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About the Institute

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

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

January 15 1958.

Dear Arthur,

Now the moment has come when "Finska Vetenskaps-societeten" would make use of your gift, the 500 \$ that were deposited in Nordiska Föreningsbanken in Helsingfors though it was payed into the bank in Karis. In order that the Society could get the money from the bank we must have an authorization, a powers paper (I dont know the exact term!) from you giving me or Finska Vetenskaps-societeten the right to withdraw from Nordiska Föreningsbanken in Helsingfors the sum. As you perhaps remember the money was put on our joint names so that you and I must make an acknowledggment for the sum 500 \$. Vetenskaps-societeten can not without our joint names get the money from NFB. You certainly know how to write such a paper. Please send it to me as insured air letter. You will later get an acknowledgement from the Society.

I have been writing on my Italian desmids and this paper is now finished - only I must make a fair copy of it for the printer. There were 168 samples, so I have found a lot of desmids, if not by far so wonderful as the Sudanese ones.

The Sudanese Desmids are quite ready for printing. I have read the last proof (it is a tedious and tiresome job!).

I am not quite sure if I have in my letters said my thanks for the Geographical Magazine 1958? If not, excuse me and do not think I am an ungrateful dog! My best thanks for your generous gift!

Have you had any information as to the printing of Scott & Prescott Arnhem paper? Of course the printing year will be 1958? I ask this because we have quoted it in "Sudanese Desmids" and I have given 1958.

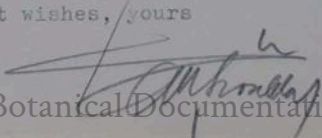
I have sent one copy of the last proof to Dr Rzóska in order that he may see it in good time. As you perhaps know he is leaving Khartoum in March this year.

Have you any drawings of the Uganda desmids. I believe you have not sent me any?

Cosmarium "tetraodontum" is changed into "tetraodon" which is the correct shape. Cf. Nordst. Index. A Greek name should not have a Latin ending.

We have had very cold - -25°C to -30°C , but in northern Lappland there has been down to -55° ! That is not for living human beings.

Best wishes, yours



Jan. 23.-58.

Dear Arthur,

Yesterday I received a wooden box containing a gift book. I was very curious when I went to the postoffice and still more so when I had got the box into my hands. So I carried the box home and opened it and - to my surprize found an extraordinarily beautiful book on American wild flowers, every one of the pictures being a masterpiece of execution. The book is not only beautiful but also of special interest to me, because I know very little of American phanerogams and so it certainly is exciting to to look at the many curious plants. But there are too a few old acquaintances from our Flora. I really do not know how to thank you for your kindness. However, you may be certain that I myself, and some of ~~my~~ my friends too will enjoy this magnificent book very much indeed. Thank you, dear friend, most heartily!

Y^{rs},

J. A. Howell

Jan 20 1958

Dear Rolf,

Enclosed are two copies of a letter to your bank authorizing them to pay the money to the Finska Vetenskaps societeten. If this is not what they want please ask them to write out a document which will be satisfactory to them, and send it to me for signature. During the nearly six years that the money has been on deposit it ought to have earned considerable interest; in this country the banks usually pay about 2% per annum on this kind of a deposit. So put up a good argument, and don't let them talk you out of it. Also the conversion into Finnish currency should be at the new rate of FM 340 per dollar, not the old rate of 290.

Glad to hear that you have finished your Italian desmids, though I did not know you had taken as many as 168 samples. I expect that pretty near all of them are the well-known European species, though you probably have some new ones. Those that you sent me were not very exciting.

As to the Arnhem Land paper, I returned the last sheets of the galley proof early in December, but have not yet seen the page proof, and don't know if they intend to send it to me for a final reading. Some months ago they gave me 1957 as the year of publication, but of course it cannot be issued until 1958. In the last issue of the Scientific American there is a review of Vol. I of the report, dealing with Art, Myth and Symbolism, and I see that the correct title is "Records of the American-Australian Expedition to Arnhem Land", and that the name of the publisher is given as the Cambridge University Press, though it is being printed at the Melbourne University Press. The FW Algae will be in Vol. III, Part 2. This affair of publishing in Australia is thoroughly unsatisfactory. As an example I have not yet been able to find out how much the reprints are going to cost, though I have asked several times; I know they will be expensive, because the price of Vol. I is \$18.50 in USA, though it is only 46.6.0 in Australia or about \$15.00. I have on hand a lot more Australian collections, partly worked up, but I shall not send the paper to Australia for publication when it is ready.

I am slowly working my way through the first rough draft of the Indonesian paper with more than 500 desmid taxa, and writing descriptions and critical comments. I thought I had all the determinations satisfactorily worked out after I returned from East Lansing last summer, but in going through them I find some that are still questionable. Just now I am stuck with a desmid that is identical with the one that Skuja called St. lacustre G.M. Smith var. rangoonense var. nov., but it is also identical (save for a few apical granules) with Joshua's vertical view of his St. cyathodes. Unfortunately the front view differs greatly from Joshua's front view of the latter, and in fact it seems to me that Joshua's front and side views of cyathodes represent different species. Also I cannot imagine why Skuja made his plant a var. of St. lacustre which has a greatly different structure. ~~It is~~ The Burmese plant is much more like St. protectum MW, also from Burma, though the apical ornament is different. So I am going to transfer Skuja's var. rangoonense to another species, either St. protectum or St. cyathodes. Skuja won't like it, but I can't help that.

So far I have done very little with the Uganda material, except to look at one or two slides from each vial and draw a few of the more interesting desmids. Are you getting ready to work on them? If so please let me know, and I will try to work them in somewhere, though I would like to finish the Indonesian paper first, and that will take several months.

I don't know what I should do in the cold temperatures you mention because I have never experienced anything colder than -12°C , and that was when I first landed in New York in 1906. Here in New Orleans we have had two light freezes this winter, with temperatures of -2°C , just enough to kill all our subtropical plants, though some of them will come back again. But Florida has been hit very hard with cold weather that has caused enormous damage to their crops of oranges and grapefruit. Just a few days ago I read that they had snow in Dade County, Florida, which is where Miami is located. Some friends of ours who went to Florida for Christmas were very glad to get back to New Orleans again and told us they had never experienced such cold there.

Dr. Reoska sent me a copy of his last report on the work of the Hydrobiological Unit at Khartoum, but I did not know that he is going to leave there. He cannot be blamed, because life and work there must be quite unpleasant for several reasons, - the climate, the lack of satisfactory equipment, and on top of all this the new political conditions. I suppose that we shall get no more Sudanese desmids, a pity.

Best regards from

January 20 1958

Nordiska Föreningsbanken,
Karls, Finland.

Gentlemen,

On April 21 1952 I deposited in the Chase National Bank, New York, the sum of \$500.00 to be placed to the credit of the Nordiska Föreningsbanken in Helsingfors.

On the same date I wrote to your head office in Helsingfors advising them of the deposit, and stating that the money was intended for eventual payment to the Finnish Scientific Society (Finska Vetenskaps societeten), and that it was to be subject to withdrawal either by me or by Dr. Rolf Grönblad. From this you will see that only one of our signatures is required. However, Dr. Grönblad has just written me that you require both signatures.

