposite in our ways, but between the two of us, etc.

Now I'll have to get with it and arrange my accommodation in St. Louis. Got the Febr. issue of Taxon, and will have to start grading the proposals. Lots of homework, still, for us tarred people.

Thanks, again, Al! I hope Betty got over her flu. Best regards, both of you,

Jan.

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or: traverse@ems.psu.edu

5 Mar., 1999

Dr. Jan Jansonius

Dear Jan:

You never did answer my query about whether my home address for you is correct–Stanley Place, or some such. It's on a card in the basement, and is the only one I have for you here. One of these days I am going to have to make up an address list so that I have correspondents' addresses both places. In any case, I should have your home address, so that I can send you another year's end greeting one of these years. I've been thinking about the one I sent a while back that you trashed. Was the problem that it didn't contain any of the usual Christian garbage?

This responds to your e-mail of 25.II, which I printed out yesterday, my first full day back at the PSU office after our trip to Chicago to deliver my paleobotanical library to the Field Museum.

You sometimes come close to making me angry. This insulting business of instructing me how to make microslides has now been going on for several weeks. I probably was making slides of pollen and spores before you had heard of them (began in 1946). Originally I used marine varnish to ring all glycerin jelly slides, and I still think it is better than fingernail polish–tougher, lasts longer, sticks to glass better. But fingernail polish is so easy to use, and requires no brush clean up, etc. If you really had studied **Paleopalynology** closely, you would have noted that it's all in there. I always use coverslips narrower than the microslides (see Fig. A.7, p. 478), to leave room for a good ring, and for several decades I have used a very good bead of fingernail polish (see p. 463, just above the caption) instead of the marine varnish to insure seal (indeed, I thought the idea was original to me–I picked it up from Maxine Abbott in 1947–she was using the stuff to make paleobotanical transfer preparations). I also have tried all sorts of other mountants and slide-making techniques and have published some of my observations. Indeed, for years the USGS credited me in publications with development of the mountant AYAF, a vinyl substance that I picked up from museum workers who were using it to replace the old, browned varnish on ancient oil paintings.

If one wants longer-lived slides, one shouldn't use glycerin jelly at all–it has the properties of a transparent artificial leather. No leathery materials are free of gradual deterioration. Canada balsam is not the answer either, as you noted in an earlier letter, and on which I have commented in print several places (e. g., p. 454 of the abovementioned book). One of the artificial resins is probably the way to go, but please remember that the fossils themselves also degrade. Thus, my

suppose we were replanting this baby in another ten years!

opinion that all the hurrah about conserving type specimens is a colossal waste of time.

The subject slide of the letter seems to be in fine shape–no degradation of the g. j. notable, as frequently it is in old slides. Those rocks contained little organic matter, and the organic residue contained only a limited number of fossils, so we made the residues very "thick," i. e., relatively less jelly than normal. They are not "pretty." I suppose it's possible that the other, relatively thick junk on the slide is the source of the oxygen that is destroying the specimen.

The slides were on my desk at PSU awaiting word from Carnegie that there is indeed somewhere in the museum a lab where the slides would be curated. So far, no answer at all. So, I have brought the package home again to have a look at the slide in which you have an interest.

It is nice that you "accept" my word that I was photographing the type. I keep pretty good records, and there is no doubt about that specimen. It is indicated by a sticker pointer. I have the mechanical stage coordinates in my photo record book. And it checks out with the England Finder location published. Imagine you doubting me, for heaven's sake.

So, now I have that old baby on the Ortholux, and I will comment on your letter's statements in para. 4. To my eye today (I am using a 95x fluorite oil objective this time), the spore seems clearly to have 4 main cells and two terminal cells, for a total of six. There really is no doubt of this. The two terminal cells are smaller, and they narrow terminally, to give the whole spore the spindle shape that it has. There is a **slightly** thickened annulus at the end of each terminal cell, but the annulus doesn't show in the bottom terminal cell in Fig. 11–it is there, however. These annuli make it appear in photos that there are actual pores or porelike openings terminally, but critical focusing does not show any real terminal pores–the terminal membranes have small breaks, rather as in the ulci of ulcerate pollen grains, but no actual pores.

I guess rather than falling into the "have you stopped beating your wife" trap, by answering questions I may not fully understand, I will let it go at that. I am enclosing a further copy of the paper with a pencilled-in illustration of what I mean in the last paragraph.

I really am delighted that anybody is interested in these little guys. I thought they were great fun to discover.

All the best. Have I ever met Kalgutkar? Give him* my regards.

Al

enclosure: reprint with tiny drawings!

* just a guess based on "Ram" — but folks from that subcontinent are always surprising me w/ their names!

Delivered-To: traverse@ems.psu.edu
From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: 'Al' <alandbetty@juno.com>
Cc: 'Traverse' <traverse@ems.psu.edu>,
"Kalgutkar, Ram" <RKalgutk@NRCan.gc.ca>
Subject:
Date: Thu, 25 Feb 1999 18:05:12 -0500
X-Mailer: Internet Mail Service (5.5.2232.9)

Dear Al,

I came in this morning to find your special delivery in the mail slot: greatly thanked for that!

Kalgutkar and I then spent some time trying to find our bearings ("tramontanen" aufs Dutch). Ram had a hard time believing it; however, now that I have spent some more time with it, I accept that this is indeed the type. It is a bit discombobbulating, however, to find that type specimens have a life of their own, and can grow older in a hurry (geologically speaking).

Before I do anything else, though, I'd like to beg you to ring the coverslips with some old nail polish -- with any luck Betty has an old bottle the color of which has gone out of style. I think that the glycerine jelly is loosing its moisture content, thereby sucking the coverslip closer to the glass base, which explains why the specimen now looks so much fatter, and shows other features hidden in the first photo.

Ram thought that there are indeed, as your generic assignment would suggest, two terminal pores. I myself am not so sure about that. Indeed, I think that apparently a central pore, in each of the internal septa, is small, but surrounded by a thickening (a pore ring, if you want). This pore is the same size for all septa. I count 6 good-sized cells in the spore, of which the most distal (centrifugal) ones are relatively small; in fact, here the pore ring almost coincides with the septal base (i.e., the thickened ring where the septum joins with the spore wall). Thus, it may seem that the pores in the septa nearest the ends of the spore, are pores that stand between the last cell and the outside world. Now, looking at the 1994 paper, I see a small dome (which was cut off) at the top of the specimen, above the small cell that comes immediately above the largest cell in the grain -- but a terminal pore cannot be proven or disproven. However, although in your new photos that dome should now be fully illustrated, it is almost invisible -- it is very shallow, barely separated from the highest septum visible in the 1994 figure. At its very (terminal) tip, I cannot see a pore (of the kind just discussed, with a pore ring), but all I see is a crack that (in photo 2) meanders to the lower right past the final internal septum (if I may maintain the orientation of the published figure). I don't think that crack is a pore. So, at the top there is a VERY small and thin-walled cell, that seems to have no pore. Of course, one could argue that in the new photos, the aspect of the spore has become a bit more oblique, and that therefore the backside of the spore wall is showing bit above the final (terminal) pore. But, if that were the case [i.e., if there are only 6 cells], that means there must be 7 septa (including 2 terminal ones).
Is it possible that the "outside" tooth-like structures are the two sides of that upper terminal cell, that now show a break in them. I can believe that, when you mounted it, that slit was the only feature of the spore top, and more open than it is now. Do we then maybe have 7 cells in all (4 on the top side, 3 below the 'middle')? Or, do we really have 4 cells in th elower half of th egrain, too?
In the published figure, there is a semi-circular feature just above where the bottom part of the figure is cropped. If that is part of the final cell, then it could mark a rim around the base of the spore (maybe a hilum?) In your photos 3 and 4 there is a distinct pore, that I think is in an internal septum. But, in the highest focus (photo 1) there is another ring, with a small pore. That looks like it corresponds with the thin bright feature just in th ebottom part of fig. 11. Could this be another internal septum, although very close to th etermination of the spore? If so, we could have 8 cells, of which 1 and 8 are very small, shallow and thin-walled; but, then,

apparently without external pores or evidence of a hilum.

I think that you took some very instructive, detailed photographs, Al. But, for the finest details, that can only be transmitted by verbal means, I must ask for the help of your eyes and experience, once more (I hope you still have the slides!): when you follow, with the focal level, the contours of the terminal-most cells -- do they have a continuous covering (even if cracked by compression), or do they both (or one of them) end in a definite pore (with a bit of thickening around them? On the initial plate I made for Diporicellaesporites, the overall aspect of your spore fits in quite easily. Maybe it does belong there. However, I can still not be sure of that. Especially because these spores are relatively old, it is of importance to describe them in precise detail.

Thanks, again, for all your effort. Best regards/wishes,

Jan.

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or: traverse@ems.psu.edu

17 Feb., 1999

Dr. Jan Jansonius

Dear Jan:

One of the problems I have to get used to is that my address files are in the cubicle office at PSU. At home I have an ancient card with an address on Stanley Place (as in Dr. Livingstone, I presume?)–is that still valid. It hasn't been used in a while because you made an insulting remark about a greeting card I sent at year's end a few years ago, and that ended that.

So, I'll address this envelope at the office tomorrow or the day after.

I have photographed the much-discussed holotype specimen with the enclosed results. I used a 54x oil immersion fluorite objective with a Leitz Ortholux setup and a Polaroid photomicrographic outfit, same one as used in making the plate for the publication, except that for that one I used a 40x hi-dry fluorite objective. The 54x gives a little more detail and almost too much depth of focus.

The specimen is not in quite as good shape as in 1991 when the photos in the plate for Traverse & Ash were made (that paper was in press about three years, believe it or not). Considerable additional cracking and squashing.. The terminal cell that is up in Fig. 3:11 is shown on the right in the enclosed photos. It has squashed down a little, and a porelike structure with an "operculum" shows at the end (esp. in photo 3). I suppose this must be where I saw the small teeth that I mention in the description. The structure no longer looks very "toothy". The "basal cell" that is down in Fig. 3:11 is to the left in the enclosed photos. This part is also squashed down a bit. In any case, it is now clear to me, that what looks like one cell in at the bottom of the specimen in Fig. 3:11 is actually two cells. The terminal one also has a circular "pore." The porelike deal shows in photo 2 quite well. The thing that looks like an annulate pore in the penultimate cell on that end, which shows in photos 3 and 4, is just part of a crack in a small fold. I would now count the number of cells in this specimen as 8. It's possible that I counted it as 7 in the original work.

The slides now go off to Carnegie. Truth to tell, I'd rather have them here, lots rather.

Regards.

Alfred Traverse

To: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
From: Alfred Traverse <traverse@ems.psu.edu>
Subject: Re: reply to yours of 99 02 15
Cc:
Bcc:
Attached:

At 05:09 PM 2/15/99 -0500, you wrote:
>Sorry, Al, if you feel insulted. I had a clear conscience, and so went back
>to the original e-mail.

WELL, OK, IF I MISUNDERSTOOD AND YOU DIDN'T ACCUSE ME OF BUCK-PASSING, NO PROBLEM.

>And there's never real malice in my comments, rest assured. Those people I
>can't stand have long since ceased to receive any communications originating
>here. THAT'S NICE. I REALLY WOULD MISS SPARRING WITH YOU.
>
>Yes, it is a pity that you didn't have enough warning to build a waterproof
>arc to save your accumulated documentation and old correspondence.

BELIEVE ME, BETTY WANTED ME TO CHUCK FAR MORE. SINCE THAT HORRENDOUS MONTH OF JULY, 1998, A WOMAN WHO IS WRITING A BOOK ABOUT A BELOVED, LONG SINCE DECEASED BOTANICAL COLLEAGUE WANTS ME TO REVIEW THE MS, AND SHE WANTS MY CORRESPONDENCE WITH THE FELLOW FOR THE ARCHIVES AT THE BOTANICAL RESEARCH INSTITUTE OF TEXAS.
FORTUNATELY, THAT CORRESPONDENCE WAS AMONG TWO BOXES OF SUCH I GRABBED AND PUT IN THE BASEMENT OF THE CONDOMINIUM COMPLEX WHERE OUR PLACE HERE IN S. C. IS. EARLIER, I NEEDED MY OLD FILES OF ASH CORRESPONDENCE TO RUN DOWN DATA ON THE SAMPLES ON WHICH I AM NOW WORKING. AGAIN, IT WAS IN A BOX OVER THERE.

specimen of fig. 11, then it's ok -- the rest was not fundamentally flawed.
>And if not -- well, that must be god's will. IN RETROSPECT, I SUPPOSE I SHOULD HAVE TRIED TO SAVE ALL PLATES OF PUBLISHED FIGURES, BUT IN THE PRESSURE OF THAT HORRENDOUS MOVE I DIDN'T THINK OF THAT, HAVING MADE THE DECISION THAT ALL SUCH DATA HAD TO GO. I AM GOING TO FIND IT INTERESTING TO SEE WHETHER THE FIG. IN OUR PAPER DOESN'T IN FACT SHOW ALL THAT NEEDS TO BE SHOWN ABOUT THAT SPECIMEN [WHEN TAKEN IN CONJUNCTION WITH THE DESCRIPTION, THAT IS].

IN JULY I ALSO DECIDED TO CHUCK ALL SAMPLES THAT REALISTICALLY ARE NEVER GOING TO BE WORKED ON AGAIN. I USUALLY TAKE ALL SUCH MATERIAL TO OUR COUNTRY PLACE AND PUT IT ON THE LANE IN THE WOODS. I KEEP A BOOK WITH RECORDS OF HOW MANY GRAMS OR KG, WHAT AGE, WHAT GEOGRAPHIC PROVENANCE. ALL CONTINENTS, ALL GEOLOGIC AGES FROM PRE-CAMBRIAN TO PLEISTOCENE. THE ROAD ITSELF IS ON MID-DEVONIAN. IT'S CALLED MISCHIEVIOUS RE-WORKING. BUT THIS TIME BETTY PERSUADED ME THAT THERE WASN'T TIME FOR THAT, AND IT ALL WENT INTO DUMPSTERS HERE. NOT A WEEK LATER I GOT A PHONE CALL FROM A GRAD STUDENT AT ANOTHER UNIVERSITY. DID I HAVE SAMPLES FROM EDIGER'S RICHMOND BASIS DOCTORAL WORK? TWO BIG BOXES OF SUCH, UNDOUBTEDLY PALYNIFEROUS, NEVER WORKED UP BY VOLKAN, WHO GOT INTERESTED IN OTHER MATTERS, HAD BEEN CHUCKED. WELL, AS YOU WOULD SAY, IT'S ALL WATER OVER THE DAM, BUT IT IS POIGNANT.
>
>I meant -- does King have any idea what is now (left) in place in Carnegie
>(Pittsburg?) IT WOULD BE EASIER TO GO TO PITTSBURGH (NOTE SPELLING) AND SEE FOR MYSELF. IF I THOUGHT IT WAS IMPORTANT.
>
>You may want to give a look at restoring (or as the case may be: finally
>finishing the mounting) of the type slides in your box. The ICBN also
>doesn't specify that holotypes must be thoroughly desiccated before being
>mounted. That is not its area of jurisdiction. But if proper methods have
>been published (see the blue book), a careful practitioner should know what
>is expected of him. YOU WOULD SEEM IN THE ABOVE NOT TO UNDERSTAND THAT PALYNOLOGICAL HOLOTYPES ARE VERY SELDOM INDIVIDUAL MOUNTS SELECTED FOR THE PURPOSE. FOR EXAMPLE, TAKE THE SPECIMEN THAT I AM IN THE PROCESS OF PHOTOGRAPHING FOR YOU. WHEN THE STUDY BEGAN I HAD NO IDEA IT WOULD BE ABOUT FUNGAL SPORES, NOR THAT THERE WOULD BE NEW TAXA. WHAT ENDED UP BEING DESIGNATED HOLOTYPES WERE SPECIMENS THAT WERE SEEN IN THE ORIGINAL RECONNAISANCE OF THE SLIDES--VERY DENSELY PACKED PREPARATIONS FULL OF ORGANIC MATTER. I WANT THE ENTIRE COVERSLIP AREA TO BE FULL OF MATERIAL, AND MY SLIDES NORMALLY ARE. YOUR SUGGESTED METHOD WOULD LEAVE LARGE AREAS OF SLIDES WITH NO PALYNIFEROUS RESIDUE. WASTEFUL AND SUPERFLUOUS, UNLESS ONE THINKS THAT LONG TERM PRESERVATION IS CRITICAL, AND THE MORE I THINK OF IT THE MORE USELESS THAT SEEMS TO ME. WHAT YOU ARE DOING WITH YOUR CATALOGUING IS A MILLION TIMES MORE USEFUL.

YOU SEEM TO BE CONVENIENTLY FORGETTING THAT AFTER YEARS OF CONSIDERATION OF THE MATTER, I AM 100% CONVINCED THAT THE DESIGNATION OF ACTUAL TYPE SPECIMENS OF FOSSIL SPORES AND POLLEN IS PRETTY MUCH FUTILE--SURE IT WOULD BE TECHNICALLY POSSIBLE TO MAKE THE SORT OF PREPARATIONS YOU WOULD REGARD AS SUFFICIENT, BUT PEOPLE AREN'T GOING TO DO IT. PERIOD. YOUR IDEA OF DESICCATING TYPE SPECIMENS COULD ONLY APPLY TO PREPARATIONS ESPECIALLY MADE FOR THE PURPOSE. IF ONE IS A GLYCERIN JELLY USER, HE/SHE ISN'T GOING TO DO ANY SUCH THING. GLYCERIN JELLY PREPARATIONS CANNOT BY DEFINITION CONTAIN DESICCATED SPECIMENS, AS WATER IS ONE OF THE INGREDIENTS OF GLYCERIN JELLY.

I WILL CONTINUE TO OBEY THE LETTER OF THE LAW--SENDING SPECIMENS OFF SOMEWHERE THAT IS LEGAL. THAT IS ALL I PROPOSE TO DO. IF THE RULES ARE CHANGED, I WILL FOLLOW THEM. TO THE LETTER, AND NOT ONE MILLIMETER BEYOND.

>

>Well, on with the fungal atlas.

> See you again soon, I hope, WELL, MAYBE IN ST. LOUIS, THOUGH YOU HAVEN'T SAID MORE ABOUT IT LATELY. MY SISTER'S HUSBAND HAS HAD SOME VERY BAD DAYS WITH HIS PARKINSON'S LATELY AND IS IN THE HOSPITAL. I SUPPOSE I MIGHT HAVE TO GO TO S. L. BEFORE THE CONGRESS. IF MY POOR BROTHER-IN-LAW SURVIVES BUT HAS TO GO TO AN ASSISTED LIVING FACILITY, THAT WILL SOLVE MY HOUSING PROBLEM FOR AUGUST. WHO KNOWS. IT'S ALL VERY HARD ON KID SISTER. AND ON POOR CHARLES, OF COURSE. HE HAS A FAMOUS NIECE, HIS BROTHER'S DAUGHTER, ONE KATHLEEN TURNER, A MOVIE ACTRESS. I NEVER GO TO MOVIES, BUT MY KIDS TELL ME THAT SHE IS CONSIDERED GOOD.

BEST WISHES. AL.

Delivered-To: traverse@ems.psu.edu
From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: "'TRAVERSE@EMS.PSU.EDU'" <TRAVERSE@EMS.PSU.EDU>
Cc: 'Al' <alandbetty@juno.com>
Subject: FW: fungal spores
Date: Thu, 11 Feb 1999 16:05:55 -0500
X-Mailer: Internet Mail Service (5.5.2232.9)

HI AL,
DO YOU MIND IF FOR ONCE I INTERSPERSE MY COMMENTS IN YOUR TEXT? THIS SURE WAS GLAD TIDINGS.

> ----------
> From: Alfred Traverse[SMTP:traverse@ems.psu.edu]
> Sent: Thursday, February 11, 1999 1:01 PM
> To: jjansoni@nrcan.gc.ca
> Subject: fungal spores
>
> Hi, there--
>
> Well, it turned out that I had indeed preserved a clearly labelled box
> with
> the Hells Creek slides, when thousands of others were thrown out in
> July.HALLELUJAH, BROTHER! SHURE THREW A SCARE MY WAY. The reason was
> clearly that they were labelled types slated to go to Pittsburgh. A slide
> has a clearly labelled pointer indicating holotype of
> the item in question. I no longer have a microscope here, however, so I
> will take it all home and at the next opportunity (probably Friday) I will
> make a series of polaroid photos of the thing for you and send them off to
> you immediately. THAT WOULD BE VERY MUCH APPRECIATED.
> I guess because of further flak I might take I'd better send the slides
> themselves off to Carnegie as soon as I finish that exercise. However, I
> have a couple of comments about the situation. Jim King, a palynologist,
> who helped me get the Carnegie designated as the official AASP-approved
> repository for type and illustrated specimens (Canada, ahem,
> wouldn't go along) FROM YOUR APPARENT MISGIVINGS ABOUT THE CURRENT
> SITUATION THERE, YOU SHOULD BE MORE APPRECIATIVE, NOW, OF THE CANADIAN
> POSITION; JUST ABOUT ALL PALYNOLOGISTS HERE HAVE SENT THEIR TYPE SLIDES
> TO THE GSC. THERE IS A PERMANENT CURATOR ON STAFF IN OTTAWA, WHO
> NOMINALLY IS IN POSSESSION OF ALL THE SLIDES. HOWEVER, AS THERE IS NO
> LONGER A PALYNOLOGISTS IN OTTAWA (AND IT IS UNLIKELY THAT THE Y WILL HAVE
> ANOTHER ONE THERE), THE SLIDES ARE NOW IN A SPECIAL CABINET IN CALGARY.
> THE Y ALL HAVE BEEN INSPECTED FOR DAMAGE, NEED OF REPAIR, ETC. AND ALL
> TYPES HAVE BEEN LOCATED, AND PROVIDED WITH AN ENGLAND FINDER LOCATION.
> [jim king] is no longer at Carnegie, having become director of the
> Cleveland Natural History Museum some time ago. Whether there is ANYBODY
> at Carnegie who will do anything with these slides other than chuck them
> in a corner somewhere is anybody's guess. I WOULD SUGGEST THAT YOU FIND
> OUT WHAT THEIR SET-UP IS. DO THEY MAINTAIN THE SLIDES, INSPECT THEM, HAVE
> THEY A MICROSCOPE ALLOWING VISITORS TO EXAMINE THEM, ETC. IF THEY HAVE NO
> DESIRE TO KEEP THEM ON A PERMANENT BASIS, THEY/WE MIGHT CONSIDER ANOTHER
> AVENUE -- MAYBE SMITHSONIAN? TUCSON? USGS/DENVER?MAYBE THIS TOPIC SHOULD
> BE RE-EXAMINED IN THE AASP NEWSLETTER? MAYBE YOU SHOULD CONSIDER THE GSC
> IN CALGARY? [no, i'm not facetious. i got to know and appreciate this
> place]
COULD YOU GIVE KING A PHONE CALL WHERE HE IS NOW, AND ASK HIM FOR AN EVALUATION OF THE SITUATION THERE?

> Furthermore, in 50 years of palynology I have NEVER used any mountant
> other than glycerin jelly, because of its terrific optical qualities and
> ease of
> handling, and I can assure you that slides containing my types from 1951
> almost certainly are now totally useless. IT IS A MYTH THAT GLYCERIN
> JELLY SLIDES CANNOT LAST. WE HAVE SLIDES DATING BACK FROM THE EARLY 1950s,
> AND THEY ARE FINE -- IN FACT MAYBE BETTER THAN THE CANADA BALSAM ONES
> (BECAUSE THEY START TO TURN YELLOW). HOWEVER, THEY MUST BE SEALED
> PROPERLY. THE OLD WAY TO DO THAT WAS BY APPLYING A RING OF ASFALT/TAR
> AROUND THE EXTERNAL EDGE OF THE COVERSLIP. HOWEVER, THE MODERN METHOD IS

> TO PREVENT THE GLYCERINE JELLY FROM REACHING THE OUTER EDGE OF THE
> COVERSLIP, AND THEN SEALING IT FROM THE AIR BY RUNNING SOME WAX UNDER THE
> EDGES OF THE MOUNTED COVERSLIP -- ON A WARM HOTPLATE -- THIS MAKES THEM
> VIRTUALLY PERMANENT. I SUGGEST THAT YOU DO THAT NOW, EVEN IF THE JELLY
> HAS SHRUNK BACK ALREADY. HOPEFULLY, THE JELLY HAS PULLED BACK CLEAN FROM
> THE GLASS, SO THAT THE WAX WILL INDEED PROVIDE A TIGHT SEAL. BUT, IF IN
> DOUBT, I SUGGEST YOU LET WAX PENETRATE AS FAR AS IT WILL GO, THEN CLEAN UP
> THE EXTERNAL SURFACES OF COVER SLIP AND SLIDE, AND FINALLY PAINT A RING OF
> NAIL POLISH AROUND THE COVERSLIP. THIS SHOULD KEEP THE MOISTURE IN AND
> OXYGEN OUT. IT SHOULDN'T BE A LOT OF WORK, AND SHOULD HOLD THE SLIDES IN
> WHATEVER CONDITION THEY ARE IN NOW.
> I'm almost tempted to go to Harvard one of these days and check it out.
> WHILE YOU ARE THERE, YOU CAN PROPOSE TO TAKE THE GLYCERINE SLIDES HOME,
> FOR ANY NEEDED REPAIRS. HOWEVER, IF THEY PRETEND TO HAVE FACILITIES FOR
> PERMANT CURATION, THEY SHOULD BE PREPARED TO DO THAT MAINTENANCE WORK
> THEMSELVES.
AND, IF THE FACILITIES THERE ARE SURVIVING THE MOVE OF KING, MAYBE IT WOULD BE A GOOD IDEA TO ONCE MORE ADVERTISE THEIR SERVICES IN TH EAASP NEWSLETTER. THESE DAYS, A LOT OF TYPE SLIDES MIGHT DRIFT INTO A PERMANENT LIMBO.
> These Hells Canyon slides are about ten years old and are already on the
> way out. But there is nothing in ICBN that tells me not to use glycerin
> jelly, or silicone oil, for that matter. NO. BUT THEN, IT DOESN'T SAY
> THAT YOU SHOULD NOT SEAL A GLYCERINE JELLY SLIDE, EITHER. FOR CRYING OUT
> LOUD, WE SHOULD HAVE LEARNED SOMETHING SINCE THE DAYS THAT ROUSE MOUNTED
> HIS SLIDES IN CORN SYROP, AND THEN STORED THEM STANDING ON EDGE...
> You folks who are so hot for actual type specimens ought to get it
> decreed that the mountant has to be so and so, size and thickness of
> slides
> and coverslips so and so, etc. Fact is, permanent preservation of these
> little guys is a will-o'-the-wisp and won't happen.
>
> I find it entertaining that you were quite willing to decree Jizba's
> Complexisporites a synonym of Illinites, based solely on examining the
> lady's PICTURES. That's in fact the way that 99.9999% of palynological
> comparisons are made. AL, I HAVE NOTHING AGAINST PICTURES. INDEED, A GOOD
> PICTURE SAYS MORE THAN A DESCRIPTION OF A THOUSAND WORDS. I COULD -- IF I
> WANTED TO -- EASILY TURN THIS AROUND, AND POINT OUT THAT IF YOUR PICTURE
> HAD BEEN CROPPED LESS AGRESSIVELY, I WOUDN'T HAVE SUGGESTED TO GO TO THE
> TYPE SPECIMEN. AND WHAT ABOUT MALJAVKINA'S HELPFUL ILLUSTRATION OF
> COROLLINA?
MAYBE THE ICBN SHOULD MANDATE BOTH (WHICH IT DOES, RIGHT NOW)!

> It's also funny that the master illustrator of fossil spores, of all
> people, doesn't see the advantages of the permanence and thus
> dependability of illustrations of the marvellous quality that you produce.
> WHAT CAN I SAY BUT -- THANK YOU.
> Hang in there, watch your mailbox for photos of the specimen, presuming
> it's still photographable. I TRUST THEY WILL BE. EVEN IF IT GETS CLOSE, A
> BIT OF WARMING, OR ADDITION OF A DROP OF GLYCERINE MIGHT HELP AS A FIRST
> AID BANDAGE.
YES, I'AM LOOKING FORWARD TO THOSE PICTURES.

> Please watch the insulting remarks. I DID WATCH FOR THEM, BUT DIDN'T FIND
> ANY.
>

------- Al

> However
> Alfred Traverse
> Palynological Labs.
> Department of Geosciences
> 307 Deike-PSU
> University Park, PA 16802
> Ph.:814-863-3419

To: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
From: Alfred Traverse <traverse@ems.psu.edu>
Subject: Re:
Cc:
Bcc:
Attached:

Dear Jan:

First, I deny vehemently that I am a buck passer. That is B. S. Not my character at all. I think that was a nasty remark, or series of remarks. The type slides for the T&A paper may have been sent to Carnegie. I am not sure. I certainly intended to do so. I will check. On the other hand, they may have been accidentally chucked, along with much else on those fateful days in July when it was touch and go if we could make the deadline, even with Betty and two grandsons helping. Same goes for the original plate for the paper. I remember deciding that one whole filing cabinet of such stuff had to be cleared out, and the contents trashed. After all, the papers were published. Betty wanted to throw out all correspondence more than five years old, but I drew the line on that, and that stuff is in the basement storage area of our State College condo. Came in handy just recently in connection with another T&A paper I just wrote--Sid's 1984 (or so) letter with data about the samples I was studying turned out to be critical, and we were able to find it.

Re the type specimen vs. illustration question, I favor the latter because the former doesn't prevailing work, period, despite occasional success stories such as yours re Illinites. I picked up your innuendo on that one, of course--see!--it really works! Gosh!

As I thought you know, we live in the country miles from neighbors of any age.
In any case, I have no real interest in the internet if that is the correct term, and mildly resent the fact that PSU has put me and (apparently, I've never seen it) a photo of me on it. I have plenty to do without fiddling around with that, but I admit that it is getting to me that various correspondents just assume one is into it. I mean it is being assumed that if one does e-mail one also does the other stuff. I might go back to just ordinary mail. Of course, with no phone privileges here at the university (except for local calls) I really need e-mail. As far as I can tell, I don't need any of the other aspects of the internet. If you are getting a big kick out of it, that's fine, and I admire you for it. By the way, there is a related matter that really bugs me. Across this whole continent it is no longer possible to use a library unless one has computer savvy. Yesterday I went to a session in the main library that was supposed to be an "introduction" to how to use the computer-based files of the libraries at Penn State. I was, of course, the oldest person of the 30 who had signed up, by about 50 years. The presentation began at a level (each person had a--I think this is the right term--terminal) that was approximately 2 km. over my head, and I left immediately. I'll go back to having a librarian do it for me.

More later about search for that plate and slide (s).

Absolutely-not-a-buck-passer

your offer re the IAPT website (whatever that means) is kind. However, from my membership on the Committee on Orthography I have friends who have been seeing that I get enough of that stuff. I just hope that the whole IAPT doesn't blow up over this. I suppose Werner would take that as an invitation to found a new organization, substitute his new Code for the ICBN and declare a "pox on all of you."

Delivered-To: traverse@ems.psu.edu
From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: 'Traverse' <traverse@ems.psu.edu>
Cc: 'Al' <alandbetty@juno.com>
Subject:
Date: Mon, 8 Feb 1999 15:26:28 -0500
X-Mailer: Internet Mail Service (5.5.2232.9)

Hi Al,

Che sera, sera -- I guess.

You complain about LSU vs PS -- and that has been a most unfortunate (and widely regretted) thing where the naives got taken to the cleaners by the wolves in sheepskin. But that is water under the bridge.

However, I am disappointed that you didn't -- as you stated (on p. 667 of Traverse & Ash) you were going to do; you didn't say 'contemplated' to do -- deposit the slides in the Carnegie Mus. Nat. Hist. At Imperial Oil we always had our type slides in a separate small container, which was carried out to the Geol Surv. Can. before anything else was considered for chucking or keeping.
True, your exodus came relatively unexpected and with little notice. Let's just say we are all sorry for having goofed in some endeavours; let's not try to pass the buck always. (I understand better now why you tout figures instead of type slides...)

Might you still have the master of the 1994 plate (or: might Ash have that)? That presumably would not have been cropped yet.

As for computer savy -- I have the benefit of kids close by who are willing to get me out of pickles. However, if you take it step by step, and work at it a bit, you'll find that it doesn't take too much time to get the "feel" for how these programs work -- all in the same pattern.
You asked how come it is all for free. Well, if you go on the internet, where searches etc. are free, you are bombarded by all kind of (paid) messages advertising all kind of services and merchandise. You just have to ignore that, go straight to the little bits that do the work for you, and get the results. Much of the work is done by "search engines" like "yahoo" or "metacrawler" or "altavista"-- there are a dozen of those -- and in a matter of seconds you'll get answers. I found the full texrt of "Amahl and the night visitors", and the titles of the CD's by Mr. Feltsman, who plays J.S. Bach in a very special way. And the new thought on scoliosis.

Try to ask some of the neighbor's kid where you live to show you some ropes. They all play computer games that are operated in the same way as adult working programs. I don't edit photos, so as to import kingkong into a family snapshot. But there is very little trick to accepting, and showing on the screen, photos of family or spores. And tell Betty that those programs also can be downloaded from the net at no charge. Whoever sold you all your computers should be willing to show you a few tricks.

All for now; do you want me to try and insert in an e-mail all the news from the alternative IAPT web-site? It would be long, though. And it is so much easier to just click on a hypertext address (you know what that is?) and let the stuff roll onto your screen -- don't have to do anything more.
Or, you can connect to the Library of Congress and check their holdings for anything.

All for now, again. Best wishes and amical greetings,
Jan.

To: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
From: Alfred Traverse <traverse@ems.psu.edu>
Subject: Re: yrs. of 1 Feb.
Cc:
Bcc:
Attached:

Hi, Jan:

Down to Jizba, I haven't the slightest idea what you are talking about in your missive.

Nevertheless, I think I'll write to the address you gave, one of these days. California Research Corp. wrings a bell, but that's all.

Reminds me that the slides that contained the fungal spores about which you wrote are also God knows where. I have been looking for the stuff for that project. Probably all got pitched last summer when I got evicted with two weeks notice. I do know that literally thousands of slides were dumped in the dumpster at the back door. On that kind of warning, what could I do but pitch?
Probably included the types.

Speaking of your exploring with the computer, however this is done, I have BIG reservations about it. A cousin in BC just found me that way (Brent Traverse). I have never met him. His grandfather was my father's brother. The correspondence was going quite well until he wanted to send me pictures of his kids via computer, and I confessed I couldn't handle that. End of relationship. He had already drained me of all the genealogical info he was after. I know a bunch of Uncle Harry's (he was an interesting guy, professional WWI veteran--Black Watch, father of 12 kids) bunch have become Mormons, and I fear that he was after the family data for that nefarious stuff.
Brent also found both our sons via the "find" thing, based as I understand it on the availability of all telephone directories in the USA and Canada. We could do without this sort of instant information about everything. Privacy is a thing of the past, huh? Talk about Big Brother.

Who puts this stuff on the net, whatever that is, and what do they get for their efforts? Puzzling that anything really worthwhile is really free. I suspect there is a lot of garbage in the lot.

I don't have Juno at all; Betty does. She brags about it being totally free (unless we use it at home, in which case we pay for long distance phone from Huntingdon to Altoona), so I gather it must be the "basic."

I gather that it is possible with my new computer (this one) and the university set up that I am using for e-mail to receive "attachments," including pictures--not only the abovementioned Brent Traverse, but also my former student, Paul Strother, have tried to send pictures as attachments in the last week (P. S. has Cambrian spore tetrads). However, I have no idea how to do this, and I must add that emeritus profs. are NOT welcome in the college's computer office, especially when they ask the technicians "stupid" questions. I long ago discovered that it is best for me to stick to the simple things I have learned to do, such as typing this letter. If I try anything creative I usually end up having everything lock up on me, or some such. Private computer helpers want $100 and hour and expenses. I tried that once, and the guy answered my immediate questions and got me up again, but it didn't seem to me to be worth $200. I find the various books more or less inscrutable.

So, no, please don't send any attached files. I can't handle them, period.
If it isn't worth making a xerox and mailing it, it probably isn't worth the trouble, period.

Which reminds me that I had written out longhand at home (because I don't have e-mail connections for any of the three computers there) the following:

Dear Jan:

Did I ever respond re "holed in MS e-net software"?

I use 5 computers in our various stations to handle e-mail, type letters, and via MS Access the data files that used to be on 3x5 inch cards. Aside from those three things I am

computer illiterate. For that matter, I have trouble with all of these if anything unusual occurs--for example, somebody trying to send me a photomicrograph, or Judy Skog trying to send me something about ICBN via "attachment," of inadvertently hitting the wrong button in ACCESS--devilishly easy to do.

Re the IAPT flap: I have the impression that "democracy" is very good for social policy, but not so great for scientific organizations. They seem to work better if a highly motivated "in group" runs them autocratically. It is clear, however, that the former set of Dutch folk who ran IAPT were MUCH better at benign dictatorship than the current Swiss/Deutsch folk are. Stafleu et al. always made us all feel that we had a voice, even when we actually didn't. And they had a marvellous sense for what wasn't going over, and acted swiftly and so competently to change the direction--at least a little.

Greuter clearly should seek to placate the rebels and throw them some nice pieces of candy, while somehow preventing the IAPT from being run out of 3-4 different offices in as many countries.

This a. m. when I checked the mail there were two pages of faxed fungal spores, apparently from you. Came through pretty well. What am I supposed to do with them? Oh, yes, I find another e-mail from 1 Feb. that explains. Unless I can find the abovementioned slides, we have to go with what's in the publication, as that is in that case the only connection to reality. Too bad we didn't get that chair in palynology. Penn State would have given that deal a whole floor, and the thousands of slides and hundreds of samples (including most thesis collections) wouldn't have been ditched. They really wanted that chair here. At LSU they didn't preserve one thing, as George H. had already left palynology several years before. There wasn't even a palynology lab.

Enough for now. All the best. Al

Delivered-To: traverse@ems.psu.edu
From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: 'Al' <alandbetty@juno.com>
Cc: 'Traverse' <traverse@ems.psu.edu>
Subject:
Date: Mon, 1 Feb 1999 17:44:23 -0500
X-Mailer: Internet Mail Service (5.5.2232.9)

Hi Al,

You don't know what you are missing if you don't spend a few hours on getting informed about how the internet works. I activated the icon for "Netscape", clicked on "search engines", clicked on "People finder", typed "Jizba" in the slot, hit "enter", and up came the address of Zdenek Jizba, 1341 Rebecca Dr, La Habra, Ca 90631 -- by mere coincidence the same address as Katherine M.M. Jizba gave in her 1962 paper.
You can write, and see if granma is still mentally bright, or physiacally breathing.
The slides, she said, were curated at the "California Research Corporation" -- is that the outfit Srivastava used to work for? Are they still in business? Should someone look into that and make sure that their collections are not discreetly dumped as roadmetal? (Unless that happened already).

Anyway, you can do the weirdest things on the internet, without having to spend much time doing so. I don't "cruise" -- but when there are some medical problems in the family, I do go and check out what the doctors on the net have to say. There is a lot of new info that gets posted there, and often stuff that your family physician might not be familiar with, yet. You also can get, at the touch of a few keys, the ICBN, the latest info from IAPT, the various letters and comments that people have submitted to the "Alternative IAPT". I can try to send you some of those as 'attached files', but need to know if you can receive those: do you have the basic juno e-mail service, or the JunoPlus/Pro, the one that costs $2.95 per month?

All for now,

Jan.

Delivered-To: traverse@ems.psu.edu
From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: 'Al' <alandbetty@juno.com>
Cc: 'Traverse' <traverse@ems.psu.edu>
Subject: D. serratulus
Date: Mon, 1 Feb 1999 14:48:39 -0500
X-Mailer: Internet Mail Service (5.5.2232.9)

Dear Al,

Thank you very much for the reprints that arrived on my desk this morning. On our xerox copy, the grey rectangle around each photograph in Traverse & Ash was not visible, and the cropping was suggested, but the amount of it unclear. Now, looking at fig. 11, I am unsure how to interpret it.
In your description there is mention of "tooth-like projections at outside edge and within septa". The latter are seen in many spores (e.g. your figs. 8, 9), and easy to verify in fig. 11. However, both ends are cut off (prob. by the printer, who didn't consult you).
I am faxing you, as soon as this e-mail is sent off, a copy of our plates with Pluricellaesporites spp, and Diporicellaesporites spp. All these figures are being scanned, and reprinted at a uniform 700x magnification. That way, it will become clearer what role size plays in the systematics of these things.
I now am waffling to which group your D. serratulus should belong. From fig. 11, I can believe that the upper edge of the spore is a thinner, but complete (i.e. closed) and rounded spore wall, as seen in P. serratus, or P. heterospora (top row, 2 and 4 from left). If that is so, it should go into Pluricellaesporites.
In our drawing of D. serratulus (row 3, 3 from left) we show the (postulated) looks of the hilum at the base. The slightly angular feature (visible in your photograph as a faint bright line) we interpreted as part of the outline of the basal "pore" or hilum. However, now that I see your photo, I wonder if it is a very thin septum, segregating a pore chamber just above the actual hilum itself (which may be closed by a septum, or may be open; or, even, may be a septum with triangular flaps -- how would I know?).
In our drawing, the artist showed two extensions of the spore wall at the top (her interpretation of the words "disk with teeth projecting out from spore body") -- although this now doesn't look like a miniature version of the septal folds, or flaps, visible at the internal septa. Do these "teeth" occur at the top or the bottom of the spore?
Did you already send the slides to the Carnegie Museum? If not, could you have a look at it, and make a sketch of what you see at both ends -- the stuff that we kind-of visualised from your description and our best guess? If you stored the master of the plate, or a spare print of the type, that might suffice to guide you. If you cannot locate those, would the Carnegie send out slides for examination?
One wonders, how important can a single itsy-bitsy fungal spore be to go to all this trouble? Then again, we have had similar correspondence with several palynologists/mycologists. We are trying to make our Atlas a dependable source, where some of the existing gaps in our collective knowledge are filled in.

I am looking forward to your reply in this matter. We need no elaborate sketch, just an indication where we guessed wrong (I now think we did), and what the basic bauplan of the top end, and of the hilate bottom end, of this spore looks like. That could go by fax, I guess.

Thanks again, and best regards,
Jan.

To: jjansoni@nrcan.gc.ca
From: Alfred Traverse <traverse@ems.psu.edu>
Subject: fungal spores
Cc:
Bcc:
Attached:

Hi, there--

Well, it turned out that I had indeed preserved a clearly labelled box with the Hells Creek slides, when thousands of others were thrown out in July. The reason was clearly that they were labelled types slated to go to Pittsburgh. A slide has a clearly labelled pointer indicating holotype of the item in question. I no longer have a microscope here, however, so I will take it all home and at the next opportunity (probably Friday) I will make a series of polaroid photos of the thing for you and send them off to you immediately. I guess because of further flak I might take I'd better send the slides themselves off to Carnegie as soon as I finish that exercise.

However, I have a couple of comments about the situation. Jim King, a palynologist, who helped me get the Carnegie designated as the official AASP-approved repository for type and illustrated specimens (Canada, ahem, wouldn't go along), is no longer at Carnegie, having become director of the Cleveland Natural History Museum some time ago. Whether there is ANYBODY at Carnegie who will do anything with these slides other than chuck them in a corner somewhere is anybody's guess.

Furthermore, in 50 years of palynology I have NEVER used any mountant other than glycerin jelly, because of its terrific optical qualities and ease of handling, and I can assure you that slides containing my types from 1951 almost certainly are now totally useless. I'm almost tempted to go to Harvard one of these days and check it out. These Hells Canyon slides are about ten years old and are already on the way out. But there is nothing in ICBN that tells me not to use glycerin jelly, or silicone oil, for that matter. You folks who are so hot for actual type specimens ought to get it decreed that the mountant has to be so and so, size and thickness of slides and coverslips so and so, etc. Fact is, permanent preservation of these little guys is a will-o'-the-wisp and won't happen.

