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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.

June 26, 1958

Dr. Isabel C. Cookson  
The Botany School  
University of Melbourne  
Carlton N. 3  
Melbourne, Australia

Dear Dr. Cookson:

Thank you for sending the copies of "Microplankton from Australian and New Guinea Upper Mesozoic Sediments" and "On Some Australian Tertiary Spores and Pollen Grains that Extend the Geological and Geographical Distribution of Living Genera". As I have said before, I always have found your papers most helpful and excellent pieces of work.

Very truly yours,

  
Alfred Traverse

AT:ml

Shell Development Company  
Exploration and Production Research Division

May 12, 1958

Dr. Isabel Cookson  
Botany School  
University of Melbourne  
Carlton N. 3  
Melbourne, Australia

Dear Dr. Cookson:

Your nice letter of April 16 was enjoyed very much indeed. By curious coincidence, it was in the same mail with a reprint from Erdtman of the paper of his to which you referred.

Though I know nothing of the facts about this case, I suppose there is great probability that he is right about this misidentification. But he overstresses the importance of the error. Presuming he is right, you have simply put the item in the wrong genus. It's happened lots of times before in botany, and the International Code provides for the possibility. Somebody should simply publish an emendation. Why didn't Erdtman do it, if he noticed the error. It would have been: Lophosoria annulata (Cookson) Erdtman. Simple as that. And it's been going on for years. That's how progress is made in taxonomy. (This has nothing to do with the general question of whether fossil pollen can be referred to extant genera.)

I will say thi, though: if an item is transferred from Cyatheidites annulata to Cyathea annulata, this must be done formally, using the standard procedures of botanical nomenclature, just as in any other nomenclatural transfer. I don't have a copy of the 1956 paper, "On some Australian Tertiary...etc.", hence cannot comment on how you handled it.

Erdtman, as so many others in this field, does not seem to realize that the purpose of giving a name to an item is simply to provide a means of referring to it (almost exact quotation from the International Code of Botanical Nomenclature). The purpose is not to show phylogenetic relationship. In selecting a name for a fossil item one can only use one's best taxonomic judgment. Hence, the ridiculousness of his position on Amylothea. It is not up to you to show that there are no other pollen grains like Amylothea but only that in your best judgment, these fossil grains are referable to that genus. He should show differently or keep still.

Erdtman is right, however, that one should use pollen preparations, not illustrations, for comparison wherever possible. In the case of many fossil pollen grains, however, the illustrations will have to suffice because the type specimens do not exist, and the illustration is all there is.

Your further visit to Eisenack is interesting. Wish I could be along to hear your discussions of the hystrichosphaerids, for instance. I find many of them in recent sediments in this region and have been breaking my neck trying to find out what they are, since it would seem that the marine biologists would be able to identify them from their extensive experience with the plankton of the same region. So far, no luck. The dinoflagellate cyst theory that several have suggested is interesting, but why won't experts on the dinoflagellates buy the idea?

You should not feel the way you do about your Tertiary work. It has been very valuable indeed; I have found your papers extremely stimulating. Everybody makes mistakes, with the exception of Gunnar Erdtman, of course! Nomenclature and other aspects of paleobotany present difficulties, of course. Erdtman just hasn't done much paleo work, or he'd have a fumble or two in his record as well. It isn't hard to get a pollen grain into the right genus when it's written out for you on the herbarium sheet. I must insist that you have made a great contribution to Tertiary paleobotany!

Do hope you can make it to Montreal, though I understand the difficulties. In any case, I am looking forward very much to meeting you one of these years.

Very truly yours,

  
Alfred Traverse

AT:ml

Shell Development Company  
Exploration and Production Research Division



April 10<sup>th</sup>

Dear Dr. Bravense

Thank you very much for your letter of 2<sup>nd</sup>

for associating yourself with the proposal of my name for Life Membership of the Botanical Society of America. My election came as a great surprise to me and although highly pleased and grateful for the honour I still feel myself completely unworthy of it. It would give me considerable pleasure to meet my American colleagues. I have toyed with the idea of attending the International Botanical Congress next year but think it very unlikely that I shall go to the great expense it would entail especially as next year I shall be going on to a minute "part-time" salary of 1/2 of my present earnings. The exchange rate with Canada and America is completely crippling for an Australian with no source of dollars at the other end.