In order to settle the matter this letter is to authorize you to pay to the Finska Vetenskaps societeten the amount of \$500.00, plus whatever interest it may have earned during nearly six years, after Dr. Rolf Grönblad has also signed this letter. I assume that the conversion will be made at the new rate of 340 FM to \$1.00.

If this is not satisfactory please send me whatever document will satisfy you, and I will sign it.

Very truly yours,

AKS/s.

Feb 28 1958

Dear Rolf,

Many thanks for the microfilm of Insam & Krieger and Nellie Carter's Indian Algae. I shall have to wait a while before attempting to enlarge them on to paper because the weather is too cold at present, and the water supply much too cold for the photography process. We have had a wretched winter; in the middle of this month we had minimum temperatures of freezing or a few degrees below for eight or ten days in succession, something I do not ever remember seeing here before. Also on Feb 14 we had the largest snowfall in New Orleans for the last 59 years, as you will see from the enclosed clipping, exactly ONE INCH!!! It caused great excitement here; some of the schools closed at noon, and many offices and shops closed early to let their employees have more time to get home, because traffic was disorganised from the slippery streets, to which we are not accustomed here. I should have liked to take some photos, but I was laid up in bed at the time with a bad cold.

Glad you liked the flower book. It was not as expensive as you may have thought; I bought it for about half-price from a list of book remainders, i.e., odd lots left over on the publisher's hands. Also I have sent you a small book of Australian flowers, but this is not as good as I thought it would be, because they are arranged in bouquets and it is difficult to distinguish the individual flowers.

Prowse has sent me a dozen samples from Malaya and said he would send drawings later. Some of the samples are good, and others very poor in desmids. The only ones that are remarkable are *Xanthidium reciborskii*, which I had never seen before, and a 3-radiate version of *Phymatodocis norstedtiana*, which also occurs in one of his Singapore collections. There is a *Pleurotaenium* that I take to be *Pl. annulatum*, also new to me, but the markings are so very faint and obscure that it will require staining to bring them out, and this I have not yet been able to try. No doubt Prowse thinks many of the other desmids are new or rare, and in fact some of them are new, but I have all of them in the Indonesian material.

I have finished the rough draft of the Indonesian paper, with descriptions and comments, and have started work on the plates. In fact I had nine plates completed, but then became dissatisfied with them, because they were designed for a reduction of 2:1 and I found I was not getting enough figures on each plate, so the total number of plates would have been excessive. So I have discarded them and started again, using a reduction of 2½:1, which will enable me to get about 50% more figures on each plate, though I shall use 2:1 for the very small species.

Your request for information on the chloroplast of *Cosm. contractum* is not forgotten, but the only kind I have been able to find so far is the one shown in your sketch No. 1, i.e., the typical furcate chloroplast. Apparently this species is not so common as I had thought, and I have had some difficulty in finding specimens. Some of my sketches made in the early 1940's, which I had labelled *C. contractum*, seem to be *C. maniliforme*, because the top view of specimens in the specified samples are circular in top view.

Here is a correction for your list of correspondents: Dr. K. Biswas has retired from the position of the Indian Botanic Garden, Calcutta, and is now Director-in-charge, Medicinal Plants Scheme, Old Hindusthan Buildings, P.O. Dharantala, Calcutta 13, India.

Best regards and good wishes,
Your friend

Your dollars are forwarded to the "Vetenskapsocieteten" at a rate of 4 dollars = 319 Fmk (giving a total of 159.500 mk) and Dr Lutner has proposed that the difference should be used for paying your copies. But that is not yet settled. I shall tell later. - I had a deadly fear for our *Micrasterias*!

With best regards, yours

Karla, March 4 1958.

Dear Arthur,

I am going to tell you the latest things. Two days ago I got from Bourrelly a paper entitled: "Algues d'eau douce du Soudan Français, région du Macina (A.O.S.) - Bulletin de l'Institut Français d'Afrique Noire. (Dakar, 1957) T. XIX, sér. A, no 4, 1957."

I knew that H. was working on desmids from Africa (because Telling told me so). And now the bomb has dropped! But, fortunately it was not dreadful as expected. There are very few of our new species that will be outmaneuvered (*Artinodesmus stellifer* is one). But I think you will very soon see for yourself. There are more than 50 printer's errors (or errors made by Mr H.) The figures are not so beautiful as ours. There are many misinterpretations or wrong identifications.

Today Hans Lutner telephoned and told me that our paper is printed and he has sent the 3 first reprints to me, one of which immediately will be sent to you. I hope it will be arranged so that you have nothing to pay for your reprints.

I just received your letter of 28 Feb.
Was there not a copy of Skjega Schreb. Thunb. 2 in the folder
that I sent? I have here some more film copies for you
Schmidle 2 c.

March 13 1958

Dear Rolf,

Many thanks for air-mailing the first copy of our Sudan paper, which came two days ago. And this morning I got a copy of Bourrelly's paper on the French Sudan. Ours makes a much better appearance than his, and I think our identifications are more accurate. Bourrelly seems to be almost as careless with his spelling as Irene-Marie!

No doubt you have sent a copy also to Prowse, and I think he ought to be pleased with it. I shall ask Bourrelly to send Prowse a reprint of his paper.

Glad to note that the money has finally been paid to the Soc. Sci. Fennica, though I am disappointed that you only got FM.319 per dollar. I understood that when the firmak was devalued last year the new rate was 340, the same as the tourist rate that I got in Finland. However, it is better than the old rate of 280, and if you can induce the Society to apply the excess to the cost of my reprints of the Sudan paper I shall be glad, of course. The bankers have the upper hand in all such financial transactions, and there's not a d--- thing you can do about it.

Yes, there is a third paper on the microfilm that you sent me, but I was not able to make out what it is, because my film projector handles only the film with small perforations along each edge. My enlarger handles the unperforated film, but its lamp is not strong enough to make the image easily readable. I have not yet made the enlargements because our tap-water is still too cold.

From Minoru Hirano came another installment of his Japanese desmid-flora. This work is valuable if you know enough about desmids to catch the many mistaken identifications, but it may cause trouble for beginners. Here are some of them that stand out at a first glance:

- Pl. LXVI. *Coem. margispinatum* Hirano is *Xanth. spinosum* (Josh.) W&W., though I will admit that it might just ~~have~~ as well have been placed in *Cosmarium*.
- Pl. XXX. Fig. 8 is certainly not the typical form of *X. armatum*. I have never seen such an elongated specimen. It might be called var. *elongatum*.
- Fig. 9 is not a Xanthidium, but a biradiate *Staurastrum*, close to the type of *St. freemanii* W&W.
- Fig. 20 also is not a Xanthidium, but probably a forma of what I have named *St. freemanii* var. *nudiceps* fa. *biradiatum* Scott & Presc. (in press).
- Fig. 7 has the right number of spines for *X. cristatum*, but the shape of the cell is quite unlike that species. I don't know just what it is.
- Figs. 16, 17 are *Arthrodesmus apiculatus* Josh., and there is not the slightest reason that I can see for transferring it to Xanthidium.
- Fig. 20 is probably a form of *A. bulnheimii*. It does not agree with *X. inchoatum* Nordst., which should be transferred to *Arthrodesmus*, if nobody has done it previously.

