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Eugene, OR
REGISTER-GUARD

corresp. J. Gray

CA. 1 Dec. 91

A cat's caper creates chaos

HAS ANYBODY out there seen a cat named Cujo, a neutered beige-and-white male with a green herbal flea collar — and an odd habit of going for your throat?

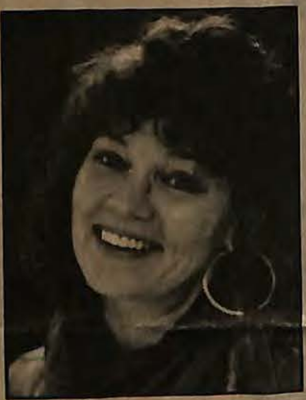
If so, please phone. Cujo's reappearance would end the agony of his owner, Terian McComb, before she leaves for Mexico.

It would also bring considerable relief to the ranks — and hierarchy — of the University of Oregon and the Eugene Police Department, not to mention the Lane County district attorney's office, all of which have become entangled in this cat caper.

At various times, the caper has reached the offices of UO Vice President Dan Williams, university attorney Peter Swan, Eugene Public Safety Director Dave Whitlow and Eugene attorney Charles Porter.

"You name it; they got involved — over this stinking stray cat," said Ron Tendick, UO family housing director.

Well, that's a bit unkind. McComb will tell you that Cujo is neither stinking nor a stray. But he was illegal, which is how this all got started.



Terian McComb

Cat's owner leaving soon

McComb, 42, is a telecommunications major who lives in the UO Amazon housing project with her daughter. Cujo, then age 7 months, lived with them until McComb got caught in September.

It's against the rules to keep pets at Amazon. Either the cat goes or you go, the UO told her.

McComb arranged to put Cujo up with a friend. But she missed him so much that she brought him back, knowing full well she was risking eviction.

Cujo, the ungrateful cad, responded by splitting.

The short version of what happened next — trust me, you don't have time for the long version — is that another Amazon resident found Cujo a week later and turned him in to the project office as a stray. Ann Menge, a maintenance worker and former student, saved him from a possibly unhappy fate by calling one of her former professors.

"She knows I have a thing about cats," said biology Professor Jane Gray. "She said, 'Look, I've got this stray cat, and if you don't get it in the next two hours, it's off to the knackers.' "

Gray got the cat.

MEANWHILE, McCOMB SPENT the next several weeks plastering Amazon with lost-cat fliers. Just before Thanksgiving, she got a call from another resident, telling her the cat had been turned over to the project office. From the office, McComb got Menge's name and went to see her at home. But Menge wouldn't tell who she gave the cat to.

So McComb called the cops. Menge wouldn't tell officer Randy Ellis either, even when he warned that she could be charged with cat theft.

Menge went to her bosses. McComb went to the UO Student Advocacy office. From there the cat tale gets really tangled.

University officials were calling city police officials. They were calling subordinates. Attorneys were calling attorneys.

"Everybody got their knickers in a twist," was the way Gray described it. Those are not to be confused with knackers.

McComb claims that the UO tried to get the cops to quash the case. But UO housing Director Tendick claims McComb must have an in with the cops. "I thought EPD had more to do than mess around with stray cats," he said.

Gray — whose identity ultimately became known amid all the claiming and calling — called attorney Porter, in case somebody was planning to charge her.

BUT WHEN I CALLED Porter, he refused comment — a clue to just how bizarre this case really is. It may be the first time that the loquacious ex-congressman refused comment on anything.

Because Gray is a biology prof and because the UO does some animal experiments, I had to ask her: Did Cujo end up in her lab? She was outraged: "Of course not! I love cats! I'm mad about cats!"

So is McComb, at least about Cujo. She talked about his most endearing trait: "Cujo will run up your leg, up your body, lay flat on your chest and start suckling your neck," she said.

Since he was named after the killer dog in a Stephen King novel, that little habit might make some people nervous. But McComb swears he's merely "aggressive in a very affectionate way."

The cops finally saw a way out of all this by dumping the case on the DA's office. You see, the Legislature passed a new law making theft of a "companion animal" — including cats — a felony.

At last report, the DA's office was trying to decide if what happened constitutes theft, and if so, who committed it and whether the cat caper will go to a grand jury. Meanwhile, McComb is getting antsy; she leaves Wednesday to study in Mexico.

Don't suggest that Gray simply give Cujo back to McComb. Gray says Cujo split again, long before she knew McComb was looking for him.

"The cat was not very happy," Gray said. "I kept it around long enough that I thought it was acclimated. But I let it out, and it disappeared."

So Cujo's presumably out there somewhere, just looking for another throat to leap for.

file as correspondence,
J. Gray

1/30/90

Al—

1. Xerox from J G proposal

2. my review (not yet sent)

3. Working manuscript on "cryptospores" - comments welcomed

Thanks

Paul

The PI has spent considerable effort addressing former critics of her proposals in addition to other "complaints" which seem not to be appropriate and, therefore, will not be addressed in this review directly. I will confine my comments to the scientific merit of the proposal. However, I must acknowledge that this area of research does overlaps with my own, and my name appears on the PI's list on page 25 which claims that I am either a "direct competitor" or that my "ability to dispassionately, fairly judge" this proposal is in question.

I will address my comments specifically to the four objectives listed in the proposal, rather than writing a comprehensive review. My comments may appear to be somewhat narrow; I intend to use specific examples in my discussion to avoid vague generalities which might appear unsubstantiated.

1. critical sampling-- The PI is either unaware or is simply not acknowledging the current efforts of others in this area. Specifically, Norma Johnson in a 1985 paper in the Review Paleobotany and Palynology has sampled stratigraphically through the Tuscarora Formation and documented the cryptospore diversity trends which exist in the Mill Hall section. This work was expanded upon and continued by two Masters theses (DeSimone, 1988 and Freile, 1988) at Boston University whose results have been presented at the Northeast GSA (1988) and at the 1989 annual meeting of the American Association of Stratigraphic Palynologists in Tulsa. These students have logged 5 sections through the Tuscarora and integrated the study of palynomorphs, sedimentology (Cotter, 1983), and ichnology into a comprehensive view of the paleontology and paleoecology of this section of the column.

The proposal does not specify where the colections will be made. There are numerous sections which could be "near Harrisburg". It is difficult to address the feasibility of the proposed sampling procedure since the specific sections are not mentioned in the proposal. Since the Rose Hill Tuscarora boundary is characterized by a change in depositional environment, one expects to find a reduction in cryptospore numbers as the boundary is sampled. The Rose Hill may yield cryptospores, but unpublished data (DeSimone 1988) and the work of Cramer and of Smith and Saunders indicate initially that this is unlikely.

2. Documentation of spore species. The PI offhandedly rejects the current taxonomy used to describe her material, citing "...a number of reasons (detailed in a Ms in prep....)" She then claims that " a purely descriptive 'traditional' taxonomic approach to biostratigraphy, such as used by Richardson and Edwards (1989) are [sic] less satisfactory than the statistical approach which I have been developing." This "statistical approach" seems to consist of measurements of tetrad size from sample horizons/formations which are then placed on a time line to show progressive, gradual increase (her figure 1.)

The PI has been able to recognize (summarized in fig. 1), four taxonomic "entities": smooth tetrads, tetrads with reticulate perispore, tetrads with rugose envelopes, and trilete spores. Although she may disagree with how other workers have classified the cryptospores, the fact remains that these "entities" which she lists in her figure 1 very well may correspond to species described and published by other workers. For example, tetrads with reticulate envelopes have been described by Miller and Eames (1982) as the taxon, Nodospora retimembrana.

Contrary to the claims of the PI, what is needed here is more "traditional taxonomy", or at least some modicum of conventional systematic practice, whereby authors publish their conclusions about the construction of valid taxa which enables others to then test those taxonomic "conclusions" and compare, contrast and possibly modify and improve upon our taxonomic basis. The PI has consistently ignored this taxonomic basis in her research, claiming instead to have better methods in mind (e.g. this lumped stratophenetic concept) or that "these names would have bored the geologists for whom our paper was intended

(Gray et al., 1983). . The research efforts of the PI will probably continue to emphasize ecological and pseudo-evolutionary trends, based on her own (and exclusive and private unpublished data) without a clear and precise taxonomic and systematic basis for those claims. [Such has been the case with her work for the past 20 years-- a claim for priorty, importance, significance, etc. without scientific presentation and documentation of the data set.] Even though the PI does not agree with the validity of the taxa of any other workers in this field, she has proposed no valid alternative or emmendation of present taxonomies and, consequently, there is no basis for comparing her work with the work of others (see example below). This may appear as a trivial or somewhat philosphical point, but most stratigraphers appreciate the purpose of taxonomy minimally as a way of labeling similar objects which enables the transfer of biostratigraphic information. Without this common language of systematics, biostratigraphic correlation is impossible.

3. stratophenetics-- The resurrection of Cope's Rule as a causal explanation for gradual increase in spore (tetrad) size as a stratigraphic method is both flawed and misleading. To begin with, there is no a priori reason to assume that Cope's Rule applies to spores, especially when the spores in question are of possibly mixed origin (they may have come from bryophyte-like plants, algae, nematophytes, etc., we are not really certain of all possible affinities). The claim is that an evolutionary "law" which is observed on the basis of the comparative anatomy of fossil vertebrate species (=whole organism) is also to be seen in the comparison of 1 to 4 cells from the haploid generation of as yet undetermined plants. [The PI would have to show, for example, that the fossil sperm of the dinosaurs also followed Cope's Rule and then apply this by analogy to meiospores of the plant kingdom.] In any case, there is no evidence for increasing individual cell size in those vertebrates which do seem to follow Cope's Rule for body size.

Regardless of the Cope's Rule problem, it is impossible to tell from her work (e.g. Gray, 1985b and figure 1 which cites "Gray et al., 1986" but which is not in the bibliography) whether the basic populations represented in the diagram demonstrate a gradual size trend. To begin with, since the PI does not subscribe to conventional taxonomic use, it is impossible to tell what objects (=kinds, species, taxa) have been measured, since the measured populations refer only to "measured spore samples" and "domimnant spore tetrads and their size." This diagram does not appear (at least they are not referenced) to include the published record of tetrads sizes from the Medina (Miller and Eames, 1982) and the Tuscarora (Strother and Traverse, 1979; Johnson, 1985). These authors have at least attempted to distinguish generic and species-level taxa from within these assemblages.

Even if a "statistical" argument showed a trend in this data set, by lumping the size data from tetrads (spores) without recognizing taxa, such a trend would be meaningless. As noted in Strother and Traverse (1979) in a paper which describes tetrad taxa, there exists in the Tuscarora Formation both smaller and larger forms of tetrahedral tetrads which were lumped originally into one species taxon. These authors noted that "the size/frequency distribution of the sample population measured to characterize the genus is slightly bimodal, implying the possibility of a heterogeneous underlying distribution." They go on to predict that, "careful examination of many samples may reveal a disjunction in the genus based on size, but at present, such a distinction has not been made" (Strother and Traverse, 1979, 12). Thus it is quite likely, that the segregation of taxa from these populations would show that both "large" and "small" tetrads are contemporaneous.

There is published data from the Power Glen sample which is displayed on figure 1. Miller and Eames (1982) recognized the following size ranges (means) among 3 different tetrad species (data in microns):

Tetrahedraletes medinensis: 25 (37) 51
Nodospora retimembrana: 34 (43) 60
Nodospora burnhamensis: 25 (38) 56

The PI has produced a range and mean from this deposit of 16 (32) 56 which is lower in mean size than the original data. This may seem trivial, but again, without reference to taxa (figure 1 is supposedly derived from published sources) how are we to understand the nature of such a discrepancy? [I need to add that the claim of the PI is that differences on the order of a few microns are significant.]

The danger in using gradual size increase as a biostratigraphic indicator need not be articulated here (see arguments in Cracraft and Eldredge, 1979), but the PI has already misused the data set in figure 1 by admitting that the age of her Massanutten sample is based on occurrence of dominant spore tetrads and their size-- the problem is that this sample represents the youngest (=largest) sample. It is the logical upper end-member to the so-called progressively increasing lineage; she has already mixed material which was derived from the "dating" method with the "proof" (=figure 1) of the validity of the method itself.

It seems clear that the PI has already formed a conclusion about evolutionary history (the size trend) and merely intends to plug in more data to support this "conclusion" without bothering to address the published data is already available on this subject. (See Johnson (1985) for a discussion on the effects of processing on Silurian cryptospores.)

4. SEM studies- This is normal for the sudy of palynomorphs and need not be addressed.

In conclusion, this proposal is flawed in its purpose, not in its recognition of an interesting and important section of the stratigraphic column, but rather in the simple and one-sided approach to science demonstrated by 1) a consistent and purposeful disregard for other people's data (Norma Johnson's work, for example) and an apparent lack of awareness of the current work of others on the geologic units in question, 2) the emphasis of importance, evolutionary significance and conclusions prior to the presentation of data upon which those conclusions are based, and 3) lack of critical approach to taxonomy in prior work, both with regard to evolution and to the more utilitarian aspects of biostratigraphy.

Stanley, S.M., 1986, Earth and Life Through Time: W.H. Freeman (excerpt enclosed)

Cloud, P., 1988, Oasis in Space: Early History from the Beginning: W.W. Norton, 508 pp (excerpt enclosed)

Wicander, R. and J. S. Monroe, 1988. Historical Geology: West Publishing Company, pp. (excerpt enclosed).

Retallack, G. (in press, due Feb, 1990)

Cowen, R. (in press, due Feb, 1990)

Additional evidence of the educational value, and the overall acceptance of the work at the post-doctoral level has been my continuing series of invitations to participate in symposia, or provide keynote addresses, involving early evolution of higher land plants and allied topics:

1983, Autecology of Silurian Organisms, sponsored by the Palaeontological Association, United Kingdom;
1984, Evolution and Environment in the Late Silurian and Early Devonian, sponsored by the Royal Society, United Kingdom;
1986, Contribution of Palynology in Early Land Plant Evolution, sponsored by the American Association of Stratigraphic Palynologists;
1987, Systematics, evolution and ecology of early land plants, sponsored by the XIV International Botanical Congress, Berlin;
1990, International Symposium on Pollen and Spores: Patterns of Diversification, sponsored by the Systematics Association & The Linnean Society of London;
1991, Origin and History of the Continental Ecosystem, sponsored by the Second International Congress on Paleoecology, Nanjing Institute of Geology & Palaeontology, Academia Sinica, Nanjing.

POTENTIAL CONFLICTS OF INTEREST IN MERIT REVIEW

[This information is added to conform to Notice No. 107, September 11, 1989, from the National Science Foundation with regard to Proposal Format Changes]

This list includes individuals with whom I have had written or oral exchanges of a vituperative nature which leads me to question their ability to dispassionately, fairly judge <u>any proposal</u> prepared by me. It also includes individuals who view themselves as direct competitors in this area of research who wish to discourage the efforts of others. In a few cases I have made additional comments or attached documents).

It should also be obvious that any grant reviewer who does not know that an acritarch is not a land plant spore and who thinks that a proposal titled "A pre-Devonian Spore Biostratigraphy" should deal with Precambrian and Cambrian acritarchs ("... the title does not reflect the subject because when I opened the proposal, I expected to read about Cambrian and Precambrian acritarchs....) is not competent to evaluate a grant proposal from me. I might question the competence of this individual as a paleontologist.

The ability in the NSF review process to hide behind anonymity obviously shields many reviewers from the need either for truth or comprehension of what they are reviewing. It also permits comments of a personal nature that have little or no bearing on the research problem. Some reviewers take full advantage of this situation for lack of accountability in the knowledge that no meaningful rebuttal possible on the part of the investigator.

Harlan Banks: I have had serious professional conflicts with Professor Banks resulting in actual published exchanges (see Gray and Boucot, 1977).

Francis Hueber: I have had professional problems with Dr. Hueber, because of published allegations by him impugning JG's professional competence. This led to a meeting between Dr. Hueber, legal council from the office of the Secretary, Smithsonian Institution, Dr. John Lance, former Program Director, National Science Foundation, and Dr. A. J. Boucot at whom the allegation was also directed. This led to a published retraction by Hueber in the Botanical Journal of the Linnean Society (copy enclosed).

Alfred Traverse: A former program director of an NSF Section has identified this individual to JG as her "worst enemy." There have been published exchanges of a vituperative nature, as well as oral exchanges. This individual has reviewed many NSF grant proposals (the style is recognizable, as I have known this man for about 40 years) and has consistently both downplayed the work and even made false and misleading statements because he is also working in the same area. Several years ago, he misdirected Boucot and me to a fossil locality (whether inadvertently or by design is unclear, by his own written admission to me) where I spent considerable NSF funds in vain while collecting and processing barren samples.

Paul Strother: A student of Alfred Traverse's who was senior author on Traverse's only published contribution to the land plant question. There have been published exchanges with these two.

Patricia Bonana: A student of Harlan Banks, with whom there have been oral exchanges (at an AAAS symposium in Detroit).

Tom Phillips: Unsympathetic to the work and good friend of Harlan Banks.

Patricia Gensel: Unsympathetic to the work.

Jack Wolfe: Potential conflicts of long standing (ca. 30 years).

Karl Niklas: There have been both written and oral exchanges of a personal nature with this individual.

CLOSE COLLABORATORS

A. J. Boucot, G. Kent Colbath.

BIOGRAPHICAL SKETCH

[This information is added to conform to Notice No. 107, Sept. 11, 1989 from the National Science Foundation with regard to Proposal Format Changes]

Jane Gray (Jacobson)

Address: Department of Biology, University of Oregon, Eugene, Oregon 97405, and Department of Zoology, Oregon State University, Corvallis, Oregon 97331

Education: A.B., Geology, Harvard University (Radcliffe College), Cambridge, Mass., 1951 (cum laude)

PROPOSAL RATING SHEET

Substantive comments on the proposal as well as an overall evaluation will be appreciated. Also, your suggestion of additional reviewers is welcome. Please return to: The Petroleum Research Fund, 1155 16th Street, N.W., Washington, D.C. 20036.

November 20, 1989

REVIEWER

~~Dr. Albert L. Guber~~
~~Department of Geoscience~~
~~The Pennsylvania State University,~~
~~Erie~~
~~Erie, PA 16510~~

DR. ALFRED TRAVERSE
Deike Building
Pennsylvania State University
University Park, Pa. 16802

PRF# 22931-AC8
Principal Investigator:
Dr. Jane Gray
Department of Biology
University of Oregon

Please return with proposal by:
December 11, 1989

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COMMENTS (Continue on separate sheet if necessary)

Jane Gray is certainly the leading figure in study of Late Ordovician-Early Silurian land plant macrofossils. She has not, however, so utterly dominated the field as she characteristically in this proposal asserts. The stratigraphic fine-tuning she proposes is interesting, but the rocks she proposes studying havedbeen rather thoroughly investigated by Strother, Traverse and various students, including N. Johnson, and most recently by several students of Strother. The distribution of palynomorphs therefore is pretty well known. It is unlikely that there are many real surprises in the palynology of the Tuscarora+Rose Hill Formations. Furthermore, there is a question of ethics here. Professor Strother and several students are involved in very thorough analyses of exactly the outcrops of Silurian rocks of Pennsylvania that Gray proposes studying. It's a free world, but does ACS really want to subsidize such duplication of effort? It would be better to give the money to Strother.

