



Hunt Institute for Botanical Documentation
5th Floor, Hunt Library
Carnegie Mellon University
4909 Frew Street
Pittsburgh, PA 15213-3890
Telephone: 412-268-2434
Email: huntinst@andrew.cmu.edu
Web site: www.huntbotanical.org

The Hunt Institute is committed to making its collections accessible for research. We are pleased to offer this digitized item.

Usage guidelines

We have provided this low-resolution, digitized version for research purposes. To inquire about publishing any images from this item, please contact the Institute.

Statement on harmful and offensive content

The Hunt Institute Archives contains hundreds of thousands of pages of historical content, writing and images, created by thousands of individuals connected to the botanical sciences. Due to the wide range of time and social context in which these materials were created, some of the collections contain material that reflect outdated, biased, offensive and possibly violent views, opinions and actions. The Hunt Institute for Botanical Documentation does not endorse the views expressed in these materials, which are inconsistent with our dedication to creating an inclusive, accessible and anti-discriminatory research environment. Archival records are historical documents, and the Hunt Institute keeps such records unaltered to maintain their integrity and to foster accountability for the actions and views of the collections' creators.

Many of the historical collections in the Hunt Institute Archives contain personal correspondence, notes, recollections and opinions, which may contain language, ideas or stereotypes that are offensive or harmful to others. These collections are maintained as records of the individuals involved and do not reflect the views or values of the Hunt Institute for Botanical Documentation or those of Carnegie Mellon University.

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.

Rolf Grönblad
Karis
Centralgatan 80

October 18th 1947.

Mr A.M.Scott
823 Perdido Street
New Orleans, La.
U.S.A.

Dear Sir,

Thank you most heartily for your letter of Sept. 20th. I am also greatly indebted to you for your kindness in sending me a lot of interesting papers. The "Biologia" I have forwarded to the Botanical Institution of the University of Helsingfors. They there took a great interest in that paper. Also the "Monthly Scientific" I have lent to several biologists who have read it with great interest. So we all thank you for them!

I am very sorry to hear that you have taken it so hard, that I proposed to include your species *Euastr. invaginatum* in *E. bilobum*. I surely do not think my figure is incorrect. It seems to agree quite well with the original figure given by Lütkemüller (Verhandl.d. zool.-bot. Ges. Wien, Vol. XLII, 1892, Pl. IX, fig. 13). You are quite right in remarking that there are differences; your forms are more elaborate, the European forms very simple. It is in my opinion only a matter of taste if you wish to make a new species or only a variety of *E. bilobum*. I should think the differences are less conspicuous than the resemblances. But if you and Prof. Prescott think it better to make a new species, I can also agree with that opinion! I hope the matter is settled and all right?

You are very fortunate in having within your reach such a richness of interesting Desmid-localities. Our Desmid-flora is much more trivial, and nowadays it is only very seldom I am able to state a "new" species here. If it should at any time happen that you get tropical or subtropical material, which you yourself do not wish to work out, I should be to take over such samples! The Brazilians were quite interesting!

I have quite recently got some reprints from Sampaio, but not the great work you are talking of. One of them was "Sinopse das Desmidiás conhecidas na Flora Portuguesa" (1945), but without figures. I shall write to him and send my last papers.

I have no experience as to the use of highly refractive liquids. I am working with material in water (living) or in a solution of formalin. I have been contented with that simple method. Staining has also been used for special reasons. You have surely a good microscope, and so you are able to discern all small ornaments in the cell-wall, also when the cell is not empty. Mostly it happens that at least one empty semicell or a cell can be met with.

I should like to add some remarks referring to the samples of yours:

- 1) Ia.32. June 29. 1941. There was a *Staurastrum* very much resembling *S. zonatum* f. *elongata* Grönbl., Brasil. fig. 301. *St. quadrangulare* var. *quadricornutum* P+S
- 2) Fla.13. July 27. 1906. There is plenty of a *Micrasterias* Sol., but it is not quite *v. ornata* nor *v. aculeata*. What do you call it? Pl. 6 fig 7

There is a *Staurastrum* that you have not figured. It comes very near to *S. bidentatum* Wittr. (= *S. longispinum* v. *bidentatum* sec. West), but they are not identical. I should perhaps make a variety of it. See also my fig 1.

- 3) Fla.36 Sept. 8. 1946. *Pleurotaenium tridentulum* is very long and not tapering towards the apices. There is also a *Staurastrum* that comes very near to *S. pulcherrimum* Cushman. The spines are of varying length, often shorter than in Cushman's fig.
- 4) Miss.1. Your *Triploceras verticillatum* is considerably thicker than the fig. in Krieger's Flora, but it agrees very well with Joshua's orig. fig.
- 5) Fla.27. I have noted *Desmidium graciliceps*, *Hyalotheca undulata*, *H. neglecta*, *H. indica* v. *javanica* (see Grönbl. Brasil.). A curious form of *Spondylosium pulchrum* with dilated lobes. *Cosmarium lobatum* (Borge, Hoehne IV:19).
- 6) *Desmidium gymnozygaeforme* in my opinion is identical with *Desmidium longatum* Wolle (Desm. U.S. XLIX:6). I have recently seen this plant from Ia-

brador. (cfr Cedercreutz, Freshwater algae from Labrador, Mem. Soc. p. fauna

Rolf Grönblad
Karls
Centralgatan 86

et flora Fennica 1944). See my fig. AA. (- The name "KYM-nozyg~~ae~~iforme is a falsely composed one; it should be "KYM-nozyg~~i~~iforme". See Internat. Rules of Nomenclature Art. 70 Recomm. XLIV on page 24: "In the formation of specific (or other) epithets composed of two or several roots taken from Latin or Greek, the vowel placed between the two roots becomes a connecting vowel, in Latin i, in Greek o; thus menthifolia, salviifolia, not menthaefolia, salviaefolia." These Rules are adopted and revised by the International Congress of Cambridge 1930 and should be used and followed by all taxonomists.'

- 6) Fla 11. Your *Micrast. piquata* seems to me nearer to *M. decedentata* v. *expansa* Turn. (Krieger 104:12). In my opinion Turner's var. has no closer relationship to *M. decedentata*, but comes much nearer to *M. piquata* Salisb. The name "*M. expansa*" is already occupied, so Salisbury's name must be taken for good, and Turner's plant made a variety of it: *M. piquata* var. *expansa* (you call it *M. piquata* var. *lata*, nova var.)

((NB. Salisbury has published a "*Mateola acutiloba*" that surely is identical with *Desmidium curvatum* Nordst., De alg. et Char. III, Pl.1, fig. 4-6))
Triploceras gracile is much more dentated than any of Krieger's figures. or any of the European forms. - In this vial I also have seen the *Closterium Braunii*, only a little smaller and simpler form, but otherwise typical. It is a rare species!

Your *Docidium undulatum* is still more undulated and elegant than "var. *perundulatum*". An old acquaintance of mine, *Closterium pseudanastomosum* Grönbl. was also in this vial quite typical. Krieger has made it synonymous with *C. subscoticum* Gutw., but I do not think he was right in doing so. - There ~~was~~ also were a few cells of *Micr. muricata*, a typical American species. From Labrador I have described a var. *simplex* of it. (Krieger. Flora)

- 7) Miss. 87. There were cells of *Staur. radians* with one *se micell* typical, but the other one = var. *extensum*. Those "dichotypical" cells are taxonomically of great interest!

Rolf Grönblad
Karls
Centralgatan 86

(-3-)

P.S. Your *Xanthidium wewahitchkense* should be compared with *X. antilopaeum* f. *major* Nordstedt, Nonnullae alg. aq. dulc. Brasil. page 26 & fig. xyl. V. It seems to me that these very likely are identical. But, as you know, already too many different forms are united to what we call *X. antilopaeum*. It should probably be more useful to exclude at least some of the most different forms and place them elsewhere, - why not as new species!

Have you noted the long forgotten figure by Ralfs (Brit. Desm. XXXV: 11), *Closterium cuspidatum* Bail. It is recorded from N. America and I believe it is without doubt the same plant as "*Spinoclosterium curvatum*" Bern. + var. *spinosum* ((why this varietal name?)) = *Closterioides spinosus*)).

If the identity is recognized, then the original name *Closterium cuspidatum* ought to be used. Also if removed in a new Genus, Baileys species-name must be a "nomen conservandum".

R.G.

Rolf Grönblad
Koris
Centralgatan 86

September 22nd 1948.

Mr A.M.Scott,
823 Perdido Street
New Orleans 12, La. USA.

Dear Mr Scott,

I am very much ashamed because I have not written to you long ago. I have got a great many periodicals which I have, after reading them, forwarded to the libraries at Helsingfors. We all are very much obliged to you for them. I myself wish to thank you awfully much for the seeds you kindly sent me. Some of them are growing in my garden or in pots, but most of the seeds I must spare for the next spring, because they must be put in the ground early in March - May, our summer being very short. Anyhow, I have had and I will have great pleasure of them.

Last but not least I thank you most heartily for the parcel containing coffee and other useful things. These I got just before my summer-holidays and so it happened that I had no occasion to write to you before my returning home. Forgive me my slowness, please!

In conclusion I am writing some remarks on the Desmids found in your samples (parcel Nr 2) from Florida.

Florida Nr 131. *Cosmarium dentatum* is a species that I had never seen before this. It seems that the marginal teeth exhibit considerable variation sometimes being very small and inconspicuous, sometimes very irregularly arranged. - Another species, *Cosmarium ovale* also shows variation of the marginal teeth, sometimes being almost smooth with the exception of a few spines at the apical angles and one at each basal angle. ^{x)}



Nr 74. *Euastrum cornubiense* has a very tiny tooth at each apical angle. West suggests that this species could as well be called *Cosmarium*. In my opinion there is no reason for making a *Cosmarium* of it. - *Micrasterias truncata* v.

^{x)} This variation has not been observed in Europe.

pusilla f. cuneata seems to me to have only little in common with M. truncata.
- Desmidiium pseudostreptonema in vertical view differs considerably from the figure given in West, Mng. V. (Perhaps W&W's fig. is not correct?)

Nr 110. Bambusina sp. (ad Desmidiium longatum Wolle? sed minus).

Nr 136. Xanthidium antilopaeum (?) var. - Very much like your fig. 2 (blue-copy)
"Fla 12. Xanth. new?" - Only the specimens observed had 4 scrobiculae around the central spines.

Nr 141. Staurastrum gemelliparum and S. quadricornutum were both present. The former has spines thus: , the latter thus: . These species have been confused with each other, but they are easily separated.

Nr 129. Euastrum spinulosum, quite typical was observed.

Nr 138. Sphaerosozoma sp. with one long papilla at each angle (not pore-organs; controlled with gentiana-violet staining!) I cannot at this moment identify it.

Nr 122. Cosmarium luscum Borge with zygosporae was observed and a drawing was made. - Gonatozygon monotaenium was very long and slender (171 x 5,8 u)

Nr 144. Hyalotheca sp. (undulata var.?) and fig. 13 & 14 Scott: blue-copies plate Nr 4. - Hyalotheca neglecta + f. major Tayl. - Bambusina attenuata (Tayl.) Gbl. - Bambusina sp. - (nova??) ← *B. delicatissima* Wolle, 1884, p. 25 Pl. 1, fig. 22

Nr. 178. The most exciting records from my point of view were the 2- and 3-radiate cells of a "Spinocosmarium" (not published?) in your sample 178 Florida. The cells were exactly identical in all other respects, but one cell was biradiate the other one triradiate. The first one you and Prescott are calling Spinocosmarium while the second is included among the Staurastrum? Why so? We have also other species in Staurastrum which can be 2-pluriradiate. I cannot quite see why some biradiate Staurastrum should be separated and be made a new Genus. The 3-radiate form mentioned above (sample 178) is closely allied to Staur. cornutum Arch. Perhaps they are forms of one species?

On the other hand there are analogical relations between Spinocosmarium quadridens and Staur. forficulatum Lund. If you make a new Genus of the biradiate forms then those in all other respects identical triradiate forms must also be included in it. But then the differences between Spinocosmarium and Staurastrum are

totally broken down. Anyhow, this is the way in which I see the question. I should be interested to know your own and also Prof. Prescott's opinions (I have quite recently written to Prof. Prescott).

Another interesting Desmid in the same sample Nr 178 is Desmidium Swartzii var. quadrangulatum and "v. quinqueangulatum" with zygosporae. Among the normal zygosporae of var. 4-angulata, there was one record of a zygosporae formed by one 4-angulate and one 5-angulate cell. A true hybrid form, certainly only between two closely allied forms of a species. But, as far as I know, such records are hitherto unknown.

My rough sketches you need not return. If you are interested to see the original drawings, I most willingly will send you them.

*Wolle 1824 T2 xxii fig 3
Zygosporae betw 2 + 3 radiate forms of X. antilopeum*

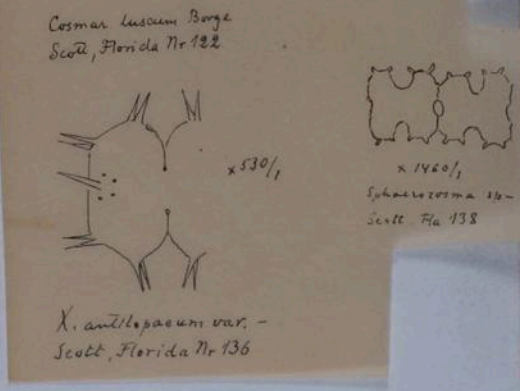
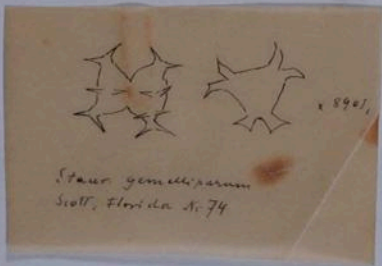
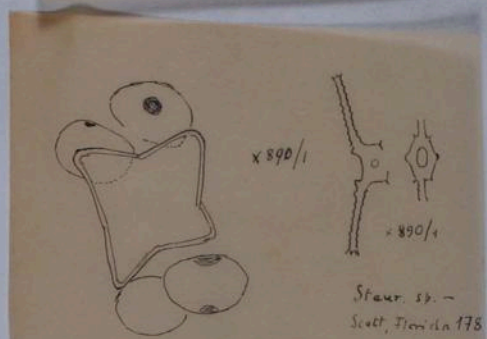
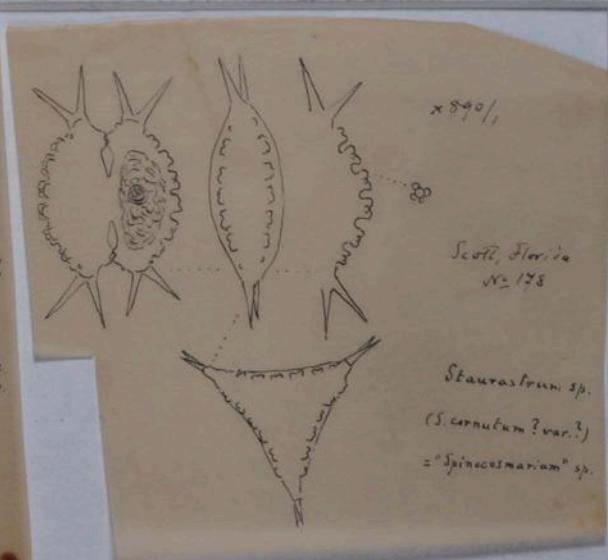
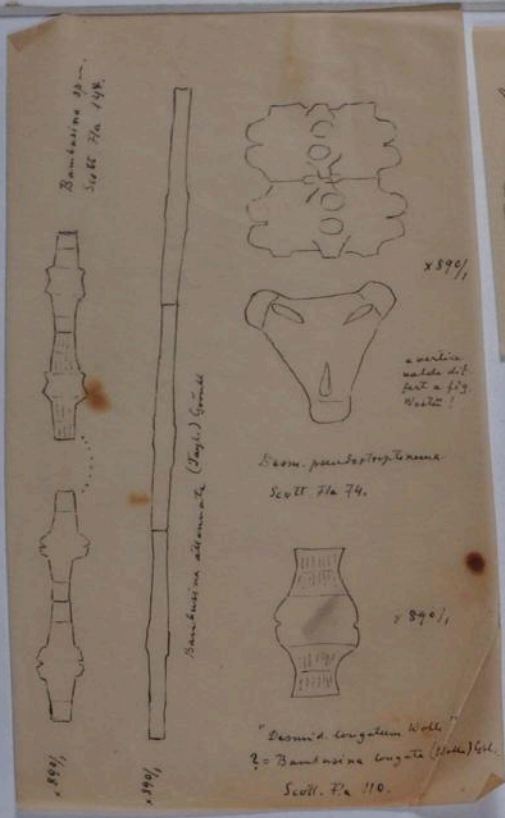
The drawings sent to Telling I have as yet not seen, but he has promised to let me see them later on. I shall be interested to see sketches of your curious Desmidium aptogonum with "cysts".

Why do you not publish more of your 2-3000 drawings? They would be very valuable to us others.

With kind regards,

Sincerely yours

G. S. West, Algae, 1916 says that Archer in 1875 found a Zygosporae betw E. oblongum? and E. humerosum



Rolf Grönblad

Karls

Centralgatan 86

July 8th 1950.

Dear Mr Scott,

It is already long ago since I have written anything to you. Meanwhile I have written to Professor Prescott and I have also received his kind answer. As far as I can see he has no objections against our collaboration as proposed by you. But I feel in some way that he does not altogether wish to lose his contact and collaboration with you. I think you may best yourself decide if, and in which way our collaboration has to take place.

As I already said, I hope next fall to have time for such work.

To morrow I am going to Sweden for the Internat. Bot. Congress and I shall also take a trip to the Swedish high mountains in Jämtland where I hope to collect arctic and subarctic material.

In Stockholm I shall also meet our friend Dr Teiling who has invited me to live with him. I think such a congress must be very tiresome, and I have no intensions to see and hear everything. But I hope to meet some foreign phycologists known to me by correspondence, and perhaps there will be useful and interesting conversations. It would have been very pleasant to see also you there.

Some days ago I received the great "Bibliographia Desmidiacearum" by Prescott & Brunel. There are not few works new to me. It is a very useful work and I hope it will be completed and finished very soon.

With kind regards,

yours sincerely,

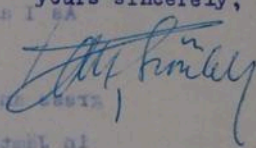
I enclose a clip from a Finnish newspaper

where you can see our permission for the

honour's cause (X) [17.5.1918]. The same day also

Teiling became honorary doctor in Lund. He

has perhaps told you about it.



Aug. 16th 1951.

Dear Mr A.M.Scott,

It is already a long time since my last letter, and you must try to excuse my long delay. I hope you have recovered from the attacks of your illness. I know they are very unpleasant.

As to your letters I can tell you that in all questions I wholly agree with you. I thought it really could be of some interest to students of ecology and associations and populations to have complete lists of the species in every sample, especially in the Ia-collections. The Fla samples are ~~so~~ too rich in species so that it is difficult to get any clear idea of "associations" at all. So I think they can be omitted and a lot of work spared.

As I have already pointed out my drawings are principally made for myself and the marking ✓ only indicates which species should be accompanied by a drawing published - and quite naturally your drawings are to be used when there are such ones by your hand. I shall mark those of your drawings which I propose to illustrate our plates. This I have not yet done. I have sent my lists of species and my drawings beforehand only because I thought you would be interested to see a first attempt to get on with your rich material. As soon as I have my lists and identifications ready I shall return your original drawings.

The 108 drawings of Staurastrum &c I have received and also "Staurodesmus" sent by Teiling. (I shall number them beginning with 2589). I hope they can be copied for you at the University in Turku (= Åbo in Swedish) I have written to my friend Dr Kallio in Åbo but I have got no answer - I think he is spending his holidays elsewhere.

The fact that not all species in your Ia-collections are represented by a drawing is very important to know and I had really not noticed it. But if the general list of species includes all names from my lists, then I think the summary list will be acceptable. In this case it was quite good

that I have worked out my specimen-lists (which are indeed not altogether complete, because I have not made any attempt to have every species included - such a work would take much more time, and is scarcely needed)

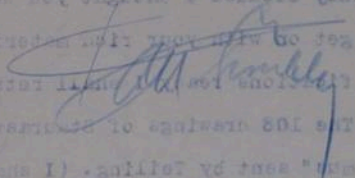
Do you consent to my proposal?

As to the size of the plates I have no idea at all at present but I shall ask some member of the Soc.Sci.Fenn. if our papers can be published in the same series as my Brazil and Lapland Desmids. The greater size would be very fine for our plates. About this I shall inform you as soon as I know anything.

I am very glad to know that I may write to you quite candidly my opinions and proposals and in order to have a good cooperation I hope you always will do likewise. I do not wish to neglect or suppress your opinions. I hope we understand each other quite well. I am also "only an amateur". Different identifications of several species by different authors is a common fact and this will never wholly be avoided. I am very much interested in what you think of my identifications and I hope you are going to write a critical letter quite soon.

With kind regards

sincerely yours,



(17.VIII.1951)

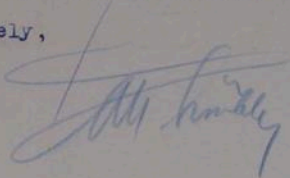
Answer to your letter of June 23th.

- 1) We shall use *Arthrodesmus* and *Staurastrum* as used hitherto.
- 2) Your find of *Cosmar.denticulatum* is very important and interesting. I think Dr Krieger would be interested to have a copy. I had just written to him on this question. He is also of the opinion that *C.ovale* has conical teeth while *C.denticulatum* has slender acute spines. May I send your copy to Krieger?
- 3) *Pleurotaen.ovatum*. Your stations for it are: Miss 44 (4.16.44) ((may perhaps be *Pl.truncat.??*)
Clinton La 4.10.40 (fig 235,236)
- 4) *Clost.cuspidatum*. You have it from: Fla 122 (fig 67), 9.21.48. and 5.10.48.
Fla 182 (fig with no Nr) 8.5.48.
I have never seen it elsewhere.
- 5) I think only *Pleurot. minutum* lacks the median girdle. Also this species must have a median line across the cell-wall as all placoderm Desmids have. It is erroneously omitted in most drawings (also in mine own).
- 8) I think your explanation of the peculiar appearance of *Hyalotheca* in La 3 & is right.
- 10) G.M.Smith's Manual of Phycology would not be without interest. I shall try the Botan Instit. in Helsingfors to buy it.
- 11) Smith's Freshw.Alg.U.S. also should be bought by the Bot.Inst. in Helsingfors
- 12) Prescott's Algae of W.Gr.Lakes I think would be useful to have.

You have had a long time to wait for my answer, but it cannot be avoided since I am sometimes too busy. At present I have had my summer holidays and so far I am at home I can work with your Desmids.

In conclusion a question: Do you wish to have all localities enumerated in the text of our publication or does it suffice with a simple catalogue of names + measurements + taxonomical notes if needed.?

Yours sincerely,



Thanks for Monthly Scientific

RL.

Sept 5 1950

Dear Dr. Grönbåld,

Please accept my heartiest, though belated, congratulations on your honorary degree of Ph.D. This is a well-deserved reward for your masterly work in dendrology and your world-wide reputation. It is indeed a remarkable coincidence that two such close friends as you and Viktor Teiling should have received honorary degrees on the same day in different countries.

I sent your letter of July 8th to Dr. Prescott in order to make sure that there would be no misunderstanding between us, and here is what he wrote in reply: "Dr. Grönbåld misconstrued my letter to him in respect to your cooperation. I tried to make it very clear that I had no mental opposition of any kind to your plan, but only indicated that I would be ready to assist you in any small way that I could by supplying information from the iconography, etc. Please do continue with your good work and coordinate your studies with Grönbåld if he is willing, and he seemed to be quite so".

Prescott has recently sent me the identifications, descriptions, and notes for two papers, on *Microsterias* and *Buustrum*. He has been working on these, off and on, for the last three years, and it was this long delay that started me worrying about the publication of the very large remainder of my material. I have sent these MES to Dr. Hannah Crossdale for the Latin translations of the diagnoses, and when they are received I shall rewrite the papers in final shape for publication. They will be published probably in the *Trans. Am. Micro. Soc.*, in our joint names.

I am now in a position, therefore, to definitely accept your very kind offer of cooperation, and I propose to turn over to you all of my drawings, and a duplicate set of all my collections of desmids from the southern USA, as well as a few from Cuba and Mexico. I request that you handle these in any way that you see fit, just as you did, for instance, with the Brazilian material. It will, of course, be necessary for you to omit the two genera *Microsterias* and *Buustrum*, and since these two papers cannot be published until next year, I am going to send you blueprints of the plates for them, together with a list of the names, so that you can tell what forms will be included in them. There are a very few *Microsterias*, and several *Buustre*, not included in these papers, because they were found too late for inclusion.

I have already started the ball rolling by sending you three boxes of samples, as follows:

Box No. 1	contains Florida collections	Nos. 190 to 252 inclusive.
2	" Louisiana "	Nos. 1 to 64, except No. 7, missing.
3	" Louisiana "	Nos. 65 to 125 (completes Louisiana).
	(Mississippi "	Nos. 1 to 18, except No. 3, missing.

Other boxes will follow at intervals of a week or two until the series is finished.

I am going to send you my original drawings, and for my records I am having photostats made of the best and most important of them, which are numbered in ink. Those which do not bear numbers are of smaller value, or no value at all in some cases; these you may discard if you wish. The reason I am sending you the originals instead of the photostats is that the latter are negatives (white lines on a black background) and it would be rather difficult for you to trace from them in drawing the plates. This was the least expensive method I could find for reproducing my drawings, but it costs plenty, nevertheless.

After you have received the first few lots of material and drawings you will be able to tell more about the way you wish to handle things, and then you can

write me again. Please remember that these drawings and samples will become your property, to deal with as you may deem best, and that you will be the "boss" as regards determinations. Of course, I shall be delighted to assist in any possible way, and it may be that you will run across cases in which my 12 years acquaintance with the desmids of this region may enable me to offer suggestions as to relationships among them. I have such a case under discussion with Prescott right now. He has identified three *Desmids* on my plate as three different species, because they are very different in appearance, but I am firmly convinced that all three of them are very closely related and belong to a new variety of *D. hypochondrum*, because all of them possess the single supracostriate granule and the "hump" on the upper lateral margin which are characteristic of *D. hypochondrum*, but which are not possessed by any other *Desmids* in this group. I am now digging up additional evidence of intergrading forms in an effort to persuade him.

My relations with Prescott will continue to be pleasant, I have every reason to believe. Last Easter he proposed to visit New Orleans and suggested that we make some collecting trips together, so my wife and I invited him to stay at our house. But his plans miscarried (they frequently do!) and he was unable to come. The day after tomorrow my wife and I are going to drive to Columbus, Ohio, (900 miles) for the meeting of the Phycological Society on Sept 11-13. The meeting itself will probably be as boring as the one in New York last winter, but I want to renew my personal acquaintance with Prescott, Taylor, Teft, Brunel and others. Irene-Marie is going to present a paper on new desmids from Canada, and I hope he will be there in person. I should like to meet this Frenchman, who must have some peculiar mannerisms, judging from his correspondence. A few months ago I sent him a box of very rich samples, but he is afraid to examine them, for fear that he may have found or may find some of my new desmids, and anticipate me in publication. I told him, politely, that he is crazy.

Recently Dr. Krieger asked me to send him some samples containing typical American *Cosmaria*, so that he could make some original drawings for the next installment of his monograph, instead of having to ~~make~~ copy illustrations of other authors. So I sent him a box containing samples specially selected for their content of *Cosmaria* that are found only in America, as well as a number of other rare species. Also I sent him sketches of several zygospores of *Cosmarium* species. He ought to get them in the next week or two.

With my best regards, and thanks for your cooperation,

Sincerely yours,

Rolf Grönblad

Karis

Centralgatan 86

Oct. 10th 1950.

Dear Mr Scott,

Parcel Nr 2 containing 63 phials with desmids from Florida has safely arrived. Thanks.

You need not send me those gatherings which I have received earlier from you. If you do not remember them (perhaps you have no list of them?) I can make a list of all material I have received. There is a lot of material in each phial.

Now some questions: Did you yourself make any lists of the species in the various samples? or not? Do you wish me to make complete list of the species in all samples? or perhaps a list of the samples at each ^{species/} sample?

P.S. To day I received four more parcels
No 3 and No 4 not yet opened.

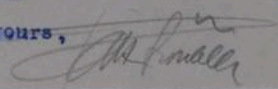
I think we should use your drawings as much as possible, because they are very good. But how about the copying of them for the tables to be printed? They must be drawn with india ink and I have not a quite steady hand by drawing with ink (with a pencil it is much easier). In my opinion the plates to be printed would become more beautiful if you would make the india-ink-copies. But if you have no time for such work I shall do my best. This matter is for you to decide. (but I should be glad to have your drawings printed without my interference).

The publications- if and when they are ready for printing- I think we should have made in our joint names? Perhaps also new species &c if you do not wish to have your name only for species found and described by you?

You must let me know all your wishes about this material.

Then I also hope that you would make the English text correct. Do tell me quite frankly if you have time to help me in this way or not.

With kind regards, yours,



Rolf Grönblad

Karis

Centralgatan, 86

Oct. 16th. 1950.

Dear Mr A.M.Scott,

To day your parcel nr 5 containing drawings of Pleurotaenium, Xanthidium &c arrived safely at Karis. Have I understood you right in this way: you have yourself copies of all figures, so that if I wish to discuss any drawing, I have only to refer to the number at the right corner?

As far as I can see at a first look there are many interesting forms, varieties and species which I have not yet seen in your samples. Oh, there is a great deal to do, and it will certainly take a good time.

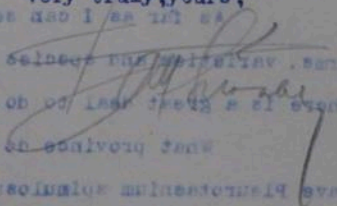
What province do you mean with the abbreviation "Ila"; e.g. you have Pleurotaenium spinulosum from a Cypress swamp N of Port St Joe, Ila.

What is the magnification commonly used in your drawings? I can, indeed, find it out from your measures; e.g. Nr 227 is x1250 resp x610. Is it right?

Am I allowed to make small remarks for myself on your sheets with a soft pencil, so that it can easily be traced out?

I am a little afraid of your original drawings if something would happen. You see, we are a very small and at present quite powerless country between West and East. No one can tell what will happen tomorrow. I tell you this only because I wish you to understand that there is a risk, a bad possibility.

Very truly yours,



Oct 21 1950

Dear Dr. Granblad,

This is in reply to yours of the 16th. Your understanding is correct that I have photographic copies, reduced to about 8/10 of the original size, of all my drawings that bear a number. Consequently, if you wish to discuss any drawing all you have to do is to mention the number.

Since August 1949, when I received my new microscope, all my drawings of demids from about 200 to about 1000 have been made to a magnification of 1600 times. Before that date the drawings were made with my old instrument, and the magnification was slightly different; I do not remember just what it was. For very small specimens, say 15x and less, I have used a higher magnification, and for large ones like *Microsterias*, and long ones like *Pleurostacium* and *Closterium*, the magnification is smaller. I have a series of oculars of powers 5x, 6x, 7x, 10x, 12x, 15x, and 20x. I use the one of these, with a 44x objective, that will give me the largest drawing that my standard sheet 7" x 8 1/2", will hold. Most of the drawings of average size demids are made with the combination 20x ocular and 44x objective, which results in the magnification of 1600x. Drawing #227 is 1275x and 620x respectively, according to my measurements from the reduced size photograph.

The abbreviation which you refer to "Fls" is really "Fla", meaning Florida. My handwriting must have misled you into thinking it was "Fla". The abbreviations of the various States of the U.S.A. which you will find in my collections are:

La.	Louisiana.
Miss.	Mississippi.
Tex.	Texas.
Ala.	Alabama.
Tenn.	Tennessee.
Ge.	Georgia.
S.C.	South Carolina.
Fla.	Florida.

You may make any notes that you wish on the drawings, and it is not necessary to make them with a soft pencil so that they can be erased. On many of the sheets you will see notes in pencil and ink by Prescott, in his peculiar writing which you can easily distinguish from mine. These notes represent his opinion of the identification, or his memoranda of similar species to be referred to. These may serve as a guide to you, but you are not bound by them in any way, not by my own attempts at identification.

I hesitated a long time before deciding to send you my original drawings, since I was well aware of the risk involved, and that is why I had photographic copies made of the most important ones. But the photo copies are not as good as I hoped, though they will serve very well in case anything should happen to the originals.

I sent you another box of samples yesterday, and have two more to follow which will complete them. It has been quite a task to make up all these samples and the boxes for them, but I enjoy the work. Your job is still ahead of you, and as you say, it will be a long one. But I think the result will be the most important work on American demids since Wollé and Irénée-Marie's books.

Rolf Grönblad
Karis
Centralgatan 86

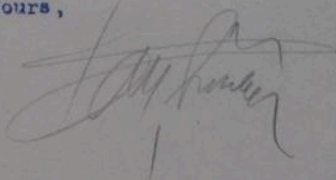
Oct. 22nd 1950.

Dear Mr A.M.Scott,

Yesterday I got parcel Nr 6 containing drawings of
Micrasterias &c.

I now understand that what I thought to be "Ila" on
your sheets is really "Fla".= Florida. Excuse my dull-
ness.

Yours,



Oct 16 1950

Dear Dr. Grönbled,

I am hastening to reply to your airmail letter of the 10th inst. I am sending you a complete series of all my collections. This will leave me duplicates those which I have previously sent you. I think it would be a good idea if you would forward these duplicates to Dr. Krieger in Berlin. A few months ago he asked me to send him some collections containing typical American Comaria, so that he could make some original drawings of them for the next Lieferung of his monograph. I did so, and have just received his acknowledgement, saying that my collections are among the best that he has ever seen. Also I sent him some sketches of zygospores of various Comaria, most of which have not been published previously; these are among the drawings which I have sent you.

By today's mail I am sending you two packages of drawings, the last one numbered 2480, completing all those that I have on hand. Telling still has a hundred or so of my drawings of Archrodesmia and monosporous Staurostroma, and he asked my permission to retain them until he has written a new paper on Staurodesmia, probably about Christmas. I shall send these drawings to you in due course.

Also by today's mail is going Box No. 4, containing collections Mississippi Nos. 19 to 97 inclusive.

Replying to your questions: I have not made any lists of the desmids in any of the samples, nor of the samples corresponding to any given species. The reason is that there are so many desmids that I could not identify at all, or only doubtfully, that such lists would have had very little value. Furthermore, I have something like 500 gatherings, and if each contained an average of 40 desmids (which I think is a low estimate), there would be 20,000 entries to tabulate, an enormous amount of work, which is, I think, of value only to the comparatively few ecologists and "associationists" like Laporte and Messikovier. You made such a tabulation in your Norway-Sweden-Finland paper a few years ago, but I wonder if it was worth the trouble. In any event, I think the associationists would have to invent a new title to describe many of my collections, for instance "Closteriето-Quastriето-Micrasteriето-Xanthidiето-Arthrodesmiето-Desmidiето-Comariето-Staurostroma", to classify them. If you think it is worth while, you might tabulate the desmids for a few typical collections, or for some of the richest ones.

As to the drawings, I have a confession to make. Like you, my hand is no longer steady enough to make acceptable ink drawings. So far all the plates which have appeared under my initials "AMS", I have drawn the plates in pencil, and then had them inked by a young lady, daughter of a close friend. I think she does them very well, and it is not every good draughtsman who can make a good drawing of a desmid, as I know from trials with other persons. I should be very glad to draw the plates for our joint paper. In order to do so, it will be necessary for you to select the drawings that you wish to appear on the plates, and return them to me. The photostatic copies that I had made, in case of accidents to the originals, are reduced in size, negatives, and rather disappointingly lacking in contrast owing to my faint pencil lines, so that it would be difficult to trace from them.

The authorship of the paper, and also the new species and varieties, should be in our joint names, Grönbled and Scott. But I should not object if you named one or two new species, St. Scottii Grönbled, for example. Would this be ethical? Prescott named St. marianense f. atypica Magnotta, in a joint paper by Prescott and Magnotta.

If you wrote the paper in German it would be more convenient for you and for European desmidologists. But since it will deal with American plants, and will

be used principally by American workers, I think it would be preferable to have it in English. Your command of the English language, even colloquial phrases, is so good that I have suspected you must have lived at some time in England or in this country. Nicht wahr? But there are still certain expressions which betray the fact that it is not your native tongue. The translations of Telling's papers contain a good many examples of this sort. Nygaard's latest paper, translated by his Chief of Police, was very good, and I found only two or three slight deviations from correct English. By the time you have the manuscript ready I shall have ample time to read it over and make any small corrections that may be necessary to make it conform to correct grammar and usage.

Last week I sent you a reprint of a small paper on new varieties of *St. Ophiura*. This is the first of a series of short papers which I had planned to publish in my own name, because of the long delay in Prescott's hands. However, I shall not publish any more of them, since you are taking over the remainder of my material.

I asked Dr. Krieger to examine a very curious *Comarium* or *Rustrum* in my collection Florida No. 122, probably a new species. It is shown on my drawings Nos. 1677, 1678 and 1679, and I named it tentatively (and facetiously) *Comarium cockeyedium*, because it is so "cockeyed". He says it is a *Rustrum*, and wants to study it further. I requested him to write you his opinion of it.

By the way, I notice that Krieger does not give any Latin translations of his diagnoses, which is not in accordance with the International Rules. I suppose that 25 or 50 years from now some purist will declare all his names illegitimate! Would you mind telling me what you think of this?

With my best regards,

Sincerely yours,

Nov. 2nd
Karls, Finland Oct. 31. 1950.

Dear Mr A.M.Scott,

Again I have to acknowledge the receipt of a parcel containing your drawings of *Euastrum Cosmarium* &c. (The last Nr is 1436). I have also received your letter of Oct. 16th, a copy of your paper on *Staur. Ophiura* and *Monthly Sc.* ^{Sept and Oct} My best thanks for them all.

As to the duplicates of your gatherings I shall do as you proposed and send them to Dr Krieger who certainly will be very glad to have them. The most beautiful and carefully made drawings are not equal to original specimens. I shall, however, wait till I have all your parcels safely here. Then I shall write to Krieger.

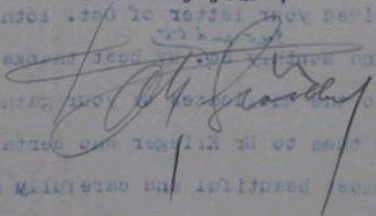
I am glad to know that you do not wish me to tabulate the species in every sample because it would be an immense work. And I think in this case it would be of rather small importance because among the very great number of species it is very difficult to see any "associations". At this moment the impression made by your gatherings on me is almost chaotic. But perhaps there are really some associations. We will see. Of course I shall write our papers in English. If you take the trouble of correcting my mistakes it will be O.K.

I have once, some years ago, written to Dr Krieger about the fact that he in several points is neglecting the Internat. Rules. (Not only in omitting the Latin diagnoses). But he thinks it a matter of no great importance. I was a member of the nomenclature committee in Stockholm last summer and I had an opportunity to see and hear how very punctilious in every small detail these nomenclaturists were. Therefore I think Dr Krieger is a little thoughtless. He has, however, a giant work before him, and I do not wish to disturb him any more. He is a very kind man and I think his life in the Russian Zone is all but pleasant.

Prof. Heimans and I gave up our membership in the nomencl. committee and proposed Dr Krieger in our place. Which was accepted.

I have looked two times through your drawings in order to get acquainted with them. In due time I shall begin with your gatherings. But it will be "tempo molto moderato".

Sincerely yours,



Karis, Nov. 9th 1950.

Dear Mr A. M. Scott,

Again I have to acknowledge the arrival of two parcels with drawings: Nr 1437- 2042 and 2033 - 2480. Thank you very much. As yet I have got 3 boxes with phials and 5 parcels with drawings. I have every day spent some hours examining your samples making drawings and notes. As yet I have preliminarily examined the samples "La. 1 - 13". It is not much, indeed.

I think it will be best to omit such species and varieties as are dubious, e.g. such as you have seen only one cell of and which are delineated only in front view, and also such as are uncertain as to the structure of the cell-wall. I hope you agree on this point.

With kind regards, yours,

John S. Burley

Dec 7th 1950

Dear Dr. Grönblad,

I have sent you under separate cover blueprints of eight plates for a new paper on Microsteries, by Prescott and myself, and enclosed is a list of the names. I think that this includes all the species and varieties that occur in my collections, though there are, of course, intergrading and slightly varying forms that it is not possible to illustrate in a short paper like this. It has been sent to the editor of Trans. Am. Micro. Soc. and I hope that it can be published next year.

I am quite in agreement with the suggestion in your last letter that you disregard anydesmids of which only one specimen has been seen, or of which only one view is shown in my sketches. Naturally, there may be exceptions to this, and if you run across anything of special interest where more information would be desirable, tell me the drawing number and I will see if I can find more examples.

By this time I hope you have received the last of the boxes of samples, containing a total of 525 vials, also a package of maps which will enable you to see the approximate location of the stations where the collections were made. Telling has just returned to me a number of drawings, principally of Arthrodesmus and the monospinous Steurastra, Steurodesmus as he calls them. I shall have photocopies made of these and send the originals to you shortly.

With my best regards,

Sincerely yours,

18818 Karis, Dec. 12th 1950.

(A1) I Dear Mr. Arthur M. Scott,

I wish to tell you that the list of your collections arrived some time ago. Also the Nov. number of "Month. Sc." I have got. My hearty thanks for them. I have at present looked through the vials "La 1 - 64" (=box Nr 1). I have also listed all species met with and I have made a lot of drawings. These are not so very rich as those from Florida but there are several interesting species.

In some of the vials (is there any distinction between "vials" and "phials"?) the material somewhat decayed (?), especially some filamentous algae (*Hyalotheca Desmidioides*) were in a curious way "dissolved", (the cell-wall perhaps affected by the preserving fluid?). Have you noticed it? You use only formaline, I believe? I have not observed such a phenomenon in my material. Do you use a very strong solution?

The curious "top-cells" of *Desmidioides Autogonum* I have observed, quite as you have drawn them. I think these filaments have quite recently grown from zygospores. There can be no other explanation. It would be very interesting to cultivate such individuals in test tubes. Have you never tried to do it? I think it would not be very difficult. I have got Pringsheim's book on cultivating algae and I think it would be worth while to try. There is also an American book dealing with cultivation of algae, and I have asked Prof. Prescott to send it. Don't you find such experiments interesting? You would have wonderful possibilities with all these variable species.

The maps of the southern states where you have made your gatherings were very interesting. I had no idea what the landscape looks like. I see now that there are numerous lakes and ponds and large swamps. (If you could lend me some photos it would be interesting. But do not take special copies for me)

Have you no detailed descriptions of the habitats with lists of the characteristic phanerogams (and mosses: *Sphagnum* or *Hypnum* coll.) as to the alga-flora. (I can tell you of similar connexions between phanerogams and algae from Finland and Scandinavia: positive, good indicators are: *Isocetes*, *Utricularia vulgaris*, *Drosera*; *Drepanocladus* and several *Sphagna*. - Indicators of poor Desmid-habitats are among others: *Lemna*, *Callitriche*, *Myriophyllum*, *Polygonum amphibium*, *Elodea canadensis*, *Stratiotes aloides*, *Hydrocharis morsus ranae*, *Chara*, &c.)

It would be useful to draw also the chloroplasts with the pyrenoids in one semicell. This is sometimes very important.

If you sometimes happen to visit mountain districts it would be very interesting to have some gatherings from wet rocks, wet mosses on dripping rocks &c. It would also be useful to have some specimens of the mosses from which the water was squeezed.

Do you keep your collections in similar vials with screwed covers? I think they are not safe without covering the whole top with paraffine. I have myself used such vials some 30 years ago, but they are now all quite dry and almost useless. I have seen some vials of Prof. Elfving now about 80 years since collected, in quite good condition. There was had been added to the preservative liquid some glycerine.

Another question which I think you may be able to study is the question of subsaline Desmids. When I look at the map of your city and when I see the large delta of the Mississippi I must believe there are innumerable possibilities of research. May I make you a question: are you very much occupied by your profession as engineer, or have you plenty of

time left for the Desmids?

To day ,Dec. 12th, I got one box with vials, and yesterday two boxes arrived, all safe. Now I have from you seven boxes. Only one of them is examined as yet. But I hope the examination will proceed slowly but firmly.

In a short time I hope to show the results of box nr 1 (La)

I have sent a book on Finland to you, and I think it will be an entertaining one because it is written by an Englishman who for a long time had lived in Finland, and who seems to know more than most foreigners of our country. Only his praise of the Finnish people is a little exaggerated.

In conclusion I wish you a happy Xmas and a happy new year.

Sincerely yours,

Dec 16 1950

Dear Dr. Grönblad,

Many thanks for your Christmas present, and for the pretty stamps which decorated the wrapper. I am not a stamp collector myself, but a friend of mine is, and I save all the foreign stamps for him.

The Book "Land of Lakes" is very interesting, and I enjoyed reading it. The author has a quiet vein of humour that made it all the more enjoyable. Although the title might more properly have been "Autobiography of Henry McGredy Bell", the book gives a lot of information about Finland and its people and its great men. In particular it gave me a better understanding of the events that transpired in your country during the last war, which were by no means clear to me at the time. I can tell you that in the USA there is a very high regard for Finland and its people.

For the last six months I have been corresponding with Dr. Minoru Hirano, Dept. of Botany, Faculty of Sciences, University of Kyoto, Kyoto, Japan, who has been kind enough to send me a number of his own papers on desmids, and also some by other Japanese authors. In return I have been able to obtain for him several papers by American and European authors that he asked for, including your two large works on desmids from Finland and Bresil, which I ordered from the Akademische Buchhandlung in Helsingfors; they have already reached him. Would you be kind enough to send him whatever reprints you can spare, of your smaller papers?

He has also sent me a considerable number of samples of algal collections from Japan, and when I get more time I am going to make up samples to send to you. They will not be for publication, because he is working on them himself. In general they are rather poor in desmids, and most of the desmids are of well-known cosmopolitan species. Nevertheless there are quite a few that are new to me, and I think you will like to examine them. In most cases he has sent plenty of material, and I am going to distribute samples to other desmidiologists, Prescott, Talling, Krieger and perhaps others.

A few days ago I received a very interesting paper, perhaps sent at your request (?), from Prof. Jadwiga Woloszyńska, of Krakow, Poland, on Fossil Desmids from Roztok near Jaslo. If you have not seen it you should write and ask her for a copy. It is written in Polish, and unfortunately I know nothing of that language, but there is a very short resume in English. There are nearly 300 excellent photomicrographs, showing not only the cell-wall ornament, but even the pore-systems in some cases. I should like to know how she did it; perhaps by staining.

The postman just brought a letter from the editor of Trans. Am. Micr. Soc. saying that the paper by Prescott and myself on Microsterias has been accepted, but he intimates that it will be some time before it appears, as they have a long list of MS awaiting publication.

With best wishes for Christmas and the New Year,
Sincerely yours,

I am enclosing photo-prints of a paper by D. Ushiyama (1923), Rotifers in Lake Suwa and Desmids in Yamaguchi-ken. It is of little value, but may have its place in your library of desmid literature.

Dec 23 1950

Dear Dr. Grönblad,

Many thanks for your long and interesting letter of Dec. 12th. Here are the answers to the questions that you raised.

"Fhial" is an old and obsolete spelling of "vial", which is any small more-or-less cylindrical glass bottle. When I first started using these screw-capped vials about 10 years ago, I thought they were completely airtight, and was quite surprised a few years later to find out that they are not necessarily so. I have since been told by others that the first time the cap is screwed on they are very neatly airtight, and I hope that will be the case with the duplicates which I have filed in the Parlow Herbarium at Harvard University. After the cap has been removed and replaced a few times, the gasket (the cardboard liner) in the cap loses its resiliency and is no longer airtight, so that water will evaporate through it very slowly. Fortunately, from the beginning I adopted W. & G. S. West's suggestion of adding about 5% glycerine to the preserving solution, to prevent total drying, and all of the samples that I sent you have about this much glycerine. It is said that a dilute solution of glycerine in water will gradually lose its water by evaporation until it reaches the proportions of 2/3 glycerine and 1/3 water and will then remain constant. But I suppose that if water were suddenly added to such a concentrated solution containing desmids, many of the cells would burst from osmosis. What method do you use? It seems to me that corks are unsatisfactory. In a lot of samples that Minoru Hirano sent me from Japan, much of the liquid has leaked through the corks, which were not paraffined. And a few years ago I asked Dr. Wm. Randolph Taylor for some material from one of his Newfoundland collections; to which he replied that most of them were totally dry, but fortunately the one I asked for still had a little liquid remaining, and he sent a very small quantity of the material, containing *Cosmarium novae-terrae*, which I wanted to check with some specimens from Miss. and Fla. My plants are the same as his, but with a somewhat more regular arrangement of the scrobiculations.

As regards culturing desmids, this is something that I have neither the skill nor time to undertake. I also have Fringsheim's book, which contains a lot of information, but nothing very clear that an amateur could follow. I think that Leporte's and Bourrelly's methods are simpler, and furthermore they have cultivated acidophilic species of desmids, while the only ones mentioned by Fringsheim are those which are tolerant of a fairly wide variation in pH and apparently easier to grow. I heard from Prescott recently that there had been a strike at the printing firm that was supposed to publish the Symposium on Algal Culture, and he had to take the job over and give it to another firm. So it is not ready yet. However, the papers that I heard in New York last winter contained nothing on culture of desmids. They were more concerned with the large-scale growing of such forms as *Chlorococcus*. I have, of course, made some amateurish attempts to grow desmids, or at least keep them alive. My first attempts were made with tap-water (note that Fringsheim mentions tap-water) and all the desmids died, but I got luxuriant growths of blue-greens and other filamentous forms which I now know to be hard-water species. I have since learned that the New Orleans city water supply has a pH that varies from 8.0 to 10.5! No wonder the poor desmids died. Later I tried water from the natural habitat, and was able to keep some of the desmids alive for weeks or months, ~~maintain~~ with the addition of a few drops of Knopf's nutrient solution; but the more delicate (acidophilic) species, which are the ones I am most interested in, quickly died, leaving only the hardier forms. I do not possess the dexterity to pick out individual cells, even dead ones for mounting, though I might succeed if I had one of Nygaard's manipulators. But even then I do not have the necessary time to experiment with culture.

every week. On week-ends, Saturday and Sunday I used to make collecting trips of 200 miles or so around New Orleans, but I have more or less exhausted the collecting possibilities within that radius. Three or four times a year I can get away for four days, at Easter, July 4th, Labor day in September, and Thanksgiving in November, and for the last few years I have been taking a 10-day vacation between Christmas and New Year. It requires four days for the trip to Florida as far south as Tampa, allowing for some collecting along the roadsides, and I can visit the southern half of Florida only on my longer vacations in the winter. I could fly to Tampa in 2½ hours, but that would necessitate renting an automobile at the rate of about \$30.00 a day, which is too expensive for me.

So for the last 10 years I have spent from two to four hours almost every night, and half the day on Saturday and Sunday (when not away on trips), in examining my collections, making drawings, and all the incidental work that goes with it. Formerly Prescott used to write our papers almost completely, but for the last three papers he has sent me only the descriptions, and I have had to arrange them, look up references and literature, and do the typing. Since scientific writing is new to me, this has been somewhat of a problem, and requires much time, but I am gradually getting the hang of it.

I am sorry that I cannot give you any more information on the habitats nor any lists of phanerogams. You see, I have no knowledge of botany outside of desmids, and though I have tried to obtain some acquaintance with the larger aquatics through a study of Fessett's "Manual of Aquatic Plants", I found that it would require as much time as the study of desmids themselves. So all I have learned is to recognize a few of the genera, and there are many others whose names I do not know. As to mosses, I cannot even be sure of Sphagnum, even under the microscope, and I don't know any of the others. By the way, there are very few Sphagnum habitats in my territory.

Also I have tried to learn something about limnology, from Welch's book "Limnology", but this is such a mass of conflicting theories and counter-theories, conflicting statements, observations and suppositions by a host of workers, that I was unable to get much benefit from it. Furthermore, almost everyone of the books and papers that I have read on limnology are written from the viewpoint of the zoologist, with scant attention paid to the algae, and desmids barely mentioned.

You must remember also that I have been working entirely alone, with only such assistance as I could get from books. Ten years ago I became a member of the New Orleans Botanical Society, thinking that I might get some help and advice from some of the members. But none of them knew anything about algae, and so far as I know there is only one man in the entire State of Louisiana, besides myself, working on FW algae. He is Dr. Lewis Flint, Louisiana State University, who has specialized on FW Rhodophyceae. In northern Mississippi there is Dr. R.L. Gaylor, who has been working in conjunction with Dr. Flint, and has also published on the Chlorophyceae of Miss. exclusive of desmids. Some years ago I sent him a specimen of Drepanaldia which he thought might be a new species, but I have never heard anything more about it. So I have had to devise for myself all the details of collecting, preserving and preparing material; the best methods of drawing them (I have made many experiments and set-ups for this); the procuring of what little literature I possess; and all the multitudinous minutiae along the way. Only once have I been on a collecting trip with a professional algalogist, Dr. L. A. Whitford, who was stationed in New Orleans for a few months. He is interested principally in the Chrysophyceae, and uses a plankton net on the end of a fishing line and rod, similar to the one shown in the photograph of friend Teiling taken in England a couple of years ago. I also have tried a home-made plankton net, ^{but do not like it} because it gets such an extremely small amount of material that in a rich habitat many forms must escape. I get much larger quantities from squeezings, and strippings from submerged stems, and this must necessarily result, I think, in a greater variety of forms, even though strictly planktonic desmids might not be included.

You mention the possibility of investigating subaline desmids (brackish?). There is plenty of brackish water in the coastal marshes of Louisiana and the delta

of the Mississippi River. An all-round algologist, familiar with all of the Chlorophyceae, Cyanophyceae, Bacillariaceae, etc., could no doubt get some very interesting collections there. But for a person like me, knowing only the desmids, it would be highly disappointing and unprofitable to go desmid-hunting in such waters. Several authorities say that desmids cannot exist in brackish water, and that the few reports of their occurrence there are probably due to accidental introduction by aquatic birds; further that the desmids could not live long in such water.

The region immediately around New Orleans has large areas of surface water in the form of swamps, marshes, bayous, rivers and lakes. With very few exceptions this water is unsuitable for desmids, and this fact is responsible for a delay of about eighteen years in my study of desmids. My first knowledge of the existence of these beautiful little plants was in 1920, when I bought a book "Aquatic Microscopy for Beginners" by Alfred C. Stokes, who is known for his work on Infusoria during the 1890's. It is written in a very elementary style, but correctly, and contains some fairly good illustrations of desmids. I was so much intrigued by the strange and elaborate forms of such species as *M. radices* (M. Sol) and *M. rotata* that I wanted to know more about them. At that time I possessed an antique microscope, so I started desmid-hunting around New Orleans, but was able to find only a very few species of *Glosterium* and *Cosmerium*, plain and uninteresting forms which I now know to be those which inhabit hard waters. I could not then understand why *Microsteris* and *Staurastrum* and *Xanthidium* were not to be found, and came to the conclusion that they did not grow in our waters, though I did not know the reason. Consequently my interest dwindled and I turned my attention to other hobbies.

In those days the two roads leading east and northeast out of New Orleans were not in existence, because of the difficulty and expense of constructing roads and bridges through the almost bottomless swamps. To reach St. Tammany Parish on the north side of Lake Pontchartrain you had to take a ferry trip of 30 miles at a cost of \$5.00 each way, and \$5.00 was a lot of money to me then. These roads were constructed about 1930, but it was not until 1938 that, by pure chance, I brought home a collection from Covington in St. Tammany Parish, and saw my first specimen of *M. fimbriata*. I was so delighted with it that I made other collections in St. Tammany and found many other beautiful desmids. Then I started hunting for literature, and in the course of my quest I went to the Louisiana Dept. of Conservation, which should know about such things, and was told there that nothing whatever was known nor written on the desmids of this state. About that time I picked up in a second-hand bookshop a copy of Conn & Webster's *Algae of Connecticut* (which I later sent to you). The drawings in this book are rather fanciful, but they gave me an insight into the possibilities, and it occurred to me that I might perhaps be able to do something for science by recording and drawing my captures. I bought a camera lucida and commenced making crude drawings, three or four species or even genera on a single sheet. Then in 1939 I made a gathering near Pearlinton (Miss. No. 1) that had such an astonishing assortment of desmids that I determined to draw every one of them, and spent several months doing so. I arranged the sketches on crude plates and attempted to identify them, but there were a great many blanks in the list. Somebody advised me to write to Gilbert M. Smith in California, which I did. He replied that he had done no work on desmids for 20 years, and referred me to Wm. Randolph Taylor, ~~whom~~ to whom I sent some of my "plates". Taylor apparently recognized that I had a number of new species and varieties, so he wrote a nice letter, explaining that he had transferred his affections to marine algae, and that he had forwarded my drawings to Gerald W. Prescott. Shortly I received a very cordial letter from Prescott offering to write up my stuff, and to collaborate on future material, which we have been doing ever since. In 1940 I bought a second-hand but modern Leitz microscope, which enabled me to do better work, and gradually improved my methods and draughtsmanship. In 1948 I purchased the Spencer research microscope shown in the photos that I sent you, thinking that the finer apochromatic objectives would enable me to better distinguish the minute markings, sometimes only a fraction of a micron in size. But I am disappointed with it in several particulars, and the apochromats require too much delicate adjusting of their correction collars for use with water mounts of varying thicknesses.

So that's a resume of my introduction to desmid study.

I am sorry that I have no photos of desmid habitats to send you. For many years I have not owned a camera, but I will borrow one and get some pictures on future trips. In the meantime I have sent a small book, "Louisians Out-of-Doors", which may give you a slightly better idea of the conditions here.

You mention an apparent partial dissolution of filamentous desmids in some of the vials. I do not recall this, but if you will tell me the numbers of the vials I will see if I can explain it. A possible explanation occurs to me: on short trips I bring the material back alive in order to examine it while fresh. The jars stand on my table for several days or weeks, but in this hot climate bacteria, fungi and moulds multiply very quickly, and decomposition starts rapidly in some of the jars, while others seem to be unaffected. It may be that the specimens you mention came from one or more of the jars with partly decomposed material.

There are no mountains within 500 miles of New Orleans, the nearest being the Great Smoky Mountains in Tennessee, a part of the Appalachian Range, and the Ozark mountains in northern Arkansas and southern Missouri. I made one trip through the southern part of the Ozarks a few years ago, but got no desmids at all. Everything was limestone. On my trip to Columbus, Ohio, last September, I took my collecting outfit along, but did not see a single habitat that seemed worth while stopping for. We came back through the very mountainous regions of West Virginia, Virginia, North and South Carolina, but there again I did not see any interesting-looking waters, though I was far too busy driving over the curling twisting mountain roads to pay much attention to the scenery. I am not accustomed to mountain driving, and don't like it. It was not until we reached the Atlantic Coastal Plain region near Savannah, Georgia, that I obtained any desmids. The twenty or more collections that I made in that neighborhood were fairly good, but contained few novelties. They were much the same as my collections from northern Florida, as might be expected.

My wife and I were supposed to start on our Christmas vacation trip this morning, but had to call it off, because of a severe attack of biliary colic, due to gall-bladder trouble, that I had last night. I get these attacks once or twice a year, and when they occur I want to be at home, not in a hotel 200 or 400 miles away. Eventually, I suppose, my gall-bladder will have to be removed.

So I have had plenty of time today to write this long letter. Hope I have not bored you with my loquacity.

Best regards from your friend,

Rolf Grönbläd

Febr. 21st 1951.

Keris
Centralgatan 86

Dear Mr A.M.Scott,

I hope you will excuse my long delay in answering your letters. I have been ill- it was influenza- not very seriously, but I have been very tired after it. I hope there will be time for me write a better letter than this one. I hope also that you need not wait a long time for it.

However, I can tell you that the first box of your samples (La 1-65) is examined. In a few days I shall send you my annotations and also my drawings in order that you may see my way of working. I have no copies, so you will, please, return them. - I have also a proposal:

I should think it best to arrange the publication in such a way that as soon as I have finished the material from La, I make it ready for publication and send it to you together with your drawings from this

area. Then you will have to correct the English text, and you can also tell me if you have opinions which do not agree with mine. Further you make the plates ready. We take in the first place your drawings, but when there are among my drawings something new or otherwise of importance (e.g. drawings with chromatophores &c.) we may take them also. The majority of my drawings are made only for myself in order to have them on uniform sheets and in my standard scale. I think it would be useful to publish at least a few lists of all Desmids in certain samples, the ecologists and associationists will be glad to see them.

I have looked through all your drawings, but I have not tried to determine them. I think it is better first to get acquainted with the material and then try to put out the names.

I hope you will agree that it is a good idea to divide the publications in several parts corresponding with the states. In this way we have at least something ready within this year, I hope.

With kind regards yours
Rud. Saccardo

March 3rd 1951

Dear Dr. Grisebald,

I also have had considerable trouble from illness this winter. Just after Christmas the doctor sent me to the hospital for a few days, because of an attack of cholecystitis (gall-bladder infection), and I had only partly recovered from that when I went down again with bronchitis which kept me in bed for two weeks. Even though I have gotten over these troubles, I seem to be lethargic, without the ambition or "pep" to go to work again.

Also this winter has been the coldest that New Orleans has experienced since 1918, and since the rooms in which I do my microscopic work and drawing are not heated as well as the rest of the house, I have been able to little work on my desmids. Yes, we do get cold weather here occasionally, in spite of our sub-tropical climate. We have had a number of freezes, and on two successive days early in February the thermometer went down to 20°F (-9°C) in the city, and it was even lower in the surroundings. All of our tropical plants have been killed, bananas, poinsettias, oleanders are gone, as well as many of the tall palm trees, and the orange crops in Texas, Louisiana and Florida have been seriously damaged, with many of the trees killed completely.

I had hoped that it would be possible to publish all of my desmids (except *Micrasterias* and *Eusastrum*) in a single large work, which would be similar to your Brazilian paper. Such a work would be of far greater value than a series of shorter papers. For instance, I have found Irene-Marie's first book (*Flore Desm. de la Région de Montreal*) extremely useful, but his subsequent papers, published in installments, are less useful because it takes so long to find what you want. You are aware, of course, that the boundaries of States, like Louisiana, are merely political lines that have no relation whatever to the edaphic features of the land, nor to the desmid flora. So the publishing by States, as you propose, would entail a lot of duplication, because the desmid flora of Louisiana is, generally speaking, quite like that of Mississippi and northern Florida, and a large majority of the species are found throughout the region. The only area where I can distinguish a difference is central and southern Florida, where the desmids become more tropical in character.

But with all these arguments I can readily understand that with the present uncertain world conditions it would be desirable to get some of our material into print at the earliest possible date, before something happens that might prevent the completion of the work. Therefore I am agreeable to your proposal to publish the Louisiana material as soon as you get it worked up.

When you send your drawings, please mark (with a blue pencil or some similar way), those that you wish to include in the plates, so that before I return them to you I can enlarge your drawings to the same scale as mine, so as to keep all of the illustrations to the same scale, as far as possible.

I think it would be a good idea to publish complete lists of all the desmids in certain collections, as you suggest, provided that you do not mind the extra work. I should like very much to see such lists, and I have often wished to prepare them, but it was impossible because there are so many that I could not identify. It would not surprise me if some of the gatherings contain even a larger number of different desmids than the one from Peckington, Miss., described in the first paper (1942) by Prescott and myself, which had about 145 different forms. Of course, you will select the samples for which you wish to prepare the complete listings, and these need not necessarily be the richest in number of species. But if you want to know those that I consider the richest I may suggest the following, all from Florida:

Figs. 76 to 79, 89 to 96, 109, 110, 197, 198. The one marked Fla. 209 contains 11 species and 2 varieties of *Microsteris*, something of a record I should think, and also many other desmids.

Last year I sent Dr. Krieger a box of my samples, and called his attention to a very curious new *Emstrum* in Fla. 122. He replied that he had found several specimens and was going to study it, so I asked him to write you about it. Did you ever hear from him?