There are probably a good many more if you have the time to go through his papers carefully. His illustrations are good and clear, but many of them look as if they were drawn by a draftsman who specialized in drawing machinery and to working only with a straight-edge and compass.

Best regards from

ROLF GRÖNBLAD
DR. PHIL.
HELSINKI, FINLAND

April 8 1958.

Dear Arthur,

I am glad you are satisfied with the printed appearance of our Sudanese Desmids. Rzóška is moving to London and his new address is "6 Blakesley Avenue, London W.5. (which you perhaps already know?). He has promised some new material from Sudan.

In my opinion the publishing of Bourrelly's paper just before ours bears witness of no tactful behaviour, because he did very well know of our work. Indeed, he must have been in haste to have his paper published before the end of 1957, because a diagnosis from that point of time must be written also in Latin or else it will not be accepted (cf. Code). Bourrelly has no Latin diagnoses. Contrary to him Thomasson in Uppsala seems to me a fine young gentleman: he might have published both *Micr. sudanensis* and *M. incredibilis* if he wished, but he did not do so. Our "friend" Telling is also a curious chap: not a single word after having received a copy of our paper!

Don't you think it would be advisable to write a supplementary note on the species and varieties published in Bourrelly's paper and in our paper under different names. The synonyms must be cleared up. And perhaps some of Bourrelly's identifications too. Of course B. has priority as to his new names and ours are to be deleted or declared as synonyms. (e.g. *Arthrodesm. stellifer* which must be *A. Heimii* Bourrelly). I hope you have time to joint me in such a note. There is no hurry indeed, but it would be useful - I believe.

A few days ago I got Minoru Hirano's paper and I am of the same opinion as you as to his figures and identifications. He has very few vertical views which is regrettable and his figures are, as far as I can see, not true representations but somewhat schematical as you say. (*Cosmar. raciborskii* has no granules - cf. fig. XXVIII:28 - but the cell wall is covered with scrobiculations).

I hope to have my Italian Desmids finished before the summer and I should be very much obliged to you if you would consent to have a look at

my English text and make ~~the~~ language correct.

I have sent copies of "Sudanese Desmids" to: Prowse, Rich.E.Norris, Alan J. Brook, Durham College, Krishnamurthy, Edne Lind, J.W.G. Lund, Michiyo Mori, Mme de Pocques, Rich. Starr, which are not European authors. I am not sure if you sent the USA paper to authors in England, but I believe our arrangement was so.

The spring is coming very very slowly this year, we have large drifts of snow and the night temperature goes down to -10°C and more. Hannah told me that they have had very little of spring as yet.

Did you hear anything about your Australian paper?

With my best wishes,

I forgot to ask for extra copies of the plates, but it will be arranged that we get 10 copies together. How many do you need? I could send them directly if you give me the names.

R.G.

April 22 1958

Dear Rolf,

The Sudan reprints arrived this morning, in beautiful condition, and very carefully packed by the printers, Tilgman; they deserve to be thanked for their scrupulous attention.

I shall distribute the copies to the same list of names to whom I sent the USA paper, which includes all the addresses in Great Britain. Of course I shall omit those to whom you have already sent. Perhaps it would be better for you to send to Dr. Kozaka, since you have been corresponding with ~~him~~ him, and no doubt he will need more than one copy. Do you know if he has a new position in London? His address seems to be somewhere near the British Museum (Natural History) in Cromwell Road.

Yes, I think it would be a good idea to publish a short paper to clear up the synonymy between the names of those species published under different names by Bourrelly and ourselves, and after I finish drawing the plates for my Indonesian paper I will go through the two Sudan papers and pick out those species that require attention. Just now I am having a little trouble with a desmid that I had identified as Cosm. auriculatum Reinsch var. bogoriense Bern., which Bourrelly transferred to C. subauriculatum W&W var. bogoriense (Bern.) Bourr. in his Madagascar paper (1949, Tsimbazaza), because of the open sinus. The plant itself is fairly common, and is identical with the figures by Bernard, Skuja, and Bourrelly, but it is also identical with Turner's figure of C. auriculatum (1892, 7:35). The shape of the cell and the sinus are not like those of C. subauriculatum W&W (Koh-Shang, 1902, 2:19, but are identical with those of var. truncatum, *ibid.*, 2:20), as you will note from the sketches enclosed; in fact the two plants differ only in the shape of the lateral angles and the number of teeth. Because of this, Bourrelly's transfer may be correct, but I cannot judge because I do not have Reinsch's original figure of C. auriculatum. Would you be kind enough to make a small tracing of Reinsch's illustration for me?

Are you not judging Bourrelly a little harshly? You yourself were trying to get our paper in print before his! And all of us are only too anxious to see our works published as soon as possible. My North Australian paper is still dragging its weary way through the red tape of Australian publishing. Three weeks ago I returned the corrected page proofs, and I hope I shall not have to read any more proofs; if I do I shall be reciting the text in my sleep! Of course it is a very big job to assemble and edit and print the reports of a large expedition like this one, comprising a dozen different "ologies", and written by more than a dozen specialists in several countries, but the editors have made it even more difficult for themselves and the authors by insisting upon the most trivial minutiae of spelling, punctuation, bibliographical abbreviations, etc. The plates have been reduced to a smaller size than the one that I was given originally, with the result that some of the finer lines have not reproduced very well. The typography is not good, in my opinion, and much inferior to the beautiful work in our papers printed in Finland, or the beautiful work in Maj-Britt Florin's paper on Swedish plankton, which I suppose you have received.

Of course, I shall be glad to look over the English text of your Italian paper. You do not need to ask such questions!

I shall be obliged if you will send extra copies of the plates to Prescott, Brunel, Irene-Marie, and Hahn. Those are the only ones who maintain iconographs, as far as I know, in this country.

Best regards, from

April 29 1958.

Dear Arthur,

Thank you for your last letter, and for the American Scientific. I send now all these American papers (incl. Geogr. Mag.) to the Geographical Society of Finland which is very grateful to receive them. There is also included the Australian paper "Walkabout" which I have subscribed for myself. First one or two of my friends in Karis read them, then they are sent to Helsingfors where Hans Luther and his father are interested to read them and then at last to the Geogr. Soc. So you see you are doing a lot of good not only to me.

Perhaps I have been judging Bourrelly "a little harshly", as you say. But it was my first impression when making a comparison with Thomasson. And I wonder why B. didn't wish to talk to me when we met at the Congress in Helsingfors? I know that he can speak both English and German (he was a German prisoner during the World War II and was allowed to work at the Plön Biological Station). He is something of a mystery to me.

