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

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



To: gastara@mail.auburn.edu  
From: traverse@ems.psu.edu (Alfred Traverse)  
Subject: book, etc.  
Cc:  
Bcc:  
X-Attachments:

Dear Bob:

I'm a comparative novice, though very enthused, with e-mail. Why do some people always send one's letter back, with the answer appended, as you do (at least in this case, did)? It frequently means that it takes two pages when (if) it is printed out. Hmmm.

No, I didn't know about N. P. coming here. Indeed, I don't even know HIM. I don't see P. H. as often as I used to.

O. K., I guess I'll agree to do a chapter, especially as you, TNT, Bill Dim. and others say they are going to do so. I still think it was handled in a weird way!

Thanks for the holiday greetings. I must say that I have found the season less trying that I have for decades, now that I am emeritus. When I was teaching and had lab projects, finals, grade cards, etc., right up to as late as 22 Dec., I found erecting a Xmas tree, etc., a dreadful bother. I wish you and yours a very happy holiday and, of course, a fine 1997. (Ich wünsche Euch fröhliche Weihnachten und einen guten Rutsch in das Neue Jahr!) AI.







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14 February, 1995

Dr. Robert A. Gastaldo  
Department of Geology  
210 Petrie Hall  
Auburn University, AL 36849-5305

Dear Bob:

Just explained via e-mail that I'm glad to be in touch via that medium, but it's hard to send reprints that way, so here's the real thing too.

Yes, the Department Head here found an interesting way to compel me to retire after several years of trying. It consisted of financial inducements that wouldn't have been enough for me, though Betty and my financial adviser thought they were very persuasive because at age 70 I could have a stroke tomorrow. What I found compelling was the threat to take my space away if I didn't retire, whereas in retirement I can keep my lab until at least 1 July, 1998, perhaps longer (depending, I suppose, on what I do to justify it). I can teach the palynology course if there is demand. I will not be replaced (by anybody in our field, that is). I have arranged for my collections and library to go to the Field Museum when, one way or another, I no longer can work with them. Fact is that ALL university situations are tenuous and subject to destructive whim and local fashions and fads.

Wish you had said what the subject of the Pfefferkorn and DiMichele work with you is. Hermann could have told you that I can't make it to the Philly meeting, as he invited me to give the featured address. It happens to be the same weekend as Betty is getting her Ph. D., and the whole family and many friends are assembling here.

Please keep in touch. I continue to think that your work is very exciting (and gets you to some neat places).

Yours very truly,

Alfred Traverse

encl.:reprints

Review of Proposal of Robert A. Gastaldo

The study of dispersed organic matter (mostly of plant origin) in sedimentary rocks is one of the last remaining more or less virgin territories in the study of such rocks. Bob Gastaldo has been a pioneer in investigation of the fate of plant parts, from living vegetation to particulate plant material (phytoclads) in sediments (plant taphonomy), and his work deserves support almost independently of the quality of this individual proposal.

The currently proposed research, to look comparatively at ancient and modern palynofacies--in this case, the suites of plant fragments that characterize certain sorts of sedimentary rocks (e. g., tracts of sequence stratigraphy)--is backed by Gastaldo's unique knowledge of his subject matter--he is after all the leading student of plant taphonomy. As Traverse (1994) has pointed out, this sort of investigation has to do with what he calls "palynolithofacies"--the accumulation of suites of certain sorts of plant fragments in response to sedimentary characteristics. There is another species of palynofacies, "palynobiofacies," which is quite different, depending on the biological environment and being a reflection of it. Both subjects deserve study. I hope Gastaldo will not totally divorce the two approaches and topics.

In any event, this proposal should be funded to keep up the flow of information from this very talented and resourceful scientist individual and his laboratory. The budget seems modest. I am sorry it does not include evidence of a graduate student who would work on the project, and I hope that one materializes.

12.Feb.95

Received: from mallard.duc.auburn.edu (mallard2.duc.auburn.edu) by pangaea.ems.p  
(4.1/PSU\_ESSC/GEOSC-2.02) id AA25031; Thu, 9 Feb 95 09:52:22 EST  
Received: by mallard.duc.auburn.edu (5.0/SMI-SVR4)  
id AA15432; Thu, 9 Feb 1995 08:52:15 -0600  
Date: Thu, 9 Feb 1995 08:52:15 -0600 (CST)  
From: Robert A Gastaldo <gastara@mail.auburn.edu>  
X-Sender: gastara@mallard  
To: Al Traverse <traverse@ems.psu.edu>  
Subject: Long time no contact  
Message-Id: <Pine.SOL.3.91.950209084108.13413A-100000@mallard>  
Mime-Version: 1.0  
Content-Type: TEXT/PLAIN; charset=US-ASCII  
Content-Length: 2406

Dear Al - I just ran across your e-mail address in the AASP newsletter supplement and thought that I'd drop you a line to see how you've been doing and to learn if the rumor about the dreaded "r-word" is true. Has the university made you an offer you couldn't refuse and actually retire? Will they allow you to keep your office/research space? And, the big question, will they replace you with some young buck?

This year I'm one of the Paleo Society's Lecturers and last November I was invited out to Oklahoma for a seminar. At that time I spent a half a day with Dick Wilson. At 89 he's still going strong, coming to work every day, and still consulting for the oil industry. It's hard to believe that he's been retired almost 20 years. The University of Oklahoma has embarked on a fund drive to raise money to build a new Natural History Museum. This will replace the old facility (which isn't much of a facility) and it is proposed that there will be a curatorial staff that would include a paleobotanist. Of course, time will be the true test of this plan, but if Dick has anything to do about it, I'm sure it will be a reality. While I was visiting, Dick showed me through the collections and ran me through some specimen suites. It's amazing how much information is crammed away into his area. It was strange, though, that just before I had to leave to catch a plane, Dick pulled me aside and asked me to make sure that the materials weren't dumped in the future. I told him that I'd do what I could to ensure that the specimens and preparations weren't lost to the scientific community.

Things at Auburn continue to plug along. It would be nice someday to have a block of time to sit down and think. With our "modified High School" curriculum (quarters) where we meet classes every day of the week, it gets very hectic. I'm looking forward to this weekend as Hermann Pfefferkorn and Bill DiMichele are arriving tonight so that we can finish a manuscript that we began TWO YEARS AGO!! I find this hard to believe, but time keeps slipping away faster each year. Once we get over the hurdle of getting it written, the next hurdle will be to get it accepted. But isn't it always the case.

Will you be able to attend the MidContinent Meetings in Philly this May? I'll be coming up and hope to see you then. Please give my best regards to Betty.

Bob

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14 March, 1994

Dr. Robert A. Gastaldo  
Department of Geology  
210 Petrie Hall  
Auburn, AL 36849-5305

Dear Bob:

Just had time to tackle the bunch of reprints you sent out on 11 Feb. Periastron paper is fascinating--mostly for me because of the plants! I need to know more about it. You refer me to literature but don't actually say what it is.

Log-transported gravels reminded me of the long-ago Brandon project at Harvard. Some of the coal contains sharp-edged quartz pebbles and cobbles, presumably from the Cheshire Quartzsite. We thought it came in on tree roots during floods.

Taphonomy paper from Palaeobotanist I had read in the original volume which I got as a participant at Lucknow in the Sahni Centenary. Gastaldo & Hue reminds me vaguely of a chapter I once read in a soon-to-be published book. Ditto Gastaldo, Allen & Hue!

The packet also contained a copy of the Dauphin Island blurb. The others were displayed on my bulletin board ("please take"). It was a disappointment because what the student wanted was a BOTANY course--trees and stuff, you know. This booklet advertises some neat material, but the closest it gets to plants is algae and aquatic vascular plants. It's a fact that at PSU (17 campuses, 76,000 students) the only real botany nowadays is in horticulture!

All the best.

Yours very truly,

Alfred Traverse

AT/et

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2 November, 1993

Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36849-5305, USA

Re: Page proofs for Sedimentation of Organic Particles

Dear Bob:

Betty and I have proof-read the entire book, using the only method that really works: one person (Betty) reading aloud from the typescript as submitted, the other person (AT) following each word of the typesetters' proof with a ruler. We hope we have caught all errors, but I know from Paleopalynology that one always misses some. I do not wish to bore you with unnecessary details, but, as most of you discovered, it is clear the CUP had a computer-glitch of some sort: e.g., (1) frequently the word at the bottom of a typescript page was run into the word at the top of the next page; (2) hundreds of hyphens were dropped.

After our go-through of the book, we went back to compare our markings with yours. Thank you for your very prompt return of your section! We hope you realize that the typesetters' proof as you received it reflects earlier editorial changes made by CUP and stylistic corrections of one sort or another made by us. Your corrections have been incorporated, along with some of our own (Kalimanta had to be corrected to Kalimantan in 4-5 places, "chapter" substituted for "article" here and there, "effects" instead of "affects", etc. In updating the Liu & Gastaldo paper we discovered that the published title is different from the one you submitted, so we have corrected that also.

All has now gone off to CUP, and we await the next development (for us probably the index and the final proofs; for you, the book itself). Thanks very much for your patience and encouragement. You've been a pleasure to work with and are a good friend to boot.

Yours very truly,

Alfred Traverse

P.S. Don't forget about the info on the botanical field program for my little friend here.  
AT/et

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2 December, 1991

Dr. Robert Gastaldo  
Inst. für Geologie - Universität  
Goldschmidtstr. 3  
D-3400 Göttingen, Germany

Dear Bob:

Just in from an interesting trip to the Birbal Sahni Inst. in Lucknow, India, and find xeroxes of an exchange of correspondence between you and Al Davis re Steve Austin. I want to reword my recollection of the facts.

Davis is right that none of us suspected SA's relation to the Creation Institute (or whatever). I did not recall what grade I gave the thesis--I tend to be generous! I do recall that the paleobotany-palynology part for which I was responsible was very good--except that Steve had swept under the rug that he had sub-contracted the lab work to a consulting firm elsewhere, without proper credit. I am sure the record, if it still exists, will show that I refused to approve the thesis until new material was added, making the situation clear. He also failed to credit some other sources. That was my objection, not the creationist link, which I never guessed until SA was gone.

That there was "something funny" about SA my wife always suspected, because he had financial and other resources around the country that no other grad student had, was always going on mysterious "vacations," and, furthermore, had a job waiting for him, about which he declined to give details (although he was at all times pleasant and seemingly cooperative).

SA took my palynology course in 1977, one of 11 students (several of whom are now professional palynologists) and got by far the highest grade. He never challenged geological concepts of time, etc., as crusading creationists invariably do. As I look back, it is, of course, abundantly clear that he was intent on maintaining a low profile until he had his degree in hand.

I thought I'd better get all of this in writing. I recall discussing SA with you here, but I apparently did not make clear that my objection to the dissertation was an ethical one, not a quality one. I may also have shared with you my feeling that the degree should be rescinded, because S.A.'s application for admission here was fraudulent, in that he never revealed that he had published "scientific" articles widely under a pseudonym. I have confronted him about this, and he said he did it because PSU wouldn't have admitted him had they known his identity Q.E.D.

Hope you're doing well with my old friends at Göttingen. All the best.

Yours very truly,

Alfred Traverse

AT/et  
c: Alan Davis

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13 May, 1991

Dr. Robert A. Gastaldo  
Department of Geology  
210 Petrie Hall  
Auburn University  
Auburn, AL 36849-5305

Dear Bob:

Well, I took your advice and rejected the Pocock and Vasanthy MS. But it was hard and convinces me that I'm not cut out for the life of an editor! It's a disappointment that my idea of a modern, cleaned-up version of the Pocock-Masran classification can't be in the book, but Stan apparently cannot, or doesn't want to, produce such.

Reviewers rejected Doug Nichols' two chapters too. He spent three days here recently working on them and promises to get new versions to me by the end of May. We'll see--that's the deadline. We are down to Scheckler now, as an author with an outside chance to get a MS to me before it's too late.

Best regards.

Yours very truly,

Alfred Traverse

AT/et

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13 May, 1991

Dr. Robert A. Gastaldo  
Department of Geology  
210 Petrie Hall  
Auburn University  
Auburn, AL 36849-5305

Dear Bob:

The massive stack of reprints finally has surfaced. All will be carefully read. Several would otherwise not have been seen. Thank you very much. Your work is of great interest to me.

Doug Nichols was just here to work three days with me on his book chapters. He insists on "mire" for peat-accumulating "marsh" or "swamp." How would "backmire" go over? Hmmm.

Hart's three chapters in, in final form. I believe I'll just have to forget the 2-3 Schecklers and go for it, to meet CUP's August deadline!

Viele Grüße!

Dein

Alfred Traverse

AT/et

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3 May, 1991

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Your letter re the assistantship has been posted and discussed. This seems to be sort of a dry year. I had a couple of good prospects last year who are now graduate students elsewhere, but this year's crop isn't too promising. However, I'll keep thinking about it and, who knows?....It certainly is a smashing opportunity!