Under separate cover I am sending you prints of three plates for a new paper on *Emstrum* by Prescott and myself, together with a list of the names. The determinations have been made by Prescott, and I might say that I am not in agreement with him in some cases, particularly the three plants shown on Pl. 3, figs. 11, 12 & 13. He has described these as belonging to three different species, while I am almost sure that they are closely related, and are variations of a new small variety of *E. hypocondrum*. But my arguments failed to convince him, so perforce I must go along with him. This will be the last paper by Prescott and myself on American desmids. The one on *Microsteris* has been accepted for publication in *Trans. Am. Micro. Soc.*, and should appear late this year.

That's all for the present. I am eagerly looking forward to seeing your notes and drawings of the Louisiana desmids.

Sincerely yours,

Rolf Grönblad
Karis
Centralgatan 86

April 18th 1951.

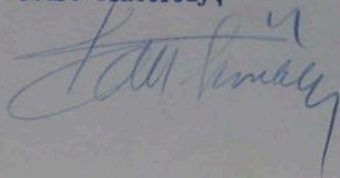
Dear Mr A.M.Scott,

I have today posted my drawings together with my lists of species of your *La - desmids*. I am preparing a long letter for you, but this short note is only to let you know that a parcel is on way.

Thank you very much for the book "The Culturing of Algae" and all other letters and drawings.

I am glad to hear from Telling that your health is better.

Yours sincerely,



April 25th 1951.

Rolf Grönblad

Karis

Centralgatan 86

Dear Mr A.M.Scott,
Thank you again for all you have kindly sent to me. There were copies of Micrasterias figures, Monthly Scientific and above all the interesting book on culturing of Algae. My very best thanks!

As I have already told you, I think it would be useful to send my lists of species together with my drawings to you in order that you may have a look at them. The lists of desmids are, however, by no means complete and I have made no attempt at having them so. It would take several years to work up such ones. I think my lists are sufficient for giving a general view of the algal sociations. Especially the very small species of Cosmarium, Euastrum and Staurostrum are

troublesome and take a lot of time if drawings are to be made by means of an immersion objective. But that you know yourself as well.

I have made a lot of notes on small sheets and I shall now try to make a letter of them.

Thank you again for all you have kindly sent to me. There were copies of *Microthlasia* *littoralis*, *Microthlasia* *littoralis* and *Microthlasia* *littoralis*. The interesting book on *Microthlasia* *littoralis* is very good indeed. As I have already told you, I think it would be desirable to send you lists of localities together with my drawings so you in order that you may have a look at them. The lists of localities are, however, by no means complete and I have made no attempt at having them so. It would take several years to work up such ones. I think my lists are sufficient for giving a general view of the algal localities. Especially the very small species of *Microthlasia*, *Microthlasia* *littoralis* and *Microthlasia* *littoralis*.

I do very well understand your wishes to have all your material published in one paper. You are quite right in what you say of Irene-Marie's first larger paper and the following smaller ones. It is sometimes very troublesome to find what you look after, in those small papers. - But as you yourself admit there are certain arguments for my proposal. Moreover, there need not be very many parts. I propose as follows: Part I = Louisiana 1-126; part II = Mississippi 1-103, Alabama 1-13, South Carolina 1-7, Tennessee 1-3, Texas 1-2, Mexico 1-3, Cuba 1-12; and part III = Florida 1-252. ((Perhaps Miss. could be part II and the small samples part III and Fla. part IV)). The most important richest part will be Fla. and this will give me a great deal of trouble.

As you see there will be only 3 (at most 4) separate parts and no one of them will be very small.

It will be very good if you can take upon yourself to enlarge those of my drawings which are to be published. Of course if there are the same figures among your drawings, we have to use them. (The fact that I have made so numerous drawings myself is because I wish to have them for my collection of figures)

Concerning the lists of Desmids from certain ~~places~~ localities I think that not only the richest ones are worth publishing but also some less rich with characteristic "associations". There is very little known of these questions in America, so I think the sociologists will be glad to have such lists.

I had some time ago a letter from Dr. Krieger. He has apparently not received my last letter. You can never tell what there may happen in the Russian zone.

It is interesting to notice that you do not in all questions agree with Prof. Prescott. I have also in a few cases ideas different from his and also different from Irene-Marie, Taylor and Walsden. If you are interested in my conception as to the different species I shall send you a list of them. I am a little afraid of offending anyone by having different views and ideas. You must not think I am so dull as to believe I were infallible. You have already in one or two cases corrected me.

I do not know what to do with *Staurodesmus* Telling. I have not as yet

accepted it because the synonymy will be very difficult and complicate.

There may be good reasons for accepting it later, but Telling has to work out all details (which I think he is going to do.). As far as his taxonomical

arrangements do not suit me always. I would propose that in such cases where you and I have different opinions this would be made clear in

a submargin note. If I write the text you could in this way have your

opinion made known. *Cosmarium mamilliferum* is correctly spelled. "Mamma" is written with two "m"s, but the diminutive "mamilla" with one only.

All figures of *Cosmarium connatum* and *pseudconnatum* without chromatophores (a vertice) are impossible to determine with something of certainty. Likewise you cannot distinguish between: *Cosm. pyramidatum* - *pseudopyramidatum*; *C. Cucumis* - *subcucumis* - *pyramidatum* f.; *C. nitidulum* - *pseudonitidulum*

and also some other Desmids. It would be very useful to have chromatophores

drawn in one semicell or in a special drawing a vertice (which is the best way). There are very many Desmids whose chromatophores are unknown altogether.

I hope you will agree on this point. I hope earnestly you may not feel in any way offended because of my remarks.

I have as yet not been able to find out the differences between certain forms of *Cosmarium ovale* and on the other hand *Cosm. denticulatum* var.

Irenee-Marie has described *C. ovale* v. *Prescottii* and *C. denticulat.* f. *Borgei*:

the former I should call *C. denticulat.* var. ..., the latter *C. ovale* var. ... But I am not sure what to do. The original *C. dentic.* is something quite different,

probably another species. If you look at Borge's fig. 24 and 25 (Exped. Ross-Rondon, 1925) neither of them is like *C. dentic.* from Australia, perhaps

fig. 25 (= var. *rotundatum*) can be retained within *denticulat.*, but fig. 24

in my opinion is another species (but not *C. ovale*, though it is nearer to *ovale* than to *denticulat.*) I hesitate very much as to the best arrangement.

C. ovale in G.M. Smith (Wisc) is not typical but I have seen in your gatherings quite similar to those found in Europe. Irenee-Marie makes a distinguishing

character of the warts above the isthmus, but I have seen a simple row of granules supra isthmum in absolutely typical *C. ovale*. I shall try to clear up this matter and perhaps also consult prof. Prescott. and Krieger. *All your la-complex contain C. ovale.*

You have no collection containing *Pleurotaenium ovatum* kept? Do you have It is a species I have never seen.

It would be very helpful if ~~you~~ ^{you would} in making your drawings always ~~you~~ add the magnification.

All my "*Euastr. abruptum*" are *E. Clastonii* (with one except only). This is in my lists of species. Also: "*Coem. excavatum*" should be *C. isthmium*; and "*C. ceylanicum* (var.)" is *C. polymorphum* or *C. Paulense* (= *C. polymorph. var. Paulense*) var....

"*Micraster. Borgei*" also should be altered in *M. fimbriata v. spinosa* (f.?) What you in America call *M. fimbriata* is not identical with the European species. In order to show you this I send you a microphoto of typical *M. fimbriata* from Finland. This foto is very characteristic, and I have never seen anything identical in American material. This may perhaps be interesting to prof. Prescott?

I once wrote of "decayed" filaments of *Nyalotheca* &c. This was in some of the first Nrs in La (ex, La.3).

Closterium cuspidatum has always a suture at the middle. (it is omitted in your bluecopy figure). Or have you seen cells without suture? It can be possible in a quite young cell developed from a zygospore.

Those of my figures which are marked with a red ✓ are intended to be published- but only in the case there is no better figure among your drawings. I should wish to have figures published as many as possible. At printing the figures must not be reduced to a too small scale.

If you have seen only one cell or semicell and I have not been able to find anything alike it may perhaps be better to omit them. Also when only a fig. "a fronte" is available.

Also if not all insignificant variations are named, which perhaps not always is advisable, I propose that as many as possible of your drawings would be published

A lot of Pleurotaenia are drawn without the more or less prominent median girdle at the isthmus. It is, indeed, always present. (7)

In many of the Xanthidia the cell-wall surface ornaments are marked as uncertain. Why do you not squatten the cell after having made a sketch of the outline? Also ~~in~~ ^{with} formaline material it is possible to press out the contents and make an accurate drawing of the ornament (it does not matter if the covering glass splits in many pieces).

I have picked out all your La-figures but I am still working with the determination of the species. There is not much undone and when I have finished I can send back these drawings in order that you may make the plates ready. I put the names on cards alphabetically registered.

You sent me plates with Euastrum drawings. Do you publish them in a joint paper with prof. Prescott? When? Or will it be in your own name? I ask because I wish to know who is the author of the new names.

In conclusion I will say that you have done a very careful work because there was very little I was able to find that you had not already seen. My lists are not intended to be exhaustive. You can look at the material as many times as you wish and always there will come something new. But the work had to be finished

This became a rather rhapsodic and long letter but I hope you will be indulgent with it.

Sincerely yours,

Rolf Grönblad
Karis
Centralgatan 86

June 6th - 57

Dear Mr. H. M. Scott

Today I have received Mr. Seward's. Thanks!
I have printed my collection of drawings of La desmids
April 19th. (also a letter). Have you got them?
I am a little anxious about them.

With kind regards
yours

Rolf Grönblad

June 13 1951

Dear Dr. Grönbld,

I am sorry that you had to write again about your drawings. Your letter of April 25th arrived several weeks ago, but at the time I was confined to bed with a double attack of cholecystitis (two attacks within a week). When I was able to get about again, I decided to defer answering until the receipt of your drawings. These came about 8 days ago, and since then I have been considering what was the best way of handling the drawings. Ordinary mail from Finland takes very much longer in reaching here than from Sweden, for some unknown reason.

You have certainly done a very large amount of work in making all these drawings, and in the detailed lists of desmids in every one of the 125 collections from Louisiana. This is very much more than I had expected, and I should hardly think it would be necessary to go to all this trouble for the remaining collections from Mississippi and Florida.

A large proportion of your drawings that you have marked thus: ✓ are included among my own sketches, so it seems to me that it would be an unnecessary duplication of effort for me to enlarge all of them. I think the simplest method of procedure would be for you to make a list of all of my sketches that you wish me to illustrate on the plates, giving my numbers, and also giving the identification of the specimen. Then I can compare the photostats of my sketches with your drawings, and determine which of yours I have to enlarge. The definite identifications are necessary, particularly with the Closteria and Cosmaria, because for me these two genera are the most difficult, and there are many species with similar or somewhat obscure characteristics that look pretty much alike to me, but which your much greater experience and familiarity with the European species enables you to separate. For the actual preparation of the plates, it would be very desirable for you to return my original drawings, because the photostatic copies that I had made are considerably reduced. When you return my originals I can send you the corresponding photostats.

A couple of weeks ago I sent you by registered mail 108 more original drawings, which friend Telling had borrowed for his studies on the polybrachate *Staurastrum*, and a few others. These are numbered 2461 to 2568, and I have photo copies of them. In his last letter Telling informs me that he has sent you my drawings of "*Staurodesmus*" (*Arthrodesmus* and *Staurastrum*). These are not numbered and I have no copies of them. Since Telling wants to get these back after you have finished with them, would it be possible for you to have photocopies made in Finland, for which I should gladly pay the cost? The drawings should be numbered before copying. I am enclosing a sample of the photocopies that I had made here, with nine drawings on a single sheet. Actually for the 2500 drawings I had sixteen drawings photo'd on a larger sheet. This sample sheet is of no value and it is not necessary to return it.

There is one point that I must call to your attention, namely, that my drawings marked "Louisiana" do not show all of the desmids that are included in my Louisiana collections. Many of them are represented by drawings from Mississippi or Florida collections. The reason for this is that when I discovered the much richer hunting-grounds in Miss. and Fla. I neglected Louisiana for some years, and when I made a few collections in Louisiana later on, I did not necessarily make sketches of desmids that were already represented by drawings from Miss. and Fla., because I expected that all of my desmids would eventually be published in a single work.

... so know is the size of the plate smaller than the size of the page. This is the first journal you expect to publish them in. It would please me if they could be published in the Acta Soc. Sci. Fenn., the same size as your Brazilian paper, because the individual illustrations would not have to be reduced so much as in the smaller American journals. For the plates that I have drawn for previous publications I have made them for a reduction of 2:1 or $2\frac{1}{2}$:1, depending upon the size of the desmids. For the larger *Micrasteris*, for instance, I have used $2\frac{1}{2}$:1.

Your suggestion of publishing in three separate parts (or four if necessary) is agreeable to me. Since many of the Louisiana desmids appear also in my collections from other States it will be unnecessary to repeat the illustrations in the later papers.

It was news to me that the chromatophores are so important for the identification of the Cosmarium species that you mention. In the cases where you have indicated that you want a top view with the chromatophores I shall be glad to enlarge your drawings, or else to find additional specimens and draw them directly. You do not need to be afraid of offending me by any of your remarks. You are an expert, and I am only a beginner, so I hope you will always feel perfectly free to write exactly what you think. Likewise, if I think I have a good reason for disagreeing with you, I shall feel free to tell you about it.

I don't think it is surprising that in some cases your ideas regarding the identification of certain desmids are different from those of Prescott, Taylor, Irene-Marie, Whelden, etc. No two desmidiologists, I am sure, would ever agree exactly on the exact identifications of any considerable list of desmids. Many forms are extremely difficult to assign; in fact I was a little bit amazed to see from the notes on your drawings that even you have had considerable with many of them. Furthermore, as W. & G.S. West pointed out, many American species show small but distinct differences from the European species to which they obviously belong. This is excellently illustrated by your photograph of *Micrasterias fimbriata* from Finland, which is different from any of the many specimens that I have seen from this country. In our forms the polar lobe is differently shaped, and the lower lateral lobules are not extended; furthermore, the small spines around the margin, in the American plants, form extensions of the lines representing the inter-lobular incisions, instead of a pair of spines being inserted in the rounded apical surface of the ultimate lobules as in your Finnish specimen. Such differences exist among desmids from other parts of the world when compared with the European forms that have been known much longer and have been more extensively studied, and this, no doubt, is what led some of the older desmidiologists, Nordstedt for example, to establish sub-species based on the forms from different geographical areas.

I must stop now. It is getting late and my wife has gone to bed, so I don't want to keep her awake with the noise from the typewriter. I shall write again in a few days discussing some of the other points that you raised in your long and highly interesting letter.

With my best regards,

Sincerely yours,

June 23 1951

Dear Dr. Grönblad,

Here are some further notes on the various subjects in your letter of April 25th.

I do not think it would be advisable to use Teiling's genus *Staurodesmus* until he gets the nomenclature straightened out and simplified; I found it very puzzling.

Until recently I did not know exactly what the difference is between *Cosmerium ovale* and *C. denticulatum*. However, I have now seen two specimens of *C. denticulatum* from North Australia, of which I am enclosing a sketch, with spines that are quite different from those of *C. ovale*, being long and slender instead of short conical teeth. Also in a collection just received from Peru, there are specimens of a form which closely approaches your var. *peruvianum* from Brazil, with the entire surface closely set with spines. In Borge's 1899 paper, *Ueber tropische und subtropische Süßw. Chlorophyteen*, there are illustrations of some forms of *C. denticulatum* with a somewhat different spination. It would seem that *C. ovale* and *C. denticulatum* are closely related, and that there are intermediate and intergrading forms between them. Now that I have seen the real *C. denticulatum* I agree with you that probably all of my plants from southern U.S. belong to *C. ovale*.

Fleuroteonium ovatum has been seen by me only once, in one of my very early collections. At that time I did not realize that the material would have any scientific value, and I usually discarded it after making sketches. I did not keep a copy of the sketch that I sent you, which is not numbered, and I do not remember where the plant was found. If you will tell me the name of the station I will revisit it at the first opportunity, and see if I can get more examples. Enclosed is a sketch of a variety of *Fl. ovatum* from Borneo, which lacks the spiral teeth, and approaches var. *inermis* M&B.

Recently I have spent some hours trying to find additional examples of *Closterium cuspidatum*, but without success because it is so rare. I kept a copy of only one of my sketches, marked Fla. 122 (Sketch No. 67), so if you will tell me in which of my other collections the plant occurs I will search them also. I have seen the plant from North Australia also, and my sketch does not show any suture. This does not prove that the suture is absent, since I might have overlooked it, though I always look for wall markings. I have also seen it in some Indonesian material, but did not draw it, because I have not yet started active work on this lot. Minoru Hirose has found it in Japan. Have you seen the plant from anywhere else than in my material?

I don't know why I have omitted the girdle band from some of my drawings of *Fleuroteonium*. The girdle is probably present in all the large species, but is lacking in some of the small and slender species like *Fl. trabecula*, *minusculum*, *tridentatum*, etc. It is barely visible in most examples of *Fl. verrucosum* (*trochiscum*).

Your suggestion of crushing ("squashing") a *Xanthidium* cell to expel the contents seems to be good, and I shall try it, though I foresee some difficulties. I make a shallow cell on the glass slide, by running a ring of two or three layers of lacquer, with a thickness of about .075 mm. This raises the cover-glass enough to permit turning fairly large specimens into the various positions required for drawing. For the largest forms I use a cell made of thin celluloid. When pressure is applied to a cover-glass supported in this manner the liquid is displaced, and the specimen with it. I have used this method for *Bostrychia*, *Leptothrix*, and

The paper on Eustrium, of which I sent you blueprint copies of the plates, will be published in the name of Prescott & Scott. It has been accepted by the Dutch journal "Hydrobiologia", and Prof. van Oye writes me that it should be printed early in 1952.

As you say, the Hyalothese filaments in Ia. No. 5 do have a peculiar appearance; the wall seems to be thickened and has a hyaline look, and the chloroplasts are much shrunken, or partially or entirely disintegrated. The other desmids in the vial seem to be normal, but in most of them the chloroplasts are partially disintegrated. The only explanation I can offer is that the material was probably brought home alive, and that it must have stood in my laboratory for some days or weeks before being killed with formalin.

In cases where I have seen only one specimen, and you have not been able to find it, also when only a front view is available, it is agreeable to omit them. There may be exceptions to this in the case of something very unusual, and if you encounter anything like this I should be glad to search for additional specimens if you will tell me the number of the sketch.

After a long delay Gilbert M. Smith has published his "Manual of Phycology", which is a symposium by many authors. The section of the Chlorophyceae was written by M.C.P. Iyengar, the Indian algologist. Desmids are barely mentioned, with a few sketches of no value. In this comparatively small book Smith has tried to include a general outline of all the algae, both freshwater and marine, with the result that everything is so compressed and condensed that I consider it is of little value. I sent my copy to a friend in Australia who is interested in marine algae.

A second edition has appeared of Smith's "Freshwater Algae of the U.S.". The sections on diatoms and desmids are practically the same as the first edition, and the same illustrations have been used. He includes the genus *Spinoclosterium* Bernard, without any reference to Bailey or Ralfs, but mentions Prescott's *Closteroides* as a synonym, and refers to Prescott's papers for a description. His bibliography is very incomplete, especially as regards European authors. I am sorry I spent \$10.00 on the book. The genera *Spinoclosterium* P. & S., and *Stauridium* Teiling, are not included.

Prescott has in press a new book, to appear probably next month, entitled "Algae of the Western Great Lakes Area", of which I am enclosing a notice. I am much disappointed that he omitted the desmids, but I suppose it would have made the book too large. If you want a copy let me know and I will send it to you.

With my best regards,

Sincerely yours,

Rolf Grönblad

Karis

Centralgatan 86

Sept. 26. -51.

P.S. Dear Mr A.M.Scott,

I can tell you news concerning the printing and publication of our Desmids. Prof. Luther writes that it is not possible to publish in "Acta Soc. Scient." (the same series where my Brazilian paper was published) because the costs for this large 4:0 size are considerably higher. On the other hand the publication most probably can be taken into the series "Commentationes biologicae" but the Society must have our manuscript ready for printing before it can be accepted. It would have been beautiful with such large plates. It is not allowed to unite two pages to one plate because there must

be no figures at the "folding". I am sorry to tell you this all, but we must be glad and content to have any publishing possibilities. Perhaps there would be some better opportunities in U.S.A.?

Now I have got an answer from Åbo (=Turku) that your drawings can be copied there. So I shall send them quite soon.

Thank you very much for the sketches of *Cl. cuspidatum* with M. Hiras's letter. It is very curious that there should not be a median suture, because such a specimen must be an individual recently developed from a zygospore having not undergone any cell-division. Perhaps staining would be of some help? (Gentianaviolet?) I think Krieger will be glad to have copies of your figures of *Cl. Cosm. denticulatum*, so I shall send him them.

Sincerely yours,

Oct 2 1951

Dear Dr. Grönbled,

The news in your letter of the 26th Sept. was as greatly disappointing to me as I know it must be to you. But since Prof. Luther bases his refusal on the greater cost of the quarto size of the "Acts" as compared with the octavo size of the "Commentationes", perhaps I can help by offering to pay part of the difference. Of course, I have no idea of the total amount of money involved, but I would be willing to pay \$100 or \$125 for each of the four parts into which you contemplate dividing our paper, to help defray the extra cost of the larger size. Again, it might be possible to reduce the total cost by publishing in only two parts instead of four, if you think such a course would be advisable, and in that case I would be willing to pay the same amount, or a total of \$400 or \$500. Personally, it would be much more to my liking if it were possible to publish all of our results in a single paper, and that would eliminate a lot of duplicate work for you, since many of the desmids occur in all of the States that are included in my collections, and would have to be listed in each paper.

Such a financial arrangement is not unusual in this country. For instance, the editor of the Trans. Am. Microsc. Soc. notifies prospective contributors that they may be asked to pay the additional cost if their papers have an unduly large number of plates compared with the number of pages of text, or if the text has an unusually large proportion of foreign languages or technical descriptions, because such text costs much more to set up in type. Prescott and I have a paper that has been accepted by this editor, which has 8 plates of Microsterias, and I should not be surprised if he asks me to pay something toward the cost. I had hoped that this paper would be published this year, but so far I have not received the proofs for correction, which means that it cannot appear before next year.

The opportunity for publishing our paper in this country is no better than in Finland, and probably there would be a longer delay. The few large quarto publications that are issued here, like those of the Carnegie Institute for example, are restricted (so far as I know) to workers in those institutions, and I have no acquaintance with any of the editors. Furthermore, all of the scientific journals in this country are swamped with papers submitted, and they cannot be increased in size because of the enormous increase in costs of paper and printing.

So I wish you would be kind enough to approach Prof. Luther again and do your best to persuade him to accept my proposal. In case they may help, I am enclosing prints of two plates of a series of twentyone, which I have just completed for a paper by Prescott and myself on desmids from Arnhem Land in North Australia. This will appear in the official account of the expedition, to be published by the Australian Government, but it will be two years or more before this is published. These plates will serve to show Prof. Luther the quality of the illustrations. Of course they are intended for reduction to the octavo page size, and the larger plate used in the "Acts" would contain proportionately more figures.

As regards the "sutureless" *Cl. cuspidatum*, are there not several species of *Closterium* in which the suture is invisible? For instance, *Cl. lunula*, *moniliferum*, *Ehrenbergii*. At any rate, published illustrations do not show any suture, though we know it must be present. It would not be possible for the specimens of *Cl. cuspidatum* examined by me and Hirano to be cells just developed

from cysts or spores. Sincerely yours

Rolf Grönblad
Karls
Centralgatan 86

Oct. 16th 1951.

Dear Mr Arthur M. Scott,

To day I received Prof. Luther's answer to your proposal concerning the publishing of our Desmids. It was very kind and positive. It is perhaps the best thing to translate it here "in extenso".

"On account of your letter I have consulted Prof. Federley ((the chairman of Soc. Scient. Fenn.)). It does not seem impossible that the Soc. would be willing to publish the treatise in the "Acta", especially when Mr Scott is offering a generous contribution to it. It is to be noted that the "Acta" in the future will be printed in a size somewhat narrower than hitherto which is caused by size of the paper now made, the sizes being standardized. The length is the same as before but the breadth is ~~some~~ $\frac{1}{2}$ cm less. The most important thing would be to know how many plates there will be and how many pages of text. ((This is very difficult to say. R.G.)) Like Mr Scott prof. Federley is of the opinion that it would be much better to have one publication instead of 3 or 4 separate papers. In this way the costs probably would be reduced with some 20.000 FMK. I wish to lay stress upon the fact that I am not any more a member of the board of the Soc. Scient. and thus I have no direct influence upon this matter, but I am only a mediator. I believe, however, that if the number of the plates is not very great and if Mr Scott is willing to pay 400 - 500 \$, the matter will be settled. Please tell me now the approximate largeness of your treatise, and then this matter will be decided."

These are indeed good news, as good as we can hope. But then the old question. Without doubt one larger publication is much better than several smaller ones. There is only one thing of which I am afraid: the longer delay of the publication. I have always been afraid of and avoided such larger undertakings because one can never tell what may happen before the work is brought to a happy end. If you think you can trust me without seeing any finished papers during this time I will promise to do my best. We must only hope that nothing will befall us because we are both of us needed to the end of our joint job.

In conclusion: if you agree with these proposals we will have only one larger opus. From time to time I will consult you and let you know and see what is done. Concerning the practical arrangement of the many details you perhaps are able to give me good advice. F.ex. how with the plates? It would be very good to have them drawn as far and as soon as possible. Perhaps it would be possible to draw them ~~when~~ we have agreed upon which of the figures are to be published. If the order of the figures later on has to be changed I suppose this could be made by clipping out figures and glueing them on to a new sheet? By the photographic reproduction the borderlines of the clippings will not be visible. (But the figures must not be numbered beforehand). What do you think of this? Or have you better schemes?

Of course I must try to simplify my manner of working and omit the extensive lists of species of every sample. How long time do I need? It is very difficult to promise anything definitely. Perhaps 2 or 3 years? I cannot promise to use all my time at disposal only for this joint treatise. I have some

X) it will be only 1 (one) cm narrower and this is taken from the margin. Thus the area with figures remains unchanged.

earlier collections which are partly worked out and which must be finished. This must be allowed, but the head part of my time will be used to our des- mids.

You have sent me an advertisement concerning "1952 Arctic Research Grants". Thanks. But I think it is not worth while to send new applications any more because I have sent such ones already twice - and have been refus- ed in spite of the fine recommendations of Finnish and American botanists.

Besides, I think I am getting too old (this fall 57). I must be contented to keep to old Europe.

Thank you for the Monthly Scientific, sept.

I have got no answer to my last letter.

If you wish to communicate directly with prof. Luther, here you have

his address:

Professor Alexander Luther
Djurgårdavillan 8
Helsingfors.

With kind regards

sincerely yours,

Nov 2 1951

Dear Dr. Grönbled,

The news conveyed by your letter of Oct 16th is indeed good, and what I had hoped for all along. Of course, the preparation of one single large work will necessitate considerable changes in your plans, and we shall have to run the risk of something happening before the work is finished. My health is much improved since my gall bladder trouble last winter, and so long as I am careful not to eat rich foods I see no reason why I should not continue in good health for a few more years. One of the risks is a possible disturbance of world peace, but it seems to me that this danger is less imminent now than it was 18 months ago when we first discussed our project. So I am in favor of going ahead with the larger work. Perhaps it might be desirable to confine this paper to my U.S. collections, and to omit the Cuban and Mexican gatherings, which could be published separately in a smaller paper, if you think it worth while.

Naturally, I can understand that you cannot devote all your time to this work, and that you have other things to attend to. But two or three years would not be too long; in fact I should think it would take you at least that time. No doubt you will have to simplify your procedure, and for my part I do not think it necessary to compile a complete list for each one of the samples; probably such lists for only a few representative collections (not necessarily the richest) from each of the States, Louisiana, Mississippi and Florida, would suffice.

As to the number of pages of text, you can judge this better than I can, from comparison with your works on Lapland and Brazil. And for the number of plates, this will depend on whether you think it necessary or desirable to illustrate every desmid in my collections, or only those which have not been shown in previous publications by Prescott and myself. For instance, I believe that all of the *Microsterias* are shown in our 1943 paper and a new one that was accepted for publication more than a year ago, and should appear next year. As to *Euastrum*, I have just returned to Prof. van Oye the corrected proof for a new paper that should appear in the next issue of *Hydrobiologia*. This, together with our 1945 paper, illustrates nearly all of the *Euastra*. I know of two that are not included, namely, a new variety of *E. elegans* (or *E. fissum*) shown on my drawing No. 1038, and the sp. nov. (?) shown on my drawing No. 5333. Concerning my failure to record the letter, I can only surmise that if I saw it I probably mistook it for *E. trigibberum* to which it shows considerable resemblance, and which I had already recorded. There may be, also, a few of the small and rather obscure *Euastra* similar to *E. binisla* and *E. sublobatum* that are not shown in these papers.

For a new paper on North Australian desmids, by Prescott and myself, I have just completed 21 plates with 284 figures, illustrating about 245 different species, varieties and forms, an average of about 14 figures per plate. But many of them are large desmids, also many of the figures show two or three views of the same desmid under one number. In this case I did not feel obliged to limit the number of plates, because I was requested to illustrate all of the desmids; also the paper will appear in a Government publication where expense is not of much importance.

In your Lapland paper you have 168 figures on 5 plates, averaging 34 per plate, an unusually high number. Your Brazilian paper has 355 figures on 16 plates, average 22 per plate.

I estimate, or rather guess, that the total number of different desmids in my U.S. collections is something like 600. If all of them are to be illustrated, and assuming that I could get an average of 20 per plate, it would

require about 30 plates, but to be on the safe side you had better figure on 35 plates.

As to drawing the plates, I could start right away, but I do not think that is practicable. It seems to me that you will have to finish your examination of the material, and decide which of my drawings and yours are to be reproduced. Then I can draw the plates while you are writing the paper and descriptions. As soon as you return my drawings I can send you the photostats of them that I had made, which you could use for reference while writing the paper. There is another suggestion that might work. I could ^{draw} each desmid on a separate sheet of paper of the same size as my original drawings, 18 x 21.8 cm, or perhaps 2 or 3 of the smaller ones on one sheet. These could then be cut out and arranged and glued to the large sheets. There is a possible objection to this, in that all my plate drawings are made on tracing paper which wrinkles badly when wetted, also the glue might show through the tracing paper. Perhaps the rubber cement that is used for mounting photographs without wrinkles might work. I do not know this material, nor whether it is light or dark in color. On the whole I think it would be safer to wait until you can return my drawings, even if it takes a little longer.

It seems to me that there is a considerable waste of space in the wide margins of the pages in the "Acta", both in the text and the plates. Even if the width of the paper is reduced by 1 cm, I think the plates could be made 1 cm wider and 2 cm longer than those in your *Brazilian* paper. But of course that is a matter for the editor and printer, in which you could do nothing more than offer the suggestion. If the plates could be enlarged by that amount it would help to get more figures on each plate.

In your former letter you asked for criticisms of your identifications. I am now going through your drawings and lists, and comparing them with my own drawings. There is quite a number of desmids that you apparently have not seen, so I am compiling a list of them, with my drawing numbers and the numbers of the collections, and shall send it to you shortly. Also I am making some notes on your sketches, and am adding a number of records to your lists. I think it would be advisable for me to return your lists and sketches, since you will need them when examining the other material. What do you say? Shall I return them?

As to detailed criticisms, I am afraid I cannot offer very much. The only genera with which I can claim much knowledge are *Microsterias* and *Baetrum*, on which I have done a lot of work, with Prescott's assistance, of course. You have a very long and extensive acquaintance with European forms, which I know only from illustrations. Outside of my own collections I have seen only tropical material, principally from Australia and Indonesia. Most of my suggestions have been noted on your sketches, and it would make too long a list to repeat here. But here are some notes:

Microsterias muricata. As you have remarked, this always (in my material) has either a single or double granule on each semicell just above the isthmus. Krüger has a copy of a figure by G.M. Smith showing this granule. I do not possess Smith's original paper.

M. truncata var. *pusilla*, and fa. *cuneata*. I agree with you that these bear little resemblance to *truncata*, and should probably be transferred to *M. decumbens*.

M. truncata var. Your drawing No. 5257, which you say is not among the figures by Prescott & Scott, is var. *convexa* P & S, 1943, Pl. 2, fig. 3. It is a small, circular form, with three small spines or granules on each side of the polar lobe.

M. foliacea. You have drawn the specific form, but not var. *ornata*, which occurs in La. No. 126. This is the first record for North America, and the second for the Western hemisphere. It is common in Indonesia.

Baetrum Wolllei. The ~~typical~~ form is quite rare in my material, and occurs in only one or two of my Florida gatherings. The one you have drawn, from Louisiana, is var. *cuspidatum*, according to my interpretation.

B. du var. *poriferum* Presc. & Scott. This seems sufficiently close to some of the

other varieties shown by Krieger to make me feel that this assignment is correct. But I have a new plant from North Australia, somewhat similar to E. dubium var. tropicum (W & W) Krieger., that Prescott has determined as a new variety of E. dubium. It is so entirely different from var. periferum that I cannot believe that they belong to the same species. I think the explanation is that Krieger's transfer of West & West's plant from E. lobulatum to E. dubium is incorrect. I am asking Prescott to reconsider his determination of the Australian plant.

Xanthidium cristatum var. uncinatum. This has a rosette of verrucae in the center of the face, and is quite rare in my material. A much more common form, that is very similar in appearance, has a group of 4, 5 or more scrobiculae in the center of each semicell, the pits sometimes being very large. I have not seen this published, and it may be a new variety.

Spinococcium quadridens and varieties. As I have told you before, I was quite surprised when Prescott created a new genus for this plant, which I think could very well go in Xanthidium. Probably you can add a footnote to the effect that you are of this opinion.

Staurastrum rectangulare var. quadricornutum Pres. & Scott. This also occurs in a 3-radiate form. It is not unlike published figures of St. zonatum. See Krieger's "Sunda" paper, Pl. 16, fig. 11.

St. ophiura var. minor, Presc. & Scott. Like you, I do not think Prescott's assignment of this plant to St. Ophiura was a very happy one. As you have noted, it is quite like St. margaritaceum as shown by Irene-Marie, and it is quite possible that it may be the plant that Wolle called St. comutum, but Wolle's figure is so vague that it is hardly safe to make the identification. There is another form in my material, quite likely the same species, that is 5-radiate instead of 6-radiate, and still another form with additional apical processes similar to those of your St. Ophiura var. perornatum from Brazil.

These are all the comments that I can think of at present. For the remainder you will have to wait until I return your lists and sketches.

Regarding the financial arrangement that I suggested, I should like to pay the money to the Society during the next 2 or 3 months, because I can spare it at present, but it may be a different story in a year from now. You see, the small firm in which I am a partner, and which provides my living, is entirely dependent upon the building industry, and the Government has placed such severe restrictions on building construction, because of the steel shortage, and the business outlook for next year is not at all good. Would you, therefore, tell me the name and address of the Secretary or President of the Society, to whom I should send the money, and also the Finnish name of the Society. Or I can send the money to you.

I asked Prescott to send you a copy of his new book, and he has promised to do so. When you receive it you will see a statement on the inside of the jacket that neither the author, publisher nor printer derived any profit from it. Of course Prescott received grants from various sources that helped to pay his expenses, but he tells me that his total reward for writing the book is FOURTEEN FREE COPIES, plus the privilege of buying some more at an author's discount. What do you think of that? The photo of Prescott on the inside of the jacket is a good one, but it has been retouched, and makes him look as if had received two black eyes in a fight.

With my best regards,

Sincerely yours,

Nov 12 1951

Dear Dr. Cronblad,

Here is a list, more or less complete, of the desmids which are not included in your lists or drawings of the Louisiana farms. However, there are several small and obscure species that I have not included, because I am not sure of them.

Sorry that I do not have time to write any more just now. My wife and I are leaving in two days for an aeroplane trip through Central America, stopping for a few days in Guatemala, El Salvador, Costa Rica, Panama, and Barranquilla in Colombia. Hope I can catch some nice desmids along the way.

Sincerely,

Dec 16 1951

Dear Dr. Grönbled,

In accordance with your letter of Nov. 25th I shall await your further advice before sending the money, and I shall send it to the Society and not to you.

For many years I have wanted to visit the Central American republics, so I took the opportunity of a slack period in our business to make the trip with my wife. From New Orleans we flew to Merida, Mexico, where the plane stopped only half-an-hour, thence to Guatemala City, where we stayed four days. Then to San Salvador, but we stayed there only one day, because we were disgusted with the poor and high-priced hotel accommodations and the attempted robbery of the taxi drivers. Thence to San Jose, Costa Rica, via Tegucigalpa, Hond., and Managua, Nig. We liked Costa Rica, and stayed there two days and would have stayed longer except for rain which caused me to cut our visit short and move on to the next place, Panama City, where we remained four days. From there we flew to Medellin, Colombia, a beautiful city in a valley surrounded by some of the lower ranges of the Andes mountains, which here are about 10,000 feet high. After four days in Medellin we flew to our last stop in South America, Barranquilla on the northern coast of Colombia. I had intended to stay there for a couple of days to investigate two large lagoons that seemed to be accessible, but again we were disappointed at the poor hotel room and unpalatable food, so we left the next morning and returned to Miami, Fla., via Kingston, Jamaica, and Camaguey, Cuba, which were merely plane stops.

After seeing these places, both my wife and I agree that we have little or no desire to see them again. The principal reason for this, I suppose, is that I do not speak nor understand spoken Spanish. I can read enough of it, thanks to my knowledge of French, to get the gist of newspaper articles, and to order a meal from the restaurant menus, but that's as far as I can go. Therefore, it was necessary, in every city, to hire an automobile with an English-speaking driver, and this, together with the hotels, proved to be very expensive, even though the rates of the exchange of the local currencies were very low; in Costa Rica the exchange was 7.40 Colones to the U.S. dollar, but they still charged the equivalent of \$10.00 U.S. for a double hotel room without meals, and in Barranquilla they charged \$25.00 per day for a poor room, including meals. Most of the hotels were fairly comfortable, but it annoyed me very much to have to pay top U.S. prices for accommodations that are distinctly second-rate by U.S. standards. In Miami, Fla., for the same \$10.00 per day, we had an excellent room in a really fine and modern hotel, with all the little comforts, even luxuries, that are provided.

We enjoyed the trip immensely. The mountainous scenery, with many active and inactive volcanoes, was a complete change from the monotonous flat terrain of the southern U.S. Two of the volcanoes (not active ones), in El Salvador and Costa Rica, can be ascended by automobile, and I made arrangements to do so. But in Salvador I refused to pay the extremely price, and in Costa Rica the trip had to be cancelled because of rain. Another thing that impressed me very much was the profusion and luxuriance of the flowers, even though we were too soon for the real flowering season which occurs in January, February and March. Not only in the cultivated gardens, but also growing wild along the country roads, were many flowering shrubs and trees, each with its own peculiar flower, nearly all of which were unknown to me. I attempted to learn their names, but the guides in most cases either did not know or refused to tell me.

only the local Spanish or Indian name, which of course is valueless. One tree in particular caught my attention. It is a tall tree, with very large ovate leaves, and a very large flower, consisting of a cluster of large reddish-orange calices, each of which more or less resembles a half-opened tulip. After hearing several different names for it, I finally learned from a botanist in Panama, that it is the African tulip tree, Spathodie sp.

I am sorry to say that my attempts at desmid-collecting were unsuccessful, and though I brought back 17 samples they constitute the poorest lot that I have collected in many a long day. The reason is that the places we visited are extremely mountainous, and almost the only visible water was swift mountain streams, frequently located in deep inaccessible ravines. I got collections from two lakes, Lake Amatitlan in Guatemala, and Lake Ilopango in El Salvador. Since I had no plankton net the only thing I could get was squeezings from aquatic plants, and both lakes were almost destitute of aquatic vegetation, being of volcanic origin, and with bottoms of small stones (gravel). In these Central American countries, the only regions that are densely populated and fairly well developed, are the upland valleys and plateaus, from 2000 to 5000 feet high. The lowlands, probably because of their hot and unhealthy climate, are poorly developed, with no automobile roads, and very few railroad lines. The few towns there are reached on horseback or muleback. To get down into the coastal plain I made a special trip by automobile from Guatemala City to the small port of San Jose on the Pacific Coast (not the same San Jose, capital of Costa Rica), over one of the two or three roads that run down to the coast. After leaving the mountains the road ran for 25 miles or so through an almost level country, on which coffee and sugar-cane are raised. The dry season had commenced a couple of weeks previously, but there was still some water in a few places, in roadside ditches and ponds. But everywhere that we stopped I could see at a glance that the water was unfavorable for desmids, and this proved to be correct on examination of the samples. All they contain is plenty of diatoms, with extremely few individuals of common *Closterium* and *Cocconeium*, and a scattering of the usual green algae such as *Scenedesmus*, *Ankistrodesmus*, *Pediastrum*, etc., and a few blue-greens.

From the long lists of desmids published by Prescott in his two papers on Panama, I had high hopes of getting some good specimens from that country, but there again I was disappointed, though I undertook long excursions into the surrounding country on three successive days. The best ones that I got were from the Summit Experimental Garden, where the director, Mr. Lindsay, showed me five ponds in which yellow, pink and blue tropical water-lilies were growing. Here again, from the associated aquatics, principally *Anacharis* and *Chera*, I judged that there would not be many desmids. The best of the five gatherings yielded about 25 different desmids, preliminary identifications of which are as follows:

<i>Closterium</i> <i>venus</i>	<i>Cocconeium</i> <i>pseudopyramidatum</i>
<i>Kuetzingii</i>	<i>impressulum</i>
<i>incilentum</i> ?	<i>quadrum</i>
<i>moniliferum</i> v. <i>vesutum</i> ?	<i>obsoletum</i> ?
	<i>angulare</i>
<i>Microsterias</i> <i>mehsbulshwrensensis</i> (fa. nov.?)	<i>pseudocoenatum</i>
<i>laticeps</i> v. <i>acuminata</i>	<i>pygmaeum</i>
	sp. ad <i>C. trilobulatum</i>
<i>Euastrum</i> <i>gemmatum</i>	
<i>sublobatum</i> ?	<i>Staurastrum</i> <i>striolatum</i>
<i>denticulatum</i> v. <i>retangulare</i>	<i>inflexum</i> ?
sp., ad <i>E. evolutum</i>	<i>Brebbissonii</i>
	sp. ad <i>St. inopaspicuum</i>
<i>Gonostonygon</i> <i>pilosum</i>	
	<i>Sphaerosozoma</i> <i>granulatum</i> .

Prescott got most of his desmids from Getun Lake, but this lake is accessible by road at only three points; elsewhere the shores of the lake can be reached only by boat, and I did not have the time necessary for any boat trips.

We did not stay in Nicaragua, because that country would not grant me a tourist card as all the other countries did, for the reason that I am not a native-born U.S. citizen, but a naturalized one. Our plane flew along the shores of two large lakes, Lake Managua and Lake Nicaragua, at a low altitude, and I got the impression that these lakes might be favorable places for collecting algae. In many places the shores are swampy, and in the shallow water offshore I saw a belt of aquatic vegetation, with water-lilies (Nymphaea) in some places. The map shows some lagoons near the city of Managua, and between that city and Lake Nicaragua I saw from the plane numerous lagoons and swampy places that could be reached fairly easily.

Again, in flying over northwestern Colombia, from Medellin to Cartagena and thence to Barranquilla, we passed over very large areas of swamps, most of which would be of very difficult access, but a few of them apparently could be reached from neighboring towns or villages with the aid of a local guide.

We flew from Miami to Palm Beach, Fla., on the east coast, and thence to Tampa on the west coast, passing over the northern end of Lake Okechobee. The land between Okechobee and Tampa presented a most extraordinary sight from the air. There were literally thousands of small ponds that I judged to be a few hundred feet in diameter, circular or semicircular in shape, many with a small wooded island in the center. I have travelled through this country several times in my car, but was unaware of this peculiar formation. Unfortunately the sun had just set when we flew over this region, so I could not see the full details. I should like to make this flight again by daylight sometime.

So that, in brief, is the story of our expedition to Central America. It is not likely that we shall ever see these places again, but if conditions are favorable I should like to see Rio de Janeiro before I pass out.

With my best regards and wishes for the Christmas season,

Sincerely yours,

P.S. I think it would be a good idea if you would send me four drawings of desmids from Florida before I return those of Louisiana. Then I can compare them, and perhaps make additional notes for identification.

31250... Land 25. XI - 57.

Dear Mr A.M.Scott,

I have told Professor Luther of your proposal to send the money to "Vetenskaps Societeten" but unfortunately the secretary of the Society has died a few days ago (Prof. Harry Federley) and we must wait and see when a new secretary is elected. This may take a little time. It is better that you do not send to me the money, but directly to the society. I would rather not be responsible for it. Shall I send to you those drawings of Florida deasids which I have made already before I continue? You could return them together with the La drawings.

Please tell me how your aeroplane trip to Central America succeeded.

Sincerely yours,
Tom Finley

Rolf Grönblad
Karis
Centralgatan 80

30.1.-52.

Dear Mr A.M.Scott,

To day I had a letter from Professor Luther who tells me that "Soc.Scient.Fenn." has got a new secretary (Prof. W.Wahl). The Council has had a meeting and the question of the fate of our Desmid-paper and of your offer of pecuniary subsidy were also debated. I am very sorry to tell you that the answer was in the negative. It was decided ~~princ-~~
~~ipally~~ on principle that the Society could not take any subsidies beforehand for a publication which is not written and ready for print and for which the costs are quite impossible to calculate. It was said that if a time of 2 - 3 years would elapse before the manuscript is finished a new inflation may reduce the considerable amount of money into a bagatelle. When our manuscript is ready for printing the question will be anew taken into consideration. Also the other question concerning the size of the plates was answered in the negative. I hope most sincerely that you will not feel offended by this byrocratic behavior of the Society. Prof. Luther writes that he personally does not doubt whether the paper will be taken into the "Acta" when finished. He writes "If Mr Scott then wishes to subsidise the printing with any amount of money this will be highly appreciated".

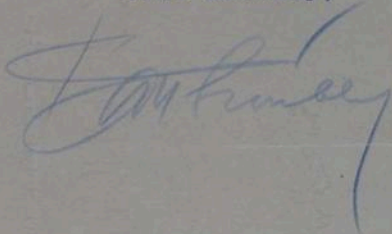
I feel this is not quite satisfactory, but all we can do is to finish the work the sooner the better. I fully comprehend that the forthcoming of the whole is depending on my capability of finishing my part. Thus I have to try to speed up a bit, if possible. (I have also to put my other desmids aside for a while - and this I do not quite like.)

Anyway, in a few days I am sending my drawings of Florida desmids, - there will certainly be more of them - and then I shall continue determining your drawings and your samples. I think the matter will be settled in some way when we have all ready.

Thank you very much for your kind cards and letters describing your interesting journey.

I shall later come to some questions in your letters.

Yours sincerely,



Feb 6 1952

Dear Dr. Grönblad,

It is, indeed, disappointing to learn of the strange action of the Council of the Soc. Sci. Fenn. in declining my offer. Perhaps they may be right from their point of view, but there is also my side of the question to be considered. In 2 or 3 years from now I think it is quite possible, even likely, that I may not be able financially to repeat the offer, and since the Society will, in all probability, print our paper either in the Acta or in their smaller journal, it would seem to me that they might as well accept the money now.

However, there is nothing we can do about it. I appreciate your offer to speed up your part of the work, but I do not think it right to put aside your desmids in favor of mine. Therefore I suggest that you finish your own work first, especially since there is probably less of it than of mine.

I am eagerly awaiting the receipt of your drawings of the Florida desmids, because I am very curious to know how you will identify some of the peculiar forms that occur so profusely in that part of the country.

In the meantime I have been working on my material from Central America. The Guatemala collections are extremely poor in desmids, but I am glad to say that some of the gatherings from Panama are better than I thought at first. But though there is a nice assortment of desmids, they are so few in number that it requires a very long search and lots of patience to find them. So far I have about 80 different ones, including a new species of *Euastrum* that is something like *E. serratum* Josh., but differing in several respects. Also there are one or two *Cosmaria* that may be new species. A strange find is *Cosmarium hexalobum*, which according to West & West has previously been reported from high Northern latitudes. I have a number of desmids that were not listed in Prescott's 1951 paper on Panama.

I hope to hear from you again soon.

Sincerely yours,

March 13 1952

Dear Er. Grönbld,

Your suggestion of depositing the money in a bank is a very good one, and perhaps I can offer a slight improvement.

The Nordiska Föreningsbanken's main office in Helsinki is a correspondent of the Chase National Bank, one of America's largest banks, in New York, and undoubtedly Nordiska has funds on deposit in New York at all times. Therefore I propose to send my cheque for \$500.00 to the Chase National Bank, with instructions to place it to the credit of Nordiska Föreningsbanken in Helsinki, because Chase does not correspond directly with the Karis branch of Nordiska. At the same time I would write to Nordiska in Helsinki, with a copy to Nordiska in Karis, instructing them to keep the money on deposit, for eventual payment to the Finnish Scientific Society, at such time as you may instruct them to pay it.

By keeping the money in New York it could remain in dollars, which would be to the advantage of the Society in case of a depreciation in the value of your marks, or to my advantage if the sum should eventually have to be repaid to me.

But before doing any of this, it would be advisable, I think, for you to speak to one of the officials of your bank in Karis, and ask him if the matter can be handled as I suggest. When you tell me that it can be handled in this manner, I shall immediately send my cheque to New York.

When writing about this money, please address your letter to me at 823 Perdido St., New Orleans, 12, La.

It is quite agreeable to me that you keep your drawings of the Florida desmids until you have finished your examination, and I can readily understand that you must continually refer to them and to my sketches. Two days ago I returned your drawings and lists of the Louisiana desmids, in two packages, by registered mail. You will see that I have made some additions to your lists, and have made notes on a few of your drawings, with the hope of helping in the identification of some of the specimens.

Would you like to see some algal collections from North Australia, Tasmania, and Japan? I have some surplus material available for distribution, and I propose to send small samples to some of my friends. There are some unusual desmids in the Australian and Tasmanian material, but many of the Japanese collections that Minoru Hirano sent me are remarkably like some of my American gatherings. The samples would be simply for your pleasure, not to be worked upon.

Also I have two or three additional collections from Florida that you have not seen, each of which contains at least one desmid not included in my drawings. I could send these in the same box.

Many thanks for the pretty postage stamps of the Olympic games. I am not a philatelist, but I send these stamps to friends who collect them.

Sincerely yours,

March 2 1952

Dear Dr. Grönblad,

In looking over my desmid drawings I found that in numbering them I had accidentally made use twice of the numbers from 2200 to 2300. This might cause confusion later on, so I have renumbered some of them, starting with No. 3000, and am enclosing herewith small photostats of the sheets with the numbers. Will you please make the changes on my original drawings, noting that on some of the sheets not all of the numbers have been changed.

Since it is going to me some years before our large paper can be published, what do you think of getting out, in the meantime, a shorter paper describing some of the many new species and varieties in my material? I think you will agree that this would be desirable, but it might not fit in with your other plans. Naturally, I desire to get credit for these novelties before someone else finds and publishes them. There are some other workers in the field, particularly in Florida. Herman Silva, one of Prescott's students, has his home in Georgia, and occasionally collects in Florida, and there are two assistant professors in the botany department of the Florida State College at Tallahassee who have published two VERY preliminary lists of algae, mostly Cyanophyceae, from northern Florida, the identifications having been made by Francis Drouot of the Chicago Natural History Museum. Also Irene-Marie is working very energetically in Canada, where the desmid-flora, on the whole, is not unlike that of southern USA, though it may not contain some of the sub-tropical forms that I get in the southern part of Florida. In his last paper he describes Cosmarium dentatum Wolle var. glabrum, var. nov., which I found several years ago and had tentatively given it the same name. Irene-Marie wrote me recently that he had made another 15-day trip to the region of Lake St. John in northern Quebec, and had brought back 1200 (1) bottles of material which he has not yet opened.

He must be a peculiar chap. Some years ago I sent him a box containing 24 vials of some of my richest collections, which I intended merely for his pleasure. But can you imagine that he has never opened the box, and refuses to do so? His reason is that he might see a new desmid in my material, and also find it in Quebec, when he would be prevented from claiming credit for it, even though I had not published it. Of course, I have told him repeatedly that such an idea is absurd, and that I should not be the least bit jealous (though privately I might be disappointed).

With the thought of a possible short paper on desmid novelties, I went through my drawings, and made a list of those that I think may be new. The list turned out to be much longer than I had supposed, and undoubtedly you will find that some of them are not new. Also it would not be necessary (nor desirable) to publish all of them that are new; I have placed a red check mark against some of the more striking examples.

Sincerely yours,

List of possible new species and varieties of desmids from A.M.Scott's collections from southern United States.

	Drawings		Drawings
Arthrodesmus sp.	57-61 ✓	Cosmarium sp.	1595-97 ✓
Xanthidium imper var.nov.	49-54 ✓	sp.	1613-18
Xanthidium sp.	340-44 ✓	sp.	1624-27
sp.	345-49 ✓	sp.	1629-30
sp. var scrobiculatum	350-55 ✓	sp.	1637-41
crisatum var.nov.	438-39	sp.	1642-43
sp.	448-50	sp.	1647-48
sp.	470-72 ✓	monoszum var.nov.	1660-62
antilopsoma		sp.	1674-76
var.eurycerum v.nov.	385-88)	sp.	1680-83
	414-19)	sp.	1711 ✓
Onychonema sp.nov.	490 ✓	Staurostrum ornithopodum, 3-radiate	1854-55 ✓ 2572-73
sp.nov. or var.nov.	489 ✓	sp.	1916-24 ✓
Bambusina sp.nov.	482	cerastes, var.nov.	1978-79
sp.nov.	620	bractatum, varieties	1991-2005
Spondylosium nitens		sp.	2041-42
v.quadrangulare	513	floriferum var.nov.	2053-54
Sphaerozozama sp.nov.	600-601 ✓	sp.	2061-66 ✓
Desmidiium Baileyi		sp.	2071-73
v. birediatum v.nov	543 ✓	sp.	2074-75
Desmidiium sp.	553-56	sp.	2076
sp.	561	sp.	2077-82 ✓
pseudostreptosma		tryssos, sp.nov.	2085-86 ✓
v. quadrangulatum	579-82	sp.	2101-02
Swartzii, 3-radiate	584-86 ✓	sp.	2114-28
Euastrum sp.	1038 ✓	sp.	2129-31 ✓
"cockeyedium" sp.nov.	1677-79 ✓	neglectum v.birediatum	2136 ✓
sp.nov. Cronbled's		sp.	2209-10 ✓
drawing 5332	3101 ✓	bicornatum varieties	2254-40
Cosmarium dentatum v.ornatum	1230-31 ✓	sp.	2256-57
v.minor	1222-26 ✓	sp.	2283-91 ✓
fa.spinosum	1221 ✓	sp.	2301-02 ✓
Cosmarium sp.	1245-46	sp.	3012
sp. cf. C.stenostum	1247 ✓	quadriripinatum, 3-spined	3026-28
beccatum sp.nov.	1250-52 ✓	sp.	3029
sp.	1261-63 ✓	sp.	3035
porrectum f.alatum	1270	sp.	3040-46 ✓
arthrodesmiforme		novae-terrae v.birediatum	2336-40) ✓
var.obesum v.nov.	1271-74 ✓		2583-85)
papilliferum sp.nov?	1287-89 ✓	sp.	2349-50
sp.	1292	sp.	2351-52
sp.	1331	sp.	2353-54 ✓
sp.	1356-60	sp.	2356-58 ✓
sp.	1388	sp.	2359-64 ✓
sp.	1390-92	sp.	2380-82 ✓
sp.	1399-1401	sp.	2387-92
sp.	1427-28	sp.	2393-98 ✓
sp.	1535-36	sp.	2400-04 ✓
sp.	1537	sp.	2405-16
sp.	1560-72	sp.	2419-21
sp.	1573-77	sp.	2422-23
sp.	1582-82	sp.	2424-36
		sp.	2437-38
		sp.	2440-40A
		sp.	2441-42
		sp.	2488
		sp.	2470-71
		sp.	2472-80

<i>Staurastrum</i> sp.	2424-36
sp.	2437-38
sp.	2440-40A
sp.	2441-42
sp.	2468
sp.	2475-71
sp.	2472-80
<i>setigerum</i> var. <i>pectinatum</i> var. nov.	3084-86
sp.	3088
sp.	3092
<i>forficulatum</i> var. <i>eximium</i> var. nov.	2543-49
sp.	2559-62
<i>ornithopodium</i> var. <i>tetradium</i> var. nov.	2572-28
sp.	2563-65
<i>Docidium undulatum</i> var. <i>semiundulatum</i> var. nov.	3482
<i>Xanthidium impar</i> var. nov.	49-54
<i>Arthrodesmus</i> sp.	57-61
<i>Penium rufescens</i> var. <i>errugatum</i>	209-210A
sp.	211-212
<i>Xanthidium</i> sp.	340-44
sp.	345-349
sp. var. <i>scrobiculatum</i>	350-55
<i>antilopaeum</i> var. <i>eurycerum</i> var. nov.	385-88. 414-419
<i>cristatum</i> var. nov.	438-39
sp.	448. 450
sp.	470-72
<i>Onychonema</i> sp. nov.	440
sp. nov. or var. nov.	489
<i>Bambusina</i> sp. nov.	482
sp. nov.	620
<i>Spondylocium nitens</i> var. <i>quadrangulare</i> var. nov.?	513
<i>Sphaerozosma</i> sp. nov.	600-601

<i>Staurastrum ornithopedum</i> , fa. <i>triradiata</i> fa nov	1854-55
sp.	1916-24
<i>cerastes</i> var nov	1978-79
<i>brachiatum</i> , varieties	1991-2005
sp.	2041-42
<i>floriferum</i> var nov.	2053-54
sp.	2061-66
sp.	2071-73
sp.	2074-75
sp.	2076
sp.	2077-82
<i>tryssos</i> sp. nov.	2085-86
sp.	2101-02
sp.	2114-2128
sp.	2129-31
<i>neglectum</i> var <i>biradiatum</i> var nov	2136
sp.	2209-10
<i>bicoronatum</i> var.	2234-40
sp.	2256-57
sp.	2283-91
sp.	2301-02
sp.	3012 2373
<i>quadrispinatum</i> fa <i>trispinatum</i> fa nov	2344-44 3026-28
sp.	2345 3029
sp.	2351 3035
sp.	2325-31 3040-46
<i>novae-terrae</i> var <i>biradiatum</i> var nov	2336-40, 2583-85
sp.	2349-50
sp.	2351-52
sp.	2353-54
sp.	2356-58
sp.	2359-64
sp.	
sp.	2380-82
sp.	2387-92
sp.	2393-98
sp.	2400-04
sp.	2405-16
sp.	2419-21
<i>subjohnsonii</i> fa nov ?	

<i>Desmidiium Baileyi</i>	var <i>biradiatum</i>	var nov.	543
sp.			553-56
sp.			561
<i>pseudostreptonema</i>	<i>quadrangular</i>		579-82
<i>Swartzii</i>	<i>quinguanqular</i>		584-86
<i>Evastrum</i>	sp.		1038
" <i>evckeyedivm</i> "	sp. nov.		1677-79
sp. nov.		<i>Grünblattdrawing</i>	5332

March 5th 1952.

ROLF GRÖNBLAD
KARIS
FINLAND

Dear Mr Arthur M. Scott,

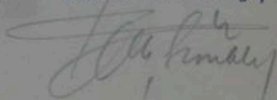
On account of your last letter concerning your proposal to pay for the printing of our Desmid-paper some 4-500 dollars and because the Soc.Sci.Fenn. has refused to take the money beforehand.I should wish to make another proposal with two alternatives:

1o. You deposit the sum in an American bank to be used when matters are ready.

2o. You send it to Nordiska Föreningsbanken in Karis with the express statement that it may not be used for any other purposes than the printing of the Desmid paper in the "Acta". If the printing in the Acta would not be realized within a time of, say, five years the money is to be returned to you. (It must be exchanged into Finnish Marks, but must then again be returned as USA dollars), which manipulation may reduce the sum). In my opinion it would be very ungrateful to refuse your friendly offer altogether.

As to the drawings of Fla. Desm. I think it will be best that I should send them to you not before I have finished the scrutinizing of your samples. I would have to compare specimens with my own drawings at any time during the determination of species. If you do not have to use my Miss. drawings quite now, perhaps you would also return these for a time to me? - I have put all other Desmids aside and am working upon your collections as my first objective.

Yours sincerely,



Apr 8 1952

Dear Dr. Grönblad,

I am glad to see that you agree on the desirability of publishing a separate and smaller paper on the new desmids in my material, and I can start work on the plates whenever you return to me my drawings of the forms that you decide are to be published. I have no preference as to the journal of publication; the *Mem. Soc. Sci. Fean.* is perfectly satisfactory, provided they can publish it without too much delay. Otherwise the Dutch journal "Hydrobiologia", edited by Prof. van Gye, is currently publishing in about six months after acceptance. All American journals are way behind; a paper by Prescott and myself on *Micrasteris* was accepted in October 1950 by *Trans. Am. Microsc. Soc.*, but will not be published until the October 1952 issue, which will be distributed about next Christmas.

There is, of course, no need to make your lists of desmids in the various collections absolutely complete. I merely added a few names to the lists because the records were easily available from my sketches.

I shall be very glad to see your drawings of the Florida desmids, and am particularly curious to know what disposition you make of some of the very strange forms that I have found.

Before replying to your question as to Teiling's proposed term "fecies", I re-read his paper on Radiation of Desmids. Though I can follow his argument in general, there are large parts of the paper that I cannot understand thoroughly because of my lack of knowledge of biology and particularly of genetics and ontogeny. In fact I do not know the exact meaning of the latter term. However, that does not prevent me from forming an opinion on the taxonomic use of his word. I think his proposal is a good one, but I think also that it is almost hopeless to induce all desmidologists, or even a majority of them, to use it. Do you know if the Committee on Nomenclature of Algae at the Botanical Congress in 1950 took any action on his memorandum? Even if they made a favorable recommendation it would take 25 years or more before it came into general use. Look how many authors disregard, or are ignorant of, Article 26 of the International Rules which requires that names of *forma* must agree in gender with the generic name. They still use the feminine ending for form-names in a genus like *Staurastrum*.

Nevertheless, if Teiling's term is to be used, somebody else besides Teiling must make a start. I am willing to use it, but if I publish any more papers in my own name, they will be very few and very minor ones. If you think favorably of his proposal I suggest that you start its use in your next paper, and since your papers are widely read and you are recognized as one of the very best authorities on desmids, your approval would go a long way towards establishing it.

If you do not agree with Teiling's proposal, my opinion is that nearly of the desmids having a number of radii different from that of the type should be placed in the rank of *forma* rather than *varietas*. There are a few exceptions that occur to me, such as *M. mahabuleshwereensis* var. *Wellichii* f. *triquetra* Romf., *St. elongatum* var. *tetrasporum* Solle, and the tri-radiate specimens of *St. ornithopodum* W. & G. that are found in some of my Florida collections. These reproduce true to their 3- or 4-radiate form, have not been found as *Jeniseiformis* as far as I know, and are very seldom, or never, found in the same habitat with the type. These, I think, are entitled to varietal rank. I do not know anything about the triquetral forms of *M. radiata* or *M. Murrayi*.

In the multi-radiate Staurastrum like St. Ophiura (6 to 10-radiate), St. sagittarium (6 - 10), St. sexangulare var. bidentatum (4 - 7), the number of radii is of much less importance. However, in my plates of North Australian desmids, (not yet published), I have illustrated a number of specimens of the two latter species, merely to place them on record.

As to the multiplicity of names that have been used, the situation does not seem to me so bad as you fear, since everyone knows what is meant. And there is nothing that you can do about it; you cannot change the names already in the literature, and each author is free to choose the name that he likes. The only thing one can do is to be consistent in his own publications. It appears to me that the names biradiate, triradiate, etc., are suitable for desmids with long radiating processes, and that trigona, tetragona, triangulata, quadrangulata are more applicable to desmids with angles but without processes, such as D. Baileyi f. tetragona Nordst. Also the Latin names, triradiate, triangulata, triquetra, should be used only with specific names derived from Latin, and the Greek names trigona etc., reserved for use with Greek specific names. But it's too much to hope that everyone would be so consistent!