Maj-Britt Florin's paper I have received. There are some identifications which seem somewhat dubious to me: Such are Fig. 22:4 which I do not think belongs to *C. acutum* which is more acute; it is difficult to judge from one figure, but it seems more like *C. cornu* (?). Fig. 1 on p. 126 has no vertical view, and is like *Arthrodesmus* -. Fig. 27:2 is not like *C. depressum*. Fig. 28:3 is perhaps more like *S. paradoxum*, but without a vertical view it is indeterminable. Fig. 29:9 may be *S. iotanum* or something else, but not *S. tetracerum*. (?) Fig. 29:5-8 are apparently representant of the same species which Georgewitch called *S. paradoxum* var. *osceolense* Wolle f. *biradiata* n.f. (quite different from Wolle's *S. paradoxum* v. *osceolense*!). But I do not see any closer affinities between this desmid and *S. tetracerum*. So I should rather like to call it "*S. cameloides*" - if not *S. eravatium* var. -??? If fig. 30:4 is *Arthr. sellatus* (or "*Staurodesmus* if you like), what is *Art. incus*? Fig. 31:3 I should call *S. Thunmarkii*, because I am not sure that it is only a "*facies biradiata*" of *S. cingulum* v. *obesum*. (If it is so, as Miss Florin seems to believe, it must not be called "*fac. Thunmarkii* Teil" because "*facies*" is not to be treated as a taxon; the correct name would be "*S. cingulum* (W. & W.) G.M. Smith v. *obesum* G.M. Smith *fac. biradiata* (without any author's name). Fig. 34:4-6 are monstrosities difficult to explain taxonomically. Fig. 35:5 looks very much like *C. monomazum* but no vertical view is given, so the question remains unsolved.

I am glad that you got your copies in good condition. If they have sent by mistake a bill to you, you must not pay it, because there has

been an agreement upon arranging the matter without your paying for the copies sent to you. Later I shall send extra copies of the plates.

When I think over the situation concerning the Congress in Montreal, when I am considering the state of my health especially and the considerable costs too, then I have arrived at the conclusion that it will perhaps be wiser not to attend at the congress. I am too much dependent on my daily habits of living, on my medicines and on my food. I think it is too late to me to do such a complicated and tiring journey. "The spirit is willing but the flesh is weak" is a Swedish saying.

I very well understand your distaste and weariness of proof reading. When I have read my last proof it is as if a heavy burden had vanished from my head. But, of course, it is badly needed, this proof reading. And what you call "the most trivial minutiae" are not without importance.

To Rzóška I have sent 10 copies as he asked, but I do not know if he has got them.

Reinsch: Contributiones ad algologiam et fungologiam (1875).
pag. 83 and pl. xiv fig. 5a, 5b.

"Cosmarium auriculatum. Semicellulae latae semioellipticae margine terminali ~~paulo-productis-nodulis-basis~~ late rotundato polis paulo productis auriculatis et spinulis 3is - 4is armatis incisura acutangula extrorsum disjunctae introrsum se adtingentes; isthmi latitudo diametri transversalis dimidio paulo magis; cytioderma dense subtiliter verruculosum; diameter transversalis diametro longitudinali aequalis.

Longit. 0.05 mm Latit. 0.05 - 0.053 mm. Franconia.

Tab. XIV. fig. 5a Specimen vacuum a fronte (72/1); fig. b semicellula speciminis vacui ab inferiore parte."

It seems to me that Turner's species and ^{his} var. verrucosum are scarcely different: the "verrucae" being most probably pore organs (?)

But they are very much like Reinsch's poor figures. (I can send a photocopy if you wish.)

I shall not enter any discussion about *C. auriculatum* because I know too little of that species, but I enclose copies of my drawings from material from Malacca and Uganda.

Best regards,

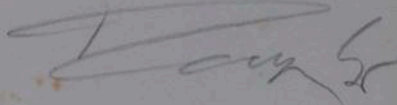
ROLF GRÖNBLAD
KARIS
FINLAND

May 16 1958.

Dear Arthur,

Thank you for your last card. The mistake concerning Spondylo-
sium clepsydra was unfortunate, but we have, neither of us, a complete icono-
graphy nor a complete list of names the consequence of which is that such
mistakes cannot always be avoided. And also people who have lists cannot
always avoid such happenings. I do not take it too seriously. I have not-
iced that my memory is getting weaker with increasing age. There is nothing
to do. I remember now very well that I have seen Beck-Mannagetta's species
but his figures are rather poor so I have not paid enough attention to them.
As far as I can judge B.-M.'s species most probably is one of the many ~~forms~~
forms of Sp. planum. That is, however, of very little help to us, because
our specific name cannot be used. I propose that we give to it a new name:
Spond. subclepsydra (or pseudoclepsydra or clepsydriforme or clepsydroides)
The new name can be published in your or my or our next paper, in which
matters very little.

Yours



June 11 1958

Dear Rolf,

I am sorry to have neglected writing you for so long. My only excuse is that I have been busy drawing plates for the big Indonesian paper. I have just finished *Xanthidium*, halfway down Plate 41 and am starting *Staurostrum* on the same plate, so it looks as if there will be about 60 plates all told!

Thanks for the reprint of your paper on conjugation in *Metrium digitus*. This is a very good account of a phenomenon that is seldom observed, and one that I have never seen.

It is, of course, a great disappointment to learn that you have given up the idea of attending the Congress at Montreal next year. The expense is considerable, but I hoped that you would be able to obtain a grant that would pay at least part of it, if not all. In fact I thought I might be able to help you in getting such a grant by obtaining some official invitations to you to attend the Congress. Such invitations would not provide any funds, but they would support your application to the Finnish Government. I am sure I could get such an invitation from the Phycological Society, and I believe that through Brunel I might be able to get for you an invitation from the Congress itself. So if there is any chance of your reconsidering, please let me know.

Naturally I can understand that such a long journey would mean quite a change in your way of living and your food; your medicines I suppose you could take with you. But would it be any greater change than your trip to Italy, where they live on spaghetti and cheese?

How about the name *Spondylosium horametrum* instead of *Sp. clepdydra*? I have two papers in press, but it is too late to include the correction in either of them because the final corrected page proofs were returned some time ago. My next short paper will not be printed until next year, so perhaps your Italian paper will appear before then.

The group of desmids included in *Cosm. auriculatum* and *subauriculatum* shows a considerable range of variation. Apparently nobody has known much about either of them, which accounts for the several different interpretations. Here are my views, for what they are worth:

Reinsch's original plant is characterized by the highly arched dorsal margin, the somewhat produced basal angles, the shape of the sinus which is linear and closed at the inner end, a fairly narrow isthmus whose width is about one-half of the width of the cell, and the "cytoderma dense subtiliter verrucosum", which West & West interpreted as "granulate". I am inclined to think that the granulate appearance was an error of observation, and also in the case of Turner's var. *verrucosum*.

Bernard (1908) created the variety *C. auriculatum* var. *bogoriense*, but his two illustrations show what I think are two different plants. His figure 181 has a sinus that is closed and linear at the inner end, while fig. 182 has a sinus that is angular at the inner end and widely open and an isthmus that is proportionately much wider. Thus I believe that the name *C. auriculatum* v. *bogoriense* should be applied only to his Fig. 181, since the shape of the sinus and the width of the isthmus correspond fairly closely with those of the species, and that his fig. 182 should be assigned to *C. subauriculatum*. Bourrelly (Tzimbazaa 1949) transferred Bernard's var. *bogoriense*

to subauriculatum, but the plant that he transferred is the one with the wide isthmus and the small angular sinus, and this I believe was incorrect.