When should I start writing you in German for the practice?

Best.

Yours very truly,

Alfred Traverse

AT/et

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29 April, 1991

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Re yours of 18 April. Guess I'll give the Postal Service a little business. Fax is great when speed is essential. Otherwise, for the original operator, it's more trouble than this. I was very amused by your tale of the added cover form.

Re Steve Scheckler, there is (may be?) news. He faxed me a couple of days ago that his MS is finished (good news), but not yet typed into a computer (bad news); no word about the illustrations (good or bad news?). Maybe he'll yet come through--but surely not for a couple of weeks yet. Your assessment of poor Steve's plight is 100% accurate, and my wife alone has (more or less) saved me from a similar fate. Actually, I too am always behind. Speaking of the textbook of paleobotany--that is really needed. We are using your "workbook," but it is too non-textbookish for the average PSU undergrad to cope. I have toyed with writing a text myself....[let's hope not!--et].

You helped me with the very difficult Pocock decision. The other reviewer (Habib) had a go at the "new-improved model" too and was equally damning. I have to drop Pocock, no two ways about it.

Schaarschmidt is having a miserable time finding housing for us in Frankfurt. Die Ursache ist die Menge von Flüchtlingen--tausende!

All the best.

Yours very truly,

Alfred Traverse

AT/et

# Auburn University

Auburn University, Alabama 36849-5305

College of Sciences and Mathematics

Department of Geology  
210 Petrie Hall

26 April 1991

Telephone: (205) 826-4282  
ATTNet: 221-4282

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

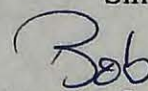
Dear Colleague,

I am writing to you to inform you that beginning in the 1992-1993 academic year I will have available a Research Assistantship for an M.S. candidate in Geology or Botany. We have been funded to begin a megafloral-microfloral biofacies analysis of the Rajang River delta in Sarawak, Malaysia, in a cooperative project with Jim Staub at SIU-Carbondale. We plan to complete our first field season next summer, with sample preparation and first year analyses following. I will be looking for an M.S. student with interests in either palynological or dispersed cuticle (phytoclast) analyses to begin his/her graduate career in September 1992. There is a Research Associate position that who will be responsible for the palynological suites and, therefore, I would be interested in a student whose attention would be focused with my interests on dispersed organics. I realize that this might be an attempt at trying to find a needle in a haystack, but I am writing to you now so that if you know of an undergraduate student who will be graduating next year (1991-1992 academic year), you could inform him/her of this opportunity.

I will be on sabbatical next year as a Senior Research Scientist on an award from the Alexander von Humboldt Foundation, Bonn, Germany. I will be collaborating with Walter Riegel in Göttingen, and will begin my residence there in early August. My address will be Institut und Museum für Geologie und Paläontologie, Der Georg-August-Universität, Goldschmidt-Str 3, 3400 Göttingen, Germany (FAX - 011 (37) 551 39 79 96; TEL - (0551) 39 79 56). If you have a potential candidate for this RA, please either contact me prior to my departure this summer or sometime during the next academic year. I would like to correspond with the student to explain the project and what his/her duties would be with regard to the assistantship. In addition, I would like to assure the student that work completed under the assistantship would translate into an M.S. thesis.

If you have any questions about the position, or know of any potential student, please do not hesitate to contact me.

Sincerely,



Robert A. Gastaldo  
Alumni Professor of Geology

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8 April, 1991

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

The "PMT" arrived in perfect order, and added to my very tiny -  
computer vocabulary.

Your review of the new Pocock MS also came. I am now sending the  
MS to one other reviewer. If he also pans it, it will simplify  
my job--in fact, we are getting near the bottom of the pan of  
problems.

A very trusted reviewer also has rejected Doug Nichols' two  
chapters, and that has really ignited a fireworks display! Oh,  
woe!

I note that you pointedly ignore my question about  
Scheckler--should I just write that off? If you could assess the  
SS problem, even that would assist me.

Thanks for all your help.

Yours very truly,

Alfred Traverse

P.S. Please note new fax number (old one still works, but new  
one preferred).

AT/et

# Auburn University

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2 April 1991

Dr. Al Traverse  
Department of Geosciences  
435 Deike Building  
The Pennsylvania State University  
University Park, PA 16802

Dear Al,

Under separate cover I have sent you a PMT of Figure 1 for my manuscript. You should have received this by now. If not, please let me know. I can have another one made and forwarded.

I have read the Pocock & Vasanthy manuscript and have made some comments directly on the manuscript. I have not, though, taken the immense pains with this version as I had with the original version.

Page 1: The Introduction needs to be reorganized and I have made one suggestion as to how to do this. I do have some difficulty with their statement concerning the presence of marine or brackish palynomorphs recovered from a coal-bearing sequence (that usually means from sediments above or below an actual coal) that definitely indicates an incursion of marine waters into the peat swamp. I know of situations where this might occur, such as in detrital peat assemblages, but such circumstances do not warrant a blanket statement that would seem to apply to thick, low-sulfur, low-ash peats that were probably formed in a raised mire. Additionally, if the peat swamp were transgressed in a relative sea level rise, then a marine sediment could be deposited directly atop of the peat but the two events unrelated.

Once again, the article is severely under-referenced. There are statements in the text that must be referenced and even some of the references they use either do not apply directly (e.g., Faegri & Iverson for samples taken from two different holes) or are missing in the literature cited.

The Geographical Setting section needs to be amplified to include the pertinent climatic and vegetational data. What is the mean annual temperature, minima and maxima? What is the mean annual rainfall, minima and maxima, and distribution throughout the year? What is the present vegetation of the region? All of these are pertinent but missing.

Page 2: The Geology Section needs to be amplified. Are these sediments truly peats or organic-rich silts/muds? What is the % Carbon? From where were the cores extracted? The setting is never discussed. Are these cores taken from limnic environments, soil profiles, point bars of streams?

The Previous Investigations section provides a rudimentary overview of the Vasanthy et al. (1980) presentation. The only reference to this work is in the form of an abstract given at IPC at Calgary and the amount of detail in this can only be minimal. The authors needs to provide

as much pertinent detail as possible within this section. All diagrams of core should be reoriented vertically. This would allow the reader the benefit of not having to turn the page to see the relationships in perspective.

There is some difficulty with the  $C^{14}$  dating discussion in this section. The authors never state how many cores were taken in the study (I presume 2 based on their statement of page 4), and the distribution of core in the study area. Also, they do not state what materials were actually  $C^{14}$  dated. There is a big difference if rooting structures were dated separately from woody material. The wood fragment may have survived for several thousand years on the surface of the area before being transported and buried at the same horizon as the rooting structures. This might explain the vast discrepancy in dates obtained. If more than one core was samples, what is the physical correlation between cores? Is a depth of 100 cm in one core equivalent to 100 cm depth in the other? There are alot of data that are missing that would assist the reader in evaluating the authors claims.

Page 5: Figure 4 is too small to be of any use. If this manuscript is accepted, this figure must be at a size where it can be read. How many samples were analyzed for the Sedimentary Chemistry? Were duplicate samples run, or just a single sample?

The Chemistry of Palynodebris section includes discussion and conjecture as to how elements might be distributed. The authors need to document other cases where there has been an upward migration of elements (on the order of a meter or more) and concentration thereof. What is the solubility of copper and its ability to be translocated under 1 atm pressure, a moderate temperature (never given), and a slightly acidic pH? The authors need to include some of the geochemical literature to demonstrate that empirical data exist that would confirm their assumptions. This same problem exists with the discussion on page 13.

Page 8: The authors state that there is a contamination from the modern activities of man. What are the contaminated materials and how can you distinguish these from other materials not introduced through anthropogenic means? Is there a  $C^{14}$  date for the bottom of the core? How long has man been interacting within the Konar Ar Basin? Could all of the plant detritus be somehow anthropogenic in origin and could varying anthropogenic activities in the recent past be responsible for the changes in character of the palynodebris?

Page 9: The authors state that they want to believe that the youngest  $C^{14}$  age date *is probably correct*, but all they use to substantiate this belief is that it represents sediment at its *correct stratigraphic position*. What does this mean? Has there been bioturbation within the core that has mixed the sediment below this horizon? What would disrupt sedimentary bedding and blend the sediments from lower in the core? How can this horizon not be at the *correct stratigraphic position*? I am confused by this point and find their arguments extremely weak.

The authors state that the palynology and inferred vegetational history has been previously discussed in the published abstract. Once again, this is only an abstract and there are no data presented therein. Also, the abstract is in a gray-literature citation. This is not readably accessible to those outside of the present palynological community and even some of these workers may not have access to these abstracts if they did not participate in the conference.

Page 10: The authors spend a short time on the discussion of the development of inertinite. They state that charcoal is formed via oxidation at normal temperatures (a hotly debated topic) and as the result of fire. Neither of these arguments has any pertinent reference included. Both hypotheses on the origin of charcoal must be referenced. The reference they chose to substantiate the claim of charcoalization at normal temperatures MUST HAVE EMPIRICAL DATA included within to be of any value. A reference that just states that this can occur without the empirical data is useless and just propogates geofantasy.

The authors state that conversion of structured material to amorphous materials involves *mechanical* breakdown in combination with fungal & bacterial degradation. What are those mechanical (physical) factors that are responsible? Where is a reference that provides the data to substantiate this claim? Have there been experiments conducted that demonstrate that mechanical processes are requisite to the formation of amorphous materials? How can these operate after burial? Do the authors believe that this material is exhumed and then buried again?

Page 11: Is there any empirical data (and references) to support their contentions that (1) orange-brown amorphous matter is the product in *an oxygen deficient, but not anaerobic, aqueous environment* and (2) that *under anaerobic, reducing conditions bacterial breakdown of structured materials produces a clear amorphous product with an overall gray cast*? It would be helpful to include a figure of pyritized wood of varying sizes as inclusions within this gray amorphous material.

Page 12: If the sediments are not lithified, how can the interval under discussion be termed shale-prone? Also, is there an SEM of the inertinite *mainly in the form of pyrite, ... representing pyritized bacteria*? How can amorphous materials be without size? I can see where they can be without structure but if they had no size they would be invisible.

Page 14: How many analyses were made and used to construct Figure 11? I assume that the X in the figure represents the population mean, but this is not stated anywhere in the figure caption. Or, is this the only sample analyzed around which the rhombs are constructed?

The straight-line inference that a low quantity of structured material equates to a better hydrocarbon precursdor due to a lower C/H ratio may be misleading because it depends on what other plant parts are incorporated within the sediment and their C/H ratios.

Once again, I am disappointed with the quality of the manuscript and the less than rigorous way in which the data are presented and interpreted. The major fault with the interpretations is with the out and out lack of pertinent literature references that provide the empirical data upon which the authors interpretations are made. Without such an attempt to provide applicable and relevent literature citations, this manuscript should no longer be considered for inclusion in the book. Although I have not read the other contributions, I would bet that this is the weakest chapter in the compilation. I suspect that book reviewers would harp more on the vague arguments and poor presentation of this chapter than on the positive aspects and chapters of the book. In all, such a poor chapter could reduce the appeal of the book (one that will be priced not too cheaply I presume) to the audience we are trying to attract. I realize that the final decision is yours, but if this were not brought up to the same standards as the other chapters quickly (and this is the second chance they have had to do this), I would reject it.

Best Regards  
Bob

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25 March, 1991

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Hope you haven't written me off because I sent the Pocock & Vasanthy George MS--at least it's comparatively short!

Finally found a geophysicist who could tell me what a PMT is. Why not send one if it's easy to get, as it would be just one more thing I wouldn't have to bother with here.

All the best.

Yours very truly,

Alfred Traverse

AT/et

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18 March, 1991

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Can't remember exactly where we are with our correspondence. Now that we have upgraded to WordPerfect 5.1 (from 5.0), your MS file has been duly incorporated. But, regarding your material, I can't find anybody in the hall who knows what "a PMT" of the figure means. I'm still asking. More about your MS later.

However, I have something else for you to consider. Just in is a new effort from Stan Pocock. It seems to me to have possibilities--gets the Pocock-Masran classification in and makes some good points. It does need some work. What do you think?--please get to it urgently. I haven't sent the MS to anybody else.

We're getting near the end of the road on the book--there really is only one MS (other than Pocock) still out for which I have any real hopes: Scheckler. I wonder if you could tell from Steve if he really is producing a chapter--I have talked until I'm blue in the face, and he keeps assuring me it'll be "in the mail soon." I know you got a MS from him for the paleobotany short-course book (which I am using with good effect in the paleobotany course).

My physical exam for the Fulbright is today. That is one of the last hurdles.

Best wishes.

Yours very truly,

Alfred Traverse

AT/et  
encl: copy of new Pocock MS

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23 January, 1991

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Yours of 8 Jan. 91, the diskette and the nice, clean manuscript all arrived in apple pie order. I will work through the MS very soon and will be back if there are problems. The new Fig. 1 looks good to me at first glance.

