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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.

THE PENNSYLVANIA STATE UNIVERSITY  
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9 January, 1995

Dr. Bruce Cornet  
27 Tower Hill Ave.  
Red Bank, NJ 07701

Dear Bruce:

We're just back from one of the most frivolous and must fun things we've ever done--we went to the Rose Bowl. Actually, we went just after Christmas and didn't come back until yesterday, and thus were able to see some other very interesting things, such as the LA Natural History Museum which, despite practically 0 paleobotany, is a very interesting place. The bird exhibit is fantastic.

Your message was on the machine here. I pay for phone personally, but can send this free. Do you have e-mail yet? It's very handy.

Yeah, largely because of a very busy Fall Semester (16 in palynology class) and our immediate departure for CA, we didn't do a very complete job of greeting old friends at year's end. We enjoyed hearing from you, of course, and wish you well for 1995, a whole week of which we have (ye gods) already completed. As I've told you before, I regard some of your current interests as having the character of religion--not debatable, based on a faith position. As is now true of religion in general for me, I have almost no interest in such matters. Forgive me, but don't try to convert me. I would still very much like to consult with you about Triassic palynology from time to time.

Now I must get back to getting the paleobotany course set up. It's now a far better course than it ever was under WS.

I enclose some reprints I don't seem to have sent you.

Yours very truly,



Alfred Traverse

encl.:reprints

From Bruce Cornet, Pat Huff-Cornet,  
Mike, Niles and Robyne Leisti  
December, 1994

Dear *Al & Betty*

From Pat:

This has been another eventful year in our lives. My work had highs and lows, and too many hours too far from home. I had 4 consulting assignments, which raised my hopes and dashed my hopes, finances, and health to the ground. Here's prayers that 1995 is a less strenuous year for me...

About Bruce: This year has been filled with excitement for me. I have participated in 1) two ET-related conventions (Cody 1994 Moon/Mars connection: Agenda for Decision, 14-18 September in Wyoming; Omega Conference: The UFO Experience, 9-10 October in Connecticut), 2) two television shows on UFOs (Japanese series called The UFO, filmed 10-11 May; Encounters, Fox 5 series, filmed 23-24 June during eclipse, aired 8 July), 3) one live cable television program (UFO Investigations, in Atlantic City, NJ, 11 October), 4) a major two-part video production (Hoagland's Mars, Vol. 3, The Moon-Mars Connection, for which I was co-producer; filmed 2 June at Ohio State University; released July; received two EB awards), and 5) eight 2-3 hour radio talk show programs (6/19 Lee Clinton, AR; 7/8 Laura Lee, WA; 7/24 Hieronimus, MD; 10/16 Don Ecker, CA; 10/18 Laura Lee, WA; 11/13 Laura Lee, WA; 11/20 Lee Clinton, AR; 12/2 Laura Lee, WA) where I talked about possible alien architecture on the Moon, and UFOs in the Walkkill River Valley, NY. An article on my publication about the discovery of a fossil angiosperm leaf and flowers in rocks 120 million years older than the accepted time of appearance of angiosperms appeared in the January 1995 issue of Discover magazine (p. 89). On 13-15 December I participated in a research trip to the Goddard Space Archive in Greenbelt, MD, where a team of eight investigators, including Richard Hoagland, examined hundreds of lunar photographs and negatives taken by the Apollo astronauts. We found many more anomalies, as well as evidence that helps substantiate previously discovered alien structures that rise 7-10 miles above the lunar surface. I was also invited to present a two hour special event talk and slide show on the lunar anomalies at a conference in Gulf Breeze, FL, called Project Awareness in 1995. And I might be contributing to the first exhibit at Disney World on UFOs - the real stuff. Awareness of my research and accomplishments on exposing the UFO phenomenon has grown by leaps and bounds. Contact with the non-humans has increased substantially this year, and now I am frequently escorted into the UFO hotspot areas by one or two craft. It is a mind-expanding experience to have a telepathic communication with beings capable of projecting visual imagery as well as verbal conversation. There is much much more to this story, and recently I discovered why I am getting so much attention and preferential treatment from our visitors. But that

subject is too extraordinary to relate here, but is consistent with the rapid increase in UFO/ET awareness in the press and on television. For those of you who are skeptics, all I can say is that there is no one so blind as he who does not want to see! The evidence speaks for itself, particularly the 100+ photographically documented close encounters I now have in my collection, many of which demonstrate technologies beyond our imagining or understanding. I challenge anyone to come out into the field with me and meet our visitors!

About Mike: On the positive side, Mike's grades are improving – from a D+ average last year, to straight A's in summer school, 79 average 1st term or C+ (A in computer science, B in Geometry, Phys Ed, and Chemistry, C in Creative Writing and US History, F in English 2), to a 71 average 2nd term (B in Creative Writing and Chemistry, A+ in Computer Science, C in Driver's Ed, F in US Hist., Geom., English 2). On the negative side, Mike had become obsessed with playing the card game "Magic the Gathering" and had begun reading about and possibly experimenting with, actual magic, white and black. As a result of learning about that over Thanksgiving weekend and his continuing to hit and threaten his brother and sister, on Dec. 4 we destroyed his card deck with bleach. In reaction, he ran away overnight and thinks he wants to go live with his dad. To be continued on the saga of trying to get him therapy covered by my health-care-plan, trying to interest him in working on his problems in therapy. Mike took the Prelim. SAT's and got 650's.

About Niles: He continues to be a joy to us and his teachers. We work on encouraging him to have and express opinions, so he doesn't always answer "I don't know" when you ask him something. He did a lot of volunteer work with the Urban School Services Corp last year, was in the audience of the TV show 'Where in the World is Carmen San Diego', plays the steel drums and the alto saxophone in the school band, and still puts off long reports til the last minute. He's taking confirmation classes at the Lutheran church. A poem of his "Legends" was published in a book of poems by Young Americans. He loves watching Nickelodeon. He is in Pre-Algebra, advanced Reading, and advanced Spelling.

About Robyne: She has become VERY musical! This year in school she is singing in chorus, playing steel drums, and playing the trombone. She's also thrilled that her Dad had another child, a son born in August.

About Pat's work: I started an assignment at Bellcore Jan. 3 working on DEC VAXes, using the 4 books I bought in Dec. to cram for the interview to get me by on my rusty DEC skills. I'd only been there a few weeks when Bellcore policy (as interpreted by my female District Manager, but not by HER boss) changed to expect consultants to work 10-15 hours of unpaid overtime a week. I declined

that request, and instead took on in April working a second job 20 hours a week two miles away for pay. In February I changed which company I was working through because Garrett found out that his partner and his accountant had been embezzling from him. So I now work for Benefits Design Group in Manhattan. At the end of June I gave Bellcore notice I wouldn't renew my contract, so my District Manager lied to me and said that they didn't have enough work to keep me busy and laid me off two days before my contract was up.

THAT job was really exciting; I found it through my contacts, and also brought my colleagues Garrett, Avery, Chuck in to do pieces of it. So while I was there, I got to work with 4 friends, and we developed an exciting prototype of a geopolitical expert system, whose demo to Kuwait was a success. I got to work with the latest tools, CD-ROM drives, multimedia boards, satellite photos, Graphic User Interface builders, neural network tools. If development had gone forward into production, each of my friends and I would have gotten about \$150,000. for every sale. That raised my hopes. Unfortunately, the managers of the two small firms had a falling out and both severed their relationships with my company and we didn't get paid \$21,000. for work we'd done, about \$8,000. of which was my time. August 10th, that job ended.

September 8, I started working for NYNEX in Manhattan on a business re-engineering, data warehousing (storing operations information, not products) application. If that job had come through 15 minutes later, I would've been working instead for Chemical Bank, which did large layoffs since then... was I better off at NYNEX? I don't know. The office politics were terrible, the managers (3 NYNEX, 3 consultants) didn't know how to manage, and it appeared that they fired anyone who (a) was technically excellent and (b) irritated the lead developer. I was team leader of Data Modelling (4 of us) and with one other person, Technical Architect for the two layers of Server Computers. I usually worked 55 hour weeks plus 3 hours a day commuting to Manhattan. I got bronchitis again (I think I'm allergic to Manhattan air pollution), it turned into pneumonia but I didn't notice and worked through it (my temperature didn't go above 100 degrees). On Halloween, my purse got stuck on my chair while I ate lunch across from work. Fortunately, they didn't get any valid credit cards, but I still have one form of id to replace. Once again (this time NOT because I chose to leave) I was laid off by my female District Manager two days before my contract term was up. She thanked me for my technically excellent work but would not give me a letter of reference.

At least my health is returning, and Bruce's future looks bright with his involvement in a return robotic mission to the Moon. It is unfortunate that he has had to abandon his previous career interests, because of so little support for his field and from his colleagues. He has discovered a whole new world of friends, and says now that his previous career was stark and devoid of life by comparison.

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5 June, 1994

Dr. Wade B. Turnock  
Principal, Red Bank Primary School  
222 River St.  
Red Bank, NJ 07701

Dear Mr. Turnock:

I have received your inquiry about W. B. Cornet (better known here as "Bruce.") Dr. Cornet is very well known to me, as he was a doctoral student of mine here for about six or seven years.

Dr. Cornet is one of the best informed persons in all areas of science in the whole state of New Jersey. He also conveys to others a sense of enthusiasm for all of nature that is very infectious.

I am sure that you are wondering why such a person, with a long and distinguished record of publication, is available for substitute teaching, and the question must be addressed, or this letter is not honest. The explanation is very simple. Bruce has a long history of getting interested in matters that are on the fringes of science and of credibility and not being careful enough to hide this from the establishment. It may shock you to learn that scientists are very conservative about matters that are controversial and don't like much publicity if it might be misunderstood. Bruce has been in "trouble" with the establishment almost constantly, though the faculty here in one of America's premier geology programs still rates his Ph. D. thesis as the best in decades.

It seems to me that you have a unique opportunity to have your kids taught by a person with years of hands-on experience in science. Unlike deans of colleges, your students won't care that Cornet has published papers espousing some far-out ideas. Bruce is sober and dependable. I hope you give him a chance.

Yours very truly,

Alfred Traverse  
Professor of Palynology

Connet

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

16 March 1994

Dear Al,

Thank you for helping me with my application for a teaching position at the Rumson Country Day School. All my references were glowing, and Jeff Garside is very interested in giving me a shot at teaching despite my lack of professional experience. Thank you! I will in no way involve the school in my extracurricular interests.

I suspect that my friend in Golden, CO, was the person who called you over concerns that I would seriously jeopardize my career if I continued my involvement in the UFO and abduction phenomena. But my involvement is not entirely of my own choosing. I can't stop the abductions. No human can. Denial is not the answer either.

Al, you know me well. I am not intimidated by controversy, and the more resistant people are to accepting an idea when there is clear cut evidence to support it, the more interested I become in it. I cannot think of a more important subject with so many implications for the future of mankind than this subject. I don't believe in UFOs - I know they are real based on careful scientific documentation and personal experiences. Unfortunately, this subject does not easily lend itself to open and objective scientific scrutiny because of so much bias. It has been labelled the "Illegitimate Science" by many. But what I have learned about this subject over the past two years is that there are many dozens of scientists, most with advanced degrees (incl. Ph.D.), who are part of a large underground community of researchers. There are literally thousands of non-professionals who have a serious and active interest in the subject. There are regional and worldwide conferences that attract many of these professionals (more so in recent years), some of whom give papers, some of which are published - even in recognized refereed journals in physics. There is a vast and legitimate literature (excluding all the "tabloid-type" junk that continues to be published by non-scientists) as great as that on any accepted subject in science. There are over 1,000 books on the subject! The only reason the literature is not widely known is that most scientists will not look for it, let alone read it, because it does not represent their religion.

I have an acute power of observation and understanding, as well as the courage to stand up to the majority if I feel that they are wrong. I have challenged and/or overturned major ideas and theories, some of which have been intentionally nurtured by prejudice and authority. One would think that my colleagues would learn from my example. But my type of probing inquiry is threatening to many, and I have suffered for it. Yet, the truth has a way of rising to the top, despite attempts by those in authority to enforce their viewpoints.

Paul Olsen distanced himself from me in the mid-1980's after I proposed a theory about the Newark rift basins that he strongly opposed. I had well and seismic data indicating that there had been considerable reverse movement and thrusting of basement rock over Triassic sediments in the Richmond basin. But conventional wisdom said that no such compressional regime could exist in a passive extensional domain. I remember talking with Al Froelich on the phone about my interpretations, and asking for his help. He told me that no one would spud a well in basement at the eastern edge of the Richmond basin to test my theory. After getting similar responses from people in the oil industry, and a cold shoulder from Texaco after giving a talk on the subject in Williamsburg, VA, I dropped it.

About six years later Amaco drilled a corehole at the southeastern corner of Wood Lake (eastern edge of the basin). They penetrated 3,200 ft. of Triassic strata, drilled through 100 ft. of granite, and then 300 ft. of coal measures before reaching true basement at 3,600 ft. You can see this thrust clearly on seismic, but nobody wanted to believe it - and certainly not from the mouth of a palynologist! Now, Paul recognizes a major thrust (seismic data) involving a Triassic normal fault in the middle of the Taylorsville basin that became reversed. That fault continues to the surface and has pushed the Paleocene Aquia Fm. over Eocene Nanjemoy Fm. (the Skinkers Neck structure: Nixon and Powars, 1984 AASP field trip volume). The seismic and well data, which I correctly interpreted and which lead me to question conventional wisdom, did not mutate or change. It came back to haunt those who so carelessly disregarded it. And like my fossil evidence for pre-Cretaceous angiosperms, the truth just won't stay buried. Similarly, my magnetic and photographic data (1,700 stations in a 20 square mile survey, and over 400 photographs) document highly abnormal magnetic anomalies and aerial phenomena in the Pine Bush-Walden-Montgomery triangle. In some areas and at some sightings the Earth's magnetic field is temporarily reversed and inverted! But most scientists today seem to have more faith in theory (fantasy) than they do in facts, and end up worshiping false idols.

I have struggled in the last decade with little professional or financial support from colleagues at academic institutions. Paul Olsen is the main exception. But he and his partner, Denis Kent, kept me at a professional disadvantage while at Lamont, which prevented me from becoming more than a technical slave, and that was long before I got interested in UFOs. Yet Paul can be as biased and opinionated as any other scientist. He holds it against me in subtle ways because I "found religion". He has a very limited personal life, and gives the idea of family little serious thought. When Bonnie died, he was no comfort at all. Instead, he challenged with extreme prejudice my descriptions of the paranormal events that happened to me (even though I captures some of them on video and had witnesses). His reaction to my more recent interests has created distance between us again.

Actually, my extracurricular interests have helped me to remain sane. What I experienced in the last decade would make a weaker person give up or even commit suicide. Yet my faith in Christ/God and his interaction with me have kept me going. He has lead me through many hardships and made me stronger. He is using my gifts and talents to open closed minds, and he would not be supporting my study of the

UFO phenomenon if it were not real or part of His plan for me. He has given me a number of prophetic visions. He first asks me for my consent before giving me a difficult project. On a retreat to a Baptist camp in Texas, He asked me if I was ready to serve Him by helping someone in need, and when I said yes, Bonnie entered my life. Ten months after Bonnie died, He asked me if I was ready to meet the next woman in my life, and when I said yes, he showed me a vision of Pat sitting at a table in an all white room. That was two weeks before a mutual friend thought that we might be right for one another, and brought us together (first by phone). I think that it is neat that we first met in person at a church for the wedding of two close friends.

So please understand that I do not have any delusions or inclination towards self destruction. I am not out to embarrass you or any of my friends. I have a calling, and my closest friend and Lord is leading me. In your heart, that is all you need to know. As Ross Perot might say if he were more intuitive, "I'm all inner ears".

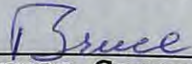
Enclosed are some reprints of my latest paper on pre-Cretaceous angiosperms. Please distribute them where there is an interest. I include reviews by Crane (Nature) and Palmer (New Scientist). Read Palmer's commentary first; then you will be able to appreciate just how biased Crane's commentary is. I also include my commentary on Crane's review. Doyle's position has come around nearly 180 degrees. Donoghue gave me a long overdue compliment at the Ames, Iowa AIBS meeting last year when he noted how his work with Doyle and mine were converging: He was pleased at the amount of overlap in our ideas and interpretations. I smiled and reflected on constants and variables.

I am nearing completion of a major contribution on two new cheirolepid conifers from the Portland Formation, Hartford basin, CT. It will have about 40 typed pages of text, 75 photographs in nine plates, 59 drawings in eight text-figures, two graphs, and two tables of palynological information. *Conanthus/Conanthoideae* - new subfamily.

I am also starting a manuscript on dispersed obligate tetrads in the Triassic: *Froelichispora*, *Placopollis*, *Froelichispora* (*Pyramidosporites*), and two new genera. The data will prove that Lockatong time follows Cumnock time, and that the Chatham-Richmond-Taylorville palynoflora ends at the top of the Cumnock Formation and its equivalent in the Taylorville basin. Lucas and Huber wrote some rather silly papers for the non-marine Triassic symposium volume that made it seem as though vertebrates knew time lines better than plants. They correlated the Lockatong and Cumnock, and disputed/rejected all palynological evidence.

Al, thank you for being my friend, and for all the support you have given me over the years, especially during my days at Penn. State.

Yours truly,

  
\_\_\_\_\_  
Bruce Cornet  
27 Tower Hill Ave.,  
Red Bank, NJ 07701

CHRISTMAS LETTER FOR 1993

from Bruce and Pat Cornet

Now that Bruce is living in Red Bank, NJ, with his new wife and adopted family (Robyne, Niles, and Mike: ages 10-14), he once again has child-rearing responsibilities and has developed some new interests. Pat, Bruce and the kids are doing well. The children spent three weeks in Finland this summer with their biological father. Pat recently got a new job through her current employer, Touchstone Industries, a computer software company specializing in debugging, programming, ultracompacting data, and optical data storage. She has degrees from Brown and Berkeley universities, and 20 years of programming experience. For three months since August her company had no income, and she and her boss were desperately looking for new contracts. They developed a new computer product involving optical scanners and compatible software, and soon sold 10-20 units. After that long dry spell it has begun to rain money again, and just in the Old Nick of time for Christmas. We have counted our blessings.

Bruce is still looking for full time employment. In his profession there is hardly anything available, now that the oil industry has collapsed and is nearly completely deflated. Academic jobs have disappeared along with jobs in geology. Yet, he is continuing his geologic and paleontologic research interests, and got two important papers published this year, along with another in press. Paul Olsen, his former employer, just told us that his new grant proposal was funded, and that he wants to hire Bruce again to continue the work that ended before when Paul's previous grant ran out of money. So, 1994 is looking up.

Due to Pat's generosity and income, Bruce was able to attend three scientific conferences this year, giving papers at two of them. At the Triassic symposium in Albuquerque in October Pat joined Bruce for a five day/four night field trip (including meals, lodging, and park entrance fees) in New Mexico and Arizona through the Painted Desert, Petrified Forest, Impact Crater, and Grand Canyon! It was our official honeymoon since getting married a year earlier in November 1992. We enjoyed the international company of 52 people, and had great fun hiking over buttes and onto mesas to look at exciting outcrops. Pat had fun collecting "dinosaur" bones and teeth. We couldn't have gotten a better deal on that trip for \$325 per person, nor had a better honeymoon!

On 20 November Bruce gave a lecture and workshop on his revealing photographic and magnetic discoveries in Pine Bush, NY, at the UFO Expo in New York City. Thousands of people showed up for dozens of talks and workshops on subjects ranging from UFOs worldwide to alien abductions. Perhaps you saw Bruce's appearance on national television in May of this year along with Ellen Crystall, with whom he has been working. They were on the television series, Sightings, reporting on some of their extraordinary UFO discoveries in Pine Bush near where he used to live in Middletown, NY. In May of 1992 Bruce became involved in studying this UFO hotspot, and has been using scientific equipment and cameras to document the phenomenon. The amount of data acquired over the last 18 months is enormous (for example, a 20 square

mile magnetic survey involving 1,700 station readings, resulting in the location of underground devices that transmit polarized magnetic photon beams into space - all on his spare time). Bruce has had 77 close encounters of the first, second, and third kinds, with alien ships coming within 20 feet of his camera. Details are available upon request.

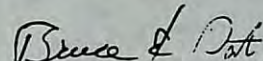
On 18 June 1992 Bruce was shot in the chest with some sort of magnetic beam while taking magnetic measurements next to a large open field, and the instrument went wild. He was not permanently injured, but after that these strange ships and lights seemed to be able to find him wherever he went. When he went up to Pine Bush, the ships came out and put on performances for him, revealing all sorts of high tech capabilities for the camera, such as cloaking devices (bending light to make the ships invisible), gravity-defying right angle high speed turns, and transformations from one kind of shape to another (many of these technological feats have been reported by hundreds of witnesses, but rarely or never before photographed).

It is ironic that Bruce's scientific interests have evolved from studying earth's past (continuing) to studying man's future. It is also ironic that a former business partner of his in Geminol, Inc., Jeff Hoffman, became one of the astronauts who recently fixed the Hubble telescope.

If you are interested or curious, we suggest that you get the book, Silent Invasion, from your library or book store and read it. The subject is on what Ellen Crystall discovered about UFOs in the Pine Bush area before Bruce became involved. She even has photographs in her book (albeit poor reproductions) of aliens standing next to their ship. We also suggest getting the book, Night Siege, by Hynek, Imbrogno, and Pratt, which is a scientific study of thousands of UFO sightings in the Hudson Valley region during the mid-1980s.

Have a joyful Christmas and prosperous New Year, and look to the stars. Not every blinking light that moves in the sky is one of ours.

With all our Love,

  
Bruce Cornet  
Pat Huff-Cornet  
27 Tower Hill Ave.  
Red Bank, NJ 07701  
(908) 747-9244



FIRE IN THE SKY

Dr. Bruce Cornet

UFO Investigator  
(908) 747-9244

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14 December, 1993

Dr. Bruce Cornet  
27 Tower Hill Ave.  
Red Bank, NJ 07701

Dear Bruce:

Your message was handed to me during Sarah's exam. Those exams are always (as far as I know) always open to the public, so you could certainly have come. I had told Paul O. that I thought it possible that you'd show up, and he said he'd have you join us for lunch if you came. Actually, the exam and attendant matters took so long that I had to leave for home before the group left for said lunch. I got back to S. C. at 7:30 p.m., and am writing this in my office while waiting for Betty to come back from State College Choral Society practice.

I used the article by Huber attacking palynology in general, as well as Cornet's use of it in particular, and your robust defense of it in the red book as a jumping off place for one of my questions posed to Sarah.

Best for the holidays and 1994.

Yours very truly,

Alfred Traverse

Message to Al Traverse

from: Bruce Cornet

12/13/93

11:45<sup>AM</sup>

Sorry I couldn't make it this morning  
Thank you for your letter  
Hope to see you in near future

Handed to me at  
Sarah Fowell's  
reference  
at James Roberts

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10 December, 1993

Dr. Bruce Cornet  
27 Tower Hill Ave.  
Red Bank, NJ 07701

Dear Bruce:

Wow, a long letter from BC, just like the old days--most of them are filed, along with reprints, with titles such as "essay by BC on monosaccates."

Sorry about being unavailable, or seeming to be. I really am an over-committed person, and when I contemplate that in addition to all that I now do I used to be rector of a full-scale Episcopal parish I just don't understand how I did it!

Glad you got the check. We were in England from 18-29 Nov., in connection with a meeting at Univ. London at which I spoke briefly. Betty and I decided to take advantage of the Thanksgiving holiday to take a little vacation, something we never did until this year.

On return from UK I find a huge pile of correspondence and other reading. It will take me until mid-January to get caught up because the palynology course is in full swing with 20 students. They are trying to get their unknown reports done, etc. This means that I haven't yet even glanced at "the red book," but I will one of these days.

Litwin, I guess, is riled with me too, for various reasons. He and Sid tried to get me to be co-author of their paper, but I declined on the grounds that it was inappropriate--I was just to be included for window dressing. I believe I should be glad now that I am not in the line of fire! I have read your comments carefully, but I need to read the papers too before saying anything much. I can say that your thesis and things that grew out of it have been of inestimable help to all of us who have tried to do palynostratigraphy with American Triassic/Jurassic sections. That includes Ron.

By the time you get this we may have talked in person? Best regards, as ever.

Yours very truly,

Alfred Traverse

B. Couv

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

19 November 1993

Dear Al,

Sorry that I did not dash you off a note when I could not reach you by phone or get my calls returned. I wanted to be sure you got the book before sending you an invoice (included signed, paid in full).

I realize that it will take you awhile to digest the papers in the "red book". For some papers, however, you may need wood-digesting bacteria in your gut to get anything worthwhile out of them. Many of the papers are excellent, while some are misconceived. There is a lot of controversy over whether the range of phytosaurs is entirely late Carnian (Tuvalian), or whether it extends down into the Julian near the paleoequator (e.g. Huber et al., p. 179; Spencer and Huber, p. 311). Even Litwin and Ash (1993) got into the act by re-dating the upper part of my Chatham-Richmond-Taylorville palynozone as late Carnian, which I challenge in my paper. Huber et al. butcher Newark biostratigraphy by correlating the Cumnock Fm. with the Lockatong Fm. Olsen, Huber, and I discussed the problem at length, and either biostratigraphy works for both plants and animals, or it doesn't work at all! That was my conclusion, implying that one set of biostratigraphic ranges can't be immutable when ranges of other taxa (plant or animal) conflict strongly with certain assumptions. Therefore, I hope that you will read my paper carefully when you have time so that we can discuss the implications and controversy.

I think the problem boils down to one of semantics: Vertebrate paleontologists do not recognize a "middle" Carnian faunal zone, and they assume that their late Carnian zone is entirely coeval with the Tuvalian. They also do not recognize a basal Cordovalian interval for the Carnian, only early and late Carnian (Julian and Tuvalian). I argue in my paper that what vertebrate paleontologists consider to be basal late Carnian in the Newark (upper Pekin Fm.) is the upper part of the Chatham-Richmond-Taylorville palynozone, regardless of age, and that by not recognizing a middle Carnian faunal zone they have created a controversy of terminology, not age. By having a "middle" Carnian zone, it necessarily overlaps both late and early Carnian zones based on a Carnian with only two zones. By counting two million year megacycles in the Newark via cyclostratigraphic marker units (i.e. in the Newark, Taylorville, and Richmond basins: Olsen, work in progress), it became evident that the late Carnian based on my palynozones covers slightly more than half the Carnian stage (see my paper, Fig. 1). By including the Cumnock and upper Pekin formations in the late Carnian, the late Carnian expands to 70% of the Carnian (10 out of 14 million years) - a little ridiculous if you ask me. Unfortunately, Litwin and Ash (1993) make the mistake of siding with the vertebrate paleontologists in order to refute my age interpretations. Refuting my work is the main thrust of their paper.

Speaking of Litwin and Ash, there is an unfortunate bias that is growing like a malignant tumor among those two and Rob Weems against Paul Olsen, myself, and now Paul's student, Sarah Fowell. Based on comments, both written and verbal, they, in short, seem to hate our guts and our interpretations. They appear to be on a bend to try and discredit Paul and his associates (NSF reviewer critique, for example) by challenging not just our interpretations, but also our integrity as scientists. Differing opinions and interpretations are OK, but the way they are going about it and the statements they are making towards us border on unethical conduct at worst and pure jealousy at best. Litwin and Weems won't return my calls or letters any more, and Litwin won't send me reprints. Not having a reprint in hand is why I made a mistake in referencing something in Litwin and Ash (1993), because no one could get me a copy of their paper by the deadline for the symposium volume. Because of what they said in their paper, I was forced to address their interpretations, but had to resort to having a friend read me the paper over the phone. Unfortunately, some data got misread (a misinterpreted graph). Its a mistake I won't soon forget, but one that could have been avoided had Litwin been more professional and courteous. Also, my error doesn't help the growing animosity between us. Perhaps you can help?

Hope you enjoyed your 10 days in England. I have never been there. Being able to go on the Triassic field trip after the non-marine Triassic symposium was an experience and education I doubt I will ever forget. Having Pat along made the trip like a honeymoon. I only wish I had been able to visit the Chinle Group when I was much younger. But like aged wine, perhaps older is better. The cost was right, and thanks to Pat's support, I was able to deliver two papers this year at national conventions. Unfortunately, Sid Ash did not attend the symposium or field trip. He apparently boycotted both after Spencer Lucas would not allow him to be one of the field trip leaders. Now Ash will probably gun for Lucas as well. How sad!

Yours very truly,



---

Bruce Cornet  
27 Tower Hill Ave.  
Red Bank, NJ 07701

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

14 November, 1993

Dr. Bruce Cornet  
27 Tower Hill Ave.  
Red Bank, NJ 07701

Dear Bruce:

I'm not sure I understand why you didn't dash me off a note that I could have filed as an invoice--would have cost you only 29 cents. Betty reports a couple of phone calls during the past frantic two weeks (I guess she told you that her mother died?).

The massive publication did come, and I am very appreciative that you took the trouble to arrange this. Thanks a lot. I clearly won't get a chance to look at it until next year--palynology course is in full swing with 20 students, and we have many other problems. This coming Thursday we are being absolutely frivolous for once and are flying to England for 10 days of pure vacation.

Check for \$40, which I understand is the correct amount, enclosed.

Best wishes as always.

Yours very truly,

Alfred Traverse

encl.:check

THE PENNSYLVANIA STATE UNIVERSITY  
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31 January, 1993

Professor William E. Friedman  
Chairman, Search Committee  
Department of Botany  
2502 Plant Sciences  
Athens, GA 30602-7271

Dear Professor Friedman:

Dr. Bruce Cornet has asked me to write you in support of his candidacy for a position in your department.

Bruce did his Ph. D. work here with me. His doctoral dissertation was once informally voted the "best Ph. D. thesis of the decade" by the faculty in this department. It was indeed a great piece of work and still much in demand by palynologists from all over the world. Bruce's master's thesis, on the anatomy and morphology of a group of Devonian ferns, was also a very superior piece of work, done under the direction of Henry Andrews.

In brief, then, Dr. Cornet has probably the best combination of megafossil and microfossil training in paleobotany that any American can boast. He is equally versed in field geology and all aspects of plant morphology and anatomy. This is a very capable man with very diverse interests and training. He is also a marvellous laboratory technician. His enthusiasm for the subject is contagious, and he is an excellent lecturer. I am sure you would all find him very stimulating to have around. So, you may well ask, if Bruce is so good, why is he still without a permanent job? The reason is that he has very unusual tastes, opinions, and personality traits. At times I found this difficult when he was here. However, I recognized that he had so much to offer that it was worth being tolerant of his peculiarities. I have constantly hoped during the decade since he left here that some university would be willing to give him a chance to show what he could do if he had the mild security of at least a semi-permanent job, instead of the succession of post-docs and other insecure positions that have been his lot. I sincerely hope that the University of Georgia will give Bruce a chance to demonstrate his really great ability.

Yours very truly,

Alfred Traverse  
Professor of Palynology

THE PENNSYLVANIA STATE UNIVERSITY  
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11 November, 1992

Professor Edward Belt  
Pratt Museum  
Box 2238, Amherst College  
Amherst, MA 01002-5000

Dear Professor Belt:

This is to support the candidacy of Dr. Bruce Cornet for the position of curator of the Pratt Museum.

Bruce did his Ph.D. work with me. He was here a number of years (about six). His doctoral dissertation was once informally voted "best Ph.D. thesis of the decade" by the faculty here, and it was indeed a great piece of work, which those of us interested in Triassic palynology still use constantly.

It has long been my conviction that as museum is where Bruce belongs. He has all the qualities needed: a good artistic sense, a flair for presentation of information, encyclopedic knowledge of all phases of paleontology. He is also an outstanding field geologist and a marvellous lab technician. It could be of interest and importance that he knows intimately all aspects of the natural history of the area within 100 miles of Amherst (he and I studied the Triassic paleobotany and palynology of the area together). Bruce would be a dedicated researcher and organizer of collections. His enthusiasm for natural history will prove infectious for students and others.

In short, I recommend Bruce for your consideration most heartily. If there are any aspects of his qualifications you'd like to discuss with me, please call.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

TO AT

DATE 11.XI.92 TIME 12:45

WHILE YOU WERE OUT

M. Bruce Cornet

Of \_\_\_\_\_

Phone \_\_\_\_\_

TELEPHONED  PLEASE RETURN CALL

CALLED TO SEE YOU  WILL CALL AGAIN

RETURNED YOUR CALL  RUSH

MESSAGE (long). Basically, he has  
found super palynoflora and megafossils  
in red shales of Hartford Basin.

Youngest so far in H.B. Palynoflora  
dominated by Corollina, is very similar  
to Agelon location (sp.?) that you and  
he sampled, but just a bit younger.

Re megafossil cones: cone scales  
reduced to single seed and its inte-  
gument on stalk. In ripe cones, the  
bracts all reflex backwards (looks  
like porcupine. Cone looks almost

The Standard Register Company

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9 September, 1991

Dr. Bruce Cornet  
Lamont-Doherty Geological Observatory  
of Columbia University  
Palisades, NY 10964

Dear Bruce:

Thanks so much for the "green book." Obviously it's a very careful job, and reflects your years of devotion to the general subject.

Had a phone conversation with Gil Brenner the other day. He says he has enjoyed getting to know you.

All the best.

Yours very truly,

Alfred Traverse

AT/et

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14 August, 1991

Dr. Bruce Cornet  
Lamont-Doherty Geological Observatory  
Columbia University  
Rt. 9W  
Palisades, NY 10964

Dear Bruce:

Haven't had any luck with the phone, so decided to write.

Question: Did the "Early to Middle Carnian  
Flora....Richmond....etc." thing from the Virginia Museum  
actually come out? I was about to send them a check but decided  
I'd better ask first.

Also, maybe I'd better check your phone number. I have on my  
card: 914-359-2900, x621 or 622. For Fax I have .....5215.  
Right?

Yet another palynology course is about to get off the ground.  
26th in the series.

Best wishes, as well as to Sarah and Paul.

~~ae~~Yours very truly,



Alfred Traverse

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

5 April, 1990

file  
cornet

Dr. John Silander  
Plant Structure & Function Search Committee  
Department of Ecology & Evolutionary Biology, U-43  
University of Connecticut  
Storrs, CT 06269-3043

Dear Dr. Silander:

Dr. Bruce Cornet, now of Lamont-Doherty Geological Observatory at Columbia University, is interested in your position as "organismal botanist." Dr. Cornet is a justly famed paleobotanist, who would fill your requirements more or less exactly. He has asked me to write a letter to you re his qualifications.

Bruce was a Ph.D. student of mine here at Penn State, and we came to know him very well in the palynological laboratories, where he was innovative and very productive. I also spent a lot of time with him in the field and was able to observe his considerable expertise in locating and sampling material important to our work.

Dr. Cornet is a very talented research scientist. His enthusiasm for our subject is famous and infectious. He is good at explaining the whole area of geology-biology to the uninitiated, and does an excellent job of both lecturing and demonstrating in the laboratory. His attention to detail is remarkable, and I am sure he could be a leader in your department.

Bruce's master's thesis with Henry Andrews at Storrs some years ago was a very significant contribution to the megafossil paleobotany of the Devonian. Later, while with me, he did very outstanding and creative work on the Triassic/Jurassic plants and palynofloras of North America. Most recently he has become famous worldwide for his important and controversial work on Sanmiguelia and other putative angiosperm-like remains in Triassic rocks.

Dr. Cornet is both geologist and botanist. As a botanist he could teach your paleobotany, plant morphology and plant anatomy courses with distinction. He studied those subjects with Andrews and others at Storrs, with Charles Hillson and me here.

Withal, Dr. Cornet is, because of his enthusiasm and dedication, regarded by some people as a "character." Nevertheless, some organization which will give Bruce a chance will be rewarded by having the services of a very talented, very interesting, very capable individual. It is long since time that a man of his stature had a permanent position. I believe U Conn would be very smart to give Bruce an opportunity.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

TO AT  
DATE 5-14-90 TIME 11:30

WHILE YOU WERE OUT

M. Bruce Cornet  
Of \_\_\_\_\_  
Phone \_\_\_\_\_

TELEPHONED  PLEASE RETURN CALL   
CALLED TO SEE YOU  WILL CALL AGAIN   
RETURNED YOUR CALL  RUSH

MESSAGE needs recom.  
letter for U Conn.  
(I have details) -  
also more re field  
trip (ask me)

Signed BT

The Standard Register Company

Done

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

4 April, 1990

Dr. Bruce Cornet  
Lamont-Doherty Geological Observatory  
Rt. 9W  
Palisades, NY 10964

Dear Bruce:

Will be glad to write whatever letters you like--I have always written super ones for you. (I'm still very sorry that you didn't get the job at the museum in Austin.)

The field trip in VA sounds marvellous--but I guess I've been to all those places several times. However, I want a copy of the guidebook. Can you get me one?

All the best to you, old friend.

Yours very truly,

Alfred Traverse

AT/et

*fill  
cornet*

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435 Deike Building  
University Park, PA 16802  
phone: (814)863-3419; fax: 814-865-3191

4 April, 1990

Carney, Sandoe & Associates  
136 Boylston Street  
Boston, MA 02116

Dear Sirs:

Dr. Bruce Cornet, now of Lamont-Doherty Geological Observatory at Columbia University, is seeking an academic position with your assistance. He has asked me to write a letter to you re his qualifications.

Bruce was a Ph.D. student of mine here at Penn State, and we came to know him very well in the palynological laboratories, where he was innovative and very productive. I also spent a lot of time with him in the field and was able to observe his considerable expertise in locating and sampling material important to our work.

Dr. Cornet is a very talented research scientist. His enthusiasm for our subject is famous and infectious. He is good at explaining the whole area of geology-biology to the uninitiated, and does an excellent job of both lecturing and demonstrating in the laboratory. His attention to detail is remarkable, and I am sure he could be a leader of an academic program or department.

Withal, Dr. Cornet is, because of his enthusiasm and dedication, regarded by some people as a "character." Nevertheless, some organization which will give Bruce a chance will be rewarded by having the services of a very talented, very interesting, very capable individual. It is long since time that a man of his stature had a permanent position.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

Lamont-Doherty Geological Observatory  
of Columbia University

Palisades, N.Y. 10964

Cable: LAMONTGEO  
Palisades New York State  
TWX-710-576-2653

Telephone: Code 914, 359-2900

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

29 March 1990

Dear Al,

It is time for me to find a stable job supported by hard money. I am using Carney, Sandoe & Associates in Boston to help me find a teaching position at a private institution.

Would you please send them a letter of reference on your letterhead. Please do not write the reference for a specific school, and be sure to mention my teaching and/or administrative ability or potential.