After seeing your sketches of plants from Uganda and Malaya I looked for them myself, and found several in Uganda #2, but in Prowse's Malaya #148 I could find only a single specimen, and that not a very good one because the teeth were poorly developed, and there were three teeth at each basal angle instead of the two in your sketch. These plants from Uganda and Malaya, and also Bourrelly's from Madagascar and French Soudan, resemble Bernard's fig. #182. If I am correct that Bernard's var. bogoriense should be reserved for his fig. #181, then a new varietal name is required for his fig. #182. An appropriate name would be var. "eurysthmius" or "euristhmius" of *C. subauriculatum*.

Best regards until later.

Sincerely,

Dear Arthur,

I had today a letter from the Secretary of the Congress with a promise, or half promise, that financial assistance can be provided. That is already something positively encouraging. So I am going to fill a form that was enclosed. Of course it is very difficult for me to estimate the costs, but I shall try to do my best. Furthermore I shall ask if there may be any possibilities to get something from the Finnish scientific funds. If so, then perhaps an invitation, as you told me, would be useful.

Kind regards,

15.VII.1958.
Karis.

HOLF GRÖNBLAD
DR. PHIL.
KARIS, FINLAND

1.VII.-58.

Today is my first holiday, so my first duty is to write to you. I have had too much to do and I was always tired when evening arrived.

Many thanks for your kind letter and Amer.Sci. Your "big" Indonesian paper certainly must be something of a very useful monograph. I hope it will appear in print in a short time. You must have been working very hard though you are in the lucky position of having possibilities to dispose of all your time on desmids. Did you take up the question about *Euastr. turgidum* - *Micrast. Moebii* and the other question about *Micr. anomala* - *Xanth. bifurcatum*? I hope you have arrived at some good arrangement? Not long ago prof. Heimans wrote me a letter asking my opinion as to *Eu. turgidum* - *Micr. Moebii* &c. The arrangement in Krieger's Flora is, however, not satisfying. These two species (and perhaps a few more) should be placed in one genus, either in *Euastrum* or in *Micrasterias* (I think I should prefer *Euastrum*). Now prof. Heimans who has some Indonesian material at his disposal, proposes a third possibility: he wishes to create a new genus "*Verrucastrum*". "Of course this new genus would have to include quite a number of species e.g. a great part of those numbered 130-147 in Krieger's list in Rabenhorst 1937". (quotation from Heimans). This seems to me not good. I told him so and I also told him that you are working on Australian and Indonesian desmids and I proposed that he should write to you. If he has not written perhaps you would tell him your opinion. It seems to me quite unnecessary that two independent and perhaps different opinions should be published nearby simultaneously (as e.g. Bourrelly and Gblad, Prowse, Scott).

Mr Thomasson, who has been in Karis recently, told me that Bourrelly's paper was incorrectly antedated 1957 though the volume of which it is a part was published in 1958. That looks rather strange.

Spondylosium horametrum seems to me good instead of *S. clepsydra*. Perhaps the change could be put into my Italian paper as you propose, or in "Algological Notes VII" which is to be published in Bot. Notiser next autumn (?). As you know I have no complete list of desmids published, so the new name would have to be controlled by e.g. Prescott?

I am sending enclosed a copy of a drawing made from Rabehhorst's exsicc. No 1727 which must be *M. Moebii* though both Borge and Krieger have described the central area as covered with granules: there are no granules but regularly arranged pits, scrobiculae, which appear circular or triangular depending on the higher or lower position of the microscope tubes. Gutwinski: Alg. Java (1902) p. 604 says: "*Membrana scrobiculata*" (his var. *javanica* seems scarcely distinguishable).

As to your proposal concerning the Montréal Congress I do not know what I should say. Of course I am very much interested in the prospect of seeing America and meeting my American friends. But still I have my doubts as to my health. I am not what I was when you were in Karis. And furthermore the costs are very considerable and I doubt whether the "State Natural Sci. Council" would give me money enough especially because I am not able to get up for a lecture. In fact, I have never in my life lectured! Nevertheless, I shall ask my friend Hans Luther what he is thinking about my possibilities. Anyway, I thank you most heartily for your very kind proposal.

Thank you very much for your careful perusal of my paper on the Italian desmids. - *Euastrum monocyllum* is correct, not *monocylum* as Krieger has written the name. (also in his quotations of earlier authors K. writes *monocylum* apparently because he thinks "*mononcyllum*" wrong). I have asked an experienced Greek teacher and he answered that both *mononcyllum* and *monocylum* are possible. Consequently Nordstedt's name must not be altered. Furthermore Krieger has made *E. monocyllum v. germanicum* a species "*E. germanicum*" and the old *E. monocyllum* (Nordst.) Racib. he has as a variety of *E. gemmatum* - as it first was described by Nordstedt. I believe *E. monocyllum* and *E. germanicum* are much closer to each another than *E. monocyllum* and *E. gemmatum*. What is your opinion? Please, tell me. (Krieger's fig. 10, 11 do not at all agree with Nordstedt's original figure 17 (1880).

Kind regards,

I have sent copies of my earlier papers together with the Sudanese Desmids to "Ing. Kurt Förster, Uetersen-Holstein, auf dem Flidd 17, W. Deutschland". No answer did I get and when I sent copies of the plates of Sud. Desm. they were returned with "Inconnue". What is **amiss**? Did you have any communications with him? Do you know anything?

July 23 1958

Dear Rolf,

It was good to have your nice long letter of July 1 and your note of July 15. The tentative offer from the Congress to pay part of the expenses of a visit to Canada is certainly a good beginning, and there is no reason why you should not send in the application, for even if you feel next year that you are unable to go, you could always notify them or return the money. As to the costs, your friend Baron de la Chapelle could give you some good information on living costs in Canada, if he is available. If I go to Montreal I shall estimate the cost at \$15.00 per day in addition to the cost of transportation there and back. This would include hotel, meals, and incidentals such as taxicabs, tips, etc. You could perhaps do it for less, but not less than \$12.00, I should think, because you would not go in for American extravagance. Let me know if there is a possibility of your getting assistance from the Finnish scientific funds, and if so I shall start pulling whatever strings I can get hold of, to help you.

n About 5 weeks ago I had a letter from Prof. J. Heimans, and I thought that someone must have suggested that he write me. No doubt he used much the same language as in his letter to you. In reply I told him that I was engaged on a big paper on Indonesian desmids, describing more than 500 taxa, with some 60 plates of illustrations. I hoped that this might induce him to defer publishing his own results until this paper appears, which cannot be until next year, however.

As to his proposed new genus "Verrucastrum" I told him that I could not see that anything much would be gained. His proposal is based on "the peculiar ornamentation of the cellwall, and transitions in outer form to *Micrasterias*". I pointed out that the main feature of the ornament is the central tumour with a rosette of granules or verrucae, but that this same ornament or a very similar one is present in many other species that have the typical *Euastrum* shape, with the deeply incised arical lobe. So the only thing that would be left for him to use in differentiating the group would be the outer shape, and I mentioned that in all the more highly evolute genera there are groups that differ greatly from the original generic concept, and that many attempts have been made to separate these genera into different genera or sub-genera. I said that if he published the new genus, I was afraid it would not be generally accepted, and would eventually fall into desuetude along with such other names as *Calocylindrus*, *Cosmaridium*, *Pleuotaeniopsis*, *Schizacanthum*, etc. I added that this was merely my personal opinion, and that it would be advisable for him to ask other desmidiologists about it.