There is one of Telling's terms that I like very much, "dichotypical", to designate specimens in which the two semicells apparently belong to different varieties or even species. A number of such cases are shown in the drawings that I sent to you. And right now I have a very curious case of dichotypy (or rather "tetrakotypy" - good word?) in material from Java and Sumatra that I am now working on. It concerns St. Wildemanii Cutw., which forms dichotypical cells with St. subtrifurcatum var. major W. & W., and with two other forms that I cannot identify and that may be new to science. One of the new? forms has a single long spine at each angle, and the other has two spines, sometimes three, arranged in a horizontal plane instead of a vertical plane as in St. Wildemanii. The two new? forms would be undoubtedly be classed as distinct species if considered separately, but the fact that all four are combined in different dichotypical cells proves that they all belong to one species.

It seems to me that St. subtrifurcatum var. major shows little resemblance to St. subtrifurcatum, and that since it combines with St. Wildemanii, its name ought to be changed. It will be some years before Prescott can identify and write up all of the strange but beautiful forms in this Indonesian material, so I am thinking of writing a short paper myself on this peculiar case, embodying the remarks of Schmidle 1902 and Fritsch and Rich 1937 on St. subtrifurcatum var. major. If I do write the paper and send it to you, would you be kind enough to criticize it? Prescott cannot do it in time. This summer he is going to teach summer school in Montana, then he is going to Point Barrow in Alaska where he will remain until near Christmas, and then he will go to Kusdor for some months. When he gets back he will be loaded with material, and will not have time to attend to my work.

With my best regards,

Sincerely yours,

18.3.-52.
ROLF GRONBLAD

KARIS
FINLAND

Dear Mr A.M.SCOTT,

I have had a talk with the manager of Nordiska Förenings-
banken in karis Mr Jari Sjöman who told me that there was no objection
to arrange the matter as you have proposed. He said it would perhaps be
best to place the money in your and my name, so that you or I can give
orders to pay it (not you and I), this for such a situation that the
money would have to be returned to you. I think your bank can arrange
the matter satisfactorily. I have informed Prof. Luther and his son
Dr Hans Luther, but not the Society.

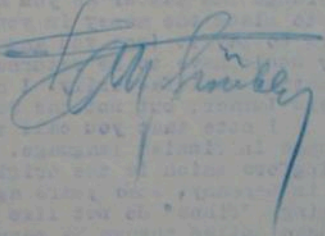
I note that you call the capital of Finland "Helsinki". This is
the name in finnish language. We swedish speaking people call it always
Helsingfors which is the original name, used always in Scandinavia and
also in Germany, some years ago also in other countries. We swedish
speaking "finns" do not like this "Helsinki", which since some years
has been pulled through by narrow-minded nationalists. (This is, indeed,
a matter of no importance, but I like to inform you). In the same way
Finland is called "Suomi".

I am very glad if you have made some remarks to my drawings.
It will always be useful to know your suggestions. I think it will be
useful to let you see my new drawings when I have finished Fla.

As I have already pointed out to you I am not trying to make the lists of every sample absolutely complete. I think this is not needed. Those lists which are to be published are intended to show the general nature of the associations, and if the names of a few species are lacking, it does not matter. Do you not approve of it?

Your new samples from Japan and N. Australia would certainly be very interesting to have. Also the additional collections from Florida, which country seems to be quite an inexhaustible source.

Sincerely yours,



April 21 1952

The Chase National Bank,
New York City, N.Y.

Gentlemen,

I am enclosing my check for \$500.00, which I request that you place to the credit of Nordiska Föreningsbanken, Helsingfors, Finland, advising them to that effect.

Very truly yours,

Copy to Nordiska Föreningsbanken.

April 21 1952

Nordiska Föreningsbanken,
Helsingfors, Finland.

Gentlemen,

From the enclosed copy of a letter you will note that I have today sent to the Chase National Bank in New York, the sum of \$500.00, to be placed to your credit.

This money is intended for eventual payment to the Finnish Scientific Society, in two or three or perhaps five years, to defray the costs of publishing a scientific paper by Dr. Rolf Grönblad of Karis, and myself. I request that you keep the money on deposit in the Chase National Bank, for my account, until such time as it is needed. It is to be subject to withdrawal either by me, or by Dr. Rolf Grönblad, 86 Centralgatan, Karis Finland.

Dr. Grönblad spoke to Mr. Jarl Sjöman, the manager of your Karis branch, and Mr. Sjöman said that the deposit could be handled in this manner.

Very truly yours,

Copies to Dr. Rolf Grönblad.
Mr. Jarl Sjöman.

The Chase National Bank

OF THE CITY OF NEW YORK

FINE STREET CORNER OF NASSAU
NEW YORK 18, N. Y.
COMPTROLLERS DEPARTMENT

New York June 19, 1952

IN REPLYING PLEASE REFER TO
1-22 JH

Mr. Arthur M. Scott
2824 Dante Street
New Orleans 18, Louisiana.

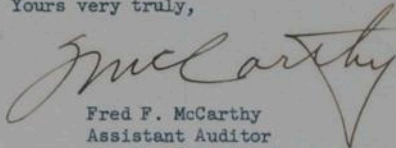
Dear Sir:

We are in receipt of your letter of June 14, 1952 concerning your check for \$500. forwarded to us on April 21 for credit to the account of Nordiska Foreningsbanken, Helsingfors, Finland.

We received a letter from Nordiska Foreningsbanken dated May 15 requesting our advice of credit concerning this entry. In our reply of May 21 to Nordiska Foreningsbanken we advised them that we had credited their account on May 1 with \$500. in settlement of this transaction.

We trust the above information is sufficient for your purposes.

Yours very truly,



Fred F. McCarthy
Assistant Auditor
Foreign Department

ic

ROLF GRONBLAD

KARIS
FINLAND

Dear Mr A.M.Scott,

Yesterday I got my Desmid drawings and lists, and today your letter with the photocopies.

I thoroughly agree with with your proposal as to the publishing of your "new species". In fact I have myself also many times thought that it would be good to make a separate publication of the new Desmids but I have waited for your initiative. Well, I shall try to prepare a list of "New Desmids from the Southern States of USA." Do you wish it published in a Finnish paper? (Mem.Soc. Sci. Fenn.)

I am glad to have your remarks and suggestions on my drawings and also your completions to my lists of species.

When I have finished the Fla viala, I think it will be useful
to let you have a look at my new drawings, so I shall send them to
you.

With kind regards, your sincerely,

What do you think of Teilings proposal
to use the term "facies" when we wish to nominate different radiated
forms of a Desmid. There have been used both "forma" and "var.", and
biradiata, triradiata...., trigona, tetragona, pentagona...., triangulata
quadrangulata, quinquangulata....., triquetra, bidentulum, tridentu-
lum... This is a rather bad and puzzling situation. On the other
hand it seems to me that the radiation has not the same value in all
genera nor in all species of one genus. Sometimes the radiation seems
very markedly fixed, sometimes (Staurastrum) you can see in one
sample 3-, 4-, 5-, 6-, 7-radiated forms of a species with all kinds of
combinations of differently radiated semicells. In such cases the
radiation seems not equal to what we are used to call varieties or
formae. I do not know what to do. What is your opinion?

Ab Nordiska Föreningsbanken

(Oy Pohjoismaiden Yhdyspankki)

1/2 UNITAS
TC

Helsingfors, 16th May, 1952.

AIR MAIL

Mr Arthur M. Scott,
2824 Dante Street,
New Orleans 18, LA.

Dear Sir,

Your Payment of \$ 500:- to our credit.

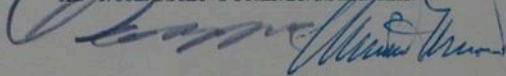
With reference to your letter of 21st April, 1952, we would inform you that we received to-day from The Chase National Bank of the City of New York, New York, their Credit-advice relative to the above payment.

We have placed the amount of \$ 500:- into a \$-disposition account in the name yourself and Dr. Rolf Grönblad, 86 Centralgatan, Karis, as instructed by you.

We are, Dear Sir,

Yours faithfully,

AB NORDISKA FÖRENINGSBANKEN



ACKNOWLEDGMENT
NON-NEGOTIABLE

THE CHASE NATIONAL BANK
OF THE CITY OF NEW YORK
PINE STREET CORNER OF NASSAU

FOREIGN DEPARTMENT
PAYING AND RECEIVING TELLERS

We acknowledge receipt of the following described item(s)
for collection, subject to the conditions stated on reverse side.

NEW YORK 15, N. Y., 4/24/52

5/1/52

DATE OF LETTER	DESCRIPTION	
4/21/52	[Faint, illegible text describing the item]	\$500.00

RECEIVED FOR CREDIT OF

NORDISKA FORENINGSBANKEN
MELSFINGORS FINLAND

To

ARTHUR M. SCOTT
2824 DANTE ST.
NEW ORLEANS LA.

FX 286

MH

June 8 1952

Dear Dr. Grönblad,

No doubt Frère Irénée-Marie has sent you copies of the several papers on Canadian desmids that he has published during the last year or so. His latest, which you may not have received yet, is a good-sized book of 208 pages constituting Nos. 1 and 2 of Volume IV of *Hydrobiologia* 1952, entitled "Contribution à la connaissance des Desmidiées de la région du Lac-St.-Jean". It will be very useful to all American desmidiologists and only a little less so to European workers.

I only received this book about a week ago, and of course have not had time to study it, but I can see that in writing our new paper on new desmids you will have to check Irénée-Marie's publications carefully, because I have already seen several species on which he has anticipated us. Here are some of them, together with other interesting species:

- Stauridium Coderii*, sp.nov. Pl.VI, Figs. 17-18. Compare my drawings 2437-8.
aspinosum Wolle, Pl.VI, Fig.6 " " " 2114-25
calyxoides Wolle, Pl.VI, Figs.12-14. " " " 2329.
- I was under the impression that I had several drawings of this species, but cannot find them in my photo copies. Also I was unaware that it has been published by Wolle; it must be in the 2nd. edition (1892) of his work, which I do not possess.
- Stauridium coronulatum* Wolle, var. *quebecense* var.nov. Pl. VI, Fig.20. This seems to be the same as *St. Ophiure* var. *coronatum* Scott 1950, except that that my plant has an elevated apical surface, not shown in I-M's drawing. I do not consider that either of them can be identified with the illustration in Wolle 1892, Pl.XLIV, Figs. 12-14.
- Stauridium comptum* Wolle, Pl.VI, Fig.19. Not the same as *St. Ophiure* var. *minus* Presc. & Scott, which you thought might be identical with *St.comptum*.
- Stauridium novae-caesareae* Wolle, Pl.VIII, Fig.4. Compare my drawings 1814-18.
palmatum sp.nov. Pl.VIII, Fig.10. " " " 2004-05.
frangens sp.nov. Pl.VI, Fig.2. " " " 2222-25.
- Desmidium longatum* Wolle, Pl.V, Fig.4. Possibly, but not necessarily, the same as *Desmidium gymnozygoforme* Presc. & Scott. Compare my drawings 550-556.
- Cosmerium novae-terrae* Taylor. Pl.XI, Fig.7. Compare my drawings 1715-20
refringens var. *minor*, Pl.XII, Fig.1.) " " " 1704-11.
" var. *major*, Pl.XII, Fig.3.) " " " 1669-74.
- Eustrium Clestonii* f. *hiens*, f. nov. Pl.XVIII, Fig.10. This is the same as *f. apertisulcatum* Presc. & Scott, to be published in probably the next issue of *Hydrobiologia*. Too late now to change it.
- Eustrium compactum* Wolle, Pl.XII, Figs. 9,10. Irénée-Marie thinks that this species was reduced, without reason, to a variety of *E. elegans*, by Krieger. But I-M is wrong by his own argument, since he says that *E. compactum* lacks any ornament in the center of the semicell which differentiates it from *E. elegans*. There are a number of illustrations of *E. elegans* var. *compactum*, in Krieger, Fritsch & Rich, Presc. & Scott, showing 2, or 3, granules in the center of the semicell. I have recently found it in North Australian material, also from Indonesia.

In another paper I-M has described *Cos. dentatum* f. *glabrum*, f. nov., which is the same as shown in my drawings 1222-1223.

In your drawing No. 5332 you show a *Euastrum* (sp. nov.?) that I had overlooked. A few days ago I found a somewhat similar *Euastrum* in Borneo material that I have tentatively identified as *E. bipertitum* Krieger, though if this identification is correct, Krieger's illustration does not give a very good idea of the appearance of the plant. I am enclosing a drawing of the Borneo specimen, from which you will see that it shows considerable resemblance to your *Euastrum*, and both of them are similar to *E. trigibberum* W & W. In fact, the visual appearance under the microscope of all three plants gives me the impression that they may be related. All three have somewhat similar profiles, the same type of central ornament, very similar side and top views, and the same kind of low, rather obscure granulation that is difficult to distinguish and draw.

Also I am enclosing a few more sketches of novelties that I have made recently, and under separate cover I am sending a package containing the following collections:

Alabama No. 5. A very nice collection, the best that I have made in Alabama, with a large assortment of species.

Florida Nos. 223, 224, 225.

North Australia, A80 and A109. These are included simply for your enjoyment, and later I shall send you some more of them. Prescott is now finishing the descriptions of the desmids, which will be published next year by the Australian Government.

I have received from the Nordiske Foreningsbanken an acknowledgement of the money that I sent to the Chase National Bank in New York, so you are free to draw on it whenever it becomes necessary.

With my best regards,

Sincerely yours,

June 14 1933

The Chase National Bank,
Pine & Nassau,
New York 15, N.Y.

Gentlemen,

On Apl 21st I sent you my check for \$500.00 with the request that you place it to the credit of Nordiska Foreningsbanken, Helsingfors, Finland, and I have your receipt dated Apl 24th. The cancelled check has been returned by my bank.

The Nordiska Foreningsbanken wrote me on May 15th, received today, that they have not yet received your advice of credit, and have asked you to investigate.

Please advise the result of your investigation.

Very truly yours,

Ab Nordiska Föreningsbanken

(Oy Pohjoismaiden Yhdyspankki)

1/2 UNITAS
TC

Helsingfors, 15th May, 1952.

Mr Arthur M. Scott,
2824 Dante Street,
New Orleans 18, LA.
U.S.A.

Dear Sir,

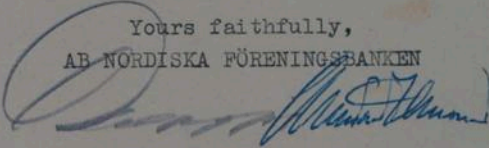
We have received your letter of 21st April, 1952, and the copy of letter addressed to The Chase National Bank of the City of New York, New York.

In reply we would inform you that we have not received the said bank's Credit-advice relative to the payment referred to in your letter. We have, however, asked them to investigate the matter.

We are, Dear Sir,

Yours faithfully,

AB NORDISKA FÖRENINGSBANKEN



Rolf Grönblad
Karls
Centralgatan 86

July 7th 1952.

Dear Mr Arthur M. Scott,

Three days ago I got your bamboo-cane with desmid samples hidden in it. I have had no time yet to look at them but surely they will prove very interesting.

I have taken time to study Irene-Marie's new book Contrib. Desm. Lac-St-Jean and I have made some remarks to such species that I have found somewhat dubious. Perhaps you also will be interested to see what I think about these species. This is only for you, I do not wish to offend I.-M. and I do not at all insist upon being always on the right track. I am sure you will not approve of all my suggestions.

- Pl. I. fig. 1. (seems ^{Somehow} like *Cl. Braunii* ?? but perhaps it is not this?) I-M writes *Cl. angustatum* var. *angustatum*; this should be var. *asperum* West. But again this is identical to var. *Boergesenii* Gutw. as you can see in Krieger's Desm. Fl. p. 365. The older name must be accepted. (I-M has a lot of typographical errors: *juncidium* pro *juncidum*, *Jennerii* pro *Jenneri* &c). - fig. 10 is called *Cl. incurvum* f. *latior* (pro *latius*); this cannot be *C. incurvum* which always must have acute ends. (perhaps it may be *Cl. Jenneri* var. *robustum*?)
- Pl. II. fig. 4 *Cl. nematodes* pro *nematodes*. fig. 11-12 "*Penium crassum*" must be a *Cosmarium* if we accept the results of Lütkenmüller's careful investigations on the structure of the cell-wall of the Desmids. It is quite absurd to call it *Penium*. According to Krieger *Penium* as now accepted, contains desmids of the old "*Penium*" which have girdle-bands, or whose cell-wall is characterized by longitudinal ridges or longitudinal rows of punctulations (which may be invisible without staining). - Never has *Penium* such a deep constriction between the semicells as in I-M's fig. 1 & 2 on pl. IV. There is, however, still some confusion as to the limits of *Penium*. (Telling is doing something with them, but I do not know which his intentions may be - It seems that I-M does not take any notice of Lütken's systematical investigations at all. (I have copies of Lütken's letters to G.S. West where Lütken has explained his aims and ideas. It seems that West also approved of them which fact also is evident from the rearrangement in vol. V by N. Carter). Now comes I-M with his arrangement of *Pen. crassiusculum* and *Pen. crassum* quite contradictory to all these matters of fact. Lütken's name is not at all mentioned by I-M.
- Pl. III. fig. 2-3. *Pl. coronatum* v. *complanatum* seems to be = *P. truncatum* v. *Farquharsonii*, or very near to it. I do not believe that the pores are correctly drawn in fig. 2, lower semicell. - Fig. 12 is *Pl. verrucosum* (cf. Krieger).
- Pl. IV. fig. 9-10. is *A. Incus* v. *Ralfsii*; fig. 12 is "*A. triangularis* v. *latius*" (not *latum*) is perhaps a new species?? - fig. 13 *Spond. pulch.* var. *effringum* nov. var. is most probably the same as in your samples Fla 11, Fla 27.

July 24 1952

Dear Dr. Grönblad,

Your letters of June 28 and July 7 arrived a few days ago, and I expect that your drawings will come shortly.

I am glad to get your comments on some of Irene^e-Marie's species, and shall send you some remarks of my own shortly. You may be sure that I shall not mention to him that you have written me about his work, for I know that he is unduly sensitive (not to say "touchy"). Probably somebody has stepped on his corns in the past. I wrote thanking him for his latest work, and congratulating him, and also offered to comment on some of his specimens, if he would like me to do so. He replied saying that he would be glad for me to do so, so when I have more time I shall write him, and try to put my remarks in such a way that they cannot possibly offend him.

You mention one of his items, *Cl. angustatum* var. *angustatum*, which may be a mistake. But in a recent paper by Elwyn O. Hughes, "Closterium in Central Canada", he states that at the last Botanical Congress in Stockholm, a new rule or recommendation was adopted, which permits the designation of the type of a species by repeating the specific name as a varietal name. Thus *Cl. angustatum* var. *angustatum* would mean the type of the species, as differentiated from any variant form.

The many mistakes in spelling must be blamed on Irene^e-Marie, not the poor printer, according to my view. You will note that there are few if any mistakes in the text; only in the names. I am somewhat surprised that the editor, Prof. van Oye, who is a desmidiologist himself, did not catch some of these glaring mistakes and correct them, when he read the MS.

Here are the answers to your questions:

1) I shall be glad to put the magnification on my drawings when you return them to me for drawing the plates. But if you wish to do it yourself, here is a very easy way, if you own or can borrow a slide-rule: Simply measure the length of my drawing in millimetres and divide this by the dimension which I give in numbers on the sheet. Thus if the drawing measures 94 mm, and the corresponding dimension is stated as 57 u, the calculation is:

$$\frac{94000}{57} = 1650x, \text{ which is my usual magnification for}$$

desmids of average size, up to 100 u long.

2) I agree that it is desirable to publish several figures of novelties rather than one. In cases where I have only one drawing I can try to find additional specimens, though this may be difficult in some cases, because a few of the novelties are extremely rare.

3) I think a list of the habitats with the corresponding numbers should be given at the beginning of the paper. Then for each item it will be necessary only to state the number of the collection. I can prepare the list when I know the desmids that you include in the paper.

4) A portion of all my USA collections has been deposited in the Farlow Herbarium of Harvard University, Cambridge, Mass.

5) I have searched half-a-dozen mounts from La. 27, but cannot find either *A. bifidus* or any other *Arthrodesmus*. I have no copies of the drawings which you have numbered A-35 to A-42 (8 drawings?), and do not remember what this *Arthrodesmus* looked like. There is an *A. bifidus* var. *latidivergens* on my drawing No. 56, but apparently the top view that you want is of some other

form. If I made 8 drawings it would seem that the specimens must have been fairly abundant; perhaps some of the drawings are from other collections in which examples might be easier to find. If you will make a tracing of a typical specimen and send it to me, together with the numbers of any other collections in which it may occur, I shall make another effort to find it and draw a top view.

I hope that you have a good time on your trip to Italy, and that you get plenty of desmids. But this may not be easy unless you have advance information where to look for them. I have found that going to strange regions, like Cuba, Mexico and Central America, to look for desmids, can be quite disappointing. Undoubtedly there must be desmids in these countries, but I was unable to find many of them in the short time that I could spend there.

Just now I am in the middle of writing a long paper on North Australian desmids, which turns out to be much more of a job than I had bargained for. Prescott has run away to Montana, and from there he will go directly to Point Barrow, the northernmost point in Alaska, and will not return to Michigan until near Christmas. So although he has written all the determinations and descriptions, I am left with the job of getting the paper into final shape, and I am not at all familiar with this work, which involves finding and verifying references, many of which are not in my small library; checking plate and figure numbers; arranging all items alphabetically, correcting many small errors in Prescott's spelling and grammar; changing or modifying some of his descriptions when they are not quite clear; and arguing with him by airmail about some of the determinations that I cannot agree with; arranging for the Latin diagnoses and copying them in their proper places, and even occasionally changing a few words of the Latin to correspond with my changes in the English descriptions. And I don't know much about Latin, so when you read this paper, if you find a few nouns not in the correct declensions, please forgive me and don't blame Miss Crossdale, our translator.

With my best regards,

Sincerely yours,

ROLF GRONBLAD

KARIS

FINLAND

August 26th 1952.

Dear Mr Scott,

Thank you for your letter of July 24th. I hope my drawings also have arrived safely. To day I am sending your drawings of *Arthrodesmus* with my descriptions (or "proposals to descriptions") of new species and varieties. I have had great difficulties in arranging the taxonomical entities and I hope you will write your critical remarks and proposals for alterations. When such a exceedingly rich material as yours is at hand, then many of the conventional ideas as to the limits of the species become somewhat dubious and obscure. It is also a matter of taste if you make a species "*A. Ralfsii*" of one of the innumerable varieties of *A. Incus*. (as made by G.M.Smith). I cannot quite see why it would be needed. Then, with the same right, the whole species *A. Incus* should be split up into several species. But what would be gained with this? I do not believe much. I have also tried to be conservative and not to put up a possibly great number of "new species". Consequently

such a species as *A. maximus* has become rather polymorphic. This can be justified by the intermediate and the dichotypical formae which are not uncommon. I am not at all urging for the correctness of my arrangements and I am most willing to take your counsel.

I have also sent to you all the *Staurostera* which you have not copied and which I have marked A1, A2, A3, &c. Among these also some new descriptions are appended.

I also hope you will kindly make my English correct. If there are some other mistakes or omissions you may do all you wish in order to make our paper into suitable condition.

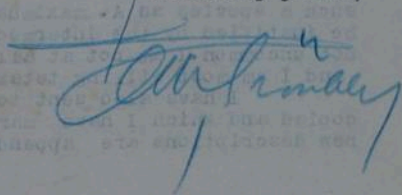
Perhaps this *Arthrodesmus*-paper could be prepared for printing already this fall? And published in *Commentationes Biologicae Soc. Sci. Fenn.*?

To morrow 27.8. I am starting for my journey to Italy, where I am going to stay about four or five weeks. The first time I am staying at a hydrabiological Insitute (at Pallanza) where one of the younger workers Dr Ravera will accompany me on my excursions. This will be very useful because he has some knowledge of the waters and other localities where algae are to be found. Thus I hope my journey will not be in vain. At any rate I shall enjoy the experiences of foreign countries and people very much.

When you have examined the *Arthrodesmi* and my own sketches please return them with your remarks.

With kind regards

sincerely yours,



No 2 *A. triangelus* + *subtriangelus*

No 624, 625, 626 are *folia* *Incus* + *dentata*

and *A. conopsea*. They are sent in the parcel sent to you.

Aug 29 1951

Dear Dr. Grönblad,

Thanks for your letter of Aug 16th, with the very pretty postage stamps. The stamps will go to my friend Mr. M. Sachlan in Bogor, Java, who is a philatelist in addition to being a lover of desmids.

I do not have the time to reply fully to your letter just now, because my wife and I are leaving early tomorrow morning on an automobile trip to the Great Smoky Mountains in eastern Tennessee, a part of the Appalachian Range. This is a pleasure trip, to get away from the terrible heat in New Orleans, but I shall take my collecting outfit along, in case we encounter any favorable collecting places.

In the meantime I am sending a few sketches of *Closterium cuspidatum*, with a copy of a letter and sketch of this desmid made by Minoru Hirano at my request. You will note that he tried to find a suture on a number of specimens, but could not do so. I have lately seen one specimen from North Australia, one from Japan, and one from my Florida collection No. 182. Only in the latter specimen could I find a suture, which was barely visible even with the finest adjustment of my 65x apochromatic objective. So I feel safe in saying that under ordinary circumstances no suture is visible. Note that the original drawing by Bailey (copied in Ralfs), also the one by Ch. Bernard, and those by Whelden, Hirano and myself, show no sutures. Prescott is the only one who has seen it, and it is possible that his specimens from Michigan may show it more plainly.

Also enclosed are a couple of sketches of *Cosmarium denticulatum* from Peru, which agree quite well with two of your varieties from Brazil. You are perfectly free to send these, and any others of my drawings or notes, to Dr. Krieger. I shall be happy if my observations can in a small way help to advance our knowledge of desmids.

Will write you a longer letter upon my return from the trip.

Sincerely yours,

Sept 23 1952

Dear Dr. Grönbled,

I have received the package containing your drawings of the Florida *decaids*, also the one with my drawings of *Arthrodesmus* and some *Stourastra*.

I shall go to work on the *Arthrodesmi* and return them to you as quickly as possible, but first I must finish a paper on North Australian *decaids* which is nearly complete. For the last week I have been laid up in bed with another bout of illness; this time it was an intestinal disturbance of some kind, accompanied with diarrhoea, and probably connected with my gall-bladder trouble. Today is my first day out of bed, and the first time I could get to the typewriter.

It is evident that you have had quite a tussle with the *Arthrodesmi*, and when three experts - Prescott, Telling, and yourself - offer three different names for the same plant, what is a poor amateur like me supposed to do? However, in a few cases I can offer some suggestions based on an acquaintance with *Arthrodesmi* from Indonesia and Australia. For instance, *A. curvata* from Australia is quite different from the American plant that you identified thus. Also *A. arvensis* from Indonesia differs somewhat from the Florida plant for which you suggested this name, being of more slender habit, with longer spines, and a smoothly convex apex, not at all truncate. I shall send drawings of these.

In the North Australian paper I am describing *A. subulatus* f. *incaesatus* f. nov., practically identical with the American plant for which you suggested the same name. This paper is supposed to be published next year, in the Botanical Volume of the Official Report on the Arabian Land Expedition. But in view of the delay in publishing the previous volumes of the Report, it is quite likely that your paper on *Arthrodesmus* may appear first. In that case I can change the authors' name from Scott & Presc. to Grönb. & Scott, accompanied with a tentative date thus: (1952, in press). I don't think it will be possible for you to get your paper into print this year.

I am going to send you a set of prints of the 22 plates of *decaids* for the Australian paper, with lists of names. Of course, they will be only for your private information, and the new names cannot be used until they are published. But I think the illustrations will be of help to you, in other genera besides *Arthrodesmus*. I am sure that you will disagree with some of the determinations, as I do myself. I have quite a long argument with Prescott on some of them, and in a few cases I have converted him, but in others (e.g., *Closterium cuspidatum*) he has remained obdurate, so I have had to defer to him.

Enclosed is a partly corrected proof (which you may keep) of a new paper on *Hesperium* that should appear soon in *Hydrobiologia*. This should help in your determinations of the American plants of this genus.

I hope that you had an enjoyable and successful trip to Italy, and that you will tell me something about it.

Sincerely,

List of desmids in Collection Alabama No. 5. (Probably not complete).

<i>Commarium evelae</i>	<i>Xanthidium Smithii</i>
<i>pseudococcatum</i>	<i>cristatum</i>
" var. nov. ?	<i>antilopaeum</i> fa.
<i>Begonii</i>	" var. <i>polymorum</i>
<i>pseudamoenum</i> fa.	" var. <i>minnespolicense</i>
<i>ornatum</i>	
<i>tinatum</i>	<i>Fleurotesium constrictum</i>
<i>quadratum</i>	<i>Ehrenbergii</i> (var. <i>elongatum</i> ?)
<i>moniliforme</i>	<i>verrucosum</i>
<i>pyramidatum</i> ?	<i>trabecula</i> var. <i>rectum</i>
<i>ordinatum</i> ?	
<i>Portianum</i>	<i>Russtrum Cistonii</i>
<i>pseudoprotuberans</i> ?	<i>evolutum</i> var. <i>integrius</i>
<i>scrobiculatum</i>	<i>validum</i> var. <i>globum</i> (very small)
	<i>denticulatum</i>
<i>Microsteris radiata</i>	<i>Bambusina Borreri</i>
" var. <i>gracillima</i>	
<i>laticeps</i> var. <i>crassa</i>	<i>Hyalotheca dissiliens</i>
<i>planatifida</i> fa.	<i>undulata</i>
<i>fimbriata</i> var. <i>spinosa</i>	<i>mucosa</i>
<i>Torreyi</i>	
<i>floridense</i>	
" var. <i>spinosa</i> } dichotypical	
<i>depauperata</i> var. <i>Mitchellii</i>	<i>Spondylosium secedens</i> ?
<i>denticulata</i>	<i>planum</i>
	<i>Netrium digitus</i>
<i>Triploceras gracile</i>	<i>Desmidium coarctatum</i>
<i>verticillatum</i>	
<i>Sucrostrum anatinum</i> (var. <i>curtum</i> ?)	
<i>brachiatum</i> (var. nov. ?)	
<i>teliferum</i>	
<i>spiculiferum</i> ?	
<i>Cerastes</i>	
<i>protectum</i> var. <i>planetonicum</i>	
<i>paradoxum</i> var. <i>parvum</i> ?	
<i>arabae</i>	
<i>polymorphum</i> ?	
<i>rotula</i>	
<i>diptilum</i>	
<i>terraceum</i>	
<i>ankyroides</i> , 5 and 6 radiate	
<i>furosum</i>	
<i>minnespolicense</i>	
<i>iostenum</i> ?	
<i>longispinum</i>	
<i>Ophiure</i> var. <i>minus</i> Presc. & Scott.	
<i>Closterium gracile</i>	
" var. <i>tenue</i>	
<i>Kützingeri</i>	
<i>abruptum</i>	
<i>dianse</i>	
<i>costatum</i>	
<i>Ralfsii</i> var. <i>hybridum</i>	
<i>turgidum</i>	
	Total, about 73 forms.

Karis

Centralgalan 86

Dear Mr A.M.Scott,

thank you very much for your sendings, copies of drawings, a proof of your Euastrum paper, Monthly Scientific, &c, &c. You must try to forgive my long delay in answering your letter. I returned from Italy October 1st and I have had very much to clear up since that day. You have sent so much to me that I am quite overwhelmed with your activity (that is very good, very useful and very laudable, indeed). There are many things to discuss and on this account I have not tried to answer your letter before I should have time to give you an answer clearing up some of my own points of views.

I begin with your Australian Desmids:

Plate I.

Fig. 1. Cannot be *C. aciculare* sensu West because the ends are far too thick. See W. & W., Mng. I. pag. 175 "...apices very narrow..". Cf. fig. (XXI:10). I don't know what I should like to call your fig. - but by no means *C. aciculare*.

Fig. 2. *C. acerosum*. All specimens of this species I have seen had a truncate apex like W & W, Mng. I, XVIII:4, but in Rabenhorst's Exsicc No 850 apices are rounded.

Fig. 6. *C. Libellula* var. *interruptum*. I very much doubt whether this is any taxonomical character at all. May be only a cell before division?

Fig. 14-15. Perhaps *C. Kuetzingii*, but why not *C. setaceum*? (cf. var. *vittatum* Grönbl. Brasil.)

Fig. 18. var. *tatrica* is not like this. Raciborski's fig. is much more rounded; (see copy), while your fig. is much more like var. *hians*

Plate II.

Fig. 11. I cannot see any difference from fig. 17 pl. 1 in Gbl. Brasil. which is called *Pl. burmense*. Joshua's orig. fig. is copied here.

Plate III.

Fig. 7. looks very strange to be called *E. bidentatum*, the central ornamentation being quite different. Cf. *E. oculatum* + var. in Krieger Flor. 82:13-14. Why not vertical and lateral view? In Euastrum vertical view is most important.

Plate V.

Fig. 7 seems also nearly related to *E. evolutum*.

Plate VI.

Fig. 7. I do not at all see why this should be an Euastrum (in spite of Skuja's propagations in Burma Desm. p. 112-113)

Fig. 9-10. I should not call it *E. dubium* at all - deep apical incision - new sp.?

Fig. 19. Why not a variety? it seems sufficiently different from the typical species.?

Fig. 18 *E. binale* v. *Borgei* seems not different from *E. binale* f. Grönbl., New Desm. III:23.

Fig. 20. *C. impressulum* is certainly not typical, if at all this spec. I think there are drawings like yours published under the name of *C. impressulum*. But if you compare with the original fig. there is a quite distinctly pronounced difference.

Fig. a vertice are most important and always useful in Euastrum.

Plate VIII.

Fig. 4 is not essentially different from Turner's fig. in *E. Ind.*

Plate X.

Your *X. superbum* is not identical with the typical species described from Finland. (Also Borge's fig. is not typical) - why not var. nov.?

Plate XI.

- Fig. 4 - vertical view ?? very important.
Fig. 5. *A. subulatus* - perhaps? - but why not *A. convergens*? (subulatus if typical has vertically directed spines, which seems fairly constant in this spec.)
Fig. 6. very much different from typical spec.
Fig. 8. has the characteristic curvature of the spines, which, however, are shorter

Plate XII.

- Fig. 5 is not = West's *subincus*, which is characterized by "a closed sinus with a dilated extremity" (Mngr. IV p. 106) - Perhaps more like *A. subvalidus* Gbl. Brasil?
But: a vertice? chlorophores? pyrenoids?
Fig. 8 - chlorophores?
Fig. 9 - "- ?
Fig. 10 - a vertice ?
Fig. 11. - not typical.
Fig. 12. why at all *C. difficile*? when the characteristic rows of pores are lacking ?
Fig. 13. *C. subreinschii*? - not impressulum.
Fig. 14. not impressulum. - cf. *C. Reinschii*, *C. subreinschii*, &c.
Fig. 17. not typical *C. obsoletum*. (is the circular area in front view a thickening of the vell-wall?)
Fig. 19. Certainly not *Lundellii* v. *ellipticum* - see *C. perforatum* v. *rotundatum* Skuja.
Fig. 23. - a vertice?
Fig. 24. - a vertice?
Fig. 26. *C. Regnesii* (not *Regnesii*), not typical : var.? I have seen quite a similar drawing published somewhere, but I cannot find where.
Sæaldi, Kjellmani, Turneri, Eichleri, Regnesi are spelled with one "i".
Cf. Nordstedt's Index. - Do you have the "Index"? I think it is not very difficult to get a copy of it. (?)

X) Plate XIII.

- Fig. 5. looks more like *pseudoconnatum*, but a vertice? chlorophores? In species of this kind both are needed for identifying.
Fig. 6. Certainly not *C. connatum*. - chlorophores? a vertice?
Fig. 7. perhaps, but chlorophores? a vertice?
Fig. 10. a vertice? chlorophores?
Fig. 12. a vertice? does not look like *C. trachypleurum* typic.

Plate XIV.

- Fig. 3. cf. also *C. trachypleurum*?
Fig. 8. not to *subcostatum* which has rounded crenulations.
Fig. 14. *S. retusum* var. (not typical)
Fig. 18 = ?
Fig. 20 = *S. bidentulum* Gbl. Brasil?
Fig. 21. not Dickiei (?)

Plate XV.

- Fig. 8. cf. also *S. mamillatum* Nordst., Brasil.
Fig. 11. See also *S. Renardii* &c., the synonymics is quite a tangle.
Fig. 14. cf. *S. longipes* var. *parallelum* Gbl. (1948, p. 421, fig. 46.).
Fig. 16. I should not like to call it *S. polymorphum*. There are already too many greatly different desmids clustered together in this "species" with its unfortunate name.

Plate XVI.

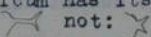

- Fig. 1. *S. leptocladum* (var. *cornutum*?) cf. Grönbl. Brasil. pag. 27.
Fig. 10. *S. setigerum*, yes, perhaps, but a fronte?
Fig. 11. I should have made a new species. *S. pelagicum* has its angles outdrawn into a process which is bifurcate at its end, thus:  not: 

Plate XVII.

- Fig. 4. *S. vestitum* is not typical - "f. *bulbosum*"?

X) Fig 14 seems to me very much like *C. cristatum* (= *C. tessellatum*) - variety??

Plate XX.

Fig. 6. Why is this called *S. pterosporum* while pl. XIV fig 17 is identified as *S. Omearii* (I think the name must not be spelled "O'Mearii" as some authors do - also Nordstedt's Index). As far as I can see there is no essential differences as to the vegetative cells, - only the zygospores are different. There are also other desmids which cannot be identified without zygospores: *Clost. idiosporum*, *Penium spinospermum* &c.

Fig. 8. Why not *Cosmarium*?

Fig. 16. You do not accept "*Bambusina*"?

Plate XXI. I have never seen *Streptonema*. Could you send me material?

Plate XXII.

Fig. 3. *C. subturgidum*: chlorophores? very important.

As you see there are many desmids among your drawings which I should like to identify in a way different from yours. With this I do not pretend to be right so you have no reason for feeling offended. And I hope earnestly you will not take it in such a way. When we are to collaborate it is quite necessary that we have a possibility of speaking quite freely. As far as I know you, I think you agree with me.

.....

As to my journey to Italy I can tell you that it was very interesting and also the material of Algae collected was rich in desmids and other algae, especially Protococcales, such as *Pedabastrum*, *Scenedesmus*, *Coelastrum*, &c. The Desmids were = *Chlorococcales sensu West - Fritsch*) not so rich as in Fennoscandia because the waters are calcareous in many parts of this country. Lakes with a very high percentage of lime are no good habitats for desmids. Nevertheless also the desmid flora was rich in species. I have had a first preliminary look at my 138 samples, but I am sure there is a lot of things which I have not seen as yet. Moreover I got some plancton samples from the Museum of the Hydrobiological Institute at Pallanza (the head of this Institute is Professor Vittorio Tonolli and he would be very glad to have your papers on Desmids. The address is: Istituto Italiano Idrobiologico, Verbania Pallanza, Italia). At this Institute I lived during 10 days, and made excursions in the neighbourhood, and also longer trips with autocar. This land between Lago Maggiore and Lago di Como is very rich in lakes and very beautiful. Then I had to stay 5 days at the home of prof. Moretti who is a zoo-hydrobiologist. He and his auto were at my free disposal and we were from early morning to sunset driving from lake to lake, from pond to pond. The professor was a very useful guide knowing all habitats of any interest. Moreover he was one of the kindest men I have ever met. After this I had a young dr Oscar Ravera from the Pallanza Institute as companion during our excursions in the Dolomites (formerly Südtirolen of Austria). Also a very beautiful country (but too rich in limestone). There was also an Italian botanist, prof. Marchesoni, who very kindly helped to plan our excursions. After this we went to Venice, Firenze (=Florence) and to Rome. Here again prof. Cannicci at the Central Laboratory of Hydrobiology arranged two Italian students of biology as company for my excursions to Ostia antica and the Albano Mountains. Then my excursions were ended and I had to see Rome as a tourist, and there was, indeed, very much, and very beautiful things to see. You see, I had always help from the Italians and only in this way it was possible to get at sufficiently good results. I did not speak Italiano and it had been very difficult to go on excursions in the countryside without native persons as guides. The climate in Italy is splendid, the nature beautiful and the people always very kind and helpful. So you see I was a very lucky chap and I enjoyed immensely my first journey to southern Europe. With English and Latin languages and a little German it was possible to get along quite well. There you have a short report of my journey.

.....

Then I shall answer some questions and proposals in your last letters:

- 1). As to the manner of using names like *Closter. angustatum* var. *angustatum* I don't like it at all. I am not aware of such a recommendation, but perhaps it has escaped my attention. (Now I see in the "Synopsis of Proposals" concerning the Internat. Rules of Botan. Nomencl. ... prepared by Lanjouw, that there has really been such a proposal as to use combinations of names like "*C. angustatum* var. *angustatum*", but I don't know if it was accepted. Anyway, it seems to me bad quite as the zoological names *Cossus cossus*, &c. in the Internat. Rules of 1935 (pag. 21) you can see: "Art. 68. Specific epithets are illegitimate in the following cases and must be rejected. -- (3) When they exactly repeat the generic name..." Examples, *Linaria Linaria* &c. - When such a combination as "*Closterium closterium*" must be rejected, why should we use similar combinations in varieties: *Closterium angustatum* var. *angustatum*? Indeed, if the very wise and learned men constituting the Committee of Taxonomy have resolved that the typical form of a species shall be named "*Clost. angustatum* v. *angustatum*", then we have to obey if we like it or not.

1.XII.-52.

I think now it is perhaps best to send this letter to you. There has been already too long delay since I began this. It is better that I continue in another letter.

Only one more question: do you think it would be possible that the person who has inked your plates could (perhaps with your aid?) make ready for me the plates for my paper on Dr Croasdales desmid-collections from N. Hampshire? Would it be very expensive? I think it would not be impossible for me to get USA dollars from the Bank of Finland for such a purpose. As I have told you, I cannot draw sufficiently smooth drawings with India ink and I have not got any person skilled enough here in Finland. And to print directly from pencil drawings - as Dr Borge made in some of his beautiful papers - would be very expensive. If there are any difficulties, you may let me know it quite frankly.

For this time I shall close my long letter with my best regards

Sincerely, yours

Dec 8 1952

Dear Dr. Grönblad,

Will you permit a small correction in the way you commence your letters? It is not customary to write "Dear Mr. A. M. Scott", but "Dear Mr. Scott", as if you were speaking to me face to face. I should like it still better if you would write "Dear Scott", or "Dear friend Scott", or "My dear friend", as Prescott and Telling do. For I regard you as a good friend, and I hope that you think of me in the same way.

You need never be afraid of offending me with your criticisms. On the contrary I welcome them. I make many errors through inexperience, and it is only if some friend will take the trouble to correct them that I can know where my mistakes exist and how to avoid them in future. Of course I had expected that you would have different opinions of some of the Australian desmids, but I am astonished and somewhat dismayed that you have found such a very long list of differences. However, I am not going to try to reply to your criticisms in this letter, but merely to answer your query as to the possibility of having Mrs. Perrine ink the plates for your paper on New Hampshire desmids.

This young lady is the daughter of some friends of ours, and we have known her since she was born, 28 years ago. She has always called me "Uncle Arthur", and my wife "Aunt Edith", though we are not related in any way. Both of us love her very much, for she is really a dear girl. Now she is married and has two small children, ages 2 and 5, who as you may imagine, take up a lot of her time. She had a little elementary biology in school, and graduated from college with the degree of B.A. in architecture. Then she worked for a year or so in an architect's office, and after she married an officer in the U.S. Navy she worked in one of the Navy offices, helping to draw maps or charts. She has done no other drawing work that I know of, except my desmid plates, and I think she does them principally because of her affection for me.

For the last couple of years she has lived in California, where her husband was stationed. He was released from the Navy a couple of months ago, and it happens that just now they are in New Orleans, staying with her parents who live just around the corner from us. She came to see us last night, and I showed her your letter. She was much flattered at the request coming from a far-off country like Finland, and the more so when I told her that you are one of the "big shots" in desmidology. She is willing to ink your drawings, but she had a suggestion that I think is very good. Before definitely undertaking the work she thinks you should send a few drawings, prepared exactly in the way you would draw them for the plates and on the same kind of paper; she will ink them and return them to you for criticism. If they are satisfactory she would then undertake the work.

Now, here are some practical details that must be considered. I have been paying her \$7.50 per plate, no matter whether there are 6 large *Micrasterias* or 26 small *Cosmaria* on the plate. I don't know what the rate of exchange is, but this is probably a lot of Finnish marks. However, it is less than I should have to pay any other draughtsman, and you must remember that a bricklayer in this country gets \$2.75 per hour.

Occasionally I have shown her desmids under my microscope so theoretically she knows what they are. But actually I don't believe she has much idea of what she is drawing, and all she can do is to follow exactly the pencil lines. If there is an imperfection in the line she will copy the imperfection, not knowing that it should be smoothed out. She has had no experience in drawing desmid chloroplasts, and this may be a little difficult for her. I shall not be able to help, because soon after Jan. 1st she will go to Memphis, 400 miles from New Orleans, where they will start housekeeping again.

Your drawings are made to a smaller scale than mine, and no doubt are less reduced in printing, which means she will have to use a thinner ink line than she does on my work.

So I would suggest that you send her, addressed to me, three or four typical drawings, say one Closterium with chloroplast, one large Cosmarium with chloroplast in one semicell and the ornamentation on the other semicell, and a large Micrasterias. Then I think you should draw another Cosmarium with chloroplast, in ink, using the width of line that you generally use for reproduction purposes.

My wife and I are leaving Dec 20th for a vacation trip through Florida, and shall return Jan 3rd or 4th. If you can get these drawings to me by airmail by that time I shall probably be able to discuss them with Dorothy before she leaves for Memphis. Otherwise I can talk to her in Memphis by telephone, but that would not be so satisfactory.

I have sent you a copy of the first ~~rough~~ rough draft of the North Australian paper, which will give you more information on some of the desmids. There were a few changes in nomenclature made in the final draft, and some minor changes in the descriptions. Also I am sending some samples containing Streptonema, from Indonesian collections, because the plant is very rare in the Australian material and you might have a hard time finding specimens. After you have examined it I should be much interested in learning what you think of my remarks concerning the resemblance between Streptonema trilobatum and Spondylium pulchrum in the Australian paper. I submitted these remarks to Prescott, who did not comment on them nor make any change, so I suppose he approves them more or less, though I doubt whether he has seen Streptonema.

Later I shall write and give you my views on some or the many desmids on which you disagree.

Sincerely yours,

Rolf Grönblåd

Keris


Centralgatan 86

Jan. 16th -53.

Mr A.M.Scott
2824 Dante Street
New Orleans 18, La.
USA.

Dear Friend,

Yesterday I was glad to receive the manuscript to Frw. Alg. Arnhem Id. The drawings (plates) I received already some time ago, and likewise the small vials with material from the Sunda Islands (coll.Sachlan). All very interesting to have for study.

There is especially one desmid, viz. *Streptonema trilobatum* which I am glad to have seen. And I am most interested in your statements in the text (mscr. p. 91-93). This is an excellent example to my old thesis that even the most exactly made drawings cannot give the same vivid idea of an alga as an original specimen itself. Quite as you say, the very first striking impression that one gets, is the great likeness to *Spondylosium pulchrum*. Really the first thoughts when I looked at this *Streptonema* were: "what is this like? Oh, indeed *Spondylosium pulchrum*". The whole appearance is quite different from what you would expect when you look at Turner's drawings. The apical connecting strings are quite mucilaginous and difficult to see (not always existing?), the lobes in end view are asymmetrical and very much of the same shape as in *Spond. pulch.* I think it may be quite possible that *Streptonema* is nothing but a 3-angulae "facies" of *Spondylosium*. It is a question to be inquired into, and a most interesting one. The material I got is not very good because the filaments are densely covered with numerous epiphytes. Have you seen the chlorophores? As far as I could judge there is one pyrenoid in each lobule (end view) and moreover a central one, thus:  To me it was impossible to get a clear and detailed front view drawing. (Did you note that there are living organisms (flagellates?) in the sample marked "Sachlan VIII"?)

What do you think of my proposals as to the *Arthrodesmus* of your USA material? Did you have any time to take a definite position? There are, indeed also other possibilities than what I have proposed.

When I see what amount of work you have been doing I feel ashamed of my slowness because I have not done more. I am afraid you think me a bad fellow-worker. If so, don't be angry, but frankly tell me your wishes. Perhaps you are quite dissatisfied with my determinations (identifications).

The labels on the Sachlan vials had dropped off (they do not fix on those ((plastic?)) vials). So I must beg you tell me which is which. I give some characteristic desmids from each of them:

- Sachlan VII → 1) *Netr. digitus*, *Pen. spiriotrioliforme*, *Staur. tauphorum*, *Ichtyocercus* &c., (athreadlike blue alga)
- Sachlan O → 2) *Streptonema*, *Euastr. turgidum*, *X. acanthophorum*, *Micr. alata*, *foliacea*, *M. mahabuleshw. var.*, *Pleur. nodosum var.*, *Cosm. obsoletum* &c., &c.
- 3) "Sachlan VIII" Sumatra. (many desmids.)

Many thanks again for the Month.Sc. which I have forwarded to the University Library.

I shall not write more this time, but I will quite soon come with a few commentations about Arnhem desmids. Am I to return your ms. shall I keep it?

It is a pity that we are at so long a distance from each other. It would be most useful to have a chat about our desmids.

Thank you also for the beautiful card from Florida. We have very bad weather, not so cold, some degrees above zero (Celsius), but the sun has been shining only two days this month. Always rain or snow or fog.

Kind regards,

yours sincerely,

P.S. I will go on with the new species or varieties among your material and send my proposals to you.

Rolf Grönblad
Karls
Centralgatan 86

23.1.-53.

Mr Arthur M.Scott.

Dear Friend,

As I promised there are some facts in your Arnhem paper, which I should wish to point out to you. Here they are:

81.109.121 p.80. (the pages are in your manuscript). You write Staur. pseudosebaldii, but there should be only one "i". Cf. Nordstedt, Index.

83.109.121.122.123 p.83. Staur. Sebaldii - should be: Sebaldi (from Sanctus Sebaldu)

3 lines ✓ p 51. Cosm.sp. C.Meneghini - should be Meneghini (from Meneghinus)

p.55. C.denticulatum and C. ovale have been confused, but the first one must have spines, the second one short broad teeth. If this limitation is kept, there will be no hesitation as ~~to~~ to the identification. This is also Dr Krieger's opinion.

61.105.117. p.62. Cosm. Regnesii - should be Regnesi (cf Nordst. Index)

p.70. Xanth. superbum. The Australian species is not quite identical to the Finlandian species described by Elfving (and myself 1938). Why not make a variety?

p.90. Why Gymnozyka pro Bambusina?

p.92. "In some works on Algae it is stated &c." - Where? It would be important to have references. Figures of new semicells growing should be most interesting.

I dont know if you approve of my remarks. If not, do take them easily.

With kind regards,
yours sincerely,

X *antipascum*
fa. *gavanice* Nordst

X *antipascum* fa. *major*
Nordst. . Normall alg.
ag. dulc Brasil page 26
fig. xylog. V.

New Orleans, La.
March 2 1953

Dr. Rolf Grönblad,
Karis, Finland.

My dear friend,

At last I am able to send you, herewith, the remainder of the *Arthrodesmus* drawings. I hope that the notes that I have placed on some of the drawings and the typewritten suggestions may be of some help to you in working up the genus. And as you requested, in some cases I have made slight changes in your phraseology to make it conform to the usual style in English.

After you have written the rough draft of the paper I suggest that you select the drawings that you consider most suitable for illustration, and number them consecutively, - 1, 2, 3, 4 etc., - also placing the definitive identification on each, which in some instances will be different from that now on the drawing. The numbering should correspond with the arrangement of the species, varieties, and formae in the paper. A brief typewritten list of the species, etc., in the order in which they appear, will help also. Then you should return to me only the drawings that you have selected, and I shall get busy on the plates.

I shall also require to know the size of the printed part of the page, which will be the size of the printed plate, and I shall arrange the plates for a reduction of 2x or 2.5x to the final size, depending on the size of the desmid.

If you would like me to read over your papers and make any small corrections that may be necessary in the style, I shall gladly do so. Your English is perfectly clear and understandable, but it shows, by various slight mannerisms, that the writer is more at home in another language. This is the case, I think, in all of the papers in English that I have seen by authors in continental Europe. The best of them, in my opinion, are Nygaard's. His Chief of Police evidently has a thorough command of English.

Now I am working through the second batch of your own drawings, principally from Florida, and making such notes and suggestions as I can. In some cases I cannot help, because the identification depends on a wide acquaintance with the European forms and European literature, of which I have little knowledge. But in other cases my familiarity with American forms may throw additional light on the matter. An example of this is *Pleurotaenium constrictum* vs. *P. burmense*, and I shall comment further on this when I return these drawings.

It has been a matter of great astonishment to me that I have had such extreme difficulty in finding additional specimens of some of the species of *Arthrodesmus* on which you requested more information. In the specific form of *A. maximus* I originally made 20 sketches from 3 collections, some of which were marked "scarce" or "rare" or even "very rare". These are not absolute terms, and depend upon my impressions at the time of examination. By "very rare" I usually mean that one specimen was seen in the search of 2, or 3, or perhaps 4 slides, which might contain from 1000 to 2000 other desmids if the collection is a rich one. In hunting for *A. maximus* I searched about 20 slides from the 3 collections, and did not succeed in finding a single specimen. It takes about half-an-hour to search one slide, so I figure that I spent the equivalent of a full day altogether, and I had similar experiences with other species. It is to be hoped that the other genera will not be as bad as this one; if they are I shall simply have to shorten the work, otherwise the task would become interminable.

Of course, it is too bad that I did not get all the information the first time, but then I did not realize the importance of the chloroplast and pyrenoids (in fact I am not too well convinced of it even now). And as you doubtless have experienced, it is not always possible to get all the information on the first encounter with a new desmid. Digitized by the Hunt Institute for Botanical Documentation, or it may prove impossible to balance it for the side and top views.

Although you attach so much importance to the chloroplast and pyrenoids, occasionally you may want to remove the cell contents. No doubt you are acquainted with Teiling's method of using sodium hypochlorite, NaClO, which I discovered independently several years ago. Now I have found what I think is an improvement, because I never had complete success with the hypochlorite alone. I use a commercial preparation named "Chlorox", sold as a household disinfectant and bleaching agent, which is a 5.5% aqueous solution of NaClO. To the water containing the desmids I add about 20% Chlorox and allow it to stand for a few hours, or over night, and then add hydrogen peroxide, the usual medicinal preparation of H₂O₂. Immediately there is a brisk reaction, with evolution of nascent ~~hydrogen~~ oxygen (and chlorine?). This usually destroys the chloroplast completely and leaves the cell completely empty, but if the destruction is not entirely accomplished, the operation may be repeated. There is no apparent damage to the cell-wall. I communicated this method to Mr. Paul Conger, the well-known diatomist of the Smithsonian Institution in Washington, and he says that he has found it very useful for cleaning delicate plankton diatoms, which are liable to damage by the usual treatment with strong acids. Also he has used it successfully in breaking up diatomaceous earth.

Friend Prescott is now in Ecuador, climbing around the Andes mountains, and looking for high altitude lakes. I hope it may be possible for him to visit me in New Orleans on his way back, but since he is on some kind of a Government assignment he has to travel by whatever means and routes that the Government provides. Going South he flew directly from New York to Panama, and thence to Quito.

Your friend Dr. Vittorio Tonolli sent me a list of the publications of the Instituto Italiano di Idrobiologia, but there is no mention of desmids in any of the works. Apparently in Italy, as in this country, the limnologists are much better acquainted with the microfauna than the microflora. In many of the limnological papers that I see, the fauna is identified to species and varieties, but the algae only to the genus. I wonder why that is.

A few years ago Hannah Crossdale sent me a dozen collections from Pudding Pond, New Hampshire, which I had requested her to get for me, so that I might try to find Hasting's *Staurastrum pulcherrimum*, to determine if it was identical with *Wolle's St. novae-caesarea*, as I believe to be the case. I was unable to find any specimens. If you do not have any of her collections from this habitat I should be glad to send you some; there is plenty available.

With my best regards,

Sincerely,

Penium rufescens var. *corrugatum* var. nov. (= *P. "Scottii"*). No doubt the curious and almost unique nature of the cell wall is what has caused you to think of the plant as a new species. But in my opinion this one differing feature cannot outweigh the many points of close resemblance to *P. rufescens*, which are: Approximately the same length, width, width of isthmus, shape of cell and shape of sinus, curvature of poles, character of the punctation, color of the wall, structure of chloroplast and number of pyrenoids. What closer correspondence could there be? So I believe it would be a mistake to name it as a new species, and do not think such a determination would hold.

In both front view and top view the corrugations do not show as lines on the surface, as you drew them, and it would be incorrect and misleading to illustrate the plant thus. Neither do the corrugations protrude beyond the smoothly rounded apical surface in front view; geometrical considerations show that the only way the corrugations could protrude beyond the apical margin would be if the central part of the apical surface were depressed. For an example of what I mean compare my figures of *St. sagittarium*, a rather remote but valid comparison. The corrugations are made visible solely by the differing refraction between the thicker and thinner portions of the wall, which causes the corrugations to appear as alternating lighter and darker bands or stripes. I attempted to show this by the device of using smaller and larger dots for the punctae or pores. This also is incorrect, because the pores are uniformly distributed as you state, and of uniform size. But for an ink drawing I think this is the most suitable way of representing the appearance of the cell, and it could not be misunderstood if attention were called to it in the text. If it were permissible to use a pencil drawing, the shading could be more accurately represented. This, however, would require half-tone plate, to which editors object on the score of expense for the more highly glazed paper.

Penium rufescens var. *corrugatum* var. nov. (= *P. "Scottii"*). No doubt the curious and almost unique nature of the cell wall is what has caused you to think of it as a new species. But there are no many points of close agreement with *P. rufescens*

Staurostrum grallatorum var. *forcipigerum* OR *St. saltans* var. *forcipigerum*.

Because of lack of accurate information on the original specific forms of *St. grallatorum* and *St. saltans* I am unable to take a definite stand on this question, but here is how I size up the situation.

I do not possess Nordstedt's original description and illustration of *St. grallatorum* (1869), and the earliest figure I have is the very poor one in *Symbologiae ad Floram Brasiliae centralis cognoscendam*, Part. V, 1867, Pl. 4, Fig. 52, with no text. Likewise, of *St. saltans* I have only Joshua's poor original illustration, and the figure a fronte in G.S. West "Algae", 1916.

Lagerheim's original figure (1885) of *St. grallatorum* var. *forcipigerum* shows a rather narrow body constricted above a basal swelling, very long arms that are unequally bifurcate at the extremities, 2 vertical spines and 2 granules on the somewhat elevated apex (4 granules in vertical view). His description reads "Var. *semicellulis supra basim saepe* ('usually') *constrictis*", so that forms without a constriction are not excluded. A year later he changed the name to *St. saltans* var. *forcipigerum*, for what reasons I do not know, since I do not have this second paper. Note that Nordstedt's figure of *St. grallatorum* (type) shows a campanulate body, unconstricted, that does not differ much from that of *St. saltans*.

St. saltans, the typical form of which is found only in the Indo-Malayan-Australasian region to the best of my knowledge, has a campanulate body, unconstricted above the base. On the markedly elevated apex there are no granules, but two opposed pairs of spines that are horizontally directed, or nearly so. The arms may be quite short or very long (in different varieties) and are unequally furcate at the ends.

Judging from your drawing #4621 you rely largely on the difference between the campanulate body and the constricted body to discriminate between *St. saltans* var. *forcipigerum* and *St. grallatorum* var. *forcipigerum*, and you refer to Cushman, in *Rhodora* 1905, which I am sorry I do not possess. Prescott once loaned me a couple of Cushman's papers, but I considered the illustrations so poor that I made no effort to obtain the papers for my library. In my series of drawings #2341-2348 you will see both the campanulate and the constricted bodies, and intergrading forms between them, so that I do not think this criterion can be used to discriminate between ~~them~~ the species, especially as Lagerheim's original description did not exclude unconstricted bodies, and actually leads you to believe that he had seen such forms. The plant occurs in many of my collections, and I could have drawn many more specimens, which perhaps would reveal even more evidence of intergradation in the body types.

The dichotomous specimen shown in your drawing #4621 you interpret to mean that the plant belongs to *St. saltans*. But G.M. Smith (1924, p. 101), says that he saw a preparation by Charles Bullard in which one semicell was typical *St. grallatorum* and the other was var. *forcipigerum*. He interpreted this to mean that the variety belongs to *St. grallatorum*. I gather from a recent letter from our friend Teiling that Bullard is still living, so there might be a faint possibility that the preparation is still preserved and could be studied.

I think it worth remarking that while *St. grallatorum* and var. *forcipigerum* have been reported from many places in North and South America, the specific form of *St. saltans* is known only from the Far East.

Your Brazilian plant, *St. saltans* var. *brasiliense*, seems to differ from the USA forms of var. *forcipigerum* only in the circle of small teeth just above the isthmus (which your text does not mention). The apical spines and verrucae are quite like some of my specimens, both in shape and number.

It seems to me that var. *forcipigerum* may be identical with var. *ungulatum* Wolle, (1864, Pl. 14, Figs. 17, 18), and if such identity could be established with certainty, Wolle's name would take precedence. His figure 18 in particular shows considerable resemblance to the forms of var. *forcipigerum* illustrated by Ireneé-Marie (1936, 1952), G.M. Smith (1924) and Taylor (1934), also to my drawing #2348. His figure 17 was transferred by Johnson to "*St. protractum*", in another paper that I do not possess, but I think the form may correspond with the long-armed forms in my drawings #2341, 2343, 2344, making allowance for the distortion of shape that is present in many of Wolle's drawings.

Borge's form from Cuba (1899), which he calls *St. saltans* fa., should also be referred to var. *forcipigerum*, I think.

There is no getting away from the fact that var. *forcipigerum* does show a strong resemblance to *St. saltans*, but I think that Bullard's dichotypical specimen is weighty evidence in favor of assigning it to *St. grallatorium*.

Among American authors who are familiar with the plant, Welle, G.M. Smith, Prescott, Taylor, Irene-Marie and myself consider the plant to belong to *St. grallatorium*. It is possible that I might have to change my opinion if I could see the 2nd paper by Lagerhain, Cushman's paper, and the one by Cedercreutz & Grönblad 1936. If any copies of the last named are available I should like to have one.

Attached are blueprint copies of four sketches of *St. saltans* from Borneo, Java, and Sumatra showing three different forms, none of which correspond exactly with the typical form as I understand it.

I have had some fun in concocting this dissertation; also it has been good exercise for my wits. I should be interested in your opinion of it.

June 28th 1952.

Rolf Grönblad
Karis
Centralgatan 86

Dear Mr Arthur M. Scott,

Many thanks for your letters and drawings which are very interesting. I have not written earlier because I am working on your desmids and I had a wish to tell you something really positive. The beginning has been very slow, indeed, but I hope we will be able to publish a first paper already within this year. I have started with your Nr 1, which is *Arthrodesmus*. There have been several difficulties as to the identification of your drawings. I have not only new species but also some new var. in this first contribution. I send you copies of a few descriptions so that you may be able to express your opinions and your wants. X)

I have not accepted "*Staurodesmus*" because the question seems not yet quite established. And I think this is also your opinion? There ^{are} also those "*Staurodesmus*" species which you sent to Telling for inspection. I have numbered them "A 1", A2, A3, &c. because I was afraid of confusing the numbers. (In your last letter ^{you} say that you did not find among your photocopies "several drawings of *S. calyxoides* Wolle". Yes, I can explain the mystery: they are among the drawings sent to Telling, and I have sent you no photocopies of them. Do you wish to have them? - *S. calyxoides* is published also in Wolle's *Freshw. alg. U.S.*, but Wollé's fig. is not quite identical to I.-M's. I send you a copy. - I have already received the new paper by I.-M. and I find there a lot of identifications which I would rather not approve of. Later on I shall tell you about them. (You must not think that I believe my opinion the only right one and all other opinions wrong. I am not so stupid. But I think all critic, if only sensible and correct, is useful.)

Some questions:

1) I think ^{the magnification of,} every figure should be given. This is for you to

do because it is impossible to me.

X) I send these descriptions together with your resp. drawings a few days later than this letter.

2). I should like to have several figures published of every new species or variety, not only one.

3). Do you wish the habitats of the new desmids given as numbers of your samples (I should prefer this), or do you wish the localities named after every description? (This will take more space).