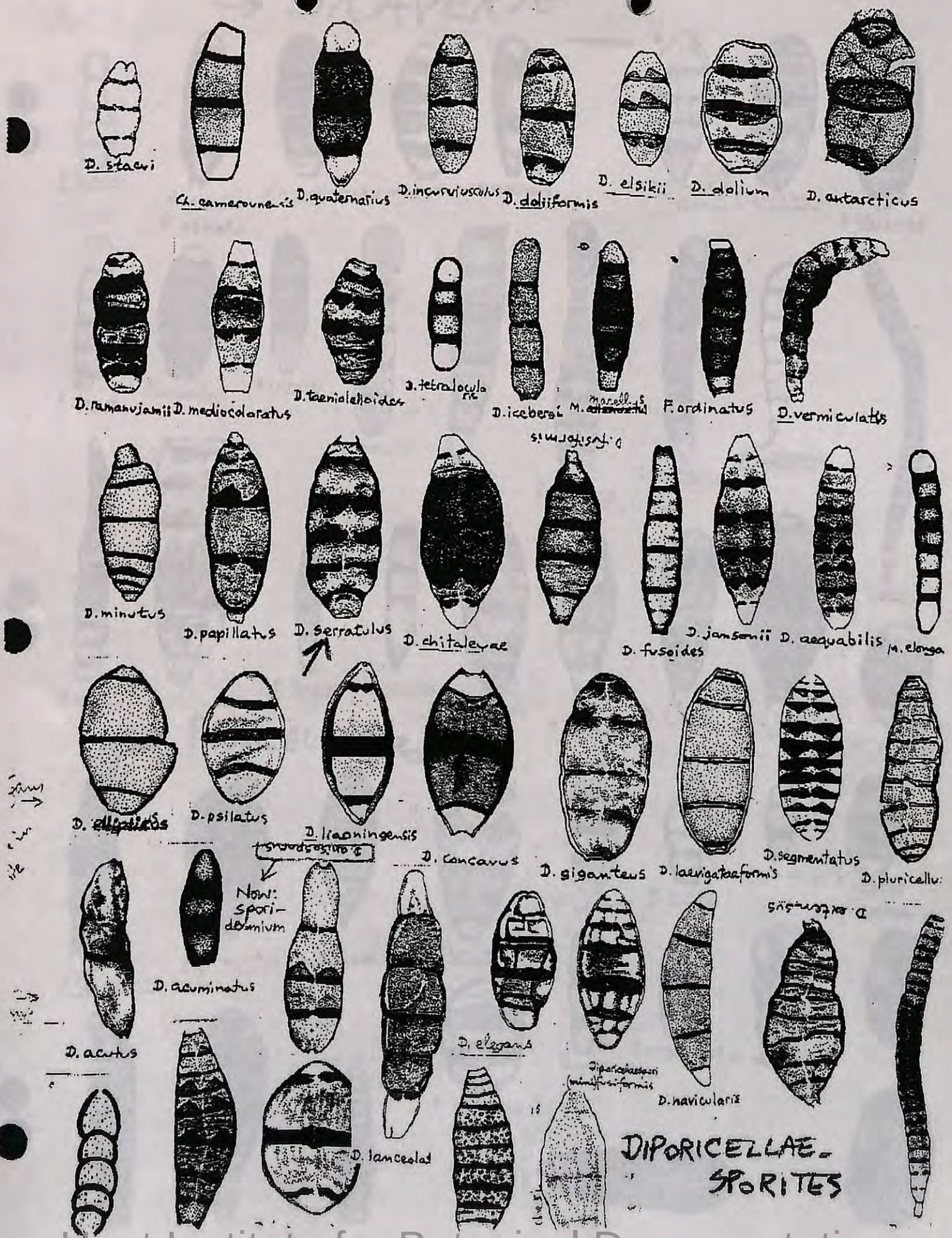
I find it entertaining that you were quite willing to decree Jizba's Complexisporites a synonym of Illinites, based solely on examining the lady's PICTURES. That's in fact the way that 99.9999% of palynological comparisons are made.

It's also funny that the master illustrator of fossil spores, of all people, doesn't see the advantages of the permanence and thus dependability of illustrations of the marvellous quality that you produce.

Hang in there, watch your mailbox for photos of the specimen, presuming it's still photographable.

Please watch the insulting remarks. Al

However



Delivered-To: traverse@ems.psu.edu
From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: 'Traverse' <traverse@ems.psu.edu>
Subject: RE: FW: Disaccites
Date: Fri, 29 Jan 1999 15:16:05 -0500
X-Mailer: Internet Mail Service (5.5.2232.9)

Hi Al,
Seems like our e-mails keep crossing like ships in the night.Yes, I did find the scale bar, and made my measurements from that. My question about figuring out the number of cells: the numbers you gave for number of cells and number of septa didn't seem to jibe -- one has a spread of two, the other of three. When I checked, I didn't have a record of having received Traverse & Ash. I'll look forward to your replacement reprint.

I agree with you that, if I were producing the Genera File now, I would be more organized, and systematic, in setting it up, and also provide formal justification for some of my comments that now often are sprinkled about. Of course, in those days every draft had to be typed from scratch, and I kept things as much to a 8.5x11 page as possible.

I never did meet Jizba, and cannot remember the scope of correspondence we had, nor have I any knowledge of her present whereabouts. Have you tried the "People finder" on the internet? There can't be all that many Jizbas., I would imagine. Whatever -- her illustrations are good, and leave little questions to be asked. What I was surprised to observe, though, was the ring furrow in the type of Illinites. Kosanke maintained that it wasn't there. I think you may well be right that there is a complex of morphologies (Bauplaene) incorporated in Jizba's material; I never had the material to be faced with that problem, so I cannot help you much, there.

Yes, I plan to maintain the Genera File for a while, yet (and eventually hand it off to Hartkopf-Froeder). However, there are more and more people asking for it to be posted to the internet, or made available in electronic form. That of course would give an opportunity to clean up many boo-boos, but it also would cut into a shrinking market, and rob us from the cash flow we need (certainly now that Hills also retired, and has no longer a hand in Department funding) to produce an "effective" publication in which nomenclatural adjustments can be made. Any suggestions in that matter? Also, not everyone has a PC that can handle fine line drawings.

The IBC things are primarily: Judy Skog's Official Circular Number 1: Committee for Fossil Plants -- January 1999.
I also am reading all proposals published in Taxon in the last two years, making notes as I go on how I want to vote on them. And, there are the matters addressed on the Alternate IAPT Web Page, administered by Vicky Funk, which I think are important enough to be considered seriously. I have not registered yet, but am leaning to attending this IBC, in order to be there to vote. What kind of accommodation would be available there? All very expensive, or also B+B?

I hope I answered all your questions. So long, Jan.

To: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
From: Alfred Traverse <traverse@ems.psu.edu>
Subject: RE: Complexisporites
Cc:
Bcc:
Attached:

Greetings, Jan:

You arose my curiosity. What "nomenclatural matters relating to the next IBC" would you have other than those that I also have?

You did get my explanation re the Traverse & Ash paper? I also have sent you a reprint, though your card said you'd been sent one previously.

Re the matter in your subject line: your comment is interesting, but I think the statement on the card that it is a junior synonym required more explanation than what was given and should then have been signed: JJ or J&H, or whatever. Also, even now I'd like to know if you also saw Jizba's material. As I believe
I said in my earlier message I have a different idea. It looks to me from Jizba's illustrations that her Complexisporites is a mixed bag, and it is probably correct that her designated type is Illinites-like. However, there are other specimens in her large array of illustrations that look like what I have, and I don't think it is Illinites. Of course, that doesn't change the fact that C. would be a synonym because of the type, but I feel at the moment that the situation is more complicated than equivalence of Jizba's whole complex with Illinites. So, it would be important to me to know if you also had Jizba's slides.

Do you know whatever happened to Jizba?

Al

Delivered-To: traverse@ems.psu.edu
From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: 'Al' <alandbetty@juno.com>
Cc: 'Traverse' <traverse@ems.psu.edu>
Subject: FW: URGENT: IAPT BALLOTS
Date: Fri, 29 Jan 1999 13:23:46 -0500
X-Mailer: Internet Mail Service (5.5.2232.9)

Al,
You may already have received this e-mail. Just to be sure, I pass it on to you. It seems that the IAPT is being run now in the same mode as the IOC, with no true input by, or consideration for opinions of, a large part of the membership.

I also will forward to you an e-mail that my son-in-law sent me yesterday. It seems that MS has engineered some big holes in the security of their e-net software; there are some patches available. I believe that Netscape (using the true version of Java) is much more secure. However, I don't know enough about computers to teach anybody. Still, you may have your own sources that you may want to discuss this with.

Yesterday I forgot to say that, yes, certainly, I am happy to receive typo's and any other errors that you may find in the Genera File.

Best regards in the mean time,
Jan.

> ----------
> From: Vicki Funk[SMTP:Funk.Vicki@NMNH.SI.EDU]
> Sent: Friday, January 29, 1999 10:37 AM
> To: jjansoni@nrcan.gc.ca
> Subject: URGENT: IAPT BALLOTS
>
> International Association for Plant Taxonomy (IAPT) ballot papers
> have been distributed without some nominated candidates supported
> by a large number of members being included. It is urged that
> these ballot papers be not completed but sent to the address in
> Copenhagen given, with a request for a NEW BALLOT. See the IAPT
> Alternative Website for more details and a history of these
> issues: http://mason.gmu.edu/~ckelloff/vfunk/
> Comments to be posted on the website will be welcome, send them
> to hollowell.tom@nmnh.si.edu
>
> PLEASE circulate this among colleagues
> in your institution and region.
> _____
>
> A Statement from Three British Institutions
>
> The following statements are made by members of the International
> Association for Plant Taxonomy at the Natural History Museum
> London (BM), the Royal Botanic Garden Edinburgh (E) and the
> Royal Botanic Gardens Kew (K).
>
> 1. We deplore the refusal of the present Officers of the
> Association to accept the nomination of Patricia Holmgren for
> either Secretary or Treasurer in the forthcoming election of
> Officers.
>
> 2. We note that this decision is in defiance of a petition posted
> on the IAPT Alternative Website by over 400 taxonomists, very
> many of whom are members of the Association, who have
> supported her nomination as Secretary. We know that many
> private letters have also been sent to the present Secretary
> urging him to accept all nominations, including a letter from
> our three institutions sent last year.
>
> 3. We regret the fact that the Secretary has continued to make

> his own nominations for Officers and Council members long
> after the deadline of 31 July 1998.
>
> 4. We regret the insult to the New York Botanical Garden implied
> in the Secretary's questioning whether that institution is
> capable of running the Secretariat. In our opinion it would
> offer an ideal home for the Sercretariat, and we believe Dr
> Holmgren is a very suitable person to take charge of the
> affairs of the Association as Secretary.
>
> 5. We strongly dispute the view apparently expressed repeatedly
> by the present Secretary that the posts of Secretary and
> Treasurer must be held by the same person, or by two persons
> from the same institution, and indeed we take exactly the
> opposite view. We note furthermore that the Secretary has no
> right to offer the editorship of Taxon after this year to
> anybody else, for that appointment must be made by the
> decision of the next elected Council.
>
> 6. We urgently call for the placement of Dr Holmgren on the
> ballot forms as candidate for Secretary in the forthcoming
> election. If this is not done we will call for a rejection of
> the ballot forms and request a new ballot. We invite the
> support of others.
>

To: Jan Jansonius <jansonij@cadvision.com>
From: Alfred Traverse <traverse@ems.psu.edu>
Subject: Re: Disaccites
Cc:
Bcc:
Attached:

Hi, Jan:

Thanks for the info on Disaccites. I find it very disconcerting when an author presents a list of (italicized) genus and species names, and lists, alphabetized with the rest, also italicized: Disaccites striatiti.

Yes, I have a cubicle in the Deike Building, and they "gave" me a replacement for my preparation lab, so I still am here fairly regularly--at least a couple of times a week. I need to have a base here at the university for various reasons. My main headquarters now, however, is in the office I have had built at home in the country. One reason you haven't heard from me sooner is that we've had terrible ice storms, and my home over there is practically cut off from the outside world. We have run out of fuel oil, and the oil truck can't get up to the house, so I am without heat in most of the house, and no hot water (I have enough residual electric heat in place to keep from having frozen pipes, etc., but it requires constant monitoring.) Betty bailed out ten days ago and lives here in S. C. in our condo, where she also has her office. Yesterday she went over to Hunt. to help me with a table for the MS I was working on--that is beyond my computer expertise. On the way up in her 4x4 Explorer she skidded off the lane on the ice, and I spent the next two hours fetching pails of crushed rock until I managed to get the vehicle up to the house. Meanwhile she did the table, made the required diskette for the MS. Then we both came to S. C. for a nice dinner at our favorite Chinese restaurant, and I got to take a shower.

On 11 Jan. I got a missive from you about Traverse & Ash 1994. I never get around to mass mailings of reprints, but when I do send out such, I record that info on your card in my address file. You were sent a copy of paper 62, which is that one, according to the card. Just in case something went wrong, however, I am sending off to you forthwith another reprint of the paper, along with copies of 71, 72, 73, and 74. Maybe you chucked your reprint collection?

Regarding the size question, that is easy to answer. The original photos were precisely 1000x. However, a scale bar is very prominently displayed. The bar is equal to 50 micrometers and it measures exactly 50 mm. So, the publishers in this case, for once, kept the magnification precisely at 1000x. You can therefore measure with a millimeter scale. Neat.

It is a little distressing to note that Fig. 11 is indeed cropped too close at top and bottom. That is not my style, and I don't remember how that happened. I presumably would have counted that specimen as having 7 cells and 6 septa. If there was a septum that I could identify as such, I counted it. I doubt that I would have counted the indistinct v-shaped structure that you observe in the photo as a septum.

One of these days I am going to open the package of supplemental J&H cards. I finally do have the rest of my J&H collection nicely shelved in the new office, so I am motivated. Are you still (I hope) accepting lists of boo-boos? I found a place just yesterday where Potonié is spelled Poronie.

Back to work. Best. Al.

Delivered-To: traverse@ems.psu.edu
From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: 'Traverse' <traverse@ems.psu.edu>
Cc: 'Al' <alandbetty@juno.com>
Subject: Traverse & Ash
Date: Mon, 11 Jan 1999 17:01:54 -0500
X-Mailer: Internet Mail Service (5.5.2232.9)

Dear Al,

First as yet best wishes for the new year to Betty and yourself.
I hope you have overcome the contretemps that plagued you while installing yourself at home.

I am finishing up a compilation of fossil fungal spores. I am now looking at a linedrawing we had having made of Diporicellaesporites serrulatus Traverse & Ash 1994. It showed two extensions of the wall up beyond the top of the specimen as on your plate. I think this was th edraftswoman's interpretation of th etext mentioning "toothlike projections".
Ram Kalgutkar (with whom I do this) has only a xerox of your paper, and the library lost their copy of this issue of JofP. I didn't find a reprint, either.
Maybe you can help me: As I interpret the xeroxed plate, the specimen is essentially truncated at the top, but the lower edge of the grain is slightly distorted (askew), and the final cell wall (septum, if you will, although there is no further cell below it) is cracked open and shows flaps, just like most of the internal septa in this specimen show. Only, because it is an end cell, the "septum" is very thin, and the "teeth" of the septal flaps are very thin. They do form, however, a small inverted "V" shaped notch at the bottom of the specimen. Can you confirm?
A propos, you give the species as possessing 6-9 cells, and 5-7 septa. How do you calculate that? Are the final cell walls (top, bottom) counted as "septa"? Everytime, or not always?
I take it that the plate shows the photos at 1000x magnification? (There are no total dimensions given in the descriptions)

Best regards and wishes Jan.

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

27 October, 1998

Dr. Jan Jansonius
11 Stanley Place SW
Calgary, Alberta T2S 1B2
Canada

Dear Jan:

At the moment I am without e-mail capability at the PSU office, where I am working (perhaps to be straightened out today--I'm having a new computer constructed in town). So, I resort to snail mail as I work on the backlog of correspondence.

I am still not straightened out following my July eviction--latest glitch is that the shelving for the home office, for which I paid $2500, turns out to be defective and must be 100% reconstructed. All books--including some I had just managed to "find"--must come off the shelves, etc. And now the department head tells me that they are going to seize the palynological processing lab, which he had earlier said I could retain "for the indefinite future." The guy who got the million dollar a year grant (for 5 yrs.) for (I am not making this us) "An Astrobiological Institute" now says he needs the lab, as well as my office space, which he previously got. Losing the lab is pretty much of a disaster for my plans to continue doing palynology, but in the short run it also will be very time-consuming. Which pieces of equipment to try to save, hoping I can get set up elsewhere, what to chuck, what to give away, etc. To say nothing of the labor involved.

This letter, however, is to respond to the correspondence between you and Greuter regarding (mostly) your list of fungal spore homonyms. Werner's opinions are just what I would have written (less elegantly, more colloquially). I was somewhat startled to read that you even considered the possibility that an epithet published as a homonym could be "transferred" under that name to another genus. Perhaps you were pulling WG's leg.

The new batch of cards arrived at the height of the chaos in the home office and are still sitting on the desk there, unopened, under piles of unsorted things, as they came out of the packing boxes from State College (there were over a hundred such large boxes). Betty and I have resolved to take no extensive trips this winter, so when we are snowed in I should be able to do a lot of settling in at "Alphabet Arboretum." Including opening

the package of Jansonius cards.

Thanks very much for continuing to keep me informed.

Very truly yours,

Alfred Traverse
Professor Emeritus of Palynology

P. S. I was really teed off by the treatment that poster by Fensome, you and others, gave my proposal. I thought it was VERY unfair. Not a balanced treatment of the subject. Says that the illustrations could be bad but doesn't acknowledge that types can also be and are often "bad." To say nothing of non-existent or not-findable.

P. P. S. Just got a call from Master Computer in town. They have to postpone delivery again. The power company just turned off their electricity for non-payment. Hmmmm.

From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: '"Al"' <alandbetty@juno.com>, '"Hans Kerp"' <kerp@uni-muenster.de>,
'"Jan Willem"' <JANWILLEM@boev.biol.ruu.nl>,
'"Traverse"'
<traverse@ems.psu.edu>,
'"wrenn"' <glwrenn@lsuvm.sncc.lsu.edu>,
'"Christoph"' <hartkopf-froeder@mail.gla.nrw.de>,
'"Davis"'
<palynolo@geo.Arizona.edu>,
'"M.C.Boulter@uel.ac.uk"'
<M.C.Boulter@uel.ac.uk>
Subject: FW: IAPT: Responses to Dr. W. Greuter
Date: Thu, 27 Aug 1998 15:57:50 -0400

Dear friends,

I just received the forwarded message, and if you hadn't received it (or were aware of what is going on) you might be interested in persuing this further.
Personally, I find very encouraging the obvious lack of support for the newfangled ideas of NCU, Registration and Biocode.

Jan

> ----------
> From: Vicki Funk[SMTP:FUNK.VICKI@nmnh.si.edu]
> Sent: Thursday, August 27, 1998 1:13 PM
> To: JJansoni@nrcan.gc.ca
> Subject: IAPT: Responses to Dr. W. Greuter
>
> CHECK OUT THE WEBSITE - NEW POSTINGS ON THE 25TH AUGUST 1998
> INCLUDING ADDITIONAL LETTERS FROM MEMBERS AND A NEW HOT BUTTON
> TO SUGGEST CONSTITUTIONAL CHANGES.
> (http://mason.gmu.edu/~ckelloff/vfunk)
>
> In reply to the *Urgent Request Letter* Prof. Greuter has sent out a
> general letter. If you have not seen this letter you can check it on the
> website. Please note that we do not accept his arguments. Detailed
> responses follow.
>
>
> Funk responds to letter from W. Greuter - 14 August 1998
>
> Dear Colleagues:
> Many of you may have seen the recent response by Prof. Greuter as well as
> replies to this message. If you have not please take a moment to read
> them

> now (appended below).
>
> If it becomes necessary I will respond point by point to the response sent
> in by WG, but for now I prefer to deal with what I consider to be the two
> major issues of this and other comments. I will say, however, that we
> spoke with everyone quoted in the letter and they read drafts of the
> letter
> and verified everything that is in the current version of the letter that
> is posted on the web.
>
> First, it has been repeatedly said that the Secretary position is a huge
> job and one that requires lots of time and institutional commitment and
> that and no one else can do it unless they are prepared to give the same
> type of time commitment.
>
> While it is true that it is a big job the way WG has set it up it is the
> feeling of many of us that it does not have to be that way. Pat Holmgren
> and I, as well as several others, went over the constitution and bylaws
> several times and came to the conclusion that this aspect of the job could
>
> change. Someone else can do Taxon (until a few years ago it was edited at
>
> US or at NY). Someone else can be Rap. Gen. Someone else can be head of
> the Nominating Committee (for committees). Someone else can deal with the
>
> finances, etc. It is our opinion that the Secretary job can be reduced
> substantially and that is exactly what Dr. Holmgren wants to do. Give
> different jobs to different people all over the Association. So, yes, WG
> is correct it is currently a big job. However, we disagree with him on
> the
> issue of whether it must always remain that way.
>
> Second, I have now heard twice that this movement will destroy IAPT. I
> think it has the potential to change IAPT but I do not think it will
> destroy it. IAPT should emerge a stronger and more representative
> society.
>
> I think there is a lot of hand waving going on but it boils down to the
> question of "How do we want IAPT run?" I just received a couple of
> wonderful emails from long time members of IAPT who claim that IAPT has
> been hijacked and the members need to take it back. I agree. I say we
> let
> the membership answer that question by voting.
>
> One final note, it has been suggested by several members that what we
> really need to do is to change the constitution. We will be setting up a

> forum on the IAPT Alternative Website for suggested changes to the
> constitution. Please feel free to make suggestions.
>
> Vicki [V.A. Funk, Department of Botany, Smithsonian Institution]
>
>
> Brummitt to Funk in response to W. Greuter - 21 August 1998
>
> Dear Werner
> Thank you for sending me your circular letter on IAPT matters. I am
> sending the following reply to Vicki for inclusion on her website. You
> are
> of course welcome to reply in the usual way. Dick.
>
> Dear Vicki - I have just returned from a short vacation and find that IAPT
>
> matters are still being actively pursued. Among other things I find a
> remarkable and curious open letter of 14 August from Werner Greuter in
> response to our joint open letter of 10 August. You have already nicely
> distilled the essence of what this debate is all about in a reply to him,
> but there are one or two other points which may require comment in view of
>
> the outspoken wording used to describe our letter.
>
> There was nothing flawed or misleading in our letter, which was carefully
> checked for accuracy before being released. Werner Greuter offers bold
> and
> defiant words early on, but completely fails to substantiate his claims in
>
> his long and involved subsequent comments. Furthermore, it seems to me
> that his account of events is a very biased one. He makes no reference,
> for example, to the discussion he had with you at the IAPT (USA) meeting
> in
> Washington D. C. in July, when I believe you explained to him (via
> telephone conference) exactly what you were proposing except that you
> could
> not name candidates for Officer posts because the people concerned had not
>
> yet agreed.
>
> Only one point in our letter is directly challenged - that of whether
> there
> has ever been a contested election for the posts of Secretary or
> Treasurer.
> Our evidence for our statement on this was taken indirectly from Taxon 24:
>

> 703 (1975), where the report of the preceding election indicated that
> there
> had been three candidates for the post of Secretary and two for Treasurer.
>
> The fact that Stafleu received 823 votes for Secretary and Greuter and
> Merxmuller received only 1 each may well suggest that the voting was
> abnormal in some way, but the names are there.
>
> None of the other points in our letter is seriously contested, and Werner
> Greuter confirms that he is indeed not yet committed to accepting the
> Officer candidates you have put forward. This was the central point of
> our
> joint letter. The response from the Secretary certainly will demonstrate
> to many people the nature of the problems you are facing, and will
> completely justify in their minds the actions you are taking in alerting
> the membership to what is going on. The suggestion that New York and
> Missouri may not be able to provide sufficient institutional support for
> two of the important Offices will raise more than a few quizzical
> eyebrows.
> There surely should be no equivocation over this. But to my mind the best
> part of the letter is the discussion of democracy. We are told that we
> must accept democracy according to IAPT rules, not those generally
> accepted
> elsewhere! Readers of the saga so far will have found how the Secretary
> interprets democracy in IAPT, and may not recognise it as the same concept
>
> as they have in their own minds.
>
> Finally the appeal for information on why these issues have been raised
> for
> public discussion (the "aggressiveness and enmity" are figments of the
> imagination) may be touching, but the answer is very simple and has
> repeatedly been put to Werner Greuter already. If he had run IAPT in a
> truly democratic way, nobody would ever have complained. But the fact
> that he has repeatedly refused to consult the Association's elected
> Council, despite requests for him to do so, is simply contrary to our
> constitution. I have said this in my earlier letter to your website, but
> he has not responded to this or any of the other points I have made. I
> also said that no organisation should ever allow itself to be so dominated
>
> in its functions and finances by one or a very few people as IAPT
> currently
> is. I could equally have said that no Officer or small group of Officers
> should ever ask for carte blanche to do as they please with the functions
> and finances of an Association in the way that Werner Greuter has done.
> All we are asking for is a fair and free vote for the next Officers and

> Council by the whole membership, and we believe we have overwhelming
> support for this.
>
> Dick (R.K. Brummitt) 21 August 1998
> Herbarium, Royal Botanic Gardens Kew
>
>
> P. Lowrey responds to Greuter - August 18, 1998
>
> Dear Professor Greuter,
>
> I have read with great interest the open letter posted by Brummitt et al.
> on the IAPT Alternative Web site as well as your response. As a member of
> IAPT, I wish to express my reactions to you, as our Secretary.
>
> First, it is my strong belief that progress is accomplished through
> rational, reasoned decision making, and that excessive use of mud-slinging
> and strong language is counter-productive. I am thus both concerned and
> upset with the current tone of the exchanges now taking place between
> members of our Association.
>
> I will respond to the authors of the letter posted by Brummitt et al.
> directly to them. Here I wish to respond to YOU, with comments on several
> of the paragraphs you have written and shared with us.
>
> The explanation you provide in paragraphs (3) to (7) appears to be
> reasonable, and would indeed suggest that the interpretations presented by
> Brummitt et al. are at odds with the facts. However, nowhere do you
> suggest that it might be appropriate to hear what Pat Holmgren has to say
> about the matter. After all, she was on the other end of the phone line
> with you, is that not correct? Are her recollections of your conversation
> in agreement with those you have presented? It would be most helpful to
> know.
>
> Moreover, you indicate in paragraph 3 that "When the IAPT Officers held
> their annual meeting in mid-July, the date for submitting nominations (set
> too late due to an unfortunate oversight) had not yet expired."
> Unfortunate indeed. Is it not the responsibility of good management to
> ensure that the timing and sequence of such critical events are carefully
> planned? By holding the annual meeting in mid-July, the Officers
> effectively introduced an unnecessary and inappropriate bias in the
> process
> of drawing up the list of nominees to be presented to the members. It is
> certainly possible that this was unintentional, but the fact remains, and
> the consequences are now being seen, to everyone's dismay.
>

> With regard to democracy, you have clearly expressed your strong
> commitment
> to its principles. And it is indeed correct that the mechanism by which
> democracy is practised in different countries varies. Aside from the
> fundamentals of democracy, however, one must also consider the mechanisms
> and methods used to make democracy operate -- and I believe that it is
> with
> regard to these questions that concern and criticism has been expressed by
> several members of our Association. Without wishing to imply anything
> specific about the way IAPT has been run, it should for example be kept in
> mind that the governments of many "democratic" countries are fully
> democratic in technical terms, but operationally are everything but
> democratic. The institutions of democracy are in place, and the steps are
> followed to the letter -- but the processes are co-opted and distorted to
> ensure that certain outcomes are obtained. Thus, I feel that you would do
>
> well to respond not simply by re-affirming your commitment to democratic
> principles, but also by addressing the issue of the WAYS in which
> democracy
> is implemented within IAPT.
>
> You have effectively responded to the recently posted open letter by
> Brummitt et al., and have affirmed with force your commitment to
> democratic
> principles. I thus find it all the more surprising that you apparently
> have NOT responded to the opinions expressed earlier by Brummitt, a
> Council
> Member (see: A Frail and Easily Wrecked Institution,
> http://mason.gmu.edu/~ckelloff/vfunk/letter2.html), which would seem to
> raise some rather serious questions regarding the processes used to
> implement democracy within IAPT. The members of our Association would be
> well served by hearing directly from you on these important matters. It
> would have been far better to do so in the pages of Taxon, but it is now
> probably too late for that to be of value in the present context.
>
> In paragraph (9) you state that our Constitution "minimises the risk of
> geographical unbalance and limits the possible impact of pressure groups".
> This is certainly an appropriate goal, and one that all members clearly
> should support. However, many of our fellow members are specifically
> expressing concern that you and a number of your colleagues are in fact
> acting as just such a "pressure group" in the way in which you are
> handling
> such fundamentally important issues as the proposed Biocode and
> Registration of Plant Names. Clearly a mandate was given in Yokohama to
> develop and test a workable system for registration. Now, as the next
> Congress approaches, members of IAPT should be presented with a balanced

> and impartial assessment of what has been done, presenting information on
> the successes, the shortcomings, and the likely consequences if
> registration were to be adopted definitively in St. Louis. Moreover, the
> Secretariat should have a neutral position on these matters, and should
> avoid taking a posture that might be interpreted (even mistakenly) as
> suggesting that registration is in fact what we need and should want.
> Proponents should certainly be provided with a forum to express their
> opinions, but opponents should also, and the Secretariat has an obligation
> to deal with both groups in an even-handed and fair manner. It appears to
> a number of IAPT members, however, that the Secretariat is "championing"
> these controversial causes, and one can legitimately question whether this
> is appropriate. The criticisms that have been expressed by some of our
> fellow members may or may not be justified -- but they should in any case
> be addressed directly rather than being ignored.
>
> The argument you present in paragraph (10) did not impress me. It may be
> correct that the anonymous group "spearheaded by Funk" chose unilaterally
> to select a subset of potential nominees. But this in no way prevented
> any
> of the affected persons (or anyone else for that matter) from being
> nominated by a third party. No censorship occurred, because the "Funk
> group" has no control whatsoever regarding who will actually appear on the
> ballot that we as Association members will be receiving shortly. Please
> note, for example, that elections for deputies in the French National
> Assembly (France clearly being a democracy) are conducted by voting for
> lists of candidates presented by political parties, and the processes by
> which those lists are drawn up is not fundamentally different from the one
> that the "Funk group" utilized. Thus, in response to your question "Is
> this to be called democratic?", I think the answer has to be YES.
>
> No one can doubt the tremendous work load and responsibility that goes
> with
> the position of Secretary, nor the huge demand that it places on the host
> institution at which the Secretariat is based, and all members should be
> very grateful. Likewise, we have all benefited tremendously from the
> successes that the Secretariats have enjoyed in obtaining very substantial
> amounts of outside support -- both direct financial support and in-kind
> assistance. Low membership fees and low cost of producing Taxon are
> indeed
> very valuable, and they are only the two most visible advantages.
> However,
> I am less convinced by your argument that presenting the membership with a
> choice for the next Secretariat is a bad idea. Clearly any alternative
> proposal must be backed up by a sufficiently strong institutional
> commitment (which is almost surely the case with both Berlin and New
> York),

> and presumably the options presented to the membership would also be
> associated with at least somewhat different proposals for the WAY in which
> the Association will be managed (democratically, of course). I see no
> need
> whatsoever to maintain the previously used procedure of negotiating the
> matter in advance. Just because the matter has been handled this way in
> the past does not mean per se that we must continue to operate in this
> way,
> nor that it is the best way. If you feel that Berlin should not "run
> against New York", that is fine; the logical conclusion then is for Berlin
> not to present its candidacy. It would, however, be inappropriate to
> prevent New York' from doing so -- that WOULD be undemocratic!
>
> In paragraph (17) you indicate that you are "at a loss to understand the
> aggressiveness and enmity that transpires from the Anderson et al. text
> and
> from a few of the consequent reactions." I would like to suggest again
> that perhaps part of the answer may be found in the comments contained in
> Brummitt's note submitted for possible publication in Taxon (and
> rejected),
> and in the fact that no response appears to have been offered to IAPT
> members in any forum. No doubt you can reply to the serious matters
> raised
> by Brummitt. But by failing to do so you have, I believe, created the
> impression that you do not care -- and if that were indeed the case, then
> it would represent an attitude on the part of our Secretary that is wholly
> unacceptable. The criticisms may or may not be just or well-founded. But
> you should not be surprised that the level of concern is today increasing
> when you appear only now to be responding, after what looks like months of
> indifference. After all, this is a democratic Association, and our
> officers (generous as you are with their time!) should nevertheless be
> accountable and should devote special attention to responding to questions
> and concerns expressed by members (especially Council members!) --
> something that a number of persons obviously feel you have perhaps not
> done
> as well as you should have.
>
> Finally, I would like to comment on the issue of financial reporting.
> While it is correct that associations and professional societies operate
> under different rules depending on the country, it nevertheless is more
> than reasonable that IAPT members (regardless of where they live) should
> be
> informed regularly about the ways in which our membership fees are being
> used. You have indicated that this will be forthcoming, and that is good.
> It is unfortunate, however, that it has taken such a strong expression of
> concern from part of the membership to make that happen. Please note that

> this concern could be regarded as justifiable if one acknowledges that the
> Secretariat might in fact be acting (intentionally or otherwise) as a
> "pressure group" (as discussed above). I personally happen to be strongly
> opposed to Registration and the Biocode, and it would concern me very much
> if a portion of my dues were being used to do anything other than
> implement
> the mandated trial and present a balance and impartial assessment of the
> issues and possible consequences to our members. Yet without financial
> information, it is impossible for members to know -- and this only serves
> to perpetuate and reinforce the sense of concern now being expressed in
> some quarters.
>
> I hope that this feedback is taken in the friendly and constructive spirit
> that is intended. We need to put the current controversies behind us --
> but that will only be possible by addressing the concerns that have been
> expressed.
>
> Respectfully,
>
> Porter P. Lowry II
> Associate Curator
> Head, Africa and Madagascar Department
> Missouri Botanical Garden
>
> Mailing address:
> Laboratoire de Phanérogamie
> Muséum National d'Histoire Naturelle
> 16, rue Buffon
> 75005 Paris
> France
> Tel: (33) 1.40.79.33.51
> Fax: (33) 1.40.79.33.42
>
>
> Davidse replys to W. Greuter*s letter - 13 August 1998
>
> Dr. Greuter:
> Thanks very much for the very detailed reply to my message. I well
> realize that there always at least two points of view in controversies
> like
> this, and that well-intentioned people may interpret the same conversation
>
> or set of facts very differently.
> Before sending my initial message to you, I did reread the
> Constitution of IAPT, which is now conveniently available on the
> alternate

> IAPT website, so I was aware that the Executive Committee is in the end
> responsible for publishing a list of nominees. However, I also assumed
> that responses by the membership would also be taken into serious
> consideration.
> What I was not aware of before from reading the constitution is that
>
> you and, I assume from your comments, the Executive Committee
> fundamentally
> consider the Secretary to be an appointed position rather than a
> contested,
> elected position. When I referred in my message to a democratic process, I
>
> had this issue in mind more than anything else. If there is never to be a
>
> second candidate for this position, why are people asked to vote?
> Furthermore, I did not see any of this explained in the constitution. You
>
> certainly present a cogent argument for the traditional way of operation,
>
> especially the institutional support involved, but if this is in fact
> what
> happens and is desired by the membership, it seems to me that that our
> constitution should be amended to reflect this reality and that we as
> members should be asked to vote on an amendment to that effect in St.
> Louis. Such a change in the constitution would basically delegate or
> transfer authority from members at large to only the council members. It
> would thus state something similar to the following: that the authority
> to
> select a Secretary rests with the elected members of the council, and not
> with the membership at large.
> You ask: "I would of course appreciate to know what, in your
> opinion,
> may be at their base. Can the mere fact that I hold a few positions be the
>
> answer? In actual facts, the positions mentioned are two: Secretary of
> IAPT
> and Rapporteur-general of botanical nomenclature, all the other functions
> being automatically linked with these two. Can it be that disagreement
> with
> me on nomenclatural issues is the cause?"
> I can hardly answer for others, but will answer from my own
> perspective. In addition to the foregoing, the following are the other
> issues that concern me.
> Finances. I am very glad to see your comments about full disclosure
> of the financial position of IAPT. That is a real concern for me
> personally, and one which I think has probably galvanized a lot of people

>
> to look more critically at the operations of IAPT. The following
> statement
> on the alternative IAPT website raised my personal concern.
> "In addition, there is a lack of information on finances. At the
> meetings of the general membership (held every six years at recent
> congresses) there has been no presentation of a financial report. Even the
>
> Council has never received any information on how much money is taken in
> each year or how it is spent. This can be contrasted with other societies
> where minutes of the meetings and full financial disclosures are published
>
> each year."
> It may well be that that IAPT is in excellent financial health, but
> with no information, how can members be sure?
> The other statement that raised my concern was: "Even repeated
> requests to the Secretary from within the elected membership of the
> Council
> for information on what is discussed at meetings of the officers has met
> with direct refusal." Surely, regular, public reports is normal for
> scientific societies such as ours.
> As far as the issue of concentration of power, I do believe that
> spreading the work and responsibility around is intrinsically better than
> concentrating it in one or two individuals, in a parallel way to the
> statement in the constitution that mandates that: "Not more than three of
> the ordinary council members shall be residents of the same country." This
>
> is not meant as criticism of the work you do in all aspects of IAPT, which
>
> is clearly prodigious and of high quality, but simply of the inherent
> danger that concentration of power always has, namely, limitation of
> different points of view.
> Regarding nomenclatural issues, I hope that most botanist are like
> me
> in that the issues themselves are the important thing, rather than the
> persons promoting certain positions for or against. As you probably know
> already, I do not favor registration (nor NCU nor Biocode, which together
> are the big three, enormously important issues to be discussed in St.
> Louis
> next year). Nevertheless, discussions and arguments pro and con are the
> important thing, and you will not find me making personal attacks against
> people holding opposing views.
> I hope that these comments are helpful in further explaining my
> concerns about IAPT and its operations. Gerrit Davidse
>
> Dr. Gerrit Davidse, John S. Lehmann Curator of Grasses Editor, Flora

> Mesoamericana
> Missouri Botanical Garden
>
>
> G. Davidse writes to V. Funk
> Vicki:
> I agree totally with you and Pat on both main points of your message. By
> implementing decentralization, there is no inherent reason that I can see
> for believing that IAPT would be destroyed. I believe that it would
> probably make it stronger in many ways, although some efficiency could be
> lost. It would be good if the main public focus of the debate could be
> switched to working on actual amendments to the constitution of the type
> Pat has suggested. I see from the constitution that this requires a mail
> vote or is there some parliamentary way that this could be switched to the
>
> Nomenclatural Session itself? In any case, it would be good to actually
> post formal amendments for public debate, perhaps with a deadline for
> comments and suggestions, and then submit a final version of these
> amendments to the council. I only know the views of a few of the present
> members of the council, and it is always tough to get two-thirds of the
> votes cast for any issue. Nevertheless, now that you have a large portion
>
> of the society paying serious attention to these issues, now is the time
> to
> move forward.
> Gerrit
>
>
> CHECK OUT THE WEBSITE - NEW POSTINGS ON THE 25TH AUGUST 1998
> INCLUDING ADDITIONAL LETTERS FROM MEMBERS AND A NEW HOT BUTTON TO
SUGGEST
> CONSTITUTIONAL CHANGES.
> (http://mason.gmu.edu/~ckelloff/vfunk)
>
>

From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: '"Traverse"' <traverse@ems.psu.edu>, '"Davis"' <palynolo@geo.Arizona.edu>,
"Fensome, Rob" <fensome@agc.bio.ns.ca>,
'"Jan Willem"'
<JANWILLEM@boev.biol.ruu.nl>
Subject: FW: -Reply
Date: Tue, 11 Aug 1998 12:38:05 -0400

Hi everybody,
I just received this e-mail -- which I thought you would or should be interested in. Time seems to be short, so I pass this on for your consideration.

Jan

> ----------
> From: Vicki Funk[SMTP:FUNK.VICKI@NMNH.SI.EDU]
> Sent: Tuesday, August 11, 1998 7:20 AM
> To: JJansoni@nrcan.gc.ca
> Subject: -Reply
>
> Please Circulate
>
> IAPT ALTERNATIVE WEBSITE
> Update 10 August 1998
> URGENT REQUEST FOR YOUR SUPPORT
>
> Dear Colleague:
> You may have received a mailing a few weeks ago notifying you of an IAPT
> Alternative Website and asking for nominations for Officers and Council
> members for IAPT. The response to this request was gratifying and this
> letter is written to accomplish three things. First, to thank you for
> your input, second, to bring you up to date on what has transpired, and
> third, to make an URGENT REQUEST FOR YOUR SUPPORT.
>
> I. The Nominations for IAPT Officers and Council.
> One nomination was received for each of the Officer positions and it was
> felt that these individuals constituted a group that has the interest and
> the institutional support necessary to carry IAPT into the next century.
> Also, many nominations were received for the Council positions. Several
> drafts of the proposed council nominations have been circulated, and you
> may have seen another longer version. Perhaps some explanation of how the
> list below came about is in order. There are only 10 positions on the
> Council and if many individuals are nominated, there is the possibility
> that it would split the vote. As it is there are 13 people and it was
> felt that the number could go no higher. Since nearly everyone nominated
> was experienced and willing to run for Council, the final list was a

> delicate balance between the country of residence of those nominated and
> their institution. It is regrettable that it was not possible to include
> everyone on the ballot. It seems likely that all nominated individuals
> will be asked to serve in some capacity at a future date. Your interest
> and support are appreciated. It is hoped that you will be able to vote
> for the nominees listed below.
>
> Nominated for: Name Country of Residence
> President Sir Ghillean Prance (UK)
> Vice-President Teuvo Ahti (Finland)
> Secretary Patricia Holmgren (USA)
> Treasurer Marshall Crosby (USA)
> Admin. of Finances Judy West (Australia)
> Council Fred Barrie (USA)
> Council Steve Blackmore (UK)
> Council Herve Burdet (Switzerland)
> Council Vincent Desmoulins (Belgium)
> Council Enrique Forero (Colombia)
> Council Peter Linder (South Africa)
> Council David Mabberley (Australia)
> Council Anthony Orchard (Australia)
> Council Sebsebe D. (Ethiopia)
> Council Sy Sohmer (USA)
> Council Peter Stevens (USA)
> Council Tod Stuessy (Austria)
> Council Warren Wagner (USA)
>
> II. The Ballot
> The above list was submitted on the 29th of July (nomination of Treasurer
> was on the 31st). No acknowledgment of the list was received from Berlin,
> but a few days later Dr. Patricia Holmgren received a phone call from the
> Secretary, Dr. Werner Greuter. He informed her that the Officers would
> not allow anyone to run against him for Secretary nor would they allow
> anyone else to be nominated for Treasurer other than his colleague Dr. B.
> Zimmer, the current Treasurer. As an historical point, while it is true
> that in the past candidates for the offices of the Secretary and Treasurer
> have often run unopposed, it is also true that there have been instances
> where more than one candidate was nominated (Dr. Greuter was an
> unsuccessful candidate for Secretary in 1975). Dr. Greuter then asked Dr.
> Holmgren to run for President instead, and suggested that he would put
> three people on the ballot for President (not including Prance). Dr.
> Holmgren has told us that she has no interest in running for President,
> and she has reconfirmed her willingness to be nominated for Secretary.
> Dr. Greuter also mentioned that he was trying to finalize the ballot form
> as soon as this week, which would apparently give us minimal time to
> convince him to accept the above nominations for Officer. It seems likely

> that, unless he changes his mind, the ballots will not include the names
> of Prance for President, Holmgren for Secretary, and Crosby for Treasurer.
> Further, it appears Dr. Greuter may try to nominate several people for the
> other Officers and Council positions, which may result in a divided vote
> and so reduce the chance of the individuals on the above list being
> elected.
>
> III. URGENT REQUEST FOR YOUR SUPPORT
> We believe that this reported refusal to nominate Prance, Holmgren, and
> Crosby is unfortunate and indicative of the fact that IAPT is operating
> with no accountability to the membership. It is this lack of due process
> and democracy that has so irritated many of the members of IAPT and has
> likely led to the decrease in membership. The website was established to
> increase membership participation in IAPT, and in the process it has
> exposed yet another instance of disregard for the wishes of the membership
> and the lack of any type of checks and balances in IAPT (e.g., no
> nominating committee for officers and council members). All of the power
> is in the hands of a few individuals. Indeed, Dr. Greuter alone holds
> nearly all positions of power (Secretary of IAPT, Chairman of the General
> Committee, Editor of the Association*s journal Taxon, Editor of Regnum
> Vegetabile, Rapporteur General, Secretary of the Bureau of Nomenclature,
> member of the Nominating Committee which appoints all other committees and
> the Rapporteur General, and Chairman of the Editorial Committee for the
> Code), and there is little recourse except revolt.
>
> We Ask That You Do Several Things:
> 1. Send a message to V. Funk (contact information below) and ask that
> your name be listed on the website indicating your support for competiton
> for all offices in the upcoming IAPT elections. Even better would be to
> write a short note to be posted on the website. You can also write or
> email Dr. Greuter directly (wg@zedat.fu-berlin.de). [Participation by
> non-IAPT members is welcome since these issues affect the whole of
> taxonomy]
> 2. Pass this letter along to everyone you think may be interested.
> 3. Make sure that your dues are paid for 1998 so that you can vote in
> this election.
> 4. Vote for the individuals listed above if they are on the ballot form.
> If their names are not on the ballot form you should request a new ballot.
>
> We ignore this situation at our own peril. At stake is the future of IAPT
> and international cooperation on nomenclature issues. Time is very
> important because decisions on the content of the ballot form may very
> well be made in the next few days. Please take a few minutes to register
> your opinion on this most recent example of flagrant disregard for the
> wishes of the membership of IAPT.
>

> Signed: R. Brummitt and V. Funk
> C. Anderson, W. Anderson, L. Skog
>
> email: *funkv@nmnh.si.edu* or *hollowell.tom@nmnh.si.edu*
> fax: 202-786-2563
> address: Dept. of Botany MRC 166, Smithsonian Inst., Washington D.C. 20560
> USA
> website: http://mason.gmu.edu/~ckelloff/vfunk
>

To: jjansoni@nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: Supp. 12, etc.
Cc:
Bcc:
X-Attachments:

Dear Jan:

I have heard from Sharron, and that seems all to be arranged--my set will arrive about Xmas, given that it must pass through Canada Post.