I further told him that I had already written a short paper, intended for publication in *Reinwardtia*, transferring *M. moebii* to *Euastrum*, and that it was then in Prescott's hands for approval. This paper was written just before last Christmas, but Prescott went away for the Christmas holidays, and immediately afterwards left for his third expedition to Ecuador, whence he returned only at the end of May. So he did not receive it until early in June. Now I have his reply, not only approving the transfer but also my suggestion that *M. moebii* should be combined with *E. turgidum* as a variety. Then it became necessary for me to investigate *E. turgidum* more carefully, so as to be able to show exactly what differences and similarities exist between it and *M. moebii*. So I checked all the descriptions and illustrations of both plants that are available to me, in Turner, Moebius, Borge 1896, Raciborski 1892 and 1902, West & West, Krieger, Bernard, and finally Playfair 1908, and then made the astonishing discovery that old man Playfair had made exactly the same transfer just half a century ago, as *E. turgidum* var. *moebii* Playf. (I think the correct nomenclature should be *E. turgidum* v. *moebii* (Borge) Playf.). This is an inexcusable oversight on my part, for which I can only blame my bad memory, for I have had a complete set of Playfair's works since about 1952, and have had to refer to them on many occasions, so that I must have read the two pages in question.

But it's funny that everyone else (except Krieger) has also overlooked this paper of Playfair's. Krieger lists Playfair's names for this and other varieties of *E. turgidum* among his excluded and doubtful species on p. 658. Neither you, nor Telling, nor Heimans, nor Haljnah, nor Prescott seems to have known about ~~ix~~ the transfer, or to have forgotten if they had seen it. G.S. West (1912a) admits that he had never seen *E. turgidum*, but says that Playfair had confused it with *M. moebii*.

After making the discovery, my first impulse was to abandon my short paper, and in the large Indonesian paper simply to use Playfair's nomenclature. But after reflection I came to the conclusion, with which Prescott concurs, that it will be better to rewrite it with the necessary changes, in order to bring the matter to the attention of present-day desmidiologists, and to present my arguments in favor of *Euastrum*, which I think are pretty strong and convincing. I am enclosing a copy of the first 5 pages of my short paper, which I had written before I made the discovery about Playfair, and which will not have to be rewritten, and if you care to comment on it I should be very glad to have your opinion. You do not need to return them.

Your drawing of the specimen from Grunow's Insel Banka material, I would refer to the specific form of *E. turgidum*. As far as I can see, these are the only differences between *E. turgidum* and *M. moebii*:

1. *E. turgidum* sometimes attains a considerably greater size than *M. moebii*, up to 150 μ long for the species, and up to 170 for var. *simplex*, according to Krieger. I have not seen any of these sizes.
2. In *E. turgidum* the polar lobes may sometimes be simple, not divided (vide Turner and Playfair).
3. In front view, the "neck" below the polar lobe is usually proportionately wider than in *M. moebii*.
4. The central tumour in *E. turgidum* seems to be relatively larger than in *M. moebii* and the pits usually triangular. I have seen only a few *M. moebii* with triangular pits.
5. In vertical view *E. turgidum* is more obese than *M. moebii*.

As you will see, these are differences only in degree, not sufficient to place the two plants in separate species, let alone different genera.

of *M. moebii*

As to the character of the ornament on the central tumour: I do not have Mobius' original description (1894). Borge (1896) says it is granulate, but I am inclined to think that this may have been an error, since granules have not been reported since then except in var. *javanica*. True that Krieger (1939) says "Warzen", but note that also he says that the description is from material collected in Koh Chang, while West & West (Flora of Koh Chang 1901) say "in centro semicellularum tumore magno dense scrobiculato". Krieger also says of *M. crux-africana* that the tumour is covered with triangular warts arranged hexagonally, but I think you will agree that this cannot be correct. Seemingly Krieger never saw actual examples of *M. moebii*. Gutwinski (1902) gives a rather poor drawing of his var. *javanica*, and shows the central tumour surrounded by something that may be interpreted as a ring of granules, though I have seen specimens of other varieties in which the deep pits create a similar effect at the edge of the tumour. G.S. West (1912a) gives an illustration of his interpretation of var. *javanica*, showing the tumour to have six or seven pits in the center, edged with a ring of emarginate verrucae. Krieger copied G.S. West's drawing on his Pl. 107, figs. 9-11, but his statement that fig. 11 is from Gutwinski is wrong; it is from G.S. West and is the side view of the same specimen shown in Krieger's figs. 9 & 10. I have not seen var. *javanica*, nor the specific form, but in var. *burmense*, v. *tetrachastriforme* fa. *latum*, v. *diploanthylum*, var. *insolitum*, the tumour is definitely scrobiculate, and frequently a hexagonal arrangement of the pits can be seen.

I see now that *monocylum* is correct, nor *monocylum* as Krieger has it. Of the three species, or varieties, *monocylum*, *germanicum* and *gemmatum*, I know only the latter, and the American form differs considerably from the European (Presc. & Scott 1945). Judging from the pictures on Krieger's Pl. 92 I should say that *monocylum* is closer to *germanicum* than to *gemmatum*, because of its narrowly rectangular vertical view, which lacks the prominent protuberances seen in the top view of *gemmatum*. However, I do not

have Nordstedt's 1880 paper, nor Raciborski 1885 nor Schmidle 1895, so Krieger's illustrations are about the only ones I have to go by.

It is indeed rather strange that Bourrelly's Sudan paper is dated 1957 but issued in 1958, though such occurrences have happened a good many times before because these scientific journals are not infrequently some months behind their schedule. One example is Turner's paper dated 1892, which Prescott insists should be cited as 1893 because he has somewhere found out that it was not issued until the latter date. Presc. & Scott, *Micrasterias I* is dated "1942, published 1943", and the *Trans. Am. Microsc. Soc.* for several years has three or four months late in appearing. However, I think Bourrelly's paper must have been issued slightly before ours, for I received my copy from him on Nov 13 1958, and it would take about three weeks in transit, while ours is marked "Printed Nov 2 1958". Perhaps Hans Luther could ascertain the exact date of issue through his library.

The address that you give for Kurt Förster is an old one, and I am sorry if I have forgotten to give you the new one. Here it is:

Ing. Kurt Förster,
Gewerbeoberlehrer,
(13b) Pfronten-Ried, 199a,
Allgäu, Deutschland.

I have not heard from him since September 1956, but about a month ago I had a letter from one of his friends, Franz Eckert, Münchenstr. 83, Ingolstadt, Germany, saying that Förster "musste im Oktober 1956 einer schweren Darmoperation unterziehen, an deren Folgen er immer noch zu leiden hat". I have sent our USA and Sudan papers to Eckert, who has developed a method for making permanent mounts of desmids in Caedax and Hyrax. A couple of years ago Förster sent me three Typenplatten prepared by Eckert, with a dozen of my Florida desmids prettily arranged in rows like diatom type-slides. Now Eckert says he is sending me some more slides with new desmids from Brazil collected by the late Dr. Lützelburg in 1912-27. He got the material from Dr. A. Pochman of Krefeld, who holds the priority on it. So far these slides have not arrived.

