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About the Institute

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

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

April 25 1960

Bear friend Teiling,

Edith and I were greatly shocked to learn of your wife's death, and we offer you our sincere and heartfelt sympathy, which, alas, is all that we can do. I think it must be impossible for anyone who has not been through the experience to comprehend the grief caused by the loss of a beloved and lifelong companion and helpmate, but at least you have your children and grandchildren to help comfort and console you.

My wife is in fairly good health, though she has been under treatment for the last three months for vertigo (dizziness), caused by a derangement of the balancing mechanism in the inner ear, the semicircular canals. The condition is considerably improved now, but the ear specialist says that it may require several months more before it clears up entirely. As for me, there is little change in my outward appearance, but I know quite well that my physical condition is deteriorating, and that I have not many more years to live.

Concerning your question about the reduced and abnormal forms of Arthrodesmus maximus and subulatus in the paper on southeastern USA, I am sorry I cannot give you a clear-cut answer. All of my collections have been made in wide-mouth screw-top glass jars (mayonnaise jars) holding about 0.25 litre. The material collected in Louisiana and Mississippi was usually brought back to New Orleans in the living condition, and when I got home I removed the screw-tops and arranged the jars in sequential order on a long bench awaiting my examination. In some of the jars the material would start to decompose and cause an offensive odour in a few days, in which case I would add formalin and replace the top. In other jars the material would remain alive and in good condition for a longer time, even several weeks. My trips to Florida required a longer time, up to 10 days, and after the first few trips I formed the habit of adding formalin at the time of collection, because the jars were subjected to very high temperatures in the baggage compartment of my car, which certainly reached 50°C., and may have gone as high as 60°C., resulting in rapid decomposition.

There is the possibility, therefore, that some of the reduced and abnormal forms may have resulted from vegetative reproduction in the glass jars while they were awaiting examination. On the other hand I am strongly of the opinion that these forms also occur in the natural habitats, and particularly those from Florida. Previously I have told you that although I have found in the USA many of the small species of Arthrodesmus typified by A. incus, I have never found any Janus-forms connecting them with Staurastrum, such as those Janus-forms that you have found in abundance in the Swedish lakes, and you know my habits well enough to realize that I would have recorded them if I had seen them, for I am always on the alert for such deviations from the normal form. In fact you had all, or nearly all, of my Arthrodesmus drawings for study, and later you sent them to Grönblad. I suppose that many or most of your Swedish lakes are fairly deep and that the water-level does not vary much during the year. The habitats from which I have collected are quite different; the water is seldom more than 2 m. deep, and I have made good collections in water only 5 to 10 cm. deep; also many of them dry up completely once a year or oftener, during dry periods.

In illustrating the 1957 paper on southeastern USA it was necessary to make a selection of representative forms of each of the various groups to which Grönblad assigned names, but my original camera lucida sketches contain many more intermediate and intergrading forms, so that Grönblad's separation into groups is more or less arbitrary.

Arthrodesmus is one of the smaller genera, but for me it is also one of the more difficult ones, because of the small differences between some of the species, the intergrading forms, and the numerous differing interpretations and misinterpretations that are to be found in the literature. I never understood your Staurodesmus paper very well, and perhaps it may help with your new monograph if I mention some of the difficulties that I encountered. If some of these comments are unfavourable I hope you will not be offended, because no offence is intended.

A considerable part of the difficulty lies in my lack of literature. Of the 78 papers listed in your bibliography I possess only 32, and among those missing are some of the most important from the historical viewpoint. For instance I wanted to ascertain why you had selected the name Std. Joshuae, and I found that it is based on one of three very poor figures in Joshua 1885, which I happen to have. I do not have Gutwinski 1891 nor Wahlberg 1913; I do have Smith 1924 showing A. incus var. extensus, which you list as a synonym of Std. Joshuae subsp. Joshuae. Apparently the most important illustration in this series is that of Gutwinski 1891.

Of course, this lack of literature is my own misfortune, but I think there are many others in the same predicament, and who also do not have access to a large library of desmid literature. Last week I received a letter from Hiroyuki Hirose, Kobe University, Japan, thanking me for a batch of my reprints that I sent him, and saying that he is very happy that he was able to identify a peculiar desmid that he had found in Japan, as Streptanema trilobatum, from my Arnhem Land paper. So apparently he has never seen Wallich's nor Turner's papers.

I do not see that any advantage is gained by your use of the category "subspecies" instead of "varietas". Trinary or quaternary names can be written just as easily using the familiar words genus, species, variety, and forma. Perhaps you should explain this more fully.

On pp. 56-57 you give a list "A" of Arthr. and Staur. species which you have combined and transferred to Staurodesmus. Then there is another list "B" of "Species probably identical because of morphological similarities", but you did not say whether it was your intention at some future date to transfer them also to Staurodesmus.

On pp. 76-77 you twice mention the section Tetractinium (Näg.) Hansg., but this apparently should have been Tetracanthium (Näg.) Hansg. It would help if you explain that this section includes those species of Arthrodesmus that have only two spines per semicell, one on each side. On p. 77 the sentence "Arthrodesmus contains, after the loss of the section tetracanthium, species with two or three spines on each angle" is not very well worded, because some species have four angles instead of the two in Staurodesmus, yet none of them (to my knowledge) has two or three spines on all four of these angles. Also the next sentence, "These are no unessential members, the genus being founded also in relation to octocornis", does not convey a clear meaning to me, and apparently has suffered in translation.

Letter spacing for emphasis is not used in Great Britain nor North America; italics are used instead. Some readers may not be familiar with this, and consequently may miss the words and phrases that you wish to emphasize.

On P. 76 you say: The new genus contains from Staurostrum: the smooth or punctate species with one solid mamilla (papilla in your Latin version), mucro or spine in each angle. To me this seems to mean that you intend to place in Staurodesmus all those Staurostra that comply with the definition, no matter what may be their type, size, or shape. The following Staurostra would comply, to name but a few: St. Prainii W&W., St. subcornutum De Toni, St. tumidum var. attenuatum Borge 1918, St. longissimum Borge 1918, St. mamillatum var. longirostre Grönb. 1945, St. Marthae Grönb. 1945, St. unicorn Turn. These seven examples are

of seven widely different types that certainly have no close affinities for each other, and there are many others that I could dig up with a little search. If you include all these in *Staurodesmus* the result would be a heterogeneous mixture little better than *Staurostrum*. It would certainly take a large bite out of *Staurostrum*, but the mouthful might be too big for many desmidiologists to swallow, and too indigestible.

You mention *St. subnygmaeum* as one of the species that belong to *Staurodesmus*. But in Scott & Grönbl., 1957, Pl. 31, I have shown several figures of two varieties, one which has no spines, and the other which may have either one or two spines at each angle. Also in *St. Wildemanii* I have shown by dichotypical specimens that this plant exists in four different forms, with one, or two, or three spines at each angle, and that each of the four varieties exists as a separate entity in its own right. Probably there are other species that may have either one or two spines at each angle, and which will be exceptions to your rule of only one spine.

When you give the detailed list of species placed in *Staurodesmus*, I think it would be desirable to give two lists; one with the new *Staurodesmus*-name first and the old name as synonym; and the other with the old *Arthrodesmus* or *Staurostrum*-name first and the new name second. At present it is not easy to find out what has happened to the various varieties of *A. incus*, which you have placed in two or three species of *Staurodesmus*.

It was indeed very generous of Hannah to offer to do all your illustrating for the new monograph; it is a big job as you know and I do also. I wish to cooperate in any way that I can; for instance by sending to you or to her any of my large-scale camera lucida drawings which she can trace without enlarging from the printed publications. Just let me know which ones you want and I will send them.

The large paper on Indonesian desmids by you & Prescott is still in the Ms. form; I have not yet found a publisher for it, and there is no telling when it will be printed. The Indonesian *Arthrodesmi* are quite interesting and you ought to know about them in preparing your monograph, so in the next few days I will send you prints of four plates of the illustrations, and photostats of the corresponding part of the text. You may keep all of them, and you have my permission to publish any of them, but please do not give the names of the new taxa because they would be nomen nuda without the formal descriptions.

It has taken me four days to write this letter, partly because I have had to refer to numerous papers, but largely because I have become so listless and lethargic that I do not seem to have sufficient energy to do anything at all. I don't know if this is due to laziness and bad habits, or if there is some organic reason for it. For several months I have done practically nothing on my desmids, though there is plenty of work needing to be done.

With best wishes and regards from my wife and myself,
Sincerely,

AEROGRAM

FLYPOST
PAR AVION

Brevporto

Mr A. M. Scott

2824 Dante street

New Orleans 18

Louisiana U.S.A.

AVSKNDARE
SENDER
EXPÉDITEUR
ABSENDER

Einar Teiling
Fil. dr.
Laurentligatan 8
LUND



GOD HELG
1960 SVERIGE 1961

FLYBEFORDRAS ÖVER HELA VÄRLDEN, UTAN
TILLÄGGSavgift, BEFORDRAS ICKE SOM AERO-
GRAM, OM NAGOT LÄGGS INUTI.

GODKÄNT AV KUNGL. GENERALPOSTSTYRELSEN

ÖPPNAS HÄR · OPEN HERE · OUVREZ ICI · HIER ÖFFNEN

Lund 28/XII 1960

Dear friend Scott,

since I got your pathetic letter of 25/IV I have so often been thinking of you. I am exceedingly sorry that you have lost your intensive energy and optimism. We are almost of the same age and I too am disagreeably aware of the changes caused life-long work. During this year I have not been able to work as I did previously, the pernicious illness and the death of my wife have wasted away much of my forces. I am so happy that my present wife has given me a home and the most devoted care that a man can obtain. Perhaps this happy life makes me somewhat indolent and lazy, last year I had periods of great diligence, perhaps an effort to escape from the sorrowful circumstances.

This part was written in a uncompleted letter several months ago. I am glad to tell you that most of my former energy has returned. Probably my laziness and incapability of mental concentration was due to a medicine for my fluttering pulse, it contained a sedative for which I was too sensible, this is my own conceit. When I stopped taking it - since my pulse was regular, the energy and love of work came back. I think it not seldom occurs that alarming weakness may depend on artificial synthetic medicines quite extraneous to human physiology, against which the sufferer is allergic I am sure that there is much agony without real cause.

I hope your lethargy has ceased and you now are eagerly occupied with your important tasks to the benefit of our lovely science.

I haven't heard of Hannah for eight weeks, at that time she had trouble because of her mother's illness, probably an operation would be necessary. Hannah is granted a sabbatical leave from Christmas to Easter with full salary. I hope her mother's illness has gone to health so Hannah can enjoy her freedom ~~sixxxxx~~ from the school job and devote her interest to the desmids.

I told Hannah about your kind offer to put to our disposal your fine ~~xxxx~~ drawings for the picts she is preparing for the Staurodesmus paper. I got the impres. in that she should like to visit you, especially for the desmid flora of USA. I hope by Prescott, you and Hannah. I think you will deliver the most part of the content, at least of the picts.

This year has brought me into events of the most varying kinds: Torä's illness and death, the scattering of my home and its equipment, the removal to this town, wedding, adaptation in a new milieu and compulsory inactivity, I do hope that the next year will be calm and successful.

I wish with all my heart that you and your dear wife will enjoy a good year 1961 with happiness and good health and for you successful activity.

Your affectionate

Eric

in



O. Nord. sk. dt.
1909.

Please return
to me. Scott

Linköping the 11th of May 1960

4-13

Dear friend Scott, Many thanks for your amiable letter and I beg you to express my sincere thanks to your wife for her sympathy. I am sorry to hear of your wife's balance troubles. My wife suffered from it during long periods and I myself feel symptoms of it (probably due to arteriosclerosis), very irritating! Your lethargy is a severe affliction for such an energetic man as you are. But I do hope that it will be passing, I have suffered from lethargy during shorter or longer periods. The joy of recovering preponderates over the vexation of lost time, and remember that Hope was the only thing that was left in Pandora's box! After the burial I spent some time in Uppsala and Lund. The latter is a lovely town with university where I have worked as an associate teacher, and an old, fine cathedral, dear for me since I received the laurel wreath and doctor's ring there. Probably I move to Lund for life-time. My two children are indefatigable in their efforts to make my life worthy of living but they live in Stockholm. I was very glad to receive your comments and speculations on the Std. problems. Your involution forms of maximum and subulatus have a certain interest especially if they now and then may occur in denaturated biotopes. Maj-Britt Florin has some similar forms in her dissertation, they are marked off. As to my paper on Std. of 1948, it was meant to give rise to continued revision by other desmidologists, I did not like to change a big lot of names with my name behind, now I am compelled to do so, I got the habit when printing the Actinotaenium paper. Of course, now 12 years later I have acquired a better opinion in several respects. I have rejected the term subspecies in this regard and already in 1950 I proposed the term facies in my paper on desmid radiation. As this is of little interest as regards new taxa it is natural if you have neglected a little chapter "Radiation and Nomenclature". Since then I have in the abstracts added an announcement containing the rejection and reference to the paper of 1950. Hannah wrote me a kind letter, the first of a long series, in which she told me that she at first did not understand the Std. concept. But she had read it again and again and penetrated the reasons in the light of taxonomical apprehension and had been convinced. "You win" she wrote. The simplest is often the difficultest to find out and also to understand. Other scientists have told me something like this: Of course Std. is quite right, it is remarkable that no one has stumbled on it before". Actually after a century of desmid research. Your objections against Std. are of course understandable from your point of view. If I make a summary of them according to your letter the result is: you find Std. too heterogeneous, too big and, for the rest, unnecessary. This genus comprises desmids with smooth cell-wall and one (sometimes doubled) radial bulge, developed as a hollow mamilla or a solid papilla + spines. There are biradiata species (formerly assigned to Arthrodesmus and Cosmazium) and tri- up to multiradiata species (formerly assigned to Staurostrum). A genus is not a collection of strikingly similar shape but forms that on the basis of comparative morphology, culture and genetics show reasonable affinities. They can be arranged in series with mutual connections. The whole can be arranged in a pattern like the branch of a tree with one or more limbs and boughs of decreasing dimensions. In this generic tree the species are placed with the simpler below and the more elaborate on the younger boughs. This is the general method, consideration must be taken that apparently simpler forms may be results of reduction which sometimes is possible to unveil by investigating a rich material. You give examples of seven different types in your opinion impossible to unite in Std. You reckon now habitually to St. and consequently

you find more "affinities" between them and *St. gracile*, *ophiura*, *alernans*, *gladiusum*, *novae-caesarea*, *brasiliense*, *marianii*, *orbiculare*, *lunatum*, *avicula*, *arctifolium*, *sexangulare*, *forficulatum*, *insigne*, *cryptothidrum*, etc in infinitum. You emphasize that you can not swallow *Std.* and consequently you find it better to conserve *St.*, this immense collection, the common feature of which is impossible to define: irradiation? No! processes? No! ornamentation? No! *Std.* has two common features: the smooth cell-wall and the radial outbulges, papillae or spines. It seems some if you find the classic general sacroset, until they are once substituted by a system of genera, that must be irrevocable, for every objection, fixed for all futurity! I remember your objection against *Actinotaenium*: "Why create such a little genus instead of a total revision of the whole *Cosmarium*?" The principles founded on the chloroplast and cell shape had no importance to *Actinotaenium* was too little to swallow, *Std.* is too big, I must say that you are difficult to satisfy. Man suffers from a general difficulty in avoiding the mighty constraint on our thinking-faculty that is exerted by the words. For an algologist it seems impossible to unite species of *Arthrodesmus*, *Std.* and *Cosmarium* into one genus. In the beginning of his work he engraved in his brain that species of *Ar.* belong to a quite different genus than species of *St.*, he is perhaps aware of the similarity but he is unable to admit the possibility of uniting them in one genus. The resemblance of certain forms of *Dickiei* with *Cosmarium* or *Ralfsi* is neglected because he is persuaded that they must be quite different; they belong to different genera. He does not consider the simple fact that these names were created at an epoch when our knowledge on desmids was extremely poor, *Ralfsi* knew only 160 species. It is true that it is difficult to delimit practically all the genera. In *Std.* the reduction (a common feature among the desmids) must be considered; for instance *Std.* convergens \rightarrow *Cosm.* subundatum, *Ac. ootocornis* \rightarrow *St.* trigonum, *St.* trigonum \rightarrow *St.* trigonum. Reduced forms (monospines) of *St.* *acrotrophorum*, *Waldemanni*, *lanatum*, *Kantha*, *inchoatum* and *tetracentrotum* do not be reckoned to *Std.*

Well, it is here impossible to give you all the principles and view-points in a letter; we ought to have discussions between four eyes during at one week or more. The difference between us two is that I have studied the phylogenetic problems from the view-point of evolution since my youth. The evolutionary mind is not acquired by reading some books about it, it is a matter of education and of a general disposition. Most instructive is zoology because of the manifold of organs and structures and the possibility of verifying their evolution ontogenetically as well as phylogenetically (in the more paleontology). Every botanist interested of generic and higher taxonomy would have his basic studies in zoology, only in this way he acquires the biological view necessary for his kind of scientific ability. And above all he must possess an intuitive view of his subject. I believe that you possess it but you had not, regrettably, the opportunity of studying biology at your (20) years age. Borge did not have it, he was aware of that and as he was a humble and practical and noble man (one of the best men I have met) he confined his talents on that phase of scientific ~~taxi~~ taxonomy where he was able to do the most of use, and he succeeded, to place him above G.S. West. Observe that desmology was not submitted to taxonomy of its own! Desmids are living things obeying the laws of life. Specialists on desmids and diatoms are often sharp-sighted and clever men, they love their subjects and are busy in enriching our knowledge of new forms. They are extremely valuable scientists in doing the necessary basic work and the best of them create like you the accurate elements for building the ever more perfect systems of the future. For this work a generic taxonomical view and all of the round biological culture is not necessary, e.g. Borge. The famous G.S. West was very busy in making new taxa but he was no great algologist; his new taxa are not seldom badly founded and he was so eager in making new

taxonomical principles

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Genera that he made the most terrible mistakes. His specific investigations had been much better if made by you!

Nature is lavishing with creations of all kinds in such an immense extent that there only in single cases is possible to fix any limits between higher groups, genera and species, varieties forms and etc. in a system like products of human creation. Every system, e.g. a genus, must be the result of the activity of experienced and upright scientists with broad-minded ability, supply of most of the actual literature and iconotheque etc. They intend to give an epitome of the scientific knowledge of the subject from the position of the actual scientific state. The progress of generic work develops slowly. When a principle is found, it is necessary to examine its value by using it in its scientific connections. If it is good or not, it will give rise to new or enlarged view-points that require further investigation, it may be found not good and rejected, but we are obliged to take the steps towards the taxonomical goal which we are able to do. Otherwise it would be most convenient to have no genera, we could mark the species with siphers, Desmid n:r 1, 2, 3 etc. Or, if we like to use names, say Desmid botrytis, Desmid incus, etc.

I do not understand you. As I have a very high opinion of your clear brain, the fault must be mine. Are you sure that your attitude against revision of desmid genera is not influenced by Grönblad? Actinotænum has been accepted by your working-fellow Prescott and Hannah has told me that he is inclined to accept Std. when I have published a list of it with the limits: "I think, Teiling will be right by and by". Even Iréné-Marie the very conservative orthodox paper has in a paper expressed its hope that Staurostrum will be divided up in natural new genera by and by and predicted that the monospinous may be the first.

I am, however, very glad and grateful for your interest and thoughts about Std. I can never be offended by honest, positive criticism. I intend to do my best in order to deliver contributions to the benefit of our science for the sake of truth, not for my own fame. My results will be proved or rejected by future research. Every contribution to the scientific discussion, even the wrong ones, gives rise to consideration in the mind of honest inquirers after truth. Objections and negative criticism from such persons who reject every proposal of generic revision because of their "repugnance to changing their once accustomed nomenclature" (Grönblad's own words to me in Helsingfors 1956), cannot influence my opinion.

I am sorry that I have been verbose and nevertheless laconic in what I have tried to express. I do hope that you are not offended by some of my opinions of desmidologists. I have, in a letter of January 1958, declared my well-founded opinion of you as scientist. It contains no exaggeration often used in birth-day congratulations. When you was here, you once sadly regretted that Fate had brought you in on the mercantile career instead of the scientific. The latter is more delimited and uncertain, and remember that a successful amateur is more appreciated than a professional! You have made a career (probably unique) at least among the desmidologists, you are a famous scientist after only 20 (?) years' work in leisure hours. Every algologist knows that you are the investigator, in papers bearing the name Prescott or Grönblad, everyone knows that the "auxiliary" names represent only the verbal part and that the whole deals with real facts carefully picked up by your sharp eyes, devoted and unwearied labour and skillful hands. This ought to console you and chase away your lethargy and pessimism. And, above all, you are happy in possessing your dear wife!

Together with your

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AEROGRAM

**FLYGPÖST
PAR AVION**



Mr. A. M. Scott

2824, Dante street

New Orleans 18

=====
Louisiana, U. S. A.

SENDER / EXPÉDITEUR

ENAR TEILING

Lektor

Kåbelgatan 10

LINKÖPING.

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FLYGPÖSTAS ÖVER HELA VÄRLDEN UTAN TILLÄGGSAKOST.
BENÄMNAS ICKE SOM AEROGRAM, OM NÅGOT LÄGGES INUTI

ÖSKERNT AV KUNGL. GENERALPOSTSTYRELSEN

TO OPEN CUT HERE / OUVRIRE ICI

Linköping 16 of April 1960

Dear friend Scott!

Our formerly fairly vivid correspondence has become sporadic, quite naturally, I understand very well that you are busy in clearing up as much as possible of your rich material while you are quite fit for work. The same circumstance is valid also for me.

Just now I/suffering from the consequences of an accident that will at bottom change my life during the years that still are granted to me; my wife died the 29:th of March. After the severe illness last summer she recovered in an astonishing manner and we had a few happy months together but in Januar her health began to deteriorate and she was transferred to the hospital. The last weeks her conscious was gradually decreasing and Death came imperceptibly.

- - o - -

As I told you in my new-year-letter I have taken up the *Staurodesmus* monography. This has been possible only by Hannah's amiable assistance, she has offered herself to shoulder the whole illustrative part. A skillful drawer who also is a very good algologist is a gift of Heaven!

I appreciate very much your rich picture material in your paper on the desmids of S.E. USA. I should however like to know if the reduced and abnormal specimens of *Std. maximus* and *subulatus* are found in their natural biotope or found in living material resting in glass. Probably they are natural but I want to have your word on it. There are several involution forms published and described bona fide from old samples of living material.

I hope that you and your wife are at good health and that your eyes have not lost too much of the sharpness that has been so successful in your research.

With my very best greetings

your sincere friend

Einar Östling

Dear friend Scott,

Lund 1/II-63

yesterday I read a deal of the fairly extensive correspondence between us during the period 1946-1960. Then it struck me that you have your birthday in the beginning of the year and that it must be a 75 years jubilee. I looked at the back side of your photo on the wall above my microscope desk and found to my great regret that I had missed the great day. Alleging my notorious bad memory I have the honour of sending **my very Best Congratulations to the Past and the best Wishes for the Future and my sincerest Thanks for your Friendship, and Three cheers for You!!!!**

I hope that your lethargy has diminished so you can enjoy microscoping. Hannah wrote that it may derive from an inexpedient medicine, I have had experience of this. In 1961 my son-in-law prescribed me a restorative but the effect was quite the reverse, until I stopped taking it - and in a few days I felt like a man again! The pill contained a very small part of a sedative and later experiments showed that I was allergic against it. The initial cause of my reading your letters was a demand from Bourrelly. He is emitting a valuable series "Initiations pratique à la Systematique des algues d'eau douce" and he is now occupied with Conjugales. He asked me if I have samples of Streptonema, Ancyronema, Euastridium, Triplastrum, Icthyodontum, Amscottia, and Allorgea. He does not know these genera par autopsie and is evidently eager to publish pict's of his own hand. I remembered that there among the samples you so kindly sent me, also one contained Strptonema and another probably containing Icthyodontum (in which I was unable to find any specimen). Another vial from Java, marked M would contain Streptonema. It did, but in the whole sample I found only two short filaments regrettably in a state not good for a drawing. Instead it contained A. curvatus not uncommon together with A. convergens and, most interesting, dichotypical specimens of these! I also found typical *Std Dickiei* var. circularis. For the sake of Bourrelly I am sorry that the vial did not contain more material.