In September or October I hope to leave for Germany to stay again with Professor and Mrs. Eisenack. I am hoping by that time to have another collection of Jurassic and Cretaceous microplankton - the paper which Prof. Eisenack & I wrote together last year has just been published in Proc. Roy Soc. Vic. but the reprints haven't come to hand yet.

And that brings me to the subject of your letter. I'm afraid I haven't sent out reprints for some time, knowing that the microplankton paper would be available early in 1958. I did send the ones you mention to Prof.

Erdtman with rather unpleasant results as you will see if you refer to Oversikt Botanisk Tidsskrift Bd. 51: 611-613. My first inclination when I received a letter in the same terms as the paper was to destroy the reprints concerned but now I have decided to enclose a note indicating the necessary correction - of course I take no exception to the correction he has made. but I do think the criticisms of my reference of *Phocinus pollen* fossils such as *Amylotheca* and *Alaungin* rather uncalled for. I changed my policy in this respect following information as to American opinion in this respect forwarded by you - but apparently have put my foot well into it!!! after validating my *Nolithofagus* types I think I will keep well away from the Tertiary - there's plenty of interest in the Mesozoic. I hope you will like the short paper on *Plant* Institute for Botanical Documentation Mary Dillman

and myself in *Microspaleontology* (April number) Probably you will see it in print before we will - Personally I think it should look quite well - they are such lovely things

I will post your reprints as soon as the plankton paper reprints arrive - thank you for your interest - I don't get much at home.

With friendly greetings and good wishes  
Very sincerely yours  
Karel Cooleson

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**BY AIR MAIL**

**AEROGRAMME**



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**I.C.C.**  
If not claimed please return to  
University of Melbourne  
Carlton, N.3, Victoria, Australia

Dr Alfred Jansen  
3737 Bellair Boulevard  
Houston 25  
Texas  
J-S.A

April 3, 1958

Dr. Isabel C. Cookson  
Botany School  
University of Melbourne  
Carlton N. 3  
Melbourne, Australia

Dear Dr. Cookson:

Just was looking through the recently published second number of the French bibliography of palynology. I find in it two references to papers of yours of which I would like to have reprints. I have not received them as yet. They are:

On some Australian tertiary spores and pollen grains that extend the geological range and geographical distribution of living genera. Proc. Roy. Soc. Vic. 69, p. 41-53, etc.

(with S. L. Duigan) The occurrence of Azolla filiculoides L. and associated vascular plants in a quaternary deposit in Melbourne, Austr. Ibid., p. 5-13, etc.

I always enjoy your papers and would much appreciate receiving these reprints.

All of us were very pleased that our proposal of your name for election to corresponding membership in the Botanical Society of America was successful. Wish you could attend the annual meetings!

Very truly yours,

Alfred Traverse

AT:ml

Shell Development Company  
Exploration and Production Research Division

University  
CARLTON, N.S.



of Melbourne  
VICTORIA, AUSTRALIA

Botany School,  
14th November, 1956

Dr. A. Traverse,  
Shell Development Company,  
3737 Bellaire Boulevard,  
HOUSTON 25, TEXAS.

Dear Dr. Traverse,

The arrival of your letter this morning has 'heaped coals of fire' on my head for not having answered your previous letter. I was very glad to have your personal opinions and to some extent have acted upon them in a small paper just sent in for publication.

I have had a very hectic year - making a collection of Mesozoic Microplankton. I had arranged to take this to Paris for fossil work with Professor Deflandre but at the last minute learnt that this arrangement was not possible. Now, on 17th November I am leaving for Germany where I hope to have the collaboration of Professor A. Eisenack of Reutlingen. I have a very good collection - at least I think so, and I think some of the types may prove of stratigraphical importance.

Mr. B.E. Balme's address is C/- C.S.I.R.O. Coal Section, P.O. Box 3, Chatswood, New South Wales. I had a few days with him recently - he is doing some excellent work on Permian and Mesozoic spores, and at present is preparing a paper on Jurassic - Lower Cretaceous types - for publication.

With many thanks for your earlier comments, and with best wishes.