Pocock is considering the possibility of doing a new chapter based on the application of his system and ideas to a particular problem--much more concise than the other thing. Incidentally, he was very complimentary to you for your review, though of course he didn't know who did it.

Congratulations on the Humboldt. I'd trade you 50% of my German fluency for 50% of your computer fluency, if such could be arranged! My reaction about the Paleogene project was similar to yours--I was told nothing of it until long after the fact--about a week ago. It is clear that we were included to impress the German authorities, and I don't really mind. My plan was to work on Devonian spores, however. Walter Riegel has already written me expressing the wish that we can visit him and Christine again. He showed us a marvellous time in the Göttingen area.

All the best.

Yours very truly,

Alfred Traverse

AT/et

# Auburn University

Auburn University, Alabama 36849-5305

College of Sciences and Mathematics

8 January 1991

Department of Geology  
210 Petrie Hall  
Dr. Al Traverse  
The Pennsylvania State University  
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Dear Al,

I know, I know, it's later than promised, but it's here. Enclosed you will find the revised manuscript as per our discussion in State College and the suggestions made by Dave Batten. You will first notice that the running heading hasn't changed, although the title of the chapter has. I didn't notice this until this morning when I was making a photocopy for my records. I have changed the running heading on the file sent to you on disk (WordPerfect 5.1 format - PHYTCLAS.MS). As per our discussion, I have also drafted an additional Figure 1 and placed it in the beginning of the chapter. This will move all other figures up one in the numbering hierarchy. I have attached photocopies of the figures with the new numbers, please correct the original figures in your possession. Additionally, I have enclosed the **FreeLance** file for Figure 1 (**FIGRUE1.DRW**) on the diskette. If you need to have a **PMT** made of the figure for Cambridge University Press, please let me know and I'll have one sent. They may be able to utilize the enclosed file.

In the first section I have included two subheadings (as per Batten's suggestion) to break up the text. They are "Interaction at the soil-air interface" and "Factors affecting decomposition." I have changed the second major heading, "Depositional Environments and Phytomacrodetritus" (as per Batten's suggestion), to reduce the redundancy. All citations should now be correct and included (it's been hell trying to find the Ray, 1959 compendium as it must have sat on someone's shelf for the past six months somewhere in the university). I hope that you'll find the corrections in accord with our review.

Thanks for the copy of the letter concerning the Pocock manuscript. I hope that something can be salvaged from it as a review of their classification system would be a benefit to the book. Their unsubstantiated data and interpretations would have been a considerable hinderance. I wouldn't hold up the production of the book for their manuscript alone, though, and hope that they can turn it around quickly.

Well, it looks like we'll be spending the next academic year in Göttingen. I've received an Alexander Von Humboldt Research Fellowship from the AvH-Stiftung. I'm scheduled to begin a two month intensive German Language course in August and the Fellowship beginning in October. We've negotiated a leave from the university and expect to have the family resident in Göttingen by the time school starts in the fall. Now comes the task of getting all of the ducks lined up before our departure (housing, etc.). I received a letter from Schaarschmidt with a copy of a report forming a working group on the Paleogene of central Europe. I noticed that your name was also included as one of the international experts who will (notice I said will) cooperate in this venture. I was quite surprised by my inclusion in the working group because the meeting at which this was decided occurred two months before I was notified by the AvH, and one month before the AvH met to make the award! In any case, I guess that we'll be seeing each other in Deutschland this coming year.

If you have any questions about the manuscript, please give me a call and we can make the corrections over the telephone. Best of the New Year to both you and Betty.

Sincerely,



Robert A. Gastaldo  
Alumni Professor of Geology

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9 November, 1990

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Your letter of 15 October came about ten days ago. I am sorry that you had such a miserable time for so long with the Pocock et al. MS. However, what you did to it and have written about it will be helpful in deciding how to procede, and I appreciate your work very much.

I go down to NYC on the 19th to spend a couple of days with Dan Habib. We will work on his MS part of the time. We will also have a very serious discussion about what to do with the Pocock et al. MS, in which your review will play a big role. (As I told you, I believe, he was the original reviewer and threw up his hands after 2-3 pages.)

I have talked at length on the phone with Stan Pocock about this. He is, after all, an old friend. I suggested that, based on a forthcoming conference with an unnamed reviewer (Habib) and on the review of another unnamed reviewer (Gastaldo), I might propose a much abbreviated outline of an alternate chapter that we could consider, but that the present MS is unsatisfactory. More later.

Change of theme! You don't say anything about the Gastaldo chapter. I hope you can get onto that comparative "breeze" soon.

All the best. Thanks for everything. I'm sorry that your visit here had to share my attention with events in the St. Louis area.

Yours very truly,

Alfred Traverse

AT/et

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15 October 1990

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Dr. Alfred Traverse  
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Dear AL and Betty,

Once again I would like to thank you both for your hospitality shown to me last week during my visit to Penn State. I found the time I spent in your lab quite enjoyable (even though you did saddle me with the following review). I hope that we may one day reciprocate if y'all get down south. Now, onto the serious stuff.

I've had to quit on page 62 of the manuscript. I can't even force myself through the last 20 pages (possibly 3-4 hours of work) because the manuscript is in such bad shape that I feel that it needs to be totally rethought, revised, and rewritten. There is so much wrong with the manuscript that it is very difficult to begin criticism anywhere. It's like a pre-rough draft. The sections are disjunct, the thoughts and points attempted to be expressed in the manuscript are disjointed. In a lot of instances, the paragraph doesn't follow the lead sentence or the point expressed in the lead sentence. The text is vastly undercited. That is, the authors make extremely broad generalizations and have no reference to the point. They oversimplify to the point of absurdity in some cases. The science that is presented is not particularly good. There are tons of data and information that are necessary for the reader as background to evaluate their results, but are missing. Where the authors do cite a reference to which the reader can go to get the necessary data, it's usually a published abstract! There are ideas and portions of the manuscript that are endlessly repetitive. Some parts are written as if the authors were addressing first year university (or even high school) students. It's almost insulting to one's intelligence. Based upon my countless hours mulling through this I recommend **rejection** of the manuscript. There are some interesting points in the manuscript, but they are not substantiated by data or citations. The manuscript would have to be completely rewritten and re-reviewed before I would consider it to be included in the book. I have included below some of the more pertinent problems with the manuscript. You can refer to the manuscript for all comments, suggestions, and revisions.

**TITLE PAGE:** The manuscript DOES NOT address these points. In no way do the authors attempt to provide an integrated discussion on any of these points except for some aspects of diagenesis (bio-diagenesis & geo-diagenesis). The manuscript should more aptly be titled: "The Masran Pocock Classification System: A Review with Applications and Suggested Topics for Further Investigation."

**CONTENT:** The manuscript should be written so that each paragraph in a section flows into the next. The use of assigning each paragraph with a numerical reference hinders the reader. In many cases these numbered paragraphs and their topical areas make the manuscript appear more disjunctly organized than it possibly may be. In addition, some of these topics are unnecessary and are almost devised as a means to lecture the reader on how to do research. The book is geared for the professional community, not the beginning student.

**Section 1:2** - The authors in the first sentence outline several parameters but only discuss the first, the proportion of organic material in a sediment. How do each of the other parameters listed affect the characteristics and texture of the sediment. What are the specific interactions between all of these parameters? When the authors list the various organic contributors, why aren't these listed in some recognized biological order (protistids first, higher vascular plants last)?

**Section 1:3** - The authors appear to have a very limited knowledge of the taphonomic biases inherent in the fossil record and how these can and have to be filtered out before reconstructions of past vegetational ecosystem histories can be attempted. This point shows up time after time throughout the manuscript.

Section 1:5 - Is this really necessary? It's written so broadly and vaguely that it provides no information beyond what most scientists (those who would read this book) consider as everyday knowledge. This same approach is also found throughout the manuscript.

Section 2 - This section provides a historical, methodological, and philosophical approach to their classification BUT does it so superficially that the reader is left wanting. This section is an unnecessary prelude to the next section where they, once again, repeat everything in this section only under the guise of their classification system. There are assumptions (unreferenced and unsubstantiated) made that need to be verified. For example, in Section 2b. they "surmise that they [amorphous translucent masses] are formed, ... in part, by the breakdown of structured organic matter." In section 2d. the authors state that fusain is formed via true oxidation which I'm not sure what this means. Is there false oxidation? Additionally, they do not use any references relevant to the formation of fusain. Scott's 1989 review, for example, could be cited here and the terminology adopted therein (and elsewhere) should be used. Fusain is the product of incomplete pyrolysis. The first sentence in the paragraph defines the fourth class as residue that is "black, opaque, and splintery in form." Then they include material (pyritized) that is "bright and golden in appearance." It appears that the authors are very arbitrary in their assignment to major classes and may, in fact (see comments on TABLE 2) not have a very tight classification scheme devised.

Section 2:2 - The authors want to use inertinite as one of the major classes (fourth major group) but do not discuss or even note the role of semifusinite, sclerotinite, and micrite in this category. Don't these apply to their fourth major class? If not, why don't the authors address this point? Section 2:2 seems to be redundant of section 2.

Section 2:3 - There needs to be major reorganization and clarification in this part of the manuscript (not the first of many parts that need this kind of treatment). Paragraphs addressing both the structured & biodegraded terrestrial and the structured & biodegraded marine debris need to be elaborated if this section is to remain. They make a generalized supposition (as they do throughout the manuscript without supportive data) that it is only "logical to suppose that the morphologically similar amorphous material in sediments will yield oil and gas ..." Why is it logical to suppose that the overall shape and not the geochemical character of the material will be the requisite for hydrocarbon production? What, if any, geochemical data do they have to support this?

Section 2:5 - the authors mention a two-class and four-class system of classification, BUT never directly tell the reader what the two class system is. Is this the broad generalization presented at the bottom of page 8?

Section 3:1 - The results of the experiments conducted by Masran & Pocock should be summarized. When they used "major groups of the plant kingdom" did they mean algae (sensu lato) vs. tracheophytes; green algae (or brown/red/yellow-brown) vs tracheophytes (in toto); algae vs lycopsids vs pteridopsids vs gymnosperms, etc.?

Section 3:2 - What are the ranges of high, medium, and low pH's? The authors never define these but use them as an integral part of their classification system. What role does Eh play in this? This parameter is never considered. In fact, neither of these parameters (and others) are never considered in any detail.

Section 3:3 - The authors state that there are TWO categories of equal rank, but in Table 2 they provide NINE categories of equal rank with various subcategories. It appears as if the authors don't have any systematic classification system devised, just a number of categories that can be used to identify palynodebris. What IS the system of hierarchy within their classification? Can they draw a dichotomous key (their two categories) with subdivisions that could be used to classify these particulates? On pages 9-10 (and elsewhere in the text) the authors state that coal maceral terms can not (and should not) be applied YET include these in TABLE 2 - CLASSIFICATION OF PALYNODEBRIS. The explanations within the text and those drafted in TABLE 2 don't correlate!

Section 3:4 - They say that they have established a "basic framework" yet this basic framework can not be discerned from the preceding discussion.

Section 4:1 - All references to their business should be placed in acknowledgements. They also state that they have "fine-tuned" the system over the years but the fine tuning ONLY MEANS a change in semantics with regard to inertinite (a coal maceral term when they have rejected other coal maceral terms!) for charcoal. Is this actually "fine-tuning"? I tend to disagree and too much back slapping (self-congratulatory rhetoric) seems to be in these sections that could be eliminated without any loss to the manuscript.

Section 4:3 - It is difficult to say whether or not the remainder of the paper which will "outline the characteristics of the different categories and their environmental significance" actually does what is stated. Section 5 is just a repeat of the previous 15 pages and could be summarized if condensed with the previous text. As far as the environmental significance of the palynodetritus, little more than the ability to segregate terrestrial from marine materials is presented. This, in itself, is no new revelation. In fact, I was extremely disappointed to refer to Figure 1 and find that little or no justification for these assemblages was provided in the text. Also, nowhere in the text are the PLATES (drafted obviously to illustrate these categories) referred to.

Section 5 - The authors again have little working knowledge of plant taphonomy. This is aptly demonstrated by their insistence of perpetrating the myth that leaves "become fragile due to the formation of an abscission layer." Yes, indeed, stomatal openings may be an avenue for bacterial and fungal attack, but if they were to read the biological literature on forest litter degradation, they would see that it's colonization of the leaf (leaf part) exterior by bacteria and their enzymatic activity that provides a foothold for fungal attack. The authors are obviously unfamiliar with the vast body of literature on degradation. The authors separate Root Remains (defined as tissues of roots and stems) from Stem Tissue as two categories of structured terrestrial material. What is the difference? Why is the latter within the definition of the former? They constantly state throughout the manuscript that "many workers" or "other workers" consider this or that, but never reference anyone! Who are these people? Again, they continue to make unsubstantiated statements such as "Much of this non-lignified material is probably derived from interfascicular cambium." How much interfascicular cambium is in a plant (proportional to other tissues), what is its probability of preservation, what experimental data do they (or anyone) have to support this statement, and why isn't it either referenced or the data provided? The same is true for two other statements made in this section.