I have 0 contact with my library at the moment, so can't check the Kremp name. From the date, I would assume that the paper was published just before Kremp left Germany, and the honoree must therefore be a German. Estella Leopold was just a graduate student at the time anyway.
I would bet that Mrs. Kremp (Eva) would know. She can still be reached at their Tucson address, which I don't have available under present circumstances. Eva is very nice and would gladly respond.

Once again I raise the question of why when you use "reply" you can't hit the option that does NOT return the original message, as this unnecessarily pushes the whole thing into two pages when printed.

I'm not going to be in the office much from this day until October, for a variety of reasons, partly having to do with various visitors (a Japanese palynologist and wife for ten days, for example), partly with the danged move. I am now pretty well settled into my cubicle in the Deike Bldg., and that is the only part of the whole mess that is more or less orderly. When the whole thing gets straightened out, it will be o. k. Betty was working in my prep lab here today, perfectly normally. She leaves tomorrow for Chicago for a 4-day reunion with childhood friends. Of the little club she and 7 other little girls started in about 1935, 6 remain alive and will be there, one coming from Hawaii!

Have fun. Al

From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: '"Traverse"' <traverse@ems.psu.edu>
Subject: RE: Supplement 12
Date: Wed, 5 Aug 1998 13:42:37 -0400

Hi Al,

Since our last contact, I recently had occasion to phone Sharron. She told me that her brother had died a few weeks ago, and that because of the pressures that had brought on over the last few months, she had fallen behind in mailing the Supplement. However, she would try to get that process back up to {3 prepositions?} speed, before the next academic year.

I came across a species name from Kremp 1952: Granulatisporites leopoldi. Do you know which Leopold was so honored? Was that a first name, or a last name, and was it a man or woman? Just a detail, that I unfortunately forgot to ask of Gerhard while he still had ears and voice. It seems to me that the ending may need fixing.

Best regards, Jan.

> ----------
> From: traverse@ems.psu.edu[SMTP:traverse@ems.psu.edu]
> Sent: Wednesday, August 05, 1998 7:54 AM
> To: sharron@geo.ucalgary.ca
> Cc: jjansoni@nrcan.gc.ca
> Subject: Supplement 12
>
> Dear Ms. Kaser:
>
> A while back I got an e-mail from Jan Jansonius about shipping my copy of
> the new Supplement. It should be shipped to me at the same old address,
> Deike Bldg., except the room number is changed to 307.
>
> Can you bill me in Canadian funds? I have an account at the Royal Bank
> and
> like to exercise it occasionally.
>
> Thanks. Alfred Traverse
> Alfred Traverse
> Palynological Laboratories
> Department of Geosciences
> 435 Deike Building, PSU
> University Park, PA 16802
> Ph.: 814-863-3419
> Fax: 814-863-7823
>

From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
Return-path: <JJansoni@NRCan.gc.ca>
To: "'bettyandal@juno.com'" <bettyandal@juno.com>
Date: Fri, 17 Jul 1998 16:01:35 -0400
Subject: RE: WG, etc.
Message-ID: <C72C9282075AD111AF6300805FFE1C8F033D20@s5-cal-r1.gscc.nrcan.gc.ca>
X-Status: Read
X-Mailer: Internet Mail Service (5.0.1460.8)

Hi Al and Betty,

No, I hadn't heard that you had been evicted on unexpectedly short notice. I can commiserate with the experience, though. Ten years before I retired, we moved from the Research Lab to the downtown bldg. Then, when I took early retirement, I was moved to another place in the building, complete with library etc. That actually happened twice -- from 14th to 16th and then back to 14th floor. Then I got kicked out altogether, and moved to smaller (again) space at the GSC, and 18 months later the misery of right-sizing started here, and I made my last move to this 8x10' cubicle. In the mean time, I have shed most of the library (to other universities, institutions and consulting colleagues), counting on the GSC library, and my reprints, to serve me. But, with the public spirit of helping visiting scientists, who also have the run of the place, and with no one curating the collection, I notice that there are suspicious gaps developing on the shelves that two years ago were crammed....

You'll be alright, Al; and once you are set up again (and with the intelligent support of Betty), you'll soon be functional and at it again. Unless the taste of the easy life has spoilt you for such thankless work. Then, you'll still find a happy compromise between science and more contemplative pursuits.

Speaking of pursuits: did you hear of the hapless teenage car thief here? He stole a car at the west end of town, and rode it out into the setting sun. However, someone had seen what happened, and soon he was tailed by police. So, near Cochrane, he ditched the car, and ran into a hayfield. Since we had double the usual amount of rain, he was unfindable, and the police were ready to pack her in, when the pollen got the better of the fellow, and a loud sneeze told the cops where to look.

The Genera File Supplement is being shipped out (Can$40) to all old addreesses -- did the Dept of Geology have a subscription? WIll they pay for Supplement 12, or return it? WIll your set come to the University? You can check for info wtih Sharron Kaser at sharron@geo.ucalgary.ca

All the best, Jan

> ———
> From: bettyandal@juno.com[SMTP:bettyandal@juno.com]

From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
To: '"Traverse"' <traverse@ems.psu.edu>
Subject: RE: addresses
Date: Fri, 1 May 1998 17:35:57 -0400

Al, I was writing a reply to you on orthography, when I was interrupted by a message from Rob -- in part on th esame topic, so I'll copy you. So, I want to get this out of the way in a hurry.

I did send a 'correction' to my earlier mass-appeal to change my address. In large part this matter became confused by the introduction of a new system at the GSC, without deleting the old system. We had no instructions on this, and depending on the length of your name, different 'rules' seem to apply. ANYWAY:

At the GSC you should be able to reach me at

jjansonius@gsc.nrcan.gc.ca

OR

jjansoni@nrcan.gc.ca

AND DON'T MIX ANY OF THESE TWO UP!

At home, I still am open for business at

jansonij@cadvision.com

I consider that my name, "jansonius", is the latinized form of the (very common Dutch) names "jansen, janson, or janssen." So, where the ICBN intructs a user to latinize a surname, by finding the stem onto which to attach the ending -ius (of which we take the genitive case to produce the specific epithet), I consider that latinized names like mine already have gone through that first step. I think few people would want to name a species "linnaeusii"; rather, they write "linnaei."

I think constructions like "jansoniusii" (derived from a hypothetical "jansoniusius") are a monstrosity, lacking in taste and euphony. I am aware though that some people -- probably those that have no Latin or feeling for any language -- have no compunctions in going that route. None of them, I bet, have a latinized name. Yes, all authors have absolute freedom to construct any name they want to. But, ask yourself, would a Roman (whose lamguage we speak in Nomenclature) ever come up with "jansoniusius? His friends would think he had a speech impediment. Or be retarded. Poor chap (homo miser)!

> From: traverse@ems.psu.edu[SMTP:traverse@ems.psu.edu]
> Sent: Friday, May 01, 1998 1:36 PM
> To: jjansonius@ess-dns2.gsc.nrcan.gc.ca
> Subject: addresses
>
> Dear Jan:
>
> I had sent you a message to the "new address" and it was immediatelyi
> fired
> back at me: the following address had unrecoverable error.
>
> I continue to have occasional problems because your J for Jan is on the
> front of Jansonius in one address and on the end in the other. That was
> diabolical.
>
> By the way, do you regard your name as Latin? I find that I am on the
> Orthography Committee, and we are dealing with all sorts of things re Art.
> 60. Some folks think that if a name is "obviously already Latin" it has
> be
> treated as such in making an epithet out of it, whereas I would urge that
> an
> author would have the liberty of regarding it as non-Latin and going with
> jansoniusii or jansoniusianum if he wanted to.
>
> Best. Al
>
> (This further change is ruining your card (I don't get the meaning of
> this) --I may have to make a new one.)
> Alfred Traverse
> Palynological Laboratories
> Department of Geosciences
> 435 Deike Building, PSU
> University Park, PA 16802
> Ph.: 814-863-3419
> Fax: 814-863-7823
>

X-Sender: jansonij@cadvision.com
Date: Sun, 29 Mar 1998 09:26:40 -0700
To: traverse@ems.psu.edu
From: Jan Jansonius <jansonij@cadvision.com>
Cc: fensome@agc.bio.ns.ca
X-MIME-Autoconverted: from quoted-printable to 8bit by pangaea.ems.psu.edu id LAA08253

Hi Al,

I copy this to Rob and Christoph (even though the latter is in the field for the next 4 weeks (für die nächste vier Wochen). Ahem.

I am reading through your long letter with Greuter's responses interspersed -- and juggling back and forth to this out-going message with my comments (I'm NOT going to intersperse in that mess myself!).

First I wanted to ask if you guys received the message in which I cited some definitions from the Glossary to the Code (Al sent me a xerox, years ago, and I sent Rob a xerox of that, this year?). I think we should refer Greuter to that, and he can straighten out some of his misconceptions regarding specimen, preparation and microscope slide.

On that theme, I also find it enlightening to read in Art. 8.1 where it is permitted to preserve a number of small individuals on one sheet or equivalent preparation (e.g., ... microscope slide). Here, then, a slide is not called a specimen, but a preparation. Also, in the first line the term "single specimen" is used, which in the third line is mirrored by "one individual". It is clear that the Code uses the word "specimen" and "individual" interchangingly, and a microscope slide is referred to as a "preparation."

I have sympathy for Greuter's objection against the term "not practical" -- in my estimation it is just as vague as the choices ("or" "or" etc. in Art. 37.3) and what we need is black/white choices.

As for the designation of figures as type in palynology, I cannot recall a single instance. Of course, the illustration of the type is (nearly) always identified either in the text, or in the plate description, or both. But then, that is a requirement of the Code. Indeed, we are currently puzzling over those cases where a type specimen is designated in the text (slide number and coordinates) but the illustration of that specimen is not identified from among more than one figures representing different individual specimens (yet, one of them may represent the type, so the requirements of the Code are met, although we cannot verify that from teh protologue). I cannot, either, recall a single instance where a species name would be retroactively considered as validly published although it was not under the current rules. [Although I am puzzled by why Greuter would be

upset -- several new clauses in the Code introduced under his term have changed the Code sufficiently that names, considered validly published under an older Code, suddenly become not validly published. And prob. the reverse can happen, too. Any change in the Code has some examples of this happening. Nevertheless, apparently in the past these incidental changes have been considered as the price to pay for a more equitable and practical set of Rules.]

Here, I copy some of Greuters comments (recognizeable by a shorter line length), so I can intersperse MINE (preceeded by two asterisks) -- it will be clearer that way:

Perhaps I should explain. Even a flowering plant type may have mould attached to it that belongs to a quite different species, or perhaps be affected by Cuscuta. There may be some circular reasoning in this, but what Art. 8.1 MEANS (and is thereby supported by 9.9) is that only those individuals or individual fragments belonging to the taxon described belong to the type proper.
** As I read these two articles, it is plain to me that they do not have in mind the possible accidental/secondary growth of fungi/algae on the type specimen. I recall a good friend of my father (his herbarium now is conserved at Leiden University), who would gather, on a single sheet, a complete flowering specimen, with roots, the dried parts of a flower mounted individually, as well as ripe seed under a piece of cellophane: the seeds would likely have come from a different specimen of the same species growing in the same area. As Art. 8.5 requires "one whole specimen," to me that means that the parts must all be attached to other parts that belong to the same plant; the seeds (in my example) harvested from its sibling would not be part of the type specimen.

Diatom specialist among others
must, I suspect, have the same kind of situation as micropalaeontologists. I gather that they try (more or less successfully, and facing the same kind of difficulties as you do) to locate an individual on a "type slide" to pin down their type concept more accurately -- but they are not forced to do so.
** I do not understand what Greuter means with "but they are not forced to do so" -- how then do they typify their species? Do they not have the requirement to designate a holotype? Can they use illustrations? I am not aware of speci

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

slide was stated to be the type would turn out to be invalid and in need of validation (again as by Art. 37).
** I guess I'm too old to follow the logic of this sentence. If I ignore the clause (is this dig at Rob of relevance to Al's argument?), I don't understand what Greuter is getting at. Earlier I thought he was concerned about taxa now considered as invalidly published (because only based on a fig.) would automatically become valid -- but here it is the other way around?

As I said, this problem (which I accept to be a real one) extends much beyond what you are proposing in your text. So again my advice: keep it dissociated. If you can think of a good solution to your question that does not affect stability, propose it separately (and either limit what you propose to the group you are working in, or get in touch with people working in other groups that may be affected).
** It seems to me that the result of Al's proposal to delete the words "non-fossil" from Art. 8.3 would be that 8.3 would still apply to all non-fossil plants, but now as well to fossil ones. That would not limit the modern botanists, but would give those working with fossils equal rights, as it were. What is so objectionable about that?

What I would like to see in the Code is splitting of the Code's type concept into two categories, say, an "assemblage type" and an "atomic type", when mention of the assemblage type is sufficient for purposes of Art. 37 (at least until some date in the future that might have to vary depending on the group concerned) but within it an atomic type may be designated by a (second-level) lectotypification if required.
** Without an explanation of these two terms ("atom- and "assemblage-types") I have not the foggiest what Greuter has in mind.

Well, I think I have addressed all issues where I feel I can form an opinion. As for the matter of the word "specimen" I feel that Greuter is not following the generally understood meaning of that English word. I also think that you should not try to avoid using it in your proposal. Do use it as you had it, but in a footnote give "your" definition of the term, and maybe even state that some (incl. Greuter) have a different understanding. You can cite that old glossary. It seems that we could do with a new glossary, in the back of the Code, to make sure that all technical terms are understood in the same way by all those who are sincere in trying to follow the requirements of the Code.
Now I must do things that spring requires of me at this time of the year.

Best wishes,
Jan.
Jan Jansonius

X-Sender: jansonij@cadvision.com
Date: Thu, 26 Mar 1998 21:26:34 -0700
To: traverse@ems.psu.edu (Alfred Traverse)
From: Jan Jansonius <jansonij@cadvision.com>
Subject: Re: generic names
X-MIME-Autoconverted: from quoted-printable to 8bit by pangaea.ems.psu.edu id XAA03273

Hi Al,

Yes, those genera are included. The Supplement should be back from the printer by the end of next week at the latest.

I read through your letter/reply from Greuter. I shall respond this weekend, but things are quite busy, here -- I'm also wading thru the new Lentin & Wiiliams, and have this fungal thing going (with Kalgutkar) and must review two MSs for Palynology. And, the first robin sung today, and the gulls are back from the Pacific coast, so I am preparing for more grafting and stuff. Hey, you should buy a Honeycrisp apple -- they are a sensation in apple circles.

Already I can say that I have never seen a paper where an illustration was passed off as type. Some papers had a picture, and if only one, it has been taken to represent the type. And, if there are pictures of two/more specimens, then there's no type, and no valid publication.

More later,
Jan.

At 12:46 26/03/98 -0500, you wrote:
>Dear Jan:
>
>Have you picked up the names Acontotetras, Chelinohilates and Cymbohilates
>from Richardson, Spec. Paps. Palaeontol. 55:7-40 (1996)? Two of them are
>what JBR now calls eucryptospores, and one is what he calls paracryptospores.
>
>Just got an e-mail from Hartkopf-Fröder in response to the letter from
>Greuter that I copied to you both. What H-F is upset about is the specimen
>thing. I don't blame him. I still await your reactions, if anyi.
>
>Best. Al.
>Alfred Traverse
>Palynological Laboratories
>Department of Geosciences
>435 Deike Building, PSU
>University Park, PA 16802
>Ph.: 814-863-3419

To: jansonij@cadvision.com
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: proposal
Cc: fensome@agc.bio.ns.ca,hartkopf-froeder@mail.gla.nrw.de
Bcc:
X-Attachments:

Dear Jan, Rob, Chris,

Well, I've heard from Dr. Greuter. He seems quite interested in the proposal, though he only marginally understands the problem.

He says a number of things that I would like you three to think about and respond as soon as you can:

1. He says that Art. 8.3 is "a can of worms" and is under big attack by non-fossil folk. He says that my proposal will confuse that business and that it could get lost in the crossfire. He says, " My advice is leave Art. 8.3 alone. My advice is stick to Art. 8.4...." Then he goes on to suggest how all the business could be taken care of within 8.4. On reflection, this seems possible, and since he notes sternly that as Raporteur he would feel obliged to urge defeat of my proposal if it messed with 8.3, it sounds like an "offer I can't refuse." [Rob will be amused--my original proposal dealt ONLY with 8.3, and I did something with 8.4 only as an afterthought, suggested by RF.]

However, and this is a big however, I have responded that I just must propose deleting the words "of non-fossil plants," and that alone for 8.3. If those words stay in, in whatever revision of 8.3 is contemplated, whatever I propose for 8.4 won't help. Besides, as I pointed out to Werner, it is a non-sensical provision for megafossils anyway. A non-preservable rock?

2. Werner is concerned that my proposal would perhaps have the effect of validating names, now not validly published, because an author may have designated an illustration as type. He says that I should add to my paper a statement about whether this is a problem. I told him that I can't imagine that it would be because paleopalynologists have either designated a "specimen" (I'll explain the quotes later) as type, or have failed to designate a type at all.

Important: can any of you come up with cases where an author designated an illustration as type, and this (post-1957, of course) name would now be accidentally validated by what I propose?

3.Then came a whole page from Dr. Greuter that completely floored me. He says that micropaleontologists use "specimen" to mean the individual microfossil, but that in the Code it is very important to insist that the term "specimen" is the whole slide (even if if has 5,000 "specimens"), not the individual microfossil to which we refer when we say "specimen.". How does THAT grab you guys?

He writes: "I am aware...that micropalaeontologists use the term "specimen" in a sense that is

not consistent with the letter and spirit of the Code, i. e., for the individual microfossil rather than for the preparation (slide) as a whole....Taxon has therefore followed a consistent policy of not allowing use of the term in the second sense. In the pre-edited draft I am forwarding to Dan I will suggest changing "specimen" throughout your text to either "microfossil" or....."

Can any of you think of a word that could replace specimen in what seems to me a logical sense? (Webster's Collegiate Dictionary: "specimen...an item or part typical of a group or a whole"--damn it, that's the individual fossil, not the whole slide!)

Please respond to these points ASAP. Best. Al

From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
Return-path: <JJansoni@NRCan.gc.ca>
To: "'Judith Lentin'" <judith.lentin@thementors.com>
Cc: "Fensome, Rob" <fensome@agc.bio.ns.ca>,"'Bettyandal@juno.com'",<Bettyandal@juno.com>
Date: Mon, 9 Feb 1998 12:36:41 -0500
Subject: RE: problems.
Message-ID: <C72C9282075AD111AF6300805FFE1C8F033C64@s5-cal-r1>
X-Status: Read
X-Mailer: Internet Mail Service (5.0.1458.49)

Dear Judi,

Sorry for not answering you earlier, but I was preoccupied by the Genera File. I just dropped it off at the UofC; there's 304 sheets in Supplement 12.

As for Versteegh, and Zevenboom: I consider both separate publications as having been effectively published. In Holland, one of the requirements for being awarded a PhD is the publication of the dissertation (some 300 copies of which must be given to the University for the purpose of exchange of scientific literature). In lieu, it may be acceptable if the paper has been accepted by a peer-reviewed journal of appropriate standing, and then the University wants the same number of reprints. There is quite a cottage industry of small printing houses that specialize in printing (and binding) of doctoral dissertations. Ceratinly, these are not printed so that 5 copies can be handed off to the examination team (and local library). These are produced in generous quantity, because you need also a goodly number to satisfy exchange among colleagues, and sufficient numbers to impress future employers, and to support general bragging rights.

Some, like Hultberg's, may consist of a number of disjunct smaller papers, and in some cases some of those have already been published in a journal, but they are indeed effectively published. So, now we must decide whether or not these particular two were also validly published.

[Sorry about the margin -- this new program tries to buck me off!]

I think that the new names in Versteegh & Zevenboom in Versteegh Febr. 1995 were validly published; even if, for whatever unfathomable reasons Versteegh also submitted the same MS to Elsevier, where they were published in April 1995 (an annoying redundancy, esp. because of the high price of RPP). (A propos, the GSC received that April issue in May).

Similarly, the names in Zevenboom's dissertation appear to me to be validly published because there is no explicit disavowel that the author did not accept the names in that publication. So, even if he did say "manuscript name" and "manuscript type." -- these are not disavowels, but may be unneeded but harmless qualifiers. Those names, by virtue of being in the printed text, are no longer "manuscript" but have been effectively published. (I trust that Judi looked for, but didn't find, any statement in the book that said that his "manuscript names were not accepted by the author.) So, after that is established, we must see if all other requirements for valid publication (type, curation, description, illustration, etc.) were fulfilled.

A propos, and this is why I send a copy to Al: when I checked the Code for precise definitions of all these things, I cought a curious Greuterism: i previous codes "Indelible autograph" included mechanical or graphic processes in which the handwriting was multiplied. However, in the Tokyo Code, Art. 30.2 does no longer considerhandwriting as

acceptable for valid publication -- now it MUST BE mechanically or graphically reproduced handwriting. And, this is of course retroactive, according to Art. 30.1. Go figure. No wonder Greuter wants to write a new Code himself, the Biocode. He should have become a medical doctor, then he could simply bury his mistakes.

I hope that I answered Judi's questions. Just because in North America (and in UK?? -- I am not too familiar with their system; in France they have a number of doctor degrees, each with its own requirements) a dissertation is normally NOT published (and because of that includes a lot of "filler" that shows that the candidate did his homework, and now knows the basic principles, as demonstrated by a rehash of them in the first part of his thesis), it should not be assumed that everywhere on this globe a thesis must be considered as not "effectively published" for only that reason. If it has an IBBS number, that already is a give-away that the thing is properly registered, somewhere, as a publication.

Best regards to all, Jan.

> ———
> From: Judith Lentin[SMTP:judith.lentin@thementors.com]
> Sent: Wednesday, February 04, 1998 9:05 AM
> To: jjansonius@ess-dns2.gsc.nrcan.gc.ca
> Subject: problems.
>
> Jan
>
> I would like your opinion on two books. Both have a publisher listed,
> both
> have ISBN numbers, both are widely circulated and both are PhD thesis.
> The
> first is a particular problem because the taxonomic section was
> published
> both in the book and in Rev. Palaeo. & Paly. The question is how to
> site
> the new genera and species. The book (Feb. 1995) is by Gerard J.M.
> Versteeg, his chapter 5 contains a number of new species, clearly
> marked
> sp.nov with Versteegh and Zevenboom as authors. They meet all
> standards for
> effective publication, so the ref should be Versteegh and Zevenboom in
> Versteeg, 1995. However, the Rev.P.P. (dated April, 1995, but I
> received it
> in September) paper is by Versteegh and Zevenboom, 1995, and is
> identical in
> every way to the Chapter 5 of the book.
>
> The second thesis is by Daan Zevenboom. He also has a number of new
> species,
> however he puts (manuscript name) behind the words sp.nov. He also
> says
> "Manuscript holotype" which makes it reasonably clear that he
> understands
> the problems of ICBN rules. However, is this effective publication?
> By
> stating Sp.nov behind the names and meeting all other rules, did he
> really
> avoid the problem by stating (manuscript name)?
>
> I firmly believe that both of these thesis meet the rules for
> effective

> publication. What is your opinion? JKL
>

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

Dear Jan:

Well, I have been pouring over your suggestions as I put the final touches on the proposal. You really have been helpful.

I didn't exactly follow your suggested rewording of the amendment itself. However, I decided on the basis of what you did say to simplify the wording a lot, in order to make it more inclusive. This also is a response to Rob's ideas. Further, Rob is right, 8.4 also will have to have a reference to 8.3, stating that for microfossils the type itself can be an illustration. So, I have rewritten the actual proposal as follows:

8.3. Add sentence at the end of present Art. 8.3: For plant microfossils it is recognized that preparation, conservation and availability of type specimens is so problematical that typification by illustration is normal.

8.4. Add sentence at end of article: Note, however, that for plant microfossils an illustration may serve as type (cf. 8.3)

Now, Jan, I've gone over all the changes suggested in your long message of 24 Jan.

I have accepted (sometimes in modified form) about 80% of the editorial suggestions. In a few cases I feel that my wording catches the meaning I am seeking to express more cogently. Some of your editings were for places where I had made actual typos and the like--I greatly appreciate those because they are very difficult to catch.

Your suggestion to delete most of the "long litany" of citations that proved that the Code is not against illustrations as types is good, and has been accepted, but I sure hope you've got that mutatis mutandis thing right--I can't check it here.

Re deletion of "Dr."--you have an earned doctorate,don't you?

I liked the stuff on p. 4 that you wrote about the problem, but if that were worked in it would enlarge the article by at least a page. I thought you and Rob thought it is too long already.

Who knows if this will be successful. I suppose there will be a lot of opposition. Indeed I could do an imitation of Chaloner's speech. Does he REALLY THINK that a repository or repositories will be set up and 30,000 single grain lecto/neotypes be prepared and designated?

Thanks again for your help.

Best, Al , Ph. D., M. Div.

From: <jjansonius@gsc.NRCan.gc.ca>
Return-path: <jjansonius@gsc.NRCan.gc.ca>
Resent-From: jjansonius@gsc.NRCan.gc.ca
Resent-Date: Tue, 23 Dec 97 9:29:57 EST
Resent-Message-ID: <vines.2JI8+aHrpoB@vns601c.gsc.nrcan.gc.ca>
Reply-To: <jjansonius@gsc.NRCan.gc.ca>
To: <traverse@ems.psu.edu>
Date: Tue, 3 Feb 1998 14:49:58 -0500 (EST)
Subject: MS Again - NO!
Message-ID: <199802031949.OAA24635@pangaea.ems.psu.edu>
X-Status: Read

Forwarded to: inet[traverse@ems.psu.edu]

cc:
Comments by: Jan Jansonius@PALEO@GSC CALGARY
Comments:

Dear Al,
I thought you might be interested in this snippet of a conversation, of which I have deleted teh antecedents -- it all has to do with the "Type" note of Fensome et al for Taxon. I think you saw a draft of it?

Anyway, the reference of types sitting on SEM stubs, or being cut up, should be welcome ammunition for you? For further info, please contact Judith Skog.

Jan (still chugging at cleansing my e-mail box)

---------- [Original Message] ----------

Yes to Jan's comments - send it off. Without a few things wrong the reviewers might look deeper!! Actually, Rob - just a couple more notes. Deng is at Beijing, China for the location of his center. And Australian in the reference is spelled with an e rather than an i. In this case, even the specimen numbers are no help - the holotype number is H17-431 with no indication as to what the 'H' stands for (possibly holotype?). And Jan's comments could go equally well here - paleobotanists will cite Deng's genus whether or not the specimens have a permanent home- many of his are also on SEM stubs, or on slide preparations. They are some of the best examples of fossil fern material coming out of China these days. And just so the two of you know - I think that his job at the Petroleum Institute is a result of his advisor being outside the political arena at the time he was ready for a job - and I am not sure what will ever happen to his specimens. I know that lodgement in the Code was probably a response to the vagaries of politics in the way of science - but we can't legislate that for other countries!

Yes - we need to be clear about "accessibility". Does it not still amaze you that we publish "sanctioned" versions of the Code in French and German, but do not sanction a version in Spanish, Chinese, or Russian which are certainly spoken and understood by many more paleo folks who are doing some of the best work?

Bottom line - Rob add in what you need and ship it out. I refuse to look at this again until January 5. There are pies and cookies to bake,

presents to wrap, beds to make for company descending, and worse yet,
still a bit of shopping to be done!!

How is that Jan - was I strong enough?????

Happy New Year to all. Judy

~~~~~~

Judith E. Skog JSKOG@GMU.EDU
Biology Department 3E1 Office phone: 703-993-1026
George Mason University Department: Phone 703-993-1050
Fairfax, VA 22030-4444 FAX: 703-993-1046
USA

~~~~~~

From: bettyandal
Full-Name: Alfred or Elizabeth I. Traverse
To: jansonij@cadvision.com
Subject: singing
X-Status: New

Hi, Jan:

Passed on your singing news to Betty.

If I tell my sister about you when she comes in two weeks she may fly up to test you. Her current research interest is in the aging singing voice. She, by the way, still is in super voice at almost 67. I keep telling Betty that she should drop out of the choral society in State College before she is embarrassed by being kicked out, but she won't hear of it.

Back to the Devonian paper. Colin tells me that if I want to bounce ideas off another human about it I must go to Ireland, Belgium or Poland.

Best. Al.

From: Jan Jansonius <jansonij@cadvision.com>
Return-path: <jansonij@cadvision.com>
To: bettyandal@juno.com
Date: Mon, 26 Jan 1998 17:27:07 -0700
Message-ID: <1.5.4.32.19980127002707.006a08f0@cadvision.com>
X-Status: Read
X-Mailer: Windows Eudora Light Version 1.5.4 (32)

Hi Al,

I'm glad it all got there -- one never knows where it will wind up.

I'm cool with you not accepting all -- or any, for that matter -- of my suggestions. But some of them might help you to look at it with fresh eyes (i.e., mine, in this case). It is the only alternative other than locking it away for a month or so, and then come back to it.

From the sounds of it, you are having a good time, and you deserve it. I'm happy to hear though that you don't mind trekking back north before all vestiges of winter have dissipated. Nothing so envigorating as seasons!

Give Betty my regards as well (tell her that in two weeks we'll sing Vivaldi's Gloria; then later, in May, Mahler's 8th in Winnipeg, and Carmina burana in Calgary); lots'a work, but fun.

Jan.

Jan Jansonius
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11 Stanley Place SW, Calgary, Canada T2S 1B2
403-243-3937
jansonij@cadvision.com

From: bettyandal
Full-Name: Alfred or Elizabeth I. Traverse
To: jansonij@cadvision.com
Subject: thanks, etc.
X-Status: New

Dear Jan:

I have to be very careful with your e-mail address, as the j is in front of your surname on one of them and in back on the other--that was devilish of you!

Yes, I got the previous one and this longie. I am now waiting to hear from Hartkopf-Froeder, to whom I also sent a copy. Then I'll make up the final version. I just hope you understand that I am VERY grateful for all the work you have done, but that doesn't necessarily mean that I will agree with you across the board! I will give every single one of your suggestions very serious considertion. (Maybe I should be grateful that you didn't have "a few more days"?)

Betty just left for the Clearwater public library so that she too can spread out her manuscript stuff. I have the Taxon thing spread around on half the surfaces in our living-dining-kitchen area, and a Devonian paper over the other half (no new genera in this one!).

We went to a seafood restaurant on Clearwater Beach yesterday with two vans of my relatives from NB and NS. The mahi-mahi was delicious, and it was fun to see all the palm trees and million dollar houses down there. Many of the Canadians who have places here are not staying as long as usual this year because of the 68 cent dollar.

Thanks again for all your efforts. I'll be back in touch soon. Must get these MSS done before my sister comes from St. Louis. (She's the professor of voice music and well known coloratura soprano.) This mobile home just doesn't really have room for a guest plus manuscript materials. After Sally leaves, we start packing up for the trek home.

All the best. Al.

From: Jan Jansonius <jansonij@cadvision.com>
Return-path: <jansonij@cadvision.com>
To: bettyandal@juno.com
Date: Sat, 24 Jan 1998 09:03:31 -0700
Message-ID: <1.5.4.32.19980124160331.0069f710@cadvision.com>
X-Status: Read
X-Mailer: Windows Eudora Light Version 1.5.4 (32)

Hi Al, when I just checked my e-mail, I found that this messsage had been undelivered because of its length. I don't understand, I had sent much longer e-mails to Bob when I was working on the book. Is it your ISP who cannot handle long files? Anyway, I shall try aagain. I take it that you did
receive the first short one, with my proposal to cut down on the changes to
Art. 8.3.=20
There are some 100-200 types on SEM stubs(e.g. Below's work on dinos); there
are a few types that were cut up (Doyle? the fellow who worked with Mme van
Campo a couple of times – you know who I mean); in both cases this resulted
insuperior information and illustrations.
Megaspores can be handled like forams; even when coated in gold, they can be
stored "manually." However, I shall think some more over what might be the
best phrasing for the new Art.; the simpler, though, the better. If it iis
more inclusive, the better again. You know the bible story, was it Abraham
(NO!!) who bargained with God about how many good men would be needed to
save the city from destruction. If we give a size (500 =B5m), then will 1 =
=B5m
over disqualify it? 2? 10?=20

Hi Al,
It is now Wednesday PM, and I shall type out some things you might consider
to look at again.

PAGE 1
The first part of this page hinges on the exact phrasing of Art. 8.3 - for
which I suggested a more generalized alternative yesterday. The last
sentence of this page might be phrased as:=20
"The purpose of this nomenclatural note is to formally propose specific
amendments to the Code to make this legitimate, and also to provide
explanatory material to justify these proposals.

PAGE 2
HEADING: you should separate this from the preceding text (which does have
its own subtitle) by inserting a title like, maybe, "Background of
palynological practice"
1-2 for "something in the neighborhood of" write "some"
3 Insert "(fide RAF, pers. comm.)" after "as well as"
4 for "[Rob, please ... stab] species of" write "a similar number in"
5 delete "various"

6-7 for "(dinoflagellate ... communication). All 25 000" write "These 28=
000"
8 delete "of these"
10 for "made from" write "of"
11 for "-commonly" write "that commonly each contain"
12 insert "under a cover slip" after "medium"
13 delete "intended"
14 for "the preparation is by attachment" write "prepared specimens are
attached"
19 for "that may exist on slides" write "on slides that may exist"
20-21 delete "but that would ... in very poor condition"
GENERAL: I prefer to put a space before and after a double hyphen, like yours in lines 10-11.

PAGE 3 begins with missing text, on which I cannot comment (I didn't keep a
copy of the earlier draft, or at least I can't locate it now).
2 delete "Many"; start next word with a cap
3 for "or even in" write "even corn"
4 for " last ... at best" write " lasted at best a few decades."
5 delete "mounting"
8 for "... in use has been ..." write "... has ..."
9 delete "as long as "
13 for "been inadequately" write "not been adequately"
13-14 delete "or washed out"

15 delete "and desirable"
16 delete whole line
17 insert "However, "before "almost"
17-18 for "are part of" write "were selected from"
18 for ", often" write "or"
19 for "When " write "Commonly, "
19-20 for "it is usually ... example, " write "or"
21 after "examination" write a semicolon instead of a period
22 for "Such " write "such " ; for "the specimens" write "those"
23 delete "critical"
24 delete "on a "=20
25 for "remove type ... remounting" write "remount type specimens."
GENERAL: The first paragraph of your section "2" can be deleted without making your arguments appreciably weaker; I can't see that it adds anything,
in this context.

PAGE 4
1-3 I would rephrase the first sentence as follows:=20
"In time, specimens can shift location and/or position on strew slides made
with media that are thermoplastic or remain viscous after preparation."
3-4 I would rephrase the text on these lines as follows:
"The marking of specimens for future ..."
6 for "described" write "not described, or "
8 after "describer" , for the period write a semicolon=20
8-11 for entire clause in parentheses, write outside parentheses:
"; even then, microscope maintenance may affect the reading of such stage coordinates."
12 delete "location per"=20
13 should you cite a page in Traverse 1988?
14 after "holotypes" write period for comma
14-18 delete "and even ... Techniques for"

18 for "marking." write "Markings of "
"18-19 delete "also imperfect and "
19-20 delete entire last sentence of paragraph. disagree utterly!

23 for "almost always " write "generally"
24 delete "to thousands"
24-25 for "are often ... materials under" write "often require"

PAGE 5
1-2 for "slides and ... and have" write "slides, even if they "
3 for "Furthermore, there " write "There "
6 for "The " write "This "
7 delete "thus"
16 delete "eventually"
19 delete "collection of "
23 for "must" write "having to "

PAGE 6
1 Here you talk about " a solution to this problem," but to me it is not
clear, from your preceding discourse, what the "problem" is: was it stated
anywhere? Maybe it would help to rephrase, and posit something like:=20
The nomenclatural consequences of applying the ICBN to the prevailing circumstances described in the foregoing have led, and will lead to more, confusion. The remedy for these nomenclatural problems is clear from what
is contained in the ICBN already:
[However, this is just off the cuff, and it will need a bit more thinking and careful use of language to find a really felicitous wording that will go
down well with the voters on this proposal.]

4 after "medium" insert ", or those on SEM stubs or in TEM sections –"
8-9 for "hasn't ... to happen." write "isn't happening."
11 for "alternatives clearly" write "alternative"
15 for "very" write "indeed"
16-17 for "apparently never" write "not"
17-18 for "at previous botanical congresses" write "sooner."

PAGE 7
9 after "wording" insert "already" Good!
10 for "so that there is no" write "to remove any"
15 for "ICBN" write "the ICBN"

PAGE 8
Instead of the long litany of essentially identical phrases contained on this page, I suggest that, following the cited text of Art. 9.1, you say something like:=20
Similar wording, mutatis mutandis, is contained in Arts. 9.2, 9.3, 9.6, 9.7, ?
9.7 (footnote), 9.14, 10.4, 37.3 and 37.5. Good

PAGE 9
4 after "impossible" insert "or"
5-6 delete "for whatever reasons"
17 after "objected that" insert "many of "
18 delete "in many cases"

PAGE 10
As far as I'm concerned, you may delete the "Dr." qualifications.=20 do you not have doctrate

Earlier, I said that the "problem" had not been stated. The problem is not
that there are 28 000 types. It is, rather, that type slides may contain
several types, published at different times, in studies from different
institutes or technical bases; and consequently, the slides have wandered,
and are easy to lose. Again, because the type slides are so small, they are
easy to lose, overlook, or carry away to a small "safe" place that remains
hidden for later researchers. Moreover, the explosion of taxonomic papers in
palynology were predicated by the use of the fossils in companies or
surveys, in response not to pure scientific curiosity, but to the need for
biostratigraphic information in industry. The work was often done in places,
therefore, that were not committed to preservation of the types. Now, with
the sharp reduction of numbers of industrial palynologists, the types are
often "lost" in limbo, before they disappear altogether. In most cases, the
illustrations are adequate to support the nomenclature based on the type
specimens, and can serve as a surrogate. Hence, there is little reason not
to legitimize that concept. Although organic-walled plant microfossils may
be one degree more permanent than things like bacteria, or others that now
are allowed to be typified by illustration, they are in the longer term as
vulnerable and impermanent. This notion has been realized only in the last
few decades, in part because palynology is not much older.

I like it but they would take over large

The above lines are my attempt to tersely formulate why we want the changes
to the Code. If I had a few more days, I would come up with something else.
However,,, it is time for me to close this. I may have given you more,
already, than you asked for. It is your proposal, so do as you see fit. I
hope sincerely that you will be successful.

All best wishes on this, =09

Jan.

Jan Jansonius
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403-292-7179 (FAX: 292-6014)
jjansonius@gsc.nrcan.gc.ca

11 Stanley Place SW, Calgary, Canada T2S 1B2
403-243-3937
jansonij@cadvision.com

From: Jan Jansonius <jansonij@cadvision.com>
Return-path: <jansonij@cadvision.com>
To: bettyandal@juno.com, fensome@agc.bio.ns.ca
Date: Tue, 20 Jan 1998 19:01:23 -0700
Subject: MS
Message-ID: <1.5.4.32.19980121020123.0069f868@cadvision.com>
X-Status: Read
X-Mailer: Windows Eudora Light Version 1.5.4 (32)

Dear Al,

Tuesday afternoon I received your parcel. I started going through it at once, and think I have some substantial proposals for modifying your text. Then, there also are a lot of marks of the kind my Bettie makes in my first/second drafts. I think that a significant number of words can be weeded out, without really affecting your argument, but with the prospect that more readers might be sufficiently undaunted to tackle and read the text. To me, it now looks too long. I have a meeting tonight, so will not be able to get at it immediately. However, if the meeting ends early, I can make a start, and then continue Wed AM – I plan to stay home tomorrow, end e-mail you from here.

For starters, to give you something to mull over in the mean time, I suggest that you stress too much the transparent light palynomorphs, and not addressing in the revised Art 8.3 those studied by SEM/TEM.

So, how does this look?:

8.3. If it is not, practically, possible to preserve permanently a specimen as the type of a name of a species or intraspecific taxon, or if such a name is without a type specimen, the type may be an illustration. For plant microfossils, that are normally studied microscopically, permanency of the preparation and morphological integrity of the specimen is so problematical that an illustration of the holotype specimen may serve as nomenclatural type.

Should this be effective as of now? In 3 years? As of 1958?

Jan

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jansonij@cadvision.com

From: bettyandal
Full-Name: Alfred or Elizabeth I. Traverse
To: jjansonij@cadvision.com
Cc: fensome@agc.bio.ns.ca
Fcc: Sent
Date: Wed, 21 Jan 1998 10:59:47
Subject: response to sugg.
Message-ID: <19980121.105859.7567.1.bettyandal@juno.com>
X-Status: Sent
X-Mailer: Juno 1.38

Dear Jan:

Yours of 20 Jan. just read. As I thought I had responded positively to almost all of your suggestions for the previous version of the MS I am little startled by the news that you now have "substantial proposals for modifying the text." The present version is, I believe, shorter than the first version because I removed substantial chunks here in there in response to both your and RF's suggestions.

You can be helpful with info from your indexing work: how many holotypes do you believe exist for palynomorrphs as SEM specimens? TEM I can't believe would be involved at all, but educate me. I had stayed away from SEM because I am not sure that the impermanence argument applies. (The larger megaspores could be mounted on cards and treated like herbarium specimens, and I didn't want to include them either. In fact, I would really like to set a size limit of 500 micrometers.). In brief, I hadn't intended to include electron microscope preparations at all but was willing to let future developments instruct us how to precede about them.