4.) In describing new species, var. &c it is desired ^(see To Prof. Hermann Paulsen) that it would be mentioned where the original samples are kept. This has been pointed out to me. It has not been customary among desmidiologists.

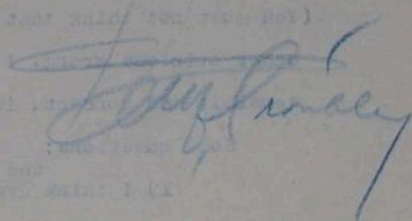
5) I send you with the same mail the rest of my drawings made from Fla material. I hope you will make critical notes on the sheets.

6). *Arthrodesmus bifidus* (your Fig. A35 - A42) has no vertical views. Such ones were very desirable. I could not find any specimens in your sample Ia 27. Could you try to find some? and make a drawing "vertice".

I must finish my letter because it is late and I am going to make an excursion to the thousands of rocky islands ^("skerries") on the shore of the south coast. I shall during 4 days have a motor boat to my disposition. It will be very interesting because there are millions of small rocky pools rich in desmids. Also our summer is at its best.

I think I have not yet told you that I have got a governmental grant for a journey to Italy in September during one month. There is nothing at all published on the desmids of Italy since the time of Delponte, more than 70 years ago. Do not be afraid of my going to neglect our joint publications: no, they remain always my first and chief object.

With kind regards,
yours sincerely,



New Orleans 19, La.
Jan 31 1953

Dear friend Grönblad,

I now have three of your letters to answer, but just now I am going to reply to the last two, namely those of Jan 16th and 23rd, leaving your long letter of Nov 7th until later, because that will require a lot of checking and investigation before I can attempt to answer your many criticisms.

But first let me apologize for not having acknowledged sooner the reprints of your two papers on Desmids from Gotland and Öland, and Desmids from West Greenland. The latter especially is very interesting and valuable; I am somewhat surprised that you were able to find as many as 9% desmids in sample No. 10 from Menyanthes lake. I note that only 2 species of *Micrasterias* occurred, and this agrees with what Prescott wrote me recently concerning his work at Point Barrow, Alaska, that *Micrasterias* is practically absent from the Arctic slope of Northern Alaska. Prescott, by the way, is now in Quito, Ecuador, apparently on some kind of Government mission. I had hoped that he would be able to visit New Orleans on his way there, but he flew directly from New York to Panama in an Army plane.

Now for your letter of Jan. 16th. I am quite glad to see that you are inclined to agree with my remarks on *Streptonema*, and after you have examined the better and cleaner material in two more tubes that I have sent you, perhaps you will be able to offer some further comments. I have not been able to get an exact idea of the chloroplasts in *Streptonema*. It is very difficult to make out the structure in side view, and the very few detached cells that I have seen have all been empty. There seems to be one pyrenoid in each of the lobules as you say, but the central one that you sketch must be the nucleus, I should think. No dividing cells nor stages in the growth were seen in the Australian material, in which I found only three filaments. If I had seen these phenomena perhaps I would have illustrated them, but the plates were drawn long before the paper was written, and before I had even thought of venturing my comments.

I had noticed that the gummed labels did not adhere properly to the plastic vials, so I used a much stronger liquid glue made for cabinet work, furniture, etc., and am surprised that they dropped off again. This glue will adhere to metal, glass, and other smooth surfaces. I wonder why it does not adhere to plastic. On the three new tubes that I sent a few days ago I used a plastic cement, whose solvent also dissolves the material of which the vial is made, so these ought to stick.

Here are the identifications of the three vials which you have already received:

from Lebak Danau (Lake Lebak)

Your No. 1 = Sachlan VII, collected near Palembang, Sumatra, along with Sachlan VIII. This tube contains *Netr. digitus*, *St. tauphorum*, *Ichthyocercus*, and a beautiful new variety of *M. filicea* with two prominent hollow swellings, each bearing two spines, on each semicell, between the base of the polar lobe and the inner end of the sinus.

Your No. 2 = Sachlan "Q", from a pond at the laboratory in Bogor (Buitenzorg) Java. Contains *Streptonema*, *E. turgidum*, *Pl. nodosum* var., etc.

Your No. 3 = Sachlan VIII, from ~~Lebak Danau (Lake Lebak)~~ ~~near Palembang, Sumatra~~ ~~along with Sachlan VIII~~ Danau Teloko (Lake Teloko) near Palembang. This material is in poor condition, but contains some rare desmids, such as *St. zonatum* var. *ceylanica*, *X. superbum*, *Cosm. cuneatum*, *Cosm. suberosum* Schm., *M. ceylanica* ad var. *Wallichiana*, *M. subincisa* var. *mandibulum*, etc., etc.

The three new vials that I have sent are as follows:

Sachlan "K") Both of these were collected, in different years, from Tembaga Swamp,
Sachlan "M") near Djakarta (Batavia) Java. They contain some nice specimens of
Streptonema, without the usual epiphytes, and if you are lucky you may
find some detached cells.

Sachlan 270. from Lake Empangau, West Borneo. This does not contain *Streptonema*, but has many other beautiful desmids that you have probably never seen, such as *M. anomala* in two different forms, *M. serotophora*, *M. apiculata* var. *lacerata*, *Cosm. magnificum* var., *C. Freemanii*(?), etc., etc.

In Sachlan "N" note *Euastrum coralloides* var. *trigibberum* Lagerh., to which I refer in the Arnhem Land paper. This also occurs, rarely, in Sachlan 270.

In Sachlan "N" note *Euastrum dubium* var. *incisum* fa. *asymmetricum* Scott & Presc., from Arnhem Land. This is the asymmetrical form of the variety which you think should not be referred to *E. dubium*. I agree with you on this, and I had quite an argument with Prescott about it, because I wanted to make it a new species, as you suggest. But he pointed out Krieger's illustration of *E. dubium* var. *tropicum* (W. & W.) Krieg., in his Monograph Pl. 79, Figs. 28, 29, which does show a considerable resemblance to the Australian plant. But Krieger's transfer of West & West's plant from *E. lobulatum* to *E. dubium* is not necessarily correct. And obviously our Australian plant cannot possibly belong to the same species as *E. dubium* var. *poriferum* Presc. & Scott, (1945, Pl. 2, Fig. 6). I regard the latter as another mistake of Prescott's. It seems to me that *E. dubium* is a "catchall" species to which several dubious forms have been referred.

You ~~may~~ may keep the manuscript, which is a copy of the second draft. In the third and final draft there were a few minor changes made, but I could not say just what they were without a lot of checking. The finished paper has been in the hands of the editor in Australia for about two months, and it is too late to make any changes in the text, and in any event I could not make changes without consulting and arguing with Prescott. Perhaps you have gathered from my letters that P. is sometimes quite obstinate and clings firmly to his opinions (e.g., *Spinoclosterium curvatum* vs. *Closterium cuspidatum*).

You suggest that perhaps I may think you are a "bad fellow-worker", and that I may be dissatisfied with your determinations. I would like to assure you in the most forceful terms that I can command, that if you have any such impression you are completely and absolutely wrong. On the contrary I am highly satisfied with our collaboration, more so in fact than with my collaboration with Prescott. I think your determinations are arrived at after more careful consideration and more thorough checking than has been the case, in some instances, with Prescott. As to the amount of work that you have done, I realize very well that you have your own affairs and research to attend to, and that you cannot put everything else aside to work on my stuff. As a matter of fact I myself could probably have done more on the drawings that you returned, though I have not been idle by any ~~xxx~~ means. Already I have spent considerable time trying to get the additional information that you asked for. In some cases I have been successful; in others still more searching will be required. Under separate cover I am returning my drawings A35 - A42 of *Arthrodesmus bifidus* and var. nov., with a new drawing giving front, side and top views that are as nearly exact as I can see them and draw them. An explanatory note is attached to the drawings. I have also done some work on the other *Arthrodesmus* drawings, and shall now proceed to finish them and return them to you. In most instances I agree with your determinations; in others I have differing ideas to offer, which you can consider and assess for what they are worth.

Another note on our collaboration. I should have liked very much to work with you on the Indonesian collections, but Sachlan sent the material to Prescott and me jointly, and I have an understanding with Prescott that he and I are to work on it together. However, I have lately made a connection with Dr. Harald Sioli, a limnologist of Belém, Pará, Brazil, who has promised to send me some collections from that region. If his promises materialize, I shall be delighted to work with you on them. Sioli has visited many parts of the Amazon river and its tributaries, and is familiar with the Fazenda Taperinha where Dr. Ginsberger got your Brazilian collections, though the farm has changed hands since 1927.

according to modern rules, but I suppose the original spelling must be maintained. Accordingly I shall write to my friend in Australia and try to have him make the necessary changes in the MS before it goes to press.

- p.55. I agree with your and Dr. Krieger's differentiation between *C. denticulatum* and *C. ovale*. I suppose that Krieger will try to assign some of the intergrading forms in his new *Lieferung* on *Cosmarium*, which he writes me is nearly finished. It will be highly interesting to see what he has done with this extremely difficult genus.
- p.70. *X. superbum*. I do not possess Elfving's original illustration. Between my drawing and yours of 1938, about the only difference I can see is the spines seem to be shorter and stouter in the Australian plant, hardly enough to make a new variety, I should think. Prescott said nothing about the possibility of it being a new variety.
- p.90. I am aware that *Bambusina* is a nomen conservandum against *Gymnozyga*, and I wrote *Bambusina* on my drawing, but Prescott changed it to *Gymnozyga*. When I remonstrated with him he replied something like this - "If we were writing a monograph it would be necessary to use the name *Bambusina*, but in a paper describing only a small number of collections from a limited territory, I prefer to use the name *Gymnozyga*". Not quite clear, is it?
- p.92. "In some works on Algae it has been stated" ... etc. Reference: G. M. Smith, *Freshwater Algae of the U.S.*, 1st edition 1933, p. 590; 2nd. edition 1950 p. 334. "Cell division in *Desmidiium*, as well as in *Gymnozyga* and *Streptonema*, is different from that of other *Desmidiaceae*. In these genera the developing semicells have an infolding of their walls (Fig. 415A in 1st edit., Fig. 246A in 2nd edition), quite similar in appearance to the replications found in the end walls of certain *Spirogyra* species. As the young semicells increase in size the infoldings become everted and they disappear completely by the time the semicells are mature. Species with flattened semicell apices develop but one replication; those with mamillate apices develop as many replications as there are protuberances". In a footnote Smith refers to Nellie Carter 1923, which I do not possess. I have also seen a similar statement in some other work, but I cannot recall just now where it was.

As I wrote on page 1 of this letter, no developing semicells of *Streptonema* were seen in the Australian material, and my remarks were based on some examples seen in Sachlan's Indonesian collections, which I expect to illustrate in a future paper. I think you will be able to see some specimens in the various stages of development in the new vials that are now on the way to you, by ordinary mail.

Our trip to Florida at Christmas and New Year was very enjoyable as a vacation, but collecting was not good, because of a prolonged drought which had partially or even completely dried up many of my favorite collecting places. Nevertheless, I brought back 16 jars of material collected in places that I had not visited before. Many of these were rich in desmids, but my examination revealed nothing that is not contained in my former gatherings from this State. So I have discarded all of them except one, which contains a very few isolated and empty cells and semicells of the extremely rare *Desmidiium curvatum*, of which I have previously seen exactly two semicells in all of the hundreds of gatherings that I have made in southeastern USA.

Enclosed is a photomicrograph that I made in 1950 of *Cosm. cosmetum*, one picture showing the profile of the cell, and the other showing the wall ornament. In the latter picture, the dark bands connecting the granules and separating the triangular pits are (I believe) internal ribs or thickenings of the wall. They are colored brown and are best seen in old and well-developed specimens.

Also enclosed are some films showing some of my first attempts at color photographs of living desmids from a Florida collection. They show *M. Torreyi*, *Desmidiium digitus* and *Cl. libellula*. The pictures are not particularly good, because I was

a blue filter of too deep a color, and also because my exposure times were largely guesswork. I hope to do better with the next lot, when I get another batch of living desmids. The pictures of *C. cosmetum* were made with a 9x12 cm Japanese microscope camera attached to the ocular tube of the microscope, while the colored ones were made with a German Praktica 35 mm camera (similar to the Ixakta) with a focal-plane shutter and ground glass focussing, also attached to the ocular tube of the microscope, with extension tubes. The colored photos should be projected on a screen to see them properly, but you can get a fairly good idea of them with a 5x or 6x ~~linox~~ magnifier. Perhaps Dr. Krieger would like to see them after you have examined them.

I think this long letter is enough for you to digest at one time, so I will close now, with my best regards.

Sincerely,

Mch 9 1953

Dear friend Grönblad,

No doubt you have been wondering why I have not written you for the last two or three weeks. The reason is that I had changed, temporarily, from a desmidiologist into a carpenter. I have been enlarging my microscope room on the ground floor of my house, and now it is twice as large as before. But all my muscles are aching from the unaccustomed work of drilling holes in the concrete floor for anchor bolts, sawing wood, hammering nails, and handling large sheets of wall board. Now it is finished except for painting, which can wait until I have rested a little.

By this time I hope that you have received the revised drawings of Scottia, the copy of Sioli's map, and the boxes of Brazilian collections. In the meantime I have your two letters of Feb 13th and 27th, and am glad that the typescript is now in the editor's hands. I feel sure that it will be accepted, and hope that there will not be too much delay in printing it.

I quite agree with you that the ink drawings do not look as nice as pencil, though they improve when they are reduced and the lines become thinner. There cannot be much difference in cost of 'half-tone' printing plates as compared with 'line' plates; I think the editors object to pencil work because they have to use a different and better quality of paper for half tones. I wish it were possible to use pencil drawings like those in some of Borge's Brazilian papers.

Dr. Kallio has written me a nice letter, and was also kind enough to send me four tubes (2 were broken) containing living specimens of the triradiate clone of M. Thomasi and haploid and diploid specimens of the biradiate var. notata. I am very glad to have seen them, though I don't know enough about cytology to know the exact meaning or significance of 'haploid' and 'diploid'. Perhaps I shall learn something about them after I have studied his papers more thoroughly. He has asked me to send him some living material of the Micrasterias species that occur here, and a little later I shall comply with his request. He writes that Miss Crossdale has sent him some specimens of M. truncata, and I have asked her to tell me the details of her procedure.

Though I am not working actively on the Brazilian material, I look at it from time to time, and have found many strange desmids. The new Micrasterias, M. Ledouxii is smaller than any heretofore known, but in Brazil No. 8 you will find many specimens of an even smaller one, shown on the enclosed sketch. It occurs in two distinct sizes and slightly differing shapes; I take it to be a new variety of M. subincisa Krieger.

In Brazil No. 14 there occurs, rrr, another Micrasterias somewhat resembling M. triangularis Wille, of which it may, perhaps, be regarded as a variety. It is somewhat variable in size and shape, and further has the peculiarity that some, but not all, of the specimens have the lateral extensions of the polar lobe divided into two unequal lobules, as in M. Mosbyi. Some individuals have one polar lobe divided and the other entire. The enclosed sketches show some of the forms.

Another strange plant occurs in Brazil No. 20, fairly frequently.

And on the truncate ends there are faint indications of two (or more?) extremely small teeth, just barely visible. I have seen it also in another of the Brazilian collections, but I don't remember which one, and in this sample the teeth are slightly larger, but it will require the 90x immersion lens to see them clearly. I have not yet tried to draw it.

The new Xanthidium with 8 (not 4) pyrenoids per semicell occurs in the Scottia sample, as you noticed, and also in several others where it is more plentiful. I have not yet been able to make out the chloroplast arrangement

From one of my friends in Czechoslovakia, probably Ruzicka, I received last week a new book which you ought to try to get, if you don't already know about it. It is entitled "Flora Plantarum Cryptogamarum URSS" and is evidently the first of a series that will deal with the Conjugatae of the entire USSR. The present volume covers Mesotaeniales and Gonatozoales. It is written entirely in Russian, of which I have not the faintest knowledge, but the illustrations are good, and there are several species and varieties that are not included in Krieger's monograph. The author is C.C. Kossinskaja, and the book is published by the Akademii Nauk, SSSR, Birgevaia Linia 1, Leningrad 164.

There is no more news about my trip this summer. I am still awaiting my steamer reservations, but probably will not get them definitely until about the middle of April. However, I have been assured verbally that I can get passage on a freight steamer from New Orleans to England sometime in June, probably in the first half of that month.

That's all for the present. With my best regards,

Sincerely your friend,

Easter Sunday, Apl 5 1953
10.30 pm.

Dear friend Grönblad,

The 3-day Easter holiday enabled me to finish up the examination of the remainder of your drawings and mine. They are returned herewith, together with such notes and comments as I can make. There are some groups that are quite obscure to me, such as *St. dejectum* and *St. megastrobilum*, so I do not feel competent to attempt a separation of the many intergrading forms.

In a few cases I have written fairly lengthy discussions, setting forth my opinions and the reasons for them. If we could only sit down together and talk things over, I am sure we could elucidate many of these problems, but I fear there is little chance of that happening. Please understand that in these discussions I am not trying to change your opinions; if my arguments appeal to you, very good; if not please feel entirely free to make whatever assignment and use whatever nomenclature you think is best.

Now I shall resume work on Sachlan's Indonesian collections, which will require several months more before I finish the examination and drawing of specimens. I expect he is getting anxious about them, since it is more than two years since he sent the material. But it takes a long time, because there are so many strange and curious forms, many of which are very complicated and difficult to draw.

A new book appeared recently, "Algae of Illinois", by Tiffany and Britton. I bought a copy for \$10.00. There is a comparatively small section on desmids, most of which seem to be hard-water forms, but the illustrations are so poor, even quite inaccurate, that it is useless to me, and would be to you also. There are few original drawings of desmids; almost all of them are copied from Krieger, G.M. Smith, Taylor, Irene-Marie, and even Wolle! A large part of the book is devoted to filamentous algae such as *Oedogonium*, on which Tiffany has specialized. This part is probably better, so I have sent the book to friend Telling for his collection. If you would like to see it, ask him to lend it to you.

That's all for the present. I'm going upstairs to bed now.

Sincerely,

Apl 12 1953

Dear friend Grönblad,

Many thanks for sending me the photoprint of the plate from Elfving's 1881 paper, and the description of *Xanthidium superbum*. Now I can see that the Australian specimen is sufficiently different that it could well have been described as a new variety. I wonder why Prescott did not notice this when he checked my identifications; there is little doubt that he has the original illustration in his iconotheca.

Elfving's drawing of *Euastrum* does not show the ^{two} very large and deep pits, asymmetrically placed, which are such a prominent feature of this plant. When were they discovered?, or is there another form without the pits?

A week ago I sent you a registered package containing the last of your drawings, together with some additional ones of my own, and my notes on many of the plants. Yesterday I received the return receipt for the first registered package that I sent you.

Now I am back at work on Sachlan's Indonesian material, which I had to put aside for nearly a year. But I can stop on this, and start on the plates for our *Arthrodesmus* paper whenever you return the drawings to me.

In Sachlan's collections I have run across some truly extraordinary examples of dichotomy between *St. Wildemanii* and no less than three other forms. One of them is easily identified as *St. subtrifurcatum* var. *major* W&W; another has two spines at each angle in a horizontal plane, shown on my drawings 5 and 37; the third form has a single spine at each angle, in a horizontal plane, shown on my drawing 63. These are not isolated cases; I have many other drawings showing these combinations.

I intend to write up these plants in a separate paper, but before doing so I must ascertain if the two forms which I propose to name *St. Wildemanii* var. *horizontale*, and *St. Wildemanii* var. *monospinum* (perhaps "unispinum" is better), have been described by anybody else. In the literature available to me the only thing I can find is *St. bifidum* var. *hexagonum* Schaarschm., of which the top view is quite like my proposed var. *horizontale*, but the front view is considerably different.

May I ask for your assistance in trying to find previous records of these two forms? Prescott is still in Ecuador and I do not know when he will return; but in any event I would rather have your opinion than his. I should be very grateful if you would look over the enclosed drawings, and then return them to me with any notes that you care to make.

Sincerely your friend,

March 16. 1953.

Rolf Grönblad

Karis
Centralgatan 86

Dear Friend Scott,

Thank you very much for all your different sendings which I have received since my last letter. This letter has been lying on my table for several weeks, but I have never quite finished it. But now I think it best to send it away in order to let you know that nothing is amiss with your letters drawings and slides. There are some questions which I should wish to discuss furthermore, but these must be left for another letter.

- 1) Vials containing algae from Borneo, Java, Australia, Florida received. Very interesting Desmids. I am glad to have seen them.
- 2). You have among your drawings a Docidium baculum (Fla 107) with "apical warts" according to your statement. I have tried to find such cells (also by means of staining with Gentiana violet) but I have not succeeded. Do you think it possible that there could have been an optical error?
- 3) Do you know (or do you have) Frohne, 1942, Notes on Phymatodocis. - Trans. Am. Micr. Soc. 61. What is it about?
- 4) Now I can tell you that I have found a young student who will make for me india-ink-drawings. So I need not trouble you any more with that. She has been drawing maps at the Geographical Institute and she is herself a botanical student, so I think she will be able to help me.
- 5) I shall try to find some material of Xanthidium superbum from Finland and send it to you for comparison with your Australian desmid.
- 6) I have written to Dr Krieger and asked if he would like to have more of your desmid-material from Fla.-(as you remember, I have some duplicate samples) but I have got no answer. I think there are "hard times" in the Soviet-Zone for all scientists. I shall once more try to write to him but you can never tell it be good for him? *Do you know anything about him?*
- 7) Spondylosium puchrum zygospore. You have among your drawings a scrobiculate zyg. of this species and you suppose that my figures in Desm. Brasil. are made from unripe zyg. a. You are quite right. I have examined my Brasil. material

2)

once more and I have found that older zyg. have a very finely scrobiculate cell wall. Your figures are quite correct and the zyg. of Sp. pulchrum must be described as scrobiculate, not smooth.

8) Streptonema in "Sachlan M" I think I had a rather good view of some cells from which it appeared that there are really one pyrenoid in each lobule and one central pyrenoid too. (it can be seen very clearly also in side view - not always, but in such cells where the chloroplasts are not much distorted). Now I see in your ^{last} letter that you also have seen this.

9). I think your opinion is very good in suggesting that very many greatly different varieties should not be united in such "catchall" species as e.g. ~~Xanthid.~~ Arthrodesmus. incus. &c.

10). I thought I had already sent to you a copy of Elfving's plate. (perhaps it was to prof. Prescott?). I shall try to find some material from Finland and send to you, if possible.

11.) You have mentioned a paper by Nellie Carter of "1923". in connexion with our discussion about Streptonema. I cannot find such a paper in Krieger's bibliography nor in Prescott & Brunel's desm. bibl. manuscr. Turner's fig. are certainly very bad. Cell-divisions I have not found.

12). Your photomicrographs are very interesting. I have sometimes thought it would be useful to have a photo showing every "new" Desmid. This would exclude every suspicion as to the correctness of "new" species. Unfortunately I have no apparatus for taking microphotos and they are very expensive. (Nevertheless I think I will sometimes try to get such one for myself).

13) Arthr. bifidus var. -(A35, -A40) I think we can make it a variety of A. bifidus. This is one of those inconvenient Desmids that do not fit quite well in any genus. The paired spines point towards Xanthidium, but the undifferentiated central area points to Arthrodesmus. (Such is also Arth. impar). It seems to me quite useless to argue about the question because it is only a matter of taste in which genus such desmids are put. I hope you agree?

14) You ask me why "Incus" is capitalized. It has been an old custom to capital-

Nordstedt's "Index". But you are quite right: this custom does not agree with modern Botanical Rules. I have talked over this matter with some professional botanists and we have arrived at this conclusion: the Rules must be maintained and the old custom deleted. Only names derived from names of persons may be written with capital letters. Thus: *Micrasterias Borgei* &c, but *Arthro. incus*, *Closterium libellula*, *Penium cylindrus*, *Closterium lunula*, *Staurastr. ophiura*, *St. oxyacantha*, *St. rotula*, *St. avicula*, &c. There is only one danger: botanists who do not know the origin of such names may perhaps think the endings are not correct and may perhaps try to change them in this way: *Closterium libellulum*, *Staurastr. ophiurum*, *Staur. aviculum*, *St. oxyacanthum*, which would be quite wrong. As a matter of fact I have seen *Staur. oxyacanthum*! (I believe it is Krieger who has misunderstood this name.) Many American authors always use small letters, but to me it seems not very beautiful to write *Staur. borgeanum*. This matter is, however, not of first rate importance, but it would be good if all authors would use the same manner of writing.

14)

~~14)~~ As to *Arthro. incus* var. *validus* I do not think the American variety is really found in Europe. I have never seen it here. We may very well call it *A. validus* (W. & W.) Scott & Gbl.

15). I have got from my bookstore Prescott's large work "Algae of the Western Great Lakes Area". As far as I can judge from a first sight it seems very useful and correct. There are unfortunately no Desmids. I had also thought of buying Tiffany & Britton's new book, but as you tell me it is rather useless I think it is not worth while my buying.

Now I shall finish my letter for this time, but another will follow very soon.

With kind regards,
yours sincerely,

ROLF GRÖNBLAD
KARIS
FINLAND

April 23th 53

Dear Friend Sears,

I wish to let you know that your numerous
sendings, letters, preparations, samples &c, have
arrived safely. I am preparing a long letter
for you with answers to your questions &c.
To day I received your drawings together with
my own sketches. All your remarks are
very useful and I think in most cases I will

It would be possible to bring my opinion in agreement
with yours. I shall now put aside all other
business for a time and try to write something
that we may publish.

With kind regards
yours

J. M. Harvey

May 2 1953

Dear friend Grönblad,

The arrival of one of your long letters with critical notes is always an occasion of rejoicing for me. The letters are read and re-read many times, and each of your comments is studied with the keenest interest. Here are replies to some of the comments in your letter of March (April) 16th.

Dicidium basulum. The notes on my drawing #185 of specimens from Fla. 107 show that I saw (or thought I saw) something on the apical margin which aroused my curiosity sufficiently to make me examine the specimens with three different objectives, 45x, 65x and the 90x oil immersion. The latter I resort to only on rare occasions, when I think I see markings that I cannot resolve with the lower-powered dry objectives. The 1948 observation were made with achromatic lenses. I have just examined some more specimens with the apochromatic lenses on my present microscope, and I have to report that I cannot confirm my previous observation. In other words, my 1948 drawing is probably erroneous and certainly doubtful. I suggest that you pay no attention to it.

Xanthidium superbum. Elfvings drawing shows two small granules between the bases of the long spines. In all the specimens that I have seen (not very many) the granules are attached to, and form part of, the large verruca from which each spine arises. I am enclosing a sketch of a specimen that I found in Sachlan VIII, Palembang, Sumatra. *X. sansibarense* Hieronymus seems to be closely related to *X. superbum*; it is of a similar size, and very similar intramarginal series of verrucae, but only two pairs of apical spines, and either single or paired lateral spines.

I am glad to note that you have found somebody who can make your ink drawings for you. This will be much more satisfactory and less expensive than sending them over here to be copied.

Enclosed is the last letter that I received from Dr. Krieger, dated Jan 13th 1953, in which he mentions the difficulty he has had in obtaining the CARE parcels that I have been sending him. CARE parcels are of different kinds, and the particular one that he refers to contained coffee, which apparently cannot be delivered in Eastern Germany. As I understand it, he had to go in person to the CARE depot in West Berlin, where the parcel was opened and made up into smaller packages, which then had to be carried across the border by any means available, even by smugglers. I wrote him on Feb. 7th that in order to avoid this troublesome and risky business, I was sending him a different kind of CARE parcel, which was supposed to be sent by registered post from West Berlin to his address. In the same letter I asked him to send me some small quantities of the African material that he mentions, if he could spare it. I have not heard from him since, though he is usually very prompt in acknowledging the receipt of the parcels, nor have I received the receipt which CARE guarantees. Accordingly I wrote to CARE in New York a couple of weeks ago, and have just received a reply stating that they have had great difficulty with the delivery of parcels to the Soviet zone; that they will accept no more orders for the Soviet zone until further notice; and that existing orders are subject to an indefinite delay. There is evidently some good (or bad) reason why he has not answered your letter nor mine. If you hear from him I hope you will let me know at once, and I shall advise you if I get any news from him.

It is quite gratifying that you were able to confirm my observation of scrobiculate zygospores of *Spondylosium pulchrum*. Also it is rather surprising that you were able to go back to your Brazilian material and find more specimens. The plant occurs in a number of my collections, but in 15 years I have seen only two zygospores, both of which are recorded in my Herbarium.

My reference to a paper by Nellie Carter "1923" is copied from a footnote on page 590 of the 1st edition of G.M. Smith's FW AL. U.S., (p.223 in 2nd edition). From the bibliography in Smith's book it turns ~~out~~ out that "Nellie Carter 1923" is the 5th volume of W&W's Mon. Brit. Desm., which was written by Carter. I have the first four volumes of this work complete, but of the fifth volume I have only photoprints of the plates, without any text, so that I cannot tell what she wrote about cell-division in Desmidiu or Streptonema.

In my letter of Jan 31st I mentioned a Japanese microscope camera that I own, and you can see the camera, attached to a microscope, in one of the large pictures of my workshop that I sent you some years ago. I do not use this camera any more, and I should be glad to present it to you if you will permit me to do so. Please let me know if I may send it. It takes 6x9 cm plates, or flat films, or film-packs (not 9x12 cm as I wrote before).

It is very kind of you to give such a full explanation in reply to my question as to the capitalizing "Incus". The explanation is quite new to me; I had not given any thought to the fact that names like incus, libellula, cylindrus, etc., are nouns and not adjectives, and in fact I hardly know enough about the Latin language to tell the difference. A year or so ago I bought an elementary book on Latin, hoping to obtain from it some insight into the rules of Latin grammar, but after looking it over I was so dismayed at the enormous complexity of the language (far worse than German, which is bad enough), that I have abandoned any thought of studying it seriously.

I have read somewhere that there is a movement on foot among botanists to change the Rule requiring capitalization of specific names derived from personal names, as the zoologists have done. This would simplify matters considerably, though I think as you do, that it is more graceful to write Borgeanum, Groenbladii, etc. The same Rule (or rather Recommendation) requires the capitalization of names derived from other genera. I don't see any good reason for this, and should like to see it changed.

The name Staurastrum oxyacanthum was used by W&W in their Monograph, I believe. It is spelled thus in my typewritten list of the plate descriptions for Vol. 5.

I have seen Frohne's 1942 paper on Phymatodocis, but do not have it now. It records the finding of the specific form ~~new~~ of Ph. Nordstedtiana and fa. minor, from southeastern USA, South Carolina or Georgia I believe. He had some good illustrations of the plant, including cell-division and conjugation between adjoining unbroken filaments. The paper was of interest at the time because it was, I think, the ~~first~~^{first} published record of the plant in N. America since Wille. Prescott and I recorded fa. minor in our first paper, January 1942. I exchanged a couple of letters with Frohne, who was investigating the breeding places of malaria-carrying mosquitoes, and made his algal collections while engaged in this work. He wrote me that he had quite a number of unidentified desmids, and I suggested that he send the material to Prescott, but I don't think he ever did so. Some of his collections were from the famous Okefinokee Swamp in northern Florida and southern Georgia, which has such very acid water (pH 4.0 or less) that the desmids ought to be very interesting. On several occasions I have attempted to get into this swamp, but it is difficult of access by very poor roads that turn into quagmires after a rain, and it seems to rain more frequently in that region than anywhere else that I know of. To reach the interior of the swamp requires a boat trip of 15 to 20 miles, and more time than I have had at my disposal.

With my best regards and good wishes,

Your friend

Rolf Grönblad

Karis.

Centralgatan 80

4.V.-53.

Dear Friend,

To day I got a parcel with your and my drawings (dated March 18) ((Closterium, Penium, Cosmarium &c.)) I am very glad to see your critical notes, they will be very useful to me. You have made a great deal of work. Most of the differencies in our views are not very important, and I think we can agree on an acceptable solution. In most cases there is no such man who can decide "cocksurely" : this is certainly right, that is quite wrong ((I am sorry that you didn't like "Penium Scottii" = P.rufescens v.corrugatum nov.var. Because, as you say, the wall structure is "peculiar and unique" Lütkemüller does not include ^{Pen. rufescens} ~~this spec.~~ in Penium, but in Cosmarium. (I had quite forgotten this!). I think we have to use Lütkemüller's taxonomy, because he is the ^{one} only desmidiologist who has seriously tried to clear up the genus Penium, which has been wholly artificial. - In a letter to prof.G.S. West (dated Baden 12.II. 1912) Lütk. writes: "...Penium rufescens Cleve, welches ich im vergangenen Sommer zahlreich fand, zeigte ohne Färbung äusserst deutlich sehr dicht stehende Poren und gehört somit auch zu Cosmarium!". I think also G.S.West in his later days agreed with Lütk. in his ideas concerning the taxonomy of "Penium". (see West-Carter, Monogr.V, pag.258). In his manuscript to a Desmid flora of Europe (in Pascher's Flora, vol.VIII, which remained unpublished) Lütkem. gives the following definition of Penium:

" Penium Bréb, em.Lütkem. "Vegetative Zellen einzeln, cylindrisch mit kreisrundem querschnitt, Zellmembran längsgestréift, gekörnt oder glatt. Chloroplasten 2, axil, mit ganzrandigen oder in Läppchen geteilten Längsleisten, mitten bisweilen querdurchschnitten, Pyrenoide 1 oder mehrere, längs der Achse angeordnet. Zellkern zwischen den Chloroplasten eingeschoben". There are only the following species ^{are} ~~referred~~ in this genus, viz. : Pen. spirostriolatum, P.margaritaceum, P.polymorphum, P.phymatosporum, P.cylindrus, P.exiguum, P.dymocarpum, P.incurvum (nov.spec.Lütk., unpublished!).

The most important character in this diagnosis is "Zellmembran längsge-
streift". (cell-wall with longitudinal striolations). These "striolations" may
sometimes be very delicate and only seen after staining with gentiana violet.
The "section" (by L. called "Tribus") Penicae Lütke is described thus: "Zell-
membran aus 2 Schichten bestehend, in Segmente gegliedert. Stelle der Zelltei-
lung praeformiert, wechselnd. Ergänzungswachstum vorhanden, atypisch". These
distinctive features concern ~~also~~ of course all species of the genus Penium.

From all this you also see why I have not referred the drawings of ^{the} 3
desmids to Penium, which you have asked about (Cosm. floridanum &c). If I should
try to summon up the distinctive characters of Penium, it would be something
like this: Pen. has (not all species) "girdle-bands" (like some Closteria),
Cosmar. has never; Pen has longitudinal striolations (or at least very deli-
cate rows of small punctulations ((which I suppose to be pores??)), never scat-
tered pores, Cosm. has never such longitud. striolat., but always scattered
pores. This is how I see the situation now. (West's Monogr., Fritsch's Brit.
Alg. and even Krieger's Desm. Fl. are of no or very small ^{help} use. Perhaps this
question should be mentioned in our publication?

Another question: the initial letter of specific names. I have writ-
ten to our friend Teiling begging him to ask some of the nomenclature spec-
ialists in Sweden. And here is the answer from prof. Nils Hylander: "On
account of your letter concerning the spelling of specific names I beg to
state what the new Rules, International Code of Botanical Nomenclature (Reg-
num Vegetabile, vol. 3, Utrecht 1952) on account of the resolutions at the Con-
gress at Stockholm 1950 direct ^{in Circumstances} of capital and small initials: All specific
and infraspecific epithets should be written with small initial letter, thou-
authors desiring to use capital initial letters may do so when the epithets
are directly derived from the names of persons (whether actual or mythical),
or are vernacular (or barbaric) names, or are former generic names." Prof.
Hylander continues: As earlier this is only a recommendation, no absolute
rule, and everyone may do as he wishes without punishment. I myself (this is
Hylander) write personal and geographical names, nominative or genitive, with


ROLF GRÖNBLAD

KARIS
FINLAND

capital initials , all other names -thus also adjectiv-
tic derivations of nomina propria- with small initials. On account
of dr Grönblads question it may be pointed out that adjectives deriv-
ed from generic names may not be written with capital initials, only
the generic name (in nominat. & genit.)

We have now to decide what to do in our publication. I am inclined to
propose that we should use always small initials. Do you agree? I thi
this will be the simplest solution, and I believe it is very much ^(nk)
used in America.

I hope you can understand my translation from prof Hylander's
letter, and also the quotations from Lütkenüller's letter and manusc
With kind regards, yours sincerely,



May 17 1953

Dear friend Grönblad,

I am glad to say that I have received another letter from Dr. Krieger, which is enclosed herewith, from which you will see that he is getting along as well as can be expected. The CARE parcel that I ordered sent to him was not delivered, and CARE has refunded the money that I paid for it, stating that they are unable to accept any more orders for delivery in the eastern zone of Germany, because of difficulties which they are unable to overcome. So I have written today to Dr. Krieger asking him to tell me whether I may send him the other kind of food parcel that is deliverable only in the western sector of Berlin, which means that he must cross the border to get the goods, and then bring them home piecemeal. Also I told him that you are anxious for news from him.

What you write about *Liatkensis* and *Penium rufescens* is completely new to me, and quite surprising. I have never seen any of his works, but I know from the writings of W. & G.S. West that they had a high regard for his ability. His amended diagnosis of *Penium* provides me with a criterion for distinguishing between *Penium* and *Cosmarium*, at least when the longitudinal striulations or rows of punctae are visible, which may not be the case when the cell retains the chloroplast. It appears to me from your letter that the longitudinal markings are the only character which will separate some closely similar species of *Penium* and *Cosmarium*, and if this is true it seems a very slender basis for separation.

If you think it advisable to transfer *P. rufescens* and var. *corrugatum* to *Cosmarium*, I have no objection, but you would have to give an explanation similar to what you wrote in your letter.

Also it is agreeable to me to make a uniform practice of spelling all specific and infraspecific names with a small initial letter. To forestall criticism from persons who are not familiar with the change, it would be necessary to quote the new (1950 or 1952) Recommendation from the International Rules. I think that some authors will not like the change, Prescott for instance, and I believe that in this country it is principally the zoologists who have ceased capitalizing specific names. Some years ago I learned, to my surprise, that the majority of members of the Am. Microsc. Soc. are primarily interested in zoology, and that for a number of years the editors have been zoologists, which accounts for the practice of using small initial letters in the Transactions of the Society. For my paper "New Varieties of *St. Ophiura*" I had to make a special request to the editor, to use the capital O.

When you feel like writing about something else than desmids, I wish you would tell me something about Dr. Krieger. I know nothing about him, and his letters are always very brief. Apparently he wrote very little on desmids before he started on his monumental monograph. Where did he get the enormous knowledge that is required for such a work? Is he a professional algologist? Soon after the war Dr. Wm. Randolph Taylor published a note in Bull. Phycol. Soc. that Krieger's son "and aide" had been killed during the war. Apparently he must have had two sons, since he writes that one had recently returned from a year's journey in Nigeria. I shall appreciate any details that you can tell me.

Sincerely yours,

May 21st 1953.

Rolf Grönblad
Karis
Centralgatan 86

Dear Friend,

This is a letter not directly concerning Algae. But in part an answer to your last one. - I was visited by a German limnologist Prof. Thienemann (address: Plön, Holstein, "West Germany"), a few days ago. We made excursions together with prof. Järnefelt to some of my old desmid hunting lakes in the neighbourhood. I told about my anxiety for dr Krieger in the Soviet Zone. When prof. Thienemann has returned home he will write to me how to do with the care-parcels. It is somewhat difficult at this moment and there are detailed instructions as to what is allowed, what not. If there is anything forbidden in a parcel it will be confiscated by the Russians. Especially all written or printed communications are dangerous - also to the receiver. Newspapers or even small pieces of any printed paper must never be used - it is the worst you can do. The simplest and surest way of sending anything to dr Krieger will be to send the parcel or letter to prof. Thienemann who will do his best to bring it forth to dr Krieger. However, it will be best to wait and see what prof. Thienemann has to tell us.

I have quite recently read a book by a Danish biologist who describes the very interesting but also terrible conditions under which excursions are made in Florida "Everglades" National Park. I think his story is quite true and not invented. Never should I dare wade in water and mud surrounded by all kinds of snakes as he did in the black night. It is terrible even to read about such events. Have you collected in the Everglades?

Apropos African desmids: Dr J.W.G. Lund (Brit. Freshw. Assoc.) has promised to send me such as far as he can get it. He has written to three friends of his who are working at different biological stations in Africa. When I asked dr Lund to send me material it was because I was curious to see how the Desmids of that continent look. And, when I wrote my beggar letter, I had an intention to share the material with you and I thought it would be a glad surprise to you.

Now I tell you about it beforehand because I see that even you have been trying
Digitized by the Hunt Institute for Botanical Documentation

to get Desmids from Africa. As soon as I have any communication about this material you will hear from me. And I think it would be very good, indeed, if you would work up it, because presently I have too much to do. Perhaps we then could jointly publish the results.

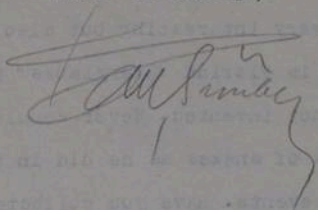
Why do you not anymore use your microscope camera? Of course I should be very glad to have such an apparatus - but I do not quite know whether I can take it from you. I am very grateful for your kind ^{offer} proposal, but it seems to me not quite correct that I should take such an expensive thing from you. "One good turn deserves another". What can I do for you? What do you think, couldn't I make for you copies of such publications that you have not in your library? I should feel much more at ease and comfortable if I were allowed to do something in this way for you. Dixi.

When I told our friend Teiling about my proposal to do as many botanists nowadays (esp. in America) do, that we should use only small initials for specific names, Teiling was quite horrified about my rude conduct. Well, what do you propose to do? X)

My next letter will be about our desmids. So long,
my best wishes,

yours sincerely,

X) I see in your last letter that you are also willing to use small initials.



May 27 1953

Dear friend Grønblad,

Our last exchange of letters must be about the quickest on record, an event to be celebrated! Some time ago Prescott gave me the name of Prof. Thienemann as a possible source for obtaining European algological literature that would otherwise be very difficult to procure, but I have not had occasion to write him. I shall be very glad to receive his advice concerning the sending of food parcels to Germany, and I am sure that his assistance will be invaluable. The only parcels that I have sent, except for desmid samples like those that I sent you, have been ordered direct from C.A. B.E. in New York, who, of course, are quite familiar with all the regulations.

Naturally, I cannot fully appreciate the consequences of living under the shadow of the Soviet rule, but I have read enough to make me very careful in my communications. Accordingly, I never make the slightest reference to political or economic conditions; my remarks are confined to my work on desmids and similar subjects, and to general enquiries concerning my correspondents' health and what they are doing. The only printed matter that I ever send consists of reprints of my papers on desmids, or occasionally a scientific paper like *Trans. Am. Microsc. Soc.*, for which I have no further use.

Our friend Rusicka collects postage stamps for one of his family, and ~~was~~ some years ago I asked if he could send me some stamps from neighbouring countries. He replied that it was absolutely forbidden either to receive or send postage stamps, cancelled or uncanceled, of any country, because they represented monetary values, no matter how small. On one of his recent letters there was a postscript, written in a childish hand, "I thank you very much for the pretty stamps" Vera Rusickova. This is his small daughter, 10 or 11 years old, thanking me for the new stamps that I had placed on the envelope. I wrote him asking if it was permissible for me to send Vera some stamps from other countries. But he answered, very carefully, that Vera would have to wait for some time yet.

I was quite amused by your Danish biologist's account of his "terrible" experiences in the Florida Everglades. I should not care to wade around in the water after dark, and I cannot imagine what he was doing under those circumstances unless he was "shining" frogs or alligators, which is strictly prohibited by law. "Shining" means wearing a brilliant acetylene or electric light on the forehead, which throws a beam of light ahead of the hunter, and is reflected by the animals' eyes as bright red or green spots, and makes it easy to shoot them. The Everglades is a vast swamp, many hundreds of square miles in extent, consisting of a very shallow basin overlying limestone rock, with occasional small areas of dry land called "hammocks", on which grow a few trees and palms of various species. It is a virgin wilderness, comparatively little explored, though its main features are well-known. A few years ago it was made a National Park, and all hunting and shooting is prohibited, though fishing is allowed. Four roads have been constructed through this swamp, at vast labor and expense, by excavating deep ditches and using the excavated rock to form the roadbed, so that all these roads are lined on both sides by the ditches, which in most places are full of aquatic plants. In the southwestern part of the Everglades the roadside ditches communicate with salt water as is evidenced by the growth of small mangrove bushes along the roadside. I have travelled these roads many times and made a number of collections, but in general the desmid-flora is rather uninteresting because of the underlying limestone formation. My collections 80-86, 173-175, are from the true Everglades, and last winter I made several others southwest of the Royal Palm Park, but discarded them because they contained nothing of special interest.

The interior of the Everglades is quite difficult of access, and since it is 1000 miles from New Orleans I have never had the time to attempt to get into it. The Seminole Indians have a large reservation in one of the wildest parts, and they reach

"civilization", as represented by Highway No. 41, by means of air-boats. These are small flat-bottomed affairs, similar to the English punt, with square bows and sterns, propelled by airplane engines and air-propellers mounted several feet above the water. In these boats they can skim through and over the aquatic vegetation, and in water only a few inches deep, where a submerged propeller would strike the bottom or become entangled in the water plants like the water-hyacinth, Eichhornia crassipes, which has a very beautiful flower something like the hyacinth, but has become a pest in many countries since it forms dense mats entirely covering the surface of canals and streams, interfering with navigation.

There are plenty of animals in the Everglades. My wife and I once saw a puma at the side of the road a few hundred yards ahead of our car. This is about the size and shape of a small leopard, with a tawny hide like a lion, but without the mane. When he heard the car approaching he disappeared into the roadside bushes, and though I slowed down at the place where he vanished, I was unable to see him again. Alligators are common and are protected by law throughout Florida; many of them can be seen floating in the water or sunning themselves on the banks, at the Royal State Park, where some buildings have been erected for the accommodations of tourists, and there are plenty of tourists. Snakes, of course, are abundant, but I suppose no more so than in southern Louisiana, which also is a land of swamps and marshes. We have three very poisonous kinds, rattlesnakes, water moccasins, and coral snakes, whose bite may easily prove fatal unless treated promptly and efficaciously by a doctor. Yet you hear of very few fatal accidents from this cause. Considering the thousands of times that I have waded in ditches, ponds, lakes and swamps throughout Louisiana, Mississippi and Florida, I think it remarkable that I have seen so few poisonous snakes. And in my experience they are always more anxious to get out of your way than you are to avoid them. The only "narrow escape" that I have had was many years ago, about 50 miles north of New Orleans. I was standing in high weeds on the bank of a stream, looking around to see what I could get, when I saw a movement at my feet, and a moccasin slithered between my legs and plunged into the water. He swam away a few yards, then turned around and lifted his head out of the water and looked at me, as much as to say "What the hell are you doing in my domain?". The only wild rattlesnake that I have ever seen was swimming across an arm of Dead Lake, near Weahitchka, Florida. This was a very large one, I estimated not less than five feet, maybe six feet long. The negro who was paddling my boat became very ~~much~~ much excited, and said that if he could catch the snake he could sell it for \$50, though I think this was considerably exaggerated. He tried to head off the snake in the water, but the snake reached land first and disappeared under a tangle of dead branches left there by some flood. The negro beached our boat, and after cautioning me not to move away from it, he obtained a long pole and proceeded to try to rout out the snake. But he was unsuccessful, and I thought he was quite lucky, because I had no idea how he could have captured the snake alive. It was so large that it would have been a very ugly customer, and I certainly would have made no attempt to help him.

When I wade in such waters I wear heavy rubber boots that come halfway up my thighs; they would be good protection against bites. My arms and hands are unprotected, but I always carry a bamboo pole with which I probe ahead of my steps, to make sure that I do not get into mud too soft to carry me, as I have done once or twice. On the other hand some of the ditches, small lakes, even swamps, have bottoms of hard white sand, on which you can walk just as easily as on dry land.

The Danish writer either was overwhelmed by the strange tropical surroundings, or else he laid on the local colour too thickly. I am of a sedentary disposition, have never engaged in hunting or any outdoor sports, yet I have not the slightest hesitation in wading and collecting in those waters. If I see a snake I simply keep an eye on him, and that's all. I am more afraid of the semi-wild cattle that roam the ranges in Florida, though never has one of them attempted to attack me.

If you succeed in getting any African desmids I should be very glad of an opportunity to look at them, and perhaps work them up in collaboration with you. I have never seen any African material, but I am finding from Indonesia some desmids that were first found in Africa, such as the Xanthidium sansibarense of which I sent you a sketch.

The reason I do not use any more the Japanese camera is that I have acquired the great American habit of wanting to do everything speedily, and with the least amount of trouble. This camera must be attached to the microscope everytime a photo is taken, and then detached, though there is a clamping attachment that can remain on the upper part of the ocular tube. Also a dark room, or at least a changing bag is required for loading and unloading the plate or film holders, unless you use a film pack which also has its disadvantages. Developing and printing is very trouble-some in our hot climate, and I have not wished to attempt it. The smaller 35 mm reflex camera that I bought last Christmas is more convenient for my purpose. It is attached to the microscope by means of extension tubes, one of which is hinged so that the camera can be swung out of the way without being detached from the microscope. The film is loaded and unloaded in daylight, and you can make 3 or 4 or half-a-dozen exposures for the cost of one of the larger ones. Of course the 35 mm negatives are useless by themselves, but you can select the best of several exposures and have it enlarged to any reasonable size.

I don't know what the Japanese camera cost when it was new, but a similar one of American make would now cost something like \$150. I paid \$35 for the Jap several years ago, and I don't suppose I could get more than \$20 for it now, so it is not nearly as expensive as you imagine. Anyway I am going to pack it up and send it to you, and if you do not use it you may pass it on to somebody else who might be glad to get it.

I thank you very much for your offer to make microfilm copies of Lütkenüller's and other papers, but the microfilm itself is useless to me. Ten years or more ago I obtained microfilm reproductions of Krieger's 'Sunda' paper and some others, and bought a projector to throw an enlarged image on a screen, but you have to sit so far from the screen in order to operate the machine that it is difficult to read the text, so I had enlarged prints made in New Orleans. They cost me considerably more than I could have bought full-size photoprints from some of the large libraries, like that of the Dept. of Agriculture in Washington, which has an enormous collection of botanical works of all kinds. However, I recently stumped them with a request from Ruzicka for copies of the plates in Gutwinski's 'Flora glonow okolie Tarnopol', which they did not possess. I am getting it from the Farlow Library of Harvard University. These photoprints cost only 10 cents per page, far less than the New Orleans shop's charge for enlarging a microfilm, or for making direct photostats.

I have acquired a pretty fair collection of literature on tropical and subtropical desmids, in which I am principally interested, partly by purchase, and also through the generosity of Teiling and Krieger, who have sent me many duplicates of old papers. I still lack many of the important European papers, like those of Lütkenüller, but I hardly think it worth while for me to spend a lot of money on them, because the tropical desmid-flora differs so radically from the European that even when I find rare species that were originally discovered in Europe, they nearly always differ enough to make new varieties. Such is the case, for instance, with the *X. superbum* that you recently mentioned, also with *Cosm. magnificum* and *Microsterium tropica* var. *polonica* and many others.

Summer arrived here with a BANG about a week ago. It started with 90° and has crept up every day until today it hit 97.3° F (36° C) an all-time record for May 28th in the history of the New Orleans weather bureau, which has been operating for 75 years. And we shall have these 90° temperatures almost constantly until the end of September. Our business office is cooled with three small electric machines similar to those in an electric refrigerator, and is fairly comfortable. At home I have a 36" attic fan which draws in the outside air through most of the windows of the house, and discharges through the attic; in hot weather this fan runs all night, though I turn it to a lower speed about midnight. In my microscope room downstairs, where I am writing this letter, there is a 12" oscillating fan blowing on me from about 6 feet away, yet I am still perspiring. You may imagine that it is not easy to work under such conditions.

For the last week I have been without any teeth, and have been living on soup and cereals. Fifteen years or more ago I had all my remaining teeth extracted and have worn artificial dentures since. They finally wore out, so I asked my dentist what

new upper and lower plates would cost. He wanted \$350.00, which was much more than I was willing to pay, so I am getting the old teeth reset in new plates for \$75.00. The cheapest you can get a new set of upper and lower plates for, from one of the 'cheap-jack' dentists, is about \$150.00, and my wife tells me that a friend of hers paid \$400.00 for a set, which probably included extraction and treatment. What do you think of such prices, and what do you get in Finland for a set of upper and lower plates? (in dollars please, - I don't know what your markkaa are worth).

This is the end of the letter. Now I'm going to look at some Borneo material and try to find a specimen of a new *Xanthidium* to get top and side views, which I was unable to draw from the first specimen found. It is like a double-sized *Xanthidium spinosum* (Josh.) W&M, = *Cosm. spinosum* Josh.

With best regards and good wishes,

Your friend

May 23rd

I was just going to the post office with my letter to you when your last letter of May 17 arrived.

Wouldn't you be interested to have photocopies of Kükenthal's papers? I have them all and I could easily take microfilm-copies which then can be magnified if you like.

I am glad to hear about Kewyer that he is alive and getting on. Write about him in my next letter.

R.G.

ROLF GRONBLAD

KARIS
FINLAND

July 21st -53

Dear Friend,

I send you only a few words to tell you that I have sent to Dr Krieger a box containing your Florida samples. It was sent c/o Dr Kurt Krieger so I hope it will arrive safely. (A parcel containing victuals, wich I sent some time ago - also to dr Kurt Krieger- arrived safely).

Two days ago I sent a small parcel to you containing: 1) and 2) samples from Italy, Lake Nemi and Lake Albano near Roma. There are fine series of *Staurastrum bibrachiatum* (= *Dichotomum* sens. West.) and *S. subgracillimum* and numerous dichotypical forms connecting these two species. It is not quite easy to ~~write~~ make drawings of these very small Desmids.

Would you try? - The third sample is from Lake Victoria (plankton) sent by ^{Edna Lind} Edna Lind, Makerere College, Kampala, Uganda. It is not very rich in Desmids, but among others there is a interesting form of *St. tonopeka-*

ligense (3 and 4-angular). The front view is somewhat different from American specimens. But I have not yet had time to have a closer scrutiny at it. Miss Lind has promised to send more material when she has got my acknowledgement of the arrival of her first parcel. So we have to wait and see.

To morrow I am going to Lapland and the northernmost Norway. This will be my third (and perhaps last) excursion to the arctic Scandinavia. I shall go by aeroplane (for the first time in my life) to N. Finland and there we shall meet with some Swedish friends and continue by automobile. If only the weather will be good it will be a very pleasant trip.

I am also preparing a letter for you, but you will have to wait till I have returned.

Did I tell you that Dr Hannah Croasdale is coming to see me in August. Wouldn't you like to see Europe and Finland and me??

With kind regards
Yours sincerely
T. Rayburnell

ROLF GRÖNBLAD
KARIS
FINLAND

Aug. 8th 1953.

Dear Friend,

The same day I arrived home from Lapland I found a parcel containing the microphoto apparatus. Thank you very heartily for your kindness. Perhaps you will send me a few lines now to use it? But perhaps Dr Croasdale who will come 26.8. can show me. If not, I can go to an optical magazine in Helsingfors. Which kind of film did you use? Extra~~x~~rapid Kodak and how long is the exposure time? It depends of course on the source of light. I think I must find out these and other details myself. Anyway, it will be very interesting to try. Do tell me if I can send you anything in return.

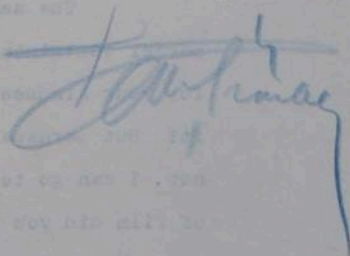
Before I went away to Norway I sent to you 3 small vials containing Desmids from Italy and from Victoria Lake in Africa.

Thank you also very much for the small book on snakes and other poisonous things. It is very interesting.

STANLEY
GARDNER

With kind regards and many thanks

sincerely

A handwritten signature in blue ink, appearing to read 'Stanley Gardner', written in a cursive style. The signature is positioned to the right of the typed word 'sincerely'.

Aug 16 1953

Dear friend Grttnblad,

Well, how did you enjoy your first airplane ride? Not very exciting, is it?, after you get off the ground. The most striking impression is how slowly the plane seems to be moving, yet you were probably going 250 km per hour or more. The large airliners travel more than 500 km per hour, yet they do not seem to move much faster than the smaller ones. I think most of Finland is fairly flat, so perhaps you did not fly over any high mountains, as I have done in this country and in Central and South America. At such times I cannot help wondering what would be the result if the pilot had to try to make a forced landing, 100 km from nowhere.

I am glad, and also a little bit surprised, to learn that Miss Crossdale will visit you at the end of this month. I had the pleasure of meeting her a few years ago, at a meeting of the Phycological Society in New York, and was much impressed by her vivacity and energy. Since then I have exchanged a number of letters with her, principally on the subject of Latin diagnoses for my new desmids, and I have also told her of the methods I use in making camera lucida drawings. I think she has adopted some of my suggestions.

had The three tubes of material that you sent arrived safely two days ago, and I have/a brief look at their contents. The Italian ones do not contain many desmids, and nothing of much interest except the *St. bibrachiatum* and its related forms, on which I am sending some notes on a separate sheet. Nevertheless, I am glad to have them.

By a curious coincidence, two weeks ago I received from Dr. Krieger two vials of material from Lake Victoria, and I wonder if these also were collected by Miss Edna Lind. Krieger did not give the collector's name. One of them came from Bukakata, which is on the shore of the northwestern sector of the lake, and the other from Ripon, which I believe is at the extreme northern end of the lake, not far from Kampala. The contents of your tube and the two from Krieger are almost identical, though yours contains small tangled masses of a filamentous alga that I guess belongs to the Cladophoraceae, and that does not appear in the other two. After the diatoms (*Melosira*, *Surirella*, *Cymatopleura*) the most numerous alga is *Pediastrum*, of which there are several species and varieties, the largest forms corresponding with *P. clathratum* as described by Schmidle from Lake Nyassa. I have never before seen the 4-radiate form of *St. tohopekaligense*, though I have several other forms and varieties from other parts of the world; it seems to agree with the 4-radiate form of var. *trifurcatum* as figured by W&N, Monograph, Pl. CLV, Fig. 13, and I have also seen it illustrated somewhere else. In Krieger's samples I noted *St. leptocladum* without the apical spines, but in yours I found two specimens that possessed apical spines that were smaller than usual. West & West, also G.M. Smith, did not believe in the existence of a spineless form of *St. leptocladum*, and accordingly refused to recognize Wille's var. *cornutum* (see remarks by Smith, Wisconsin Phytoplankton, pp 102-103). I have not previously seen the spineless form.

There is not much that I can write you concerning the use of the microcamera; perhaps Miss Crossdale can help you to get started, but you will have to learn by experiment. The Kodak extra-rapid ~~film~~ film (known here as Plus-X) is too fast for micro work; a slower film is better because it gives more contrast and a wider latitude in exposure. I used Kodak Verichrome which is a medium speed. The time of exposure may vary from 1/2 second to 10 seconds, but it depends upon so many variables that it is hardly possible to calculate it until the correct exposure has been found by experiment for one certain set of conditions. The variables are: character of subject, whether

light, medium, or dark; the intensity of the illuminant; the degree of opening of the condenser diaphragm; and the total magnification. Note that if the magnification is doubled, the exposure must be increased four times; in other words the exposure varies as the square of the magnification. A good way to make a trial exposure is to pull out the protecting slide of the film pack one-quarter of the way and make an exposure of say 1/2 second; then pull it out to half-way and expose for another 1/2 sec., then to 3/4 of the way and expose for 1 second, then pull the slide entirely out and expose for 2 seconds. This gives exposures on one film of 1/2, 1 and 1 1/2 seconds, and from the resulting negative you ought to be able to tell which exposure is most nearly correct. Of course you must be careful not to disturb the focus or anything else when you are pulling out the slide. The method is shown on the enclosed old negatives made several years ago. I do not know what the exposure times were, and apparently none of them is very good.

The circular viewing glass with the clear spot in the center is attached to the side of the camera and enables you to view the subject right up to the instant of exposure. There is provision for adjusting the position of this viewing glass so that the subject will be in exact focus on it, at the same time that it is in exact focus in the plane of the film. This adjustment is accomplished by placing a ground (frosted) glass on the open top of the camera, as nearly as possible in the plane that the film will occupy, and then focussing the object on the ground glass with the aid of a small fixed-focus magnifier. It helps if the ground surface of the glass is lightly oiled to make it somewhat more transparent. When this has been done the position of the circular viewing glass is adjusted so that the object is sharply focussed on it also, and the screws are tightened to fix it in position. Then you know that when you have focussed correctly through the viewing glass, the object is also sharply focussed on the film.

You will find that it is impossible, except at very low magnifications, to photograph desmids so that both the profile and the surface ornamentation are shown. This is illustrated in the pictures of *Cosmarium cosmetum* that I sent you, and in the excellent photos of fossil desmids by Woloszynska. The reason is that the higher power objective lenses have practically no depth of focus, only a few microns, and there is no way of overcoming it with the usual micro-camera. It might perhaps be done by using a low power objective, say 10x, and a very long camera, two or three feet long. Alexander Graham Bell, the telephone inventor, used a camera ten feet long for photographing small insects, and got some remarkable pictures.

It is not at all necessary to send me anything in return for the camera, but you might have some duplicates of desmid papers that I could use. If so, please tell me their titles and I shall inform you whether I possess them. The list of what I do not possess would be far too long. Also I should be glad to get, from time to time, samples of collections that you may get from any tropical or subtropical countries, such as those from Uganda. By the way, since Miss Lind is going to send you more material, I suggest that you ask her to try to get collections from small lakes, ponds, roadside ditches, and swamps, and in the form of squeezings from aquatic plants rather than plankton net gatherings. Such habitats, in my experience, are far richer in desmids than the large lakes.

Many thanks for the samples, and for the pretty postage stamps that you placed on your last few letters.

Sincerely,

I was puzzled by your statement that in the material from Lake Nemi, Italy, you had found *St. bibrachiatum* and *St. subgracillimum*, together with dichotypical forms connecting these two species. After digging out Bohlin's paper on the Azores, I found that what you undoubtedly intended to write was *St. gracillimum* var. *biradiatum* W&W.

But I am convinced that Bohlin was in error when he referred the plant shown in his figure 12, and the lower semicell of his figure 13, to *St. gracillimum* var. *biradiatum*, for two reasons. Firstly, *St. gracillimum* is basically a triradiate plant, according to W&W, and the biradiate form is merely a variety; it would be incorrect, according to my view, to refer a strictly biradiate plant like *St. bibrachiatum* to a basically triradiate species. This argument is a matter of personal opinion, with which you may not agree, but I think you cannot disagree with the following one.

W&W's description of *St. gracillimum* reads in part "apicibus profunde bifurcatis", and their drawing shows the ends of the processes divided to form two long and very slender spines; also their figure a vertice shows the processes tapering to an extremely fine point representing the spines. This is quite different from the truncate ends of the processes in *St. bibrachiatum*, which are furnished with three (or four) minute spines, and which are essentially similar to the ends of the processes in such closely similar species as *St. tetracerum*, *St. excavatum*, *St. perundulatum*, *St. Smithii* Telling (= *St. contortum* G.M. Smith). ~~Bohlin~~ West, in ~~his~~ 1908 paper on Algae of the Yan Yean Reservoir, also states that Bohlin was mistaken, though ~~how~~ he does ~~not~~ give ~~him~~ his reasons. West also states, in the same paper, that there is little question that *St. bibrachiatum* var. *cymatium* has had a direct origin in *St. tetracerum*. (I suppose that by implication this would include the specific form of *St. bibrachiatum*). In this connection you might refer to my drawings Nos. 2447-2456, made in 1941 and 1945, before I knew of the existence of *St. bibrachiatum*, and before I had acquired either Bohlin's or G.S. West's 1908 paper. The first of these drawings, No. 2447, is marked "St. tetracerum forma?"; others are marked "St. tetracerum fa. octoceram"; and in the last one apparently I decided to "raise" it to the rank of "var. octoceram".