Yours sincerely,

*I. C. Cookson*

I. C. COOKSON.

November 7, 1956

Dr. Isabel C. Cookson  
Botany School  
University of Melbourne  
Melbourne, Australia

Dear Dr. Cookson:

Would you be able to supply me with the address of B. E. Balme with whom I would like to exchange reprints and correspond? I would appreciate your help.

Yours very truly,

Alfred Traverse

AT:rlg

Shell Development Company  
Exploration and Production Research Division

February 3, 1956

Dr. Isabel C. Cookson  
Botany School  
University of Melbourne  
Carlton N. 3  
Victoria, Australia

Dear Dr. Cookson:

Your letter of January 24 came today, and I was very glad to hear from you again. Unfortunately, I am working with my files from Denver not yet unpacked, and with my notes and correspondence for the last few months in great need of organization, pending the arrival of my office furniture. As a result, it is not possible for me to write the sort of well-documented letter that I should like to write. Moving a laboratory from place to place is rather wasteful of time and effort.

I have sent you a copy of the mimeographed version of what I said at the palynological symposium in September. The sentence in my letter to which you refer is about some of the things I said in this report. In it I dealt at some length with the validation of your taxa that Ashley Couper published in 1953, (In his monograph on Mesozoic and Cenozoic Pollen and spores of New Zealand.) You will note that he redescribed your sporotypes as genera, using the conventional notation, "Cookson ex Couper" that is used whenever a genus is validated. In his new descriptions, Couper designated your items as the types. In my report last September I suggested that Couper's validation didn't go far enough, since you had not originally designated type specimens. I felt that Couper should have nailed the thing down by designating one of your specimens or, at least a specific one of your illustrations as the type. Couper agrees that it would be desirable to designate type specimens and says if he were writing the paper now, he would do so. Since I wrote that report last summer, however, I have discovered that the International Code of Botanical Nomenclature provides no support for me in the letter of the law. I mean that type specimens of fossil plant organs are still not required. Even the requirements as to illustration, believe it or not, are still not explicit. In short, if validation of your taxonomic units (taxa) were required, Couper's 1953 publication is sufficient. (Though he and I both feel that designation of type specimens is proper procedure, despite not being positively required.) The remaining area of doubt in my mind is whether or not it is essential that a genus be designated a genus, a species as a species, to be valid. Couper validated your taxa because they were designated sporotypes and sporomorphs instead of as genera and species. I shall have to dig in the rules a good bit more on this question. However, in Europe I met several people, who should know, who say that your names are quite valid despite having been described at sporotypes and sporomorphs. I just haven't the answer to this intriguing question.

It does seem obvious to me, though, that the best practice, and one to which nobody could take exception, would be to describe genera as genera, species as species, to publish illustrations, one of which for each species is of a specimen designated the holotype. It may be that less than this will get by, as far as the letter of the law is concerned; but why not provide more than is required instead of what may be the legal minimum?

Although I don't suppose that it would affect the validity of your names, for my own information I should like to know whether it was your intention that the sporotypes be equivalent taxonomically to genera, and sporomorphs to species. It had been my understanding that at least some who used the terms sporotypes and sporomorphs felt that the classifications of pollen and spores is rather outside the pale of normal plant nomenclature and that, therefore, the names have no standing as botanical genera and species.

The practical importance of the standing of your taxa, is that we shall all have to know what name to apply to the units in question when we mention them. Are they genera Cookson (despite having been described as sporotypes) or genera Cookson ex Couper (following the validation of 1953)? This may seem a trivial matter but really isn't if we are to be precise. With the huge numbers of entities that are likely to be described, precision is of some value.

Your statement for the introduction to your new paper seems admirable to me, except for two things:

1. Instead of form genera and form species, you should say, "form and organ genera and species." The distinction is that an organ genus or species refers to an organ about which some knowledge of botanical relationship exists. Banksiaoidites, for example, would be an organ genus, not a form genus.

2. I am not sure that I understand what you mean by "maintaining the validity of the earlier types". If you mean the validity of the items themselves, of course you are right. Your work is very neat and well done. As to the validity of the names as generic and specific names, there is some division of opinion that perhaps you should note. (Incidentally, I wonder if the best solution might not be for you to publish a "Revised List of Fossil Pollen and Spores", or something of the sort, giving generic and specific names, type specimens, and citation of original publication and illustration.)