I have problems with your proposed wording for the actual emendation, inasmuch as "possible" is not a comparable adjective. It is either possible or not possible, period. Impractical is another matter.

Very important: NO, I didn't want ANY time frame in the proposal. I intend it to go back forever. That's the problem, 20,000 missing types! Back to Potonie senior in the last century.

I do greatly appreciate your editorial efforts, and I will listen carefully and do my best to respond adequately. Thank you very much.

Have to pick some grapefruit. Best.

Al

From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
Return-path: <JJansoni@NRCan.gc.ca>
To: "'bettyandal@juno.com'" <bettyandal@juno.com>
Date: Mon, 19 Jan 1998 14:26:18 -0500
Subject: RE: ice message
Message-ID: <C72C9282075AD111AF6300805FFE1C8F033C4D@s5-cal-r1>
X-Status: Read
X-Mailer: Internet Mail Service (5.0.1458.49)

Dear Al,

The funny codes represent letters with an accent or other diacritic marking. Usually I can see what they would be like. I don't know why the program wouldn't simply delete the symbols from the letter, but that is asking too much. However, as I can't write code for a computer, who am I to criticize?

I can fathom why you would dislike reading French (maybe for lack of training), but why would you "despise" it? I don't read deep intellectual works in French, but do get pleasure out of the original phrasing of Maigret books by Simenon, say, or Voltaire's Candide. And I used to get a kick out of reading Beaudelaire's Les fleurs du mal, out loud when no one was in earshot. There are no more despicable French as in other nationalities,

Waiting for the priority parcel,

Jan.

> ———
> From: bettyandal@juno.com[SMTP:bettyandal@juno.com]
> Sent: Sunday, January 18, 1998 8:06 AM
> To: jjansoni@nrcan.gc.ca
> Subject: ice message
>
> Dear Jan:
>
> My Canadian cousins are getting a kick out of the bilingual message
> you
> copied to me about the effects of the ice storm on government
> employees.
> As a former Bureau of Mines employee I identify easily with the
> "officialspeak."
>
> However, I should tell you that the message was barely legible, being
> filled with inexplicable symbolic business. The most common one is
> >=20,
> but G=E9n-E9 is also very common. When the French part starts, there
> are
> so many sets of such symbols that I suppose I couldn't read it even if
> I
> could read French (which I despise).
>
> This has happened before with things that are "copied" to me, and I
> wish
> I could figure it out.
>
> Best. Al.
>

From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
Return-path: <JJansoni@NRCan.gc.ca>
To: "'bettyandal@juno.com'" <bettyandal@juno.com>
Date: Fri, 16 Jan 1998 12:42:40 -0500
Subject: RE: proposal
Message-ID: <C72C9282075AD111AF6300805FFE1C8F033C44@s5-cal-r1>
X-Status: Read
X-Mailer: Internet Mail Service (5.0.1458.49)

Hi Al, Just opened your e-mail (I was cut off for a few days because the office is in the process of switching to new programs – can't seem to stop MicroSoft). Nothing has arrived here yet; maybe you expect too much from our postal system, and it may be that in the east (much mail now gets channeled through the Toronto hub) the effects of the ice storm are still felt. After all, in Montreal this is the first day that businesses in the downtown core are allowed to open and resume business.

As soon as your MS comes in, I shall read thru it, and e-mail back comments as per your instructions. Might it be worth sending the file (as an attachment) thru e-mail? We could refer to it by parafraph, or even by referring to keyword(s) that the computer can easily find?

Greetings,
Jan

> ———
> From: bettyandal@juno.com[SMTP:bettyandal@juno.com]
> Sent: Wednesday, January 14, 1998 1:33 PM
> To: jjansonius@ess-dns2.gsc.nrcan.gc.ca
> Cc: fensome@agc.bio.ns.ca
> Subject: proposal
>
> Dear Rob and Jan:
>
> I have now gone through my MS with both of your letters and edited
> copies, making most of the changes you both suggested, and some others
> either Betty or I came up with. In a few instances I thought your
> suggestions changed my meaning, and I found some other way of doing
> what
> I thought you wanted, without damaging my ideas.
>
> I am about to head to the nearby shopping center on my bike to make
> the
> xerox copies. Then I will go to the post office and see what I can do
> about a swift delivery time.
>
> When you get the MS, please (I'm sorry!) read it again and then e-mail
> me
> last minute suggestions, with page and line nos. From Rob, as you
> will
> see on p.2, I still am hoping to get an acritarch number (doesn't
> count
> tasmanitids, etc., of course, but that's small potatoes).
>
> I am hoping to get this thing off to Taxon no later than next Monday.
>
> Best and many thanks. Al
>

From: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
Return-path: <JJansoni@NRCan.gc.ca>
To: bettyandal@juno.com, "'fensome@agc.bio.ns.ca'" <fensome@agc.bio.ns.ca>
Cc: "Jansonius, Jan" <JJansoni@NRCan.gc.ca>
Date: Fri, 16 Jan 1998 13:28:04 -0500
Subject: RE: Counts
Message-ID: <C72C9282075AD111AF6300805FFE1C8F033C49@s5-cal-r1>
X-Status: Read
X-Mailer: Internet Mail Service (5.0.1458.49)

Hi all, If this works, both Al and Rob should receive this. I think Rob hit the hammer on the nail when he said "names" (not= spp/taxa). I also concur with the boundaries getting hazy at times (esp. early times); maybe for this exercise we should go back in time and refer to the totallity as "Hystrichospheres"?

My fax number is (403)292-6014; that of Rob I think is (902)426-4465

Jan.

> ———
> From: Rob Fensome[SMTP:fensome@agc.bio.ns.ca]
> Reply To: fensome@agc.bio.ns.ca
> Sent: Friday, January 16, 1998 3:19 PM
> To: bettyandal@juno.com
> Cc: jjansoni@nrcan.gc.ca
> Subject: Counts
>
> 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: jjansonius@gsc.NRCan.gc.ca
Return-path: <jjansonius@gsc.NRCan.gc.ca>
Reply-To: <jjansonius@gsc.NRCan.gc.ca>
To: <traverse@ems.psu.edu>, <fensome@agc.bio.ns.ca>
Date: Mon, 5 Jan 98 13:37:01 EST
Subject: ...no subject...
Message-ID: <vines.2JI8+rqGgoA@vns601c.gsc.nrcan.gc.ca>
X-Status: Read

Good morning to you both, Al and Rob, and a happy New Year it may be for you!

I am currently immersed in the fungi compilation of Kalgutkar, and try to put my view of nomenclature on it. I submit the text below for your perusal – and criticism, if you think I'm off the mark. My only hesitance in standing by my text is that I don't have a pre-1956 Code, and I don't know how Art. 36 was phrased in it. {THE INTERSPERSED TEXT IN CAPS IS ADDED BY ME ONLY IN THIS E-MAIL}

Al knows that I have fretted before on the topic of validly published genera that did not have any content of species when they were (validly) published. Apparently, that was not uncommon in botany in the early days. In palynological literature, I have always looked for any argument that would argue against their valid publication.

This is all for now. I finished the update for the Genera File over the holidays; all I have to do is stamp the card numbers on each page. I'll hold off till I hear from Len that he wants to take it to the printer: it is conceivable that there are new genera in some of the magazines that appear to be backlogged (due to the postal strike, still?), like Palynology (did you get your copy yet?). I can easily fudge some more genera in, but the renumbering on the pages would be awkward. The update will contain 296 pages (cards).

(Al, what was your home e-mail address again; I seem to have misplaced it).

GENUS: Dyadosporites van der Hammen 1954 ex Clarke 1965, p. 90.

Dyadosporites van der Hammen 1954, Bolet=EDn Geol=F3gico, v. 2, no. 2, p. 15 (nom. nud.).

TYPE: Dyadosporites ellipsus Clarke 1965, p. 90, pl. 1, fig. 13 (by monotypy).
Non: Dyadosporites umirensis van der Hammen 1954, l.c. (nom. nud.).

ORIGINAL DIAGNOSIS: Fungal spores bilocular (didymosporous), elliptical, central septum simple, cell wall psilate to finely

puntate pore at apex of each cell.

punctate

AUTHOR'S COMMENTS: Fungal spores do not occur as true dyads (half of a tetrad group) but rather, occur as didymospores (two-celled spores). Even though the generic prefix dyado- is misleading, the generic name Dyadosporites must be used here because it is the first validly published name for fungal spores of this type.

where is this concept from? needed for readability

OUR CLASSIFICATION: Fungi Imperfecti, Didymosporae.

OUR REMARKS: Jansonius & Hills (1976, card no. 882) remarked that "although van der Hammen gave a diagnosis and the name of the type species, the latter was never described [or illustrated]. Thus it is here considered that his form-genus was a nomen nudum or a provisional name, because no species were assigned to it."

no illustration

use one version

(The edition of the I.C.B.N. then in force appeared not to require a content of species, but only a validly published generic diagnosis. However, Art. 36 of the 1956 ICBN stated that "In order to be validly published, a name of a new taxon of fossil plants published on or after 1 Jan, 1912 must be accompanied by an illustration ... showing the essential characters, in addition to the description." {I TAKE IT THAT THIS GOES FOR ALL RANKS OF TAXA; THE CODE DOES NOT RESTRICT THAT. JJ.} The 1994 Code is clear in requiring a type specimen for a generic name published from 1958 on. This type specimen must be the type of a species. In the 1994 Code, the old Art. 36 is now Art. 38; however, the words "new taxon" are replaced by "new taxon ... of specific or lower rank." We here take the radical point of view that once a taxon has been declared invalidly published, and still-born, under the Code then in force, it cannot be resurrected merely by later changes in the Code.)

who did that?

Clarke [1965] treated the genus Dyadosporites as having been validly published, although it is here considered that he was the first author to validly publish the generic name, and the first to assign a species [Dyadosporites ellipsus Clarke 1965] to it; the latter becomes the type species through the principle of monotypy. Dyadosporonites Elsik 1968 is a junior taxonomic synonym of Dyadosporites.

To: jansonij@cadvision.com
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: response
Cc:
Bcc:
X-Attachments:

Dear Jan:

Had to stay home yesterday because there was huge activity re the new addition to our home. Eight enormous trucks came with shale (Middle Devonian) and crushed dolomite (Late Ordovician) for the platform leading to the new garage (the old garage becomes my Arbeitszimmer). The electricians were there to work on the lighting for the new garage, the door openers, etc. The head guy for the contractors was there to work on the plans for the abovementioned work space. Etc. In my spare moments during the day I planted about 200 bulbs for next spring: tulips, daffodils, crocuses, Chionodoxus, Scilla. Also sorted six bushels of apples picked by my high school boy while we were in Germany. Mostly from one tree, which makes the best apples I've ever tasted. Also roasted this year's crop of chestnuts.

So, your messages of Sat.-Sun. are being digested on Tues.

As I told you, I had no way of knowing if I'd already sent you corrections on some sheets--they DID look familiar. Also, it is true that I have trouble sometimes deciding what is JJ and what is quote. (Of course, since you translate foreign language material, you become responsible in a secondary sense, nicht wahr? Reminds me of secondary relics in seminary days. Hmmm. As you know from previous correspondence, I have always felt strongly that you should indicate that these things are translations, state the original language and give the name of the translator, unless it is one of the editors)

As far as I know, the single germinal in angiosperm pollen is invariably distal--for grasses, lilies, palms, Magnolia, the works. Therefore should be sulcus. The gymnosperm/angiosperm distinction in this instance is irrelevant.

I think of palynological terminology as being distinct to palynology. In this case I have to admit Erdtman was right. Somewhere I have a blistering response from the GREAT MAN after I pointed out to him that his use of "monotreme" was unfortunate, because the term was already in use in zoology (for egg-laying mammals). By using "levigate" you are being quixotic (even if right).

I think an author has the right to change Dettmann to Detman if he likes. It jars me that folks are now, after all "correcting" Engelhardia to Engelhardtia. I guess Linne's several "pensylvanica", "pensylvaniensis" will be next. For years we've taught students that if L. wanted Acer pensylvanica, it was o. k. Reminds me of famous quote of A. Lincoln. His wife was Mary Todd. Lincoln was not too fond of his (Southern, aristocratic, slave-owning) in-laws and said: "One d is enough for God, but the Todds require two." Whether the fact that R. says the epithet comes from Latin entitles you to alter the name into your concept of what he

should have done is certainly debatable. It would be at least as ok in my opinion to regard it as indeclinable. (And I thank you for teaching me how to spell that word!)

Back to work. It will amuse you that I am writing an article for Taxon in which I propose that illustrations be accepted as types for microfossils.
The fact is that very few actual type specimens are really available for study. We work in fact from illustrations. Even if an elaborate scheme for
prepartion of 25,000 neotypes or lectotypes were put in place (how many millions of dollars?) they also would degrade and/or vanish in a few decades. When I finish the piece I will send it to you for comment.

All the best. Al.

From: jjansonius@gsc.NRCan.gc.ca
Date: Mon, 8 Dec 97 14:18:01 EST
To: <traverse@ems.psu.edu>
Reply-To: <jjansonius@gsc.NRCan.gc.ca>
Errors-to: <jjansonius@gsc.NRCan.gc.ca>
Subject: ...no subject...
X-Incognito-SN: 100
X-Incognito-Version: 4.10.136

Dear Al,

I received your letter and MS on Friday (the first day of deliveries after the postal strike), and have read through it a couple of times since then. I have suggested a number of small adjustments and some deletions. I understand, from various people, that Greuter runs a tight ship, and looks for conciseness.

I also saw on TV the results of the dump of snow you had, and don't know if you can reach your office to read this. I tried to find your home e-mail address, but forgot where I put that. Maybe you should give that to me again.

Anyway, I plan to fax the annotated MS to you, so you can see where I mucked with things. It will be rather confusing (smell the roses?) to try to explain it by sentence/paragraph numbers.

A few more philosophical things: should you broaden your argument and accept an illustration for all paleobotanical taxa? If you want to stay out of that, you'd have to give bounds for what is included in your proposal: megaspores, seeds, fungal stuff? Should you speak only for "acid-resistant organic-walled" microfossils? Should there be a size limit? This will unbedoubtedly be asked by reviewers, and might best be answered now.

Some parts of your text have an aggrieved tone (to which I am tuned, because I tend to argue things that way, and immediately get a reaction from my wife) that may not be the best manner to convince people of the strength of your proposal. Rather, I'd stress that it is amazing that no one has yet proposed what you are espousing: it is overdue.

I shall fax the MS pages later, and set the machine so it will transmit overnite when the phone rates are lower. So, if you can get in to the Deike Bldg., you should know to look for it.

Yes, I agree with your position. It would be interesting to know why fossil plants have been treated in such exceptional ways in the Code. There are more productive things to do, though.

I won't make it in 1997, the Genera File will prob. be going to the printer in the first week of January. Can hardly wait so long!

Have a happy holiday, and don't overdo the snow shovelling. My neighbor had a stroke doing hard yard work (2 months ago) and now is reduced to lolling about, reading etc., but not capable of doing anything productive - - we hope Physiotherapy will get him out of that, but he suffered a lot of damage. We must realize that we are getting old. Happy holiday, again, anyway (although we may be in touch before that passes?). Greetings,

Jan.

To: jjansonij@cadvision.com
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: apples, etc.
Cc:
Bcc:
X-Attachments:

Dear Jan:

You seem to be staying home a lot lately. Hope you are o. k.

I just mailed off copies of my proposed MS for Taxon--I sent it to 3303 etc., but maybe I should have sent it to your home address. Is it the same as I have on card from 20 yrs. ago? Hope you can tackle the MS right away, as I should get it off to T. soon.

The one (of 15) apple tree that makes sensationally sweet apples is a "Spigold" Or maybe they spelled it Spygold--I think the former. Came from the same source as all of my fruit trees, Miller Nursery in Canandaigua, NY. I try ner to buy nursery stock from places south of here.

I'm glad you take my criticisms in (mostly) a kindly light. My feeling is that I can't be helpful by muffling my fire.

Yeah, I feel great, though I have either to take naps or caffeine to get through a long day. However, the construction projects don't prove much about me--the workers are doing the job---well, my helper and I will remove a ton of broken up concrete on Saturday--resulted from breaking up a slab that used to occupy the location of the front of the new room. It will go on one of two roads in the woods: Rte.Traverse-2. Sooner or later we will also have to complete flooring of the new attic and move the shelving, etc., from the old garage to the new garage. The old garage becomes my work room and will also have a lot of shelves, but in different places and more esthetically pleasing in quality. Ye gods, the old shelves were put up on two rainy Saturdays by a helper and me 20 years ago. The then 15 yr. old kid is now manager of a local store, with a wife and school-age children.

Time for lunch. Best. Al.

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

13 Nov., 1997

Dr. Jan Jansonius
Institute of Sedimentary & Petroleum Geology
3303-33rd St. NW
Calgary, Alberta T2L 2A7
Canada

Dear Jan:

Enclosed is the preliminary MS I said I'd be sending. I propose to get it into Taxon asap.

Could you please let me have the MS back with your suggestions?--both as to particulars and generalities.

Thanks in advance and all the best.

Yours very truly,

Alfred Traverse
Professor Emeritus of Palynology

encl.: MS

X-Sender: jansonij@cadvision.com
Date: Tue, 11 Nov 1997 17:06:37 -0700
To: traverse@ems.psu.edu (Alfred Traverse)
From: Jan Jansonius <jansonij@cadvision.com>
Subject: Re-response

Hi Al,

Just read your response; I agree with much what you say -- to varying degrees. Yes, I feel myself responsible for what is in the File. If I change a word or sentence in quoted material, I'll annotate it with "JJ." But most of my comments are in separate "Remarks"at teh end of the entry. As for translations, even if I have it translated (by some Chinese or other, who knows Chinese well enough but English not so good), I may mess in the wording, word order and even sometimes the interpretation of the Chinese. I have, long ago, learned about something called "benign neglect," a notion akin to the attitude "if it ain't broke, don't fix it, already"; it makes me do things that I am comfortable with, without feeling the need to document every niggling detail before being asked to justify.

I am happy with your initiative to make illustrations equivalent with types, at least in palynology. I'll gladly read it and discuss it with you. There isn't much time left, either, for such proposals. Rob F. is working on a few, too.

Do you know the name of the fine apple you are enjoying? (Wish I could get a piece of it to graft it onto my tree...).

Best of luck with the construction projects -- you sound like you are full of vim and vigor. Solong,
Jan.
>

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X-Sender: jansonij@cadvision.com
Date: Sun, 09 Nov 1997 17:22:10 -0700
To: traverse@ems.psu.edu
From: Jan Jansonius <jansonij@cadvision.com>
X-MIME-Autoconverted: from quoted-printable to 8bit by pangaea.ems.psu.edu id TAA29176

Hi Al,

Yesterday I sent you an e-mail with comments on your corrections. I thought that there was more, and I now found those pages on my desk.
So, if you are still interested in it, I'll have a few more comments for you.

I have an eery sense of deja-vu in writing this letter, but don't have a copy of it on my home computer: did I send this already from the office? - anyway, I have added a statement in the Intro of the Addenda/corrigenda that all our text is in English, in many cases translated by ourselves, but sometimes (edited) from English versions in the protologue. And, that we do not, as a rule, provide a source for these translations.

I agree with you that "emendations" have been badly overdone in palynology, but even when a new so-called emendation adds nothing to our understanding, it is useful to have the possibility to verify that in the File - so you'll know that you don't have to ask for another inter library loan to check that out. It is a precarious balancing act: I don't want to give publicity to attention seekers, but I try to provide as much information as people might want to have at their fingertips.

As for Pseudoaquilapollenites: I didn't make a comparison with Duck Choi's genus, because there are many comparisons to be made along that line. Art Sweet thinks that, regardless, there is no reason not to simply use Aquilapollenites.

Re Platanipollis / Platanoidites, the latter having only "one pore per colp": Grabowska in this (quoting Pacltová 1982) made a comparison with modern Platanus / Platanipollis, in which, she says, the endogerminal has an ill defined, ragged edge (making it look as if there are three lalongate endopores stacked one above the other). I am just quoting these good folks, and have no intimate knowledge of these pollen.

Hamiapollenites Wilson was published in January 1962, Hamipollenites Jansonius was late in the summer that year.

Some of your objections pertained to stuff that I quoted (often in edited form), and therefore cannot alter. Those are the bits that follow in the hanging indent, without me going back to the margin.

I think this is all. Solong, then,

Jan.

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X-Sender: jansonij@cadvision.com
Date: Sat, 08 Nov 1997 17:03:36 -0700
To: traverse@ems.psu.edu
From: Jan Jansonius <jansonij@cadvision.com>
Subject: File corrections

Hi Al,

Thank you for the note announcing your return from the Heimat (it seems like you do feel at home there, at least in the non-smoking areas). Glad you had a good time.

I want to thank you for the pile of annotated pages you sent me just hours before you left. I found them most helpful. Your critical eye helped me see better. As you may know, I'm assisting Ram Kalgutkar in finishing his compilation of the fossil fungi. In so doing, I came across a motherlode of new genera (i.e., ones not in the File), and included a good number in the current MS. You were right, however, that not all of them warrant inclusion. Some are not even likely candidates to survive the acids. And, yes, in the Addenda/corrigenda there are a good number of corrections that you had sent me over the last several years.

On the MS for Xuipollis, I made a comparison with Brenneripollis, which I said was monocolpate. You suggested that should be monosulcate. I am not clear on the correct use of those two terms: I have used sulcus when dealing with gymnosperms, and colpi when dealing with angiosperm. And, of course, Russians and Chinese use words/characters translating as 'furrows' or 'grooves'. If, in a tricolpate, the colpi are equatorial, don't we assume that this form developed from a monocolpate form? And, how do we determine whether in Liliaceae the furrow is polar or equatorial (sulcus or colp)? I guess that would involve a study of the polarity of the pollen content, in a live grain. I assume that type of study has been done a long time ago. Maybe, even, the results are included in the three volume book, or in your Paleopalynology - but I am working at home right now, and have no references here. After your questioning, I changed '-colpate' to '-sulcate' on the card for Xuipollis. I'll have to educate myself on this one, however, at some time.

I double-checked some German uses of caps (Monatsberichte der Deutschen Akademie...; Zeitschrift der Deutschen geologischen Gesellschaft), and they do use caps in the original. I guess, they think of it as "Deutsche Akademie..., Monatsberichte thereof"?

I've used "levigate" for smooth/psilate/polished, because in all English/American dictionaries that is the only form indicated ("laevigate" was found in one as 'obsolete,'); levigate is what botanists also refer to as glabrous. It is derived from the Latin levis=smooth, cf levigare=to

polish, make smooth; and hence levigatus=made smooth. Laevus is left (and 'stupid'!), and found in words like laevorotatory, etc. I don't know where the widespread use of "laevigate" in palynology originated, but it is, I believe, anomalous, and restricted to our little world (e.g. in the glossaries you referred to).

I have modified the entry on Entylissa; I can send you a xerox or fax of the new adjusted text. My argument is (remains) that Ishchenko validated the generic name, and was the first to assign species to it. Pot. & Kr. 1954 assigned Azonaletes caperatus as type (neotype species), the same species assigned as type species of Ginkgocycadophytus (with lectotype specimen: Liuber in Liuber & Val'ts 1941, p. 72, pl. 16, fig. 256a) by Samoilovich 1953, p. 72. The latter thus becomes an obligate jun. syn. of Entylissa, even though it received its type a year earlier. [I know that you feel inclined to accept Naumova's 1939 paper as valid publiction of all the names she included in her diagram; but, if you'd do that, you'd have to upset a number of other names that then would need conservation in order to maintain stability in the nomenclature.]

Under ACROSTYCHUMSPORITES I used the words "coarse roster"; I guess I lapsed into Dutch (and the Germans may use the same term: roster or raster). But, a check with my colleagues showed that what I had meant to say was: a "coarse screen."

Finally, regarding the freedom to correct a name. The Code indicates that some typos and orthographic errors MUST be corrected. Examples of things to be corrected, for instance, include the confusion between albus/altus (white/tall). I think that, say, Detmanispora should be corrected to Dettmannispora. However, although the Code gives us freedom to compose a word in any way, it also states that names are Latin, or treated as if they were Latin. Thus, if Richardson - who will admit to having very little Latin - states that the epithet in Acontotetras inconspicuis is derived from Latin inconspicuus, an adjective, then I will treat the modified ending as a typographic error. The adjective inconspicuus can be declined in the normal Latin way (inconspicuus, inconspicua, inconspicuum); but what is "inconspicuis"? Is it a noun in apposition? Is it a plural ablative? Is it a newly invented adjective (inconspicuis, inconspicuis, inconspicue)? Is it a fantasy name, meaning nothing? Since Richardson committed himself and said the name came from Latin, it should have to be recognizably correct to a Roman. I agree with you that the Code is somewhat subjective and irrational in its interpretation of what may, what may not, and what should be corrected (and generally is less tolerant to corrections in generic names than in adjectives). However, the examples it gives cover a fairly wide swath, and I think that, in particular, Latin adjectives used as epithets should be given in their correct form, enabling later workers to decline them as needed without running into problems. Don't you agree that it is one

thing to allow complete freedom in coining a new name, but another to use that freedom as an excuse to justify (and give diplomatic immunity to) lazy and/or incorrect uses of (pseudo)Latin?
The fact that increasingly people have no notion of Latin and its elementary rules is no excuse, either, for shoddy work. It is part of the whole package of producing new nomenclature (which is a prerequisite for doing taxonomy). One can always find a willing volunteer to check the Latin. Or, do you think that, since Chaloner talked a congress into accepting descriptions in "English or Latin", we will soon see a proposal to have names in either of these two languages? (Unhappily, I could find some examples in the existing literature, already.)

Well, Al, I'll get on with producing the File, and produce some more masters for the printer. I'm almost there, but still have some 12-15 Chinese ones that I have only in (incompletely translated) Xerox copies, with poor plates. I asked Mao Shaozhi for help, but a lot of Chinese have retired in the last few years, and apparently, they are no better organized than most of us, and records are hard to find and verify for those taking over. (I know how that is with regard to the records in the old Imperial Oil lab.) Also, the old generation does not have a personal relationship with the new one, and thus communications are frustrating. Oh, well!

My regards to Betty, best wishes to you, and thanks once more for making me look at things with less preoccupation.

Jan.

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14 October, 1997

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Dear Jan:

As I just e-mailed you, I worked on the great sheaf of papers whenever possible in UT-ID-OR-NV.

The Addenda provoke only a few comments:

4580. Polyvestibulopollenites is not italicized. Nor is Alnipollenites.

4817. Again, the epithets are not italicized.

4819. Same problem, with Sofrepites and Stephanoporopollenites.

4383. Presumably it is Tiwari, not Tewari.

309. Psiloschizosporis needs to be italicized.

4667 & 1036. Generic names need to be italicized.

4883. Same non-ital. problem.

1122. do

4739. italicize gen. name

4948. do

2388. do

4964. ital. gen. names

4969. do

Some of these corrections look familiar--as if I had once called your attention to them.

Now for the sheets. I ams sending you herewith all of the sheets

on which I have scribbled in red. This will require you to look at both sides! I am returning only the sheets with notations.

Abditusdyadus Descr. 1. 2. "Levigate" I suppose is from the island dialect of English. Every glossary I checked has "laevigate," and you should change it. Actually, I much prefer psilate.

Acontotetras. Why is this orthographic "correction" different from changing Linnaeus' "pensylvanica" to "pennsylvanica," which I thought everybody agrees is wrong? I find no place in the Code (Arts. 23, 45 & 60 seem to be the pertinent ones) that says an author cannot have his own concept of correct Latin, except insofar as grammatical matters are concerned. In fact, the Code says that an author can even make up names out of whole cloth--an arbitrary combination of letters.

A number of places: you have a tendency to verbosity occasionally: "were not published in a valid manner." This should be: "were not validly published."

Dipterocarpacearumpollenites I am not happy about this one at all. You get into a whole lot of stuff that is not nomenclaturally relevant and clutters up the story.

I think you tend to give too much attention to emendations. They have no nomenclatural standing as such, and you are in effect encouraging the idiotic use of "emend." as if it were part of the name. As you know, some people even give the emending author in parentheses.

For the rest, I will ask you to read the red marks on the original sheets, enclosed. I have to get packed for Germany.

All the best. You are really producing a nice monument to your career with these cards.

Yours very truly,

Alfred Traverse
Professor Emeritus of Palynology

enclosures:MS sheets

To: jansonij@cadvision.com
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: reporting in
Cc:
Bcc:
X-Attachments:

Dear Jan:

We are back from the Utah-Idaho-Oregon-Nevada trip and leave in 48 hours for three weeks in Germany.

While away I worked whenever possible on the stack of sheets from you.

I now have your e-mail of 28 Sept. From it I deduce that it will work just as well to use Word Perfect and snail mail to send you what I found. I was working without any files, and it could be that much of what I thought is somewhat trashy for that reason, but I'll send it anyway.

Thanks for looking into whether there's a Canadian Philatelic Service in the Post Office Department.

Best. Al.

Ever been to Crater Lake? That is one incredible place. It was snowing when we were there.

To: jansonij@cadvision.com
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: package
Cc: jjansonius@gsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Jan:

Just to report that the package arrived on Friday (19 Sept.). I wonder how much it would have cost to have sent it by one of the fast delivery services? Anyhow, I will get at this task immediately.

Now for a Bob Clarke-like question. I am a sort of philatelist. That is, I collect stamps that have plants on them. (Did you know what fossil plant is the only one ever to appear on an American stamp? I know of one that was on a Canadian stamp, but am not sure it was the only one.) Does the Canadian Post Office have a philatelic branch, as the US one does? I would like to obtain the whole set of the neat plant stamps like the ones on your pkg. Please ask at a P.O. for the address of the philatelic service, if there is one, and I'll get their catalog.

We leave for Utah a week from tomorrow, that is if Sid Ash can get out of a jury summons for the day our field trip to Oregon is supposed to start. We'll be back from that for just five days before we leave for Germany.

The addition to our house in which you expressed mild interest is now under roof. Betty is very conservative and is unhappy that it makes our establishment look a little more baronial--she is very low profile by nature. I love it--the only thing about the "Princess" Diana deal that really interested me were the aerial shots of the Spencer spread--wow!! What a layout!! Then I read that in recent centuries they had been rather down on their luck and didn't really own the whole thing any more.

From the Adirondacks I can report that our parking area is being badly eroded and will require 5K to fix. Also, big news, turkeys have had a population explosion and are all over the place. Great. Deer also. Not so great. It would make sense to reintroduce mountain lions, but of course along with the deer, they would take the odd dog, cat or inattentive juvenile human.

Best. Al.

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

Dear Jan:

Yours of 4 September was somewhat puzzling to me, as much of it assumes I know things about your project that I don't know--you have said very little about the project as such--we have just discussed problems engendered by it.

But apparently something is on the way, I hope with a little covering note--or will it all be obvious?

The Add.-Corr. have seemed very important to me, and I very faithfully annotate all cards when they come in. That's how I know that the mistake in Paläbotanik hadn't been previously noted.

I keep my "cards" alphabetically in three-hole notebooks. 34 binders so far! Folks who have them loose in shoeboxes have no chance of being systematic.

Cheers. Al

From: jjansonius@gsc.nrcan.gc.ca
Date: Thu, 4 Sep 97 17:52:18 EDT
To: <traverse@ems.psu.edu>
Subject: re: thanks

Dear Al,

I just copied what I have for the File (40 minutes); I'll dump it off myself at the post office, I can hardly stick the survey with the cost, and anyway, it might get delayed by the weekend interlude.

On my disk the thing looks cleaner, and more consistent (e.g., all "v." now are "vol." What I would like you to look for most is the interpretation of questionable things. Most of the newer things are humdrum, but even there we may have overlooked some things.

I'll leave for Boston (and Wood Hole) over one week (Thu), then come back on the 21. I hope to start printing the things on landscape format before then, and expect that the whole job will take three weeks. In that time I hope to finish some 10-15 new but incomplete Chinese entries. So, mu hope is to go to th eprinters by the end of October, so the Supplement can be safely out before the end of the year.

The print-out of the directory "genlist" is a tally of all genera. Missing in this lot, I now realize, is the "Addenda/corrigenda" file, which I should send you later. That's where the small stuff is accounted for, but most folk probably don't pay too much attention.

Gotago,
Jan

Date: Thu, 4 Sep 1997 13:13:52 -0400 (EDT)
Resent-From: jjansonius@gsc.nrcan.gc.ca
Resent-Date: Tue, 2 Sep 97 16:46:38 GMT
Resent-Message-ID: <vines.2JI8+qki1oB@vns601c.gsc.nrcan.gc.ca>
To: <traverse@ems.psu.edu>, <fensome@agc.bio.ns.ca>
From: "traverse" <traverse%ems.psu.edu=%GSC=CALGARY@gsc.nrcan.gc.ca (Alfred=Traverse)>
Subject: prev. letter

Forwarded to: inet[traverse@ems.psu.edu]
inet[fensome@agc.bio.ns.ca]
cc:
Comments by: Jan Jansonius@PALEO@GSC CALGARY
Comments:

Hi Al,

You are right: the systems can work beautifully and be a real help -- and then a spanner flies into the works and frustrates us as God frustrate the Queen's enemies.

I still had your letter on file, and here it is. I also, just to make sure, forward a copy to Rob (as you intended to do). Maybe he had received it, as well. My system at work has an other annoying trait: I can only hold so many messages on file before the "mailbox" is full and will not accept any more. Then, when I purge the mailbox, it goes into the "trash", but because we are linked to a greater mother computer, someone comes by every night and takes out the trash. So, yes -- at home I have a long list of files in the trash bin, but not in the office. So, that is why I can't recover that earlier message of yours that I didn't print out (and to which you answered). Oh well, we shall survive.

If you have one address typed, do you hit the <enter> key? That ought to put a space after the first address. If your system puts them together, maybe you should try to type a <,> after the first address, then a space, then start the second address. However, I'm not really a computer nerd, so I don't know if this advice is of any use.

I'll have to send this (because I don't dare exit this screen) and then check your previous letter to see if you wanted (or not really) a copy of the MS of the Genera File cards. Bye for now,

Jan.

---------- [Original Message] ----------

Dear Jan:

Yours to me of 6 May didn't have to do with a particular genus, at least you don't mention one. Rather it has to do with your reasoning that the Code doesn't seem to (you) to make it clear that the holotype of a new taxon must be illustrated. The reasoning is rather long, and I would have to make a xerox of it to send you, but the out box for your e-mail system should still include your letter to me, unless you deleted it. I only delete those suckers about once a year, because it is often nice to be able to retrieve such a letter--for example my answer to yours of 6-V, which I don't seem to have printed out after sending--very uncharacteristic. (I am always in Betty's doghouse for printing too much. The ink cartridges are $90 US.) You apparently DO have a copy of my letter to you of 24 Aug. because you mention it, so you can see the thrust of my argument.

I had forgotten the business about RP's miscoordination of erste und zweite Mitteilung (nicht "erster"-- ahem). In any event, I STILL think it is a big mistake to resurrect Corylipollenites. It's very iffy and not needed. Just let it remain entombed, I'd say. There's a far better argument for using Corollina instead of Classopollis, and nobody (well, practically nobody) seems to care. Why are you DOING this? You have a perfect excuse not to do it--that no type was designated. Since you are proposing to designate one now, you admit that without such designation, it is in limbo. Let sleeping dogs lie, etc., etc. Please? As I previously pointed out to you, the presentation in 1934 by RP is so confused, that it is really dreadful for you to be involved in trying to pump fresh DNA into this dead can of worms. Seize whatever excuse not to do it--no type--good, good. Admit that old Rob was right all along in wanting to sweep it under the rug, even if he gave the wrong reasons for doing so.

Aren't you using an e-mail add. for Rob that he doesn't want us to use? He asked me (I think) to use the above employed version.

I was born on Labor Day, 1925, and have ever since celebrated either the date (7 Sept.) or LD, depending on which is more convenient. This year, however, there are two parties, one on each day, so I feel very festive for a week. Got some nice tools at party no. I yesterday.

Today I got a package from UK with all the missing numbers of J. of Micropal. and a nice note from Jim Riding apologizing for the mixup--we're talking about 3 yrs. of publications. Curious, huh? I owe this all to your bringing up the generic name by Dufka, and discovering that I not only didn't have that number but quite a few others as well..

I don't know where I got the idea that Linne came from Linden, and I can't at the moment find any support for the idea, although if Linde or Linte could become "lime" it perhaps could become Linne. Some teacher or other will have told me this. I am sure I didn't just make it up.

If you are content to react to my post mortem proof-reading, that's fine with me. By the way, I think I called your attention to the fact that there's a as yet not noted (in Supplements) misprint in line 2 of card 4856:....ik, not ie.

Enough for now. Must get home and mow a few acres.

Best. Al.

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X-Sender: jansonij@cadvision.com
Date: Sun, 31 Aug 1997 20:28:35 -0600
To: traverse@ems.psu.edu
From: Jan Jansonius <jansonij@cadvision.com>
Subject: Corylipollenites
X-MIME-Autoconverted: from quoted-printable to 8bit by pangaea.ems.psu.edu id WAA21368

Dear Al,

I am at home now, took a big shief of paper that (I thought) contained all the prints of earlier e-mails to you. However, no luck for the one of 6 May, the last one you answered. Could you let me know to what genus my problem referred. That should be enough clue for me to apply your counsel.

Below I copy the text of Corylipollenites as I have it now. I don't think we can follow Robert's "voiding" because that was not in accord with the Code, and thus would not solve anything. Either you try to follow the code consistently, or you do a Stuchlik. We wouldn't want to follow that example.

Let me know if you see problems. I think that although sometimes one must make a decision where the code gives some leeway and lacks precise strictures, we must make use of those, and set the precedent. Most of our problems can still be worked out, palynology isn't yet so old that we need to apply for conservation or rejection. So, here goes. Please send a copy of your reply both to the office, and to my home address.

CORYLIPOLLENITES Potonié 1934

Arbeiten aus dem Institut für Paläobotanik und Petrographie der Brennsteine, vol. 4, p. 53

Type: Corylipollenites coryphaeus Potonié 1934, ibid.
Pollenites coryphaeus Potonié 1931, Braunkohle -- Zeitschrift für Gewinnung und Verwertung der Braunkohle, vol. 30, p. 332, pl. 2, fig. 15 (nomen nudum)

Diagnosis (combined description): "Ovaloid to somewhat spherical, in polar view more or less triangular with convex sides, in side view more or less oval, outline smooth, exine punctate to flecked, apparently two-layered. The three exitus are probably best described as foveae*) or vestibulate pores; pores circular, in part surrounded by a small labiate thickening of the exine. In consequence of that thickening, the pore appears in top view to be surrounded by a doubly delineated ring, in optical section it appears to be evaginated. The exoexine does not appear to be thickened in this lip or annulus." Size ca 13-34 µm.

*) "Fovea" in Potonié's terminology at that time refers to "the most simple case of a strongly localized germinal apparatus. It consists of a small circular groove in the exoexine, of which the bottom is generally formed by the intexine."

Remarks: Potonié had not yet provided a diagnosis for the genus Pollenites in the first publication of a series of four articles in 1931/32. Thus, although the name "Pollenites coryphaeus" had been effectively published in 1931, the name had not been validly published. This makes possible the later validation (by combined description) of the species when it was assigned to the monotypic new genus Corylipollenites. (See discussion on the latest card for Pollenites, which name was validly published in the second publication.)

A small problem, however, arises. Although Corylipollenites was validly published in 1934, no holotype was explicitly designated. While the combined description was accompanied by one figure (pl. 2, fig. 10), the specimen it illustrated has all the hallmarks of Platycarya pollen. Yet, the name suggests affinity to the Betulaceae. Furthermore, Potonié in 1934 cited in the list of synonyms the page and figure numbers of these species: Pollenites coryphaeus; P. microcoryphaeus; the subspecies P. coryphaeus punctatus, gracilis and tetraexitum; and, P. bituitus moderatus (all also nomina nuda). All of these show typical betulacean morphology. It is up to a later worker to select a holotype (lectotype) from among the specimens Potonié referred to or included in his 1934 publication. We here decide to maintain, as lectotype, the specimen figured and effectively published in 1931 as P. coryphaeus, which was also cited by Jansonius & Hills (1976) as type on the previous card issued for Corylipollenites (# 614). This specimen appears very similar to the type of Triporopollenites, and the latter genus conceivably might become a junior taxonomic synonym of Corylipollenites.

Potonié 1960 (Synopsis, vol. III, p. 117) incorrectly considered the name "Coryli?-pollenites" to be "void" because he had attached a question mark to the prefix "Coryli-." To the contrary, the ICBN explicitly states that such a mark of taxonomic uncertainty is not a cause for making invalid the publication of the name involved.

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From: jjansonius@gsc.nrcan.gc.ca
Date: Thu, 28 Aug 97 14:42:52 EDT
To: <traverse@ems.psu.edu>
Subject: various

Dear Al,

Thank you for the replies to several of my (much) earlier e-mails. I have printed them out, and shall take them home for further digesting. Two points I wanted to address immediately (I was home for 10 days, because my son was over from Montreal, and that was enough reason for a series of get-to-gethers with the other kids, who both live in Calgary.We aslo went bicycling in th eountains for a few days, and walked in and around Calgary. Now, the summer is just about kaput, and we can return to work.

You said that I should accept Pollenites as validly published in 1931. That I do! However, Potonie's first publication, in a series of 4 that appeared in 1931-1932, did not have any description (or diagnosis) of the name Pollenites. It was in the second published paper, but that cannot make it acceptable retro-actively, can it? The trouble is that his erster Mitteilung was preceded in print by his zweiter Mitteilung. Anyway, one isn't out of the woods, either, by accepting all those names as validly published under Pollenites -- then many of those names are not any more available for later validation in a combined description. What is your "starting point" of valid publication of the generic name "Pollenites"?

The comments on my MS for Jan Willem were in part very valid, and I wished you had sent them before the paper was in print. However, just because you have developed some habits doesn;t mean that the world should conform. You say that Nus-koi-spo-ri-tes is ok; do you also have some cotton-easter in your garden? People burn in-cense in temples, but that should not be reason for pronouncing a specific epithet "canadense" as "ca-na-dense" (although several I know are dense enough to do that). Some variation in pronounciation is unavoidable, as I pointed out (and I expressed some personal preferences, as well). But essentially the pronounciation guides were lifted from Brown, Genaust and Stearn. SAome of your 'exceptions' pertain to teh situation where Latin is derived from names in the vernacular, where the 'national' pronounciation should be followed. Fuchsia, or Fyu-shia? Anyway, I think that all Scotch will applaud your statement that no red-blooded Englishman can or should be brought to roll an rrr. As for Linnaeus coming from a linden tree (instead of the time-honored cabbage), that was new to me. I checked the Swedish dictionary, which gives that 'lind' is their word for lime or linden. Carl came probably from a German line, as the vernacular indicates: Carl von Linne(with an accent that can't be produced in my e-mail program here); 'von' is not a Swedish word. I have no idea where Linne would have been, or what it might have meant at one time. Chuck Linden looks like a folk etymology.

Yes, in early June I kind-a announced that I had my Supplement MS ready; as you had suggested, I also did make small xeroxes of the illustrations and glued them on the text. However, it is a lot of work to even copy them (290 entries), and where you lately have been on the road a lot, and not apt to retort to my utterings as readily as was your wont, I didn't make a copy for you so far (it is a matter of time, and money for shipping), because I honestly thought that I would have things at the printer by now. However, Hartkopf-Froeder sent me more Chinese stuff in July that is incomplete, in part needs translation, etc. So, I have been delayed, waiting in part for additional data from China, where they also seem to hold vacations this time of the year. I plan to begin printing out the individual pages beginning next week, and that alone should take me some 3-4 weeks. In that time, I hope to clear up, as well, the last things to do with these Chinese items, so that all will reach the finish line at about the same time. If you reply that yes you want to have a look at things as they are now (the first go around was very rough, and in places very "from the hip"), I'll be happy to provide you a set, i.e., if you think you can react to them before I have them at the printer; because, otherwise you can read them in printed version only a few weeks later. This is not meant in a critical way, but explains why there was a hiatus in our mutual flow of discussion.