I had a request from the Editor of *Nova Hedwigia* to write a necrologus of Grönblad but I must refuse, it would take too much time to get informations about his "curriculum vitae", I don't know any relatives of his and know nothing about his life, our conversation and correspondence have been confined to desmid chat. I wrote to Cedercreutz who I am acquainted with and persuaded him to take the task. He is an old friend and collaborator of G:s. Hans Luther will write the official necrologue for the Fennian Sci. Soc. and also a smaller for *Revue Alg.* So it will be well done. Every person who is a probable subject of a necrologue should write the necessary facts himself, a kind of will, to the benefit of the necrologist, unless he has written his memoirs. I have collected a very good material for a necrologue of P.M. Lundell, but it took much time of writing and even a trip to his last homestead.

I am busy with the *Staurodesmus* paper and am hoping that Hannah will visit us this summer for discussions and drawings. Microscoping is more amusing but I have difficulties with my neck, the cartilagous disks between the cervical vertebrae are more or less calcified and the nerves get in pinch which is causing a troublesome head-ache. My microscope has a straight tube so I am compelled to keep the head in an ache-causing angle down and forwards. I fear I must procure a more modern microscope like yours, but such ones are now very expensive. The university has no instruments to lend and an application to a great Research Foundation gave no result.

With my best wishes to you wife,

your old friend

Ernst

Dec 30 1960

Dear friend Teeling,

I owe you a thousand apologies for not having written to you for such a long time. There was no special reason for the delay; simply that I have been in such a state of lassitude, or lethargy, or maybe just plain laziness that I seem unable to summon up enough energy to do any work, or even to answer letters. Actually I did write an answer to your long letter, in which I attempted to refute some of your arguments, and to advance some new ones of my own. But after re-reading it I did not send it, for I realized that it would simply prolong the discussion without producing any useful results.

Hannah wrote me some months ago that you had been through a period of depression in which you were unable to work, which is easily understandable; but she added that you had recovered from it. No doubt she told you that she and I and Prescott met last June in Philadelphia for the purpose of discussing our procedure for the North American Desmidiaceae. One of the subjects that came up was the list of genera that we would recognize, including three of yours, Groenbladia, Actinotaenium and Staurodesmus. We agreed to use the first two, but thought it advisable to suspend judgement on Staurodesmus until the appearance of your revision of that genus, so that we may know exactly what it includes and what is excluded. However, in the paper on Indonesian desmids by Scott & Prescott, there are perhaps four or five species that I included in Cosmarium, which I would now place in Actinotaenium; but it was too late to make the changes in the manuscript.

The Indonesian paper will appear in Hydrobiologia, next Spring, I hope. Three weeks ago I received the page-proof, and was quite disgusted with its appearance. I had requested that it be published in one issue instead of being divided into two parts. While they have complied with this request, the publisher or printer was so anxious to keep the number of pages to a minimum that he has compressed the text to such an extent that there is no style at all, it is just mere printing. I was so infuriated that I fired a hot letter of protest to Prof. van Oye, and have just received a placatory reply saying that another proof will be sent me, and that he is sure the printer will try to comply with my wishes. I wish I had sent the paper to Nova Hedwigia, whose typography and paper are really excellent.

Mme. Kossinskaja has sent me her new work on desmids, Vol. 5 of the Flora Plantarum Cryptogamarum USSR. A large number of the illustrations have been "lifted" bodily from the monographs of West & West and of Krieger, and probably the text also. However, it will be valuable because it is presumably up to date until 1952 when it was started, and because it presents illustrations by Russian authors whose works are unknown in this country, and probably in other countries outside the Iron Curtain. It is surprising that her bibliography contains so few citations of American authors, but there is a footnote at the beginning of the bibliography, which, as nearly as I can figure it out, means that only those works are cited which are useful for the determination of Russian desmids. I know nothing of the Russian language; in fact the Cyrillic alphabet baffles me, and though I bought a small Russian dictionary I cannot ~~now~~ get very far in translation, for I am ignorant of word-forms and grammar and syntax of their language.

Dec 31 1960

This morning the postman brought your letter of Dec 28th, and I am very glad to know that you have recovered, and now happily settled in your new home with your wife. Some months ago Hannah mentioned the possibility of coming to New Orleans during her sabbatical leave, so that she could copy some of my drawings. However,

that is now impossible. A couple of weeks ago I telephoned to Prescott in Michigan about another matter, and I also told him that, much to my regret, I had made no progress whatever on the task of listing and tabulating my USA desmids with data as to their dimensions, habitats, etc., which I had promised to start immediately after my return from Philadelphia, and that I did not know when, or if, I could obtain the energy to make a start on it. So I arranged with him that I should send all my USA drawings to him, and that Hannah could go to East Lansing and work on them there, where she will also have access to his own drawings, his iconograph, and his very complete card index. The drawings will be returned to me after they have finished with them, but I regard it as very doubtful if I shall live long enough to publish them myself.

Another reason for this decision is that my wife has to undergo an operation in about two weeks, which will mean a week in the hospital, and two or three weeks convalescence at home. She has been in poor health all this year, with several different ailments. I too have had trouble with my heart and lungs which are in poor condition, so that even a small physical exertion causes me to gasp for breath.

Recently I have received Dr. Kuno Thomasson's second paper on Lake Bangweulu. It seems almost incredible that the two asymmetric desmids Allorgeia incredibilis and Micr. sudanensis should have been found together in three such widely separated places in Africa, by three different persons, and in little more than three years. He also has a number of other very peculiar and interesting desmids. I am glad to note that his drawings are gradually getting better, but there is still room for much more improvement; many of the Bangweulu illustrations are incomplete because the wall ornament is not shown, or only one view is shown. The text of this paper is poorly organized; his arrangement of genera in alphabetical order has nothing to recommend it, in my opinion, and has the disadvantage of separating such closely related genera as Dicidium-Plaurotacnium-Triploceras, Tetmemorus-Euastrum, and worst of all Arthrodesmus-Xanthidium which are at opposite ends of the alphabet. There are several silly mistakes in the spelling of botanical names, and in the citation of authors' names. There is, as usual, whatever in his arrangement of the figures on his plates, instead of keeping all the species of one genus together on one (or more) plate or plates, the genus Staurastrum is scattered over half-a-dozen plates, and on Plates 7 and 8 he has no less than six different genera on one plate. Some of his determinations are certainly wrong, and others I regard as highly doubtful. His Staurastrum galpinii sp. nov. is nothing more than St. rotula with convergent processes and unusually high apical verrucae; I have a drawing of an American specimen almost identical with his.

In his paper on Planctic Staurastrum from New Zealand, p. 243, he has commented on the well-known subject of inaccuracies in old papers caused by the fact that the original drawings had to be reproduced by an engraver. But he has gone further, and has questioned whether there may be inaccuracies in recent papers because the authors' drawings have been inked by a draughtsman. This may be a perfectly innocent question, and I hope it is. But it points directly to Grönblad and Teiling and Scott, because we three, I believe, are the only ones who employ another person to ink their drawings. I am going to prove to him that the inking process has not introduced any inaccuracy whatever in my illustrations. I happen to have on hand two sets of blueprints of some of the plates from the Micrasterias paper by Prescott & Scott, 1952. One set of prints was made from my pencil drawings for the plates; the other was made from the same drawings after Mrs. Perine had inked over my pencil lines. I am going to send these blueprints to Thomasson, and I think he will have to admit that there is no inaccuracy.

In conclusion, my wife and I reciprocate the good wishes that you sent us for 1961. We hope that you and your wife will have every happiness in your new home and surroundings.

Sincerely your friend,

Have you heard of the death of Fr. Irenee-Marie? Apparently it occurred some months ago, and I did not know of it until Prescott told me in a recent letter.

Linköping, New Year's Day 1960.

Dear Friend Scott!

Many thanks for your welcome and interesting letter. I regret the delay and use this day, the Day of Good Intentions to write to you. The period of a retired person may under favourable circumstances develop into a brilliant finale of an industrious and successful life. I think that this is applicable to you, you have a scientific hobby and possibilities to realize your passion for desmids, you are not quite lacking money and, last but not least, you have a dear wife and no grandchildren to be worried about. Perhaps the most important condition is a good health and I hope that Providence is gracious also in this respect. I see from your letter that you have had a marvellous journey, just such a dream-trip that everybody with sense of the beauty in Nature as well as Culture must envy you. I do.

I am glad to find that your eyes permit you to be busy at the microscope. It must be a special pleasure to deal with a material containing lots of new and interesting desmids, much more amusing than the work of assorting and revising the stack of taxa accumulated during more than a century by often inferior scientists with poor literary and optical means of assistance. Most of the (earlier) taxa are founded on insufficient material, even on one single specimen. This depressing opinion became relevant by my work with Actinotaenium and is too much affirmed concerning Staurodesmus. Only the foolish scientist who deals with monograph work and thus is obliged to sweep up everything published about his subject, is able to perceive it. The one who investigates individuals in the microscope and wants to find names, more or less applicable for them, avoids carefully everything irrelevant. I have picked up my rather extensive collection of excerpts and pictures of Staurodesmus which have been resting in the box. I feel obliged to consider the public appeal given by Bourrelly in Revue Algologique, last volume.

When my wife began to recover and the agony for our common future decreased, I took up a series of plankton investigations charged by provincial associations for protection of our lakes, especially an investigation of a great number of synchronous samples from our greatest lake Vänern.

Ms. Gauthier sent me her paper on *Allogea* and I was glad of her appreciating words on page 94. Her research seems me a valuable emendation of the separate genus to which your *incredibilis* must be allotted. This species is hitherto known as a dichotypical variety. The simple semicell may represent an atavism of the primitive form, probably its f. *typica* will appear in future investigation. Like other kinds of asymmetry may be considered the result of mutative activity. Hybridism does not give rise to new genes, it only places the genes into new combinations according to its laws. As far as I know, hybridism is hitherto not known among the unicellular organisms that do not have differentiation of somatic and generic nuclei.

I hope your paper on the Indonesian desmids will appear soon enough for me to enjoy it, probably it will be a bigger gun than your paper on desmids from S.E. of USA.

Tora is as healthy as one can expect. She is able to go up and down the stairs and can make short promenades at my arm. I myself enjoy a good health but I hope that the new year will be somewhat better than the gone one.

I wish you and your wife a very Good and Successful Year 1960.

The best greeting also from my wife,
your sincere friend

Einar

Linköping Aug 20 1959

Dear friend Scott!

To my nasty surprise i have found that I have ~~not~~ omitted to answer your letters of Dec 20 and March 6. This is very shocking, ordinarily I am a reliable correspondent, thus I hope that you apologize my delay which not at all depends on any decrease of my friendship to you. At first I postponed the answer, finding that you were so intensively busy with your Indonesian desmids; you as well as i myself being anxious to make the best possible of the rest of our days. I do hope that you are in a better position for it than I am. This year my wife and i myself have been suffering from a special kind of influenza, very lingering and ~~prostrating~~ prostrating. The only work of print was a little history of our orchestra which I was compelled ~~to write~~ ^{write} to ~~write~~, it took muck time compared with the few pages. I have also disposed certain parts of my library (not of course the phycological), my little collection of arms and other valuable things, that ~~will~~ be sold off at too low prices in case of the breaking up of our home after our death.

In June we had a recreation fortnight at our Tebo cottage and after Mid-summer I settled Tora at the nuns of S:cta Birgitta in the old, charming town Vadstena at the Vetter shore, and drove to Aneboda. I recovered ~~good~~ ^{well} enough but Tora did not, and in the middle of July I was obliged to fetch her back to Linköping for Röntgen treatment. Since then she has been very ill, a complicated pneumonia together with other lung maladies, not exactly diagnosed as yet, but the doctors are fearing cancer. Only the last week a bed has been accessible at the hospital here. The situation is rather dark in as much as I myself is not quite healthy.

My scientific work is at present quite put aside and must be postponed to a future that we hope will be more favourable.

I am sorry that I must tell you these bad news and I hope that you and your wife are in the best of health and that your eye sickness does not decrease your successful work of our beloved desmids. I also hope to live long enough to be able to admire the paper of your marvellous Indonesian desmids.

Perhaps you find my attitude concerning the future too pessimistic, but at my age death is an inevitable close reality. One of my principles of life has been that a moderate pessimist is happy so far as the surprises met with must all be positively agreeable. That is why I am hoping that my next letter will contain news of more cheerful nature.

With my best greetings to your wife,

your sincere friend

Emil T.

Oct 2 1959

Dear friend Teiling,

Your letter of Aug. 20th probably arrived in New Orleans on Aug 24th, the very day that my wife and I left on a long automobile trip to California; thus I did not get the letter until our return a few days ago.

Both Edith and I are much distressed to learn of your illness, and especially that of your wife, which does, indeed, appear to be serious. Such troubles are, unfortunately, the natural concomitants of old age, and there is little that I can say except to extend my sympathy and to hope that things will turn out better than they appear at present. You and I are about the same age, and sometimes I feel despondent just as you do, knowing that I have not many more years to live, but usually I can shrug off my troubles and forget them. My wife and I are in fairly good health, but neither of us is very strong; we tire easily, and I get out-of-breath after the least exertion, because of a weakened heart and lungs.

Our California trip was really wonderful. My wife has not driven a car for many years, so I had to drive all the way; but we took it in easy stages, taking five weeks to drive 6084 miles, or an average of less than 200 miles a day. We saw many marvellous sights, such as the Saguaro (giant cactus) Forest, the Petrified Forest, and Meteor Crater in Arizona; the Sequoia Forest, Yosemite Valley, and Lake Tahoe in California; Death Valley partly in California and partly in Nevada; the Big Bend National Park in Texas; the huge 200-inch telescope on Mount Palomar in southern California; and Marineland of the Pacific near Los Angeles. No doubt you have heard of and seen pictures of most of these places, which are of world-wide renown. We stayed four days in San Francisco, and found it a most interesting place, with some of the steepest hills that I have ever seen in a large city; so steep that I was actually afraid to drive up or down them. The Golden Gate Park in San Francisco is one of the most beautiful that I have seen, with a very large glass conservatory containing some amazing exhibits of tropical plants and flowers, equalling if not surpassing those in the green-houses of Kew Gardens near London.

We made a similar trip in 1927, 32 years ago, and the difference in conditions then and now is astonishing. At that time driving through the deserts was a frightful experience, the blast of hot air was like that from a baker's oven, yet you could not close the windows or you would literally roast. This time our car was air-conditioned (cooled) like an electric refrigerator; so we rode with the windows closed, excluding the wind and noise, and with the interior of the car at a comfortable 75° F., while outside it was probably 120° in the sun (there is no shade whatever). Most of the desert is sandy and stony waste, with a small amount of vegetation; many kinds of cactus, sage-brush, and other xerophytic plants whose names I do not know. In some places water has been brought to the desert by irrigation canals, or by drilling artesian wells, and it is astonishing what this barren soil will grow when water is provided; many kinds of vegetables, alfalfa, even maize, and it is refreshing to see these green places amongst the brown expanse of desert.

Since our return I have been busy answering the correspondence that accumulated during our absence, and trying to get my mind down to business again. The big paper on Indonesian desmids was finished some months ago, but the editor of "Reinwardtia" did not answer my enquiry as to whether he could publish it, and I have had to write him again. My next job is to resume work on a nice

lot of Brazilian collections that were sent me some years ago, and which are partially worked up; it is the same material in which the genus Amscottia was found, and it contains many new and unusual desmid species.

No doubt you have heard of Hannah's unfortunate experience last summer with "shingles" (*Herpes zoster*). I just had a letter from her saying that she had recovered, but that it had left one side of her face partially paralyzed, but even that is now slowly improving.

Have you seen the paper "Desmidiaceae asymétriques", by Mme. L. Gauthier-Lievre, illustrating the same two asymmetrical *Micrasterias* that were first described by Grönblad, Frowse & Scott from the Sudan, namely M. sudanensis and M. incredibilis? The second one she has assigned to the new genus Allorgeia, as A. valiae, and it is slightly different from ours, but only enough to justify a varietal name. Grönblad has informed me that Kuno Thomasson has also found these same two *Micrasterias* in new material from Lake Bangweulu in Northern Rhodesia. It seems to me an almost incredible coincidence that these two weird desmids should have been discovered by three persons independently, in three widely separated places in Africa, all within the space of a year or so. I think we must assume that these asymmetrical desmids with greatly differing semicells must have evolved from symmetrical ancestors, and the only plausible cause that I can see is hybridism. It is significant that one of them, M. incredibilis (or Allorgeia) has progressed so far as to develop varieties of its own. I shall be much interested in seeing whether Thomasson's specimens are identical with ours from the Sudan, or with Allorgeia, or perhaps are different from both of them.

Edith and I send our best regards to you and your wife, and we hope sincerely that in your next letter you will have better news to report.

March 6 1959

Dear friend Teiling,

Last January I received a postcard from your friend Carl Huzel, acknowledging the papers that I sent him, and saying that he would send one of his own when he was able to get around. No doubt you know his very small, fine, yet precise handwriting, which I think is quite remarkable for a man of his age. The paper arrived a few days ago, and I have written him that I consider it a very fine piece of work, extraordinary in that he found no less than 154 genera of algae in such a small area. His drawings are excellent, and appear all the more beautiful for having been reproduced by the half-tone process using a very fine screen, about 7 or 8 lines per millimetre. In the foreword to the paper the Editor pays him some very nice and well-deserved compliments. A particularly happy phrase is: "Und schliesslich entpuppte sich Huzel als ein Zeichenkünstler von hohem Rang".

I suppose you have heard about P.C. Silva's proposal No. 58, submitted to the Montreal Congress, in which he advocates the elimination from Art. 13 of the Code the "Later starting points for Algae", and recommends that the nomenclature of all algae start in 1753. It was printed in Taxon VII(7), Sept. 1958, and if you have not seen it I recommend that you do so. I have never met Silva, but understand that his interest is in marine algae. Also, he thanks Prof. Papenfuss for critically reading the Ms. of his article, but Papenfuss also is a marine phycologist. It's a pity he did not ask a desmidologist like Prescott to read it before rushing into print; had he done so he might have received a surprise.

There is no need for me to tell you what a serious effect on desmid nomenclature would result if this proposal should be approved by the congress; you know this much better than I do. Perhaps there is not much chance of its being approved, but I am trying to stir up some opposition in this country, and I can inform you that Prescott, Brunel, Irene-Marie and myself are very much against it. I also wrote to Hannah, and though she has not yet replied, I am quite sure that she also will disapprove it. I have asked Prescott to tackle the big-shots like Taylor and G.M. Smith, because he has much more influence than I have. Maybe you can do something similar among the Swedish phycologists. Grönblad is very much perturbed about the proposal, which he says would be "catastrophic", and he has asked me to join him in a short article to be published in Taxon, refuting and opposing Silva's arguments. However, this may not be printed in time to do much good.

Nothing else new over here. I am typing the final draft of the big Indonesian paper, but it's a slow and tedious job, for I am not so accurate on the typewriter as I used to be.

I hope that everything is well with you and your wife and family, and I send you my best regards.

Sincerely,

Ljnköping 30/VI/1958

Dear friend Scott,

I was very glad to get your letter. I suspected a moment as a theoretical possibility, that you had been influenced by Grönblad's antipathy against me. Glad to hear that your great work on the Indo-nese desmids soon will be finished and am hoping that only your name will be seen on the author's place and the publishing difficulties will be solved satisfactorily.

I am grieved to learn that your health is not so good as it ought to be. At our age over the seventy the human body must be worn, especially by such an active life as yours. The cataract is a special affair, perhaps you are right in your suspicion that the drawing from the microscope has accelerated its development in unfavourable direction. I have "peeped in the glass" as Borge used to say, since 1909 and my eyes are still very good, the senile presbyopia is inevitable and of little importance.

Now to your questions! Bourrelly's paper is nothing for you, dealing exclusively with Chrysophyceae. I am just working out a review of it, a time-consuming job, but Bourrelly is a very good man, a noble character and a very sincere friend, he has given me several proofs of true friendships, when we were room-mates in Helsingfors and I was very depressed, physically and mentally, he behaved extremely amicably like a younger brother. Huber-Festalozzi's book in spe will deal only with green planktonic algae, the small part on desmids is probably of little interest for you, I suppose.

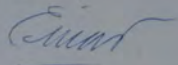
Your problem *Euastrum* or *Micrasterias Moebii* is an intricate one. I have looked at the sheets of my iconothèque, in this respect fairly meagre. Wallich's original type description and picture is, I think, correctly assigned to *Euastrum* because of the shape of the apex with its undivided lateral parts (his frontal isthmal ornamentation may be misapprehended). From this point of view Playfair is right. Schmidle's var. *Grunowii* is incomplete for a generic judgement, no picture a vertice. Möbius' and Borge's *Euastrum* show a quite different apex and also more elaborate lateral parts of the corpus. This same is valid for Wests' pictures (in the papers of Koh Chang, Singapore and also Notes V-14), Gutwinski's and Bernardis' pictures. Mori's picture shows a small indication of a bipartition. I find it convenient to avoid a combination at present, until more material with intermediate forms has appeared. This is a compromise quite justified. It is not to be denied that there ^{may be} two different species deriving from a common original form, thus forming a link between the conventional genera. In Sweden we say: "it is no use to dispute the shape of the pope's beard", to split ~~the~~ hairs is the English expression, I think. If you unite the species to one *Micrasterias* I think you are obliged to call it *turgidum*, which in this case is considered to be the original form taxonomically. If you accept the structure of the apex and the lateral parts of the corpus as specific differences in accordance to the traditional custom in desmidiology Krieger's diffuse concept of *Eu.turgidum* must be confined to the species erected by Wallich, by side, it is singular that Krieger has neglected Wallich's description, which implies the type, and published Okada's picture. Well, this is my diffident contribution to your question, please.

I have some doublets of Swedish stamps that I shall send you for your little philatelist friend. A son of my daughter's has begun to collect stamps so I have given him most of my surplus store.

I do hope that you will preserve your health enough for being able to perform your important scientific work, not at least your part of the USA desmid flora to delight and favour of all desmidiologists.

My wife and I myself send you both our heartily wishes,

Your friend



Dec 20 1958

Dear friend Teiling,

Many thanks for your prompt reply to my letter, and I am glad to see that both you and your wife are apparently in good health.

Since writing you, Prof. Brunel has been kind enough to make for me a photocopy of Wallich's original illustration and description of *Euastrum turgidum*, which I had not previously seen, nor did Prescott have it in his iconotheque. Now I can understand why Krieger did not use this picture for the type, because it looks suspicious. I think that Wallich's front view shows the central tumour much too large, and that he misinterpreted the ornament as granules instead of pits, though it must be admitted that it is theoretically possible for an Indian version of the plant to exist that would look like Wallich's drawing. His description mentions a "minute terminal notch", which I believe must have been an optical illusion caused by the presence of two closely opposed granules just below the center of the apex, which occur in some other similar *Euastrum* species. (Cf. *E. spinulosum* and vars. on Pl. 7 & 8 of Sudanese Desmids). Turner's figure of *E. turgidum* may perhaps have been influenced by Wallich's, because he also shows a remarkably large tumour, and indicates something like an apical notch. Turner's side view shows the apical lobe undivided, but his vertical view shows it divided, and also the upper lateral lobes doubled, so there may be two versions of the plant, one with the apical lobules divided, and the other simple.