May I urge that you write Ashley Couper (Sedgwick Museum, Cambridge University, Cambridge, England) about the Trilites-Trilatas question? I remember that he is conversant with the problem, and I am hesitant to tackle it without my library.

Best wishes,

Sincerely yours,

*AT*  
Alfred Traverse

AT:bjs

Shell Development Company  
Exploration and Production Research Division

University  
CARLTON, N.3.



of Melbourne  
VICTORIA, AUSTRALIA

Botany School,  
24th January, 1956.

Dr. A. Traverse,  
Shell Development Company,  
Exploration and Production Research,  
3737 Bellaire Boulevard,  
HOUSTON 25, Texas, U.S.A.

Dear Dr. Traverse,

I have now to thank you for two letters, both of which I have been very glad to receive. I'm afraid I do not quite understand the portion of the second letter which reads, "I now realize that the International Code of Botanical Nomenclature does not (unfortunately) require typification by specimens. This has an important bearing on some of the things I said in my paper relative to your work and Couper's treatment of your taxa". Could I trouble you to amplify the latter part?

I am preparing a short spore paper for early publication, and should be glad if you would be so kind as to give me your opinion of the following portion of the introduction, which has been the direct result of your first letter.

"Whilst the system of nomenclature used in the present paper is essentially the same as in previous ones, it has become apparent, from contact with overseas palynologists, that there is a general feeling that spores and pollen grains should be treated, like other plant organs, as form genera and species rather than as sporotypes and sporomorphs (Erdtman 1947). Following a letter from Dr. Alfred Traverse giving the substance of recent American discussions on this subject, I have decided to conform to the opinion of the majority and to abandon the use of the terms sporotype and sporomorph, at the same time, however, maintaining the validity of the earlier types."

Furthermore, I believe that the use of Trilites Erdtman for trilete microspores is not acceptable. Is this so? In my Kerguelen paper I followed Erdtman's suggestion of Triletes for megaspores and Trilites for microspores. The new paper may be a good place to alter this, if you, with your greater taxonomic knowledge, think it fit. Mr. Balme of Sydney, if I remember correctly, uses Triletes for both micro- and megaspores, but Couper has followed my lead. It would be much easier to continue as before, and Couper, for one, might prefer this. What do you think?

I'm sorry to trouble you in this way, but you mix more with palynologists than I do.

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BY AIR MAIL

AEROGRAMME



Dr. A. Traverse,

Shell Development Company,

Exploration and Production Research,

3737 Bellaire Boulevard,

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Cookson/Botany

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University of Melbourne  
Carlton, N.3, Victoria, Australia

If anything is enclosed, letter will be sent by ordinary mail.

First fold here

I am glad you have found interest in the microp plankton paper - it was rather rushed I fear. I have another small one in course of publication - there are no nomenclatural difficulties in this field.

With many thanks and best wishes,

Very sincerely yours,

*Robert A. Cookson*

Second fold here

January 18, 1956

Dr. Isabel C. Cookson  
Botany School  
University of Melbourne, Carlton N. 3  
Melbourne, Australia

Dear Dr. Cookson:

Having now returned to the U. S. I can follow through on my promise to send you, under separate cover, a mimeographed copy of what I said at East Lansing last September. As a result of criticisms at the meeting, and also from subsequent discussions, I now realize that the International Code of Botanical Nomenclature does not (unfortunately) require typification by specimen. This has an important bearing on some of the things I said in my paper relative to your work and Couper's treatment of your taxa.

I am also sending you a copy of my Brandon lignite monograph and a reprint of my Betryococcus paper. The latter could not have been written without your help.

Many thanks for the three reprints that came during my absence from Houston. I am especially interested in the paper on fossil microplankton.

With very best wishes.

Yours very truly,

AT

Alfred Traverse

AT:hmp

Shell Development Company  
Exploration and Production Research



December 8<sup>th</sup> 1955

Dear Dr Travers

I have just read in the Microfossilologist of the meeting proposed by the Palaeobotanical Section of the American Botanical Society and the fact that you were to talk about our work in Australia & N. Zealand.

I have long been worried about the nomenclature adopted by us for fossil spores and pollen grains. When I commenced this work in 1946 I was a lone wolf in the Southern hemisphere - with no experience and no one to advise. I turned to Dr Erdtman & in the Terquem paper tried out the proposed nomenclature - I have more or less kept to this ever since & Couper has followed it for the N. I. types. I would greatly value your criticism and am writing to ask you to be so good as to tell me what happened at the meeting.