So, I have dealt with the backlog of e-mail I found upon my return, and now shall do some more proof reading of fungal stuff (for Kalgutkar, and Elsevier). I was impressed by the building boom at the Traverse compound; I am relying on the Survey for facilities. When they dry up, I think I shall retire, and do some more gardening and apple growing, painting and bicycling, before I jump through the looking glass.

Thank you again for your comments on my earlier missives. Hoping to hear from you soon, and best wishes for Betty as well,

Jan.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: figs. of types
Cc: rfensome@gsc.nrcan.gc.ca

Dear Jan:

With the exception of the Stuchlik business, which I still think we should follow through, I believe this will clear my desk of JJ business, however briefly.

I refer to yours of 6 May, with the trepidation that you have previously confessed that you don't necessarily keep copies of correspondence.

It is very clear from Art. 8 of ICBN that the holotype of a new species (etc.) MUST be illustrated. In my reading of 8.4, that could not be more clear.
That Art. 8 is not cited in Art. 32 where the basic requirements for valid publication are listed does not mean that a name published in conflict with Art. 8, even if validly published, cannot be illegitimate. In fact, names in conflict with Art. 8 ARE ILLEGITIMATE, even if validly published.

If more than one specimen is illustrated, one of those illustrated must be identified as the type. What could be more clear?

I hope this is helpful--but it won't be if you can't find a copy of your letter that provoked it!

Hallelujah--they have poured the footers for the addition on our home that will become my palynological hindquarters when PSU ultimately kicks me out. I lost the microscopy lab two weeks ago.

Best. Al.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: "Corylipollenites"
Cc: rfensome@gsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Jan:

Now for a go at the stuff about Corylipollenites you sent Rob and me a while back.

Well, as you know, I have never bought the package that Pollenites was not validly published in 1931, because I think it can be argued that "Pollen grains in general" is a perfectly legitimate description, albeit very blah. The Code has never required a diagnosis--a description is sufficient, and it could, in theory, consist of one word.

I have told you before that if one accepts Pollenites as pub. in 1931, it gets one off of many hot seats. Here is one of them. Cory. is a mess. Pot. knew that, and I suspect wanted to wash his hands of it. For one thing, Tafel 2, Fig. 10 is clearly Platycarya (there is no doubt of this). That was a boo-boo. But fortunately, if you would go along with accepting Poll., Cory. was not validly published in '34 for lack of a clearly designated generic description. Furthermore, the whole concept is so confused on p. 54 of Pot. '34 that if anybody started seriously using this name one of us should propose it for nomina rej.

I think to revive this thing as a generic name for Platycarya-oid pollen is terrible. Let it die. It would be confusing to everybody now to have still a third card with the revival of Cory. with a new little figure showing utterly (well, not quite utterly) different characteristics. Drop it. See the advantages of accepting Poll. as pub. in '31?

Furthermore, if you DID go your route, it seems to me that it would be much more logical and more understandable to the paly. public to designate one of the '31 specimens as lectotype--it would be like the ones on the previous cards. I would then note that fig. 10 in '34 is clearly a MISTAKE because the pollen illustrated is obviously juglandaceous. In my best opinion, having known Pot. reasonably well, fig. 10 was some sort of boo-boo made in preparing the plate. I think schöner Robert should be followed in "voiding" this thing, even if he grasped at the wrong straw in stating his reasons!

Now some more questions: where did the figure on cards 4856 and 614 come from? They are not quite the same. . I suppose they were drawn from Pot.' 31. Ideally your little figures should have had a tiny lit. ref. Too late!

I guess we never caught the typo on 4856 at the top:"Paläobotanie", did we? I hereby catch it.

Best regards. Al.

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

Dear Jan:

Periodically I go through the stack on my desk and try to get caught up. This time I find yours of 13 Jun 97 to janwillem etc. re pronunciation, being a first part of a general nomenclatural thrust. I have the following comments:

Para. 1: Some of my students always found it amusing that if Linnaeus were an American today he would be called "Chuck Linden." Had he translated his name into Latin instead of just latinizing it, He would have been Carolus Tilia. Not so?

Para. 2: At one point you say that the total verbiage of a certain clause was "based on Roman foundations." But I couldn't figure out what clause you were talking about in order to check it. You could have bracketed it or underlined it?

Paras. 3-5: I think that botanical Latin is naturally subject to local variations in pronunciation, some of which will be closer to romance language standards than others. As long as the names are WRITTEN the same way by everybody, I don't thing one has much to worry about. At first it drove me bonkers in Germany that Rosaceae was pronounced: ROS-AH-ZE-JEH (z is like English ts, of course), but I got used to it. After all, there is no standard pronunciation of English either. Try garage, for example. In Texas the l in salmon IS pronounced. Etc. So, I come to your main points with somewhat jaundiced eye and ear!

Para. 6: under o you list "not" as an example of how to pronounce this vowel. Are you aware that to speakers of standard (that is majority) English, that word is pronounced: NAHT?

y you describe as pronounced like French pUr. I haven't the slightest idea what that means, which is illustrative of your problem. y in German is pronounced exactly like ü--maybe that is what you mean?

Para. 7: you say that oe is pronounced like OII--I have no idea what that means, but you go on to say that it is ok to use German ö. That is not always pronounced the same--contrast möglich with Göttingen. In most words, such as möchte, it is pronounced almost exactly like the ir in English girl. Some Europeans, not English speakers, will deny this, but they don't understand that English lacks a real r. When we say r we pronounce it like a vowel.

Para. 8: c: always as in Cat. How about Rosaceae?

g: always as in Go. How about Angiospermae? Angelica? I for one will go on saying these with soft g. (Not what the Turks mean by soft g, which is SILENT!)

j: like the Y in yellow. Baloney. To me it will never be Yussiaea or Yuglans.

r: always with trill. For many millions of English speakers, this is impossible, for we don't make the r sound at all, haven't from infancy. The tongue is lifted slightly from the floor of the mouth, period. This causes me to have problems in German, as I cannot make a clear distinction between Fahne and Farne or between Höhe and höre when I am speaking. So, this advice is for me and millions like me quixotic. By the way, many language teachers in German and Russian have wasted many hours trying to teach me to say "r." With zero success.

s: like sit. How about Rosa?

t: not like ti in nation. How about Nasturtium?

v: like w in we. Baloney. It will never be Wictoria (Nymphaeaceae)

Para. 9: ch as k: how about Chinchona? I never heard it as Kinkona.

ng: as in finger, not as in singer: in many English accents, finger does get the same pronunciation of inger as in singer. In New York City Long Island is pronounced: Long Gisland. Etc.

th: as t How about Theodora, Theobroma, and Narthecium? You'll never get most English speakers to drop the th in such words, even though they'll do it (mostly) for Theaceae. (Because we are used to "tea.")

Lastly, in the last full paragraph, you tell me not to say Nus-koi-sporites. I've never heard anybody ever say it any other way.

In short, I found the article VERY interesting and productive, but somewhat quixotic.

Now, regarding the "PS to AL"--this had to do with Supplement. You said you'd have it in 4 weeks, which would have been mid-July. I think I said, fine, I'm eager to have a go at it. But it never has come. Status?

All the best. Al.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: not val. pub.
Cc: fensome@agc.bio.ns.ca
Bcc:
X-Attachments:

Dear Jan:

Your bringing up the new generic name by Dufka opened a can of worms here, and I'm glad. It revealed that something has happened to my subscription to J. of M., and this has me started on getting it fixed.

Well, Betty made me a xerox of the paper in the library, and I have studied it thoughtfully.

Sorry, old friend. I don't agree. The rules are there to discourage folks from doing things by implication. Dufka (even, ha-ha, with John Richardson's systematic counsel--see acknowledgments) blew it, and that's too bad, but it's a fact. He needs to state clearly that the type specimen of the species he attempts to transfer is the intended type for the new generic name. It wouldn't even have been too much to provide info about the holotype for McGregor's species, though that is not required. I agree that "it would seem that he intended" for the type specimen of R. chartulatus to be the type for the new generic name, but he failed to follow the rules and he must go directly to jail, must not collect $200, etc. I suppose, if you feel bad about the poor fellow, you could write him and offer him the possibility of validating the name on your cards--then it would be "in" Jan. et al., and he would remain author instead of the "ex" approach, which makes you et al. the authors.

It could be argued that you make life easier for yourself by being a strict constructionist on this sort of thing. If you let Dufka off the hook, next there's a case that's 5% weaker. What then?

Your argument about "diagnosis" etc. doesn't carry much weight.
Dufka didn't designate a type, period.

The business in 37.3 about collection number, etc., clearly doesn't apply to paleobotany. Besides, Dufka and McGregor are exemplary in designating the corresponding info for palynological samples.

Sorry to wreck your weekend, if I did.

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Mon, 11 Aug 97 15:10:11 EDT
To: <fensome@agc.bio.ns.ca>, <traverse@ems.psu.edu>
Subject: please HELP

Hi Rob, and Al,
I need help!

If you have access to Journal of Micropalaeontology, you may turn to vol. 14, p. 71, where Dufka proposed a new genus Rugosisporites. On the line below, he gives the name "Rugosisporites chartulatus (McGregor) n.comb." On the next line again, he gives the "Diagnosis." etc. Now, he uses the word 'diagnosis' only for circumscription of his genera; for his species he uses "description". So, from the context (and age-honored practice)it is clear that Rugosisporites chartulatus is the type of the new genus. HOWEVER, he didn't precede that name with the word "Typus" or whatever the ICBN (art. 37.4) now demands.

My question is: can I accept the new genus as validly published in 1995, as is? Or, must I be pedantic, and put in the required detail and make it valid as "ex Dufka in Jansonius, Hills & Hartkopf-Froeder 1997"? I hate doing the latter, if for no other reason that other "requirements" of Herr Greuter (e.g. Art. 37.3, last sentence) are not observed without anyone objecting. I think that the type is clear enough "by implication" (a principle that served us well in the past), and still observed in other parts of the Code.

Dufka gets a bit sloppy with the valid publication of his type species, though: he does give the necessary basionym and bibliographic detail following the diagnosis of the new genus, but does it in context of combining the new comb. with the "cf." in the name because his specimens, although quite similar structurally, are almost half the size of McGregor's material. Thus, he apears to include McGregor's type as a synonym of his "cf." forms. However, it is obvious, also from his description and Remarks, that that is not the case.

Would it bother you if I go ahead and accept things as Dufka gave them to the world?

I am now really getting close to the end of the Supplement of the Genera FIle. I have a directory with 9G bites, and 290+ files, each representing a new or emended genus. I made some 160 new drawings, borrowed a few, and shall use (and upgrade where necessary) a good number of old ones. I haven't sent any more to Al, because I was too busy! Also, between the scrutiny of Hills and Hartkopf-Froeder and the support of the spell checker, I am confident that the drudgery is paying off in a less sloppy product than we imposed on the market in the past. However, when I have hard questions like the above

one, I need the support of you guys.

I know that it is summer and all that, but if you could send me an answer to the above at your earliest convenience, I'd appreciate. Enjoy the weather as long as it stays nice: last week Tuesday we had 34 Celsius, then Saturday morning we woke up and saw hoarfrost on the roofs untill the sun hit it and burned it off. Some climate, here!

Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Wed, 9 Jul 97 11:07:51 EDT
To: <traverse@ems.psu.edu>
Subject: re: varia

Hi Al,

Thank you for the various info and observations. I'm glad my moral high ground proved to be a safe haven.

I'll miss you in Woods Hole -- and so will many others. Within AASP too there are many who are sorry about how things turned out. In a sense, AASP got screwed as much as you and Penn State were. Now, a number of AASP people are trying to make the best of a difficult situation. I find that in every organisation there are good guys and bad guys. I would be happier if you said "I'm getting too old to attend all those things" than hearing that you are still "punishing" whoever; you might be punishing yourself at the same time? I don't know how effective the principle of "forgiving" is; myself, I tend to "forget" bad experiences and just see what's new around the corner. Turn a few corners and it's hard to discern what you have left behind, but memories of past good experiences will stay with you. Good grief -- and its only 9 AM, where do I find the prose?

Till next time around,

Jan.

PS: Don e-mailed that Excrescentipollenites is restricted to Late Maastrichtian and fairly common along the USA East Coast. It does indeed have an atrium, and the bulge is made up of thickening of intexine and exoexine.

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

Dear Jan:

I suspect Excrescens.... is just a preservational or teratoid variation of Momipites.

Vijaya (once Ms. Singh) is a very curious person. She gave poor Schaarschmidt fits in Frankfurt. You were well out of it. Had you rec. her, you'd still be paying for it. She would have popped up in Calgary, for sure.I

I am still not convinced that it wouldn't have better for all of N. Amer. north of the R. G. to have evolved as the rest of Brit. N. Amer. did into a parliamentary democracy.

Have no plans to attend AASP meeting. Even though one of my former students is running it. It will take a while longer for me to get over the bad treatment I got from many in that organization. I've already had my trip to Massachusetts for the year--my 50th reunion at Harvard. Like you, however, I still enjoy palynology.

All the best. Al

From: jjansonius@gsc.nrcan.gc.ca
Date: Mon, 7 Jul 97 13:13:51 EDT
To: <traverse@ems.psu.edu>
Subject: ...no subject...

Dear Al,

Thanks very much (if belatedly) for the info on Excrescenspollenites; I typed it up over the weekend, and last night did the drawing. Looking at the photos, I was wondering if the stratification of the exine is as described. There also appears to be an atrium. However, that is another matter, and I have asked Don about that.

The genus Verrualetes was, if memory serves, by H.P. Sing & Saxena. Let's wait for a week to see if India replies. I also have asked Christoph Hartkopf-Froeder if they have it in Krefeld. He said no, but there is one library in Germany that lists the serial, and he'll try to get an ILL of it, and then xerox/fax the page(s) I'll need.

Tiwari, if I remember well, asked me some (6?) years ago if I would write a recommendation to Senckenberg where Vijaya had applied for a scholarship. Since I had never met the person (nor had any other business with her), I replied that I could not do that. He wrote back that she was most deserving and merited my recommendation. I had to decline once more -- what else could I do, I still didn't know her or her work. I was a bit surprised that there was a repeat request. I understand that she made it to Germany without my help anyway, so the good people that did know her must have lent support. That's the way it is supposed to work, anyway.

I enjoyed your gritty memories of the loyalist exodus. Will there ever be peace between the Irish; let alone Greek and Turks on Cyprus? The Dutch seem to have overcome their aversion of the Spanish -- they now go there for cheap vacations, and one princess even shopped for a husband there, without the House of Orange suffering for that. For that matter, you now live harmoniously among the USAers, don't you?

Will you attend the Woods Hole meeting? Or have you given up on AASP etc. I'll have to be there, the last time in a daze on the dais; the next meeting after will be in Mexico, I don't know yet if I'll be there. As the philosopher said: "there is a time to gather stones, and a time to throw stones away." Still, palynology keeps interesting me.

Love those e-mails. I'll be in touch again,
Jan.

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

Dear Jan:

Today I discover two messages of 4 July from you. They seem to be the same. Neither mentions the subject of my previous one to you--the new genus Excrescenspollenites, or some such name.

Regarding Verrualetes: no, I apparently don't have the reprint to which you refer, but there is a small problem you could address before I write to Tiwari, whom I regard as a good friend. Which Singh is it? (There are a handful in palynology alone.) One of them now goes by "Vijaya." No initials, nothing else, just "Vijaya." She is a real character. She might even be the Singh in question, for all I know. Occasionally I am on good terms with her, and she might help. So, please send a complete reference.

Yes, 4 July was a "holiday." I have never paid much attention to such things. This one is especially a non-event because my ancestors were 100% Loyalist, living in New York, at the time of the event celebrated. They were run out in 1786 after many indignities, and that is why I was born in Prince Edward instead of Long...Island.

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Fri, 4 Jul 97 17:21:23 EDT
To: <traverse@ems.psu.edu>
Subject: new name

Thanks, Al, YOU MADE MY WEEKEND! Things are coming along. Did I ask if you have a reprint of Singh & Saxena 1984 (Proceedings of a Symposium on Evolutionary Botany and Biostratigraphy) published in India, wherin they published a new genus Verrualetes (that I don't believe I have in the Genera File), to which Rao (Rev. Pal Pal., 86:335) referred.

I have written, e-mailed and faxed Singh since Jan 1966, but get zero on my request (to translate a Dutch saying). I just today addressed a repeat requeat to Tiwari himself (but he may not consider me his friend). We'll see. Might you have that?

Have a nice weekend (isn't this a vacation day in the USofA? What are you doing in the office?)

Jan

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: checking in
Cc:
Bcc:
X-Attachments:

Dear Jan:

It's Sunday, and I am in the office while Betty is at church. As soon as she gets here, and we've eaten the lunch that she made for us (ye gods, that reminds me that I left it in the hot car, better go to parking lot and get it and put it in refrigerator asap.....).... we leave for the Adirondacks for a week. As I've probably said before, we have a place on Big Moose Lake--family heirloom from her family. You can't sell an heirloom. So, we have to care for it, as it is the largest single piece of our net worth. We rent it for the high season (Jul.-Aug.)

I know that I am behind on my correspondence with you--there's a JJ pile on my desk, and I will get to it as soon as possible. I AM interested in looking at the stuff for the Supplement and am sure I can do it when we get back from the mountains. I have carpel tunnel syndrome surgery scheduled for 15 July, but that shouldn't be much of a challenge really. We have no more travel plans until late Sept.--back to Big Moose--and October--Germany.

Just back from 50th reunion of my college class--much was made of fact that the Marshall Plan was introduced at the original Commencement in 1947--I had taken extra courses, graduated a year early and was already studying with Hamshaw Thomas at Cambridge, England. In the Commencement program for this year there was a long history of Harvard Commencement in which I discovered that until 1896 the whole proceeding was conducted in Latin. There were orations in Latin, Greek and Hebrew. All that remains is a Latin oration by a graduating senior. The guy this year did a suberb job (a "pony" was printed on a sheet of paper inserted in the official program). All of this is to comment on the first paragraph of your C. N.--fluency in Latin was expected at the top American universities until a century ago, and traces remained as late as when I got my bachelor's degree in 1946--I got an S. B., not because my major was biology but because I couldn't pass (or even attempt) the Latin literacy test (I was educated in Michigan where Latin was almost dead--I had one year of it in junior high). A friend of mine from some Boston school majored in math at Harvard and got an A. B. because he could read Latin!

English is now certainly the international language. Too bad, Esperanto would be better. Etc.

Hang in there. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Fri, 13 Jun 97 18:37:46 EDT
To: <janwillem@boev.biol.ruu.nl>
Cc: <traverse@ems.psu.edu>, <fensome@agc.bio.ns.ca>
Subject: ...no subject...

Hi all,

Attached is a WP3.1 file of the text that will appear below these lines. It is intended as a first in a short series of various aspects of botanical nomenclature, for the AASP Newsletter. I shall polish it in th enext few days, but will wlecome any comment that you may have.

I have used soem info from Stearn's Botanical Latin; and also Helmut Genaust (it cannot be more precise) : Etymologisches Woerterbuch der botanischen Pflanzennamen.

Jan.

PS to Al: I am almost ready with a final MS for the Supplement of the Genera File. The pages are much cleaned up. and have a sketch where new ones were needed. Would you be interested to look thru them for comments (I hope to close the whole thing in another 4 weeks). Or is that too short a fuse?

CAPITA NOMENCLATURAE -- I

Botanical nomenclature is a basic tool for communication among all those in botanical sciences. The value of this single worldwide "language" was first recognized by Linné, who wrote his name as Linnaeus, because all scientific (and diplomatic and spiritual) communication was done in Latin. Using Latin, the young Swede could lecture at the University of Harderwijk and be instantly understood. (Harderwijk is a now small city in the Netherlands.)

check finder

In our age, few people still speak Latin, although a some (like myself) have a rudimentary knowledge of its rules and grammar. It would seem that English (particularly that form of the language based on Roman foundations -- e.g. the total verbiage in this clause) now is the lingua franca in commerce and science, and the most widely understood. However, botanical nomenclature is still conducted in Latin. Either names are (based on) Latin (or latinized Greek) words, or at least the names, based on other words from the vernacular or invented, are treated as Latin.

what clause

Communications between scientists are oral or written. This small chapter will deal with the oral aspect of nomenclature. Of course,

my increasing age makes it no easier to hear clearly what people say; and if I don't pay attention, I even may miss that they are speaking at all. However, for many years prior I have been troubled at meetings by the wild variety of pronounciations of Latin, that hindered my understanding of the points the speakers were trying to make.

There is classical Latin, church Latin, vulgar Latin and botanical Latin; there also is the "traditional English gardener's" pronounciation that is commonly used by English botanists. Each may have some particularities, but, except for the last, the vowels should be fairly consistently pronounced the same in each. Many pronounciations of our Latin names, however, are (too) strongly colored by the speakers native tongue. (Tongue-in-mouth disease?) This may make for uncertainty about the forms being discussed. Particularly the "English-garden" variety pronounciation can be very confusing, because it uses the shifts in phonetic value of English vowel that differ from what is used in most other (continental) languages. For instance, regina sounds like "ree-djzeye'-nah," instead of the more classical "ray-ghee'-nah."

This variance in Latin pronounciation dates back to past ages. Essentially, it is the reason for the divergent developments of the Romance languages. However, these influences have plagued users of Latin throughout time. Stearn (p. 53) cites Erasmus who in 1528 related how a French ambassador addressed the court of Emperor Maximilian (all in Latin) "with so Gallic an accent that the Italians thought he were speaking French; a German, in reply, sounded as if he were speaking German; a Dane, speaking third, might have been a Scotchman, so marvellously did he reproduce the Scotch phonetics."

It would be easier to explain the correct pronounciation if the reader has some knowledge of another language; in Italian (and the other Romance languages), as in German, the vowel values are not too different from what they were in Latin. The correct (classic) phonetics, then, are as follows (the first example as for long, the second as in short vowels):
a: fAther, Apart
e: thEY, pEt
i: machIne, pIt
o: nOte, nOt — Naht!
u: BrUte, fUll
y: French pUr, French dU [if necessary 'thEse'] French?? ü = y in German

Two vowels following each other are always pronounced individually:

Co-to-ne-as'-ter (the second 'o' is long), not cot-on-easter.
However, Latin has some genuine diphthongs (which are per se long):
ae: AIsle, German KAIser
au: hOUse
ei: rEIn (or rAIn)
eu: pUff [however, Germans might use 'oi']
oe: OIl [however, Germans might use 'ö'] = ie ü gin!
ui: WE, French oUI

Most consonants are as in English, but the following need attention (NB: in 'church Latin, the consonants are essentally as in modern Italian):
c: always as in Cat Rosacea
g: always as in Go, Get — Angrosperma? Angelica ?!
j (consonant form of 'i'): Yellow — Salong! Yugan Lacca? Juglans
r: always with a Scotch roll or Italian trill — Lipodocite frame
s: Sit, gaS Rosa!
t: Table, naTive (not as in 'nation') Nasturtium
v (consonant form of 'u'): We — Salong! Victoria?
z: as 'z' or 'dz'

There are a few double consonants, generally of Greek origin:
ch: as 'k' (or if possible as 'k-h') — Rosaceae Chinchona ?!
ng: fiNGer, siNGle (not as in 'singer') — i nung accente ~~is~~ fingen dur ~~ng~~ ng w. fing
ph: as 'p' (or if possible as 'p-h'); by many as 'f'
th: as 't' (or if possible as 't-h') Theodora! Theobroma
Nathanie

This ends the guide to proper pronounciation of Latin words (including latinized Greek). However, a large number of our names incorporate elements of personal or geographic names fom many parts of the world. These are often spelled as in the local vernacular, in spellings that do not always utilize the same phonetic values as the European languages, although many of these languages have systems of romanization if they employ different alphabets (or other writing systems, e.g. Chinese, Arabic). Xixi in Chinese would be pronounced 'Sheeshee' in English, Qiqi as 'Tsjeetsjee.' If we were to change the orthography of such names to approach more closely the phonetic convention of Latin, we might find it easier to pronounce them without extraneous knowledge of Chinese, but the written form would not be instantly recognized as that of an author whose name would be familiar in a list of cited literature.

Here, then, we have a dilemma that needs compromise and common sense. It is customary, and appropriate, to pronounce such parts of a name as closely to the vernacular as is possible, recognizing that non-native speakers may have problems as local tongue-twisters

are produced too quickly: Warszewiczella thus comes to sound like "var-she-vi-chel'-la." Similarly, Menziesia is spoken "men-zies'-i-a", Heuchera "hoi'-che-ra," and Choisya "shjoah-see'-ya"; which presupposes some knowledge of Scotch, German, French etc. pronounciations. For instance, the Italian "chi" (='kee') differs from the French "chi" (='shee'); the Polish "cz" corresponds to the English "ch" (='tsj') and to the Italian "c" before "i" (='tsjee').

The stress (based on subdivision into syllables) is almost as important in recognizing the spoken word. Also the length of a vowel is important in this respect; long ones are indicated in a dictionary by a line over them, short ones by a hollow curve. In Greek, the length of a vowel is indicated in the case of 'o' or 'e' by use of different letters (omicron amd omega; epsilon and etha). In Latin, these distinctions are not present. The division of syllables is sometimes quite different from the standard wordbreaks in the English dictionary (where inexplicably one finds stra_tig_ra_pher next to strati_graph_ic; in the Latin convention, these would be stra-ti-gra-pher and stra-ti-gra-phic).

In classical Latin, the stress in words of three or more syllables is on the penultimate one when it is long (i.e when it ends in a long vowel or a diphthong), or when the last two vowels in the word are separated by two consonants. However, if the penutimate syllable is short, the stress is on the third syllable from the end (flo'-ri-dus, la-ti-fo'-li-us).

In long words there may be a lesser stressed syllable in the first part, and a stronger stress in the last part (Nus-ko'-i-spo-ri'-tes; NOT Nus-koi-spo-ri-tes).

Attachment Converted: C:\TEMP\capita1

From: jjansonius@gsc.nrcan.gc.ca
Date: Wed, 7 May 97 11:19:24 EDT
To: <traverse@ems.psu.edu>, "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: Corylipollenites - can I do?

Dear Al, Rob,

I just faxed to both of you the MS for the new card on Corylipollenites, intended for the next Supplement of the Genera File.

It will be clear why I want to go this route: if pl. 2, fig. 10 (in Potonie 1934) is accepted as type, the genus will be one for Platycarya type pollen, because that clearly is the type of pollen Potonie pictured.

If Corylipollenites is to be resurrected this way, it appears that Triporopollenites (with type T. coryloides!) might be a junior synonym. Or can the expert find subtle but meaningful differences between the two concepts, that are backed up by the respective types?

In a way, this problem is an extension of the problem that I have wrestled with in the last months (and bothered you about, too): what is the obligatory type, and how much leeway do we have in manipulating the process of selecting a type after the fact? For that matter, does Al concur with my estimation (and that of Art Sweet) that the 1934 specimen is Platycarya, rather than Corylus?

Looking forward to your responses -- I think I am on terra firma, but then again, I feel I am also treading new ground.

Jan.

→ A. Traverse
→ R. Ferguson

CORYLIPOLLENITES Potonié 1934

Arbeiten aus dem Institut für Paläobotanik und Petrographie der Brennsteine, v. 4, p. 53

Type: Corylipollenites coryphaeus Potonié 1934, ibid.
Pollenites coryphaeus Potonié 1931, Braunkohle -- Zeitschrift für Gewinnung und Verwertung der Braunkohle, v. 30, p. 332, pl. 2, fig. 15 (nomen nudum)

Diagnosis (combined description): "Ovaloid to somewhat spherical, in polar view more or less triangular with convex sides, in side view more or less oval, outline smooth, exine punctate to flecked, apparently two-layered. The three exitus are probably best described as foveae*) or vestibulate pores; pores circular, in part surrounded by a small labiate thickening of the exine. In consequence of that thickening, the pore appears in top view to be surrounded by a doubly delineated ring. in optical section it appears to be evaginate. The exoexine does not appear to be thickened in this lip or annulus." Size ca 13-34 µm.

*) "Fovea" in Potonié's terminology refers to "the most simple case of a strongly localized germinal apparatus. It consists of a small circular groove in the exoexine, of which the bottom is generally formed by the intexine."

Remarks: Potonié had not yet provided a diagnosis for the genus Pollenites in the first of a series of four publications in 1931/32. Thus, although the name "Pollenites coryphaeus" had been **effectively** published in 1931, the name had not been **validly** published. This makes possible the later validation (by combined description) of the species when it was assigned to the monotypic new genus Corylipollenites. (See discussion on the latest card for Pollenites.)
A small problem, however, arises. Although Corylipollenites was validly published in 1934, no holotype was explicitly designated. While the combined description was accompanied by a single illustration (pl. 2, fig. 10), the specimen it figured has all the hallmarks of Platycarya pollen. Yet, the name suggests affinity to the Betulaceae. Furthermore, in 1934 Potonié included in a list of synonyms the species: Pollenites coryphaeus; P. microcoryphaeus; the subspecies P. coryphaeus punctatus, gracilis and tetraexitum; and, P. bituitus moderatus. All of these show typical betulacean morphology. It is up to a later worker to select a holotype (lectotype) from among the specimens Potonié referred to or included in his 1934 publication. We here decide to maintain, as lectotype, the specimen figured and effectively published in 1931 as P. coryphaeus, which was also cited by Jansonius & Hills as type on the previous card (# 614) issued for Corylipollenites. This specimen appears very similar to the type of Triporopollenites, and the

latter genus conceivably might become a junior taxonomic synonym of Corylipollenites.

Potonié 1960 (Synopsis, vol. III, p. 117) incorrectly considered the name "Coryli?-pollenites" to be "void" because he had attached a question mark to the prefix "Coryli-."To the contrary, the ICBN explicitly states that such a mark of taxonomic uncertainty is not a cause for making invalid the publication of the name involved.

From: jjansonius@gsc.nrcan.gc.ca
Date: Tue, 6 May 97 17:26:45 EDT
To: "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Cc: <traverse@ems.psu.edu>
Subject: figs of fossil plants as type

Dear Rob, Al,

If we must read (and observe) not just the spirit of the ICBN but also literally apply its injunctions, I come to the following observation:

Art 32 sets out all conditions that must be fulfilled for valid publication -- and in 32.1 a number of other Arts are listed of which the requirements must be satisfied: in particular, in this respect, the Articles 33-45 are listed. Among those is Art 38 which says that since 1912 a description must be accompanied by a fig showing essential characters (or reference to one published earlier).
I note that the requirements of Art. 8 are NOT part of the conditions set out in Art 32.1; consequently, I would say that it cannot override Art 38, or be applied to determine validity of the publication.

When I read Art 8 very closely, it says that 'the type of a fossil species is the specimen whose fig. accompanies or is cited in the valid publication of the name.' Now, this sentence contains a cross-reference to Art 38, so the editors were aware of both articles. The first sentence of Art 8.4 pertains to the case where there is a single figure.
The second sentence pertains to the case where more than one figure have been given: in that case "one of those specimens must be chosen as the type." However, it doesn't say that this choice must be published at the same time for publication to be valid -- rather, it says that a type must have been chosen. For the publication to be valid, we still only need that the figure(s) show the essential characters (Art. 38). As long as there is a number for the holotype (by which it can be found again), and as long as that type is represented among the figures supplied, the Code seems to be satisfied.

(This para should be after the next one.) This means that I can be a bit more lenient in accepting as validly published some new genera where the fig of the type was not identified, although it had been given an identification number. Only if (and the onus appears to be on us) we can demonstrate that the holotype was not among the figured specimens, can we conclude that the publication was not validly done.

Art. 37.1 states that publication is valid only if a type is indicated (and here there is a cross-reference to Art. 8), but it does not require that the illustration of this type is identified. I'm sure that, if the editors had

meant to say "the figure of the type" rather than "the type," they would have done so if that is what they wanted.

Any disagreements? Any agreement? All agreed? What would Judy say?

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

Dear Jan:

Re the 'r' in Work, etc. The truth is that English, as spoken in North America (=standard English), has no real 'r' at all. Work should really be spelled wök. Foreigners seldom understand this. I recall trying to convince a Swiss that ir in girl is pronounced exactly like German ö. A language with no r is unacceptable to a Swiss (to say nothing of a Venezuelan).

I explained to you that I would keep 'c' for use in ch for the sounds that are indicated in Turkish by a c with a tail.

Thanks for additional comments on spore taxonomy. I will think about it, but I believe that it is not a good idea to be guided by extant spores in delimiting fossil generic units.. Ye gods, it puts me into ol' Norman's camp in this respect.

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Tue, 11 Mar 97 11:31:56 EST
To: <traverse@ems.psu.edu>
Subject: answers?

Dear Al,

Because you are so persistent in asking for my opinion on Concavi-, Concavissimi- etc.: here are some ideas.

Pocock could be (and often was in his publications) a very quick and sloppy worker. At the time, I couldn't make sense of why he was applying some of the generic names in the way he did. That held for trilete spores, as well as dinos. It was one reason why I later re-examined Stan's types and tried to sort out what he really had been looking at.

Obviously, there was no merit in Pocock's 1962 position that Concavissimi was a jun syn of Concavi; he quietly retreated from that position in Pocock 1965 (for year 1964) in Grana.

One reason why I don't want to become involved in these evaluations is that one can use these 'genera' as pure morphological units, with purely arbitrary delineations. Or, one can try to equate them as best as possible with natural genera (or groups) of extant ferns, and so try to understand what are taxonomically meaningful characteristics. I know way too little of fern spores to becaome involved in such work. Still, to me that is the best course to follow.

So, Concavi to me groups torate spores with (concavely) triangular outline, and essentially unornamented spore wall. Concavissimi groups triangular spores with more or less strongly concave sides and that are uniformly ornamented with distinctly delineated (round) grana or verrucae. Neither is ribbed or cicatricose (who said they could be?)

As for the difference between Appendicisporites and Plicatella (are the projections more or less than 10 micron), I have no competence in judging that question: what do recent ferns do in tha respect? At least, Burden & Hills spelled out what they subscribe to.

Re 'trilete' -- in Punt et al.'s glossary they suggest that '-lete' is derived from 'laesura' (scar, cut), although I cannot find this word in the Latin dictionary, and only implied in the full Webster's (LL injury). Possibly there is an aural association with 'caesura'? Thus unsatisfied, I looked a bit further back, and thinking that Paulus Reinsch was the first to introduce the term (though not yet as a generic name), I got to his 1881 book on the structure of coals, where he derives (p. 50) 'Triletes' from

'tri-' and Greek 'lethein' (=being hidden). The Greek dictionary shows me 'lethe' (as in lethargic; both etha), but also has a reference to the Dorian form 'lanthanoo' (='lethoo') which has the meaning of escape, elude notice, be unseen; etc. So, Paulus was right in his etymology. I don't know that it has relevance to our question about the gender of the effective suffix -letes, or -etes.

From the divine to Calendula ('goudsbloem') and Tagetes ('afrikaantjes'); the latter is in Canadian seed catalogs subdivided into French and African marigolds.

As for mallow/mellow -- I had never picked up a difference in pronounciation (the mallow in 'marshmallow' seems to rhyme with mellow much more than with the 'a' in marsh. AND INDEED, now that I looked a bit further after already having typed this whole paragraph, I find that Webster's also hears what I hear, but only in the whole word 'marsmallow' -- it is the stand-alone mallow that comes out more like an 'a'). As I was typing 'mellow' in my letter to Gea, the association came up in my mind, and then I remembered how you wanted simplify spelling. However, I now understand you wouldn't go for a more phonetic one? I still am puzzled why Webster's give the different pronounciation for 'mallow' -- as I said I cannot hear the difference with mellow. However, the English speakers here (more than the Canadian) confirm that there is a (be it ever so subtle) difference. However, I would suggest that the ICBN would consider them functional homonyms.

And as I said before, one should err on the careful side about changing the spelling of English. For one, it is really nice that in the present system few should have difficulty with reading Shexspier's original text. Yoo dont vant to skwander the heritadj of so manee djenerations. Ugh!

Did I thank you (and Betty) for ending th ecopy of Amtaspora? It arrived in good condition.

I discover myself, now, that it is MY letters that I don't keep. I seem to just press the 'sen' button, and am done with it. However, I shouldn't keep doing that. TROPENBOS (your copy of that just arrived by snailmail, also) is the name of van der Hammen's company or foundation for managing his land holdings in perpetuity; it is not someone's name.

O yes; last week I asked the Chinese librqarian here for some info on top of a Chinese reprint. He said he couldn't translate on word (3 characters), and speculated that it was the name of somewhere. So, he got a dictionary. I said:"I have a bigger dictionary", but he said that probably would do -- the first character he thought was 'fu', and shure enough a few seconds later he had found the same character. The second he thought was

"jing or something like it', and sure enough it was jing; together it was about the Fujing Fm. He apologized for his slowness, because "his romanisation of the mandarin pronounciation was not very confident" he said. So, for the properly instructed it seems that Chinese characters cannot be said to "not be supposed to sound."

From: jjansonius@gsc.nrcan.gc.ca
Date: Thu, 13 Mar 97 13:48:38 EST
To: <traverse@ems.psu.edu>
Subject: re: response re spores

Hi Al,

Just a few comments -- some real work is awaiting. I don't know why you maintain the 'r' in work, Lord, etc. I learn in kwire praktis dat dere anet one in dem wods. Your fonetics work in Spanish because it is a neolatin with few loaned words (some arabic, I guess). It seems counterproductive in English -- the more you 'free up' spelling, the more you are prone to get local variants that make no sense in other parts of the world. Better to keep things as they are now (why fix it if it aint broke). As for chinese (why didn't you write 'sinese'?) I didn't say that the mandarin pronunciation is best or standard -- rather I was surprised to find out that there IS a phonetic (fonetic is ok by me) element in these characters that can be used to retrieve it in a dictionary -- probably there is a similar cantonese dictionary along the same lines.

No I didn't think that Concavisporites included corrugate forms -- however, from one of your earlier messages I had been left with the impression that one of the sources you quoted had so emended (?) that genus -- I couldn't find that, though, myself. And of course it would have no merit. And it is the cicatricose spores that I don't want to comment on.

As for using modern taxonomy as a guide in sorting/grouping dispersed spores -- my notion is that things like that can be a valid guide to what characteristics are significant in making meaningful taxonomic decisions. For example, if we find a schizeaeceous fern producing cicatricose spores some of which have perforations in the cicatrae, then it probably makes no sense to maintain separate genera for perforate and non-perforate cicatricose spores. Wouldn't you agree? It is ok for you to maintain strictly morphological differentiations, but things need an informed perspective. And, palynologists need an informed intuition. If you hear music by Mozart, you wouldn't think it should be classified with the work of Alban Berg.

Torate is from German origin, I think. It means "provided with tori (sing. torus)", ie swellings. It is derived in a similar manner as 'zonate', 'cingulate', etc.

I guess that's it for now.
So long, Jan.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: response re spores
Cc:
Bcc:
X-Attachments:

Dear Jan:

Your missive of 11 Mar. is very useful.

I don't think I agree about trying to figure our modern fern affiliations as a cornerstone of handling Mesophytic spores. I would go for the pure morphological unit route.

What do you understand by "torate?" I have never been sure I really understand.

In answer to your para. 5, last sentence: you implied that Convavi. etc. were cicatricose by saying that you didn't want to comment on such spores. I assume you were talking about Appendicisporites etc., but that was not the primary focus of my missive.

I was expecting that Dutch for marigold might be something along the lines of Studentenblumen. Hmmm.

I see considerable merit to your idea that English spelling shouldn't be reformed because one comes to recognize the words as "things." However, despite the merits of your notion, I would still urge the elimination of all psilent letters immediately. I would abolish c everywhere that it means k and everywhere it means s. I guess that takes care of c. except for making the ch sound. I would abolish ph (replace with f). I would abolish q instantly. This is a different matter, but I would also abolish the word the as functionless. A/an would remain. All of this probably makes you kwake in your boots. Filosofikally, I am sure that I am on rite trak. That's enuf for me.

I have a komplete file of our entire korrespondens. After I send you a message, I bring it up in "out" and print it out.

I never disputed that Chinese karakters are assosiated with sounds. What I am sure I said is that sound they have in Mandarin is not sound that they have in Kantonese, to say nothing of Japanese. It seems kwiksotik to me to insist that right sound is sound in Mandarin.

Bak to work. Best. Al.

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

Dear Jan:

Thanks for yours of 5 Mar. Well, yes, I really did intend to make those other transfers and somehow flubbed it. Thanks for the advice.

Regarding the lousy spores I am studying. What I really wanted your opinion on is the Concavisporites vs. Concavissimisporites questions that I raised. (They are not cicatricose in any sense that I can think of.) It just seems to me, as I noted in the message of 5 March, that the generic description for the two genera doesn't agree with the forms Stan described (as Concavisporites) in 1962, and which Burden & Hills illustrate in 1989. I was hoping you would look at your two cards vis a vis the SAJP and B&H papers re this matter. (See also my missive of 2 Mar.)

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Wed, 5 Mar 97 14:52:12 EST
To: <traverse@ems.psu.edu>
Subject: re: spores, etc.

Hi Al,

As I munch my lunch and try not to drop crumbs into the keyboard:

-as I know little about the biology of extent cicatricose spores (and related forms) I am in no position to advise on what to do with them. Should you contact Mary Dettmann, and/or Geoff Playford, on that score?

-in your 1994 papaer you gave an impeccable table for the new combinations on p. 268-269. The thing I bumped into under Parthenopollis (in the second full paragraph) is not a transfer; but then, it was not clear if you intended to transfer Pflug & Thomson's species, or the forms so named by Thielle-Pfeiffer (which might be something different?). Since you didn't effect any new nomenclature there, I would say: let the matter rest where it stands, and wait for another paper (if there will be one) for clearing this up. However, no harm was done, and it is not important enough to put something in Taxon.

Bye for now, lunch is over! Jan.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: spores, etc.
Cc:
Bcc:
X-Attachments:

Dear Jan:

Thanks for putting me onto a more thorough search of the Grana paper. I had already looked at it, but from the title and the general shape of the paper I never guessed that Concavi. things would be where they are, and I missed them. As I said before, my things match C. punctatus pretty well, C. asper I now think less well, but I am pretty sure that these species and several others should be removed from that genus because of sculpture and shape--but I am not about to do it on the basis of the miserable fossils from the Riddle Fm.!

I look forward to what you think I could do about the transfers (etc.?) I should have taken care of in the '94 paper but didn't---note to Taxon?

Best. Al.

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

Dear Jan:

Our poor team lost in the first round. We chucked our tickets to the remaining 8 games and drove home (860 km.)--after a night's sleep, of course.

Ye gods. The machine is FULL of JJ messages!

I guess I'll try to do something with the whole thing in one return message.

1. Marsypiletes. You are now thinking along the lines that I was, I guess. I thought the darned thing just doesn't "look" feminine. You raise the question of possible precedents from modern plants. I don't have time at the moment for a search of lists, but I immediately thought of Tagetes (in English, "marigold," in German "Studentenblume"--I never have been told why--what's it called in Dutch?). Tagetes is feminine. But what about -letes? I have looked in my various sources here to no avail--I thought Erdtman would give me the etymology of trilete, etc., in his glossary, but he doesn't. My Latin dictionaries don't help. Punt seems to say that it's an adjectival ending from laesura, but I'm not convinced that's right. I guess at the moment I'm stumped.

By the way, since you mentioned it in the same missive, our team was knocked out in the first round, we chucked our other eight game tickets, and headed home, after one night in the motel, and a total of about 15 hrs. in Indy. The conservatory in Columbus, which we visited, along with the art museum and some other things, were very enjoyable and give us a feeling that the whole trip wasn't a waste.

2. Re plant family names. One should know both versions of the few family names that have alternatives. I haven't checked but seem to recall that it's only nine families. One must be prepared to use whichever version a publisher demands. One of the non-traditional names really bothers me: Arecaceae. Impossible to distinguish sufficiently in speaking from Ericaceae. I use Palmae unless compelled not to.

3. Re Traverse 1994. That thing was read by all sorts of reviewers, but why the devil didn't I send it to JJ? I sure made a bunch of bloopers.

Re Parthenopollenites: I think that the colpi have ordinary round pores, as is typical of Vitaceae. I can't answer the question about the length of columellae from my notes. For that matter, it would require sections and TEM.