My drawings show two differing forms; the smaller one may perhaps correspond with the specific form of *St. bibrachiatum* (I do not possess Reinsch's original figure), or perhaps with fa. *brevior* W&W from Madagascar. Note that in this small form the two-armed semicell of dichotypical specimens has prominent "shoulders" at the basal angles, which are quite lacking in the four-armed semicells. I have taken these "shoulders" to be characteristic of *St. tetracerum*, but I may be mistaken about this. My drawings of the larger form, with longer and more slender processes, seem to check very well with var. *cymatium* W&W, and probably should be referred to that variety.

The question arises in my mind as to whether *St. bibrachiatum* is entitled to a specific rank. Krieger also doubts this; see his Sunda paper, p. 198, and Pl. XV, Fig. 18, which he calls *St. excavatum* var. *planctonicum* fa. *bibrachiata* (should be fa. *bibrachiatum*). It is a noteworthy fact that in material from southeastern USA, Azores, Sumatra, Australia, and Italy, the bibrachiate form is always accompanied by the simpler form with two arms per semicell, and with dichotypical forms connecting the two. Even in Madagascar W&W found one abnormal ~~semicell~~ cell with 7 arms, as I did from Mississippi (my drawing, or rather list, No. 2450). In my collection Miss. 9, the 8-armed cells are much more numerous than the 6-armed or 4-armed cells. In your material from Lake Nemi the reverse is true; the 4-armed cells are fairly common, while the 6- and 8-armed cells are rather rare. Bohlin, G.S. West, and Krieger make no mention of the relative frequency.

So far I have only taken a brief look at your Italian material. From the few specimens that I have seen of the bibrachiate form, I think it agrees very well with var. *cymatium* W&W and with my larger forms from USA. I have seen a lot of the four-armed cells, but do not know what name I should assign to them, because they show a resemblance to several other *Staurastrum*, e.g. *St. tetracerum* (rather remote), *St. excavatum* var. *planctonicum* Krieger (close), *St. Godeffroyi* (Godeffroy) (local except

St. bibrachiatum (2).

for size), and *St. Smithii* Telling (= *St. contortum* G.M.Smith). For my part I cannot see much, if any, difference between the plants of Krieger and G.M.Smith. I have found such plants in USA, S. Australia, N. Australia, and the Island of Bali collections, and have assigned them to *St. Smithii*, though they could equally well be assigned to *St. excavatum* var. *planctonicum* Krieg. However, I do not feel inclined to accept Krieger's plant as a variety of *St. excavatum*, because it lacks the characteristic "excavated" apex.

There is one detail in which your Italian specimens differ from previously described forms, - they have four minute spines at the ends of the processes, while other authors describe them as trispinate (G.S.West, Yan Yean) or bispinate (W&A, Madagascar). However, W&A's Madagascar drawing shows two spines in front view and two in vertical view, which would make four altogether, if the drawing was correctly executed. Bohlin and Krieger do not mention the number of spines. It is sometimes very difficult, and occasionally I have found it quite impossible, to ascertain whether there are 2, 3 or 4 spines on the processes of such tiny desmids, even after a determined effort to do so.

You say it is "not quite easy" to draw such small desmids. I would make it much more emphatic and say that it is very difficult. I drew one last night, after a lot of trouble caused by the specimen swaying back and forth by a minute amount, just a couple of microns. Finally I found that the movement was caused by an oscillating electric fan that was blowing on me and also on the microscope, causing a small variation of pressure on the cover-glass. After I stopped the fan the specimen remained still, balanced on edge for the vertical view, and I was able to complete the sketch.

I shall make drawings of all the different forms that I can find in your material, and send them to you a little later.

Sept 17 1953

Dear friend Grönblad,

From Brazil I have THE MOST WONDERFUL DESMID IN THE WORLD,
worthy, I believe, of a new genus. Will send a drawing tomorrow, and a sample
in a few days.

This is merely to whet your appetite.

Best regards

Sept 18 1953

Dear friend Grönblad,

Some time ago I told you that I hoped to get some algal samples from Brazil, which had been promised to me. But the promise was so long in materializing that I had almost given up hope. However, I gave my correspondent some gentle nudges by sending him two books on the Mississippi River, which were valuable to him, and this has resulted in the arrival a week ago of 30 bottles of samples. I hope that you will do me the honour of collaborating with me in the working up of the desmids, and perhaps you know of two other phycologists, one for the diatoms, and one for the other algae. Please let me know what you can do in this respect.

The samples are of very uneven quality. They come from two general regions, one in the region of the lower Rio Tapajos, which joins the Amazon a short distance above Santarem; the others are from the region of the Upper Rio Negro, in northwestern Brazil, a little-explored region from which it is almost certain that no algal gatherings have been made. Unfortunately, the Rio Negro samples are almost devoid of desmids, the only ones that I have seen so far are a very few small *Cosmaria* and *Staurastrum* and *Glosteria*, of little interest. This is perhaps due to the very acid water, of pH 4.7, 4.6, 4.1 and perhaps even lower; 4.1 apparently was the lower limit of his testing outfit.

The samples from Rio Arapiuns and Rio Tapajos region are better. Some of them seemingly contain few desmids, but they are of very interesting species, mostly unknown to me, and some that look to be new to science. Also some varieties like *Gymnozyga armata*, and *Spondyl. desmidiiforme*. There is one splendid collection, containing a large assortment of desmids, including made of the long-rayed *Staurastrum* described in your paper, with the curious *St. arthrodesmiiforme*.

But the prize of the lot is #11, from Lago da Boca do Igarape Mente, on the Arapiuns River, which is a tributary of the rio Tapajos, and #10 from the same lake. Both are plankton net gatherings, with only a minute quantity of material, so I am going to add #10 to #11, so as to get more to divide with you, and Dr. Krieger, and Prof. Prescott.

In these two collections occurs THE MOST WONDERFUL DESMID IN THE WORLD. It is illustrated on the enclosed drawings, of which the top view is more or less schematic. The great size of this plant, the unusual shape of the body, the corona of conical hollow lobules on the upper semicell only, the great length and unusual curvature of the processes which have curved teeth on the exterior margin only, all are features that set it apart from all other known desmids. But the most remarkable feature is that the two semicells are not alike. The lower semicell does not possess the corona of lobules, and the processes are curved in the wrong direction. You will be inclined to think that this drawing was made from an abnormal specimen. Nicht wahr. I have seen a dozen specimens, all of which are built according to the same plan, differing only in size and in minor variations of the spination of the processes. I have seen nothing to indicate any abnormality.

This dissimilarity of the two semicells is altogether unknown in desmids, to the best of my knowledge, though there have been described several species and varieties with varying degrees of asymmetry, *C. obliquum*, for example. In such desmids the two semicells are usually mirror-images of each other. The phenomenon in the Brazilian plant is much more profound than any mere form-difference, involving questions of genetics and cytology that I am not competent to discuss. I think that even specialists in these two lines will have difficulty in formulating a plausible theory to account for it, and I should like to see our friend Heiling trying to divide the 10 and 6 arms and 8 apical lobules to get a symmetrical distribution to fit in with his radiation theory.

It would be particularly interesting to see some individuals in the process of division, but so far none such has shown up.

The unique features of this plant, and especially the different semicells, are sufficient, I think, to justify the creation of a new genus. In my opinion there would be as much reason as for the distinction between *Phymatodocis* and *Desmidium* which is based on the difference in the process of cell-division; or between *Docidium* and *Pleurotaenium*, or between *Arthrodesmus* and *Xanthidium*, to say nothing of the controversial genera like *Triplastrum*, *Euastridium*, *Spinoclosterium* and *Spinocosmarium*.

I have sent a copy of the drawing to Dr. Krieger, and in the next couple of days I shall send you and him a sample of the material, which, however, will be in a minute amount. Also I am including in the package a sample of the very rich Brazilian sample above-mentioned, and a sample of a new and rich collection that has just arrived from North Australia, in which you will find such rarities as *Xanthidium multicorne*, *Triploceras gracile* var. *elegans*, *M. mahabuleshwarienses* var. *reducta* and a brand-new variety, and many other novelties.

I realize that you will want to see actual specimens of the Brazilian desmid before expressing an opinion, and perhaps you will want to communicate with Dr. Krieger concerning. I am greatly curious as to what you experts will think of it, so please don't keep me in suspense any longer that you have to.

Sincerely yours,

P.S. Some years ago I saw, in one of my Florida collections, an object that I now realize must have been close to the Brazilian desmid, if not identical with it. Only one specimen was seen in the Florida material, and it was so badly obscured with adherent mucus and debris that I was unable to decipher its structure, and to draw it was quite impossible. Of course I knew it was something unusual, but I could not believe that it was a desmid, and reluctantly I had to abandon it as another of the many unexplainable mysteries that I see under the microscope. I have never seen another until now, but maybe it will show up again in Florida one of these days.

ROLF GRONBLAD
KARIS
FINLAND

Sept. 22. -53.

Dear Friend Scott,

Thank you very much for your first report on THE MOST WONDERFUL DESMID IN THE WORLD. I am almost bursting with curiosity. A truly new Genus among the Desmids would indeed be exciting.

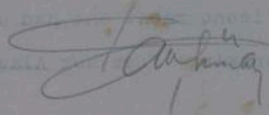
Two days ago I just began writing a letter to you, but there are so many topics to be discussed, and many questions of yours to be answered that it will take some time to finish it. But I shall not keep you waiting for a long time any more.

Miss Croasdale stayed for nearly 3 weeks in Karis going through my library and my drawings and making a lot of copies for her iconograph. She had also brought with her a part of her (very good) drawings from her Alaska Desmids. It was very interesting to have a

look at them and to talk over matters with her. I think Dr C. will present a very good publication when she has finished her work. She has not yet published many papers on Desmids but she seems to know a great deal about them and she ^{was} ~~is~~ very energetic being up every morning at about 6.00 (when I still was in my bed to 09.30). I enjoyed very much the time because it is very seldom I have any occasion to have a personal talk about Desmids with anyone. Once more I try to tempt you :couldn't it be possible for you to make a European trip during the next year? and come to see me? I think there would be no lack of topics. And how useful for our joint work it would be. Please , do think seriously of it.

Now I shall remain waiting for THE WONDER OF DESMIDS, and try to have my letter for you finished.

So long, with kind regards,
yours,



The difference between the two comes into view when you put out:

Pseudoxanthidium
Staurastrum Des
 Scott

or:

Staurastrum xanthidium

or:

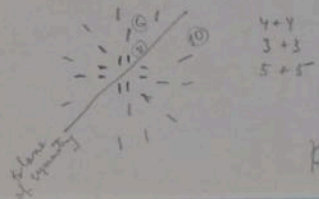
Xanthidium
staurastrum forme

or:

Scottia mirabilis !

My first impression of your drawing in front view was: hmpf - a *Xanthidium* my second, when looking at the vertical view was: - ? -

Scheme of symmetry (5 Testing):



Rhd

ROLF GRONBLAD

KARIS
FINLAND

Dear Friend Scott,

This is another expres-letter. Your air-mailed Desmids arrived this morning. As soon as I had free time at my disposal I carefully opened the smallest vial, took very little of the precious material by means of a pipette, placed it on a objectglass, put a small piece of coverglass among the algae (in order to get more easily vertical views), put a coverglass over the whole and looked into my microscope. At first I caught sight of some familiar Brazilian plankton Desmids - quite as you told me there would be - and then very soon my eyes met a most wonderful Octopus-like monster - beautiful monster, indeed - there was your NEW DESMID. I can quite imagine your feelings of excited surprise. This has certainly nothing to do with a Xanthidium, - which was my first impression when looking at your drawing. There is nothing like it. If a new Genus should be avoided - but why? - the nearest one should be perhaps Staurastrum. The asymmetry of the cell seems - we don't know - to be quite a normal character - not a monstrosity or an abnormality. (Of course it would be useful if you could get more material, especially in living condition - but I don't know if it may be possible in tropical climate). Perhaps very little of the material in much sterilized cultivating liquid would succeed. If your friend could manage it. Cell division? Chloroplasts?).

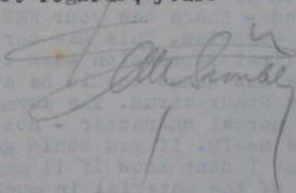
Cosmar. obliquum, which you mention is asymmetrical in another way: both semicells being exactly identical in shape - I don't know the technical term, - but the New Desmid seems to have constantly different shapes of the semicells. This may (?) be caused by rapid cell division, but that we don't know so far.

In one semicell that I could manage to keep still in vertical view there were 9 apical processes - which is still better for confusing our friend Telling's radiating system: as I showed 8-6-10 can be divided into two symmetrical halves 4+4, 3+3, 5+5 and Telling could say that the cell is biradiate - as he says of Staur. leptacanthum - but try to do it with 9-6-10! (I am quite sure that the apical "warts" were 9 in that one semicell, but maybe it was an exception?)

I am very glad that you sent me also material, not only drawings, because otherwise I should perhaps have said to myself: facile dictu, incredibile auditu, mirabile visu.

I think also Krieger will be glad to have material: the "wonder" can not be denied. It will be very interesting to hear about Krieger's opinion.

Best regards, yours



Sept 28 1953

Dear friend Grönblad,

Thanks very much for your quick reply to my letter and drawings of the new desmid. Here's an equally quick response, written $3\frac{1}{2}$ hours after your letter's arrival.

I must confess to considerable astonishment that you could find any resemblance whatever to a Xanthidium, even in front view and without reference to the vertical view. I am fairly well acquainted with nearly all the known species of Xanthidium, including such exotics as *X. superbum*, *sansibarense*, *acanthophorum*, *horridum*, *spinosum*, *Freemanii*, *Burdillii* and *v. alternans*, *trilobum*, *subtrilobum* and var. *inornatum* (not *subtrilobatum* as Skuja writes it erroneously), *hastiferum*, *multicorne*, and *octonarium*. In fact I have two new species that nobody else has overseen, one from Australia and the other from Brazil. In none of these can I find any similarity, or even a suggestion of similarity, with the new desmid.

But the principal objection, of course, is the radial symmetry in vertical view. All Xanthidia, without exception, are compressed from front to back, saving the very few trigonal variations of well-known forms.

So I am afraid that the first three names that you suggested, "*Pseudoxanthidium staurastroides*", "*Staurastrum xanthidiiforme*", and "*Xanthidium staurastriforme*", are not suitable. The fourth one, "*Scottia mirabilis*", I think you must have thrown in just as a sop to appease my desire to see it placed in a new genus. Nevertheless, I have to admit that I should be quite proud to have my name associated with this beautiful plant, though not exactly in this form. One of my friends, who saw my drawing, said he could only think of a particularly horrible spider or tarantula with too many legs. To me it suggests an elaborate chandelier from an old-world Paris salon. The name "*Chandelierium*" has occurred to me, as being better than the Latinized form "*Candelabrum*", which means merely a candlestick, and suggests the 7-branched candlestick of the Jewish synagogues. But all this depends upon the opinions of Prescott and Krieger, which I am eagerly awaiting.

I am also surprised that you do not think the difference between the two semi-cells is very important. To me it seems of fundamental importance, since it is absolutely unknown in the Desmidiaceae, and must involve the very genes and chromosomes whose arrangement determines the architecture of the cell. This seems to me to be of far more significance than the artificial distinctions between pairs of similar genera, as I mentioned in my former letter. Again, I await the opinions of Prescott & Krieger.

Your tale about Miss Croasdale arising at 6 am corresponds exactly with what I should have suspected of her; I told you that I was much impressed by her vivacity and energy. I am quite sure that she enjoyed her visit with you, as much as you enjoyed her company. I too suffer from not being able to talk personally with anyone who knows about desmids; I doubt if there is anyone else in New Orleans, and only one other in Louisiana, who could tell a *Decidium* from a *Pleurotaenium*, let alone an *Arthrodesmus* from a *Xanthidium*. The latter decision, of course, is not always easy, and later on I shall have some comments to make on your Brazilian desmids, *X. antilopaecum* v. *Nordstedtii* (Fig. 152) and *A. longispinus* (Fig. 177). They seem to me to differ only in the number of lateral spines, 3 in the former and 2 in the latter; the chloroplasts are identical, arranged in 4 quadrantal lobes (a vertice) and 4 pyrenoids per semicell. I have seen both of them in the new Brazilian material.

Your invitation to visit you in Finland is greatly appreciated, and you may be assured that I have not been absent from my thoughts. There are few things that I

should like better, and I could foresee many pleasant conversations and many animated arguments, because there are a number of cases where you are I do not see eye to eye. This must be the case, I suppose, between any two workers in any scientific field.

Whether such a visit will ever be possible is more than I can say at present. I can now tell you something that I have not announced before, namely, that at the end of this year I am going to retire from the engineering profession and from business. This means that my income will be greatly reduced, and in fact I shall have to watch my expenditures closely. A trip to Europe would be expensive, since I should have to take my wife along, at least as far as England, where perhaps I could leave her with my sisters in Devonshire, and continue by myself to Sweden and Finland and (perhaps) to Berlin to see Dr. Krieger. (By the way, does he speak English?. I cannot speak German)

Now I'm going to switch over to my microscope table and finish drawing *St. Boergessonii* var. *elegans* f. *pulcherrimum*, and *St. penicilliferum*. Last night I got one view of each, but the cell was too shallow to permit of turning them for the other view. This Brazilian collection, #20 from Rio Arapiuns, is so amazingly rich in numbers of long-rayed *Staurastrum*, that it is occasionally difficult to separate them. In one small tangled group, held together by mucus, there were seven different species of *Staurastrum*, one of *Arthrodesmus*, two filaments of *Melosira*, and one of *Mougeotia*!!!

I wish that you would be kind enough to send a copy of your Brazilian paper to:

Dr. Harald Sioli,
Instituto Agronomico do Norte,
Belem. Para. Brazil.

If you do not have any more copies perhaps you can buy one from the Akademische Buchhandlung. Sioli is the man who sent the Brazilian material, and while he does not know much about desmids, he will be familiar with all the localities mentioned in your paper, and the illustrations of all the strange forms will be an added incentive for him to get more material for us. He has, in fact, promised to send more stuff in a few months, from the region between Belem and the Atlantic Ocean, where he is now making some investigations. I am asking him to send you two papers that he has published, describing the limnological features and water chemistry of a small tributary of the Rio Tapajos, and a larger paper on some features of the lower Amazon. They are written in Portuguese, but there is a German summary of the first, and summaries in English, French and German of the larger one. There is nothing about algae in them, but I found them interesting in that they gave me a little better insight into some of the conditions down there.

Auf wiedersehen!

Your easy division by a single median plane will not satisfy friend Teiling, who will want to divide the vertical view into radial sectors, each containing an equal number of lower and upper processes and apical lobules. I can imagine him scratching his head while trying to do it.

Oct 13 1953

Dear friend Grönblad,

I have delayed answering your second letter about the new desmid until I could get replies from Prescott and Krieger. Now I have received them, and an enclosing copies herewith. The three opinions, yours and Prescott's and Krieger's, coincide pretty closely with what I anticipated. I thought that you and Krieger would agree, with some hesitation, that the new plant is entitled to separate generic rank; and I almost knew that Prescott would accept it enthusiastically as a new genus. As you will note, he speaks of "almost a new family". But I am sure you have ~~made~~ gathered from his publications that he is prone to be a little bit wild in such matters (e.g. Spinoclosterium and Spinococciarium).

When Dr. Krieger's letter arrived just a few hours ago, I glanced over it quickly, and my heart sank down into my boots, because I interpreted his second sentence to mean that he had previously found the organism in other Brazilian material, though I could not understand how anyone who found such a remarkable plant would keep quiet about it. After my wife and I had finished dinner I re-read the letter more carefully several times, and came to the conclusion that I had over-looked the little word 'the': Ich habe in dem brasilianischen Material den fraglichen Organismus öfter gefunden; id est THE Brazilian material that I sent him. So everything is OK again; the honour is all mine, and I do not have to share it with anyone.

Krieger points out that some Staurostra occasionally exhibit processes that curve in different directions on the two semicells. That is correct, and he has named one of them as a new species (*St. variodirectum*, Sunda, Taf. 27, Fig. 5). But such occurrences are, apparently, mere local variations, since normal specimens are found along with the variants in the same collection. A somewhat similar condition occurs in *Micrasterias*, notably in *M. radiata*, as everyone knows.

These comparisons, while legitimate, have little bearing on the Brazilian plant, in which the differing semicells owe their differences to a much more fundamental cause, I think. You and Krieger base your opinions solely on morphology, while Prescott sees, as I do, that physiological ~~phenomena~~ phenomena are involved, which he calls 'polarity', and which I cannot discuss intelligently. Note that Prescott saw "a couple" of specimens in the process of cell-division, confirming that the difference in the two semicells is constant. Though I have seen perhaps 30 specimens I have not come across any examples of cell-division; also in all cases where I have been able to count the apical lobules there have been just eight; the one with nine lobules that you saw must have been an exception.

Both you and Prescott mention the desirability of obtaining living material, and of course I shall ask Dr. Sioli to try and get some. But the difficulties are enormous. I don't know the exact location of the Igarape Mental (not Mente as I misspelled it on my drawing), but it is probably 100 miles from the nearest town, Santarem, which in turn is not less than 500 miles from Belem where Sioli lives. There are no roads in that part of Brazil; all travel is by boat on the Amazon river and its tributaries. Even if Sioli by some miracle could get more living material, the problem would be in shipping it to some laboratory where it could be cultivated; I could do nothing with it. Shipment would have to be by airplane, and somebody would have to possess sufficient influence to see that it was carried in the cabin; if it went in the cargo compartment with other mail it would be frozen at the high altitudes at which these planes travel, 20,000 feet or more. So, you see-----

The next thing to be done ~~is~~ is to get the plant published as quickly as possible, and I hope that you will undertake this task, or arrange to have it provided

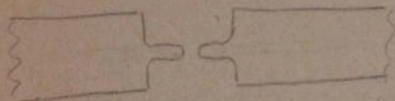
the Latin diagnosis. In the meantime I shall make other drawings and have them inked. You suggested the name Scottia mirabilis, which would be entirely acceptable and highly pleasing to me. Perhaps Scottia mira or mirum (depending on whether you make Scottia feminine or neuter) would be a somewhat more striking cognomen. Please forget the name I suggested, candelabrium; that was just a passing fancy. Some authors, perhaps, would name this plant Siolii, after the collector, but there are several new species that can be named in his honour. For instance I have the smallest Micrasterias in the world, only 34-36 μ long, like no other species, but closest to M. simplex. Also there is at least one, maybe two, new species of Xanthidium, several new Euastra, and the Staurastras are so numerous that it will require a lot of checking to find out which of them are new.

The second part of Krieger's letter refers to a very large Pleurotaenium that occurs in Borneo 270, which is one of those that I sent you. It resembles the plant described by Ch. Bernard as Pl. Treubii, which Krieger cites as a synonym of Pl. brabecula var. maximum. I asked him to let me know what he made of the markings on the cell wall, which are peculiar and deceiving. When the microscope is focussed on the uppermost, nearly horizontal, part of the cylindrical cell, the markings appear, bei Tiefeinstellung, as a dark annulus with a light center; bei Hocheinstellung, as a light annulus with a dark center; thus showing that some structure in the cell-wall is acting as a lens, whether a convex or concave lens; I have not been able to figure out. When the focus is lowered to the sloping side of the cylinder (oblique view), the markings appear quite distinctly to be nearly hemispherical granules projecting above the cylindrical surface, and this is where the deception comes in, because when the focus is lowered still more, to an optical section through the wall at the level of the axis of the cylinder, there are no projections beyond the outer margin of the wall such as would be caused by granules if they were present. However, in this optical section I do think that I can see some very slight curved elevations that could not be called granules. In a former letter Krieger gave a sketch, which I reproduce below, showing his conception of the structure of these markings, which in his view are pits. I replied that I could not see them as pits, and suggested another possible construction, which I thought would give rise to the deceptive appearance of granules in the oblique view. Krieger seems not to like this, because I do not show a pore extending through the wall, which he says is necessary, in some pores at least, for the passage of gases and liquids. This is not quite correct, I think. Pores that would permit the passage of liquids and gases need not be large enough to be visible; stains penetrate quite readily the wall of Netrium which has no visible pores. The inter-molecular spaces would be quite large enough for this purpose, and would be quite invisible even with the electron microscope.

He now asks what you have to say about it. Would be kind enough to take a look, or rather several looks, at this Pleurotaenium, and tell me and him what you think? Perhaps you can crush some of the cells, and thus be able to see the broken edge of the cell wall.

It is now midnight and I must go to bed. Mit herzlichen Grüssen, Ihr

Alm Scott



Krieger's idea

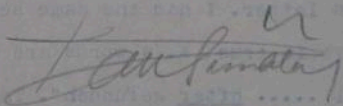


But in any case I think you will have to ask Dr Krieger about the meaning of his dubious sentence.

The name "Scottia mira" will be very fine. I think it must be feminine if the genus is named "Scottia" and not Scottium, which latter sounds badly. Candelabrium I did not like, and "Chandelabrium" is impossible. So we agree again.

About Pleurotaenium I shall write to morrow. I think the cell-wall is like my Brazil.fig.21 and text p.11. - but I shall try to look at it in my microscope .

With kind regards, yours



(If it should be so that Krieger has already found The Desmid in some material earlier sent to him I think the best way would be that you & Krieger as joint authors would publish it?)


Rolf Grönblad

Karls

Centralgatan 86

Oct. 22. 1953.

Dear Friend Scott,

I have now tried to find out the structure of the cell-wall of your *Pleurotaenium* sp. from "Borneo 270". Sketches are enclosed so that you may get an idea of what I have seen - or what I think I have seen. It is very difficult to see the structure quite clearly. (I think microtome preparates would be useful - if made by a skilled microtommist. I have no such possibilities.) - Anyway at first I thought that every pore was surrounded by a small "circular wall" (being situated on the top of a small gemmula):  This was, however not correct. As you say, when the cell-wall is seen in optical section there are no granules on the outer surface - but perhaps a very slight elevation consisting of the darker stained material (probably = Lütkenmüller's "Porenorgan"). This darker material goes through the whole outer layer of the cell-wall (in other words: fills up a pit in the outer layer. What there may be in the inner layer is very obscure (I think there is a very thin canal going through it (=Porenkanal) and ending on the inner surface of the cell-wall in a small pin's head, which also appears in a darker colour (when stained). (see my sketch "c")). I do not believe in Krieger's idea. - It is the darker stained "pore-organ" in the outer layer that in the "oblique view" on the sloping side of the cylindrical cell appears as a "granule" - very deceiving indeed! If you use staining - very simple - methylviolet and afterwashing with potassium acetate - I think you will be able to see more details. Of course, I am not sure that my idea is correct in all details. (Perhaps my Brazilian *Pleurotaenium* fig. 200 is like this, but I have not yet tried to inquire into that question.

You write that Krieger and I base our opinions on "Scottis mira" solely on morphology. No. I am quite agreed with Prescott's physiological

speculations. Though I am not capable of a scientific discussion as to the fundamental causes, "das Ding an sich" as old Kant would say.

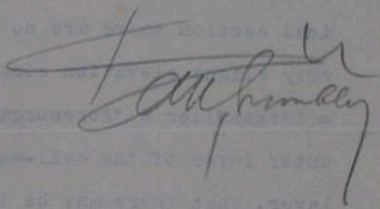
I would not like to put up a new family, but most willingly a new genus. I am quite convinced that "Scottia" cannot be united with any genus of the Desmids hitherto known.

I return enclosed also the microphotograms by Hirano which you sent me some time ago, and also a letter by Krieger.

I see that there are also other questions in your earlier letters which I have to answer very soon.

I have had too much to do the last months, but now I am going to give all possible free time to your USA Desmids.

Kind regards, yours,



Nov 1 1953

My dear friend,

Two letters in one day from Rolf Gronblad!!! That is an event to be remembered, and shows, I think, that you are just about as enthusiastic over the new desmid as I am.

I have written to Krieger, quoting your remarks, and have asked him to clarify his ambiguous sentence, and tell me the true situation; whether or not he has seen the plant in material other than that which I sent him. Naturally I am hoping that we have misinterpreted his language.

I have been studying the desmid, and have now seen something like fifty specimens. Here are the measurements of thirteen consecutive specimens:

L spr	111	108	115	121	105	126	112	113	119	108	109		
cpr	171	170	181	189	159	186	177	168	185	168	180		
W spr	51	50	51	54	55	48	57	55	52	53	50	48	57
cpr	132	132	147	155	159	132	153	162	141	153	151	137	141
I	27	28	33	33	29	30	32	30	34	29	28	34	

The widths with and without processes are those of the lower semicell, which is always somewhat wider than the upper one.

In every specimen that I have seen the chloroplast seems to be somewhat deteriorated; the cell-contents stain uniformly with all the stains that I have at my disposal: methylene blue, Nile blue sulphate, crystal violet (= methyl violet, nearly), malachite green, safranin, basic fuchsin, Delafield's hematoxylin, and iodine in potassium iodide. In no case have I been able to see the pyrenoids, or even an indication of them. In one specimen, in a vertical view looking down on the upper semicell, I saw the chloroplast to be six-lobed, as shown in your sketch. In four examples of detached semicells I have seen the chloroplast, at the level of the whorl of 10 processes, to be five-lobed, each lobe with two extensions into two of the ten processes. In these four specimens I was not able to get a clear idea of the chloroplast arrangement at the level of the whorl of six processes, because it was obscured by the dark (stained) material below it. My present conception of the chloroplast structure is that it has five lobes in the wider part of both semicells, and six lobes in the upper and narrower part; probably each lobe has one pyrenoid as you have sketched.

In all specimens, except one, where I have been able to make an accurate count of the apical lobules, there were eight. In the one exception there were also eight lobules present, but there was an empty space for a ninth. Since you have seen two or three examples with nine lobules, it shows that the number may be either eight or nine, which will give friend Teiling "furieusement à penser".

Of course I want you to write the desmid up for publishing. In my first letter I asked if you would do me the honour of permitting my collaboration with you on all of the Brazilian material, of which I now have about 30 bottles, and except to get more from Dr. Sioli early next year. It is only natural for me to send this material to you and not to Prescott or Krieger, because many of the collections come from the same region as those you described in your 1945 paper, and contain many of the same elaborate desmids, and also a number of new ones. The thought of asking Krieger to write up the new desmid never entered my head; I sent it to him and to Prescott simply to get three independent opinions on my proposal for a new genus, and it was always my intention that

you should do it. I am not clear as to whether my name should appear with yours as joint author of a paper in which a new genus is named after me; probably it would not be desirable. What do you think?

I have plenty of material to keep Prescott busy for some years. There is Sachlan's Indonesian material, - very rich, which was sent to me with the understanding that Prescott and I should work it up jointly. My work on this is nearly finished, and probably would be completed if it had not been for all the excitement of the Brazil collections. Also I have just received a new lot of gatherings from North and Central Australia, which will form a natural sequence to the two previous papers on Australian algae by Prescott and myself. One of these papers, a short one on South Australian desmids, was published early this year by the Royal Society of S. Australia, but it was so horribly mistreated by the editor that I am paying to have it reprinted. Publication of the second paper (which you have seen) has been deferred again; perhaps it may appear next year.

It is, of course, perfectly satisfactory for you to discuss the new desmid with Dr. P. Kallio, and with any others whom you consider competent. The more opinions we can get on the physiological questions involved, the better it will be.

Many thanks for the information and sketches of the probable pore-structure of the Borneo Pleurotaenium. Your explanation seems quite logical, especially in view of the illustrations by Lütkenmüller, as copied in Krieger's monograph. (I do not have any of Lütkenmüller's works). Since K. asked for your opinion, I have copied part of your letter and have sent him your sketch, with a request that he return it to me. Also I told him of an experiment that I have made, which throws some additional light on the matter, and tends to confirm your suggestion. Like you, I have no possibility of getting microtome sections made, but I thought I could crush and break some of the cells, and thus get a view of the broken edge. So I placed a little of the material on a slide under a cover-glass, put a piece of paper on the cover-glass to prevent finger marks, and then rubbed the cover-glass backwards and forwards several times under heavy pressure. To my surprise few of the cells were broken, and those few were twisted apart, leaving a ragged edge that seemed to show a fibrous structure and prevented any clear view of the pores. The other cells and semicells were flattened completely, as proven by the fact that the pores were in uniform focus over the entire width of the cell, and also by the fact that it required only 5 or 6 microns movement of the fine adjustment to focus through the cell from the extreme upper to the extreme lower surface. The wall did not break even with this very severe deformation, but a new phenomenon appeared. At the margin of the flattened cell, there appeared at each pore a tiny rectangular projection, whose width was about equal to the apparent diameter of the pores, and whose height was about one-third of the thickness of the cell-wall (ca. 2 microns). The only explanation that suggests itself to me is that these projections represent plugs of hardened mucus that were extruded from the pores by the heavy pressure and the deformation of the cell wall. This fits in very well with your suggestions. In addition, if such a mucus plug possessed a slightly convex outer surface that projected slightly from the wall in the normal state, it would form a lens-like structure that could account for the alternation from light to dark with change of focus, and also might cause the illusion of projecting granules in an oblique view. If you repeat this experiment, which is quite easy, I should like to know what you see.

It would be helpful if you would give me a detailed account of your staining method using methyl violet followed by "washing" with pot. acetate. My amateurish staining is simply "hit-or-miss", and it usually misses when I most want it to succeed.

With my best regards, yours sincerely,

ROLF GRONBLAD
KARIS
FINLAND

Nov. 12th, 1953.

Dear Friend Scott,

Enclosed I send a first outline to a latin diagnosis and
and an English description. Don't you think we ought to have this
"Monstera miraculosa" published already within this year? But where?
What would you like? a) Commentat. Biol. Soc. Sci. Fenn. (this goes =
very slowly); b) Botaniska Notiser, printed in Lund, Sweden; this goes
faster, I think, and it has old good phycolog. traditions since the
times of Nordstedt, Borge & c; c) Hydrobiologia (paper not very good)
d) any American publication?

I hope you will correct and change the text quite as you wish.
It is for you to decide whether you wish to appear also as author.
Really you ought to have all honour of this new Desmid. There has
been published a paper by Insam & Krieger where a Cosmarium Insemi,
Insam & Krieger was published. (This was after Insam's death). To me

it looked a bit curious, so I wrote to Krieger about it. (But I cannot find his answer among his very many letters.) "Scottia mira Scott & Grönblad" - I don't know if it looks very nice? I shall ask my professional botanical friends. (You could do the same).

Of course I am most willing to cooperate with you as regards the Brazilian Algae. Try to forgive me that I do not always answer to your questions. I know there are more of them, but I have also made a list of questions which I am to answer. I would like to have something ready for print and I hope to have some "New and Interesting Desmids" ready for you - soon, I hope. (I have not at all opened my vials from the last summer's excursions).

Thank you again for the photo cassettes, which I got today. Also Mnthl. Scient. arrived. Many thanks. I have not yet experimented with the micr-camera but I have asked two friends: a amateur photographer, who is very skilled, and an optician in Helsingfors. I shall at my next visit to Åbo (=Turku) ask dr Kallio to show me the proper usage. You see, I am myself very clumsy and ignorant as photographer.

My "staining method" with "washing" is very simple. I use:

- a) Gentiana violet 0.2 "Anilinwater" 18 ccm Spiritus 2 ccm or
 - b) Methylviolet 0.1 Anilinwater 9.0 Spiritus 1.0
- these solutions I use without dilution, and I put with a glass staff or a pincet as much as needed to the edge of the coverglass, then I look into the microscope how the staining is going on, and when the cell-wall is sufficiently stained (1 minute or so) the solution of potassium acetate is put to the edge of the coverglass and a piece of blotting-paper to the opposite edge until all staining liquid

ROLF GRONBLAD
KARIS
FINLAND

has vanished - and the preparate is ready for use.

The staining liquids can be diluted if you wish slower staining. The Kal. acetate-solution is the "official" one. - When all staining liquid has vanished the cells appear very clearly and beautifully stained. Of course empty cells are best. I am going to send you the basic paper by Lütkemüller. (which you may return when you have studied it enough). Another small paper by Lütkem. is a doublet, which you may keep for yourself. (Cylindrocystis)

Today (14.XI) I got Krieger's letter. Everything is OK. I have misinterpreted his words. (but they were very obscure indeed.)

Now I am going to the post-office with this letter before the office is closed.

Kind regards,
yours,

Dec 5 1953

Dear friend Grönblad,

For the last two weeks I have been under the weather with some kind of gastric ailment, which at first I thought was a recurrence of my gall-bladder trouble, but which the doctor now says is a disturbance of the small intestine. At any rate I have been tired, listless, and disinclined to do any work. This is the reason I have not replied sooner to your letter of Nov. 12th with the tentative diagnosis of the new desmid.

Of the four journals that you mention, I think that *Botanika Notiser* is the best choice, with *Hydrobiologia* second. There is no chance of getting anything published in this country for at least a year, and more likely two years. But I think we should wait for further information that I have asked Dr. Sioli to give, e.g., the exact location of the Lago da Boca do Igarape Mental, with latitude and longitude, and a sketch map if possible. Also I have asked him to send you copies of two of his publications, which will give you some background information. Enclosed is a copy of his last letter of Nov. 4th, in which he says he is sending copies of two manuscripts that he has recently sent to *Archiv für Hydrobiologie*. These have not yet arrived, but this week I got from him a box containing 16 vials, 14 of them new, and the other two are replacements of one missing and one broken vial in his first shipment. The new lot is from the region near Belem, but unfortunately most of them have either no desmids at all, or very few. Some of them evidently came from salt or brackish water, at the mouth of the Rio Guama, where the city of Belem is situated, because their principal content is a large quantity of marine or brackish-water diatoms such as *Coccolodiscus*, *Aulacodiscus*, *Aptinaptichus*, *Triceratium favus*, and several other genera that I cannot name, along with *Surirella* and *Orthosira* which may or may not be fresh-water diatoms, depending on the species. But strange to say, with these salt-water diatoms there is one peculiar little 3-radiate *Staurastrum*, shaped something like *St. Cerastes* but probably not related to it, that I would swear must have come from an acid-water habitat; evidently it must have been carried down the river from some more favourable locality. Two or three of the vials contain a fair assortment of desmids, mostly well-known species. Evidently Sioli has not yet learned the knack of collecting desmids, not the "signs" that indicate favorable habitats. In his own work he seems to be interested mostly in zooplankton (like most limnologists), and I should judge that Hustedt has given him instructions for collecting diatoms, since many of his collections bear notations like this: "Von toten Blättern am Ufer abgesaugt"; "elag auf Sand und Blättern abgesaugt". I don't think this is a good way to get desmids!

I am going to send you portions of all his samples, about 46 up to date, but this will have to wait until after January 1st, because for the rest of this month I have much to do in preparation for my retirement from the firm of Ole K. Olsen Co., with which I have been connected for 44 years.

Your diagnosis of the new genus is very good, but there are a couple of small changes that I would like to suggest, and a few words to add:

1. You describe the cells as "rather large" ("mediocres"), but in my opinion they are very large. *Scottia* must be compared with *Staurastrum*, and I could name very few *Staurastra* that are larger.
2. "Cylindrical" seems hardly the correct word to describe the shape of the whole cell, which is certainly not cylindrical in the same sense as *Penium* or *Pleurotaenium*, for example. Don't you think "ovate" or "ovoid" would be better?

3. I suggest a slight change if your description of the processes, as follows:
"All processes are hollow, and are furnished with a few acute spines on the outer margin at the region of their greatest curvature, and all processes terminate in three slightly divergent spines, of which the inner one is twice as long as the other two".
4. "The shape of the chromosomes we have not been able to establish as to their details, but it seems that there are 6 (or 5) axile longitudinal "laminae" or "bundles", from which pairs of fine extensions go into the processes of the lower whorl, and single extensions into those of the upper whorl. The number of the pyrenoids is not known, but in vertical view there sometimes can be seen a single pyrenoid in each lamella".

From a reading of Sioli's papers I gather that the "Lago" da Boca do Igarape Mental may be a fairly large and semi-permanent pond, left behind on the river's flood-plain when the water-level is lowered during the dry season. He has photographs of such ponds in one of his works, and we have similar ones along the Mississippi River, and other rivers in southern USA.

In your comments on the new genus, I think it would be desirable to mention that I saw in one of my Florida collections, some years ago, a plant that I now believe must have been either identical with, or very close to, the Brazilian desmid. Only one specimen was seen, and it was so badly entangled and obscured by a mass of mucus and adherent debris that I was unable to make out its structure, and it was quite impossible to make a drawing. After working on it for an hour or so, trying to get it into a position where I could distinguish its structure, I finally had to abandon it, but I have a distinct recollection of the upwardly-directed and converging processes. No record was kept, and now I have no idea of what part of Florida it came from.

Of course I have asked Dr. Sioli to try to get more material from this habitat, and in a living condition if possible, and I suggested that if he is unable to revisit the spot, he may know of some other collector who might be travelling in that neighbourhood. But as you will note from his letter, he is trying to get away from Amazonia and hopes to get a position in southern Brazil. This is not very good news for us, but if he leaves the Inst. Agron. do Norte, I shall try to get another man, Dr. Paul Ledoux, to continue the collecting. He is a botanist with the Agronomical Institute, and perhaps may be able to get better desmid material than Sioli.

You will note from Sioli's letter that apparently he has been able to see your paper, *De Algis Brasiliensibus*, so if you have not already sent him a copy, it is not necessary to do so.

Are you planning to attend the 8th International Botanical Congress in Paris in July 1954? There is a very faint possibility that I might be able to obtain a grant from the National Science Foundation to partially defray the travel expenses.

Your staining and washing method looks very simple, but now I must find out what "aniline water" is. Perhaps it is described in the paper by Lütkenmüller which you are sending.

With my best regards,

Sincerely,

Rolf Grönblad

Karis

Centralgatan 86

Dec. 15th 1953.

Dear Friend Scott,

I was very sorry to hear about your illness and I hope earnestly that you will recover thoroughly and very soon. Good health is of fundamental importance.

I shall write to "Botaniska Notiser" and ask them if our *Scottia*-description could be published there very soon.

Last Sunday I was on a short trip to Abo (=Turku in Finnish) to see my friend dr Paavo Kallio. He is a rather skilled microphotographer and I had brought with me the apparatus you sent me. He had himself a Swiss camera of nearly the same construction. So we tried my apparatus taking pictures of *Scottia mira*. The result of our manipulations was not very good but not quite bad either. Now I know how to do, and there is needed only a lot of exercise and many experimentations. Also, I must have a new and much better light source, it is quite an essential thing. I see now that the microphotographing will take much time - but I will try. Did you take any microphotos of *Scottia mira*? If you have a good one, I think it would be useful to publish it together with the original description. I think it must be taken by a rather low magnification and then enlarged (because the processes are all in different plans).

To dr Sioli I have already sent a copy of *Desm. Brasilienses*. If, in any way, it would be possible to get living specimens for dr Kallio, who, as you know, cultivates *Micrasterias* and other *Desmids*, it would be very good. But the only possible way is that dr Sioli would catch from quite fresh material some healthy-looking cells by means of a capillary pipette and put them into sterile nutritive liquid (not more than some 10 cells in a flask of 100 ccm, with only about 20 ccm of liquid in it). The numerous zooplanktons and all kinds of bacteria and fungi are very injurious to the life of *Desmid* cells. In spite of all these difficulties it would be worth while to try to send living cells. But in order to save time of transport dr Sioli (or another collector) ought to send the living material directly by air mail to dr Kallio (Turun Yliopisto, Turku, Finland).

The flask must be carefully sealed by means of paraffine. I saw a flask with living *Micrasterias*-cells in dr Kallio's laboratory which had been kept sealed for nearly a year! - Well, I don't know if this plan can be realized, but if so, then it would be wonderful. (You have Kallio's papers on *Micrasterias*?)

Your 4 suggestions as to the completion of the description of *Scottia* are all good and useful.

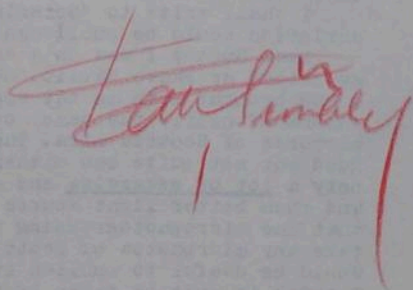
I have asked about the possibility of your being a joint author to *Scottia* - but, as I thought, it seems not advisable, it would not look quite "proper". If you wish to be the author, then a name such as "*Scottia*" cannot be used. Sorry.

I had not thought of attending the Congress in Paris 1954, and I have not tried to obtain a grant for that purpose. Now, I think, it will be too late (?). But if you could get a grant for a visit to Finland it would be wonderful and we should certainly do good work together here. Why couldn't you get such a grant, when Hannah Crossdale got it?

"Aniline water" is made in this way: 3% aniline oil (=aniline) is added to distilled water. This mixture is frequently (several times) shaken during 1-2 days. (This is from a botany handbook by F. Elfving). It is not quite necessary to use "aniline water" though it is better than ordinary distilled water.

Now I must finish my letter and wish you a cheery Christmas season and a HAPPY NEW DESMID YEAR.

With kindest regards,
yours,



ROLF GRONBLAD

KARIS
FINLAND

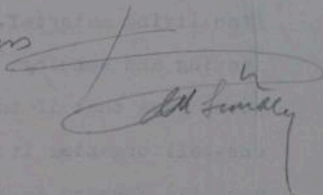
Dec. 31st 1953.

Dear Friend Scott,

When I returned from my Xmas trip I found a letter from "Botaniska Notiser" telling me that the *Scottia* description will be accepted to be printed. Of course they must see the manuscript before the definitive acceptance. And Dr Kallio is preparing a theoretical analysis of the polarity question which he finds very interesting. He finds our subject of such an importance that he says he would be ready to make a journey to Brazil in order to find living material. He is going to write to you about the collecting and sending, giving better instructions than I am able to do. He says that if the polarity question could be studied in a one-cell organism it would be of first class importance. So I hope you will forward Kallio's instructions to your collector in Brazil. We shall not wait till these studies are finished, but only as

long as you need for making your definitive drawings. If you have no other person to ink the figures, I am sure Hannah Croasdale will be glad to do that. (She is also capable of that). A front view, a end view from both ends, a ventral view (from the isthmus), and some different top coronas (with 8 and 9 lobules) and an optical section showing the structure of the chromatophores should be published. If you can get a fairly good microphotogram it would be also useful to publish. The final manuscript will be sent to you for acceptance.

Prosit Neujahr!

yours


Jan 4 1953

Dear friend Grönblad,

Today is the first of the long holiday that will last, I hope, for the rest of my life. Now I shall have plenty of time for my desmids, and many other things. One of the first things is to acknowledge your two letters of Dec. 15 and Dec. 31; the latter arrived this morning, with your very good sketches of "Scottia mira", and your note that the paper describing it can be published in Botaniska Notiser.

I am sorry to say that I had never heard of Dr. Paavo Kallio until you mentioned his name in a recent letter; I should be very glad to have his papers on Micrasterias, and also details of his methods for cultivating desmids, including the formula (or formulas) for the nutrient solution. It is surprising that he can keep Micrasterias alive in a sealed flask for nearly a year, but you do not say what kind of Micrasterias!!! Some species, e.g. *M. pinnatifida* and *M. radiata typica*, are probably easy to grow artificially, since they can tolerate a wide range of water chemistry, with pH up to 8.0. But other varieties of *M. radiata*, e.g. var. *dichotoma*, var. *alata*, var. *gracillima*, etc., and other species like *M. Torreyi*, *Johnsonii*, *floridense*, *arcuata*, *alata*, are found only in acid waters, and according to my reading, these soft-water desmids are much harder to cultivate than hard-water species. For "Scottia" a special formula might be required, buffered to a pH of 4.5, and flasks of Pyrex or similar resistant glass would have to be used, otherwise the acid water would dissolve too much calcium from a softer glass.

Today I have sent you by registered (ordinary) mail, two manuscripts by Dr. Sioli, mentioned in the copy of his letter which I sent you. Also enclosed in the package is the paper by Lütkenmüller which you so kindly loaned me. Thank you also for L.'s paper on *Cylindrocystis*, which I shall have to study very carefully to see if I can fix in my mind the obscure characteristics that separate it from *Penium* and *Cosmarium*.

You will find Sioli's papers very interesting, I am sure, and I would like you to pass them on to Dr. Kallio and Dr. Krieger, and others whom you think they would interest. Dr. Kallio will like especially the water analyses, particularly those of the Igarape Mental, and the rather scanty data on the Lago da Boca do Ig. Mental. These "Lagos", which occur on several of the tributary streams of the Rio Arapiuns (not Arapium as you spelled it in your Brazilian paper), turn out not to be flood-plain lakes or ponds, as I had imagined, but wide Lake-like enlargements of the mouths of the smaller streams where they join the larger river (Mündungsbucht). This is an unusual formation, and I have not been able to get a clear idea of the cause. Sioli comments on the relative abundance of the zooplankton in these streams, (principally Crustaceans and Rotifers), as compared with the very scanty phytoplankton, and surmises that the dearth of plant life may be due to the paucity of nutrient chemicals in the water and its high acidity, but this leaves unsolved the question of where the animals get their meals, which must, directly or indirectly, be microscopic plants. Of course we know that running water is not generally a favorable habitat for desmids, but I should have expected a larger quantity of desmids in the still waters of the Mündungsbucht. In one of his illustrations (Fig. 6 I believe), Sioli shows two belts of aquatic plants at the head of the Lago, where the stream enters it ("unterwasserräsen" and "submerse wasserpflanzenwälder"), the latter attaining a height of 0.5 m or more. If the water contains sufficient nutrients for these plants, then there is enough for desmids also, and these underwater plant-beds are where I should look for them, either by squeezing or stripping the plants.

Personally I don't like plankton collections, because you get such a small quantity of material, and frequently only a limited variety of desmids. I find I get better results from squeezings or strippings, depending upon the type of aquatic plants that are present.

If Dr. Kallio is really serious, his proposal to go to Brazil for the purpose of getting living specimens of "Scottia" is truly astonishing, and the biological significance of this curious plant must be much greater than I had thought. But such a trip seems to me to be about the only way, and certainly the surest way, of obtaining living plants preserved in such a way that they can be transported to Europe. It is true that Sioli is a skilled limnologist and knows a lot about water chemistry, so that he could probably make up the sterile nutrient solution, but it might be difficult or impossible to get the Pyrex flasks in Brazil. I have had to send micro. cover-glasses and chemicals to a friend in Uruguay, which simply could not be bought, even in a large city like Montevideo; and Sioli for a long time was unable to get corks for the bottles that he sent me! Further, the picking out of the individual desmids from the collected material, and transferring to a nutrient solution, requires a dexterity that can be acquired only after considerable practice, and I certainly would not expect to succeed at the first try, especially with a rather fragile cell like Scottia. I have had several of them break at the isthmus during manipulation.

Of course, I have written to Sioli, asking about the possibility of getting living specimens, and telling him of the importance that you and Prescott attach to the cultivation of such material in order to observe the phenomena of vegetative and perhaps sexual reproduction. But I have not heard from him since his letter of Nov. 4th, of which you have a copy. It appears that he is frequently away on collecting trips for several weeks, though I should have thought he would be home in Belem for Christmas. You will have noted that he wants to leave the Amazon region and is considering a position in southern Brazil, for which he can hardly be blamed. But even if he does leave Belem, there are several other botanists on the staff of the Instituto Agronomico do Norte, whose names you will find on the inside front cover of the Institute's publications, which I have asked Sioli to send you, and I am quite sure that Dr. Kallio would receive a cordial welcome from them, and all the assistance that they could give.

I am now working on the definitive pencil drawings, which will include the several views that you have suggested. When they are finished, I shall send them to Mrs. Perine in Memphis, Tenn., for inking, and I hope to be able to forward the finished drawings to you by the end of this month. At various times during the last couple of weeks I have been trying to find a specimen suitable for photographing, but all of those that I have seen have been disfigured by adherent mucus and debris.

Now for the big news. I have made up my mind to go to Paris for the Botanical Congress, whether or not I receive a grant. After the Congress I propose to visit Krieger, provided he can meet me in West Berlin; and then on to Finland for the great pleasure of meeting Rolf Grönblad in person, after so many years of correspondence. This would be in the latter part of July, and I could stay in Karis for two or three weeks. Then to Sweden to see our friend Teiling for a few days, and back to England where my wife will be waiting at my sisters' house in Devonshire.

WHAT DO YOU THINK OF THAT?

A Happy and Prosperous New Year is the wish from your friend,

3

ROLF GRÖNBLAD
KARIS
FINLAND

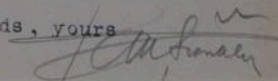
11.1.-54.

Dear good Friend,

Today I got your got your last letter with the surprising and very pleasant news. This is, indeed, BIG NEWS. I hope most earnestly that your intentions will be realized. You are welcome to Karis whenever you come. I shall try to arrange my holydays as far as possible, for the time of your visit. If you ^{are contented with} simple and not very comfortable living you can stay in my house but if you want more comfort you can stay in a nearby hotel and have your meals in my home. You have to see and then to decide.

I have also written to Dr Kallio and told him your communications. I am sure he will write to you very soon.

With kindest regards, yours



Jan 18 1954

Dear friend Grönblad,

Your letter of Jan 11th arrived yesterday, and I thank you very much for your invitation. It is too early to make any definite plans, but it appears that my visit with you would be probably during the last week in July and the first week or two in August. My friend Prescott hopes to be able to attend the Congress in Paris, and if he goes there I shall want to be governed somewhat by what he does.

Here are some of my first attempts at photographing Scottia. They are not good, and quite unsuitable for reproduction in your paper, but they have a certain interest for visual examination. I am going to make more pictures, but first I shall have to obtain a lower-power objective, 5x or 6x, because even with the 10x it is not possible to get more than two of the processes in focus at one time, on one semicell.

This morning I received a brief notice from Dr. Harald Sioli that he has left Belem, and has accepted a new position in southern Brazil, as he mentioned in his last letter. This undoubtedly accounts for the fact that he has not answered my last two letters. I am afraid that this means that we shall get no more collections from the Amazon region; however, I shall make an attempt to interest another botanist, Dr. Paul Ledoux, of the Instituto Agronomico do Norte in Belem, who made one of the collections that Sioli sent me. Further, Sioli may be willing to make some collections in his new location, after he is settled there. His new address is:

a/c Servico Especial da Saude Publica,
Caixa Postal 1191,
Belo Horizonte (Minas Gerais),
Brasil.

Belo Horizonte is about 200 km north of Rio de Janeiro, and if we are lucky this region may prove to be as favorable for desmids as Rio and Sao Paulo.

I am now in the process of making up samples for you of the 45 Brazilian collections, and shall send them about the end of this week. My drawings of Scottia are now in the hands of Mrs. Perine for inking, and I hope she will be able to do them promptly.

With my best regards,

Sincerely,

ROLF GRÖNBLAD

KARIS
FINLAND

Jan. 24th 1954.

Dear Friend Scott,

Here you have a "IInd edition" of the intended description of *Scottia mira*. I have put in your suggestions and there are a few other additions and changes. I beg you to criticize the new text. Do you wish to have the authorship in this way or would you rather like to have the whole paper signed "Grönblad & Kallio"? It is for you to decide. Because this is your alga and I feel myself as merely an intruder.

Have you succeeded in getting a microphotogram that could be used in the publication?

I have to tell you bad news : I have got an "ulcus ventricul'

which was verified by means of Roentgen photograms. I have not been quite well for a long time and this was to be suspected. However, it might have been something worse, and this illness may be cured in 3-4- weeks if all goes well. But for the future it is a memento.

Again, with my best wishes, yours

u
~~Leah~~
Leah

ROLF GRÖNBLAD
KARIS
FINLAND

Febr. 1st 1954.

Dear Friend Scott,

I am very sorry to find that ^{even} ~~also~~ my last diagnosis to Scottia was incomplete. I have altogether forgotten the vertical view! Thus, after the words "twice as long as the other two" must be added:

Vertical view circular, at the outer margin with 10 radiating processes which are denticulated at their ends; ^{x/} inside the margins with 6 similar processes and in the central area with 8 (-9) teeth arranged in a circle; this latter is lacking in the vertical view of the lower semicell. yours

L. Grönblad

x/ I think it is not necessary to describe the details of the arrangement of the spines in the vertical view?

Feb 6 1953

Dear friend Grönblad,

I am sorry to hear about your stomach ulcer. This is a very common ailment in this country, where the "highspeed living" is said to be a contributing cause. A friend of mine suffers from it periodically, especially when he is worried about his business, and he tells me that it is a very painful and annoying trouble. As you say, it can be cured by careful treatment and dieting in a few weeks, but there is always the possibility of it recurring. I hope that you will be soon over it, and that it will be a permanent cure.

The second edition of your description of *Scottia mira* is OK, though the description of the habitat will have to be changed in view of the information contained in the paper by Sioli that I sent you, and some additional data in a letter that I have just received from him. Enclosed is a sheet with a suggested description of the habitat, which you may incorporate in whole or in part. The authorship of the paper should be as you have shown, with Kallio's notes on cytology as a supplement.

Two days ago I took to the postoffice a package with the final drawings to send you, but the clerk refused to accept the package for registered mail because it was not sealed in accordance with their damfool regulations, so I had to bring it home to re-wrap it. But that night, while looking for good specimens to photograph, I found an individual in process of division (the first I have seen in something like 100 specimens), in which there is a young and partly developed lower semicell attached to the old upper semicell. This is so important that I was obliged to revise my diagrammatic representation of the supposed method of cell-division, which I have done and have sent the new drawing to Mrs. Perine for inking. I should get it back in a few days and will then forward both drawings to you. I also took some photos of the dividing cell, but have not developed them yet. I sealed the slide containing this specimen with melted vaseline, and have sent it to you by this airmail. You will no doubt show it to Dr. Kallio, but unfortunately nothing can be made out concerning the cell contents. When last seen, the specimen was inside the ink circle on the cover-glass, but since it is in water it is quite possible that it may move, especially because of the large air-bubble in the mount. The ink can easily be washed off the glass.

Just at this point in writing this letter, your airmail letter of Feb. 1st arrived, with the additional description of the vertical view. This is OK, and I agree that it is unnecessary to describe the details of the spination in vertical view.

Enclosed are some new photos, which are somewhat better than the first lot, though I have not yet succeeded in getting all the processes sharply focussed, and in fact I am not at all sure that this can be done. Nevertheless I think that two or three of them might be suitable for reproduction in your paper. I have taken 57 photos already, with many different combinations of lenses, and with blue, green, and red filters in an effort to get better contrast and sharper pictures. I even mounted one specimen in potassium-mercuric iodide, K_2HgI_4 , which has a refractive index of 1.65, and ought to give a greater depth of focus, but the improvement, if any, was not noticeable. Perhaps the last roll of film, not yet developed, will have some better pictures, and I hope the specimen showing cell-division will turn out well.

Also enclosed is a copy of Sioli's last letter, very interesting. But you will note that there is no possibility of his getting any more collections from the region of the Amazon river, though he proposes to get desmids from the places that he visits in the course of his new duties. Thus if Dr. Kallio wants living specimens in the future there is no other way than for him to go to Brazil and get them himself.

If he should make the trip, I am sure that Sioli would give any information that would help, also that the Instituto Agronomico would assist in any way possible, such as laboratory facilities in Belem and possibly Santarem. The map that Sioli sent me is a poor and very faint photographic copy of part of a navigational chart of the Amazon river, on which he has inserted in ink the names of the small rivers and towns. I shall try to get a photostat copy for you, but I doubt whether it can be copied again successfully. However, Sioli says that a good map of the region will appear in his paper to be published in Archiv für Hydrobiologie, perhaps this year, and you might refer to that.

I have sent you two boxes containing samples of all the Brazilian collections made by Sioli. They are numbered Brazil 1 to 32 inclusive, except Nos. 10, 11, and 20 which I sent you previously. Also Nos. 49 to 62 inclusive, from a different region. There are no collections with numbers between 32 and 49. You already have Sioli's notes concerning the habitats of Nos. 1-32, but I am not sure if I sent you his notes for Nos. 59-62. Please let me know this, and if necessary I will copy them. To fill out the second box I included four samples from Australia and Tasmania; these are simply for your pleasure and information, because Prescott and I are going to work them up. I hope you will permit me the honour of collaboration with you on all the Brazilian stuff, including such as we may get in the future. There is a tiny *Micrasterias* (sketch enclosed) that I would like to name *M. Ledouxii* (or *Ledouxi*?) after the collector, which may be a small inducement for him to continue collecting desmids, and we should watch for one or more new species to name for Sioli. There is a *Xanthidium* that I believe to be a new species and would be appropriate for this purpose.

Yesterday I sent you a reprint of a short paper by Prescott and myself on Desmids from South Australia, which is different from the paper that you saw in Ms form. The South Australian paper was originally published in 1952, without submission of any proof, and it was so full of printer's errors, and the plates had been reduced to such an absurdly small scale, that I paid to have it set and reprinted.

That's all for the present. With my best regards,

Sincerely,

P.S. Dr. Kallio has not written me, but has sent three of his papers on *Micrasterias*. I find them extremely interesting for two reasons; first because *Micrasterias* is my favourite desmid; and second because these papers are the first that I have seen where the author gives exact details of his culture experiments, and also gives the reasons for the formation of the various kinds of abnormal forms that develop as the results of experimental procedures. His establishment of a 3-radiate and even a 4-radiate clone of *M. Thomasiana* is a really extraordinary achievement.

I have sent him several of my last papers, and also a sketch of a very remarkable specimen of *M. pinnatifida* that I found a few years ago, in which an abnormally 4-radiate (teratological) semicell possessed two distinct isthmuses, and gave birth to twins, i.e., two normal semicells had developed from the two isthmuses.

Also I am writing to him, thanking him for the papers, and offering to send him preserved specimens of any of the many beautiful species of *Micrasterias* that I have from many parts of the world in addition to the USA. Perhaps at a later date I could send him living *Micrasterias*; I should think that *M. Torreya*, which is highly variable in nature, would be a suitable subject for experiment.

Explanation of the drawings of *Scottia mira*.

- Fig. 1. Front view.
2. Vertical view of upper semicell.
3. Vertical view of another upper semicell, with the lower whorl of processes omitted.
4. Basal view of lower semicell, seen from underneath.
5. Ventral view of lower semicell, seen from the isthmus; the lowest whorl of six processes is omitted.
6, 7, 8, 9. Vertical and oblique views of apical coronae, with 8 and 9 teeth.
10. Slightly tilted front view, showing a fully developed upper semicell with an attached partially developed lower semicell.
11, 12. Diagrams showing supposed sequence of cell-division.

Fig. 2 shows 8 teeth in the corona, with a space for a ninth tooth, which was not present.

Figs. 1 - 9 are drawn to a magnification of about 1000, and Fig. 10 about 850. All of them are from different specimens.

The large plate is designed for a reduction of 2.6/1, which will make the magnification when printed about 385, but this will have to be checked with the proof of the printer's plate.

Habitat: Lago da Boca do Igarapé Mentai; approximate location $2^{\circ} 38' S. lat.$, $55^{\circ} 34' W. long.$ In a straight line it is about 85 km from Santarém, in the direction $W 15^{\circ} S$, or a course of 255° . 'Igarapé' is a native (Tupi Indian) word meaning literally 'boat-way', which is used, only in this part of lower Amazonia, to designate any stream, even some which would be considered good-sized rivers in Europe. The Ig. Mentai is a tributary, which flows into the south bank of the Rio Arapiuns a short distance ~~below~~ below the confluence of the Rio Maré and Rio Arua, whose junction forms the Rio Arapiuns. The latter in turn flows into the Rio Tapajós a short distance above its junction with the Amazon River.

The 'Lago da Boca' of the Ig. Mentai is not a true lake, but a lake-like widening and deepening of the stream bed just at the point where it joins the Rio Arapiuns. This phenomenon ('Fluss-See' or 'Mündungsbucht') occurs on many of the small and large rivers in this region; its cause is not known, but may be due to a local sinking of the earth's crust. In the case of the Ig. Mentai, the 'Lago' is about 2 km. long, 500 m. wide, and 5.50 m. deep in the dry season; in the rainy season it is somewhat deeper but not much larger, because of the fairly steep banks. The stream itself, just above the enlargement, is 10-15 m. wide and about 1 m. deep, with a flow of perhaps 1 m./second. The stream discharge, therefore, is so much smaller than the volume of water in the lake that there is no appreciable current in the latter. Perhaps for this reason, the lake plankton, principally zooplankton, is much more abundant than that of the stream. The lake water is brownish in colour, and has a pH of 4.5; this rather high acidity is a characteristic of most of the rivers in the region.

This information has been derived from letters written by Dr. Gioli, and from the manuscript of a paper by him, entitled _____, which is to be published in Archiv für Hydrobiologie, and also (in Portuguese) in the Boletim Técnico do Instituto Agronômico do Norte, Belém.