I was greatly disappointed that no lead was given at the Paris Congress. The postponement of the question of <sup>uniform</sup> nomenclature until 'Monheal' may lead to further confusion. I am appalled by Pflieg's paper. Those of other German workers and feel that very soon now I will leave Palynology for true palaeobotany. We have beautiful leaf material from brown coal - which if septemnerally attached should yield some interesting results.

I do hope you do not mind my troubling you in this way. I often wish that your country was more accessible to me. I still feel very isolated & lonely. especially as Miss Kathleen Rice has left here to marry an oil geologist in Western Australia. With the seasons greeting & good wishes  
Sincerely yours  
Kathleen Cooper

Dr. ISABEL C. COOKSON,  
Botany School,  
University of Melbourne,  
Carlton N. 3,  
MELBOURNE  
Australia

The Hague, December 29th, 1955

Dear Dr. Cookson:

Thank you for your very nice letter of 8 December, which was forwarded from Denver to my new headquarters in Houston, Texas, where I shall be working for Shell Development Company, a subsidiary of Shell Oil. I much enjoyed your letter and will send you a copy of the report I gave at the B.S.A. paleobotanical meetings as soon as I am back in the U.S.A. and get straightened out again.

I came over here in November for a brief look around at the European laboratories and to see Shell's (Bataafsche Petroleum Maatschappij's) palynological setup here. It has been a wonderful experience. When I go back to Texas in about two weeks I shall set up a laboratory for fundamental palynological research. I am at heart a paleobotanist though, as you know, I was employed by the U.S. Bureau of Mines as a coal petrographer.

In brief, the discussions at the meetings in September, to which your letter refers, showed that all of us feel strongly that pollen and spore nomenclature must follow paleontologic and biologic convention, i.e., that the names used must be used in accordance with the International Code of Botanical Nomenclature. This, it seems to me, is a considerable advance in thinking, since only a few years ago when I was a graduate student there was by no means agreement on this point. I have just had a good visit with Potonié and also with R.A. Couper (who is in Cambridge), and I am happy to report that we are in complete agreement on essentials, with only minor differences between us. (For example, Potonié does not feel that I am right in making organ species for genera of recent plants, though he admits that the rules do not specifically enjoin this sort of behavior.) Both Couper and I feel that your nomenclatural treatment is basically quite good, with the exception that you designated things as new sporomorphs instead of as new (organ!) species. There is some doubt as to whether this invalidated your descriptions, but, as you know, Couper removed any remaining doubt as to the taxa which concerned him by simply validating your units. (There is doubt that it was necessary to do so.)

I am thoroughly convinced that the situation is not really so bad and that the International Code provides techniques for correcting such bad things as do exist. I would insist, however, that all of us designate new organ species of pollen (and we must realize that they are organ species) as just that instead of as sporomorphs. New organ genera should be called that, not sporotypes. The case is really not in the slightest different from that of other fossil organs. Although I have been persuaded that actual type specimens are not required by the letter of the law, I think it would be better to designate such specimens. Although there seems to be some ambiguity on the point in the letter of the law, I feel that where more than one illustration of a new organ species is published, the author should clearly state which illustration shows what he considers to be the type.

To summarize, I feel that we ought to apply rigorously the International Code of Botanical Nomenclature to palynological publication. I also must add that I realize fully that you were a pioneer in the field, that hind-sight is easier than foresight, and that I sympathize with the problems you were faced with, having very little precedent (and much of that bad) to go by.

For one, I would regret it very much if your misgivings about the nomenclatural difficulties would drive you out of palynology. Nearly everybody agrees with you about Pflug's work. I am confident that the nomenclatural problems can be solved.

I am sorry to hear that you have lost Miss Pike and fully appreciate that you must feel isolated. But surely there will be a paleobotanical expansion in Australia one of these days.

Very best wishes for a happy 1956.