Please send your letter directly to:

Carney, Sandoe & Associates  
136 Boylston Street  
Boston, Massachusetts 02116

and keep a copy in the event that I should ask you to send additional references.

Sincerely yours,



Bruce Cornet  
(914) 359-2900 ext. 621

P.S. My field guide book project for the Paleobotanical Section of B.S.A. is nearly complete, and the fieldtrip on 5 August 1990, the Sunday before the conference at the University of Richmond, VA, should be excellent. Olsen and I have written about 60 pages with 40 plates and figures that cover four stops, one in the Taylorsville basin and three in the Richmond basin. This contribution will be the first compilation of floral data of Carnian age for all the Newark basins, and will highlight the new vertebrate fauna from the Richmond basin. It will be the seed or catalyst for renewed interest in Triassic floral evolution.

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(814)863-3419

8 September, 1989

Dr. Bruce Cornet  
Lamont-Doherty Geological Observatory  
of Columbia University  
Palisades, NY 10964

Dear Bruce:

Have just finished "first reading" of your Richmond Basin paper. Seems to be a nice job of putting together things that have interested you for a long time--as you observe on the "sticky note" adhering to plate 4. Congratulations on getting it out.

A very minor nomenclatural comment: when name with a silent vowel on the end is made into a generic name, the silent vowel should be dropped, and then a double-i should be used, thus traversii. Shinner long ago named a modern plant of Louisiana for me, Erigeron traversii, and that seems to be the correct procedure. However, I do not believe this is an "error" to be corrected. The whim of the original author must be followed, as with Linnaeus' pensylvanica.

Also, in the future a complete citation for Placopollis should read: Placopollis Koob ex Cornet. However, the short citation would be Placopollis Cornet (the "Koob ex" is not essential).

Whoops, we're driving up to the Deike Bldg. as I write. Gotta dash. Best.

Yours very truly,

Alfred Traverse

AT/et

THE PENNSYLVANIA STATE UNIVERSITY  
DEPARTMENT OF GEOSCIENCES  
PALYNOLOGICAL LABORATORIES  
435 Deike Building  
University Park, PA 16802  
(814)863-3419 or (814)865-6711

6 February, 1989

Dr. Bruce Cornet  
Lamont-Doherty Geological Observatory of  
Columbia University  
Palisades, NY 10964

Dear Bruce:

It was a thrill to get the Ediger et al. MS. I was supposed to prod you occasionally about this, and you know that I did, but when you were moving, moving and resettling it didn't seem useful. It looks to me as if the compromise parts of the thing have been handled well. I hope Bruce Goodwin will think so too.

I have read the MS carefully and have red-penned suggested changes. I am returning only those pages on which I wrote, viz. 1-4, 6, 11, 13, 15, 18-21, 29-31, 33, Fig. 2, Fig. 4.

The most important matters are: 1. the sentence that runs between pp. 1-2 in the abstract needs to be divided for readability and to avoid the implication that we have Otterdale pollen data; 2. I have a problem with the "small eastern basins" statement on p. 6--it needs a little more explanation. The rest of my comments are orthographic and/or stylistic. I believe it would be appropriate to acknowledge Wilkes, as he was quite helpful to Ediger and me. I hope this now goes on through.

Best wishes.

Yours very truly,

Alfred Traverse

P.S. 1. Your mailing label is really impressive!  
2. Did you see current number of Pennsylvania Geology, which contains the surprising news (p. 11) that Litwin is reworking your Jacksonwald locality? Why didn't they get Cornet to rework Cornet?

AT/et  
cc: Ediger, Goodwin  
encl: MS pages

29 Nov. 88

filed as corresp.

Dr. Bruce Cornet

encl. 11/14/88

office: 12401 Westheimer Rd.  
Geo-Science Lab, Superior Oil Co.  
Houston, TX 77077  
(713) 531-2203

home: 13567 Portobello ~~W.D. Dine~~  
Houston, TX 77083  
~~(713) 530-3183~~

~~Ch. Schumb.~~ } 713-464-8007 } 14222 Keplerleyton,  
713-558-5701 } Houston #411  
77079

26-37, 41, 43, 44, 45, 46, 47

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435 Deike Building  
University Park, PA 16802  
(814)863-3419 or (814)865-6393

12 September, 1988

Betty Cornet  
7952-13 Locke Lane  
Houston, TX 77063

Dear Betty:

Betty and I appreciated your nice note which awaited our return from palynology meetings in Australia. Good to know about your Cereus. Last year we read a German novel of about 1840 in which the flowering of such was featured. We once invited friends over to witness the event.

Good to know Bruce is now in NYC area. Presumably we'll hear directly one of these days. I am somewhat concerned that he'll put me out of business (ha!).

Best wishes.

Yours very truly,

Alfred Traverse

AT/et

ca. 20 Aug 85

7952-13 Locke Lane  
Houston, Texas  
77063

Dear Al and Betty,

I just couldn't let another day go by without writing you folks and telling you we were thinking of you so much lately! The Night Blooming Cerise Plant you had given Bruce is blooming! one on my birthday - it was exciting to observe - at its gorgeous height at about 1:00 AM - <sup>becoming</sup> an upright flower (reminded me of the Magnolia blossoms here, but much more delicate in texture) then closing and falling slowly down until the next night. Exciting, eh? It must bloom about every 11 years, and hope I'm still here in 1999 to see it again! 😊 I have the plant now for safe keeping! Preserved one blossom.

Bruce finally got moved to New York State, arrived the 17th of ~~September~~ August, after 5 days of slow travel - a huge Rider Truck

filled with personal possessions,  
research files and papers, and  
his truck in tow. I'm so grateful  
they got there safely, a slow trip,  
as the load was so heavy - all his  
rock collections too! 😊

Bruce will be at the LaMonte-Doherty  
Geological Observatory - Route 9 W -  
Palisades, N. Y., 10964. He is there  
on a two year research - post  
doctorate - thru Columbia Univ. I'm  
so thrilled he is out of Houston  
and all the stress and strain of  
its economy - oil research and  
development couldn't be at a lower  
point, and it's been frustrating on  
him and all the others. me too! 😊

Hope you all are well and enjoy-  
ing life to its fullest. Would love  
to hear from you and hear all  
your news. I keep myself busy,  
but will miss Bruce's visits - he's  
always such fun and full of  
challenging projects. His wife, Bonnie  
is not well, a cancer patient. It's  
sad! Affectionately,  
Elizabeth Cornet.

---

# PARR ENERGY, INC.

---

P.O. BOX 347 • PERRY, GA 31069

June 17, 1988

Professor Alfred Traverse  
Palynological Laboratories  
435 Deike Bldg  
The Penn. State University  
University Park, PA 16802

Dear Professor Traverse,

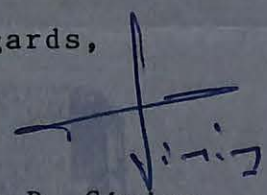
We hereby authorize Dr. Bruce Cornet to deliver to your Department for a Ph.D. Program a set of samples from the ITT RAYONIER #1 Well that we drilled last month in Appling Co., Georgia.

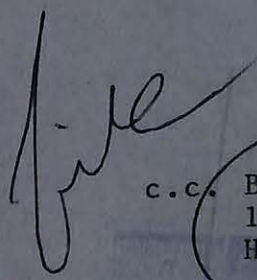
We understand that these samples will be kept in your Department and will not be used for other purposes than palynological research.

We hereby request that

- (1) The data and results shall not be shared with anyone until they become public information
- (2) A copy of the thesis shall be made available to our company as soon as the work is completed
- (3) Dr. Bruce Cornet shall be on the Ph.D. Committee.

Best Regards,

  
Pierrick R. Simian

  
c.c. Bruce Cornet  
14222 Kimberly L #411  
Houston, Tx. 77079

Professor Alfred Traverse  
Palynological Laboratories  
435 Deike Bldg.  
The Penn. State University  
University Park, PA 16802

*file:  
Connet*

June 20, 1988

Dear Al,

Pierrick Simian, President of Parr Energy, Inc., sent a letter to you authorizing the use of the cuttings samples from the ITT Rayonier #1 well for a Ph.D. program. He does not want the data and results to be shared with anyone (unauthorized) until they become public information, i.e. available through the State of Georgia geological survey, which is probably after a period of one year. Since it will take at least a year for the student to process and thoroughly study the samples, this requirement should impose no burden. In addition, a copy of the thesis needs to be made for Parr Energy, Inc. Finally, I requested that Simian require that I be placed on the student's Ph.D. committee, so that I can insure that Parr Energy's wishes are met, and so that I am not excluded from this project as I was by Ediger from his project. Ediger did not even have the courtesy of sending me a copy of his thesis on the Richmond Basin for making the Horner and Bailey well samples available to him and for allowing him to do his doctorate on that basin after I had staked a palynological "claim" to it by causing two test wells to be drilled for palynological samples.

There are two minor requirements that I wish to add to this list:

1) That a complete set of slides be made for me of every sample processed from the ITT Rayonier #1, and

2) That I also receive a copy (with photographic or printed plates) of the thesis upon completion.

You will be responsible for seeing that the student meets these requirements. I do not intend to use my set of slides for anything other than a reference and study collection. I will not publish any information derived from the slides without the written consent of the student that uses these samples for his or her doctorate.

If the samples from this well are not used for a Ph.D. program, they must be returned to me unless otherwise instructed by Parr Energy, Inc. By accepting these samples, you also agree that any and all palynological preparations of the samples will be made available to me (or Parr Energy, Inc.) upon request, preferably as a separate set of slides. I do not want the situation that arose with Ediger repeated, where I could not study the palynoflorules recovered by Ediger from the Richmond

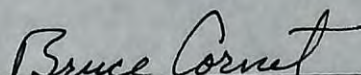
Basin without his permission (which was denied). That information and those samples are supposed to be public.

Since the samples from the ITT Rayonier #1 (T.D. 4,154 ft.) are cuttings samples, I hope that you will encourage the student assigned this well to work it the way industry palynologists do, that is by processing all shale samples, making a distribution chart of all identified taxa, and using tops to define age boundaries and unconformities. Such a study will prepare the student for a job in industry and make him or her aware of the problems inherent with cuttings as well as the solutions for dealing with them. A mud log (omitting geochemical and any other sensitive data) will be made available so that the student can select appropriate samples for processing. An electric log will probably be available in a year or two after the state makes that information public. Electric logs of nearby wells are currently available from the state.

There are about 134 samples through the Cretaceous, most of which are 10 foot samples through slow drilling shale sequences, and 20-30 foot samples through fast drilling sandstone sequences. The Cretaceous section in the well is about 2,014 feet thick, with the Upper Cretaceous being about 1,874 feet thick and the Lower Cretaceous about 140 feet thick, although the Upper/Lower Cretaceous boundary is not certain and the Lower Cretaceous could be thinner. Formation tops were picked by log correlation with the Felsenthal-Weatherford W.E. Bradley #1 about six miles away.

The Upper Cretaceous is a sequence of deltaic sandstones interbedded with thick prodeltaic and interdeltic shales, and includes a complete sequence through the Cretaceous/Tertiary boundary event (a chalky white zone is also present: ash?). There are a number of reddish soil zones separating organic rich shales containing leaf cuticles and foraminifera in the Lower Cretaceous sequence, while the Cenomanian is reported to be missing(?) in Georgia. We drilled through a fossilized tree stump or log on top of one unconformity. See enclosed list of samples by depth. This information is given for use in applying for NSF funding for your student.

Sincerely yours,

  
\_\_\_\_\_  
Bruce Cornet  
14222 Kimberley Ln. #411  
Houston, TX 77079

CC: P. Simian, Parr Energy, Inc.

PARR ENERGY ITT/RAYONIER NO. 1  
 APPLING COUNTY, GEORGIA

Depth Corrections for Cuttings Samples

<u>Original Depth</u>	<u>Corrected Depth</u>	
500-510	520-530	Add 20 feet for correction.
510-520	530-540	
520-530	540-550	Top of reef complex.
530-540	550-560	
540-550	560-570	Oligocene reef
550-560	570-580	
560-590	580-610	
590-620	610-640	
620-650	640-670	
650-680	670-700	
680-700	700-720	
700-740	720-760	
740-770	760-790	
770-800	790-820	
X		No sample collected.
810-830	830-850	
830-870	850-890	
870-900	890-920	U. Eocene reef
X		
X		
X		
X		No samples collected.
X		
X		
1140-1160	1160-1180	Add 20 feet for correction.
1160-1170	1180-1190	
1170-1180	1190-1200	
1180-1190	1120-1210	
1190-1210	1210-1230	
1210-1220	1230-1240	
1220-1230	1240-1250	
1230-1240	1250-1260	
1240-1260	1260-1280	Bottom of reef complex.
1260-1280	1280-1300	
1280-1300	1300-1320	
1300-1330	1320-1350	
1330-1350	1350-1370	
1350-1370	1370-1390	
1370-1380	1390-1400	
1380-1400	1400-1420	
1400-1430	1420-1450	
1430-1460	1450-1480	
1460-1490	1480-1510	
1490-1510	1510-1530	
1510-1520	1530-1540	

---

M. Eocene Tallahata Fm.

Original DepthCorrected Depth

1520-1530  
1530-1550  
1550-1560  
1560-1570  
1570-1590  
1590-1610  
1610-1640  
1640-1680  
1680-1700  
1700-1710  
1710-1730

1540-1550  
1550-1570  
1570-1580  
1580-1590  
1590-1610  
1610-1630  
1630-1660  
1660-1700  
1700-1720  
1720-1730  
1730-1750

Tallahata Fm.

---

1730-1740  
1740-1750  
1750-1772  
1772-1800  
1800-1820  
1820-1830  
1830-1850  
1850-1870  
1870-1910  
1910-1950  
1950-1970  
1970-2000  
2000-2010  
2010-2040  
2040-2060  
2060-2070  
2070-2080

1750-1760  
1760-1770  
1770-1792  
1792-1820  
1820-1840  
1840-1850  
1850-1870  
1870-1890  
1890-1930  
1930-1970  
1970-1990  
1990-2020  
2020-2030  
2030-2060  
2060-2080  
2080-2090  
2090-2100

L. Eocene

Paleocene?

---

2080-2120  
2120-2140  
2140-2160  
2160-2170  
2170-2180  
2180-2200  
2200-2220  
2220-2240  
2240-2260  
2260-2280  
2280-2300  
2280-2300

2100-2140  
2140-2160  
2160-2180  
2180-2190  
2190-2200  
2200-2220  
2220-2240  
2240-2260  
2260-2280  
2280-2300  
2300-2320

U. Cretaceous?

Depth correction during drilling due to error in pipe tally.  
Subtract 46 feet for corr.

2380-2400  
2400-2410  
2410-2420  
2420-2440  
2440-2450  
2450-2460  
2460-2480  
2480-2500  
2500-2510  
2510-2520

2334-2354  
2354-2364  
2364-2374  
2374-2394  
2394-2404  
2404-2414  
2414-2434  
2434-2454  
2454-2464  
2464-2474

Original DepthCorrected Depth

2520-2530	2474-2484
2530-2540	2484-2494
2540-2550	2494-2504
2550-2570	2504-2524
2570-2580	2524-2534
2580-2590	2534-2544
2590-2600	2544-2554
2600-2610	2554-2564
2610-2620	2564-2574
2620-2630	2574-2584
2630-2640	2584-2594
2640-2650	2594-2604
2650-2660	2604-2614
2660-2670	2614-2624
2670-2680	2624-2634
2680-2690	2634-2644
2690-2700	2644-2654
2700-2710	2654-2664
2710-2720	2664-2674
2720-2730	2674-2684
2730-2740	2684-2694
2740-2750	2694-2704
2750-2760	2704-2714
2760-2770	2714-2724
2770-2780	2724-2734
2780-2810	2734-2764
2810-2850	2764-2804
2850-2876	2804-2830
2876-2902	2830-2856
2902-2916	2856-2870
2916-2930	2870-2884
2930-2940	2884-2894
2940-2950	2894-2904
2950-2970	2904-2924
2970-2990	2924-2944
2990-3010	2944-2964
3010-3030	2964-2984
3030-3060	2984-3014
3060-3080	3014-3034
3080-3090	3034-3044
3090-3100	3044-3054
3100-3120	3054-3074
3120-3140	3074-3094
3140-3160	3094-3114
3160-3170	3114-3124
3170-3180	3124-3134
3180-3190	3134-3144
3190-3200	3144-3154
3200-3210	3154-3164
3210-3220	3164-3174
3220-3230	3174-3184
3230-3240	3184-3194

Original Depth

Corrected Depth

3240-3260	3194-3214
3260-3300	3214-3254
3300-3330	3254-3284
3330-3350	3284-3304
3350-3360	3304-3314
3360-3370	3314-3324
3370-3380	3324-3334
3380-3390	3334-3344
3390-3400	3344-3354
3400-3430	3354-3384
3430-3460	3384-3414
3460-3480	3414-3434
3480-3510	3434-3464
3510-3520	3464-3474
3520-3560	3474-3514
3560-3570	3514-3524
3570-3580	3524-3534
3580-3600	3534-3554
3600-3620	3554-3574
3620-3640	3574-3594
3640-3660	3594-3614
3660-3680	3614-3634
3680-3700	3634-3654
3700-3720	3654-3674
3720-3740	3674-3694
3740-3750	3694-3704
3750-3760	3704-3714
3760-3790	3714-3744
3790-3800	3744-3754
3800-3810	3754-3764
3810-3820	3764-3774
3820-3830	3774-3784
3830-3840	3784-3794
3840-3860	3794-3814
3860-3870	3814-3824
3870-3880	3824-3834
3880-3890	3834-3844
3890-3900	3844-3854
3900-3910	3854-3864
3910-3930	3864-3884
3930-3940	3884-3894
3940-3950	3894-3904
3950-3960	3904-3914
3960-3970	3914-3924
3970-3980	3924-3934
3980-3990	3934-3944
3990-4000	3944-3954
4000-4010	3954-3964
4010-4020	3964-3974

----- ?

----- ?

U. Cretaceous?

-----

L. Cretaceous?

4020-4030  
4030-4040

3974-3984  
3984-3994

Original Depth

Corrected Depth

4040-4050	3994-4004	
4050-4070	4004-4024	
4070-4080	4024-4034	
4080-4090	4034-4044	
4090-4100	4044-4054	----- ?
4100-4110	4054-4064	
4110-4120	4064-4074	----- ?
4120-4130	4074-4084	
4130-4150	4084-4104	L. Cretaceous
4150-4160	4104-4114	-----
4160-4170	4114-4124	
4170-4180	4124-4134	Triassic - altered
4180-4190	4134-4144	
4190-4195	4144-4149	
4195-4200	4149-4154	
4200-bottoms up. T.D.	4154	Diabase?

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

3 November, 1987

Dr. Christopher J. Durden  
Texas Memorial Museum  
The University of Texas at Austin  
2400 Trinity  
Austin, TX 78705

*Bruce  
Cornet*

Dear Dr. Durden:

Yours of 20 October '87 to Alan Davis re the curatorial post at Texas Memorial Museum was referred to me by Professor Davis, as my interests, research and teaching include paleobotany.

One of my former students, Bruce Cornet, is currently unemployed, or at least underemployed, as one of the casualties of the oil industry depression. He has very wide experience and interests in geology, including paleobotany (his Master's thesis with Henry Andrews was on a fossil fern), palynology (with me), vertebrates (fish mostly), and invertebrates (for example, conchostracans). He is one of the best field geologists I have ever known. All of the tasks you mention would be "duck soup" for Cornet. I believe this is a good match, and I would like to nominate him. Address: Dr. Bruce Cornet, 14222 Kimberley Lane, #411, Houston, TX 77079.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

THE PENNSYLVANIA STATE UNIVERSITY  
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3 November, 1987

Dr. Bruce Cornet  
14222 Kimberley Lane, #411  
Houston, TX 77079

Dear Bruce:

As you can see, I have nominated you for a curatorial job in Austin. Clearly the job is beneath your dignity, but it's a foot-in-the-door, and you could build it into something good. I believe a museum such as Texas Memorial would be a good place for you to be in the long run. If I were you I'd get in an application (enclosed) and go see them, hat in hand, promptly. "Any port in a storm...."

All the best.

Yours very truly,

Alfred Traverse ✓  
Professor of Palynology

AT/et  
encl: copy of letter to Dr. Durden  
application form from "

THE PENNSYLVANIA STATE UNIVERSITY  
DEPARTMENT OF GEOSCIENCES  
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435 Deike Building  
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(814)863-3419 or (814)865-6393

13 October, 1987

Houston Independent School District  
Personnel Services  
3830 Richmond Avenue  
Houston, TX 77027

Dear Friends:

Your evaluation form for Bruce Cornet has come in. I have known Dr. Cornet well for many years, beginning in 1972, when he arrived here to do a Ph.D. under my direction. However, I feel that the form, with its various categories, mostly does not apply to the relationship we've had. Therefore, I hope that you'll accept this letter in lieu of the form.

Bruce is one of the most interesting people I have ever known, and it is agreed here that his 1977 Ph.D. thesis was the best such in decades. He is imaginative, industrious, enthusiastic. I would imagine that his work with students of biological and/or earth sciences would be exceptionally stimulating to them. He has a wealth of knowledge about all aspects of science and is very good at communicating his knowledge--he speaks very well. However, my only direct experience with his teaching is his work here for one semester as a teaching assistant in our large enrollment geology course. He did very well.

As a high school (or middle school) biology teacher in your system, Bruce would surely turn students on. He has a lot of ability, is dependable, has a good sense of humor, is well organized and works well with others.

Yours very truly,

Alfred Traverse  
Professor of Geology & Biology

AT/et  
encl: HISC form (signed but not completed; see explanation above)

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2 October, 1987

Dr. Bruce Cornet  
14222 Kimberley Lane, #411  
Houston, TX 77079

Dear Bruce:

"Adobe" Chair--are you sure it isn't a cruel hoax?

Anyway, I will of course write an adulatory letter. Which reminds me that I would ask others over the phone (so you can evaluate the tone of voice). There are a lot of folk out there whom you may regard as friends who would say things that would give search committees some problems.

Best.

Yours very truly,

Alfred Traverse

AT/et

Professor Alfred Traverse  
Palynological Laboratories  
435 Deike Building  
The Pennsylvania State Univ.  
University Park, PA 16802

September 27, 1987

Dear Al,

Texas Tech University in Lubbock has advertised a position in the AAPG Explorer for an Adobe Chair in Geosciences. I have given your name as one of my three references. The chair will be awarded by September 1988.

"Applicants, having a Ph.D. in geosciences or a closely related field, are expected to possess an understanding of hydrocarbon origins, accumulations, and recovery techniques, as well as entrepreneurial insights into the economic exploration for and marketing of hydrocarbons. The recipient will be expected to be involved in teaching and to develop an appropriate research program."

If contacted, I would greatly appreciate your assistance by responding to their requests. I have asked for an interview, which will be necessary if I wish to have any chance at getting the appointment.

I understand that the position was created specifically for professionals in the depressed oil industry, and my qualifications fit the requirements. My research program will center around the value of dry holes and accurate geologic interpretation (which is dependent on good paleontological, geochemical, and geophysical data) in the economic exploration for oil and gas, and the need for an informed exploration program by independents. Education of the investment sector may be the most important and neglected area of the oil industry. The cyclical nature of investment is the product of "the one well stand", with too much emphasis placed on immediate investment return.

Sincerely yours,

*Bruce*

Bruce Cornet, M.S., Ph.D.  
14222 Kimberly Ln. #411  
Houston, TX 77079  
(713) 558-5701



New Mexico Institute  
of Mining and Technology

Socorro, NM 87801

Department of Geoscience

(505) 835-5634

12 May 1987

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

Dear Dr. Traverse:

On behalf of the Search Committee I wish to extend my thanks to you for writing a letter of reference in response to an open position in Geochemistry for a sabbatical leave replacement in the Geoscience department, for the applicant **Bruce Cornet**.

Letters of reference such as that submitted by you were extremely important to the Search Committee when reaching a list of finalists for this position.

Thank you very much for taking the time to write.

Sincerely,

A handwritten signature in dark ink, appearing to read "Philip R. Kyle". The signature is written in a cursive style with a prominent flourish at the end.

Philip R. Kyle  
Associate Professor of Geochemistry  
(505) 835-5995

THE PENNSYLVANIA STATE UNIVERSITY  
DEPARTMENT OF GEOSCIENCES  
PALYNOLOGICAL LABORATORIES  
435 Deike Building  
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30 March, 1987

Dr. David I. Norman  
New Mexico Institute of Mining & Technology  
Department of Geoscience  
Socorro, NM 87801

Dear Dr. Norman:

*file*  
I am responding to your inquiries about the qualifications of Bruce Cornet regarding teaching and research in connection with the temporary opening in your department.

Bruce is very well known to me, as he did his Ph.D. research here under my direction. He is a very talented and hardworking researcher, whose Ph.D. thesis was an extremely important work. It was characterized by one of my colleagues as the most important Ph.D. thesis in the history of our department. Dr. Cornet is a somewhat controversial figure in our field, as much of his research is unorthodox, and some of his best contributions are not yet published because he won't cut out parts others find offensive, to get them in print.

He was very good in all aspects of our mutual endeavors--field work, especially. His interests are quite broad in geology. Since leaving here, he has worked capably for several oil companies and has organized his own oil-drilling venture (Geminoil, Inc.).

As far as teaching ability is concerned, I can say that as a teaching assistant for a couple of terms, he did quite well here. He is a good speaker. He is a very interesting person. He will turn students on.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et



New Mexico Institute  
of Mining and Technology

Socorro, NM 87801

Department of Geoscience

(505) 835-5634

20 March 1987

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

Dear Dr. Traverse:

*file*  
Dr. Bruce Cornet has applied for a temporary position to replace Philip Kyle while he is on sabbatical leave for the 1987-88 academic year. He has given your name as a referee.

The duties of the person selected will be to teach several courses in the area of geochemistry-petrology and to interact with graduate students in these areas.

I would most appreciate it if you would write a letter addressing Dr. Cornet's qualifications regarding teaching and research.

Sincerely,

David I. Norman  
Associate Professor of Geochemistry  
(505) 835-5404

23 January, 1987

Prof. Bran Potter  
Department of Forestry and Geology  
The University of the South  
Sewanee, TN 37373

Dear Dr. Potter:

A former Ph.D. student, Bruce Cornet, tells me that he has applied for your assistant professorship position. Bruce asked me to write you in support of his application, and that is the purpose of this letter. He is, along with many other talented scientists, currently unemployed (though continuing to do research) as a result of problems in the oil industry.

Bruce is a very talented and hardworking researcher, whose Ph.D. thesis here was an extremely important work (characterized by one of my colleagues as the most important Ph.D. thesis in the history of our department). Dr. Cornet is a somewhat controversial figure in our field, as much of his research is unorthodox, and some of his best contributions are not yet published because he won't cut out parts others find offensive, to get them in print.

He was very good in all aspects of our mutual endeavors--field work, especially. His interests are quite broad in biology and geology. Before coming here he did an excellent and much quoted piece of work for an M.Sc. in the Biology Department at the University of Connecticut. Since leaving here, he has worked capably for several oil companies and has organized his own oil-drilling venture (Geminol, Inc.). In the latter activity he displayed considerable administrative ability, even though the wells drilled turned out to be dry holes!

As far as teaching ability is concerned, I can say that as a teaching assistant for a couple of terms, he did quite well here. He is a good speaker. He is a very interesting person. He will turn students on.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

Professor Alfred Traverse  
Palynological Laboratories  
435 Deike Building  
The Pennsylvania State Univ.  
University Park, PA 16802

January 14, 1987

Dear Al,

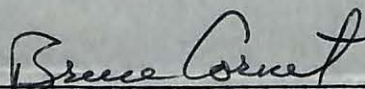
Another teaching position in geology has emerged. Would you please send a reference letter, like the one I requested for Oberlin, to:

Prof. Bran Potter  
Department of Forestry and Geology  
The University of the South  
Sewanee, Tennessee 37375

Dr. Potter has indicated an interest in me for an assistant professorship in their joint department; he says that my resume indicates experience that relates to both fields within their department. He has asked for names and phone numbers of references, and I have given him yours. But just to play it safe, perhaps a short letter would help.

Thank you.

Sincerely yours,



Bruce Cornet, M.S., Ph.D  
14222 Kimberly Ln. #411  
Houston, TX 77079

Professor Alfred Traverse  
Palynological Laboratories  
435 Deike Building  
The Pennsylvania State Univ.  
University Park, PA 16802

*Handwritten notes in blue ink, including the name "Traverse" and several illegible scribbles.*

November 8, 1986

Dear Al,

You asked me to request in writing any slides or palynological preparations from Volcan Ediger's Ph.D. thesis material. I would prefer fresh preparations of the following core samples, but would be willing to borrow and return slides if no residue remains for some of the samples.

Well VACH-2, samples 201 to 242 (samples 226-242 may be barren of palynomorphs).

Well VAPW-2, samples 155 to 160 (samples 108, 112, 113, and 114 may be the only productive samples).

Well Tuckahoe-1, samples 1 to 20.

Outcrop samples: VE-1, VE-2, VE-3, VE-4D, VE-5A, VE-5B, VE-6A, VE-6B, VE-7A, VE-7B, VE-8, BG-103, BG-104, BG-105, and BG-106.

I am not sure which samples from the wells and outcrops will be useful. Presumably, if a residue exists and was saved, it will contain something.

For your effort (there appear to be a lot of samples) in preparing slides, I will send you the results of my counts and identifications, as well as any comments about correlations with the Horner and Bailey wells. I am interested in comparing Ediger's work with mine. I believe I can do a more thorough and meaningful job.

Thank you for your help and support.

Sincerely yours,

*Bruce Cornet*

Bruce Cornet  
14222 Kimberly Ln. #411  
Houston, TX 77079

P.S. I am waiting on Goodwin for maps to complete the figures.

*stomed & care?*

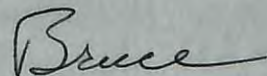
I take it that you did not volunteer comments from Doyle and others at the AASP meeting over the phone because they were not too encouraging or complimentary. I am not sure if anyone at the meeting saw my paper on Sanmiguelia, because several colleagues and university libraries that receive Evolutionary Theory hadn't received my volume in time for the AASP meeting. I probably can predict the criticism. Sergio Archangalsky and Anne Reymond examined the specimens at Texas A & M last Tuesday, and they are overwhelmed with the degree of preservation, clarity of detail, amount of material and information, and the completeness of the fossils. Both paleobotanists are now convinced I have the key, and are pretty upset with Hickey and Doyle for their censorship of papers that go against their theories or beliefs. Archangalsky said he intends to write and publish a commentary on the problem, since Hickey rejected a paper of his for Science, which describes some South American Early Cretaceous angiosperm leaves that do not fit Hickey and Doyle's scheme of things (too bad!).

Doyle has told others at the Univ. of California that he doesn't believe Sanmiguelia is an angiosperm (but he hasn't seen the specimens). Dilcher told me once in jest that if I publish a paper on Sanmiguelia and conclude that it is an angiosperm, I will be crucified. If that happens, guess who the losers will be. I regard Hickey as the bully on the block or inquisitionist of "heresy". Defending a theory or territory by blocking those who have contradictory evidence will only reflect badly on the defenders. There are a growing number of paleobotanists and palynologists who are fed up with the politics (nonscience) of angiosperm paleobotany, and I predict that their voices will be heard once they read my paper.

Before you are overly influenced by my critics, I suggest that you carefully read my paper. My interpretations may be strong, but so too is the evidence. I hope and pray that you will see what I see. No one is more blind than he who does not wish to see. The words of critics can cut like a sword, but the sword can cut as easily in their direction as it can in mine.

I have for a long time been impressed with the suggestion that you made in an NSF grant proposal (which led to the grant that supported me at Penn. State) that the Triassic may yield clues to angiosperm origins. You cast some seeds on unknown soil and it took some time for the tree to grow, but look at the fruit on the branches! I cannot thank you enough for the opportunity and objective environment for scientific inquiry at Penn. State.

Thankyou.



Bruce Cornet

24 October, 1986

Dr. Bruce M. Simonson  
Chairman, Department of Geology  
Oberlin College  
Oberlin, OH 44074

Dear Dr. Simonson:

*✓ file  
Cornet*

A former Ph.D. student, Bruce Cornet, tells me that he has applied for your position in paleontology, etc. Bruce asked me to write you in support of his application, and that is the purpose of this letter. He is, along with many other talented scientists, currently unemployed (though continuing to do research) as a result of problems in the oil industry.

Bruce is a very talented and hardworking researcher, whose Ph.D. thesis here was an extremely important work (characterized by one of my colleagues as the most important Ph.D. thesis in the history of our department). Dr. Cornet is a somewhat controversial figure in our field, as much of his research is unorthodox, and some of his best contributions are not yet published because he won't cut out parts others find offensive to get them in print.

He was very good in all aspects of our mutual endeavors--field work, especially. His interests are quite broad in biology and geology. Before coming here he did an excellent and much quoted piece of work for an M.Sc. in the Biology Department at the University of Connecticut. Since leaving here, he has worked capably for several oil companies and has organized his own oil-drilling venture (GeminOil, Inc.). In the latter activity he displayed considerable administrative ability, even though the wells drilled turned out to be dry holes!

As far as teaching ability is concerned, I can say that as a teaching assistant for a couple of terms, he did quite well here. He is a good speaker. He is a very interesting person. He will turn students on.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

24 October, 1986

Dr. Bruce Cornet  
14222 Kimberley Lane, #411  
Houston, TX 77079

Dear Bruce:

Enjoyed yours of 6 Octobdr. Too bad about your various MS problems.

The Sanmiguelia stuff sounds great. You may yet make a believer of, say, Henry Andrews.

Re the Richmond Basin MS, I am sure I can mediate the age discrepancy-- indeed, I will mention and illustrate it in my talk at AASP next week.

But what I need from you to get this rolling is the items of drafting (figures) you were going to contribute. Once I have those, I'll act as the "editor" to get together an MS all can (I hope) accept.

Thanks for good wishes. They are reciprocated.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

Professor Alfred Traverse  
Palynological Laboratories  
435 Deike Building  
The Pennsylvania State Univ.  
University Park, PA 16802

October 11, 1986

Dear Al,

Finally a teaching position has been advertised in Geology for which I am well qualified. Would you please send a letter of recommendation to Bruce M. Simonson, Chairman, Department of Geology, Oberlin College, Oberlin, Ohio 44074.

The advertised position is for a paleontologist with a comprehensive background in geology, a broad specialization in paleontology, and some teaching experience. The position starts during the 1987-88 academic year. Initial appointment will be for a period of 4 years. The main teaching responsibilities will be a semester course in paleontology, a semester course in Earth history, and 2 other semester-long courses (or their equivalent), 1 of which might be an advanced course related to the incumbent's specialty. Incumbent will also be expected to undertake a program of original research, for which start-up funds are available.

My Curriculum Vita emphasizes my broad training and background in palynology, paleobotany, geology, and vertebrate paleontology. I have been a lab. teacher or assistant in four courses. Having been in industry for nine years, my list of publications is not as long as that of an academic counterpart, but certainly longer than that of most people in industry. Much of my research time during the past eight years has been spent collecting data. Only in the past two years have I had the time to devote to writing (having a personal computer helps), and as you know, I have two publications in 1985-86, and am working on two other manuscripts. I suspect that they will want me to teach some invertebrate paleontology. I have had introductory invertebrate paleo. as part of general geology at U. Conn. I am confident that I could teach a broad-based paleontology course that is well balanced between the sciences of paleontology. My oil company experience and geological background would fit in well with a geology department consisting of only 4 full-time faculty members.

I am looking to you, Al, for help in getting me out of this oil slump and back into academia. Once my financial worries have been lifted, I will be in a much better position to work with you on current and future research and writing.

Sincerely yours,

*P.S. Deadline Nov. 20, 1986*

Bruce  
Bruce Cornet  
14222 Kimberly Ln. #411  
Houston, TX 77079

Prof. Alfred Traverse  
Palynological Laboratories  
Deike Building  
The Pennsylvania State University  
University Park, PA 16802

October 6, 1986

Dear Al,

Thank you for your recent letters. I pray that the Lord will comfort you during the days of uncertainty with your mother. The past several years with Bonnie and her health problems have been equally uncertain and emotionally draining, but I have grown stronger in willpower and determination to get through adversity.

The last month has been very hectic. I have been pushing two other manuscripts through the review process, succeeding in getting one accepted, but losing to overly critical reviews on the other. My colleagues want my data on angiospermid pollen, but they don't like all of my interpretations. Unfortunately, censorship in science walks a fine line at the dark edge of Constitutional guarantees of freedom of press and speech and beliefs. How many ideas have others published that have later fallen to new discoveries or interpretations? Why should my ideas be squashed simply because today's "experts" are not in agreement with all of them? What Bob Clarke and AASP trustees did in rejecting my pollen paper has hurt science more than it has hurt me, because I am now determined to publish that manuscript elsewhere before attempting any formal systematic presentation. I will endure and have my day!

For the past several weeks I have been travelling, not because I am suddenly rich, but because of the kindnesses of others. I made another trip to Sunday Canyon, returning with several more Sanmiguelia inflorescences and flowers, as well as a new and exciting inflorescence that may change ideas about the Bennettiales. Bonnie and I just returned from our honeymoon to Disney World in Florida - a gift from her father. Consequently, I have had little time for our joint manuscript, and my concerns now for a job are priority one.

I am disappointed that Volkan edited out my proposed compromise to the age of the Richmond Basin sequence, and pushed his interpretation in figure 5 by showing the Productive Coal Measures and Barren Beds as only Ladinian. Having been buffeted repeatedly by reviewers of other manuscripts, I am sensitive to those types of changes that neglect or bias one side of the issue. Volkan's statement that the early Carnian now includes the middle Carnian obscures, if it doesn't conceal, the possibility of a middle Carnian age for the younger Richmond Basin sediments. Obviously, Volkan will continue to change the manuscript to suite his objectives and interpretations, and you must mediate in order to provide a balanced opinion of age.

I do not want to coauthor a paper that favors a Ladinian-early Carnian age at the expense of evidence which I feel more strongly indicates an early-middle Carnian age. Volkan's statement that the coal measures produce a palynoflorule that more closely compares with that of the Ladinian ignores earlier data that lead me and others to recognize a strong resemblance between that assemblage and middle Carnian palynofloras. The only strength to Volkan's argument comes from the abundance of Aratrisporites. As much as we both now recognize that Aratrisporites increases in abundance going into the Middle Triassic, there is little else in the coal measures palynoflora that favors a Ladinian age. Thus, I am not opposed to suggesting that the oldest strata in the Richmond Basin may be late Ladinian; I am opposed to ignoring the data that, based on current knowledge, more strongly favors an early to middle Carnian age for the coal measures.