Section 5:2 - Another elementary generalized and fairly rudimentary discussion on palynomorphs is included in this section. They also provide a contradictory statement concerning a purported inverse relationship between distance from shore and palynomorph abundance. The last sentence in this section is, again, one of those ideas that could be written at length about but is relegated to a very generalized, almost useless, statements.

Section 5:3:2 - They state that there are examples of this type of humic staining but do not support this by anything that has been documented. If it hasn't been documented, they should document it within the context of the manuscript. Why is the reader supposed to believe them on faith?

Section 5:3:3 - The authors state that fungi & bacteria will enhance the hydrogen indice of the sediment and/or plant material in the sediment. They attempt (unconvincingly) later in the paper to infer that this has occurred. Their discussion of their experiments, data collection, and introduction of materials leaves a lot to be desired. At this point in the manuscript, though, they make this blanket statement without any reference either to previously published literature or to anything that will be presented later in the manuscript.

Section 5:4:1 - This section is very confusing, highly disorganized, and contains some undocumented (and some outrageous) statements. How do the authors know that the fusinitized wood observed in mangrove peats (and, hence, already buried) wasn't burned before burial in the regenerated forest? They imply that the wood was converted to fusinite while it was buried in a wet environment. Don't peats dry out during droughts, and don't they burn upon ignition by a lightning strike? If they don't believe this they may want to read about the peat swamp fire in Kalimantan during the mid-1980's. The same is true of the outlandish and unsubstantiated statement concerning the conversion of logs to fusinite in a submerged forest. If this is the case, it should be documented and put to the test. It should not be presented as an anecdote. Do the authors have any experimental data to support their claim that fusinite is formed by oxidation (addition of oxygen from water?) in the tidal zone or that the winnowing effects of waves and currents can also result in the conversion of wood to fusinite?

Section 5:5 - This is the first time that sclerotinite (a coal maceral term that the authors suggest should be shunned yet they use) is introduced in the manuscript. The section doesn't mention or discuss the origin, the characteristics, or the implications of this type of palynodebris. Again, they push the unsupported statement that fungi will assist in the enhancement of hydrocarbon source rocks but don't place a timing constraint on this enhancement. Is it pre-burial, post-burial, pre-lithification, post-lithification?

Section 5:5:2 - They authors believe (and it appears only to be a belief) that the biodegradation of lignite by basidiomycetes will impart a distinct coloration to material adjacent to the site of decomposition. What is the scale of diffusion? Is it on the order of millimeters, centimeters? What is the concentration of this organic stain, and what is the diffusion range before the stain is in such small quantity that it becomes ineffective? Where are the data to support this?

Section 5:5:3 - They authors believe that mycorrhizal development could influence hydrogen richness in forested areas. Is this speculation or are there any data to support this? Have they, or others, run geochemical analyses in these forested areas to see what the IO:IH ratio might be for isolated fungal hyphae? The IO:IH ratio for isolated rootlets is extremely low and I would suspect, based upon hard data, that the IO:IH ratio for fungal mycelia is similar. Another question to be addressed is the maintenance of the cellular contents of fungi when the cellular contents of the host detritus is degraded?

Section 5:7 - Why don't the authors illustrate the large number "of living or dead bacteria" within the amorphous palynodebris? Has this been documented conclusively elsewhere? If so, why isn't this cited? In PLATE 4 it is difficult to tell from the photocopy if these are bacteria or some alteration product. Is there any biochemical evidence to support the interpretation? The authors state that "there can be little doubt that these amorphous materials are principally the result of bacterial breakdown...", but I have not been convinced that they have aptly demonstrated this either experimentally or in nature. They also provide a color guide relative to the Eh (without parameters defining low, medium & high) without acknowledging the role that pH could play. Can the authors draw a phase diagram plotting Eh vs pH where coloration of material can be plotted? Do the authors have any experimental data to demonstrate that "brown staining probably results from ..., thus be assumed to have been derived from higher plants." Is this a guess? On what basis is this statement made? Conjecture? They also state that these generalizations they have made do not apply universally. What, then, is the purpose of developing these criteria if they have no use?

Section 5:7:3 - The authors state that "under the influence of water currents (within which environments of deposition?), a gelatinous mass of amorphous material will break up into nearly spherical ... fragments ..." What experimental evidence do they have, or previously published results, to support this statement? What are hollows? The fragments accumulate in these, but the term is vague. Do the authors mean depressions (for example, ripple troughs)? How do these fragments "dry out" while resident in these hollows if these hollows are subaqueous? They are described as rounded (without details as to their morphology) and smaller (without any size reference), and they appear to be identical to dried algal spores. How are these told apart?

Section 5:8 - The 2 1/2 page paragraph should be broken down into appropriate-sized and topical sections. The authors state that some algal remains may be mistaken for remains of higher plants. What are the characteristics that might be confused, and what are the criteria that can be used to separate them? The authors provide no assistance to solving these problems. They also state that different microscopic techniques may be used to separate such materials, but again do not provide any criteria for the reader. Is this supposed to be a well-kept secret such that only the initiated are the ones privileged enough to learn of these criteria? Do these criteria exist at all?

Section 5:9 - The authors imply that diatoms thrive in damp soils. Do they have a reference for this?

Section 6:2 - Most of this section belongs scattered elsewhere in the text and that part which is repetitive should be dropped.

Section 6:3 - How do you determine *a priori* what is significant data? The authors also state the obvious to established researchers -- that "logging" must be "consistent for all samples involved in a specific study..."

Section 7:3 (Page 39) - Finally, there is a reference to a Figure!!

Section 9:1 - There has been volumes written on the interaction of microbes and degradation pathways and processes in the biological literature. None of it is utilized by the authors in this section. Why? Aren't they familiar with this literature? If they are but feel that it is too complex a subject to discuss in a short section, it should be omitted rather than giving it the substandard treatment they have included. What evidence (data or referenced citations) do the authors have to verify their statement that "organic matter subjected to bacterial alteration ... as much as one third of its weight in bacteria..."? Which residual compounds (in the broadest of sense; lipids & proteins) are good sources of hydrocarbons? Is this all based on "reasoning?" Are there any geochemical or biogeochemical data?

Sections 9:2 & 9:3 - The authors try to provide some data relevant (I think) to their ideas (?testing hydrocarbon enrichment via bacterial decay) but the amount of data that are provided the reader is insufficient to document their claims. For instance, where is the Konar Ar Basin in India? Is it Holocene, Tertiary, Jurassic in age? What are the sediments (rocks?) from which the samples were taken. Are these sediments lithified or unlithified? Is there any thermal maturation that might have affected the basin? The authors provide columns of the sampled cores but never describe the sediments nor the abbreviations (A-C) of biozones. What are these biozones? How are they delimited, what delimits these, and why do the authors refer the reader to a published abstract (where there can't be any detail of the investigation published) as the only citation relevant to this work? If the work hasn't been previously published, it needs to be before the authors can send a reader to that manuscript for data that is not included in this manuscript. The data were statistically treated - what statistics were used? How were they normalized? The discussion of the entire section is meaningless without defining what each of the zones is and what each represents. Their conclusions (pg 46) appear to be based on circular reasoning.

Section 9:4 - Why do the authors use the formula  $H(12)/C$  [(Hydrogen multiplied by 12) divided by Carbon] to devise a hydrogen richness ratio? Is this standard practice? As far as I know, the quantities obtained via pyrolysis, for example, of each of these elements is used without modification to determine a H:C ratio. They state that their regression line is a straight-line relationship, but the plot is widely scattered and they do not provide a Goodness of Fit statistic. What is the  $r^2$ ?

Again, the authors refer to sediments studied at Slide Brook, but give no data of this site, the sediments recovered, or anything that would provide the reader the necessary basic background information so that the reader can evaluate the proposal.

Section 9:5 - The opening statement in the section is an admirable goal, but the authors have provided nothing substantive within this manuscript to realize this aspiration.

Section 9:7 - The authors find (not surprisingly) that C/N ratios become lowered down hole. Without knowing where the Konar Ar Basin is actually located and the influence of man's agricultural practices that might be affecting the pore water chemistries, it is impossible to know how good their data set might be. Do the authors have any C14 data on the recovered palynodebris to determine how old the plant parts might be (preman)? Are the sediments Holocene, Pleistocene, or PrePleistocene?

These detailed comments could continue for another several pages, and on the manuscript you can read the continuing commentary. On page 62 of the manuscript (where I have thrown up my hands in exhaustion) I raise the point that - "I don't believe that you [the authors] have proven this point [that palynodebris analysis allows one to obtain a more detailed interpretation of paleoenvironments]." The authors have not addressed the topic that is stated in their title. In addition, they have provided such generalized, unsupported, and nebulous discussion that the credibility of the authors is brought into question. They have demonstrated a very poor understanding of palynodebris taphonomy and genesis, and they are supposed to be some of the authorities in the field. The manuscript is very, very, very disappointing and would detract from the scope of the book. I can't recommend the acceptance of this manuscript and would urge you to have any rewritten draft exhaustively reviewed by others.

Bob Gustaldo

The Pennsylvania State University  
Department of Geosciences  
Palynological Laboratories  
435 Deike Building  
University Park, PA 16802  
phone: 814-863-3419  
fax: 814-865-3191

12 October, 1990

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

It was neat to have you here! We all profited and hope you enjoyed it too.

Unfortunately, I have had a difficult time since then. I spent some quality hours with Mother last week. Then flew to MI and loaded up furniture and drove my truck home through terrible rainstorms (some snow!). No sooner back here than Mother suddenly died, and Betty and I are getting ready to go right back out there for her funeral, etc.

I look forward to hearing from you about Pocock et al. Habib says he thinks, if completely rewritten, a chapter could come out of it. I'm going to see him in a couple of weeks about it (and his chapter)--so, your appraisal, proposal, etc., will be very helpful.

Also, of course, we need your revised and augmented chapter.

Yours very truly,

Alfred Traverse

AT/et

Fax to: Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36849-5305

FAX NO.: 205-844-5748

Schedule for visit to Penn State of  
Dr. Robert A. Gastaldo  
of Auburn University

Mon., 1 Oct. 3:00 p.m. Dr. Gastaldo arrives, State  
College/Univ. Park airport, USAir. Will be met by Dale  
Beeson (A. Traverse has 2:30-3:20 class).

Rest of afternoon w. A. Traverse, Deike 435.  
ca. 6:30 p.m. Dr. Gastaldo has dinner at Tavern  
Restaurant w. Carl Keener, A. Traverse, and guests.

Overnight at home of Traverses, Huntingdon Co. (phone  
643-1958).

Tues., 2 Oct. Morning w. A. Traverse in his office.

11:30 a.m. <sup>RAG</sup> Dan Cosgrove, Richard Cyr, A. Traverse meet  
in 435 Deike, go to Nittany Lion Inn for lunch.

ca. 2:45 p.m. Head for Mueller 8 to set up for lecture  
and sign voucher in Shelly Stephenson's office.

3:30 p.m. Mueller 8, refreshments and seminar:  
"Plant taphonomic processes operating in terrestrial coastal  
environments: examples from the Gulf Coast and Indonesia."

6:30 p.m. AT and RAG join Roger Cuffey, Bill Duke,  
Jennifer Robinson in Deike parking lot to go to Gamble Mill,  
Bellefonte, for dinner, after which RAG and Traverses go to  
Traverse home for overnight.

Wed., 3 Oct. RAG with AT in his office, and/or with interested  
graduate students in their offices.

12 m. RAG and AT to Allen Room for lunch with Alan  
Davis, Eric Barron, Rudy Slingerland. Early afternoon ad  
lib.

3:15 p.m. Set up for lecture in Deike 22.

3:30 p.m. Refreshments before lecture.

3:45 p.m. ESSC lecture by RAG: "The utilization of  
plant macrofossils in interpreting sedimentary facies and  
time: examples from the Carboniferous Warrior basin,  
Alabama."

5:15 p.m. Carmen Moy takes RAG to airport.

# BIOLOGY SEMINAR

**TITLE:** Plant Taphonomic Processes Operating  
In Terrestrial Coastal Environments:  
Examples From The Golf Coastal  
Plain and Indonesia

**SPEAKER:** DR. ROBERT GASTALDO  
Department of Geology  
Auburn University  
Auburn, Alabama

**DATE:** Tuesday, October 2, 1990

**TIME:** 4:00 p.m.

**LOCATION:** 8 Mueller Lab.

Refreshments will be served at 3:40p.m.  
in 8 Mueller prior to the seminar.

If you would like to set up an appointment or  
visit with Dr. Gastaldo, contact Dr. Al  
Traverse at 863-3419

PENNSSTATE



Hunt Institute for Botanical Documentation

*ESSC Seminar*

---

"THE UTILIZATION OF PLANT MACROFOSSILS  
IN INTERPRETING SEDIMENTARY FACIES AND TIME:  
EXAMPLES FROM THE  
CARBONIFEROUS WARRIOR BASIN, ALABAMA"

Robert A. Gastaldo

Auburn University  
Department of Geology

Wednesday

October 3, 1990

3:45 P.M.