I am really embarrassed by the transfer business--didn't you find another such before too? I

let that slip through the cracks. I MEANT TO MAKE THE TRANSFERS. Please keep a list of such. Maybe I should somewhere publish a little not completing the business--Taxon, perhaps! Ouch.

Re Gordonipollis: I don't recall whether the "cover of the colpus"--I guess you mean the colpal membrane--had granules--I think so but can't check that at the moment. Not verrucae, I'm reasonably sure.

3. Yeah, I should have sent you a copy of Greuter's letter. Sorry.

4. No, my students and I have not worked specifically on the salts you mentioned, though my guess based on other localities would be Jurassic. You might wish to contact my former student, Sarah Fowell, about this matter. sjf@ldgo.columbia.edu. She and I are still working on the Fundy Basin Triassic-Jurassic. For all I know she has actually studied the salt, per se.

5. Re yours to Gea Z. I sent you, I think, a copy of my response to her, which was only a notelet, compared with your massive rejoinder. I think when you get my little letter to her you will see where I stand, which is against changes in general. Engelhardia vs. Engelhardtia and Acer pensylvanica vs. A. pennsylvanica are illustrative. In both cases, the "wrong" spelling should stand.

Where did you get the idea that I would spell mellow "mallow"? That really confuses me. Mallow is the English name for Hibiscus spp. I suppose it is cognate with Malva, as in Malvaceae.

But mostly, I am embarrassed about the 94 paper's glitches. As you doubtless recognized, the paper was based on almost zero new info--it was just a revision of the 51 and 55 publications, to bring the nomenclature into line--and to do a new take on the stratigraphic position, based on comparison with dated horizons that were not available to me in the late 40s and earliest 50s. Nevertheless, those transfers should have been made correctly. What to do?

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Thu, 27 Feb 97 17:57:53 EST
To: <head@quartz.geology.utoronto.ca>
Cc: <traverse@ems.psu.edu>, <palynolo@geo.arizona.edu>,
"Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: re: Re: Are paleo-palynologists becoming old-fashioned?

Dear Al,

I don't think we are becoming old-fashioned when we keep using the proper old names for some old families; as a matter of fact, the code authorizes the use of the politically correct 'alternatives', but doesn't suggest anywhere that they should be preferred. I like the poetry of the Labiatae (for which the alternative is Lamiaceae, not what you had in your e-mail, Martin); the intuitive correctness of Compositae; the symbolism inherent in Cruciferae; etc. No, I don't think we should coerce anyone to veeraway from the old values and customs.

Greetings all,
Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Thu, 27 Feb 97 13:09:39 EST
To: <traverse@ems.psu.edu>
Subject: Nova Scotia Triassic

Dear Al,

Sorry to bother you again, but...

The request from the geologist I'm working on (I hinted to that in my last e-mail) involves the age of the salts underlying the Cret./Jur. strata in the general Grand Banks area (Whale Basin) of the Canadian East Coast offshore. John Utting thought that you (with a student?) had published on that, and a check of Paleopalynology seems to support that possibility -- but there were no references in your text that might bring us closer to a source.

If you were involved in some (relatively recent) work on that topic, I would much appreciate any references to literature; the main question seems to be: is the salt of Jurassic or Triassic age?

Solong,
Jan

From: jjansonius@gsc.nrcan.gc.ca
Date: Thu, 27 Feb 97 12:36:32 EST
To: <traverse@ems.psu.edu>
Subject: THANK YOU

Dear Al,

Thank you for the copy of the paper on Amtaspora, and the copy of your e-mail to Greuter; although I wasn't 100% clear on the points numbered 1-5, your answers are sufficiently informative for me to get the drift of your position, and what Greuter had been raising. I will come back to this later, but first have to finish a rush request from a geologist.

Bye, Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Wed, 26 Feb 97 16:41:01 EST
To: <traverse@ems.psu.edu>
Subject: Traverse 1994

Hi Al,

I was working on line drawings for Pathenopollenites and Gordonipollenites this weekend. I had some questions pop up in my mind that I pass on for your comment:

The type of Parthenop. shows a nice round pore (os?) in the central colp, but in the other two colpi it seems that there are two bright spots that might be considered as reflecting a pore: double pores in one colp? (it could be). or an optical illusion caused by an oblique compression? I wished that you had given a new description of the type, now that a new genus is based on it. I also puzzled over the relative thicknesses of endexine/ektexine; how long are the columellae?
And for that matter, it is rather unclear what you had in mind when you say "Also transferred to this new genus are the forms referred to T. marcodurensis by Thielle..." -- you didn't transfer anything, did you?

Similarly, I wished you had given new info, based on a new examination, of the type of Gordonipollis -- I can't really say whether or not I see columellae, or the high muri you said are present. And is the cover of the 'colp' ornamented with scattered granules/verrucae?

Got to run!

Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Wed, 26 Feb 97 14:16:56 EST
To: <traverse@ems.psu.edu>
Subject: re: spore

Grana, 5(2): 183

I've been thinking on Marsypiletes cretacea -- the Code says to follow gender assigned by author. But, the Code also assigns a fixed gender to several suffices (e.g. -ites, -oides, etc.). We've known for many years the genus Triletes, which is masculine. Should we petition (propose) that -etes be added to that list, and make it masculine, as -etes is treated in Triletes? Brown lists -ites as a Greek suffix indicating 'having the nature of, like'. Is -etes just an ignorant variant of -ites? Can Triletes be used as a precedent to treat Marsypiletes as m? Is this sort of ending characteristic for fossil names, or might there be precedence among the extant plant genera?

Enjoy yourselves in Indianapollis etc. Greetings,

Jan.

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

Dear Jan:

We are about to set off for Columbus and Indianapolis. Today, however, I have been working on the same little problem about which I got your help a while back--the samples that contain execrable examples of Appendicisporites (al. Plicatella) jansonii. I have another spore that is obviously very close to an item that Burden and Hills, 1989, (AASP Contrib. 21, p. 126) call: "Concavissimisporites asper (Bolkhovitina) Pocock". Troubles are: 1. I find no place in Stan's estimable works where such a transfer is made--can you illuminate? 2. The Concavisporites/Concavissimisporites situation seems to be a mess, with species being transferred back and forth, etc. This item sure doesn't agree well with the "dumbbell shape" that is supposed to characterize Concavissimisporites. Hmmmm.

I hope you can at least guide me to the appropriate SAJP paper and, if possible page. Other comments would be appreciated. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Wed, 19 Feb 97 14:26:23 EST
To: <zijlstra@cc.ruu.nl>
Cc: <traverse@ems.psu.edu>, "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: correction of names

Dear Gea,

It was nice to hear from you again after several years -- so reassuring that we are all still on our hobbyhorses.

I have long found the guidelines on spelling corrections in the Code perplexing and inconsistent: why (Art. 60.1, ex 3.) must "benghas" be changed to "renghas," but not (60.3, ex. 6) "Lespedeza" to "Cespedeza," or "jamacaru" to "mandacaru"? If there are factors in the respective protologues that justify these decisions, they ought to be specified in these examples. On their own, as given in the Code, they make no sense. Still, the examples 2 and 4 make eminent sense to me. It is 55 years ago that my dad explained to me that the Ginkgo tree was named for the Chinese word for it ("Gingko"), but that a butterfingered typographer saddled us with the incorrect romanization "Ginkgo" -- which still is the official name.

Another source of wonderment is why Art. 60.8 does nothing more than make a rule out of a recommendation (60G, which itself has no binding force, and contains no cross reference to Art. 60.8); why not import the text of 60G into Art. 60.8? However, maybe the following paragraph points to arguments against that.

Significant numbers of palynologists in the past have used the device of the connecting vowel to indicate whether a spore is "alete" (-a-), "trilete" (-i-) or "monolete" (-o-) [e.g in: Reticulatasporites, Reticulatisporites or Reticulatosporites]. I would consider it a disadvantage to bring all these connective vowels in accord with classical Latin practice -- after all, the Code gives us freedom to compose our names from any source whatever, and in an absolutely arbitrary manner. This latter freedom (Art. 20) lets all those off the hook who didn't experience the pleasure of Latin grammar and vocabulary, and who have no eye or ear for Latin phrasing -- who will coin epithets like 'fenestratous' (fenestratus), 'infidelus' (infidelis) etc. If those words are obviously meant to be Latin (and they almost look and sound like the true thing), should we have the duty to help our brethren and make the necessary corrections when we come across them (like in 60ex 2, 60ex 4). Or, do we protect their 'barbaric' mistakes as if sacrosanct (as in Ginkgo)? One reason for rationalizing all such 'semi-Latin' names would be the problem of grammatical correctness in cases of transfer: the feminine form of "fenestratus" is "fenestrata" -- is the feminine of 'fenestratous' also 'fenestrata', or is it 'fenestratoa'? (A propos, I never understood why an erroneous connective vowel in an adjectival epithet could (should) be corrected, but not an equivalent error in a generic name. Art. 60.8.) You say

that the liberty to correct (or not) as given in Art. 60.3, should be taken away, and replaced by specific guidelines for what and when to correct, and when to maintain forms corrected earlier. My rebuttal to that would be that it is practically not possible to cover and presage all possible errors (people are always more stupid than you think) and it would soon become a quagmire of regulations.

Even if people were required to give an etymology/source of their new names, it might help to work with them intelligently. The Code, however, DOES allow a name to be formed totally arbitrarily; such names would be beyond the reach of corrections -- even a typo would not be evident. However, we might have a rule that only (but then all) names that are purported to be Latin or latinized should be subject to corrections if proper procedure in compounding, derivation, grammar etc. had not been followed (either for reason of ignorance, or in case of error).

Before I go further into your questions, I have a suggestion: few have Latin any more, and many people make unnecessary errors in coining names; also, too many people publish in obscure journals (for which reason the ICBN may require, in the future, a thing called "registration", or publication only in "accredited" journals (will Elsevier fund future budgets of International Botanical Congresses?) I think it might be a less onerous remedy to require prospective authors to just submit their new nomenclature to the scrutiny of an official committee that will check names for correctness and otherwise appropriateness? The authors can then also state where they plan to submit the MS, and a yearly list could come out containing all new names and where they were submitted. Names not on that list would not be validly published. It seems a simpler task to me than what is being proposed now. However, it still would put sincere and aspiring, but underfunded taxonomists in less developed countries at a disadvantage, because even the existence of such simple rules might not be effectively communicated to them.

Anyway, back to your letter. In principle, especially as I get older and start to mellow ('mallow' Traverse would write, as he likes a simpler standard spelling) I let more names go unmolested by my tendencies to reform and correct. It's a job for schoolmasters, 'correcting.' Of course, I understand that computers do not like deviations, and everything must be just so. However, computers are now smart enough to correct my spelling as I type, and I can program them to tolerate alternatives and variations. One thing about published corrections: they rarely are seen by the same people who saw the original error. And, if they see a correction, it might not register with them -- you know how difficult it is to proof your own MSs, and how easily you see the errors in someone else's writing. Also, there are so many legitimate variants.

Your immediate question revolves about Annona, which has been incorporated, in the reduced form of 'Anona,' in the names Anonaceaephyllum

and Anonaspermum. Sloppy work maybe?, possibly under influence of shifts in pronounciation and changes in phonetic values attached to vowels and consonants? (I must think of the spelling changes in the Dutch language every decade, that are defended by a few appointed ones at a high level: will you henceforth eat 'pannenkoeken'?) Still, once these 'Anona-' names have been published with a single 'n', I think it is best to maintain them unchanged as validly published -- see Art. 20.1.

And how would you treat the variations "Paleo-' and 'Palaeo-'? Would you select one of these and make all others conform?

My conviction is that education is a far better way to unify usage in writing and orthography, than is policing. Many people don't care too much because it is really not so important. Personally, I think it is better to have good taxonomy in sloppy nomenclature, than sloppy taxonomy in impeccably latinized form. The point of course is that, where you and I can spot sloppy nomenclature immediately, we may not be qualified to criticize the underlying taxonomy.

No, I do not know if these names with single 'n' are in (common) use -- I am just a palynologist, and do not keep abreast of the paleobotanical literature. Ask people of the IOP (Taylor, Boulter, Collinson, Stockey, Kerp, Dolph, Dettmann, etc.), they might have an answer. However, for your argument I think it should not be important whether or not a name has been used in the last several years. In all branches of paleontology, it is common to go back to literature 50 or even 100 years old, which can still be totally relevant. We should avoid emulating the librarians (in the local oil companies, but even the public libraries) who toss and recycle anything that has "lost relevancy" and has not been loaned out in the last 5 or 10 years. This is how a large part of the manuscript music of J.S. Bach was "put to good use," ie for wrapping fish in it! It is NOT relevant whether or not a name is "in use" -- much of our science is not "in use" these days. But it will not loose relevancy, just because it has lost some immediacy. The funny thing is that, where there is less money for curating old collections (books, specimens), there is enough money for compiling NCU etc.

If Schwenkia was good enough for Linnaeus, and if it took 200 years before a nitpicker in my generation took offence and made it into "Schwenckia," I would say "a pox on his house" -- didn't he have anything better to do? Is that what society expected, when they subsidized his doctoral research? And what would Mr. "Lespedez" think of all this, why doesn't Brummit also correct "Lespedeza"?

I think that the ICBN was a nice guide on how to make names, and how to deal with names already published: how to sort the chaff from the grain. Lately, however, there is a vocal minority who want to remake the world; or at least botanical nomenclature, in the image of what they see as the platonic ideal. These are the people that just compile lists of NCU (and then you have to ask me if I can vouch for some undoubtedly useful names having been used recently) and who then will make that list safe from interference from the rules of the ICBN. (These people know better than you and me? I find

it some conceit of the compilers of NCU-3, when they ignore Art 60.5, and go with Arthraerva instead of Arthraerua.)

Finally, I am sorry to see so much time and effort wasted in sterile exercises as NCU-2, NCU-3, a "BioCode", etc. The problem as I see it is that the rules allow some folks to make themselves 'legitimate' (oblivious of the fact that they offend the silent majority with their antics). As long as it is "validly published" we cannot ignore it (them). Yet how many scientists, honest in their endeavours but short on resources, even have the latest (and only legitimate) edition of the ICBN available? And, why would our code change so much every five years that one has to make a thorough study of it (before it is changed again). Why would reliance on only a 1988 edition of the Code, if no newer one is available, be so laughable?

I am sorry that in the end I have not been able to give you any firm information -- just an earfull of my spleen; of course, I don't know who first changed Schwenkia to Schwenckia, and where (so I couldn't check the 'why'). However, my notion is that we need fewer regulations and fewer straight jackets; in tandem, we need better education so people will treat nomenclature with understanding and respect.

I hope you do not mind that I send a copy of this letter (and your fax to me) to Al Traverse and Rob Fensome: between us, we have a fairly active interchange of problems and questions on nomenclatural matters arising. Also, in rereading your letter I note that you belong to the "seldom correctors" -- I hope I, too, will be considered to fall into that group.

Hartelijke groet van
Jan J.

From: jjansonius@gsc.nrcan.gc.ca
Date: Tue, 18 Feb 97 14:30:25 EST
To: <traverse@ems.psu.edu>
Subject: various, incl Chaloner

Dear Al,

I enjoyed your little discourse on the biocode, and what a Code ought to do (and what to leave alone -- like well enough). It struck me that you should send a copy to Rob Fensome. Actually, you might consider polishing it up a bit, and submit it for a wider audience to read.

I also had fun with your divertissement on the Chinese theme, which included several new bits of info (like the application of characters in dyslectic study).

Yes, the address of sender on my e-mails is puzzling -- has something to do with the fact that it has to go thru the big server in Ottawa. The address you used (jjansonius@gsc.nrcan.gc.ca) is the correct address.

I had a letter from Gea Zijlstra, who has problems with corrections of names, when they are to be allowed, and when not. And not necessarily should an 'allowed' correction be adopted. And how to accept corrections 'officially'? I shall shortly send you a copy of it by fax, and later of my reply -- that way you can jump in and get rid of your two-bits of experience and wisdom.

As always, in a rush, but with my best,
Jan.

HERBARIUM HEIDELBERGLAAN 2 3584 CS UTRECHT THE NETHERLANDS

CC → A. Traverse

Utrecht, 17 Feb. 1997.

Dr. J. Jansonius,
c.o. Inst. of Sediment. and Petr. Geol.
3303 33 Street NW
Calgary Alta
T2L 2A7 Canada.

Beste Jan,

May I ask you opinion on the spelling of two names? They are not of pollen/spores; nevertheless Wim Punt suggested me to ask you.
As the secretary of the Committee on Orthography (established at the Tokyo Congress) I am considering cases of spelling corrections. I think the main source of the present confusion is the present Art. 60.3 (formerly 73.3), according to which there is a 'liberty to correct'. I think this liberty should disappear, there should be guidelines indicating when correction is necessary / when corrections of earlier authors are acceptable, and for other cases, corrections should not be admissable.
I have a number of examples:
cases in which earlier corrections have not been followed
cases in which two spellings are in use (original and corrected)
cases in which corrections made long ago are accepted by everybody
cases that are uncorrected, even though according to the ideas of some people, such names should be corrected.
It is the latter category that causes me to write to you. I have the impression that palaeobotanists seldom correct.

There is the genus *Annona*, name-giving for the family Annonaceae. There are the genera (when published, compared with '*Anona*') *Diclinanona* and *Stenanona*. These spellings are in use, workers in this family belong to the category of 'seldom-correctors' (a category to which I also belong). ING also has *Anonaceaephyllum* Andreánsky 1963 and *Anonaspermum* Ball 1931.
My questions: are those two names for genera of fossil plants in use? Have they ever been corrected into *Annonaceaephyllum* and *Annonaspermum*? These are the most important questions. If you might have further information, I would also like to have an indication of how many species are recognized in each genus. (If you have no idea, for the latter question you should refer me to a specialist in Tertiary leaves / seeds.)

A few examples to compare with:
Schwenkia L. 1764 was (in 1963) corrected into *Schwenckia*; NCU-3 (1993), however, went back to *Schwenkia*!
Two genera on publication were compared with '*Schwenkia*': *Protoschwenkia* and *Schwenkiopsis*; in 1992, Brummitt corrected them into *Protoschwenckia* and *Schwenckiopsis*, and these spellings are in NCU-3!!
Arthraerua on publication was compared with '*Aerua*', which is an orth. var. of *Aerva*; Brummitt 1992 retained the original spelling, now it is NCU-3 that 'corrected' into *Arthraerva*!!

I hope you can give me some information. Thanking you in advance,
Yours sincerely,

Gea Zijlstra

Gea

e-mail: zijlstra@cc.rug.nl

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

Dear Jan:

Ye gods and little fishes. Your two messages of the 14th carry the "from" address as: Jan=Jansonius%PALEO%GSC=CALGARY@bio.ns.ca.
WHAT is going on? I hope this one to your "old" address makes it.

I enjoyed all stories in your long message. That's the same one in which translation is discussed. I don't think you're right about copyright problems, though. Photocopy is no worse than just copying the text verbatim. But I think there is some sort of limitation as the length that counts, not so? I don't think quoting (copying) part of a paragraph of one page counts. But I'm not a lawyer.

For a very small amount you can buy a device that stamps numbers sequentially on a sheaf of papers. I have such.

The difference between English and Chinese characters is that Chinese is actually a family of languages no more closely related than Russian and Dutch. The pictographs were employed instead of phonetic symbols because the empire could communicate with people who spoke these different languages (they are not "dialects") using the pictographs but not with phonetic symbols depicting only one of them. My very good friend, Ken Hsu, explained all this to me in Houston when we often dined in Chinese restaurants. He would design a meal, ignoring the menu, but could not converse with the waiters, who were Cantonese, because he is from Shanghai. He would take a piece of paper and write in characters what he wanted, and they would immediately understand. As an aside, I could mention that my brother-in-law is a professor of reading--in a school of education. He says that dyslexic American kids can be taught to read English effortlessly if it is written in Chinese pictograms. Incidentally, there is no way that Chinese characters "are supposed to sound"--except in one of the Chinese languages, for instance, Mandarin. They are sounded completely differently in Cantonese. The Japanese adopted the characters a thousand years ago and about 60% of Japanese text is Chinese characters. Japanese is totally unrelated to Chinese (it's related to Turkish--very remotely), so the Japanese pronounce the characters as Japanese words. The principal problem with the whole system is that it is approaching impossible to have a dictionary in which a person can find his way around. The Japanese use two different phonetic alphabets in addition to the Chinese characters, so lexicography is a nightmare there

President's Day means only that you can't go to bank or post office.
When I was a kid Washington's birthday was important and fun. Of course, at home I had to deal with parents who were both closet Loyalists!

Best. Al.

From: Jan=Jansonius%PALEO%GSC=CALGARY@bio.ns.ca
Date: Fri, 14 Feb 97 17:52:58 GMT (Original AST)
To: traverse@ems.psu.edu
Subject: re: yours of 6-II-97
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al,

I'm back at the computer. Your mom was thinking of Michiel de Ruyter, who chased the English pirates from the proper shipping lanes, right up into the Thames estuary -- then returned home with a broom tied to the top of the mast: he had swept the seas clean! He is the most popular of the Dutch admirals, a clever sailor, and a good man.

Yes, I'd love to know if vdH has an e-mail address; and if not: what is his mail address. Now, I have no means of reaching him, and the Amsterdam people have not been very diligent in forwarding such info.

You can say that it is in regard to a revaluation of his old nomenclature, what of it is ok, what can be salvaged, and what needs remedial action and what form should that take. He probably does not have a copy of the last (current) edition of the ICBN.

Yes, DO open the package from Bob and see what you received and how you like it. I would much like to have your opinion. Also, Colin and I are carefully (without yet committing ourselves to anything) canvassing and collecting opinions on what is missing or treated insufficiently in these three volumes, with the idea that we might work up a fourth volume in another year or so.

About the lost volumes: there haven't been all that many (but still too many). They are sent by "private bag"; the original idea to use UPS was too expensive. Now they go fourth class, and it is not possible to insure such. However, insurance would be too expensive, so Bob is taking his chances and hopes our losses will not be too large. Where the lost copies wind up is unknown.

This brings to mind the pericles of a set of the Genera File. John Utting met a young palynologist working on Permian problems in Kazakhstan; she had virtually no library, so he asked us (Hills and me) if we would give a set to her, if he would pay for teh shipment. Ok. He prepaid shipment with KLM through Leningrad to Kazakhstan; In Petrograd, customs intercepted it, and sent KLM/Utting a bill for &250 for excise and custom clearance. After protests that this was a gift from the Canadian government to a Russian Institution, this was lowered to $100 plus $100 for storage (it had been sitting there for so long). We told them that there was no commercial value, and that we would not pay such charges. They answered that the Kazakhstan man could come and clear it: he would have to fill out forms to make an

appointment, but they could not guarantee that they would see him within a week from him arriving in Petrograd, and then there might be further delays. Well, she didn't have time/money for such nonsense. So the upshot was that the File just disappeared -- in the landfill? (Who would have use for that on the black market?) Finally, Judi Lentin, who travels a lot to Russia, took a copy as personal effects, which now sits in Moscow with Val Krasilov till Kazakhstan can come and collect it.

You are of course correct if you say that translations ought to be properly acknowledged; it is too late to start that in the Genera File -- it would make a complete mishmosh of it, with people thinking that all those without notes would have been in English. If Deo volente there is time, I might consider a total rework of the whole thing, and verify for myself that the tranlations and information proferred are all correct and complete. For now, a lot of people find the thing usefull, but I am the first to say (and have said it to many) that it is merely a starting point for sorting out nomenclatural problems -- but it is out of date in part, incomplete in others, and occasionally in error and misleading. Its the best, though, on the market for what it is, if you are interested in that kind of thing. More important might be to give fuller bibliographic references for everything. And as for gluing xerox of original text: that would soon require a massive correspondence to ask permission to use copyright material. As it is, I'm skirting what is legal, and trust that people will acceot common sense in these matters.

Your experience with dropping massive term papers is not in my ken. But it did show me the value of numbering pages (I also think that many of such papers are filled with repeating what is well known already, anyway. A frugal editor might show them how to save some trees). I might do that next time around (if you want to see the mess again); then, I should also have a xerox of the illustrations.

As for English spelling, I have grown mellow on that; especially since I took some evening sessions in Chinese, I have developed the realization that English is much like Chinese in that there are so many homophones (so, sew, sow; to two too), verbs used as nouns and vice versa (a good sing; plow), and only the merest trace of formal grammar (of coarse you can parse a sentence, but compared to French or German there is nought). These things make the language easy to speak. If you have a bit of background in other languages, and know the phonetic principles used in those, it is actually easy to spell correctly in English. The non-phonetic aspect of English' spelling is a help to combat ambiguity, for English is just as non-phonetic in its writing as is Chinese (where with training it is easy to see how, approximately, a character is supposed to sound).

As it is, the language is changing rapidly enough, we don't need to aggravate things by changing spelling as well as meaning. And ask the Dutch

populus what they think of the fourth change in spelling rules and new application of more fonetics in so many decades. All know that bureau must be French (from Bordeaux, of course); but it is more difficult to 'recognize' buro - which might be a young neighbour? -- naybor, you prefer?)

I think I will first finish the Genera File supplement (1992, the last one was); then, I'd be hep to write something on Stuchlik, and vdH; and I would enjoy a joint venture. But now, I have my plate full with that, and the big compilation of Ram Kalgutkar, that I am helping to edit and pilot through the nomenclatural shoals (shoels?, sholes? shouls?).

Have a nice weekend, enjoy your offspring (ofspring?) and President's Day -- I know you must love Clinton.

Jan.

From: Jan=Jansonius%PALEO%GSC=CALGARY@bio.ns.ca
Date: Fri, 14 Feb 97 15:26:21 GMT (Original AST)
To: traverse@ems.psu.edu
Subject: re: sundry
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Dear Al,

Tell Bwetty that she might consider celebrating at home, with a home cooked specialty that used to be a favorite of her (daughter, that is). With grandchildren underfoot, we now often go to a bombproof place, so Bettie has a chance to appreciate everybody's company; but when fils aine is over from Montreal, he prefers the old meals (nothing fancy) that are hard to duplicate in the commercial world.

I find that actually I had most (all?) of your e-mails printed, but they were covered by more recent sediment -- you know how things get dusty.

I think I'll stay with M. cretacea; if the original author was thinking feminine, and when there is no official gender attached to "-etes", I'll leave well enough alone.

Enjoyed the bit of history!

Will react to ther trifles etc. later. In haste,
Jan

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: yours of 6-II-97
Cc: rfensome@gsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Jan:

Now I note that it is YOUR letters that you don't always copy. My comments about travel were occasioned by your characaterizing me as a "gypsy." Watch it, fella, that's right up there with my mother's favorite exclamation, "Wouldn't that beat the Dutch!" I believe that expression started in UK when Admiral what's-his-face was terrorizing shipping in the 16th century.

If the ING says, per your quotation (I haven't checked it), "It is not intended that these volumes publish new....." that is all the disclaimer that is needed to prevent them from accidentally publishing new names.

Yeah, I'd love to see vdH's place. One of my former students is now prof at UC Berkeley. It was she whom we visited last year to give my fossil collections to. She works all the time in Colombia and has met vdH. Should I ask her for info--if so, what?

Your inadvertent suggestion that I take the new books to Indy for spare time reading isn't bad. I still haven't even opened the package. I really felt bad that Bob C. had to ship me two copies for me to get one. I did sent him the money for the reprints and my small contribution toward page charges when I finally got the books. But who got the ones that went awry?

Enough for this one. Best. Al.

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

Dear Jan:

Just discovered that all restaurants in town are booked because of (I am not making this up) Valentine's Day--so our plans to celebrate a daughter's birthday at a restaurant is down the drain. Betty is on the phone trying to get take out somewhere. What a mess.

So, I have a little while while Betty is on the phone to grab her computer and send you comments on various matters.

First, you should have got a copy of a missive to Doug Nichols about the spelling of the specific epithet for the type of Marsypiletes. He responds that it should remain cretacea, and he just erred in the publication to which I referred. I am not so sure this is right. Opinion, please.

It is discouraging to me that you seldom (apparently) print out my letters to you. I don't print everything, but JJ letters I certainly do print--couldn't possibly work from a few scribbled notes or whatever.

My idea of your using the original text of descriptions contains no suggestion that you would have to "type" them! I was thinking of xerox.
Surely such technology is available in Alberta? If you use translations, it needs to say "original in Russian" or some such wording IN EACH CASE.
This is simply bibliographic standard practice.

English needs a total spelling reform, not the trifling steps American English has taken. q should be abolished. c should be abolished. School should be spelled skool, etc.

All MS should have numbered pages. Take it from a reader of student's thesis material who has occasionally dropped a swatch of it on the floor (or worse). It only takes a couple of minutes to do this little chore, and it is so helpful.

I'd like to see us go ahead with the Stuchlik counter, and am ready to help in whatever way I could.

"Heighth" is undoubtedly, you are right, baggage from my rural Eastern Canada origins. Makes sense, huh?--length, breadth, heighth. But I know it isn't right and just slipped. I dear dad, though an Anglican priest, until his dying day said "filum," "elum," and "sipper" (instead of film, elm and zipper), among many other things. He occasionally slipped and said "izzard" for the last letter of the alphabet, etc. He was from the Gaspe, but we aren't of French Canadian

origin. The John Traverse who landed at Paspebiac in 1786 was a loyalist, a member of one of the Loyal American Regiments, stationed in New York City until the end of the war. He was in the quartermaster department. Traverse is indeed a French name, but that goes back to Norman time.

Just made a xerox of the Amtaspora paper, and Betty is stuffing it in an envelope to mail to you. Means "official spore"--right?

Sign off here to take care of another matter, but I'll be back. Best. Al.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: Marsypiletes
Cc: nichols@usgs.gov

Dear Jan:

Have been working with a student here who is using palynology as a stratigraphic tool in her work in the Cretaceous (Castlegate and other formations). In her systematic section she describes her specimens of the abovenamed genus as "Marsypiletes cretaceous." When challenged on the spelling, she referenced and showed to me a paper by Doug Nichols in which the name is indeed so spelled.

I note on card 3574 that the original spelling of the specific name is cretacea. I am very "konsequent" about recording all of your revisions, so I guess you haven't picked this one up yet.

However, let's assume that the orthography needs to be changed to masculine gender (Art. 62.4 says so for -ites but doesn't directly mention -etes): would it really go from cretacea to cretaceous and not to cretaceus?

If there ever was a question that was down one of your alleys, this is it!

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Mon, 10 Feb 97 16:46:59 EST
To: <traverse@ems.psu.edu>
Cc: "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: Ross' names (incl Foveotriletes)

Dear Al,

This feels like old times. Your reply and discussion made me see several things in a different light, and I think I'm closer to the truth.

Ross didn't say how we should interpret his quotation marks. He also is rather loose with the use of names that look like (and in part are/were) fossil spore genera (Tricolporites, Monolites, Trilites), but that he uses more like 'turma' names. These are (mostly) the ones that are in quotes. You are right: they should all be ignored -- i.e., they were not (intended to be) validly published. That then includes "Triorites vestibulus" and "Trilites scrobiculatus" (but should it also include "Microlepiidites psilatus"? I guess yes). Now, the next to take notice of the species T. scrobiculatus are Weyland & Krieger 1953; however, they do not give a full bibliographic citation of the basionym, yet treat it as a 'n. comb.' Of course, it cannot be a new combination because the old combination didn't exist in the world of the ICBN. Thus the authors of the binomial Microreticulatisporites scrobiculatus are Weyland & Krieger; these also figured a single specimen that shows pitting of almost an order finer than that shown in Ross' specimen. Are they conspecific? And, which one is the type specimen: did Ross effectively publish a type specimen even if he didn't publish a new species? If W & Kr didn't explicitly refer to Ross' specimen, is their sole illustrated specimen automatically the type? On reflection, I think it must be so. (Obviously, both specimens belong to the same genus). Or, must one of these specimens be designated lectotype? (In which case, would the worker making the selection have to say where it is stored? but, the species and genus are valid without a type, already)

I have the feeling that J & H, if they want to straighten things out, will be bound to the present ICBN rules as much as any author -- which in practice will mean that we will have to alert, prod and guide a lot of people that are hard to find and hard to correspond with, and maybe indifferent -- it may be that a number of loose ends or unclear situations will remain so in future. I increasingly think that the changes in the ICBN were not for the better. Jeder Consequenz fuehrt zum Teufel, and I think that Greuter is trying to be too consequent; unfortunately, he is dragging us all to the devil with him.

xoxoxoxoxoxoxoxox

I am now developing a bit of momentum in getting the next Supplement

of the Genera File ready. So, this weekend I sat behind the computer for hours, and I have dealt with a good number of the MS pages that you read through; I nevertheless find that the application of the spell checker is useful. I also came across your letter of 26 September -- I don't think I had responded to that one yet. So here are some answers:

The MS pages with only a name are entries in an early stage of development: I still need translations and sometimes still need primary literature. They were already included for your benefit too, to see if you had included all those in YOUR records (if you still maintain those). It also is a double check for you to see if you have a new name that is totally absent in my files.

I now list the "type species" as "type"; however, I still give a description of the 'type species', which includes all other specimens; and also, which is the basis for a number of older genera. I don't understand why the term has to banned completely; I have no truck with political correctness. (Many women made fine chairmen). Also, I thought that the grammatical use of "he" as an indefinite pronoun (equivalent to the German 'man', which undoubtedly is very close to 'Mann') was very functional, and in no way sexist. Type species is a live term, and I wont bury it as long as it is kicking and screaming.

I did state in the intro of the first installment of the File that I had translated all text into English. This was of benefit to some of my colleagues, and I think to many peole (and now English is even mandatory for new descriptions!). Your idea to give the text untranslated is not practical (I am not going to type Russian or Chinese). Of course, although I did many of the translations myself (and rather sloppily in some of the older records that were not then intended for publication anyway), many were tranlated by hired translators or foreign colleagues. The cards are really a first line of defense, but those really 'into' nomenclature will want to check things out themselves, anyway.

/ but you must! say "original in Russian" etc. !

I am with you on simplification of English spelling; still, I make mistakes. I recently read that in England itself, there were early efforts to have color for colour, etc.; these were stifled by, was it school reforms? (I forgot, but I remember that it is not strictly an American initiative.) The Americans tend to go too far in that direction, such as having 'forewards' in a book, instead of on the soccer field.

Really unusual words (you cite 'cochleate') may need a dictionary for some. But then, that is not a bad thing, they might pick up a few more words off the same page. I shall not intrude with my presumptions of what I think some beginning readers may need help with to cross the road. Mostly, I wasn't the first one to use them, because they wouldn't come to my mind so readily!

You ask, why didn't I number the MS pages. Well, they were all printed individually, and then sorted into alphabetic order, and xeroxed double-sided (to save trees). It is not practical to number them; however, you found that practical to refer to earlier remarks, and I found it easy to follow your references. Together, we got there!

Re the Stuchlik names, I am rephrasing them and cutting out a lot of the verbiage. You helped me with that, too. However, I have to refer to some parts of the ICBN; till then, I am leaving some of the spleen out. And, really, that can stay out, later, too -- if we get to write that paper: it will have to be cooly factual. Still, we better make sure that we head them off before they publish a sequel. Yes, the Genera Cards are meant to inform on the existing literature (your 'Congressional Record'), but also it has been a source for straightening things out (like adding info that was initially not there, sometimes prohibiting the publication of a name to be valid). I'd like to maintain that stance, although I have become more circumspect with the years; as you will see, there are many homonyms in the File that I have never replaced, because I figure that that best be done by someone more familiar with the forms in question. Too bad nobody seems to care much.

Len is a quick and accurate proof reader, and has been (and is) of great help in getting the publishing and distribution done (and the financing of all that; you remember how tricky that could be) and in maintaining the administration of the system. He also contributes new literature and some info on especially megaspores. But otherwise, yes I am typing most of the text (and I intersperse my initials in that, rather too freely, I sometimes fear. JJ)

I puzzled a bit over your "heighth" (musing about the pronounciation of high, height, etc. when you described the intensity of fall coloring you anticipated in the Andirondachs (Insley Camp at Roosevelt Rock); but then at the 8th hour I reckoned that your French-Canadian background was shimmering through.

Am I chewing on your ear too long? If so, say it. In the mean time, thanks for your help and verbal sparring, which is of much value to me.

Best regards,
Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Thu, 6 Feb 97 13:01:20 EST
To: <traverse@ems.psu.edu>
Subject: ...no subject...

Dear Al,

Thank you for the replies to some of my earlier letters. I have concluded that I will have to keep copies of the e-mail I send to you, because in some cases I have almost forgotten what I was asking or arguing about. However, I hope to recover those ideas (I just found that the ones on vdH's names were still in my mailbox, and I now printed them). Similarly, I don't recall what I said that made you think I petulated about your travels -- I am not myself a roving man, and I am happy to stay put in mon petit monde and watch things wax and wane, and adapt to change. But if you like travelling, I hope you enoy your trips together. I hadn't been aware that you had such a (long suppressed?) yen to wander, and maybe my expectation had been that you would sooner be home dealing with important matters like nomenclature. However, I will say that as I grow older and a bit more tolerant of things in general, I become more annoyed by firebrands like Greuter (cum suis) who want to recreate the world and force all into THEIR straightjacket. So many things that ought to be B/W now show seams of grey and other colors that indicate that there may be more possibilities of different interpretation, and other ways of understanding, of how things should be referred to and looked at.

I did look in the ING, and on p. xii-xiii (hidden in a long intro) I found these statements: "The ING is a compilation of previous published information. It is not intended that these volumes will publish new combinations, new lectotypifications or substitute names or effect a change in the status of a taxon. However, it is possible that such errors unintentionally have become part of the Index." "It must be stressed that in controversial cases the ING data need to be verified" ...and the existence of 'various opinions about the interpretation of the rules of the ICBN among ING collaborators is another reason for doing this.'

The statement that "it is possible that..." doesn't sound much like a disclaimer to me, but rather that if such unfortunate things happened, it was not intended but now we'll have to live with them. Yet, the exhortation to verify controversional cases implies that the ING is not (necessarily) the final word. Too bad that a disclaimer was not printed on the first page, just by itself. Many will not take the time to plough through the Preface and Intro, and will approach the data from a wrong perspective.

Yes, of course, only those vdH names with a modern type need to be replaced. I'll try to get an address for him -- I tried a bit before, but I didn't get an reply yet. I'll try a different venue. Thank you for indicating a willingness to become involved in this. I understand that vdH bought a big

tract of land in Colombia, and is in the process of rehabilitating it to its natural state. Apparently, it is becoming a bit of a show piece, and attracting many visitors into the jungle. (You think you might want to go look?) A propos, what was the 'proverbial hooker's nightmare' about?

Maybe the comma in the compuserve 'number' address was the reason for my e-mail to van Hoorn bouncing back to me; have you tried the list of e-mail addresses in the AASP webpage to make a contact with Amsterdam? I re-sent my e-mail to van Hoorn with a period instead of a comma, and it didn't yet bounce back -- maybe I did make a contact, then. Thank you!

I kept a copy of my e-mail to you discussing the Triletes problem in the xerox copy of Rina's paper. From your reply I deduce that I had come to a conclusion with which you agreed.

Finally, as I went through the annotations you made on the MS of the new Genera File cards, I noticed your puzzlement about Chokierian (which I had misspelled) -- you will find this term on pp. 474 and 475 in "Palynology: principles etc."; curiously only one of these two tables contains the term 'Namurian.' How did you like the book, anyway? Suitable for taking along to Indianapolis for bedside reading?

Enjoyed your childhood reminiscence and felt the safety and warm comfort of gran's skirts. What was her nickname for you?

Best regards,
Jan.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: T94
Cc:
Bcc:
X-Attachments:

Dear Jan:

NOW, I'll try to deal with yours of 10 Jan. re my '94 paper.

1. You are absolutely right about "typicus"--what a clanger. I had that firmly entrenched in my mind and didn't bother to check the Code, I was so sure. I just cited the article from the index, I guess. However, according to me it is not a good name anyway. I'll mention my error if I ever publish something further on the stuff.

2. Re Rhuspollenites, you are right that I should have listed T.-P.'s R. ornatus in the synonymy. The rest of what you say may be true also, but is taxonomic opinion. At the time of writing I had a different opinion.

3. Re Parthenopollenites: again, you are right, I should have made the transfers and listed the species. Of course, the editors were already on my case for the paper being too long. I guess this doesn't affect the legitimacy of P.

4. I thought in both Vitispollenites and Rhuspollenites that my language meant that, "if she knew about it, and if she agreed that any fairminded person (ahem) would so consider it, she should have....." Again, I guess this doesn't invalidate the idea.

In answer to the question you posed, no, I don't think these matters need to be addressed in the cards, but they are your business, and if YOU think so, go ahead and do it. My opinion is that the cards should address only what has been done, including what has been done errantly, but not what might or "should" have been done.

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In the same letter, the Triletes business. Strange, because I just wrote a letter to Tom Taylor, taking him to task for publishing a paper about "Rhabdosporites spores....", so called in both title and abstract, while in the paper itself one discovers that these are the spores of Tetraxylopteris, and the spores studied all come from the sporangia thereof--not a Sporae disp. in the lot. I said that he should have said in the title: "Spores of Tetraxylopteris (=dispersed spore Rhabdosporites)" or some such thing.
His answer was that he couldn't do that because R. is found in several other genera of Devonian plants. But that's exactly the reason why they shouldn't be called Rhabdosporites in the primary reference. Rina Scott, of whom I never before heard, was partly right--when she had the spores in a lepidodendrid, they should get the name of the megafossil, and should be called Triletes only when dispersed (except in parentheses or some such notation). Rina was

inadvertently applying the zoological rules. Once they find a connection, it is assumed to be invariable, and the name with priority takes over--unlike in botany where alternate names for different parts or stages of life cycle are tolerated (even encouraged incorrectly by the likes of Tom Taylor).

I need to take a nap. Later I'll tackle the other two letters I have from you.

Best regards. I note your petulancy about my travels. Are you jealous?
Next we go in three weeks to Indianapolis for a basketball tournament. Then to Italy for three weeks. One of my earliest memories is when I was 3. I left home on my tricycle and rode into town to check out the pickle factory where my father had recently taken me on some sort of business.
They called my dad when I got there, and I was in big trouble, but my dear grandmother was visiting at the time, and I hid in her skirts for protection when I got home. It's true that I have always loved travel and have done a lot of it.

Al

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: vdH etc.
Cc: rfensome@gsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Jan:

My eyes are tired from microscopy for the day, so I am trying to take care of some other things.

One of the items is the van der Hammen business. On more mature reflection, I am inclined to agree that the rejected names route is probably not the way to go--better to let the marketplace, as it were, take care of it.
However, I would like to call your attention to the fact that I didn't call for blanket rejected status for ALL vdH names--only for the ones that have the famous type problem. It would have been treated on the basis of a case-by-case study.

Yes, I think that if you know how to get in touch with vdH, it would be great to try to get this taken care of amicably. What needs to be done is erection of new genera with new types and transfer of all species. Because of people having put species into those genera, which they published in God knows what journals, the job is probably not going to be easy.

Now, again about the palynological names in ING. You were in a way involved in that mess, though in no way responsible! Perhaps you will recall that when the ING project had been going for several years and had spent almost all of the funds appropriated, the responsible persons one morning, literally about two months before the final deadline for the whole thing, woke up and said, "Hey, I hear that there are genera and species for fossil spores and pollen out there, formally published and all--what are we going to do about that?" I think it was Stafleu who first realized this was the case. I got a phone call, begging me to help. I spent every waking hour for a few weeks on the project, including a quick trip to Calgary to get help from you, using the then nascent cards. I am not happy about the results--I warned them that my effort was incomplete and informal. But they were very glad to get it to keep their own skirts clean. I have paid no more attention to that piece of work and don't feel very responsible. I was assured at the time that nothing that I did would be considered as effective publication of anything--I believe but am not certain that the required disclaimer was published in ING. Do you know?
In view of the fact that you think ING might be a source for Retitricolpites, this is an important point.