Please bear with me. I will try to find some time to make corrections to the manuscript, but my attention has been occupied by other important matters lately, particularly money. Once the Sanmiguelia paper appear in print, which should be shortly, one of my worries and concerns will be lifted. I will first send back the corrected manuscript with changes to some of the mock-up figures. Once the text and figures are accepted by all of the authors, I will begin to draft the final figures. Otherwise, someone will spend lots of time drafting for naught.

Best wishes to you also, and may your personal concerns be replaced by the peace that comes with faith.

Sincerely yours,



---

Bruce Cornet  
14222 Kimberley Ln. #411  
Houston, TX 77079  
(713) 558-5701

30 September, 1986

Dr. Bruce Cornet  
14222 Kimberley Lane, #411  
Houston, TX 77079

Dear Bruce:

Thanks for the birthday card. I still enjoy birthdays, but they are sure piling up. (Think of my mother!)

I need to hear from you re the Ediger et al. paper--drafting job, etc. VE has left me with the job of coordinating the whole mess. I will try.

Best wishes to you also for success in the coming year.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

20 Sept., 1986

Dr. Bruce Cornet, etc.

Dear Bruce:


Self-typed, so watch out!

The enclosed copy of letter to B. G. makes the meaning of the various xerexed sheets clear--I presume you have a paginated copy of the MS.

As you know, I'm supposed to act as the de facto editor for all of this. I would like your comments on BG's corrections, additions, etc. I would also like your revisions and figures you are drafting, etc.

We are off again to Indy, as you will note in the BG letter.

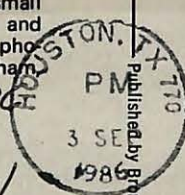
Best regards,

  
Alfred Traverse, etc.

enclosures

A 0.05mm thick section through the stem of small palm *Rhapis excelsa*, stained with safranin and hematoxylin. Magnification ca. x 20. (Color photograph by M.H.Z., Harvard Forest, Petersham, Massachusetts)

9/7/86



Sinclair Lewis

HAPPY 150th  
ANNIVERSARY  
USA  
14  
AUG 1880, 1986

Dear Al,

Happy birthday and  
thanks for the card. Hope  
this year brings you  
NSF funding and some  
bright young students.  
If you hear anything via  
the grape vine about my  
work, please inform me.

Gene

Published by Gremlay & Company, Inc., Boston, Mass. 02210



MIKE ROBERTS  
BERKELEY 94710

SA2195

Prof. Alfred Traverse  
Palynological labs.  
Deike Building  
The Penn. State Univ.  
Univ. Park, PA 16802

Volkan S. Ediger  
Alfred Traverse  
Dept. of Geosciences  
435 Deike Building  
The Pennsylvania State Univ.  
University Park, PA 16802

*file* *Conrad*

June 22, 1986

Dear Volkan (and Al),

Enclosed please find my revisions to our manuscript. The text-figures will have to be redrafted, and I volunteer to do that task, since I possess the data necessary to make the corrections. I have asked Dan Ziegler of SEPCo to join us as a coauthor; as a coauthor he will provide the necessary seismic lines, their reduction for publication, and interpretations of seismic stratigraphy. I have put my revisions in bold type, so that you can distinguish them from your original. Much more data needed to be added for clarification, since descriptions of some of the members were inadequate or incorrect. I have incorporated most of my ideas about structure and stratigraphy, and together with your ideas and those of Goodwin and Ziegler, we should have a significant contribution.

There are several areas of important disagreement, which I have addressed in the paper. The first is the age of the Richmond Group. Instead of giving only one interpretation, I have presented two interpretations of age. Perhaps after reading my arguments, you may want to go with only one interpretation. After reviewing the data, I feel that there is no strong evidence for an age older than Carnian, and have compromised with an early to middle Carnian age assignment.

The Boscobel quarry exposures and the coal measures on the western side of the basin do not belong in the lower Tuckahoe Formation. Seismic and palynological data indicate that they are part of the upper Tuckahoe Formation, i.e. Vinita Beds Member, and that there are actually two coal measures of significantly different age in the Richmond Basin. I have modified the text to include this information.

The detailed lithologic descriptions of each formation and member are based on careful and comprehensive geologic interpretation, and if you have any problems understanding the stratigraphy from my discourse, perhaps I need to add something, such as detailed time-slice maps for each member, showing concepts of facies relationships through time.

You made a comment that the northern part of the basin could contain younger strata than the Tuckahoe Formation.

I agree. Thin remnants of Otterdale or Turkey Branch strata could exist north and south of the James River, but identifying them is the problem. If you understand my descriptions of structural development and facies relationships for the Turkey Branch Formation, you will also understand that the northern part of the basin had to have been structurally arched or domed during Turkey Branch time. It probably remained a positive area once formed, and probably supplied some reworked Tuckahoe sediments to the Turkey Branch Fm. Even though sediments may have lapped up onto that dome or arch, it continued to seek erosional base level, because basement underneath was being thrust and forshortened through wrench tectonics in that area.

A seismic line across the basin just south of the James River shows several basement thrust faults splaying upwards through the Triassic section, with the probable displacement of older strata over younger strata. Palynologically, I have found no strata in outcrop that are older than the middle Vinita Beds Member in the area of the seismic line (when correlated with the Horner well). The coal measures on the eastern side are separated from the Vinita Beds by a major west-dipping reverse fault that is visible on the seismic line. Instead of basement shallowing on the eastern side, it seems to deepen. The Productive Coal Measures Member may have been thrust from the east up and over Vinita Beds, since basement appears to continue eastward past the point where outcrops of Petersburg Granite say the basin should end. Also, basement deepens on the western side south of the James to over 5,000 ft. The shallowest part of the basin there is in the middle of the basin (as suggested by Goodwin, 1970). The horst that Goodwin (1970) mapped on the western side appears to be floating on top of Triassic, as though it represents either an overslide from the east, or perhaps even a detached western block that slid eastward into the basin during deposition.

We are assuming, of course, that the coal measures around the Midlothian and Blackheath districts correlate with the coal measures of the Winterpock District and those near the base of the Horner and Bailey wells. We could be wrong! Do you have good palynoflorules of the Blackheath and Midlothian coals? The few palynoflorules I have of the northeastern coal measures look different from those of the Winterpock area! Keep in mind the fact that the Adamson well showed only traces of coal above the Lower Barren Beds Member, and that our knowledge of the coal flora comes almost exclusively from the Winterpock area (northern coals only seem to preserve remains of articulates). Is it possible that the deltaic complex at the top of the Vinita Beds Member contains the majority of coal in the northern part of the basin? For now, at least, we can continue with the concept that eastern coals are the lowest coals, unless you wish to re-examine your slides.

I have completed and submitted two manuscripts, one on angiospermid pollen, and the other on Sanmiguelia lewisi. The pollen paper is now being revised. The reviewers will let it be published, but are not happy with the way I presented the data (no systematic descriptions; descriptions are collated with interpretations). The S. lewisi paper is 66 pages long with eight plates, and it systematically gets to the heart of the controversy with critical new data. If I can get it past reviewers largely intact, concepts of angiosperm evolution will change dramatically. Either that, or we will have two camps doing battle, with a war of attrition resulting. I also included descriptions and illustrations of several angiospermous fruiting structures from the Richmond Basin.

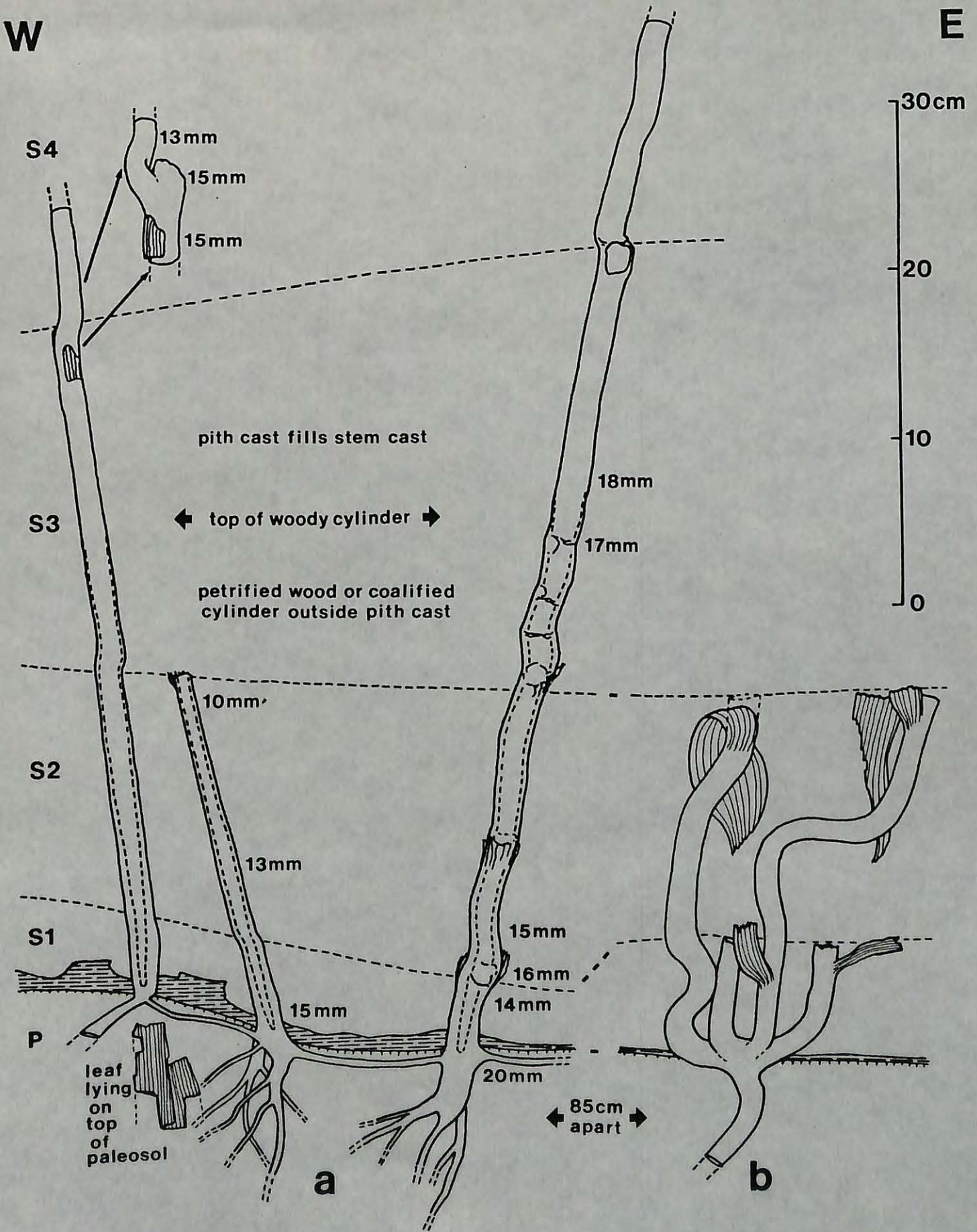
Just this last month I retrieved a megafossil collection from storage that I had collected in the Taylorsville Basin during July, 1981. After examining the numerous types of plants in the collection, I found another Triassic angiosperm. The palynoflorule from that locality contains common Liliacidites pollen. The remains of this plant include flowers that seems to possess a small multiloculate central gynoecium with a long apical stigma, surrounded by lobed petals and sepals! Its well preserved stems with individual anastomosing and bifurcating vascular bundles and lanceolate leaves with anastomosing and bifurcating parallel venation suggest that it may be a small herbaceous monocot. By the way, we have all been to that locality.

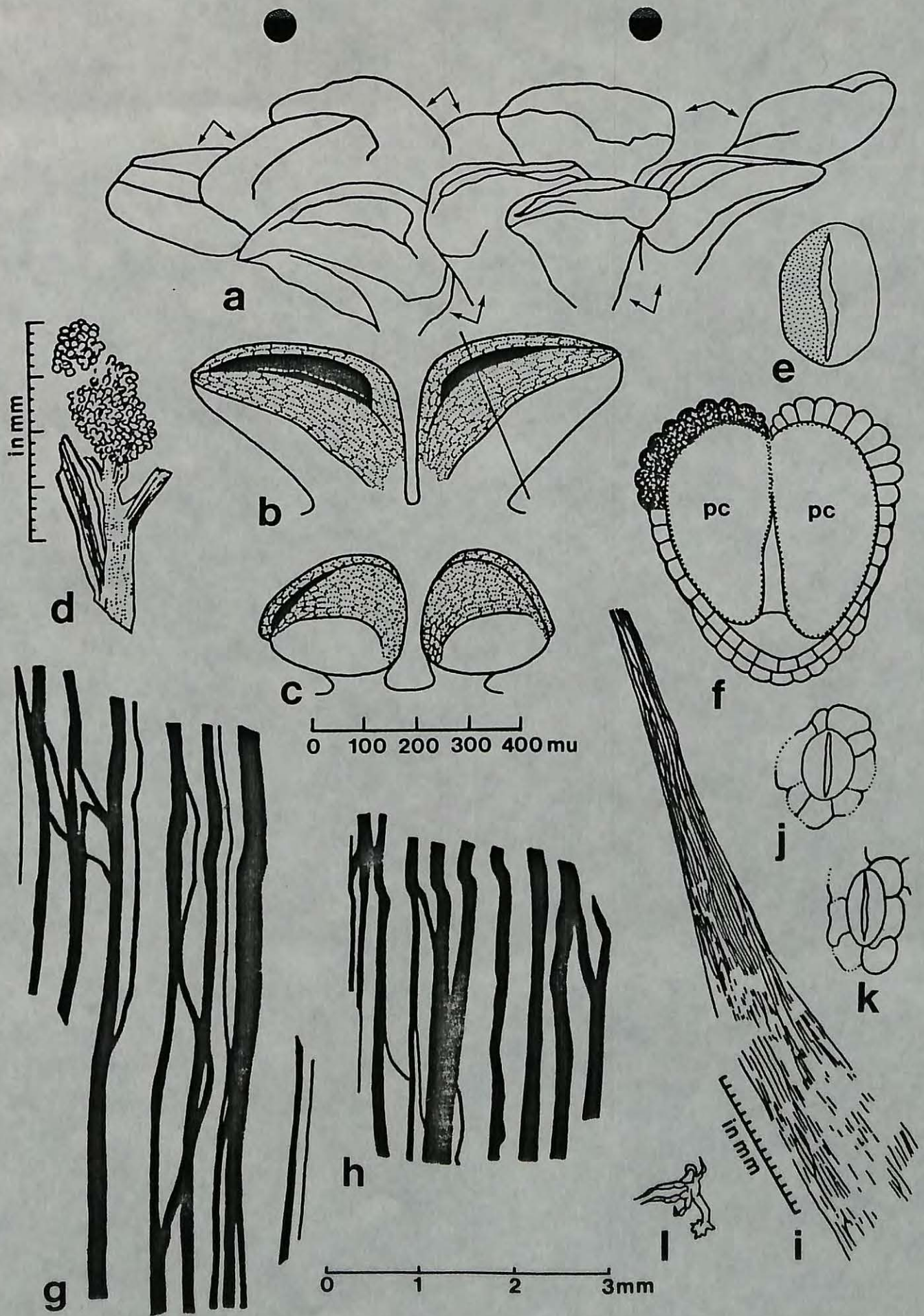
Sincerely yours,

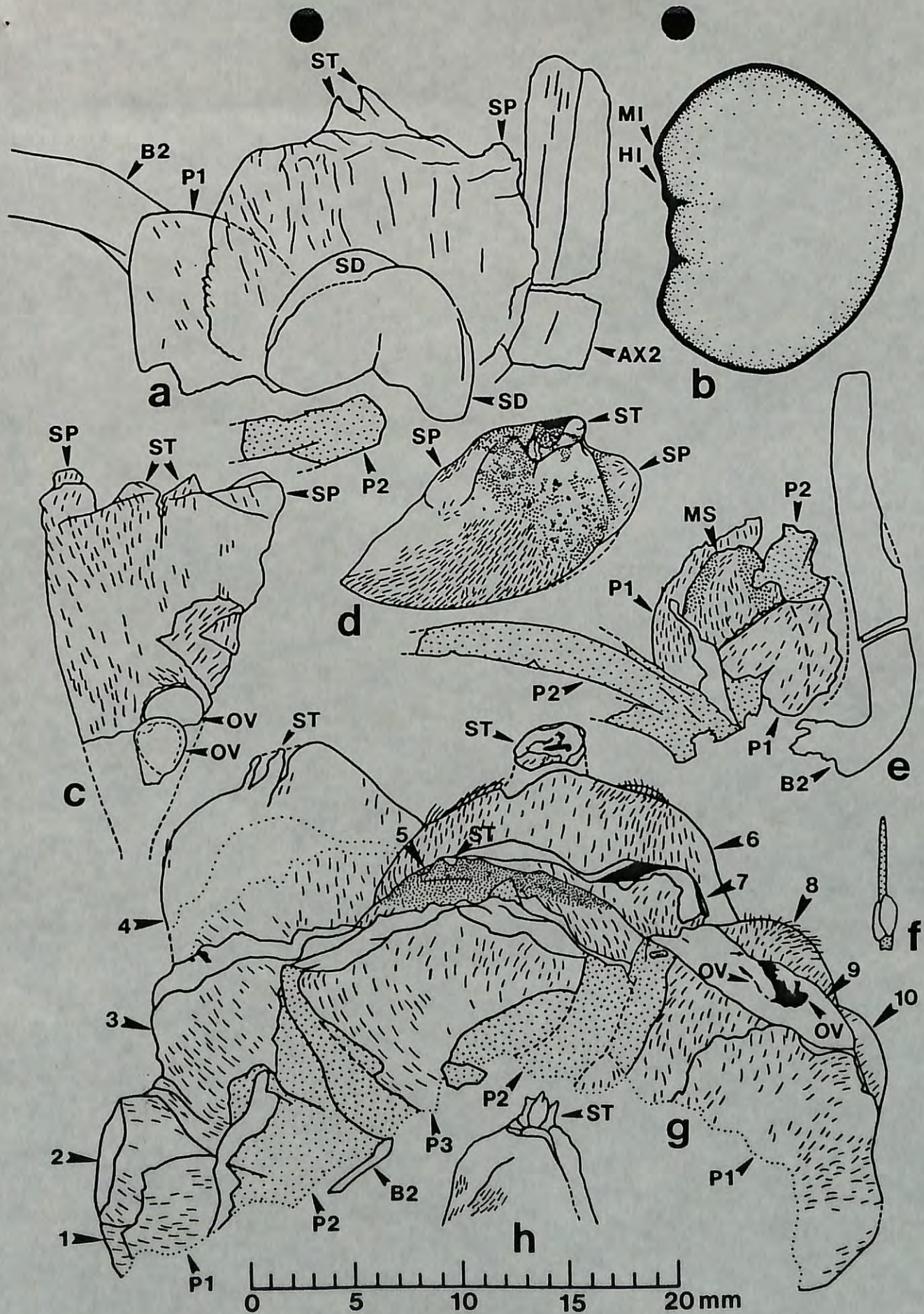
Bruce

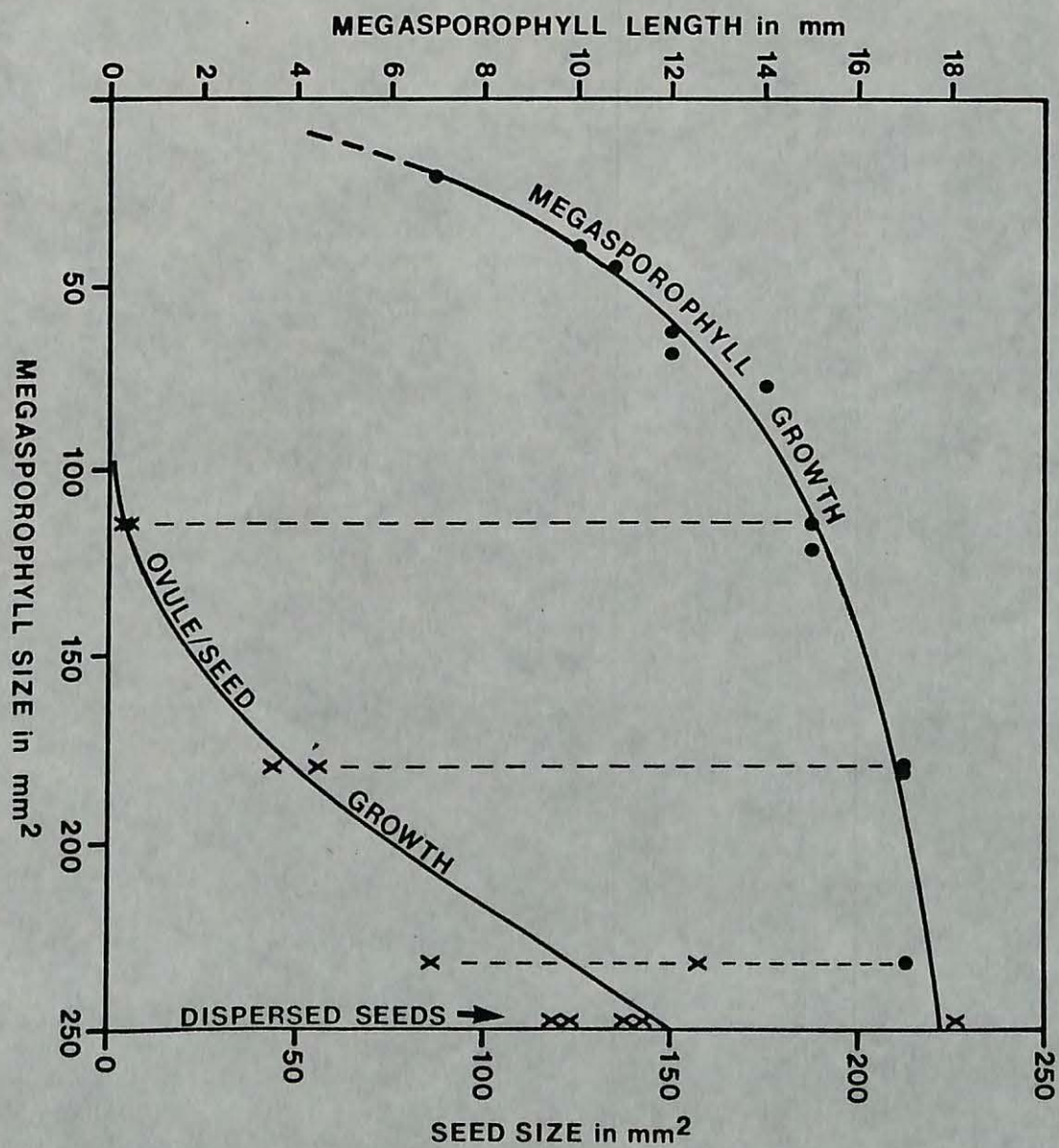
Dr. Bruce Cornet  
14222 Kimberley Ln. #411  
Houston, TX 77079  
(713) 558-5701

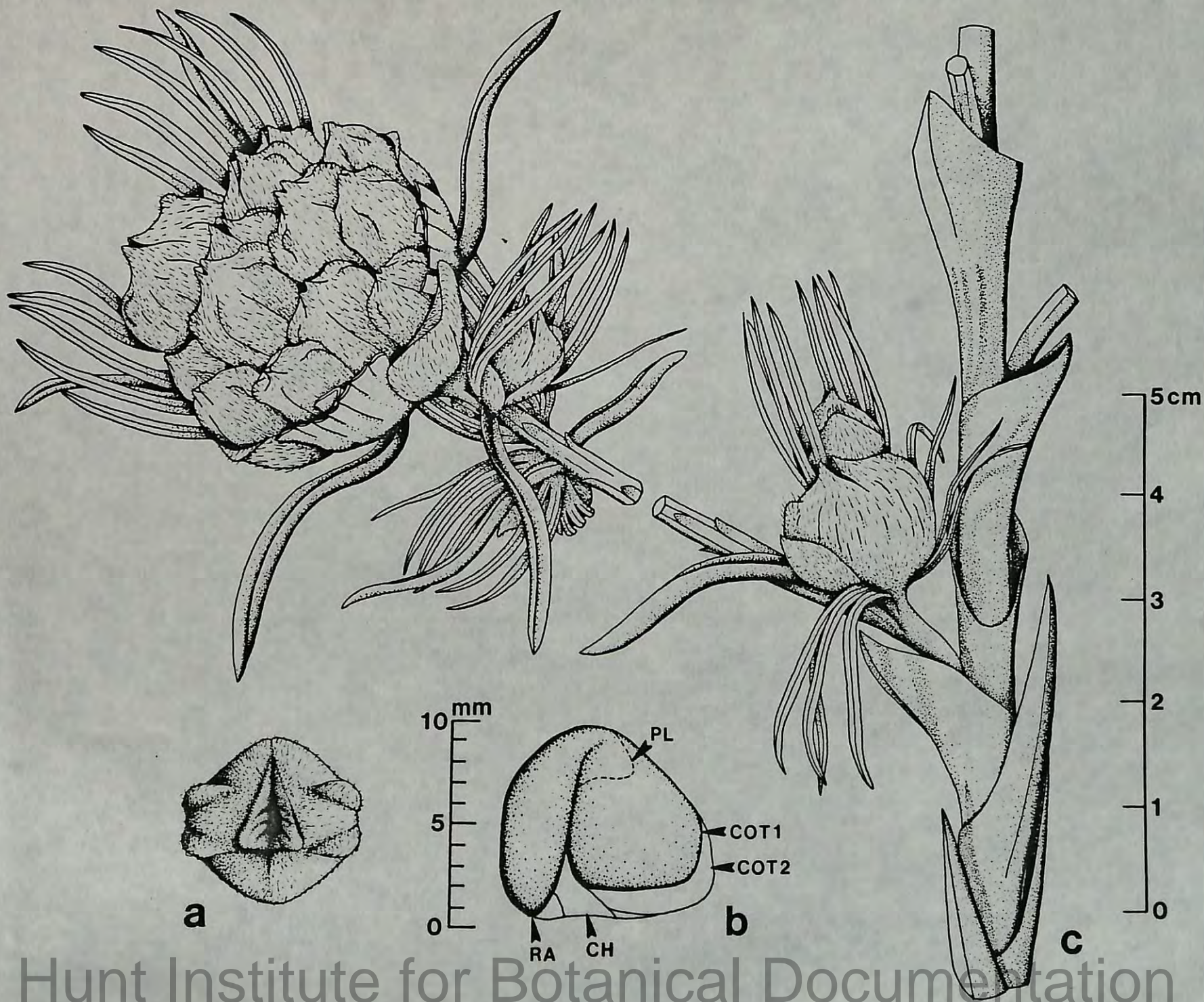
cc: Dr. Goodwin  
Dr. Traverse











OVULE AND POLLEN BEARING REPRODUCTIVE AXES OF SANMIGUELIA LEWISI

Reconstructions based on tracings of nearly complete specimens with minimal interpretation.



*Synangispadix fourrieri* Cornet gen. and sp. nov.

*Axelrodia burgera* Cornet gen. and sp. nov.

29 May, 1986

Dr. Burce Cornet  
14222 Kimberley Lane, # 411  
Houston, TX 77079

Dear Bruce:

What's with the new address? Is it just a new business address, or a change of residence? We hope you are seeing some bright spots ahead!

Volkan and I have yours of 13 May re the MS. Volkan will have to make revisions acceptable to both parties!

We like your "Turkey Branch Formation" and the member names better than B.G.'s "Chesterfield Formation", etc. We hope you will go along with the careful combination of ideas Ediger eventually comes up with.

We look forward to getting your revised MS.

Best.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et  
cc: V. Ediger

3 April, 1986

Dr. Bruce Cornet  
13567 Portobello Drive  
Houston, TX 77083

Dear Bruce:

Re your literature request--this is a very busy time here, and I can only take a little while to try to help. ~~First~~ of all, you should write Brugman (Mr. W. A. Brugman, Lab. Palaeobotany & Palynology, Heidelberglaan 2, 3584 CS Utrecht, Netherlands) requesting a copy of his 1983: Permian-Triassic palynology. I send just a few pertinent pages. Van der Eem (1983) Aspects of middle and Late Triassic palynology. 6. ...Ladinian and lower Karnian.... (Rev. Palaeobot. Palynol. 39:189-300), you can presumably get in the library (let me know if that's not the case). Reitz's thing is probably not so easily obtainable--I enclose a xerox. The three theses we were sent from Utrecht are too big a xerox job, of course.

Hurriedly, all the best to you.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et  
encl

P.S. I'd try to cooperate with Volkan on the MS, if I were you. I believe when it comes to who should be 1st author, an amicable solution will be found.

Alfred Traverse  
Volkan S. Ediger  
Palynological Laboratories  
The Pennsylvania State Univ.  
Deike Building  
University Park, PA 16802

March 6, 1986

Dear Al and Volcan,

In my recent letter, I did not take into account that Weems (1980) had separated out the transgressive lacustrine sequence of the middle depositional unit and called it part of the Falling Creek Member. In his paper and map of the Taylorsville Basin, Weems identifies the Falling Creek Member along Stagg Creek, and the 1985 Fieldtrip Guidebook describes two shale-bearing outcrops (Stops 1A and 1B) along Stagg Creek as belonging to that member. I had said in my letter that Weems had placed the transgressive lacustrine sequence in the Stagg Creek Member, but since he didn't, we can amend his sections to include that section along Stagg Creek in Weems' Newfound Member. Fortunately, Weems picked another sequence of outcrops along Falling Creek to be his type section for the Falling Creek Member.

In the Appendix to Weems (1980), the stratigraphic sections described along Stagg Creek can be amended as follows: Intervals 1-32 = Stagg Creek Member. Intervals 33-179 = Newfound Member. Both members are retained in the Doswell Formation, while the Falling Creek Member is removed from that formation, since it is equivalent to the Vinita Beds. The Stagg Creek Member is about 763 feet thick along Stagg Creek, whereas in the Richmond Basin that member ranges from 250 ft. on top of structure (J.R.Hicks well) to just over 800 ft. off structure (Horner and Bailey wells). The transgressive lacustrine shale sequence along Stagg Creek (Weems' Falling Creek Mbr.) measures 1160 feet thick, whereas in the Richmond Basin (Horner and Bailey wells) that sequence measures about 1150 feet thick to the base of floodplain channel and overbank deposits (Weems' old Newfound Mbr.).

Palyniferous samples from the base of Weems' Stagg Creek Mbr. along Stagg Creek have a unique correlation with samples from the first hundred feet of the Stagg Creek equivalent in the Horner well. Palyniferous samples from plant-bearing shales at Stop 1B along Stagg Creek correlate moderately well with samples from the base of the lacustrine sequence in the Horner (base of the new Newfound Mbr.), and palyniferous gray to black shale interbedded between thick

sandstone beds (Loc. M) near Stagg Creek (Equivalent to Interval 72 in Weems' Falling Creek Mbr. along Stagg Creek, Appendix) correlate very well with shales at the base of the transgressive lacustrine sequence in the Horner. I have yet to process and correlate samples from Stop 1A, which should tell us where to place the base of the Newfound Member along Stagg Creek, if we elect to extend the Newfound Member as opposed to creating a new member.

It is important to realize that the Taylorsville Basin section along Stagg Creek contains nearly the full sequence recognized in the Richmond Basin subsurface as the middle depositional unit. Falling Creek (stratigraphic section 2 in Fig. 3, Weems, 1980) contains a sequence that palynologically correlates with the upper Vinita Beds. The Falling Creek section interfingers with deltaic sandstone bodies to the north. Sandstone becomes more common upwards in the section, and shallow wells drilled along South Anna River show that thin shales within a dominantly coarse sandstone section correlate with shales at the tops of Bauma cycle turbidites along the type section of the Falling Creek Member. Thus, it appears that there is a major facies change laterally from dominantly shale to dominantly sandstone, and the Falling Creek section (2) is transitional along the downslope margins of a major delta.

Weems interprets the Falling Creek section (2) to be about 50 ft. above the base of that member. Yet palyniferous samples from shales to the east (eg. stratigraphic section 3 in Fig. 3, Weems, 1980; incl. Locs. A, B, and F) correlate with the lower Vinita Beds. The absence of Aratrisporites in all but three samples (and in those only one specimen was counted in each) precludes a correlation with the lowest Vinita Beds or the coal measures. No samples have been processed from the lowest strata on the eastern side of the Taylorsville Basin, where Weems (1980) maps his Stagg Creek Member. Since Weems bases his identification of the Stagg Creek on the first occurrence of massive sandstones below the lowest limestones or carbonaceous shales of the Falling Creek Member, the significance of the sandstones on the eastern side of the basin is uncertain. If those sands truly belong to the Stagg Creek Member, then they are younger, not older, than the Falling Creek, and they overlie, not underlie, the Falling Creek Mbr. to the east. Keep in mind that the Stagg Creek Member unconformably overlies the Vinita Beds and Falling Creek Member.

Since only about 50 feet of sandstone-dominated section occurs east of section 3 (Fig. 3), faults could have either lowered the Stagg Creek Mbr. or raised the Barren Beds. The presence of Petersburg Granite in fault contact with these sandstones does not help the picture, because the structural

and stratigraphic relationships of the Petersburg are in question.

The Petersburg is generally thought to underlie Triassic sediments of the Richmond and Taylorsville basins. Yet, in those wells that have penetrated basement, the Petersburg was not present. Instead, a hornblend gneiss with some granitic gneiss and quartzite stringers or veins was present. In the southeastern part of the Richmond at Winterpock, the coal measures and underlying Barren Beds lie directly on top of extensive outcrops of hornblend gneiss. To the north at an old quarry along Coalboro Rd., Petersburg Granite can be seen to replace the hornblend gneiss across some major fractures or faults. Within those fractures are clay dikes from below, and mining reports (hearsay) indicated the existence of shales underlying the granite. Further to the north on the eastern side of the Richmond (near Lucks Lane), islands of Petersburg can be found isolated by surrounding Otterdale Sandstone, and it is clear that the Petersburg, at least locally, overlies the Otterdale. In the middle of the Richmond Basin weathered boulders of Petersburg Granite, schist, and hornblend gneiss lie directly on top of the Otterdale. In other words, a cataclastic zone may underlie the Petersburg. In the Boscabel Quarry the Petersburg Granite is isolated by faults from Triassic sediments, and, again, hornblend gneiss and metavolcanics underlie the Triassic.

In the Taylorsville Basin, the Hylas Zone cataclastics underlie the Triassic or are in fault contact with the Triassic. The Petersburg Granite seems to lie on top of both the Hylas Zone cataclastics and Triassic strata. For these reasons, it is important that the Petersburg Granite not be described as true basement. There is enough evidence to suggest that the Petersburg may overlie the Triassic, and it could represent an allochthonous Paleozoic thrust sheet that has either slid over or been thrust over the basins from the east. If it overlies the basins, then the Richmond and Taylorsville may be interconnected in the subsurface. Furthermore, U.S.G.S. Line #1, which crosses the northern tip of the Richmond and travels east of the City of Richmond, shows true basement to lie anywhere from 3,000 to 8,000 feet below the surface. Large basement ramps and thrust sheets project upwards, forming enormous anticlines and synclines, presumably overlain by Triassic sediments. Each syncline could represent another Triassic basin. The lack of shallow seismic data precludes the recognition of an overlying mass of granite. The seismic interpretation by Harris, deWitt, and Bayer (1982, U.S.G.S. Chart OC-123) is in error, since the actual processed seismic line shows resolution and detail that contradicts their interpretation of the subsurface distribution of Petersburg Granite.

In summary, the Stagg Creek and Newfound members can be redefined as the only members of the Doswell Formation. The Otterdale Sandstone can be called the Otterdale Formation. The Vinita Beds, Productive Coal Measures, and Barren Beds can be redefined as members of one formation, the Falling Creek Formation. I am not sure how the rules of stratigraphic nomenclature apply here, but there is a Falling Creek in both the Richmond and Taylorsville basins, and the same or near-equivalent strata crop out in both.

In my previous letter, I made a mistake when I said that the Otterdale SS and Vinita Beds belong to the Tuckahoe Group. They belong to the Chesterfield Group, and the Productive Coal Measures and Barren Beds belong to the Tuckahoe Group.

Thank you for my indulgences, and please consider my interpretations. I look forward to a joint paper that is both significant and accurate.

Sincerely yours,

*Bruce*

---

Bruce Cornet  
13567 Portobello Dr.  
Houston, TX 77083

cc. Dr. Goodwin

Mar 1986

Dear Al and Volkan,

Please share or copy the enclosed reprint. There are so few available, that I have to ration them out. It took more than a year just to get the page proofs, and many of the corrections were not made in the final publication. Paul Olsen will be sending me his corrections, and I will forward a copy onto you. The enclosed errata corrects some of the major errors, but not all the typos.

Thank you, Al, for giving me the opportunity to do this work, and I hope it brings you some long overdue recognition.

Sincerely yours,

*Bruce*

\_\_\_\_\_  
Bruce Cornet

10 February, 1986

Dr. Bruce Cornet  
13567 Portobello Drive  
Houston, TX 77083

Dear Bruce:

Many thanks for the packets of NC samples. I was especially interested because of beautiful examples of our Placopollis friends on your slide. One of these days I'll process the samples and have some more fun!

Just got a letter from Univ. Mo.-KC re the deanship, and I wrote a very upbeat letter. However, I suspect your lack of academic-administrative credentials will be fatal.

Have you canvassed small colleges of Texas/Oklahoma/Louisiana/Arkansas area? There are many of them. You are a good teacher and could do them a lot of good. Stephen F. Austin in Nacogdoches, etc., etc. I'd just go in person and ask for a job.

Best.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

10 February, 1986

Dr. Ronald MacQuarrie, Chair  
Search and Screening Committee  
School of Basic Life Sciences  
University of Missouri-Kansas City  
5100 Rockhill Rd.  
Kansas City, MO 64110-2499

Dear Dr. MacQuarrie:

Your letter of 7 January re my former Ph.D. student, Bruce Cornet apparently took a long time to reach my desk. I hope this has not inconvenienced you.

Bruce is a very talented, imaginative, hard working and resourceful researcher in our field. Indeed, his Ph.D. thesis here was one of the best and most influential doctoral dissertations I know of. He was also very good in all aspects of our mutual endeavors--field work, especially. His interests are quite broad in biology and geology. Before coming here he did an excellent and much quoted piece of work for an M.Sc. in the Biology Department at the University of Connecticut. Since leaving here, he has worked capably for several oil companies and has organized his own oil-drilling venture (Geminol, Inc.). In the latter activity he displayed considerable administrative ability, even though the wells drilled turned out to be dry holes!