I have asked Prescott to check the name *Spondylosium horazetrum*, but he is now teaching a summer course at the Univ. of Montana Biological Station in Montana, and does not know if there is anyone at East Lansing who can check it. He will not return to Michigan until about the middle of August.

Best regards from

October 29 1958

Dear Rolf,

Of course I shall be glad to give you my opinion on the new Sudanese desmids. Send me your drawings and I will return them promptly.

You did not lend me your Playfair papers, and they must have been misplaced somewhere. I have my own complete set, that I obtained from Australia eight or nine years ago

Your drawing No. 3667 from Labrador can probably be identified with *D. longatum*, though the cells are considerably shorter than Wolle's measurements. It is also like *D. gymmozygoforme* Presc. & Scott (1942), which you think is identical with *D. longatum*. I think I have told you that *D. gymmozygoforme* is biradiate, not triradiate as Prescott described it, or at least I have never seen a triradiate specimen. I do not have Nordstedt's figure of *longatum*, which he transferred to *Gymmozyga*, and the only other illustration I know of is Irene-Marie's in *Hydrobiologia* Nos. 1-2, 1949, Pl. 5, Fig. 4. His dimensions are similar to yours. I-M gives a very curious and incorrect reason for assigning it to *Desmidiium* instead of *Bambusina*, and since I believe you do not read French very well I give a translation of his remarks on pp 43-44:

"This species has all the characters of most species of *Bambusina*, and at first we placed it in this group. A closer study of the cell and its reproduction by doubling, without invagination or interior folds of the membrane between the new cells, shows that the Old Master was correct in assigning this species to the genus *Desmidiium* rather than to *Gymmozyga* (*Bambusina*). We have specimens in division which look exactly like the first two cells of Wolle's Fig. 6 on Pl. LX. This must be the 1892 edition which I do not have. Scott without invagination such as is always seen in the reproduction of *Bambusina*. The apex is slightly constricted at a short distance from the end, as in *Pl. subcoronulatum* var. *detum*".

In the first edition (1884) of Wolle's book, Pl. XLIX, Fig. 6, he shows four short diagonal lines at the junction of the first two cells which are in the process of division, and I interpret these as indicating the infolding of the wall. In any event, Irene-Marie is wrong, because all species of both *Desmidiium* and *Bambusina* develop in the same way, with inward folds of the new cross-walls. I am not at all clear as to the distinction between *Desmidiium* and *Bambusina*, and I should be grateful if you would tell me your ideas about this.

One feature of your Labrador plant that favors its assignment to *Bambusina* is the longitudinal striations, which as far as I know occur only in *Bambusina*. I am enclosing a sketch of a somewhat similar plant from Sumatra, which differs in the decidedly angular profile of the central inflation, instead of the rounded profile of *D. longatum*. I propose to call this *Bambusina brebissonii* Kütz. fa. *constrictum* fa. nov., but this can be changed if you think it not correct.

I suppose you have noticed in the 1956 Code p. 202, that the plant formerly known as *Gymmozyga moniliformis* and *Bambusina Borreri* is now to be called *Bambusina Brebissonii* (Kuetz.) Kuetz. Do you know why the author's name is given twice, the first time enclosed in square brackets? In *Gladophora* the type is *G. oligoclona* (Kuetz.) Kuetz., with the first citation of the author's name in ordinary parentheses. This is correct, because Kuetzing originally named it *Conferva oligoclona*.

Your drawing No. 6837 is *M. Moebii* var. *javanica*, as you have noted;

it is the only variety that has a ring of verrucae around the central tumour, and this, of course, is definitely a *Euastrum* character.

Drawings 6838 and 6839 show forms that differ from anything that I have seen either in illustrations or as specimens. They are considerably larger than the dimensions given by Krieger for any of the varieties of *M. Moebii*, and almost equal to the largest figure that he gives for *Euastrum turgidum*. The very wide neck and small projection of the polar lobules resembles *E. turgidum*, but the lateral lobes are produced instead of being rounded. They might be considered as a new variety.

Drawing No. 6969 shows the top view only; it looks more like *M. Moebii* than *E. turgidum*.

Drawing No. 6848 corresponds very well with a plant that I have from Borneo, which I am calling var. *tetrachastriiformis* f. *latum*, f. nov., because its general shape in front view is nearest to West & West's variety.

I am still struggling with the problem of whether to include *E. turgidum* with *M. Moebii*, and don't seem to be able to make up my mind what to do with them. Prescott is not of much help, because he is so damnably busy that he simply does not have the time to give serious thought to the question, and additionally he has not seen any actual specimens of the plants, though I have sent him samples from the collections. From one point of view it would be desirable to ~~include~~ make *M. Moebii* a variety of *E. turgidum*, as Playfair did (1908), because some of the varieties of *Moebii* are hardly distinguishable from *turgidum*. On the other hand it seems to me that some of the newer varieties, such as var. *tetrachastriiformis* W&W, var. *insolita* ~~Prescott~~ Scott & Presc. (1958), and var. *diplocanthyllum* Scott & Presc. (unpubl.) differ too much from the type of *E. turgidum* to be included with it. I should like to know your views about this.

Another possible solution would be to make *E. turgidum* a variety of *Euastrum Moebii*, but I don't know if this could be done because of priority; *E. turgidum* was described by Wallich in 1860, while *M. Moebii* was only discovered by Moebius in 1897. What do you think about this?

Prescott tells me that he has no record of the name *Spondylosium horanetrum* having been used previously, so you would be safe in publishing it as a correction to *Sp. clepsydra*.

I asked Dr. Jules Brunel for information as to the exact date of publication of Bourrelly's Sudan paper, and here is his reply:

- (1). My own copy (Brunel's) of the reprint was received March 18 1958.
- (2). The complete issue of *Bulletin de l'I.F.A.N.*, Vol. 19, Ser. A, No. 4, is dated Octobre 1957, but the printers' colophon on the last page of the issue is dated Novembre 1957. I could not find any date more exact than that in any part of the issue.
- (3). If you want to enquire from the publishers, here is the address: Institut Francais de l'Afrique Noire, B.P. 206, Dakar, Afrique Occidentale Francaise.
- (4). If I were you, to make doubly sure, I would also enquire from the printers: Imprimerie Protat Freres, a/s de M. Th. Monod, gerant, Macon, France.

I did not write to either of these addresses, because it seems to me there can be no question, in view of the printers' date November 1957, that Bourrelly's paper was actually and "effectively" published before ours.

Brunel further writes as follows:

"I take this opportunity to point out that the first of two Errata printed on a slip glued into the brochure, and reading thus: *Page 34, line 8 from bottom; after Fig. 226, 227 add: 236, 237" implies that a more important correction should be made at two other places:

- (1). The whole text on *Cosmarium Salisburii*, p. 32, should be deleted.
- (2). Page 50, line 2 from the bottom, should read (*Cosmarium*) *transvaalense* instead of "*Salisburii*."

"Am I right in assuming that these corrections should be made, or am I mistaken in some way? If I am right, you might perhaps inform Dr. Grönblad, who would possibly distribute another list of Errata including the two suggested above".