22 Deike Building

Coffee at 3:30 P.M.

Hunt Institute for Botanical Documentation

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

11 September, 1990

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Your MS was sent for review to a well-known researcher in the general area of palynodebris. Then I independently reviewed the MS before consulting x's work. At that point I often seek a third review, if there looks to be a problem. That did not seem to be the case here.

However, I do have some important comments that I hope you'll address. I note that they are, for the most part, along the lines of the reviewer's, though I am a bit more critical than he.

First, I think it is essential for you to take a little time to come up with some sort of diagrammatic figure (I know from your previous work that you're good at it!) to stand up front in your chapter to show the fate of different tissues, etc.--with arrows--rather along the lines of, say, the oxygen cycle, or sources and dispersal of the gross national product. This would greatly help and catch the interest of the reader. As it is, one reaches p. 10 with nothing to look at. I think that is psychologically undesirable.

In your writing, I have trouble with long, modifying phrases and incessant and repetitive overuse of "these," "those," and "the," among a number of what seem to me to be glitches in writing. If you'll give me license (you'll be final arbiter, of course), I can smooth and condense some of the language, without hurting the sense at all.

So, I'm sending you the pages from both reviews that have editorial markings, plus the reviewer's letter. Please address all of the points. Maybe you could bring the revised MS here with you in three weeks? Perhaps even a preliminary draft of the additional figure? Also, we'd like a diskette containing your MS (IBM-compatible; Word Perfect 5.0 or software easily convertible to same preferred). This would ease our final editorial work considerably.

I believe the reviewer makes a good point about abstracts. Maybe you could produce one? I believe I might get back to various

authors, suggesting they produce abstracts ex post facto, or give me license to have a go at writing my own precis (which was what I intended to do for all), to be placed at the head of each chapter. You can help me by discussing that when you're here.

All the best.

Yours very truly,

Alfred Traverse

AT/et

encl: 1) xeroxed pages with comments from reviewer;  
2) " " " " " " AT  
3) " letter from reviewer

**AUBURN UNIVERSITY  
AUBURN, ALABAMA 36849  
FAX INFORMATION COVER SHEET**

**FAX NUMBER: 205-844-5971**

**DATE: 15 August 1990**

**NUMBER OF PAGES IN THIS TRANSMISSION: 1**

**TO: Dr. Al Traverse**

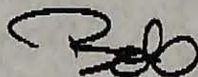
**ADDRESS: 435 Deike Building - Department of Geosciences - The Pennsylvania State University - University Park, PA 16802**

**FAX NUMBER: 814-865-3191**

**FROM: Dr. Robert A. Gastaldo  
Department of Geology  
210 Petrie Hall  
Auburn University, AL. 36849-5305**

**COMMENTS: Dear Al,**

Here is my Social Security Number (149-36-1952) and my home address (1137 Owens Road, Auburn, AL. 36830) as you requested for accounting purposes for my forthcoming lectures. See you in October. Best Regards.



THE PENNSYLVANIA STATE UNIVERSITY

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

3 August, 1990

Dr. Robert A. Gastaldo  
Department of Geology  
210 Petrie Hall  
Auburn University, AL 36849-5305

Dear Bob:

Yours of 26-VII-90 in hand. Everybody is looking forward to your visit! I'll meet you at the airport on Mon., 1 Oct., at 3:30 p.m. (presumably USAir, though United now also flies here). The rest of that day you're mine! There will be perhaps a little time to work with you on your MS, about which you'll hear from me very soon. You'll stay with us at "Alphabet."

Tues., 2 Oct., you speak to Biology at 3:30 p.m. per your title, "Plant taphonomic....Indonesia." Earlier that day we'll visit some of the botanists (the new president of PSU, Dr. Thomas, is a botanist, as you doubtless know--he was pres. of U.A.). That evening we'll have a nice dinner in a "typical" local restaurant, with a few people who'll want to meet you. Tues. a.m. I hope we can talk about your chapter and other parts of the nascent book. On Wed., 3 Oct., I'm told by ESSC you'll speak to them at 4:00 p.m. That should wind up about 5:00 and we can easily get you to the airport in time for your 5:50 flight.

Somewhere along the line I would like you to visit with my graduate students--maybe we can set that up for late afternoon on the 1st, after your arrival. We'll work on it. Could you provide your s.s.# and home address, please?

All the best.

Yours very truly,

Alfred Traverse

P.S. My palynology class will be required to hear at least one of your lectures!

AT/et

# Auburn University

Auburn University, Alabama 36849-5305  
College of Sciences and Mathematics

Department of Geology  
210 Petrie Hall

26 July 1990

Telephone: (205) 844-4282  
ATTNet: 221-4282

Dr. Eric J. Barron, Director  
Earth System Science Center  
College of Earth and Mineral Sciences  
The Pennsylvania State University  
College Park, PA 16802

Dear Eric,

I apologize for replying late to your letter of 22 May concerning my forthcoming visit to Penn State. I have written Al Traverse about my lecture in the Biology Department and have scheduled an additional day in order to present a seminar at the Earth System Science Center on 3 October. I have scheduled my departure from State College at 5:50 pm EDT so that the lecture can be set for either the morning or afternoon. I have indicated to Al that I would like to talk about plant taphonomic processes in the Biology lecture. Therefore, I would like to discuss "The Utilization of Plant Macrofossils in Interpreting Sedimentary Facies and Time: Examples from the Carboniferous Warrior basin, Alabama" at the Center's colloquium. I hope that this will be amenable to your schedule.

I appreciate the opportunity to discuss my research with your group and look forward to meeting you in the near future.

7:00 pm  
3:10 am

5-6571  
22 Deike  
3:30  
5:30

ESSC Seminar

10/3/90

22 Deike

3:45 p.m.

Coffee @ 3:30 p.m.

Sincerely,

Bob

Robert A. Gastaldo  
Professor of Geology

# Auburn University

Auburn University, Alabama 36849-5305  
College of Sciences and Mathematics

Department of Geology  
210 Petrie Hall

Telephone: (205) 844-4282  
ATTNet: 221-4282

26 July 1990

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

Dear Al,

I apologize for replying to your 22 May letter so late. It's been one of those summers. I would be delighted to spend a few days in University Park and would like to present results of my research both to the Biology Department and the Earth System Science Center. I have written Eric Barron separately about the other lecture. The title of the lecture I have chosen to give on October 2<sup>nd</sup> is:

*Plant Taphonomic Processes Operating in Terrestrial Coastal Environments: Examples from the Gulf Coastal Plain and Indonesia.*

With regard to my arrival schedule, I have contacted our local travel agent and have made the following reservations. I will arrive in State College\University Park on Monday 1 October at 3:00 pm. I have scheduled departure late Wednesday afternoon at 5:50 pm. If there is any difficulty with this particular flight schedule, I could change the reservations to leave the following morning (I have found a reasonable fare on USAir).

Again, I appreciate this opportunity to visit Penn State and bring a different kind of paleobotanical research program to the attention of your students. I am looking forward to being able to spend some time with you and your laboratory.

Sincerely,



Robert A. Gastaldo  
Professor of Geology

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

22 May, 1990

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Andrew Clark of the Biology Department, Chairman of the Seminar Committee, has authorized me to send you his department's invitation to present a lecture on Tuesday, 2 October. We think all four of the topics you suggested in your earlier letter would be satisfactory. I would think that "Plant taphonomic processes...tropical and temperate Holocene deltas" would be especially apt. If you have still another idea that you believe would be better, that would be o.k. too.

The Biology Department will cover your travel expenses plus a \$100 honorarium. You will be staying with us, if that's o.k. (the department would also cover a room for you if you prefer).

When you get around to it I need:

1. Your social security number.
2. Title of talk you are presenting.
3. Details of your travel plans. I'll meet you at the local airport (State College/University Park), which is now served by many daily flights via USAir Express and United Express.

All the best.

Yours very truly,

Alfred Traverse

COPY

AT/et  
c: Andrew Clark

*This is a copy of a handwritten note mentioning that he would want to present a talk on the subject of...*



May 22, 1990

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
210 Petrie Hall  
Auburn University, AL 36849-5305

Dear Dr. Gastaldo:

I understand from Al Traverse that you are planning to visit Penn State on October 2 to give a seminar in the Department of Biology. Since much of your research is of interest to the Earth System Science Center and the Department of Geosciences, I would like to extend an invitation for you to visit either on October 1st or the 3rd and to present an additional seminar. Either of the following two seminar topics that you suggested to Al would be very enjoyable (a title of your choice would also be welcome): "The Utilization of Plant Macrofossils in Interpreting Sedimentary Facies: Examples from the Carboniferous Warrior Basin, Alabama" or "Plant Taphonomic Processes in Coastal Sedimentary Regimes: A Comparison of Tropical and Temperate Holocene Deltas".

If you can arrange to spend an extra day at Penn State, the Earth System Science Center would be happy to provide a modest honorarium in appreciation of your time and effort.

I look forward to hearing of your interest.

Sincerely,

Eric J. Barron, Director  
Earth System Science Center

/ded

cc: A. Traverse  
D. Egglar

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

22 May, 1990

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Andrew Clark of the Biology Department, Chairman of the Seminar Committee, has authorized me to send you his department's invitation to present a lecture on Tuesday, 2 October. We think all four of the topics you suggested in your earlier letter would be satisfactory. I would think that "Plant taphonomic processes...tropical and temperate Holocene deltas" would be especially apt. If you have still another idea that you believe would be better, that would be o.k. too.

The Biology Department will cover your travel expenses plus a \$100 honorarium. You will be staying with us, if that's o.k. (the department would also cover a room for you if you prefer).

When you get around to it I need:

1. Your social security number.
2. Title of talk you are presenting.
3. Details of your travel plans. I'll meet you at the local airport (State College/University Park), which is now served by many daily flights via USAir Express and United Express.

All the best.

Yours very truly,

Alfred Traverse

AT/et  
c: Andrew Clark



# The University College of Wales Aberystwyth

University of Wales

## Institute of Earth Studies

Aberystwyth, SY23 3DB, UK

**Director:** Professor D Q Bowen

**Head of Geography:** Dr C R Lewis

**Head of Geology:** Dr M R Dobson

Professor J W Aitchison

Professor R A Dodgshon

Professor J R Haynes D.Sc

Professor J A Jacobs

Professor J Lewin

Professor M J O'Hara FRS

Professor J A Taylor

Professor R C Whatley

Professor Dennis Wood

Tel: (0970) 623111

Direct line: 622573

Fax: 0970 622659

Telex: 35181 ABYUCWG

8th May, 1990.

Professor A. Traverse,  
The Pennsylvania State University,  
Dept. of Geosciences,  
Palynological Laboratories,  
435 Deike Building,  
University Park,  
PA 16802, USA.

Dear Alfred,

The genesis and sedimentation of organic 'nannodetritus'

by R.A. Gastaldo

This is an interesting and useful paper - if a bit verbose. I felt I could have done without some of the section on the "fate of organic detritus" and some of the discussion at the end, both of which partly repeat what is said in the long section starting on page nine. I found my attention wandered in places, despite my interest in the subject matter.

I have made a few minor corrections and suggested some similarly minor alterations directly on the manuscript. There is no need for me to refer to these here. I think it is a pity that the editorial policy has been "introduction only - no abstracts". I felt this paper could have done with an abstract; so could the other MS I have just refereed and my own contribution with Warren Kovach!

More important points and questions for Bob Gastaldo to consider are as follows [numbers refer to points indicated on the MS]:

1. I am really not very happy with the word "Nannodetritus". For a start, from the etymological viewpoint it would be better spelt "Nanodetritus", but on top of this, the implication is that extremely small (nanoplankton-sized) particles are going to be discussed whereas, in fact, this is not the case. I would prefer the title to read "... sedimentation of microscopic organic detritus" or even "... of phytoclasts". For the most part it is phytoclasts that are being discussed. I see no need for another term, especially one that has misleading connotations.

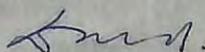
2. I also wonder whether the title shouldn't be made more specific because,

as noted on p.2, it is principally the constituents of terrestrially-sourced detritus that are discussed, and there is no consideration of environments other than those relating to deltaic and shallow, near-shore marine deposits.