Grünwald has sent me a copy of a drawing that he made many years ago of a specimen from Grunow's original material from Insel Banks, in Rabenhorst's *Exsicc.* No. 1727, which he queried as "*E. turgidum*, *M. Moebii*?" This has a large central tumour, though not so large as Wallich's, ornamented with triangular pits in hexagons and a raised granule in the center of each hexagon; the upper lateral lobes doubled, and apical lobules divided. In my opinion it does not differ enough from the species to be made a separate variety, and thus Krieger was correct in eliminating var. *Grunowii* in his monograph, though he had used this name in his Bunda paper.

An easy way of avoiding my difficulty would be, as you mention, not to make any change at the present time, but to wait until the possible discovery of more intergrading forms. On the other hand, I have been studying this question for several years; have seen more specimens and at least as many varieties as the Wests, who did not actually see *E. turgidum*; and have accumulated sufficient evidence, I believe, to convince any reasonable desmidiologist of the necessity, or at least the desirability, of transferring *M. moebii* to *Euastrum*. If I take no action now, it is likely that my evidence will be lost, and that nobody else will have the good fortune to see so many different varieties for many years to come. So I am going to make the change, even though it may cause more arguments among future workers.

My friend Dr. Gerald W. Prowse, from Malacca, whom I met in London three years ago, will arrive in New Orleans tomorrow morning (at 4.45 a.m.), to stay two days with us, on his way home to London on leave. Mr. Sachlan, from Bogor, also had told me that he expected to be with us at Christmas, but a few days ago I had a letter saying that his visit is postponed until next Spring, because the winter in USA would be too cold for him. That is a sensible decision.

The best wishes for Christmas and the New Year to you and Mrs. Teiling, from my wife and myself.

Sincerely your friend,

April 11 1958

Dear friend Teiling,

Sure, I'll be glad to send my papers to your friend in Germany. But first it would be desirable for you to tell me his name and address, nicht wahr? What's the matter, old chap? Are you starting to slip so soon after reaching the ripe age of 70?

Also I will send reprints to Mrs. Florin. Her paper that you sent me is without a doubt one of the best examples of typography that I have ever seen in this line, and she was very fortunate in obtaining a generous grant and the gift of the plates for the coloured frontispiece. The work itself seems to be very well done, and you have contributed largely in identifying all those desmids. But I cannot make any good use of the paper, for the desmids are of the common European species, of no special interest to me, and I know practically nothing about the other algae and diatoms. So I would like to give it to someone else who can use it profitably, for instance Hannah or Prescott, if they do not already possess copies. Please tell me if I may do so, and to whom I should send it.

Ruzicka's group seems to have picked a bad date for their meeting, September 1959, which is only a month after the IX International Bot. Congress in Montreal. I don't know the Vienna meeting that you mention, but how about your trying to come to Montreal? It would be delightful if either one or both of you could attend, and in that event I would go also; if not I think I shall stay home. There won't be much about desmids at the Congress, and the object of my going there would simply be to meet old friends again, and to make new acquaintances among the big-shots of algology.

Thanks for your remarks on the chloroplast of *Euastrum turgidum*. I also wanted your opinion of my proposed transfer of *Micrasterias moebii* to the genus *Euastrum*. Would you approve or disapprove such a transfer?

You are astonished that Bourrelly found 213 taxa in three tubes from the French Sudan, but Grönblad and I have about 235 from two stations in one lake in the old Anglo-Egyptian Sudan, and Prescott and I have 321 taxa from five tubes from North Australia. All the foregoing include miscellaneous algae except diatoms. In about 46 collections from Indonesia I have more than 500 taxa in desmids alone; the other algae are not yet worked up. And I think that considerably more than half of them came from one swampy region in south Sumatra. I'm still putting them on plates; am up to Plate 22 now, and I guess there will be about 40 in all!

The weather here has finally started to warm up. In the last two weeks we have 20°C., and even 25° on one day, but it is still cool at night, and our automatic oil-burning heater goes on every morning for a couple of hours.

Best regards to you and your wife, from your friend

Linköping October 24, 1958.

Dear friend Scott,

many thanks for your paper. I had not received any sign of life from you for a long time and was fearing that you was sick. I asked Hannah but she knew nothing. She had just lost her only brother, he died suddenly by a stroke.

That is why I was very glad to ~~hear~~^{find} that your silence depended on busy literary work, I congratulate you to the happy end of a paper that has taken so much time to be printed. I am very grateful for the separate "figures", they save me from much time-spending and delicate work. I have just begun cutting them for my iconotheque. Hitherto I have observed some of the inevitable misprints, all of them without any importance. The paper is a valuable contribution to the ~~xx~~ knowledge of a district almost unknown in this respect. The extremely rich material of pictures increase the taxonomical value; the reader receives an exact concept of the taxons behind the names, a condition precedent for eventual discussions. It is a very acceptable circumstance that your fine pictures are published to such an extent, for the benefit of all present and future desmidiologists. I think you now would write *Micr. bifurcata* instead of *Xanthidium*. I have not as yet looked at the paper enough for discussing any names, I suppose your *St. gracile v. elongatum* is due to Prescott's opinion, he has a very personal view on this taxon.

I have been a fairly idle fellow this year, probably the age abate my formerly just passable energy. I have been successful in selecting purchasers for certain of my old arms and parts of my library, in order to save money for the estate. My disabled grand-children form a permanent worry for the future, you are happy that you have no descendants.

The summer has been a bad one, cold and rainy, even Bourrelly complains the same as to the Continent. In August it seemed to become a failure of the crop but September brought much better weather, reminding of a good summer but more temperate. This meteorological consolation is still continuing, happily.

I am just occupied with an interesting investigation, viz. the changes of the phytoplankton associations in a series of six great lakes connected by the main river of our county, and also its final reservoir in the Baltic. The changes are instructively depending on the natural and artificial differences of these waters. My main works, the regional distribution of the phytoplankton in Swedish lakes and also the *Staurodesmus* monograph have now increased to such a deterrent degree that I hesitate to set about the final work. This will take several years and I fear that my life will not reach for both subjects. Huber-Pestalozzi, 83 years old, is just finishing the last part (*Desmids and Chlorophyceae*) of his extensive work "Das Phytoplankton des Süßwassers" and the Swedish diatomist (female) Astrid Cleve-Euler used the years 70 to 80 of her life to finish a rather complete diatom-flora of Sweden and Finland. Such vitality is, however, very scarce, there is little probability that I would possess this tenacity of mind.

The harvest of phycological papers of this year is very poor, at least in my letter-box. In April I received Bourrelly's dissertation, delivered in February 1954 but only now printed. It is an extensive work containing 412 pages and richly illustrated. In fact it is a monumental work for our knowledge of the very great and diversified group of *Chlorophyceae*. I intend to write a review of it in *Botaniska Notiser*, though this is a very time-spending job. Bourrelly has in it investigated a rather rich material of Swedish plankton.

Just now I am proof-reading a little paper of geographical interest. During my 34 years as a professor in training-college I have stimulated my pupils to investigate their native-place, town or parish, according to a comprehensive scheme considering both nature and culture. Many of these 186 regional histories are great volumes with valuable statements of source. In certain countries they would be approved as dissertations. I have made a catalogue of them and written a little discussion emphasizing their general and individual importance.

Hoping that you and your wife are quite as healthy and happy as we are just now, I enclose our very best wishes!

Your sincere friend *Enart*

Nov 5 1958

Dear friend Telling,

I owe to you (and several others) many apologies for having neglected to write you for so long. The reason is, as you suspected, that I have been busy all this summer and autumn with "literary" work. Recently I finished drawing 62 plates (!) plates for a big paper on Indonesian desmids, and they are now being inked by my friend Mrs. Perine. Hannah is doing the Latin diagnoses for the many new taxa, and when she returns the descriptions I have to rewrite the paper in final form, together with an introduction and some tabulations, and compile a bibliography; then it will be completed. Probably there may be some difficulty in getting it published in *Reinwardtia* because of its length and the large number of plates, which is almost double the number that I first estimated.

The Australian paper was exceptionally troublesome because of the peculiar ways of Australian editors, the many questions that they asked, and the minute care with which they went over the manuscript. This was the first lot of truly tropical desmids that I had seen, and I think it was the first for Prescott also. If I were doing it today there are several identifications that I would change, in fact I did change a few of them during the proofreading. *St. gracile* v. *elongatum* is one of them; it was Prescott's identification as ~~but thought~~.

Xanthidium bifurcatum will probably have to be assigned to *Micr. anomala*, in view of some intermediate forms that I have found in Indonesia, and another new one (unpublished) that I have from north Australia. Playfair's varieties also will have to go with *M. anomala*, but the Australian forms are too little known for me to attempt the reclassification at present. I have another lot of about 20 Australian collections, from various parts of that continent and Tasmania, that are partly worked up. When I finish them, the paper will not be published in Australia; I don't like the peculiar way they do things over there.

I have not seen Bourrelly's large paper, and doubt if it would be of much interest to me if it is a general work dealing with all the Chlorophyta, and especially the European ones. He wrote me two nice letters thanking me for the Sudan and Australian papers.

If Huber-Pestalozzi's work on desmids is anything like Krieger's I should like to have it, and would be obliged if you would tell me when it is published, for I usually do not learn about such things until several years after they appear.

It seems that everyone, including myself until recently, has overlooked the fact that Playfair (1908) transferred *M. Moebii* to *E. turgidum*, as var. *Moebii*, and also created some other new varieties from Australia. Concerning this transfer, G.S. West (1912a) merely wrote that Playfair had confused the two plants, though I believe that neither of the Wests ever saw *E. turgidum*. Krieger, in his list of excluded *Euastras*, also rejected Playfair's nomenclature, saying that the plants belong to *Micrasterias*. But Krieger himself was confused to some extent, for he lists *M. Moebii* v. *javanica* as a synonym of *E. turgidum*, though v. *javanica* is the one variety that can easily be distinguished, since it has a ring of verrucae surrounding the central tumour, not possessed by any other variety.

As far as I can see, the only differences between *M. moebii* and *E. turgidum* are that *turgidum* is usually considerably larger, the neck below the polar lobe is wider with a consequently smaller projection of the polar lobules on each side, and in vertical view is more rotund or turgid than *M. Moebii*.

These are comparatively small differences, and for a time I thought of reverting to Playfair's nomenclature. On the other hand, some of the later discovered varieties, such as *v. tetrachastriiformis* W&W, *v. insolita* Sc. & Presc. (Arnhem Land), and *v. diplocanthyla* (unpubl., Australia), differ too radically from the original *E. turgidum* to be easily assigned to that species. Then I had another thought that would solve the difficulty: to make *E. turgidum* a variety of *Euastrum Moebii*. But I do not know whether that would be permissible for priority reasons, since *E. turgidum* antedates *M. Moebii* by 30 years or more. Would you tell me your opinion about this, please?

I have sent copies of my last three papers, SE USA, Sudan, and Arnhem Land, to your friend Huzel in Stuttgart, and hope he has enjoyed reading them, though I have heard nothing from him.

The summer here was a little cooler than usual, with abundant rain that caused flooding in some parts of Louisiana. About two weeks ago the weather broke, and now we are having daytime temperatures of 20-21°C, and 15° at night. Our automatic heating system went on by itself for the first time about a week ago, and it runs for an hour or so in the morning until the house warms up to 21°.

My wife is very well, thank the Lord, but my state of health is declining noticeable. Old age, of course, and the doctor says there is nothing that can be done about it. For several years a cataract has been developing in my right eye, and eventually I shall have to have the lens removed. That will put an end to my microscope work, for even if it were possible to draw with the camera lucida using the left eye, I don't think I should care to risk it. The ophthalmologist has several times told me that working with the microscope does not affect the sight, but I suspect that my intensive use of the instrument for 20 years, using mostly the right eye for drawing, is at least partly responsible for the cataract.

I am glad to hear that you and your wife are both well, and I send my best wishes to both of you.

Sincerely,

If you get any "pretty" foreign postage stamps that you do not want to keep, will you send me some of them, by ordinary mail? The young son of a friend of mine has just started collecting them.

Linköping 15. April 1958

Dear friend Scott!

Excuse! Evidently I was too lost in my pleasant memories of old Huzel and his sweet wife, thus I forgot the name: Obergewerberat Carl Huzel, Amstätterstrasse 49. Stuttgart-Neudelfingen, West-Deutschland. I regret the miss for your sake, for my own I was favoured by enjoying a sooner letter from you!

As to Florin's paper, Hannah has got it and probably also Prescott, whose adress I have given to Mrs F. I do not know your principles of the extent of a library, I have a lot of abstracts of which I at present have ^{immediate} ~~no use~~ and for works that are pleasant to look at or possess a personal interest I always have space enough on my bookshelves.

The Montreal Congress is completely out ^{at} of my interest (if I had money to go to America I should go to New Orleans), ^{at} such mammoth congresses it is very difficult and often impossible to arrange the wished acquaintances. They imply a reminiscence from the turn of the last century with its delimited communications, the new and effective line is the specialists' meetings. I gave up to attend the Paris congress and the informations from attending friends have affirmed my opinion. And this congress was possible from the economical point of view. I am afraid that a very great part of the attendants are pure tourists.

You can be quite convinced of my benediction as to the transfer of Micr. Moebii, in as much as I have ^{recommended} ~~recommended~~ it in my Asymmetry paper, p.55.

Your microphoto is beautiful, the flat micr. specimens and certain smooth Gosmaria fit for this kind of reproduction, giving the outlines fairly correct.

I would wish that you could send some hotness to us, just the last days the day temperature has begun to step over 10°C and the night temperature is about zero! I had planned to drive to Västergötland on Friday, but some of my roads seem to be a little risky to miss a well-paid lecture.

Next month I shall go to Lund, an old tradition. It is always the same marvellous experience to live through the wonder of the spring's awakening manifested by the wonderful change of the beech forest from black naked trees to bewitching mist-like clouds by the yellow-green small leaves.

The best regards to you and your wife!

Your friend

TEILING

Linköping 3. April 1958

Dear friend Scott!

Many thanks for your gratulation letter with many kind words. Tell my thanks also to your wife, I was very glad to receive her ~~amiable~~ letter. It was amusing to get a letter from another USA-woman than Hannah. My birthday was not so quiet as I had hoped, there was a stream of visitors, flowers, letters, telegrams and gifts, representing friends from childhood up to present time, some of them rather surprising persons and corporations in the memory of which I could not imagine to be kept. Happily my wife is now in good health, an important condition at such an event.

During the (very necessary) tidying up of my studio I found the draft of a letter to Ruzicka as an answer to his invitation. Because of my wife's illness and other troubles in October, I had not made the fair copy and had the good conscience, I had written the letter! This discovery was very annoying, ~~xx~~ the delay could cause R. to suspect that I was ^{too} angry with him to answer. When I received the invitation I was positive to go, but ~~afterwards~~ later I have heard from my Checkian quartet friend who visited Sweden in February that the cost of living in Checkslovakia are rather high and the tourists' agency told me that tourists are not too welcome, troubles with the visum, suspicious watching over foreigners etc. The poor fiddle players changed their salaries in clothes and other useful thing, (their luggage was enormous) it is understandable when they told me that a homely dress commanded here a price of about 20,-. I visited Prag in the latest happy year, 1938, and I think that visit now would be a disappointment. Further, the talks with the algologists, both they and I speaking a foreign language! No, I think I ^{save} ~~spare~~ my money to the Vienna congress next year, if I have forces to go.

I hope you have got Mrs Florin's dissertation. She is a pupil of mine as regards the green algae, she is a specialist on diatoms and pollens and is a very clever geologist. Her paper is very good, she got a high certificate and ~~xx~~ was appointed a docent. The typography is high class as all items of Acta Phytogeographica. She would be glad to receive some papers of yours!

I just got your Sudanese desmids with many interesting and remarkable desmids. Sudan seems to be very rich in desmids, it is striking how many desmids ~~currently~~ found in only three samples! I have not yet penetrated the paper nor copied it, a time-vasting job. The microphotos, however, prove the minimal value as individual pictures for taxonomical work. ~~This method is useful only for showing algal associations by low magnification.~~ ~~at least concerning most desmids,~~ for certain flat (centric) diatoms it is a good aide. ~~Compare Florin, pl. 17-20.~~ It is understandable that Grönblad, being a bad drawer, is anxious to compensate this defect, though the manner is somewhat ridiculous; no scientific-minded desmidiologist can consider such photos as type-pictures of new taxa, he says discreetly "together with the drawings".

I see that I have not commented your question of the observation on *Nastr. turgidum* (your letter 6.XII.57). The new semicell's chloroplast is formed by the old one and the total creation of the new semicell is organized by the nucleus by the aid of hormones, auxines and similar, carried ~~forwards~~ ^{communicated} probably mainly forwards by streams in the cytoplasm, this is the present opinion, I think. It is quite possible that ~~strangs~~ ^{communicated} are developed during the rapid building up of the chlpl, probable especially in such an elaborate one as the tetracentric chlpl. The nucleus is scarcely visible without staining with orcein or, often better, lacmoid (also called resorcin-blue). Cytological investigation is difficult, not at least depending on the changes that occur by the contraction of the coagulating cytoplasm.

In our Nordic countries we say desmidiaceer or desmidiéer. I prefer desmidology as more correct than desmidology, the basic name is Desmidi-um, Desmidi is the wordstem. Compare syndesmology, the knowledge of articulations, ligament = desma. I quite agree with you in the critical view-point on Spondylosium. The catenate feature is no valid character generic. Spondylosium will later be brought together with the smooth Cosmaria in a future new genus. The main cause of my transfer is aimed at an effort to definitively separate panduriforme from C. moniliferum, to which it by routine is connected. I am quite convinced that the present

concept *C. moniliforme* is a very collective species, especially in plankton. The rounded shape of the semicell is a feature of simplicity and may be polyphyletic, it must not quite simply be considered a "specific" character,

Your preferring quite symmetrical desmids for depicting is of course a matter of taste common to many more or less romantic desmidologists. In most cases this is the true shape but in many cases it would be fallacious to idealize every desmid chanced upon, I mean the vertical symmetry. Hannah has quite correct depicted many of her Alaskan desmids asymmetrical, thus giving a true manifestation of the unfavourable conditions of the actual biotope. I suspect that several pictures of desmids from arctic and alpine biotopes are idealized by the author or the lithographer (engraver). In certain cases deviations can give valuable hints to the phylo-

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Mr. A. DANTE
 2824, Dante St.
 NEW ORLEANS, LA.
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gy, deviations that a "symmetric-minded" explorer often puts aside as being "abnormalities", compare my asymmetry paper, fig. 23.

I hope that you and your wife are quite well, I think your winter has been somewhat coolish as well as ours, here the temperatur only these last days has reached up to zero. Such a cold March has not occurred since 1888, (I remember it very well, the disagreement to change my mother's +37° Celsius into the coldness of the bad world). The water temperatur may be rather low which has an influence on the plankton production. Well, everybody has his own special troubles!!

The best wishes to you both from my wife and
 your sincere friend,

EMMAR

EINAR TEILING

Lektor
Klostergatan 10
LINKÖPING,

BREVKORT
CARTE POSTALE



Mr. A. M. Scott

2824, Dante street

New Orleans 18.

Louisiana, U S A.

P.M. to my letter of yesterday:
Dear friend Scott! I just got a letter from a German who in 1937 wrote a flora of the algae in a bog. It is a very good work the correctness is proved by more than 450 excellent pictures. He was an "Obergewerberat", about an official factory inspector and his algological work is the result of a sedulous and successful activity at the microscope and literatur. We have corresponded since long and met in Cambridge in 1953. We and his very nice wife chatted and discussed and had pleasant company, and I must say that they are extremely fine personalities, splendid specimens of the good old German spirit and character. We correspond regularly and very cosily. He is now more than 80 years and regrets that his eyes are not capable of microscope-seeing but he is anxious to read his beloved algological literature. He is very glad still to be reckoned to the fellowship of the algologists and is movingly thankful for my papers. I am sure that you would like him very much. That is why I dare to propose you to send him a couple of those abstracts that you can spare: "the day's good deed". I do not know if he still possesses any copy for you.

My best wishes, your friend

E. M. A.

March 4, 1958

Dear friend Telling,

I know that you will be 70 years old sometime this month but I do not know the exact date, so I am writing this early in the month, in the hope it will arrive in time.

Your friend was correct when he said that seventy years marks the definite entrance into the old-man's age; it is the traditional Biblical life-span of man, "three-score years and ten", but with the advance of medical knowledge it is no longer so exceptional as it used to be. With your notable activity of mind and body you have every right to expect many more years of enjoyment and productivity, and that is what Edith and I wish for you with all sincerity.

You wrote that our friendship has meant much to you both emotionally and practically, but I wonder if you realize that it has been even more valuable to me, and for both of these reasons. In fact I think it is rather remarkable that such a friendship could be built up solely by means of correspondence until my brief and thoroughly enjoyed visit with you nearly three years ago. Your informal and chatty letters are greatly appreciated by my wife and me, and re-read many times. Not only that but your criticisms and explanations have cleared up for me several questions in which I was somewhat confused. Frequently I refer to your papers, which I keep handy on a desk near to my microscope table, where I can just turn around and reach for them instead of having to get up and dig for them among the others in the bookcase. This I have much to thank you for in the improvement of my desmidological work.

I have been working on the plates for the Indonesian paper, but after completing nine of them I became dissatisfied because I was not getting enough figures on one plate. They were designed for a reduction of 2:1, so I have started anew with larger plates intended for a reduction of $2\frac{1}{2}$:1, which will enable me to get nearly 50% more figures on each plate. This drawing is the part of the work that I like best, and I find it hard to tear myself away from it to attend to the ordinary things of everyday life.

The big paper on North Australian algae is still in press, after $5\frac{1}{2}$ years. I returned the galley-proof before Christmas, but it lay idle for two months while the editor was away somewhere on a vacation. The first volume of the reports of the Expedition has been published, dealing with certain aspects of anthropology of the aborigines. It is called "Records of the American-Australian Expedition to Arnhem Land, Vol. I", by C.P. Mountford. I have not seen it yet; it is quite expensive, \$18.00 in this country. The algae will be in Vol. III, part 2, and I can only hope that it will appear some time this year.

Once more, Edith and I wish you "Many happy returns of the day", which doubtless you will spend in company with your good wife, to whom also we send our best regards, and surrounded by your friends and children and grandchildren. Good luck and good health to all of you.

Sincerely, your friend

Jan 23 1958

Dear friend Teiling,

It was very kind and thoughtful of you to write to my wife and send her the photo of Lundell for presentation to me. She had intended to acknowledge it before this, but she caught a bad head cold and has been in bed for three days. I had never seen Lundell's picture before, though I have written "Lund.(1871)" scores of time, particularly in my new paper on Indonesia, where many of his species occur. Like many Swedes he was tall and good-looking, and his face shows evidence of keen intelligence. I shall have the picture framed and hand it on the wall of my lab. together with yours and Grönblad's and those of Krieger, Ralfs, Brebisson, Nordstedt and Borge.

Your letter to me came exactly on my birthday. At first I was puzzled by your reference to my 71st birthday, for I am 70 years old, not 71. Then it occurred to me that perhaps it is the Continental custom to count the actual day of birth as the 1st birthday, while in England and USA the 1st birthday is reckoned as the day when a baby is 12 months old. Of course it is really the first anniversary.

Well, your flattering remarks concerning my desmidological ability caused my head to swell for a short time. Then I calmed down and realized that although you wrote them in all sincerity, your opinion rates me too highly. Grönblad is a better systematist than I am, because of his twice-longer experience, his larger collection of literature and better acquaintance with it, and his remarkable memory which impressed me very much when I was in Paris. He would look at one of my drawings showing a desmid that I had not been able to determine, sit back and think for a few moments, then announce that he remembered something like it in so-and-so's paper from 1869 or 1892. Then he would take the paper from his shelf, flick over the pages, and sure enough there would be the illustration, maybe not identical with mine, but something closely similar.