As far as teaching ability is concerned, I can say that as a teaching assistant for a couple of terms, he did quite well here. He is a good speaker. He is a very interesting person. *He will turn student on.*

I am not able to comment on academic-administrative relationships as you request. I am not aware that Dr. Cornet has university experience, except as long-time student, and teaching assistant, in the pursuit of his three degrees.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

13 January, 1986

Dr. Bruce Cornet  
13567 Portobello Drive  
Houston, TX 77083

Dear Bruce:

I know I've struck out before on this sort of thing--BUT, is there a chance you could find a chunk of "Del & Hope Locality 43" (locality PK2)? I have a slide but no residue. Would like to process it. Is the "43" a locality of Hope's? If you can't come up with it, maybe I could call Bob Hope.

Thanks. Best.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et



UNIVERSITY OF MISSOURI - KANSAS CITY

*file Cornet*

Office of the Chancellor

5100 Rockhill Road  
Kansas City, Missouri 64110-2499

Chancellor  
(816) 276-1101  
Vice Chancellor/Academic Affairs  
(816) 276-1107  
Vice Chancellor/Administrative Affairs  
(816) 276-2754  
Vice Chancellor/Development  
(816) 276-1105  
Vice Chancellor/Student Affairs  
(816) 276-1141

January 7, 1986

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

Dear Dr. Traverse:

Dr. Bruce Cornet has recently submitted an application for the position of Dean of the University of Missouri-Kansas City School of Basic Life Sciences, and has listed your name as a reference.

The Dean Search Committee would very much appreciate having your candid assessment of Dr. Cornet's qualifications for the position of Dean. Of particular interest is your specific knowledge of Dr. Cornet's background in teaching and scholarship, administration and professional relationships with university and community groups. Please feel free to provide us with any additional information you feel relevant.

Thank you very much for assisting us in our efforts to select the best possible candidate for the position.

Sincerely,

*Ron MacQuarrie*

Ronald MacQuarrie, Ph.D.  
Interim Dean  
School of Basic Life Sciences  
Chair  
Search and Screening Committee

Dear Al and Betty,

We are looking to 1986  
with hope, since 1985 has  
not yet shown us any mercy  
with the Houston economy and  
job opportunities so discouraging.

Thank you for the info on a possible  
job in South Africa, but with  
our personal problems and Bonnie's  
health, we could not accept an offer.

No company's insurance program  
will take Bonnie, and her personal  
insurance would lapse if she  
left the country - so here we stay.

I am looking to possible work  
with Texaco as my only salvation  
since the USGS job in paleontology  
is out. Please pray for us and  
we will for you during these  
difficult times. With hope, peace  
in our hearts, and love we shall all  
make it. Best wishes, Bruce and Bonnie Lee

- 85

May you have the gladness

of Christmas

which is HOPE

The spirit of Christmas

which is PEACE

The heart of Christmas

which is LOVE

THE CORNET FAMILY  
BRUCE, BONNIE LEE, AND CHUCK

22 November, 1985

Dr. Bruce Cornet  
13567 Portobello Drive  
Houston, TX 77083

Dear Bruce:

Enjoyed contacts with you in Williamsburg!

Info on South Africa job enclosed.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et  
encl

TO AT

DATE 20 Aug. '85 TIME 12:35

WHILE YOU WERE OUT

M. Bruce Cornet

Of \_\_\_\_\_

Phone \_\_\_\_\_

TELEPHONED  PLEASE RETURN CALL

CALLED TO SEE YOU  WILL CALL AGAIN

RETURNED YOUR CALL  RUSH

MESSAGE Has finished MS

on Trias / Jurass.

angiosperm pollen.

(He mainly called to

find out how to get Ron.)

Signed BT

The Standard Register Company

PS Form 3811, July 1983

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

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

- 1.  Show to whom, date and address of delivery.
- 2.  Restricted Delivery.

3. Article Addressed to:  
 Dr. Bruce Cornet  
 13567 Portobello Drive  
 Houston, TX 77083

4. Type of Service:
- Registered
  - Certified
  - Express Mail
  - Insured
  - COD

Article Number

Always obtain signature of addressee or agent and **DATE DELIVERED.**

5. Signature - Addressee

X *Bruce Cornet*

6. Signature - Agent

X

7. Date of Delivery

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



DOMESTIC RETURN RECEIPT

PS Form 3811, July 1983

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

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

- 1.  Show to whom, date and address of delivery.
- 2.  Restricted Delivery.

3. Article Addressed to:  
*Dr. Bruce Cornet*  
*13567 Portobello Dr*  
*Houston, TX 77083*

4. Type of Service:	Article Number
<input checked="" type="checkbox"/> Registered <input type="checkbox"/> Insured <input type="checkbox"/> Certified <input type="checkbox"/> COD <input type="checkbox"/> Express Mail	<i>R096908 303</i>

Always obtain signature of addressee or agent and DATE DELIVERED.

5. Signature - Addressee  
X *Bruce Cornet*

6. Signature - Agent  
X

7. Date of Delivery

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



DOMESTIC RETURN RECEIPT

TO AJ  
DATE 14-V TIME 10 p. m.

WHILE YOU WERE OUT

M. Bruce Conner  
Of \_\_\_\_\_  
Phone \_\_\_\_\_

- TELEPHONED  PLEASE RETURN CALL   
CALLED TO SEE YOU  WILL CALL AGAIN   
RETURNED YOUR CALL  RUSH

MESSAGE Is consulting for  
Show Oil Co. - drilling  
another well in Rich.  
Basin THIS WEEK. - You  
& for Volkan invited.  
See me.

Signed BT

The Standard Register Company

referred this to Volkan Ediger  
on 16-V-85 - will call

2 May, 1985

Dr. Bruce Cornet  
13567 Portobello Drive  
Houston, TX 77083

Dear Bruce:

Here are the bisaccate drawings & MS back. I made copies. But truth to tell, I already had copies of at least some of these from you, going back to years ago--that was why I wrote about the plates of figures. Is there some caption material to accompany them? In plate 1, what is #4? In Plate 2, why are there 3 versions of Alisporites but only 2 of Sulcati.- and 1 each of the others? It's certainly an interesting set of drawings. In case there are no captions, could you scribble some explanation on the enclosed xeroxes and send back (stamped envelope enclosed.)?

All the best.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et  
encl: original drawings and MS.  
stamped envelope  
xeroxes of 2 plates of drawings

1 May, 1985

Dr. Bruce Cornet  
13567 Portobello Drive.  
Houston, TX 77083

Dear Bruce:

Sorry that it has taken me so long to get down to the business of responding to the packet of materials from you. We'll finish that up promptly and get everything back to you--insured, etc., probably in several installments. I especially appreciate loan of your gorgeous negatives-- I note that you were (are?) going to name a species for me--thanks!

If you want to take part in the Flagstaff meeting--go ahead; it wouldn't hurt anything. The recent Fisher & Dunay caper, however, was a bad blow, as the systematics were a major part of Ron's thesis.

S

Sorry about the Richmond wells--that's Ediger's thesis project, and he has been apparently reluctant to do much with the cuttings from you. I guess he is afraid it wouldn't be cleared for publication? Thus, he has worked mostly (entirely) on the outcrop samples we collected, plus cores from William and Mary and VA Survey. He's very independent, and I don't always know exactly what he's up to. In any event, I'll talk to him some more, if you're saying that there will be 100%-no-problem about release of whatever he comes up with from Horner & Bailey wells. I have to admit to Ediger whenever the subject surfaces that you've never said that it's completely free of encumbrance. He has studied about 20 samples. But you're welcome to whatever we've done here, old friend. We naturally hope that you'll hold off on publication on the Richmond Basin until Volkan is finished.

Now, about your thesis materials. It is actually my policy that all Ph.D. students leave duplicate rock samples for Penn State collections, of the principal productive samples studied--that's a normal procedure. It's so that interested parties (such as Frederiksen) can restudy the stuff if they wish. As you know, you left in such a cloud of dust (Betty did a great deal of the last 15% of the legwork on your thesis) that I was grateful to get the cuts of residues and slides that I did get, and up to now I never have worried about it all. However when Norm F. phoned me about coming up here, I thought I had duplicate rock samples from your thesis localities. He has now come and gone, and I let him sub-sample what he wanted from my own productive samples from the Basins--about 200 samples from here and there that interested him. Yes, rock cuts from say at least the primary productive localities referenced in the Cornet & Traverse paper, plus other Hartford Basin samples mentioned in the thesis (a dozen samples, I guess?) would be very helpful for the PSU collection. I will restrict distribution of sub-cuts from them in whatever way you like.

Cornet, pg. 2

Back to Richmond Basin and Volkan Ediger, because your letter goes back to it--he has passed his candidacy exam. His comprehensive is on 3 May. I am not funded (at all) now, so he is a teaching assistant, and is anxious to get through, and get on with life (an advantage of not having an R.A.?). I believe he'll be through by June, 1986. I am sure I speak for Volkan in hoping that you won't publish your Richmond study until his thesis is complete, about a year from now.

I had better stop here and get this off to you. More later about the matters that relate just to the textbook project.

Best wishes.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

Dr. Alfred Traverse  
The Pennsylvania State University  
Palynological Laboratory  
Deike Building  
University Park, PA 16802

April 2, 1985

Dear Al,

Enjoyed our phone conversation and hope you have success in writing your laboratory and teaching manual the way you want. Enclosed please find the negatives to my manuscript on the Richmond Basin angiospermoid pollen. Although the plates for the manuscript are completed, if anything should happen to these negatives, many hundreds of hours of work will be lost. I am not sure that the single grain mounts have withstood five to eight years without any deterioration. Please take care of them as if they were your own. Also enclosed are my thoughts on the generic differentiation of Triassic bisaccates, as you requested; copy and return. The Dockum-Chinle palynoflora, done while at Exxon, is a copy for you to keep. Michael Morales has requested my participation in a symposium on the Triassic of the American southwest, but any contribution by me on the palynology would conflict with Ron and your work, and I don't want that. Please advise; you may want to give a paper, including some of my data from Exxon.

I am concerned that we may not have an understanding of the type of cooperation necessary if industry and academia are to work together. I provided you with cuttings from the Horner and Bailey wells in return for palynological information derived from the study of those cuttings. I have had to ask for information, and when I did not get back what I had hoped to get, I processed the wells myself and constructed distribution graphs, of which you have copies. Now you want me to provide outcrop samples from my thesis localities, not for your work, but to give them to the U.S.G.S. I may be deeply involved in the geochemical analyses of not only the wells drilled to date in the Newark, but also of all the outcrop samples, which I still have. I would like to know the extent of your involvement with government and industry, because we may have a conflict of interest if I am trying to earn a living from my knowledge of and work in the Newark Supergroup. I have no problems with the U.S.G.S. (Norm Fredrickson) duplicating my work, provided they do not publish their results before we complete our study.

It has now been about four years since I gave you the well cuttings, and I would like to publish the distribution chart for the Horner well. How long

has it been since Volcan started writing his thesis? Our agreement states that following one year after Volcan's study has been completed, which is more or less determined by the time he starts writing, you may publish anything you want or do anything with it that you want. On the one hand you can make public information that was previously confidential, but on the other hand I too can publish that information. I do not want to jeopardize Volcan's work by publishing the palynological distribution chart I generated for the Horner No. 1, so I am relying on you to tell me when he has passed his thesis requirements. If he plans on extending his thesis for more than a year from now, we may have problems if the publication of the distribution chart undercuts his work. Please advise.

Please understand that I am not saying that I will not provide you with any of my thesis samples. I need more information before I cut my thought 😊

Sincerely yours,

*Bruce*

Bruce Cornet  
13567 Portobello Dr.  
Houston, Texas 77083

Dear Aland Betty,

12/21/84

This year has given me some of the greatest challenges of my life, as well as some of my most rewarding achievements. Love is grand, and when two people are in love as much as Bonnie and I are, no challenge seems too big. Scary, yes, but not insurmountable.

Thank you for your Christmas card and note. We are late on getting out our cards because this Christmas is going to be one of the best of my life now that I have a family upon which I can embellish my love.

Today is significant, because a rig will move onto my Damon Mound oil prospect. We should know the results by year's end. Best wishes,

Bruce

Connet

22 August, 1984

Dr. Bruce Cornet  
13567 Portobello Rd.  
Houston, TX 77083

Dear Bruce:

These photos just surfaced--perhaps you  
would like for your album?

All the best. Am off to Calgary.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et  
encl: photos

Prof. Alfred Traverse  
Palynological Laboratories  
435 Drake Building  
The Penn. State Univ.  
University Park, PA 16802

July 6, 1984

Dear Al,

Enclosed is a small diagram on the possible relationships of dispersed Tetrads from the Newark I found in one of my slide boxes. Note that the proposed sequence for these "Tetradopollen" is from a structured alveolar wall to a simple or massive wall. My proposed names for genera are Placopollis (1), Crateropollis (2), and Pyramidisporites (3), etc.

Best wishes,

Bruce

Connet

Prof. Alfred Trautvein  
 Palynological Laboratories  
 435 Drake Building  
 The Penn. State Univ.  
 University Park, PA 16802

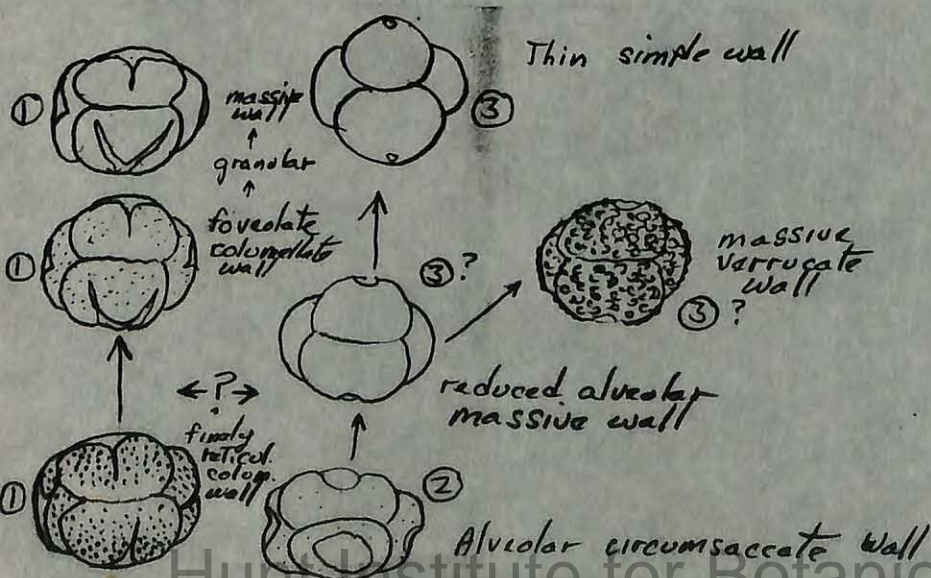
July 6, 1984

Dear Al,

Enclosed is a small diagram on the possible relationships of dispersed Tetrads from the Newark I found in one of my slide boxes. Note that the proposed sequence for these "Tetradopollen" is from a structured alveolar wall to a simple or massive wall. My proposed names for genera are Placopollis (1), Crateropollis (2), and Pyramidisporites (3), etc.

Best wishes,

Bruce



5 July, 1984

Dr. Bruce Cornet  
13567 Portobello Rd.  
Houston, TX 77083

Dear Bruce:

Good to talk to you on 3-VII re "Placopollis" and other matters. (I believe some sort of descriptive nomenclatural note on P. is called for. Want to do it together?) If the Cornet & Olsen paper you described comes out, please include as an "Acknowledgement" some version of the following:

"The palynological research for this paper was originally a part of the senior author's Ph.D. thesis at Penn State University and was supported in part by National Science Foundation grants GA-36870 to Alfred Traverse."

It all gave me a headache because it reminded me of Dunay's thesis. After I dreamed up the project and worried myself sick for years about its funding, etc., the work appeared as "Dunay and Fisher" with no reference to Traverse at all. (Except that "New Genus A" was described as a species traversii of an existing genus.)

Best wishes.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

29 June, 1984

Dr. Bruce Cornet  
13567 Portobello Drive  
Houston, TX 77083

Dear Bruce:

Thank you very much for phone conversations, the letter of May 25th, and the palynological data concerning the wells Horner-1 and Bailey-1.

I also ~~would~~ like very much to do a joint paper and to exchange information we've got. However, you are a few steps further from me as far as palynological data is concerned. As it is obvious from the charts you sent me, you did a nice job concerning the vertical variations of basic spore/pollen groups in Horner-1 and Bailey-1 and the stratigraphic correlation of these wells.

As I told you by phone, I just began producing palynological data from about a dozen of the outcrop samples. By now, I have tried to gather all the geological and palynological info on the Richmond Basin, and have worked on the palynological processing techniques. Some of these studies will be published under the title "Sieving techniques used in palynological processing with special reference to the 'MRA' system", and will be presented as a paper at AASP meeting in October, titled "Some theoretical and practical considerations about palynological processing".

Enclosed is the geologic map of the Richmond Basin compiled from all the previous work by me. Any of your suggestions are welcome. Let's try to keep in touch and inform each other about progress on our common subject.

Yours truly,

Volkan S. Ediger  
Ph.D. Candidate

VE/et  
encl: Geol. map of Richmond Basin  
cc: Prof. Traverse

27 June, 1984

Dr. Bruce Cornet  
13567 Portobello Drive  
Houston, TX 77083

Dear Bruce:

Just got the copy of what you sent Volkan as of 25 May, 1984. It is good of you to be so helpful, and we look forward to future cooperation. Your generic identifications were of considerable interest and agree with what I've seen.

Do you remember the item you called "Placopollis" (Pl. 15, no. 3-4)? I find a lot of them in some Richmond Samples, and I note that you've identified it in your samples. At first I thought they aren't really palynomorphs-- they are variable in size and the walls are very hard to demonstrate. However, they seem to be stratigraphically very significant and there are specimens such as you figure that at least mimic a perfectly good tetrad. I note that tho' you say it's "No. 25 in Table II", in fact you omitted No. 25 on the table. Do you remember why? Shouldn't this guy be published?

Best wishes.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

**SUPERIOR OIL**

*Copy for A.T.*

Volkan S. Ediger  
The Pennsylvania State University  
Deike Building  
University Park, PA 16802

May 25, 1984

Dear Volkan,

Thank you for your letter of 18 May regarding your work in the Richmond Basin. From the number of samples you collected I hope you have better results than I did when I worked the basin for my thesis. I am very interested in the five well samples, and per our phone conversation, would like to trade data and slides. The palynological work I have completed on the Horner No. 1 will be useful to you in correlating the other wells and in developing a better understanding of the stratigraphy and facies changes in the basin.

If you need any help in processing samples, send me some cuts. The slides of the Horner well I sent you via Al were the ones I used for my counts. I am enclosing some sample count sheets for your understanding of how I zoned the Horner well. I am also enclosing the distribution graph for the Horner No. 1. More data will follow as we begin to exchange data and samples. Please keep these data confidential, as they represent a lot of work and my competitive edge in oil exploration in the area.

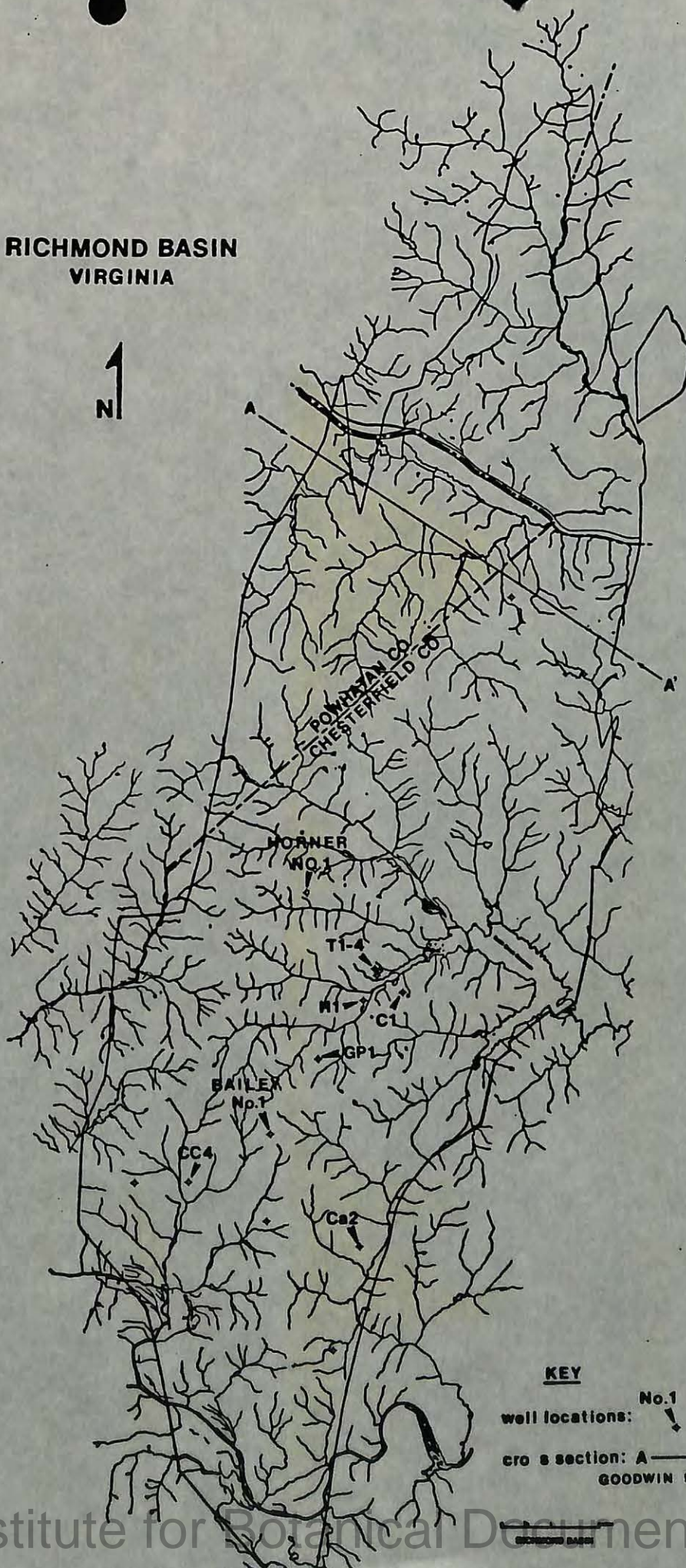
I would very much like to co-author a paper, with you as senior author, if you can put your outcrop and well data into the paper. Al can also co-author with us if he contributes advice and help. I will help you with correlations, since my experience in and understanding of the basin seems to be ahead of the competition. I would like you to write or edit the final version and take care of publication arrangements, etc. Al knows how I hate those hassles. I have nearly completed an industry report that will be the nucleus of my contribution.

Sincerely yours,




Dr. Bruce Cornet  
13567 Portobello Dr.  
Houston, TX 77083  
(713) 530-3183 residence  
(713) 531-2203 office

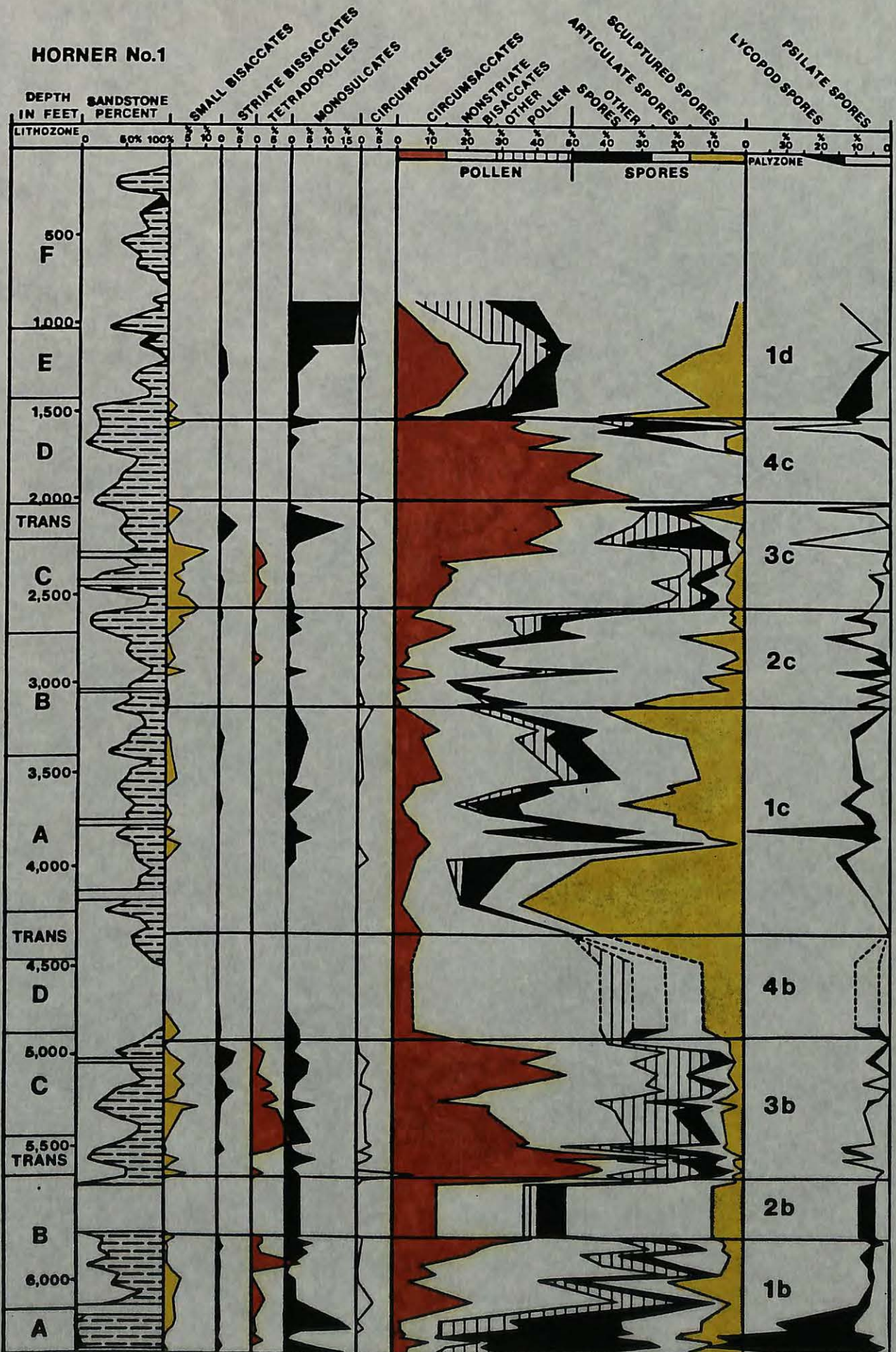
**RICHMOND BASIN  
VIRGINIA**



**KEY**

well locations:  No. 1  
cross section: A — A'  
GOODWIN 9701

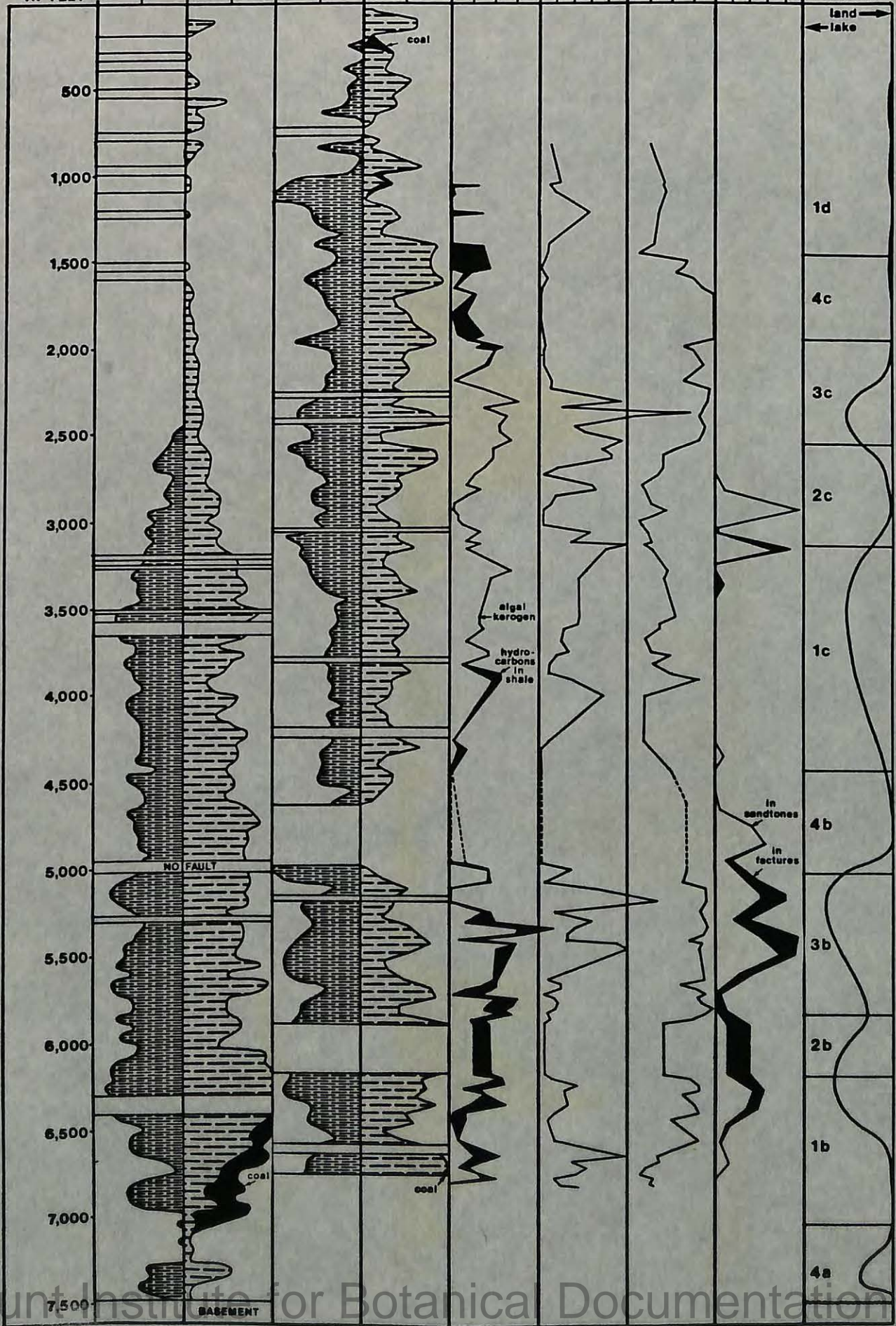
HORNER No.1

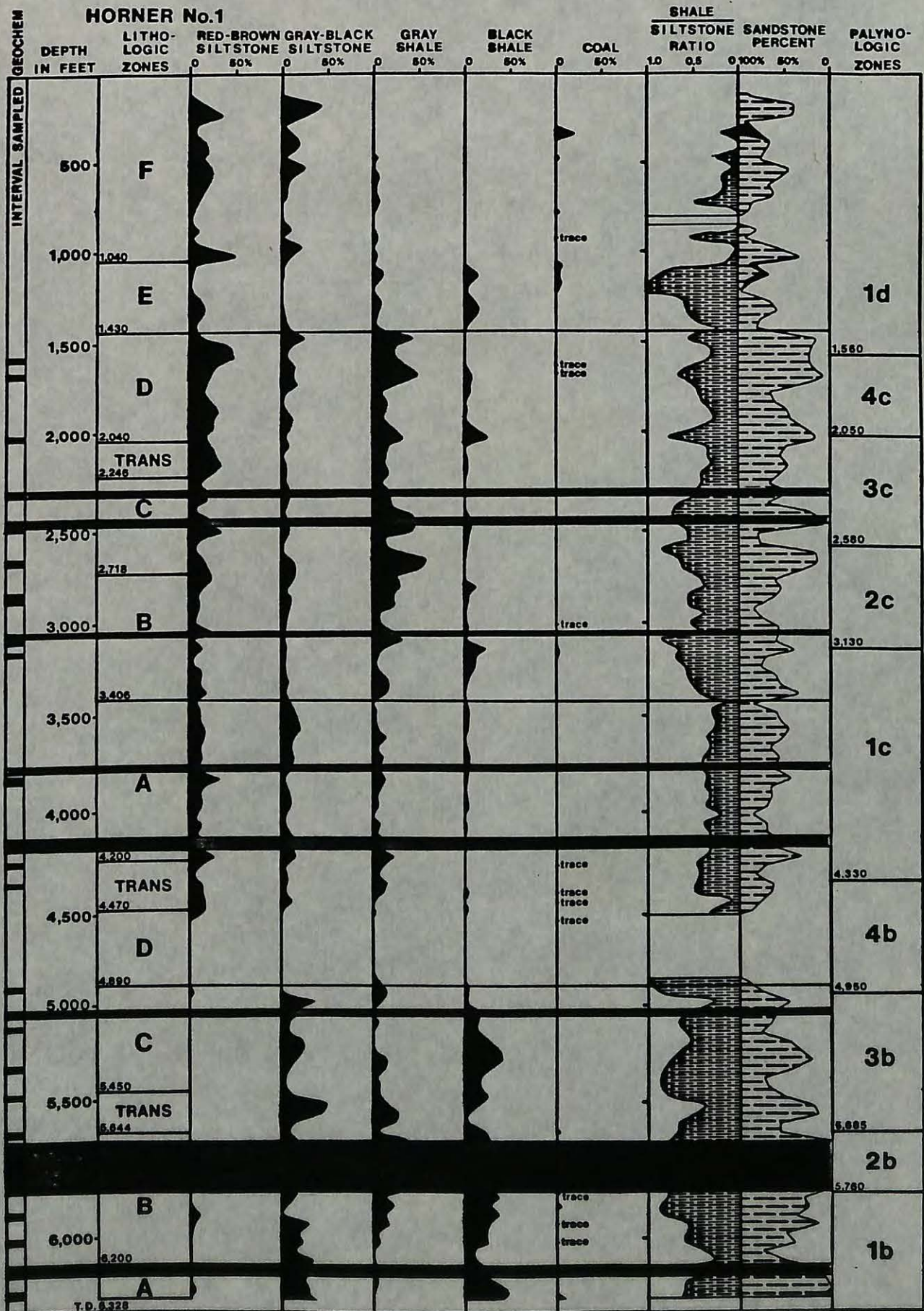


BAILEY No.1

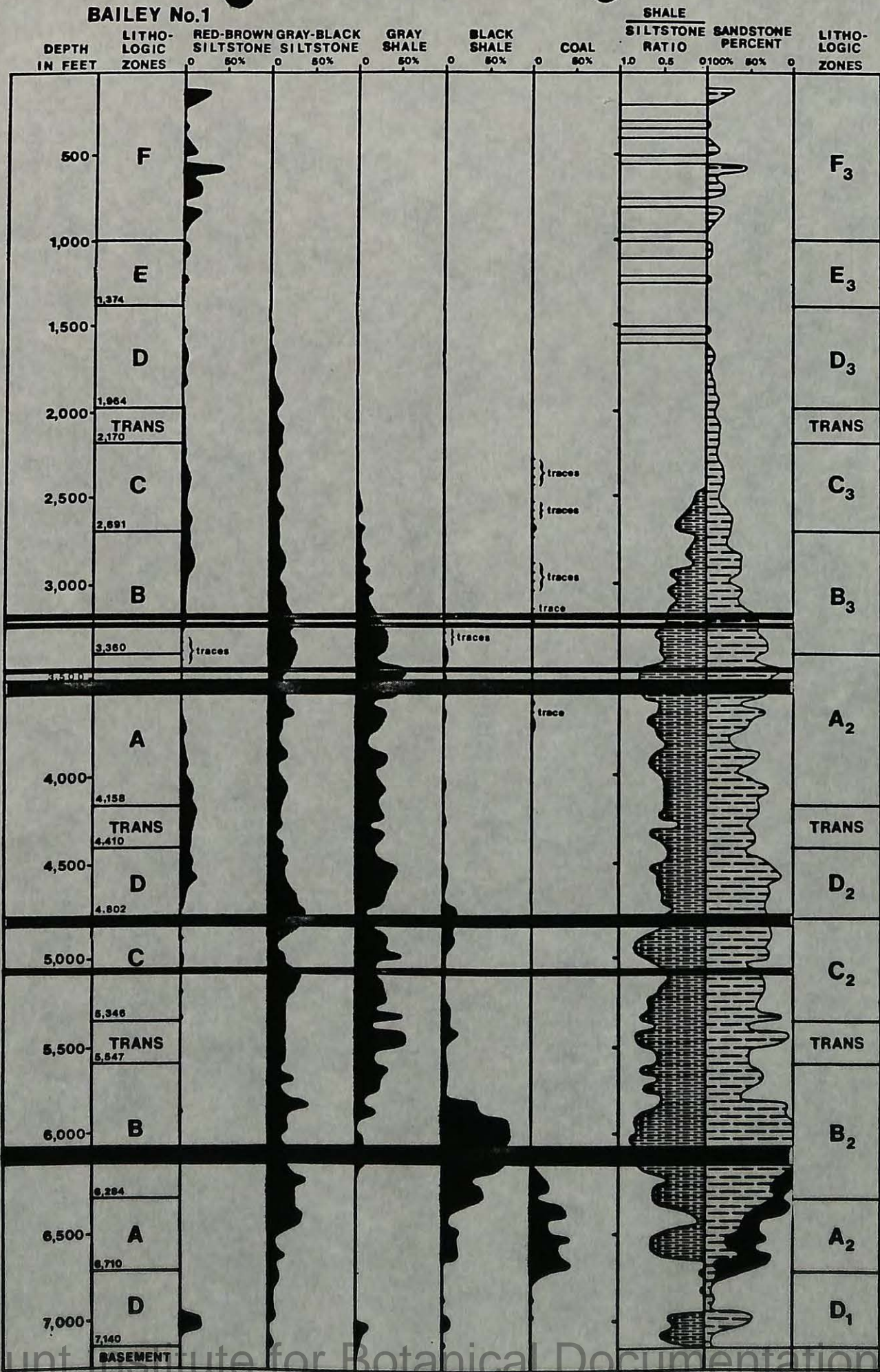
HORNER No.1

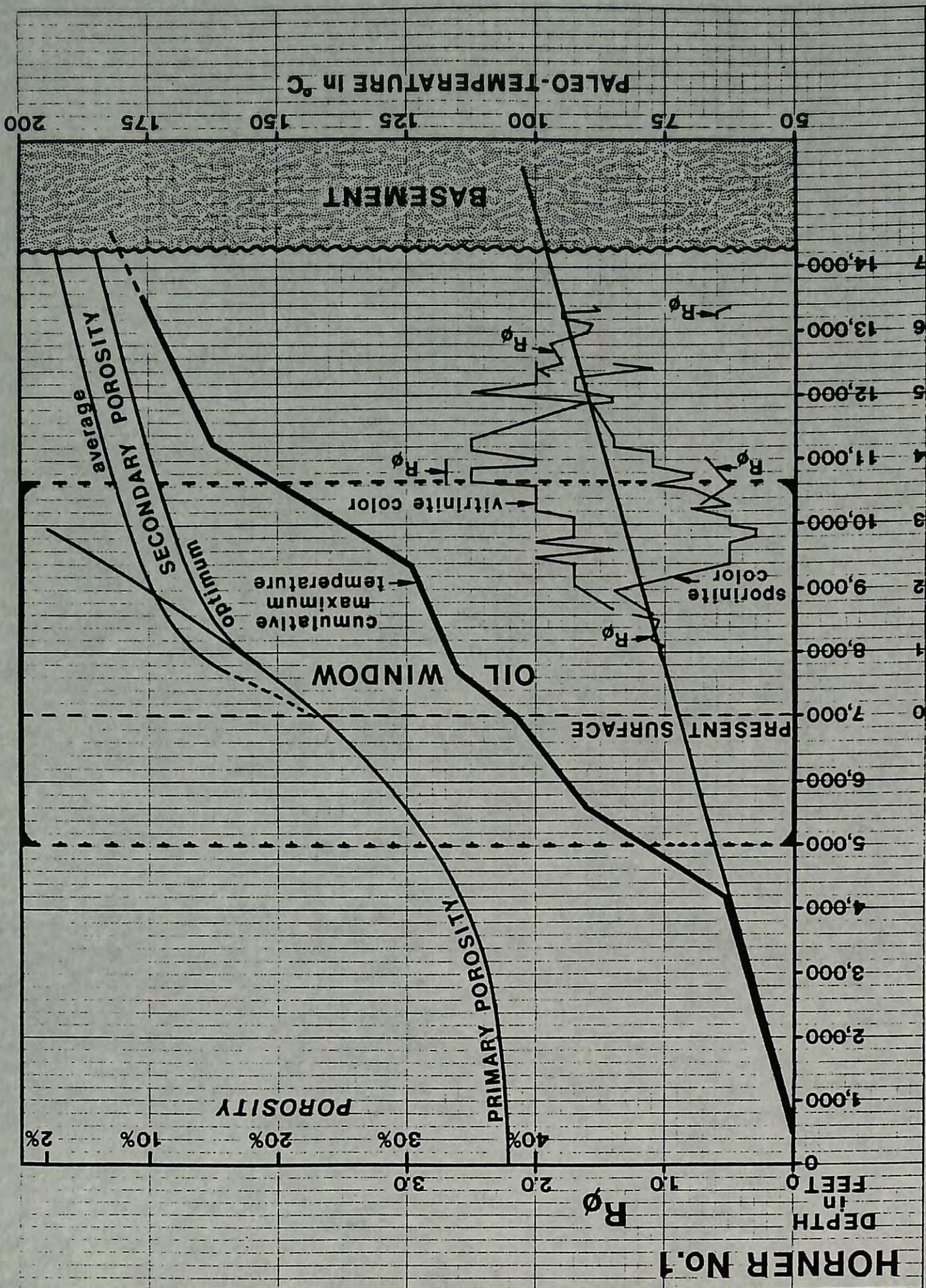
SHALE SANDSTONE SHALE SANDSTONE AMORPHOUS RELATIVE POLLEN/ OIL  
 COMPOSITE SILTSTONE SANDSTONE SILTSTONE SANDSTONE KERAGEN PALYNOMOR. SPORE BLS PER LACUSTRINE  
 DEPTH RATIO PERCENT RATIO PERCENT PERCENT ABUNDANCE PERCENT ACRE X1000 TRANSGRESS/  
 IN FEET 1.0 0.5 0.100% 50% 0.10 0.5 0.100% 50% 0 50% 100% 10 20 0 50% 100% 5 10 15 20 25 REGRESS