OVER-ALL EVALUATION

( ) Truly Exceptional
( ) Excellent
(X) Very Good
( ) Good
( ) Fair
( ) Poor

Please refer to insert page for definition of these categories.

OTHER SUGGESTED REVIEWERS

Paul K. Strother
Department of Geology
Boston University

Signature and Date
11 December, 1989

COPY TO BE RETAINED IN REVIEWER'S FILE

NATIONAL SCIENCE FOUNDATION

PROPOSAL EVALUATION FORM

NSF Form 1 (4/84)
Supersedes All Previous Editions

PROPOSAL NO.	INSTITUTION	PLEASE RETURN BY
EAR-8903990	Oregon St Univ	02/05/89

PRINCIPAL INVESTIGATOR	NSF PROGRAM
Jane Gray	STRATIGRAPHY & PALEONTOLO

TITLE Evolution of Early Terrestrial Ecosystems

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

Jane Gray is a very talented, well known and productive scientist, whose reputation in Silurian paleobiology is well earned. That she begins this proposal with "I", and repeats that pronoun, or "my" or "Gray" over 20 times to the bottom of p. 3 is characteristic egocentricity, as are the constant references to "my spore work," "My fossil-based spore data," etc., implying that others are not, or are only marginally involved.

Dr. Gray simply ignores, plays down, or belittles the work of others. Norma Johnson's (1985) study of the Tuscarora section at Mill Hall, PA, for example, is described by Gray (p. 14) as among studies of "samples from isolated exposures"! On the contrary, anyone who has read Johnson's 53-page paper in <u>Review of Paleobotany & Palynology</u> presumably would recognize that the Mill Hall section covers the Tuscarora without break from Juniata to Rose Hill, even including the Castanea facies at the top of the Tuscarora. The Mill Hall Tuscarora section is more complete than the Millerstown section, despite Gray's claim for the latter, and Johnson covered it very thoroughly, from bottom to top.

Paul Strother was actively engaged in palynological study of the Silurian of Pennsylvania before Gray was, and he and his graduate students at Boston Univ. are still very actively involved in such studies--two master's theses have already come out of this work and will provide the basis for forthcoming publications and research. Strother certainly has "priority" for the Pennsylvania non-marine Silurian work, in which he is actively engaged--Tuscarora through Clinton. If he is also applying for NSF support for Silurian work in PA, it would be a travesty for Gray to get it instead.

(continued on separate sheet)

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

Gray has marked the NSF-supported papers with a special symbol. Although one doesn't know which papers go with what grant, it is clear that Dr. Gray has been very productive of high caliber work.

Identity of reviewers will be kept confidential to maximum extent possible.

OVERALL RATING: ☐ EXCELLENT ☐ VERY GOOD ☒ GOOD ☐ FAIR ☐ POOR

REVIEWER'S SIGNATURE

Alfred Traverse

REVIEWER'S NAME (TYPED)

Dr. Alfred Traverse
Department of Geosciences
Pennsylvania State University

OTHER SUGGESTED REVIEWERS (OPTIONAL)

REVIEWER'S COPY

Continuation of review of Proposal No. EAR-8903990 (Jane Gray, PI)

Whether Gray will find a completely different suite of things at Millerstown from what Norma Johnson found at Mill Hall seems somewhat doubtful to this reviewer. Both Strother and Johnson have macerated material from many localities in Pennsylvania, including Millerstown.

So, the essence of this proposal boils down to Gray hoping to find large pieces of, or whole, arthropod animals in her Millerstown macerations. Except for that, her proposal duplicates Strother's present work and Johnson's published work. It is for this reason I do not give this proposal high marks, despite the fact that Gray is a top person in the field of Silurian studies.

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

6 May, 1987

Dr. Jane Gray
University of Oregon
Department of Biology & Geology
Eugene, Oregon 97403

Dear Jane:

Thanks for yours of 21 April. Re the book--it is in the type-setting process. That's all I can say. As to the $ you paid--I thought they were refunding all such, but I will personally stand by your claim. I hope you kept your receipt or cancelled check!

Let's all bury the various hatchets, as I've previously suggested. I am sure I don't err about you and AB arriving here in the midst of registration, totally unannounced. I have practically total recall. But it was a long time ago, in any event.

As I've said before, I admire you very much and wish you all the best.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et

April 21, 1987

Dear Al:

Best thanks for the reprints and for your letter of March 12. Sorry for the delay in acknowledging both, but I must give the usual excuse --overwork and low pay! In any case, sometime over a beer or two, or something harder, we can debate the issue of past remembrances-- which appear not to tally for any of the parties most intimately involved. As for my responses to anything, they are always totally unpredictable, and erratic -- if you knew me better you would know that. AJB on the other hand, is almost invariably a gentleman to visitors, especially distinguished ones and graduate students! Someone appearing totally unannounced does not faze him at all-- he loves surprises. I am not certain about the significance of your inked in comment-- but I will take it at face value, although you found it necessary to stress that you <u>really</u> meant it-- should I be suspicious of the <u>really</u> (my underlining-- yours was on the "mean").

Has your book finally appeared. I bought and paid for a copy years ago-- I hope the publisher intends to honor that committment.

Best wishes,

Jane

Jane Gray
Biology, University of Oregon etc.

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

12 March, 1987

Dr. Jane Gray
University of Oregon
Dept. of Biology & Geology
Eugene, OR 97403

Dear Jane:

Yours (undated) written on my letter came in an envelope with Boucot's (not your) return address. Hmm. I suppose that's also related to the mail-charge caper, as the typing of "Boucot" is the same (slightly elevated e's and a's) as in your note. In any event, I look forward to the batch (?) of reprints.

According to my records, I've sent you a copy of practically everything for years. I suppose you probably have just chucked 'em, without opening the envelope. I enclose a few nondescript items of which copies were apparently never sent--at least it isn't recorded that they were. You told me in NYC you did get a copy of Norma Johnson's master's paper, which I asked her to send you. It was a product of this lab, of course. Many profs would have insisted on co-authorship.

Isn't it funny (I've often had the experience) that different people remember the same events <u>entirely</u> differently. I recall that you and AB arrived here one afternoon, <u>totally</u> unannounced (no phone call, no letter) during registration when I was interviewing dozens of students. You and he were very abrupt and demanding and declined to wait until the next day, and I recall being <u>very</u> annoyed with the whole affair. I don't recall misdirecting you--certainly not on purpose--but under the circumstances as I recall them, it would have been understandable, and a <u>neat</u> idea. Maybe I am more devious than I realize! How would JG and AB have responded if the roles had been reversed?

I just re-read your paper in the Smiley volume with further interest and profit. The way the taxodiads dominated the whole Northwest for millions of years is fantastic. I'm looking at stuff from the Eocene-Oligocene of the Arctic that is even more taxodiad-city.

Good to hear from you.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT/et
enclosures

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

2 February, 1987

Dr. Jane Gray
Department of Biology and Geology
University of Oregon
Eugene, OR 97403

Dear Jane:

Whatever happened to wad of reprints you said you'd send? Sure would help with trips to the library. I have nothing post-1975. I guess that's when I fell from grace?

Best.

Boucot

Oregon State University
Department of Geology
Corvallis, Oregon 97331-5506

Yours very truly,

Al

Alfred Traverse
Professor of Palynology

AT/et

Dear AL: On slowest mail I have sent on reprint(slowest because we get charged for all mailings going out). Boucot suggests that I tell you that you "fell from grace" sometime in the 70's when you misdirected us to a locality in PA from which we took considerable trouble to collect and process barren samples!

Have you sent ME any reprints lately?

Jane

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

Merrill Lynch Cash Management Account
JANE GRAY

128

Oct. 2 1986 25-80/440

Pay to the order of Alfred Traverse $60 00/100

Sixty (60) and NO/100 Dollars

BANK ONE.

BANK ONE OF COLUMBUS, NA
Columbus, Ohio 43271

Memo AASP Registration Jane Gray

⑆044000804⑆ 3017778380⑈ 0128

AT

Many Thanks again.

Will send reprints shortly – waiting for Geol. Mag. paper! JG

GEOLOGY
Eldridge Moores, Editor
(916) 752-8938
Department of Geology University of California Davis, California 95616 U.S.A.

June 3, 1982

file

Jane Gray
Dept. of Biology & Geology
University of Oregon
Eugene, OR 97403

Dear Dr. Gray:

We recently received a Comment from A. Traverse and P. Strother concerning your paper "Caradocian land plant microfossils from Libya" which appeared in the April 1982 issue of GEOLOGY. A copy of the Comment is enclosed. This letter is to inquire whether or not you wish to write a Reply to the Comment.

The Forum in GEOLOGY performs an important function in providing space for short, crisp, provocative exchanges. It has only two firm rules in this regard; first, that there be only one Comment and one Reply between each set of correspondents, and second, that neither Comment nor Reply exceed four typed double-spaced pages (including references, with names of journals and publishers completely spelled out); two pages preferred.

Please feel free to discuss the questions involved with the author directly, or through our office. We like to print our Forum contributions as promptly as possible. If you do plan to write a Reply, please let me know by return mail and then send the Reply within about two weeks. If we do not hear from you within one month of the date of this letter, we will have to assume that you do not intend to submit a Reply and we will proceed to publish the Comment alone.

We are pleased that your article generated the interest it has, and we'll look forward to hearing from you soon.

Sincerely,

Eldridge Moores lw

Eldridge M. Moores
Editor

EM:lw
Enclosure
cc: A. Traverse and P. Strother

December 12, 1975

Dr. Jane Gray
Museum of Natural History
University of Oregon
Eugene, OR 97403

Dear Jane:

Thanks for yours of 8 November. I am enclosing a couple of xerox sheets which answers your questions about the special volume dedicated to Isabel Cookson. I am interested in the fact that you have seen my review of the volume because I haven't. Our library does not seem to have the number in which it appeared. It was written for Earth Science Reviews and submitted for publication over a year ago. I suppose maybe it is in the fourth number for this year, which hasn't come here yet. I also enclosed a xerox copy of the page proof, for whatever interest it might be to you.

Thanks for the compliment on my critique of the genus-box, etc., notion.

Regarding my work on the Lower Silurian from this county--I'll send you a piece of that when I get around to it. I must warn you that I have quite a few irons in the fire and it may be a while. Best wishes.

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

Enclosures: Xerox copies.

October 22, 1975

Dr. Jane Gray
Museum of Natural History
University of Oregon
Eugene, Oregon 97403

Dear Jane:

Your paper with Boucot, "Color Changes in Pollen and Spores; a Review" is great! I have really enjoyed reading every word of it this evening. It is maybe of interest to you, as I think I mentioned earlier, that I have found beautifully preserved and very slightly coalified spores in rocks from Silurian to Pennsylvanian age in this (Centre) county of Pennsylvania within the last few years, although rocks of the same formation just a county or two away invariably show highly coalified fossils. My guess is that we are inaa pocket of very little metamorphism here, although the geologists don't talk much of other evidence for the same phenomenon.

Anyway, I did want to let you know how much I have profited from reading your paper. I am looking forward to seeing you again one of these years. Best wishes, I am

Yours very truly,

Alfred Traverse
Professor of Palynology

AT:jb

UNIVERSITY OF OREGON

Museum of
Natural History

EUGENE, OREGON 97403
telephone (code 503) 686-3033

November 6, 1975

Dr. Alfred Traverse
Department of Geology and Geophysics
Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

Thanks for your recent paper, the blast at Hughes (well deserved!) and the book review. Norm has a number of crack-pot ideas, or so it seems to me. I think that I have told him so on more than one occasion, although we have not been in communication for quite a long time (who afterall likes to be told that their ideas are crack-pot?).

May I ask a favor of you? I would like to have some of your Silurian stuff from Center County to extract that you know contains spores. Not with the idea of publishing ahead of you or anyone else, but for comparative purposes. I would eventually, of course, like to make note of the material in a large Ms now in preparation stages (or perhaps late preparation stages is more accurate), which deals with the stratigraphic distribution and environment of occurrence of pre-Devonian spores. Most of the Ms is based on occurrences that have been dredged up as a result of the Gray-Boucot efforts, although I have been forced on some occasions to make reference to spore occurrences published by others. Where possible, however, I like to see and verify the specimens, since not everyone in my experience, calls the same objects spores that I do. At the time that it becomes necessary to refer to this material in the Ms, I will kindly acknowledge your help and refer to anyone who might have described and published on this material in the interval.

Someplace I saw a review by you of a special volume dedicated to Isabell Cookson. I cannot find the review, and I have seen no other reference to this volume, which I would like to order for the Library here. Can you give me the complete reference and indicate the cost if you have such information. Do you have a copy of the review-- where did it appear?

Try Earth Science Reviews + get xerox for my records

Love,

Jane

Jane Gray

JG:rb

March 5, 1969

Dr. Jane Gray
University of Oregon
Museum of Natural History
Eugene, Oregon 97403

Dear Jane:

It is perhaps superfluous to be one's own PR man, but in an effort to leave no stones unturned, I thought it wise to state modestly that graduate students in palynology are being trained here under my direction, and that we would therefore appreciate being grafted into the grapevine along which information about various job opportunities is said to travel.

Also, I would appreciate it if you would consider advising prospective graduate students in palynology who want to sample education elsewhere to keep the possibility of Penn State in mind. We have laboratory and library facilities as good as any, located in the new Deike Building (College of Earth & Mineral Sciences). The personnel and library of the Catalog of Fossil Spores and Pollen and our "palynological data project", as well as the various researchpprojects, make ours a stimulating place palynologically. We now have three full-time professional palynologists (H.T. Ames, R.B. Sanders, A. Traverse), as well as a paleobotanist with a strong collateral interest in palynology (W. Spackman). Spackman and I now both have joint appointments in biology and geology, so graduate students can organize work here toward degrees in palynology-paleobotany in either biology or geology. In terms of flexibility of requirements, this is a very desirable arrangement. For example, a botanically oriented palynologist can now enroll in biology at Penn State and does not need to satisfy the various local requirements for a degree in geology. Yet such a student has available to him at Penn State first class instruction, facilities and contacts in all those area of geology which may be pertinent to his work and interests.

Please keep us in mind.

Yours very truly,

Alfred Traverse
Associate Professor of Geology
Editor
Catalog of Fossil Spores and Pollen

UNIVERSITY OF OREGON

Museum of
Natural History

EUGENE, OREGON 97403
telephone (code 503) 342-1411

February 7, 1969

Dr. Alfred Traverse
Department of Geology and Geophysics
The Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

My conscience has been pricking me for some time, although not sufficiently to cause me to spring into action. I hope that you will forgive your "dear old friend."

I doubt that I will attend the Botanical Congress because of other matters pending. However, if you would like to come down to Eugene I would be very glad to see you, and show you the lab and possibly some of the local paleobotany-- although you may be "paleobotanied out" by that time.

Best thanks for the information on the pollen per gram questions. It does for the time being answer my questions. I have had to temporarily stop this work anyway because of the shortage of funds-- although hope to be able to get back to it next year.

The above statement more or less goes for everything else too-- and especially the computor program. My grant for this work was very small-- ca. $5,000-- but I regard it as adequate to do the job. I don't know what you heard at the AIBS or from whom, -- I had not mentioned this to anyone, although I have had the grant from NSF for quite awhile. My objective is simply to be able to retrieve morphological data from my modern pollen collection as it continues to expand. The same system will be usable for fossil, of course. I may add in time a few gimmics such as stratigraphic data, geographic distribution, etc., but my main objective now is to be able to pick out morphological information accurately and quickly. If it seems successful and worthwhile I had intended to make the program public, but would not at this time be much help to you I'm afraid. In the last Pollen et Spores you probably saw the program for the same sort of thing described there. My basic problem at the moment (besides lack of money) is that I've got to do for myself what teams of people are doing elsewhere, and there are only so many hours in the day.... To answer your question I do have a continuing interest in this area and perhaps I will have made some progress by August.

Please do forgive me for the delay and then for being so little help.

With all best wishes and warmest regards.

Jane

Jane Gray
Curator of Paleobotany
Associate Professor of Biology

JG:rb

September 23, 1968

Dr. Jane Gray
Museum of Natural History
Eugene, Oregon 97403

Dear Jane:

How pleasant to hear from you again! I thought of you during the recent AIBS paleobotany meetings in Columbus--kind of thought you might be there. I do hope that, at the very least, Eugene isn't so far from Seattle as to preclude my seeing your setup while in your neighborhood (??) for the Botanical Congress next year.

I'll do my best to answer your query re pollen per gram of sediment, which I think is still the best practical way of telling the "true" amount of pollen in a sediment. (This means that I regard M. Davi's APF as an elegant technique but highly impractical.)

1. I sometimes dry the original sediment, sometimes don't, depending on circumstances. It is generally desirable, and I generally do it. Drying at this stage doesn't bother the pollen, I feel pretty sure--though I don't have data to prove it.

2. However that may be, the amount of water, or the amount of glycerin jelly, or the amount of anything else in either the final residue or on the final slide is not relevant. The technique, as I guess you know, involves weighing the amount of residue on the final slidesand then computing the proportion that represents of the total residue. [The residue on the slide and residue in the vial contain, I presume, the same proportion of the original sediment or rock sample that is represented on the slide. In other words, one assumes that the final residue after maceration, etc., is the total available residue. I suppose that assumption does in some cases not correspond with reality (as the diplomats say), but I have evidence (unpublished--would it be worth publishing?) that the results are consistent for duplicates. You are doubtless right in saying that the results are approximations, but I feel pretty safe that when I say that the ppgs, for x-coal is 100,000 and for y-shale is 1,000, that I am in=the right general area in each case.

May I shift gears? You may have heard over the over-efficient grapevine that I was persuaded by the AGI and NSF to take on the job of developing a publicly available, computer-based system for palynological data storage and retrieval. Got a sizable grant for the purpose, about which I am duly nervous. But never mind that--I heard at AIBS that you have worked up a method for classifying the morphological variations of pollen. That sounds pretty vague, but that's because I'm vague about it! I suppose it's an extension of the PV2, PC3 business? Would you be willing to share it with me? Do you have a continuing interest in this area? If so, might you be interested invisiting here for a few weeks sometime to work with us on the problem? I have honorarium money for others but can't travel myself under terms of the grant!

Best wishes, dear old friend.

Yours very truly,

Alfred Traverse
Associate Professor of Geology

AT:kwc

* There are two oil company consortia at work on the same thing.