I have looked over my Sudan drawings, but do not seem able to determine what is wrong. Will you look into the matter, and advise me?

Glad to say that at last I have finished drawing the plates for the big Indonesian paper, 63 of them, and Mrs. Perine is now inking them. Hannah is doing the Latin diagnoses, and when I get them I shall have to retype the paper in final form and then send it in for publication.

Have you heard any more about the possibility of getting a grant to enable you to go Montreal next year? I sincerely hope that you can make it.

Best regards, from

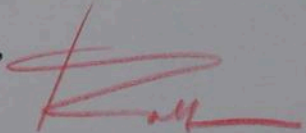
Dear Arthur,

Would you be so kind as to look at my short paper on
Cylindrocystis and make such corrections and emendations
as you find useful.

I have got your letter and I shall very soon give my answer.
Thank you very much for the copy of Arnhem-Land desmids.
It is excellent. I was glad to see that you do not accept
Spinoclosterium!

Kind regards,

11.XII.-58.

A red handwritten signature, likely 'R. S. G.', written in a cursive style.

I do not know when your 70th birthday was, but I know it is sometime
in 1958. So please accept my most hearty congratulations!

Alger, 30 decembre 1958

Copy of a letter from Mme. Gauthier-Lievre
to Rolf Grönblad.

Monsieur et cher collègue,

J'ai bien reçu votre publication, en collaboration avec Mr. Scott, sur les Desmidiées du Soudan, publication tres interessante pour moi, qui depuis de nombreuses années étudie les algues africaines. M. crux-melitensis var. tropica et St. clevei var. africanum sont bien identiques à M. radians var. ambadiensis et St. furcatum var. asymmetricum f. depauperatum, la question de priorité ne se pose même pas, - votre publication est bien antérieure a la mienne. La même question se posera pour une note que j'ai donné à l'impression au début 1958 et qui n'a pas encore paru. Elle a pour titre "Les Desmidiées asymétriques", je figure votre M. sudanica, sous ~~xxx~~ nom M. sudanica Grönl. inédit, car Teiling en publie une figure. L'autre desmidiée est M. incredibilis dont je fais un genre nouveau, le genre Allorgeia; me basant sur les travaux de Teiling je l'ai retiré du genre Micrasterias, les lobes apicaux n'ayant des dispositions classiques de ce genre. Je vous adresserai la publication dès qu'elle aura paru, et à ce moment je vous demanderai votre avis sur cette algue étrange. Les très nombreux individus que j'ai examiné sont un peu différents des vôtres, qui, a mon avis, sont des formes anormales.

M. Quézel est absent en ce moment; dès qu'il sera de retour je lui demanderai s'il possède encore les clichés des photos du Tibesti. S'il les a je vous en ferai tirer quelques exemplaires.

Avec l'expression de ma haute considération, veuillez accepter, mon cher collègue, mes meilleurs vœux pour la nouvelle année.

L. Gauthier.

Dec 15 1956

Dear Rolf,

Thanks for your congratulations, which I accept 6 weeks in advance for my 71st birthday, which will be on January 25 1959. Yes, I'm an old man now, and feeling it more and more every day.

Just two days ago I had a letter from Hannah about the Latin diagnoses for my new Indonesian paper. At the end she wrote "The news from Finland sounds bad. Do you know how it affects Rolf". I replied that we don't get much news from your country, and all I know is that the USSR or GGGP has stopped buying Finnish products like timber and plywood, resulting in a financial stringency in Finland, but that I did not see how that would affect you personally. You had better write her when you get a chance and reassure her, and me also.

Enclosed is your paper on *Cylindrocystis* with a few small corrections. Is tesselatis the best translation of "cubical"? Tessera and tessarae are the Latin for die and dice, used for gambling, but my dictionary gives the meaning of tesselatus as "checkered or tesselate", (divided into squares). For "cubical" it gives cubicus.

Francis Drouet, in his book "Revision of the coccoid Myxophyceae", claims to have examined original specimens of many desmids named by the early algologists, in such places as the British Museum, etc., and he says that some species of *Cylindrocystis* are not desmids at all, but belong to some other family or families of the Chlorophyceae. I have seen the book but do not have it now, so I cannot say any more about the subject, but it might be advisable for you to borrow it and see what he has to say.

Our friend Dr. Prowse from Malacca will visit us in New Orleans next Sunday and will stay for two days, Dec. 21-23, on his way home to England on leave. Also Mr M. Sachlan, from Java, wrote several months ago that he expected to get a grant for a year's study in the USA, and hoped to visit us about Christmas time. It would be nice if both of them could meet here, though it would be rather a strain on our accommodations to put up two of them at the same time.

Prof. Brunel has been kind enough to make for me a photocopy of Wallich's original illustration of *Euastrum turgidum* and the accompanying description. I think you do not have this, so I have made a pencil tracing for you. The smaller figures are presumably about the same size as the original, and Brunel further enlarged the front view, but unfortunately none of them is very clear, though the text came out quite sharply. Judging by the figures of other desmids on Wallich's plate, I should think that the profile of his front view is fairly correct, though the conical granules seem to be too big, and the size of the central tumour is exaggerated and there is little doubt that it is scrobiculate, not granulate. His description refers to a "minute terminal notch", which I believe must be an illusion caused by two closely opposed granules just below the apical margin, such as are frequently seen on other *Euastra* of similar types, e.g., *E. spinulosum*. The side view shows the apical lobe to be simple, not divided.

Turner's figures of *E. turgidum* are similar to Wallich's, and may perhaps have been influenced by them, as regards the size of the tumour and the apical notch, though it is, of course, not impossible that there may be an Indian form of the plant with such a large tumour and with a pair of subapical granules. Turner's side view shows the upper lateral lobes simple and the apical lobe undivided, but this does not agree with his vertical view, which shows the upper lateral lobes doubled, and

the polar lobe divided. So there may be two forms, one with a simple and the other with divided polar lobe.

Your drawing No. 2125, from Grunow's Insel Banka material, should be assigned to the specific form of *E. turgidum*, I believe. Thus Krieger seems to have been correct in eliminating var. *Grunowii*, and recognizing only var. *simplex*.

After seeing Wallich's illustration I am more than ever puzzled as to the advisability of reinstating *E. turgidum* var. *moebii* Playf., ^{assigning} and all the other varieties of *M. moebii* to *E. turgidum*. Some of the varieties seem to me to differ too much from the original concept to be easily accommodated under *E. turgidum*.

Telling has just written me, after seeing my drawings of the new varieties of *M. moebii* in the Arnhem Land paper, and also of var. *diplocanthyllum* and var. *tetrachastriforme* fa. *latum*, that he believes it would be better, for the present, not to make the change, but to wait until more intergrading forms may be discovered. However, I am so convinced that *M. moebii* really belongs to *Euastrum* that I am anxious to make a change, though whether to *E. turgidum* var. *moebii*, or *E. moebii* I cannot make up my mind.

Last night we had our first freeze of the winter season, only 2° below the freezing point, but that is enough to kill our banana trees and the foliage on the poinsettias. Both of them will come up again, but it is a pity that it had to happen just before Christmas.

Best wishes to you for Christmas and the New Year, from your friend