**BAILEY No.1**





WELL NAME: HNR #1

DEPTH: 880-890, 1110-1120'

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates		
2	Large Bisaccates	3	6%
3	Small Bisaccates		
4	Circumsaccates		
5	Tetrads monoporate		
6	Monosulcates	10	20%
7	Circumpolles		
8	Articulate spores	29	58%
9	Lycopod spores		
10	psilate spores	7	14% <i>Peltoidospora</i> <i>Dictyophyllidites</i>
11	Sculptured spores	' 1	2% <i>Foucasporites</i>
12	NO. of Traverses	LRLRLRLRLRLRLR	
		50 palynos / 15 traverses	3.3 / travers
			26% / 74% Pollin spores

WELL NAME: HNR#1

DEPTH: 1110-1120

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates		poorly preserved
2	Large Bisaccates	12	13.5%
3	Small Bisaccates		
4	Circumsaccates	11	12.4% <i>pseudocorymbia, Pritinasporites, Valleriasporites, Fuzoia laur.</i>
5	Tetrads Monolepate		
6	Monosulcates	A 16	18.0% Monosulcate A/C Conusporites
7	Circumpolles	1	1.1%
8	Articulate Spores	42	47.2% small & large Calamagrostis
9	Lycopod spores		Dictyophyllidites
10	psilate spores	2	2.2%
11	sculptured spores	5	5.6% Verrucosip. Granulatis. Erythrograuurites
12	NO. of Traverses	LRLRLRLRLRLRLRL	
	A = Angiospermid pollen	89 palynous / 15 traverses	5.9 / traverses
		44.9 / 55.1% pollen spores	

WELL NAME: HNR-042

DEPTH: 3260-80'

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates		
2	Large Bisaccates		59
3	Small Bisaccates		
4	Circumsaccates		27 mostly Bharadwajispora Vallasporites
5	Tetradis monoporate		
6	Monosulcates	 GAA "	Laqueella (2) conch monosulate large monosulate sp. 6
7	Circumpolles		
8	Articulate spores		33 small + large
9	Lycopod spores		7 Lycospora - Aratrisp? Murospora
10	psilate spores		16 Todisporites Dictyophylidites
11	Sculptured spores		55 Osmundacidites
12	NO. of Traverses	LRLRLRLRLRLRL	
	G = Gramineae acropites A = Angiosperm pollen	209 palynos / 15 traverses	13.9 / traverse

WELL NAME: HNK-043

DEPTH: 3510-30

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates	00	
2	Large Bisaccates	48	many highly oxidized, few worked?
3	Small Bisaccates	//	Rugobivesiculites Minutisaccus
4	Circumsaccates	14	/// Carebropollenites Vallasporites Shardulungispora
5	Tetrads Monoporate		
6	Monosulcates	10	- Equisetospores monosulcates (A) monosulcate spp Canerospores
7	Circumpolles	1	
8	Articulate Spores	54	large + small alab Colpomospora many thin bags
9	Lycopod spores		
10	psilate spores	18	Todisporites Dictyophyllidites
11	sculptured spores	29	Cyclotriletes, small + large reticulate comate forms Retriulatisp.
12	NO. of Traverses	LRLRLRLRLRLRL	
	A = Angiospermoid pollen G = Grayamonoolepites O = Ovalipollis	178 palynos / 15 traverses	11.9 / traverse
	Acritarchs	3	Wilsonastrum checked slide /// Staplinidium? 67.9 x 19.3 76.2 x 18.0

WELL NAME:

HNR-058

DEPTH:

4760-10

GENUS:		SPECIES:
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS
1	Striate Bisaccates	
2	Large Bisaccates	
3	Small Bisaccates	
4	Circumsaccates	Patinasporites
5	Tetrads monoporate	
6	Monosulcates	A
7	Circumpolles	
8	Articulate spores	
9	Lycopod spores	
10	Psilate spores	
11	Sculptured spores	
12	NO. of Traverses	LRLRLRLRLRLRLRL
	As angiospermid pollen	28 palynas / 15 traverses 1.9/traverse

WELL NAME:

HNR-059

DEPTH:

5000-10

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates	5 ooooo	Protoperid. Parillinites ovalipollis
2	Large Bisaccates	5	67
3	Small Bisaccates	3	A. parvus Tridisporea
4	Circumsaccates	5	Patinasporites Vellaspores 99
5	Tetrads Monoporate	5 E	Crateripollis placopollis
6	Monosulcates	3	Camerosporites
7	Circumpolles	1	Calamospora laricoid
8	Articulate Spores	5	Cingulizonates Aratrispor.
9	Lycopod spores	3	Todispores Deltoidospora
10	Psilate spores	3	Converruc. verrucosisa cyclotriletes Osmundacid.
11	Sculptured spores	5	
12	NO. of Traverses	LRLRLRLRLRLR	
	o = ovalipollis	214 palynos / 12 traverses 17.8 / traverse	
	E = equisetosporites		

WELL NAME:

HNR-060

DEPTH:

5030-60'

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates	/ 1	Parillinites Lunatisporites Protobaplagium
2	Large Bisaccates	/ 85	85
3	Small Bisaccates	/ 16	Small Platysaccus Triadispora A. Porus Minutosaccus Pityopollenites
4	Circumsaccates	/ 99	Patinasporites Vallaspores Lunatisporites
5	Tetradis Monoporate	/ 7	Placopollis Crateropollis larger-small
6	Monosulcates	E       / 18	
7	Circumpolles	/ 10	Camerasporites larger small
8	Articulate Spores	/ 2	
9	Lycopod spores	/ 5	Aequitriletes Anatrisporites Chomatriletes
10	psilate spores	/ 12	Todisporites Dalloides Dictyophyllidites
11	Sculptured spores	/ 6	Conversicos Cyclotriletes Famulatisporites OSMundacidites
12	NO. of Traverses	LRLRLRLR	
	E = Equisetasporites	271 palynos / 8 traverses 33.9 / traverse	



WELL NAME: HNR-65

DEPTH: 5240'-5270'

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates	(2)	Protoglyp. parvilites
2	Large Bisaccates	<del>     </del> (54)	
3	Small Bisaccates		
4	Circumsaccates	(16)	Tetrad P. T. sp. Vall. sp.
5	Tetrad Monoporate	(9)	Placopollis Crateropollis
6	Monosulcates	(2)	
7	Circumpolles	(4)	Comosporites spp. Paracirculina Duplicisporites
8	Articulate Spores	(16)	
9	Lycopod spores	(2)	AraTrisporites Chonotriletes
10	psilate spores	(5)	Todisporites Deltoidospora
11	Sculptured spores	(10)	Comosporites cyclotriletes Granulatisporites Osmondacidites
12	NO. of Traverses	LRLRLRLRLRLRL	
		120 polymers / 15 traverses 8.0 / traverse	
		Pollen 72.5 % Spores 27.5 %	

WELL NAME:

HNR-067

DEPTH:

5300'-5330'

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates	0	
2	Large Bisaccates		83
3	Small Bisaccates		12 granosaccus Pityosporites Pityosporites Alisporites
4	Circumsaccates		62 Euzonalesp. Patinasporites Vallasporites
5	Tetradis Monoporate		17 Placopollis, Crateropollis
6	Monosulcates		3 Camerosporites
7	Circumpolles		5
8	Articulate Spores		14
9	Lycopod spores		
10	psilate spores		17 Deltoidiopsisora Todiopsisora Dietycophylloidsite
11	Sculptured spores		14 Cyclotriletes Granulasp.
12	NO. of Traverses	LRLRLRLRL	
	o = Ovalipollis		
		228 polyms / 9 traverses	25.3 / Traverses

WELL NAME: HNR-068

DEPTH: 5470-5490'

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates		
2	Large Bisaccates		13
3	Small Bisaccates		
4	Circumsaccates		17
5	Tetrads Monoporate		5
6	Monosulcates		3
7	Circumpolles		
8	Articulate Spores		
9	Lycopod spores		
10	psilate spores		2
11	Sculptured spores		2
12	NO. of Traverses	LRLRLRLRLRLRL	
		44 Polymers / 15 traverses	2.9/travers

*Pseudobryon. Patinasporites*

*Vallasporites*  
*Crateropolles*  
*Placopolles*

*Comesporites*

*Todisporites*  
*Dictyophyllidites*

*Convolutispora*  
*Granulatisporites*

WELL NAME: HNR-078

DEPTH: 5760'-5790'

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates		
2	Large Bisaccates	16	25.0%
3	Small Bisaccates	" 2	3.1% <i>Triodispora?</i>
4	Circumsaccates	25	39.1%
5	Tetrads Monoporate	" 2	3.1% <i>Creticopollis floripollis</i>
6	Monosulcates	A 1	1.6%
7	Circumpolles	' 1	1.6% <i>Cameosporites</i>
8	Articulate Spores	10	15.6%
9	Lycopod spores	" 2	3.1% <i>Micatisporites</i>
10	psilate spores	' 1	1.6% <i>Microphyllites?</i>
11	Sculptured spores	4	6.2% <i>Cannondaleid. Reticulatispora (Triletes?)</i>
12	NO. of Traverses	LRLRLRLRLRLRLRL	
	A = angiospermoid pollen	64 palynas / 15 traverses 4.3/traverse	
		73.4% / 26.6%	
		pollen spores	



WELL NAME: HN 028

DEPTH: 230'-6260'

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates	0 1	0.5% Ovalipollis
2	Large Bisaccates	19	9.4%
3	Small Bisaccates	2	1.0%
4	Circumsaccates	8	4.0%
5	Tetrads Monoporate	3	1.5%
6	Monosulcates	8	4.0%
7	Circumpolles		
8	Articulate spores	28	13.9%
9	Lycopod spores		
10	psilate spores	7 3.5%	91 45.0%
11	Sculptured spores	35	17.3%
12	NO. of Traverses	LRLRLRLRLRLRLRL	
		202 palynos / 15 traverses 13.5/traverse	
		20.3% / 79.7%	
		Pollen Spores	

WELL NAME: HNR-089

DEPTH: 260'-6290'

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates		
2	Large Bisaccates		16 10.0%
3	Small Bisaccates		1 0.6%
4	Circumsaccates		4 2.5%
5	Tetrads Monoporate		1 0.6%
6	Monosulcates		1 0.6%
7	Circumpolles		
8	Articulate Spores		30 18.8%
9	Lycopod spores		67 41.9%
10	psilate spores		10 6.2%
11	Sculptured Spores		30 18.8%
12	NO. of Traverses	LFLRLRLRLRLRL	
		160 palynae / 15 traverses	10.7/traverse
			14.4% / 85.6%
			pollen spores

Pityopollenites  
Patriasporites  
Vallasporites  
Placopollis

Artisporites  
Dalloosporia  
Tadispores

Cyclospores  
Cyclospores  
Cyclospores

WELL NAME: HNR-092

DEPTH: 6528' T.O.

GENUS:		SPECIES:	
DEPTH	COUNT/TOTAL	DESCRIPTION & REMARKS	
1	Striate Bisaccates		
2	Large Bisaccates	15	8.7%
3	Small Bisaccates	2	1.2%
4	Circumsaccates	7	4.0%
5	Tetrads Monoporate		
6	Monosulcates	29	16.8%
7	Circumpolles		
8	Articulate Spores	37	21.4%
9	Lycopod spores	50	28.9%
10	Psilate spores	6	3.5%
11	Sculptured spores	27	15.6%
12	NO. of Traverses	LRLRLRLRLRLRLRL	
		173 pollen / 15 traverses	11.5 / traverse
			30.6% / 69.4%
			pollen spores

Protohaploxyprum

Pogoniasporites  
Valliasporites

Pilasporites  
Laricoidites  
Calamospora  
Arctisporites

Deileidospora  
(some large)

(some large) → Granulatisporites  
Rauisticoid

TO AT

DATE 4 Nov. 1983 TIME 1:00

WHILE YOU WERE OUT

M Bruce Cornett  
of \_\_\_\_\_  
Phone \_\_\_\_\_

TELEPHONED  PLEASE RETURN CALL   
CALLED TO SEE YOU  WILL CALL AGAIN   
RETURNED YOUR CALL  RUSH

MESSAGE Did pkg. come?  
I said yes & you had  
just written here.

Signed BT

The Standard Register Company

31 October, 1983

Dr. Bruce Cornet  
Superior Oil Co.  
Geo-Science Lab, 12401 Westheimer Rd.  
Houston, TX 77077

Dear Bruce:

This is to acknowledge receipt of the 29 slides from the Horner Well, which have been checked out to Volkan Ediger. Also, the beautiful Permian slides from Africa were received and are already being used by 423 students. However, could you tell me a little more than "Permian"? Anything would help ("Late Permian"; whatever).

Good to hear from you. Thanks again for the slides. All the best.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

8 April, 1983

Dr. Bruce Cornet  
Geo-Science Lab, Superior Oil Co.  
12401 Westheimer Rd.  
Houston, TX 77077

Dear Bruce:

Thanks for your kind letter of 31 March. Bad news, though--the GSC slide was shattered all over the place. Funny--the other package had the HNR #1 slides all stacked together in a small box. Apparently they supported each other, and all of the slides were 100%. This supports my previous observation that the only safe method for a single slide is a small, sturdy box with styrofoam "worms" surrounding the slide on all sides. A padded envelope does not protect sufficiently against compression--probably this envelope was under a huge box at some point. Anyway, I have glued the pieces to a slide, and await instructions--it's pretty bad. The HNR #1 slides are not mentioned in the letter but I presume are for Volkan--I have turned them over to him.

Your list for Fownes Head slide seems to have the same story pretty much as my previous collections there. However, does this come from the M.Sc. thesis material I mentioned over the phone? (See enclosed copy of letter). The professor from McMaster said that his student had sent you some stuff. This slide is from a GSC-Jon Bujak-locality. Please clear up this point for me.

All the best.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

*file as corresp.*

Dr. Bruce Cornet  
Exxon Co., USA  
P.O. Box 2189  
Houston, TX 77001

Geminoil Inc.  
One Allen Center, Suite 1000  
Houston, TX 77002  
713-651-7875

(In business for self--3  
partners as of IX-81.  
Drilled well in Richmond  
Basin.)

home: ~~#538~~

13567  
Portobello Dr. 7900 Westheimer  
Houston 77063

~~713 785 4998~~

*Partner as of IX 82:  
Gunter Weisbrich*

26, 27, 28, 29, 30, 32, 33, 34, 35, 36, 37, 41

23 April, 1982

Dr. Bruce Corrat  
GeminOil Inc.  
One Allen Center, Suite 1000  
Houston, TX 77002

Dear Bruce:

I just stumbled on the enclosed. My memory is spotty on this, but I am pretty sure they came from you originally. If so, can you tell me what they were part of? From what sources did you compound the info, etc., etc.?

All the best. We're working away on the Triassic and other things.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et  
encl



GEMINOIL, INC.  
One Allen Center  
Suite 1000  
Houston, Texas 77002

Telephone  
(713) 651-7875  
TWX  
910-881-2694

Professor Alfred Traverse  
Palynological Laboratory  
Deike Building  
The Pennsylvania State University  
University Park, PA 16802

8 December 1981

Dear Al,

During the months of July through November of 1981 two wells were drilled in the Richmond Basin, VA by Cornell Oil Company and Geminoil, Inc. The Horner #1 was spudded on 11 July and drilled to a depth of 6328 feet. The Bailey #1 was spudded on 9 September and drilled to a depth of 7443 feet.

Cutting samples from these wells will be and have been made available to you for palynological study and analysis. At our last meeting on 14 August we discussed the confidential nature of any studies made on these cuttings, but I wanted to give you and any of your students working on these wells as much freedom in reporting your palynological findings as possible.

On 4 December you called me to ask for a letter containing the provisions, conditions, and limitations pertaining to the palynological analyses of the cutting samples and to the reporting or publication of the results to those analyses.

Geminoil, Inc. has recently purchased  $62\frac{1}{2}\%$  of Cornell Oil's interest in the Richmond Basin, and Geminoil has farm outs from Cornell Oil for  $12\frac{1}{2}\%$  and from Lyco, Inc. for  $12\frac{1}{2}\%$ , giving us a total of  $87\frac{1}{2}\%$  and control over the project. Therefore, this agreement is between (1) Geminoil, Inc. and (2) Professor Alfred Traverse, his students working on this project, and any or all of his designated representatives for this project. It is your responsibility, Al, to inform your students and representatives of the provisions, conditions, and limitations to this agreement.

Professor Alfred Traverse hereby agrees that:

All non-palynological information regarding and derived from the cuttings, electric logs, mud logs, and geochemical analyses and reports for the Horner #1, Bailey #1, and any additional wells drilled by Geminoil, Inc. in the Richmond Basin is confidential and cannot be released without prior written consent during the term of the study and for a period of one year (12 months) following the completion of the study. "Study" is here defined as the time it takes to acquire all necessary information for the writing of a thesis, with the exception of minor additions to the thesis once writing has commenced.



GEMINOIL, INC.  
One Allen Center  
Suite 1000  
Houston, Texas 77002

Telephone  
(713) 651-7875  
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910-881-2694

All non-proprietary palynological information may be reported and published without prior consent or permission; Geminoil requests that you keep us informed about any information released through professional talks, published papers, and conversations with outside interests. We would appreciate reprints or copies of any written reports or publications stemming from the study of our wells. We also request appropriate acknowledgements in professional talks and papers (e.g. Dr. Bruce Cornet and/or Günther Weisbrich, Geminoil, Inc., Houston, Texas).

#### Non-proprietary Palynological Information

1. Taxonomy and nomenclature of palynomorphs.
2. Stratigraphic ranges and distribution of palynomorphs in wells.
3. Well depth information for palynological samples.
4. Age interpretations.
5. Climatic interpretations.
6. Generalized paleoenvironmental interpretations (see below).
7. Palynological zonation, zone correlation, and evidence for missing or duplicated sections in wells.

#### Confidential Information

1. Detailed paleoenvironmental reconstructions that would reveal water depth, richness of organic material (excluding coal), distribution, thickness and extent of sandstone beds, and distribution, thickness and extent of source rock beds.
2. Thermal alteration (TAI) and unoxidized color of palynomorphs (condition and preservation of palynomorphs excluded).
3. Composition of organic residues, organic content (TOC) of palynological samples (excluding coal), and thickness of coal seams.
4. Detailed stratigraphy revealed by the wells (condensed summaries permitted).
5. Detailed structural interpretations for the wells.
6. Chronology and duration of diagenetic and tectonic events for the Richmond Basin (based in whole or in part on well information).
7. Hydrocarbon potential of the Richmond Basin.



GEMINOIL, INC.  
One Allen Center  
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6. Chronology and duration of diagenetic and tectonic events for the Richmond Basin (based in whole or in part on well information).
7. Hydrocarbon potential of the Richmond Basin.

If there are any questions concerning the proprietary nature of any information, please contact us. Remember, after a year from the time the writing of a thesis has commenced, all information becomes public. The only time you need be concerned about including information in a talk or publication is before that year has passed.

Please sign both copies and return one copy to me.

Yours truly,

Dr. Bruce Cornet  
Dr. Bruce Cornet, President

I, Dr. Alfred Traverse, agree to the terms set forth in this agreement:

Name: Alfred Traverse Date: 8 Feb. 1982  
Witness: Ronald J. Gitwin Date: 8 Feb 1982

23 September, 1981

Dr. Bruce Cornet  
Geminoil Inc.  
One Allen Center, Suite 1000  
Houston, TX 77002

Dear Bruce:

As you know, the sample boxes did come, and I would like to express profound thanks. I have a coupleooof new Ph.D. students. What would you think of my getting one of them into the Richmond Basin?

Your mother phoned me the other night to tell us how thrilled she is about your present ventures. She seemed a little annoyed with me that I too wasn't just "glowing", but I should have emphasized more that I had been sound asleep (if was 10:30 p.m. our time). Also, it is true that I have mixed emotions. Unquestionably your commercial ventures will mean that some things will be back-burnered, at least for a while. Not so? However, I noted with pleasure how happy you are, and that did please me, of course.

Thanks again for the samples. Let me have your reactions to proposal in para. 1. Best wishes, as ever.

Cordially,

Alfred Traverse  
Professor of Palynology

AT/et



**Dr. Bruce Cornet**  
President

**GEMINOIL, INC.**  
One Allen Center  
Suite 1000  
Houston, Texas 77002

Bus: (713) 651-7875  
TWX: 910-881-2694  
Res: (713) 785-4998

11 August, 1981

Dr. Bruce Cornet  
c/o Dr. W. C. Elsik  
Exxon Company, U.S.A.  
P.O. Box 2189  
Houston, TX 77001

Dear Bruce:

When I got back from Europe recently there was a message on my desk, "contact Bruce Cornet". But I haven't the slightest idea how to do it--so am casting this bread on the water and hoping for the best! Naturally, I am wondering whether it's about "Alfredospora" (fond hope!), Sanmiguelia, or buying oil shares (rumor I heard).

Often think of you, old friend. Am off to Australia (God and the air traffic controllers willing) tomorrow for IBC. Back circa 1 September.

All the best!

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

10 Dec. 1979

Dear Alfred and Betty,

Hope this card finds you well and in the spirit of Christmas. Thank you Al for your letter and message about Ginny. This will be my first Christmas without her. She sent me an emotion-filled letter wondering was it all worth it? pray for her. I am doing well. I am beginning after two years to settle into my Job. I have already started work on Alfredgallis traversci. ☸ The cutting from your Night-blooming cerise is doing very well. I have many fond memories of State College. My beard has grown!

Merry Christmas

AND BEST WISHES FOR A HAPPY NEW YEAR

Love,

BRUCE CORNET

31 July, 1980

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506 (North 1207)  
Houston, TX 77036

Dear Bruce:

I enclose all of the materials re your paper, exactly as I presented it--I wanted you especially to see the few changes I made during my preparations in Cambridge--mostly brief deletions where I wasn't sure I could see what I was supposed to see--and, of course, the addition re your NW Texas material. God knows what mutual friend might say to you re my presentation, but I think I did a good job! The only negative comments made there were that it's time for you to publish some of your data and ideas (Hughes, for example, said so very forcefully), so that they can be considered formally. I really have to agree. (W. Klaus has written me to congratulate me on having such a distinguished format student!)

The trip to UK was fun and very educational and very expensive! Now we're trying to dig out from under. Without either ICP presidency or the parish, I might make it!

All the best.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et  
encl: slides, MS

21 March, 1980

Dr. Bruce Cornet  
Gulf Research & Development  
P.O. Box 36506 (North 1207)  
Houston, TX 77036

Dear Bruce:

= These photos and negatives showed up the other day--I didn't think I should throw them away. I'll let you decide. Have you priced fossil fish lately? You and Nick missed your calling.

Best wishes.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

# Gulf Research & Development Co.

HOUSTON TECHNICAL SERVICES CENTER

P. O. Box 36506  
Houston, TX 77036

7 April 1980

Professor Karl Niklas  
Division of Biological Sciences  
Section of Botany, Genetics & Development  
Plant Science Building  
Cornell University  
Ithaca, New York 14853

Dear Karl,

Thank you for your 1 April letter and reprint (no fooling), which I am finding most informative and interesting. Your concept of factors influencing fossilization is comprehensive and reassuring. Your approach is commendable.

I would be glad to exchange arguments, data, and speculations. I will keep you informed of my progress.

I believe that the use of "early" in reference to Early Cretaceous angiosperms will bias concepts of angiosperm evolution. I do not see how angiospermy can be defined on the basis of their Early Cretaceous diversification. A double standard will arise if angiosperms can be demonstrated in the Triassic, because we cannot have "early" angiosperms spanning more than 100 million years of time! If Sanmiguelia, for example, is found with a monocotyledonous inflorescence, its vegetative organs and habit would come very close to "modern" concepts of angiosperms (Tidwell et al., 1977, *Palaeontographica*, 163).

I am concerned with your use of a rubber ruler when you referred to my Oxfordian pollen as angiospermoid. Stellatopollis spp. is by definition as angiospermous in the Jurassic as it is in the Early Cretaceous. Even though our degree of uncertainty about affinity is less in the Barremian, the problem there is still the same. We are taking on faith that isolated organs reflect the evolutionary status of the plant. I can appreciate the philosophical problems involved, but we must stick with the facts. An isolated organ can be angiospermous. Pollen tells us more about the reproductive system of an unknown plant than do leaves. Two isolated organs may increase our certainty about affinity, but they tell us no more about the morphology of unknown organs than one isolated organ.

Certainly pollen could evolve before other organs in angiosperm ancestors. The diversity in Late Triassic pollen types indicates considerable experimentation with an apparently new mode of pollen germination. If exine structure is related to function, as I believe it is, then my angiospermoid pollen has structure and morphology that indicates clumping during dispersal and germination on a stigma. If insects were involved in the evolution of an angiospermous wall, presumably



A SUBSIDIARY OF GULF OIL CORPORATION

they would have had an equal effect in the evolution of a gynoecium or ovary. The wall structure of my pollen shows a significant modification of the endexine, with its eventual loss by the Norian. A spongy endexine as in angiosperms implies rapid germination following dispersal, because there is no longer the protective envelope of a laminated endexine as in gymnosperms for the prolongation of pollen viability. If rapid germination occurred, it would indicate a more precise timing with ovule receptivity. Ovule receptivity would be coordinated with stigmatic receptivity. Size of the ovary would become a factor if the ovules developed to nearly mature seed size before fertilization. By reducing ovule and ovary size, energy would be conserved. The stage would then be set for the evolution of a reduced gametophyte and double fertilization.

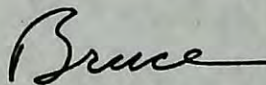
Convergence in female or male organs toward angiospermy in known gymnosperms does not fit the scenario depicted above. Corollina spp. retains a laminated endexine even though stigmatic germination on the lip of a bract pouch containing the ovule(s) is indicated. Caytonia spp. have pollen tubes and bisaccate pollen, even though the ovules are enclosed in an ovary.

Certainly there are a number of interpretations of function based on morphology, but I do not see my angiospermoid pollen evolving independent of the gynoecium or ovules. The degree of convergence toward the morphology and exine structure unique to angiosperms makes me suspect that the plants producing angiospermoid pollen in the Triassic were either angiosperms or they came very close in reproductive function (closer than the Gnetales)!

Your presentation of various interpretations of my angiospermoid pollen in your paper is commendable. I am not sure that you have to invoke a sister group to explain the appearance of angiosperm-type pollen well before the Cretaceous. You seem to be having difficulty in reconciling the possibility of angiosperms 100 million years before their radiation. Consider the mammalian example and you may find an analogous problem. Do we interpret the data according to our preconceptions and ignorance, or do we look for the truth?

Thank you again for the reprint and your correspondence.

Sincerely yours,



Bruce Cornet



Cornell University  
Division of Biological Sciences

Section of Botany, Genetics & Development  
Plant Science Building  
Ithaca, New York 14853

1 April 1980

Dr. Bruce Cornet  
Gulf Research & Development Co.  
Houston Technical Services Center  
P.O. Box 36506  
Houston, TX 77036

Dear Bruce:

Thank you for the xeroxes of the Oxford Clay pollen. They are truly spectacular! While not a palynologist, I can appreciate your superb technique as well as the implications of finding pre-Cretaceous angiosperm pollen grains.

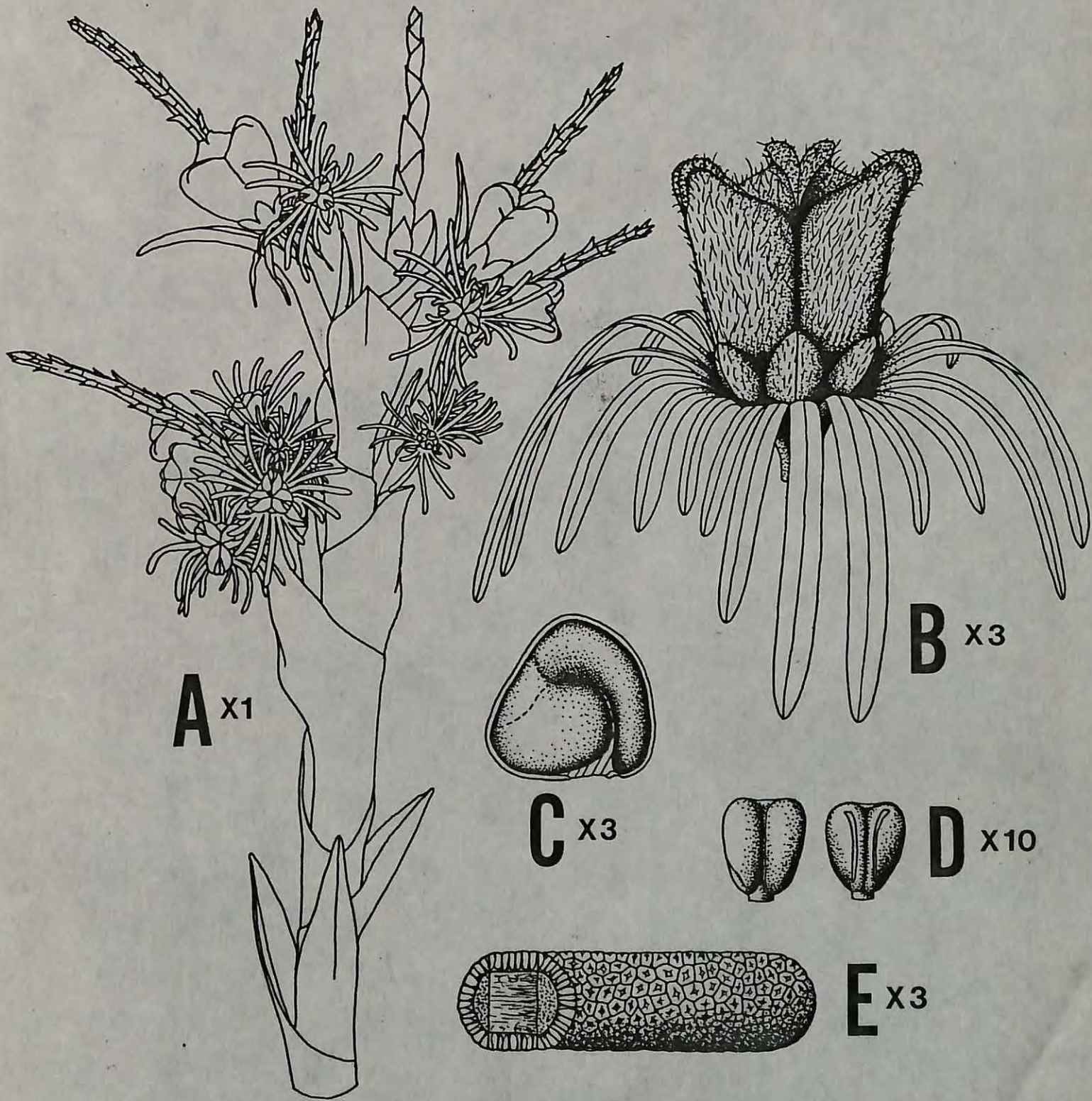
I have enclosed a reprint of an article which in part discusses some of my thoughts on angiosperm evolution (please check pp. 50-62, 62-68). We take the "additive view" to angiosperm evolution, namely that characters may have evolved out of phase during the initial portions of their evolution (see p. 53). Quite frankly, I do not know precisely what your data really signify! Two possibilities clearly come to mind: (1) the angiosperm sister group was pre-Cretaceous and developed microgametophytes of an angiosperm-like nature before other characters, or (2) gymnosperms paralleled the pollen grain evolution of the angiosperms in the Jurassic. Neither of these two are mutually exclusive, and perhaps the only way to logically resolve the question is to seek out the presence of other angiosperm characters in the Jurassic.

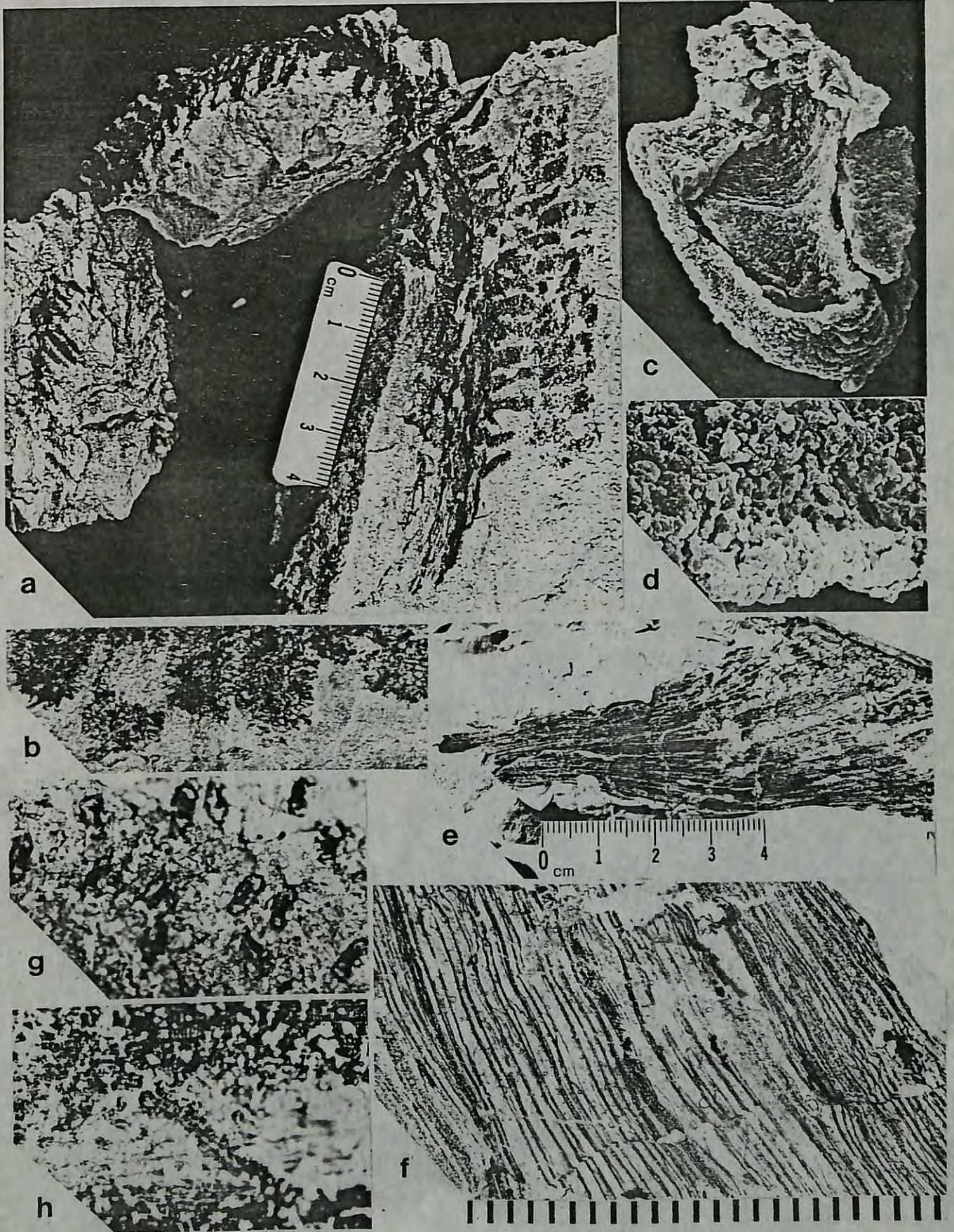
These are only first impressions of your data and I would be pleased to respond to arguments, data or speculation as you see fit. Let me, however, encourage you to pursue your research in this area, which is truly fascinating.