It is quite likely that I have seen more American and tropical desmids than anyone else now living, since Dr. Krieger's death. You will be surprised and delighted, I am sure, when you see the many unusual new species and varieties in my forthcoming papers. Also I am getting more information on some of the known species, e.g. *St. tauphorum*, which few people have seen. This plant is plentiful in the Indonesian material, but it seems to vary continuously in such a way that I found it impossible to separate my drawings into groups representing the specific form, and *fa. burnense* W&W, and var. *sumatranum* Krieg., because of intermediate forms connecting all three of them. So I am proposing to delete these names, and have referred all my specimens to the species. Also I found it necessary to change one of Skuja's determinations, though I expect he won't like it. I refer to *St. lacustre* var. *rangoonense*, of which I have numerous specimens that agree exactly with his drawings. But his drawings and mine also agree exactly with Joshua's vertical view of *St. cyathodes*, though not with his front view, which in my opinion is not reconcilable with the vertical view. There is also a considerable resemblance to *St. protectum* W&W, which in turn is very much like the vertical view of *St. cyathodes*, as W&W remarked. I have never seen *St. lacustre*, nor has Prescott, but from G.M. Smith's drawings it is a plant of very different habit and structure, so much so that I am astonished that Skuja even considered it. So I am calling my plants *St. protectum* var. *rangoonense* (Skuja) Scott & Presc., comb. nov.

I know that my drawings are good, but even there there is one man who can beat me, Ruzicka. His illustrations of the obscurely marked *Cosm. obtusatum* are better than I could do. What surprises me is the poor desmid illustrations published by persons who must know that they are poor, and who could do better. Take Prescott,

for example. He is a really clever artist; the walls of his office are hung with a couple of dozen beautiful small colored pictures, done in pastel crayon, showing scenes from the many distant places that he has visited; some others are in a comical vein, one represents six or eight flagellate infusoria harnessed to a tiny coach which they are pulling over the sandy floor of a desert, the driver being another infusorian using his flagellum as a whip; it is quite cleverly done. Last summer I saw his original plate drawings for a new paper that he has in press on some Brazilian algae, and many of his figures seemed horrible to me, particularly the Micrasterias, skewed, distorted, asymmetrical. To me nearly all specimens of Micrasterias are so close to perfect symmetry that my sharp eye can see no deviation from it. Again, I think that a desmid illustration ought to portray a perfect specimen unless there is a very good reason for depicting an imperfect one, so I take great pains to make my figures symmetrical, even if my camera lucida sketches are not quite so. All the best desmid illustrators have had the same idea; it is very seldom that you find an asymmetrical figure in the plates of West & West, Borge, Nordstedt, Lundell or Krieger, except in some of the older papers where the figures have suffered somewhat in the hands of an engraver.

I think I told you that for the new paper on Sudanese desmids, which ought to be published soon, Grönblad would not let me draw the plates, but preferred to have Miss Tikkanen do them, partly from my camera lucida sketches and partly from his own. I have seen the proofs, and must say that I am rather disappointed. Her ink lines are good, but whoever arranged the figures on the plate was not careful enough to get all the figures exactly vertical, with the result that the plates look rather "sloppy" to an expert draughtsman.

The typing of the rough draft of my new Indonesian paper will be finished tomorrow, though there remains a short introduction to be written, and the compilation of an index and some statistical tables. There are more than 500 taxa, and an interesting sidelight is that considerably more than half of them come from southern Sumatra, in the neighbourhood of Palembang. By comparison the collections from Java and Borneo are not nearly so rich, though this may be accidental, of course. The region around Palembang is shown on my maps as a vast swampy area, with almost no towns or roads.

My birthday was spent very quietly at home. There was no party because neither of us has any relatives here, and very few friends. In the evening we went out for dinner to a famous German restaurant, Kolb's, where we ordered wienerschnitzel, which disappointingly was not at all good, and not like it used to be when the original proprietor, Conrad Kolb, was alive. We seldom eat in restaurants nowadays, because the meals are so very expensive. I received a few congratulatory letters, including one from Bourrelly (your suggestion?) and another from Prowse, who says he has sent me some new collections from Malaya; however these are simply for my enjoyment because he will work up the desmids himself, I hope with assistance from Grönblad or myself.

My wife is very proud of your letter and will show it to all her friends, even though none of them know what a desmid is! I also esteem it highly, and it is gratifying to know that the work that I love so much, and on which I have spent so many thousands of hours and dollars, is appreciated by those who are able to judge it. Almost nobody remembers the name of the engineer who made the structural design even of important buildings; only the architect gets the credit. So the only thing that I shall be remembered for is my desmid papers, and these I hope will be in use for a great many years.

Edith and I send to you and your wife our best thanks, and wishes for the good fortune that you both so richly deserve.

Sincerely your friend,

Dec 6 1957

Dear friend Telling,

My wife and I were glad to read your nice chatty letter and to know that everything is going well with you. We were particularly pleased to hear ~~xxxxxxx~~ that the X-ray photos showed that your wife's condition is no worse and that she is somewhat reassured. That must be a very unpleasant thing to live with, but all the worry in the world will not do any good. My wife is obsessed with the idea that she will die suddenly from a heart attack, because that is what happened to her mother and elder sister. But they were big stout women, while she is small and I think inherits her father's physical traits. Our doctor does not find anything wrong with her heart or blood pressure, but that does not get the idea out of her head, for she is a little "worry-wart". (That's a slang expression). My own condition is probably worse than hers, but I refuse to worry about it because I am of a phlegmatic disposition.

Congratulations on acquiring a new grandson. The weight of 5 kilos seems enormous, 50% more than normal; probably when he grows up he will be as tall as you, and twice as wide and twice as thick! Your daughter's behaviour also seems quite unusual, though I know little about such matters.

Your mention of the autumn coloring of foliage aroused my interest, for just a few months ago the National Geographic Magazine had an excellent article with many colored photos of such trees, also a map that showed the various places in the world where such brilliant coloring could be seen. I was astonished to learn that southern Sweden and Norway, and Great Britain, are among the few places in Europe where the autumnal display occurs, though I forget the reason. I don't remember it from my boyhood days in England. In New Orleans and southern Louisiana we see very little of it, because so many of our trees are evergreens, and also because of our climate. Last Nov. 16th the max. temperature was 85° F., the highest ever recorded for that date, and today it is 75°, not unusual for this time of year, though we had some colder weather in between. The only time I have seen a really good display of the autumnal coloring was in October 1954 when we drove from Montreal to New Orleans by way of New York State. In a place called Bear Mountain State Park I was literally amazed at the splendid display of color, and was disappointed because I had just used my last color film and could not buy another since it was on a Sunday, so I did not get any color photos of it.

I also received from Ruzicka an invitation to the algological symposium at Prague, but of course there is no chance of my going, even if our Government would give me a passport, which they might or might not do. It would be nice to meet him and other European workers, but I cannot speak or understand any other spoken language than English, though I can read some others fairly well.

Your last paper came about a week ago, many thanks. Sorry that I cannot go along with your transfer of *C. moniliforme* to *Spondylosium*, as I have explained previously. There are other desmids that occur in chains, for instance some species of *Pleurotaenium*, such as *Pl. subcoronulatum* v. *detum*, in which I have seen chains of five connected cells, and I am convinced that in the natural habitat the chains are much longer. The reason why we do not see longer ones is that such aggregations must be ~~extremely~~ extremely fragile on account of the great length and slenderness of the cells. Recently I have seen numerous examples of *C. regnlii* f. *catenata* from Indonesia, up to 14 cells long, the individual cells exactly like the typical form, and not distinguishable from it.

I have just written a rather lengthy dissertation on *Micrasterias moebii*, which I am transferring back to *Euastrum*, where it originated. It seems rather temerarious for an amateur like me to set up his opinion in opposition to that of the old masters West & West, but I think that I can prove that the few features in which it resembles *Micrasterias* are far outweighed by those resembling *Euastrum*. I have not seen the specific form, but I have two of the known varieties from Indonesia, and two new varieties and one new form from North Australia, also some examples of *Euastrum turgidum*, so I am fairly well acquainted with it. I have considered at some length each individual feature of the plant, - its general shape in front, side and vertical views; the divided polar lobe; the lateral lobes which may be either simple or divided; the doubling of the upper lateral lobes where they exist; the size and ornament of the central tumour; the ornamentation of the cell wall; and the chloroplast. The chloroplast, by the way, is tetracentric as shown by Gutwinski (1902) as you have pointed out, with 3 to 5 pyrenoids in each section visible in front view, and with numerous fine filaments, sometimes branched, extending out towards the cell wall. I have not yet been able to determine definitely if there is a cross connection at the center between the 4 sections as you show for Type F-6-Eu. In 3 specimens I could see no such connection; in a fourth I thought I could see something like a connection, but it is very difficult to see through such a large cell (100 to 120 μ) when it is balanced on end. I assume that this cross connection, where it exists, is only at the level of the isthmus, and that the four strands connect the sections to the nucleus. Please tell me if this assumption is correct. Incidentally, the nucleus was not visible in these preserved specimens; it contains no starch and therefore does not stain with iodine as the pyrenoids do.

The discussion that I have written is entirely my own work; Prescott knows nothing about it yet, though it will have to be submitted for approval since it will be published in our joint names. I know that Grönblad will approve, because a couple of years ago he suggested that I make the transfer when writing up the plant. And I should like to hear your opinion, based on the above bare outline.

Do you have in Swedish or Norwegian or Danish a word similar to "desmidiological"? or desmidiology? In French there is "desmidiées", "desmidiologie", etc., and in German I think I have seen "desmidiologisch". In Portuguese there is "desmidias" and in Spanish "desmidiaceas", always used in the plural. The reason I ask this question is that Prescott in looking over another small paper that I wrote, has changed "desmidiology" to ~~the~~ "desmidology" without the second "i". I think he is wrong but at present I cannot prove it.

That seems to be all the news for the present. Both of us send our best wishes to you and Mrs. Telling and your family for a very happy Christmas and New Year.

Your friend,

Grönblad has been ill with influenza which developed into pneumonia, and had to go in the Mjølballstad Sanatorium. He wrote at the end of October that he was better, but very tired and unable to do much work. But Hannah wrote a week ago that she had a very despondent letter from him saying that he was miserable, and that the weather was very bad; they had seen the sun for only 10 days in 3 months. Please write him one of your cheery letters.

Linköping 19. Jan. 1958

Dear Friend!!

I congratulate you with all my heart to your 71th birth-day!!! A very good friend of mine since 51 years had his 70-year's day last X-mas and when we met in Stockholm a week later, he said with a sign joking: "The 70-years' day is the definite entrance into the real old-man's age". He is quite healthy in mind and body, a lector emeritus, and a very good botanist with many papers on his list. His words imply an old opinion which however is not valid in our time. Curiously, good desmidiologists are often long-lived: Brébisson 74, Ralfs 83, Nordstedt 86, Lundell 89, Borge 75, etc.

Just now I am exceedingly sorry that I do not manage English, only in my own language I would be able to express to the full my feelings towards you. Thus, you must apprehend my poor words as they are meant and not as they sound.

A seventy years' age is nowadays not so singular as it was in days of yore, but a 70-years' day is a welcome opportunity for a friend like me to express my appreciation of your desmidiological deed and your amiable friendship which has been emotionally - and also practically - a valuable asset to me.

With all my heart I hope that you will maintain physical health and mental buoyancy for many years, working free from business troubles in your lab and studio. You have such an immense material and detail knowledge of American and tropical desmids to bring out in print. It is my sincere opinion when I tell you that you as a sharp-eyed observer and scrupulous drawer are superior to all desmidiologists just now. Your basic research is the pre-requisite condition for exact description of the desmid units and consequently also for the distinguishing of genera and families. That is why your most important deed is specific description, also of their variation. The verbal work with diagnoses is a routine job, tedious and time-spending, every clever botanist with literary sources can do that on the basis of your exact pictures and descriptions, every desmidiologist honours you as the real discoverer and investigator, consequently the true author.

Proceed on your special part of science, lately found but excellently cultivated. You can be proud of the fact that your name is mentioned with collegial estimation and admiration by the desmidiologists now and in the future. Believe me, I am speaking on behalf of all the contemporary desmidiologist- friends.

Your friend

Einar Teilmann

Linköping 10. nov. 1957

Dear friend Scott!

Tempus fugit, an old truth, intensively realized by persons of our age who mean that they still have something of their mission in life to perform. This sounds somewhat prententious and I must confess that the delay of not answering your letter is not to be excused by such subtle reasons.

I have been making a razzia in my box-room which is encumbered by books of all kinds, mostly scientific, collections I have made of my geological literature, valuable abstracts that clamoured for new owners who could make full use of them. After having made gifts to certain friends of ~~various~~ such items of which I could suppose a welcome receiving, I duplicated a list and sent it to geological institutes. In order to get rid of it I fixed extremely low prices. Consequently I have been occupied by making parcels, a very tedious job, and I am just managing a similar list of my botanical literature (except the algological one). Afterwards the very big part of the geography expects its turn.

My wife has regrettably become a little hypochondriacal, she is quite natural alarming herself about her cancer danger. At last I drove ^{her} to the hospital and, on the basis of about 40 röntgen-photos the specialist could calm her that he was unable to state any increase. This reassured her and I could have a little vacation, I had received a pressing invitation to friends in Uppsala and wished to visit my children in Stockholm. We just had a marvellous autumn and, ^{with} a very good friend I drove through a landscape that flamed in all colours of red to yellow. My son is just promoted to an army-director of the ingeneer-corps and I visited him and his nice family some days in his modern and extremely beautiful and comfortable new house. He is a very good son and we have always been very good friends, I have always from his childhood been his confidant even in his most intimate problems. He is very clever and has been very happy in constructing anti-aircraft guns, ^{and} his work had just proved successful after years of intensive labour here and abroad. Thus he was able to slacken a little and we had evenings ^{with} of very good chat. Just at that time my daughter got a son who had as much as possibly postponed his entrance into this bad world, consequently he was rather big, 5 kilos! His mother worked at her husband's doctor's-consultation until the pain was too alarming, then she took the car and drove herself to the maternity-hospital; one hour later the little boy was out! Six days later she worked at the consultation, she was bored with the idle decumbiture, ^{she said}.

I drove further to Uppsala and had a very pleasant time there. At the Växtbiologiska (plant-biological) institution the professor and five teachers are old good friends of mine, really true friends, and the students working there are very nice, there as well as at the limnological institution in Lund the students consider me an "uncle" and call me Nestor, since I am the oldest of the active limnologists, my first paper published in 1909.

I made a little visit at Skuja and we had a good chat. He is no good discussor, he considers his opinion definite and unchallenged, but he is the best specific taxonomist in the world and actually all-round, but his scientific training is not sufficient for reliable biological discussion and higher taxonomy, sometimes his ideas are striking naive.

I have sent my desmid papers to Mr. Frowse and he has politely acknowledged the receipt. From your letter I have appreciatingly noted ~~your~~ the cases of new-found facies in his collections. My algological work has been resting, that is why this letter but contains gossip about my personal circumstances, probably of very little interest to you. The other day I received an invitation to an algological symposium in Praha next year, planned by the algological section of their botanical association. Ruzicka, the secretary, wrote me very amiably, probably he has come over his antipathy against me.

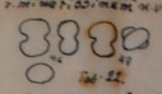
Your attitude to tobacco smoking is human, I understand it and agree to you. I have, however been compelled to decrease my too intensive pipe-smoking a little, my stomach makes objections.

Our best wishes to your wife and yourself.

Your friend

EMAN.


C. subgymnocarpa
 2. M. 1887, 201, 1887, 1887, 1887



lo 6.5-6.8, la 2.6-6,
 li 2.5 μ .

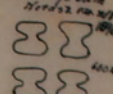
G. S. Nord: Fr. 10.
 Mj. Columbia

C. tortum Lagerh. et Nordst. n. sp. 1883



lo 16-20 μ , la 14-17 μ , ca. 10.5 μ , li 10-11 μ
 Habit.: Bog pool, Fjörnäs, province
 Härjedalen, regio subalpina.
 "Alga aquae dulcis urvicostae"
 fasc. 38, p. 17.

C. asphaerostomum Nordst.
B. strigatum Nordst. n. sp. 1883



lo 10-11, la 9-10,
 li c. 3.5 μ .
 Habit.: parts
 of Uppsalas.
 "Alga aquae dulcis" Nordst.
 35, p. 16.
 (n. 1264).

Dear friend Scott!

Linköping 26/VIII 1957.

What a blessed day, this Aug. 26: the postman let two letters from USA, from you and from Hannah! Both letters sent Aug. 22.

Your little Cosm. is very curious, both because of 1) the retuse apex, 2) the wide isthmus, and 3) the twisting. Character 1) is very scarce amongst the Cosmaria, e.g. C. contractum v. gartanense and f. in West: Fr. w. Algae N. Ireland Tab. 2:9, 10. The wide isthmus occurs in C. subaretoum, alphaerosporum, inconspicuum and tintum. In the latter retuse apices are depicted by Borge (Algenfl. Schwed. 5, Tab. 1:17) and Grönblad (Tvärminne). Grönblad has a curious bioculatum v. hians in Desm. Schles. Tab. 1:39, probably not *loculatum*. Taken altogether, the numerous pictures of the small and smooth Cosm. form a very confusing concept. G.S. West (Fr. w. Algae Columbia, Tab. 22:46, 47) has a twisted subaretoum v. minutissimum. I do not possess this paper but I enclose a drawing of my copy. Cosm. tortum is quite different, as you will find from the copy. I consider it a primitive Staurodesmus. Well, this is not much of what you hoped to receive, I fear. I think you are quite justified to describe it as a separate species, in as much as the torsion is constant both in Indonesia and Africa.

Glad to hear of your rich harvest of your intensive work at the microscope. I am awaiting the results with impatient interest. I am quite aware of Grönblad's valuable work in your latest paper. When I told you my sincere appreciation of you, I did it from the point of view, that you have done all the concrete work: collecting, investigating, drawing and preliminary distinguishing. It is much easier to sit down at a table ready spread and eat the delicacies than the procuring, selection, preparing and serving, though the detailed commentary of the menu in a correct manner is highly worthy of appreciation!

I have delayed in sending the abstracts of the asymmetry paper until I can add the paper on *Ducellieria* now published. In it I have given a list of plankters from the little lake we visited together at the trip in S. Östergötland.

Some days ago Kuno Thomasson visited me on his journey back from Denmark. We had a good phycological chat and he was very imposed of your investigation and drawings. He is an Estonian belonging to an old Swedish family, Estonia was a part of Sweden from 1561 to 1721! We received about 30000 fugitives from the Baltic countries during the war, e.g. Skuja. I know Kuno fairly well, he was a pupil of mine at the Aneboda laboratory about ten years ago and we have discussed phycology both here and in Uppsala especially these last years when I have lived there for shorter sojourns. I think he is about 35 years old, as long as I am and with a pleasant face. He is married with a very charming come patriot who is very artistic, she is appointed as a drawer at the university, she is a very good cook and a dinner table arranged by her, is something to remember. Kuno is assistant at the "Växtbiologiska institutionen" (Swedish växt = plant). He is energetic in a pleasant manner and I like him very well. He spends all his money on literature and is making an iconotheque. Regrettably he is not so good a drawer as his wife, he has sent me terrible pictures of planktic Staurostra from Sweden. His American pictures I have not seen before the publication, otherwise I could have done some corrections. His paper on Lake Bangveolo has not yet arrived to me but I will send him your drawings of the beautiful *St. fuellebornii*. His predilection for Irénée-Marie's drawings may depend on the manner of depicting a desmid in three views in geometrical fashion. As a rule he is, of course, right in his opinion that drawings imply the fundament of desmid taxonomic distinguishing, formerly a picture was valid without any diagnosis but not inverse. But they must be correct. The personal factor, however, too often prevents it is ideal, shortcomings of the microscope, of the eye, of the ability of observation, of the drawer's phycological knowledge and his capacity of drawing, all of these factors put obstacles to the ideal correct picture. Happily, no longer the lithographer is a factor negative! Microphotos are applicable only in very few cases. But this you are aware of quite as well as every earnestly working desmidiologist.

The *-is* of adjectives and adjectival nouns is inflected: *-is*, *-is*, *-e* in masculinum, femininum and neutrum respectively. The correspondence between generic and specific names is sometimes neglected, especially this happens

suffix

when a species is transferred to another genus.

I am very sorry to hear that your heart and lungs are not quite well, are you sure that the acceleration of the puls does not depend on psychological disturbances? For me, constipation is chronic since my stomach operation. The other day I tested my "eye-sharpness", I had 1,67, compared with the normal eye =1,0, "exceptionally good" said the doctor.

I have made an inventory of my psychological library in order to get a valuation of it, hitherto I have found more than 5000 copies. There is a slight possibility that the university of Uppsala will procure it with the condition that I maintain the disposal of it as long as I want. I do hope that this will fit, it would be a good thing to know its fate if I would die suddenly. I suppose that there is a market for it mm abroad but I prefer to see it conserved for swedes.

AEROGRAM
FLYPOST
PAR AVION

STREET: 20224

INSTRUCTIONS: Over the window, press the registration
mechanism. Cut out address on back. Insert into
envelope at right. Close envelope.

ADDRESS: Mr. A. J. P. P. O. P. t.

TO OPEN CUT HERE (POWERS) 10

Mr. A. J. P. P. O. P. t.
20224 Darte street
New Orleans 18,
Louisiana, U.S.A.

10/10
10/10

Well, now I think that you are bored by my chat, I am an inveterate letter writer, few of my friends are.
Our best wishes of health for you and your wife and good luck in desmid hunting!

Your sincere friend

Cirar

Sept 16 1957

Dear friend Telling,

Many thanks for your very prompt response to my request for information about *Cosm. tortum*, and also for the copies of the two other *Cosmaria* that you made. I have been quite busy for the last two weeks, and this is the first chance I have to acknowledge your letter. Your suggestion of making a new species of the little twisted *Cosmarium* had also occurred to me, but before doing so I must study it some more, for there are two other very small *Cosmaria* in the same collection which vary somewhat in their appearance, and I must find out if the twisted plant is related to them. One of them is *Cosm. staurastroides*, which however is different from the twisted plant, but there may be intergrading forms.

My part in the distribution of the USA desmid paper is finished, at last. Grönblad and I combined our two lists of correspondents, and he is sending to all the European addresses, and I to the rest of the world, which resulted in an approximately even division. Already I am beginning to receive congratulatory letters and postcards from persons who usually do not take the trouble to acknowledge. I even have one from Francis Drouet, the specialist on Cyanophyta, who is reputed to be a rather irascible and cranky person. I have never met him but I well remember my first encounter with him, back in 1943. Prescott had written the descriptions for our first *Euastrum* paper, and was supposed to get the paper in shape for publication, but had to leave in a hurry for Ecuador to find new sources of Cinchona bark, to make quinine. So he dumped the descriptions in my lap and asked me to write the paper. It was my first attempt at scientific writing, and I made a lot of errors, some of which apparently were glaring enough to catch the eye of the editor, who sent the paper to Drouet for criticism. I don't think Drouet knows much about desmids, but he caught a lot of the mistakes and returned the paper to the editor with a savage criticism, which was passed on to me. I was quite flabbergasted and didn't know what to do, but fortunately Prescott returned soon afterwards, and I passed it on to him, and he rewrote it. Probably it is fortunate that this happened on my first attempt at writing, for it taught me several lessons which I shall never forget!

Glad to have the information about Kuno Thomasson, which gives me a mental impression of him that will be useful when studying his papers. Perhaps by this time you have seen his paper on Lake Bangweulu, and can understand my remarks about his drawings! Also no doubt you will have noted the rash statements he makes, such as that the 'imposing' revision of the coccoid Cyanophyta by Daily & Drouet 'is not of much use'.

It will certainly be a very good thing if the Univ. of Uppsala will purchase your algological library now, and allow you the use of it until your death, and I can easily understand your wish that it should remain in Sweden. In my will I have provided that my small library and all of my drawings (probably 7000 or 8000) are to be donated to Dr. Prescott, to be used by him or disposed of as he sees fit.