UNIVERSITY OF OREGON

Museum of
Natural History

EUGENE, OREGON 97403
telephone (code 503) 342-1411

September 3, 1968

Dr. Alfred Traverse
Department of Geology and Geophysics
Pennsylvania State University
University Park, Pennsylvania 16802

Dear Al:

In a recent paper you described a method for determining the number of fossil pollen grains per gram of sediment. You dry moist samples presumably to remove any variable in the weight of the sample as a result of different water retention capacities of the matrix but then the weight of the final, highly polleniferous residue is determined from a wet sample. You disregard the possibility that some variability in the weight of samples might be introduced at this point, by undetectable differences in the amount of water present. What is your reasoning here? Do you think that differences in the amount of water in the residue are likely to be insignificant at this point? If all the samples at the beginning of the processing had been wet, i.e., received in wet condition, and if the matrix had been very similar, such that its water holding capacity had been about the same would you have bothered to dry the samples? Have you noticed or attempted to find out if the drying of the samples in any way effected the preservation of the pollen, i.e., have you noticed any undue amount of breakage in the pollen or anything else that might be attributable to the drying? Finally did you notice or attempt to find out if any of the extraneous organic material in the sample was less readily removed following the drying? I recall a statement, in Faegri and Iversen I believe, in which they state that samples should never be allowed to dry out before preparation because some of the organic constitutents normally readily removed in the standard chemical treatments, now are essentially inert.

My feeling in part about all of these methods for determining pollen grains per gram of sediment is that they are more approximations than anything else. Having done a great deal of checking, I feel that it is virtually impossible not to lose part of the residue in the extraction. Depending on the components of the sample, this can be much worse for some samples than for others.

Warm regards as always,

Jane

Jane Gray
Curator of Paleobotany
Associate Professor of Biology

JG:rb

no answer req'd
file

UNIVERSITY OF OREGON
MUSEUM OF NATURAL HISTORY
EUGENE, OREGON 97403

March 31, 1965

Dr. Alfred Traverse
511 East 47th Street
Austin, Texas

Dear Al:

Sorry there will be no reprints of any of the articles in the Techniques Handbook. The whole point is to raise money for the Paleontological Society. The book is, of course, for sale from the Freeman Company. As yet I have seen no advertisements, and do not know the price. I would imagine that it might be rather expensive.

I have nothing against anyone in Austin one way or another; maintaining grievances or bearing grudges never pays!

I do hope that you will keep in contact with me occasionally after you are ordained and perhaps we may be able to get together one of these days.

Best wishes always,

Jane
Jane Gray

JG:rb

UNIVERSITY OF OREGON
MUSEUM OF NATURAL HISTORY
EUGENE, OREGON

PALEOECOLOGY LABORATORY

July 15, 1963

Dr. Alfred Traverse
511 East 47th Street
Austin 51, Texas

Dear Al:

Best thanks for your note. I had eventually intended to have further correspondence with you concerning silicone oil. You have said nothing in your paper about it, so there is nothing to delete. I am not certain that you can "afford" to ignore silicone oil as you have done-- not even token acknowledgement seems rather poor treatment for what is apparently the most promising new mounting medium to make its appearance in a long time.

You did mention in a note to me earlier that the refractive index is "too high", but since it is 1.4 and just slightly lower than glycerin jelly, you were obviously in error there. Moreover, as you know, silicone oil is not a plastic. It is so inert, that I fail to see how anything can happen to pollen grains mounted in it. In his note for the Techniques volume, Svend Andersen says that he has had unsealed slides in silicone oil since 1958 and that they show no changes at all, including in the size of the grains. I really wonder whether that would be possible for unsealed slides of glycerin jelly. Silicone oil also comes in a wide variety of viscosities. At the moment I am using oil of much higher viscosity than Andersen uses. Thus it seems probable that the stuff would keep and mail quite well, without movement of the coverslip. I will, however, seal all my slides made with silicone oil, as I have been doing with glycerin jelly. I would like to know, however, the substance of the recent conversations to which you refer in your letter, and with whom you had them (so that I may draw my own conclusions concerning their reliability) and how much experience the persons in question have had using silicone oil with pollen. Afterall, most of us now using the stuff have a great deal at stake and I think it only fair to us all that if faults with the oil exists that they be brought out as quickly as possible.

I have made a few very minor changes in the paper. Otherwise it is now ready to go as is.

I am very sorry that you will not even look over my contribution to the Handbook. I did not intend that you really do a serious editing job. I am trying however, an entirely different approach, by trying to take some of the hokus-pokus out of the extraction, instead of simply repeating extraction schedules without an indication of why they are performed. I am not certain how successful this will be, since it seems likely that most people merely want to know what to do, rather than why to do it. But I do understand your problems.

With best wishes, as ever,

Jane

UNIVERSITY OF OREGON
MUSEUM OF NATURAL HISTORY
EUGENE, OREGON

PALEOECOLOGY LABORATORY

June 10, 1963

Dr. Alfred Traverse
511 East 47th Street
Austin 51, Texas

Dear Al:

I have just arrived home (about 3 hours earlier) from a 10 day field trip to find your manuscript and letter and the earlier paper returned to you. I've now finished reading the re-write hurriedly, and really do find it a big improvement (even if you don't want to throw any kudos in my direction, in stressing that you have accepted mainly my minor (underlined) suggestions). Come now friend and admit, that someone's comments even if they are silly or indicate confused thinking, do help one to think more clearly about what he is writing. I feel certain that anything that I have to say now with regard to the paper will be minor, but if on further more careful reading I have any comments or suggestions you can count on hearing from me again. Incidently I did not find any of your comments particularly acid-- in fact I enjoyed them all. I hope that you did not take my comments as being acid-- I did not intend them that way-- they were made really only in the interests of clarity and in some cases brevity in writing. I have no criticism at all of the fact that you did not discuss all variations of all techniques. I have been forced to write the section on extraction techniques (because I could get no one else to do so), and am also attempting to synthesize rather than to endlessly repeat schedules which merely incorporate minor variations in methods. That is my main criticism with Browns' volume-- which in my opinion loses much of its value for the beginner, just because there is no attempt to cut out the underbrush and leave the trees standing.

As for the glycerine jelly, silicone oil business I am in a quandry. From what I have been able to read, the refractive index is not as unfavorable as you would make out. Moreover, some pollen workers seem to think this is a great advantage in providing greater contrast for the grains. In fact in a contribution for the volume written by Svend Andersen, the merits of silicone oil are highly extolled (at least the section on pollen techniques will not be free from minor controversy-- it will make for more interesting reading surely). For the moment what I plan for all future modern pollen slides is to mount one slide in glycerine jelly and one in silicone oil, for comparative purposes. I do also plan to store my stuff in silicone oil, just because of some question, (Cushing vs. Traverse) concerning what happens to pollen in glycerine jelly. I am also in the process of taking the glycerine jelly out of all my old stored vials and converting to silicone oil. Another 10 - 15 years and we will surely know whether that is a mistake! I also hear from Lucy Cranwell (Smith) who had it via the grape vine, that Faegri is sold on silicone oil, although Iversen still has some doubts. At any rate your rewrite of the discussion leaves no doubt that you disagree with Cushing, and that is important to get on record in such a way that it is clear what you have in mind. How about giving me your recipe for glycerine jelly, by the way? I want to know what you consider "properly made". As for the hot water, KOH method of dissolving "rubbery glycerine jelly", how much water and how much KOH, and what solution of KOH-- 10%?

As for your other comments-- it goes without saying that you are the author, and that your style should prevail unless there is reason to think that it could be improved. We all have pet expressions and ways of approaching things which others do not appreciate-- you will admit that it is difficult not to get carried away sometimes when reading someone elses manuscripts. But after all Al, we both are on the same side since I did solicit the article from you and would not have done so had I reason to feel antagonistic to what you would write.

I surely hope that you will go to Edinburgh-- I understand that there is a possibility that we may participate in the same symposium.

With all best wishes, as always.

Jane

Jane Gray

JG:rb

I would like very much to have another pollen worker read my section for the Techniques Hand book for comments & criticism. Would you have just a little time this summer so that you could manage to do it for me? I would really appreciate your comments - - -

JG.

May-63

Dear Al,

The enclosed letter was written before I had your address and note of the 26th.

I certainly am glad to hear that things are working out, and that you are managing to keep up with pollen work (you may be getting more done than the rest of us!).

I have somewhat chewed up the manuscript-- I hope that you will not be annoyed. Except for my beginning remarks most of the suggested changes, however, are fairly minor. I don't thing that it will take too much of your time to go through the thing. As I have indicated in the letter, I will have the ms finally typed up again here if there is no way for you to get it done.

Yes, I would agree that Austin is an improvement over Houston-- but you should see the Pacific Northwest. This is really magnificant country, although the inhabitants are a little conservative for my tastes.

Best always,

Jane Gray

[JG modified from J.I.]

Preparation of Modern Pollen for Reference Slides

1. Place flower(s) in evaporating dish or 50 ml. beaker with 10-15 ml. of KOH (10%).

2. Boil on hot plate exactly 60 seconds.

3. Remove from hot place and pour contents through small porcelain sieve (Coors 3A) into a second beaker or evaporating dish. Rinse sieve (over the dish) with distilled water to wash through all pollen.

4. Transfer KOH with pollen in colution to a 15 ml. centrifuge tube.

5. Centrifuge pollen down, decant the KOH.

6. Rinse two times with water, stirring thoroughly, centrifuging and decanting between each rinse.

7. Add 10-15 cc. glacial acetic acid. Stir and centrifuge again, decanting the liquid.

8. Have water boiling before this step. Make solution of 1 part sulphuric acid: 9 parts of acetic anhydride. Measure exactly. Add the sulphuric to the acetic anhydride. Heat will be generated. Mix as rapidly as possible and rapidly pour into the organic residue at the bottom of the centrifuge tube. Stir. (Be sure that all stirring instruments are absolutely dry and avoid contact of acetolysis mixture with water.)

9. Rapidly transfer tube to boiling water bath. Boil exactly 60 seconds and remove tube from water bath.

10. Centrifuge, and decant carefully into waste beaker.

11. Rinse, stir with distilled water, centrifuge and decant two times. Finally, rinse with a solution of 1:1 glycerine and water. Before cnetrifuging this last solution, heat for one minute in a hot (but not boiling) water bath. Then centrifuge for 10 minutes. Check to make certain all pollen is down from this more viscous solution.

12. Decant glycerine and water solution and turn tube up on small piece of filter paper to drain. Can be left overnight or for several days to dry.

13. Add small piece of glycerine jelly to dried pollen and place in hot, but not boiling, water bath. Stir carefully to mix, but try to avoid air bubbles when stirring.

14. With glass rod (clean) or spatula, dip out small amount of pollen and place on cleaned cover slip (wash all cover slips and slides in alcohol, and dry). Invert cover slip on glass slide. Set aside to dry thoroughly -- making sure glycerine jelly sets before cleaning and sealing.

Essentially as learned in Iversen lab —

See Brorson Christensen; 1946 Measurement as a means of Identifying Fossil Pollen. Dan. Geol. Undersøg. IV. Raekke, Bd. 3 Nr. 2 for comments on effect of treatment on appearance of grains, 29 May, 1963

Dr. Jane Gray
Paleoecology Laboratory
Museum of Natural History
University of Oregon
Eugene, Oregon

Dear Jane:

Here I am, right on my promised schedule. I am returning both the original manuscript, with some red-ink comments on your comments. These are occasionally rather acid, but you know me and won't mind.

Your minor suggestions I accept almost 100%. Two heads are better than one.

I don't remember what I told you in the original transmittal letter, and don't have my letter files unpacked as yet (that's a summer project, Greek and Hebrew studies permitting!). So, I'll risk boring you by commenting on my original modus operandi. I began by assembling techniques from everybody. At the end of it, it was obvious that the techniques fell into families, only three of which are really significant. That is why the paper is not truly encyclopedic. For the purpose for which this is intended, it seemed rational to provide only the major methods, but with enough literature references (e. g., to Brown), so that anybody could find out about the other procedures. That is why Grayson's procedure is not outlined as such. It is in the Faegri family. The Gray-Iversen procedure I have now mentioned on p. 13 in what I think is the appropriate way--calling attention to its major difference from what the reader already has before him.

The second general thing I should relate, if I haven't already, is that I wrote the paper to be an independent contribution. In case you had chopped it up too much I had proposed to publish it separately somewhere. That accounts for the introductory bit about pollen in general. If you are going to have such a blurb at the beginning of your section, of course you won't want to repeat it in front of the modern pollen sub-section.

Also in connection with the status of the paper as an original contribution, I am using this means of publishing my aluminum block innovation; I think this is one of the best technique improvements I've ever made. I am also putting in my observations vis a vis Cushin glycerin jelly. (Incidentally, I really both agree and disagree with C.: he was really observing that it is not the glycerin jelly alone that causes the degradation when he observed that the phenomenon is related to slide thickness. He made that very telling observation, but didn't go far enough in drawing conclusion .

Now, then, to comment on recommended changes that I don't feel disposed to agree with about:

1. I put all the schedules at the end on purpose, as I regard all the rest at explanatory narrative, including the bit on slide-making. I think it would chop the thing up to put explanatory narrative both before and after the schedules. Please see it my way!! All of the explanatory material bears on the schedules.

2. I think the single-grain blurb should stay. It is used for modern pollen--especially for preparing an isolated grain for various sorts of study (e. g., various infra-red, etc., studies).

3. Don't begrudge me my kudo for Erdtman. I am the author, after all. He and his brother did publish the original paper on acetylation for palynology, and he was mostly responsible for getting it established. No getting away from the fact.

4. I didn't quote the B. Christensen paper for the same reason that I minimized all technical references. (For example, the references to the original Erdtman and Erdtman acetylation paper.) There would be no end to it.

5. I don't feel disposed to say more about silicone oil than I have. In Erdtman's schedule I say all that he said, as far as I can recall. As far as its use as a mounting medium is concerned, I had its index of refraction run, and that was all that I had to know. Glycerin jelly is still the best. And if it's properly made, it is essentially permament. How permanent is permanent? Who has a fifteen-hear old silicone oil slide to compare with my fifteen-year old glycerin jelly mounts? (I wonder if I ought not to attempt a paper on my really extensive study of various plastics? Tschudy liked one that I developed (a mixture of things), but it still wasn't as good as plain old g. j.)

It will be important to carry the Shell designation in your footnote, as per the title pagge. There is no point of giving my present address, as it is strictly temporary. Shell will forward everything, wherever I happen to be.

My typing is not up to the electric machines, but I think is adequate. The type is bigger, so the number of pages is perhaps one page longer than it would be with the small type.

I finished out the term here barely alive. The work is much more demanding of my time than graduate school. Or am I just older? I finished on top of the class quite comfortably--with only 12, that's not much of a distinction.

Sure would be good to see you again. I suppose, all things considered, it's not likely you'll be coming to Austin much. I don't look like getting to Oregon in the near future, if you'll excuse my slang. Only part of the country I've never seen. What's the pictur on unconverted, convertible heathens up there?

Yours with affectionate best regards, as ever,

Al

UNIVERSITY OF OREGON
MUSEUM OF NATURAL HISTORY
EUGENE, OREGON

PALEOECOLOGY LABORATORY

May 1, 1963

Dr. Alfred Traverse
511 East 47th Street
Austin 51, Texas

Dear Al:

What's up? I'm waiting still for some agonized outbursts from you with regard to the returned manuscript, but the silence is even worse. I hope that you will return the altered copy to me as soon as possible. Don't worry about the typing. As I have already indicated, I have several sources for getting that done.

I appreciate how busy you are, of course!

With regards to Bette.

Sincerely yours,

Jane

Jane Gray

JG:rb

UNIVERSITY OF OREGON
MUSEUM OF NATURAL HISTORY
EUGENE, OREGON

PALEOECOLOGY LABORATORY

March 25, 1963

Dr. Alfred Traverse
511 East 47th Street
Austin 51, Texas

Dear Al:

I have finally had an opportunity to go through your manuscript with great care. Most of the comments liberally sprinkled throughout are self-explanatory. I have suggested deleting the entire first part on pollen and spores (since I had planned something of that sort for the Preface to the section) and substituting an introduction something along the lines as I have written. Some of this was taken from your original ms. My effort is not polished at all, and I intended that you should use it only as a suggestion and work something out in a similar vein. I have suggested some other re-organization elsewhere in the paper. I have also suggested that you look up an article by Brorson Christensen, if you are not familiar with it. I have indicated the reference on the mimeographed sheet enclosed. I think that some of the data discussed in Christensens' paper might be mentioned beneficially in your article--what do you think? I have also suggested that you should re-examine Cushing. Some of your statements contradict his, and I don't know whether you intended that or not.

I have kept here the photographs, and the first couple of pages of the manuscript. If you have no way to get this typed after you have worked it over except through expense to yourself, please let me know. I will have it done here.

The paper as a whole was fine, and certainly adequately covered the topic. If there is anything that you would like to discuss concerning my comments please let me hear from you, although I will be somewhat handicapped in the discussion since I lack a copy of the paper. Two copies of it were supposed to have been sent originally, I believe.

With all best wishes.

Sincerely yours,

Jane

Jane Gray
Adjunct Assistant Professor
(Biology)

Enclosure

JG:rb

UNIVERSITY OF OREGON
MUSEUM OF NATURAL HISTORY
EUGENE, OREGON

August 8, 1962

Dear Al,

I have heard from Paul of your future plans. I'm sorry that you did not tell me directly (since we have had recent correspondence), and that I had to hear the news round about. I thought we were better friends than that.

I think I can understand your motives well enough, although perhaps you are even more complex than the most complex of your acquaintances. I much admire your courage in making such a decision to enter the seminary. I'm sure that it was not an easy decision for you and Betty to make. There's lots more that one could say - but expressing it is much too difficult.

I hope that this move will bring you real happiness and that I might have the chance to see you once in a while in the future.

With all best wishes

Jane Gray

THE UNIVERSITY OF ARIZONA

TUCSON

GEOCHRONOLOGY LABORATORIES

October 31, 1961

Dr. Alfred Traverse
Shell Development Company
3737 Bellaire Blvd.
Houston 25, Texas

Dear Al:

As you may or may not have heard, the Paleontological Society is sponsoring a special volume dealing with paleontological techniques. This is to be published some time next year by Freeman Company, I believe. The enclosed mimeographed sheets will explain in a little detail what we are about. I am responsible for the section on Pollen and Spores, which I've more or less divided up into several subtopics as briefly indicated on the sheets.

* Not indicated, by an oversight, but to be included, is a section on preparing modern pollen for comparative purposes. As you know, there are a great many methods from the simple KOH techniques to very highly elaborate techniques such as the one proposed by Faegri in a 1954 issue of the old Micropaleontologist and Grayson's somewhat elaborate procedure.

I wonder whether I might count on you to undertake writing up this section for the Techniques Volume? I have been thinking along the lines of a general introduction outlining the necessity for good comparative material, limitations of some methods as regards size and shape alterations, general objectives of the comparative collection, etc. to be followed by a compilation from the literature or private sources (probably more or less in cookbook style) of all the various techniques which are available for processing modern material. You may have other and/or better ideas on how to organize this section.