3. The section "fate of organic detritus" is unrelieved by any subheadings, and the title itself could be more informative, especially since the next section is headed "fate of phytomacrodetritus ..." which is also organic detritus. I think it would be useful if a few subheadings could be inserted so that the general areas of discussion are immediately obvious to the reader. At present it is necessary to wade through the text to find out.
4. What are the polymers and in what groups of plants do they occur? Such a loss through degradation could bias the potential fossil record, but how? I don't feel I have learned anything here.
5. The emphasis is supposed to be on "nanofidetritus" so I think this heading should be modified. In it you explain how phytomacrodetritus (ugly word; is it necessary?) can be degraded and eventually reduced to microscopic particles, so why not say so here?
6. Surely the formation of pyrite framboids is only one of the biproducts of bacterial processes, in this case via sulphur-reducing bacteria.
7. Can you actually see the organic particles in the sediment (not environment); might not some/much of the coloration be owing to the presence of heavy/opaque minerals? The low TOC suggests this might be the case.
8. Isn't the generation of bacterially-induced methane likely to be related to lowered rather than raised oxygen levels?
9. Nipa is the correct generic name; nipah is a Malay word for the same thing.
10. Why should they show signs of organic maturation? In what way? This surely cannot be in the usual geological sense which implies thermal alteration?
11. I have looked at a great many palynofacies preparations but have hardly ever seen resinous material that I would consider has an amorphous aspect. I can think of a few freshwater samples that have yielded yellow flakes of varying shapes and sizes which are, I suppose, more or less amorphous, but little else. Small resinous droplets and lumps are more common.
12. I have made the point several times that in palynological preparations charcoal is often splintery (e.g. in Batten 1973, Palaeontology 16, p. 26 and in 1981 - same volume as for Cope '81, cited above).
13. Black bits which may well be charcoal are present in rocks as old as early Devonian (I have seen them!).
14. Why do you need to make this word plural? (see below and elsewhere in the text).

Hope this is O.K. Recommended for publication after minor revision.

Best wishes,



D. J. BATTEN

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

I N T E R O F F I C E C O R R E S P O N D E N C E

Date: 11 April, 1990

From: Alfred Traverse, Professor of Palynology, 435 Deike

To: Steve Schaeffer, Assistant Professor of Biology,  
507 Mueller

Linda Maxson tells me that you are now in charge of the Biology Seminars schedule. You may know that I hold a joint appointment with Biology.

I have a colleague at Auburn, Bob Gastaldo, who is doing very interesting things with plant fossils, especially in terms of their paleoecological significance. I would like to arrange for him to visit Penn State and give a couple of seminar lectures. Earth System Science Center over here will sponsor one. If we can find a suitable date, it would be nice to have him speak under Biology Dept. auspices also. The honorarium would also help a bit with the expenses. I enclose a list of potential subjects. Any of the titles can be given a more biological slant (these are aimed at ESSC):

ESSC is more free with dates, so if you are agreeable to the proposition, could you suggest a couple of dates when it would be possible--presumably next Fall?

Best.

AT/et

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

21 March, 1990

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

As I write we are negotiating the 25 miles over hill and dale to work (you'll be relieved to hear my wife is driving), and I am reading...your reprints. I'm lecturing later this a.m. on, among other things, Artisia, so your log paper was very interesting for that reason alone. (The acknowledgment of a "swamp goddess" aroused prurient interest. Hmm.) The article on the back re Ediacaran stuff was good serendipity! The taphonomy of Mobile Delta plants is fascinating. "Biostratinomic" was a new word for me. Enjoyed the paper. Thanks for all.

Yours very truly,

Alfred Traverse

AT/et

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

20 March, 1990

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

This is finally an answer to yours of 2 January. Today is my first day back to the office after the latest flu relapse. It's a little daunting that my medical group this time referred me to the gerontologist, a lass of maybe 29, who told me that I wasn't allowing for the fact that people my age don't bounce back as they used to. Hmm. By the way, how are you now?

Thanks very much for the review of Caratini's MS. I'm now back to him with request for revision--I sent a xerox copy of what you said, but I blocked off the identifiable parts, in case you didn't want Claude to know who did it.

Now I can also get at the seminar business. I would think we ought to try to get Biology to support one, and Geoscience/ESSC another. More about that a little later. Unwritten agenda: impress PSU people with the value of having a paleobotanist here.

As of today your MS hasn't arrived--maybe tomorrow!

Best regards.

Yours very truly,

Alfred Traverse

P.S. If you have a fax number, could I have it?  
I'll comment on the reprints later--many thanks for them!

AT/et

# Auburn University

Auburn University, Alabama 36849-5305  
College of Sciences and Mathematics

Department of Geology  
210 Petrie Hall

Telephone: (205) 844-4282  
ATTNet: 221-4282

15 March 1990

Dr. Al Traverse  
The Pennsylvania State University  
Department of Geology  
Palynological Laboratories  
435 Deike Building  
University Park, PA 16802

Dear Al,

As per our conversation, you will find enclosed a manuscript entitled "The Genesis and Sedimentation of Organic 'Nannodetritus'" composed of about 17,000 words and 10 figures (20000 words and 10 figures maximum as in the contract). I hope that it fulfills the bridge in the book. When the chapter has been reviewed, please just send the text corrections and retain the original figures.

I look forward to hearing from Eric Barron and anticipate the possibility of coming to Penn State for a lecture. See you soon.

Best Regards,



Robert A. Gastaldo  
Professor of Geology

# Auburn University

Auburn University, Alabama 36849-5305

College of Sciences and Mathematics

Department of Geology  
210 Petrie Hall

Telephone: (205) 826-4282  
ATTNet: 221-4282

2 January 1990

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

Dear Al,

Please find enclosed a reviewed manuscript for Caratini's contribution. I have spent a considerable amount of time on the grammar and content of the manuscript in order to get the most out of the review process. I understand how difficult it is, at times, to get the ideas across in English. I have some overall comments concerning the manuscript:

1. I think that there more could be elaboration on the results of the different OROGEN studies. I have copies of some of these and even in the originals, the documentation and reporting style is brief - almost too brief. A lot of the time the data are presented and discussed without any synthetic depth. I think that the book provides the opportunity for synthesis of the data and development of its significance.
2. It would benefit the manuscript to delve into the reasons for the palynomorph component maturation (not evolution) during transport. Caratini mentions this fact several times that by the time these plant parts are undergoing sedimentation, they have already reached a state of maturation. What are the biological/biogeochemical processes responsible for this maturation? How far are they matured? Will this topic be considered by any other author? If so, will the phenomena reported herein be discussed specifically? If not, something should be added to this manuscript.
3. With respect to assumptions made on page 5 - Caratini believes that the amorphous matter is very fragile and altered during transit. What criteria does he use to believe that these are "fragile?" If the material is resistant to acid treatment, how fragile can it actually be? If this extremely small-sized material actually moves in suspension load, how will it be broken (mechanical fracture I guess) during transport? Is it a function of animal feeding mechanisms? Is it a function of pressure at depth? How does it "disappear" along the way? I believe that this needs to be addressed.
4. A discussion is presented, but data not provided, for a decrease in pollen seaward with a corresponding increase in dinoflagellate cysts (pg. 6).
5. In the section on the Kayar canyon, Senegal, sedimentary facies relationships are not presented relevant to the palynofacies. In fact, in most instances, we have no idea as to the sedimentological context from which these samples have been recovered! In this instance, from where in the core is the sample taken? Is it in the turbidite sequence and, if so, from which part of the turbidite sequence? There is a large difference between the various parts of a typical Bouma sequence, and palynomorph distribution may be affected by its distribution.
6. I think that a summary paragraph presenting the described relationships should be included. *(at the end)*
7. I did not find several references cited in the text (see pink highlighter), and several references missing.
8. I believe that a bar scale would be appropriate on the photographs, and an expanded explanation provided for Fig. 2, pts. I, II, III, IV.

I wasn't able to get as much accomplished over this Christmas break as I had hoped. There was/is some sort of virus and bacterial infection sweeping the town and we (note the plural) have been hit with one of the infections. I am still in the process of getting it completed and, without any foreseeable obstacles, I should have the manuscript to you sometime this month.

With regard to possible seminar topics, I can offer the following:

**Sites of Organic Carbon Deposition in a Tropical Coastal Delta: The Mahakam River Delta, eastern Kalimantan, Indonesia.**

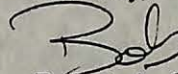
**The Utilization of Plant Macrofossils in Interpreting Sedimentary Facies: Examples from the Carboniferous Warrior basin, Alabama.**

**Plant Taphonomic Processes in Coastal Sedimentary Regimes: A Comparison of Tropical and Temperate Holocene Deltas.**

**Sedimentological Aspects of Plant-Part Preservation: Case studies from the Carboniferous and Holocene.**

I believe that one or more of these might be of interest to the various groups on campus. I hope that you and your family spent a joyous holiday season and wish you the best for the coming decade.

Cordially,



Robert A. Gastaldo

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

20 November, 1989

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Good to hear you the other day.

Here's a xerox of the Caratini MS for review. I have not read it myself yet. From skimming it seems a bit meagre, but that's o.k., if it's good enough.

Glad to know that the "nanodetrinitis" paper is coming along. I look forward to getting it.

When you find a few minutes, let me have a couple of suggested titles for seminar you could give here. The Earth System Science Center needs talks on organic matter/environment interfaces, but I could also arrange more conventional geology through the Geoscience Dept., or various aspects of paleobotany through the Biology Dept. Eventually I am hoping I could convince people here to revive paleobotany at PSU. So, I want to keep its existence before the public here.

All the best to you.

Yours very truly,

Alfred Traverse

AT/et  
encl: MS xerox

THE PENNSYLVANIA STATE UNIVERSITY  
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7 June, 1989

Dr. Robert B. Cook  
Department of Geology  
210 Petrie Hall  
Auburn University  
Auburn, AL 36849-5305

Dear Dr. Cook:

Yours of 16 May was here when I returned yesterday from a six-week absence.

I endorse most heartily the idea of a University Professorship for Robert Gastaldo. He is a dynamic researcher whose relative freedom from onerous duties will result, I am sure, in accomplishments in science that will far outweigh what this new status for him will cost Auburn. Dr. Gastaldo's research in all aspects of organic sedimentation is of great importance and is already recognized worldwide. He also finds time even now to write widely in paleobotany and in allied aspects of paleontology. I use the book he organized on fossil plants extensively in my paleobotany course here at Penn State. He is currently producing a chapter for my forthcoming book on palynomorph sedimentation, and I was delighted to get the assistance of such a busy and talented man. Bob Gastaldo will be a credit to Auburn's select group of University Professors.

Because this letter has already been delayed by my absence, I will conclude here with the hope that my brevity does not obscure my enthusiastic support for the nomination.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

# Auburn University

Auburn University, Alabama 36849-5305  
College of Sciences and Mathematics

Department of Geology  
210 Petrie Hall

16 May 1989

Telephone: (205) 844-4282  
ATTNet: 221-4282

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

Dear Dr. Traverse,

I am writing to you to enlist your support for the nomination of Dr. Robert Gastaldo, Professor of Geology, to a University Professorship at Auburn. The University Professorship is a pre-eminent faculty rank established to attract or retain eminent scholars who have received national and/or international prominence as a distinguished scholar in their field. The sole responsibility of a University Professor is to continue his/her professional activities at the same high level evident at the time of his/her appointment. The University Professor is expected to teach a light load of their choosing although they may also elect, in any given term, to devote all of their energies to research, scholarship or extension activities. The rank of University Professor is retained by the faculty member until resignation or retirement from the University.

Dr. Gastaldo's scholastic achievements during his tenure at Auburn are numerous. This is especially noteworthy in light of very heavy teaching loads (12-18 contact hours per week) during his early career in the department. In the early 1980's, he was awarded a Fulbright Fellowship, and recently was a visiting professor at the University of Paris-Sud, Orsay, and the Institut Français du Pétrole, Rueil-Malmaison. Dr. Gastaldo has been a visiting curator and research collaborator at the Chicago Field Museum and the Smithsonian Institution, respectively. He has assisted in developing a paleogeographic-based coal exploration program for BHP-Utah International. Additionally, his extramural funding has been awarded by highly competitive granting agencies including the National Science Foundation and the American Chemical Society. Since his employment in the Department of Geology, he has published over 45 manuscripts, 35 of which are in national/international refereed journals or proceedings. He has been the most professionally active faculty member in the Department, and the most successful, to date, in grantsmanship. He is recognized as one of the most active researchers in the College of Science and Mathematics. Outside of university-related activities, he is an elected member of the Auburn City Council, and participates as a coach in various recreational programs. Enclosed is a copy of the Curriculum Vitae that will be submitted along with his nomination.

The nomination of Dr. Gastaldo for a University Professorship does not, of course, guarantee his appointment. The recommendation decision from the Committee for appointment is based largely upon the letters of support submitted by colleagues knowledgeable about the professional contributions of the applicant. This is where I need your assistance. I would like to submit a full dossier to the Vice President for Academic Affairs early this summer. Therefore, if you would be willing to write a letter of support for Dr. Gastaldo's University Professorship nomination, I would appreciate receiving it during the early part of June. Thank you for your time and consideration in this matter.

Sincerely,



Robert B. Cook, Head  
Professor of Geology

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PALYNOLOGICAL LABORATORIES  
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6 February, 1989

Dr. Robert A. Gastaldo  
IFP  
B.P. 311  
92506 Rueil-Malmaison Cedex, France

Dear Bob:

Yours of 20 Jan. just arrived.