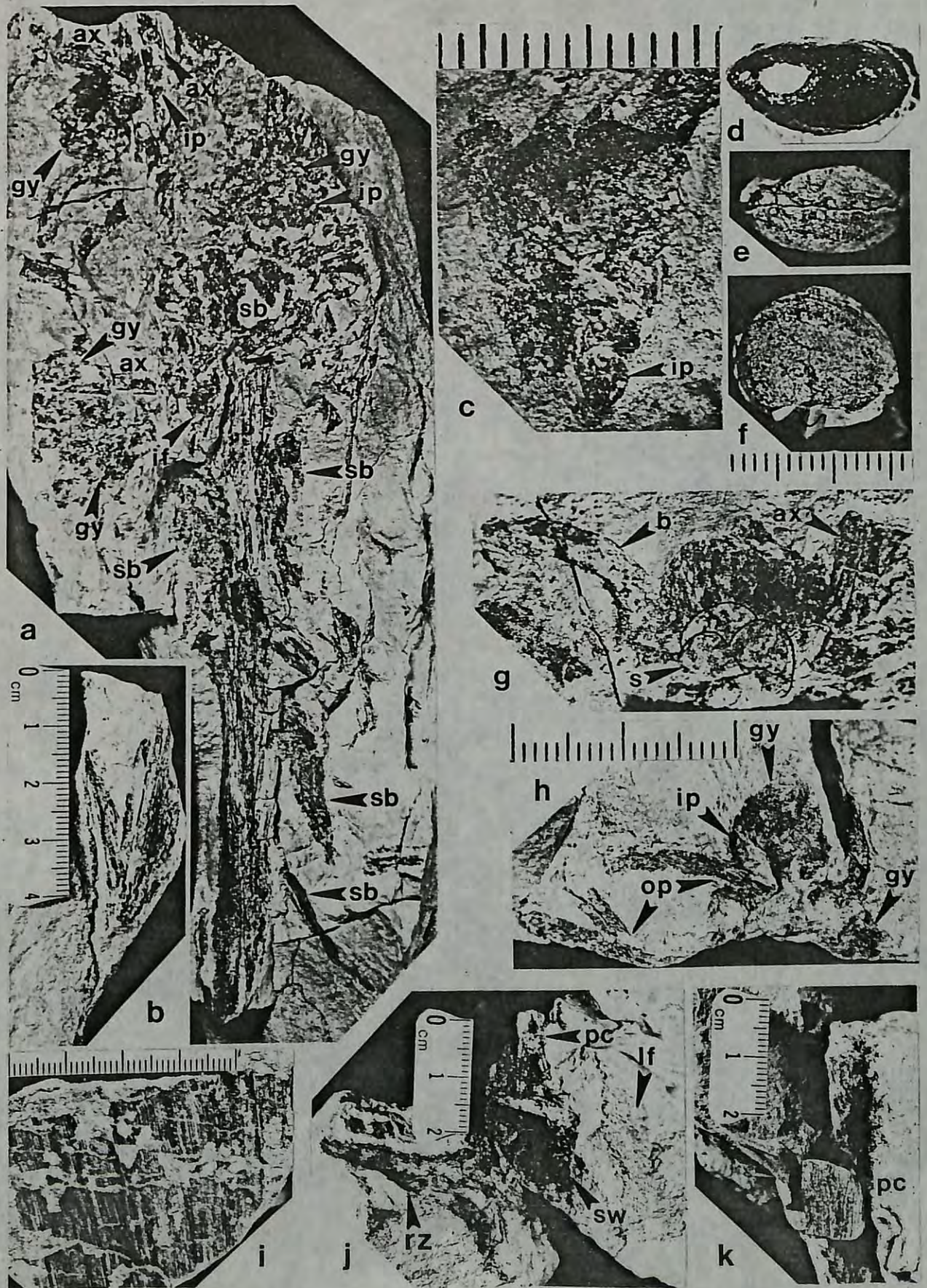
With kind regards,

Karl Niklas

sk  
encl.







# Gulf Research & Development Company

HOUSTON TECHNICAL SERVICES CENTER

P. O. Box 36506  
Houston, Texas 77036

13 February 1980

Dr. Leo J. Hickey  
Division of Paleobotany  
Smithsonian Institution  
Washington, D.C. 20560

*file  
Cornet*

Dear Leo,

Where did you get the idea (Geotimes, 1980) that my angiospermoid grains from the Late Triassic and Early Jurassic actually belong to a group of gymnospermous-seed plants? I did not say that nor do my data indicate that. On the contrary, my data show a relationship between angiosperms and polyplicate-producing gymnosperms. I am not sure of your motives in presenting the Geotimes article, but I will assume until otherwise indicated that you obtained misinformation about my Dallas talk. So that you have it straight from the horses mouth, I have enclosed my abstract and additional information to support my viewpoint.

It would be highly premature to attempt a classification of my angiospermoid pollen without megafossil evidence. The data now in hand show unique combinations of exine structure and pollen morphology known only for angiosperms. If any conclusions about affinity should be drawn, they would have to indicate an angiospermous one. Yet, because of uncertainties about rates of pollen evolution and the significance of pollen morphology and exine structure, it is best to leave my discoveries unclassified. The majority of botanists and paleobotanists to whom I have talked admire such an approach. In my talk I left the question of affinity open, although I did present the idea that my pollen types are probably representative of early stages in angiosperm phylogeny.

What my data show is a sequential evolution of pollen morphology from polyplicate to monocotyledonoid and dicotyledonoid types. Presumably the pollen was not very far ahead of the gynoecium in its evolution. Insects probably played a major roll in pollen evolution, and I find it hard to believe that the gynoecium would not have changed to an equal degree. Although remote pollen germination and a closed carpel do not an angiosperm make, I have every reason to believe that my angiospermoid grains have something to do with the early stages of angiosperm evolution.

Did you grasp the significance of the noteworthy palynomorph in the ICP Newsletter? I've got Sanchezia-type pollen in the Oxfordian, as well as Stel-latopollis-type Liliacidites (see enclosures)! Can you explain that? Can you explain polyplicate-pollen morphology in other angiosperm families? I think it makes a much better story.

It is important that my work be correctly cited. A correction is in order.

Sincerely yours,

*Bruce*  
Bruce Cornet



A SUBSIDIARY OF GULF OIL CORPORATION

Hunt Institute for Botanical Documentation

TO AT

DATE Thurs. 14 Feb. TIME 10:30

WHILE YOU WERE OUT

M. Bruce Cornet

of \_\_\_\_\_

Phone \_\_\_\_\_

TELEPHONED  PLEASE RETURN CALL

CALLED TO SEE YOU  WILL CALL AGAIN

RETURNED YOUR CALL  RUSH

11:15 BE THERE!

MESSAGE

Wants to discuss article by  
Hickey in Feb. Geotitles  
(I tried to locate - hasn't  
come in yet to CMS lab. or  
field offices)

B.T.

Signed

MOORE BUSINESS FORMS, INC. E

5 February, 1980

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506 (North 1206)  
Houston, TX 77036

Dear Bruce:

Do you have knowledge of any summer employment possibilities for an outstanding recent bachelor's graduate? I have a student here now, Martin Farley, who will be getting his bachelor's degree in May. He is one of our top graduates for the last few years, and will be going to graduate school somewhere in the fall. It would be very helpful to him to get some practical summer experience if one of the oil companies could employ him as a summer helper. Perhaps you'll find time to let me know if Gulf has any such opportunities. Mr. Farley is writing most of the oil companies the usual exploratory letter, but I have know from the past that frequently nothing much happens as a result of such letters, so it would be helpful if you could explore the situation in Houston.

I'm also looking forward to hearing more about Alfredospora (Alfredopollis?), and hope you've made some progress on that. By the way, there is no reason why it can't be spora instead of pollis, despite the fact that the entity itself may be a pollen grain. Best wishes as always.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

xxx

7 January, 1980

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506 (North 1207)  
Houston, TX 77036

Dear Bruce:

I enclose a xerox copy of a letter from Klaus--  
answer to an earlier one of mine. Perhaps you should  
respond directly. It would bypass the headaches you  
will have with Palynology.

All the best for 1980.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et  
encl: xerox of Klaus letter

XXXXXX

29 November, 1979

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506 (North 1207)  
Houston, TX 77036

Dear Bruce:

How serious were you about making Dunay's "New Genus A" a new genus (Alfredospora)? Do you have enough specimens to produce a comment paper on the dealie, with the nomenclatural change included? If so, I believe I can get it pushed through in J. of P. I would greatly enjoy the little joke!

Day after Thanksgiving your "ex" dropped in on the way to State College to visit a friend. It was good to see her. Also was good to see you in Dallas.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

XXXXX

23 October, 1979

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506  
Houston, TX 77036

Dear Bruce:

Thought you'd like to see this. That statement  
on pp. 89-90 is irritating!

See you in Dallas.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et  
encl: copy of letter to Dunay

XXXXX

3 October, 1979

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506  
Houston, TX 77036

Dear Bruce:

The Donna Roberts about whom you phoned represented some sort of a very fancy employment agency trying to find a stratigraphic palynologist with experience. She asked if I knew one who would contemplate a change from his present employer, and I mentioned you, but cautioned her very sternly not to call you at your place of employment or otherwise to jeopardize your position there. I made no promises for you at all, but it seemed to me something you might want to look in on, and I didn't want to queer that possibility for you. Hope it was all right. See you in Dallas.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

Fidelity Omon Skin  
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XXXX

7 September, 1979

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506  
Houston, TX 77036

Dear Bruce:

Thanks for the birthday greetings. Sorry I was not present in person to receive them! Interesting to hear of new turns in your life. Of course, we still love Ginny, too, and are sorry it didn't work out.

One of my current undergrads was looking for a special studies project, and I gave him some of the Taylorsville samples from Weems to work on, referring him to your thesis.

Can you give me the reference to Weems thesis (your thesis does not reference one) if there is one? Better yet, do you have a current address for Weems, so I could get in touch?

Hoping you can help.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

TO AT

DATE Wed, 5 Sept. TIME 4:30

WHILE YOU WERE OUT

M. Bruce Conner

of \_\_\_\_\_

Phone \_\_\_\_\_

TELEPHONED  PLEASE RETURN CALL

CALLED TO SEE YOU  WILL CALL AGAIN

RETURNED YOUR CALL  RUSH

MESSAGE Happy birthday!  
Worcester final last Thurs.  
Definite angioperms in  
Truncis, etc. (See me for  
details)

BT  
Signed \_\_\_\_\_

102705

XXXXXX

18 July, 1979

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506  
Houston, TX 77036

Dear Bruce:

Thanks for yours of 1 July--came while I was on a (non-professional) trip to New Brunswick to visit relatives (including the Evans family with whom you once over-nighted in Woodstock--they now live near St. John).

The Bock books are on their way to you. Thanks for the \$30.00. Those heirs of WB were idiots to destroy all the books!

Don't worry about the Cousminer thing. (I am going to be funded it appears.) I mentioned it to emphasize that I wasn't out of line in being concerned at the time about the "leak". Harry is a good, personal friend whom I like very much.

I agree re the idea of some joint papers on the Newark Supergroup and will be in touch before too long about some of that. I would not regard it as ethical to do otherwise, if I am so lucky as to get some publications out!

Now for some (unsolicited, I know) Dutch-uncle advice--your "angiospermoid pollen" evidence is fascinating, but don't muddy your contributions re biostratigraphy by coupling the two things. The angio bit is highly controversial--and, in my opinion, you are susceptible to the criticism that you have been a tad extravagant in your claims. On the other hand, your detailed palynostratigraphic work is and was a fine contribution and should be published whether the angiospermoid stuff is or not. (Jim Doyle told me in Ottawa that you have got something out in some offbeat journal--do I get a copy?)

If you were here I can imagine our having a good arm-waver about all of this! But please remember that I have always been your friend and have wanted you to put your best foot forward so that your true worth would be recognized. I

Cornet, pg. 2

don't want you to have to found your own journal and buy a printing press, as has happened to several acquaintances of mine of whom you at times remind me too much!

All the best. Let me hear more about araceous Equiseto-  
sporites! (One problem is that most araceous forms have little sporopollenin.)

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

# Gulf Research & Development Co.

HOUSTON TECHNICAL SERVICES CENTER

P. O. Box 36506  
Houston, TX 77036

1 July 1979

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

Dear Al,

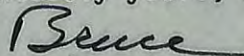
Thank you for your informative letter of 20 June. I most certainly would like to purchase the copies of "The American Triassic Flora...." and "Systematics of Dichotomy..." for \$30.00 dollars (which is enclosed). They are collectors items even if Bock's botany must be read with a sense of humor.

Cousminer doesn't have that much from the Newark. If he thinks he has a selling point with a dozen slides and no additional residue or rock samples, you have a gold mine. Don't let him undercut you with B.S. I am in a worse bind than you in that I would love to publish more on the Newark but have no time or money to do the kind of job necessary. My manuscript on Late Triassic angiospermoid pollen is nearing completion; it will contain a chapter on the palynostratigraphy of the Newark Supergroup, as well as some modified figures from my thesis. If you can get the student help to do the basic research, counts, etc., I would love to coauthor some papers with you on various aspects of Newark palynology. Perhaps a few short papers would be best. Just to get the material in print you can take senior authorship. You need that exposure for university position; I do not. I will help in any way possible!

I may have the palynological evidence from the Jurassic necessary to prove that angiosperms existed since the Triassic! During the routine study of outcrop samples from the Jurassic of Europe, purchased by Gulf for foram, dino, and spore-pollen analysis, I discovered a Callovian sample from the Oxford Clay that contains a large percentage (0.001%!!!!) of large specimens of Liliacidites sp. There is no question that these specimens are fossilized, and it is very unlikely that they are contaminants. No other angiosperm pollen types are present. But the spectacular aspect of this discovery is that these flat pollen grains are virtually indistinguishable from the pollen of Lilium bulbiferum, which has a unique type of reticulum composed of closely spaced clavae joined at their heads. The clavae at "triple junctions" of the reticulum are triangular in shape just as are those of L. bulbiferum. They are a little larger than the extant species. There is no way Doyle can say with any credibility that the monocots didn't exist in the Middle Jurassic!!! I would go so far as to say that the Liliaceae existed then. With my Triassic evidence, that statement is no longer absurd!

Yes, the Araceae do produce pollen indistinguishable from Gnetaceaepollenites and Equisetosporites. That I will prove!

Sincerely yours, and best wishes,

  
Bruce Cornet



A SUBSIDIARY OF GULF OIL CORPORATION

Hunt Institute for Botanical Documentation

XXXXXX

20 June, 1979

Dr. Bruce Cornet  
Gulf Research & Development Co.  
Houston Technical Services Center  
P.O. Box 36506  
Houston, TX 77036

Dear Bruce:

Thanks for return of the library book. Knowing you as I do, I assume that you xeroxed her from stem to stern! Did I tell you that Bock's heirs disposed of the stock of back copies as waste paper? Ouch.

I have, as the result of considerable detective work, come up with one additional copy each of "The American Triassic Flora...." and "Systematics of Dichotomy....". If you're interested, I'll sell you the pair for the purchase price I paid, plus phone calls to find them, for \$30.00 postpaid.

I just reviewed a proposal by Cousminer that overlaps, nearly undercuts, mine to work on the Jurassic-Triassic of North America with Sid Ash. Cousminer's biggest selling point is "all the material he has from Cornet's doctoral work". I hope we have as good! I remember that you didn't understand at the time why I was concerned about the shipments to Cousminer.

Hope you continue on an upward course.

Yours very truly,

Alfred Traverse\*  
Professor of Palynology

AT/et \*dictated by AT, signed in his absence

P.S. Dear Bruce: Thanks so much for the prompt and thorough reply to my inquiry about your slides, etc. Should do the job.

Betty

# THE PENNSYLVANIA STATE UNIVERSITY

DEIKE BUILDING

UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences

Department of Geosciences

Palynological Laboratories

Area Code 814

~~X 365-6343~~

865-2342

5 June, 1979

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506 (North 1207)  
Houston, TX 77036

Dear Bruce:

At last I am accessioning your slide and residue collection you made for us before you left. For the most part I was able to match up the slides and residues with the card file and also (more important) with actual locations described in your thesis. I added significant page numbers from the thesis to the cards so we can identify the samples easily. Thank you very much for making things easy for us for the most part.

However, I do have questions about some of the slides. All of the items listed in Section 1 below represent labelled slides <sup>and residues</sup> which I could not match up with cards. Could you please identify them? I realize that many (e.g. the very first one) are very thoroughly labelled, but I still don't know just what collecting locations they represent, so cannot match them up with ~~residues~~ or pages in the thesis. I could have guessed on quite a few, but didn't want to risk mistakes.

## Section 1 Questions

(all notations are direct quotes from slides)

### Novacaesarea Group

- 1) upper Towaco Fm, Western edge basin, North of Watchung, NJ  
BT-4 2-25-75 P. 410 RV-TOW an early sample provided by P. Olsen from same zone.
  - 2) Newark NJ 23A-8 P. 367 SPAS-23A
  - 3) " " 23A-7 " " "
  - 4) Jacksonwald RT 562, Loder A-3, 9-25-75 station between OLA and JB2, not indicated in Thesis.
  - 5) Metlar's Brook below Laminite, New Brunswick, NJ 9-22-75 a "barren" soil zone with plant fragments.
- Conewago Group *Upper part of Lockatong Fm. - P. Olsen collected it late in writing my Thesis. Produces only palynomorph ghosts and poorly preserved Patinasporites.*

- 1) Pa, NO, YH
- 2) New Oxfd. Fm, York Haven, PA (incidentally, I assume that 1&2 are same)
- 3) " " Brown 2-3
- 4) " " " 2-4

same as 1+2 but different color rock.

locality mentioned in 1973 Science paper, Al and I visited it before discovering the plant beds - same place as LCC (P. 91.) but a few meters stratig. higher.

② circled numbers are for number of slides made.

Taylorsville Group

- 1) A
- 2) Ec
- 3) Ion
- 4) K'b

} localities provided by Weems. Will send copy of his rough stratigraphic location map. That is all I have. Al may have copy of letter from Weems. (1974?)

Chatham Group

- 1) Deep River N. Carolina HU-17190 ← Harvard Univ. sample from lower? Pekin Fm. provided by Barghoen.

Culpeper Group

- 1) Thoroughfare Gap, Cotlett's Branch CTB ← P. 446 WEB
- 2) " " Milbrook Quarry #3 ← P. 447, MRK

Richmond Basin

- 1) Blackheath coal mine underclay of lowest coal, richmond Basin  
Tributary Falling Creek *New locality not in thesis, collect. by P. Olsen.*
- 2) Dictyopyge fish beds, Richmond Basin, Barren Beds in Boscabel Quarry

← *BB1 (on p. 47 mentioned, 1st pp, line 8)*

Hartford Basin

- 1) NG INTER - SS (4) *Just above labeled in sandstone interbed of shales 1973 Science paper. (near 2 meter mark on Fig. 1)*
- \* 2) DK *at 6 meter mark on Fig. 1, Science paper.*
- 3) BSA-7 N. Guilford CT underclay 8-23-74
- 4) BSb " " " "
- 5) BSilt
- \* 6) BS *lowest sample in Fig. 1, science paper (different preparations from same unit)*
- 7) SCL-3
- 8) S C3-3
- 9) SOIL (2) ← *at 1 meter mark on Fig. 1, Science paper*
- 10) BLS 23AB ← *2nd sample mark above base on Fig. 1 in Science paper*
- 11) NSL BS } *lake bed? in Fig. 1*
- 12) (SSCL) LS
- 13) Hadley, Ma, Portland Fm HAD001-3 Yale Specimen, *Pagiophyllum matrix. That's all*
- \* 14) LTG1, LTG5 *same as DK, at 6 meter mark on Fig. 1, Science paper*
- \* 15) LTS2
- \* 16) LT17, LT 18, LT 25, LT 26, LT 36, LT 37, LT 43, LT 47, LT 105

Section 2

Other questions

\* LT = light colored claystone  
 DK = dark " "  
 BS = basal siltstone (nothing more :))

Taylorsville Group

Is M' the same as M'b? *yes no: They are different samples from the same unit at the same locality*

Culpeper Group

Does WED (on slides) = WEB (on cards, residue, thesis)? There are no slides labelled WEB and no " " , thesis location labelled WED (P. 446), slides labelled CTB for Cotlett's Branch.

(17) = consecutive numbers on slides.

Richmond Basin

There are six slides with no cards or residues labelled as follows:  
Richmond Basin, Rt. 6, #12a-4, #12a-5, #12b-4, #12b-5, #12c-4,  
#12c-5. How do these fit into thesis?

*all = VBI and V8g.  
This was a later sampling of the same  
palyniferous unit. a, b, c are  
subdivisions of that unit.*

Hartford Basin

Does P1=PL1? *yes*  
Does PL=PL1? *yes*  
Does SMF "top" and/or "near top" = SMF 12? *yes*

That should do it. I hope you can figure out what I want. Perhaps some of the slides and residues have no location numbers and/or letters referable to the thesis. There is no real rush on this. I decided to sort the collection and make a permanent list when Carmen Tavera wanted to borrow some of the slides and had some questions about referability to thesis locations.

I was very distressed to hear about you and Ginny. Living is full of too many stresses these days.

Yours very truly,

*Betty*

Betty Traverse

*Hope this will help.*

*most from memory - as if only yesterday 😊*

*Best wishes,*

*Bruce*

# Omni

MAY 1979 \$2.00

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*CORNET*  
*p. 36*

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XXXX

31 May, 1979

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506 (North 1207)  
Houston, TX 77036

Dear Bruce;

Thanks for yours of 26 April. I very much regret it has taken so long to answer it, but the month of May is one of the two worst in the year, as you may remember (the other one obviously is December!). Too bad about the divorce. You sound as if you were resigned to it. It's just that it's hard for us to imagine you two apart. Glad that you were able to get what you wanted out of the Bock book and that it will be "returned to sender" one of these days.

Sorry about the rejection of your paper on angiospermoid pollen. I'll bet that if you had me around as your rectifier still we could get a couple of things out about all of that! On the other hand, that would probably be hard on your nerves and mine.

That was fun about the Cedar Rapids "Cornets". Since you sent me that clipping, I have been noticing other uses of the word cornet in titles such as that of a rock group around here. Hang in here.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

26 April 1979

Dear Al,

I THINK I FOUND THE REASON FOR ALL MY PROBLEMS. DO YOU HAVE AN SUGGESTIONS?

I hope that I am not a basket case.

More seriously, George relayed your message about palynological localities in the Dan River Group. I know of none. Paul Olsen tells me he has found a sample with badly preserved palynomorphs, which gives him hope of finding a paleoenvironment that preserves pollen and spores.

I will soon be returning the copy of Bock (1969). Thank you so much for its use.

I am nearing completion of yet another manuscript on my angiospermoid pollen. This one will be my last attempt; my first manuscript to Nature was rejected due to prejudice on the part of the one reviewer! I am including all of my ammunition on palynology and avoiding the megafossil evidence for now.

Vaughn Bryant has invited me to give a lecture at Texas A & M on my discoveries this coming Monday. At the AASP luncheon today I spoke at length with Vaughn and Jim Canright (Executive Meeting representatives), and was pleasantly surprised to find Jim more open to my ideas than he was at Phoenix.

Ginny filed for divorce last week. She is determined to end our relationship, even after several months with a marriage counselor. Frankly, I am glad this hell will be over soon (at least in 60 days). I find myself rebounding from a lengthy emotional depression. She is no longer the right woman for me.

On a more cheerful note, my mother is doing very well here in the interior designing business. Job prospects for geologists have reached an all-time high. I am happier at my job than I have been in the past.

Give my best to Betty. Hope all is well with you. If there is anything I can do, let me know.

Sincerely yours,

*Bruce Cornet*

Hunt Institute for Botanical Documentation

Sports

The Houston Post  
Tues., April 24, 1979

D

# Cornets run past Angels

## Post News Services

CEDAR RAPIDS, Iowa — Denise Sharps scored 31 points to lead Iowa to a 110-101 win over the Houston Angels Monday night and force a fourth game in the Women's Professional Basketball League championship playoffs.

The win was the first for Iowa in the best of five series. Houston leads the series 2-1 and needs one win to take the WPBL title. The next game in the playoffs will be Thursday in Des Moines.

In addition to leading all scorers, Sharps held Houston's 5-foot-11 center, Paula Mayo to 13 points. Mayo, suffering from a badly mangled index finger on her shooting hand, scored 25 points in the last Houston victory.

"You can't say enough about Denise Sharps," Cornets Coach Dan Moulton said. "She said, 'Put me on Paula Mayo.' She doesn't let it (defense) affect her scoring."

Iowa slapped on a full-court pressure defense on the way to a 51-41 halftime lead. The Cornets led by as many as 16 points in the second half before foul shots cut the lead to the final nine-point margin.

Iowa out-rebounded the Angels one of the few times this season, 70-50. Belinda Candler, who averages 11 rebounds per game for Houston, was held to five, while Joan Uhl and Doris Draving grabbed 37 between them for Iowa.

Mollie Bolin and Doris Draving each scored 19 points for the Cornets and Joan Uhl, whose position in the starting lineup was taken by Connie Kunzmann, had 12.

### HOUSTON (101)

Mayo 5 3-5 13, Candler 4 2-2 10, Aulenbacher 6 5-6 19, Jones 9 9-14 27, Holleyman 0 1-2 1, Kenlaw 2 2-4 6, Johnson 3 1-3 7, Wadell 0 2-2 2, Washington 1 3-5 5. Totals — 35 32-47.

### IOWA (110)

Sharps 14 3-6 31, Bolin 7 5-8 19, Draving 6 7-10 19, Tucker 2 2-2 6, Kunzmann 3 3-7 9, Uhl 5 2-2 12, Green 2 2-2 6, Penquite 0 0-0 0, Rutter 0 4-4 4, Thomas 2 0-0 4. Totals — 41 28-41.

21 20 23 37-101  
Houston 23 28 23 36-110

Total fouls — Houston 28, Iowa 40. Fouled out — Uhl, Kunzmann, Chapman, Mayo. A — 2,250.

12 February, 1979

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506 (North 1207)  
Houston, TX 77036

Dear Bruce:

I am sending you under separate cover the library's copy of Bock's book you wanted.

There has been this delay because I was to give a lecture at U. of PA in Philadelphia last Monday, and I was sure I could turn up a copy of the book for you. However, my hosts at Penn had had the same thought and had failed utterly. There are no copies at the Academy except in the library, and Professor Pfefferkorn had not succeeded in finding any trace of Bock's materials in North Wales. Hmm.

I would like you to send the book back to me, not to the library.

I think of you often--especially when I talk of the many interests we had in common. All the best.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

# Gulf Research & Development Co.

HOUSTON TECHNICAL SERVICES CENTER

P. O. Box 36506  
Houston, TX 77036

7 January 1979

Professor David Dilcher  
Department of Biology  
Indiana University  
Bloomington, IN 47401

Dear David,

Thank you so much for your letter, reprints, and especially for the manuscript on early angiosperm reproduction. Of all the paleobotanists dealing with the problem of angiosperm evolution, I have been impressed with your objectivity and commitment to a balanced and unprejudiced viewpoint. Your latest manuscript pleases me because you stress the fact that paleobotany will ultimately prove the manner of angiosperm evolution, not the study of recent plants alone. You have presented a number of ideas which contrast with popular concepts. I support you fully, regardless where our data and conclusions eventually lead us. I have been hampered and misled by current dogma and prejudice. It has taken me about five years of concentrated effort to rid myself of many ideas that have apparently warped my interpretations of the fossil record. I cannot work objectively with such preconceptions of what I should find. I might go one step further than you in your manuscript by challenging the concept of the conduplicate carpel as being basic for floral construction. Perhaps your presentation is best - lead the botanical community away from textbook beliefs one step at a time.

But before you read my ideas below, I would like you to digest the enclosed copy of my latest manuscript sent to Nature (please excuse the typo's). It is a response to a provoking statement by Bakker (17 Aug. Nature 1978) that "...no trace of angiosperms has been found among Newark pollen and leaf floras." I intend to present my case in full as objectively as I can; this manuscript is a start.

The pineapple-like fructification is constructed much like that of Ananas, except that no detailed information about the pistils (i.e. number of locules, etc.) can be obtained from the compressions. Only cuticles and the more resistant parts of seed coats remain. Yet, the evidence demonstrates without doubt that this inflorescence was composed of over 150 flowers, each subtended by an elongate bract possessing the same type of cuticle as that of the bract-shaped leaves below the fructification. The cuticles of each flower are thinner and their cell patterns differ from those of the bracts. Several layers appear to encircle the seeds. In one cross section, a thin cuticle (perhaps belonging to the pistil) completely envelops one seed in the section. Another section may demonstrate four seeds in one ovary. Dissection of the flowers under a binocular dissecting scope revealed multiple seeds per ovary. Although the style, stigma, and the free parts to the perianth are missing, the fruits are in every respect angiospermous! See enclosed manuscript for further descriptions.



A SUBSIDIARY OF GULF OIL CORPORATION

Hunt Institute for Botanical Documentation

Bock (1969) unfortunately describes this fossil as an araucarian conifer, naming it Primaraucaria weilandii. The absence of a woody cylinder is demonstrated by the thinness of the compressions of the leafy shoots and fructifications. No coally compressions are present; no impression of a thick axis is apparent. No conifer is known to possess such a non-woody axis with leaves bearing anomocytic stomata!

Bock illustrates other reproductive structures closely associated with the fructification. He called them male cones, but did not demonstrate or attempt to recover pollen from them. If they are pollen organs, they may be elongate male inflorescences with expanded bract-shaped leaves at the base; these bracts give the appearance of an undifferentiated perianth. The floral bracts are large, and they obscure or hide any flowers; yet, they bulge out enough to indicate a considerable thickness of tissues underneath. Therefore, I cannot be sure that I am dealing with an inflorescence. Its large size and shape suggest that I could be. Primaraucaria weilandii would then be diclinous with male and female flowers in separate inflorescences.

In addition, I have a spectacular reproductive structure from Bock's collection that raises some very important questions on the origin of the terminal Magnolia flower. The flowers of this inflorescence closely resemble those of the round fructification described above, but they are each fused to their respective bracts, and possess even longer pedicels than those of the pineapple-like fruit. I can see the outline of the fused bract on each floral compression because of its coarser papillate cuticle. These flowers resemble carpels, but they each terminate in a blunt pore-like opening that may represent the scar for the attachment of dehiscent perianth and stigmatic parts. If these structures are mature, they cannot be compared with anything but mature carpels and fruits. I am making particular reference to their long pedicels.

Associated with this inflorescence, but not obviously attached due to the termination of the rock, are numerous, long, latrorse laminar stamens. The anthers even demonstrate a granular tapetum, but no pollen is present. The orientation of these stamens strongly suggests that they subtended the carpel-like flowers on the axis which is partly preserved on the specimen.

I am compelled to ask if this specimen may be demonstrating how the Magnolia-type of flower and carpel evolved. The original flower or "carpel" would have been pedicellate, unipistillate, epigynous, and unisexual. Selection for beetle pollination caused significant reduction in floral parts, resulting in the adnation of bract to flower and the reduction of the apical free part to the perianth. With such reduction, numerous gynoecea would be brought closer together with less area presented to the foraging beetles. The remainder of the perianth around the pistil served as an additional protective layer to the pistil, eventually becoming fused to it.

The male flowers, which in this species may have been positioned on the lower part of the inflorescence axis, were reduced to but a single pair of anthers. The subtending bract became the broad lamina as the remaining anthers fused with it. The advantage of such a structure would be an enlargement and strengthening of the pollen organs against the foraging of beetles. The resulting flower would have "carpels" and "stamens" that had identical vascular supplies or trace patterns. The Magnolia flower has accessory bracts or bud scales that are difficult to derive from a simpler flower. These bracts or bud scales would be readily explainable in terms of a reduced inflorescence. The similarity of carpels and stamens to leaves would be real - they were each partly derived from subtending bracts, which are the homologs of leaves! Your specimen in Fig. 51 may be better explained as a reduced

inflorescence. It may also show how the ventral suture or elongate stigma could evolve secondarily in response to an increase in number of ovules. Think about it.

If you are concerned about the derivation of a Magnolia-type stamen from a flower and its bract, consider Phelodendron (Araceae), for which male flowers have in some species been reduced to a similar degree. Also consider Gnetum, for which the male flowers are reduced to a single stamen-like pollen organ surrounded by its bract!

Angiospermoid pollen was found clinging to the bracts of the pineapple-like fructification. The type of pollen is most significant, because it represents the most specialized angiospermoid pollen from the Richmond Basin. Placopollis raymondii is a large (60-90  $\mu$  diam) tetrad of predominantly trichotomosulcate pollen grains. The distal exine of this pollen is unusually thick, and is underlain by dense stands of columellae, many of which are apparently fused to their neighbors. SEMG's show the presence of scattered tectal perforations, but I have found some specimens with a fine or small reticulum. In addition, the surface of each grain is sculptured with microbaculi, which support my interpretation that this pollen type is highly specialized. P. raymondii Koob is present in nearly all palynoflorules of the Richmond, Taylorsville, and Deep River basins. In the Richmond and Taylorsville it reaches about 12% abundance in the upper Vinita Beds and upper Falling Creek Fm. In the Cumnock Fm. (Deep River Basin) it reaches as much as 50%. Clearly this angiospermoid pollen type was produced by a successful and relatively abundant plant. It does not surprise me that I may have found megafossils of that plant in Bock's collection from the Productive Coal Measures of the Richmond Basin.

The large size, thick wall, and density of such a pollen tetrad would not have lent itself to wind dispersal. The thick wall, presence in tetrads, and micro-sculpture suggest that it was insect dispersed. P. raymondii may have been carried by beetles, and if its presence on the cuticles of Primaraucaria is significant, then it may support beetle pollination for that plant.

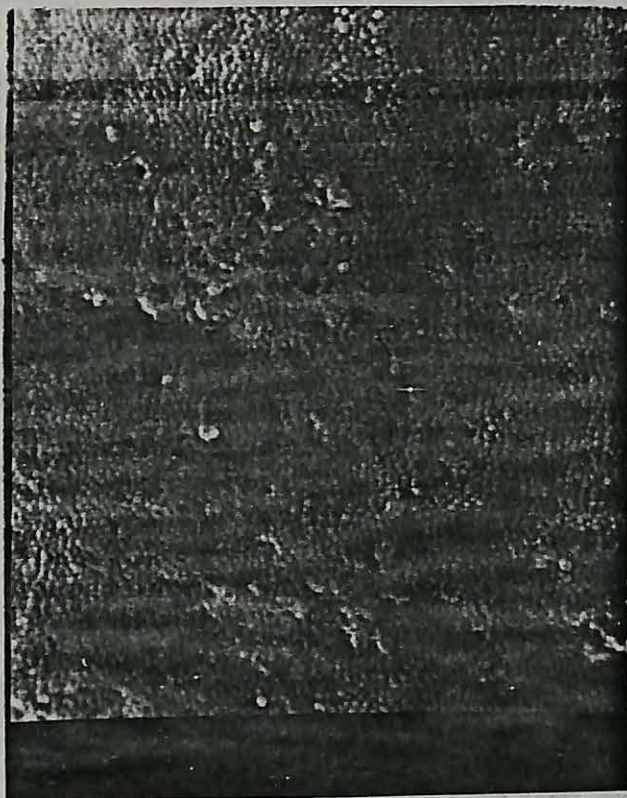
You may be questioning how the simple flower and inflorescence could be primitive. Could the inflorescence be a reduced fertile pteridosperm frond? The pistil might be derived from a terminal cluster of ovules surrounded by bracts or pinnules, i.e. a Paleozoic cupule. The palm flower can be very simple, showing three ovules each opposed to a pistil valve. The flowers of Tamarix spp. are equally simple in construction and their bract-shaped leaves may never have undergone an expansion phase similar to that of most dicot leaves. Thus, the simple flower could be derived from a shortened fertile pinna; the pistil from the union of three or more pinnules. These pinnules would later become conduplicate as the pistil was divided into three or more locules. Therefore, the conduplicate carpel may have a different origin in different groups of angiosperms. I would be cautious about stressing the conduplicate carpel as the basic structural unit of the flower. I feel that it may not be basic or even primitive for all angiosperms.

I hope that the above discussions will help you. I am impressed with the round inflorescence you describe from the Middle Cretaceous. If my Late Triassic megafossil fructifications are angiospermous, your Platanoid balls might be viewed as more primitive than you formerly thought.