I would be delighted if you would undertake to organize and write this section, and I hope that I may hear from you very shortly concerning your willingness to do so. You will note that there is much time available, so you needn't feel crowded in that respect.

With best wishes,

Jane

Jane Gray
Research Associate

JG:jw
encl.

ONE HUNDREDTH ANNIVERSARY

1862 ▪ THE LAND-GRANT COLLEGE AND STATE UNIVERSITY SYSTEM ▪ 1962

UNIVERSITY OF OREGON
MUSEUM OF NATURAL HISTORY
EUGENE, OREGON

Dear Al:

Sorry to miss seeing you in Tucson-- I was looking forward to our periodic get together to review old times. But glad to have your note and am anxious to see the manuscript. I am not worried about the length.

Your last sentence has admirably inflamed my curiosity! Since you gave me <u>no reason</u> at all for not coming to the meeting, your proposition to divulge the <u>real reason</u> for not coming seems something of a nonsequitur. Paul had originally communicated that it had everything to do with the arrival of the latest Traverse offspring. Is there more to it than that?

With all best wishes,

Jane Gray

THE UNIVERSITY OF ARIZONA
TUCSON

GEOCHRONOLOGY LABORATORIES

December 8, 1961

Dr. Alfred Traverse
Shell Development Company
3737 Bellaire Blvd.
Houston 25, Texas

Dear Al:

Splendid -- I'm delighted to hear that Shell has come through! Per your questions of the first paragraph: didn't I send you along with my first letter a mimeographed pamphlet? On the last page of this document there were all sorts of instructions, including information on the number of pages and the deadline date -- September 30, 1962. I am in short supply of these items now, thus do not inclose one.* However, if you have never received a copy please let me hear from you. The number of pages suggested is 10 typewritten pages. However, if this is going to be inadequate, I have told people to disregard this instruction. There seems little sense in holding to a limit which will negate the reasons for writing the paper in the first place.

You are, of course, free to organize and cover the material as you will. I was told that the techniques volume should be as complete as possible. When I asked whether there should be any editorializing concerning certain methods, no one could imagine what I meant. Soooo I've sort of gone on my merry way. No doubt you have seen Clair Brown's volume on techniques. This seems needlessly repetitious in certain places -- preparation of modern material is one of them. Much of this could be glossed over with a sentence or two, indicating where the variation lies. Perhaps you could take care of most of the techniques that you don't want to go into by simply stating what the possible variations are.

If I can be of any help please let me hear from you, and do tell me whether you ever received the mimeographed sheets.

With best wishes,

Jane

Jane Gray
Research Associate

JG:jw

*P.S. We have dittoed more copies of the instructions and other information and one is enclosed.

THE UNIVERSITY OF ARIZONA
TUCSON

GEOCHRONOLOGY LABORATORIES

November 21, 1961

Dr. Alfred Traverse
Shell Development Company
3737 Bellaire Blvd.
Houston 25, Texas

Dear Al:

Your letter of November 8 was most welcome. However, perhaps it was not overly encouraging. I would like very much to have you do this write-up since I also believe that you are "ideally suited" to attempt it. But let's hope that you receive word from "the Queen" within a reasonable time, since if it is going to be impossible for you, someone else, alas, will have to be approached. At the moment, my thinking has been so undirectional that I can't think of anyone else to even suggest ... sooo.... Surely, Shell couldn't object to this project -- by the treatment of modern pollen you won't be giving away any company secrets.

Hopefully yours,

Jane

Jane Gray
Research Associate

JG:jw

November 28, 1961

Dr. Jane Gray
Geochronology Laboratories
University of Arizona
Tucson, Arizona

Dear Jane:

It is good to be able to report that I have secured permission to write the section on preparation of modern pollen and spores per your invitation of October 31, 1961. At this point I should have some further information from you as to the number of pages this can run too, and the deadline.

This should be fun, though we may disagree mildly about emphasis, etc. For example, I am doubtful about the merit of explaining in detail some of the absurdly complicated techniques - or at least more than one of them.

Looking forward to hearing from you, I am

Very truly yours,

AT

Alfred Traverse

AT:pjh

bc: Technical Files (P. Hyatt)

Information Copies:
Dr. D. V. Higgs
Dr. A. Traverse

Shell Development Company
Exploration and Production Research Division

UNIVERSITY OF ARIZONA
TUCSON

GEOCHRONOLOGY LABORATORIES

June 22, 1960

Dear Al:

Many thanks for your thoughtfulness! We think the picture is pretty fine of all of us (no comment from Schoenwetter ~~is~~ who is away for the summer). The color is the most intriguing thing about it.

No I will not be at the AIBS-- have recently received $24,000 grant from the NSF to continue my pollen work in the Pacific Northwest. Will be in the field for about a month and a half including all of August. Have already but in 3 weeks in company with Chaney and Fry, collecting delicuous looking samples. We're managing to recover pollen from just about everything we touch. Will have my hands full for the next two years, but hope to have some definitive results for middle-late Tertiary sediments of this area.

With best wishes, and many thanks.

Jane

Jane Gray

May 25, 1960

Dr. Jane Gray
Geochronology Laboratories
University of Arizona
Tucson, Arizona

Dear Jane:

It had slipped me that I promised a copy of the "quartet shot" - they turned out quite well and I'll have the copy made. I'm going on vacation this week and you'll probably not get the item until June.

Hope you all are flourishing. Best regards to Ted, Paul, et al.

Very truly yours,

AT

Alfred Traverse

AT:mpd

Shell Development Company
Exploration and Production Research Division

Dear Al:

I have been patiently expecting a copy of the famous quartet shot from the hill. I'm quite willing to pay for a copy if Shell can't afford it.

With best wishes,

Jane Gray Jane

THE UNIVERSITY OF ARIZONA

TUCSON

GEOCHRONOLOGY LABORATORIES

SEVENTY-FIFTH ANNIVERSARY OF FOUNDING

January 22,1960

Dear Al,

Many thanks for your note of Jan. 18, and enclosed kodachromes. The later are now carefully filed away with the rest of my collection of microphotographs of pollen grains. Delighted to have them. Further inspection of the tricolpate, reticulate job from the N. Dakota leonardite has convinced me that it is very similar to some of my Alaska Cretaceous grains, affinities as yet unknown.

Hopefully you will come back to Tucson again before too long and come and have dinner with me-- cooking is included among my other, obvious talents! Hope also you will send a duplicate of the Traverse, Martin, Schoenwetter, Gray photograph taken on Tumamoc Hill-- am most anxious to see it.

With best,wishes,

Jane Gray

UNIVERSITY OF ARIZONA
TUCSON

GEOCHRONOLOGY LABORATORIES

2 December 1958

Dr. Alfred Traverse
Shell Development Company
3737 Bellaire Boulevard
Houston 25, Texas

Dear Al:

Thank you for your note of November 25, with the word of explanation about Dr. Iversen. I had not intended to suggest that you were responsible for an oversight. I simply had a vague recollection that Iversen's name had been brought up in some context, and thought that it had been at this year's meetings, although I was obviously mistaken. There is so much palaver goes on at these meetings that it is sometimes hard to keep track of it all, unless one is temperamentally atoned to it.

Things are very fine here work-wise, and I am enjoying the surroundings in addition. As a bit of advertisement, will mention that Paul and I will teach a year's course in palynology in the fall. If you hear of anyone interested, you might let them know of us.

Best wishes to Betty.

Sincerely yours,

Jane

Jane Gray

JG:hg

November 25, 1958

Dr. Jane Gray
Geochronology Laboratories
University of Arizona
Tucson, Arizona

Dear Jane:

Re your footnote on the ballot: you're apparently thinking of last year. Iversen was nominated last year but not, to the best of my knowledge, this year. However, I should also point out that in the letter to the membership after the Bloomington meetings, I listed the nominees and called for further nominations by mail. So, you could easily have nominated Iversen if you had wished to. Probably would have been a good idea, but I had already nominated Thomas and didn't feel like nominating everyone myself.

Best wishes.

Very truly yours,

AT

Alfred Traverse, Secretary
Paleobotanical Section, B.S.A.

AT:mpd

UNIVERSITY OF ARIZONA

TUCSON

GEOCHRONOLOGY LABORATORIES

September 22, 1958

Dear Al:

Any relation between A. T. and one of the "characters" in Judge Parker must be purely coincidental!

Many thanks for the field trip data, and sorry that there was some confusion caused by Alan's inability to get in touch with you prior to the trip.

Trust by now that you have my change of address card. If not please note as above, for an indefinite time.

Best wishes to Betty,

Friends of A. T. at the Geochronology Laboratories

T. Gray

May 23, 1958

Dr. Jane Gray
Department of Geology
University of Texas
Austin, Texas

Dear Jane:

After conferring with Dr. Frey, who has been working with me in organizing the Fifth National Pollen Conference, we have decided to transfer your interesting-sounding paper, "Cretaceous Plant Microfossils....", to the Pollen Conference. We also transferred several other papers. This leave the Paleozoic spore program on Monday, the 25th of August, and the Pollen Conference, on Thursday, the 28th, opening and closing the Paleobotanical program in a palynological flurry arranged according to the geological time scale! Hope you are agreeable.

Also note that the title you submitted to me differs ~~to~~ from the one Frey got in that you appended "Cretaceous" to the one sent him. I have put the more specific title on the program, but Frey gets last look at it, and there is still time to alter the title, if you wish to write him to that effect.

I have been wondering how your plans are faring and hoping that something can be worked out.

Very truly yours,

Alfred Traverse, Secretary
Paleobotanical Section, B.S.A.

AT:ml

cc: Professor David Frey

Jane Gray

Texas U. Adds Woman To Geology Dept. Staff

Austin—The University of Texas geology department has captured a rarity—its first young woman instructor, Miss Jane Gray.

Miss Gray, who is only 26, did research for her doctor's degree at the University of California, Berkeley. Her teaching and research field is paleontology, the science that investigates life of past geologic periods by examining fossils.

Even more specifically, Miss Gray is a paleobotanist. She is working in a new area of paleontology—palynology—the study of fossilized pollen grains found in the earth's strata.

Miss Gray, with two assistants is building up a "pollen herbarium" for the university's geology department. The students are probing the biology department's herbarium in an effort to get pollen from all available plants.

The role of the women in geology is demanding, she said, requiring "as much perseverance as brains."

"It is not always easy," she added, emphasizing that the best rule is remaining "non-temperamental."

Although born in Nebraska, she regards herself as an Easterner. The daughter of a regular Army colonel, she attended Sawanhaka High School on Long Island, N.Y. She received her bachelor degree from Radcliffe College in 1951, before going abroad on a National Science Foundation predoctoral fellowship to study with an internationally known pollen analyst, Dr. Johannes Iverson, in Copenhagen, Denmark.

May 17, 1957

Miss Jane Gray
Department of Geology
University of Texas
Austin, Texas

Dear Jane:

Thanks for a good, if too short, visit. We ended by not discussing some of the things that I wanted your opinion on. Such as Grayson's proposal, for example. We didn't air that business at all, though it came up several times. I also have some definite questions about your thesis that will have to wait until next we meet. But it was aprofitable meeting, and, naturally, I immensely enjoyed seeing an old friend again.

Do you have the dope on those papers re moss spores handy? I would also appreciate the transliteration of the Russian title, if you find it. (Not the translation, just the transliteration.)

Hoping to see you in Berkeley and/or Palo Alto in August, I am,

Yours very truly,

AT

Alfred Traverse

AT:rlg

Shell Development Company
Exploration and Production Research Division

THE UNIVERSITY OF TEXAS
DEPARTMENT OF GEOLOGY
AUSTIN 12

May 22, 1957

Dear Al:

The Rudolph paper and photostat arrived in good order. Except for the photomicrographs, the photostat copy is in better condition than the original-- thanks a lot.

I'm glad you enjoyed your visit re letters of May 17 to me and to Ellison, and hope that you'll be prompted to come again and more often in the future. I tried, as you will recall to talk shop-- but for some reason, you seemed disposed to put business, aside during this visit. Ihope your specific questions re thesis will keep, however, till this summer, or some other time in the near future. Needless to say I enjoyed our visit and seeing you again. The years seem to have brought about remarkably little change in you. Hopefully, the next ten will enable me to say the same thing. Incidently I am flattered that you were so greatly impressed by the department and "the general vigor of the faculty people" considering that the only other member of the staff with whom you can be said to have had any extended discourse, beside myself, was Ellison.

I have located the transliteration. It is as follows:

Materialy po palinologii i stratigrafii, Vsesoiuznyi geologisch eskii institut, Leningrad .

The above also includes the publishing institution. The moss spore papers are as follows:

McClymont, (1955), Spore studies in the Musci, with special reference to the genus Bruchia, The Bryologist, vol. 58, no. 4, pp. 287 et. seq.

Terasmae, J., (1955), On the spore morphology of some sphagnum species, The Bryologist, vol. 58, no. 4, p. 306 et. seq.

Their coverage is by no means as extensive as it might be, but every little bit is a help.

In exchange for all this information, I'd appreciate it if you would inquire about obtaining for me some of the Recent surface samples that are currently engaging your attention-- and about the possibility in the future, of obtaining well-core samples (pollen-bearing) for instruction purposes. Browning was able to send some material to Barghoorn from Long Beach and if you make it quite clear that the samples are to be used for instruction purposes in an elementary course, there should surely be no serious objection. I'm sure that if you give your support to this project, that few questions would be asked.

Best wishes till August,

Jane

February 21.

Dear RR -

Your letter of the 14th arrived today without application blank for membership in the B.S.A. Somehow it was overlooked by your secretary I imagine.

Chaney wrote me of your visit - I'm glad you two have finally become acquainted. Chaney to my way of thinking is one of the most remarkable and interesting of people - I trust you found him so also. I will always be grateful for our three years of contact and friendship - although we ~~at~~ only really became friends as of last year I'd say (instinctive mistrust of female paleontologists I guess). As a whole one can hardly blame him.

At the moment my schedule is such that it is practically impossible to get away or I'd have been in Houston before this. I hope things will ease up some next year.

Best wishes,
Dana

Pal ~~est~~

February 14, 1957

Dr. Jane Gray
Department of Geology
University of Texas
Austin 12, Texas

Dear Jane:

Just came back from a profitable visit with Chaney and others in that region. I think that we have a good program in prospect for the Stanford meetings, and both Chaney and I are hopeful that you <u>will</u> take part. In fact, I am counting on it.

I enclose the blank for which you asked. Please check the space for our section. You may return the completed blank along with the questionnaire, as you suggest. This will provide a means of notifying me that you have joined us, which I would not ordinarily know until the Society's secretary gave me notice. My experience is that this can be a long process.

Feel free to call me for your seminar whenever you like, though, as you say, next year would probably be better than this year, considering what I could say about my work at the moment.

I appreciate your compliments! Why don't you come down and visit us? As I indicated earlier, I would like to have you come whenever it would be convenient for you.

Yours truly,

AT

Alfred Traverse
Secretary, Paleobotanical Section, B.S.A.

AT:pjd

Shell Development Company
Exploration and Production Division

Gray

THE UNIVERSITY OF TEXAS
DEPARTMENT OF GEOLOGY
AUSTIN 12

January 19, 1956

Dear Al,

Sorry that I've delayed so long in writing to you. I have intended to thank you for suggesting my name to Stanolind, but at the moment, and for the forseeable future, I'm very happily situated here at the University. Am still waiting on equipment, as you also have been in the immediate past, consequently am getting very little in the way of real research done, but have plenty to keep me busy as you will realize. Have spent the past three days marking invertebrate paleontology lab final examinations and lab notebooks and it is a relief to have it finally completely out of the way. Will be tied down with invertebrate and probably micropaleontology (forams, ostracods etc.) next semester, but the following year will give a course in palynology either here or in the botany department. Plan to get a few lectures on pollen into the course on micropaleo with the possibility of weaning away some of the students. Also sometime in the future plan to organize a course in paleobotany, either in this department or botany, but don't know just when yet.

I am very sorry that plans for you to address the seminar here have not materialized. I have not forgotten that I have extended a tentative invitation to you, but things in general are highly disorganized, and since I do not have the final word on getting you down here, nothing has happened. From what you have indicated about the present status of your own research, you no doubt are just as happy about the whole thing, and there is a great deal of time in the future for us to get together.

I am not a member of the B. S. A., but would like to join and should have some time ago. I will be in California this summer and plan to attend the meetings and possibly to present a paper. Chaney mentioned in a recent letter that he had spoken with you, and suggested that I might like to be on the program. This, however, is all very iffy. At any rate, I would appreciate it if you would send me the application blanks, and I will return the ballot-questionnaire with them to you, if that will be satisfactory.

I trust that things are well with you, and your much expanded family (very handsome, I might add) and I look forward to seeing you, if not this next year, then in the summer.

Best wishes,

Jane

THE UNIVERSITY OF TEXAS
DEPARTMENT OF GEOLOGY
AUSTIN 12

October 14, 1956.

Dear Al -

Sorry not to have gotten in touch with you before via other than a change of address card. I've been intending to thank you for your invitation to visit the Shell lab. which I intend to take advantage of one of these days real soon, and, of course, extend to you an invitation to visit us. I hope that perhaps you will care to come up and give a seminar. We have special guest day on Thursdays and since there is much interest here in pollen work you would be very welcome on the program. We can discuss this at greater length if you are willing and make more permanent, and definite arrangements in the near future. In the meantime I trust that you are going to the G.S.A. meeting at Minneapolis and we can meet ~~there~~ there and become reacquainted after all this interim of years. I am looking forward to seeing you again.

Texas weather is certainly frightful after California and I gather it is worse in Houston than in the central area. I'm gradually become adjusted now.

I trust all is well with you and your family -

Best regards,

Jane Gray.

March 8, 1956

Miss Jane Gray
Department of Paleontology
University of California
Berkeley 4, California

Dear Jane:

Here we go again. I think we are really getting things accomplished. Certainly, putting some of these ideas on paper is helpful to me.

You are quite right that nearly all plant species of extant plants are, in effect, organ species. But I must stress the qualification. Species of extant plants are not supposed to be organ species, and they are so, in effect, only because of imperfections in the descriptions. The theory is that the species are species of plants, i.e., whole plants, and an interested person can go to the type material or collect more material from the type area and complete the descriptions. Fossil plant organs, however, are in a philosophically quite different category. With relatively rare exceptions they are just isolated organs (e.g., sporae dispersae!). They are described as organ species and are then, in my opinion, merely organ species, not (necessarily) species of plant. I think my example in the last letter of Nyssa in the Brandon formation is an excellent illustration of organ species that probably never can be linked with plant species of Nyssa.