Your title and abstract are super. I would only suggest a slight change in the title: "Nanodetritus: Genesis and Incorporation in Sediments." It seems to me that "incorporation" calls for an explanation of "in what?". Maybe you've had the letter from me with a copy of a letter from Cambridge UP. I hope they'll take us. If not, I have back-up plans. Author directions, as I explained in that letter, must wait until we're official somewhere. I don't think this need slow us down too much, as the things affected are usually minor, and nowadays can be handled by the word processor easily.

All the best.

Yours very truly,

Alfred Traverse

AT/et

I.F.P.  
B.P. 311  
92506 Rueil Malmaison Cedex  
France

Department of Geosciences  
Palynological Laboratories  
435 Deike Building  
University Park, PA 16802

A l'attention de Prof. Traverse

Rueil, le 20 January 89

Vos ref : 14 December 88  
Nos ref : 28 December 88

Messieur,

In response to your letter, please entertain the following as the title and abstract for my contribution:

GENESIS AND INCORPORATION OF NANODETRITUS

The term "nanodetritus" is herein defined as those small and generally unidentifiable fragments of terrestrial plant parts. This material is often found concentrated along bedding surfaces of various siliciclastic depositional environments. Additionally, it may be distributed within sedimentary environments providing the source materials for potential hydrocarbon generation. Genesis of this detritus is principally the result of mechanical fragmentation under shallow water conditions. Examples of these biostratinomic processes in Holocene siliciclastic regimes are discussed and collaries in the ancient rock record are presented.

I hope that this abstract will be appropriate for your purposes. If you feel that you need to add, modify, or change anything in the abstract to better sell the prospectus, feel free. You are ultimately the Editor. I'm looking foward to receiving the style instructions.

Best Regards



R.A. Gastaldo

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28 December, 1988

Dr. Robert A. Gastaldo  
IFP  
B.P. 311  
92506 Rueil-Malmaison Cedex  
France

Dear Bob:

Thanks so much for yours of 14 December from France. I am delighted you can do a chapter for the palynomorph sedimentation volume. It will be a book, published probably by Cambridge University Press. I enclose a preliminary proposal, outline, etc., that recently went out to the already (more or less) committed authors.

I would think in terms of your chapter going right after Batten & Kovach, as a new II D 3. I believe you should cover both genesis and sedimentation of "nanodetritus", including your actuo- studies and fossil examples, but I want you to feel free to "do your own thing." Your idea as to time-frame is fine: late May, 1989.

I now need a title and a (very general) abstract, from which to write a precis for the revised proposal.

As you can see from the enclosures, I will be sending out instructions to authors as soon as the question of publisher is settled.

All the best to your for 1989. It will be hard for you to match your 1988 performance!

Yours very truly,

Alfred Traverse

AT/et

encl: recent mailout to prospective book contributors

IFP  
B.P. 311  
92506 Rueil-Malmaison Cedex  
France

The Pennsylvania State University  
Department of Geosciences  
Palynological Laboratories  
435 Deike Building  
University Park, Pa 16802

A l'attention de M. Traverse

Rueil, le 14 December 88

Vos ref : ?  
Nos ref : ?

Dear Al,

I received your letter dated 30 November today and in as much as my family and I are leaving to spend the Holiday Season with my relatives in Italy, I thought that I would respond to your request. I would be very pleased to contribute a paper on "nanodetritus" to your symposium volume. Is this going to be a special issue of a journal, or is it going to be a book? Whichever, if you could send me the specific details on manuscript preparation (editorial suggestions), anticipated ms length, anything in particular you would like me to address (the genesis of nanodetritus and incorporation into the sedimentological record?), please let me know soon. I will have to complete the manuscript when I return to Auburn in early March. I believe that I can have the finished manuscript to you sometime in May, but I can't guarantee it by the beginning of May. As you know, when I return to the lab all hell is going to break loose with my 4 graduate students lying in wait for their major professor who has been off for the past 5 months. Two of them are completing their M.S. theses, the other two will have beaucoup data to show me. I know that you can understand. If this nebulous time schedule is alright with you, I'll do my best.

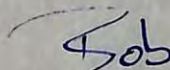
The Mahakam samples from all but the well protected depositional sites have quantities of nannodetritus. In some cases I'm sure that it is the result of field sieving by our Indonesian assistants, but in most cases (as determined from field observation) the nannodetritus has been deposited within the bed. The Mahakam is heavily tidally overprinted. In lateral distributary channel bars 50 km upstream, there are tidal mud-drapes alternating with ripples fluvial sands. In these sites and those of the delta front, beds of nannodetritus occur. The fragmentation of the plant material is due to its reworking after transport from the site of introduction into the system. I don't think that the mechanical breakup is a one shot affair. Rather, the saturated and already weakened macrodetritus is subjected daily to wave and tidal fluctuations. This constant

agitation over time provides the impetus for fragmentation. I'm going to Orsay University tomorrow to examine some of the detritus under SEM to see to what degree biodegradation has played in the problem.

In any case, this mechanical breakup of macrodetritus to micro- and nannodetritus is very apparent in the development of the detrital peat beaches on the delta headlands. There is a slurry of plant detritus within the swash zone that is worked by wave action providing the mechanism by which stratification in the peat beaches occurs. The beaches are constantly being built up and eroded, resubjecting the plant materials to mechanical fragmentation. This became obvious when we visited the same peat beach two days in a row. On the first day there was a large tree base embedded within the peat beach near the beach/water interface. There was no storm that evening so there wasn't any particularly rough seas. The tidal cycle was in the middle of the Spring Tides, and the winds were moderate. The next day upon our arrival the tree was standing high and dry, no longer embedded in the peat beach but completely uncovered. A local orang from the fishing village had stacked his sacks of detrital amber on top of it. Meanwhile, towards the landward end of the beach a spit of detrital peat was developing. It's obvious that unless quickly buried in clastics, the organic detritus is vulnerable to mechanical fragmentation.

Thanks for the opportunity to participate in the symposium proceedings from Brisbane. I had hoped to attend the IOP and the Palynological conference but time wasn't on my side. I had to give up an opportunity to attend a field meeting in Kiev for the Subcommittee responsible for the selection of the Mid-Carboiferous Boundary section in late September. If I had gone, I would have had only 6 days to complete my preparations for Indonesia before departure. When I return to Auburn I'll check my agenda and propose several possible dates for a seminar at Penn State.

Best Regards



R.A. Gastaldo  
Professor of Geology

THE PENNSYLVANIA STATE UNIVERSITY  
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435 Deike Building  
University Park, PA 16802  
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30 November, 1988

Dr. Robert Gastaldo  
Institut Français du Pétrole  
B.P. 311  
92506 Rueil-Malmaison-Cedex, France

Dear Bob:

Thanks for yours of 8 November. I have arranged for all the appropriate forms and info re graduate admission to go to Mr. Li. As you say, it's not possible to assess his chances without the inimitable numbers (though I find they correlate poorly with research results later!).

The rest of your letter was fascinating. I am putting together a volume on palynomorph, "palynodebris", etc., sedimentation, based on a few of the papers from "my" symposium at Brisbane, but also including a number of other chapters. Do you think you could pull together a short chapter on "nano detritus" and its significance per your results? I'd need a MS by about next May. You could speculate a bit more than in a journal article! Let me know asap.

I am sure we can arrange for you to give a seminar here, whenever. It would be very helpful for us all. Would you suggest a time later on? '89-'90 sounds good to me.

All the best. What a marvellous experience you've had!.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

Gastaldo

8 NOVEMBER 88  
I.F.P.  
c/o A.Y. HUC  
B.P. 311  
92506 RUEIL-MALMAISON

Dear Al,

THANKS FOR RESPONDING ABOUT MR. LI LAN. PRESENTLY, HE IS WORKING WITH RICHARD LEARY AT THE SPRINGFIELD MUSEUM IN ILLINOIS AS I'M AT I.F.P. UNTIL MARCH. I JUST RETURNED FROM A FIELD SEASON IN THE MAHAKAM DELTA, KALIMANTAN, BORNEO. I'M COOPERATING WITH GEORGE ALLEN (TOTAL-COMPAIGNE FRANÇAISE DU PÉTROL) AND ALAIN-YVES HUC (IFP) ON A VIBRACORING PROJECT TO UNDERSTAND THE DISTRIBUTION OF ORGONICS AND THEIR ORIGIN IN THE MODERN DELTA. THE PROJECT WILL CONTINUE AFTER I RETURN. I'M ASSESSING THE PLANT TAXONOMY AND DISTRIBUTION OF MACRODETRITUS (INCLUDING DISPERSED CUTICLES - A SUBCONTRACT TO GARY UPCHURCH). IN THIS TIDALLY-INFLUENCED DELTA WE'VE ALREADY BEEN ABLE TO GET A GOOD HANDLE ON WHERE THE ORGONICS ARE ACCUMULATING AND WHY. PROVENANCE OF THE MATERIALS IS PARTIALLY UNDERSTOOD AND WE HAVE TO ASSESS THE ANOMALIES (EQ. CHUNKS OF AMBER ACCUMULATING IN SIGNIFICANT QUANTITIES IN THE DETRITAL PEAT BEACHES). IN ANY CASE, IF GEOSCIENCES WOULD BE INTERESTED IN A SEMINAR ON EITHER PLANT TAXONOMY OR THE MAHAKAM DELTA PROJECT, CONTACT ME UPON MY RETURN. SPRING IS ALREADY HECTIC WITH A FEW MEETINGS AND SEMINARS AT YALE AND THE SMITHSONIAN. MAYBE WE COULD ARRANGE SOMETHING FOR ACADEMIC YEAR 89-90.

WITH REGARD TO MR. LI LAN. I WILL WRITE TO HE AND RICHARD LEARY ABOUT THE POSSIBILITY. MR. LI PLANS ON TAKING THE TOEFL EXAMINATION THIS DECEMBER AND THE GRE EXAMINATIONS NEXT FEBRUARY. THE LEVEL OF PERFORMANCE ON THESE EXAMS WILL

determine whether or not he will be competitive for a  
PhD. program. If you would be kind enough to have  
PENN STATE send an application form for admission to  
MR. Li c/o Richard LEARY, Department of Geology, ~~STATE~~ Illinois  
STATE MUSEUM, SPRINGFIELD, it would save time and needless  
correspondence. My letter of recommendation is home on an IBM  
format. Although I have a copy with me, IFP is using either their  
VAX mini's, their CREY mainframe, or Macintosh's. I can forward  
it upon my return if MR. Li applies to PENN STATE.

once again, thanks for your encouragement and support of  
my research efforts. I hope that we'll be able to meet and  
talk in the near future.

Best Regards

Bob.

Gastaldo

THE PENNSYLVANIA STATE UNIVERSITY  
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10 October, 1988

Dr. Robert A. Gastaldo  
Department of Geology  
210 Petrie Hall  
Auburn University  
Auburn, Alabama 36849-5305

Dear Bob:

Your letter of 9 August about Li Law came during my prolonged absence at 3IOP and 7IPC. I should have shuffled the mail, as the oldest items on the bottom of the stack are just now coming to light!

It is astonishing to hear that Auburn doesn't have a Ph.D. program in geology. Plenty of places with less claim to credentials have one.

Yes, we are the largest university Earth Science institute in North America, and of course offer a Ph.D. in all sorts of aspects of geology, including coal petrology, and paleobotany-palynology. As I'm sure you're aware, however, I get mostly students in palynology. I have had a couple in paleobotany, who were technically in Biology and got their degrees as of that department.

Whether Mr. Li could get admitted as a graduate student in geology here I can not assess. If I had his GRE scores, for example, it would be helpful in commenting. Admission is competitive, and the competition is quite keen. If a student is admitted, the implication is that he will be supported here, but this is somewhat tricky for a foreigner, unless he/she is very good (can be so measured by TOEFL) at English. Teaching assistantships are the major means of support. I would be happy to evaluate Mr. Li's GRE and TOEFL scores before he applies, if you like.

May I add that I think you do very interesting work. I wish I knew you better.

Yours very truly,

Alfred Traverse

AT/et

THE PENNSYLVANIA STATE UNIVERSITY  
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2 February, 1987

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36849

Dear Bob:


Have received and have been reading in P.S. Spec. Pub. 1. I'm not entirely convinced that the creationists are worth so much ink and paper, nor that giving them publicity doesn't play into their hands. However.... it is a nice piece of work.

One matter greatly interested me. I was on the infamous Steve Austin's Ph.D. committee here. He never uttered a word about being a fundamentalist, though I recall some sparring at his comps that made sense later in that light. I also recall that I wouldn't pass his Ph.D. thesis because the palynology was "cooked." I got him to admit that a person at a "Loma Linda University" had done it for him--and appropriate changes were made. At that time I'd never heard of L.L.U. That, and the fact that my wife kept commenting that he seemed to be loaded with cash and wasn't looking for a job "like a normal student" should have made us more suspicious.