Karis, Febr. 7th 1954

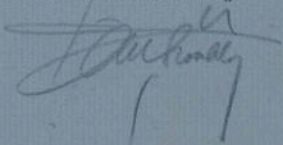
Dear Friend Scott,

With this I only wish to acknowledge the receipt of Dr. H. Sidi's
manuscripts. I already had a look at them and I found them very
interesting. I shall return the manuscripts to you very soon...

These were not sent to Luther after
all, but pertain to the Brazuel
drawings of Sidi + Foster

Dr Sioli or any other collector may be able to get still quite different material with still more surprizes for us. Only the way of collecting must be explained. This you have perhaps already done? Scottia is ready for being sent to Botaniska Notiser. as soon as you send the drawings.

Yours faithfully



Dear Erlend Scott,

Karis, Febr. 7th 1954.

With this I only wish to acknowledge the receipt of Dr H.Sioli's manuscripts. I already had a look at them and I found them very interesting. I shall return the manuscripts to you very soon. - It seems to me that the locality where Scottia was found is "not very far off". At least it may be possible to get more (and also living material). When you compare the list of species in my Brazilian paper with the lists made by Borge from other parts of Brazil, you will find them very different. I think

Li-thami-1000 paper received!

Febr. 12. 1954.

Dear Friend Scott,

Today I received your letter with enclosed papers. Thank you.

I shall write an answer immediately.

1) The plates are OK, but your original drawings look much more natural and beautiful (as do always pencil drawings). However, there is nothing to do, since pencil drawings cannot be reproduced without very heavy costs.

2). Your microphotos are quite good. I think it is not possible to get better ones. I shall take the best of them for the printer. In Botan. Notiser they do not take special plates for the drawings but the figures are printed "in the text".

3). Don't you think it possible that some other person at Santarem or Belem would be willing to gather living *Scottia* for Kallio?

I shall tell Dr Kallio all information concerning living *Scottia*. I am sure he will be very grateful if, sometimes later on, you could send him living *Micrasterias* cells. I am glad to know that you were interested in Kallio's *Micrasterias* cultures.

5) Dr Sioli's Nos 49-62. I have the list of them.

6) The small *Micrasterias* certainly is new. (*M. Ledouxii*). The *Xanthidium* I think I have seen in the *Scottia* sample (?). The 4 pyrenoids are very remarkable. But can you make out the structure of the chloroplasts? I shall also try to look at this species.

7) Thank you for Dr Sioli's letter. I have already read his descriptions of the ecological conditions in Amazonas. Very interesting.

8) A lucky strike that you found *Scottia* with cell division.

Karis, Finland Febr. 27th 1954.

Dear Friend Scott,

I wish to tell you that the typescript of the Scottia paper has been sent to the editor of Botaniska Notiser. I don't know if it will be accepted or not, but I hope it will be. Dr Kallio's "appendix" was also ready. When the first proof sheet is printed I will send one reprint to you so that you may be able to make some corrections if needed. We will hope the best for our first joint product.

The new drawings showing the c [redacted] will be sent to Bot.Not. as soon as I have got [redacted].

Then I shall return to your co [redacted] in USA and try to get the Artarodesmus finished.

With kind regards

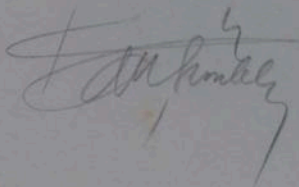
ROLF GRÖNBLAD
KARIS
FINLAND

11.III.-54.

Dear Friend Scott,

Today I received your new drawings ^{of} for *Scottia* cell division and they are already forwarded to Botaniska Notiser, Lund. I had also a letter from the editor who told me that the paper will be accepted for printing in Bot. Not. So it seems that everything is OK. I got also with the same mail the small box with vials. Thank you again for good cooperation.

With kind regards,



April 3 1954

Dear friend Grönblad,

Before I forget it again, I wish to ask you to order for me 75 reprints of your Scottia paper, so that I may send them to a number of algologists in this and other countries who are probably not on your list of correspondents. If you will have the printer send them direct to me together with the invoice, I shall send him a remittance immediately.

Next, I must thank you for the very pretty postage stamps, both on the envelopes and on the separate sheet that was enclosed in one of your letters. I am sure that my friend Sachlan, in Java, will be much pleased with them. By a curious coincidence, I had to interrupt the writing of this letter for 15 minutes, to take a squint at the contents of a new tube of stuff from southern Borneo, sent by Sachlan, which the postman just brought. It is a very nice collection with a lot of desmids, including such exotics as *M. radians*, *M. ceylanica*, *E. coralloides* var., *E. spinulosum* var. *burmense*, *E. flammum* var., *E. substellatum*, *St. javanicum* var. *apiculiferum*, *X. subtrilobum*, *X. acanthophorum*, and many others that I cannot identify offhand. I shall send you some of it.

Also I must thank you for the maps of Finland and the region around Karis. I like geography, and often study maps just for the sake of learning something about far-off countries, especially those from which I have received algal material. Your maps will be very helpful, and I am especially glad to know the locations of some of the places mentioned in your plates, Työrinne for instance, which are too small to show on the large ~~xxxx~~ maps that I possess.

Dr. Kallio was kind enough to send me four tubes containing living specimens of his cultured *Micrasterias* *Thomasiana* and var. *notata*. Two of the tubes were broken, but from the remaining two I got some excellent specimens of his very remarkable tri-radiate clone, and also of the haploid and diploid specimens of the variety. Unfortunately I have no knowledge of cytogenetics, so I cannot appreciate the significance of 'haploid' and 'diploid'.

About ten days ago I made a special trip out in the country to get some *Micrasterias* for Kallio. I thought it would be easy, but the results were not too good. One of my favourite ponds, which always contains an abundance of *Micrasterias*, was inaccessible because of muddy roads, and though I got some nice collections from other places, the number of *Micrasterias* cells was small, though there were ~~xxx~~ a total of eight species and varieties. So I sent Kallio three tubes as samples, to see if my procedure is OK, whether the desmids reach him alive, and if he can successfully place them in culture. After hearing from him on these points I shall get more stuff for him, but I shall have to go further afield for it.

Yesterday I visited two steamship offices to get some definite information about my trip to Europe, but the result of the conversations was so disappointing and uncertain that I have just about made up my mind to fly instead of going by steamer. I shall make up my mind next week, and then I can tell you when I expect to be in Finland. My plans are somewhat flexible, and I can adjust the time of my visit to suit your convenience. So please tell me if the last week in July and the first week in August, or the first two weeks in August, would suit you, or if some earlier or later time would be more convenient for you.

The question of the Finnish climate is bothering me a little; I do not know what to expect. I have not lived in a cold climate for nearly fifty years, and all my clothes are suited to this subtropical climate. In the photographs that Miss Croasdals took in Finland, which she loaned to me, the people are wearing heavy clothers, with overcoats in many cases, so I know that I shall have to buy some heavier things. It would help if you would tell me the daily maximum and minimum temperatures that can be expected in your part of Finland during July and August.

Enclosed are a few sketches of some Brazilian desoids, and I have sent you more under separate cover by ocean mail. These are only a few of the more striking species; probably you have seen them already.

That's all for the present. With my best regards,

Sincerely yours,

April 5 1953

Dear friend Grönblad,

Sorry to learn that one of my boxes was split open and four of the vials broken. I have sent you replacements for the four.

The box must have received very rough treatment, and anything can be broken if sufficient force is used, but this is the first time it has happened to me. Please tell me if this box was one of the new type, in which I used a glued and interlocking joint at the corners. I changed to this joint because I thought it would be stronger than the old nailed joint, but if it is no stronger, or even not as strong, I shall discontinue using it, because it is considerably more trouble to make.

I did not know that Kossinskaja is a woman. I have ^{only} one of her papers in which she shows some American and sub-tropical desmids, particularly *Micrasterias*, that it seems surprising to find in Russia. If you will give me her complete address I shall gladly send her reprints of the papers by Prescott & Scott, if you think it would be advisable, or perhaps I could send them to you and let you forward them to her. The Akademii Nauk SSSR, both in Leningrad and Moscow, receives several copies of the *Trans. Am. Microsc. Soc.*, so no doubt they subscribe to other American journals also, and Kossinskaja could see them if she wanted to. On the other hand she has a long list of Russian publications that I, and no doubt other American workers, have never heard of, and which would be of very little use even if we could get them because of the language difficulty.

You seem to be correct about the *Closterium*-like desmid in Brazil 20 and 21. I had overlooked the almost imperceptible constriction at the center which together with the apical teeth would place it in *Pleurotaenium*. It does show some resemblance to *Pl. tridentulum*, and perhaps should be classed as a new variety of that species. How did you make that very long drawing of it? Not directly with the camera lucida, I am sure; I suppose it is a freehand sketch enlarged from the original one.

Yes, the Amazonian collections contain an entirely different assortment of desmids from those shown in Borge's papers, which were collected mostly in southern and southeastern Brazil. To me the most surprising feature is the abundance of long-rayed *Staurastras*, many of them highly elaborated, and such as are evidently adapted to a planktonic existence; I wonder what we should see if we had some squeezings from the Lago da Boca do Igarapé Mental; I find that squeezings give a much larger assortment of species and genera than plankton collections. *Micrasterias* is rather rare, and *Cosmasium* notably so, considering the size of this genus. Maybe we shall get some more from Amazonia one of these days.

Sincerely yours,

P.S. Can you tell me if Dr. Krieger speaks English? I am considering going to see him on my way to Finland, but we should be unable to communicate with each other if he does not speak English, for I neither speak nor understand spoken German.

Rolf Grönblad

(1)
April 12th 1954.

Karis
Centralgatan 86

Dear Friend Scott,

I have not any news from Botaniska Notiser. But I shall order for you 75 reprints of the Scottia paper.

The time of your visit to Finland you can choose whenever you like. Only it would be good for me to know as soon as the date is settled. I should wish to take my holidays at the same time so that we could use our time as far as possible together studying Desmids, especially your collections and drawings from the Southern USA. (Perhaps you wish to visit Tvärminne Zool. Station, Dr Kallio, &c and perhaps you would like to make some excursions in the surroundings?). I should recommend July as the very best time. June is usually a little too cold, the first weeks of August may be quite suitable but the days are beginning to shorten. And when you once come to the North you ought to see our nights without darkness. If you are coming in July (resp. the first week in Aug.) you need not bring with you any "heavy clothes". A raincoat or a light overcoat is naturally needed. The following data I have found out for you from a Finnish geography book. : The average temperature in S. Finland is (for the time in question) 15-17°C. (centigrades), the highest temp. 32-36. Our summer, ~~is~~ especially July is rather warm and dry, but you can never tell beforehand what the summer is going to be like. Our climate is rather inconstant and variable, depending to a great extent on the direction of wind. Usually we have SE winds faintly blowing and very little rain in July, but there have been years with rain almost every day also in July. This is, however, very uncommon. If you need some clothes for excursions you can borrow something from me or you can buy here. The sea water is usually 20°C or more, in lakes up to 24°C. There are cloudy days in July from 4 to 10 in S. Finland. The amount of rainfall for a year is 620 mm. In Helsingfors (the Finnish speaking people say Helsinki, but I belong to the Swedish speaking minority) - the rainfall ~~is~~ in June, July, Aug. is 51, 59, 83 mm respectively. If you have not very bad luck I think you who are accustomed to subtropical climate, will enjoy our short but rather pleasant summertime. (Miss Croasdale was here last summer rather late in September: that is not summer, but fall. - If you are content to live without luxuries in my bachelor's house which is, indeed, very simple and lacking of modern commodities, you are welcome to do so, and it will save you money for better things than hotel bills. But if you wish to have a room in our hotel (which is not much to boast of) you can get such a one and have your meals in my home. But that we may settle when you arrive here. At any rate, we will have much to do and I hope you will reserve sufficient time, so that we can go through all your drawings and have animated discussions and conversations about all the critical topics.

You are to tell me what you would like to see in Finland besides Desmids in Karis and I will try to arrange what I can for you. I hope much of our meeting and I wish you heartily welcome to Finland.

Then I have to answer to your questions:

- 1). *Euastrum cuspidatum* Wolle you have certainly identified quite correctly. *Xanth. trilobum* is much larger in size: long 81, lat. 77, crass 42u (sec. Nordstedt 1869) Also rather different in shape.
- 2). *Euastrum subtile* Borge is OK. I should not like to make it a variety of *E. cuspidatum*. I think they are quite sufficiently different. The open sinus may cause a "forma" name. Of course such matters (are questions) of taste only.
- 3). *E. evolutum* seems to be capable of almost unlimited variation. That would be something for friends K&I to investigate.

Temperature
is given in
Centigrades
or Celsius°

0°C = 32°F
20 = 68
30 = 86

$\frac{9 \times 8}{5} + 32 = F$

- 4). Xanthid. antilop. v. Nordstedtii Gbl. Of course this is not much like X. antilopaeanum. Only, I did not wish to create a new species. I think X. antilop. is artificial and should be cleared up as to its specific constituents. But that is not easily made. - If a "new" spec. were to be made the name "Nordstedtii" is not advisable because there is already a X. Nordstedtianum Reinsch. (The Int. Nom. Rules admit both -ii and -ianum to be used at the same time - but if that can be avoided, so much better).
- 5.) Euastrum cosmariiforme sp. nov. (Note: there is a E. cosmarioides West published). Seems very much like Eu. politum Krieg. (52:2-3). A new species must never be founded upon one (semi)cell only. Or may not be.
- 6.) Euastr. spec. nov. is very funny. Certainly new, I think.
- 7). Staur. wolleanum "var. brasiliense". If the only difference is the 7-angularity it should not be given a rank of variety. Only "forma", or if we accept Teiling's proposal "facies" neptagonum (Greek) or sept(em)angulare (Latin). ((There is a Gentiana septemfida but perhaps septangulare would be less clumsy than "septemangulare")).

Answer to your letter 12.IV.-54.

- 8). Elfving's drawings of X. superbum and Eu. pingue are incomplete as to the details. In Finnish specimens of X superbum there is always a thickened central area with peculiar scrobiculations (cf. Grönb. in Bot. Not. 1938 p.53, fig. 6a, c, d - - I hope you have got a copy?)
- 9). Elfving has apparently not seen empty cells of E. pingue, else he would have mentioned the peculiar scrobiculations. Specimens from Finland, as far as I have seen them, always have these "stigmata" asymmetrically arranged. I think there are really no forms without pits. Curiously enough West Mng. does not mention the lacking of pits in Elfving's drawings.
- 10). Staur. subtrifurcatum. - S. Wildemani is an obscure question. I have met with something very much like S Wildemani in Finland (publ. in Finl. Desm. 1921 p 80-81 and pl. III, fig. 1-3 & 4-5). I enclose two never published drawings from Finnish specimens found 1927. -

Literature: S. Wildemani Gutw., Alg. Java (1899) XL:61; S. subtrifurcatum f. bidens Schmidle, Alg. Nyassa (1902) pag. 73, pl. II:9; - West, Alg. Burma XVI:15 and pag. 215 with discussion of related species; - Schmidle, O. Afrika Desm. (1898) III: 17-18; - Fritch & Rich (1937) p. 212-213, fig. 26A 26B and p. 213 (discussion.) they suppose S. Wildemani to be synonymous with S. subtrifurcatum f. bidens Schmidle), - Krieger (1932) XIX:12 supposes S. Wildemani to be a form of S. subtrifurcatum; - S. subtrifurcatum f. major Schmidle 1902 (= West in Journ of Bot. 1900) is very much like your drawings.

As a result of a comparison of the different opinions I should think that St. subtrifurcatum West 1896 is not = S. subtrifurcatum Schmidle 1898 nor = S. subtrifurcatum f. bidens Schmidle 1902. - West's species has a vertice concave = retuse sides, not rounded angles as Schmidle's species. Schmidle's species & also his f. bidens belong together and are variants of the same species as Gutwinski called S. Wildemani. Since there cannot be two different S. subtrifurcatum I think it is name should be retained for West's species 1896. Gutwinski's name should be used for his S. Wildemani 1902 which is = S. subtrifurcatum f. bidens Schmidle.

Your fig. 37 would be S. Wildemani v. horizontale,
 " " 63 " " " " var. unispinum (or monacanthum in Greek)
 " " 49-50 " " " " var. subtrifurcatum (Schmidle) Scott not West.

Your new variety names should be established only if you have found cells of all of them without dichotypic characters, I mean pure, normal cells. At any rate you ought to publish them all. And you have to decide for yourself. I don't know if you will have much help from my speculations.

ROLF GRONBLAD
KARIS
FINLAND

Answer to your letter Apr. 5th, which just arrived.

- 11). The broken box was of the old type with nailed joint. Indeed, it must have been very roughly treated.
- 12). I don't know Kossinekaja's address any more and I have no communications with any Russian authors since the Finn-Russian wars. I believe private scientists in Soviet are not allowed to free correspondence with foreigners. If you send your papers I think they are gladly received, but you will get no answer. Nobody in Finland whom I know has a correspondence with Soviet people. Our University library receives sometimes but very irregularly Russian scient. papers. That is all I know.
- 13). The *Pleurotaenium* (*tridentatum* var.? or nov.sp.?) and other very long Desmidiids I draw in 2 or 3 pieces, but always with a camera lucida.
- 14). I really don't know if dr Krieger speaks English. You should ask him. But perhaps French? Do write and ask him.
- 15). As you see I also have bothered myself with "*Gymnozyga armata*" and "*Desmidium curvatum*". But I should like to put them in a separate genus because they are so very different from other species of *Bambusina* resp. *Desmidium*. What do you think of accepting Salisbury's name "*Mateola*"? Sometimes I believe I have seen very delicate striae in the apical part (quite as in *Bambusina*), but I am not sure. Have you noticed anything like that? - Of course the spines or the projections of these two species do not make any absolute obstacle for uniting them either with *Bambusina* or with *Desmidium*. (We have in *Pleurotaenium* species such as *P. Kayei* and *P. spinulosum* which ~~are~~ bear very little resemblance to *P. minutum* and *P. trabecula*.)

16). Shall I return your pencil drawings such as this "Gymnozyga armata" and others. And Sioli's descriptions of Amazonia? Please tell me what you wish to have back. Indeed, such things that you do not need just now, you can bring with you when you are going home from Karis, don't you?

This is a rather long and perhaps confused letter. I hope you will understand what I am meaning.

With kind regards, yours sincerely,

Rolf Grönblad

Karis

Centralgatan 86

16.3.-54.

Dear Friend Scott,

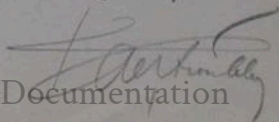
Thank you very much for your letter of 9.III. with the pencil drawings from Brazilian Desmids. I have not yet opened the box that you sent me because I have not been quite well. (My ulcer is much better. Indeed, there^{is} nothing more of the ulcer to be seen in the Roentgenphoto. But I have caught the worst of influenzas, and that is something very bad. I am always quite down. But also this illness is much better : today I shall open the box and have a first look at the new Desmids.)

The Russian book by Kossinskaja I have. I gave a copy also to Dr Croasdale when she was here. Some time after that I wrote for another copy which I should have sent to you. But it was out of print I was then very sorry, but now I am glad to hear that you have got the book. Kossinskaja has published some good papers on arctic Desmids which she sent to me. But presently we have no connexions with Russian scientists. (If you look at the list of literature in Kossinskajas book you surely will notice that the list is very incomplete. Apparently Miss (or Mrs?) Kossinskaja does not know what has been published in other parts of the world. USA is the worst place and the root of all evil in the eyes of Russian people.

Miss Croasdale sent me a negative lens, such as you use in making your pencil drawings. Today I have got it fixed to a stative with a long arm in the same way as you have it. I think it will be a little difficult to draw by means of it till I get accustomed.

When I hear definitely about Scottia from Bot. Notiser I shall write to you.

With kind regards,
yours,



P.S.

When I opened your box there were 4 vials quite crushed and empty. I don't know what they have done with the box. It seemed as if every vial had been opened because the covers were all a little loose. The box itself was a little broken: one corner was opened to a fissure. (Can you understand my description?) I cannot at all imagine what has happened to the box. They must have dashed and smashed it very carelessly. The crushed vials were Nrs 2, 15, 22, 25. It has never happened before. Very sorry!

R.G.

P. S. II) Enclosed I send
the drawings I made later this
evening. I was lucky to find
an empty cubicle of the
Yantheidium. It looks very peculiar.

The Clostridium-like form I think really is a *Mucrotaxium* not very unlike *P. tridactylum*^{small}

Dear Friend Scott,

When I sent to you my pencil drawings of Sioli's
Desmids I forgot to write that you need not return them
by air mail. You can take them with you when you come
to Finland.

20.IV.-54.

Sincerely yours
H. Ahmayer

May 19 1954

Dear friend Grönbld,

More than five weeks ago I wrote our friend Telling, telling him of the proposed date of my visit to him, and asked him to let me know whether it would be more convenient for me to visit him before or after going to Finland, since I could arrange it either way. But he has not replied. Here is my schedule:

Leave New Orleans July 5, and arrive in London July 7. Stay in London 1 week. Arrive in Paignton, Devonshire, July 15, and stay there 10 days. Leave London July 25 or 26, and arrive in Stockholm same day, and to Linköping either the same or following day, depending on what time the plane arrives. Spend 2 or 3 days with Telling. Leave Stockholm July 30 or 31, and arrive Helsingfors same day, and to Karis the same day or the following. Spend 10 to 14 days in Karis, and then return to London. My wife will stay in Devonshire with my sisters. Leave London Sept 8th for the return home.

My address in England will be:

1/2 Mrs. E. W. Nixon,
"Greenhow", Roundham Gardens,
Paignton, S. Devon., England.

If you should happen to get from Botaniska Notiser the proof of the Scottia paper, and if you want me to read it, you can send it to this address at any time between say July 1 and July 25, or between Aug 15 and Aug 31.

The information you gave me about the weather in Finland is very helpful; I am surprised that you sometimes have temperatures as high as high as 32-36°, which is what we get continuously here for 4 months, ~~July, Aug. & September~~ June, July, Aug. & September. But it indicates that my regular clothing ought to be suitable, providing the weather is not too unusual, and this will help on the return trip, for we are going home by way of Paris, Nice, Lisbon, Azores, Bermuda, Bahamas, Miami, and New Orleans, and we shall run into some hot weather in some of these places.

I should prefer to stay at the hotel in Karis, which Miss Croasdale told me is quite comfortable, and take my meals with you as you have so kindly offered. This will be less strain on your hospitality, and less work for Aili. Miss Croasdale said that I ought to try to learn a few words of Finnish, but I have not been able to find a phrase-book nor grammar.

Of course I should like to visit Tvärminne, and also to see Dr. Kallio, who, by the way, has never acknowledged the 3 tubes of living material that I sent. Aside from these, I do not know of anything special that I want to do or see in Finland. Perhaps we might see the President of the Soc. Sci. Fennica, and find out if there is any better prospect now of publishing the USA desmids in the "Acta".

I have written Dr. Krieger, but have no answer yet. It appears that I shall have to abandon the idea of going to Hohen Neuendorf, for if I even mention East Germany on my passport application it is likely to result in my not getting any passport at all, even for the other countries. He might be able to meet me in West Berlin, but then I should not see his laboratory and library, nor see his methods of working. I thought Hohen Neuendorf was a suburb of Berlin, but it appears to be

30 to 50 km north of the city, and that puts the matter in a different light.

Your suggestion of putting *Gymnozyga armata* and *Desmidiium curvatum* in a separate genus is something that had not occurred to me. There are good reasons for making such a change, but do you think it would be generally accepted? In addition to *P. Kayei* and *P. spinulosum* there is *P. australianum* (*Docidius australianum* Borge), which Krieger mistakenly transferred to *Triploceras*. Also there is the controversial case of *Closterium cuspidatum* Bail. If you did make the change that you suggest, I suppose that Salisbury's name *Mateola* would have to be used, on priority grounds. Do you know if *Mateola* has any meaning, or is it just a fanciful name?

The enclosed notes on *Euastrum cuspidatum* summarize my thoughts on this new desmid from Brazil. It is certainly not the same as the *E. cuspidatum* from southern USA and Newfoundland; in fact new species have been founded on smaller differences.

Like you, I think I have seen faint indications of striae on the end section (the everted part) of *Gym. armata*. In *Bambusina* the striae are really very small longitudinal corrugations, which I think are caused by a circumferential compression of the wall while it is being squeezed through the internal tube during the process of eversion. You can see this in an end view of good specimens showing the striae plainly in front view.

From the Brazilian drawings that you made, I see that you have found some desmids that I have not yet run across. *Desmidiium curvatum* is one of them. This did not appear in the fifty or so mounts that I made from the collection while I was photographing Scottia. The only examples of this plant that I have ever seen are a few empty semicells from one of my Florida gatherings; one of my sketches shows the infolded wall during the formation of the new semicell, quite similar to *Bambusina*. I shall keep your drawings and bring them with me to Finland.

From time to time I look at the Brazilian material and make some sketches. One of them is enclosed, showing the very pretty *Staurastrum Marthae*, of which I have seen only one specimen so far.

That's all for the present. With my best regards,

Sincerely yours,

P.S. You do not need to return any of the Brazilian drawings that I sent, for I have duplicates of them. Perhaps it would be better for you to keep Siol's manuscripts and I can get them from you when I come to Karis.

May 24 1954

Dear friend Grönblad,

Enclosed is a letter from Dr. Krieger, just received this morning, which I ask that you return after perusal. His suggestion that he should meet both of us in Katiss appears to me to be a really excellent one, and I hope that you will urge him to try to obtain the Auslandspass. I am writing him today by airmail that I think such a meeting would be delightful, and also profitable from a demidological point of view. In order to help I am suggesting that he allow me to pay part, or perhaps all, of his travel expense, which should not be very great. I do not know exactly what danger he refers to, from leaving his house uncared for, whether robbery or something else, but perhaps he could arrange with someone to stay there during his absence. At any rate, I sincerely hope that this meeting can be arranged.

In my letter of the 19th I forgot to thank you for the information concerning my dichotomous specimens of *St. Wildemani* and *St. subtrifurcatum* var. *major*. I have all the references that you cite, but I thought that perhaps you might know of others. The two drawings of your own evidently belong to the same group, and I wonder if you would permit me to publish them in the paper that I propose to write, with acknowledgement of course. I shall bring these drawings to Paris with your others.

No doubt you will soon receive a paper by Irene-Marie, apparently the first of another series treating of the desmids of the Trois-Rivieres region. I have done no more than read it through, but this reading revealed an astonishing number of errors in spelling, even more than in his last paper. These are not printer's errors, for there are only a couple of misspelled words in the French text; nearly all of them are in the names of authors. On pages 8 and 12 he criticizes an author in a manner which is not only unnecessary but in extremely bad taste; on top of that he misspells the author's name, Hughes, as Hugues in a dozen places, in fact I do not think he has once spelled it correctly. On page 26 there are the following errors:

juncidium for juncidum
A. Cushman for J. A. Cushman
L. O. Borge for O. Borge
Grönblad for Grönblad
Croasdale for Croasdale
Hughes for Hugues
Kützingii for Kuetzingii, and on page 27 Kützingii

all the way through the paper he has omitted the initial from your name and from Kützing's; sometimes he used the initial in (Müll.), more often he omits it.

a a of

Sombody ought to tell him, in friendly way, about these errors, which he must surely be unconscious. I considered doing this, but decided against it because I know him only by correspondence, and because of his irascible disposition he might easily take offense at it. Do you happen to know anyone who is acquainted with him personally?

I await with impatience your reply as to Krieger's proposed visit.

Sincerely

ROLF GRÖNBLAD

KARIS
FINLAND

May 29, 1954.

Dear Friend Scott,

Today I got your letter with the suggestion that we should try to take Dr Krieger to our meeting in Karis. I approve of your proposal very much indeed, and I have accordingly written a letter to Krieger with an invitation and a promise that he will have no costs to pay in Finland. If you pay for his travelling, then I may pay for his stay. I think it would be splendid and I think we all should profit from such a meeting.

My drawings of *Staur. Wildemani* are at your disposal, and if you wish to publish them you may do so. This species is very rare in Finland and, indeed, I have not seen it since those drawings were made. (Do you know that it was also published in "Desm. Keuru"?)

I have not yet got any new paper by Irene-Marie. It is not nice to criticize other authors in a bad manner. Of course the paper

Irene Marie's paper arrived today!

A printer's proof of Scatterra miss first arrived. I shall ask them to send one proof by air mail to you. Only absolutely necessary changes or corrections may be proposed. Please return the proof to Botanisches Land. And write it up. Top is for 1/20/37.

^{a/} by Hughes is not very good one. But nevertheless, critic should be polite always. I have once ("A critical review... 1931") said things about an author (H. Bachmann, Beitr. Alg. W. Greenlands) that were not nice and I know that the author became very angry. But I was rather young and unexperienced and some body ought to have warned me. Well, it is long ago and will not be repeated. - Also I know I.-M. only very little and would rather not try to put him in order. Prof. Brunel was once I.-M.'s teacher, but I don't know how he would take remarks upon his former pupil. Printer's errors and misspelled names are very irritating, nota bene if they are frequent.

Dr Kallio told me that your Micrasterias grow well, so I think he is glad to have got them. In my opinion pure cultures surely will be of very great importance when once the limits of species and varieties are to be scientifically drawn up.

Kind regards, sincerely,

(Enclosed Klinger's letter)

Copy
ROLF GRÖNBLAD
KARIS
FINLAND

30.5.-54.

Lieber Dr Krieger,

Unser Freund Mr Arthur M. Scott hat mir geschrieben dass Sie möglicherweise nach Finland kommen könnten so dass wir eine gemeinsame Besprechung verschiedener Fragen unter den Desmidiaceen haben könnten. Scott erzählt mir dass er wenigstens teilweise die Reisekosten für Sie zahlen werde. Wenn Sie hier in Karis in meinem Haus wohnen und essen (wenn Sie mit meinem anspruchslosen Haus zufrieden sind!), so könnte es geordnet werden dass Sie keine Kosten während Ihres Aufenthalts hier haben müssen. Auch Ihre wenige Reisen hier innerhalb Finland konnte ich gut bezahlen. Sie sollen nicht fürchten dass Sie dadurch mich belästigen. Meine Kleinigkeit will ich so gerne für Sie tun. Wir können dann eine freundschaftliche Besprechung allerlei Probleme unter uns haben,

die sehr nützlich wenigstens für Scott und mich sein würde. Wenn es
Ihnen helfen kann möchte ich Ihnen eine "Einladung zum Desmidiologen-
kongress in Karis" senden? Wollen Sie mal versuchen?

Scott soll Ende Juli hier eintreffen (die Zeit ist noch nicht ganz
festgestellt).

Mit vielen Grüßen, freundlichst Ihr,

ROLF GRONBLAD

KARIS
FINLAND

May 31, -54.

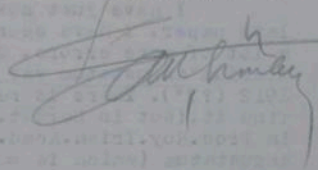
Dear Friend Scott,

I have just now finished looking through Irene-Marie's curious last paper. I have seen all those errors that you pointed out to me and a lot of more errors. Quite fantastic ones, e.g. on p.11 there is a *Closterium angustatum var angustatum* West ("par W. West pour l'Irlande en 1912 (?)"). There is really no such variety, nowhere. At least I cannot find it. (Not in Nordst., Index nor in Krieger's D.Flora) W. West has 1912 in Proc. Roy. Irish Acad. published a "var. *asperum*, nova var." of *Closterium angustatum* (which is = var. *Boergesenii*). I.M. must have confused the names "*asperum*" and "*angustatum*". A little lower on the same page 11 he writes about a "*Closterium acerosum* var. *acerosum* Cooke". Again, Cooke has never published such a name, nor has anyone else done so. These two varieties with repeated specific names are invented by I.-M. Mirabile et incredible! - And ~~she~~ he is speaking of "une erreur psychologique". I don't know what to think about it all? I cannot understand the problem of I.-M. Is he so utterly absentminded or confused? He can do a lot of harm with such bad work. On p.13 a paper by Raciborski is called "De nonnullis (algis) novis vel minus cognitis qu(a)e in Polonia/c/a inventae sunt... publié en 1889 - should be 1885. Such errors are dangerous. - On p.26 there is "*C. juncid/ium* var. *brevior* - pro *brevius*". As far as I can see the latter name is the correct one. If ~~you~~ has made an error in writing *brevior*, it is OK when it has been corrected. (I have myself as you know, used wrong endings after "forma": *Closterium ...um forma elongat~~a~~* pro *-um*. But this was quite a common custom among earlier writers: "*forma parva, f. maxima, f. laevis, f. involuta, &c.*" regardless of the gender of the specific name.

H. Rabenhout

I don't know what to do. Perhaps best : nothing? Prof. Brunel
has been I.-M.'s teacher. Would you tell him?

Sincerely,

A handwritten signature in dark ink, appearing to read "J. M. Brunel". The signature is written in a cursive style and is positioned to the right of the typed word "Sincerely,".

June 10 1954

Dear friend Gröbblad,

The slide that I am sending in the small box is merely one of my temporary water mounts, sealed with vaseline, and contains a single specimen of the new desmid from Sumatra. On one of the labels on the slide there is a circle, corresponding to the red lacquer ring, with a pencil dot indicating the approximate position of the specimen. If you will center the dot in the microscope and then traverse the slide in an east-west direction, the desmid should appear on this line, provided it does not move too far during transit in the mail. I am now trying to capture another specimen to send to Dr. Krieger. Enclosed are sketches of the five whole cells and two semicells, which are all that I have seen so far. Note that all of them are alike as regards the principal features, with only very small variations in size and curvature. My sketch of the first specimen seen shows five pyrenoids distributed along an axial line, but other specimens may perhaps indicate that there are many more pyrenoids, scattered as they are in the larger species of *Pleurotaenium*. The chloroplasts in all examples have been badly deteriorated so that their structure and arrangement is not determinable, and you will note that this is the case in nearly all the desmids on the slide. This is the case in many of the collections that I receive from abroad; I do not know the cause, but my guess is that it may be caused by the use of too much formalin, as evidenced by the strong smell. Have you encountered this condition? and what do you think is the probable cause?

This extraordinary desmid possesses the feature of unlike semicells, the differences consisting of different curvatures, which are constant, and differing structure of the apices. The apex of the "upper" semicell (as I have drawn it) is somewhat elevated and has three small depressions, one in the center, and also a lateral protrusion on each side. Arising from each of these protrusions is a very delicate hair-like spine, barely visible. These spines are not mucus threads, in my estimation, because they taper to an excessively fine point, while mucus threads are of uniform diameter, and further they are curved while mucus threads are straight.

The "lower" semicell has a flat apex that is not perpendicular to the longitudinal axis of the cell, but forms a considerable angle with it. Not quite in the center of the flat apex there is a small obcircular notch, flanked on each side by a small, solid, raised granule, such as is frequently seen in *Sumatrum*. At each of the apical angles there is a stout spine, one usually larger than the other, and forming different angles with the longitudinal axis. From the tips of ~~these~~ ^{each} spines arises a very delicate hair-like spine, prolonging the axis of the stouter spines, and even more difficultly visible than those on the upper semicell. There are no other such hair-like spines anywhere else on the cell, and ~~hence~~ that they are really spines and not mucus threads is proven, I think, by the fact that they arise from the solid larger spines.

At the base of each semicell there are 10 large, downwardly pointing teeth, those of the two semicells intermeshing with each other. These teeth are very similar to those of *Sumatrum asperum*, which however do not intermesh, while the intermeshing is like the rounded teeth of *E. gnathophorum*.

The cell body is circular in cross-section, but near the poles it is somewhat flattened to form the biradial apices. Just below each apex there is a pair of large pores ("mucus pores"), but I have not been able to see any other markings on the cell-wall, in empty semicells, even at high magnifications (900x). The mucus pores are situated in the thickenings of the cell-wall.

When I first saw this curious desmid, the thought of *Ichthyocercus* came to mind, for there is a suggestion of that genus in the overall picture. After consideration of the several conflicting characteristics I concluded that it could not be assigned to that genus even neglecting the unlike semicells, and when this is taken into consideration I cannot see that there is anything else to do except to make a new genus.

Now, if we disregard momentarily the apical structure, the circular cross-section of the body and the basal folds and teeth are characteristics of *Docidium*, though the plant differs greatly from any *Docidium* hitherto known. On the other hand, the two types of apical structure can each be matched in various species of *Euastrum*, and the paired mucus pores just below the apex with no other pits or pores anywhere else on the body are found nowhere else but in *Euastrum*. Also the basal teeth, as I have pointed out, are *Euastrum* characters. It seems to me, therefore, that the plant combines the characters of *Docidium* and *Euastrum*, and for this reason I have suggested the name "*Docideuastrum*" for a new genus to contain it. I am not wedded to this name; another that occurs to me is "*Skenmatium*" or "*Scemmatium*", derived from the Greek *skemátos*, meaning "a subject for speculation".

Enclosed is a copy of part of a letter from Prescott, from which you will see that he is undecided, or only half-way convinced, about a new genus, and thinks it could be assigned to *Ichthyocercus*. I don't agree with this, and am going to point out that his position now is inconsistent with his opinion on *Scottia mira* which he admitted was entitled to new generic rank. But in *Scottia* the only reason for a new genus was the unlike semicells, for if both semicells had been alike, whether with or without the apical corona of teeth there could have been no question of assigning it elsewhere than *Staurostrum*. In the new plant, in addition to the unlike semicells, there are several other features, the peculiar curvature, the circular cross-section, the basal teeth, and the *Euastrum*-like apices and mucus pores, which would prevent it, in my opinion, from assignment to any existing genus.

I am sorry that I can send you only one specimen; later this year I hope to get an additional quantity of material from Sachian, who is studying it in connection with his fisheries work. He is also going to send me a lot of other gatherings from Sumatra, where he has just made a trip of 2500 km, visiting a number of lakes.

There are several desmids in this collection from Manggala swamp which seem to be new species. I call your attention to one filamentous form that I believe to be *Hyalotheca*; it is apparently is the same as one that I found in Florida, shown in my drawing No. 482, from collection Florida 92. I queried this as *Bambusina*?, but in the Sumatran material I have seen filaments with many young semicells that are formed in the usual manner, not by infolding of the walls. Enclosed is a sketch of the Sumatran plant, with a zygospore, of which many can be found.

I await with great interest your opinion on this new desmid, and also your remarks on what anyone would have regarded as the infinitesimally small probability of one individual finding two such lusus naturae in the space of a few months.

Sincerely,

ROLF GRONBLAD
KARIS
FINLAND

June 17th 1954.

Dear Friend Scott,

Like Prof. Prescott I am of the opinion that you are an exceptionally lucky man, a "Rufus Thompson" (but who he is, I don't know). The richness of wonderful species in the tropical countries is quite excessive and incredible. The Indonesian samples are, moreover, taken by a skillful collector. I think you have been excited again. Who would not be so. I agree with Professor Prescott in suggesting the new Desmid near to *Ichtyocercus*, and indeed, it would not be impossible to put it there. BUT on the other hand, there are ~~there are~~ such peculiarities which give you a good right to create a new Desmid Genus upon ~~this~~ "odd critter". As you pointed it out in your letter, it is asymmetrically built, quite in the same way as *Scottia* (a propos the generic name: why not "Sachlania"? But I don't wish to intrude upon your christening rights.). The basal teeth in your NEW DESMID are, I should think, sufficient to separate it from *Ichtyocercus* in analogy with the basal warts in *Docidium* which separate that genus from *Pleurotaenium*. Of course there is no compulsion why not make it a *Ichtyocercus* (and I guess that Krieger perhaps would not make a new genus?).

Ichtyocercus has pyrenoids in an axile line according to Krieger (D.Fl. pag. 449). It is very deplorable that the chromatophores are quite deteriorated. Apparently there is a fault in the conservating fluid. Perhaps too strong solution of formaline. (I put only a few drops into each sample of about 20-30 ccm). You must tell Mr Sachlan to make some experiments in order to find the best concentration. ~~It~~ is really a pity that all chromatophores should be spoiled. As to the structure of the cell-wall it would be useful to try with staining. Then you can also be quite sure about the nature of the fine hairlike spines. And, moreover, perhaps you would be able to see something of the chromatophores just when the staining is beginning. (Mucus threads are not always uniform in diameter but may be tapering).

I had to search a good time before I could find the one cell. "Dociduastrum" seems to me a little "constructed". As to "Skeumatos" this word could be used without any "latinisation". There is an alga (Fucoideae) with the name Filum (= the Latin word for "thread") and there are also other nouns used as generic names. Skeumatos would not be bad in my opinion, but perhaps Saclania would do as well? But you have to decide for yourself, of course.

Summa summarum: you are a lucky fellow!

In the same sample there are several thread-like Desmids: Hyalotheca or Bambusina. Which of them must be inquired into. I enclose a few sketches.

Also a new drawing of Xanthidium superbum from Finland is enclosed. The specimen was a little deformed. By the same mail I send you a preparate with 3 cells of X. superbum so that you may be able to compare them with Australian specimens. They are in glycerine which makes them very transparent so that it is difficult to see the structure of the cell-wall. In the same preparate (slide) you can find one cell of each: Micrast. brachyoptera and M. fimbriata, the typical European forms of them. There are also cells of M. crux melitensis, Xanth. antilopaeum var laeve and var. basiornatum (the latter not known from USA?). Cosmar. quinarium and C. taxichondrium typical European forms. Artared. Bulnheimii, &c.

I have sent you a small "parleur" in Engl.-French-German-Finnish. Finnish is a very difficult language and you need not learn it because people in this part of Finland speak Swedish. But perhaps you wish to look at the book just for fun.

I have proposed to Teiling to come to Finland, but he seems not to be interested. I will do it once more. I think he is not ~~he is not~~ lacking as to pecunial resources. His wife is on a trip to Italy. I do not ~~think~~ think you should offer him to pay the costs for a trip to Finland because it is only a bagatelle.

Perhaps we ~~may~~ ^{ought to} talk over the question about writing to Brunel. I don't know what to do. Perhaps you could do it in a very polite and kind manner. My English is too poor to diplomatical letters.

It is very good that you arrive already in July. I hope you will reserve plenty of time, because we have really much to do.

I hope that I have answered to all your questions.

My best congratulations to THE NEW DESMID.

Sincerely yours

P.S. I could not find the sample earlier
so I was not able to send you any material
as I promised.

P.S. To day de Krieger's answer came.
He will try his best to get a passport.
I think he is glad to come.

ROLF GRONBLAD

KARIS
FINLAND

June 26th 1954.

Dear Friend Scott,

Your letter carrying the bad news arrived today. I am very sorry that such a misfortune should happen to you. I think this must have been an unpleasant disappointment to you, but I can tell it was so also to me. Twelve months are not a very long time, but, indeed, as you say, much can happen. But we will look forward to the next year with good hope and try again. Meantime I have to take up the work with your Desmids and try to get ready with the Arthrodesmi and get ahead with a better speed. I had of late done very little work on your material because I thought we could settle a lot of questions easier and better when meeting personally.

Well, dear Friend, I wish you better luck and I hope nothing bad will happen to either of us - in spite of the bad looking sit-

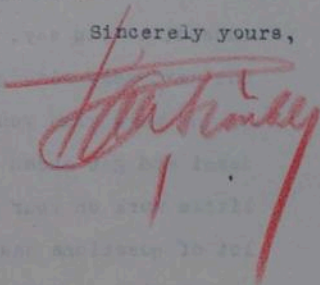
uation of the world.

I do not think Dr Krieger will use your money when you yourself cannot come. And I think it would not be quite just either.

So we must continue to work as best we can and talk over the questions by correspondence. I am glad to hear that you can sit and work with your microscope. In bed the time would be very long.

My very best regards to you.

Sincerely yours,



July 8 1954

Dear friend Grönblad,

The road-map of southern Finland is a very fine one, much more detailed than the American ones, of which I sent you some; but the latter are distributed gratis by the large oil companies.

This morning I received the proof of your paper from Botaniska, and have read it carefully, and returned it by airmail. There were two small corrections that I made, one in the text, and one in the legend of Fig. 13, where Kallio had the word "bifurced" instead of "bifurcate". These are not important, but I did request the editor to invert the group of photos of *Scottia*, which were printed upside down. The paper is very nice, but I was a little disappointed that K. did not have a little more to say about the cytological problem of *Scottia*. But I suppose there is not much that can be said at the present time, except guesses at the possible cause of the polarity. Maybe one of these days we shall catch some of the critters alive, and get them into cultivation.

I have heard no more from Krieger, and I suppose he is now trying to get permission from the authorities to go to Finland. Your cordial invitation, and your suggestion of a Desmidiological Congress ought to help greatly. I sincerely hope that he will be able to go.

Teiling has written me that during July he will be occupied with family matters and other affairs, but that he will be free during August, so it is now settled that I shall visit him after I leave you. I now propose to leave London on Monday July 26, and arrive in Helsingfors the same day; the exact time of arrival I shall not know until I buy my plane ticket in London, which I shall do soon after I get there on July 7th, and then I can advise you definitely. I can spend two weeks in Finland, provided you can put up with me for so long. Here is another thought that you might consider, if Krieger is successful; how about inviting Teiling to take part in the "Congress"? You and he are good friends, so if you think well of the idea you might write him and ask what he thinks of it. I gather that money is not too plentiful with him since his retirement, and he may not feel that he can afford the trip, though it would not be very expensive by steamer. I do not dare to make the same proposal that I made to Krieger, for Teiling is sensitive about receiving favours that he cannot repay, and he requested, some time ago, that I discontinue sending him books and journals. However, I should be only too glad to help if there is any way in which it can be arranged.

Also I have your letter regarding Irene-Marie's latest paper. I had not noticed the discrepancies in nomenclature that you mentioned; as a matter of fact I am not familiar enough with *Glosterium* to catch them. But all these, together with the many misspellings, are a serious matter, and I should think that any author would be glad, though he might be dismayed, to have them brought to his attention. I met Dr. Jules Brunel some years ago, and my wife and I had dinner with him and Prescott at Luchow's, a famous New York restaurant. Since then I have exchanged some cordial letters with him, and he was one of my references for my unsuccessful application for a grant to the Paris Congress. So I think I know him well enough to write him about it, and ask if, as I-M's teacher, he could broach the matter to I-M. Do you think I should do it?

The same day that I received your last letter, there was one from Kallio also, perhaps written as the result of a prod from you. He found 8 of the 9 species and varieties of *Micrasterias* in my samples, and is now cultivating them successfully.

The 9th one, which he did not find, was *M. foliaceae*, and as this is fairly common in my collecting grounds, I shall get some for him, and also other species, next autumn, when the weather gets cooler. He also sent me five tubes of his cultures of *M. Thomsiana* var. *notata* and *M. rotata* var. *evoluta*, and one containing *M. Sol* var. *extensa* F&S, which is one that I sent him. Most unfortunately all the tubes were broken and the contents lost, though there was no damage to the wooden box nor the wrapping of the tubes. I think the breakage is caused by his method of drawing out the end of the glass tube and fusing it to make a seal. This makes the glass very thin, and also lowers the air pressure inside, so that the tube is very delicate. No doubt this was of sealing is necessary where the contents are to be placed in culture, but I am not able to do this, and for my examination it would be just as satisfactory to use corked tubes. I have written him, and of course I shall discuss the matter and others when I see him in August.

When I wrote you last year that I had found a new desmid genus, no doubt you thought I was an exceptionally lucky fellow, as indeed I was.

What will you now think if I tell you that I have discovered ANOTHER NEW GENUS, this time from Sumatra?

AND WHAT WILL YOU SAY IF I TELL YOU THAT THIS SECOND NEW GENUS, THOUGH VASTLY DIFFERENT FROM SCOTTEA, POSSESSES THE SAME PECULIARITY OF UNLIKE SEMICELLS?

I can imagine you saying: Oh, this is too much! Scott must have had a nightmare, and dreamed all this.

Well, now you may open the small envelope and look at the photograph. Tomorrow I will send you some drawings and a single specimen on a slide. It is extremely rare, and I have only a minute quantity of material. So far I have seen five specimens, all alike except for very minor variations. I have sent one to Prescott, and shall try to catch another for Krieger. This one will be written up by me with Prescott's help, for it is in a new collection belonging to the series sent me by Mr. M. Soehlan, of Bogor, Java, but I hope you will be kind enough to give me your opinion of it after you have seen the specimen and drawings, and I shall ask Krieger to do the same.

Four weeks from today we shall leave from New York, so it won't be much longer until we meet in person, to which happy event I am eagerly looking forward.

Sincerely yours,

July 9 1954

Dear friend Grönblad,

Well, things are going OK so far. The plaster cast has been on my foot and leg for 17 days, and is not too uncomfortable. I can hobble around the house on crutches, and even, with a little difficulty, get downstairs to my laboratory and typewriter. But I can only use the instrument or machine for about 15 minutes at a time; then my foot swells and becomes painful, so I must elevate it and give it a rest. In two weeks more the doctor will remove the cast, but then I must walk on crutches for at least two weeks more, and I expect it will be several weeks more before I shall be able to walk for any distance.

Many thanks for your letters, the drawings, the slide, and the Sohlman conversation book. This is an excellent, by far the best that I have seen, and I have told Miss Crossdale about it in case she ever goes to Europe again, as she threatens to do. But the Finnish language, as you say, seems to be an especially difficult one, with no resemblance whatever to any of the languages of which I have a smattering. In glancing through the book I could find only a few words, mostly technical ones, that I could recognize. One of them struck me as rather curious, 'oilcan' = 'eljikannu'. I don't think I shall try to learn any of it.

I was greatly interested in your opinion of the new Sumatran *desmid*, which is very similar to Prescott's ideas. Enclosed is a letter from Krieger, and you will see that he accepts the proposal for a new genus, and does not even suggest placing the plant in *Ichthyocercus*, rather surprisingly. (By the way, note the spelling of *Ichthyocercus*, with two h's). Since nobody likes my name *Docidocentrum*, I have written Prescott sending him copies of your and Krieger's comments, and have suggested the name *Sachlandia bipolaris* (or *bipolare*, I don't know which is correct). I have made some more photos of more specimens, which are enclosed. One of them, showing a 'lower' semicell, was focussed to bring out the remains of the deteriorated chloroplast, and it seems to show that my original sketch indicating 5 pyrenoids per semicell is probably correct, though there may be 6 instead of 5.

In the batch of drawings of this plant that I sent you, I believe that I must have included two copies of one sketch, which is numbered either No. 7, or No. 8. This shows a front and side view of a 'lower' semicell, and on one of the sketches there is a third view in a tilted position. If you have two sketches, please return one of them to me, it doesn't matter which one; you may keep the others.

I was glad to see the specimens of *I. superbum* on your slide. Compared with them my Australian plants have a longer body but much shorter spines, and the lowest spine on the semicell is single; this could have been named as a new forma. From Sumatra I have some examples of about the same body size, but with even longer spines than yours. I am enclosing a sketch showing one of them. A species that seems to be closely related is *I. sansibarense*, of which a sketch is enclosed. There is some variation of the spination in this plant. Sometimes the lateral spines are paired, and sometimes the apical spines are single.

M. brachyptera is a species that I have never seen before. The specimen on your slide is somewhat different from the three figures in Krieger's monograph. The polar lobe is decidedly tapering instead of having almost parallel sides; the terminal spines on the lobules are longer and more curved; and I was unable to find any surface spines on the body although I looked carefully for them. The specimen seemed to show some signs of crushing or flattening, which might distort the shape of the

M. Grux-Melitensis is another species that does not occur in my own collections, though I have seen it in material from Japan and also from Uruguay. These foreign plants seem to be pretty much the same as yours.

The European form of Cogn. quinarium is somewhat different from the USA form, and this may account for your opinion that the USA plants are identical with a South American species. I am not able to look this up just now.

I am not acquainted with X. antilopaeus v. basioratus, and cannot look it up, but the plant on your slide which I take to be this variety, with a small spine below the paired lateral spines, seems to me rather close to certain forms which I have thought to belong to X. cristatum, in shape and size. I think you will find something like this among my American drawings.

Your M. fimbriata also differs from the American plant, the principal difference being that the marginal spines do not arise from the 'corners' of the lobules, but from points considerable inside the 'corners'. I do not recall having seen anything like this in any Micrasterias of this type.

I hope that you will proceed with the Arthrodesmus paper, and in cases where there is doubt I shall be perfectly satisfied if you will use your own judgement, just as if you were working alone on your own material. When you return my drawings which you wish to use as illustrations, I will proceed to make up the plates.

From Krieger's letter you will see that it requires some three months for him to obtain a passport. I shall write him in the next day or two, and suggest that he start his efforts next year on Nov 1st instead of April 1st, so as to allow plenty of leeway. It will always be possible for me to adjust my schedule to suit you and him.

With my best regards and good wishes,

Sincerely yours,

P.S. The only things I know about Rufus Thompson are that he is a professor at the University of Kansas, Lawrence, Kan., and that the roster of the Phycological Society lists him as specializing in Dinoflagellates. In the 2nd edition of G.M. Smith's FW Alg. U.S., there are x number of drawings by Thompson of this and other algal groups, also a listing of his publications up to 1950. Apparently he is not interested in desmids.

ROLF GRÖNBLAD
KARIS
FINLAND

July 26th 1954.

Dear Friend Scott,

This is the day you should have arrived here if all would have gone well. I have been 10 days on an excursion to ^{the} Archipelago of Åland in the Baltic Sea. (The weather has been exceptionally bad: cold and rainy). When I arrived at home I was met by a letter from Dr Krieger's wife telling me of the unexpected sudden death of our friend. (Perhaps you also have got a letter?). I was quite struck down by this sad news. We shall never meet him. It is indeed a very heavy loss to us all who are working with these algae. Who is there to finish Krieger's "Desmidiaceenflora"? I don't know. At ~~this~~ present time there seems to be no one able to continue his work.

Thank you for your letter, photograph and drawings. I am glad that you pointed out to me my wrong spelling of Ichthyocercus. It is a name I have used and written very seldom, so I really did not notice the two "n"s.

Apropos wrong spellings - Irene-Marie has again a new paper on Penium and Pleurotaenium with numerous misspellings. Even his identifications are not always quite so as I should like to have them. What do you think of his new Penium denticulatum? It looks like a Cosmarium? It would be interesting to have a little of his material. When I.-M. is drawing the structure of the cell-wall and indicates small warts or punctulations or pores he apparently makes them look as if they were in (oblique or longitudinal) rows even if they in reality are scattered. You can never be sure what they really are like. This is very troublesome. Furthermore, his "new" varieties seem not always to be acceptable. Cf. e.g. pl. I. fig. 3 Pl. constrictum var. laeve, nova var. I should like to call it P. coronatum - I think so - Pl. constrictum is quite different. - Another example: P. paludosum nova spec. How can it be distinguished from P. coronatum v. fluctuatum

I.-M.'s P.Ehrenbergii v. columellaris and P.Ehrenbergii var.constrictum
Krieger, Desm.-Fl. 43:5? And in conclusion: his "Latin" is very peculiar
and difficult to understand. It is not like anything that other people
call Latin. (This was also Miss Croasdale's opinion). Are you going to
write?

If you choose "Sachlania" it must be "bipolaris".

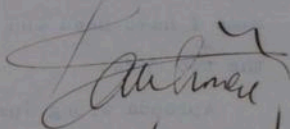
Xanthid. antilop. v. basiornatum is drawn in Grönb. New Desm.
pl.IV, fig. 17-18. and in Eichler & Raciborski (1893) III:31 and Dick 1919
XVI:2. Do you have these papers or copies of them? If not, I can send you.

Micr. fimbriata: I think the Finnish specimens are like Ralfs'
drawings. The American specimens are constantly different- perhaps a for-
ma or variety?

I hope your ankle is getting better and I hope our meeting will
be realized next summer.

With my best wishes,

sincerely,



As to your photographs:

The "organism" from Brazil No 53 with nyaline parallel rays "may be"
a "inner hair" of a water plant, e.g. Nymphaeaceae. I don't know the
exact English term. In German: innere Haare. But it may as well have
something to do with Spongia. I believe I have seen something like it.
A professional botanist can tell you, I think.

Krieger's last letter is enclosed.

The Sachlania drawings No 7 are enclosed.

July 31 1954.

Dear friend Grönbld,

Yes, I received the printed announcement of Dr. Krieger's death, and was very much shocked by it, especially as it was only two weeks previously that he had written me the letter which you returned, concerning the new desmid from Sumatra. The announcement was sent in the name of Frau Gertrud Krieger, but the envelope was addressed in the handwriting of Dr. Kurt Krieger. So I wrote immediately to his son, expressing my deep sorrow, and asking him to convey my heartfelt sympathy to his mother and the other members of the family.

As you say, it is a great loss to all his friends and to all who are working on desmids. His Monograph is my Bible; just a few days ago I found a new species of *Euastrum* from Australia, and I know it is new because there is nothing like it in Krieger's work, the only one that covers the desmids of the entire world. I fear that the Monograph will never be completed, at least in my lifetime. You are the only one I know whom I believe capable of doing it, if you had the courage to undertake it, and were willing to do nothing else for the next 20 years. But you would need the resources of Krieger's library, which must be one of the most complete in existence, his iconotheque, his collection of 6000-7000 bottles of samples, and his notes accumulated over many years.

A year or more ago Krieger wrote me that the next installment of his work, dealing with *Cosmarium*, was just about finished, and that he began to see some prospect of being able to publish it, which had theretofore seemed impossible because of the poor conditions in Eastern Germany. I mentioned this in my letter to Kurt, and my hope that it will still be possible to publish it. But there would remain the work of checking and proof-reading, which would be a tremendous job for anyone except the author. Krieger had also written a short paper on the desmids in four samples from Peru that I sent him last year, and in March this year he wrote that he would send it in for publication in May, presumably to the *Deutsche Bot. Ges.*, but I have heard nothing more about it. The samples were not too good, but K. said he had found about 60 desmids in them, and I think there were a couple of novelties.

I also had noticed the new misspellings in Ignace-Marie's latest paper, on *Penium* and *Fleurotaenium*, and I have just about made up my mind to write to Dr. Jules Brunel on the subject, and ask if he thinks he can venture to call I-M's attention to it. But I shall wait until I receive the reprints of your 'Scottia' paper; then I can send one to Brunel, which will give me a good excuse for writing him. As to *Penium denticulatum*, it certainly looks like a *Cosmarium* to me, and I cannot imagine what reason he could have had for assigning it to *Penium*. For his varieties of *Fleurotaenium* I cannot say very much, for I have a great deal of difficulty myself in discriminating between some of these very similar forms. And I do not know enough about Latin to tell 'good' from 'bad'; perhaps it is 'religious' Latin, the kind that is used in the Catholic church.

You were correct in your identification of the unknown object from Brazil 53, except that it turns out to be an external hair instead of an internal one. Prescott identified it for me, and says that it is otherwise classed as a "peltate scale". In a book on general botany I find a rather poor sketch of something similar, with a note that such hairs occur in three families of land plants. It is quite surprising to me that such a structure can be called a 'hair'; I should have thought that

My ankle is progressing reasonably well, I suppose. I am now walking, or rather hobbling, without crutches, and the cast was removed nearly two weeks ago, a great relief to be free from that dragging weight. I am wearing an elastic anklet to keep the foot from swelling too much, but after it has rested on the floor during the time it takes to write this letter it becomes painful.

Lately I have not done any serious work with the microscope, but have amused myself looking at a new collection from North Australia, extremely rich in desmids and with a number of rare tropical forms. There is the new *Buastrum* mentioned above; a new spinous variety of *M. lux*; a new variety of the highly variable *M. Koebii*; a beautiful new variety of *M. alata*; *Xanthidium bifurcatum* forma; *Pl. minutum* var. *attenuatum* with a retuse apex instead of the usual truncate or rounded apex; an abundance of what I take to be the typical form of *Pl. burmense*, also of var. *curtum* (Scott & Prescott, *Archaes Land*, Pl. 2, fig. 11); and many others. I shall send you a sample next week for your enjoyment. Some of the desmids are collapsed or distorted, which was caused by partial desiccation of the material from leakage of the water during the transit from Darwin to New Orleans. Fortunately the sediment remained in the jar, and after I added water and a little formalin it was almost as good as new.

Now it is twelve o'clock, so I am going upstairs to get my lunch and rest my foot. So long, and good luck!

Sincerely,

Thanks for the snapshot of you and your tent, but I wish that sometime you would send me a large portrait that I can frame and hang on the wall together with other noted desmidiologists. I asked Kurt Krieger if he could spare a photo of his father; I have never seen Krieger's picture and have no idea what he looked like.

*Snapshot removed and placed
in HI portrait collection*

ROLF GRONBLAD
KARIS
FINLAND

Aug. 30.-54.

Dear Friend Scott,

I am afraid I have kept you far too long waiting for my answer. But there are some good reasons: I have not been quite well and so my algological studies have made only very slow progress. The doctors declare that my ulcer is gone - and that is OK, but there seems to remain a hypersensibility that is very unpleasant. I cannot sit for any longer time at my table or microscope. This is a most unhappy condition, and I earnestly hope that my health will improve soon.

I wrote to Bot. Notiser concerning the reprints and I hope

I have not got my copies. you have got your set already. (Have I not told you this??).

As to the Desm. Flora by Krieger I have proposed that Messikommer or Skuja or Prescott would be chosen to finish the work. I think there is no competent person in Germany. I myself

dare not take such a burden. My working capacity is far too low.

I have finished the manuscript of the desmids sent to me by Dr Crossdale 1947-48-49. But it is to be type wrought still. There is a new variety of *Xanth. antiopaeum* viz. "var *americanum* Scott & Grönbl." if you permit me to publish it by that name. It is also in your Southern States collections, and among your drawings. May I do so? Then I have also quoted your remarks upon *Staurastrum majusculum* - *S. minnesotense* v. *depauperatum*. The N.Hampsh. specimens are totally without accessory spines, only very short teeth at the angles are remaining, sometimes single sometimes two at each angle. Krieger who saw my drawings suggested *S. majusculum*. May I put in your remarks?

Perhaps you wish to see the text before it is published. Then I shall make an abstract for you.

In my next letter I hope to be able to tell you something about your own collections.

Kind regards, sincerely

Sept 6 1954

Dear friend Grönblad,

Some time ago you said, in one of your letters, that Dr. Paavo Kallio had thought of possibly going himself to Brazil to try and get living specimens of Scottia. This seemed a rather wild idea; nevertheless I wrote to Dr. Sioli and asked him to give whatever information he could that might be helpful to Kallio. Several months later I learned that this letter never reached Sioli, so I had to repeat the request, and I have just received his reply, written after his return from Germany, where he spent a couple of months this summer. I am enclosing two copies of that portion of his letter referring to the proposed trip, and the information he gives will be quite helpful if Kallio really undertakes the trip.

As you will note from Sioli's letter, he is probably now in the Amazon region, but a long way from Santarem. The Rio Solimoes is another name for the upper part of the Amazon River, above its confluence with the Rio Negro; the Rio Purus is a tributary which joins the south bank of the Solimoes about 200 km above Manaus (Manaos); and the Japura is another tributary joining the north bank of the Solimoes some 600 km above Manaus. I knew beforehand of his proposed visit to Amazonia, and though he had informed me that there was little prospect of his being able to go to the Lago da Boca de Mentai, I had offered to pay part of the extra expense if could manage to do so.

numbered X-104
A few weeks ago I sent you by airmail a sample of material from Arnhem Land in North Australia, one of a new series on which Prescott and I will collaborate. This is a really extraordinary collection, containing such a wealth of rare and new forms that I have seldom seen. There are 10 different Micrasterias, 8 Xanthidium, 2 new species of Euastrum, a Staurastrum (probably new) somewhat resembling St. Boergesensii, and many other noteworthy desmids. Many of them are extremely rare; in some cases I have seen only one cell, and in one instance only a single damaged semicell of what I believe to be a new spinous variety of M. Torrezi. This, of course, requires confirmation. Though I have examined certainly more than 50 slides of this material, I am still finding an occasional novelty, and it seems to be inexhaustible. I am enclosing a list of the Xanthidium and Micrasterias and a few of the Euastrum forms, but you must not expect to find all of them.

Sachlan has sent me a somewhat larger supply of the material from Sumatra in which I found the new Ichthyocercus-like genus, and I am sending you a small quantity. If you are lucky you may find a few examples, but it is very rare. In another collection from Sumatra I found a very curious desmid that seemed to be a cross between Staurastrum and Euastridium. At first I thought it was new, but it turns out to be (probably) Euastridium staurastroides N. Carter. I do not possess Carter's 1926 paper, and the only illustration I have of her plant is the diagrammatic top view in Telling's paper on Radiation of Desmids, which checks very well with my specimens. According to Telling you do not accept the genus Euastridium; would you mind telling me your reasons? Sorry that I cannot ~~xxx~~ send you any of this material; the total quantity that I have is less than one drop.