About eight years ago (he probably doesn't remember it--I was a graduate student) I took a bus trip to Washington and thrashed this whole thing out with James Schopf, relative to my paper on Mesoxylon. Schopf insisted then and I suppose still insists that species such as my Mesoxylon thompsonii are not organ species but species of plants. I can still hear him saying, "Traverse, Mesoxylon thompsonii is not a species of cordaitean stem. It is a species of cordaitean plant." I respect J. M. Schopf tremendously, but as you can see from my preceding paragraph, I am recalcitrant on this point. The fact (which Schopf brought out at the time) that species of extant plants are also described from fragments of plants is irrelevant--it merely means that plant taxonomists are most unfortunately not thorough. Their descriptions can be expanded.

As to your contention that Chaney and the rest have not described organ species, I stick to my guns and wonder whether you understand what I mean by an organ species. I have before me Chaney's work of 1940, Carnegie Inst. Pub. 507, with Hsen Hsu Hu, "A Miocene Flora from Shantung Province, China." To pick an example at random, turn to page 72. Cornus miowalteri new species is most certainly an organ species for leaves of the genus Cornus. ("This species may be distinguished from...by its broader form and smaller average size...") It is clear that Cornus miowalteri is merely a species of leaves, i.e., an organ species. It makes no difference that Chaney may have intended that it be taken as a species in the ordinary sense of the genus *Cornus*. Whatever was Chaney's intention, the species is nevertheless only an organ species, and I strongly suspect that he didn't intend that it be anything else. By the same token *Liquidambar mangelsdorfiana* Trav. is not a new species of *Liquidambar* but a new organ species of *Liquidambar* pollen.

To me, this organ species concept is the key to the whole problem. If one comprehends that species of dispersed fossil pollen are organ species, not to be confused with species of plants, much confusion is pushed aside. Again, I hark back to our *Nyssa* problem. As long as we thought we were coining new specific names for *Nyssa* plant species we were in trouble, for we knew that it's quite likely that one of our pollen forms equals two or three of the fruit forms. Realize that all of the forms are organ species, and this difficulty falls away.

Please do keep me informed of your progress. I would welcome further correspondence on this fascinating subject.

Yours very truly,

AT

Alfred Traverse

AT:lbd

Shell Development Company
Exploration and Production Research Division

March 3, 1956

Dear Al:

Thanks very much for the Bureau of Mines leaflet, and for your very eloquent, but somewhat unconvincing letter of February 21. I'm happy to hear that your "understanding of the situation in Tertiary palynology is clearer than ever before", since the more I study and see and think, the more confused the whole matter becomes. I hope that this is attributable in part, at least, to a difference in temperment, rather than to intellect, since I would hate to think that the cards were so unevenly stacked. But to be serious--or at least semi-serious--

With reference to organ species, an excerpt from a recent letter from Dr. Just is highly relevant to my point of view.

> As I look at the whole matter, two aspects are intimately linked, hard to separate, but definitely in need of separation while the material is being worked, namely identification and systematic treatment vs. nomenclature or names to be assigned to fossils being studied. Dr. Schopf has pointed out that for most part you may well deal with novelties. Have you ever taken the time to read many descriptions of new species of plants, especially of those from the tropics? You will be amazed to find that none, or few, contain data on pollen, seeds, or the like. The only botanist who ever recommended complete descriptions of plants, as far as I know, is the late Professor J. W. Moll, of Groningen, whose Phytography as a Fine Art appeared in 1934 (E. J. Brill, Leyden). He called these complete descriptions pen-portraits. Professor I. W. Bailey and his students have approached this goal most closely. In other words, the majority of species are inadequately known, the details of organs or parts of their life history remaining to be filled in by those who come later and care less about how many novelties they can describe. Completing such descriptions is fairly easy when the plant is known or the material can be backed by voucher specimens. The paleobotanist is less fortunate. He has to work with fragmentary material and knowledge, an excruciating existence at times, but one with its own fascinations and satisfaction. Many neo-botanists find it easier to describe a novelty rather than monograph a whole genus or family to find out whether or not their material has been described before. To be sure, this saves time, but it also increases greatly the number of names that have to be disposed of by some later workers.

In other words, and very simply, all botanic species are organ(s) species; the line, indeed if we can even so term it, which demarcates true botanic species from organ species must be drawn with the faintest of strokes. Your utilization of a Paleozoic and Mesozoic paleobotanical concept to suit your immediate needs, while, of course, not forbidden by the International Rules (it probably hadn't occurred to them to forbid it) does not in my opinion absolve the problems of Tertiary pollen work, but greatly magnifies them for those who will deal with synonomy to follow. Furthermore, let us be quite clear, on a matter in which you are mistaken--none of the Tertiary Paleobotanists from this part of the world, or elsewhere with few exceptions, describe organ species. Chaney has never in any of his publications described an organ species. A perusal of some of the literature will readily convince you that this is the case. This, of course harks back to the original point, that one must not confuse the issue of describing species from leaves (or other isolated parts, including pollen) with that of making organ species simply of morphological variants of these parts. And, of course, in all cases where the relationship of isolated parts can be readily established with modern material, they are called by the specific name already establish- for the first of the detached parts to receive this name. They are not in organic connection to be sure, but to paraphrase R. W. Brown--the evidence is so convincing that they may have been connected that little doubt remains as to the unity of their identy. E. W. Berry has indeed described organ species--one of the few of the modern school of American Tertiary paleobotany who has done so with extant genera. But like Potonie and Kremp, I feel that such action is not entirely in the best interests, although I readily admit its expediency in the problems which we jointly face every day.

As a final remark, let me add that I think that you are worrying unnecessarily about the possibilities of any ofyour species being "reduced to synonomy with items published by the Germans". It seems hardly likely from evidence now at hand that we deal with species in common intercontinentally, any more in the Tertiary than at present. There are a few cosmopolitan species to be sure, but very few. Realizing the closeness of the relationship between the Eastern United States and Asia, botanically speaking, Asa Gray labored for years under the belief that we were dealing with species in common. Subsequent studies, however, have proven that despite the fantastically close relationship there's not a species in common. Any proposal to conclude at this stage of the game, that we are dealing with species intercontinentally in common in the Tertiary, should certainly be viewed with extreme schepticism. Certainly at least until we can be sure that we deal with distinct species (fossil and modern). How can we ever know that we have actual species in common to two widely separated areas, if we must deal with organ species even from a single locality, which may or may not coincide in their limitations with actual botanic species?

With very best wishes, and hoping that indeed we can get together sometime in the near future.

Very sincerely yours,

Jane

February 21, 1956

Miss Jane Gray
Department of Paleontology
University of California
Berkeley 4, California

Dear Jane:

Your interesting letter deserves a voluminous reply, but I shall try to condense what I would like to say. It is regrettable that we couldn't get together for a few hours, as I think we could come to some important points of agreement. As a result of recent conferences I have had with Couper, Faegri and Erdtman, as well as extensive correspondence with Cookson, I think my understanding of the situation in Tertiary palynology is clearer than ever before.

First, about the word "sporomorph". It was not my intention to use the word in the Cookson sense, of something similar to a species but distinct from it. I used the word in a general sense only. I regret that I used the word at all, but it is (I think) clear from my descriptions that I was ~~not~~ describing new organ species. Incidentally, as a result of correspondence with me (she says) Cookson is dropping her sporomorph-sporotype usage. Her units are perfectly good genera and species, and Couper, as you may know, has validated many of them by simply republishing them as "Cookson ex Couper". Faegri, who is very conversant with International Code, feels that it is doubtful that validation was required, though it is still an open question. I am sending you a little paper of mine on this issue, among others; please note, however, that I was not right in stating therein that Miss Cookson's lack of typification was crucial. The rules, most unfortunately, did not require typification.

Secondly, let us be clear about organ species and genera and form species and genera. All of my species are organ species, with one exception, which is a form species. The difference is that form species and genera are applied when virtually nothing is known about relationship: e.g., the genus Tetradopollenites Pflug. Potonie and Kremp are unhappy with me for creating organ species for recent genera, but they admit that the International Code does not forbid it. In fact, Chaney and dozens of other paleobotanists have been doing it for megafossils for years. But please remember that they are organ species. For example, I have not made a new species of Gordonia but only a new organ species thereof! I think if you follow my reasoning on this point, you will understand what I have done.

I readily agree that it is entirely possible that I have overspeciated and underspeciated (more probably overspeciated in most instances). It is also quite possible that Siltaria does not belong in the Fagaceae. I could only use my best judgement at the time, and many of the decisions were most difficult. The International Code makes ample provision for transfer of taxa. If you (or I, or anyone else) believe that Siltaria should be transferred out of the Fagaceae or that the Quercus species should be combined, all you need do is publish a "Revision of the Microfossils of the Brandon Lignite."

To be honest, what concerns me far more is the likelihood that many of my things can be reduced to synonomy with items published by the Germans. In 1951 it was impossible to make comparisons with the German things, other than by illustration. Potonie is just now recovering some of his original type slides, for example. But if synonomy becomes a factor, that is also provided for in the rules.

Perhaps a word about one of the specific examples you mentioned would be helpful. About Liquidambar: what I meant was that my observation of variability in the pollen of the modern species made it seem possible that both of my organ species of pollen (sporomorphs, I unfortunately said) could be produced by one species of plant. This points up the importance of understanding that microfossil species are organ species. To restate, I have created two organ species of Liquidambar, which are, in my judgement, separable. But I pointed out to the reader that these two organ species, while distinct, may not represent two actual species. On the other hand, I have made three organ species of Nyssa pollen. Last time I talked with Professor Barghoorn, he had some eight (or more probably) organ species of Nyssa fruits. Does that mean that these twelve organ species were produced by four, or eight, or five actual species? Nobody knows nor likely ever will know. We paleobotanists must deal with organ species, which sometimes are coterminous with true species. For example, in addition to pollen, we have an organ species of Gordonia capsule, one of Gordonia wood, and one of Gordonia flowers. It looks clear that there is only one actual species of Gordonia involved.

It would be well at this point to quote and emphasize a statement from the International Code of Botanical Nomenclature adopted by the 7th International Botanical Congress (1950): "The purpose of giving a name to a taxon is not to indicate its characters or history, but to supply a means of referring to it." (Article 9, p. 14). If you make that a part of your thinking on this subject, you are closer to an understanding of the principles involved than 95% of the world's paleontologists.

If you have further questions, let's have them. With best wishes, I am,

Very truly yours,

AT

Alfred Traverse

AT:hmp

Shell Development Company
Exploration and Production Research

not actually a carbon copy - but all of the substance of discussion that follows.

February 15, 1956

Dear Al:

I have been having some correspondence with Schopf and Just concerning taxonomic procedures when dealing with pollen. Somethings have come up which necessitates this letter to you and a carbon copy is being sent shortly to Schopf. Again I am in the position of having to ask you to explain yourself to my satisfaction. If my stand on these matters is wrong I shall be glad to be put in the right before my thinking has advanced further on the subject.

There are a number of points re taxonomy in your report which do not seem to me to conform with what would be best practice. I have selected a few examples, simply as a matter of discussion.

Firstly, I cannot agree with you on the matter of the creation of two new genera, on the basis on which they were done, and the taxonomic procedure following. In the case of Siltaria for example, it seems to me that the logic involved in assuming that a mixture of mutually exclusive characters (in Quercus and Castanea) on a single grain (Siltaria) necessarily means that this pollen even belongs to a member of the Fagaceae, is untenable. If its affinities in view of the present limited amount of knowledge of pollen, suggest that it may be a member of the Fagaceae, then it would have seemed wiser to indicate such doubt as to its true affinities by the creation of some sort of form genus name involving the family name, which would suggest that its affinities are more of an assumption than a certainty. This while not entirely a taxonomic problem does involve the creation of a new genus following "botanic taxonomic norm" and hence is a serious matter.

A number of cases arise in the determination of species. Three notable ones involve:

1. Liquidambar: two species of Liquidambar are described in a manner that indicates no essential dissimilarity in the grains involved. The comment following in part: " Liquidambar styraciflua L. has forms that resemble each of these sporomorphs, and it is possible that these two apparently distinct sporomorphs are variant pollen types of a single speciesof Liquidambar." (p. 54)

If modern comparative material displays a range of variation comparable to that of fossil material, what is to be gained by the creation of two species, with doubtful lines of demarcation? In the case of Cyrilla on the other hand, where only one species is described, this stand is taken: " that this is within the range of possibility of pollen for a single species... ... it is possible that several species of the Cyrillaceae are represented." (p. 57)

2. Quercus: Quercus will probably always create a difficult problem for pollen workers as you know. Seven species of Quercus were created which as described seem to indicate recognizable differences. However, in a note to follow this comment is given:

"The sporomorphs for oak pollen, as determined for this report, may be artificial, with two sporomorphs representing variants of a single plant or one sporomorph being produced by different species." (p. 51)

Would not the creation of a form species name have been more appropriate,

i. e. Quercus albapites, for example, with a discussion, or chart (or both) showing the variants, which perhaps might have been described as forms or varieties, if the need was felt for separation?

3. Ilex: Five species are recognized that seem distinguishable from the pictures and descriptions. A note follows:

"As xis true also for Quercus, there is great range of form in the Ilex pollen of the deposit, and a careful study was made to determine the sporomorphs that can be distinguished. The number of Ilex sporomorphs described does not means that all of the Ilex pollen in the deposit was necessarily produced by 5 species of Ilex, but only that the author finds there to be 5 distinct forms of Ilex pollen." (p. 59

There are numerous other cases of this sort, and my approach to all of them, I think, would be to describe and chart the variability in some manner, but not to make species out of forms that I am unsure warrent specific distinction. Naturally one is limited as to the amount of comparable modern material than can be seen, but in cases, for example as the above, were only a few species of modern Ilex pollen could be examined, one can determine at least if the variation seen in the fossil forms seems to be comparable with what variations exist in the modern forms. If the ~~fa~~ amount of variation of the fossil material warrents specific distinction from what is known of the modern, than species should be created with no apology. If uncertainly exists, on the other hand, that the variations are of specific category, then I don't think they should be given specific rank. This is the kind of problem that arises all the time concering palynological work in the oil companies, where knowledge of modern pollen is limited or lacking entirely, and entities are separated on characteristics which are of doubtful specific value. The sole purpose of modern comparative collections, in addition to identification of material, of course, should be for their value in establishing those characteristics that are of specific category, to facilitate work with fossil material. Where differentiations do not exist in modern material, then we must go easy with fossil material.

I don't regard these points as quibbling-- all are serious matters, for as Schopf has said to me in one letter (this is simply an apt comment, has no connections whatsoever with your work or anything to do with it)-- "If standards are relaxed in some instances, any determinations that an author makes are apt to be lightly regarded. Laxity in species identification cheapens and stultifies the whole systematic procedure."

Sporomorph as defined by Erdtman is as follows and well known to you of course:
"Genetically fixed pollen or spore types of a plant species may, if necessary, be referred to as 'sporomorphae'... The terms sporomorpha and sporomorphidium can be used in an abstract as well as in a concrete sense(as designations of spore types, or of individual spores belonging to these types). In palynological investigations of fossil material (particularly Tertiary and older), it is often impossible to make a distinction between sporomorphae and sporomorphidia. Morphologically different 'sporomorphs' may sometimes represent only one species. On the other hand the representatives of a certain 'sporomorpha' may not at all be the product of one species only; in spite of ther uniform appearance they may just as well be the product of several species or come from plants belonging to different genera or even to different families." (Erdtman, 1947 etc.)

If you have used the word throughout the report in the last sense, rather than the first, as it would seem from usage in ~~xxxxxx~~ various places, then it would seem that it has been inappropriate to follow "botanic taxonomic norms" in attaching specific names to your material following the "same procedure that would be followed with specifically distinct material collected from modern plants." (p. 90) If such material represents sporomorphs than some allowance should be made for this in the application of specific names.

Perhaps they should be called "new sporomorphs" as Cookson does, rather than "new species".

Thanks once again for your trouble,

Sincerely yours,

Jane

Dept. of Paleo.
University of California
Berkeley 4, California

Dear Al:

I thank you for sending me a copy of your paper on "Occurrence of the oil-forming alga Botryococcus in lignites and other Tertiary sediments." and, of course your letter of last week--I had seen Cookson's stuff on Bot., Pediastrum etc. some time ago but thanks for mentioning it. Hoffmeister's "coolness" I would say, was probably due to my having, as my friend Dr. Proskauer, down in Botany put ~~Sincerely yours~~, it, "the wrong genes".

Best wishes,

The slip "Taxes" for texas is no doubt seasonal -

Jane

February 4, 1956

Miss JANE GRAY,
Department of Paleontology,
University of California,
B E R K E L E Y 4, California
U.S.A.

The Hague, December 29th, 1955

Dear Jane:

Thanks for your letter of 10 Dec., which came here during my recent side-trip to Krefeld, to see Potonié, et al, and to Britain to see Simpson, Godwin, and others. I am now getting ready to head back to the States (via air on 2 January). I plan to answer your letter in proper detail, when I get straightened out in Houston in perhaps three weeks. This is just a stop-gap.

Re my thesis: there should no longer be need for borrowing the manuscript, as I think that the thing is now out, albeit with lousy format and reproduction, as U.S. Bureau of Mines Report of Investigations 5151. If so, it can be obtained free from U.S.B.M. in Pittsburgh, by just asking for it. I was told when I left New York that it should come out in December. If something has happened to this timetable, let me know by a postcard, and I'll send you one of my copies of the manuscript.

Be sure to keep calm about all this job business and feel free to call on me for help of any kind you may regard as appropriate.

Sincerely yours,

AT

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

Dec. 10, 1955

Dear Al,

Your reprimand, whether so meant or not was deserved. It did occur to me, at too late a date that perhaps I was misguided in my information in so far as Houston was concerned. However, it was an excusable mistake for two reasons - 1) the Houston Shell men I mentioned had never heard of Kuyl and where familiar with your namex & 2) Shell was, at least slightly unethical in that never once had it been mentioned that two jobs were available there. It was simply always discussed in terms of one job - so how was a poor girl to know? As for a job - I'm not really too worried at this particular time but my pride is hurt which is probably good for me. As for oil company jobs - unless they are in research establishments such as your own - frankly, Al, I wouldn't give you 2¢ for any of them. You don't know what its like - the "research" for one who is interested in fundamental pollen research is, to put it mildly highly frustrating. My two summers in Long Beach have taught me that - which is the reason I was anxious over the Houston job - a job with Shell - doing what and where they tell me is always available - but do I want it? Oil Co. research doesn't mean research in anyone else's language - you're there for one purpose and one purpose only - to produce the goods. As far as they're concerned pollen may as well be tin-cans stuck in the sediment for their convenience - in other words Al, because of this one sided dedication there would be no opportunity at all to scientifically further knowledge of pollen in any respect whatsoever. I'm not being dramatic over this - talk to any of the old oil company stratigraphers and paleontologists that now populate our University faculties - we've several in our department - there are a number in the geology department. They quit the oil companies despite better pay and less responsibility because they were interested in their work and realized that they weren't furthering it a mean interest. A number of the Professors have asked me how I liked the work and weren't at all surprised at my reaction. This isn't saying I wouldn't work for an oil company or for Shell - it is saying that unless its a research job in a research organization, it'll be anything but a permanent position because I've no desire to let my interest and little knowledge die a slow but inevitable death. There are other comments I could make on your comments that would be entirely justified - particularly on the matter of statistics - one can "prove" anything one wants with statistics - This is what makes it such an agreeable subject and so amenable to scientific use. I agree with you (no one would dispute it - even the most ardent feminist which I am not) that statistics do "prove" that women are less reliable than men on a long term job basis. But do statistics prove this for women on the level which is the only significant one for the case in question i.e. on the Ph.D. level. Women, in general, I think it might safely be said who spend 5-6 years of hard work and worry are in general not so immediately anxious to simply "give it all up" and take up the Housekeeper - Home-maker trade (unless after being so ceaselessly brow-beaten ~~that~~ they become so frustrated that they are willing to commit such an intellectual suicide) This is absolutely no reflection whatsoever, on women who only want homes and children, but to class all women by gender, as you say, rather than in a manner which would be of some significance is more than ridiculous. This becomes then not an individual judgement, but one leveled against an ever increasing population of women for whom home and occupation is an acceptable fact. All I'm saying, Al - these statistics are fast becoming out moded - they aren't any longer in tune with the existing realities - Consequently, I feel arguments based on them are untenable.