It was not, however, until I got your book that I realized that he had published (as "S. Nevins"--maybe, however, Steve Austin is really the pseudonym?) creationist claptrap long before he applied to PSU--1973. If the application form asked for all publications then, as now, he was guilty of fraud for covering it up. (I am told that he has been asked why he kept "S. Nevins" secret, and he honestly replied, "Penn State wouldn't have admitted me if they had known.")

He performed very well in all courses here--including my palynology--and, aside from the palynological subterfuge cited above, I know of no way he could have been denied a Ph.D. It was a near-perfect scam (crime?).

Yours very truly,

  
Alfred Traverse  
Professor of Palynology

AT/et

THE PENNSYLVANIA STATE UNIVERSITY  
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30 March, 1987

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Thanks very much for the reprints that came a while back--just read with interest. I once visited the Glasgow stumps with Walton (1947) and thus was interested in your reasonable interpretation. Your research on the method of fossil plant preservation vs. sedimentation, etc., is of great interest to us. Keep up the good work!

Yours very truly,

Alfred Traverse  
Professor of Palynology

P.S. My room address has been 435 since 1976 ("529" is now a drafting room).

AT/et

# Auburn University

Auburn University, Alabama 36849-3501

Department of Geology  
210 Petrie Hall

Telephone (205) 826-4282

7 April 1986

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

Dear Al,

I have enclosed a copy of the program for the Plant Taphonomy Symposium scheduled for Monday afternoon, 16 June, in the Atlanta Convention Center. The only ms that doesn't really fit the symposium is the last paper of the day which was added by the meeting organizers. Bill Fritz was going to submit a paper on taphonomy in volcanoclastic terrains, but time wasn't on his side. He'll be addressing this at the Fossil Plant Short Course in November.

The short course for the Paleo Society will be held in San Antonio on Sunday, 10th November, in the San Antonio Convention Center and, as previous short courses, are free. The schedule of speakers is: FRITZ- volcanoclastic terrain taphonomy; SPICER - taphonomy in fluvial/limnic systems (the PRF has granted us money to bring Spicer from England); GASTALDO- taphonomy in deltaic systems; KNOLL- acquisition of land; GENSEL- early vascular land plants; SCHECKLER- Devonian-Mississippian transition; DIMICHELE, PHILLIPS and WILLARD- Peat accumulating systems of the Carboniferous; MAPES and GASTALDO- Late Paleozoic terrestrial clastic paleofloras; ARCHANGELSKY- Gondwanan paleofloras (PRF has provided some money to bring Sergio to the meetings; Texas A&M, University of Tennessee, and Auburn University [still in the works] have provided honoraria to aid in bringing Sergio to the US); ASH- Early Mesozoic paleofloras; HICKEY- Cretaceous angiosperm diversification; WOLFE- Trends in Tertiary floras. A volume of short course notes will be available for a nominal fee of approximately \$10.00.

I hope that this is sufficient information concerning these two forthcoming events. If I can be of any further assistance, please do not hesitate to contact me.

Sincerely,



Robert A. Gastaldo  
Associate Professor

- 1:30 **R.J. Burnham:** Taphonomy of Plants in a Paratropical Fluvial System  
1:55 **F.J. Rich:** Turning Over an Old Leaf—Taphonomy of Plant Remains in Lacustrine Sediments  
2:20 **R.A. Gastaldo:** Phytotaphonomy in a Subtropical/Temperate Bayhead Delta  
2:45 **M.H. Scheihing, H.W. Pfefferkorn:** Plant Taphonomic Processes in the Orinoco Delta: A Modern Analog for the Incorporation of Plant Remains in Deltaic Clastic Facies of Pennsylvanian Age  
3:10 **E.C. Kosters, G.L. Chmura, R.A. McBride:** Accretion and Origin of Organic Horizons in the Mississippi Delta  
3:35 **A. Raymond:** Plant Taphonomy in Tidal Swamps  
4:00 **L.H. Fisk:** Taphonomic Bias in the Pollen and Spore Record: A Review  
4:25 **O.I. Ece:** Depositional Environment Stratigraphy, Paleogeography and Organic Maturation of the Desmoinesian Cyclothemic Excello Black Shale in Oklahoma, Kansas and Missouri

## Monday Afternoon, June 16

### SEPM Poster Session I Poster Area, Exhibit Hall Authors in booths from 3:00 to 5:00 p.m.

#### SEPM Barrier Island Sequences and Coastal Sedimentology

- Booth  
5 **J.D. Beuthin:** Lower Pocono Embayments in Western Maryland and Vicinity Related to Latest Devonian Sea-Level Rise  
6 **R. Boyd, S. Penland\*, J.R. Suter:** Stratigraphic Signature of Coastal Transgressions  
7 **S. Penland, J.R. Suter, R. Boyd:** Inner-Shelf Shoal Formation Through Transgressive Barrier Submergence  
8 **T.S. Isacks, T.F. Moslow:** Sedimentary Environments, Evolution, and Stratigraphic Framework of a Laterally Prograding Transgressive Barrier Complex: Timbalier Island, Louisiana  
9 **A.J. Lomando, P.W. Britt:** Stratigraphic Dipmeter Applications in a Clastic to Carbonate Shoreline Transition: A Core-Dipmeter Comparison  
10 **M. Paulsen:** Variability Within the Upper Offshore Facies  
11 **R. Wright, M.J. Kisucky\*, S.G. Lucas:** Paleogeographic and Stratigraphic Constraints on the Lower Cretaceous Mesa Rica Delta System, East-Central New Mexico  
12 **A.O. Tammam, G.D. Wach:** The Geology of the Syncrude Oil Sand Leases

#### SEPM Southeastern Coastal Plain Deposits

- Booth  
14 **S. Cabe:** Cenozoic Shallow Marine Sands of the North and South Carolina Sandhills Area  
15 **R.E. Carver:** Rates of Intrastratal Solution of Heavy Minerals in the Southeast Atlantic Coastal Plain and their Potential for Dating Sedimentary Events  
16 **D.J. Colquhoun, K.T. Steele:** Pre-Early Miocene Age for Upper Coastal Plain Chronostratigraphic Units in the S.E. U.S.  
17 **R.H. Fluegeman, Jr.:** Foraminifera from the Paleocene Clayton Formation Lithostratotype; Barbour County, Alabama  
20 **H.I. Yi, E.C. Kosters, T.F. Moslow:** Recent Sedimentary Facies in an Interdistributary Basin, Mississippi Delta  
21 **R.A. Renken, R.A. Barker, R.W. Lee:** Hydrogeology, Hydrochemistry, and Computer Simulation of Ground-Water Flow in the Southeastern Coastal Plain Aquifer System  
22 **S.W. Snyder, A.C. Hine, S.R. Riggs, S.W. Snyder:** Miocene Unconformities, Chronostratigraphy, and Sea-Level Cyclicity: Fine-Tuning the Early Neogene Relative Coastal Onlap Curve for the North Carolina Continental Margin  
23 **D.T. King, Jr., M.C. Skotnick:** Facies Stratigraphy of an Upper Cretaceous Chalk-to-Clastic Transition Zone and the Discontinuity-Bounded Genetic Packages Created by Rapid Sea-Level Change on the Campanian Shelf of Central and Eastern Alabama

#### SEPM Clastic Sedimentology

- Booth  
24 **M. Vetter, G.R. Brakenridge:** Framework Mineralogies of the Hartford and Deerfield Basins' Sandstones and Mudstones: Independent Evidence for Provenance, Current Directions, and Tectonic History  
25 **A. Bailey, E. Kosters, J. Blackson:** Mobile Inorganic Constituents in Peat and Their Loss During Diagenesis  
26 **S. Bloch, R.K. Suchecki, J.R. Duncan, K. Bjorlykke:** Porosity Prediction in Quartz-Rich Sandstones: Middle Jurassic, Haltenbanken Area (Offshore Central Norway)  
27 **D.M. Bush, O.H. Pilkey, Jr., R.W. Rodriguez:** Impact of River Mouth Migration on Continental Margin Sedimentation  
28 **A.R. Carroll:** Pennsylvanian Fan-Delta Deposition Resulting from Tectonic Uplift Along the Southwestern Margin of the Anadarko Basin

\*Denotes speaker other than senior author.

|  |                                   |   |
|--|-----------------------------------|---|
| PROPOSAL NO.<br><b>EAR-8609158</b>   | INSTITUTION<br><b>Auburn Univ</b> | PLEASE RETURN BY<br><b>04/09/86</b>                 |
| PRINCIPAL INVESTIGATOR<br><b>Robert A. Gastaldo</b>  |                                   | NSF PROGRAM<br><b>STRATIGRAPHY &amp; PALEONTOLO</b> |
| TITLE<br><b>Megafloral Paleontology and Paleogeology of Early Pennsylvanian Deltaic Systems in the Black Warrior Basin</b> |                                   |   |

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

Gastaldo's proposal is for a worthwhile project on the cutting edge of paleobotany. There is very little research being done in North America on the geological side of paleobotany, especially with the return of Hermann Pfefferkorn to Germany and his student, Scheihing, to industry. Therefore, based on Gastaldo's previous work and the general situation, I would favor funding just to keep our hand in. However, I find the proposal to be imaginative, well conceived, and well-written, and it deserves support on merit. My copy of the proposal lacked page 10 with the all-important summary--too bad.

I really have only budgetary comments. The NSF policy has apparently been to regard a PC as the sort of lab furniture that the university through overhead should cover. I believe that was a correct position, and should be sustained. Auburn is apparently offering to cover half of the PC. They should cover the whole thing. Software and accessory hardware needed for the project could be part of the budget. The travel budget is inflated by multiple trips with a small personal pickup truck. I suggest U-Haul or other rental of a larger truck for 2-3 days, to cut down the number of trips and attendant personnel costs.

Gastaldo's performance on EAR 84-07833 is certainly all that can be expected in the time-frame and for the amount of money granted. It was a pilot-study and, if the current proposal is funded, will build into the on-going project.

Please include, in a separate paragraph(s), comments on the quality of the prior work described in the "Results from Prior NSF Support" section.

OVERALL RATING:  EXCELLENT  VERY GOOD  GOOD  FAIR  POOR

REVIEWER'S SIGNATURE  


REVIEWER'S NAME (TYPED)  
**Alfred Traverse**

OTHER SUGGESTED REVIEWERS (OPTIONAL)  
  
**Dr. Mark H. Scheihing**  
3400 West Park Blvd., Apt. 2079  
Plano, TX 75075

**Dr. Alfred Traverse**  
Department of Geosciences  
Pennsylvania State University

REVIEWER'S COPY

|   |                                   |   |
|---|-----------------------------------|---|
| PROPOSAL NO.<br><b>FAR-8516176</b>  | INSTITUTION<br><b>Auburn Univ</b> | PLEASE RETURN BY<br><b>08/31/85</b>                   |
| PRINCIPAL INVESTIGATOR<br><b>Robert A. Gastaldo</b>   |                                   | NSF PROGRAM<br><b>STRATIGRAPHY &amp; PALEONTOLOGY</b> |
| TITLE<br><b>Megafloreal Paleontology and Paleogeology of Early Pennsylvanian Deltaic Systems in the Black Warrior Basin</b> |                                   |   |

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

The proposer could have made this proposal a much more readable document with a few helpful diagrams, a map or two, and other illustrations, plus double-spacing. The proposal as it reads and looks like a legal brief. But the subject is fascinating, and Gastaldo is fast becoming a world leader in it. He has outlined in the proposal a good piece of work, which with the help outlined, he should be able to accomplish. Fossil plant paleoecology is an important field at the boundary between biology and geology. Gastaldo's work is important and deserves support. He is very productive and energetic. Auburn apparently recognizes that they have a good man and seem to be supporting him. The equipment and other resources are adequate. The budget is unremarkable and reasonable.

OVERALL RATING:  EXCELLENT     VERY GOOD     GOOD     FAIR     POOR

REVIEWER'S SIGNATURE

REVIEWER'S NAME (TYPED)

**Alfred Traverse**

OTHER SUGGESTED REVIEWERS (OPTIONAL)

**Dr. John S. Bridge**  
Dept. Geol. Sci. & Environ. Studies  
SUNY-Binghamton  
Binghamton, NY 13901

**Dr. Alfred Traverse**  
Department of Geosciences  
Pennsylvania State University

REVIEWER'S COPY

14 March, 1984

Dr. Robert A. Gastaldo  
Department of Geology  
Auburn University  
Auburn, AL 36830

Dear Bob:

Just read with profit your Pecopteris paper in JP. Perhaps you'd pardon my mention of a couple of nomenclatural gremlins on p. 73. "...is an invalid species" is incorrect usage. "Invalid" in ICBN refers to "not validly published" but should only be used in the following way--so-and-so was "validly (or not validly) published". In any case, the correct expression for your situation is "illegitimate". The "nomonym" (left col., para. 2, l. 21; surely you mean homonym) was perfectly validly published but is illegitimate.

Best wishes.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et