My ankle is healing nicely, but the leg muscles are weak from disuse, and I get very tired after walking for half a mile. Old age has something to do with it, too!

With best regards and good wishes,

Sincerely yours,

Sept 6 1954

Partial list of some of the interesting desmids in Collection X-104 from a slough at Oenpelli, Arnhem Land, North Australia.

<i>Xanthidium hastiferum</i> fa. <i>javanica</i> Turner	cc
<i>acanthephorum</i>	r
<i>bifurcatum</i>	rrr (only 1 seen)
<i>superbum</i> , the Australian form	rr
<i>multicorne</i> fa.	r
<i>subtrilobum</i> var. <i>inerme</i> Skuja	rrr (only 1 seen)
<i>armatum</i> var.	rrr
<i>antilopaeum</i> , near var. <i>maximum</i>	rrr
<i>Micrasterias ceylanica</i>	cc
<i>laticeps</i>	rr
<i>truncata</i> var. <i>semiradiata</i>	r
<i>alata</i>	rr
<i>alata</i> var. ' <i>parallela</i> ' var. nov.	rrr
<i>lux</i> var. ' <i>spinosa</i> ' var. nov.	c
(This is the <i>M. lux</i> fa. in Scott & Presc., Arnhem Land, unpublished).	
<i>mahabuleshwarensis</i> var. <i>surculifera</i>	rr
<i>Torreyi</i> var. <i>Crameri</i> fa.	rrr
<i>Torreyi</i> var. ' <i>arnhemensis</i> ' var. nov.	rrr (only 1 semicell seen).
<i>Euastrum pulcherrimum</i>	rr
<i>asperum</i>	rr
<i>praemorsum</i> , 2 new varieties.	1 common, 1 very rare.
<i>longicolle</i> var. <i>capitatum</i>	r
<i>longicolle</i> , a very large new variety which also occurs in Indonesia,	rr.

Sept. 10.-54.

ROLF GRÖNBLAD

KARIS
FINLAND

Dear Friend Scott,

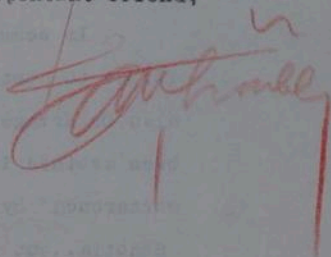
Again I must tell you bad news. From Botan. Notiser I had a letter sent by Dr . A. Becherer, Conservatoire Botanique, Geneve which ran in this way: --"Darf ich Sie darauf aufmerksam machen, dass Art 74 der Nomenklaturregeln (Homonym-Artikel!) eine Scottia Gr. 1954 nicht zulässt? Da es schon eine Phanerogamengattung Scottia R.Br. 1812 (= Bossiaea Vent.1800) gibt."

It seems obvious that "Scottia" must be altered. I quite forgot to control the possibility of using your name, and so did also Dr Krieger. It is a very unhappy situation. And it could have been avoided if I had only looked in my "Ethymolog.--Botan. Handwörterbuch" by Wittstein. There I now see Scotia, Scottia, Scottea, Schotia., but I had no doubt as to the possibility of using your name as a generic name.

Now the editor of Bot. Not. proposes to use "Neoscottia" in accordance with "Neopatersonia" &c. But I should rather like "Scottiella" in accordance with "Westelia" &c. Would you approve of it? It was unfortunate that I didn't propose that name from the beginning. If the name Scottiella can be used (- I shall ask Mr. Becherer. -) the correction will be published in Bot. Not. December number.

Of course I ought to have been cautious. Your name is not quite uncommon. I have only to beg your pardon for this unhappy case.

Your repentant friend,



Sept 13 1954

Dear friend Grönblad,

I am very sorry to learn that you are not feeling well, even though your ulcer is cured, but I feel sure that it is a temporary condition that will right itself in a few weeks. I can sympathize with you, for I have had similar experiences after my attacks of chloecystitis; also my enforced rest this summer from the broken ankle has left me in a weakened condition, from which I am now slowly recovering, thank the Lord.

No doubt the Scottia paper has been published by this time, but I have not yet received the reprints, and in fact have heard nothing more about them since I wrote to the editor, returning the proof, and ordering the reprints. I hope they arrive before the end of this month, for then I am going to drive up to Michigan with my wife, to visit Prescott for a week or so, and use his iconograph and library, and with his help to determine a couple of hundred desmids from Indonesia and Australia, which I cannot identify certainly with my own resources. I hope by that time I shall be in shape to make the trip. It is a rather long one, about 1900 km each way, but I intend to take it easily and drive only about 400 km per day. Going by train would be easier and cheaper, but I had promised the wife a trip somewhere to make up for the missed voyage to Europe, and both of us like travelling by auto.

I rather doubt whether Prescott would be able to undertake the continuation of Krieger's work. In addition to his teaching duties he has large accumulations of material from his travels in Alaska, Ecuador, and Mexico. Lately he has been working on some collections from the Fiji Islands, which, however, he says are very poor, and that either desmids do not exist there or the collector did not know how to get them. Also he has in contemplation an ambitious work on Desmids of the U.S., in which he has proposed that I should collaborate. I don't know just what my collaboration would be, except to place at his disposal all of my drawings, after you have published them, of course.

Naturally, I am glad to have you publish Y. antilopaeum var. americanum either with our joint names as authors, or yours alone, or yours and Croasdale's, whichever is most appropriate. I have forgotten just what I wrote you about Staurastrum minnesotense var. depauperatum, but if Croasdale's specimens are like my drawings 1967 and 1968, I think they belong to minnesotense and not to majusculum. This seems to be proven by the dichotypical specimens 1961 and 1965, and the many forms in collection Mississippi 92. Anyway, I am quite content to rely on your judgement, and you may publish anything I have written, if you think my remarks are correct and worth while.

Now I have some more news about the new desmid genus from Sumatra, which I had proposed to name 'Sachlania heferopolare'. Sachlan send me a little more material from the same collection, and I have found a few more specimens, in all 13 whole cells with different poles. But the 14th specimen threw a monkey-wrench into the machinery by having both poles alike!! Since this discovery I have been searching for more examples, but no more have turned up so far. So now I am quite confused, and don't know what to do with the funny little beast. Telling warned me about this, and said that the 7 specimens that I found in the first part of the collection were a very small number on which to base an assertion of constant form, and that the dichotypical plants might be a local mutation of something else. It now appears that he may be right, but if so it seems to me that there must be another form with both apices of the fishtail type, which has now shown up so far. No more material is available than what I have, so I shall have to keep on with the search, but it is possible that the problem cannot

be solves until ~~forget~~ Sachlan has another opportunity of visiting the spot and getting much more material. I have asked him to do so, and also to sending living material to Kallio if he can. He has already written to Kallio for instructions on collecting and shipping; I wonder if he will get an answer, for K. does not seem to like letter-writing.

I am enclosing a sketch of the 14th specimen with similar poles, and shall send you a very small quantity of the material. If you are lucky you may find one or two of the dichotypical forms, but they are still extremely rare. It will take much more luck to find one of the 'equipolar' or 'homopolar' forms, but if you do I would like you to make a sketch of it, or better yet, try to capture it on a slide and send it back to me.

Thanks for the photo of Dr. Krieger taken 18 years ago. His wife has sent me two other photos, more formal ones and taken more recently, and I am having some enlargements made from one of them. Do you have this recent photo? If not I will send you one of the enlargements when I get them. The home-made microscope is interesting; what was the occasion for making it? Krieger was a remarkably handsome man, quite different from the type which I had mentally pictured, and it will always be a matter of regret to me that I was unable to meet him.

With my best wishes for your speedy recovery, and best regards,

Sincerely,

Sept 15 1954

Dear friend Grönblad,

It is indeed unfortunate that the name Scottia has already been used for a genus of Phanerogams. But I cannot blame you, for such a possibility had never entered my mind; even if it had, I should not have had any way of checking it outside of the freshwater algae.

There is nothing to do now except change it, and I prefer Scottiella to Neoscottia. If you find after checking with Dr. Becherer that Scottiella is permissible, it would be advisable to prepare a small typewritten (not type-wrought) slip to be pasted in the reprints. And so that yours and mine may be identical, I ask you to tell me the wording that you will use. I might suggest something like this:

The name of the new genus is changed from Scottia to Scottiella (Bot. Not., December 1954), for the reason that the former name has already been used for a genus of Phanerogams, Scottia R. Br. 1812 (= Bossiaea Vent. 1800).

I wonder if you would be kind enough to look in your Wittstein and see if the name Sachlania has been used previously? There would seem to be little chance of such an occurrence, for Sachlan is an unusual name. I don't know whether it is Javanese or Dutch; it seems to me to have a flavour of Holland, but the man is a native of Java, judging from his photo, though he might have had a Dutch ancestor.

Cheer up, and don't take it too hard! Prescott made a similar mistake in naming Cosmarium rotundum in our South Australian paper. Someone has written him that the name was used in 1892 by Turner, for a quite different species.

Sincerely,

Sept 19 1954

Dear friend Grünblad,

I wrote you on the 15th concerning *Scottia*-*Scottiella*. By one of those curious coincidences that I seem to encounter frequently, yesterday I was glancing over some papers on Japanese algae that had arrived a few days previously from a man of whom I had not heard before, Hiroshi Fukushima, and I ran across the name *Scottiella nivalis* (Shuttleworth) Fritsch, a cryophile or snow-alga. I checked the name in the 2nd edition of G.M. Smith's *Freshwater Algae of the U.S.*, and find that it is spelled correctly, though Smith cites it as *Scottiella nivalis* (Chod.) Fritsch. It belongs to the Oocystaceae, together with *Westella*. There is also a *Westiella* in the Cyanophyceae.

similar I suppose that on general principles it would be undesirable to have two such names as *Scottiella* and *Scottiella*, though they belong to different families. On the other hand, you have found *Scottia*, *Scottia*, *Scottea* and *Schotia*, and there are the *Westella* and *Westiella* mentioned above.

So if you agree, I propose that you should use the name *Scottiella*, provided that it is not pre-empted. Or if you feel that *Neoscottia* would be better I am prepared to go along with that, though I do not like it as well.

The missing link in the *Sachlania* mystery has turned up at last, after much searching. Yesterday I found a single specimen which has both poles of the 'fishtail' type, as I wrote you last week, and as shown on the enclosed sketch. So now it appears there are two distinct types, one with both poles of the slightly elevated kind, and the other with both poles fish-tail shaped, and that the first 13 specimens that I found are dichotypical combinations of the two. Sachlan wrote that he has seen 10 specimens, presumably dichotypical since I called his special attention to this point. So we have 23 dichotypical specimens against one each of what appear to be the two basic types. Why the great disparity in numbers? Another mystery! I am going to discuss this with Prescott early next month when I see him in Lansing, but I must confess that I do not see, at present, on which specimens we are going to base the new genus.

In one of his letters Prescott mentioned that Fritsch died recently; another great loss to algology. I had never corresponded with him, though he was good enough to send me an almost complete set of his papers, at Teiling's request I think. Miss Crossdale said that her visit with Fritsch was one of the best she had in England, and I had intended going to Cambridge to see him.

The Japanese papers mentioned above are written some in English, some in French, and others in Japanese with a resume in English or French. Those dealing with desmids are of value only as records of species found, all of which are well-known forms, and the desmid illustrations are not too good. The author's address is:

Dr. Hiroshi Fukushima, Biological Institute, Yokohama Municipal University,
Kanazawa-ku, Yokohama, Japan.
He would be glad to exchange publications.

Sincerely,

Sept. 19. 1954.

Dear Friend Scott,

Thank you for your two letters. I have forwarded Sioli's letter to Dr Kallio (and I ^{also} in a friendly way said that he must not neglect answering ~~to~~ letters').

As to the genus *Euastridium*: I do not categorically refuse to accept this genus, it was merely a suggestion. *Euastridium* is very closely allied to certain species of *Staurastrum*: *S. meriani* (apex 4-5-6-7-8-angul^{ar}, basal part circular); *S. capitulum* (apex 3-4-ang., bas. 10-12-crenate); *S. pileolatum* (apex 3-4-angul., bas. p. 18-21-crenate); *S. rhabdophorum* (apex 3-4-ang. with 3x5 or 4x5 marginal and 3x3 or 4x3 intra-marginal and 3x6 or 4x5 basal verrucae). These are only a few examples. The symmetry of *Euastridium* is therefore not contradictory to *Staurastrum*. The general shape is not different from *Staurastrum*. What is there then against the *Euastridia* simply *Staurastru*? I do not know. If there are no weighty reasons against *Staurastrum*, why then make a "new" genus?

You have of course also noticed the great resemblance (not identity) between your *Euastridium* and a hypothetical triangular form of *Euastrum turgidum* (var. simplex).

Then to "*Saccharia heteropolare*". Now, since you have found one specimen with both ends alike, I should not make ^{peculiarity} ~~this character~~ a generic character. There are enough of reasons why it should be separated from *Ichthyocercus*, which is its nearest relation. (e.g. the basal teeth, which are the only distinction between *Docidium* and *Pleurotaenium*). Furthermore the specific name "*heteropolare*" is not good. The first part is Greek, the second part Latin. Such mixed names should be avoided according to the Nomencl. Rules. (see Art. 25. Recomm. g. "nomina hybrida"). You could use "dispar", "dissimilis", "inaequaliceps" &c.

Please do not send ^d your reprints before the name is corrected

a new mistake will be avoided.

The home-made microscope was made of an ocular and an objective from my microscope, the tube was made of cardboard and Krieger's camera stative. We had to carry all our equipment in our knapsacks which were sufficiently heavy.

Good luck for your trip to Michigan.

And my sincere greetings to prof. Prescott.

yours,

Sept. 20th.

Today your third letter of Sept. 15. just arrived. Unfortunately also Scottiella cannot be used because there is a Scottiella Fritsch 1901. So there apparently remains only Neoscottia. But I must ask some friend to look after this name Index Kawensis and some Cryptogam Index. (I think Kallio can do it in Abo (=Turku). - Sachlania is not in Wittstein but perhaps Prescott or Taylor could look after in some more complete Indices. It seems always dangerous to create ^a new generic names and it is very difficult to control its validity.

I shall ask From Bot. Notiser copies of the Notice which shall be printed in the December number (or if you wish we can make such a typewritten slip ourselves as soon as possible).

I am glad that you that you take this matter in such a kindly way

I shall write to you as soon as the matter is OK, yours

R.G.

Sept 27 1954

Dear friend Grünblad,

It seems that my opinion on Euastroidium is the same as yours; I do not see the necessity for the genus. The generic diagnosis of Staurastrum has been stretched so far that these few additional forms could quite easily be included. When I found the first specimen of which I sent you a sketch, I queried it on the drawing as 'Staurastrum? Euastroidium?', and when I had a chance to look it up I found that it was probably Euastroidium staurastroides. Friend Teiling was good enough to copy Carter's illustration, which I did not possess, and it appears that my plants are somewhat different, enough perhaps to make a new variety.

After more hours of tedious searching I have now found a single specimen of 'Sachlania' with two fishtails, one on each end, like the sketch enclosed. So it appears that the first examples that I found are dichotypical combinations of the two types, one of which has both poles slightly elevated, and the other has both poles of the fishtail type, with erect spines at the angles. But now there is the problem of why the dichotypes should be so much more numerous than the 'basic' types. I have seen 13 dichotypes and Sachlan has seen 10, against one each of the equipolar ones. I have asked Sachlan to try and get more material, from several different parts of the swamp, and when I get it I may perhaps find the answer.

Here is the history of the word 'heteropolar'. Prescott originally suggested 'Bipolaris' for the generic name. Hannah Crossdale pointed out that this simply means two-poled and all desmids are two-poled. Her Latin professor suggested 'Allopolos', and she wrote that this should be pronounced with all the o's short, and accented on the second syllable, similarly to 'metropolis'. I did not like the sound, and devised the word 'Heteropolos', with the first 'o' long, and two accented syllables. Then there occurred to me the possibility of additional species being discovered without the feature of dissimilar poles, so I thought a non-descriptive genus name would be better, and proposed to use 'Sachlania', retaining 'heteropolos' for the specific name. But species names are usually adjectival, so I dropped the Greek termination -os and added the Latin one -are, which happens to make it coincide with the Latin word 'polare'. But the Latin 'polus' was derived from the Greek 'polos', so it seems to me that my name was not a hybrid after all.

But this argument is a moot one now, in view of the new developments. Teiling writes rather anxiously, begging me not to use the name 'Sachlania'. He says that such a great honour should be reserved for persons who have made important contributions to the science, and points ^{out} that in the Desmidiaceae only you and I have been so honoured. (Perhaps he does not altogether approve of 'Scottia' or 'Neoscottia', for it is quite evident that my small accomplishments can in no way be compared with those of such eminent authorities as Ralfs, de Brebisson, Borge, Nordstedt, et al.). So I am going to follow his advice, and devise new names for both the genus and the species, after discussing the situation with Prescott, whom I expect to see next week in Lansing.

My wife and I will be away for three or four weeks, and I shall not have an opportunity of writing you again until we return. I hope that your indisposition is gradually leaving you, and that soon you will be feeling like your old self again.

The reprints from Bot. Not. have not yet arrived. Perhaps the editor is holding them pending the publication of the correction, but in any event I shall not distribute them before we agree upon the method and wording of the correction.

ROLF GRÖNBLAD
KARIS
FINLAND

Oct. 25th, 1954.

Dear friend Scott,

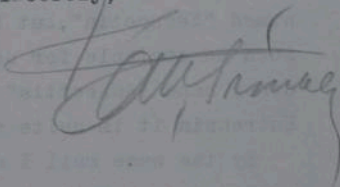
Thank you for your suggestion as to the rechristening of our troublesome friend Scottia. Today I have written to the editor of Bot. Notiser and proposed either Neoscottia or Amscottia. To me the first one seems nicer, the second one being (in my opinion) a little "constructed" but by no means unacceptable. There is an Australian fly named "Neoscotia", but belonging to the realm of zoology, it does not mean an obstacle for using "Neoscottia" in botanical sense. On the other hand "Amscottia" is certainly "free" and in accordance with Entransia it is quite acceptable. However, the editor may decide.

By the same mail I am sending to you a typescript which is intended for a publication in honour of Prof. A. Palmgren's 75th birthday (in Soc. p. Fauna et Flora Fennica). There I have made an attempt to theoretisize on the problem of *Staurastrum bibrachiatum*. Now I beg you to look through my text and make my bad English to correct

language. I should also be grateful for any suggestions and opinions that you may wish to propose or add. Moreover I think it would be correct to have you as joint author- if you give your consent to it. Please, return the manuscript as soon as you are ready.

I hope you and your wife have enjoyed your visit to Michigan. Professor Prescott has - I am told by Miss Croasdale - a very good library and a fine collection of copies of drawings (=iconotheca).

With kind regards, yours sincerely,

A handwritten signature in dark ink, appearing to read "J. A. Howell". The signature is written in a cursive style with a long, sweeping tail that extends downwards and to the right.

Nov 9 1954

Dear friend Grönblad,

I am returning the Ms. of your paper on *St. bibrachiatum*, after making some small corrections in your English. As to the title, I think Dr. Luther's proposal is somewhat more descriptive than yours, and it does not matter much whether you insert or omit the words "on" and "a", which you queried. Either way would be quite correct.

There is no necessity for my name to appear as joint author, for after all I did nothing more than make a few sketches, and had no part in the writing. Your proposal to describe a single species as consisting of three different forms is quite unusual, as you remark, and I think some people will not agree with it. I wish I had seen your paper before I went to East Lansing, for there I agreed with Prof. Prescott to handle a very similar problem in a different way. I refer to *St. Mildemani* and *St. subtrifurcatum* var. *major*, which occur in dichotomical combinations, along with two other previously undescribed forms. We agreed to describe all of them as varieties of *St. Mildemani*, which would not coincide with your views.

Of course I am in complete accord with your statement and that of W.H. Camp that a species must be considered as a kind of population, many of whose members may differ more or less from the individual specimen that was chosen as the type. In some species the differences of individuals are hardly great enough to be worth describing or recording, but in such peculiar cases as that of *St. Mildemani*, for example, I think we need names for separating and referring to the variant forms. For this reason I believe that the use of varietal names is justified, even though the different forms may not be true varieties in the technical definition of the word 'variety'.

My wife and I had a very enjoyable trip of 31 days and 6700 km, visiting many places that neither of us had seen before. Unfortunately our start had to be postponed for a couple of weeks, and as a result we ran into some disagreeable weather, cold, much rain, even the first snow of the season on two days, which I did not like at all. We spent a week in East Lansing with the Prescotts, who treated us royally, and I had some highly interesting and profitable talks with him on desmidological subjects. Most of the daylight hours I spent in his office, going through his iconograph in an attempt to identify the numerous desmids from Indonesia, Australia and Brazil that I could not find in my own library. Previously I had never seen an iconograph, and had little idea of how it was arranged. So I was quite surprised to find that it consists of four large drawers of a regular steel filing cabinet such as is used in business offices, with thousands of sheets of heavy paper of this size, each sheet bearing from one to twenty pasted photographic copies of the original illustrations. Each genus is in a separate folder, or many folders, the two largest ones, *Cosmarium* and *Staurastrum*, ~~xxx~~ each occupying almost whole drawer. The species are arranged alphabetically, which seemed rather strange to me, though perhaps it is the most practical one. But it results, for instance, in *Staurastrum O'Mearii* coming next to *St. Ophural*. It is very easy to look up any given species, say *St. furcatum*, and there you will find a dozen sheets with perhaps a couple of hundred illustrations showing every form that any author has described as *St. furcatum*. But when you have a number of strange desmids, as I had, there is nothing else to be done except to lay out as many of the drawings as you can in a reasonable space, and then wade laboriously through the whole *Staurastrum* section in the hope of finding something that will match. It took me two days, with some interruptions, to go through the *Staurastrum* section, and I cannot say that I did it very thoroughly, for I simply did not have the time. I did succeed in identifying some

of my unknowns, but there are a good many others that "passed through the sieve", as Prescott expressed it, and which remain as probably new species. Also there remained some others that I was not able to place satisfactorily because of the seemingly inextricable confusion existing in some groups like the *St. furcatum* already mentioned. Of course this was my first experience with an iconograph. Prescott has built it up himself over many years and naturally can use it much more easily, but if I were making one I would arrange the species in groups of similar characteristics, as in West's Monograph, and with a separate index so that a given species could be located instantly.

After leaving East Lansing we drove to Detroit, passing through Ann Arbor on the way, and I stopped at the Univ. of Michigan to see Dr. Wm. Randolph Taylor. Since my visit was unannounced and he was busy with his classes, he could only give me 15 or 20 minutes; nevertheless I had a pleasant chat with him and was glad to renew the acquaintance.

At Detroit we crossed into Canada, and drove through Toronto to Montreal, where we stayed for three days, seeing the sights. We called on Dr. Brunel at the Institut botanique and were very cordially received by him. He has a very pleasant place to work; a spacious office, a very good library and herbarium, a large laboratory with all the latest equipment, in a new and modern building. I would like to own a microscope like his; it is a binocular Zeiss, with a permanently affixed monocular tube carrying a 35 mm camera, so that when he finds a specimen that he wishes to photograph, all he has to do is to turn a knob to divert the light into the camera, and then push the button to make the exposure.

I talked with Brunel about Irene-Marie and the many mistakes in his works. He smiled and replied that he was well aware of the situation, but that unfortunately it was hopeless to try to do anything about it. He cited an instance that occurred a few years ago, when he (Brunel) was editor for the Canadian Assoc. for Adv. of Science, and had to reject a paper by I-M because it was so badly prepared. I-M shrugged his shoulders, figuratively speaking, and did not correct nor resubmit the paper, which consequently was never published. Brunel seems to be the only phycologist who knows I-M personally; none of the others with whom I talked have ever seen him.

I-M lives in the small town of La Pointe-du-Lac, about 90 miles from Montreal and directly on the highway between Montreal and the City of Quebec. I had intended driving to Quebec and calling on I-M on the way, but on the day we left Montreal it was raining heavily and the weather prediction was for more rain and possibly snow for several days. I don't like driving under those conditions, so I turned the nose of our car southward and headed down through Vermont into New Hampshire to call on our friend Hannah. We arrived in Hanover early in the morning and spent all that day with her, having lunch at her house, and she had dinner with us in the evening at the Hotel. She is a most remarkable person, so full of life and vim that it makes me feel like an old fossil. While we were looking for her office in the Zoology Building she passed us in the corridor, actually running, to answer a telephone call or something. I had some very enjoyable talks with her, and I think she appreciated my visit, for there is nobody else in Dartmouth who is interested in algae, and consequently nobody with whom she can 'talk shop'. She has adopted my suggestion of separating the mirror of the camera lucida from the instrument and carrying it on a separate stand, so as to make larger drawings. But now her drawings are really large; she showed me a sketch of a *Microsterias* that was twice as large as mine, and far too big for practical purposes. She also showed me a book on general botany for which she drew the illustrations some years ago; sections through plant stems, details of cellular structure, and all the other things that are shown in such a book. Her drawings are exceptionally good, equal to the best that I have ever seen; I did not know that she was such a good draughtsman.

Also I was quite surprised to find that Prescott, in addition to being a phycologist, is a very clever artist. The walls of his office are covered with dozens of crayon drawings in pastel colours, showing scenes from the many different countries that he has visited, all of them extremely good. How he finds time to do it is more than I can imagine.

After leaving Hanover we went to Boston, passing Harvard University on the way, and I stopped at the Farlow Herbarium to make the acquaintance of the Curator, Dr. I. McKenzie Lamb, with whom I have had some correspondence, on the subject of my collections which I have been depositing there for the last twelve years. A year or more ago, Hannah was doing some research at the Herbarium, and found that some of my ~~xxx~~ earlier collections had dried out more or less completely. Fortunately, from the first I had added 5% glycerine to all the vials, so they can be restored by simply adding more solution, which Dr. Lamb has kindly agreed to do, and also to seal the seal the screw caps with melted paraffin. (In future I am going to use 10% glycerine).

We stayed two days in Boston, seeing some of the sights, a very interesting city and one of the oldest in the USA, though far from being old in the European sense. Then we headed southwest on the return trip, and that is when we ran into more bad weather, having the ill-luck to travel just behind the front of a cold wave, one of those huge masses of cold air that periodically sweep down from the Canadian northwest and cover the entire central and eastern portion of the USA, even reaching down to Louisiana and Florida occasionally. In Virginia, which is considered one of the southern states, we ran for a whole day through a rather heavy snowfall, which fortunately melted when it hit the road, though there was plenty accumulating on the fields on each side. Probably this sounds trivial to you, but for us it was a real adventure, for it is only once in 15 years or so that we see any snow in New Orleans, and then it is only a few flakes that melt immediately. I have lived here for 48 years, and can remember only two occasions when there was as much as an inch or two of snow on the ground which lasted for a couple of days.

Now I must get busy answering the many letters that have accumulated during my absence. One of them is from Dr. Kurt Krieger, which I quote, in part, as follows:

Teiles

"Die Veröffentlichung des ~~TEILWAISE~~ Cosmarium der Desmidiaceenmonographie stösst auf sehr grosse Schwierigkeiten. Ein fertiges Manuskript existiert noch nicht, dagegen ein wohl vollendeter Zettelkatalog mit Zeichnungen, der jedoch in der Ostzone im Hause meiner Mutter steht und infolge seines Umfangs nicht ohne weiteres nach West-Berlin gebracht werden kann. Falls ich das schaffen kann, so erhebt sich die Frage, wo es einen Spezialisten gibt, der die Monographie vollenden kann. In Deutschland ist im Augenblick meines Wissens keiner zu finden. Ausserdem liegt der Verlag auch in der Ostzone und ich weiss nicht, ob er noch arbeitet. Die unglücklichen Verhältnisse erschweren hier jede Arbeit. Ich wäre Ihnen für eine Mitteilung sehr dankbar, wer nach Ihrer Meinung die Arbeit meines Vaters vollenden könnte."

Apparently, therefore, the work is far less complete than I had believed from what Dr. K. Krieger wrote me, and it is going to be extremely difficult to find anybody who can finish it. In this country the only one whom I believe capable is Prescott, and I do not see how he could possibly undertake it in view of all the other irons that he has on the fire; further, his knowledge of German is no better than mine, and I am sure he could not write it. The two whom you have suggested, Skuja and Messikommer, are probably capable, though I don't like Skuja's drawings, and I wonder whether Messikommer has sufficient knowledge of the desmids of other continents than Europe.

The reprints of *Scottia* arrived just before I left for Michigan, and I took one of them to Prescott, and cautioned him that the name will have to be changed. The others I shall not distribute until the correction is made. Either *Neoscottia* or *Amscottia* is agreeable to me. I like the latter better because it is more personal, and after all, every new name is 'constructed' (or artificial) when it is first used.

With my best regards and good wishes,
Your friend

Rolf Grönblad

Karis

Centralgatan 86

Nov. 17th 1954.

Dear friend Scott,

I had just yesterday written a letter to you and I was going to the post office when your letter arrived. So I have to rewrite this letter. Thank you very much for your interesting account of your long drive. It must have been an interesting journey but perhaps a little tiring. I see that you don't like cold climate. I think you would find Finland in the fall most disgusting. Cold, dark and rain -and snow. I who am accustomed to this miserable weather am longing for next spring every day. I should like to live in warmer countries. I am glad to hear about your visit to Hannah, because I know she was expecting to see you. I think she is a very agreeable person, always kind and ready to help and always interested in our dear Desmids. Your story about Irene-Marie was rather peculiar. I think there is nothing to be done.

It was useiul to you to see Prescott's iconograph. I

used to go to Dr. Forge, but his collection of figures was not alphabetically arranged, he had a system of his own which was quite a good one.

I am sorry you didn't like to join in my *S. bibrachiatum* paper. I made to you the proposal because even your material from USA and your drawings are used, and I did not like to do it without your collaboration. But if you permit me to do so, I am contented.

Some days ago I got from Miss Edna M. Lind in Uganda 5 small vials with Desmid material which was very rich in Desmids, especially exotic species. I have been examining them for the last days sitting up to 5 o'clock in the morning. It was more exciting than to read a thriller. Now I think you also are interested in the material, so I am going to divide fairly with you. I have made about 80 drawings but I shall not spoil your enjoyment by telling you what I have found. Only: the most beautiful Desmid was a new variety of *Euastrum spinulosum*. So far I have seen only 2 cells but a good drawing was made. (It was in vial no. 4). A giant *Pleurotaenium* was nearly 1400 μ of length (perhaps *P. eugeneum* var. or *P. simplicissimum* var.). Well, you may find out for yourself what there is. I shall

Rolf Grönblad

Karis

Centralgetan 86

try to send the vials by air mail.

The *Arthrodesmus* in your USA material are really very difficult. I am afraid of making too many "new" varieties and species, especially when there are intermediate and connecting forms. Do you know Lefevre in *Encyclopedie Biologique*? It is a most fascinating treatise on the polymorphism and variation in Desmids. You ought to read it. If you cannot get it otherwise I could lend it to you.

You will be interested to find *Staur. Wildemani* and *S. subtrifurcatum* together in one of the Uganda samples (no. 5). /cf. West 1896, Schmidle 1898, West 1900, Schmidle 1902! / Nordst. Index suppl. p. 122/.

Thank you for correction of my text.

I think I told you the new name of "Scottia" will be Amscottia. I have ordered for you. dr. Kallio and for myself reprints of the Note in *Bot. Not.* Dec. 1954 where the changed name will appear.

Kind regards, yours

ROLF GRONBLAD
KARIS
FINLAND

Nov. 19th, 1954.

Dear friend Scott,

A few words as a continuation of my last letter:

If you don't wish to join as an author I must ask your permission to mention the Mississippi finds of yours and to reproduce your original drawings from that material. I am not going to use your material and your drawings for my own papers if you don't like it.

I willingly admit that my proposal as to the "species" *S. bibrachiatum* may seem somewhat strange, but it is one way, -the other, more usual, is to describe varieties. Some of my professional botanist friends here did not disapprove of my ideas. Others have to decide, approve or reject. If my statements seem to you too categorical, you may take it as depending on the fact that I don't know all subtleties of the English language. The whole matter is to be taken merely as an "experiment". I have no iconograph of my own and this is a serious disadvantage. I feel greatly handicapped because it takes very much time to look through the hundreds of reprints. Up till now I used to rely upon my memory, but I admit of my memory not being anymore as good as in younger days. It is, however, too late to begin with an iconograph, so I have to go on in my old fashion.

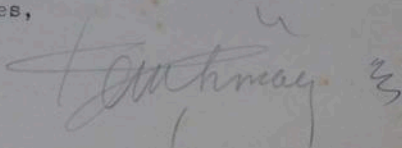
As to *Staur. furcatum*, this is a species very often misinterpreted by various authors. The species originally described is quite smooth without any granules.

A microscope like the one you saw at Dr Brunel's Institute I saw last week in Helsingfors in the Limnological Institute (Prof. Järnefelt). Such a one we should have!

It is to be regretted that Krieger's Flora was not finished. Apparently it will be very difficult to find any one who would be willing and capable of continuing the work. Teiling told me that Prof. Kolkwitz who has now escaped to Sweden proposed that several persons should divide the work. But that seems to me no good settlement. Messikommer has refused.

Again, best wishes,

*/from Carl Jensen.



Nov 24th 1954

Dear friend Grönblad,

I hasten to answer your two letters of Nov 17 and 19, for the tone of your second one makes me fear that I have, quite unintentionally, given you some wrong impressions, and I don't want even the slightest misunderstanding to arise between us. My thought about the authorship of your *St. bibrachiatum* paper was that the paper was conceived and written by you, and that you should receive all the credit for it. My own contributions in the way of specimens and a few sketches were so small that they would have been quite adequately recompensed by acknowledgements in the text. I should be glad to place such specimens and drawings at the disposal of any serious desmidiologist for use in his own papers, as I did with Dr. Krieger for example, though he did not live to use them. Also I sent material containing dichotypical specimens of *St. Wantzschii* to our friend Teiling, who mentioned them in his paper on "Radiation". But if you think it more correct, and if you wish to start our joint papers with this one, I am only too glad and happy to accept your generous offer. So everything is now OK, yes?

In my remark that some persons might not agree with your treatment of the various forms of *St. bibrachiatum* I did not include myself; to me it is, as you say, experimental. I meant persons like Prescott and Skuja who have their own fixed ideas which it is very difficult to change. For my part I should never venture to 'disapprove' anything that you did, for your experience and knowledge of desmids is so very much greater than mine. To be sure, I will argue with you, or with anyone else, about the classification of a desmid if I feel that I have a special knowledge of it; if I fail to convince you I will gladly accept your decision.

I shall be delighted to see the samples from Uganda, which will be only the second lot I have seen from that continent. About a year ago Krieger sent me two vials, one from Bukakata and Ripon, on the northwestern and northern shores of Lake Victoria, both in Uganda. Perhaps they also were collected by Miss Lind. They were rather poor in desmids, their principal content being diatoms and a very large number of Pediastrums, more than I have even seen in one collection. When your letter arrived I was looking at a slide of Borneo material, in which there are some nice specimens of *Euastrum spinulosum*, not quite the same as any of the forms shown by Krieger, but not differing enough to make a new variety. I am curious to see what your new one looks like; *E. spinulosum* is quite rare in my experience, though I found it once in Louisiana.

A few months ago Mme. Gauthier-Lievre sent me reprints of some of her papers, and in one of them it was mentioned that she possessed collections from West Africa and French Equatorial Africa, partly collected by her and partly by others. I wrote in acknowledgement of the reprints, and asked if she would be willing to exchange some of this African material for my collections from USA or from other parts of the world. Though there has been plenty of time I have had no reply from her, so I suppose she is not interested.

Yes, I do have Lefevre's work, and have read it many times. I wish that I also possessed his previous paper on the technique of desmid culture. It is quite interesting to learn that *St. Wilderani* and *St. subtrifurcatum* var. *maius* occur together in the Uganda material. I have found them together in North Australian material, and in several collections from Indonesia. Just now I am struggling to write a paper on their relationship, and am having a hard time with it. For a paper

of this nature is quite different from a simple listing and description of species found. However, I think I have succeeded in proving that var. *major* can have only a very remote relation to the USA species *St. subtrifurcatum*, if indeed there is any genetic relation at all. So I am transferring it to *St. Wildemani* under the name var. *subtrifurcatum* Scott & Presc., comb. nov. A strict adherence to the Rules would require the retention of the varietal name *major*, but this would result in a misleading name, for the variety is no larger than *St. Wildemani*, and for this reason I prefer to use the name var. *subtrifurcatum*. Do you think this violation of the Rules is permissible if I explain the reason in the text? You know the old sayings "Rules are made to be broken", and "There's an exception to every rule, even this one".

Glad to hear that *Amscottia* will be used instead of *Neoscottia*. I really like the former much better.

The weather here is very pleasant now, bright and sunny, with minimum of 10 and maximum of 20°C. Tomorrow, they say, it will be somewhat cooler, but it will warm up again in two or three days.

That's all for the present. With my best regards,

Sincerely

ROLF GRÖNBLAD
KARIS
FINLAND

Dec. 1st, 1954.

Dear friend Scott,

Thank you for your kind letter of Nov. 24th. You may be assured there is no misunderstanding whatsoever between us and there never shall be. You need not be afraid of that. - I am glad if you consent to have your name as a joint author on the paper on *Staur. bibrachiatum*. There is a little difference when you sent to Dr. Krieger or to Telling some specimens or drawings that they could use in their publications. To me you have sent all your American material together with your drawings in order to have them worked up and jointly published. I should feel I were misusing your trust if I should alone publish anything of this material. So everything is OK. You are quite right in saying that you will argue with anyone if you have an opinion of your own. As we are to work together we must tell each other quite frankly what we think. I am not any authority. You see?

Today I have sent by air mail 5 vials with the Uganda material. I hope you will enjoy it. There is one more vial with very many curious Desmids from Sudan (Lake Ambadi which lies on the Bahr el Ghazal, a tributary of the White Nile, an extremely interesting region, too remote for easy visitation. The water is much more acid (pH 6.8-7.2) than the White Nile (8.0-9.6).") They were sent to me by Mr. (Dr.?) Gerald A. Prowse, University College, Khartoum, Sudan. I have sent him today my Brazil Desm. and *Amscottia* papers. If you would send some of your papers too, I think Mr. Prowse would be very glad. Perhaps he will send us more of this extraordinary material. - You will find among other very peculiar species two new *Micrasterias* which are asymmetrical in the same way as *Amscottia*! and "*Sachlania*". Isn't it curious that such "critters" have been found simultaneously in Brazil, Indonesia and Africa? You may be prepared to find whatever oddities in those tropical regions.

I have asked Mr. Prowse to send more of the material if only possible. He sent also some photographs of his drawings - but but these are not very clear and there are only front views which are not sufficient for identification. There are some species which I have seen in the Uganda material too. But some new *Micrasterias* and *Euastrum* and *Staurastrum* are so funny that you will not believe your eyes.

I enclose my first microphotograph, it is only *Pediastrum* from Lake Victoria. But you see that I have succeeded. A friend of mine taught me how to manipulate with the apparatus and I found it goes quite easily.

Who is Mme Gauthier-Lievre? I don't know her. Has she published on Desmids? Her address??

Digitized by the Hunt Institute for Botanical Documentation

I have to change the list accordingly.

Dec 7 1954

Dear friend Grönbled,

Your nice long letter of Dec 1st just arrived, and I am happy that our small difference about the authorship of the paper on *St. bibrachiatum* is now satisfactorily removed.

The vials of African material have not yet arrived; it will take a couple of days more while they go through the Customs Office, where all packages have to be examined to be sure that no diamonds or other valuables are concealed therein. But your drawings of the new desmids from the Sudan are literally amazing! Long ago I promised myself that never again would I be astonished at any shape that a desmid might take, but I find that I cannot keep the promise. I am just as excited as you are, and shall hardly be able to sleep until the stuff gets here. And when it does arrive it will be goodbye to my work on *St. Mildesani* for some days.

It is indeed very remarkable that four asymmetrical desmids should have been found in little more than a year, by only two men, and from such widely separated regions. There is no other *Micrasterias* even remotely approaching either of your two new species, and only one other with long curving spines (*M. ceratophora*). Krieger thought that the latter was teratological from Joshua's drawing, and only began to believe in it when I sent him photos and drawings. What he would have thought of *M. miraculosa* I don't know; perhaps he would have said, like the man who saw a giraffe for the first time, "there's just ain't no such animal". And *Luastrium Frowsei* is totally unlike any other species that I have ever seen or heard of. And you ask if I will collaborate with you on this extraordinary stuff! FOOLISH QUESTION. I can hardly wait until I get some of them down on paper.

Your photomicrograph (not microphotograph) was not enclosed. No doubt you overlooked it in the excitement. It is fortunate that you started with an easy subject like *Pediastrum* which is nearly flat and very thin. Thicker ones are more difficult, because microscope lenses have such an extremely small depth of focus that you cannot focus sharply on the margin and upper surface of the desmids at once. There is one advantage in your larger camera as compared with my 35 mm; the grain of the ground glass is not so apparent. I attempted to overcome this by attaching a cover-glass to the focussing ground glass with glycerine, temporarily. It is somewhat of an improvement, but the refractive index of glycerine is too low, so the grain is still apparent and interferes with sharp focussing. Canada balsam should be used; it has the same refractive index as glass, and theoretically the grain ought to disappear, but I don't want to spoil the camera for ordinary photography.

I don't know much about Mme. Gauthier-Lievre. I think she is the widow of H. Gauthier-Lievre who is listed on P. 132 of Krieger's Monograph, and her name appears on page 664. Her address is: Dept. de Botanique, Université d'Alger, Alger, Tunisie, North Africa. She sent me three or four short papers on EW Algae from North Africa, but I cannot put my hands on them just now for my 'filing system' is not up to date. They contain few desmids and nothing startling; apparently her region is unfavorable for them. I do not know if she has published anything from Equatorial Africa.

Yes, I have the two books you mention on culturing algae, but the methods of Bourrelly and Kallio seem to differ considerably from those described. My friend Sachlan is thinking of attempting it, and Kallio has sent him copies of his papers.

a single or double granule immediately above and below the isthmus. Taylor showed the single granule on his illustration of a Newfoundland specimen, and this drawing was copied in Krieger's Monograph. I think I have called attention to this on one of my drawings. The figure in Prescott & Scott, 1943, Pl. 1, Fig. 4, does not show any granule, but I think this was an oversight, either on my part or Prescott's. The plates for this paper were drawn by Prescott, most of the figures being copied from my drawings.

As regards your question concerning *E. conicus* and *E. cuneatus* var. *solum*, I can only say that this is another instance where my opinion differs from Prescott's, and the determination was his. I argued with him about it, but did not succeed in convincing him, so I had to defer to his much greater experience and authority, as I would to yours. Enclosed are some sketches of some similar *Dusstra* that I have found recently from North Australia.

Yes, I think it would be an excellent idea to publish your English version of Krieger's obituary in the Bulletin of the Phycological Society. I suggested something like this to Prescott right after his death, but I believe that nobody over here knows much about his life (or yours either). So I shall write Prescott and ask him about it. In the meantime I shall gladly go over your Ms. and make whatever corrections may be necessary.

Here is something that you can do for me in return. Enclosed is the rough draft of a paper describing the results of Krieger's study of a small lot of Peruvian material that I sent him about 18 months ago. His son was kind enough to send me the original notes and sketches, and I have assembled them into shape for publishing. I have not yet drawn the plates. Dr. Krieger's script was very difficult for me to read in many places, and there are some words here and there that I am quite unable to decipher, which I have underlined in red pencil on the Ms. and his notes. Will you please see if you can make them out, and insert them. If not perhaps you can supply other words that will fit the context and the drawings. While you are reading it I wish you would see that I have the correct version of the German equivalents of the English "the", i.e., der; die, das, den, des or dem; and also that I have a sufficient number of accents sprinkled around. There is a problem in synonymy concerning *Cosca*, *trapezicus*, which I cannot solve, and I hope you can straighten it out. Also in the case of *St. alternans* I cannot understand K.'s description, part of which is deleted. Finally, what do you think of my statement concerning *Cosca*, "Scottii" and *C. depressum* var. *elevatum*? You have seen the latter from Brazil, and probably also the plants from Florida that I refer to, my drawings #1535-36. I intend to publish the paper partly in English and partly in German, so that my part in it will be quite clear. Do you think that is OK?

In the next few days I shall make up a package of literature for Mr. Browse, and shall ask Prescott to send copies of those of which my supply is exhausted, and some of his own as well. I have often wondered what kind of desmids were to be found in the vast papyrus swamps of the Upper Nile, and if Browse will send more stuff perhaps both of us will be able to find out. My small stock of African literature seems to show that the regions covered are not especially rich in desmids, nothing like Indonesia or Brazil, or even USA. Pritsch & Rich's papers on South Africa have a fair number, as also does West & West's Madagascar paper, but Bourrelly's and van Oye's contain comparatively few, and Schmidle's are not rich. So your material from Uganda and Sudan will enrich our knowledge of the Dark Continent considerably.

Will write again after looking at the samples. In the meantime, best wishes for Christmas and the New Year, from your friend

Dec 11 1934

Dear friend Grönblad,

Here are some more of Dr. Krieger's drawings, and mine, of the Peruvian desmids, accidentally omitted from my last letter. When you return these please send by ordinary mail; airmail is too expensive for a package of this size.

In that letter I mentioned an illustration of M. muricata by Taylor, relying on my memory which is worse than yours; of course the illustration is by G. M. Smith, probably from his Palisades paper which I do not possess.

During the last few days I have been reading up on African desmids, and I happened to see that Fritsch & Rich (1937) transferred M. incus var. validus to M. subulatus var. validus, a change which seems quite inappropriate. You will have to list this as a synonym if you make a new species of the plant, as M. validus.

I have sent all my reprints available to Dr. Frowse at Khartoum, and have asked Prescott to send the others. Also I wrote Mr. Frowse a short letter commenting on the extraordinary new species, in the hope of helping to induce him to send you more material from the Upper Nile region. The stuff is really remarkable both for the exceeding strangeness of the two Micrasterias species, for the other novelties, and for the great diversity of forms; the longer you look the more you find; and there is no imagining what will turn up next. The relative percentages of the various genera also are curious, but we shall have to wait until we get a complete list. It seems to me that this one collection would be worth publishing by itself, without waiting for more; what do you think?

So far I have seen about six specimens each of M. "miraculosa" and M. "sudanensis", all alike and with different semicells. My experience with "Caoblania" in which 24 dichotypical cells were seen, and then just one each of the two normal cells, has made me somewhat suspicious. Sometimes I wonder whether Amscottia actually exists in two different forms, each with symmetrical semicells, though in this case and in M. miraculosa the differences are so great that I can hardly believe it possible. On the other hand, I can conceive of two different forms of M. sudanensis with equal semicells.

The Uganda samples, or some of them, also are quite interesting, with a few novelties, but far outclassed by the Lake Ambedi stuff. If you know the location of the Uganda habitats, I should like to know them and see if I can find them on my map. Are you in correspondence with Miss Lind, and do you expect to get any more collections from her? If so you might call her attention to a large swampy region some 50 miles north of Kampala which might be a favorable place for desmids. My map shows a road running right through the swamp, Kampala-Bombo-Nakasongola-Masindi, so the area might be fairly easy of access. I shall gladly send her some reprints if you say so.

Sincerely,

Dear friend Scott,

Today I have finished the deciphering of the Krieger-Scott manuscript. I had good assistance of Dr. Lundström who is a teacher in German in our school. I think it is OK now. I have put in some suggestions ~~but perhaps~~ but I don't know if you will approve of them.

Here are some "pensées meditatives" about our asymmetrical Desmids. Now, afterwards, when the first excitement has cooled off, I must confess that I think our *Amscottia* might, perhaps, have been included in *Staurastrum* (with many ???). However, at its first appearance it looked very strange and well worth of a new generic position. But now, when we have seen that there are three other asymmetrical Desmids the generic separation seems less indispensable. -(Of course *Amscottia* can be justified)-. There is evidently no uncertainty or doubt as to the two asymmetrical *Micrasterias* from Sudan: they are true members of the genus *Micrasterias*, although extremely peculiar ones. As to your "*Sachlania*" -(I don't know what you are going to call it?)- I should not anymore hesitate calling it *Ichthyocercus*. There seem to grow some extraordinary and exceptionally built Desmids in the tropics, as if the creator-nature should have had an attack of insanity caused by the poisonous vapours of the tropical swamps. It is not at all unlikely or improbable that there are also symmetrical individuals of all these scarecrows - but we don't know them as yet. (Can you from my bad English find out what I mean?).

I have finished my material from Lake Ambodi (or is it Ambadi ?). I hope, however, that Mr. Browse is going to send us some more. I think it will be useful to send all my drawings to you, because there are some species of which I have seen only 1 or very few cells. I have not had time to try to identify all of them, so I hope you will make notes and complete my drawings, especially where I have failed to make "a vertice" or "a latere" drawings. (e.g. *Cosmarium*).

Many of the drawings in Schmidle's and Fritsch & Rich's papers are very poor if not incorrect too. It seems that Schmidle and the Wests did very often have quite different opinions as to the identity of species. Fritsch, although a skilled phycologist was no good taxonomist. Don't you agree?

In one of your figures of *Micr.* "*incredibilis*" (or what it may be christened to) you have drawn 5 spines at the top of the lateral lobes. Are you quite sure of it? (please forgive my question!). I have always

were more than 4. A drawing of the semicell with 4 lateral lobes would be useful but I have failed to make such a one. Perhaps, when we get more material there will be better luck.

Krieger's obituary in German is already finished and it will be published by the Soc. p. Fauna et Flora Fennica. The English version is ready for your corrections, ~~so I am going to send it to you~~ and I see from my "postbook" that it has been sent to you 10.xii. You may make any changes you wish to it.

Arthro. incus var. validus is of course not to be altered according to Fritsch & Rich.

You asked me about the Uganda habitats. I do not know much about them. Miss Edna M. Lind did not give any information on this point. From the labels in the vials I can read:

- 1) Uganda, L. Bunyoni
- 2) Uganda. Brick pit near Kampala
- 3) Dam near Mbarara
- 4) Dam near Soroti /this is the richest sample/
- 5) Dam 2 (Wera) near Soroti. I shall write to Miss Lind[†] and tell her what I have found and ask for more material from other places telling her of the swamp 50 miles to the N. But I think she is very busy with teaching in the College.

I shall now finish this letter, more in the next one. Cosm, polymorphum subsp. paulense and C. quinarium and other questions I shall try to discuss in my next letter. Lundell's paper is extraordinarily exact, one of the finest from the last century. I can send you a film with photocopies which you can have enlarged. If you wish any other films I can easily get them and send to you.

With Christmas greetings,
your,

I shall be away on a short trip to my sister between Christmas and New Year.

† She is going to write about plankton from Uganda.

The Krieger-Sachs manuscript to send back by the same mail. (The other papers later by ordinary mail.)

Jan 7 1955

Dear friend Grönblad,

Yesterday the postman brought a nice surprise, a tube with more of the Lake Ambadi stuff from Mr. Prowse, at least 20 times as much as you sent. I hope he sent some to you also; if not let me know and I will send you half of the new lot; there is plenty for both of us. There was no letter accompanying the tube, unless a letter had fallen out of the envelope, which was in very bad condition and had been patched up twice, once in New York and again in New Orleans.

I am very glad to get more material, for I had not completed examination of the first lot, and now I shall have an opportunity of finding additional specimens of species that appeared only once or a few times, and of which consequently I had not obtained all the information. It is curious that you found 104 desmids and I listed 106, but that each of our lists contains species not appearing on the other. And in the new stuff I have already seen several that are not on either list, for instance Gonatozygon aculeatum and pilosum (or pilosellum). Also I found a beautiful empty cell of Euastrum "Prowsei", and was able to get complete drawings of it; before I had seen only one cell and one semicell, and could not draw either of them because they were entangled in mucus. This plant, in addition to its strange shape, is most peculiarly built; it has a large and deep depression on the face, extending almost from the apex to the isthmus, seen in front view as a vague closed line, and only identifiable as a depression in top view, or rather an optical section a short distance below the top. And this morning I found and drew a fine specimen of St. Fuelleborni var. in which the hollow processes on one semicell were furcate at the extremity, while on the other semicell they terminated in simple spines; the latter is the most common form in this collection.

Here are a few comments on some of the Ambadi desmids: (Ambadi is correct) M. incredibilis. You say that one of my drawings shows 5 spines at the top of the lateral lobes; I suppose that you mean the four lateral lobes of the lower semicell. I have no recollection of this drawing, and I did not keep a copy of it, but you may be sure that if my drawing shows 5 spines, I actually saw 5. In the apical and basal views of a lower semicell that I sent you just before Christmas, you will note that some of these lateral lobes have 3 and others 4 spines. The number and direction of these spines seems to be variable, and I think it is easily possible that there may have been 5 on the specimen that you refer to.

M. "ambadiensis"! This is evidently the one that you refer to as M. depauperata v. nov. After receipt of your list I checked it again, and now think that I was too hasty in assuming it to be a new species, and that you are probably correct in making it a variety of M. depauperata, in spite of the obvious differences.

D. pseudostrontonema on your list is evidently the one that I listed as unidentified. Perhaps it may be pseudostrontonema, but it does not agree very well with W&W's original illustration, nor is it the same as the Florida plant that you identified thus, my drawings Nos. 578-582. The differences are: shape of cell, brown coloration and very thick wall, and especially the circular pads on the apical surface. I think the shape and position of these pads are valuable diagnostic characteristics in Desmidiaceae, they seem to be pretty nearly constant for each species. In the Ceylon plant the pads are broadly elliptical; in the Florida plant narrowly oval; and in the Ambadi specimens exactly circular. D. bengalicum has circular pads

Triploceras gracile v. bidentatum fa. I don't think the Ambadi plant should be assigned to var. bidentatum, in which the two spines are actually part of a bifid verruca.

If you do not think the African plant is a new variety (as I listed it), I suggest that it be referred to var. bispinatum Taylor, which I think is a perfectly good variety although Krieger regarded it as synonymous with var. bidentatum. Compare my drawing No. 605 of a specimen from Mississippi.

C. parialis. I have no illustration of this, only of var. minor Josh., and the zygospore of the species by M&W (FW, Alg. Maine). Nordstedt's Index lists it in two of Wille's works that I do not possess. Do you know of any other illustrations besides Cohn's? Fritsch & Rich's C. favum var. africanum is not well drawn, particularly the front view, in which the projecting granules at the apex do not correspond with ~~either~~ either their ~~front~~ top or side views. I agree with you that many of Florence Rich's drawings are not good, and that some of the determinations are incorrect, though I do not know whether she or Fritsch made the identifications.

A. arcuatus? var. "stellifer". Is this the very heavily built Arthrodesmus with the beautiful pattern of triangular pits on the face? and of a similar shape to A. convergens? If so, it does not belong to arcuatus which has upturned spines. I would make it a new species, because there is no other Arthrodesmus with any decoration whatever. If you don't agree as to the new species, it must be assigned to A. gibberulus Josh. (See remarks by M&W, Ceylon, p. 192, about A. curvatus and A. gibberulus).

X. cristatum v. Repondet f. africana Schm. This is the plant that I listed as X. subtrilobum? v. nov., and is evidently identical with Schmidle's plant. He comments on the resemblance to subtrilobum, and the resemblance would have been much more apparent if he had given a top view, for there is a relatively huge projection of the central rosette when seen a vertice, even greater than in subtrilobum which I know very well from Indonesia.

How to answer some of your remarks and questions. I am inclined to agree with your "pensées meditatives" that since we now have four asymmetrical desmids in three genera, the discovery of another new one would not necessarily require the creation of a new genus, even though its characters might not fit into any existing genus, like "Sachlania". In fact I am half-way ready to admit that "Sachlania" might be placed in Ichthyocercus, but before deciding on this I must wait for additional collections that Sachlan has promised to get in the next two or three months; also Prescott has a considerably say-so about the matter. However, I think he is not unfavourable to placing it in Ichthyocercus, even though he is not averse to creating new genera (Spinocosmarium!).

Your photomicrographs of M. sudanensis show the plant very clearly, but there are some improvements that could be made. The uneven lighting indicates that the lamp is not correctly aligned with the optical axis of the microscope, and should be moved slightly to one side or the other. The radiating streaks on the photos show (I think) that the condenser was raised too high; lowering it slightly would eliminate the streaks and give a more even lighting. The tiny black specks on the prints are caused probably by dust on the upper lens of the ocular; this must be perfectly clean.

I have heard nothing more about Brunel & Prescott's Index Desmidiaceorum, and strange to say I completely forgot to ask about it when I saw them recently. The reason undoubtedly is that the book is of so little value to me that I have only referred to it on two or three occasions. Prescott has applied to the National Science Foundation for a 3-year grant, and if he gets it, as seems not unlikely, he expects to publish a volume on Desmids of the U.S., or perhaps Desm. of N. America. He has named Hannah Croasdale, W. E. Wade, and myself, as collaborators in the project, but I do not know any details at present. Dr. Wade is a former student of Prescott's, and is now an assistant professor of botany at Michigan State College; I met him in East Lansing a couple of months ago. He is very enthusiastic about desmids, and after he has acquired more experience I think he will make valuable contributions to the science

The Krieger-Scott manuscript and Krieger's obituary have not arrived yet, but I suppose they are coming by ordinary mail. I asked Prescott about publishing the obituary and received a short note from him saying that he was going away for the

Christmas holidays and did not have time to write more fully, but that he would reply in detail upon his return about Jan 10th.

My wife and I went to Florida for the Holidays, as usual, and this time I drove all the way down the peninsula, almost to Cape Sable, the southernmost point of the mainland, in Lat. 25° N. I got within about 5 miles of Cape Sable, but could go no further because a bridge was being repaired. This region is an almost impenetrable mangrove swamp with salt water, but curiously the water becomes fresh again just a few miles to the north, though small mangrove bushes are growing along the roadside ditches. In fact I got a few desmids in material stripped from a mangrove root, and just a short distance further north the ditch is filled with aquatic plants (*Myriophyllum* and others) from which I got some good collections. All told I brought back about 30 collections, all from new habitats, though I had collected previously in some of the localities. Most of them are rich in desmids, but nearly all of ~~them~~ the species are already represented in my collections: there is one *Cosmarium* that may be new, and I have seen a form of *M. crux-melitensis* that is new to me, and another unusual one that seems to belong to *M. rotata*. Further examination may perhaps bring other novelties to light, but the results on the whole tend to confirm my belief that I have now obtained a very large percentage of the desmids of southeastern USA.

With my best regards, sincerely yours,

Jan 15 1955

"Dear friend Grönblad,

Enclosed is your Ms of Dr. Krieger's Memoriam; I have made some changes to bring your English into line with standard usage, and I hope you will not object to them. I thought of rewriting it and sending it in for publication in the Bull. Phycol. Soc., which Prescott says can be done, but it occurred to me that you might want to look it over first.

Thanks for your letter of Jan 7th with your drawings and discussion of *C. quinarium* and *C. polymorphum* v. *paulense*, etc. I shall have to wait for the arrival of Krieger's drawings and notes in order to compare them with yours. "*C. trapezicum*" was mentioned in one of K.'s notes with a reference to you, but just now I don't remember which *Cosmarium* he was discussing.

It is fortunate, and something like a miracle, that the second lot of material from Lake Ambadi reached here OK. The tube was wrapped with cotton and then with plastic adhesive tape, but then placed in an ordinary letter envelope which was almost falling to pieces. I am sending you half of it by airmail. This second lot gives me the impression that it is a different collection from the same lake. I say this because the notable desmids like the two *Micrasterias*, which were fairly common in the first lot, are much rarer in the second; and the second has a number of species that did not appear in the first; mostly small *Cosmaria* and *Staurostra*. I now have a total of about 134 desmids and am finding more on each new slide that I look at. Yesterday, for example, I found two new *Euastra*, one a new species for which I propose the name *E. disthaericum*, and the other belonging to the group of cruciform *Euastra* shown on Krieger's plates 90 and 91. It does not agree with any of them in front view, but from the top view I judge it to be closest to *E. bellum*. Unfortunately I was not able to make satisfactory drawings of it because the little devil was close to the edge of the cover-glass and persisted in drifting slowly. In my efforts to control it I finally lost it entirely, and can only hope that another specimen will turn up.

There are 4 or 5 very small *Cosmaria* that I have not yet drawn because I have been unable to make out the ornamentation, which I know to be present, and outline sketches without the ornament would be useless. Also some of my drawings are incomplete in that one or more of the lateral or vertical views is missing; perhaps your drawings and mine will supplement each other in this respect. Your list of 117 desmids was not enclosed in your letter.

When you first wrote about the Sudan stuff you spelled the sender's name Prowse; now you spell it Browse. Which is correct? His description of Lake Ambadi is quite interesting; I have seen fragments of *Ceratophyllum* and *Utricularia* in the material. In southern USA *Ceratophyllum* frequently grows in water of a rather high pH, and I do not regard it as specially favourable for desmids, though occasionally I have got some fairly good collections in squeezings from it. I am rather surprised to find so many of Fritsch & Rich's South African desmids in Lake Ambadi, which is 8° or 9° north of the equator. It is greatly to be hoped that Prowse will be able to get more material later on, in spite of the remote region. There is a town named Ghabat al'Arab, or Wang Kei, at the junction of the Jur river and the Bahr el Ghazal, and my map shows that it can be reached by road from Khartoum, a distance of some 1200 or 1300 km, but there is no telling what this road actually looks like, or if it practicable for an auto.

With my best regards,

Sincerely,

by Rolf Gebwiler
January 15, 1953
REVISED IN BIOGRAPHY
FILE HI

Dr. Phil. Willi Krieger, in Memoriam.

Dr. Phil. Willi Krieger was born October 21st 1886. He was a teacher but applied himself chiefly to scientific botany, especially to the study of freshwater algae. He made several excursions in order to collect material for his researches e.g. to Iceland, Roumania, Caucasus, Finland and Dalmatia. His graduation thesis was entitled "Zur Biologie des Flussplanktons. Untersuchungen über das Potamoplankton des Havelgebietes. 1927." His scientific activity was, however, interrupted for nearly four years by ~~World~~ World War I. being called out from 1915-18. In 1945 he was head-master of a school and as such was highly esteemed by teachers and pupils. At last, after having retired, he was working in electron microscopy at the Institute for Micromorphology of the Max-Planck Society. The results were published jointly by Dr. Krieger and Dr. Helmcke in several papers on the structure of the silicified cell-wall of the Diatoms as seen by means of the electron microscope. The principal work by Dr. Krieger was, however, the large Desmid Flora "Die Desmidiaceen" in Rabenhorst's Kryptogamenflora. As is known this valuable work never was finished by the author - a fact very much to be regretted by all students of the Desmids. Only the genera Spirotaenia, Mesotaenium, Ancylonema, Roya, Cylirocystis, Netrium, Penium, Closterium, Docidium, Pleurotaenium, Triploceras, Ichthyocercus, Tetmemorus, Euastrum and Micrasterias were published.

All these personal data are from Mrs. Gertrud Krieger and the list of the publications at the end of this article is from the late Dr. Krieger's son, Dr. Kurt Krieger. For this kind information I am very much obliged.

My first personal touch with Dr. Krieger took place when at the turn of the year 1934-35 we were invited together with Dr. Sven Thunmark by the grand old man of the Desmids, Dr. O. Borge, to stay over a fortnight in his home in Stockholm. In this very hospitable home we had extensive and instructive discussions on various questions of phycology. It was here I found in Dr. Krieger a good and friendly man. From that time there has been a regular correspondence between us which continued to his death.

The second time I saw Dr. Krieger was when in the summer of 1936 he came to my home in Karis. After staying only a few days there we set out for an excursion to northern Norway and Finnish Lapland. In the vicinity of Narvik in Norway and of Kilpisjärvi in Finland we made numerous excursions collecting freshwater algal material.

On these sometimes tiring hikes the good humour of Dr. Krieger never failed. He was also a very skilled photographer and his photographs, especially of the peculiar and beautiful vegetation of this arctic-alpine area, are a source of continual pleasure to me.