However, it seems to me that at this point the plot thickens, as far as this particular generic name is concerned, and that may illustrate how difficult the whole subject is--and perhaps introduce some fear that vdH might introduce still more mischief accidentally, as I doubt that he is on top of the literature.

Please see CFSP 34-56-57 for a comment I wrote long ago on this generic name. Too long to summarize here. But in brief, the name R. was first published by vdH as a subgeneric name in 1956. When vdH and Wym. in 1964 elevated it to generic status, they were running into the fact that Pierce 1961 had already done this--validly published. I would regard R. vdH and Wym. as a homonym of R. Pierce, in addition to its other wooly problems. Your cards don't seem to pick this up, or perhaps you don't agree. Doesn't really matter, except as an illustration that this whole thing of the vdH names is as complicated as the proverbial hooker's nightmare.

So, yeah, it would be great if you could get ol' vdH to cooperate. If I can be of assistance, say in co-writing a paper that tries to pull as much as possible together, to get all the species properly assigned and stop the needless carnage, of which Ms. Hoorn's effort seems to be an example, I'll try my best to help.

I will try to get a copy of the Hoorn paper by the technique you suggest. Can you provide a more complete address at which to try my luck? The e-mail address you sent for Hoorn looks defective, as I have never seen a compuserve address with a comma in the first part--well, I've only seen a few, but they all had two numbers with a dot between. I guess I would go snail mail to de Vries Lab, but I don't have an address for it.

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Mon, 3 Feb 97 11:33:49 EST
To: <traverse@ems.psu.edu>
Subject: re: spore genus

Dear Al,

No, there is no 'Reticulatisporis' card. I checked in Krutzsch, who compares with 'Reticulatisporites'; thus the typo that so far went undetected is my error. I'll note the correction in the next Supplement. I guess I was not keen enough in 1976. For that matter, the cartoons in those early cards start to grate on me, too: they were made freehand with a fountain pen, but really are not up to a comparison with what I have done in the later updates.

Thanks for pointing out this discrepancy! Best regards,
Jan.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: spore genus
Cc:
Bcc:
X-Attachments:

Dear Jan:

I was working with a student this morning, and I was showing the person card 4667, Foveasporis. In para 2 of the "diag." a generic name, Reticulatisporis, is mentioned. Is there a card for that generic name? I don't find it in my files (I have all cards in 3-hole binders, filed alphabetically). As far as I know, this would be the first case of missing card in the history of my collection. If there is a Reticulatisporis card, could you send me a xerox of it? Thanks. Al T.

From: jjansonius@gsc.nrcan.gc.ca
Date: Thu, 30 Jan 97 12:18:43 EST
To: <traverse@ems.psu.edu>
Subject: Amtaspora

Dear Al,

Thanks for the comment about Amtaspora. I am having difficulty locating a source for Current Science vol. 53:202-204 (Sarkar & Singh).

If you have a copy of it, I would appreciate if you could send me a xerox. A particular concern is the quality of the illustration (which never was very good in the first place, in that journal): would it be good enough to make a line drawing from?

Best regards, Jan.

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

Dean Jan:

Now, I'll have a go at yours of 15 Jan. 97, re Ross etc.

Before I even get out the Ross publication, I'd like to say that you have a slight tendency to agonize too much about decisions you have previously made to exclude this or that or to include this or that. Your first instinct on such matters was almost certainly better than a later product of (as my mother would have said) "feeling your neck."

It doesn't matter what "system" Ross said he was following, or even that he uses the expression sporomorph instead of species, as long as it is obvious that he is using binomial nomenclature in the conventional manner, which in part he was. Thus, Gleicheniidites is o. k. No question, in my fallible opinion. Ross's names with quotation marks are a little different question, because he only uses the marks on some of the names. On p. 30 he says that he takes some of his names more seriously than others--the ones to be taken more seriously are the ones labelled n. spm. I would say that when one of his names both lacks "n. spm." AND has quotation marks that is enough to conclude that he did not accept the name in his own publication, as he specifically says that these names are not of permanent value. On this reasoning I would reject "Triorites vestibulus" (quotation marks, no n. spm. notation). On the same reasoning I would reject "Trilites scrobiculatus". Is rejecting scrobiculatus going to cause mischief? I just don't think you can read Ross's p. 30, para. 4, l. 8 any other way than saying he didn't consider the names without "n. spm." as permanent. If not permanent, then certainly not to be considered nomenclaturally.

So, when I come to your material for the next Supplement, I have a problem with scrobiculatus. According to my own generic cards, that I have always done independently of the J & H things, I note that I say that scrob. is "not validly published in Ross." I have the validation as from Weyland & Krieger 1953, and the basionym of the transferred sp. as Microreticulatisporites scrob. W. & K.

There is also Foveotriletes Puri 1963. I think we have been in touch about Puri's work previously. And F. Pierce is already taken care of.

Now, in the same letter there is the question about what to do when additional matter must be added to complete the publication of a name. I have always assumed that, yes, when one does this and thus becomes author of the name (the ex in front of J & H can, of course, be omitted), one must complete the requirements as they are at the time of this final publication. That seems simple logic, as J & H become the actual authors of the name, in 1997 (or whenever). I wouldn't blame Werner for this one. It is just obvious.

I guess that winds that one up. I'll tackle another tomorrow!

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Wed, 15 Jan 97 14:20:16 EST
To: <traverse@ems.psu.edu>
Cc: "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: ...no subject...

Dear Al, 97 01 13

Please peruse the following. It seems I am starting to consider a change of heart on the validity of generic name Foveotriletes and the status of the names attached to its type. In particular, I'd like you to also read again what Ross 1949 wrote (p. 30, l. third new paragraph). It strikes me that he actually is serious about his names (i.e. he accepts them in this publication?), but not secure about the eventual botanical affinity that may be assigned to the form species that he recognized and 'named' in quotation marks. I now consider these question marks as having the same function (and effect) as the "question mark or other indication of taxonomic doubt" discussed in Art. 34.2 (1978 ICBN; Art. 34.1 in latest ed.). Now, it is true that Ross stated that for part of his taxonomy he is following Erdtman's system for fossil sporomorphae. However, he used in effect and to all practical appearances binomial nomenclature, and that is why in 1978 I considered Gleicheniidites as validly published in 1949 (although it was tagged as a 'n. spm.'). Was that the correct action on my part? I think so (and I followed Potonie), and it definitely would not be to the advantage of nomenclatural stability to overturn that name at this point. However, if I accept Trilites scrobiculatus and Gleicheniidites as validly published in Ross 1949, then I have little choice but to also accept as validly published (with a combined description) the name Triorites vestibulus, which would make that generic name a year older than the validation in Cookson 1950 to which I had held so far in the Genera File; and also it would have a quite different type. This definitely would affect some established nomenclature! Then again, something like: <"Tricolpites aspidatus"-types> - as introduced by Ross on p. 35 - makes one wonder about how firmly he "accepted" his nomenclature in the sense of the ICBN. What is your take on all this - I would very much appreciate your perspective, which is based on a long history of dealing with spore nomenclature.

If Triletes scrobiculatus was not validly published in 1949, Potonié becomes the 'ex' validating author of that name? Please also scrutinize the Remarks in the new text (below) on FOVEOTRILETES for the next Supplement.

FOVEOTRILETES van der Hammen ex Potonié 1956

Synopsis 1; Beihefte zum Geologischen Jahrbuch, H. 23, p. 43

Type: Foveotriletes scrobiculatus (Ross?) Potonié, ibid. (designation of lectotype)

Microreticulatisporites scrobiculatus Ross in Weyland & Krieger 1953, Palaeontographica, Abt. B, Bd. 95, p. 11 (basionym not provided, hence new combination not validly published)

Trilites scrobiculatus {in?} Ross 1949, Bulletin Geological Institute, University of Uppsala, vol. 34, p. 32, pl. 1, fig. 5

Diagnosis: "Type specimen (from illustration) ca 38 µm; trilete miospores, amb triangular with rounded corners; meridian in type distally broadly rounded, proximally with three flat facts. Trilete rays ± extending to the equator; exine densely and finely reticulate, the small lumina of ± equal size, and ± circular in shape."

Microreticulatisporites is larger, has a subcircular amb, and its less regular and finer reticulate sculpture is less sharply incised than it is in Foveotriletes.

Monotypic.

Remarks: Triletes subgen. Foveotriletes is a nomen nudum in van der Hammen 1955 (Boletín Geológico, vol. 2, no. 2, p. 14).

Foveotriletes is a nomen nudum in van der Hammen 1956 (Boletín Geológico, v. 4, no. 2-3, pl. 3).

Foveotriletes Pierce 1961 (Minnesota Geological Survey, Bulletin 42, p. 20, 26, pl. 1, fig. 6) was validly published, but is an illegitimate name because it is a junior homonym of Foveotriletes Potonié 1956.

Although Ross published the binomial "Trilites scrobiculatus" in quotation marks, the specific name is here considered as having been validly published, because we think it was accepted by the author in this paper; the quotation marks were said to indicate that the author regarded the name as 'only a working name', to be used until a closer connection with the 'natural system' was achieved. The names in quotes were said to be not in the Erdtman "sporomorpha system."

Waksmundzka 1992 (Prace Panstwowego Instytutu Geologicznego, vol. 136, p. 26) proposed the following emendation of the generic diagnosis (here somewhat edited):

Trilete miospores, amb subtriangular, sides convex, apices rounded; trilete rays thin, equal in length to full spore radius; spore surface ornamented with foveolae of 1-2 µm in diameter, evenly distributed (foveolate

sculpture); exine 1-2 µm thick; thin hyaline zona of variable width sometimes occurs in equatorial sector (see remarks below).

Waksmundzka (ibid.) stated that, according to analysis of recent material, foveae have been acknowledged as a more important element in differentiation of such spores than the presence of a thin hyaline equatorial zona, the width of which depends on the length of maceration to which the spore is subjected (Dettmann 1963, p. 66). Accordingly, she listed Sestrosporites and Vallizonosporites as junior synonyms of Foveotriletes Potonié, emend. Waksmundzka.

Description of type species: 20-36 x 31-48 um; corners somewhat bowed against the proximal pole; proximal wall sharply facetted, distal wall arched; exine ± 2.5 um thick; distinct sculpture of small rounded pits.

Cretaceous (Upper Santonian-Lower Campanian); Scania, Sweden.

Rob, I thought you might want to be kept posted on this matter; you may not have (all) the literature I referred to -- if you want, I can copy the necessary pages for you. Erdtman's "system" of sporomorphae in its typical application took the form of a trinomial. Some of the latter were later validated as binomials (e.g. by Cookson 1950).

Another question that arose: if someone in the past goofed in execution of all the requirements of the present ICBN for valid publication, can I set things straight by correcting past errors, or must I now jump through the hoops that since have been put up -- e.g.: if an Indian author in 1970 published a new name (monotypic, with combined description; J Palyno 5:68, pl. 1, fig. 1, 2) and designated two separate specimens as "holotype," can I now select one of these and say "ex J & H hoc loco," or do I have to say now where the type specimen is kept, etc., before the name can be considered validly published? I fear the latter would be maintained by Greuter; but, it sure makes things difficult.

Well, I'll keep sending puzzles like this one; let me know what your thoughts are on them.

Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Fri, 10 Jan 97 12:50:32 EST
To: <traverse@ems.psu.edu>
Subject: various

Hi Al, I'll import two files into this text, and I think both 'letters' to you are self-explanatory:

Dr. A. Traverse
Deike Bldg. 345, Penn State Univ
University Park

Dear Al,

I have started in earnest to get the next Supplement of the Genera File done. Thus, I revisited your 1994 paper in Palbo 82: 265-297.

There are a few things that I think you should have another look at, although it may not be worthwhile to do anything about them.

On p. 285, col. 2, l. 18, you call on Art. 24.3 to question the legality of the epithet "typicus" - however, this article deals only with epithets of taxa in the rank of SUBspecies or lower.

Then, a few lines lower, you use a strange logic when you say that "she failed to observe" that her material was identical to your Rhus carbogena, but that, nevertheless, "thus she should have used" the latter name for her type species (I notice with gratification that you, too, cannot do without that term). I think that you should have included Rhuspollenites ornatus as a junior synonym immediately below the cited basionym, to make more explicitly clear what you were doing here. And, over-page, the correct name of the species including the type of the species should be given as R. carbogenus (not: R. carbogena). Still, when I compare my drawing of the one type with yours of pl.2, fig. 22, it would seem to me that there may be small differences that might make an expert on the pollen of Rhus consider that they might not belong to the same natural species. There are differences in the relative size of the ornamentation, as well as in the degree of bulging at the equatorial pores (and, also, in geographic locale).

Under Parthenopollenites (p. 287), you are not very clear about a "transfer" to this genus; maybe taxon 104 of Thiele-Pfeiffer "belongs" in that genus, but a "transfer" in my understanding results in a new combination - which is not the case here. You might also have explained why the true T. marcodurensis of P & T should not be transferred to Parthenopollenites.

For Vitispollenites, the same comments apply as I gave for Rhuspollenites.

However, I don't think that (p. 288) she was OBLIGED to presage and follow your line of thought as explained in 1994; rather, I think that it is your subjective opinion (which may well be right) that differs from the opinion she chose to publish in 1980.

I am interested in your comments, as you look back on that paper. I don't think, though, that there is cause to make any comments in the Genera File on these matters?

Hope you enjoyed your trip,

Jan.

Dear Al, 97 01 03

I have a little problem with the genus Triletes; if you will check the Genera File, you will see that I considered the name to have been validated by Bartlett, who referred to the earlier diagnosis of Reinsch, and who was the first to use it in binomial nomenclature by assigning three new species to it. Since then, I have found a small 1916 paper by Rina (Mrs. D.H.) Scott [On the megaspore of Lepidostrobus foliaceus. New Phytologist, 5:116-119, pl. 8]. Good old Rina looked through more than 1500 slides and found over 40 specimens of a megaspore that she "had temporarily given the name Triletes diabolicus" (l.p., p. 116); it is a megaspore with a big gula on the trilete mark, which she said resembled the "swimming apparatus" of Azolla. Rina then goes on to look at in situ megaspores in the sporangia of Lepidostrobus foliaceus, including slides of cones from the collections of Prof. Williamson, and concludes that "there is little doubt that the dispersed spores and those in situ are the same," ... that those cones were not simply homosporous, but rather heterosporous and (p. 118) "... that Triletes diabolicus must disappear and become the megaspore of Lepidostrobus foliaceus."

All figures on her plate and in her text are identified as megaspores of L. foliaceus, none as T. diabolicus. The question I would like you to respond to is: was Rina Scott the first to use Triletes in a binomial combination, thereby validating the name as its de facto author; or, did she publish a preliminary name, that was rejected by its author in the same paper? Now that I have typed this out, it appears to me that I have the answer already: I have no choice but to find that Rina did not accept the name in her paper, and hence did not publish it in a valid manner. Fortunately, that conclusion may also least interfere with the established nomenclature of megaspores.

OK! It all fits in one message, and I don't think I have to "attach" these files separately. You should be home soon, I hope there is still room in your

e-mail box. Best regards,

Jan.

From: Jan=Jansonius%PALEO%GSC=CALGARY@bio.ns.ca
Date: Mon, 23 Dec 96 14:43:44 GMT (Original AST)
To: traverse@ems.psu.edu
Cc: fensome@agc.bio.ns.ca
Subject: re: vdH names, ugh!
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi Al, Rob,

I realize that Al may not see this for some weeks, but I'd like to jot down a quick response. I do not think that rejecting all vdH's names is the solution, because some of them were, accidentally, perfectly validly proposed and legit, while many others are indeed also validly proposed, but unfortunate in that they were made obligate junior synonyms of extant taxa. The point is that many species have been assigned to his genera, and I don't know how rejection of these generic names would affect the status of these validly proposed species. I am afraid that we might muddle things only more; I think a compromise must be reached in which we can assign all such spp to legitimate genera, while having those replace the illegitimate ones that vdH served up. Obviously, his "fully artificial" system of nomenclature, which he contrasted with the "approximate artificial" system of Potonie, is an out-dated concept -- we now have form taxa that are subject to the rules of the ICBN.

I'll try to make contact with vdHammen himself and find out what he is planning to do. Maybe a joint effort with him will take the controversy out of what we might arrive at. YES: peace on earth and all that. (Who would have thunk Al would turn into such a gypsy, a veritable road runner -- beepbeep.)

Happy New Year, all!

Jan.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: vdH names, ugh!
Cc: rfensome@gsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Jan & Rob:

We have been gone a couple of days, visiting a daughter and family (early Xmas visit) and now will be with the other daughter and her family for a couple of days. Then we take off in our diesel truck for a totally frivolous trip to Key West. Will stop at many places, such as Fairchild Tropical Garden, along the way. About a dozen of my Canadian cousins winter in the general Clearwater area of FL, and we have long promised to see what their winter quarters are like--will now do it, etc.

Jan's e-mail of 19 Dec. is too loaded to be taken care of in the time I have available. It goes into the pile to be cared for when we get back, 17 Jan.
In the meantime, I have another idea, however. Why not have one of us, or two of us, or all three of us, submit a proposal to Taxon to reject all of the dubious vdH names--it works just like conservation, as you both know. I wonder if that isn't what this case really calls for? As I said in my last message to JJ, the vdH names in part clearly fall under the longtime source of confusion provision of the Code, but that is dependent on opinion. Nomina rej. are not any longer a source of anything. They are OUT.

Happy holidays and a great New Year to you both. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Thu, 19 Dec 96 11:41:40 EST
To: <traverse@ems.psu.edu>, "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: vdH names

Dear Al (and Rob, who is getting 'copied').

I was relieved to hear that Al indeed sees things as I do, and as we have it in the Genera File. Let me calmly summarize what is happening here.

Carina Hoorn gave me a copy of her thesis, which looks like it is effectively published (presumably in time for her thesis defence which was 9 September 1994): 153 pp, 13 plates; she cites permission from Elsevier to (re)print material that they had copyright on because it had been printed (in part) and accepted (in other part) for publication in Palaeo-geography/climatology/ecology (vol. 109, nr. 1 -- May 1994; vol. 112, nr 3-4 -- December 1994). In Holland, a Ph.D. thesis must be published by the author, or the text must have been accepted for publication by a major (refereed) journal; she did both, which really is overkill, as well as poor useage of funding, and now creates some problems about priority dates for some of her new species. However, that is something that I am not now concerned with. The point is that, if you don't have a copy of this thesis, you can find it in two parts in Palaeo3. You also could write to the Hugo de Vries Laboratory, where her research was carried out; one of the requirements for passing a PhD exam is giving the institution some 200-300 copies for exchange. They still might have spare copies waiting for the asking. (In a buss. card folded into the thesis, she gives an e-mail address: 102341,3641@compuserve.com)

The paragraph "Systematics" in her thesis (p. 110) is also printed in Palaeo3 (109:37). I copied the relevant parts of it in my previous e-mail.

In Farr et al.'s ING, I can not find any introductory paragraph or comments on the vdH names, or how they were handled. I guess Hoorn's reference is indirect, and directed more at the INTERPRETATION of the rules as expressed in the following tomes, where a number of Sporae dispersae names are listed with their types.

One example is Retitricolpites, which in the ING is given as being validly published "ex v.d. Hammen & Wijmstra 1964", with the type "R. ovalis v.d.Hammen & Wijmstra". Van der Hammen & Wijmstra 1964 (Leidse Geol. Mededel., vol. 30, p. 234) actually wrote: "The genotype, R. ornatus v.d.H., is a species based on a recent pollen grain. Potonie (1956-1960) thinks that this is not advisable. In the case that the foundation on a recent species should be proved not to be correct, which we do not believe, we would hold Retitricolpites ovalis as lectogenotype." Then, they describe this new species. (Inasfar as vdH & W. didn't positively designate the species R.

ovalis as type species, the true "author" of Retitricolpites may be the ING collaborator who accepted that species as type.)

In the list of contributors to the ING I can find only Al's name as having dealt with the "Sporae dispersae." However, it wouldn't surprise me if, say, Gea Zijlstra had had a hand in some of them.

I take it from Al's reply to my earlier e-mail that he doesn't agree with what now is in the ING under this generic name, and a dozen or so similar ones from the same source. What bugs me is that such an authoritarian source can be so wrong; and, that other people follow it so slavishly (although in the Introduction the editors clearly warn that the list was compiled over many years, and to different standards by different contributors during that period, and that the authority of the ING is a derived one only; varying opinions among ING collaborators about the interpretation of some rules (of the ICBN) is another reason to verify the ING data in controversial cases).

What worries me is that Hoorn mentions a paper by v.d. Hammen "in prep." where all his old names will be rescued and validated. His names were validly published in the first place, I think, and therefore all species assigned to them are validly published. However, since these names are obligate junior synonyms of extant plant genera, the generic name is illegitimate, and the species must be reassigned to a legitimate and validly published generic name. By assigning a new type (which vdH & Wijmstra did not do unequivocally anyway), they created a validly published junior homonym, which thus again is illegitimate. Should we contact vdH and ask about this work "in prep."?

Should Al put a little piece in Taxon bringing this whole issue into the limelight? (I would be willing to cooperate, if you want, because I will now have to make a number of new cards for the Genera File, anyway.)

I thought it a nice touch that Al responded so positively to the name Hoorn: one of the gems of the Prov. of North Holland. It is incredible that the 25 or so km north of Amsterdam stretching to Hoorn and a bit beyond, has hardly changed in its appearance in the last 350 years. A few years ago, I cycled from A'dam to Hoorn, and yes, ate "pannekoeken" in De Waag (which is rather unsightly with all the beams and posts shoring up the unstable facade). If you are in the area again, do consider renting bicycles, and take the Ij ferry behind the NS central station: from there it takes 20 minutes and you are on the dykes and in the meadows and small villages that all figure just the same way on Rembrandt's etchings. Boy, I get homesick.

Looking forward to your response, once more. Het beste,

Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Tue, 17 Dec 96 18:38:35 EST
To: <traverse@ems.psu.edu>, "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: ...no subject...

Dear Al, Rob,

I finally started to clean up some of the papers that I have piled to one side in the last while. Thus, I came across a Ph.D. thesis by Carina Hoorn, which she gave me in Houston. She has worked in South America, under tutalage of v.d. Hammen and his school. In the thesis, she is using some of the Hammen genera that were originally typified by specimens of modern taxa. Potonie had rejected those as illegitimate, and I followed that. However, Carina quotes Farr et al.'s Index nominorum genericorum as authorizing the use of those very names because they were not only illegitimate, but also invalidly proposed, and therefore the names can be used again with a new type.

I was taken aback a bit by this. Obviously, in Amsterdam they don't think one needs to be too concerned about spelling etc., as Carina perpetuates the use of Psiladiporites, although that name was published by Varma & Rawat 1963 as Psilodiporites, because Psiladiporites was the form of the name as given by v.d.Hammen & Wijmstra 1964, Gonzalez Guzman 1967, and as used in the name of a pollen zone by Germeraad 1968, Lorente 1986, Muller et al. 1987; and therefore Carina Hoorn prefers that wrong orthography "in order to avoid confusions." (Palaeo3, vol. 109, p. 29 -- look it up!).

Hoorn (l.c., p. 37) then throws this one at me:

A number of the new species recognized in the studied samples is placed within the form genera Psilatricolpites, Retitricolpites, Retitricolporites, Bombacacidites, Syncolporites, Syncolporites and Psilastephanocolporites. The names of these taxa (but not Bombacacidites. JJ) were originally proposed according to the concept for palynological nomenclature developed by v.d. Hammen (1954a, 1956). Contrary to the provisions of the ICBN, this concept allowed for a typification of taxa of fossil pollen and spores by selecting recent types. According to Jansonius & Hills (1976) such a procedure would create illegitimate names that are later synonyms of recent taxa and ought to be discarded in the classification of fossil pollen and spores. However, following the interpretation of the Index nominum genericorum plantarum (Farr et al. 1979) such names are invalid. Since invalid names have no status under the ICBN, they can be subsequently validated. Pending a review (v.d.Hammen, in prep.) of the present status of all the names proposed by vdH ('54a, '56) it can be confidently claimed that

Psilatricolpites, Retitricolpites, Retitricolporites, Bombacacidites, Syncolporitesand and Psilastephanocolporites have already been correctly validated by designating fossil types.

Rob may not have all the literature in house. Therefore, I give a brief example: Psilatricolpites. The genera File cards will provide most of the needed raw data. However, I quote vdH & Wijmstra (l.c., p. 234): Psilatricolpites v.d.H. 1956a (Diagnosis: tricolpate pollen grains with a psilate sculpture. The genotype, Psilatricolpites incomptus vdH, is a species based on a recent pollen grain. Potonié (1956-1960) thinks that this is not advisable. In the case that the foundation on a recent species should proved not to be correct, which we do not believe, we would hold Psilatricolpites clarisimus as lectogenotype.) Follows an emended description of vdH's earlier "Tricolpites clarissimus" based on a new specimen. There was no full bibliographic info on teh basionym. And, this is not designating a type, but more a lame afterthought (...In case that,...which we do not believe). More definite than the alternative, they confirm here the type to be the grain from the recent species.

Now, when I started to build the Genera File, I wasn't in it for "knowing better"; I wrote to many people, incl Lanjouw, Stafleu, Potonie, Traverse, and others, and tried to be as objective as possible. It galls me that now apparently there is a new interpretation of these vdHammen publications that I am unaware of. If I am wrong, I'll be happy to change things around in the Genera File. Well, maybe not happy, but I'll do it. However, how can a name be not validly published if it is effectively published, has a diagnosis, and a designated type? If a name is validly published, it still can be incorrect or illegitimate, but a species assigned to such a name is validly published. If such names are NOT validly published, the species assigned to it also are not valid -- this opens a big kettle of fine fish that now need to be sorted.

However, I see no evidence of that being the case. Al, would you please explain to me HOW you arrived at the interpretation that got in the Farr index?

PS: Bombacacidites is not a vdH name, and should not have been included in this list. I tried to find a justification in the introduction to Farr's index for these vdH names being not validly published in '54a. '56, but cannot find any discussion in vol. 1. All I can find is Traverse's conclusions as written down in shorthand. Some of those are wrong anyway, e.g. Retitriporites, which was introduced by vdH as a subgenus of Triporites, but never

used by him in generic rank. When Ramanujam used the name in genus rank, he deserves full credit for it in that rank; if you want to include a ref. to vdH's earlier coining of the name, his name should be in parentheses, followed by "ex."

I wished this thing had come up a year earlier, because it would have been a fine example to do a biopsy on in Ch. 2 of our book. No matter, though, we still must deal with this, and duke it out and come to a shared understanding that all of us can accept as correct.

MERRY X-MAS!
Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Tue, 17 Dec 96 14:13:27 EST
To: <traverse@ems.psu.edu>
Subject: ...no subject...

Amice,

Thank you for the lion card, and particularly for the good wishes. They are returned with feelings of friendship and 'genegenheid' (one of those Dutch words that I can't quite translate - goodwill comes close, maybe).

Anyway, if we have had some friction in times past, particularly regarding your book -- which I still regard as a great achievement (but not without some flaws. I hope our collective tome will last as long) -- it always has been in a spirit of knowing and appreciating each other well enough that we can do some sparring without having to be afraid of damaging our long-standing friendship.

Looking at the lion, it struck me that the name of the sculptor is missing -- not important to the class of 1940? (I noticed the same thing in Calgary, where several popular landmarks ignore the name of the sculptor, but give in detail the name of the mayor and other dignitaries during whose rain the piece was accepted).

I haven't had much contact with Bob Clarke, lately. He is still so busy with the backlog of book orders, balancing the books for the IX IPC, and editing/type setting the Proceedings Volume, that I have tried to stay out of his hair. However, I suggest you send him an e-mail pointing out that you didn't get the book for which you have paid. Maybe it did get lost in transit. I think it is beter if you do that directly. I also hope that you get to see it sometime soon.

Merry X-mas, both of you! I am not sending cards, but my wishes are as sincere!

Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Tue, 29 Oct 96 17:17:17 EST
To: <"SMTP@601C@GSC OTTAWA[<traverse@ems.psu.edu>]"@gsc.nrcan.gc.ca>
Cc: <traverse@ems.psu.edu>
Subject: re: types

Dear Al and Sarah,

I must have been on drugs when I had the illusion that there were two separate specimens indicated as type of C. schlischii. I thought that I had seen that in the plate description. However, now I can clearly see that there are two different species both with their own illustrated type and one other specimen. I am sorry that I have caused you to waste valuable time on this; you may paste my picture to the scratching post of Ginger.

I started to write this message too quickly, and may have polluted your return address; so, there also is a copy to your address (all I can think of to ensure that at least one copy will reach you). Sorry, once again; please accept my apologies.

Best regards, Jan.

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

26 September, 1996

Dr. Jan Jansonius
Institute of Sedimentary and Petroleum Geology
3303-33rd St. NW
Calgary, Alta T2L 2A7
Canada

Dear Jan:

Have finished with the sheets re new edition of cards. I have a few comments that I add to what I wrote in red on the MS sheets.

1. Many of the sheets contain no info other than the name of the genus and sometimes a bit of biblio info in addition. I can't do anything with those and thus would like to see the final versions before publication.

2. Despite the fact that it has been past practice, you should stop the listing of "type species." I believe you should substitute the expression, "type of generic name." Just "type" would suffice, but I think the more complete expression would be better.

3. I find it useful to know whether a genus was monotypic when published. I guess the only way the cards could make me happy about this is ALWAYS to say, "Monotypic when published," or "Seventeen species referred to genus when published."

4. I think that the expression "description of type species" should be replaced by "description of species including the generic type" unless there is a special case requiring some other expression.

5. I am aware that I have not brought this up before (but should have), but when information on the cards is not the original information but a translation of same into English, bibliographic convention demands that an expression such as "in Russian," or "original in Russian, translation here by JJ," be provided. Even though this was not (ever?) provided, it is not too late to reform. Actually, on reflection, I believe the best procedure would be to provide the original, in whatever language, plus the translation. In some cases this would save the user of the cards from a lot of library work.

6. I convinced the publishers of <u>Paleopalynology</u> that American

English is now standard English. This is really obvious. Spellings such as "honour" and "centre" are now archaic and only used by a small percentage of users of English, mostly on islands. Eventually, even Chaloner and Scott, the editors of the book, at least reluctantly came to accept this.

7. Sometimes I wonder if really ODD terms should not be defined in brackets. E. g., "cochleate" on "p. 45," which reminds me to comment that you should have numbered the pages of the MS.
8. p. 48 (and other places): I wonder if it would not be better if you tried (I know it's hard) to be absolutely straight about the matter on these cards regarding Stuchlik and Co. Here, for example, you could say that the names are not legitimate for such and such reasons, period. Then, you and I could publish the paper we have discussed elsewhere with all appropriate bile included, letting Stuchlik et al. have it between the eyes. But the JJ cards should perhaps be regarded as like the Congressional Record and just record the facts?

9. This is just between us girls. What is Len's role nowadays that justifies his being listed as co-editor? Aren't the cards <u>really</u> the work of JJ?

Now I must get on with the rest of the Jansonius pile. I am determined to finish it before leaving for our place in the Adirondacks (Insley Camp at Roosevelt Rock) day after tomorrow. The autumnal coloration should be at its height. (Isn't English spelling weird? How does a foreigner ever learn high, height, heighth?)

Best.

Yours very truly,

Alfred Traverse

encl.:edited MS

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: -pollis, etc.
Cc:
Bcc:
X-Attachments:

Dear Jan:

Whoops, there's another JJ message in the pile.

Sorry, I don't agree. The first person who claimed that -pollis was masculine has no general standing for making that determination. It only means that subsequent people who use that generic name must accept the determination. It has nothing to do with other -pollis names. 62.3 seems to me quite clear about this matter. If we want -pollis and -sporis names to be accepted as masculine, we need a proposal to that effect. I find nothing in Art. 62 that supports your contention on this. The ruling would direct that -pollis and -sporis be added to 62.4.

St. Louis is a fun city. I could spend a month in the MBG.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: names etc.
Cc:
Bcc:
X-Attachments:

Dear Jan:

You are right, when I reviewed the manuscript with Sarah I failed to catch "undeclinable," though I did catch many other spelling and grammatical errors. That is not Sarah's strong point. Of course, you spelled whether as "wether" and Arabic as "arabic," and I could provide a further list of hundreds of such from my complete file of your letters and messages (and I am not counting all the dialectical forms, "spelt," "colour," etc.). You are also right about lumen, where it should say lumina. By now I am sure you realize that there is never an end to finding such things. You also made hundreds of them in the original versions of the cards. Or is it only dozens? I didn't actually count.

It has always been my position that Traverse can be converted to a specific epithet in two ways perfectly legally: traversii or traversei. This is because one can argue that my name ends in a vowel (indeed, it is pronounced when I go to German-speaking countries). Or one can argue that it ends in a consonant, with a silent e appended (which was my father's intention when as a young man he convinced his father and brothers and sisters to add an e to Travers because he had found out that our ancestor who originally landed at Pasbebiac, Quebec (=Lower Canada) in 1786, fresh from duty in the British army in New York, spelled his name Traverse. He did, it's true, but he also spelled it Travers and even Travis, apparently.).

I believe that schlischii, like traversii, is o. k. if the final e of the person's name is silent. It would not have been in the original language, but that is not necessarily important. If he calls himself "sleesh," or "slish" I believe schlischii is o. k.

Furthermore, as I understand the Code, the original author can treat the name of the person to be honored any way he/she pleases, provided only that having done that (e. g., dropping e from Traverse), the correct TERMINATIONS are added, per Rec. 60C.

I tried hard to talk Sarah out of ginker and tatoo, but to no avail. Finally, I said, you can get away with it, it's not strictly illegal, but you better add the statement that it's indeclinable. Ginker, by the way, is Sarah's (I am not making this up) cat. She refused to say that in the paper. Sarah is coming here next week because we are working on another Fundy paper, and I will discuss these matters with her, and I will also endeavor to be more careful with the next MS, presuming that she also writes it, which is probable.

In the Strother et al. paper I can, I think, claim that I never saw the final form of the thing. Furthermore, I didn't know (and still don't know) whom Paul was honoring with the vavdrovii. If it was a woman, you are of course right, and it can be corrected.

As to the Visscher, et al., paper I can certainly says that I never saw the MS at any stage. Indeed, when I got my first reprint request I thought it was an error because I knew of no such paper. I have no knowledge at all about the names you mention.

Finally, that brings me to the "book." No, I have never received anything for the huge pile of cash I sent in for it. Had I used your form of pseudo-dollar, it would have been a hundred of them. Ha.

Best. Al.

Jansonius

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: types

Dear Jan:

Sarah Fowell is with me for a few days, and we went over the correspondence between you and me re her and my paper. She wants me to query you further on something I hadn't reacted to, viz.: in your para. 4 (e-mail to me of 18 October 96) you say, regarding our Cycadopites schlischii, "Is this species validly published...since Pl. 3, figs. 2 and 3 are both indicated as...holotype." We have carefully gone over that part of the paper and find no justification for your statement. The caption to Pl. III clearly states that figs. 3 and 4 (not 2 and 3) are of C. schlischii, but the caption also clearly designates "...3. Holotype...." Under the description for the species, the information about the holotype clearly states "...Plate III, 3;...."

Could you please let us know where you found the incorrect information, as we could include a correction in a current paper, or elsewhere?

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Fri, 18 Oct 96 13:09:18 EDT
To: <traverse@ems.psu.edu>
Subject: Fowell & Traverse

Hi Al,

Just looked through the reprints received from you. May I make a few comments?

In Fowell & Traverse, under Cycadopites, the etymology of the name "ginker" is givdn as "the ... epithet ... is undeclinable" -- this is not etymology: I still don't know what the derivation of that name is (what is it, by the way). And why make "undeclinable" if we have "indeclinable" already?

C. tattoo is a barbarian name; why not "C. compunctus" (=pricked; notis compunctus is Latin for tattooed)? This method of coining names is no better than the arabic of Below, or the old-Spanish of Cramer. In the description, it should be "lumina" for "lumen", if you use a plural verb. The description itself does not specify whether it is the (internal) reticulum that looks as if it was 'pricked', or wether it is the outer surface of the exine that is perforated (as the statement in the "Etymology" paragraph seems to suggest).

If a species named for Traverse has the epithet "traversei" , why would you use "schlischii" for commemorating Schlische -- shouldn't that have been "schlischei"? And, if I read Art. 60.11 correctly, should this now have to be corrected to the latter form [see Rec. 60C.1(a)]? Is this species validly published anyway, since Pl. 3, figs. 2 and 3 are both indicated as being the holotype?

In the paper by Strother, Al-Hajri & Traverse, it should surely have been Ambitisporites vavrdovae (not vavrdovii).

In the paper by Visscher et al., I wonder why Reduviasporonites was considered as a senior synonym of Tympanicysta and Chordecystia; the cells in the latter forms are an order of magnitude larger, leaving aside other morphological characteristics.

Of course, I couldn't find anything wrong with your Chapter 2 in the "book" (but then, I didn't look either); did you get your copy of the whole set of three volumes yet? I'd like to hear how you like it. Colin and I are half inclined to consider a fourth volume, if there are enough suggestions to cover overlooked or neglected areas.

That's all for now,
Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Fri, 18 Oct 96 11:27:05 EDT
To: <traverse@ems.psu.edu>
Subject: re: Greuter

Hi again Al,

Re Huns -- they don't necessarily have to come directly from a German background, but they fit comfortably in that mold.

I don't agree with your interpretation of the function of suffixes in names. I think that Art. 62.2 and 62.2(a) sufficiently answer our questions: compounds ending in a masculine word (and the first constructor of the word _pollis determined it to be masculine) are themselves masculine. A new name, also ending in -pollis, could not be made feminine because it is not fully "arbitrarily formed" in the sense of 63.3: the last part is now an established word or suffix.

You are right, planning too far ahead makes no sense. But, it is something to look ahead for; I'll make up my mind later about attending the big one in St. Louis (do they still play the blues, there?). See you there then, maybe (or will you ever attend another AASP meeting)?

I will run a final draft of the Genera File update by you, and shall be happy to acknowledge your input. Some of the incomplete ones were sent because there were some really hairy nomenclatural issues, that I felt ill at ease with, wondering if my exegesis of the Code was on the mark. And, if you tell me what was wrong with them after the thing is printed, a lot of people miss out on the insight you can provide.

Best wishes,
Jan.

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

Dear Jan:

For the record, Greuter is a Swiss.

re -pollis, etc., it is my understanding of the Code that each generic name is a new ballgame. That is, one author could make up such a name and treat it as feminine, and he/she should be followed later. Another might make up another, different, generic name with the same termination and treat it as masculine, and he/she must be followed, etc.

Yes, I plan to be in St. Louis--I even have a sister who lives there. But it is folly for people in their 8th decades to be predicting anything so far in the future!

Re the generic file MS sheets. Many were incomplete, and if I am going to be helpful, especially if such help is acknowledged, I should see them in final form. I am good at catching misspellings, including those that you have intentionally introduced, such as "colour" and "spelt."

All the best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Thu, 17 Oct 96 13:47:29 EDT
To: <traverse@ems.psu.edu>
Subject: re: nomencl.

Hi Al,

I didn't realize that the _sporis/-pollis gender was still an issue, after I tried to explain (in Elsevier's Review, and AASP Newsletter) the point of view that I was brought to accept: that the precedent of the first author made them masculine (although the principle of common gender might make one think that a feminine gender would be allowed at least for the megaspores -- which brings us back to the problem of how to recognize one on purely morphological grounds). The ending in -is of this suffix is no reason for retroactively making them all feminine (although I tried to do that for some years in the Genera File: mea culpa!).

I don't remember anyone having tried to interprete _spora as masculine; and -polla I have never encountered. This is not an issue, and shouldn't be made one.

I was happy to learn your impression of Greuter (who I only know through letters); he seems to be a true hun, but a very clever one. Don't quote me on that, I still haven't met him. Will you be in St Louis at the next IBC? Should there be a vigorous and large Nomenclature session, there?

Before I forget again: thanks for the pile of Genera MS that you read through. I shall incorporate your remarks in the files.

All for now: greetings,

Jan.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: nomencl.

Dear Jan:

Yours of 16-X much enjoyed. The thing I most agree on is the outrageous application of liberty by the Editorial Committee. True, this is the worst it has ever been under Greuter, who is a very arrogant man. But it is not a new invention. Years ago I even expressed myself in print about actions of the group when a carefully crafted proposal of mine on organ genera, etc., was adopted by the Nomenclature Session, and then gutted by Jim Schopf in the Editorial Committee so that what was intended by the proposer and adopted by the N. S. was Not in the Code.

I remain convinced that what should be done is a proposal in Taxon by one of us stating that -pollis and -sporis names are to be treated as masculine. I am pretty sure that the N. S. will then vote to "refer to the Editorial Committee," but if the Paleobotanical Committee has overwhelmingly voted for the proposal, it is pretty sure to be included.

In the meantime folks are bound to follow the author of whatever genus it is. In any event, I can't imagine people treating -spora names as anything but feminine. Do you think it should be included in the -pollis, -sporis proposal? Are there any -polla names?

Thanks for the home e-mail address. We also have one: alnbet@aol.com

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Wed, 16 Oct 96 14:27:51 EDT
To: <traverse@ems.psu.edu>, <nfrederi@usgs.gov>
Subject: gender issues

Hi Al,

I received today your snail-mail message to Norm.

I reread my 1988 bit on -sporis and -pollis (it was also printed in Elsevier's Review), and I had some difficulty with my phrasing and punctuation. I think I could express myself more clearly now. [I also find that my understanding of how to handle names is still improving; even some confusing puzzles are getting solved.]

I also compared ICBN 1988 ,Art. 76, with the 1995 Art. 62 -- I am appaled by the poor job Greuter(?) has done in rewriting parts of that, esp. the examples. He changed meanings, and makes nonsense of what was clearly stated in 1988. This may be the basis for a published gripe about the leeway the Editorial Committee have given themselves.

The way I understood why tree names are feminine is that in Roman religious practices each tree represented a wood nymph; this gender then was expressed easily in their grammar. [It makes it a lot more attractive to become a tree hugger; to us, a tree is little more than a pile of pulp.]

In Latin, words ending in -is are (as far as I know) all of the third declension, and all of feminine gender (as are virtually all words in -as, aus, -x, -o, es and consonant+s -- the latter including, I suppose, "glans penis"); however, there are some exceptions (all masculine) among those ending in -is: caulis, collis, fascis, lapis, mensis, orbis, piscis, pulvis and all words ending in -nis and -guis. Further, all classical names of trees (and cities, islands and countries) are feminine, as are the words domus, manus, porticus and tribus (the endings of which would suggest a masculine gender). The names of words referring to a male are masculine, as are essentially all names of rivers. Reasonably so, these genders are maintained when Latin words are used as names. [Note that the ICBN, 62.1.Ex1, suggests that there are some tree names that are not feminine. I don't know which names they have in mind. Also note that so far I referred to words ending in -is, not words ending in the suffix -is; the latter are all adjectives.]

I think it would be germane to include in an ICBN Example the suffixes -sporis and -pollis as having masculine gender. However, if we follow the rule that the first author must be followed (which was Potonie), these suffixes are clearly masculine, and later authors ahould follow that precedent.

That's all for now folks! O, yes, Al: I now have also an e-mail address at home: jansonij@cadvision.com. It might come in handy, although I have all my reference literature in the office. Best regards,

Jan.

THE PENNSYLVANIA STATE UNIVERSITY
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PALYNOLOGICAL LABORATORIES
435 DEIKE BUILDING
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E-Mail: traverse@ems.psu.edu

8 October, 1996

Dr. Jan Jansonius
Institute of Sedimentary and Petroleum Geology
3303-33rd St. NW
Calgary, Alberta T2L 2A7
Canada

Dear Jan:

Okay, so I didn't make it before we left for the mountain place last week. So, here's the latest batch of papers from you, which I guess you wanted me to edit.

This will require only a few comments, as some are the same as for the previous stack.

On a number of these nascent cards I regard the stratigraphic info as inadequate. Why not help us poor boobs out by always providing the period name? p. 2 is an egregious example. Also p. 8, which I think probably also contains some errors in the strat. citation.

You should give consideration to modernizing your usage on μ. Best usage now is to write μm, and I think it is more important to have the cards reflect current usage when they appear, than to have the cards uniform.