Sincerely yours,

A. Traverse,  
c/o N.V.De B.P.M.,  
GA/Pal.Lab.,  
30, C.v.Bylandtlaan,  
The Hague

American address:

Shell Development Co.,  
3737 Bellaire Blvd.,  
Houston 25, Texas

Region V  
Box LL, University Station  
Grand Forks, North Dakota

October 27, 1954

Dr. Isabel Cookson  
C/o Dr. Georges Deflandre  
105 Boulevard Raspail  
Paris 6, France

Dear Dr. Cookson:

Thank you for your letter of October 13 from Paris. I used some color photomicrographs of your Botryococcus slides in my recent paper before the American Institute of Biological Sciences, on Botryococcus in the Brandon, Vermont, lignite. I had thought you would be back in Australia now, and my request for modern Botryococcus material was prompted by an earlier letter from you suggesting that you might subsequently have such available. If you ever do, I would be very pleased to have some. I much appreciate your cooperation in sending me the fossil material from Warlands Creek.

It seems an interesting coincidence to me that you are working on fossil dinoflagellates, for I turned one up in the Brandon lignite and have had some correspondence with Dr. Deflandre about it. It took me several years to find out what the thing is. Our American authority on dinoflagellates, R. Thompson, says it is a species of Peridinium.

Very truly yours,



Alfred Traverse  
Coal Technologist

ATraverse:vh

cc: Lankford  
Region V  
Traverse  
File 625.1  
C File

105 Boulevard Raspail  
Paris VI  
October 13<sup>th</sup> 1954

Dear Dr. Traversi.

My colleague Miss Kathleen Pico has forwarded your request to me. Certainly, you can make use of the slides I sent you in any way you wish, but I'm afraid I have no means of helping you out with fresh material of *Botryococcus*.

I have been in Europe since April & most of the time, except for two months during the summer when I was in England and Scandinavia, in Paris working with Professor Deflandre at the above address. I had found quite a large number of Dinoflagellates and Haptophyta in some Australian marine deposits (Tertiary and Cretaceous) and thought it a good opportunity whilst on leave for the Botanical Congress to have specialist help with their classification. I am leaving Paris on November 30<sup>th</sup> for London from whence I expect to sail for home on Dec. 8<sup>th</sup>.

With best wishes

Yours sincerely

Karel Cootson

Region V  
Box LL, University Station  
Grand Forks, North Dakota

September 29, 1954

Via AIR MAIL

Dr. Isabel C. Cookson  
Botany School  
University of Melbourne  
Melbourne, Australia

Dear Dr. Cookson:

Several months ago you very kindly sent me slides of your preparations of the carbonaceous clay at Warlands Creek, N. S. W., showing examples of Botryococcus. I have found the slides very useful for comparison with Botryococcus colonies from the Brandon, Vermont, lignite. I recently gave a paper orally on this occurrence and contemplate publishing a short paper on the same subject. I hope you will have no objection to my using photomicrographs I have made from your preparations. Could you tell me, for my records, what stain was used in the stained preparation? (One was stained, one unstained.)

I would very much appreciate it if you ever have material available of the modern Botryococcus Braunii Kützing, if you could send me a slide or two. Or perhaps you could advise me where to beg some.

Thank you for your help.

Very truly yours,

*AT*  
Alfred Traverse  
Coal Technologist

ATraverse:vh

CC: Lankford  
Region V  
Traverse  
File 625.1  
C File

TELEPHONE  
F 0484

The University



of Melbourne

Melbourne, N.3. Jan 26<sup>th</sup> 1954

Dear Dr. Travase,

I am forwarding two slides of a *Delavay* deposit from New South Wales in which colonies of *Bobyococcus* are abundant. There is no need for you to return them - Unfortunately I have no mounts of the living form but should be able to get some fresh material will send it on later. I am leaving for Europe on March 13<sup>th</sup> but hope to be back in Melbourne by January 8<sup>th</sup> 1955.

I am very glad that you found something of interest in my paper - Best wishes

Hunt Institute for Botanical Documentation  
Lancelotti

Region V  
Box LL, University Station  
Grand Forks, North Dakota

February 8, 1954

Air Mail

Dr. Isabel Cookson  
Botany School  
University of Melbourne  
Melbourne, Australia

Dear Dr. Cookson:

Your cooperation in rushing the Botryococcus slides to me was much appreciated. They arrived in perfect condition on January 31. The air mail service between here and Melbourne is so good that I can scarcely believe it.