I don't remember if I have made this request before; if I have please forgive. I would like you to send reprints of your own papers, and any duplicates you may have of other works, to Dr. Gerald A. Frowse, Fish Culture Research Station, Batu Berendam, Malacca, Malaya. He is a young Englishman, about 28 years old, who has worked at the British FW Biol. Station at Ambleside, and for short periods at the British Museum in London. His last place was at Khartoum, whence he sent to Grönblad and to me the remarkable collections from Lake Ambadi in the Sudan. I met him and his Chief, Dr. Julian Rzeska, in London a couple of years ago.

He is a desmid enthusiast like me, and is in the same position that I was 20 years ago; living in a very rich desmid region, but with neither the experience nor literature with which to identify his finds. He has just published a small paper, An introduction to the desmids of Malaya, hoping to arouse interest in the subject among the Malaysians. The text is OK because it is, of course, derived from authoritative works, and his drawings are reasonably good, but unfortunately he did not submit his sketches to G. or me for a check of his identifications, with the result that many of them are lamentably wrong. I have sent him a list of the correct names, as far as they can be determined from his illustrations, and I expect that he will be dismayed when he reads the letter. As might be expected, the desmid-flora of Malaya is quite similar to that of the southeastern Asian region in general, but already he has discovered several new species. One of them is a very peculiar Pleurotaenium, similar to Pl. spinulosum (Wolle) Branel, but with the small curved spines more closely spaced and confined to the prominences of the undulate body, the intervening hollows being devoid of spines. Also he has a new 4-radiate facies of Streptonema trilobatum Wall., hitherto known only as 3-radiate, but unfortunately he has published it as Phymatodocis followed by a question mark!! In material that he has sent me I have seen a 3-radiate version of Phymat. Nordstedtiana, while all previously known species of that genus are 4-radiate. So there are evidently many new things still to be discovered there.

So I am trying to help him all I can, and am asking my friends to send him their literature, and any duplicates they may happen to possess. I sent him a large package last week, and Hannah has sent him some.

My wife and I continue in reasonably good health, though neither of us can undertake much exertion. The possibility you mention that my accelerated pulse might be caused by 'psychological disturbances' is unfortunately not true. The acceleration is a physical fact, easily demonstrated by walking up one flight of stairs, or drinking one cup of our strong coffee, or smoking one cigarette. Yes, the smoking habit, despite my wife's efforts, has not quite stopped. The habits formed during more than fifty years of smoking are apparently too strong for my will-power to overcome, and apparently medical science, which can almost perform miracles, can offer no help in overcoming the tobacco habit.

But I am of a very phlegmatic disposition, and am neither alarmed nor worried by my condition. I don't want to change the routine of my life by giving up my cocktails before dinner, or my strong coffee, or my cigarettes; and if my life is thereby shortened by a year or two, ~~xxx~~ I shall never know it.

The best regards and good wishes to you and Mrs. Telling, from

Your good friend,

August 20 1957

Dear friend Telling,

Your letter dated Aug 13 and postmarked Aug 14 was delivered to me at 10 am on Aug 16, only 2 days in transit, a record I should think. It must have made remarkably good connections in Stockholm and New York.

The extravagant words of praise that you employ almost make me blush with embarrassment. Almost, but not quite! For I know that this paper is a good and important contribution to the science of desmidiology, and to the literature on our American desmids in particular. But you must not forget Grönblad's share of the credit, for I could never have done it by myself, and if Prescott had helped me the result would not have been as satisfactory.

The next paper to appear will be "Sudanese Desmids" by Grönblad, Scott & Prowse, describing about 200 desmids from a single lake in the Sudan, which ought to be published in the next two or three months. There will be 23 plates of desmids and some other algae, plus 6 plates of photomicrographs by Grönblad. I was somewhat disappointed that G. would not let me draw the plates, but for some unknown reason he preferred to have Miss Tikkanen do them, possibly because of the miscellaneous algae which he handled alone. However a lot of my camera lucida sketches will be used, together with some of his own.

After that will come the long-delayed paper by Scott & Prescott on FW Algae from Arnhem Land, now dragging along its slow course through Australian red tape. It will be published by the University of Melbourne, and will have 20 plates of desmids drawn by me and 8 plates of miscellaneous algae by Prescott; I expect to receive the proof soon, and it may appear early next year. There are 251 desmids obtained from 5 vials from 5 different localities.

Now I am working on ~~the~~ a paper describing the desmids from Borneo, Java and Sumatra, describing about 500 desmids from 46 collections, which may be compared with the 377 desmids obtained by Krieger from 146 gatherings. The greater richness of my material may be due to the fact that the collector, Mr. M. Sachlan, is a desmid enthusiast like me, and I gave him instructions on where to look for desmids and the best way of collecting them, by taking squeezings instead of, or in addition to, plankton hauls. When I was in East Lansing I spent 8 days, from 9 am to 6 pm, wading through the many thousands of sheets in Prescott's iconograph (or iconothek). He gave me 3 days of his time, at a period when he was exceedingly busy, and together we went through my drawings comparing them with the sheets that I had pulled out of the iconograph, and coming to decisions on their identification. But his checking had to be done hurriedly, and since I returned home I have made quite a few changes in the identifications.

There is one very small Cosmarium that we were unable to determine, and perhaps you can help me with it. It is shown on the enclosed sketch, and as you will see it has two ellipsoidal semicells twisted 90° at the isthmus, and with the apical margin indented; no other distinguishing features. It seems to be a distinct taxon because I also found it in the Sudanese material. In the Sudan paper Grönblad assumed it to be a twisted form of *C. goniodes*, but I am quite sure it does not belong to that species, and now G. has asked me what I want to do with it. There is some resemblance to *C. tinctum* and *C. subarctoum*, but these have convex apices;

also to *C. emarginatum* and *C. staurastroides*, but these have facial swellings. In my efforts to identify it I have come across the name *C. tortum* Lagerh. & Nordst., in Wittr. & Nordst. Exsic. No. 1486, and in Fasc. 35, p. 16, with Figs. 1-8. I do not have this, nor does Prescott, but I believe you have it in your library. Judging from the name, there seems a possibility that this plant may be identical with mine, so I would be very much obliged if you will copy the illustration and description of *C. tortum*.

Prescott told me when I was with him that he had not received a copy of your paper on asymmetry in desmids. If you have not already sent it to him will you please do so?

Kuno Thomasson has been sending me his papers, which are of considerable interest, but I know nothing about him. Can you tell me some details? How old is he, and how much experience has he with desmids? In his last paper, on Lake Bangweulu, he makes a statement that seems rather nonsensical to me, that Irene-Marie's "Fore Desmidiale", with its excellent drawings, could in many cases very well take the place of Ralfs'. Again he expresses the opinion that only drawings giving a clear interpretation of the alga in question should be accepted for modern taxonomic work. But his own drawings are in need of considerable improvement; some of them are hardly adequate for certain identification, or for critical comparison with others. His *St. Fuellebornii* var. *bangweuluensis* seems to have given him a lot of trouble, judging from his remarks, and his drawings are far from complete, as you may see from the enclosed sketch of a specimen from the Sudan which is very close to his, and perhaps identical. If you know him well it might be a kindness to give him some hints about these things.

It is wonderful news that your wife's health is so very much improved. I am glad to say that both of us are well, and enduring the heat as best we can. My health has returned to its condition previous to my illness of last April, but my heart and lungs are not in the best of shape; the least exertion causes my pulse rate to increase considerably. However that it one of the penalties of old age that nobody can escape.

My best regards and good wishes to you and Mrs. Telling.

Sincerely,

P.S. I notice that Thomasson uses the suffix -ensis with the neuter genus *Staurastrum*. I have been under the impression that -ensis is the feminine form, and that -ense should be used with masculine or neuter generic names. Which is correct?

Linköping 13/VIII 1957.

Dear friend Scott!

Hearty thanks for your letter of 10 June and for the doubled papers on desmids from Sumatra and Peru, especially the former of greatest interest though I was somewhat acquainted with it by your letters and pictures, sorry that I was unable to find any specimen in the sample you sent me. Since Miasummer I have been absent from home until now.

Many thanks for the extensive volume, the result of your intensive investigation of Florida and Louisiana. I think this is the greatest amount of new taxa collected in one volume since Turner 1992. I have not read every page but carefully studied the pictures, a very interesting and enjoyable pleasure from several points of view. I have found many fine contributions and interesting cases applicable to my viewpoints on morphogeny owing to your lavishing richness of brilliant and exact pictures. It was, indeed, an appreciable luck that you chose desmidiology as your micrological aim, living as you do in this rich desmid area.

I also got reprints of the plates and I must compliment you for the very careful and convenient arrangement; it was easy to cut out the series with sufficient space of the margins for writing the names, I think only a few of them were so narrowly surrounded by pictures that I had to copy them by hand. I appreciate also that you have not labelled every deviating form as a var or f, there are enough of them. Surely, this paper is one of the very few extensive desmid lists of novelties, compared with the similar works of Borge, Grönblad, West and Skuja it possesses the valuable advantage of being the result of careful and intensive inventory of different biotopes. I have received so many examples of your intelligent and scrupulous research that I am well aware of your capacity and I congratulate you with all my heart to the appearance of this manifestation of your penetrating desmidiological ability. I am sure that more ones will follow, soon enough, I hope.

I think that you and your wife had a pleasant trip to Michigan and a good summer in spite of the heat which is still strong, I suppose. Our short Nordic summer is soon over, it was delayed, only July has given temperatures of more than 20° Celsius.

I am glad to tell you that we are quite well, especially there is a marvellous difference between the condition of my wife's health now and twelve months ago.

I had a letter from Hannah with joyful informations.

The best regards to your wife and yourself from us both,

Sincerely, your friend

Einar Telling

June 10 1957

Dear friend Teiling,

It is indeed a great pleasure and an honour to receive the first copy of the Abstracts of your paper on Desmid asymmetry, with the special dedication inside the cover. This is quite an adequate recompense for the small help that I was able to give, and which I offer again at any time that I can be of assistance.

I have sent you by ordinary mail two copies each of two new papers; one on Ichthyodontum, the new genus from Sumatra; and the other describing a small lot of desmids from Peru which had been worked up by Dr. Krieger, the work being left unfinished at his death. The large paper by Grönblad and myself on USA desmids has been in the printer's hands since early this year, and ought to appear soon. Also there is a sudden burst of activity from Australia, after five years, from the editor who is getting out the official report of the 1948 Arnhem Land Expedition; so this paper by Scott & Prescott may be out by the end of this year. I hope so, for I am going to need it for reference in preparing a new paper on Indonesian desmids.

Next week my wife and I are going to East Lansing, Mich., where I shall work for a week or so in Prof. Prescott's office consulting his iconograph in the identification of more than 1000 drawings of Indonesian desmids, representing about 250 different taxa. Some of these are easily determined because of their large size and curious shapes, but I have already had a lot of trouble with some species. For instance you might think that St. saltans Josh. would be easily to identify from the drawings by Joshua, Turner, and West, in spite of the poor quality of the illustrations by the two earlier authors. But I have at least three varieties which must be referred to saltans, but none of them agree with any drawings that I can find in my collection. There is a possibility that one of them may be identical with St. orientale Turn., but it is impossible to be certain of what he was trying to draw. I have other similar puzzles with St. Freemantli, St. formosum, St. Gutwinski, etc., etc.

A few weeks ago I sent a \$1 bill to the Royal Irish Academy, and have just received a copy of W&W's paper on FW Algae of North Ireland; in beautiful condition considering that it is 55 years old. The paper and the engraved plates look just like new. I am afraid that many of the journals that are being published nowadays will not stand the test of time so well.

Glad to say that my health continues to improve, and I am almost back to normal. Ordinarily my wife and I would drive up to Michigan, a 4-day trip, but my doctor forbade it, thinking it would tire me too much, so we are going to fly instead.

I hope that everything is going well with you and your family. Please give my best regards to your wife. Will write again after our return.

Sincerely,

Lund 2/V¹957

Dear friend,

I was very sorry when hearing of the disastrous subsequences of your p~~a~~avers with Prescott. You must have been hit by an extremely evil bacterium that could cause such a terrible infection! I hope that you are recovering quite normally and that there will be no consequences as regards your good heart. Besides the physical troubles one is provoked by the unneeded loss of capacity for work, especially unpleasant for aged men like you and me who have an extensive material claiming to investigation and, compared with younger men, having no time to spend on steril bed-lying. I have many such cases of my own to regret.

I am now in Lund. Yesterday I took part of the students' attendance for the Rector magnificent and the musical festival of choir-singing to the honour of spring. The 1st of May is in Sweden a holyday, the official entrance of spring-time with festivities, among others evening fires on hills all over the settled districts. At this day the students put away their hats and use the white caps.

I am going to rest here a fortnight and "rub" myself with the acadarian life and scientists. I possess a lot of good friends and former pupils here and I am childish enough to appreciate and enjoy their amiable friendship.

The progress of spring is delayed, the great Magnolia trees are not yet burst into blossom, even in this most southern part of Sweden, but we hope for a good summer, nevertheless, the days becoming longer and 15-20 hours of sun-shine will give marvellous results.

I do hope that I soon shall get a letter with good news about your health and I enclose the best wishes to you and your wife.

Your sincere friend

E. M. A.

Linköping 8/III 1957.

Dear friend!

Many thanks for your immediate return of the proof. The curious inconsistency between delay after receiving the manus and the sudden hurry at the printing seem to be a general feature. The Swedish botanical editors are surcharged by papers, not at least from foreign botanists which in most cases must be refused; the members of the associations have precedence. This implies no cause of the mentioned behaviour, probably the printers are overloaded. Most of the dissertations are printed in Uppsala and Lund and actually in spring, and the Swedish dissertations are usually very thick.

Your case with the Australian algae is inexcusable if the editor has not given any acceptable reason for the delay. Is there no print possibility in USA, ^{by} which you could accelerate the Australian activity?

I received an appeal of rapid proof reading, otherwise the paper must be delayed to the summer volume. Thus, I must send it but prepared the editor about your more scrutinized proof which I, because of your rapid execution, was able to send him three days later. Probably he has taken it into consideration and the paper will appear the 1. of April.

To your *Euastrum dubium* var. *incisum* f. *asymmetricum* I added a reference to Behre's *Eu. mirum*. It seems to me that at least most of the semiradial asymmetry cases are inconstant formae, if there are any possibilities of discriminating varieties and formae practically in desmidiology. Possibly they appear spontaneously as the facies.

I am very curious about your foreshadowed photos, I thank you in advance.

By the way, I do want to compensate you for your expenses. I don't remember the costs of the manus, do you?? I still possess some dollars of those you kindly exchanged for me from my American checks several years ago.

Her is all well, my wife is still recovering, she appreciated your kind wishes very much and she sends her best greetings, as well as your sincere friend

TEILING

Linköping 3/II 1957.

Dear friend A.M.,

Please excuse my long silence ^{at} ~~after~~ ^{to think you!} having received your letter (with correct margins) and the manus-pages for which I am very grateful to you. I am very glad for all your changes; Englishmen seem to feel somewhat supercilious against foreign English and I am anxious not to cause the blamers more vexation than necessary. I have worried by my wife's health and also by my little paper which I must deliver before 1. Febr. Now I have sent ~~it~~ it and am feeling a slackening in tension, especially as my wife almost daily is telling me joyful bulletins of the progress of her health.

Many thanks for your note on Spondylosium panduriforme. This genus is at present fairly dualistic but it seems me justified to allocate panuriforme to it. The round Cosmaria with inserted sinus contain so few characters that can be used ^{or} considered specific, that it is appropriate to separate panduriforme from the heap of Cosm.moniliforme. Curiously Grönblad has depicted it and called it Spond.moniliforme, but this one is something quite different.

Many thanks also for the rich and valuable sending of pictures of Micr.anomala (and allied X.bifurcatum) and several other Micrasterias species from your abundant archive of marvellous tropical desmids. I have not yet had time to penetrate them but I shall later tell you eventual "simple thoughts of an idle fellow". It is no flatter when I say that you are a worthy member of the ~~active~~ ^{eminent} ~~ixix~~ society of altruistic desmidiologists as par example Brébisson, Nordstedt, and Borge who for the sake of common use gave their experiences with open hands. As the selfish man I am, I keep hold of your amiable promise to take over Prescott's paper on ecological literature, thank you. I got two copies of Staur.wildemani from you, but, when I had the scissors in my hand I could not use it and used instead the Indian ink. It is a sin to destroy useful things and I know how annoying the lack of own abstracts is, I have got in return some of my earliest papers after appeal to some very old good grinds, non-botanists. Thus, I am able to return one of the abstracts when and if you like.

Last spring in Lund I observed Cholnoky's paper. I do hope he is a diatomist (I am not able to judge) about other algae he is rather ignorant. Curiously he has called six n.sp. after the zoologist-collector, his ability of finding convenient names seems to be very small. Gonzales & Gandhi's Euastrielle n.gen. is unknown to me. The Hindu algologists seem to be disinclined in sending abstracts, I was obliged to give Ipengar several hints until he revenged my sending.

Skuja has printed a very thick volume about Swedish plankton, 400 pages and 63 plates in 42!! It contains not so many desmids, only five plates, all of them

superartistical. He is a marvellous observer and possesses a "snap-shot eye", that is why he has enriched our knowledge on the naked and delicate flagellates on living material more than any now living scientist. He must possess eyes with a retina more rich in rods in the fovea than an ordinary human eye, something like the eye of a condor. Nevertheless, there must exist an amount of imagination, it is impossible to discern the quite regular and uniform shape of the minute apical verrucae of e.g. Staur.chaetoceras and the cytoplasmatic structure of very small flagellates without staining.

The other day I got a letter from Alan Brook, a very nice Scottish algologist, that enjoyed me. I have found, in a sample from Lake Corrib in Ireland at the excursion in 1953, an extremely reduced form of St.armigerum var. furcigerum without the apical processes and the radial proc. shortened and developed into solid spines. This lake is one of the few habitats of Wests' var. reductum, the reducing trend seems to be inherent in this lake. Brook did not know my find and now he had found it and asked for my opinion and if he could make a new species of it (young people are always anxious of making new species, I do not blame, also I have been young, very young). I sent him my pictures, my opinion ^{and} my benediction and promised to search for more transitional stages. I also sent him my pictures of a corresponding reduction in St.pelagicum from the same lake. I fear that he had not observed it, possibly it may be taken for St.lunatum. This lake is one of the tho known habitats of Cosm.corribensel I am sorry that my material is not much richer, but the bus was waiting and the driver sounded with his horn very impatiently. In this case it is much more interest to elucidate the evolution of a new organism, it is more valuable to clear the relation than to present an isolated species.

expressed in
The aim of the science, nearest the taxonomy, is to find the relation between the organisms. The primary work is the inventory action of the scientists which are capable of distinguishing the diverse organisms in words and pictures, giving a real basis for further investigation and coordinating action. It is inconceivable, that scientists, for instance our common friend Gb, can ^{main} ~~maintain~~ an adverse position against constructive activity, evolutionistic treatment of species and genera, at least when I am practising ^{morphology} this work on desmids. Gb rejects the comparative, the only true principle of culture and, for the rest, our present knowledge of the desmids is too small to allow any generic conclusions (1). I don't know if he is aware of his motives, I do hope they are subconscious.

I was affected by reading your amiable regret of the situation Gb

contra Tg. It is absurd to ^{ascribe} ~~search~~ the cause in his disagreement of some rearrangements in Actinotaenium or ~~in~~ Staurodesmus, I am sure that there are many similar cases in other genera do not disagree him at all. Well, I do not intend to trouble you with a complete relation of this interesting psychological case; you are a good friend of his and is bound to him by the collaboratorship, but I hope and really beleive that you are also my friend and I will safe you from an inconvenient intermediate position in regard to your feeling to us both. I posses a placid temper and have consistently avoided resent (it takes too much force and time!) and as a teacher I have had opportunities of practising this attitude. In ending this chat I want to say that I know Gb very well, even better than you, quite natural: Gb and I have never been dependent by collaboration, we have met personay and discussed algological problems and circumstances of life ^{especially in our} ~~in an earlier~~, very intensive correspondence, we have the intimate contact caused by common language and similar education. And, my opinion of him, also his wrongs sides, has been confirmed by common friends, quite spontaneously. Of course we are still friends but I avoid discussion of generic taxonomy and changes on the whole. He has confessed that he hates all changes of already learned names and especially new concepts, depending on what? We change Christmas gifts, by tradition novels of our favourite authors. I got a letter a week ago, he writes that he has been sick and is not quite recovered. I was glad to hear that your paper on American algae has reached the stage of proof sheets.

I had a letter from Hannah three weeks ago. The doctors are not clear of her mother's sickness, she tried to leave the bed but after a few days she must return. Poor Hannah, " we are both discouraged" she writes. I think you would do the day's good deed if you wrote her a little letter, excuse me if you find me inconsiderate. She is sorry that she is unable to work with her Alaskan algae, she has done a good work in the parts hitherto appeared. Now she ~~will~~ will make pictures for an agricultural text book. In December the aubhor, prof. Poehlman at the University of Missouri, visited her for instructions. He is a friend of Sweden and they had "a good speech" about Swedish country and people, he ^{been like us} has studied our plant improvement.

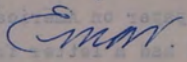
I hope that your "basal and local formation of fat" is cured now so that you have got rid of the cushion. I myself must use a rubber seat, for I am so scraggy that the skin of my buttock is squeezed by the ischium. I have, however, increased my weight after the operation and has just passed 72 kg but there is a long way to the former 78, a weight rather constant during 45 years.

The first week of this year I spent in Uppsala and had a joyful time with my algologist-friends. Have I told you about ~~professor~~ Astrid Cleve-Euler, professor honoris causa, the second woman of this kind. She is extremely intelligent, one of the finest diatomists and has also made fine works in quaternary geology and chemistry. She is 82 years old, but ~~seems~~ ^{looks} to be fifty and her humour and discussing ability makes the impression of woman of 35! She is very critical, ^{and feared by many persons,} but I have been happy enough of having her unrestricted friendship. Some years ago she published a Diatom flora of Sweden and Finland, 960 pages and 236 plates in 4^o! Her father was a famous chemist and inventor of new elements, one of her sons is professor of physiology, (recently guest-lecturer in USA,) another is envoyé etc, good inheritance, but her husband is the Nobel-prizeman von Euler, chemist.

We have had a bad winter almost like a spring with only a few days a temperature below zero. This is worth many millions ^{of dollars} every day in spared cost of heat ^{ing}. By this we have got back the hot water, a blessed thing in the household.

My wife thanks for your kind wishes and we both send our sincerest regards to your wife and you.

Your good friend

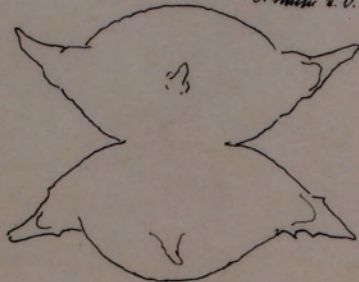


P.S. Of course my words about Gb must be considered personal and confidential.

St. armigerum var. *furcigerum*



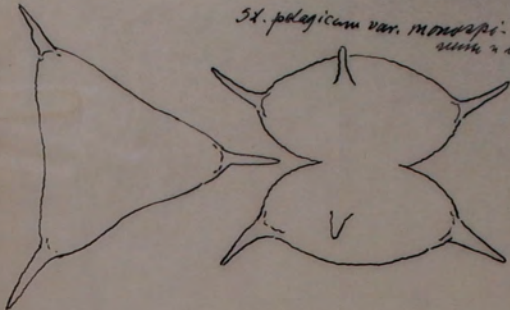
St. armigerum var. *simplicis-*
sinuatum n. v.



St. pelagicum



St. pelagicum var. *monospi-*
rosum n. v.