Weird insular dialect forms such as "spelt" have got to go.

Hang in there. Betty and I took a 7 mile hike over very difficult terrain a week ago today (in the Central Adirondacks). During one interval I regaled her with the story of my misinterpreting a sentence or two from you a while back about your production of the drawings for the cards. I thought you meant that you now have infirmities of the hands that make it torture for you, so I recommended that you find help. Speaking of awareness of one's age reminds me that when I last took that hike we were with Betty's parents in 1952. They were then about the age that our older children are now!

Yours very truly,

Alfred Traverse

P. S. Enclosed, your MS sheets and a few reprints. I never get around to general mailouts.

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

Dear Jan:

Just in from recent trip to St. Louis (Missouri Botanical Garden's arboretum in the country SW of S. L. was explored and found quite interesting). Your message about your fine fettle (except, apparently for the drawing hand) is noted with great pleasure, especially as my brother-in-law, who is a year younger than I, now has Parkinson's pretty bad and in general seems 80, and my sister, who is six years my junior now acts like a very old lady (she declined to accompany me to the arboretum--too much walking). Furthermore, two of our friends died during my six day absence. I missed one funeral yesterday and will attend the other in an hour.

Hang in there. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Wed, 18 Sep 96 13:20:18 EDT
To: <traverse@ems.psu.edu>
Cc: "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: Genera File

Hi Al,

Just to make clear: I don't mind making the drawings. In fact, I take pride in doing them, because I try to express features that I noticed and think are of taxonomic significance, even if ignored in the original description, thereby providing more info than is available in the protologue (though, of course, I base myself on the protologue). Conversely, I may show visibly in the drawing some features that are verbally described by the author, but cannot be discerned in his illustration. I also find pleasure in doing the drawings, but yes, I am behind in them for the next update. And as long as the weather holds, I will be out in the garden (which was neglected in the last few years) and on Calgary's pathway system with Bettie, to maintain our health and physical shape (and that preventative maintainance work takes increasingly more time). But no, I'm not complaining; I hate to think that I left a depressed picture in your mind.

Jan.

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

Dear Jan:

Glad to hear that there are no presses to hold on the "cards." Well, sort of, but it is depressing to hear that the holdup is your artistic work. Under the circumstances you should not be wasting your time on it. Ron Litwin, my doctoral student now at USGS, is a skilled paleo artist (that's what he did for a living at the Carnegie until he came here for his degree). For sufficient credit and free cards maybe he would be willing to do it. A better solution would be to find a UC student who is a good artist--perhaps even a biological illustrator student--who would do it for peanuts--charge a little more for the product, but don't waste your time on something that I can read between the lines depresses you.

I put Rob on for cc because I am in the office for 10 mins. before catching the plane to St. Louis, and I had some wakeful moments during the night wondering if homonyms might be an exception to my dictum that names not validly published have no real standing (and that, thus, one is under no obligation to give "ex" for a generic name--the character who failed to pub. the thing validly gets no obligatory credit for publishing the name--the person who finally supplies the necessary items for valid publication is the author, period.) Anyway, I thought I recalled that any name effectively published, even if not validly published, had to be considered for homonymy, but apparently not--see Art. 51.1.

So, off to the airport! You both be good. Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Tue, 17 Sep 96 18:28:40 EDT
To: <traverse@ems.psu.edu>
Subject: re: generic "cards"

Dear Al,

I have read your various e-mails that recently came in and have printed them, so I can digest them better, and compare the one with the other. Will respond in due time.

No, I didn't know of any problems -- hope nothing serious.

And again, no -- unfortunately I have not made the progress I wished for with the new File Supplement. I have only some half dozen figures, and those take me an hour or so. So, there is no presses to hold, yet. We will see you later, than!

Best regards to both of you,

Jan.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: generic "cards"
Cc:
Bcc:
X-Attachments:

Dear Jan:

I am about to leave for St. Louis for about a week because of a family problem I may have mentioned earlier. I have begun working on the cards and am finding quite a few boo-boos, as well as a number of things you may not want to accept but would want to consider. Please hold the presses until I get back and get this job finished.

Best. Al.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: book reviews
Cc: mcgregor@gsc.emr.ca
Bcc:
X-Attachments:

Dear Jan:

Six weeks is so long in cybertime, that I suppose I might as well forget this, but I don't like unfinished business, so with due apologies, here goes to answer yours of 31 July re book reviews for the famous book I ordered and paid for on 23 July but have yet to see.

Yes, I kept a file as complete as I could make it of journals where my Paleopalynology was reviewed. Incidentally, I am STILL hot under the collar by recalling that certain palynologists who have NEVER to my knowlege taught a college course in palynology had the nerve to review the book negatively without apparently noting that the author clearly labelled it as a teaching book and that they were not fully qualified to comment..

One of the abovementioned bastards published two negative reviews in different journals (so as to get two copies of a book he said he detested!). Said bastard had his wife contact me a few months ago because he is now teaching a palynology course, and did I know how to get hold of a few copies of the book as he desperately needs them? My reaction was: Huh...!? I guess I shouldn't feel too bad: there were only three negative reviews, two by the aforementioned anal orifice.

Here's the list. It probably isn't complete, because when Unwin-Hyman went out of business shortly after publication, they stopped monitoring the reviews for me.

The Australian Geologist (Playford)
Excerpta Botanica, Sect. A, vol. 52. no. 1 (V. Wilde)
Micropaleontology (Wingate)
E. Schweizerbart'sche Verlagsbuchhandlung (Storch)
Cretaceous Research (Batten)
Palaeontology Newsletter (Batten)
Geoscience Canada (Martin Head)
Sedimentology (Carole Gee)
Grana (Faegri)
Quaternary Research (Anon)
Palaios (J. V. Ward)
Rev. Palaeobot. Palynology (Jansonius)
AASP Newsletter (Nichols)
Palynos (Felix)
Nature (John Birks)
Can. Assoc. Palynol. Newsletter (Beaudoin)
Geological Magazine (N. F. Hughes)

IOP Newsletter (Faegri)
Geology (S. Jacobson)
Journal of Sedimentary Petrology (S. Jacobson)
International Aerobiology Newsletter (S. A. Hall)
CIMP Newsletter (Anon, as far as I can tell)

A review in Science would have pretty much completed the picture, I think.
As an interesting (?) sideline note I would like to mention that in the Harvard Magazine, put out by my alma mater, there is a page for books by alumni. My book was submitted for inclusion, but Harvard declined even to list the TITLE, as they "don't acknowlege publication of textbooks."

Carry on. Al.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: type for generic name
Cc: rfensome@gsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Jan:

Punt is "right," that is in accord with latest usage, when he "corrects" the expression "type species" to "type." The type of a generic name now is a specimen. Normally it is the holotype specimen for a specific name. The specific name is normally the name for the species that would incorporate the specimen designated as type for the generic name. In the case of the already existing genera and species, this means that the holotype of the species originally designated as "type species" is almost always going to be the type for the generic name. There are instances, however, in which the holotype for the species that includes the type for the generic name will be a different specimen from that designated for the generic name.

It would not make publication of a new generic name illegitimate if the author used the old designations, providing everything else is in order, but if one talks about type species nowadays the up-to-date systematist looks down his/her nose at you. I slipped on this one day in Greuter's office in Berlin and he referred to me as "antediluvian."

In my opinion, you should abandon micron for micrometer (or better, just use the symbol therefore: mu followed by lower case m.) You should also modernize by not saying type species any more. You could make this easily understandable by saying: type of generic name: such and such a specimen. In practically all cases you could simply say: type of generic name: holotype specimen for Terriblespora jonesii.

Your paragraph four seems redundant. It is covered by what I just said above--stop saying type species. "Nomenclatural type" is not necessary. Just type of generic name. Holotype for the specific name. Etc.

Type species was an idea I was comfortable with, but I must acknowledge that it was a very slippery idea, whereas a specimen is comprehensible. However, as you know, my hobby horse is that in palynology we should accept illustrations as types.

You must stop saying Leningrad too.

Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Fri, 13 Sep 96 13:22:11 EDT
To: <traverse@ems.psu.edu>, "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: type or type species?

Hi again,

What about the following?

Ram Kalgutkar has a longish MS article on Early Tertiary fungal spores, with a lot of new taxonomy. He has under the name of a new genus a line "Type species: ...", and then under the name of that new species he gives the holotype of that species. Wim Punt (Elsevier) crossed off the word "species", so that now underneath the name of the new genus we have "Type:...(name of the new species)", and then the holotype identified under that species.

Now, in Art. 10.1, it says: 'for citation of a TYPE, the SPECIES name alone suffices, i.e., it is the full equivalent of its type'. Does this mean "type species"? My question is: is it wrong to continue the use of "type species" in taxonomic works? I have continued its use in the Genera File, if only for consistency of format (just as I continue to use "micron" instead of "micrometer"). If one now gives the "type" of a new genus, shouldn't one then give the number of the type slide, and the plate and figure number, maybe add the specific epithet assigned to it?

If the ICBN now speaks of "type" rather than "holotype", what does this short-hand term stand for, "type species"? Can the term "type species" be interpreted as the equivalent into modern English of the term "typus" (see Art. 37.4)? Or, could we (see Art. 7.2) use the heading "Nomenclatural type" instead of "Type species", which works out to the same thing but is more bureaucratese than the familiar older term.

Lord, they sure messed with things, and messed them up. What do you think: must we wage war upon "type species"? Or, shall we give it a refuge?

Jan.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: message of 20.VIII.96
Cc: rfensome@gsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Jan:

This one had to do with valid publication, and seems to be part of a vast fossil fungal project, in which you are assisting some person from the infamous subcontinent.

Some of what you wrote in the first ten lines or so is just a repeat of what the Code says and does not require more comment. In line 13 you say "...if figures of more than one specimen...one of those...must be chosen as the type." You go on to say, "Still, the latter statement ...is not a requirement for valid publication...."

Let's talk about that first. So what? Typification IS required per 37.1, and without such a name CAN be validly published but is nevertheless ILLEGITIMATE. Trash can be validly published, but legitimacy is what counts. For my taste it would be better if the Code abandoned the separate concept of valid publication and made legitimacy/illegitimacy the touchstone. I've noticed in the past statements by well known palynological systematists such as "So and so is invalid." They clearly meant illegitimate.

Referring to the second paragraph above again: The statement that if there is more than one specimen, one must be selected as type means clearly that if there is only one, it IS the type. "when or by whom that choice must be made": It means that the selection must be made at the time of publication by the author(s) or the name is illegitimate. At that point another person can supply the missing information, and he/she becomes the author of the name and the date is the date of the final provision of necessary information (Art. 45).

In the 1989 paper cited, the new species with a single figure are in my opinion acceptable, but the one, Diporisporites planus, for which there are two figures, it doesn't help that a slide and coordinates are given--there could be five specimens in that field.

I believe you are over-reacting about requirements for collector's name, etc. in Art. 37. That clearly refers to field collections of plants. It would correspond to citing slide numbers, etc., for us. It means that such info must be cited if the sort of specimen implies existence of such. For example, historically many collectors of plants did not always (or even never) give specimens

numbers. Thus there might be ten seemingly identical specimens in various herbaria from totally different populations collected about the same time by the same botanist. The Code is saying that designating a specimen of dried plants as a type requires some sort of specificity. The words "any other detail" gets situations such as our where this doesn't apply off the hook.

That seems to do it for this item.

Have a nice day. Al.

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

Dear Jan:

O. K., here goes with a start at answering the JJ queries--what a pile! Wish me luck.

We start with the most recent item, re Mariusa, because it is easy.

I. You are right. Art. 7.2 now needs cleaning up of the language. (I have been comparing the 1958 Code with the current one.) "Element" goes back to a day when a type was in some instances (e. g., generic name) NOT a specimen. Art. 9 used to say: "The type...is a single specimen or other element." As there is a clear need in 7.2 to clean this up, why doesn't JJ propose same in Taxon immediately?

9.2 doesn't mean that a type could consist of more than one specimen. It clearly means that the holotype designated could turn out to be non-definitive. This is an absurd example, but suppose that a pollen grain of a grass were chosen by some nut as holotype for a new variety, but it turns out that it could belong to any of 50 species.

I don't think it is necessary, indeed it would be redundant, to state in 9.4 at what date it became insufficient to provide only syntypes. It never was more sufficient than designating no type was--this was sufficient until typification was made obligatory.

In sum, Mariusa [do you know how to underline in e-mail?) is obviously not validly published.

On to the next.

Best. Al.

P. S. for Rob: the above is apparently a new e-mail address for you. Should I scratch out the different one I now have and use this?

From: jjansonius@gsc.nrcan.gc.ca
Date: Tue, 10 Sep 96 17:13:18 EDT
To: <traverse@ems.psu.edu>
Cc: <kalgutkar@gsc.nrcan.gc.ca>, "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: ...no subject...

Dear Al, I came across the name Marusia in a compilation of fungal taxa in preparation by Ram Kalgutkar. I translated the French as follows (you may have some corrections), However, the fact that the authors designated only "syntypes", without even identifying these with the published illustrations, makes me think that this name was not validly published. Yet, the ICBN is ambiguous: Art 7.2 speaks of "that element" as a nomenclatural type; 9.1 states that a holotype is "the one specimen". Yet, 9.2 talks about a holotype "consisting of more than one taxon", which implies that the holotype may consist of more than one specimen; and 9.4 defines syntype as "any one or more specimens cited when no holotype was designated, or any one or more specimens simultaniously designated as types." It would be clearer if at least in the last case the date after which this practice is no longer accepted were specified.

Please give me your thoughts on this. The description follows here:

MARIUSIA Pons & Boureau 1977

Revue de Mycologie, vol. 41, p. 350

Type species: Mariusia andegavensis Pons & Boureau, ibid., pl. I, Pl. II, fig. 1-7 (syntypes no. 8119 B/5-26)

Diagnosis: "Stromata dimidiate, flat, rounded, ostiolate or not, formed by radially disposed sinuous hyphae. Mycelium superficially hyphopodiated. Intercalated stigmopodia. Phragmospores."

Monotypic.

Description of type species (combined from original 'diagnosis' and 'description'):

Stromata brown, flattened, solitary, round, 45-100 um in diameter (70-90 um average), barely inflated, consisting of thick-walled sinuous hyphae radiating from a central point; ostiole at maturity (once stroma reaches 50 um) 10-40 um. Margin fimbriate. Cells of stroma 1-3.5 um in diameter and 2.5-13 um long, lateral walls thick (0.5-0.75 um) and sinuous. Some superficial free hyphae forming a light brown felty network, generally branching at right anglesto the stroma outline, but some (up to half) at angles of 10-20 {O}. Cells of mycelium about 1-5 um in diameter, 5-20 um long,

sinuous.
Intercalary hyphopodia (irregularly inflated stigmocysts) numerous, at regular intervals, often separated by a number of normal mycelial cells, greatest width 6-8 um, greatest length 5-10 um, sending out, through the cuticle, palmate haustorial suction devices into the subjacent epidermal cells. No mycelial setae. Although neither young nor mature stromata contained spores, four-celled phragmosporiate (3 septa) ascospores are present in the felty mycelium, that are betwen 15 and 20 um long. The central cells are widest (6-8 um) and 3-5 um long. Cells with fine smooth walls, 0.25-0.5 um thick, light brown, with one rounded terminal cell, and one that is elongate and connected, with a mycelial filament, to the stigmopodia. Slightly constricted a the septa.

Middle Cenomanian; France.

Remarks: Affinity is with the Microthyriaceae. The illustrations show five peltate fruiting bodies of various sizes (various stages of maturity?), as well as details of the haustorial suction cells and mycelium cells. However, even if all these would be found on the same leaf fragment (which is not indicated), it is not clear to us that all would belong to the same species. Since not one single specimen from among those illustrated was designated as holotype, we interpret the species, and hence the genus, as not validly published. The illustrations on the two plates are not keyed to the slides that are cited as containing the syntypes.

Best regards,
Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Tue, 20 Aug 96 11:44:07 EDT
To: <traverse@ems.psu.edu>
Cc: "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: Valid publication

Hi Al,

I am assisting a GSC palynologist (Ramakant Kalgutkar) with his compilation of species and genera of fossil fungal spore/fructification names. A couple of Mexicans proposed a number of new species, and mostly illustrated them with one figure (automatically the type: Art. 37.3), and in the description they provided a slide number with coordinates. However, Art. 37.1 requires that per 1958 "the type of a name must be indicated." Art. 38 requires that per 1912 a new species "must be accompanied by an illustration or figure showing the essential characters," in other words, it is not necessary that the illustration be that of the type. Art. 8.4 states that the type "is the specimen whose figure accompanies the valid publication of the name," but this is not a requirement; and, "if figures of more than one specimen were given one of those specimens must be chosen as the type." Still, the latter statement again is not a requirement for valid publication, and the code does not specify when or by whom that choice must be made.

The case in point is the paper by Enrique Martinez-Hernandez & Ana Cecilia Tomasini-Ortiz, 1989, Esporas, hifas y otros restos de honges fosiles de la Cuenca ...; Univ. National Autonomico de Mexico, Inst. Geologia, Revista, vol. 8, num. 2, p. 235-242. The authors gave a slide number for each of their new species illustrated by a single figure; for Diporisporites planus (p. 238), they have two figures, none of which is identified as the type, but they do give a single slide number with coordinates -- by implication this represents the type, and hence is an "indication"? My quandary: can we stretch the code far enough to accept this as valid publication, or will this have to be an "ex" Kalgutkar species?

So what? Typification is required for 37.1, so it a name can be validly published, but nevertheless illegitimate. For my taste, it would be better to eliminate i.e., if only one is given, not nec. at time of publication, of course — if not made, a date-person can complete to dead end & author.

legitimate

no — illegitimate

yes

I find the code very vague, imprecise and sloppy in these matters, while it makes pedantic "exact" requirements for valid publication that everybody in paleopalynology at least is ignoring (Art. 37.3: "Citation of the collector's name and/or collecting number and/or date of collection and/or reference to any other detail of the type specimen or illustration is REQUIRED."). If this were applied to the letter, I don't think we have any validly published names in palynology -- mind that there is no date since when these requirements are required! And, if we let this thing slide by as not really a touchstone for judging whether a name is validly published or not, why should we pay attention to any other equally silly requirement (e.g. Art. 37.4: the word "typus" or "type" must be attached to the holotype -- a single specimen is no longer sufficiently clear "indication"?)?

if this is sad

yes — this is the case you cite that is not absolutely certainly the case — as is to w. accountants could show especially c. that fact

Glad you let me blow off steam. I'll send a copy of this to Rob Fensome, for his files (and maybe, Rob, you may have some comment?). I also would like to send a copy of this to Greuter -- does anyone of you have an e-mail address for him?

Valete!
Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Wed, 31 Jul 96 16:47:36 EDT
To: <traverse@ems.psu.edu>
Subject: book reviews

Dear Al,

We are currently trying to determine where and how many book reviews we ought to "engineer" for our three-volume book. Bob doesn't want to give away too many copies, but on the other hand one would like to see some reactions amd evaluations (and hope they turn out favorable!). We were thinking of various newsletters (AASP, BMS, Palaeontological Ass., Palynos), Review Pal+Pal, J. Paleo, Micropal., maybe some French and/or Spanish magazines; maybe some general geological magazines? When I talked this over with Colin this morning, he suggested (he is at home recovering from a prostate operation) that I contact you and ask if you still remembered where reviews of your book had been published. Do you?

Can one ask a reviewer to submit his review to more than one journal/magazine? (or: would the first publication of it be "protected" by copyright?)

If you have any suggestions on this topic, we would be happy to hear.

Best regards, Jan.

From: jjansonius@gsc.nrcan.gc.ca
Date: Fri, 26 Jul 96 11:33:04 EDT
To: <fensome@agc.bio.ns.ca>
Cc: <traverse@ems.psu.edu>
Subject: re: illegitimate name?

Dear Al and Rob,

I like the reasoning of Rob, and commend him on his clear sight that picked up the loophole in 52.2 which allows us to drain off the bathwater but keep the baby. The foremost consideration of the Code is maintaining the stability of nomenclature -- that, we are doing here.

In reflecting on this, it seems to me that Linne had a good idea, and put in place a nomenclature so practical and consistent that his method was adopted readily by everybody. Even by those who didn't really understand what was going on. So, errors were made, and somehow we got a book with rules to sort out the mistakes and keep everybody straight. Now, it appears that it is fashionable to think of the Code and the method as all important and having priority, and the nomenclature will have to fit it. Putting the cart before the horse. I agree with Rob that it is prudent to err on the side of conservatism, and maintain as much as possible. For that matter, in the Genera File there are many homonyms flagged but not replaced, because I thought that would be best done by people who at the same time could clean up any taxonomic problems that might be involved. (Was that the best thing to do, or should those homonyms all be replaced in a rote manner?)

Thanks, Rob, for pointing to the way out of this mess. It seems like common sense, but Codes (of law etc.) never seem to be able to use just common sense -- it is not OBJECTIVE. This is one case where I appreciate the sloppiness that can be found in the Code, or was it superior foresight of the editors?

Best wishes,
Jan.

From: Jan=Jansonius%PALEO%GSC=CALGARY@bio.ns.ca
Date: Mon, 22 Jul 96 15:18:12 -0300
To: traverse@ems.psu.edu
Subject: re: illegitimate name?
X-Incognito-SN: 101
X-Incognito-Format: VERSION=1.75 ENCRYPTED=NO

Hi AL,

Thanks for your speedy answer.

I agree with you on 1) that Ediger created an invalid name; and 2), that it would have been acceptible if he had not used the word 'synonym', but the words 'different aspect' or something like it.

Too bad, I had a vague doubt and hoped that you would have seen a loophole to pull it through into "valid" territory.

I didn't know about Carol Bryant's bout with cancer -- but someone said (forgot who) that it wouldn't have helped things that she had been, and was, a heavy smoker.

The $75 for the book is a disappointment, but then, when authors in the book began to coopt more and more contributors for their chapters (some for minor, others for considerable contributions), we lost control and couldn't honor an early promise of a free copy. However, the price is right, and the consumer price is so low that you will understand why a commercial outfit can give some free copies and still turn a profit. We, however, will have to nearly sell out before we break even (at $100 + 15 for h&p). This is done with a purpose: we didn't want to deprive anyone -- until of course the thing is sold out.

I did send you 2 parcels with MS for the next G.F., the last a few months ago. You were kind enough to comb thru the last two Supplements after they were printed, and mortified me with pointing out a heap of errors that could (and should) have been prevented. I thought that if you felt charitable, you might do it again for the next Supplement, but this time before the errors were in print. Also, there are some interesting puzzles in there regarding nomenclatural procedure. Now that the Code has been turned upside down I am less sure (cocky) of myself, and need all the help I can get to help other palynologists to stay on the straight and narrow. So, when the evenings start to lengthen again, maybe you could give it a bit of attention. I hope to put the final touches on by end of November, and have the thing in print before year-end. I now have some 130 new/updated cards in the computer, but no figures yet. Also, Len Hills had a part of his colon removed, last week, and I have no idea how that affects a person. He is the one that made the production possible; and although he was just pushed into early retirement (any connection?), he will maintain a position at the U of C -- so

the production of this Supplement will still be administered there. I am cultivating Christoph Hartkopf-Froeder (at Krefeld!) to eventually take over the business -- he should do a good job.

Best regards, Jan.

Cc: rlonsome@bsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Jan:

On checking the e-mail and noting a message incoming from you I had big guilt feelings, as I KNOW I owe you some considerable pile of comments on this and that nomenclatural subject. Most recently there arrived a bunch of sheets from you that appear to be new G. F. cards. There was no explanatory note. Did you want me to check them, or what?

Edger was a Ph. D. student of mine. He is very bright and very independent. I did not make big headway in getting him to learn nomenclatural procedures. He is also a protege of Bill Stalks. If you wish to reach him personally, the e-mail address is: bstl@petrol.ipec.gov.tr. It's an address for a bunch of folks, so you use it and then before the message give Volken's name.

It appears to me that the name was not validly published, being not accepted by the original authors. I just checked my reprint, and I can add nothing more to what you conclude.

Now to an important side issue. It seems to me possible to have a perfectly legitimate name based on a preservational state. Megamastil paleobotany is full of such names. Thus, had Volkan made it clear that this is what he was doing, and avoided saying the naughty word "synonym," it might have been o. k. Agreed?

It's stunning to learn that authors don't get a free copy of the book. The only book I ever added provided such to the authors. However, I will send in my $75 asap.

Sorry to hear about Vaughn's personal accident and about his wife, who had a horrible experience with cancer just a couple of years ago. I suppose the chemotherapy may have led to the stroke(s)?

Take care of yourself. Best, Al.

To: jjansonius@gsc.nrcan.gc.ca
From: traverse@ems.psu.edu (Alfred Traverse)
Subject: illegitimate name?
Cc: rfensome@gsc.nrcan.gc.ca
Bcc:
X-Attachments:

Dear Jan:

On checking the e-mail and noting a message incoming from you I had big guilt feelings, as I KNOW I owe you some considerable pile of comments on this and that nomenclatural subject. Most recently there arrived a bunch of sheets from you that appear to be new G. F. cards. There was no explanatory note. Did you want me to check them, or what?

Ediger was a Ph. D. student of mine. He is very bright and very independent. I did not make big headway in getting him to learn nomenclatural procedures. He is also a protege of Bill Elsik's. If you wish to reach him personally, the e-mail address is: bati@petrol.tpao.gov.tr. It's an address for a bunch of folks, so you use it and then before the message give Volkan's name.

It appears to me that the name was not validly published, being not accepted by the original authors. I just checked my reprint, and I can add nothing more to what you conclude.

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Sorry to hear about Vaughn's personal accident and about his wife, who had a horrible experience with cancer just a couple of years ago. I suppose the chemotherapy may have led to the stroke(s)?

Take care of yourself. Best. Al.

From: jjansonius@gsc.nrcan.gc.ca
Date: Fri, 19 Jul 96 18:16:11 EDT
To: <traverse@ems.psu.edu>
Cc: "Rob Fensome" <rfensome@gsc.nrcan.gc.ca>
Subject: Name validity

Dear Al,

In checking a MS of R. Kalgutkar, I ran into a problem that I cannot unravel on my own.
Ediger & Alisan 1989 (Rev. Palaeobot Palynol., 58:154) propose a new species Lacrimasporonites traversii. In the discussion following the description they state "These forms are highly similar to L. cupuliformis, but L. traversii is not flat on one side and this seems to be a result of flattening. The three-layered wall of L. cupuliformis is probably formed by the boundaries of the inner part. Therefore, these taxa can be considered as synonymous."
I can't really understand what the man is saying -- but is he saying that the two are the same, in different forms of preservation? Did he, or didn't he, validly publish L. traversii?

The book is out. You can get a copy for the "contributor price" of $75 (h+p incl), if you send a cheque to Bob Clarke (NOT to Vaughn Bryant, who is still recovering from a cut to his leg -- a glass pot fell and exploded and almost killed him -- and taking care of his wife who is recovering -- we hope -- from a double stroke). I'd like to hear how you think it turned out.

Now, I will have to start on the Genera File, and see if I can get that out by year-end. Len Hills just had a major operation and will be out of commission for some time; this comes on the heels of an ugly "early retirement" procedure -- but he should still have a pied-a-terre after all that is settled.

Best wishes to you both,
Jan.

cc: RAF

Date: Thu, 25 Apr 1996 17:12:09 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: jjansonius@gsc.emr.ca
Cc:
BCc:
Subject: archives, etc.
X-NUPop-Charset: IBM 8-Bit

Dear Jan:

This won't match the thoroughness of yours, which I did read carefully.

The main difference between us on this issue is that you apparently think that controversial matters are better handled by informality and poor record keeping. That's what dictators want people to think. For years I had to attend faculty meetings here on the same day that I presided as rector over the parish vestry meeting in the evening. It was very interesting to contrast the two things. The department pretended to be democratic but was in fact autocratic. Motions were introduced and passed, but no real minutes were kept and were never later read or discussed. Things would be approved, but the department head or college dean would simply toss it out the window, etc. At the church I presided, there was a very fixed agenda. I called for the secretary to read the minutes of the last meeting, and I had to justify every item that had been passed but a committee or I had failed to act on in the intervening month. The treasurer read his/her report, and there was debate about practically every item. As rector I was not a member of the parish but of the diocese, to whom I reported. It was all the model of democracy and orderly action.

The IFPS problem, to the extent there is one, is the product of too little formality and record-keeping, not too much--in my humble opinion. Actually, it sounds rather petty to me. It seems to me that efforts should be made (and would probably be successful) to get the host country to "eat" any losses. Profits should go to the next organizing committee, not to the IFPS general fund. I agree wholly on that. But IFPS would be the normal conduit for the money to be so passed, and it would be in the IFPS treasury for a time. If this isn't provided for in the IFPS constitution, it would be good for someone to begin action to get the constitution changed. But I don't think anybody is guilty of high-handed sequestration of funds, etc. For one thing, we're talking about peanuts. I made far more money on the stock market last week than the total of all the sums you're talking about. Of course, it was a good week for me!

I never heard the story about the final meal at Aix. That's rich. Funny, the same thing happened at the banquet at Brisbane. Service team was totally overwhelmed.

If I were you I would turn my mind to more useful subjects.

But I DID enjoy your message. Best. Al.

Date: Tue, 23 Apr 1996 11:24:05 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: jjansonius@gsc.emr.ca
Cc: jecbot@imap1.asu.edu
BCc:
Subject: archives, etc.
X-NUPop-Charset: IBM 8-Bit

Dear Jan:

"Welcome to the club" implies that you were there first, which I deny.

You are a very talented and accomplished guy, but you don't have a clue about archives and archival management. The purpose is to assure preservation of records, for perpetual possible use. AASP, for example, archives at Hunt, so does IAPT, including the paleobotanical section thereof. So does Botanical Society of America, etc. When I became president of what is now IFPS (I led the move to get it created from ICP by simple name change), one of my early moves was to try to get as much of the early records together as possible. I was only to a very limited extent successful. The Norman Hughes and earlier records mostly simply could not be found--I am still from time to time trying. When I completed my tour as pres. Geoff Norris and I shipped our files to Caratini, my successor, but during his presidency it was decided at a council meeting to stop shipping the stuff around endlessly, and after the intervention of one presidency to archive the materials--for one thing, each presidency's files fill at least a large file drawer. Hunt agreed to take it, and ICP (=IFPS) voted to do it. Periodically, I have been renamed to see to it that this occurs efficiently. I spend a couple of days a year on the project. Of course, the stuff could simply be shipped to Pittsburgh, but having me be responsible allows me to check on how well it's going with our materials at Hunt (excellent, so far). Having both AASP and IFPS archives in neighboring files is for the future potentially very helpful for historians of science. I believe that the custom is now well established for a retiring president to ship HIS/HER files to the new president, but to send me (or my successor as archivist) the "old files." That way the new president has the previous interregnum's files, but not TWO crates of stuff.

Your statement that IFPS "hides records" is imaginary. None of the IFPS (ICP) presidents would have any motivation to do so. This all begins to remind me of the heat I took from AASP about my ICP presidency. I was soured on AASP by that whole experience.

You should take a trip to Hunt. You'd be impressed, I think. The sad thing vis a vis ICP-IFPS is that the Jonker-Hughes early papers were NOT systematically preserved.

Best regards, old friend. Al T.

Received: from emr1.emr.ca (emr1.emr.ca [132.156.36.1]) by pangaea.ems.psu.edu (
Received: from vns601c.gsc.emr.ca by emr1.emr.ca (4.1/SMI-4.1)
id AA28034; Mon, 22 Apr 96 11:04:51 EDT
Received: by vns601c.gsc.emr.ca; Mon, 22 Apr 96 11:04:04 EDT
From: jjansonius@gsc.emr.ca
Date: Mon, 22 Apr 96 10:44:40 EDT
Message-Id: <vines.2JI8+KztSlB@vns601c.gsc.emr.ca>
X-Priority: 3 (Normal)
To: <traverse@ems.psu.edu>
Subject: re: archives
Content-Type: text

Hi Al, Welcome to the club: I spent nearly all day yesterday (my birthday) at the keyboard working on the Index of our book. What is it with us -- are there no nicer things than palynology in the world? I guess not!

When Owen referred me to the IFPS archivist - you - for the information I requested, I thought that you would hold, or have access to, the papers of the last decade, say. What it amounts to is that you are just the trans-shipper? Isn't it the responsibility of an organization to look after its own archives? Now, the IFPS documents are not in the IFPS archive, they have been donated to the Hunt collection, richtig? Can you remember when IFPS decided to have their archives sent to Hunt?

What starts to bug me is that IFPS gives out no information, and is very adept at hiding recordse referring to quite recent occurrences.
I understand that you do not have immediate access to the "archive"; still, I would very much like you to answer the new questions: 1) Are the IFPS papers now part of the Hunt collections; 2) when was the decision made to have them at Hunt? Thanks for your help, and best regard again (I hope we can work on the Stuchlik thing later this summer? And, when will the new edition be ready?)

Jan.

Date: Sun, 21 Apr 1996 14:26:13 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: jjansonius@gsc.emr.ca
Cc:
BCc:
Subject: archives
X-NUPop-Charset: IBM 8-Bit

Dear Jan:

I think you are wrong, that archives serve no purpose and might as well be recycled, unless the housing unit has a staff big enough to provide the sort of service you desire. If you wanted to check the type slides in Potonie's collection, you'd have to go to Krefeld, nicht wahr? If you want to check the IFPS archives, you need to visit Pittsburgh--neat museums!

Lecture circuit? The last one was in Spain going on two years ago. What are you talking about?

I suppose you heard that Bob Kosanke died last week. Going on 79.

Yes, I still think we should write a joint paper on the Stuchlik mess. No to do so would let it slip by to bedevil others. I still have the papers on my desk and will, definitely, eventually do something about them. I think I previously mentioned that I also have the letters on other matters in the same pile.

Yes, I am still very active in palynology. I have a grant, from NOAA, for example, to study a core from Honduras, in connection with Mayan agriculture. I need to finish the second edition of my book, etc.

If you will observe the day of the week this is, it should confirm that I am not loafing too much.

All the best. Al.

Received: from emr1.emr.ca (emr1.emr.ca [132.156.36.1]) by pangaea.ems.psu.edu (
Received: from vns601c.gsc.emr.ca by emr1.emr.ca (4.1/SMI-4.1)
id AA05354; Fri, 19 Apr 96 13:12:18 EDT
Received: by vns601c.gsc.emr.ca; Fri, 19 Apr 96 13:12:09 EDT
From: jjansonius@gsc.emr.ca
Date: Fri, 19 Apr 96 12:48:13 EDT
Message-Id: <vines.2JI8+MZwRlB@vns601c.gsc.emr.ca>
X-Priority: 3 (Normal)
To: <traverse@ems.psu.edu>
Subject: re: IFPS archives
Content-Type: text

Hi again, Al!

OK, I understand that you are not the keeper of the Archives of IFPS. Why would Owen refer me to you: is he not aware that the paper actually is at Hunt?

Also, you did not inform me how I could get hold of the information I requested (originally from Owen, who passed me over to you): do Hunt have clerks to look through these stored documents and xerox info to forward on request? Will they have to return boxes to you so you can fish it out? Do I have to go to Hunt? (Or, would you?)

I just want a few facs, Ma'me. What would you suggest is the procedure to follow this up? And, should the Treasurer of IFPS keep a summary of the statements of prior treasurers in his own files? What is the use of shipping things off to Hunt, unless it is a last resort. If the documents are virtually inaccessible after that, we might as well find other ways to junk and recycle them?

Boy, you sure are linking into the lecture circuit -- it is almost like you joined the navy: to see the world! Hope you have a pleasant time while you are away.

I did get a reprint from Stuchlik (of the Polish paper), whom had been away on a stint abroad. He agreed that there had been "some taxonomical and nomenclatural errors." He also asked advice on how to handle naming Neogene spores very similar to Pteris -- should he use Cingulisporis, Cingulatisporites, Pteridacites or Pterisisporites. I'll have to answer him soon, and check these things out a bit more. Do you still want to publish on that Polish paper, and the goofs therein?

Are you still active in palynology, or kinda like winding down and sliding out of things? I realize that one somewhere will have to draw a line. Some quit cold turkey, I'm still plodding on for now.

Best wishes, Jan.

Date: Thu, 18 Apr 1996 12:43:12 -0500 (EST)
From: "Alfred Traverse" <traverse@ems.psu.edu>
To: jjansonius@gsc.emr.ca
Cc:
BCc:
Subject: IFPS archives
X-NUPop-Charset: IBM 8-Bit

Dear Jan:

As IFPS archivist, my job is to assemble records from the various presidencies and see that they are deposited at the Hunt Botanical Library, on the campus of Carnegie-Mellon Univ., in Pittsburgh. The archives are available to me only for the brief period after I receive them and before I load them up and drive to Pgh. Hunt also has AASP archives and many others.

I feel very guilty that I have never got to several interesting problems you raised a while back. Still there on my desk awaiting action. Trouble is, your problems NEVER can be flashed out off the top of my head!

Betty and I leave for Korea and Japan in two weeks. I will be giving three palynological lectures, but it's mostly a trip for fun.

Cheerio. Al.

Received: from emr1.emr.ca (emr1.emr.ca [132.156.36.1]) by pangaea.ems.psu.edu (
Received: from vns601c.gsc.emr.ca by emr1.emr.ca (4.1/SMI-4.1)
id AA01093; Wed, 17 Apr 96 11:17:04 EDT
Received: by vns601c.gsc.emr.ca; Wed, 17 Apr 96 11:17:00 EDT
Resent-From: jjansonius@gsc.emr.ca
Date: Wed, 17 Apr 96 11:17:04 EDT
Message-Id: <9604171517.AA01093@emr1.emr.ca>
Resent-Date: Tue, 16 Apr 96 17:13:12 EDT
Resent-Message-Id: <vines.2JI8+bhERlB@vns601c.gsc.emr.ca>
X-Priority: 3 (Normal)
To: <traverse@ems.psu.edu>
From: <jjansonius@gsc.emr.ca>
Subject: Financing IPCs
Content-Type: text

Forwarded to: inet[traverse@ems.psu.edu]

cc:
Comments by: Jan Jansonius@PALEO@GSC CALGARY
Comments:

Dear Al, Please find attached a copy of an e-mail message I sent yesterday to a few people (Owen as IFPS Secr., the others present or future AASP Councillors of IFPS).
Owen just answered saying that he didn't have the books of previous Treasurers, and that you should have custody of them, as IFPS archivist. I shall also copy Owen's message to you -- if you need "authorization". In reading Art. 15 of the IFPS constitution, it strikes me that "deposited into" would be more apt than "donated to".

I think you understand what I'm after, from my letter to Owen. If you have further questions, please e-mail me (or phone). Best regards, Jan.

---------- [Original Message] ----------

Dear Owen,
This afternoon I received a copy of your message to Merrell; I printed it, and shall file it for later reference. All that rests to be don eon the book is the index, and that is in Colin's hands till Thursday.

At our last Mid-year Board meeting we discussed the constitution of IFPS, in particular the stipulation (Art. 16) that any surplus funds remaining from an IPC be sent to IFPS.
As I discussed this topic with you, you know my reservations. Now, the IFPS constitution does not require a full set of independently audited books. The complications of 'foreign' systems of government are not addressed: there are several places where it is not possible for mere scientists or mortals to send hard valuta across the border.
After Brisbane I felt so sorry for the poor Aussies for having lost so much money: who would help to clear that debt -- would the Organizing Committee be responsible? (Having lived through that fear myself, I could well imagine their despair). After Aix, we all felt that there was a large surplus: there had been hundreds more registrations than anticipated, and the planned-for venues had not been expanded -- there had to be a lot of extra cash.
However, I later learned that Brisbane had a positive saldo of some $6000 left, and Aix eventually sent some $800 over to you?
This afternoon, I received an e-mail copy from Bob to Vaughn, warning that, although things started to look positive, things still could go very wrong: "look at what happened in Brisbane". So, the Brisbane myth of big-time losses is still alive.

When we discussed these things in Chicago, David Pocknall asked if there were financial statements/documents that are part of the yearly financial statements of IFPS, where the accounts for all this are available for public scrutiny. I said that I didn't know, but would enquire (as current AASP Councillor).
So, I herewith ask: could you send me a xerox of the statements of financial accounting of the surplusses of the Brisbane and Aix IPCs? I think you might not have one for Calgary or earlier, because in those days there was no IFPS requirement that a surplus should be relinquished. If I remember well, the Calgary surplus (about $2400?; nobody seems to remember) was sent directly to Brisbane to help them pay for the first expenses. We had found that it was important to have some seed money (in our case supplied by the University comvention centre).
How much was the Brisbane surplus, and how was it used? Did it all go to Aix? I seem to remember someone saying that "the IFPS coasted on that for a long time." I find Art. 16 of the IFPS constitution very loosely written: a surplus is held "in trust" for transmittal "if needed" ...
What if it is NOT "needed", is it then automatically IFPS's to do with as seems reasonable? Should it not be kept as insurance for a shortfall of a future IPC? Has this clause been a requirement for accepting volunteers offering to organize the next IPC? And, if $1.50 is insufficient for IFPS to finance its activities, we should raise that amount as much as is necessary. But, we should not have a IPC surplus as one of the pilars holding up IFPS.

I had some correspondence with Jack Medlin (USGS, Reston), who was in the organizing committee of the IGC in Washington 4 years ago. They set up a non-profit organisation, under IRS rules (thus with stringent books and independent audits) that went out to collect funding from industry, government and institutions. If there had ben a shortfall, they would have had to go beg for more subvention. However, as it turned out, they had a modest surplus. UIGS requested that the money be sent to them for unspecified programs, or general revenue. However, the committee declined and set up a trust fund under AGI, which partially financed the travel of 23 US geoscientists to this year's IGC in Beijing.

My point is: there are other ways of doing business than that of IFPS. I'd like to make this whole issue a point of discussion at the first IFPS Council Meeting in Houston. But, in order to discuss this intelligently, I should have the information I requested above. Personally, I should be happy to do away with the last sentence of Art. 16, particularly because IFPS does not address at all the situation where an organizing committee loses money -- then who is responsible? Morally, IFPS, because they hold surplusses "in trust". But that would be hard to enforce legally, and across borders. Practically, it would be the organizing committee and its members.
Finally, it bugs me that the IFPS constitution does not address the subject of accountability and independent audit. And, how would all this work out if the Secretariat is at a Russian or Chinese address? I'm not suspicious, I just do not know how.

Under the IFPS constitution, my discussions will be too late to result in any amendment (6 months being required); still, if we don't start such a discussion, the thing will never be resolved. And it doesn't require 6 months' notice to have a new point on the agenda?

Looking forward to your reply with interest, and with best regards,

Jan.

CC to Reed and Francisca (Councillors Elect), and David P.

1995

Scolecodonts

need xerox of this for filing with samples for Fall

Dear Al,

I never have been the best organized person, and the increasing lack of secretarial assistance does not help. Thus it is that I had a pile of correspondence on my desk with various requests that took more than just casual effort, as well as input from our technician in the lab. That pile became a thorn in my side, so early this year I started to systematically clean it up.

So I came across an old request from you (± 2 years old?) for some material containing scolecodonts. Some of my best material was from core, of which there is none (or little) left. Enclosed please find the residue of two cored intervals from "Bear Biltmore #1", location 7-11-87-17-W5 ; the footages are on the vials (the 3-676 refers to entry in our processing record book).

township range meridian

The age of these samples is Frasnian. There should be lots of material to play with. I hope it still is of interest to you; if it had taken less of a concerted effort and delving into stored records, you would have had it sooner.

I tried to obtain a copy of Puri 1963; I got the reference from you, but it was too long for you to copy. Our librarian can find no listing for it in the computer. Would you consider loaning it to me (by registered mail) so I can copy it? I promise to return it in a week from receipt.

Best regards to Betty + yourself,

Jan Jansonius 1995

Scolecodont Samples

6308 Bear Biltmore 7-11-87-17-W5

1015-1030' Book ~~3-675~~ no residue

1002-1014'

1044-1054' 3-676

1054-1075'

1076-1091' = 3-677

Jansonius Scolecodont material