It was the intention of Dr. Krieger last summer to come to Finland where we were to meet with our mutual friend Mr. A. M. Scott from New Orleans. There would have been an interesting program to talk over and I think this would have been profitable to all of us. Unfortunately there were difficulties in the way of Dr. Krieger's getting his visa, our meeting was put off, and finally came the news of Dr. Krieger's sudden death on July 15th 1954.

In his work on the Desmids, his numerous smaller articles as well as in the great Rabenhorst's Flora, Dr. Krieger showed himself as being a distinguished expert of this family of algae, having a thorough knowledge of the very extensive literature on it. This work is no compilation but a quite original and up to date comprehensive revision. In the treatment of the species there may, of course, be instances where one cannot thoroughly agree with the author. But I think there is no possibility of anyone writing such a book in such a way that everyone would agree with all his arrangements. The older authors especially have made their diagnoses and their drawings so obscure, sometimes so incorrect, that it is nowadays almost impossible to know what the original specimens have really been. At any rate there can be different opinions as to their identity. - The part as yet finished of the Flora is very helpful and valuable. It is to be hoped that this "magnum opus" will be finished by someone else before the gigantic work set down in the notes by Dr. Krieger has become out of date. But this must be done very soon, indeed.

The name of Dr. Krieger will be honoured as long as people are working on the taxonomy of the Desmids.

A list of the papers on Algae by Dr. W. Krieger follows:

1. Zur Biologie des Flussplanktons. Untersuchungen über das Potamo-
plankton des Havelgebietes. - 1927. Pflanzenforschung, Bd. 10.
2. Die Gattung *Centronella*. - 1927. Ber. Deutsch. Bot. Ges. Bd. 45.
3. Zur Algenflora des Grossen Prüssnickses. - 1928. Verh. d. Botan. Ver
d. Prov. Brandenburg, Bd. 70.
4. Algologisch-monographische Untersuchungen über das Hochmoor am Diebel-
see. - 1929. Beiträge zur Naturdenkmalpflege, Bd. 13.
5. Algenassoziationen von den Azoren und Kamerun. - 1930. Hedwigia, Bd. 70.
6. Untersuchungen über Plankton-Chrysoomonaden. - 1930. Botan. Archiv, Bd. 29.
7. Die Desmidiaceen der Deutschen Limnologischen Sundaexpedition. - 1932
Archiv für Hydrobiologie, Suppl. Bd. 29.
8. Die Algen des Naturschutzgebietes Schildow. - 1933. Das Naturschutz-
gebiet Schildow, Teil II.
9. Kolkwitz & Krieger: Zur Oekologie der Pflanzenwelt, insbesondere der
Algen des Vulkans Pangerango in West-Java. - 1936. Ber. Deutsch. Bot.
Ges., Bd. 54.
10. Insam & Krieger: Zur Verbreitung der Gattung *Cosmarium* in Südtirol. -
1936/7 (Hedwigia, Bd. 76.
11. Süßwasseralgen aus Spitzbergen. - 1938. Ber. Deutsch. Bot. Ges., Bd. 56.
12. Die Desmidiaceen Europas mit Berücksichtigung der aussereuropäischen
Arten. - Rabenhorst's Kryptogamenflora von Deutschland, Oesterreich
und der Schweiz. Teil I, Lieferung 1, 1933. Lief. 2, 1935. Lief. 3,
1937, Lief. 4, 1937. Teil II, Lief. 5, 1939.
13. Krieger & Kolbe: Süßwasseralgen aus Mesopotamien und Kurdistan. -
1942. Ber. Deutsch. Bot. Ges., Bd. 60.
14. Süßwasseralgen aus Griechenland. - 1943. *ibid.* Bd. 61. 1950.
15. Desmidiaceen aus der montanen Region Südost-Brasilien. - *ibid.* Bd. 63.
16. Krieger & Helmcke, J.-G.: Feinbau von Diatomeenschalen in Einzeldar-
stellungen. Die Gattung *Achnantes*. - 1951. Zeitschr. für Wissensch.
Mikroskopie, Bd. 60.
17. *idem.*: Demonstration einiger raumrichtiger Rekonstruktionszeichnun-
gen von Diatomeenschalen. - 1951. Verh. d. Zoolog. Ges.
18. *idem.*: Feinbau der Kieselschalen der Diatomee *Cyclotella comta* (Ehr.)
Kütz. - 1952. Ber. Deutsch. Bot. Ges., Bd. 65.
19. *idem.*: Neue Erkenntnisse über den Schalenbau der Diatomeen. - 1952.
Die Naturwissenschaften, Bd. 39.
20. *idem.*: Kieselalgen im Elektronenmikroskop. - 1952. Kosmos, Bd. 48.
21. *idem.*: Untersuchungen über den Kammerbau der Diatomeenmembran. -
1952. Ber. Deutsch. Bot. Ges., Bd. 64.

22. idem : Feinbau der Diatomeenschalen in Einzeldarstellungen 3. Die Gattung *Melosira*. - 1952. Zeitschr. für Wissensch. Mikroskopie. Bd. 61.
23. Helmcke & Krieger: Diatomeenschalen im elektronenmikroskopischen Bild. I Teil. - 1953. Bild und Forschung, Abt. Biologie. Berlin-Wilmersdorf.
- ~~24. idem =~~
II Teil. - 1954. *ibid.*
24. Krieger, W. & Scott, A.M. 1955. *Einige Desmidiaceen aus Peru.*
In preparation.

ROLF GRONBLAD
KARIS
FINLAND

Jan. 20th, 1955.

Dear Friend Scott,

Two days ago I got a new sample from Mr. Prowse, this one better packed in a wooden case. (I enclose his letter which you may, please, return to me). The material is somewhat different in character from the first one. There are only very few *Micrasterias*, but on the other hand there are some species which I did not find in the first sample. (e.g. *Gonatozygon monotaenium* and var. *pilosum* Ndt?, *Staur. tripes* Ndt., *Hyalotheca undulata*, *Euastr. nov. sp.?*, &c.). Please note: all my identifications are merely provisory suggestions.

Here are some answers to your last letter:

- 1) *Desmid. bengalicum*, *pseudostreptonema*, *asymmetricum*, *occidentale*. The *L. Ambadi* *Desmidium* fits to no one of these, perhaps nearest to *D. asymmetricum* (not sensu *Irenee-Marie!*). As you say the connecting processes ("pads") in vertical view are very characteristic, nearly circular and situated near the centre, not in the angles. Shall we make a new species?
- 2) *Triploceras gracile* var. - Perhaps better new variety. Spines much longer and thinner than in Taylor's variety. In the basal part of the semicells the spines are paired and diverging from short processes.
- 3) *Cosm. pardalis* Cohn. Cohn's fig. is rather poor, but it is like our *Ambadi* species. And also like Fritch & Rich, which gives a good idea of the general habitus. Also the ornamentation looks very much like F. & R.'s fig. Wolle's *Desm. U.S. ed. II* has a fig. of the same type and is not quite bad. (I have only a photocopy of this work).
/Wolle's fig. in ed. II of *Euastr. Hastingsii* is more correct than W&W's fig. of *Eu. attenuatum* in *Desm. U.S. 1896/* - this species I also found in *L. Ambadi*.
- 4) *Arthr. "arcuatus v. stellifer"*. Of course you are right. I must confess that my identification was made from my memory - I didn't check any drawings. I should think: nova species? /*A. gibberulus* in Joshua is very poor/. The very thick cell-wall, yellow in color and covered with triangular pits is quite peculiar. (There are some *Arthr.* with decorated cell-wall: among others your *A.* from Fla., *A. nothochondrus* and *A. incrassatus v. cycladatus* in Wolle ed. II and in *Frw. Alg. U.S.*)
- 5) *Xanth. crist. f. africanum* - is better transferred to *X. subtrilobum* var. nov.
- 6) Thank you for your hints as to my photomicrographs. I have been very ignorant of the methods used, but now I have got a good book. I didn't know anything about "Köhler's method" which is very important to know and to use.

I hope to get better results after some time when I have had more exercise. But I was glad because I didn't altogether fail in my attempts.

7) Krieger's obituary was sent 10.XII. Krieger-Scott manuscr. was sent 22.XI. I remembered it was sent by air mail - but perhaps it was not so. Krieger's notes &c were sent 4.I.-55. I have sent a letter 8.I. by air mail. And 13.I. I sent all my drawings of *L. Ambadi desmids.* I hope all these will arrive safely.

I have now over 130 species from *L. Ambadi.* Here are some additions: *Glost. incurvum*, *Cl. tumidum*, *Pleur. baculoides*, *Eu. attenuatum* var. -, *Eu. capitatum*, *Eu. nova* sp. (6503), *E. elegans* f., *E. insulare* v. *silesiacum* f., *Cosm. meneghinii* f. *C. monomazum* typical, *C. obtusatum*, *C. pseudoexiguum*, *C. regnesi*, *Staur. tetracerum*, *S. unicolorne* Turn., *S. tripes* Ndt.?, *Spaer. granulat.*, *yaloth. undulata*

8) As you can see from my fig. of *Eu. cuneatum* v. *solam* (= *E. solum*?) the vertical view is considerably different from your Australian species. It is almost circular with 4 small "protuberances" clearly visible.

9) *Micr. "incredibilis"* seems to be somewhat variable as to the end spines of the lateral lobes, being 3-4-5 in number. Here I wish to pay attention to the variability in many details of many of the *Ambadi* species.

10) I cannot quite see why Krieger (*Sunda Exp.*) uses the names *Pleurotaen. tessellatum* (*Josh.*) Lagerh. and *P. trociscum* West when we have the old good name *P. verrucosum*. / Furthermore K. writes "tessalatum" (p.168 and pl.VI.) which is incorrect. / - *Penium pyramidatum* (W&W) Krieg. must be *Cosmarium*? On pl.VIII fig.14-15 Krieger writes *Cosm. connatum*. But *C. connatum* is in vertical view elliptical, not circular. On the contrary *C. pseudoconnatum* has a circular vertical view. I would refer Krieger's both fig.s to this species. In our Uganda sample 4 there are both *connatum* and *pseudoconnatum*.

With kind regards, yours,

11) Have you seen friend Teiling's new paper on *Actinotaenium*? And what do you think of it? It has been an immensely troublesome work. Perhaps it is a good idea to separate those cylindrical smooth species from *Cosmarium*.

Jan 29 1955

Dear friend Grünblad,

I have received the package of your drawings of the Lake Ambadi desmids, and the Ms of the Krieger-Scott paper, but Krieger's drawings and notes have not yet arrived.

Thank you very much for all the trouble you went to in correcting the Ms, which will make it easy for me to put in final shape. I cannot tell what to do about the few doubtful items, such as *C. polymorphum* "Paulense", *C. trapezicum*, and *C. Scottii*, until I get K.'s drawings and notes, and compare them carefully with your remarks. I do not see any use in transferring *C. polymorphum* to a new species, since there appears to be a lot of confusion already.

Your drawings are very interesting, and you have seen several desmids that have not shown up in my examination so far, e.g. *M. divisa*, *M. praemorsum* f., and a new *Euastrum* species with a large hemispherical swelling on the face and numerous verrucae. On the other hand I have quite a number that you have not seen in Prouse's first sample, most of them came from the second sample which contains a lot of different ones, mostly small *Cosmaria*, *Euastra* and *Staurastrum*. I now have close to 150, and am still finding one or two more in each new mount, so I am going to continue my examination as long as more of them appear. Then I will return your drawings and send you mine, with such notes and identifications as I can make, and you can pick out those that you think should be illustrated. Here are some more notes:

Euastrum "corpulentum". I suppose that by a stretch of the imagination this could be assigned to *E. aspullaceum*, but all I can see is a vague overall resemblance. Note that in vertical view the polar lobes of the two plants are quite different.

E. attenuatum var. I take this to be identical with *E. splendens* F. & R., which as you observe, is merely a variety of *E. attenuatum*. The illustration by Stephens in Fritsch & Rich's paper is not well executed in my opinion; it shows the apex of the polar lobe as crenate in front and side views, while the view a vertice shows an inner ring of circular granules and 12 corrugations around the margin. Thus Stephens' view a vertice corresponds rather closely with the USA and Lake Ambadi forms, which have an elevated or domed apex, with the same two rings of granules, of which the outer one extends partly down the lateral margin.

M. tropica v. "*sarculifera*". It seems to me that your varietal name is not quite appropriate, because in the Sudan plant the central feature is simply the usual swelling surmounted by either a single or double granule; quite different from the long cylindrical process in *M. mahabuleshwarensis* v. *sarculifera*.

St. Bidentulum Chld. I agree that there does not seem to be sufficient reason for making a new species out of this, and that it should be returned to *St. orbiculare* v. *denticulatum* Nordst.

That's all for the present. With best regards,
Sincerely,

Feb 7 1955

Dear friend Grönblad,

The 3rd sample from Sudan arrived safely, and I have examined a few slides from it. It is pretty much the same as #2, but I have found a couple of desmids that were not in the previous samples. One of them is *A. incus* v. *validus*, slightly different from my USA forms in that there is no flattening of the lower part of the ventral margins. Prowse's letter is returned herewith, and I am sending a short letter that he wrote me, which you will please return. His suggestion for a joint authorship with the three names in alphabetical order is perfectly satisfactory to me.

Also I have your supplementary drawings, and I think I have seen nearly all of the desmids, though I must admit that I had overlooked *Pl. minutum* v. "*Bourrellyi*", which is fairly common, and somewhat different from my form from Florida. I am still finding more desmids; there must be 160 of them by now, but pretty soon I shall have to stop. So far I have done nothing with the Uganda samples except to look at a single slide from each of them.

I have sent your obituary notice of Dr. Krieger to Prescott, and asked him to have it printed in the Phyc. Soc. Bulletin, also asked him to have some extra copies of the Bulletin run off and to send 25 to you. If you want more please let me know.

Telling has mentioned in his letters that he was preparing a paper on *Actinotaenium*, but I have not yet seen it and did not know what it concerned, for I looked in vain for the name in my reference works. So it is a separation of the smooth cylindrical species of *Cosmarium*! I shall be much interested in reading it.

Here are replies to some of your comments:

1. *Desmidiium* sp. I don't quite know whether to make this a new species. Off-hand I don't know where to look for an illustration of *D. asymmetricum*, and it is not in Nordstedt's Index or Suppl, so it may take some time to find it. Without having seen *D. asymmetricum* I think perhaps the Ambadi plant might be made a new var. of *D. bengalicum*.
2. *Triploceras gracile* v. nov. is OK.
3. *Cosmarium pardalis* Cohn. I have found two illustrations of this, Skuja 1949 XXXII, Figs. 5 & 6, and Bourrelly & Manguin 1949, Pl. 4, Fig. 45. Both of these illustrations show a cell that is rectangular in front view, and Skuja shows it as also rectangular in top view, the cell being greatly compressed from front to back. There is no resemblance whatever between these plants and *C. favum* v. *africanum*; in the latter the semicells are elliptical in front and top views. I have an incomplete sketch of a *Cosmarium* from L. Ambadi that may be *C. pardalis*; only 1 specimen was seen and I was unable to turn it over to see what the top view looks like. (Sketch enclosed).
4. *Arthrodesmus "stellifer"* sp. nov. is good. I do not know *A. nothochondrus* nor *A. incrassatus* v. *cycladatus*, since I have neither of Wolle's books that you mention. I have seen what I take to be *A. gibberulus* Josh. from Indonesia; it looks very much like *A. convergens* v. *mucronatus* Borge, in Scott & Presc. Arnhem Land, Pl. 11, Figs 9-11, but of course it does not have the apical spines.
5. I agree with the transfer of *X. cristatum* ~~cait.~~ f. *africanum* to *X. subtrilobum* v. nov.

8 & 15. E. "solum". I have seen two forms of this; one very narrow and with almost straight sides, the other with the base much wider, but have seen none with the double basal protuberance shown in one of your drawings. Your dichotylous specimen shows that the two forms are related, as I suspected from the Australian plants.

13. I have photocopies of three papers by Van Oye, but do not find them of much use. Apparently the Belgian Congo is poor in desmids, at least those places that have been investigated, but it is a huge territory.

14. St. Fuelleborni. The Ambadi specimen with bifid processes is quite similar to Schmidle's original illustration, but not identical. It has four processes instead of six, eight apical verrucae instead of 15, the basal verrucae are much more elaborated, and the basal portions of the processes much more inflated. I think we should make a new variety of the form with simple processes, and a *fa. bifidum* for the one with forked processes.

10. I also cannot see any difference between *Pl. tessellatum*, *Pl. trochiscum*, and *Pl. verrucosum*, except that there are no apical granules in *trochiscum*. (I have never seen anything that could be referred to *trochiscum*). In his monograph Krieger relegates *tessellatum* to synonymy with *Pl. verrucosum*, and transfers his Sumatran plants to the latter species. I have four or five forms that I can distinguish from each other, and some of them do not agree with anything in the monograph. As to *C. connatum* and *pseudoconnatum* I must confess my inability to distinguish between them in certain cases.

18. You say I may keep your Uganda drawings (not arrived yet) until I come to Finland. How about the Ambadi drawings? Do you wish them returned sooner?

19. Last week I booked our passage on the S/S "Ryndam", sailing from New York May 3rd, and arriving at Southampton May 10th, and have asked for a return passage in late August or early September. I propose to visit you during the last week in ~~July~~ and ^{June} the first one or two weeks in ~~August~~ if that will be convenient for you. I think we should plan on three weeks, if you think you can stand my company for that long! I shall have to wait a little while before I can tell the exact dates.

Here are some new comments:

M. alata. I think I have previously seen specimens of the typical plant with four terminal spines on the processes, but cannot remember whether it was from USA, Brazil, Indonesia or North Australia, and none of my existing drawings of the typical form show any more than three. However, the small new form from Unawalerke, N. Australia, has four spines, and the new variety, var. "parallela" from Oenpelli, N. Austr., may have two or three or four on various processes of the same semicell. Therefore I think that the number of spines cannot be used as a differentiating characteristic.

C. pseudoamoenum v. *basilare*. I think that the plant that you have identified thus belongs to the *C. elegantissimum* group, because according to W&W *C. pseudoamoenum* and *v. basilare* are elliptical in end view and their granules are irregularly arranged. In the Ambadi plant the end view is circular, and the granules are arranged in beautifully regular longitudinal and transverse rows. It looks very much like *C. elegantissimum* var. *simplicius* f. *major* F. & R. 1937, and I cannot see much difference between this and *C. mansangense* W. & W. I listed the plant tentatively as *C. mansangense*.

Hyalotheca undulata. In Ambadi I have a plant of very similar appearance to this, but narrower in side view than in front view, which would make the end view elliptical. I have identified it tentatively as *Spondylosium secedens*. L. 12; W. 8; I. 6.5; T. 6.5-7.0

Have you found *Gonatozygon* Kinahani in the samples? I have seen several specimens that might be this, but most of the apices are bulged outwards into a somewhat conical form, which frequently occurs in isolated cells of *Mougeotia*. So I am not sure which it is.

With my best regards, sincerely,

Dr. B. B. B.

Desmidiaceae from Lake Ambodi, Sudan. leg. Gerald A. Prowse, Khartoum.
Det. R. Grönblad and A. M. Scott.

- 4 *Cylindrocystis* (crassa?)
Closterium infractum f.
 parvulum v. angustum? f.
Pleurotaenium maximum
 trabecula f.
 minutum
 "- var. elongatum
 cylindricum?
 eugeneum?
 caldense
Docidium baculum
Triploceras gracile v. bidentatum f.
 verticillatum f.
Euastrum solum (= cuneatum v. solum Ndt.)
 sinuosum var.-
 nova species (No 6416)
 biceps
 pulchellum var.-
 elegans var. prorum?
 ampullaceum var. nov.
 subcrassum? var. nov.
 praemorsum f.
 nova species (No 6443)
 " " (No 6350)
 spinulosum v. burmense f.
Micrasterias pinnatifida f.
 radiata f.
 divisa var. nova
 "incredibilis", nova sp. (6352)
 "sudanensis", nova sp. (6353)
 ceylanica
 depauperata var. nova (6414)
 alata
 radians v. aequalis (=radians fig. 1 in Krieger D. Fl.)
 tropica var. surculifera, nova var.
Cosmarium cucurbita var. attenuatum?
 zonatum var. compressum
 floridanum (= *Cylindrocyst.* angulata W&W)
 pseudocoenatum var.-
 elongatum f.-
 diplosporum v. majus
 cucurbitinum var.-
 moniliforme
 pyriforme
 binerve
 norimbergense?
 gonioides f.-
 pseudopyramidatum
 variolatum var. cataractarum
 bioculatum? f.-
 contractum var. ellipsoideum f.-
 pseudonitidulum? f.-
 - (No 6421)
 * (No 6425)
 pachydermum? var.-
 Stephensii?
 pardalis (= favum v. africanum Fr. & R.)

- Cosm. taxichondrum var.?-
 monomazum var.-
 nova spec. -(6374)
 -- (6389)
 Engleri (= C.pseudosulcatum Rich)
 Salisburii Fr.& R. (= ? C.Bewsi F.& R.)
 pseudamoenum
 Xanthidium calcarato-aculeatum Schm,?
 antilopaeum? var.-
 cristatum var. Delpontei f. africana Schm.
 Arthrodesmus arcuatus? var.nova. ("stelliferus")
 triangularis
 Bulnheimii v. subincus?
 -- var. (6380)
 -- var. (6406)
 subulatus? "facies" triquetrus (or Staurastrum sp.)
 Staurastrum bidentulum Gbl. (= orbiculare v.bidentulum Ndt)
 Omearii
 corniculatum var.?
 Wildemani/subtrifurcatum
 Sebaldi v. ornatum?
 leptocladum v. simplex Fr.& R., f.-
 -- (6408)
 -- (6370)
 Fuelleborni var.-
 furcatum var.- (6445)
 -- var. minus Fr.& R. ("minor")
 Desmidioid pseudostreptonema
 Phymatodocis irregularis ("irregularis" Schm.)

Addenda (without drawings):

- Cosmar. exiguum
 decachondrum f.-
 connatum
 sp. (6263 Uganda)
 geminatum (Skuja, Burma)
 Hammeri
 glyptodermum (=Uganda)
 tumidum
 Closterium porrectum v. angustatum
 aciculare v. subpronum (=Uganda)
 Micraster. foliacea v. ornata
 Euastr. Luetkemuelleri (=Gbl, Brazil)
 Staur. brachiatum (=angulare)
 cuspidatum
 dejectum
 setigerum, typ.
 rotula
 Onychonema laeve + var. micracanthum.

ROLF GRONBLAD
KARIS
FINLAND

Febr. 18th 1955.

Dear friend Scott,

Here are some notes to your letter:

Arthrodesm. incus v. validus. I have at last begun to realize what this variety is like. I do not believe it has been found in England or in Europe at all although West 's Monogr. reports it. In no case have I seen it, not in Finland nor in Scandinavia.

Thank you for your help with the note on Krieger. I am content with 25 copies.

Didn't you get Teiling's paper on Actinotaenium? I will write to him. Perhaps it will be right to keep Actinotaenium separate from Cosmarium. But T.'s synonyms I do not like. It is, indeed, very difficult to decide what is what. Sometimes the old drawings and descriptions are impossible to interpret.

Desmidiium sp. from Ambadi is in my opinion too much different from Desm. bengalicum. D. asymmetricum Grönbl. is published 1926. I enclose two drawings which are somewhat different from the drawings published: the connecting processes (in vertical view) are more rounded, almost circular and not much different from L. Ambadi spec./ Didn't you get a copy of my "Finnländische Desmidiaceen aus Keuru, 1920?/"

Cosm. pardalis Cohn is obscure. I am sending a photocopy of Cohn's paper. (Also of Lundell).

Euastrum "solum" (or E. cuneatum var.-) appears to have two related but different forms: one narrower almost Tetmemorus-like with a simpler chromatophore and another more cuneatum-like with a more elaborate chromatophore. This difference as to the chromatophores is notable.

Staur. fuellebornei seems to be variable as to its ornamentation.

Cosm. connatum and pseudoconnatum as originally described are quite different. The vertical view of connatum is elliptical with "bicentric" chromatophores (two pyrenoids), while pseudoconnatum is perfectly circular with 4 pyrenoids. Later authors have confused them, erroneously, I think. L. Ambadi drawings you may also keep till you come to Karis.

I see you are intending for a long stay in Europe. Whenever you can come to Finland you are welcome. Beginning with the last week in June will be OK. I am sure I can "stand your company" as long as you wish to stay here. I hope next summer will be warmer and pleasanter than last year.

I think I also have to finish ^{at} examining the L. Ambadi material. Of course it is possible that some more species could be found / I have only quite recently found my only specimen of Xanth. sansibarense! Note the vertical view with a small protuberant thickening on each side in the middle.

Schmidle's fig. has no such median protuberances (nor your fig. from Borneo).

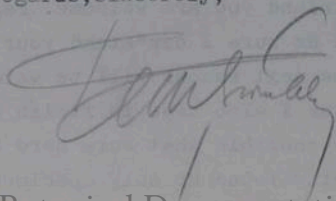
I cannot insist upon having seen *Gonat. kinahani* quite certainly. But I have seen something I don't like to call *Mougeotia*. The apices were not bulged outwards but almost straight and the apical angles somewhat thickened. The chromatophores were a little different from those commonly met with in *Mougeotia*. But I am not sure about this matter.

I enclose my first endeavour to make a description of *Micr. sudanensis* and *M. incredibilis* (would you propose better names?). The Latin and the English diagnoses are not quite identical. There are perhaps some lesser details omitted, but I think the drawings make such characters better evident than very long and very complicated descriptions with words. It would be useful if you would criticize them and make some amendments to them. When you have time, please, ~~4445~~ write some definitions at least in English to some of the new *Ambadi* species. I think you could be able to make them less clumsy than what I do. These asymmetrical species are especially difficult to describe. What should the two semicells be called? it is not possible to use "upper" and "lower" as in *Amscottia*. In *M. incredibilis* I have used "the simpler" and "the more complicated" semicell. Have you any better names?

I am still struggling with my photomicrographical apparatus. I am little by little making advancements but a lot of plates are still spoiled. I have tried with *Micr. incredibilis*, but it is very difficult because the processes are not in one plane. There is a method of highening or lowering the tubus by means of the micrometer screw: half time in one plane and another half time in another plane. ^{In this way,} I have succeeded in photographing the structure (e.g. scrobiculations) of the cell-wall of two of the *Cosmaria*. In drawings the quincuncial arrangement of such scrobiculae are made too regular: in reality they are not so. I think some such photomicrographs should be reproduced together with drawings.

Thank you for the picture-book from Florida. It is, indeed, pleasant to look at such pictures when we have here full winter, everything covered with snow. (In Lapland there is now $-40^{\circ}\text{C}.$!). What do you say of that?

My best regards, sincerely,



Rolf Grönblad

Keris

Centralgatan 86

11

12) Name of our Sudanese friend is Prowse, Gerald A.

Your post card from Florida (sent 30.XII) arrived 24.Jan.!

13) Do you know the papers by Van Oye? I got them from the auth some time ago : but the drawings are mostly very bad.

14) The bifid ended processes of *Staur. fuellebornei* are quite like *Schmidlès* typical species. I think this is really the same, only *Schmidlès*'s fig. is not so good as yours.

15) From *Euastrum "solum"* I have found a dichotypical cell (I can send later a photomicrograph). One semicell is broader, something like your *N Austral.* x-104.

16) *Krieger's* in Memor. is enclosed and rewritten with your corrections. Do you think there is too much of "myself" in it? If so, you ^{of} may make any changes you like. The life of Dr *Krieger* is very shortly told, but I did not get more information from Mrs *Krieger*. And I didn't like to ask once more.

17) your vial containing material II from Sudan arrived safely. It is not quite the same as material I. Some days ago I got from Mr *Prowse* a new

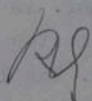
vial, sample III, also a little different from sample II. I can send you a portion of it if you like.

18) After some time I shall send also my Uganda drawings and a list of spec. You need not return my drawings before you have used them for comparison with your own drawings and made your notes and suggestions on them. Perhaps you should keep them till you can bring them with yourself to Karis.

19) Have you any plans as to your European trip next summer? I hope you are sure to come. We will have a lot of things to do. You are welcome whenever you like to come, but I should be glad to know of it in good time.

And, please, excuse me all my bad English! Because I know there are very many mistakes. I cannot help it. Very sorry!

yours

A handwritten signature in dark ink, appearing to be 'R. G.' or similar, written in a cursive style.

April 4 1955

Dear friend Grönblad,

Today I have returned by airmail to Dr. Lars von Haartman the galley proof of the paper on St. bibrachiatum, having made a few small corrections.

I notice that you have added a remark in the last paragraph regarding St. Rosei Playf., which does not seem quite correct to me. You will find a picture of this plant in Scott & Presc., Arnhem Land, Pl. 18, Fig. 8, and a description in the text (p. 82 in my copy, I don't know if yours is identical). Also I have a new variety from North Australia, shown on enclosed sketch, in which the upper processes are twice as long as the horizontal ones, and with a different construction at the ends of the processes. From these drawings I think you will agree that there is merely a very slight superficial resemblance to Dichotomum elegans W. & M., and no real relationship; therefore I thought you might want to omit or change the last paragraph. If you do, please write Dr. von Haartman immediately.

Sorry that I have not answered your letters sooner. All your sendings of Dr. Krieger's drawings and notes; your two supplemental lots of Lake Ambadi drawings; those from Uganda; and the photocopies of Wolle and Cohn, have arrived safely, and I thank you very much for them. In the next day or two I will write you a long letter. The time of my arrival in Helsingfors is set tentatively at June 25th, subject to confirmation of the plane reservations.

Sincerely,

April 4 1955

Dr. Lars von Haartman,
Universitetets Zoologiska Institut,
Helsingfors, Finland.

Dear Dr. von Haartman,

Herewith I am returning the galley proof of the paper on Staurastrum bibrachiatum by Dr. Rolf Grönblad and myself. There are a few small corrections which I have indicated in ink, to distinguish them from Grönblad's.

Today I am writing by airmail to Grönblad, communicating certain new information on Staurastrum Rossi Playf., which might perhaps make him desire to omit the last paragraph of five lines, or to change it, and am asking him to write you at once if he wishes to do so.

Sincerely yours,

P.S. Please order 100 reprints to be sent directly to me, together with the printer's invoice. This is in addition to whatever number Grönblad may have ordered for himself.

April 9 1955

Dear friend Grönblad,

I have sent you by registered mail 283 of my drawings of the Lake Ambadi desmids, and have also returned all of yours. A few of my drawings of *S. Wildemani* and *v. subtrifurcatum* have been sent to Prescott together with the 's.' of my paper concerning this desmid, for his approval, and he will return them to me. Also in Lake Ambadi there are the following desmids which I did not draw, either because they are so well known, or for some other reason:

<i>Cl. aciculare v. subpressum</i>	<i>Cosm. mic. Cucurbita</i>
<i>Fl. minutum</i>	<i>X. sansibarense</i>
<i>Dooid. baculum</i>	<i>St. tetracrum</i>
<i>M. foliacea v. ornata</i>	<i>O. laeve v. micracanthum</i>
	<i>H. mucosa</i>

There is a total of about 188 taxa represented in my drawings and the above list, and there are 12 or 13 more shown in your drawings which I have not found, making a total of very close to 200. Possibly Frowse's drawings may show some others that neither you nor I have seen. I think it must be very seldom that a collection has been worked over so thoroughly. The reason I sent them is so that you may have some extra time to consider them before I arrive, and also because I must cut down the weight of baggage since I am going by air from London to Sweden and Finland.

Frowse wrote that he is leaving the Sudan and does not expect to return there. That is bad news, for it probably means no more of this wonderful stuff from the Bahrel-Ghazal region. However, I suggested that he request his colleagues at Khartoum to send you any additional material they may get from that region. I wrote him immediately giving him my address in London, and asked him to get in touch with me by telephone on certain dates that I have free, provided he is also in London then. It will be very interesting to meet him in person, and I hope it will be possible.

The authorship of the paper, Grönblad Frowse & Scott, is quite allright, but such a string of names would be cumbersome as authors of the many new taxa. Therefore I suggest that your name alone be used as author of all new taxa. Possibly Frowse's name or mine might be attached to a few novelties that one of us may have seen and you have not.

I am returning your descriptions of *M. sudanensis* and *M. incredibilis*. I do not see how I could improve on your descriptions which seem quite adequate, and the names also are quite suitable. There seems to me no reason why the two semicells could not be referred to as 'upper' and 'lower' provided you add a phrase such as "in the position as drawn, which seems the most natural one". I have taken some photos, but none of them are suitable for reproduction; like you I am having trouble and spoiling a lot of film and paper. Sorry that I have not been able to write any more descriptions of the new species, nor shall I be able to do so before our departure, which is only three weeks away. But we can do this in Finland.

I received Teiling's *Actinotaenium* paper shortly after you wrote last time, and have read it carefully several times, but there are many things that I do not understand. One of them is why he excluded the species with sculptured wall, like *O. striolatum*; this seems very strange to me. I wrote him about this, and his reply says that he is convinced that "the granulate baculiform species belong to a quite different line of evolution, deriving from cosmarioid ancestors". Undoubtedly he must have good evidence of this; nevertheless I cannot get it into my head. Perhaps

a viva voce discussion with him will clear things up for me. But I don't see just what is gained by splitting off this one section of Cosmarium, and it seems to be a mere surmise that Krieger may have made similar revisions in his unfinished treatment of this genus. Dr. Kurt Krieger wrote me that his father had not prepared a manuscript, and that all that exists is a list in card index form, and a lot of drawings.

Have you seen Bourrelly & Manguin's "Algues d'Eau douce de la Guadeloupe et Depensances", 1952? It is a paper-bound volume of 231 pages and 31 plates, of which 11 are devoted to diatoms, 5 to desmids, and the others to miscellaneous algae. The desmids are mostly cosmopolitan, but he has a few new species and varieties. The book was published by the Societe d'Édition d'Enseignement Supérieur, 99 Blvd. Saint-Michel, Paris 5^e, and is rather expensive, - 1800 Fr., for its size and binding. Bourrelly expresses surprise at the comparatively small number of desmids as compared with the diatoms. I am not surprised, because from what I have been able to learn by reading about the West Indian islands, I gather that most if not all of them have soils that have been derived from limestone or serpentine, so that they are rich in calcium and magnesium. My own small experience in Cuba agrees with this.

No doubt you have seen Irene-Marie's latest installment in the Revue Algologique. This man is a puzzle. After his long essay on *Closterium cuspidatum* a few years ago, he now seems to doubt whether it is a desmid at all! And yet he has found it in quantity in the Canadian lakes. I have seen only a few specimens, but they are all alike, no matter whether they come from USA, Japan, Sumatra or North Australia. The plant is a typical *Closterium*, in shape, size, cell-wall, chloroplast, and moving crystals in the end vacuoles; the only point of difference is the terminal spines. And it is utterly incomprehensible how he can continue to spell 'Hughes' as 'Hugues', repeated 20 times or more in this paper. Of course he pronounces the word in the French fashion as 'eugays' instead of the English way as 'hews', but you would think his eye would register the difference in the printed appearance of the two words.

Well, I have most of the arrangements for our trip completed and paid for in advance, but there are still a few to be cleared up. For instance, I am flying from London to Stockholm, but the direct plane leaves too early in the morning, so I picked a later flight which means changing in Amsterdam. I have the confirmed reservation from Amsterdam to Stockholm, but not from London to Amsterdam. Also I have reservation from Stockholm to Helsingfors, but not from Helsingfors back to London by way of Copenhagen and Hamburg. And yet all the arrangements are being handled by an experienced travel agency, and I started working on it two months ago. It requires such telephoning and much running back and forth. Each trip from my house to the business district of New Orleans means the loss of almost the whole day. Still, it must come to an end pretty soon now.

With my best regards,

Sincerely,

ROLF GRÖNBLAD
KARIS
FINLAND

April 16 1955

Dear friend Scott,

Today I received your letter of the 9th. I was surprised to hear that you are leaving for Europe already after a couple of weeks. You will have a long stay in England then? I must tell you that I unfortunately sent you a set of drawings from Ambadi and Uganda two days ago by ordinary mail. So you scarcely will receive them before your departure. Can you make arrangements as to having them returned to me (by air mail)? Because there were some new species which I have not seen ~~before~~ earlier. Among others a desmid that perhaps could be interpreted as a f. ovale (not triangular in end view) of *Staur. wildemani* (with a photomicrograph). After numerous unsuccessful attempts I have also at last got a fairly good photo of *Micr. incredibilis*.

Meanwhile I am trying to write descriptions of new desmids in your USA collections.

I shall not send any more letters or drawings to you to America. In proper time I hope to hear from you from England. I hope it will be possible to you to meet Mr Frowse, it would indeed be interesting.

Miss edna Lind has promised some more samples from Uganda

I have not seen Bourrelly's nor Irene-Marie's last publications. My experience with the French authors is that you must beg and beg before you get anything out of them. So I think I must buy B. & M.'s book. I don't know quite how much 1800:- Fr. will be in Finnish Mark. - There cannot be any doubt whatever as to the fact that *Closterium cuspidatum* is a good Desmid! But Wolle has reported it as belonging to Protococcales and Wolle seems to be something of "noli me tangere" to I.-M.

I few days ago I got from "Akademiska Bokhandeln" in Helsingfors a new part of Kossinskajas Russian Desmid-flora. If you would like to have it /I think I sent you the 1st part?/ - I coul order for one copy for you? It is not expensive: only 700:- finmark. This part contains Penium, *Closterium*, *Pleorotaenium*, *Triploceras*. It is a big volume of over 800 pages, but there are also other Algae init. The book can be out of print very soon. It is written in Russian language.

Sincerely,

Apl 26 1955

Dear friend Grönblad,

Yes, I am going to spend five weeks in England before going to Sweden and Finland. We leave here next Monday, six days from now, and arrive in Southampton May 11th. I shall fly from London to Stockholm on June 20th, spend four full days with Teiling, and then fly from Stockholm to Helsingfors by Swedish Airline Flight SK 750, arriving in Helsingfors at 2.25 P.M. on June 25th. My wife will remain in England with my sisters in Devonshire. I shall return from Finland to England to pick her up, and then we are going on a 28-day conducted tour that will take in parts of Holland, Belgium, Switzerland, Italy and France, terminating in Paris. We shall stay another week in Paris, then fly back to New York on Aug 28th. The trip is more ambitious than the one I planned last year, but I figured that I might as well spend some more money on it, because it is the last big voyage that we shall ever be able to make.

Would it be best for me to proceed directly from Helsingfors to Karis? Or would you like to meet me in Helsingfors and stay there a couple of days, so that we could meet some of your friends in the Soc. Sci. Fennica? I will leave it up to you to decide, and you can write me in care of my sister, Mrs. E. M. Nixon, "Greenhow", Roundham Gdns., Paignton, S. Devon, England.

I have spoken to the postman about your letter with the Ambadi and Uganda drawings, and he will return it to you by airmail when it arrives. I shall leave with him sufficient money to cover the postage.

The *Staurostrum Wildemani* of oval shape sounds very interesting; this species turns out to be much more variable than anyone dreamed of. By this time I suppose you have received the photos of the North Australian variety with the three asymmetrically directed spines arising from the apical surface. Yesterday I sent to Java the Ms. and drawings of the paper by Scott & Prescott on *Staurostrum Wildemani*; it will be published in *Reinwardtia*, a new journal taking the place of the old *Bull. Jard. Bot. de Buitenzorg*.

1750

Fr. 1800 is \$6.15, which is Fr. ~~1800~~ at the tourist rate of exchange of 340 Fr. per dollar, or Fr. 2570 at the official rate of exchange of 231 Fr. per dollar. But there may be a quite different direct rate of exchange between Finnmarks and francs; your bank could tell you.

By the way, I would like you to ask your bank in Karis if they can cash my American Express Traveller's cheques at the rate of 340 Fr.; according to information from the Finnish Consulate-General in New York this can be done only in the large cities like Helsingfors, Turku, etc., and it would be highly inconvenient to have to go there every time to cash a check. Perhaps they could send my check to Helsingfors to be cashed. It will require some careful figuring, for if I have any Finnmarks left over at the end of my visit, I cannot convert them into dollars again, so the consul says. That doesn't seem fair, does it?

Will you please purchase for me Kossinskaja's book, and ask the Akad. Bokhandeln to mail it to: Mrs. Edna D. Coteh, 8324 Pritchard Place, New Orleans 18, La. She is a friend who lives just around the corner, and will keep the book until we return. I shall stop all mail deliveries to our residence. I will pay you for the book when I see you in Finland.

April 30 1955

Dear friend Grönbld,

A last note to say that I have sent you by registered mail all of your Brazilian and Uganda drawings.

Also I have sent a 1-pound can of the special coffee that we use in New Orleans. Please keep it until I arrive and we will enjoy it together.

En attendant le plaisir de vous voir, veuillez agréer, Monsieur, mes salutations empressées. In other words "See you soon".

Sincerely,

ROLF GRÖNBLAD

KARIS
FINLAND

(tel.:Karis 131.)

May 18th 1955.

Dear friend Scott,

Just a few words in reply to your letter of Apl.26th.

If you arrive in Helsingfors on June 25th it is not much use to stay there because it is Midsummerday, which is celebrated by all people in the country side so that the town will be almost empty. I am quite sure not one of my botanical friends will be in town. So I think it best that you continue directly by bus to Karis. The officials at the airport will direct you to the bus station and I will be waiting for you at the Karis end. (If you prefer to stay in H:fors immediately on arrival or wish me to meet you there, could you wire me from Sweden?). If you wish to look about in Helsingfors we can easily make a trip sometime later. Some of the botanists I know will be at Tvärminne Zoological Station, not far away from Karis. (Dr. Luther who is certain to be at Tvärminne told me today that he could take us on a motor boat excursion in the archipelago ("skerries") of Ekenäs for a day /or 2 if you can sleep in a tent? and if the weather is good /. Then Dr. Kallio is in Åbo (=Turku in Finnish) and I shall ask him when we can see him. The University is having its 3 month's summer holidays.

I have written to Akademiska Bokhandeln, Hfors, asking them to send a copy of Kossinskaja's book to New Orleans.

I have made enquiries about your traveller's cheques at Nordiska Föreningsbanken in Karis. I can tell you that my bank here in Karis can cash your cheques at the same rate as in Hfors, so that is OK.

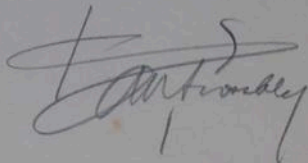
Your note of April 30th and the last set of my drawings also arrived

On May 7th I had a letter from Mr. Prowse from London. He will see you and talk about the Sudanese Desmids. I think it is very useful for our publication that you will be able to meet each other, and so you will tell me all about it. It would be nice to see Mr. Prowse, too, in Karis.

And so I shall finish for this time.

Best wishes for your voyage,

sincerely,



ROLF GRONBLAD
KARIS
FINLAND

Sept. 5th, 1955.

Dear Arthur,

I was really very happy when your letter arrived, because I had heard nothing from you since your card from Luzern. I already was afraid something had happened to you. I very well can think how tired you must have been after having seen nearly the whole of Europe. I should not like such a kind of running about. After my 4 weeks in Italy nothing could have stopped me taking the straight way home. Now I am writing diagnoses to the new USA desmids. It is - as you know - a troublesome and slow job. Sometimes I have to look at the original specimens, especially when there are no notes on the chloroplasts.

Mr Prowse wrote a nice answer to our letter and he seems not to feel offended. Here you have a few lines, : "I fully understand about the decision not to use my drawings, especially as the fine stippling would not reproduce well. The problem of getting different views under high power, which is undoubtedly necessary for seeing the finer wall ornamentation, is one which has troubled me. Where I have been able to indicate the ornamentation it has been from permanent mounts I have made, but this means that there must be enough specimens to give views in all three planes. I tried mounting in water, but the slight pressure of the oil-immersion is enough to turn end views sideways. Mr Scott says he uses a x20 eyepiece, but ^{with} the one we had in Khartoum I got a certain amount of distortion. What do you do about this problem? Thank you very much for sending the information to Dr Rzoska..... I hope to be off to Malaya at the end of September and I shall certainly be sending you all the Desmid material I can, fresh material by air if possible." I was very happy to get such a kind answer.

Would you try to get a good photo of *Cosmerium corrugatum*, n. sp. I have not been successful. And I think a photo would give a good idea of the "corrugations".

The name of my friend who was driving us by his car is Mr. Börje Ridd-
erstad (addr. Karis). I have seen him today and we have been talking about
photomicrography. As you know I am just a beginner and nearly 50% of my
plates are wasted. A few days ago Dr Luther was here for his investigation
in the river Svartån. We were talking on Teiling and his radiations and
Staurodesmus and Actinotaenium. Luther who is a very active and well-in-
formed professional botanist does not at all approve of Teiling's ideas.
This is a good consolation to me, because friend Teiling cannot bear any
critic from me.

I enclose a sheet of Mr Camp's letter that I have found on my table.

I don't know if you would have liked to see more of Finland and to vis-
it some other places in other parts of our country. But we had our work
and I thought that was the most important purpose for your being here, and
the time got on so fast that I didn't think of making any more proposals
for excursions. Now afterwards I have thought of it and perhaps it was stup-
id of me not to do more for your pleasure. If so, you must try to forgive
me.

Once more I wish to tell you that your visit was a great pleasure to
me and that it was very useful to me to have a talk on the Desmids, espec-
ially because I estimate your knowledge very much indeed. This I don't
say as a phrase of courtesy only, but it is my very true opinion.

My best thanks for the two books you sent me. They seem quite useful.

I wish you a good rest in England and a happy journey home.

Best regards, yours

If your brother in law wants more
seeds I can easily collect some more
of them for him.

Today I have seen *Mic. bilocata* var. *ornata*
in L. Anbadi's (He had killed the type according to
Purvis's drawing - which apparently is incomplete!)

Nov. 3rd 1955.

Dear Friend,

I am really anxious to know how you are. I am afraid you are not because you have not written a line since your return to New Orleans. Our friend Teiling too, some days ago, asked if I knew anything about you. So you see we should be happy to have some news from you.

I should not be surprised if you were tired after such a big tour through almost all of the countries in Europe.

Well, I am busy almost every evening (and night) writing diagnoses to the U.S.A. new Desmids. It is not at all an easy business, on the contrary a rather laborious and difficult one. I think you will have a great deal of correcting job with my English text. You are of course quite free to make any changes you like in order to make the text more like correct English. Of course you have to watch for other errors too.

There are a few cases in which I have changed the systematical names which we had agreed upon. But if you don't like my proposals, tell me so and we will find a "modus vivendi".

Have you done anything with those drawings of yours which you brought with you in order to make further investigations?

Your drawing 1198 *Cosmarium ralfsii* var. *spinigerum* n.v. has no vertical view and no chromatophores. Both are very important. Especially the chromatophores and pyrenoids (which are incorrect in W&W, Mngr II.) I have tried to find a specimen but I did not succeed. Would you try?

What do you think of a sentence like this (in describing *Euastr. protuberans* n.sp.): "In (the) centre of (the) semicells (with) a large protuberance." Could the articles and the preposition be omitted? The sentences in a diagnose need not be regular ^{and} complete but in an abbreviated style?

Now I have finished about one half of the whole pile. But there remain some bad things: *Staurastrum*, *Xanthidium* and the threadlike desmids. Shall I send to you what I have finished? I suppose you don't want to have *Arthrodesmus* separately published, but all in one paper? It seems to me not impossible to have all this material finished within a few, perhaps 2-3 months. What do you think of the inking? Who shall do it? Your Miss X, or my miss Tikkanen?

Have you done something more with Sachlan's Brasil. desmids?

Have you looked at my diagnoses of *Micrast. incredibilis* and

M. sudanensis? Please do it, and tell me what you think about them, or make any corrections you think advisable, and return corrected to me.

Do you remember that you "as a joke" proposed Amscottia to be named "candelabrium" ^X. Now I have seen in a book this name used: Euphorbia candelabrium! What a pity!

Today 4.xi. I got a card from the customs in Helsingfors telling me that you have sent me 200 cigars! I am very glad, and most grateful for your kindness - but a little ashamed too. The only one person who has got cigars as a gift from America is Jean Sibelius, our famous composer - and I am the second! I ought to be proud - and, indeed, I am. Thank you most heartily, my good friend, - but please do not do more such foolish things.

I am eagerly waiting for the last news from you.

With my kindest greetings,

If your original proposal was "Candelabrium"

I am just having a hard tussle with *Kautilia antipascua* var
and *X. cristata* var. It seems almost hopeless!

Simultaneously I send you a proof sheet from "Records from the Herbarium" call 1947-48 by H. Cronberg & M. Nord, so that you can see the plates as inked by Mrs. Tikkanen.

Nov 20 1955

Dear Rolf,

I am really ashamed for not having written to you and Teiling since my return, and I ask your forgiveness. The fact is that both my wife and I were really tired out, even with our month's rest in England. The flight across the Atlantic from London to New York was a miserable one! Fifteen hours sitting in the same seat, all in darkness, and unable to sleep even though we took sleeping pills. The noise from the engines was frightful, far worse than any other plane that I have flown in.

Since our return I don't seem to be able to get my mind down to business again, and have been puttering around the house, doing a little of this, a little of that, and a little of something else, and making not much progress in anything. I have taken some photomicrographs, most of them failures, but there is one negative of *Cosm. corrugatum* that looks fairly good, and I will send it to you as soon as I have printed it.

Glad to hear that Mr. Prowse did not appear to be offended with the letter that we wrote him about his drawings. I have heard nothing more from him, but hope that he will send you more material from Malaya. There have been a number of papers on desmids of the Indo-Malayan-Indonesian region, but there are plenty more novelties to be discovered. I have dozens of new species from Sumatra, Java and Borneo.

The form of *Micr. foliacea* that occurs in the Lake Ambadi material is var. *ornata*, as you have seen. I listed it in one of my letters but did not draw it; I don't remember why. It is exactly like the one that is found quite commonly in Indonesia.

You were not remiss in not suggesting other excursions in Finland. I think I got a pretty good idea of the country in the southern part of your country from our visit to Turku and our automobile rides with Mr. Ridderstad, also the train and motor trips to and from Helsingfors. By the way, your taxi driver thought I wanted to go to a hotel in Helsingfors, and I could not make him understand that I had to go to the airport. So we were at an impasse, but fortunately I had the address of the PAA Airlines in Helsingfors, so I went to their office and got one of the girls to go outside with me to the taxi and tell him to take me to the airport. Even then he did not know the road very well, and took a wrong turning which was not discovered until we had gone 15 km on the wrong road. So we finally arrived at the airport with just about five minutes to spare before the plane left. But the girl from the office apparently had phoned to the airport to tell them that I was coming, and they would probably have waited a short time if we had been late. I have sent a box of small cigars ("cigarillos") to Mr. Ridderstad and hope he gets them OK. Smoke one of them and if you like them I shall send you a box also.

It is not necessary to send any more seeds to my brother-in-law. He has all that he can possibly do in keeping up his flower garden, his orchard of 20 apple and pear trees, and his vegetable garden in which he raises a dozen kinds of vegetables. He is two years older than me, and though he is much more accustomed to outdoor work he admits that all this is getting too much for him, and that he will have to slow down. One day when I was out walking with my sister we passed the bush

with the red-and-yellow flowers, similar to the yellow broom plant in your garden. It was September and of course the flowers were all gone, but there were no seed pods. Possibly it is a sterile hybrid, one of the many exotic plants that were given and planted in this public garden by a horticulturist. However, I am sure that my sister and her husband will keep this in mind, and get seeds for you if there is any way of doing it.

I am glad to hear that you are making such excellent progress with the tiresome and difficult job of writing diagnoses for the new USA desmids. If you will send me those that you have ready I will go over them and make any slight corrections that may be required in your English. Of course you must send me the drawings to go with them. You ask about the sentence "In center of semicells a large protuberance", omitting the articles and preposition. In my view this is allright, though there are many purists, like Prescott, who would not like it. I can see no reason for writing a lot of unnecessary words; that is why I adopted Irene-Marie's abbreviations like "sep" and "cap" (sine et cum spinibus), and omitting the Greek letter mu for microns. I believe that the genus *Arthrodesmus* should not be published separately. The new species and varieties should be included in the paper describing only novelties or poorly known species. The other *Arthrodesmus* should be included with the other genera in a later and more general paper dealing with the species already known.

As to the drawings, I think that I should make the pencil layout on the plates and send them to you so that you can have Miss Tikkanen ink them. This will be quicker (and cheaper!) than having my friend do them. She now has two small children who keep her pretty busy, and she seems to have little spare time. Of course I shall be glad to pay the cost of Miss Tikkanen's work, or to share it with you if you would feel too badly about this.

Cosm. ralfsii v. *spinigerum* n.v. I found three specimens in two collections, and enclose sketches of two of them. In the vertical view it would seem that the lateral lobes of the chromatophore ought to be furcate, according to Telling, but the drawing ~~shows~~^{shows} them the way I saw them. In none of the three specimens could I distinguish the pyrenoids, though one specimen was stained with iodine. However, I got the impression that there are several pyrenoids located in the ridges of the chromatophore, pretty much as West shows them, Monograph II, Pl. LVII, Fig. 10. However, West's drawing is incorrect in showing the chromatophores of the two semicells completely separated. In my specimens the ridges of the two semicells all converge towards the nucleus and isthmus, and the two central ridges (in front) view appear to pass through the isthmus to join the corresponding two of the other semicell.

While looking for this *Cosmarium* I found a specimen of *X. Torreyi* with well-preserved chloroplast showing two pyrenoids per semicell, and made a sketch of it which is enclosed. The two pyrenoids distinguish it from *A. impar* which has only one per semicell. The shape of the cells in the two plants is quite similar, the only difference being the very narrow, almost closed, sinus in *X. Torreyi*, and the consequent alteration in shape of the basal angles. Also enclosed is a sketch of another relative of this group that I found a week or so ago while looking at some Sumatran material. Only one specimen was seen and I was not able to turn it for the other views. I suppose it to be an *Arthrodesmus* because it has only one pyrenoid per semicell, though it shows some similarity to *X. sexmammillatum* and var. *pulneyensis*, which however has 2 pyrenoids.

You asked me to return your diagnoses of *M. incredibilis* and *M. sudanensis*, with corrections, but I find that I returned them to you in my letter of April 9th. Perhaps you filed them with my letter instead of with the Sudan material and drawings.

You also asked if I had done anything more with *Sachlan's* Brazilian material, but I think you must mean *Gioli's*, because *Sachlan's* came from Indonesia. No, I have done ~~that~~ nothing more on it, and am afraid that I shall have to get some other things out of the way first, as you also will have to, I believe. But if you wish to go ahead with it, I can perhaps put off something else. What do you say?

I have made a few attempts to find more specimens of the desmids on the drawings that I brought back with me from Karis, but with not much luck so far. In three cases I have been able to get the information on the chloroplasts that you wanted, but some of these desmids are going to be very hard to find, perhaps impossible. One of them was found in collection Louisiana No. 7, but this collection no longer exists. Some years ago I dropped the vial on a concrete floor and it smashed to smithereens and I was unable to recover any of the sediment. So in this case we shall have to wait until ~~ix~~ the plant turns up at some future date.

The proof sheet showing some of Miss Tikkanen's drawings has not yet arrived, but if you are satisfied with them I am sure I shall be also, and everyone else.

I agree that *X. antilopaeum* and *X. cristatum* are terrible tangles to try to systematize, especially with the several new forms from USA. Once you proposed that we should not add any more new varieties to *antilopaeum*, and I think that is an excellent idea.

Your letter and one from Teiling received yesterday have made me realize that I must get down to business and produce some results, so I can promise that you will see a difference from now on, and you will not have to wait so long for answers to your letters. Teiling has ^{asked} for sketches of desmids that exhibit asymmetry in various respects, and I have several that I shall copy and send him in the next day or two. Now I must stop writing and copy two sketches that are to go in this letter, so it will catch this evening's post.

With my best regards, and apologies for my neglect,

Your friend

Dear Friend,

Now I have got three sendings from you, and I thank you most heartily for them all. The cigarettes, the two papers on Louisiana and the book which looks very interesting. Many thanks for your kindness.

I am going on with the descriptions of your desmids from USA. I have come so far that I think it is not impossible to have them finished before the end of this year. They are written on sheets, one sheet for every species or variety. I think it is not needed to rewrite the text because the printer most probably can use the sheets for printing. Only, you have to correct my poor English and you may perhaps also have some other changes to make if you cannot approve of my systematical manipulations. Howsoever, I am very curious to know what you think of my work.

Some other topics.

Have you noticed the strange likeness between *Micrasterias moebii* (Borge) W. & W. var. *burmense* W. & W. /Krieger, D.Fl. 107:7-8/ and *Euastrum turgidum* Wall. /cf. Krieger, D.Fl. 89:7-8/. Well, there are differences, but not nay such of great importance. Both of them have the double polar lobes &c.

Your fig. 1677 you had originally called "*Cosmarium*" tortuosum but then changed into *Euastrum*. The more I look at it, the more I feel inclined to call it *Cosmarium*, especially after having seen specimens of it. I would like to call it *Cosmarium*.

Do you think *Cosmarium* /*Penium*/ *corrugatum* could be identical to Wolle's *Calocylindrus costatus*, Desm. U.S., XII:13? One must always think of the inaccuracy of Wolle's drawings.

Could you try to make a drawing of the chromatiphores in *Xanthid. wewahitchkense*. I cannot decide whether there are in each semicell 4 plates with one pyrenoid in each, or each plate with several pyrenoids (it looks so)?

From your letters I see that you don't have several papers by Nordstedt, Schmidle, myself &c. If you tell me which papers want I should be glad to send you photocopies!

I hope very soon to have a letter from you.

With kindest regards,

Hannak has promised
in a letter to send the
correct the Latin diagnoses.
And she will do it without payment.

Dec 3 1955

Dear Rolf,

Glad to have your letter of Nov 22nd, and to know that you received the packages safely. Please tell me if you had any trouble in getting the cigarettes and how much import duty you had to pay on them, so that I may know whether it is worth while to send you any more.

I have tried to make a drawing of the chromatophores of *X. wewahitchkense*, but without much luck. There are plenty of specimens in the collections Fla. 27 and 137, but in all of them the chloroplast is so much decayed that it is impossible to see the structure, except that there are 4 plates per semicell. In Fla. 182 there are a few specimens in a somewhat better state of preservation, and I have sent you a slide containing three specimens, in one of which it seems that there are about 10 or 12 pyrenoids in each plate. However, I am not certain that these are really pyrenoids; they might be artifacts resulting from the disorganization of the cell-contents. Also enclosed are two sketches showing what I have seen, but as I have noted, I would not like to show this on a formal drawing unless you can confirm the observation. With your better control of the staining process you may be able to get more information from examples in Fla. 182, and if you do you could insert it on my drawing.

The funny-looking plant in my drawings 1677-8-9 is one of the borderline forms that could be assigned to either *Cosmarium* or *Euastrum*, and whatever you do will be wrong in the eyes of other desmidiologists. Krieger thought it should go in *Euastrum* after he had seen my drawings and actual specimens, but if you wish to call it a *Cosmarium* it is OK with me.

After examining Wolle's illustration of *Calocyclus costatus* I think there is quite a possibility that his plant may have been the same as our *Cosmarium corrugatum*. His drawing shows a much greater constriction at the isthmus, but may not be an accurate representation. His dimensions are greater than ours, but the ratio length/width is just about the same, 1.8 for his and 1.9 for ours. The color of the cell-wall in his description and the number of visible 'costae' are just about the same as in ours. So I suppose we must change the name to *Cosmarium costatum* (Wolle) Gbld. & Scott, though the adjective 'costatum' seems a misnomer and 'corrugatum' would be better though not permissible.

When I received the *Micrasterias* fascicule of Krieger's monograph, I noticed immediately the remarkable resemblance between *M. Moebii* v. *burmense* and *Euastrum turgidum*, and I was quite surprised that Krieger had not transferred one of them, in view of his propensity for making changes and new combinations. I have seen several varieties of *M. Moebii* from Indonesia and N. Australia, also a few examples of *E. turgidum*, but there are some specimens that I do not know what to do with, for they could be assigned to either one. Further, *M. Moebii* and its varieties are quite variable; the upper lateral lobes may be entire or divided, and in one semicell these lobes may be divided on one side and entire on the other, as I have shown in the Arnhem Land paper, Pl. 8, Fig. 6.

On p. 624 of his monograph, Krieger lists *M. Moebii* v. *javanica* Gutw. (1902) S. 603, Taf. 40, Fig. 58, as a synonym of *E. turgidum*, but on p. 43 of the *Micrasterias* fascicule he recognizes it as a good variety and refers to the same illustration, but without any explanation of his former listing. On his Plate 107 he attributes Figs. 9 & 10 to West, and the side view Fig. 11 to Gutwinski, but there

is nothing like Fig. 11 in Gutwinski's 1902 paper, and I do not know where it comes from. This var. *javanica* seems to depend principally upon the existence of raised verrucas on the central tumor, which would be very unusual. In all the forms of *M. Moebii* that I have seen the tumor has deep pits, which in well-developed specimens are triangular in shape and arranged in a hexagonal pattern.

Hannah has sent me a reprint of the first part of her paper on Alaskan desmids; very well written and excellent illustrations. But she has drawn many of the *Euastra* with the left and right margins slightly different, or in a skewed position, which gives them a curious appearance to me. Yes, I know that sometimes there are such irregularities, but the large majority of desmids are exactly symmetrical in front view, and I select perfect specimens for illustrations, if I have several to choose from.

There are other species of desmids which ^{exhibit} a constant and regular asymmetry, usually in end view, being twisted around the vertical axis. Teiling wrote me recently about this, and I have sent him a number of drawings which represent a considerable addition to his list.

We have had our first cold weather this week. On three days the lowest temperature was 33-34° F. (1°C.). This is a month too soon; usually we do not get such cold until the end of December or January. If it drops two or three degrees below freezing it kills some of our subtropical plants, like bananas and poinsettias.

You asked me to let you know the names of papers that I do not possess. It will take some little time to look them up and list them, and I will do so later. Now I am working on the drawings of USA desmids that I brought back with me, but it is a tiresome business looking for one certain desmid, which may be extremely rare, among a million others.

With my best regards and good wishes,

Sincerely,

Dec 31 1955

Dear Rolf,

A few days before Christmas I received the proofs of Miss Tikkinen's illustrations for your new paper. I like them very much; the lines are nice and smooth and of even width, not an easy thing to accomplish. For my drawings, most of which will be double-lined, I think she could make the outer line just a trifle heavier, and the inner line the least bit lighter.

In this paper you have a plant that you have identified as *Euastrum everettense*, which undoubtedly is the same as what Prescott called *E. obesum* v. *crassum* in our 1942 paper (Desm. from Mississippi). I don't think that was a good determination, as I think I have mentioned to you before, but I am not satisfied that the plant can be identified with Wolle's *E. everettense*. Of course you have noted that Krieger thought Wolle's illustration represented *E. ampullaceum*, which of course is incorrect, but it shows how the inconsistencies in Wolle's drawing gave an erroneous impression to such an experienced man as Krieger. My own idea is that the plant should be made a new variety of *E. ansatum* (that is, my specimens and yours). I have just found another form, shown on one of the enclosed drawings, with a very prominent central 'tooth' or 'nose' just above and below the isthmus, for which I propose the name var. *nasutum*.

I see that you also list *Pleurotaenium burmense* v. *extensum*, so evidently my arguments have failed to convince you that this plant belongs to *E. constrictum* and has no relation whatever with *E. burmense*.

For the last several weeks I have been working assiduously to find additional specimens in order to complete the drawings that I brought back from Karis. It has been a tiresome job, but I am getting near the end now. In some cases I have not been able to find any more specimens, so these drawings will simply have to be disregarded and laid aside until such time as the plant turns up again. In other instances I have been lucky enough to find one or two more examples, but still have not been able to get the required information; these also will have to be omitted. But in the majority of cases I have obtained the additional evidence that will enable you to make the determinations, so that the novelties can be included in our next paper.

During these examinations I have run across several unusual items that I have recorded, and you will find them among the enclosed sketches. They are not new, but some are interesting enough to be illustrated in a future paper; for instance the 5-radiate form of *St. novae-caesareae*, the only one I have ever seen. I shall forward the remaining drawings as soon as I have completed them.

For the last week we have had rather warm weather for this time of the year; on Christmas day the thermometer reached 78°F (25.5°C), but today it is down to 38°F (3.5°C), not so pleasant for us, though you would probably think it quite mild. How is it in Karis, are you snowed-in for the winter yet?

Thanks for your Christmas card, and best wishes for the New Year, from
your friend