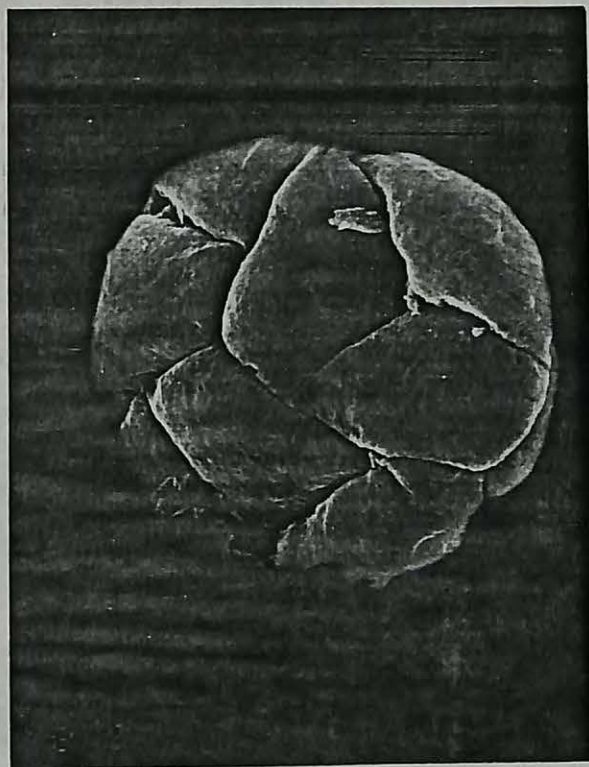
Sincerely yours and best wishes,



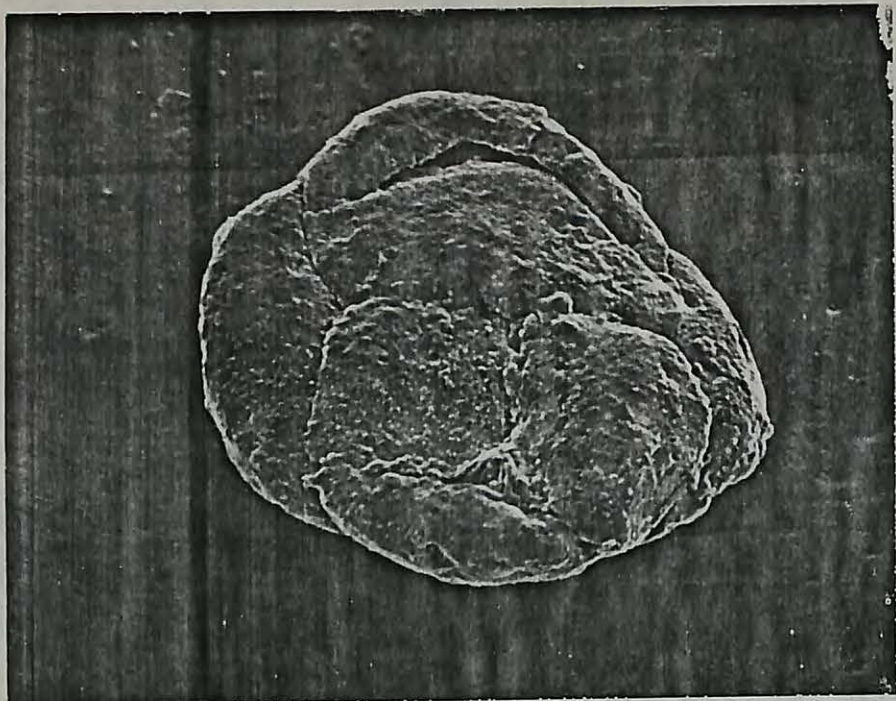
Bruce Cornet



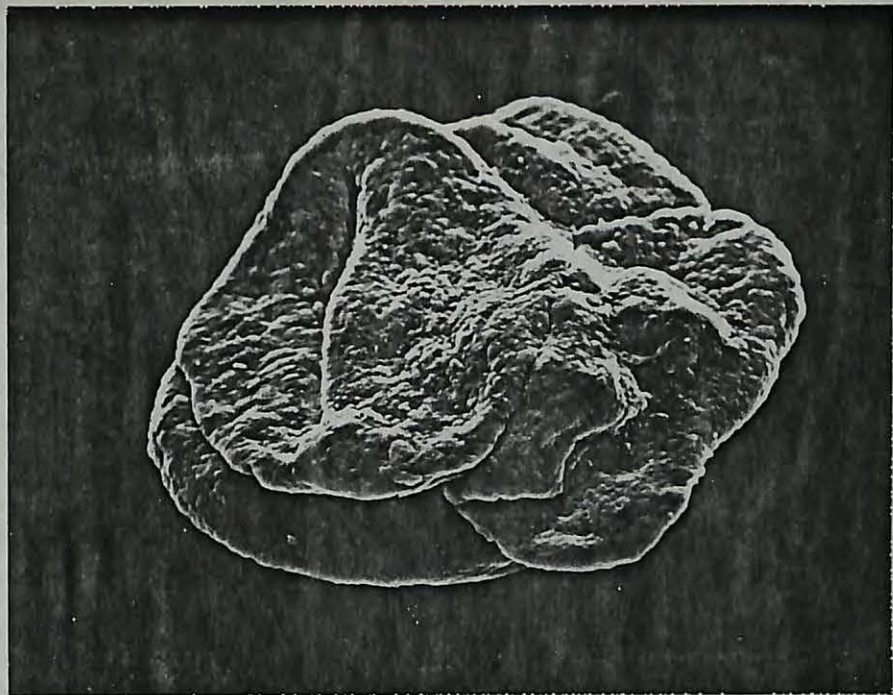
"Placopollis raymondii"  
C4 VB4



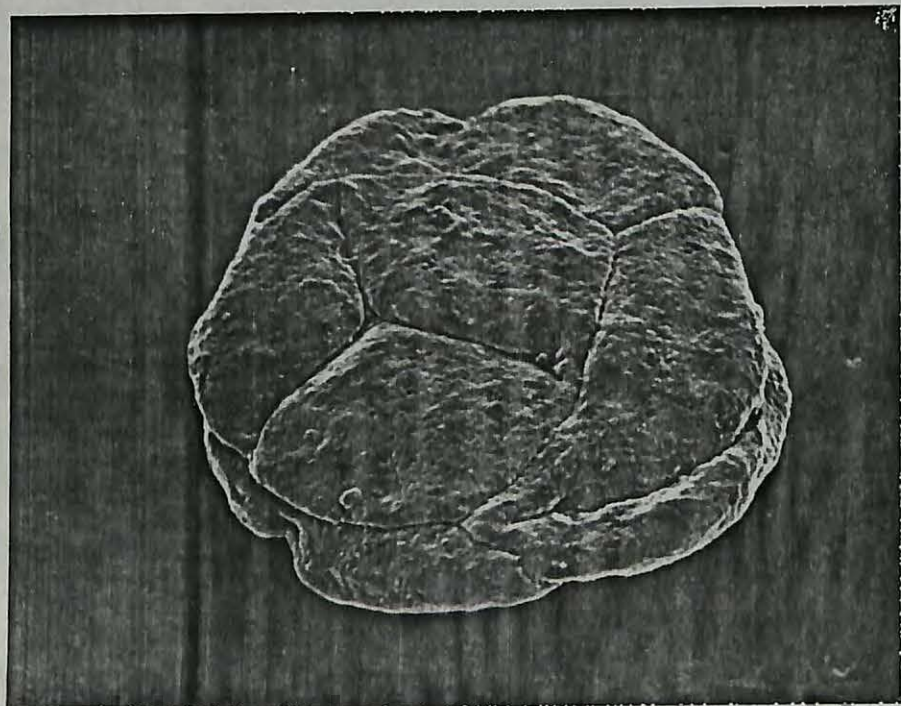
"Placopollis raymondii"  
C4 VB4



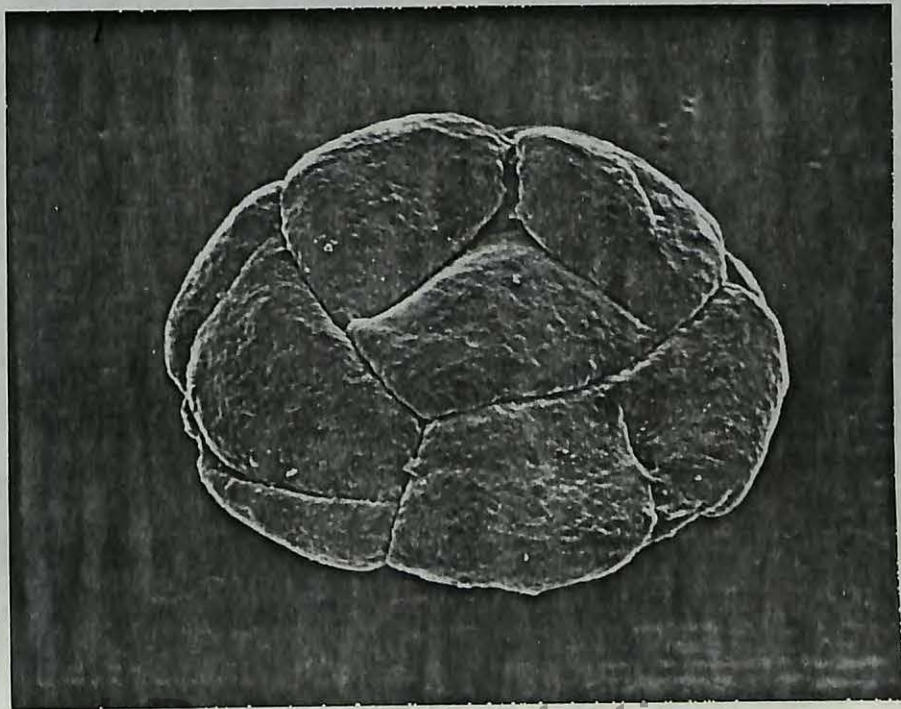
43  $\mu$  diam. VB4-112 1.25K 1mm = .8  $\mu$



71  $\mu$  diam. VB4-104 1.3K 1mm = .769  $\mu$



66  $\mu$  diam. VB4-112 1.25K 1mm = .8  $\mu$



63  $\mu$  diam. VB4-112 1.25K 1mm = .8  $\mu$

# Gulf Research & Development Co.

HOUSTON TECHNICAL SERVICES CENTER

P. O. Box 36506  
Houston, TX 77036

19 December 1978

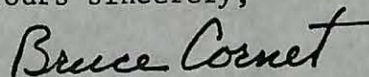
Mr. David Davies, Editor for NATURE  
McMillan Journals Ltd.  
4 Little Essex Street  
London WC2R 3LF England

Dear Mr. Davies,

Bakker makes a statement about the Newark Supergroup in his article on "Dinosaur feeding behavior and the origin of flowering plants" (Nature 274, 661-663, 1978) which is incorrect and misleading. He states on page 661 that "... no trace of angiosperms has been found among the Newark pollen and leaf floras." James W. Walker and I have a manuscript in preparation on rare but diversified angiospermoid pollen from Newark rocks of Carnian, Norian, and Rhaetian age. Bakker's acknowledgement of James Doyle, who has been well aware of our work for about four years, and who has referenced my abstract on these discoveries (Doyle, 1978, Cour. Forsch. - Inst. Senckenberg, 30: 54-61), makes such a statement potentially irresponsible.

Would it be possible for you to publish in NATURE my comment and reply to Bakker's paper? I have enclosed a brief article with plate that will adequately explain our discoveries, and set the stage for the release of more detailed information.

Yours sincerely,



Bruce Cornet  
Senior Geologist and Paleopalynologist



A SUBSIDIARY OF GULF OIL CORPORATION

Hunt Institute for Botanical Documentation

Dinosaur feeding behavior and the origin of flowering plants: Comment and reply

COMMENT

Bruce Cornet, Gulf Research & Development Company, Houston Technical Services Center, P.O.Box 36506, Houston, Texas 77036

Bakker (1) presents a novel theory for the sudden appearance and subsequent radiation of angiosperms in the Middle Cretaceous. His evidence and ideas add to our understanding of the dynamic floral and faunal changes that occurred in the Early Cretaceous, which opened opportunities for the rapid migration and diversification of angiosperms, presumably in the form of riparian shrubs and small trees. His theory, however, provides no information about the origin of angiosperms; his use of the Newark Supergroup (2,3) of eastern North America as a test area for the absence of pre-Cretaceous angiosperms is unfortunate:

For at least four years I have been studying numerous and diversified species of angiospermoid pollen (4,5) from Newark rocks of Carnian, Norian, and Rhaetian age (6,7,8). These species are distinguished by their tectate-columellate exine structure and reticulate to perforate sculpture. Thus far, the Richmond, Taylorsville, Deep River, Culpeper, Gettysburg, and Newark basins have produced rare (usually less than 1%) angiospermoid pollen grains. Species from basins containing only Carnian strata, e.g. the Richmond and Taylorsville Basins (3,8), resemble those of some monocots in possessing a coarse proximal reticulum supported by prominent columellae and a fine distal reticulum underlain by obscure columellae. Aperture number varies from five to two; all apertures are restricted to that portion of a grain possessing a finely reticulate sculpture. Those species which are best described as monosulcate have two closely-spaced subparallel sulci separated by an operculum, which may or may not be reticulate. Some species resemble the pollen of Calectasia cyanea (Xanthorrhoeaceae) (9) in aperture complexity. Size varies from 24  $\mu$  to 88  $\mu$  in length. Shape varies from elongate-boat-shaped to round. Trichotomosulcate and zonosulcate aperture types are present. Twelve new species belonging to six new genera have been recognized for the Richmond Basin of Virginia (10). Some of these species are illustrated in Figure 1.

This assemblage of angiospermoid grains is found in Richmond Basin palynoflorules containing numerous species of age-diagnostic pollen and spores: Patinasporites densus Leschik, Vallasporites ignacii Leschik, Camerosporites secatus Leschik, C. pseudoverrucatus Scheuring, Pseudoenzonalasporites summus Scheuring, Triadispora spp., Ovalipollis ovalis Krutzsch, Duplicisporites granulatus Leschik, Parillinites pauper Scheuring, Striatoabieites aytugii Visscher, Lunatisporites acutus Leschik, Lagenella martinii (Leschik) Klaus, and Aratrisporites spp. (3,8).

During the Late Triassic the paleoequator was migrating south across eastern North America (11). By the Early Jurassic the Hartford Basin of Connecticut and Massachusetts was about 10 degrees north latitude (12). During the middle Carnian, the Richmond Basin of Virginia was probably less than 10 degrees from the equator. The megafossil and microfossil floras of that basin, particularly from the coal measures, are rich in ferns and their allies (3,13,14), suggesting wet tropical conditions. Newark deposits of late Carnian age indicate a generally drier climate that periodically cycled between humid and arid conditions (2). In the Norian the climate appears to have become less cyclical and more seasonally arid as the paleoequator migrated further south (15). By the late Norian and Rhaetian the palynofloras

of the Newark were dominated by non-striate bisaccates, circumsaccates, and Corollina spp. (3), and red floodplain deposits predominated where lakes had periodically filled large basins (16).

Angiospermoid pollen of the Newark Supergroup changes in composition and morphology through the Late Triassic in response to climatic change and distance from the paleo-equator. By the Rhaetian, the distinctive forms of the Carnian and early Norian (with well-developed reticulate-columellate wall structure) are no longer present. They are replaced by monosulcate, monoporate, and zonosulculate morphotypes with densely to sparsely spaced tectal perforations and small or obscure columellae (demonstrable in TEM cross sections). The Rhaetian species tend to resemble ranalean pollen of some Magnoliales and Winterales (17), whereas species from Carnian and early Norian strata find their nearest analogs in monocot pollen (18).

Outside of the Newark Supergroup, possible angiospermoid pollen has been tentatively recorded in the Chinle Formation (8) of the southwestern United States, and in the Rhaetian of Europe (19) and Arctic Canada. Prior to the Rhaetian, however, such pollen appears to be geographically restricted to tropical or subtropical areas near the paleo-equator. Major worldwide floral and faunal changes in the Early Jurassic (3,21) disrupted the distribution of Triassic communities, resulting in the apparent disappearance of many Triassic plant taxa. Tropical areas of Jurassic deposition near the equator are poorly known or have limited access in subcrop. Before the above discoveries were made, tropical areas in the Triassic were also poorly known.

Whether or not these Late Triassic angiospermoid pollen grains were produced by angiosperms, they possess unique combinations of characteristics reported only for angiosperm pollen (5,21). The very structure of their exines raises many questions of function and significance. Many botanists will undoubtedly require megafossil evidence, particularly angiospermoid pollen recovered from indisputable flowers, before accepting the existence of Late Triassic angiosperms. Nevertheless, palynological evidence as strong as that reported here must be seriously considered in any future theory of angiosperm evolution. Bakker was premature in stating that there is no trace of angiosperms among the Newark pollen and leaf floras.

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Figure 1 Angiospermoid pollen from the upper Vinita Beds, Richmond Basin, VA. a, trisulcate sp. 4, oblique distal view, 57  $\mu$  long; b, trisulcate sp. 4, oblique proximal view showing coarse reticulum and prominent columellae, 56  $\mu$  long; c, trisulcate sp. 4, oblique proximal view, 47  $\mu$  long; d, monosulcate sp. 6, proximal view, 46  $\mu$  long; e, monosulcate sp. 1, oblique lateral view, 45  $\mu$  long; f, monosulcate sp. 1, oblique distal view showing operculum, 44  $\mu$  long; g, monosulcate sp. 16, proximal view, 35  $\mu$  long; h, monosulcate sp. 16, proximal view showing columellae under reticulum, 32  $\mu$  long; i, monosulcate sp. 16, oblique apical view, 24  $\mu$  in diam.; j, trisulcate sp. 7, showing four sulci with a suggestion of a fifth one, 36  $\mu$  long; k, trisulcate sp. 7, apertures in tricolpate configuration, note manner of attachment, each grain 31  $\mu$  long.

Dear Al and Betty,

Dec 78

We finally made the big move and bought a 2 bedroom, 2 bath condominium in a lovely old white brick apartment complex built in the 50's. Ginny and I are trying very hard to overcome her desire for independence, but it may take a trial period of separation. Don't think I won't be keeping our love strong - that is our best hope to overcoming our problem. If Merrill Mining doesn't come through soon, I may seek a teaching position in Virginia not only to keep active in the Newark, but to actively help Merrill find financial support for drilling. I have just completed the plates to part I of a manuscript on my angiospermoid pollen:

11 new species, 5 new genera (one species, Pentecchino-pallis traversus, named for an inspiring advisor). I hope Walker can understand my objective interpretation of these data, but his prejudices may be a problem.

Best wishes, and



May your halls be decked with happiness at Christmas and all year!

Love,

Bruce and Ginny

P.S. We are trying hard to get through our difficulties & with God's help, I think we'll make it - Ginny 😊

4 December, 1978

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506 (North 1207)  
Houston, TX 77036

Dear Bruce:

This will be an altogether too brief answer to your delightful letter of 20 November. I am putting great hopes on the outcome of the matters mentioned in paragraph three.

It was fun to hear about your having been at the Northeastern Paleobotanical Meeting earlier this fall. I am sure I would have enjoyed being there myself and wish it had been possible. I did in fact consider it, but just having returned from almost two weeks absence made it not feasible. You are right regarding ignorance of palynology among megafossil paleobotanists--though it is certainly not universal. Leo Hickey would surprise you with how much he knows! The shoe fits on the other foot as well, however. Most of our paleopalynological friends, particularly in the oil companies, know nothing at all about plants, fossil or recent.

You are a good friend, Bruce, and I will always look forward to our meetings, as infrequent as they probably will have to be now that we live so far apart. I especially appreciate your support regarding the Mesozoic work here and hope that funding and students develop to make it possible for the things to go on. Best wishes to you for the holiday season.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

# Gulf Research & Development Co.

HOUSTON TECHNICAL SERVICES CENTER

P. O. Box 36506  
Houston, TX 77036

20 November 1978

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

Dear Al,

Thank you very much for your card and letter of 15 November. I will keep your grant proposal in strictest confidence. I am encouraged that you are pursuing the Early Mesozoic, and I want to help in any way possible. As soon as I can find a permanent home for my rock and sample collections, and have enough spare money to properly organize my collections, I will be sending you cuts from my thesis material. I only hope that something happens soon so that I can once again become involved in the Newark Supergroup.

Enclosed please find a picture that I took of you at Phoenix, while you were giving Norie's paper on the destiny of all organic material. After 600 m.y.'s of Phanerozoic, we are probably all just recycled crap. Norie will probably like to mess with that idea ☺. I have sent a copy of the picture to her as well.

Ginny and I will be seeking professional counselling. I hope it will work, but Ginny seems to have her mind made up to move away from the problem rather than face it. I will keep you posted.

I attended the First Northeastern Paleobotanical Meeting at Harvard Forest in Petersham, Massachusetts on the 3-5 of November. Henry N. Andrews gave an invited speech on important contributors to the history of paleobotany. Most of my old friends of U. Conn. days were there. Paul Olsen and I each gave talks. My talk on "Early polyplicate and angiospermoid pollen and their possible bearing on angiosperm evolution" was enthusiastically received, and created a lot of discussion on the subject. Most of those attending were hesitant about concluding anything about plant evolution based solely on palynomorphs. I was measurably disappointed at the lack of knowledge about and prejudice against palynology among all those paleobotanists. Andrews surprised me the most by his inability to distinguish between spores and pollen: he wanted to know why some of my (monosulcates) spores couldn't have been produced by ferns. Yet, I am glad I took the time and trouble to deliver my talk. Now they are aware that a revolution may be brewing that can not be ignored by making excuses. One way or the other, my discoveries will have to be correctly explained.

I enjoyed our conversations at Phoenix, and hope to have many more in the future. Give my best to Betty and your family.

Yours very truly,

*Bruce*

Bruce Cornet



A SUBSIDIARY OF GULF OIL CORPORATION

15 November, 1978

Dr. Bruce Cornet  
Geni Research & Development Co.  
P. O. Box 36506  
Houston, TX 77036

Dear Bruce:

Thanks for your help with the proposal! I enclose a copy of it for your files. At this stage it of course should not be circulated.

It was good to see you in Phoenix and especially to share a Mexican dinner with you at Exxon's expense!

I have thought much about your current difficulties and can only reiterate the good advice I gave you--get some counselling. I suppose University of Houston or one of the other Houston outfits has a family counselling service. My experience with such centers through my parish has been good. They are not expensive.

Fritz Cramer spent two days here on the way back to Spain, and left an indelible print!

All the best.

Yours very truly,

Alfred Traverse  
Professor of Palynology

encl: copy of proposal (confidential)

APOLLO 11 VIEW OF EARTH — Most of Africa and portions of Europe and Asia can be seen in this spectacular photograph taken from the Apollo 11 spacecraft during its translunar coast toward the Moon. The spacecraft was already about 98,000 nautical miles from Earth when this picture was made.

16 Oct '78

Dear Al, Thank you so much for the thoughtful birthday card. It finally arrived. ☺ I wish I had thought of you on your birthday in September, perhaps next year. Belatedly, Happy Birthday! Hope to see you in Phoenix, where we can resume our discussion of angiosperm evolution. Give my best to Betty.  
With all our love,

JOHNSON SPACE CENTER, HOUSTON, TEXAS SC-3

ASTROCARD



Professor Alfred Traverse  
Palynological Laboratories  
435 Dicke Building  
The Pennsylvania  
State University  
University Park, PA

16802

1 September, 1978

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506 (North 1207)  
Houston, TX 77036

Dear Bruce:

I just read your paper with Olsen and others in the current number of Science. It is certainly a fine job and reflects well on you. It does remind me, however, to ask whether you intend to get your thesis published in the fairly near future--some sort of a monograph--that certainly should be accomplished.

By the way, I'm sorry I had to cut you off on the phone the other day. It just was not personally convenient at that moment to go on. I was enjoying the conversation. Best wishes.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

9 August, 1978

Dr. Bruce Cornet  
Gulf Research & Development Co.  
P. O. Box 36506  
Houston, TX 77036

Dear Bruce:

Your letter of 11 July arrived while I was on a two-week vacation with Betty. When I got back on 21 July, I left immediately for University of Toronto to work with Geoff Norris on ICP business for a few days. Now I'm back at the old desk trying vainly to clear it before getting down to some practical work. (A bad dose of poison ivy on my arms from some work in my woods last Saturday is not helping much.)

As you know, I have been a Cornet-watcher for quite a few years, and I think I understand you pretty well. I have found you to be very stimulating, incredibly productive, very headstrong and very capable. I am not at all surprised that Gulf is making good use of your abilities. I only pray that Gulf has less red tape and bureaucracy than Shell because you would have had a fight with some minor "book"-quoting official at Shell, I'm sure. I can't imagine a man more likely to produce good results on an important problem than you--nor one more likely to blow it all by telling some critical person to go to hell. (Maybe we're a lot alike!)

Now, as to your materials on ?angiospermoid-pollen?--I have read the text and looked at the pictures, and I feel that you have made a really interesting contribution--and, after all, the conventional approaches have done very little good! Why not be unconventional under the circumstances? I am therefore prejudiced in your favor.

However, you asked for opinions, so I'll tell you that it seems axiomatic that if you're presenting a very controversial idea, you should present your facts conservatively. It is unfortunate that you don't, because it gives your opponents an unfortunately easy target to shoot your idea down because of presentation of your facts. At the moment, I don't have time to go into great detail, but as examples:

1. It is startling to say that Spathiphyllum produces Equisetosporites pollen as you do in a couple of places on p. 3 of your letter to Kickey. Surely you don't mean that literally.

Cornet, pg. 2

2. You say (loc. cit.) that tricolpate pollen may be (reduced) polyplicate--but surely the orientation of colpi is wrong for such a simple idea?

3. Fig. 2--you say all figured specimens are from Vinita beds, but two are admittedly "hypothetical".

4. Figures A1-A2 and associated text are practically all theoretical and therefore prejudice your pollen picture, which is based on facts.

5. You also seem determined to get Hickey's ire up (... "you didn't have the benefit of my discoveries....etc."). That's too bad, because the truth is that your emphasis of the Araceae should endear you to him--it is right down his alley! (But if the Araceae were basic, how on earth did vessel-less angiosperms get that way? The wood problem is really a bad one for you and for Hickey's "riparian weed" hypothesis.

Oh well, I've got to get on with it, and I mostly want to thank you for a very stimulating letter and data, which I'll treasure! (Good for my angiosperm lectures in evolution class this fall!)

Whatdya mean, the ICP picture was flattering? Humph. Thanks re S.S. rebuttal. Greetings to Ginny. Re house-purchase: it doesn't help to wait if you have the down payment. Inflation seems now to be world policy.

All the best.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

# Gulf Research & Development Co.

HOUSTON TECHNICAL SERVICES CENTER

P. O. Box 36506  
Houston, TX 77036

25 May 1978

Dr. S.A.J. Pocock  
Imperial Oil Limited  
Exploration Service & Research  
339 - 50 Avenue South East  
Calgary, Alberta, Canada T2G 2B3

Dear Stanley,

Thank you for your letter of 4 May, which provides some helpful suggestions and critical observations.

I, too, have extensively worked with the Corollina group, as I am sure you are aware. In addition to pollen, I have studied megafossils of ovuliferous cone scales from the Liassic portion of the Newark Supergroup. Some scale-seed complexes approach angiospermy almost to the degree that Corollina exine structure does (see enclosures). One specimen of an ovuliferous scale (Fig. III:5E-F) is carbonized, i.e. it is beautifully preserved in three dimensions (retrieved from sieving a loose shale). Aside from its black color, I can see cuticular detail and stomata on its surface. A mature(?) seed developed within an enclosed pouch, access to which was limited to a stigma-like opening on the adaxial side of the scale. The maturing seed grew to the point of exceeding the size of its pouch, rupturing its walls, and fragmenting part of the scale. No part of the seed can be seen protruding through the stigma-like opening (that doesn't prove that the ovule didn't protrude). Perhaps pollen landing on the stigma-like flap germinated there, sending its pollen tube through the opening to the ovule. If pollen were not drawn into a micropylar chamber, but germinated outside the ovary, either on scale tissue or on integumentary extensions of the ovule, the resemblance to certain angiosperm systems of fertilization (e.g. Tamarix) would be indeed close. Coincidentally, the vegetative shoots of Tamarix resemble those of many Cheirolepidaceae (including Frenelopsis).

The wide variation in adaptive strategy of cone scales associated with leafy shoots of Brachyphyllum and Pagiophyllum in the Newark suggest considerable experimentation with seed dispersal and fertilization for Newark Corollina producers. Because of extreme conservatism of vegetative parts, wood, and pollen, niche distinction must have been a problem. Gametophytic or sporophytic incompatibility of pollen landing on the wrong cone would have decreased hybridization of closely related species, thereby promoting evolutionary trends which would eventually reduce competition and maintain niche distinction.

The Cheirolepidaceae represent a highly successful group of gymnosperms, which may owe their success to adaptability to edaphic conditions characteristic



A SUBSIDIARY OF GULF OIL CORPORATION

Hunt Institute for Botanical Documentation

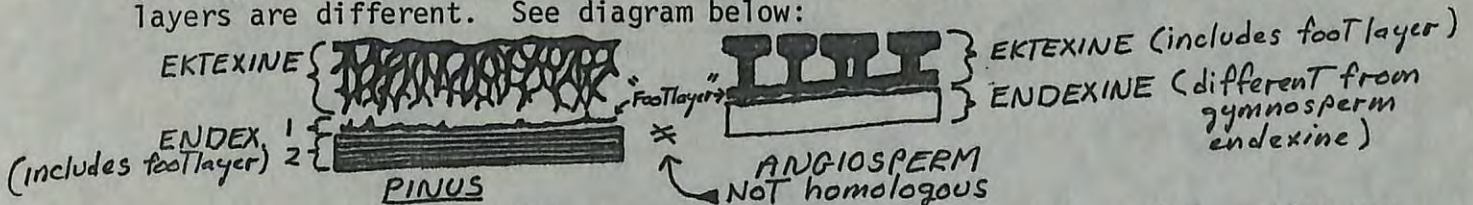
of the Jurassic. The abundance of wood in the Liassic portion of the Newark, including silicified trunks up to 3 feet in diameter (Southbury Basin, CT), indicates that at least some (and perhaps most) were arborescent.

The scarcity of angiosperm-like pollen in the Jurassic may be due to a combination of factors: 1) herbaceous habit of most pre-Cretaceous angiosperms (excluding the palm type of habit), which was determined by their monocot-like(?) stem anatomy (angiosperm wood, and therefore the continuous cambium, does not appear in the fossil record until the Albian); 2) restriction to specialized habitats by their growth habit, thereby precluding competition with arborescent Cheirolepidaceae; and 3) the scarcity of habitats in the Jurassic which would support populations of angiosperms large enough to leave a recognizable fossil record.

Finally, the restriction of early angiosperms to areas with equable climates (i.e. the tropics) is a recurrent theme in the literature. Perhaps most areas of the world during the Jurassic were too cool, or when hot they were too dry for primitive angiosperms. If angiosperms existed since the early Late Triassic (you can guess my inclination on that subject), they must either have been very rare elements of dominant floras and/or geographically restricted to certain niches and climates. Finding megafossil evidence for Jurassic angiosperms may require considerable effort and expense. The Upper Triassic holds more promise for success, but will not provide an answer to those critics who ask, what happened to the angiosperms in the Jurassic?

Concerning your second topic for discussion on the matter of sexine and nexine, do you realize that the footlayer of angiosperm pollen is chemically very similar to that of the sexine? That means that the angiosperm footlayer cannot be homologized with the footlayer of *Pinus* pollen, which has a separate origin from the sexine. I talked with Walker at length on this subject, and he feels that the footlayer, columellae, and tectum are subdivisions of the same layer. The sexinal derivation of the footlayer, columellae, and tectum for angiosperms (exclusively) are part and parcel of his theory of angiosperm exine evolution. In no way can you compare the angiosperm footlayer with a chemically and ontogenetically different gymnosperm footlayer.

I do not have to prove that the observations of Willemsse (1971), for example, are incorrect just because he called the outer distinct layer of the *Pinus* nexine the footlayer. That terminology is unfortunate and misleading. The problem is one of semantics. Stratigraphic position does not always justify correlation, particularly when the chemistry of two comparably positioned layers are different. See diagram below:



I look forward to future correspondence with you and any information you wish to share on *Corollina*. Your last statement about tempering my conclusions is good, and I have taken your advice to heart.

Sincerely yours and best wishes,

*Bruce Cornet*

Bruce Cornet

Figure III:5.

- A. Possible fragment of ovuliferous scale, #ER018, East Round Top loc. (ERT), Feltville Fm., Washington Valley Member, N.J.
- B. Bract? to seed cone possibly bearing seeds like Fig. 5, C, #ER014, East Round Top loc. (ERT), Feltville Fm., Washington Valley Member, N.J.
- C. "Schizocarp" consisting of two winged seeds possibly embedded in ovuliferous scale, which fits well into bract? in Fig. 5, B, #ER085, East Round Top loc. (ERT), Feltville Fm., Washington Valley Member, N.J.
- D. Ovuliferous scale-bract-seed complex consisting of a circular thin ovuliferous scale, fused in part with a larger bract, which projects above scale; two seeds occupy a central position, supported by a pair of funiculi; each seed appears to possess its own micropylar projection adjacent to attachment of funiculi, vascular trace in black; distally seeds appear as a single ridged seed, hemispherical in shape, #ER012, ER013, East Round Top loc. (ERT), Feltville Fm., Washington Valley Member, N.J.
- E. Ovuliferous scale-seed complex consisting of a subtriangular ovuliferous scale with basal abaxial attachment scar (to bract) and adaxial flap to internal pouch containing one mature seed; a single ovoid seed has matured to the point of rupturing the scale; fragments of scale cuticle cling to seed, cuticle on scale papillate to verrucate (proximally), cuticle missing on parts of scale; seed glossy and finely grooved; seed-scale complex beautifully preserved in three dimensions but black; this illustration abaxial side, Midland Loc. 6 in Licking Run, between basalt flows in Liassic strata of Culpeper Basin. Also found at East Round Top loc. (ERT), Feltville Fm., N.J.
- F. Adaxial side of same seed-scale complex showing flap to pouch.

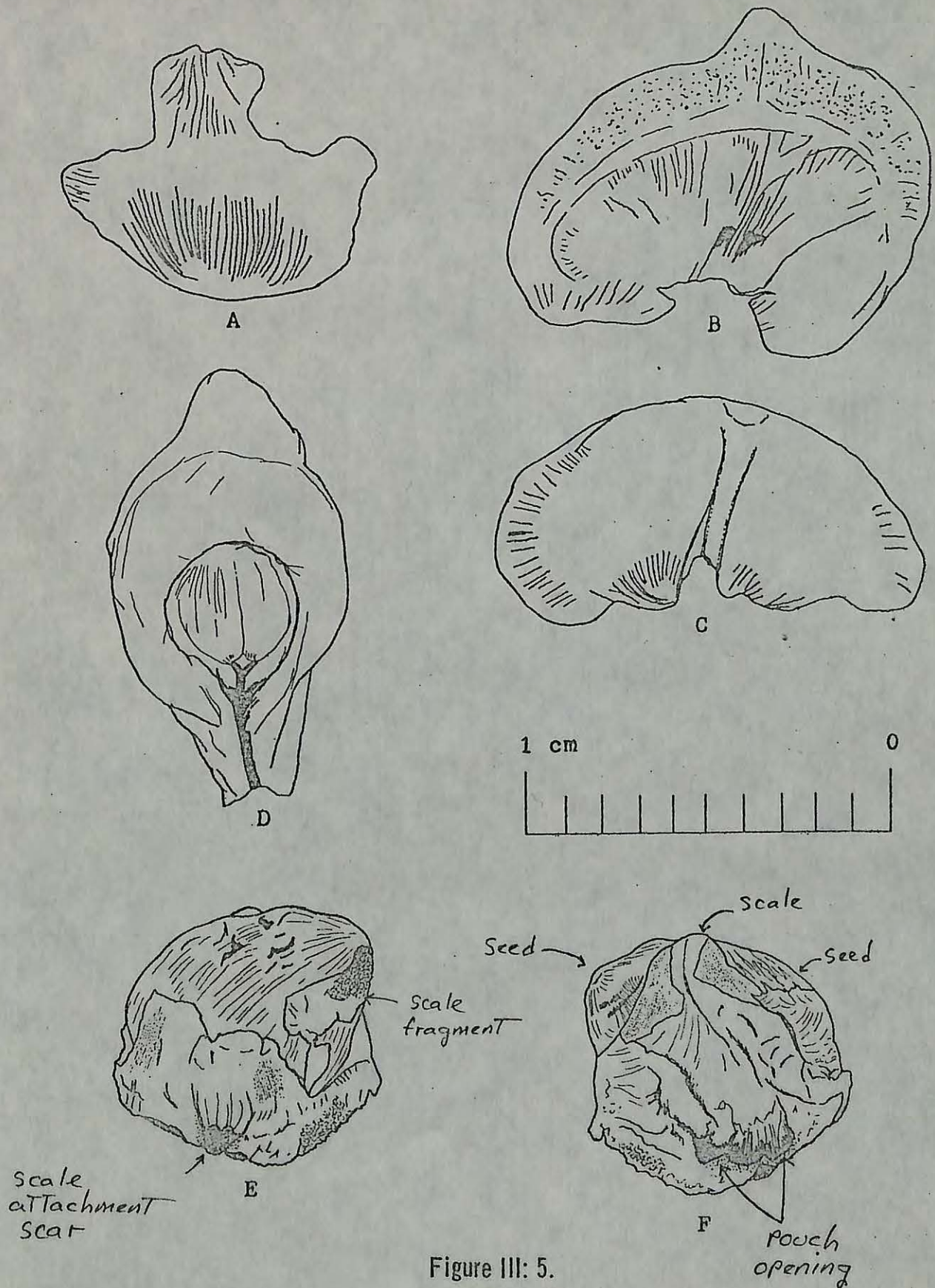


Figure III: 5.

Figure III:6.

- A. *Masculostrobus* sp. 1, globular type, papillate cuticle, #ER003, East Round Top loc. (ERT), Feltville Fm., Washington Valley Member, N.J.
- B. *Masculostrobus* sp. 2, elliptical type, faintly papillate cuticle, thick woody scales with distinct shoulders, #ER006, East Round Top loc. (ERT), Feltville Fm., Washington Valley Member, N.J.
- C. *Masculostrobus* sp. 3, globular type, papillate cuticle with rounded to baculate papillae; this cone contains *Corollina meyeriana* pollen, well preserved cone in three dimensions, Midland loc. 6 in Licking Run, (MID-6), Culpeper Basin, Va.
- D. *Masculostrobus* sp. 4, globular type, smooth? cuticle, similar in size to cone attached to *Pagiophyllum* sp. 6 in Fig. 6, E, #ER011, East Round Top loc. (ERT), Feltville Fm., N.J.
- E. *Masculostrobus* sp. 4, globular type, smooth? cuticle, attached to leafy shoot of *Pagiophyllum* sp. 6 with papillate cuticle, fine grooves faintly present on some leaves and cone scales; preserved more in three dimensions than Fig. 6, D, #ER008, ER009, East Round Top loc. (ERT), Feltville Fm., Washington Valley Member, N.J.
- F. *Masculostrobus* sp. 2, elliptical type, faintly papillate distally, scales sharply pointed, preserved as impression, #ER005, East Round Top loc. (ERT), Feltville Fm., Washington Valley Member, N.J.