You ignored the matter of your thesis. The Univ. Interlibrary loan will charge me about $4.00 to get ahold of it from Harvard. In view of my notes on same - I'm not sure it would be worth it, if you will answer my questions (this necessitates, of course, my accepting your identifications completely, more or less blind folded so to speak). Were all your specimens pictured and described? How extensive was the description? Did you designate "types" in the ~~conventional~~ conventional sense of the word or did you simply pick out typical grains to represent a genus? Did you discuss your views on nomenclatural systematics? I think our points of view differs here somewhat and I know, of course, how you approached the problem – but I'm interested in why – I brought this up to Barghoorn at the G.S.A. Meetings but couldn't get much out of him. I suggested that possibly your designations simply in terms of Xus sp. perhaps have had something to do with publication difficulties. Has anyone approach you on this subject. To whom the thesis has been submitted for publication? Did you attempt comparisons of your material with any recent species in particular? Have equivalent recent species been designated by either Barghoorn or Spackman with their stuff from the lignite (or any of their material been ~~[illegible]~~ named specifically for that matter?) – and if so did you compare pollen of the same species with any of your material. This about sums up those particular questions which I've had most in mind. I must admit if all your stuff is pictured I'd be very tempted to get ahold of the thesis. If you did not discuss your views on nomenclature in your thesis I would appreciate a summary of any opinions you held then

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The Hague, Netherlands

Afsender: Gray
Dept. of Paleontology
University of California
Berkeley 4, California
U.S.A.

PLEASE OPEN HERE

or now on the subject. This is a poignant question and while I don't think your approach to the problem may be the "right" one, I'm not immediately sure just what is the "right" or at least, most justifiable approach – I've already lost more than a little sleep in this consideration. My thesis, of course, falls heir to some special problems because of following close on the heels of an extensive monograph of the same material and because of established nomenclatural practices in the western American school of Tertiary Paleobotany – I'm looking forward to your response –

Best regards –

Jane Gray

Merry Christmas & Happy New Year!

UNIVERSITY OF CALIFORNIA

MUSEUM OF PALEONTOLOGY
BERKELEY 4, CALIFORNIA

January 18, 1955

Dear Al:

Picked up your thesis this afternoon, i. e. Report of Investigations 5151. See there have been considerable changes in matter of nomenclature. Your approach I find now far more reasonable than previously and similar to what I am using myself. As usual I seem to be trailing in your footsteps--could have written your appendix on Nomenclature, in fact have already written what is so similar that it looks as if you had done it. I suppose it is important enought to incorporate in mine anyway--but is sort of disallusioning--but is bound to happen to usall at one time or another I guess.

I have also quite an algal flora, you might be interested in. Botryococcus is there, but of less interest to me than several species or varieties of Pediastrum, and apparently a couple of other genera including Tetraedron (a single specimen--in good shape but folded up so it wouldn't illustrate well). Strangely enought had been preparing a short paper for Micropaleontology on this subject.

At any rate don't bother to write your views unless you've additional comments other than are in the paper. IT is quite clear--although I can't quite agree with you on the Phlugian system with all their subspecies, varieties etc. To me a nomenclature, especially if artificial and hence intended to be utilitarian, should be first and foremost easy to use. Their system, in general is so confusing that it would be of little use to other than those intimately acquainted with it. Furthermore, since Phlug is interested in stratigraphy and not botany it seems to me that a ~~xxx~~ number system would do almost as well as an involved artificial nomenclature--i. e. as the Shell system. Like yourself, of course, I have never thought that a genus once well identified should be given an artificial name.

They have done a not unreasonable job, it seems on your thesis and it appears to be a good and thorough piece of work--I'm glad to have a copy and hope in the not too distant future that I can do as much for you. I'm sorry that the plates are not at the end all together and in some respects the ~~xxxxxx~~ manner of running all the contents together without due separation makes for a bit of confusion. The reproduction of the pictures is certainly good--be thankful for that.

Dr. Chaney has just examined your paper and finds it of considerable interest, although he's never been happy with the "Barghoorn curve of modernization". At any rate he has asked me to pick him up a copy. Are you sending one to Axelrod?--you probably should.

Best wishes,

Pome

Friday 13, 1956 —
Jan.

Dear Al —

Gather you are now in the process of settling down in Houston - I'm glad to hear your thesis is finally about to appear. Since we are in the Hearst Memorial Mining Building we have a Bureau of Mines office down stairs. They tell me they usually receive the Reports of Investigations about 2-3 days after they are issued in Pittsburgh. They have both 5150 and 5152 but not yours. I gather that as yet, it has not appeared. If it does not come out with the next couple of weeks, I would appreciate your sending me one of your copies. There is not an immediate rush - I've run into some considerable snags are far as photography is concerned and can't possibly finish before Sept., I don't think. Unless I can overcome this problem, I won't possibly be able to publish the thing. Chaney is as little concerned as always about problems, other than those that immediately concern him so don't get much help in that direction.

What did you think of them in Germany? Potonie seems {all right} [alright] but Philiny - I presume you met him, annoys me to death as I mentioned previously -

The enclosed speaks for itself and I gather it seems the "hair tearing period" was short lived, indeed. One of the first year graduate students - whose interests are primarily invertebrate paleontology - and at least have nothing whatsoever to do with pollen - was recently interviewed by a man from Carter who also told him of Hoffmeisters, immediate and pressing needs for people who knew or where interested in pollen - They even offered to take him on - Soo - it goes -

If you don't receive a post card from me in a couple of weeks saying that 5151 Report has been issued you might think to send along one of your copies.

Best wishes,

Jane -

Carter

THE CARTER OIL COMPANY
RESEARCH DEPARTMENT
1133 NO. LEWIS
TULSA, OKLAHOMA

P. S. WILLIAMS
CHIEF OF RESEARCH

POST OFFICE BOX 801

January 6, 1956

Miss Jane Gray
Dept. of Paleontology
University of California
Berkeley 4, California

Dear Miss Gray:

Thank you for your letter of December 18. Due to my recent absence from Tulsa the letter has remained unanswered. Yes, I recall meeting you at the G.S.A. meeting in New Orleans. At that time we were both interested in meeting J. M. Schopf. Dr. Harry Ladd introduced me to him the following day.

You inquire about the possibilities of becoming connected with our microfossil group at The Carter Oil Company Research Laboratory. I regret to say that at the present time we are unable to offer you a position here.

Your work sounds very interesting and important. I am sure you will not find it difficult to get connected with other microfossil laboratories as there seems to be an increasing demand for persons of your qualifications.

Sincerely yours,

Wm S. Hoffmeister

William S. Hoffmeister

WSH:gh

January 26, 1956

Miss Jane Gray
Museum of Paleontology
University of California
Berkeley 4, California

Dear Jane:

Thanks for yours of the 13th and 18th of this month. I am glad that you made contact with R. I. 5151. Unfortunately the authors of such things are provided with only 25 copies and are prevented by regulation from obtaining more. However, anyone writing to the publications office of the Bureau in Pittsburgh will be sent a copy free. The twenty-five copies I was sent were set aside for foreign distribution. Indeed, they are already all gone.

I am glad you thought the reproduction satisfactory. I was not quite horrified, but pretty close to it. The plates were offset printed like the rest of the thing, and they do not do justice to the originals...a far cry from the sort of work one gets in Palaeontographica, for example. I agree wholeheartedly with you about the format, but I had absolutely no control over that and was simply overruled on such items as my suggestion that the plates should be together at the end.

I also feel that we are probably in total agreement about Herr Doktor Pflug. But like them or not, a good share of his genera are validly described and must be accepted, as I read the international rules. If I had my thesis to do over again I would have said some things differently, of course...but that's life.

Your discovery of Pediastrum, etc. is very interesting. You should look at Miss Cookson's several papers on Tertiary occurrences of Pediastrum and related things. She has just published a new one with Deflandre. (He, by the way, has a large list of publications on fossil flagellates, dinoflagellates, and the like). I have microfilm of some of these things.)

Am mystified by Hoffmeister's coolness. But you would have found his laboratory just you want to avoid: straight applied work. Unless you are content to do applied work, fitting in the basic around the edges, you are going to be hard to satisfy. Perhaps an academic job would best fit your needs (Post-doctoral fellowship?) but remember that in a teaching post you will spend most of your time in "applied" work too. Consider the compromises that poor old Al T. has had to make: four years of coal petrography in North Dakota!

With best wishes as ever.

Very truly yours,

Alfred Traverse

AT:hmp

Shell Development Company
Exploration and Production Research

Dr. Traverse

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

October 5, 1953

Miss Jane Gray
Department of Paleontology
University of California
Berkeley 4, California

Dear Jane,

You are my creditor for three pieces of mail since I last wrote you. That fine letter of April 16, by the way, had the largest ratio of words per square inch I have ever seen in handwriting and must have taken much space engineering. I have enjoyed all of your letters and hope that I shall be fortunate enough to continue on your list of regular correspondents as your work progresses. May your research and study at Berkeley be stimulating, profitable and successful, as it has every right to be! Your past year, at Iversen's laboratory and elsewhere, was probably more valuable to you than even you realize at the moment.

I was able to make the Madison meetings of A.I.B.S., in September. Bill Spackman presented a joint paper on the Brandon lignite, with Elso and me as coauthors. The pollen work was dealt with only as a part of the whole picture. Clair Brown gave a short paper on some of his pollen work with Louisiana lignite. The paper was not very informative. I remember that you met Dr. Brown in Europe and had some comments with which I am in agreement. A gentleman named W. L. Moren, of the California Research Corporation, La Habra, made some comments after Brown's paper indicating that he is doing Tertiary pollen work for California Standard Oil Company. He is obviously uninterested in any aspect of the work other than stratigraphy, but you may have contact with him. His training is botanical but on the physiological side.

You may have heard about the palynological conference to be held at Boston in December in connection with the A.A.A.S. meetings. It is being arranged by Cain, Wilson, Sears, et al. Stanley Cain, whose address is: School of Natural Resources, University of Michigan, Ann Arbor, has sent out preliminary notices. If you didn't get one, perhaps you'd like to write him. Should you happen to be in the East for the Christmas holiday, you might like to come and/or give a paper. The emphasis, of course, will be Pleistocene, but I may possibly give something on the Brandon lignite.

A copy of my J. of P. blurb has reached you. The monograph itself still lies fallow. This is partly my fault for being slow with the work of revision. Partly it is also due to lack of satisfactory assurance of publication. It is difficult to revise a manuscript without knowing where and in what form it will be published.

Now, for my reactions to your letters: Your summary of activities in the April 16 letter was enormously informative, and your criticisms very interesting. I think I should have it framed as a source of information about the European palynologists. Especially I am glad to know about Thomson and his associates from first hand. Previously all I knew was from his papers and from Elso, who did not convey at all the impression you have. About Pflug I knew nothing except his papers. Your description of his methods are extremely helpful to me in understanding his work and, more especially, his attitude on palynologic taxonomy. I have been in correspondence with Friedrich Mürriger, whose wholehearted support of an artificial system is similar to and apparently derived from Pflug's. I have Thomson and Pflug's recent "atlas", by the way. The information you have about Pflug's proposed phylogenetic work is a bit startling in view of the poor view you say he has of systematic botany.

Your reference collection appears to be growing satisfactorily. Because of preoccupation with other matters, mine has been almost static since I left Harvard. I have about 850 species represented. From what I saw last November, the Harvard working collection has now grown from this base to about 50% more than my collection. Your idea of an exchange system sounds very good to me. It would be mutually advantageous for us to prepare, and exchange, duplicate reference slides of each new item, as available. However, I should say that the best way for you to get slides from my present collection would be to come to Grand Forks and make them from my duplicate material, which is neatly catalogued and preserved in small vials of glycerin jelly. Of course, I shall be glad to supply you any items you may request from time to time.

I do hope you can make the G.S.A. meetings in Toronto. If you do, you surely could arrange to visit Grand Forks on route there. Perhaps you will choose to pass up G.S.A. for the A.A.A.S. Palynological Conference in Boston, in which case I would similarly expect to see you then.

Hoping to see you before 1954, I am

Very truly yours,

AT

Alfred Traverse
Coal Technologist

ATraverse:dm

cc: Region V
Lankford
Traverse
File 625.1
C. File

Sept 1, 1953

Dear Al,

Thank you for the separate of your article which appeared in the "Journal of Paleo." I must not have received that issue for some reason for I had no recollection of seeing the article at all and couldn't locate that number of the Journal after receiving a copy from you. I did take rather extensive notes on your Thesis, however, so I do have the material in more detail than appeared in the summary.

It's certainly good to be home, although I enjoyed my year in Copenhagen immensely and certainly did profit by the contacts there. You will be interested in hearing I know, if someone has not already told you, that Kirk Bryan's daughter Margery, who was a botany major at Radcliffe and just graduated this year, has received a Fullbright and will spend it in Copenhagen working with Dr. Iversen. Dr Deevy from Yale will also apparently be there most of the winter also - or at least so I gather from the Radcliffe Alumni notes - seems Mrs. Deevy was a Radcliffe girl, a fact I wasn't aware of previously.

I'm leaving for California on the 8th of September and mainly wanted here to give you my address for future reference. I've no idea as yet where the GSA meeting will be this Fall, but I shall definitely try to get to it, if it is within any reasonable distance of San Francisco, and suppose that you will do the same. I've heard much already about the Department of Paleo., Dr. Chaney, the University and San Francisco and am looking forward to the 15th when the term opens. I've also made arrangements to live in International House which likewise should prove an interesting experience.

If you did not get a copy of Thomson's and

Pflug's monograph and would be interested in looking same over, I would be happy to lend it to you for inspection. If we both attend the GSA meetings I will bring it there, otherwise or even anyway I could mail it to you if you like. I've a feeling that perhaps you think I was carried away in my criticism, and although I may have been a bit more enthusiastic than necessary, the criticism was justified as I'd like you to see for yourself.

Give my very best regards to Betty, and if you have occasion to see or write Dr Barghoorn, I wish you would remember me to him also —

Sincerely yours

Jane

Address:
Department of Paleontology
University of California
Berkeley 4, California

Dear Al,

April 16, 1953

Several months ago I recall a promise of a letter and no doubt you do also. First, I'll give you a general account of activities this past time and then also attempt to answer more specifically some of your questions. I may have mentioned in some letter my intentions of coming to Germany for a short time to see Thomson and Potonié. Decided April was a good time and arrived in Krefeld on the 30th of March. Thomson had been ill for several months and so was still home in Liblar (in the heart of the brown coal district). His assistant Dr. Grebe took me in tow for several days and then I went to Liblar itself for a couple of days (about a week and 1/2 accurately). Thomson and his wife are now living under very severe conditions in an apartment? given them by the director of the brikett factory near there. They have lost all their possessions 3 times being first described as Russian refugees and came to Liblar 6 years ago with packs on their backs and a bit of hand luggage. His wife is from the old "Petersburg". The name Thomson stems from Swedish ancestry of several hundred years ago. Thomson himself is a voluminous talker and I heard as much of his family and history as I did of pollen analysis. He speaks his own variety of English (self taught) which is at any rate understandable. He retires this fall on a small pension but will go, at the invitation of the San Salvador government, to that country for 1/2 year to study the flora and bogs there and hopes to return to settle permanently in Bonn. He is primarily a geologist, but seems to been an excellent botanist, at least in systematics, knowing, so he claims all the plants in middle Europe. He was proud of this knowledge which was displayed constantly and I for one was not in a position to doubt its authenticity. The visit, however, did much to cement my opinions that I made the right decision in going to Iversen for the year and the amount of time I have spent with Thomson is quite sufficient, despite your suggestion of a year of it. He has a very high opinion of Iversen and regards him as the present leading light in Quaternary and Recent pollen studies. I also had much conversation with Dr. Pflug Thomson's leading student in Tertiary work. It was really there that I saw the defects in their work and was glad for Iversen. Pflug has written his thesis and now teaching stratigraphic pollen analysis in Aachen - you may have seen some of his many publications. To begin with neither Pflug nor Thomson has a collection of Recent pollen. Thomson places the blame on loss of his collection in Estonia. Pflug merely says his interests are stratigraphic and not floristic, hence the dismissing this lack of collection. And point blank - he doesn't know pollen. The primary method is to compare his grains with pictures of those published by other workers. In fact he went through my pictures of pollen from the Eocene one evening and gave practically everyone of my grains an artificial name merely by comparing pictures. With some of his pictures, I suggested other possibilities of family identification and in most cases he had never even heard of the genus let alone see it. It is the belief that grains below the middle Oligocene cannot be identified to family or genera without question. Consequently Thomson and Pflug have given all grains that fall here geologically an elaborate system of artificial names, including generic, specific and varietal names. If some family of Recent plants, or a genus suggests itself this name may be appended to the artificial or general morphological name given the grain. This even applies to grains that look to be obvious in their identities i.e. Alnus, Ulmus, Pinus etc. An elaborate influx of new names (generic) has been due to their taking all Potonié's, Kirchheimer's etc. grains designated earlier as "Pollinites so-and-so" and ~~[illegible]~~ coining a new generic name (based on Iversen's morphological system) and keeping Potonié's old specific name. This they say is excusable as "Pollinites" is not a name (generic) as it designates all pollen and as Potonié has no types for these grains. Professional tiffs, I might add between Thomson and Potonié are now to showing. I hope some of this is clear - it is difficult for me to put down all my thoughts in a good order on this matter - perhaps because I feel so strongly about the matter. When, however, Tertiary pollen analysis degenerates to a system of comparing pictures for identification and coining names on a flimsy basis, I'm done with the study. At any rate, I talked critically to Pflug about knowing the Recent pollen, before launching on such a system and I think it made some impression on him - at least he said so. He will take over Thomson's students when he retires and speaks of getting one of them to work in that manner, building up and critically studying expectable recent pollen for brown coal studies. An ~~elaborate~~ elaborate atlas by Thomson and Pflug is due to be published shortly by "Paleontologica" (or what-ever the German name is) with pictures of all their grains, together with their new names. Perhaps you can get a copy gratis, although the no. of their free copies are very limited and they are making quite a fuss about who's to get one. The 2nd big work of Pflug alone is a phylogenetic scheme for tracing all pollen back to the ferns, ~~[illegible]~~ ultimately (or as he says - where else can they have come from?). He will attempt to show the developmental lines of pollen grains - the elaboration of the morphological features from the triradiate scar. Some of his pictures, I will admit are very suggestive IF, he has interpreted them correctly. I for one will prefer to see the grains themselves before I can judge! I mistrust him primarily because of his lack of knowledge of Botany, pollen and pollen morphology and for several blunders he made in examining my pictures i.e. one of my grains was folded in a manner such that he attempted to tell me it bore a triradiate scar; one of my obvious tetrads also was no tetrad but a grain with triradiate scar etc. A new morphological scheme will also appear in this work - based on the aperatures entirely - the sculpture and structure of the grain are of no importance they say. Pflug has a good mind, nevertheless and is a hard and intensive worker who undoubtedly will play an important part in pollen studies when his work matures somewhat. He is young and impetuous now - or as Iversen said [illegible] some of his work — its too much Phleg and not enough pollen analysis. As for pollen collections — how large is yours? My basic collection is very rudimentary as yet including only some 400 grains specifically applicable to Tertiary studies, but about 150± addition of Recent pollen grains. The first one was made primarily using La Mottes new G.S.A. Memoir 51, no doubt a copy of which you have, and includes representatives of most of the genera of plants heretofore described as macrofossil from the U.S. Tertiary. I have worked no more on it feeling that it is best to see which specific age deposits Ill be dealing with before elaboration. I will be most happy to exchange slides with you and will recruit Phleg also if one of his will-be students starts a collection. Thomson had been my main hope for exchange over here, but as he has no collection can't be worked in. Potonié works no longer on Tertiary material, so he's out also. It appears that Tertiary study is going to be confined for some time to a small group — Iversen has such a system of exchange between about five other Quaternary workers that is ideal and a necessity I feel. As for next year am going to Calif. with $1,500 Fellowship in Paleo. which am pleased with. Your comments on Chaney were pertinent, incidently and from his kindnesses to me so far know will be extremely cooperative. As for letter of NOV. 4 (yours):-

1. Don't worry about Kroner on books. They are probably authors free copies and Iversen won't be "out" anything. Furthermore he is only too happy to get the things in its justified circulation.