Linköping 12/IV 1957

Dear friend Scott,

the other day I got the latest volume of Botaniska Notiser and in a couple of months I hope to be able to send you an abstract "honoris causa". Quite rightly your name is cited about 40 times in the paper, I have the impression that you gave written it, and I am glad of that, I can not enough estimate your collegiality and scientific altruism according to the illustrious examples of Brébisson, Nordstedt and Borge.

Just now my desmid work has been left aside to the benefit of quite different tasks, for instance I have exploited my plankton knowledge for classifying lakes from tropical point of view in order to add some crowns to my retiring allowance. Among other commissions I delivered a lecture at the annual meeting of the Water Charge Federation of Östergötland, a fairly surprising experience; I think I have never been so obviously appreciated after any lecture.

I tell you my best thanks for the rich collection of desmid micro-photos which arrived exactly on my birth-day, thank you. I must say that you are a successful photographer within this intricate kind of art. It is a pity that its use is confined to flat algae and even the best photos can not replace a good drawing for taxonomical use. In giving more comprehensive pictures of smaller magnifications the algal microphotos, e.g. your photos of *Micrasterias foliacea* form a valuable aid and substitute of drawings. I have used it for total pictures of *Anabaena* colonies the ~~main~~ structure of which is very characteristic, a drawing of such a colony is an extremely time- and eye-spending work hardly possible to perform.

I hope that you are all right and suppose that you are busy in desmid work, proof-reading etc. I do hope that your silence does not depend on a unilateral position in the conflict between me and Ruzicka-Grönblad. Now I have passed through the healing process and avoid every thought of this sad affair. That is why I beg you in your letters not to touch this thing.

The strange weather continues in being quite abnormal, short periods of spring and even summer are mixed with periods of cold winter periods with snow. It is a good thing that the trees are adapted not to fold out their leaves until the new water of the spring has arrived from the root to the buds.

I am glad to tell you that my wife has recovered in a wonderful manner after a radical Röntgen treatment of the tumour at the cranial base. During several years she has declined every invitation to be in society with friends and colleagues, but now she confesses that she accepts with joy, a very good sign of restored joy in living. The great importance of this for me is easy to understand.

Now, at last, the best greetings to your wife and yourself
from your friend

P.S.

Perhaps it can be of interest to hear, that I have bought Wests' A Contribution to the Freshwater Algae of the North of Ireland directly from the Editor for the original price, four shillings(1) Royal Irish Academy, 19 Dawson street, Dublin, the Secretary. Very strange that the epithet Royal still is maintained in the land of Sinn Fein.

Einar Teeling

Linköping 6/XII 1956.

Dear friend Scott!

Since the beginning of the last year I have a manuscript of a paper on a new genus of the Heterococcales I have been collecting Åsore material, for instance in the little lake in which we fished plankton during our little trip last year. I have now finished it and added some notes on algae found in the actual lakes, especially our little lake. I enclose these notes with ~~my name~~ a petition for your assistance, especially concerning the correct English. Hannah has corrected the first part, but I think she has so much to do just now that I don't like to trouble her. Her mother has been sick, has been operated but without visible result. Consequently Hannah is very busy with her job at the school, her household and the old sick mother. It seems she is obliged to trust in herself in all these respects.

If you possibly have time for it, I should be very glad to get the ~~manuscript~~ in return rather soon. Because of special reasons (priority) I am anxious to get it printed soonest possible and the Editor's reception of it one week sooner or later can be of importance, if I can have it printed in the March volume of Svensk botanisk tidskrift.

The whole last month has been quite destroyed for me. My love of work has been quite missing because of the trouble with Ruzicka and also Grönblad who has appointed himself to judge and also prosecutor against me, without having taken part of my comments and letters to R. This is quite personal!

My wife is at the hospital. She has a tumour on her neck, that must be diagnosed and, if necessary, operated. Earlier she has been operated twice for it, and she fears that it may be cancer. I am afraid that Christ-

Dear friends Greeting!

Since the beginning of the year I have collected a number of new genera of the Heliconiidae. Instead I do hope it will be for you and I send my very best greeting of A Happy Christmas and A Successful New Year for your wife and for yourself!!!! I sincerely hope you will like my new book, especially our little lake. Hannah has corrected the first part, but I think she has so much to do just now that I don't think she will be able to do it. Her mother has been operated upon but without visible result. Consequently Hannah is very busy with her job at the hospital and the old sick mother. It seems she is obliged to trust in herself in all these respects.

If you possibly have time for it, I should be very glad to get the manus in return rather soon. Because of special reasons (priority) I am anxious to get it printed soonest possible and the editor's reception of it one week sooner or later can be of importance, if I can have it printed in the March volume of Svensk botanisk tidskrift.

The whole last month has been quite desolated for me. My love of work has been quite missing because of the trouble with Erik and also Gröndahl who has appointed himself to judge and also prosecutor against me, without having taken part of my comments and letters to him in quite general! My wife is at the hospital. She has a tumour on her neck, that must be dissected and, if necessary, operated. Earlier she has been operated twice for it, and she fears that it may be cancer. I am afraid that Christ-

Emma



Linköping 18/XI, 1956.

Dear friend Scott,

many thanks for your letter, always welcome and longed for. Your idea of *X. bifurcatum* as a possible synonym of *Micr. anomala* is very interesting and very good, indeed, I should like to have found it myself. At first I must congratulate you to your microphoto, it is an intricate to get, especially such an elaborate desmid in a position, that all its distal parts will be sharp and clearly depicted, I think you must have used a minimal opening of the diaphragm. I have compared it with the pictured in my iconothèque of *bifurcatum* viz. Borge's and Playfair's, and with *anomala*, viz. Turner's, Wests', Huber-Pestalozz and Joshua's pictures. I do not know if there are any more. Turner's are not clear in all details as to the apical processes (from an exciccate? in this case understandable), as well as Wests'. In this regard Borge's and Playfair's are all right. Borge has depicted evidently hollow processes, but Playfair's are somewhat dubious, though I believe that his lateral curves mean to indicate the frontal curves of the corpus, not the exterior limitation of the cell room against the (in this case) solid emergence of the structure like *X. armatum* et. consortes. An other character is the structure of the polar lobe. Borge's, Playfair's and yours (important) pictures have a broad base with rounded sinus between the polar lobe and the lateral lobe, but this feature occurs also in Huber's picture, which nevertheless is a true *Micr.*, even if its relation to *anomala* is somewhat questionable. The rounded sinus between the polar and the lateral lobe is found also in other *Micr.*, e.g. *depauperata*. Playfair's *bifurcatum* and especially his f. *juvenilior* take in this respect a transitional position, an Australian race? It is interesting that almost all processes of his main type are doubled compared with his *forma*, according to Borge's species *Pis forma* is more agreeing with Borge's. I think, as a result of these "desk fancies" that *bifurcatum* would be placed in *Micr.*, if as a synonym of *anomala* seems me fairly doubtful; the main point is the transfer and that it represents a peculiar type of *Micr.*

In this connection also Playfair's *X. pulcherrimum* comes into the limelight (Playfair New Desmids Found in N.S. Wales, Tab. 4: 70). I thank you for the hint, I hope you will not find me disloyal, if I discuss *pulcherrimum* as a probable *Micr.* in the list of *Micr.* which I deal with in my asymmetry paper. There is another addition viz. about *St. cristatum*, that also has been found in a semiradial form (Gisl.: Beobachtungen über die Desmidiaceen-Flora der Moore im den Kirchsee, Tab. 17: 12) that I shall add in the proof-sheet. Of course, I have no idea of anticipating your opinion of *bifurcatum*!

In my asymmetrical paper I have accepted your proposal to indicate the position of the apical processes with letters instead of siphers, thank you! Your proposal of isthman instead of my isthmal is submitted to a Latinist and he confirmed my opinion. The stem of the word isthmus is isthm-, the termination is -us, the suffix -alis means the position, e.g. *apicalis*, *lateralis* etc. The termination -anus means origin or connection, e.g. *mediterranean*, *caledonian* etc. I am very glad for your corrections and linguistic hints, I have collected them in a little personal dictionary for my further use.

When rereading some of your previous letters, I found in your letter of April 5. your opinion of the value taxonomic of the zygospores. I agree in full, but I have found that, in certain cases, pictures can, here as well as elsewhere, can be misleading, viz. in those cases where immature spores, which are smooth, have been taken for mature ones.

Your letter in May 29.: I would like to possess Prescott's Guide to the literature on ecology etc. Prescott has not sent it. Have you possibility of sending me a copy, I dare not ask him. I still possess dollars of those you kindly changed for me seven years ago. I am not a member of Phycological Society, I was, but only one year, and Prescott sent me the Bulletin as a gift during his time as secretary. I think there are some of the limnologists at the University of Lund, from which I can have the last copies with recent literature.

Your *Spondylosium desmidiiforme* is very interesting with its double asymmetry. I enclose a copy of Wests' picture, if you have any use of it. Wests say: "This species occurred in abundance, and the filaments were of relatively great length. The specimens (twisted)

mens differ somewhat from those described by Borge from Brazil, but not sufficient to be regarded as a distinct variety. The cells are a little smaller, and the lateral margins on each side of the median constriction are not so rounded as figured by Borge. In many cells the vertical view is somewhat asymmetrical (vide fig. 13 b, b'), and the cell-wall in the vicinity of the lateral margins is delicately and sparsely punctate. This Resmid could almost equally well be placed in the section *Didymoprium*, of the genus *Desmidiium*. Borge describes the presence of a mucous envelope round the filaments, but there was no trace of such a mucous investment in the Trinidad specimens.

I am glad that you got the exiccate in America. I have forgotten to mention that, at least our "Riksmuseum" does not send away "literary" exiccates. I had the luck of getting Borge's dry samples, containing *Coelastrum tricuspidatum* for examination. Sorry I have not yet returned it. Your letter Oct. 29. I am very glad to hear the slight extent of Ruzická's influence on Grönblad as to the facies. It was interesting to hear of your find of biradial *St. novae terrae*. I got the impression, that you have found for variation which is not known in the 3-radiate facies. In such cases, it is, of course, necessary to place the facies name before the variety name, at least until the same varieties are known also in the 3-radiate facies. I am rejoiced at your find, because in my proposal to the botanical congress in 1950 I called attention to the possibility of placing the facies name "before or after the indication of varieties or forma", see Radiation p. 325.

New abstracts arrive very seldom, some weeks ago I got two papers from Karl Behre: *Die Süswasseralgen der Wallacea Expedition* and *Die Algen bei Siedlung einiger Seen*. There are desmids in both, the most remarkable is a dichotypical *St. aciculiferum* + *furcatum* (in the latter paper). Are you in change with him? His address is Lesumstrasse 51, Lesum bei Bremen, Ostdeutschland.

Many thanks for your interest in my variation paper and the literature references. I know that there is a variety of a *Viola* only occurring on zinc soil and thus I have examined the algal flora in the ditch that leads away the water pumped out from the zinc pit, but I have not found anything extraordinary nor any increasing growth. I shall take contact with Kallio in this respect. It is a genus that there is something to be made out of because of the varying development of the distal parts of the processes. I have gratefully noted your examples on my list of dichotypical specimens.

As you see from the head of this letter I have drawn an exlibris for my physiological library. I made a stamp for it in order to avoid the tedious pasting. My ordinary exlibris is too great for abstracts. Prof. Heimans and also Kuno Thomassen have used a *Micrasterias* as a frame for their exlibris, very decorative.

Our best wishes to your wife and yourself from
your friend

Your letter in May 1951 I would like to possess. I have not sent it to you because of the difficulty of sending it to you. I have not asked you for a copy of your book on the physiology of the leaf, but I think that it would be very interesting to have it. I have not seen it, but I think that it would be very interesting to have it. I have not seen it, but I think that it would be very interesting to have it.

March 1 1957

Dear friend Teiling,

It is curious how editors will hold up manuscripts for months and then insist upon getting the corrected proofs returned by the next mail. But I am only too glad to help in any way that I can, so your Ms. is being returned the same day, after about 4 hours work. I started listing the changes so that I could put them in a letter to avoid returning the Ms., but I found so many small orthographical and typographical errors that it is easier to send the whole thing back than to type them out. There still remain many places where the language could be improved by changing a few words, but I suppose this is the final proof, and I am sure the editor and printer would squawk if they were made.

The passages that you asked about, on pp. 6, 10, 14, 22, 23, 29 are all OK, and I have no changes to suggest.

The omission of the stop ('period') after the titles and name of author seems to be a rather general printer's custom. My manuscripts are always written with these periods, as is usual in typewriting, but the editor nearly always deletes them.

Ruzicka's name must be spelled with the diacritical marks as I have shown; to omit them would be to risk an inkblot or a printer's error.

A week ago I received a reprint of Behre's paper on the Wallacea expedition, after I had begun to think I should never get it, and somewhat to my dismay I find that he has some new species and varieties that are also described as new in the paper by Scott & Prescott on Arnhem Land in North Australia, which has been in the editor's hands for 3½ years and is still not printed. I have written to Australia to ascertain if there is still to make the necessary corrections, for Behre's names, of course, have priority. There is one that I want to call to your attention, namely Euastrium mirum Behre, on p. 19 of your Ms. This is identical with E. dubium v. incisum f. asymmetricum Scott & Presc. on your page 20. But this Australian form asymmetricum also occurs in a symmetrical version which is so close to the specific form of E. dubium and var. propicum as shown in Krieger's Monograph Pl. 79, that Prescott and I have made it a new variety, var. incisum because of the deeper apical incision. I do not think that Behre's plant should be considered as a new species, and I shall allow our name to stand, though calling attention to Behre's name if there is still time to do so.

Yes, I received your letter a couple of weeks ago, but have not had time to reply; I will do so soon. I was very glad to hear that your wife is better. Please give her my best regards and wishes for a continuation of the recovery. A few days ago I sent you by ordinary mail a package of photos some of which will interest you, I think.

Sincerely,

Linköping 25/II 1957.

Dear friend!

Today I received the proof of the "asymmetrical" paper and immediately I sent a copy to you. On the one side I thought that you would be interested to see it in print, on the other side I should like very much to hear your opinion of the language in the parts written by me after your scrutinizing. In the hurry I forgot to mark these parts, they are:

- p.6, the passage "In this connection,....., ~~section~~ section": OK
p.10 " " "In the following..... semicell". OK
p.14 " " "Spondylosium desmidiiforme.....Tab.464:13)". OK
p.22 " " "St.cristatum failing". OK
p.23. three pieces: "According to rather old". OK but ---
p.29 the passage: "In fig.39 genetic centra". OK

Only the language is to be examined, of course not the references. Will you, please, note the changes proposed in a letter, referring to page and line, it will do. Do not send back the proof!

I hope there will not be too many misprints, by a superficial glance I have found:

p.19 Frontal region

p.19 Dr. Ruzicka

p.20 middle: "Curved and inflected radii" is a separate title.

p.21: Std. Olearii

p. ³⁰ "Taxonomical results" is to be placed on p.29 before "Microsterias", a curious mistake by the editor! Another curiosity: he has taken away the stop after title, name and certain subtitles, why? I do not know.

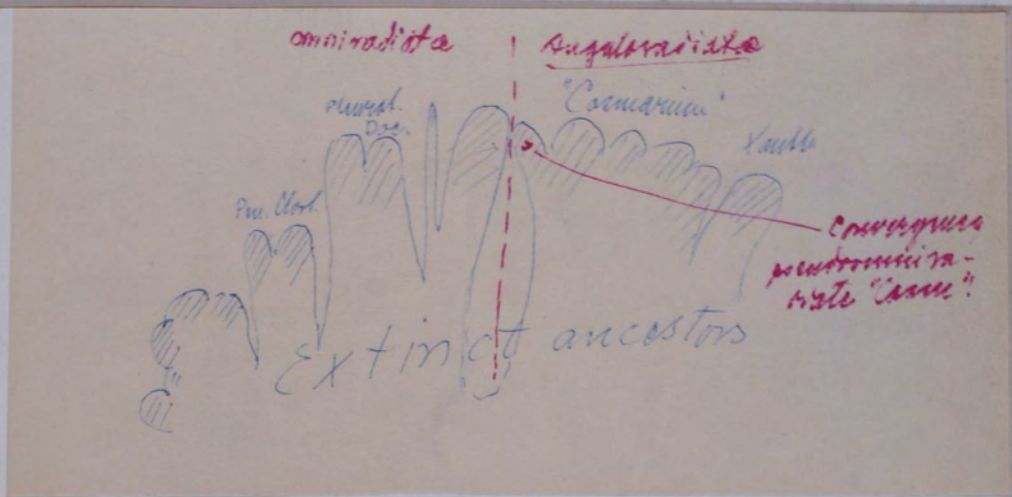
I am sorry to say that the editor is anxious to get my proof as soon as possible. If such a hurry is inconvenient for you, please don't care a bit about my diffident inquiry of scrutinizing, I think that a desmidiologist may understand the paper in its present state, that is enough for me.

I sent a letter in the beginning of this month, I hope you have got it. Karel Rosa has sent me a very singular Actinotaenium, living in the soil of a forest, he has cultivated it and sent a rich material of zygots.

The best greetings to you and your wife, excuse, I mean your wife and yourself, don't tell my unpoliteness to her!

your sincere friend

TELLING



Dec 10 1956

Dear friend Teiling,

It took me only about two hours to correct your manuscript, including the time required to look up some of the algae you mention. The Ms. is returned herewith, and I hope you will not think I have made too many changes. Some of them are mere personal preferences, but all of them will, I believe, make the paper "more readable" and better English.

You did not ask for comments, but I venture to say that I don't see that much, if anything, is gained by transferring Cosm. moniliforme f. panduriformis Heimerl to Spondylosium. Nearly all species of Spond. are simply Cosmaria which have formed the habit of not separating after division. I don't remember if I have told you previously that Kallio has cultured Micr. foliacea and has found that if the culture is exposed to too much light the cells do not form filaments, but remain unattached, in spite of the supposed 'interlock' of the apical processes. Quite possibly the same thing might happen with cultures of Spondylosium, which then would then revert to Cosmarium. In Micr. foliacea the apical attachment is so strong that filaments almost always break at the isthmus of a cell, rather than at the apex.

Again, your criterion for distinguishing Spond. from Cosm. must be a filament with at least 4 cells, for many desmids are found in attached pairs. I have frequently seen panduriformis, but never more than 4 cells attached, as you mention, so it just barely meets the minimum requirements. The difference in chloroplasts is a specific character, I think, not generic.

If you make this transfer, to be consistent you would also have to transfer such plants as Cosm. regnellii f. catenata, Krieger, which I have seen from Indonesia, and Cosm. ruttneri, which I have not seen. And what about the species of Pleurotaenium that form chains? I don't suppose anybody knows how long these chains are in nature, because they are so easily broken in collecting and subsequent handling, but I have seen them 4 cells long.

I have wanted to reply to your letter of Nov. 18, but have been thrown back in my work because of an operation for the removal of a sebaceous cyst on my buttock, the same one that gave me trouble when I was in Sweden and Finland, and also on many subsequent occasions. The incision is now healed, but I still have to be careful to sit only upon sponge rubber cushions. I have copied a number of my drawings for you, showing various forms of M. anomala and X. bifurcatum, which I know will interest you, and am sending them by ordinary mail. From North Australia I have three forms of X. bifurcatum, and all of them are different between themselves, and also different from the Indonesian 'race'. One of them is the same shape as X. pulcherrimum Playf. It possesses 4 extra facial processes, and the chloroplast is a central plate with ridges extending out to the various lobes, as in M. anomala and many other Micrasterias. Playfair describes and figures his X. pulcherrimum as having a chloroplast with 6 parietal bands visible, and this, if correct, would be an argument in favor of retaining it in Xanthidium, it seems to me, for I believe there is no Micrasterias with parietal chloroplasts. (Cf. chloroplast of X. armatum in Krieger, Monograph, p.29, fig. N). What is your opinion on this?

Perhaps your "Latinist" is theoretically correct regarding the spelling of the word 'gathmian', but Webster's dictionary says he is wrong, and the reason, of course, is that so many English words do not obey the rules. I shall have

I shall have to abandon my preference for 'isthmial', which I must have picked up from Prescott, who used 'supraisthmial' in some of his earlier papers.

Also by ordinary mail I have sent you Prescott's paper on Literature on Ecology. It is useless to me, so you may keep it if you wish; or you may return it if you also do not find it of much value.

Did you receive the paper by Scott & Prescott on St. Wildemani? I sent you two copies, so that you could cut out the illustrations in one of them. Thanks for giving me the name and address of Karl Behre. My friend Sachlan in Java also told me that this paper contains some desmids from Celebes, of great interest to me. I have written Behre asking for his papers, and have sent him some of mine in exchange. The only other desmid paper I have seen recently is a photocopy of one by B.J. Cholnoky, "Ein Beitrag zur Kenntniss der Algenflora des Mogolflusses in Nordost-Transvaal". This man seems to be a diatomist who knows very little about desmids. He gives a list of well-known desmids, and illustrates half-a-dozen 'new' species, some of which are certainly, and others probably not new. Most of them are undeterminable because of his poor and insufficient illustrations. You will find the complete citation in Bull. Phy. Soc., No. 27, April 1956. There is another paper cited therein which I have not been able to obtain, by Gozalves & Gandhi, "Euastrionella, a new genus in the family Desmidiaceae". This is an abstract published in Proc. Indian Sc. Congr. 40(3):66. After this citation somebody has placed the words "nomen nudum" in parentheses; I take it to mean that the description is insufficient or that there is no illustration, but I am curious to know what it can be.

I am extremely sorry to learn that you are so upset over the argument with Grönblad. He mentioned in one of his recent letters that he had written you concerning his disagreement with some of your re-arrangements, but I know nothing about the details. And all that I know about Quatka is the little that you told me. R. did not mention anything in his last letter to me, about a month ago. But I know Grönblad well enough, and you know him so very much better, to realize that he is such a fine and good and kind man that he would never willingly do anything that would cause distress to anybody, let alone a very old friend like you. So I hope I can help to convince you that whatever he has written was done purely in a sincere and friendly spirit. Surely, surely, surely a difference of opinion about desmid genera is not sufficient cause for a rift between old friends!

The news that your wife is in the hospital is not at all good, but I hope that the diagnosis will show that her fear is unfounded. I also have a 'tumour' on my neck, and another above my ear, but they are simply fatty cysts. Let us hope that your wife's is the same.

I have not heard from Hannah for a couple of months; I knew her mother had been ill, but thought she was on the road to recovery, and am very sorry to learn about the operation. Fortunately Hannah lives only a short distance from the college, and can run back and forth in 5 minutes, but it must be quite difficult nevertheless, and I don't suppose they have any too much money.

Everything is going nicely here in New Orleans. Christmas will soon be here, but we have no children nor relatives, and few close friends, so the holiday season does not mean very much to us. Perhaps we shall go away somewhere; I have not yet decided.

Please give my best regards to your wife, and my sincere wishes for her speedy recovery.

Your good friend,

Oct 29 1956

Dear friend Teiling,

Glad to hear about your Italian trip and that your wife derived a good deal of benefit from it, even though you do not seem to have enjoyed it very much. My wife and I cared least for Italy ~~and~~ of all the countries that we visited; we went there from Switzerland, one of the cleanest places I have ever seen, and by comparison most of Italy seemed rather dirty.

I have again examined Skuja's front view of X. Burkillii v. alternans, and now I see the markings that you mention, and which I had formerly overlooked. That shows how his peculiar style of drawing, with much black stippling, succeeds in merely confusing the interpretation. You think these markings represent outbulgings, but might they not also be considered as attempts to show, in dotted or dashed lines, the margins of the underlying lobe as seen through the overlying one? On referring to my own drawings of this plant, I find that on two of them I have drawn dashed lines to represent exactly this feature. Of course, since Skuja has seen your reconstruction and corrected it, his version must be accepted as correct, but I still have my doubts.