Comparison of the microfossils I mentioned in my letter of January 20 with these Botryococcus colonies convinces me that the fossils from Vermont are at least closely related, if not identical. This is very interesting in helping me understand the sedimentary sequence in the Vermont deposit.

Presumably the visit to Europe you mention in your note of January 26 will include the botanical meetings in Paris. It should be a wonderful experience. I wish it were possible for me to attend the palynological sessions. Maybe I can make it for the 1958 meetings!

Very truly yours,

*AT*

Alfred Traverse  
Coal Technologist

ATraverse:dm

cc: Region V  
Lankford  
✓Traverse  
File 625.1  
C File  
E. S. Barghoorn

Region V  
Box LL, University Station  
Grand Forks, North Dakota

January 20, 1954

Air Mail

Dr. Isabel C. Cookson  
Botany School  
University of Melbourne  
Melbourne, Australia

Dear Dr. Cookson:

Your recent paper, "Records of the Occurrence of Botryococcus Braunii, etc." was of special interest to me because of the abundance of microfossils apparently identical with your Botryococcus forms in a Tertiary (Oligocene?) silt from Vermont (U.S.A.) that I have investigated.

Would you be willing to send me, on loan, preparations of the living or fossil forms, or preferably both? I would return them to you a few days after receiving them here.

Very truly yours,



Alfred Traverse  
Coal Technologist

ATraverse :dm

cc: Region V  
Lankford  
Traverse  
File 625.1  
C File

Box LL, University Station  
Grand Forks, North Dakota

September 22, 1953

Dr. Isabel Cookson  
Botany School  
University of Melbourne  
Melbourne, Australia

Dear Dr. Cookson,

Thank you very much for the reprints of your interesting papers, which arrived recently. I am now doing research on the microstructure of lignite here, having completed my doctoral work at Harvard on the pollen and spores of the Brandon lignite. I am hoping to have the monograph on that work in print before long.

Very truly yours,

*AT*

Alfred Traverse  
Coal Technologist

ATraverse:dok

cc: Reg. V.  
Lankford  
Traverse  
File 023  
C file

UNITED STATES  
DEPARTMENT OF THE INTERIOR  
BUREAU OF MINES  
Region V  
Box LL, University Station  
Grand Forks, North Dakota

May 14, 1952

Dr. Isabel Cookson  
Botany School  
University of Melbourne  
Victoria, Australia

Dear Dr. Cookson:

Thank you for the reprint of your paper on  
"Tertiary Araucariaceae from Southeastern Australia...",  
which was forwarded to me.

I am now located here at this new laboratory for  
lignite investigation doing research in coal paleobotany  
and petrography.

Sincerely yours,

ALFRED TRAVERSE  
Coal Technologist

cc Region V  
Dr. Traverse ✓  
File

Dr. Isabel Cookson  
The University of Melbourne  
Melbourne, N. 3, Australia

Dear Dr. Cookson:

It was a great pleasure to ~~get~~ your gracious and friendly letter of 21 February. The report on your current research was most interesting. I have also received the packet of reprints, for which I am most grateful. I hope to be able to send you something on my work with the Brandon lignite before long.

As the time comes for me to put my research on this Tertiary coal into print I am having a very difficult time deciding what to do about nomenclature. The wisdom of creating form-genera for plant parts which might well belong to living genera which are not included in one's reference collection seems to ~~me~~ at least dubious. Even those pollen-grains which belong to extinct genera presumably belong to extant families or orders. Does not this make the problem entirely different from that of, say, Pennsylvanian or even Cretaceous, pollen and spores?

Yours sincerely,

Alfred Traverse

# The University of Melbourne

MELBOURNE, N.3

Dec. 21<sup>st</sup>

1957

Dear Dr. Traverso,

Thank you very much for your letter of 2<sup>nd</sup> December - It's encouraging for those of us who work in such a far away country as Australia to have contact with, and the interest of overseas workers in the same field.

Under separate cover, I am forwarding the reprints you asked for. At present a colleague and I are preparing an account from macroscopic remains of Araucaria and Agathis from Victorian deposits of brown coal. We are including a section on pollen grains of both living and fossil members of the Araucarioaceae.

With best wishes for the progress of your work - about which I shall be interested to hear.

Yours

ours sincerely  
Kathleen Coote