2. Jørgensen and everybody else you can be sure who is remotely interested has heard of your work. Jørgensen unfortunately, I don't think will ever do much himself. In the first place he is not too well, 2nd in his present capacity on the Survey he deals with everything non Quaternary or Recent. Thus, although Tertiary stuff is his first love he must work with everything else, from Dev. on up that has spores, pollen or what have you — he has not worked on Tertiary since Ive been here but with some core materials of Permian age, I believe. 3rd and actually most against him, he is a painstakingly slow worker, so meticulous and detailed that he doesn't see the forest for the trees. His work is beautiful — what there is of it. His initials or name is Peter and address same as mine.

3. On Christensens observations:- It isn't actually the same technique of preparation. This is significant or not in the sense I take you to mean. As I see it his study shows that the KOH Treat + acetolysis on Recent grains makes them most comparable in details of appearance to fossil grains, which already due to their fossilization have undergone certain changes. Consequently that manner of preparation is one of pseudofossilization of Recent grains that has not only the least effect on size in comparison with a fossil grain of presumable same genus, but gives the morphological features and appearance most like fossil grains prepared like wise. In acetolysis alone, the difference between Recent and fossil grains appear to be greater, not only because of the treatment, but because of fossilization. Furthermore it isn't only size changes that are noticeable as his work shows — but more important the basic shape and aperatural changes. I would like to say more in comment on your comment on glycerine use but space won't permit now. Best wishes incidently from Heidelberg were [illegible] for [illegible] days

Best to Betty — Jane

[illegible] to Norwegian [illegible] anything new that I know Phleg [illegible] opinion he has of himself.

Afsender:
Grant
Danmarks Geologiske Undersøgelse
Charlottenlund, Danmark

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Dr. Alfred Traverse
Lignite Research Laboratory
Grand Forks, N. Dakota
U. S. A.

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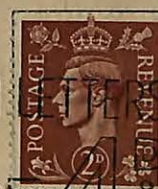

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July 13 1953

I hope you'll forgive me, mailing
this from the home of "the other
school," but I didn't get to it in
London as intended. With no previous
bias however, can say in all honesty that
Oxford can compare neither in general
beauty nor architecturally with Cambridge
which we truly found extremely impress-
ive. However, after the gay ~~—~~ whirl
of 11 days in London most anything
will seem dull. Britain is wonder-
full no doubt about it and I wouldn't
have missed this 6 weeks for the
world. I didn't have any response
from you on my bombastic letter before
leaving Copenhagen, but trust some-
thing will be waiting at home.
Unfortunately don't expect to get to Grand
Forks en route to California, but no doubt
we'll have an opportunity to compare notes,
professional etc. shortly. Very best to Betty, Jane Gray

Dr. Alfred Traverse
Lignite Research Laboratory
Grand Forks, North Dakota
U.S.A.

Jan. 14, 1953

Dear Al,

I enjoyed your letter and the Christmas with an excellent picture of your handsome little son. I can't see the family resemblance, to either you or Betty but I suppose that it is perfectly apparent to Paul's proud parents and grandparents which is usually the case as I recall. I have no doubt but that he is also as bright as he looks and I'm sure you must have your hands full at this stage.

I was interested in your paper and wonder if there is something like a copy of it available or hasn't it reached that stage yet. I expect that at sometime in the not too distant future I may be engaged in somewhat similar work and will find you a doubly valuable person to know! The results of H.U.-Y. game were indeed deplorable and as such undeserving of further comment - but then I've gotten in a four years habit of expecting nothing much else (Tsk! Tsk!)

I haven't the time to make much of this letter now as I have my hands full attempting to meet deadlines and getting others to meet them also, but promise after Feb. 1, to bring you up to date on recent developments here and attempt to answer a few of the questions you posed last time, with a sprinkling of miscellaneous comments thrown in -

I am applying for renewal of my National Science Foundation fellowships - but they warn us that the competition will be as keen as last time so not to expect too much. Consequently, I am applying for a University fellowship - at the request of Chaney and wonder if you'll be so kind as to scratch a few doodles on here (I hope you don't take me literally) and mail it back to the address given at the bottom of the page by Feb 15. I take the liberty to ask you for these things - I hope there won't have to be too many others - as I feel you have an appreciation not only of the nature of the work, but that you know, perhaps better than any-one else, the kind of thing I have been doing and of my plans etc. Iversen, of course, has been asked for one also - and I'm sure finds them quite a task, as he is unfamiliar with the form and somewhat justly, I'm afraid, dubious of their value. As it is "California here I come", ~~for~~ financial circumstances permitting, and since you question the decision, I feel that it at least is deserving of comment now. Firstly, Chaney has not turned palynologist in any sense of the word, although from his comments he is interested and has an appreciation of its value and no doubt limitations also. Interest and appreciation count at least 75% of the struggle in my estimation. Secondly, he has in my opinion the broadest familiarity with our Tertiary deposits and a good many of the same bred in Asia. His publications demonstrate this familiarity and show excellent insight in the application of this knowledge to various allied principles of paleobotanical research i.e., in my opinion he has come a long way in integrating the mere determination of plants (not at all a consideration to be belittled) with paleogeographic, paleoecological etc. considerations. Although perhaps, on occasion his thinking does overstep the facts, it is a way of thinking that I should at least like familiarity with and should enjoy being exposed to for some time. There will be plenty of time for criticism and comment when I can see thoroughly how and why he thinks as he does - Who else would you suggest that I work under? Barghoorn's a good man and is a close second in choice. He has the advantage over Chaney that perhaps he would be more material help as far as identification is concerned, but his other activities have kept him from the amount of active work Chaney has accomplished (he's considerably younger, of course). Wilson of Mass. State although unknown to me through publications has undoubtedly the most extensive knowledge of pollen types of anyone now in U.S. - he's at a poor school - I refer to monetary status. I had thought to work under him last year but some grad students at Illinois who ~~but~~ had been at Mass. State previously suggested that financial considerations cannot be neglected. In short I must think Calif. & Chaney the ideal set up — an opinion not entirely confined to myself. But aside from an exclamation you have neglected to comment further on what you must consider an adverse situation. I should be glad to hear what you say, as I value your opinion, although I doubt if I should change my mind.

My best to Betty,

Sincerely yours,

Jane

Lignite Research Laboratory
Grand Forks, North Dakota

19 January, 1953

Dear Jane,

Your letter of 14 January came this morning. I have noticed a distressing tendency of letters to me to become mired ever deeper in the the "pending" pile, so I answer anything that I want to be sure to answer--immediately. I have some old and trusty correspondents who have been waiting (probably breathlessly) for more than a year, so see how lucky you are! My promptness is the more amazing when you consider how far short of an adequate answer to my last letter your most recent missive is. (Although at first glance it does seem long and ample.)

I shall be pleased to send in the dope to U. of C. for you. (As far as I can see, their report is among the simpler ones. Now, the job we do on applicants for the freshman class at Harvard is a <u>real</u> screening. As a R. girl you probably find this incredible.) Please feel free to call on me for this sort of help as often as necessary.

It is surprising that you assume that I deprecate your going to U. of C. and Chaney. I don't at all. He is certainly the most distinguished Tertiary paleobotanist in the Western Hemisphere. But, as you freely admit, he is no palynologist, and you might, in pollen study, do as well or better at Harvard. The value of having training at several different places and at the principal centers of research is, of course, obvious. It is too bad that money must be a factor in all of these decisions. A year with Thomson, et al., might be the best deal of all. But that is merely reflection. B am much more sympathetic to a year (or more) at California with Chaney than you think. The prestige value will be enormous (not to be discounted): as far as the geological crowd is concerned, I have noted, Chaney is it. In short, my exclamation was not intended to convey disapprobation--merely surprize.

We have just had a siege of the "flu". Betty and Paul have both been ill, and this has necessitated my breaking in as a housewife-nurse. What an ordeal! That is no job. It's slavery.

Best wishes from the land of thirty below.

April 5, 1952

Dear Al,

I was glad to hear from your letter that you and Betty are pleased with your present situation, and although you have not found the winter too unpleasant, I hope (rather optimistically) that you will find the summers as much to your liking, knowing that there is practically nothing worse than heat and dust. Unfortunately I am hardly in a position to talk since I plan to be here this summer and knowing the Mid-west, I am expecting the worst.

I am very happy to be able to tell you that I received notice by telegram last night that I have been awarded a National Science Foundation Fellowship for next year. I certainly do thank you for anything that you may have contributed toward this happy event. I had thought at first that I would go back to Cambridge and have not completly thrown out the idea, althought just about so. Pollen studies as you well know are something that as far as I can see are matters of individual research, and I feel that although "talking out" problems of interpretation are a great help, I can always disperse letters in short order to those such as yourself and Professor Barghoorn who might have something contributory to say. Then I always have Mr. Kosanke near at hand, who although not pre-eminently concerned with Tertiary problems at least has had considerable experience in dealing with kindred situations and has expressed an interest in my work. I am very interested, incidently in this Kentucky deposit which either Dr. Barghoorn or Mr. Kosanke may have mentioned to you at some time. It is in the western part of the State and in its stratigraphic relationships and abundance of megafossils seems to bear a considerable resemblance to the Brandon lignite, although it has not been investigated at all. As a guess, one would think that perhaps it is Eocene, because of the relation of the Wilcox Embayment, which although shows no direct connection with the area, was widespread enough to suggest that perhaps one did exist during Eocene times. We might even fit the Illinois Tertiary(?) down in the southernmost part of the State, which I am sure Mr. Kosanke mentioned to you, into the picture. I have worked up some of this Illinois material, and had a look at some of the pollen in it, but for lack of pre-study of a reference slide collection, I am naturally at a loss for proper identification, although one might hazzard a guess about the identity of some of the material. I hope that I ~~wouldn't~~ WON'T have too great a problem collecting pollen of the presumed living constituents of the Eocene flora, but one can see from Berry's mongraph that the segregation since Eocene times has been even greater than that taking place in the eastern Flora since Brandon times (expectable of course). I hope in addition to working up my M. A. thesis this summer (which is being worked on now) to start on the reference collection, which will take considerable time, because my equipment is somewhat less adequate than yours was.

Incidently do you get the "Pollen and Spore Circular" edited by Dr. Sears at Yale? I presume that you do but thought I would mention it, since it often contains reference to work being done on Tertiary deposits elsewhere. Also you might be interested in two publications of the Danmark Geologiske Undersøgelse which you may not have seen. I have sent for copies, but as yet have heard nothing. These are:

Measurement as a means of identifying fossil pollen by B. Brorson Christensen, published in 1946, Danmarks Geologiske Undersøgelse, IV.Raekke. Bd. 3, Nr. 2.

Pollenmorphologische Definitionen und Typen by Johs. Iversen og J. Troels-Smith, published in 1950, ibid, Bd. 3. Nr. 8.

The first discusses the various distortions (shape, dimensonal) that take place with all the standard types of treatment both for recent and fossil pollen and is worth reading. The second is mentioned in the bibliography of Faegri and Iversens textbook and contains the originals from which many of their diagrams were printed, plus many additional drawings plus an excellent compilation of palynological definitions and vocabulary.

How is your work coming? I hope you are both enjoying it and deriving some profit from it. Do let me know when and where your thesis is to be published and by all means I certainly want a reprint.

Give my very best regards to Betty.

Sincerely yours,

Jane

Jane Gray

U. S. Bureau of Mines
Grand Forks, North Dakota

21 April, 1952

Dear Jane,

It was most pleasant to hear from you and especially to learn the good news of your fellowship. If my recommendation meant anything, it couldn't have done you any harm--I was certainly strongly pro-Gray, as you might have expected. You have a wonderful opportunity to work for your degree with few encumbrances. The field is wide open.. If you enjoy the work, youre all set, but, in any event, the time will have been well spent. I had a similar opportunity for the two years immediately following my undergraduate work. At the time I thought I would go into plant genetics, so that I didn't really get to work on my graduate work proper until the third year out. I have no regrets at all.

Your comments on the deposits in Kentucky and Illinois were most interesting. You may know that Spackman is on the trail of an old lignite mine in Pennsylvania. Doubtless there are many other similar occurrences which will pop up from time to time. I have no doubt that the pollen method can be used to straighten out much of what we know and don't know about the Tertiary. Please keep me informed about the progress of your work.

Reference collection: ah, yes, some problem. As a matter of fact the question is much on my mind at the moment because I have absolutely no herbarium available here. I am convinced that there just is no adequate substitute for herbarium collecting. A thought which is occurring more and more frequently to me is that we should share reference material, providing that the same methods of preparation are employed. At any rate, I want to expand my reference collection but feel that it will be some time before I can do so.

Thank you for the tips about the literature. I am writing Sears to be sure that I am on his list. I have been at some time, but he may not know of my new address.

A preliminary paper on the Brandon work is coming out in the Journal of Paleontology fairly soon. The bulk of the thesis is still without a publisher. I trust that the situation will be remedied before it becomes chronic.

My work, so far, has been of two sorts: getting ready to work in this laboratory and some quite elementary petrographic studies. I think I can say that I have whipped the laboratory into very good shape, considering the lack of equipment when I came here and the short time I have had, as well as the utter lack of help. (I don't even have as much help as I had last year. Betty is far too busy with a host of other things, most

April 3.) My petrographic studies have yet to turn up anything sensational, but I have a good research program set up, and it will bear fruit ere long. Next Monday I make my first sortie into the field, a trip to the Truax-Traer lignite mine in Velva, N. Dak.

Betty joins me in sending you our best wishes. Best regards to all.

Alfred Traverse

Alfred Traverse

UNIVERSITY OF ILLINOIS

~~May~~ June 1, 1952

Dear Al,

I was very happy for both you and Betty to hear of the birth of your son Paul. I'm sure that you must be very happy.

I have finally decided what to do next year and thought that perhaps you might be interested to hear of my plans. I am sailing on July 22 from New York to study for a year with Dr. Johs. Iversen in his laboratory in Charlottenlund, Denmark. The National Science Foundation has no objection to changing institutions of study and they will pay my expenses over there, any tuition, plus other expenses. I have written Dr. Iversen, and he said that he will be very happy to see me. He will be away till about the middle of August, but said that a Mr. Ingwersen, an expert in Tertiary pollen will be visiting in his laboratory and that he will be very happy to look after me in his absence. It really sounds delightful and I am looking forward to the year very much. I hope very much to move in with a family so that I can learn the language. I shall be there a year, or perhaps longer, but my plans as of now include coming back to Harvard to write my thesis under Professor Barghoorn. The fellowship is renewable, so I should be able to swing it financially. I intend to keep in touch with you, for I know that you will be interested in hearing of the people I meet. I hope to get up into Stockholm and down into Germany to meet a few of those who have worked with the German browncoal deposits. I'm quite sure it will be a wonderful and profitable year.

I don't know as I had mentioned to you what I was doing here. Had been working on some pollen which I isolated from some cores which Dr. Hough had taken from the bottom sediments of Lake Michigan. As far as I know this is the first pollen obtained from any of the Great Lakes. The core I am working on is 34 feet long and appears to end in glacial varves at the base. A large part of it is red and contains no pollen from the two samples I have run through. Am putting a third through acetylosis now just to make sure-- but then surely none is expectable from the color that suggests oxidation. I won't have time to study the trends now, but have about 8 slides from the pollen containing part, which I intend to take with me. This was originally going to be turned into a Master's thesis, but now perhaps a paper. Also am working up some samples which I got from some Eocene and Cretaceous sediments on a field trip this spring. One is from leaf beds in the Jackson formation, collected along the Alabama River. The other is from the Cretaceous Ripley formation. You may recall that Berry has a Professional Paper for the U. S. G. S. on the macrofossils from the Ripley-- so it will be of interest to see if I get anything and what it is. I shall also take these slides with me-- for I'm sure Dr. Iversen would be interested in seeing them. So few deposits have actually been investigated to see if they do contain any pollen-- that this kind of thing is really merely reconnoissance on my part, just to see where it can be found. I also had a rather thrilling experience on this trip-- found in one of the Cretaceous outcrops we saw, a log about 3-4 feet long, in amazing preservation-- it was lignified to some extent, of course, but one could see structures very well. Brought it back here for one of the fellows in the Botany Department to look at-- don't know whether he has worked on it yet-- butat any rate it was very exciting to find.