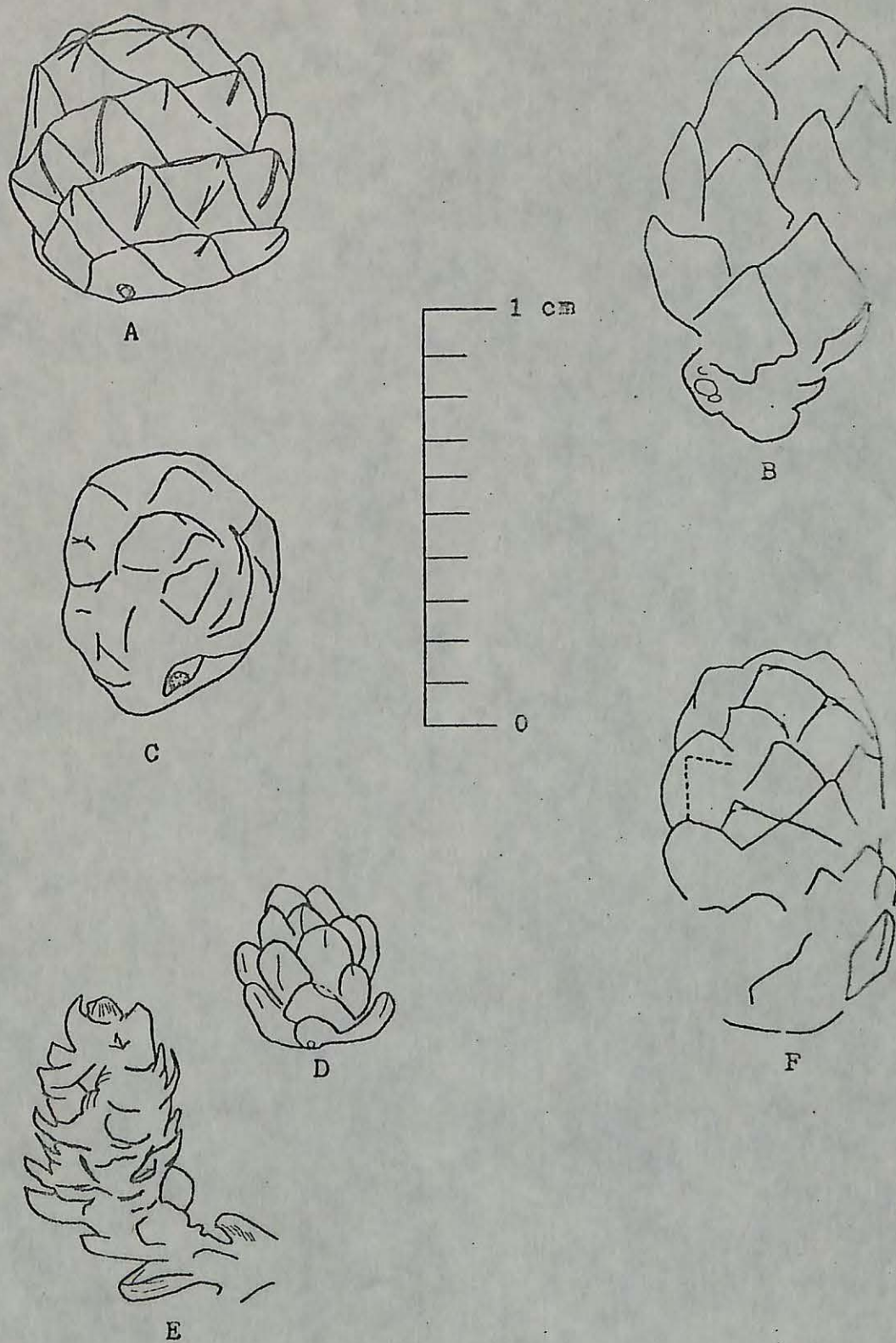


Figure III: 6.

Figure III:7.

- A. Ovuliferous scale-seed? complex with cone scale branch and cone axis; ovuliferous scale bilaterally symmetrical with two circular areas possibly indicating positions of seeds, proximal surface of one side covered with large spines and tubercles (shown in part); edge of scale incomplete, possibly related to Fig. 7, B, Roseland Quarry, unit 10, Towaco Fm. first gray cycle from top of formation, N.J.
- B. Ovuliferous scale-seed? complex showing nearly complete margin or wing; similar to Fig. 7, A, but proximal spines smaller, East Berlin Fm., Interstate 91 road-cut at interchange to Ct. Rt. 9 to Middletown, Ct., top gray cycle in formation.
- C. Bract? possibly belonging to ovuliferous scale in Fig. 7, A, papillate distally, A1013m, Roseland Quarry, Towaco Fm., top gray cycle in formation, N.J.
- D. Bract? similar to Fig. 7, C, papillate distally, East Berlin Fm., Interstate 91 road-cut at interchange to Ct. Rt. 9, top gray cycle in formation.
- E. Ovuliferous scale, reconstructed in part, one lobe isolated to the left, papillate distally, low papillae and tubercles proximally on pointed lobes of scale, #RS002, A753m, Roseland Quarry, Towaco Fm., top gray cycle in formation, N.J.
- F. Bract?, Roseland Quarry, Towaco Fm., top gray cycle in formation, N.J.

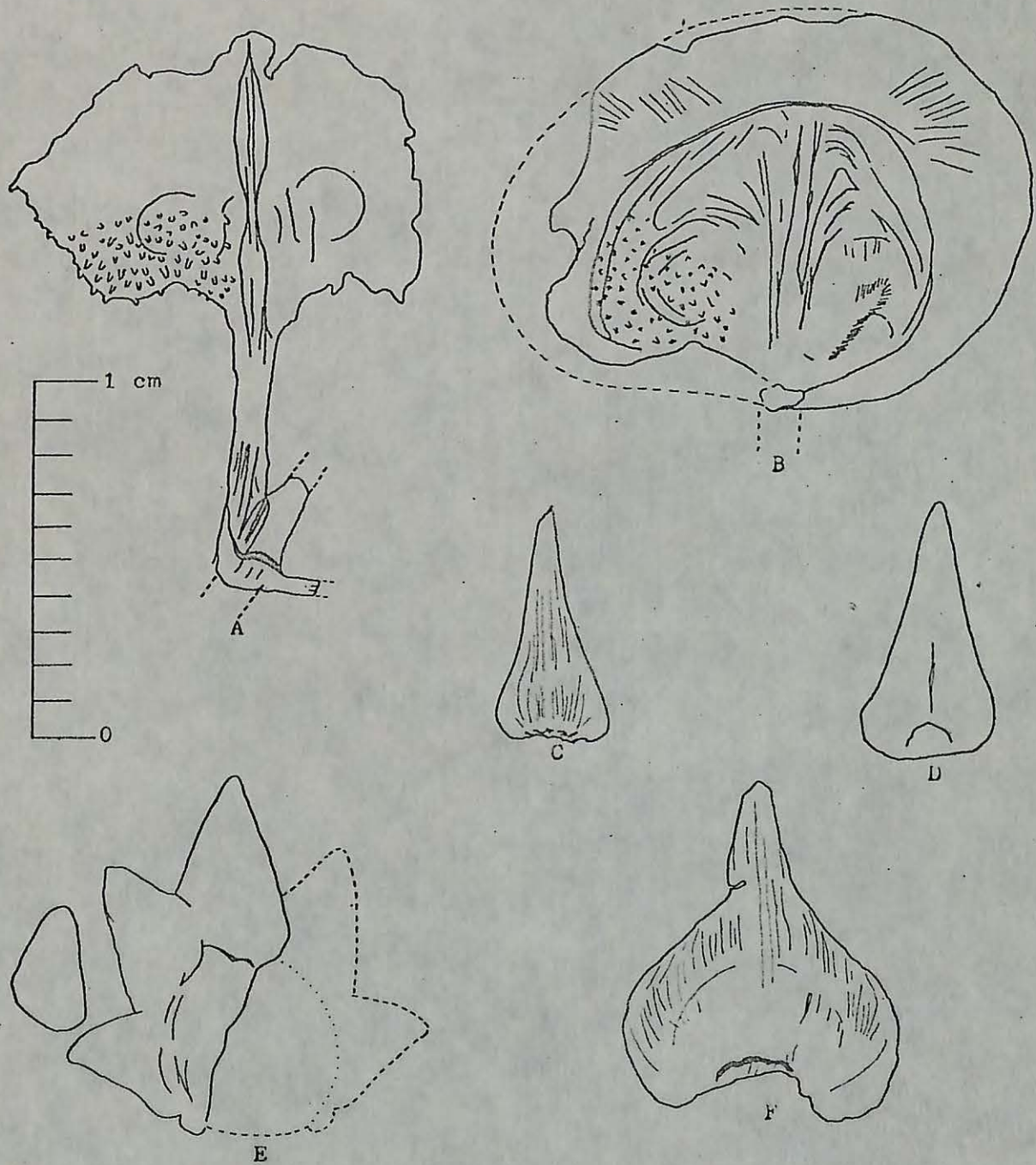


Figure III: 7.

Figure III:8.

- A. Ovuliferous scale, smooth cuticle except for proximal low tubercles on one side, the other side possessing two elliptical vascular scars of attachment to bract; distal lobes subdued, with one lobe on abaxial side (with vascular scars indicated by dotted line; a cavity or pouch in central part of scale presumably held one or two seeds, Holyoke Shopping Center loc. 7 (HOL-7), lowest Portland Fm., Ma.
- B. Ovuliferous scale, smooth cuticle, central part of scale wrinkled, distal lobes possibly three to four sided, Holyoke Shopping Center loc. 7 (HOL-7), lowest Portland Fm., Ma.
- C. Ovuliferous scale, smooth cuticle, referrible to *Hirmerella muensteri*, #HR013, Holyoke Power Co. Dam Loc. on Connecticut River, lower Portland Fm., Ma.
- D. Ovuliferous scale, smooth cuticle, three large distal lobes, two smaller intermediate lobes, #YPM012, Horse Race on Conn. River, upper Turners Falls Sandstone, Deerfield Basin, Ma.
- E. Ovuliferous scale; smooth cuticle, #AG001, AG002, Agawam Bridge loc. on Westfield River, middle Portland Fm., Ma.

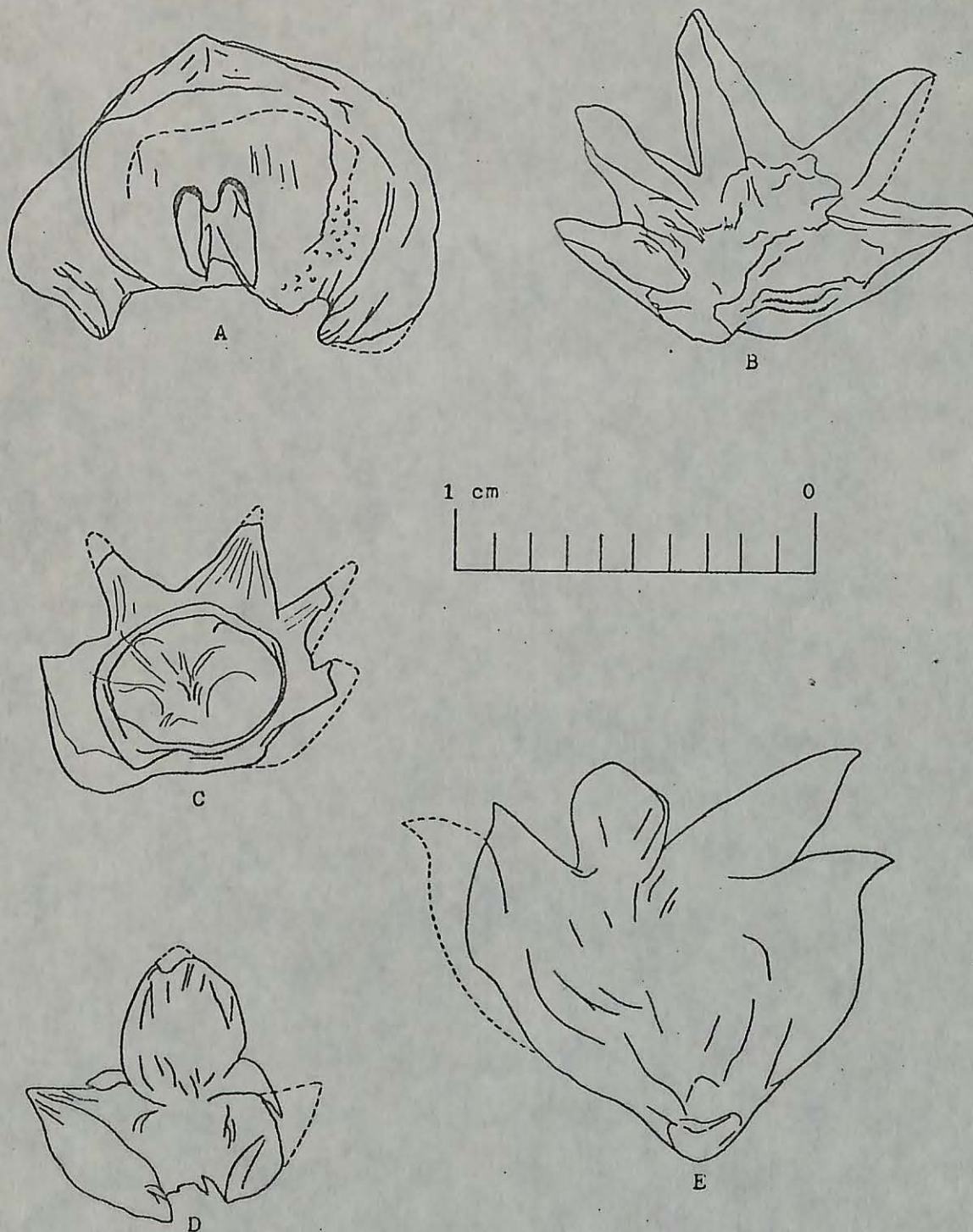


Figure III: 8.

RECOGNITION OF PRE-CRETACEOUS ANGIOSPERM POLLEN AND ITS  
RELATIONSHIP TO FOSSIL POLYPLICATE POLLEN

BY

Bruce Cornet

Diversified angiospermoid pollen has been recovered from tropical Late Triassic strata of the Newark Supergroup, eastern North America and from the tropical Chinle Formation of Arizona and New Mexico. Although monosulcates are dominant, pollen with more than one aperture is more common and diversified than most theories of angiosperm pollen evolution would allow. Aperture diversity includes disulcates, trisulcates, zonosulcates, tetrasulcates, pentasulcates, spiraperturate tricolpates, and spiraperturate polycolpates. All morphotypes have a reticulate-columellate exine and can be compared to polypligate pollen through intermediate morphotypes in the same assemblages.

Two complexes of angiospermoid pollen are present in the middle Carnian of Virginia and New Mexico. A monocotyledonoid complex appears to be derived from a Steevesipollenites prototype, while a dicotyledonoid complex is apparently derived from an Equisetosporites prototype.

The monocotyledonoid complex began with the loss of furrows on the proximal side of an auriculate polypligate. As furrows disappeared, the intervening ridges broke up into irregular rows of verrucae and gemmae. Germination was restricted to the distal side as a comprehensive gemmate-clavate sculpture evolved through continued ridge modification. Apical auriculae were reduced in size and shape to gemmae or clavae, and the five remaining furrows became ektoapertures. Presumably, continuous apertural endexine extended beneath the five ektoapertures as a holdover from the primitive polypligate condition.

A pentasulcate demonstrates how the heads of prominent clavae became joined by a crude reticulum, which is perfected in more derived morphotypes. Continued loss of ektoapertures resulted in different combinations of sulci ranging from trisulcate (two equatorial and one distal sulcus) to compound monosulcate (two adjacent sulci separated by a reticulate ridge or operculum). A Recent example is Calectasia cyanea pollen (Liliaceae), which may be very primitive in having four closely-spaced distal ektoapertures separated by three operculi.

The dicotyledonoid complex began with the evolution of solid ridges supported above a thin ekstexinous footlayer by short peg-like columellae (e.g. Equisetosporites chinleana). Deep foveoli broke the ridges up into bands of reticulate-columellate exine. A Virginia example has numerous reticulate-columellate ridges that are rotated 90 degrees on opposite sides of the grain. This morphotype is duplicated by the modern pollen of Sanchezia nobilis (Acanthaceae), which also possesses paired apical auriculae (a primitive polypligate characteristic). A spiraperturate tricolpate, also from Virginia, possesses a coarsely reticulate-columellate exine and demonstrates that the rotation of ridges and apertures (or furrows) is a primitive characteristic shared by monocotyledons (e.g. Holochlamys in the Araceae) and dicotyledons (e.g. Sanchezia and Thunbergia in the Acanthaceae; Saritaea in the Bignoniaceae).

Rare monocot and dicot pollen types were recovered from an Early Oxfordian surface sample of Englaht. One pollen type is indistinguishable from that of Sanchezia spp.; the other is identical to that of Lilium bulbiferum. Both discoveries are consistent with Late Triassic angiospermoid pollen evolution and increase the possibility that dicots and monocots diverged from a common ancestry in the Early Mesozoic.

1 March, 1978

Dr. Bruce Cornet  
Gulf Oil Tech. Services Center  
P.O. Box 36506  
Houston, TX 77036

Dear Bruce:

Good to talk to you on the phone the other day, and, of course, I very much appreciate your congratulatory message!

As I told you over the phone, I have been intending to write you about the short course dinoflagellate materials which I have assembled in a notebook and have made good use of in Bio.-Geo. 423. I really did appreciate that nice gesture on your part, and I am sure it will continue to contribute to the success of the palynological program here.

Your suggestion that we consider the cost to you of the slide duplication plus other costs of the project as a write-off against the binding and shipping costs for your thesis is very agreeable to us. Best wishes to Ginny, and thanks again for calling and for the dinoflagellate assistance!

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et

9 June, 1977

Mr. Bruce Cornet  
Gulf Research & Development Co.  
P.O. Box 36506  
Houston, Texas 77036

Dear Bruce:

I suppose that the two clear-winged sphinx moths (Hemaris sp.) came from you. I gave them to the Frost Entomological Museum with a note that they came from the garden at 252 Homan. If that is incorrect, I suppose for future scientific accuracy, you should let me know. The strange millipede, by the way, is a polydesmid, probably a species of Leptodesmis.

Now down to some more important things. I will have the typing corrections done as soon as I get from you the title page, the acknowledgments, and a "vita"-- I am afraid I slipped up in not telling you about the requirement that that be provided for a doctoral dissertation. It simply should state when you were born, where, major facts about your education, etc. I enclose the similar item from Bebout's thesis. The typing is probably going to be done by Debbie Myers, but they don't want to begin until they have everything complete. I guess that means we also need the bibliography as well.

Hope that you had a pleasant trip South and that all is well with you and Ginny in your new endeavor. You will certainly be missed here! Best wishes.

Yours very truly,

Alfred Traverse  
Professor of Palynology

AT/et  
encl. Bebout vita

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Al, perhaps you will  
rewrite this letter and send it  
yourself. Bruce

Dear Dr. Jansonius,

Thank you for your letter ("in haste") of 28 July. Al and I chose to distinguish Circulina from Corollina based on Malyachkina's original descriptions and illustrations. Our choice was ~~confirmed~~ supported by photographs provided by ~~the~~ Samoilovich (plate 4, fig 9; plate 5, fig 10). Klaus's emendation of the genus Circulina was ~~rejected~~ <sup>because</sup> C. meyeriana naturally belongs in Corollina according to our discussion in the paper. We further feel that C. funifera and C. simplex do not belong in the same genus. C. funifera seems to be a Corollina, while C. simplex is quite distinct, and probably represents isolated endaxial bodies of Corollina. As a form genus, we chose to maintain Circulina as spherical bodies ~~of~~ of uncertain affinity, in agreement with Malyachkina's description and illustration of Circulina simplex. We do agree with you that <sup>the species</sup> C. meyeriana was first proposed by Klaus, but nowhere, I hope, do we make the mistake of giving that species to Malyachkina. Please reread our descriptions and discussions carefully. C. meyeriana remains as Klaus described it, but Circulina is not regarded by us as being identical to Klaus's emendation of that genus.



Corollina

Circulina

C. Torosus

C. compacta

C. meyeriana

C. funifera

C. simplex

# THE PENNSYLVANIA STATE UNIVERSITY

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College of Earth and Mineral Sciences

Department of Geosciences

Palynological Laboratories

Area Code 814

865-6543

865-2342

*file correct  
Cornet*

October 17, 1975

Dr. John F. Sutter  
Department of Geology and Minerology  
The Ohio State University  
Columbus, Ohio 43210

Dear Dr. Sutter:

Here is hoping that this letter and reprints will reach you before you leave for Salt Lake City. Your isotopic dates support my palynological data and Paul Olsen's vertebrate data that the oldest basalts in the Newark Supergroup are probably Jurassic (at 195 my), as also indicated by previous isotopic and paleomagnetic data. The youngest strata in the Portland Fm. (more than 11,000 stratigraphic feet above the Hampden basalt; new computations) are probably Middle Jurassic or younger, since the middle Portland Fm. (4-5,000 stratigraphic feet above the Hampden) is Pliensbachian or younger, and the Prosauropods and Protosuchians in the upper Portland indicate correlation with the Navajo Sandstone of probable late Liassic-Middle Jurassic age. Thus, we can be reasonably certain that there are Portland strata at least as young as 170 my (Liassic-Middle Jurassic boundary). Deformation of the Hartford Group probably started before deposition had ended, since the upper Portland Fm. appears to be restricted to a smaller syncline with its own structural relationships in the east-central part of the Hartford Basin. Thus, your dikes could have been emplaced after the southern part of the Hartford Basin was deformed (in an area where the Gaillard Graben and Pomperaug "Graben" could be independantly rotated and "rippled"), but before the central part of the basin had been deformed. Also, I must stress that the Newark Supergroup basins should not be generally called grabens, because strong evidence is accumulating that few if any of the basins have border faults, except of course internal grabens such as the Gaillard, etc., from which all good evidence for border faults in the Hartford Basin has come (See Faill, 1972; MacFadyen et al, abstract in GSA 1975 Program). Also note in my recent paper with Traverse that the central part of the Hartford Basin received less sediments during the period of extrusives, but supports the greatest thickness of Portland.

I hope your talk is received well. I will be giving a talk at the "Triassic" convention in Middletown, CT at Wesleyan Univ. on Nov. 8-9th, which will be an eye opener, to say the least!

Sincerely yours,

W. Bruce Cornet, Jr.

file as  
correct:  
Cornet

## Around The Clock

Historical tales  
in area rocksBy WAYNE A. SMITH  
Recorder Staff

History that precedes American Indian culture by several million years may be difficult to assimilate accurately and imaginatively, yet there linger ample traces of it in rock formations up and down the Connecticut and Deerfield River Valleys.

One of the highlights of the Northeast Utilities Connecticut River tour from the Northfield Mountain tailrace area is brief, but interesting recognition of the strata which makes this waterway one of the most beautiful in the country.

Rocks that have been formed by lava flows, that have been pushed up from the depths of the earth, and brought down by one or more of the ice ages which have spread generations of chill across the northern areas of the continent can all be found.

Adding depth to these facts is a study completed recently by W. Bruce Cornet, Jr., and Alfred Traverse of the department of Geosciences of the Pennsylvania State University at University Park.

Adding still further interest is the fact that Cornet has area relatives, including Mrs. Leo Pennegar of Colrain who has forwarded to us with some of the findings made by these two men through research of a study titled: Palynological Contributions to the Chronology and Stratigraphy of the Hartford Basin in Connecticut and Massachusetts.

Cornet, a distant cousin of Mrs. Pennegar and the son of Mrs. Betty Cornet of West Hartford, springs from Colrain ancestry and is a nephew of the late Raymond Flagg of Gill.

In a recent letter accompanying this study which Mrs. Pennegar is donating to the Henry N. Flynt Memorial Library, Cornet notes:

The rocks around Deerfield, Sunderland and Turners Falls have been referred to in the past as being of Triassic Age, which means that they were thought to be about 215-200 million years old. The same is true of the rocks in the Connecticut Valley of Mass. and Conn. That is why teachers frequently refer to these rocks as Triassic rocks. My work on dating the rocks using fossil spores and pollen grains trapped in the rock (polynology) is now showing that the rocks are of Early Jurassic and Late Triassic Age, not just Late Triassic. The Early Triassic is slightly younger in terms of geologic history, but in terms of man's limited concept of time, the Early Jurassic extends the age of rocks in the Deerfield and Connecticut Valleys by about 15 million years. Thus, the age-range of rocks in Central Mass. and Conn. is now believed

to be 205-185 million years ago. Rocks of slightly older age (215 million years old) can be found in New Jersey, Pennsylvania, Virginia and North Carolina, but not in Connecticut and Massachusetts, according to my work.

The rocks which I am studying occur in pockets or basins in the earth. These basins began to form during a period of earth's history not long before the Atlantic Ocean existed. At that time Africa, Europe, and North America were joined together at the edges of their continental shelves. However, these continents soon began to pull away from each other during the latest Triassic, forming cracks or rifts between them on the continental shelves, cracks which were filled as quickly as they formed by "oceanic" basalt and magma. As Africa finally pulled away from North America, the great stress from pulling caused areas on the East Coast of North America to sink more rapidly than before. Cracks or fissures formed along the edges of basins in Connecticut and Massachusetts, as well as in New Jersey, Pennsylvania and Virginia. Lava or basalt filled many of these fissures and flowed out onto the surface and into the basins, forming extensive "flood" basalts. Volcanoes also formed in central Mass., New Jersey and Virginia. It is at this time in the earliest Jurassic when volcanoes were active that the Atlantic Ocean was beginning to appear as a long narrow "Red Sea". As North America continued to move away from Africa and Europe, the Atlantic Ocean grew in size. North America is still moving away from Europe and Africa at ever so slowly a rate.

Evidence of this continental break-up or rifting can be seen in the lava flows, which occur in the Deerfield and Connecticut Valleys. In Massachusetts the Deerfield basalt flow of earliest Jurassic Age can be seen on Route 2A at Turners Falls. Holyoke Mountain and Mt. Tom rise above the valley due to the resistant nature of the lava flows that project up into these mountains. Lava flows can also be seen along I-91 between Northampton and Holyoke.

When the great stress of pulling had sufficiently decreased (the new ocean floor and mid-Atlantic ridge of fissures and lava flows then absorbed most of the stress), the rocks of the East Coast began to rebound to their original shape. However, basins filled with rock also existed there. The rocks in these basins were then deformed and tilted as the rebound phenomenon continued. That is why the rocks in the Deerfield and Connecticut Valleys are tilted.

*comp. as Cornet*

THE PENNSYLVANIA STATE UNIVERSITY

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UNIVERSITY PARK, PENNSYLVANIA 16802

College of Earth and Mineral Sciences  
Coal Research Section

Area Code 814  
865-6544

April 12, 1975

Mr. Neal K. Resch  
5 Hooper Avenue  
West Orange, New Jersey 07052

Dear Neal,

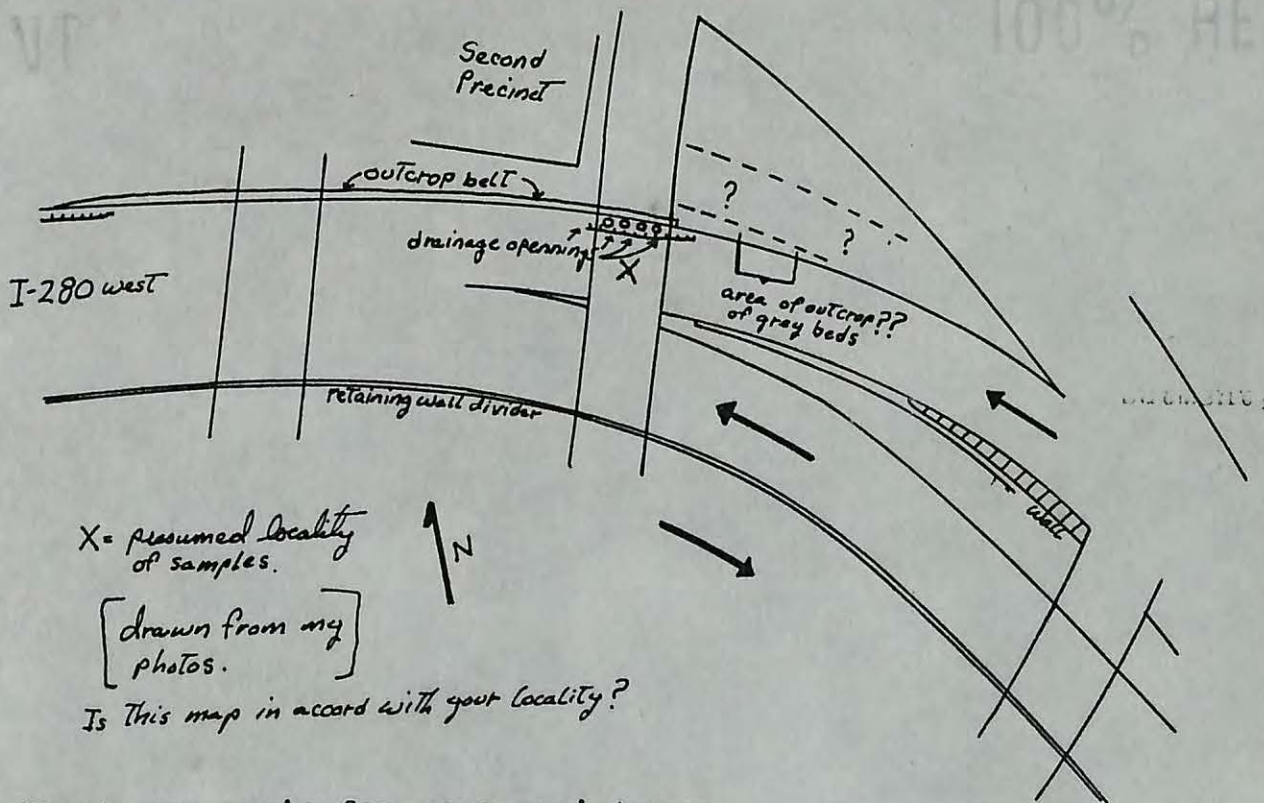
Thank you very much for your letter of 5, April and information about the Second Precinct locality. When we get reprints of our Geoscience & Man manuscript, we will send you one.

Since I wrote you, but before receiving your letter, Paul Olsen and I passed the Rt. I-280 locality and examined the tan-brown sandstone at the base of upward fining red cycles, which we assume represents the top of the sequence you provided in your letter. Based on observed dip we calculated that the grey/black cycle should outcrop just east of the bridge columns, north abutment, as indicated on the accompanying map (area now under wood chips and shrubs). I am very appreciative of knowing that your samples came from a drainage excavation and that pictures were taken. I would be willing to pay for copies of those pictures, preferably as 3½ by 5 inch glossy prints.

You also mentioned that you still have some black samples from the locality. I would be indebted to you for some of that material, particularly any grey or greyish black siltstone samples, especially if they contain small plant fragments. Dr. Baird showed me two specimens from the locality, one of which had several good fossils of Estheriella or Cyzicius, the other good megafossil plant leaves

of Brachyphyllum sp.

As I mentioned in my last letter, the Second Precinct palynoflora records part of a very significant floral change in the Newark Group, which may coincide with or result from climatic change due to active rifting between North America and Africa. Some major tectonic activity occurred at about this time, because the base of strata in the Hartford, Deerfield, and Culpeper Basins stratigraphically correlates with the zone of floral change in the Newark Basin. I am giving a talk at the Graduate Student Geological Colloquium at P.S.U. on 25 April; the abstract to my talk is included with this letter.



Thank you again for your assistance.

Sincerely yours,

*Bruce*

W. Bruce Cornet, Jr.

UNIVERSITY OF ST. ANDREWS

*file Cornet*

*Al,*

*Some interesting info.*



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16 May 1973

Mr. Bruce Cornet  
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Dear Mr. Cornet,

I apologize for so long a delay in responding to your very interesting letter. I have been on sabbatical, and away from St. Andrews recently; I also note our secretary delayed some time before sending your letter over.

My interest in Triassic palynology goes back to 1960-61 when I isolated the first palynomorphs from the East Berlin Formation (sample donated by John E. Sanders, then at Yale University, and my thesis advisor at that time). I concentrated on macerations from the Carboniferous of eastern Canada, however, but made enough noise about the discovery that Grace Somers Brush, then at Princeton (by this time it was 1964) attempted to work the Triassic of the Newark Basin. She got nothing from it at all, and claimed that the rocks had none (glad to see you got some). Upon moving to Amherst College in 1966, I tried to get a student interested in doing the macerations. He made some half-hearted attempts, but was not cut out for the job. I then found out that a M. SC. thesis was done on the Massachusetts Triassic palynomorphs; a thesis done under Wilson whilst he was at UMass before he moved to Oklahoma. I have not got my notes, but her thesis is still at UMass, deposited both in the Botany Dept. and in the Geology Dept. It dates from about 1965 or so. It was her ~~date~~ correlation with the European scheme that I used, and supplied to Don Baird (who if he has kept that old correspondence, should have the name of the girl and the title of the thesis so you can read it when you go to Amherst in June). *Also some work was done on North Carolina Triassic palynomorphs by another chap that she cited -*

Sorry, I cannot help you any further on this. It would tickle me pink if you would mention that I obtained palynomorphs from the East Berlin Formation in December 1960. John Sanders lost those slide preparations...and Grace never did believe me, especially after her disastrous results (she had a large NSF grant and all....).

Best wishes,  
*E. Belt*  
Edward S. Belt

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*file  
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June 25, 1973

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Dear Nick,

It was great to see you again. I really enjoyed our visit. Al was very glad to meet you and Dr. Peoples, and enjoyed learning about the discovery and development of the Dinosaur Park. He was very impressed with the work you are doing at the North Guilford locality.

The main reason I am writing this letter is I recall you expressing an interest in our new plant locality near Holyoke, Mass. discovered by Paul Olsen. After examining the dozens of beautiful fronds of Clathropteris meniscoides, excellent impressions and compressions of Equisetites spp., and possibly another articulate related to Neocalamites I could not help but feel that we had stumbled on perhaps the best plant locality in the Hartford basin. The lithology was perfect for spores and pollen, and I have just been able to process a few samples. The palynoflora is dominated by trilete spores produced by ferns and their allies, with only about 25% of the palynoflora composed of coniferous Circulina and Classopollis grains. Qualitatively the palynoflora compares closely with that from the North Guilford red-bed flora, but quantitatively the palynoflora reflects a local dominance of water-loving articulates and ferns. The lithology and presence of rhizomes in-situ

strongly suggest a local paludal environment perhaps created in an abandoned river channel or around an oxbow lake. The thick sandstone and conglomerate facies above and below the plant bed further suggest a fluvial near-channel environment.

You suggested that you might like to collect fossils at this locality, and saving them for me the next time we meet. I am very much in favor of this since there are still some unanswered problems about the identity of some of the fossils. Relatively long, unbranched stems parallel one another through the plant-bed layers. No leaves have been found or even attached. The stems resemble those of Neocalamites and Lobatannularia, and are perhaps identical to the non-descript branches referred to Loperia carolinensis. It would be very helpful if you were able to find the leaves of this plant, since it might be useful for age determination if it is a Neocalamites.

Also, the relatively uniform composition of the flora may only be due to local concentrations of certain species. With further looking, other ferns and plants may be found. I hope so. If you do decide to collect at this locality, be sure to collect all pieces of a complete individual, and keep the pieces associated. Wrap them in a soft tissue paper or kleenex.

6/26/73

I just completed another sample, this time the lowest gray clayey siltstone with abundant evidence of root penetration and rhizomes. Supposedly this layer of finely laminated siltstone was laid down before the plants took root. Thus, this layer would contain a higher number of Circulina and Classopollis grains, reflecting air-borne and water transported pollen from the upland (extra-basin) coniferous forest. It does. 80% or more of the palynoflora is composed of

Circulina meyeriana, Classopollis simplex, Classopollis sp. 1 and Classopollis sp. 2, just as at North Guilford. Cycadophyte pollen is present, perhaps a new species. This palynoflora is in contrast to the one previously obtained where trilete spores dominated and Cycadophyte pollen was very rare. The previous sample largely came from the Clathropteris layer above the root zone.

The age similarity of this plant bed to the North Guilford locality is interesting and perplexing. The dip of the beds and the aerial distance from the Holyoke lava-flow unit indicate a computed 1,500 feet of intervening sediment. Even if this figure is off by  $\pm$  250 feet, the plant locality is at least 1,000 feet below the Holyoke lava-flow unit, a thickness greater than the maximum for the Shuttle Meadow Formation! This means that the new locality may be correlative with either the Talcott Formation or the New Haven Arkose in the southern part of the basin. If none of the Talcott lava flows reached Massachusetts, but a similar thickness of sediment was laid down as Sanders figures for the Talcott in the Gaillard Graben (1,000 feet), then perhaps this plant locality is correlative with the Talcott Formation. It is interesting to note that a change in dip occurs in the upper part of the New Haven Arkose of the West Springfield Quadrangle, suggesting that perhaps not all of that formation is equivalent to the New Haven Arkose in southern Connecticut. The plant bed locality apparently occurs above the level of change in dip.

Since I do not have any palyniferous localities from good New Haven Arkose, I do not know how far down the Shuttle Meadow-type palynoflora extends before a change occurs. The upper Khaetian-basal Liassic palynoflora of England changes little across the Triassic-Jurassic boundary. But when a change occurs in the middle Khaetian, it is readily distinguishable. I am hoping that the

megafossil plants will shed some light on the relative age. If I can identify Neocalamites hoerensis, Equisetites grosphodon, or Lepidopteris ottonis (a fern-like Pteridosperm), a Lepidopteris-Zone age is indicated (Rhaetian). If, however, Dictyophyllum muensteri, D. nilssoni, Thaumatopteris brauniana, Todites princeps (all ferns), Equisetites sarrani, or Lobatannularia carcinoides (previously called Neocalamites carcinoides) are found, a Thaumatopteris-Zone is indicated (predominantly Liassic).

Already megafossils have helped in confirming an age in the Culpepper basin at Leesburg, Va. The palynoflora was dominated by Circulina meyeriana and Classopollis simplex (very good specimens). However, few other types of miospores were present. Recently I discovered a relatively rich megafossil locality within 90 stratigraphic feet of the previous palyniferous locality. Both localities have a similar palynoflora, but the megafossil locality produced identifiable specimens of Todites princeps, as well as Equisetites (perhaps E. muensteri, Rhaeto-Liassic). Even though T. princeps is not a good index fossil for the Liassic, it is restricted to the Thaumatopteris-Zone and younger rock, never being reported from older rock in North America, Greenland, and Europe. <sup>and Japan</sup> Thus a wide-spread fern for the Jurassic becomes important at the Triassic-Jurassic boundary! This fern is also quite distinct from other Todites and osmundaceous ferns, making it easily identifiable.

If you do decide to collect at the new locality, I suggest that you finish removing the plant bed where we removed the overlying conglomerate. There are few other places where the overburden is as little.

Hope you and Pam have a happy summer.

Sincerely yours,

P.S. your collecting will <sup>be</sup> ~~be~~ <sup>against</sup> ~~be~~ <sup>of</sup> ~~of~~ <sup>the</sup> ~~the~~ <sup>same</sup> ~~same~~ <sup>as</sup> ~~as~~ <sup>the</sup> ~~the~~ <sup>one</sup> ~~one~~ <sup>at</sup> ~~at <sup>the</sup> ~~the <sup>new</sup> ~~new <sup>locality</sup> ~~locality~~~~~~~~