Surprised that Ruzicka is venturing into the field of higher taxonomy. From the little that I have gathered about him in exchanging letters during the last 10 years or so, I should think that is somewhat beyond his capacity, and like you, I think he would do better to confine his efforts to descriptions and drawings, as I am doing. It would not be much use sending me his Ms. and your comments, for I am not able to read German well enough to be able to follow a closely reasoned discussion.

Apparently I did not express myself clearly in referring to Grönblad's promise to Ruzicka. What he promised was that after the term 'facies' he would not show the name of the author. He has used 'facies' many times in our forthcoming paper, which ought to be in the printer's hands by this time. Now I see that you agree that the author's name is unnecessary, and this is my opinion in general, though I think there may be occasional cases where the author should be given credit. For instance, I have some remarkable biradiate forms of the 3-radiate St. novae-terrae, which show considerable variation within the biradiate structure. Perhaps Grönblad, in his final revision of our paper, has raised them to varietal rank.

About 10 days ago I received your paper on M. mahab. f. Wallichii, and have read it with great interest. You certainly have some remarkable collections of this plant, both for the great number of specimens and the extreme variation in the development of the processes. In my experience none of the varieties of M. mahab. occurs in quantity, so there must be some favourable influence in your lake. Also I have never seen v. Wallichii, though I have a few specimens of the closely similar v. bengalica from Indonesia. Rather strangely, M. americana is quite scarce in southern USA; I have seen only a few specimens of the typical form.

It is tempting to suppose that the zinc content of the lake water might have something to do with the teratological forms, so I looked in my small library to see if I could find out anything about the influence of zinc on plants, since I had read that it is one of the 'trace elements'. I found the following interesting reference in Welch's "Limnology", 1st. edit., 1935, p. 189:

"Several other elements appear in the analyses of plants, but, in some instances at least, it has not been convincingly shown that they are essential. For example, zinc, when present, is known to be absorbed by plants, and, according to some experimental

work, it stimulates growth. However, results to date show considerable disharmony, and the true role of this substance is still in the dark. According to Hutchinson (1932) zinc appears to be toxic to *Gleocera* under experimental conditions. This same author claims that the amount (0.00065 g. per litre) which is present in the waters of Bear Lake, Idaho, killed in three days those micro-Crustacea used in experiments and suggests that the very restricted fauna of Bear Lake is probably due to its zinc content. Further study is required before such conclusions can be regarded as having much weight".

The reference Hutchinson (1932) is:

Hutchinson, G.E. Experimental Studies in Ecology, I. The magnesium tolerance of Daphnidae and its ecological significance. *Int. Rev. d. ges. Hydrobiol. u. Hydrog.*, 28: 90-108.

Another paper by the same author is: 1930. On the chemical ecology of Lake Tanganyika. *Science*. 71: 616.

Today I spoke with one of the chemists at the New Orleans Waterworks about another matter, and I mentioned the zinc content of your lake. He thought 0.6 ppm (parts per million, which is the way it is expressed over here) would be very high, and much higher than is usually encountered.

Paavo Kallio has a summer cottage on Lake Eljäjärvi, some 50 km N.E. of Turku, and the house is situated on property owned by a company that is mining zinc ore just a short distance away, perhaps 2 or 3 km. He told me that he would have to move if the company decided to extend its mining operations. Perhaps this lake or others in the vicinity, which receive drainage from the surrounding hills, may contain zinc, so he might be able to tell you something about it. Also perhaps he could give some information about the effect of zinc on algae, ^{desmids} or try some experiments to find out.

The fact that *f. Wallichii* change into *f. typica*, and vice versa, does not seem too surprising to me, for similar phenomena are known in other *Microsterias* species. For instance, in some of the American varieties of *M. radiata*, I have seen occasional dichotypical cells combining the variety and the specific form, showing a tendency to atavism. Also several authors have illustrated dichotypical cells of *M. pinatifida*; Cf. Presc. & Scott 1952, Pl. 7, Figs. 8,9,10. These three specimens were taken from habitats separated by some hundreds of miles, with no possible water connection between them. But all such finds have been isolated ones; I have never seen the phenomenon on anything like such a large scale as in your lake. It would be a very interesting experiment to transplant some of the *M. mahab.* from Lake Trysjön to another lake with no zinc content, and see what happens to them. I think it would not be difficult to make a device that would keep most of them in one place, e.g., a wooden framework containing a large mass of *Utricularia* from which most of the water would be squeezed and then the water containing the desmids poured on it slowly, and the whole thing submerged in the lake water.

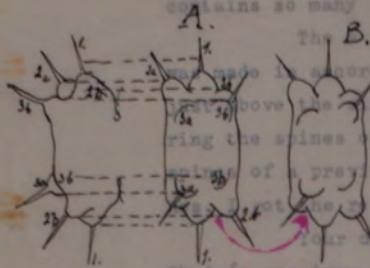
Rather to my surprise, about a week ago I received from the Farlow Herbarium at Harvard three volumes of Wittr. & Nordst. *Exsiccatae*, and as soon as I saw them I remembered that you had shown me one of them in Linköping. (My memory is getting very bad). So it is not necessary for you to do anything about the request I made. I have examined material from the three numbers cited by Borge, with the following results; In No. 539 I found one filament of five cells, but it was attached to a mass of debris and I could not detach it. In No. 554 I found no specimens (Borge found one). In No. 1270 there are plenty of filaments, most of them much distorted, but I found one isolated cell from which I could determine that it is asymmetric, with the projection at opposite ends of the long axis in the two semicells, in end view, and identical with my specimens. But the question is still unsolved as to whether the larger form on which the species is based (Borge 1903) is asymmetric. And I have not been able to ascertain if the two types of asymmetry exist in the same filament.

Best wishes to you and Mrs. Teiling, from your friend

Linköping 21/X - 1956.

Dear Friend Scott!

Many thanks for your very valuable sending of pictures and survey of the concerned desmids. I hope you have not wasted too much time on it, but the whole is a valuable contribution to my iconotheque that already contains so many pictures of your skilful hand and sharp eye.



The B. var. alternans reconstruction of Skuja's X. Burkillii var. alternans corresponds to his front picture. In this you can see some shadowings above the sinus, and I interpreted them as outbulgings paired to those bearing the spines on the opposite side. Provided that these bulgings lack their apical of a previous symmetrical semicell with paired apical and subapical spines, not of the reconstruction marked A, Skuja corrected it into B. Your drawing of the Borneo specimen lack the spinebearing bulgings of Skuja's specimen and belongs to a somewhat different ~~strain~~ strain. It may be that the shape a vertice of the semicells is more consolidated to a more "compressed" type like X. sexmamilatum. The bulged bases of the spines and the stouter corpus speaks for this supposition, provided that the primitive form is of this type of X. cristatum.

We had a good journey in Italia though somewhat tiresome. Just at my about trip to Italy for a Botanical Documentation in der Algologie" for which he wanted compartnership of Grönblad and me. I put it in my bag and used it as night literature. I was very disappointed! R. delivered severe demands on a correct use of species, varieties and formae and discussed these concepts together with subspecies, morphae (Ruzicka's favourite term), ekomorpha, status and facies. This is very good, but when explaining ~~the~~ and discussing these concepts he glided out in improvisations that revealed the most horrible misunderstandings. I wasted more than one week in writing 14 pages of corrections. I do hope that he will confine his talent to distinguish sp. var. and f., i.e. describing and drawing in which he possesses a very good ability. Probably he will not enjoy my letter but I consider it the best, in order to prevent a public foolness or at least a drivel, inexpedient for him.

Schweiz"
Pascher's Süßwasserflora ~~des~~ Deutschlands, Österreichs und der Schweiz is complete except the desmid part. The first volume appeared in 1913 and consequently several volumes are not modern. Prof. Schussnig asked me for a proposal and, knowing that all the possible, i.e. capable desmidiologists were eagerly occupied with their own work, I proposed Ruzicka, mentioning my opinion about his qualifications. I do not know if R. will accept. He is inclined to do it, of course, and he wrote that he thinks he will be able to perform his task as a parallel to his Czeckian desmid flora.

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Now, I must beg your pardon for my failing answering your letter of Sept. 11 and arrived the 17. Not before the 19:th I was able to send off my mscr. to the Editor and ~~then~~ ^{then} I had only three days for all preparations for our journey.

At present I shall confine my writing to your information about Grönblad's and Ruzicka's collaboration in order to detain the use of facies in the nomenclature. I fear that G. has ^a somewhat diffuse understanding of the facies concept. In his "Desmids from USA" 1956, p. 31, he has recommended the use of facies and has used it concerning St. cuspidatum, Johnsonii, ophiura, rotula (the last with quotations marks!). The two former he called fac. ovale which must be interpreted as biradiate. The biradiate cuspidatus is earlier described as Arthrodon constrictus by G.M. Smith.

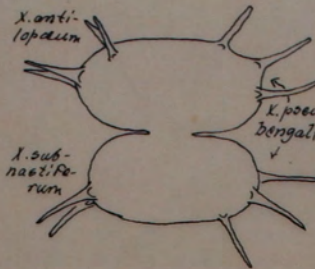
In my proposal ("Radiation" p. 325) I have put author's name after my examples of the use of facies in the cases, where the facies names are previous species, variety and forma names. At this time the facies view-point was quite new and in such cases the best manner of explaining is to give examples. In a general use of facies in the nomenclature, fac. with attached epitheta ending in -radiata, ~~form~~ -gona, or -radiata a following author's name is not necessary, in many cases hardly ^{to miss out} possible. Personally I do not like the extended use of author names even in formae, especially in the case of var. or f. minor, major, extensa and similar. It is quite ridiculous. The manner is quite indifferent, but the term fac. must be used, expressing a character of great morphological significance, since the ^{diverse} radiation implies a basic feature of the ~~mixxxx~~ structure of the anguloradiate desmids. Remember, that every anguloradiate desmid specimen appears in one or other facies. and also that the difference between the bi- and pluriradiate facies during most of the "desmidiological" age has been considered a generic distinguishing character. I think this is a sufficient proof of its morphological significance.

At present you are one of our most producing desmidiologists and I do hope that your activity and production will be quite as vivid during many years. That is why you have to penetrate the facies concept and take your position. I have developed my knowledge and view-points in my commentary to Ruzicka. If you are interested of informing yourself of R.'s peculiar motives of refuse and my motives of using it, I am willing to lend you Ruzicka's macr. and my commentaries. It seems very singular and almost incredible ~~that~~ ^{what} G. has promised R. not to use the facies term (at all events he has used it in the Croasdale paper, communicated Nov. 15 1955!) and not told it to me, such a behaviour would be unfair play after having been my friend during more than 30 years!

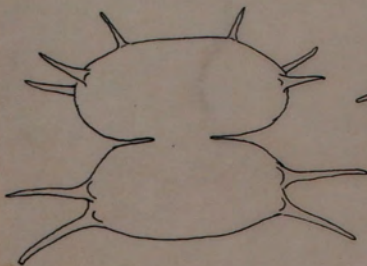
Our journey was favourable for the health of my wife. The otitis ^S had at first kept her in the bed for weeks and afterwards prevented walking, ~~th~~ she was extremely weak, I must lead her very slowly and she always wanted to go by car if the distance exceeded 4 hundred meters. I refused and kept her walking and "bussing". In the busses she was obliged to stand and cling to the horizontal bars. This exercise, however, increased her physical conditions in a high degree. ~~at the~~ ^{at the} end of our visit she was able to climb the Palatinum and was very pleased at it.

My and my own best regards to you and your wife. Have a pleasant journey.

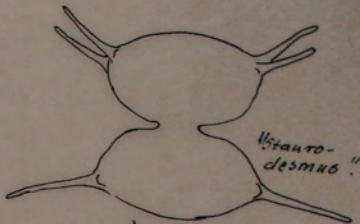
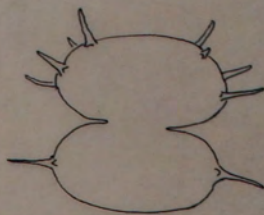
X. anti-
lopaeum



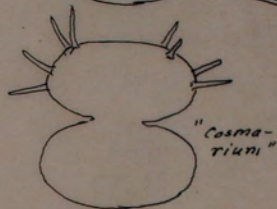
X. sub-
naeliferum



X. pseudo-
bengalicum



"*Hystero-*
desmus"



"*Cosma-*
rium"

Teiling, Aug 56

Linköping 16/IX/ 1956.

Dear friend Scott,

I am just finishing my "asymmetrical" paper. When assorting the pictures forming the substrate of my paper, I found a drawing of yours that still puzzles me, viz. Xanthidium Burkillii var. alternans. I had proposed the name sexmamillatum, but in your letter 5/IV you maintain Burkillii because of the paired lateral spines and the scrobiculations. A comparison between this picture (Sachlan "X" lake, East Borneo) and your sexmamillatum var. pulneyense (Sachlan 102, Sumatra) I am unable to find any specific difference. Your Burkillii is not a vertice divided into two lateral lobes as is West's picture (Burma). Scrobiculations are not likely specific characters.

In my view both your pictures a Fronte are quite agreeing, a vertice your Burkillii differs from your sexmam. only in the displacement of the subapical spines to the left (and this in a very small degree) and the apical spines turned to the right, in contrast to var. pulneyense. The position of the two apical spines contradict a supposition that they can be the surviving of the four apical spines of Burkillii. Skuja's var. alternans differs in the first hand from f. typica by its lateral median spines. The two apical and also two subapical are semiradially "survivors" of the normal four. Have you noted that Skuja's picture a vertice is incorrect, he has approved my reconstruction.

I beg your pardon that I trouble you with this discussion but I am anxious to be quite convinced of every word I publish. Of course I can write (Scott det.)

I have been working very intensively after my return to Uppsala. Next week Tora and I are going to Italy, by air to Milano and then by bus to Pallanza, to Istituto Italiano di Idrobiologia, a great scientific station with botanical garden, founded by a very rich "Dottore". We get a guest room free and the fare is very inexpensive. After a week we shall go to Rome, probably I would rather rest at Spallanza, but I am not alone. We have had such a cool and wet summer that we want to feel a real summer as it may be in Italy in Autumn. I have always avoided flying and had hoped to have my feet on the good old soil until I shall rest in it, but Tora has eagerly incited me to enjoy flying and now she has spared money for the tickets during long time, consequently I must give up.

Spallanza is situated at Lago Maggiore and the institute has cars for the staff and the guests, so I hope to get plankton samples from several lakes.

No letter from Grönblad, he was anxious to get my opinion about his (and yours) paper on St. Bibrachatum but I am not sure that he liked it.

I got a letter from Ruzicka. I had animated professor Schussnig to offer the final elaboration of the desmid part of Pascher's Süsswasserflora to him and he is now very irresolute as to this proposal. Lütkemüller began - and died, Grönblad continued but gave up, who will finish it?

The best wishes to you and your wife from both of us,
your friend

EMAN

P.S. Do you still maintain St. furcatum var. asymmetricum?

Sept 11 1956

Dear friend Teiling,

I was distressed to hear of your sister's death, and I beg you to accept my deepest sympathy. I can understand something of your feelings, for only last week my elder sister in England, who is 75 years old, had to undergo a double operation, - not serious for a younger person, but potentially dangerous at her age. We had an airmail letter from my younger sister saying that the operation was performed satisfactorily, and that the patient is progressing as well as can be expected.

How is your wife now? I hope that she has recovered completely from her illness earlier this year. Please give her my best regards. I am glad to say that both my wife and I are fairly well, and we shall feel even better when the hot weather is over, but that will not be for another month. September is usually the worst time of the year, for everyone is tired of the heat and humidity.

I greatly enjoyed your long and discursive letter, which I have gone over several times. Many of the things you say are completely true. I have conscientiously tried to tell 'the truth, the whole truth, and nothing but the truth' in my writings and drawings, and to follow the accepted rules of classification and nomenclature, and to give credit where it is due. In the forthcoming paper by Grisebald and myself I have refrained from including two new desmid varieties because I happen to know that another man has found the same plants and is preparing a paper in which they will be described. If I am still not entirely convinced of the correctness of some of your changes, you must ascribe it to my inability (which I freely admit) to comprehend the principles of higher taxonomy on which your views are based.

I could still argue about some of the points that you discuss, but I believe that neither of us would profit by a continuation of the discussion, so I propose that we let the matters rest as they are. OK?

The paper on the new genus from Sumatra, which has been named Ichthyodontum sachlanii, may have been printed by this time, for I returned the corrected proofs a couple of months ago. However, I do not expect to get the reprints until about Christmas, the last package of reprints took exactly three months in transit from Java.

Now I can tell you about something that I think will interest and even astonish you. From Brazil I have a curious little desmid that exhibits a strange form of asymmetry. Before describing it I must relate a little history. Your friend Borge in his 1902 Regnell'schen paper described and figured Sphaerozosma desmidiiforme, and from his vertical view, Fig. 23c it is seen to be quite symmetrical, though his front views, Figs. 23a and 23a' show the upper semicell projecting on the right a little beyond the lower semicell, and the lower projecting on the left slightly beyond the upper, also in one of the two cells in Fig. 23a' the sinus is not quite centered. G.S. West transferred Borge's plant to Spondylosium, I suppose because of the lack of apical granules (I do not have West's paper). In his Sao Paulo paper (1918) Borge described a "forma tenuior" of Spond. desmidiiforme, but gives only a front view, which, however, also shows

the two semicells projecting slightly beyond each other and alternately to the right and left. It is this form tenuior with which my specimens seem to agree in size and front view. Borge distinguishes two forms with acute and obtuse-angled sinus; I cannot say that my specimens belong with either one of these two forms, because the shape of the sinus varies considerably, both in reality and because of the varying angles at which the cell is seen in the strongly twisted filaments.

But in vertical view of isolated cells my specimens show a curious unilateral asymmetry, in the form of a projection beyond the generally elliptical outline of the cell, and this projection is deflected to one side of the long axis of the ellipse. Somewhat similar projections and deflections occur in some species of Desmidiium, e.g. D. swartzii v. amblyodon and D. pseudostreptonema. But these in vertical view are 3-, 4- or 5-angular forms, and the projection occurs at all of the corners.

But this is not the strangest feature of the Brazilian plant. As you will see from my sketches, the asymmetry occurs in two forms. In what may be called Type 1, the asymmetry occurs on the right-hand side of both semicells; in Type 2 the asymmetry is on the right side of one semicell and on the left side of the other. Further I believe, though not yet certain, that both types occur in the same filament.

You will observe that in Type 1 the lower semicell could be made congruent with the upper one by a rotation of 180° about the horizontal axis. In Type 2 it would require two rotations, one of 180° about the horizontal axis and another of 180° about the vertical axis. How's that for a curiosity? I think that this phenomenon has not been observed before in desmids, at least I cannot think of any instance. I seem to have extraordinary luck in discovering such peculiar forms.

If both types occur in the same filament, it means that sometimes a Type 1 cell will in division give rise to a Type 2 cell, and vice versa. Determination whether this is really the case is made quite difficult by the strong twisting of the filaments, about 90° in the length of 10 to 12 cells, so that only one cell in each 20 can be seen in exactly true front view. The adjacent cells on each side of this one are tilted up or down (in the line of sight) by about 8° , the next ones by 16° , etc. At greater tilts it becomes impossible to tell which of the semicells carries the projection and at which side. Even in a true front view there is little difference to be seen; the one bearing the projection extends a little further, and at the place usually called 'the upper lateral angle' there can be seen a very slight angularity, as I have indicated in my sketches. The filaments are quite long; two that I counted had 181 and 227 cells; they are usually curved considerably and entangled in masses of debris, so that I have not been able to rotate the filament on its axis with sufficient gentleness to be able to get successive cells into the true front position.

There is another feature of my specimens in which they may possibly differ from Borge's, namely the undulate margin of the elliptical profile in end view. This is not at all visible in front view, and best seen in a slightly tilted end view. In true end view the undulations can be detected with difficulty, but would easily be overlooked unless one had previously seen the tilted view. They are caused by two parallel rows of rather widely spaced pores encircling each semicell, which can sometimes be seen easily, at other times only with difficulty or not at all. In the slightly tilted end view, the margin that is tilted down shows the undulations quite distinctly, but the margin that is tilted up usually does not show them because of the different angle at which it is seen.

If this plant had not previously been assigned to Spondylosium I should have been inclined to place it in Desmidiium, but I believe it would fit

into either genus. There is one species of Spondylosium, S. pulchrum, which is asymmetrical in end view, resembling a two-bladed propeller such as is used on small boats; also the apical swelling develops from an infolding of the apical wall, though I believe Grönblad is the only one besides me who has seen it. This infolding is said to occur only in Desmidium and Gymnozyga (Bambusina), but it also occurs in Streptonema trilobatum which strongly resembles Spondylosium pulchrum. So far I have not seen any early stages in the division of my Brazilian plant, so that I cannot say whether it shows the infolding.

That is quite a long discussion of a very small desmid, and I hope it has interested you. It would be very desirable to ascertain if my plant is identical with Borge's "forma tenuior". Do you know if the original collections, which are enumerated by Borge, are still preserved in one of the museums in Sweden, and if so whether it would be possible for me to obtain a little of the material? Also he refers to three of Wittrock & Nordstedt's exsiccatae, Nos. 539, 554 and 1270. Probably the curators of these valuable relics of the past would not be willing to lend them to me, since I am unknown in Sweden except to you. Alternatively, would it be possible for you to obtain some of the original collections, and/or the exsiccatae, and compare with my specimens? I could send you a very small quantity, and fortunately the plant is fairly common in filaments, and isolated empty cells can be found without too much trouble.

Here is a question that you discussed with Grönblad, but I am not clear as to your position. He and I had agreed to use the term facies as a definite taxonomical category, ranking just below forma, which means that the facies name would have to be followed by the author's name, and would have to agree in gender with the genus. Sometime during the last year Ruzicka (a lawyer) has persuaded him that this usage is incorrect, that facies should be considered merely as a descriptive word without taxonomical rank, and consequently must not be followed by the author's name, and the facies name should therefore be in the feminine form. I do not think this correct, but because G. has promised R. not to use facies as a category, I have agreed in so far as our next paper is concerned. As with most arguable questions, there is much to be said on both sides. G. refers to St. meriana which occurs in radiations from 3 to 9. I know this plant only from pictures, but I imagine that one collection might contain several different radiations, and consequently this character may be considered more or less trivial and not worthy of separate names. St. ophiura is another example, as is the closely similar St. sagittarium known in radiations from 6 to 11. But in one collection from N. Australia (the only one so far to contain the 11-radiate form) there is only the 11-radiate plant, so that this character seems to be constant and inherited. Rather strangely, I have failed to find in my library formal definitions of species, varietas, and forma, but I believe that a requirement of each of them is that they must 'breed true', so that the 11-radiate St. sagittarium ought to be entitled to a rank somewhat higher than facies, if this word is used merely as a descriptive term to denote rather trivial variations of radiation.

Thanks for your sketches of X. antilopaeum, showing cells with some or all of the spines missing from one semicell. I too have seen occasional examples of this, but since they were quite rare I have, as you say, regarded them as accidental and monstrous variations. The phenomenon seems to be more common in Arthrodesmus, and in our next paper I have recorded several cases in which all spines were absent from both semicells, so that the plant could be mistaken for a Cosmarium if it were not for connecting forms. What the significance may be I don't know; perhaps they are 'reversions to type', or 'throwbacks' to the ancestral Cosmarium-form.

Your concept of small lakes as culture vessels is interesting and plausible, especially if they contain a comparatively small number of species.

I expect that some of my habitats contain just as many myriads of individuals as your Swedish lakes, but a feature that I have found in many tropical collections is the large number of species. In the first paper by Prescott & Scott (1942) there are 145 different desmids, obtained from the squeezings from only a few handfuls of aquatic plants in a roadside ditch. Since then I have not been able to make any exact counts because so many of the desmids were unidentified, but I think that some of them are even richer in number of species. From Lake Ambadi in the Sudan, too small to show on most maps, Grönblad and I have recorded 200 desmids, and I think that a few more could be found if we had time to continue the search. In such material some of the species are exceedingly rare, only one or two specimens having been found. This is vastly different from your plankton catches with many thousands of specimens of the same species. I should quickly tire of examining such collections.