I was glad to hear that a preliminary paper will be published on the Brandon deposit in the Journal of Paleo-- since I recently have taken a subscription to this Journal and shall not be obliged to ask you for a reprint.

Your work sounds of considerable interest-- and I hope that I shall be fortunate enough to find something when the time comes up. I should prefer to teach, so that I may have free reigns on my research program-- but we will see. I had a letter a couple of months ago from Mr. Paul Averitt, who is head of the

UNIVERSITY OF ILLINOIS

Coal section of the Fuels Branch of the U. S. G. S. He wanted to know if I would be interested in going to work in the Survey's Laboratory in Columbus Ohio with Dr. Schopf. It seems that they want to expand and would like at least one botanist, geologist, chemist and physist on the staff. He says that they will be doing work with Tertiary as well as Carboniferous stuff. Of course, my present plans are somewhat prohibitive, but at least it is something to keep in mind.

I'm sorry to have labored the details of what I am doing, but perhaps you will forgive this over enthusiasm at the moment !

Please do give my best regard to Betty and your small son. I will let you know how things are from time to time and please do the same.

Sincerely yours,

Jane

Jane Gray

Gray
Department of Geology
University of Illinois
Urbana, Illinois

Lignite Research Laboratory
Grand Forks, North Dakota

25 June, 1952

Dear Jane:

Thanks for interesting letter of June 1. It was good of you to felicitate us on the arrival of little Paul. He is growing and changing so rapidly that I feel sometimes that I ought to stop this coal research altogether and observe him full time.

Your decision to go to Europe next year of course pleases me. You may remember that I was anxious to do the same thing last year, though I'll admit that professionally this experience out here will be more valuable. Eight years at a stretch is near the maximum academic exposure a man can expect, I suppose. How fortunate you will be to have the wonderful opportunity of working with Iversen. I can tell from his published work that he is a terrific scientist. His papers are so precise and clean and clear. By all means try to meet some of the German workers while you're at it, especially Thomson, who is apparently the star pollen analyst of Tertiary coals in Germany. I tremendously envy you this chance (not in the begrudge sense, of course!), and I know that you will get the maximum of good out of it.

My advise to you would be: see all you can, read all you can, talk to everyone you can. Do not confine your endeavors to pollen analysis. Presume, however untrue it may be, that you are never to be in Europe again and act accordingly. Absorb as much of the European feeling and attitude as possible. You will find ~~xxxx~~ your well of, what shall I call it, personal culture perhaps, filled to such an extent that you will be able to draw on it constantly, even if you should end up in some such hole as Grand Forks, North Dakota (may you be spared!).

By all means I should like to hear about your training, observations and so forth. Never worry about going into too much detail. You have no idea (I presume) how starved I am for conversation, professional and otherwise. Letters from my friends in the outside world will help me retain my sanity. Furthermore, your work is of considerable interest as being so near my speciality.

Woe, that my job here has practically stoppered my paleobotanical work. I have done, however, a tremendous amount of general paleobotanical reading in my off moments, and I am sure that will pay off. I too, as you know, aim to get into the academic swing some time. I hope that this is just another dream. One thing is certain: I must take steps to get out of government work and out of North Dakota before either has a chance to win me over. I am in no immediate danger.

My job here continued to be fascinating, full of possibilities. But I am too far from professional contacts, too far from friends and relatives, too far from civilization,.... too far....Besides, we hate Grand Forks. There just isn't a bloody thing here. Ask a native what the advantages of Grand Forks are, and he begins babbling about things three hundred miles away. One of the favorite arguments is that Grand Forks is in the geographical center of the North American continent (which is true). Very well, I say, that is precidely what I have against it. Its equiDISTANT from everywhere.

All of us join in wishing you bon voyage.

Sincerely,

Alfred Traverse

Oct 29, 1952

Dear Al -

This long enduring silence on my part, following on the bubble of enthusiasm of last June, may have been indefinitely prolonged if I had not at the moment felt great desire to express righteous indignation, due mainly to the appearance of Erdtman's new book. Perhaps you have not seen a copy yet - we have just recently received ours here. It represents much work surely, no one can deny that - but some of the sneaky tricks concerning his morphological treatment vs. that of Iversen and Troels-Smith of 1950 really hit below the belt. Iversen doesn't say much being the kind of splendid, fine person that he is but from occasional comments here and there one can tell that he feels it. One of the worst things Erdtman has done of course, are to re-use already established terms in Iversen's morphology over again in a different sense that originally used. You will find instances of this scattered through the work. I went to the office here to inquire if you had purchased a copy of Iversen & Troels-Smith's "Pollenmorphologiske Definitioner und Typer" - for if you had not I had planned to send you a copy pronto. Your letter of April 29 was produced, however, and at least I am reassured that by now you have something worthwhile available on the subject. Having formerly entertained a fairly favorable impression of Erdtman, I find that mine is ~~fast~~ beginning to coincide ~~that~~ ~~that~~ with that fairly general about here - that the man is merely trying to establish a reputation for himself. At any rate I have established myself as a member of the "Iversen-school" and have no intention of using the copious supply of terms which Erdtman has invented. It seems important to take a firm stand on this subject because a) unfortunately there are apt to be many impressed by the apparent display of thoroughness and perception which the size of the new volume suggests b) and similarly unfortunately, the earlier publication of Iversen and Troels-Smith has been kept in considerable obscurity because of publication in the annals of the Danish Survey. I do not know, of course, how you feel about this, but hope that you can spread the word of the this smaller publication as much as possible to any and all even remotely interested, as I shall do also.

As I believe I had mentioned at some time I sailed on the 22nd of July aboard a Norwegian ship - the "Oslofjord". After sighting the light house at Cape Race, Newfoundland we headed Europeward across the North Atlantic, making a first stop, of 11 hours at Bergen, were I was fortunate enough to get several pictures, both of the fjord and of the town from a mountain above despite the rain. Then we sailed down the unbelievably wild and beautiful Norwegian coast making two other less extensive stops at coastal towns along the way - Stravanger and Kristensand. We arrived in Copenhagen on the morning of the 21st were amid various signs of welcome held by the stay-at-homes to greet their wandering kin etc. I noted one reading "Very welcome Miss Gray" held by a middle sized blond "Scandinavian-appearing" gentleman, constantly craning his neck this way and that looking for something that might resemble his notion of Miss Gray. When I came out of customs I discovered that he was a Mr. Ingwersen - a geologist-botanist attached with the D.G.U. (Danish Geologiske Undersøgelse) and he came accompanied by two other geologists and a bunch of red and white carnations (red and white being the Danish colors). The other two were attached to the Danish-American prospecting company - one a friend of a Professor of mine at Illinois, who had been asked by Dr. Kummel to meet me at the boat. Mr. Ingwersen is studying the pollen in the scanty amount of Tertiary brown coal here in Denmark and during the absence of Dr. Iversen temporarily in Jutland was to act as my "scientific advisor." Dr. Iversen returned about 2 weeks later and a most interesting and wonderful person I have found him to be as well, as you have commented, a very keen scientist. His laboratory is well organized and competently run by himself and 3-4 assistants, although it is comparatively small. The micro-scopes and various apparatus is of the very best, however. They have a splendid collection of Recent pollen, but this is for the most part of northern genera, and for Scandinavian Quaternary stuff, with the exception of a small number of slides prepared by Ingwersen for his studies. Consequently, using La Motte's new G.S.A. Memoir 51 - a copy of which you no doubt have - as a guide, I have been preparing slides of pollen of all genera of plants described as macrofossils from our Cenozoic stuff. This comprises some 3-400 genera including mosses, algae etc. with which I shall not bother. I must have approx. 200 slides now and hope to complete the whole lot before Christmas vacation, in so far as all the material is available from the National Herbarium here, were I have been doing the collecting. As I mentioned that I have some Tertiary material with me, I intend next year to work up identifications on as much of the pollen as possible, make single slides of my unknowns, and perfect the technique of photographing the grains. They are making excellent photographs here, not only of their fossil material, but of the recent pollens as well. The samples for analysis are always stained, always examined in glycerine, rather than glycerine jelly and photographs of the grains are made in glycerine also, so that a single grain may be turned and shots taken from all angles. I think that this latter is an excellent practice. If it is desirable to keep a single grain for documentation purposes, it is then "fished" from the slide with a small lump of glycerine jelly and mounted permanently. Staining is done, usually with Fuchsin as mentioned in the Book, and during the final washing with water is usually added the whole being then centrifuged and decanted. You might find it usual to try this, if you have not done so already. The staining practice was one that I have not practised hitherto. The ~~[illegible]~~ acetolysis method practised is not exactly Erdtman's - due to the fact that Christensen's investigations (as no doubt you have noted from that publication) have shown this not to be most satisfactory for size-changes. The sample (herb. material or peat) is always boiled in 10% KOH first and then acetylised. With herbarium material - a small part is placed in a small porcelain dish - boiled with 10% KOH and then the whole dumped into a small porcelain sieve with the smallest opening possible. The material drained there is then placed in centrifuge tube, centrifuged and decanted. Water added, centrifuged and decanted. Glacial acetic acid added, centrifuged and decanted, then the hot mixture of Acetic Anhydride and H_2SO_4 is added and the tube held in water bath for 1 minute, then centrifuged and decanted and washed with H_2O several times. This might seem a bit slower to you at first than the mass water bath method but one can always work ~~4~~ at a time very ~~[illegible]~~ conveniently, or for that ~~[illegible]~~ matter any number by marking the test tubes - and then by the time one waits for the water bath to boil from scratch in Erdtman's method it probably takes no longer. There isn't too much else to say on this at the moment. I might mention some magnificent microtome sections of pollen grains that I have been made by this same Christensen of the previous paper. Unfortunately the man is an archeologist and not continuing in pollen work, although he is a real artist. These microtome sections were made by a technique developed by himself and unfortunately described in a paper in Danish with only a very unsatisfactory summary in English. As it contains some of his drawings, however, I shall give you the reference and perhaps you may care to look it up at some time: -

Christensen, B. Brorson (1949) Om mikrotomsnit af Pollenkorner, Saertryk af Meddelelser fra Dansk Geologiske Forening, Bd. 11, Hf. 4, København.

At any rate if I have time perhaps I can persuade Dr. Iversen to get him to give a demonstration.

Various persons of renown as well as students have visited the lab. off & on. Three of the latter from Faegri's lab in Bergen were here for a month, followed by a visit from Faegri himself for a very short time and unfortunately I only got the chance of merely being introduced to him, but no doubt he will appear again some time. Godwin from Cambridge was also here for several days and I got to know him better. He and Iversen are planning a conference in August 16 on problems of the development of Quaternary vegetation, which is to be very select - attendance by invitation only and they are inviting only 2-3 workers from each country (in so far as there are this many in any). From U.S. are coming (at least have been invited) Deevey, Sears, L.R. Wilson. During the conversation we got talking of Barghoorn and I mentioned your name, suggesting that he might have met you when you were in England - he said that he had. After I first arrived I also met a Clair Brown from Louisiana State Univer., Professor of Botany, who is doing some work on pollen from the Wilcox - perhaps you have met him. If you have no doubt you have heard him express dissatisfaction with the work of Thompson & Potonié et al. He was visiting various lab. over here and stopped for several days there. If you will pardon my saying so, he struck me as rather a "loud-mouth" so I don't know how much credibility to give to his various criticisms, although, no doubt, some were justifiable. I have been in correspondence with Thompson & Potonié and will go down there some time this spring. So much for academic activities.

Copenhagen is a delightful city and I know my way around now very thoroughly. They have a marvelous ballet and a not-so-good opera, both of which I attend fairly regularly - being ridiculously cheap by our standards, as indeed most things are here except clothing which is more or less on a par with home. I have made various all-day excursions to various points of interest in the surrounding areas - namely Frederiksborg castle and Kronborg castle (the alleged home of Hamlet), however, have done no other traveling. Having several friends from Illinois here in various parts of Europe on post doctoral fellowships, however, we are planning Christmas in Florence, and that should be very exciting. I also hope to get to England for a month, probably in July before I sail on the 13 of August. As for next year, as far as I know am going to Berkley to work under R. W. Chaney and HOPE get my degree. As this is a fairly recent decision and as I ~~hope~~ had had some correspondence with Barghoorn as to the possibilities of returning Cambridge-ward, am now having some difficulties composing letter to effect that I have changed my mind. This is far from a personal matter in so far as Barghoorn is concerned, but I frankly have little appetite for returning to the Harvard geology dept. for various reasons, some indeed personal.

Before this develops into a book I will send you my best wishes - to you, Betty and your small son, who no doubt by now, is developing by leaps and bounds -

Sincerely yours

Jane Gray

Lignite Research Laboratory
Grand Forks, North Dakota

4 November, 1952

Dear Jane,

It was a great pleasure to get your very interesting letter of 29 Oct., which was postmarked 30 Oct. and arrived here in the quite incredible space of about 60 hours. The details of your observations on Danish palynology (or is the term in as bad odor as its author, in Denmark?) were of great value and most stimulating. I hope you will be able to keep up the flow of correspondence. Your descriptions of the trip and your amazing reception were also interesting. Don't the Europeans have a wonderful "way" about them?

I am, as you know, in concert with you about Iversen's work. I used his definitions throughout in my study of Brandon, and if I could get that thing published it would stand in this country as an advertizement for Iversen. Chronica Botanica is, however, blanketing the land with fliers about Erdtman's book, and Erdtman is pollen to most Americans who know about it.

I wrote to the Survey for a copy of the work by I. and Troels-Smith. They sent me two copies, and, although I indicated my willingness to pay, no bill was sent. Dr. Iversen wrote a very courteous note seeming to imply that the papers were complimentary copies. I don't want anyone to out 10 kr. Will you explore this subject for me?

Please tell Mr. Ingwerson about my work. Could you supply his initials and address in you next letter?

Comments on the Danish technique: I think the method of studying single grains in glycerin is very nice and certainly essential for careful study of a crucial grain. The mass-technique has merits, too. For instance, I examined literally thousands of Cyrilla grains from Brandon and saw them in every conceivable orientation (as well as state of preservation and stage of development). I would (and did) use a modification of their "liquid" technique to supplement my mass preparations.

Christensen's observations on size changes are certainly significant, but, providing that the same technique is used for reference and fossil material, does size change matter? You see, I have a large reference collection and, hence, am a vested interest. I regret that I didn't get into the pollen racket earlier, in time to have made more use of my time in Europe, or, perhaps, to have gone back for a year in Denmark. (I didn't even begin my reference collection until 1949.) Also, I was, to a very large degree on my own. I do not mean to be unflattering to Dr. Barghoorn, who has done so much for me and whom I admire, but he would be the first to say that pollen has not been one of his primary fields of work, and we learned the ropes together on the Brandon problem.

Your mention of Godwin is embarrassing in a way. I may have told you that when I went to England I was fresh out of college, had no intention of being a paleobotanist and didn't have a very thorough background even in general botany. (Though I had a decent record at Harvard and shot a creditable game of pool. As far as I can remember, I never lost to a gov. major, which is saying something.) I had never heard of Godwin or Thomas and wouldn't have known what to do about it if I had. Yet, though they would perhaps be amazed, my contacts with them enthused me enough to win me for the profession. My professional training I picked up after I left Cambridge University. It hurts to think how much more I could have done while I was there. But, if I had, perhaps I'd have been unable to take time for cycling in the Lake District, hiking in Cornwall, skating in St. Moritz, etc. I have few regrets, really. This is rather musing about what might have been.

Speaking of Cambridge reminds me that the Textbook of Modern P. A. acknowledges linguistic help from "Miss Jean Allison." I wonder if she is the same one I knew at Cambridge Botany School. If so, she will doubtless also remember me as a frivolous American who cared rather more for beer at "The Mill" than for seminars at the B. S.

I too met Clair Brown, though in passing, at the A. I. B. S. meetings at Cornell in Sept. (The Bot. Soc. is included.), which Betty and I attended during my vacation. The meetings were very enjoyable in every way--especially for the many, all too short contacts with old friends. Drs. Barghoorn and Spackman and I had a real old-time session or two.

This bombshell about Chaney and Berkeley! Will you give up pollen? Or has Chaney gone palynological?

My work continues interesting and varied. I still have no help but now have acquired a terrific set of equipment. There is, unfortunately, little time for pollen. I have made myself into, I hope, a reasonably competent coal-petrographer in a year's time, and I'm going to the G. S. A. meetings in Boston next week to give a short paper on some petrographic investigations of North Dakota lignite, illustrated with kodachromes of sections I have made. Since I am to be in Boston, I'm going to take advantage of the situation to do some bibliographic work at Harvard, which work will require my presence until Nov. 22, which, as it happens, is exactly the date of the Harvard-Yale game!

Once again may I say how much I appreciated your letter.

Yours,

December 18, 1951

Dr. Alfred Traverse
U. S. Bureau of Mines
B. L. L. University Station
Grand Forks, North Dakota

Dear Al:

I hope that you don't object to my calling you by your first name, Dr. Traverse, but I thought that perhaps removed from the various academic confinements of teacher-pupil relationship you will perhaps be a bit more lenient on me in this regards.

I am in the midst of applying for a National Science Foundation Predoctoral Graduate Fellowship for next year and being placed under the stress of acquiring the names of "four or more persons to whom we may send blanks for confidental statements..." etc., your name came to mind. If I get the fellowship for next year, I will stay here, but at least I shall be removed from the various confinements of an academic assistanceship-- not that these are too strenuous-- but sufficiently to put a crimp in what one may consider free time.

If you feel that you can do this for me, I should be happy to submit your name.

Sincerely yours,

Jane Gray

Grand Forks, North Dakota

5 January, 1951

Miss Jane Gray
Department of Geology
University of Illinois
Urbana, Illinois

Dear Jane:

Indeed, I expect you to call me Al. I might just as easily have met you on a Harvardman-Radcliffegirl social basis in Cambridge. Then, I would be lucky to get the contraction of my Christian name rather than one of my less fortunate nicknames.

I am glad you found P. & S. of B. L. interesting. I feared when I got back so soon that you were disgusted.

Betty and I are just about settled comfortably in our little house, one of those "temporary" housing jobs of the University (I have honorary faculty status at U. N. D.). It is one of the best such outfits I have seen anywhere, and the rent is very modest. Believe it or not, we like winter here. It is never above freezing, usually below zero. One knows just how to dress (warmly). The air is clear and dry. The snow is granular and dry. One's feet are never wet. Colds are rarer than I have ever seen them.

Our automobile is less enthused about the climate: one has to have an electric heater for the engine (runs on house current from a long extension cord) to get a car to start on -30 mornings.

We were pleased to get your card and hope to hear from you from time to time. Oh yes, about the business end of this letter: I'd be most pleased to do whatever I can in the way of a recommendation for you, now and in the future. If my say-so will get you in, you're in.

Sincerely yours,

Alfred Traverse