Many of the habitats that I have collected from are quite small and shallow, and are subject to enormous variations in temperature and chemistry when the water evaporates to very low levels or dries up completely. This may account for the large variations that I have recorded, the many Janus-forms and dichotypicals, not in your Staurodesmus species, but in larger and more highly developed, ^{ones} in St. elegantissimum, for instance, I have radiations of 2, 4, 5 and 6, and Janus-forms combining 2- and 4-radiate semicells. There is a new variety of St. Johnsonii, highly elaborated, in a 3-radiate form and a Janus-form 2- and 3-radiate. St. brachioprominens occurs in several forms of such greatly different shapes that we have had to give them varietal names, and there are individuals with extra arms that cannot be said to have any regular degree of radiation.

So it may be that the conditions of existence for my desmids are pretty near as severe as for yours, but that the factors which vary are no doubt different in the two cases. It seems to me that the conditions in natural habitats, whatever they may be, that give rise to so many different forms, perhaps differ only in degree from the unnatural conditions of cultures in vitro, which produce so many kinds of monstrosities. What taxonomic value can be attached to such forms is more than I can determine.

Your find of M. tropica in Sweden is indeed unusual. It seems to be a rather rare species even in tropical waters. I have seen only very few specimens. There is, of course, the var. polonica from Poland, of which I have found a closely similar but more slender and graceful form in North Australia and quite recently in Indonesia.

I think I have written quite enough for one time, so will close now with my best regards and good wishes.

Your best friend in southern USA,

Sept 22 1956

Dear friend Teiling,

I am only too glad to share with my friends any knowledge that I may possess concerning desmids, and I hope you will continue asking me about any tropical and subtropical species in which you are interested. You would be surprised at the large number of them that I have in my files.

Accordingly here is a further discussion of X. Burkillii and X. sexnamillatum, with their varieties. None of the existing drawings of these plants is satisfactory, according to my standards of draughtsmanship. W. & W.'s front view of X. Burkillii was evidently made from a specimen that was not exactly level, and even a small tilt will cause a considerable change in the apparent relative position of the spines in the camera lucida drawing. I recognize the symptoms, for the same thing frequently happens to me. When this occurs I try to balance the specimen more exactly and make another drawing. Sometimes this is impossible, and then I change the drawing to show the spines in their true position, which can be seen visually in nearly every instance, though it may be impossible to keep the specimen in the true position long enough to make a drawing. Some persons might think this 'faking' to be illegitimate, but I consider it not only legitimate but absolutely necessary. The only purpose of publishing a drawing is to enable other workers to identify it and compare it with their own drawings or specimens, and it is far more important to publish a correct drawing, even if slightly 'faked', than to insist upon an exact reproduction of a faulty drawing, such as those of Walle and Turner, many of which are impossible to identify.

Examining W. & W.'s front view in detail, you will see that on the left side of the upper semicell the two lateral spines are shown very close together and apparently rising from the same lobe. That this is incorrect can be seen by comparing this pair of spines with the other three pairs. Again, on the upper semicell the spines in each of the two apical pairs are shown close together; on the lower semicell they are widely separated. These inaccuracies are caused by the tilted specimen.

In W. & W.'s vertical view of X. Burkillii note that the apical group of 4 spines is rotated clockwise. I have made two drawings of this specific form, one from Java and one from Borneo, in which the apical group of spines is rotated counterclockwise. I am sure my drawings are correct, while there is a faint possibility that W. & W.'s drawing might have been reversed in transferring it to the plate. Again, their vertical view makes it appear that on each side there are two lobes at the same level (note the rounded corner of the angle formed by the lines between them). This is decidedly incorrect; one of the lobes is higher than the other in front view, so that in vertical view it overlaps the lower one, and consequently the lines between the lobes cross each other, and the corner of the angle must be sharp. I have drawn many Xanthidia of several species in which this feature occurs, and I know exactly what I am writing about. W. & W. must also have known about this, and I am surprised that they drew the feature in the way they did. To make the matter clearer, I have made a revised and enlarged copy of their drawing, indicating the manner in which it should have been made.

Now let us take a look at Skuja's drawings of var. alternans. His front view is no doubt correct as to the general shape, but I don't agree with his showing some of the lobes cut off by a sharp line. Such a line cannot be seen in the actual plant; the lines bounding the lobes gradually disappear

into the complicated curved surface. But this is not very important. I had noticed for myself that his vertical view does not correspond with the front view, even before I saw in your manuscript that he had conceded its incorrectness. I have given an enlarged reconstruction of the vertical view, and I should like to know how it compares with your reconstruction, which I have not seen. Of course, my reconstruction may not be correct as regards the general shape and the angle of divergence of the spines; there is no way of determining these points, so I had to be more or less guided by W. & W.'s illustration and my own drawings. You will note that Skuja's vertical view shows the overlapping lobes and the sharp angle between them that I mentioned above, but there is not, and cannot be, a central lobe as he shows. I suggest that you send my reconstruction to Skuja, if you have time, and see what he thinks of it.

Iyengar & Bai's illustration of X. hexamillatum v. pulneyensis (should be pulneyense) is crude, like many of their other drawings, but it is recognizable and agrees in its general features with the specific form, with just enough differences to warrant a varietal name. One of the essential features of the specific form is that all 6 spines and lobes are in the same vertical or frontal plane, a characteristic of Arthrodesmus (cf. A. octocornis). In var. pulneyensis accordingly to Iyengar & Bai, and also in my plants which I have assigned to this variety, the two lateral spines and lobes on each side are in the vertical plane, but the two apical spines and lobes are slightly rotated counterclockwise.

To bring out more clearly the points that I have discussed, I have made enlarged copies of the previous illustrations of these plants, to about the same size as my drawings, so that you can superimpose them for comparison. Also I have prepared in tabular form a list of the similarities and differences between them, and have underlined in blue and red pencil the characteristics that I consider important in establishing their identities. I believe that the aggregate of these similarities and differences established quite clearly that my var. alternans belongs to A. Burkii, and that my var. pulneyensis belongs to X. hexamillatum. If you are still unconvinced, I suggest that you send my drawings and tabulation to Grönblad, and I will make a small bet, say \$1 to Kr.1, that he will uphold my contention.

Last summer my wife and I stayed for 1½ days at Stresa, on Lago Maggiore, and our train passed through Verbena Pallanza. I know that Grönblad visited his friend Dr. Vittorio Tonolli at the Inst. Idrobiologico. From the two samples that he sent me I get the impression that the larger Italian lakes are not favourable for desmids, but I hope you will have better luck.

Lago Maggiore is one of the most beautiful spots that we saw in our European travels. Near Stresa there are some small islands, called the Borromian Isles, and one of them, Isola Bella, is well worth a visit. The whole island is occupied by a luxurious mansion, some hundreds of years old, with many things worth seeing. But I expect you would be more interested in the large gardens and terraces, beautifully laid out and maintained, and planted with many subtropical trees, shrubs, and flowering plants.

The American asymmetric form of St. furcatum has been named var. scaevum Scott & Gold. (in press 1956); I am enclosing a sketch of it. The larger asymmetrical form from Indonesia of which I sent you a sketch some time ago has not yet been named. I still think it belongs to furcatum, but do not know what Prescott will want to do with it.

Ruzicka wrote me some months ago that he was considering the undertaking of some large work on desmids, but I did not know it was the completion of Pascher's work. I have never seen Pascher, and know nothing about it. If it includes only European desmids Ruzicka is probably capable, but I think he has had little or no experience with those from other continents. He is

an excellent draughtsman, and his pictures of such obscurely marked desmids as C. obtusatum are far better than I could make.

In re-reading your letter I see that there are some points that I have not covered. You remark that my figure of Burkillii a vertice is not divided into two lobes like West's illustration. To some extent you are correct, and there are two reasons why my picture gives this impression; 1) Both West's and Skuja's figures show the spines arising abruptly from the lobes as if they were pins inserted into drilled holes. This is an easy way of drawing them, but quite incorrect; spines just do not grow that way. My figures show the lobes, or hollow inflated bases of the spines, as being gradually and smoothly ~~into the center of the spines~~ attenuated into the solid spines, which is the way they actually grow. I think that West's and Skuja's figures emphasize or perhaps exaggerate the lobe-like appearance, while mine do not. 2) My specimens are not exactly identical with either West's or Skuja's, and there is no reason why they should be, considering that they come from Java, Sumatra, and Borneo, distant 2400 to 3000 km from Rangoon, equal to the distance between Stockholm and Tunis in N. Africa. My plants seem to be of a more slender habit, relatively wider, with longer spines and less prominent lobes. But observe, please, that my plants, both the species and var. alternans are built according to the same structural design as West's and Skuja's respectively, i.e., they have the same number of lobes and spines arranged in exactly the same way, alternating on either side of the median plane.

You mention the "lateral spines" and "apical and subapical spines" in Skuja's var. alternans. If my reconstruction of his vertical view is correct, as I believe it to be, there is no lateral median spine, and the "subapical spines" are the upper ones of the lateral pair. In other words Skuja's plant is exactly equivalent to the specific form except that two of the apical spines are missing. Give this some more thought.

Again, you say that the position of the two apical spines in my var. alternans shows that they cannot be considered as "survivors" of the four apical spines of the species. Why should they be "survivors"? Why could they not be "forerunners"? If West's had discovered the 2-spines form first it would have been the species, and then the later-discovered 4-spines form could very well have been called "var. evolutum".

If the scrobiculations of my var. alternans had been in the center of the face I would have agreed that they were not of much specific value, but in the supraisthmian position they constitute a feature quite unknown in any species of Xanthidium except X. Burkillii. Therefore I consider them of great importance in establishing the identity of my plant as a variety of the species. W. & W. show and describe only 4 pits. In my plants there are about 8, which diminish in size from the center to the sides of the isthmus; the outer ones are very small and difficult to see; perhaps W. & W. were unable to see them.

Now this long dissertation comes to a close. I hope that my argumentative disposition has not caused the letter to become boring.

With my best wishes,
Cordially,

Characters	<i>X. Burkillii</i> W. & W. from Burma	<i>X. Burkillii</i> sec. Scott Java & Borneo	<i>X. Burkillii</i> var. <i>alternans</i> Skuja Burma	<i>X. Burkillii</i> var. <i>alternans</i> sec. Scott Borneo	<i>X. sexnamillatum</i> v. <i>pulneyensis</i> Iyengar & Bai India	<i>X. sexnamillatum</i> v. <i>pulneyensis</i> sec. Scott Sumatra
Length ssp csp	44 73-77	45 67	42-44 68-73	45 81	49-52 75-78	50 86-93
Width ssp csp	44-45 77-81	51-54 76-84	42-43 77-80	48 94	40-45 68-84	48-57 96-99
Isthmus Thickness	16.5 (?) 24	22 27	17-18 22-24	23 27	10-12 30	12-13 27
General shape	<i>X. Burkillii</i> used as reference	<u>Very similar to</u> <u><i>X. Burkillii</i></u>	<u>Similar to</u> <u>species</u>	<u>Similar to</u> <u>Skuja's plant</u>	<u>Differs consider-</u> <u>ably from species</u> <u><i>X. Burkillii</i></u>	<u>Differs consider-</u> <u>ably from species</u> <u><i>X. Burkillii</i></u>
Isthmus	<u>Wide</u>	<u>Wide</u>	<u>Wide</u>	<u>Wide</u>	<u>Narrow</u>	<u>Narrow</u>
Sinus	<u>Very short, linear,</u> <u>ampliated, sharp-</u> <u>ly differentiated</u> <u>from ventral</u> <u>margin.</u>	<u>Very similar to</u> <u>species</u>	<u>Very similar to</u> <u>species</u>	<u>Very similar to</u> <u>species</u>	<u>Widely open, not</u> <u>linear, not ampli-</u> <u>ated, continuous</u> <u>with ventral</u> <u>margin</u>	<u>Very similar to</u> <u>Iyengar & Bai's</u> <u>plant.</u>
No. of spines per semicell	8	8	6	6	6	6
Arrangement of spines	<u>Alternating on</u> <u>both sides of</u> <u>frontal plane</u>	<u>Same as species</u>	<u>Similar to species</u> <u>2 apical spines</u> <u>slightly rotated</u> <u>clockwise</u>	<u>Similar to</u> <u>species, 2 apical</u> <u>spines slightly</u> <u>rotated clockwise</u>	<u>2 lateral spines</u> <u>on each side are</u> <u>in frontal plane.</u> <u>2 apical spines</u> <u>rotated slightly</u> <u>counterclockwise.</u> <u>Large, internal</u>	<u>Same as in</u> <u>Iyengar's plant.</u>
Central thickening	<u>Small, internal</u>	<u>Small, external</u>	<u>None shown</u>	<u>Small, external</u>	<u>Large, internal</u>	<u>Large, internal</u>
Supraisthmian scrobiculae	<u>Present</u>	<u>Not seen</u>	<u>None</u>	<u>Present, very</u> <u>prominent</u>	<u>None</u>	<u>None</u>
Granules on each ventral margin of semicell	<u>None</u>	<u>None</u>	<u>None</u>	<u>None</u>	<u>One shown</u>	<u>2 visible</u> <u>2 not visible</u> <u>Total 4</u>

Note. The query that I have placed against W. & W's dimension for width of isthmus indicates that it does not agree with the width as determined from their illustration, and which is about 19-20 μ .

Characters	<i>X. Burkillii</i> W. & W. from Burma	<i>X. Burkillii</i> sec. Scott Java & Borneo	<i>X. Burkillii</i> var. <i>alternans</i> Skuja Burma	<i>X. Burkillii</i> var. <i>alternans</i> sec. Scott Borneo	<i>X. sexnamillatum</i> v. <i>pulneyensis</i> Iyengar & Bai India	<i>X. sexnamillatum</i> v. <i>pulneyensis</i> sec. Scott Sumatra
Length ssp	44	45	42-44	45	49-52	50
csp	73-77	67	68-73	81	75-78	86-93
Width ssp	44-45	51-54	42-43	48	40-45	48-57
csp	77-81	76-84	77-80	94	68-84	96-99
Isthmus	16.5 (?)	22	17-18	23	10-12	12-13
Thickness	24	27	22-24	27	30	27
General shape	<i>X. Burkillii</i> used as reference	<u>Very similar to</u> <u><i>X. Burkillii</i></u>	<u>Similar to</u> <u>species</u>	<u>Similar to</u> <u>Skuja's plant</u>	<u>Differs consider-</u> <u>ably from species</u> <u><i>X. Burkillii</i></u>	<u>Differs consider-</u> <u>ably from species</u> <u><i>X. Burkillii</i></u>
Isthmus	<u>Wide</u>	<u>Wide</u>	<u>Wide</u>	<u>Wide</u>	<u>Narrow</u>	<u>Narrow</u>
Sinus	<u>Very short, linear,</u> <u>ampliated, sharp-</u> <u>ly differentiated</u> <u>from ventral</u> <u>margin.</u>	<u>Very similar to</u> <u>species</u>	<u>Very similar to</u> <u>species</u>	<u>Very similar to</u> <u>species</u>	<u>Widely open, not</u> <u>linear, not ampli-</u> <u>ated, continuous</u> <u>with ventral</u> <u>margin</u>	<u>Very similar to</u> <u>Iyengar & Bai's</u> <u>plant.</u>
No. of spines per semicell	8	8	6	6	6	6
Arrangement of spines	<u>Alternating on</u> <u>both sides of</u> <u>frontal plane</u>	<u>Same as species</u>	<u>Similar to species</u> <u>2 apical spines</u> <u>slightly rotated</u> <u>clockwise</u>	<u>Similar to</u> <u>species 2 apical</u> <u>spines slightly</u> <u>rotated clockwise</u>	<u>2 lateral spines</u> <u>on each side are</u> <u>in frontal plane.</u> <u>2 apical spines</u> <u>rotated slightly</u> <u>counterclockwise.</u> <u>Large, internal</u>	<u>Same as in</u> <u>Iyengar's plant.</u>
Central thickening	<u>Small, internal</u>	<u>Small, external</u>	None shown	<u>Small, external</u>	<u>Large, internal</u>	<u>Large, internal</u>
Supraisthmian scrobiculae	<u>Present</u>	Not seen	None	<u>Present, very</u> <u>prominent</u>	<u>None</u>	<u>None</u>
Granules on each ventral margin of semicell	<u>None</u>	<u>None</u>	<u>None</u>	<u>None</u>	<u>One shown</u>	<u>2 visible</u> <u>2 not visible</u> <u>Total 4</u>
Note. The query that I have placed against W. & W's dimension for width of isthmus indicates that it does not agree with the width as determined from their illustration, and which is about 19-20 μ .						

Characters	<i>X. Burkillii</i> W. & W. from Burma	<i>X. Burkillii</i> sec. Scott Java & Borneo	<i>X. Burkillii</i> var. <i>alternans</i> Skuja Burma	<i>X. Burkillii</i> var. <i>alternans</i> sec. Scott Borneo	<i>X. sexmamillatum</i> v. <i>pulneyensis</i> Iyengar & Bai India	<i>X. sexmamillatum</i> v. <i>pulneyensis</i> sec. Scott Sumatra
Length ssp	44	45	42-44	45	49-52	50
csp	73-77	67	63-73	81	75-78	86-93
Width ssp	44-45	51-54	42-43	48	40-45	48-57
csp	77-81	76-84	77-80	94	68-84	96-99
Isthmus	16.5 (?)	22	17-18	23	10-12	12-13
Thickness	24	27	22-24	27	30	27
General shape	<i>X. Burkillii</i> used as reference	Very similar to <u><i>X. Burkillii</i></u>	Similar to <u>species</u>	Similar to <u>Skuja's plant</u>	Differs consider- ably from <u>species</u> <u><i>X. Burkillii</i></u>	Differs consider- ably from <u>species</u> <u><i>X. Burkillii</i></u>
Isthmus	<u>Wide</u>	<u>Wide</u>	<u>Wide</u>	<u>Wide</u>	<u>Narrow</u>	<u>Narrow</u>
Sinus	Very short, linear, <u>ampliated, sharp-</u> <u>ly differentiated</u> <u>from ventral</u> <u>margin.</u>	Very similar to <u>species</u>	Very similar to <u>species</u>	Very similar to <u>species</u>	Widely open, not <u>linear, not ampli-</u> <u>ated, continuous</u> <u>with ventral</u> <u>margin</u>	Very similar to <u>Iyengar & Bai's</u> <u>plant.</u>
No. of spines per semicell	8	8	6	6	6	6
Arrangement of spines	<u>Alternating on</u> <u>both sides of</u> <u>frontal plane</u>	<u>Same as species</u>	<u>Similar to species</u> <u>2 apical spines</u> <u>slightly rotated</u> <u>clockwise</u>	<u>Similar to</u> <u>species 2 apical</u> <u>spines slightly</u> <u>rotated clockwise</u>	<u>2 lateral spines</u> <u>on each side are</u> <u>in frontal plane.</u> <u>2 apical spines</u> <u>rotated slightly</u> <u>counterclockwise.</u>	<u>Same as in</u> <u>Iyengar's plant.</u>
Central thickening	<u>Small, internal</u>	<u>Small, external</u>	None shown	<u>Small, external</u>	<u>Large, internal</u>	<u>Large, internal</u>
Supraisthmian scrobiculae	<u>Present</u>	Not seen	None	<u>Present, very</u> <u>prominent</u>	<u>None</u>	<u>None</u>
Granules on each ventral margin of semicell	<u>None</u>	<u>None</u>	<u>None</u>	<u>None</u>	<u>One shown</u>	<u>2 visible</u> <u>2 not visible</u> <u>Total 4</u>
<u>Note.</u> The query that I have placed against W. & W's dimension for width of isthmus indicates that it does not agree with the width as determined from their illustration, and which is about 19-20 μ .						

Characters	<i>X. Burkillii</i> W + W from Burma	<i>X. Burkillii</i> sec. Scott Java & Borneo	<i>X. Burkillii</i> <i>v. alternans</i> Sulu Burma	<i>X. Burkillii</i> <i>v. alternans</i> sec. Scott Borneo	<i>X. sexmammillatum</i> <i>v. pulneyensis</i> Iyengar & Bai India	<i>X. sexmammillatum</i> <i>v. pulneyensis</i> sec. Scott Sumatra
Length SSP	44	45	42-44	45	49-52	50
OSP	73-77	67	69-73	81	75-78	86-93
Width SSP	44-45	51-54	42-43	48	40-45	48-57
OSP	77-81	76-84	77-80	94	68-84	96-99
Isthmus	16.5(3)	22	17-18	23	10-12	12-13
Thickness	24	27	22-24	27	30	27
Ratio W/T	1.9	1.9-2.0	1.8-2.0	1.8	1.3-1.5	1.8-2.1
General shape	<i>X. Burkillii</i> used as reference	Very similar to <i>X. Burkillii</i>	Similar to <i>X. Burkillii</i>	Similar to <i>X. Burkillii</i>	Differs greatly from <i>Burkillii</i>	Differs greatly from <i>Burkillii</i>
Isthmus	Wide	Wide	Wide	Wide	Very narrow	Very narrow
Sinus	Very short, linear, amplified, sharply differentiated from ventral margin.	Same as <i>X. Burkillii</i>	Same as <i>X. Burkillii</i>	Same as <i>X. Burkillii</i>	Widely open, not linear, not amplia- ted, continuous with ventral margin	Same as Iyengar & Bai's plant.
Number of spines per semicell	2	2	2	2	2	2
Arrangement of spines	Alternating on both sides of frontal plane	Same as <i>X.</i> <i>Burkillii</i>	Similar to <i>X. Burkillii</i>	Similar to <i>X. Burkillii</i>	2 lateral spines on each side in frontal plane. 2 apical spines slightly rotated.	Same as Iyengar & Bai's plant
Central thickening	Small, internal.	Small, external	None shown	Small, external	Large, internal	Large, internal
Supraisthmal scrabicolae	Present	Not seen	None	Present, very prominent.	None	None.
Granules on each ventral margin of semicell	None	None	None	None	1 shown	2 visible, - 1 2 not visible Total 4

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Linköping 8/ 8/ 1956.

Dear friend Scott!

I am just home from Finland, where I attended the limnological congress and delivered a little lecture, one of the few on algae; the most common themes were Typology of the lakes and Water pollution.

Only a week before my depart my sister died, suddenly, and I was mentally and physically depressed. The manner of living at the congress also deviated very much from my home living and I suspected that the excursion week with lodging in railway wagons and intensive daily work together with >200 members speaking English and German would be too much for me, thus I gave up the attendance of the excursions to which I had looked forwards with so much pleasure. I met Grönblad in Helsingfors and also visited Karis one day. We had good talks and discussions. I am glad to tell you that he, at least then, seemed to be convinced of the correctness of my taxonomical ideas. He considered that Actinotaenium is all right, he only does not agree to some of my species arrangements, but he admitted that he had a definite antipathy for using ^{other} ~~new~~ names than the traditional ones. He also accepted the principles for my distinguishing of Groenbladia. His reluctance to use Stauroidesmus, the validity of which he agreed to, depends on the circumstance that I had not given the limits of it. He considered that several of the old genera do not correspond to the present content of desmidiology and that they need revision. This was more than I had expected from his letters and I do hope that he will continue on this path.

In this connection I must correct your suspicion (your letter of April 5, page 1, piece 3) that my criticism of him would depend on pique. I am quite convinced of the validity of the taxonomical principles, published in my latest papers, and I can assure you that I had blamed his attitude precisely in the same manner, if these principles had been presented by an other scientist! My estimation of him as a very prominent desmidiologist concerning species, is very high, but I can not, of course, omit to blame his resistance in regard to questions in which he is devoid of the necessary qualifications. It is impossible to procure a solid opinion of genera and higher taxonomy without extensive and intensive studies and knowledge of the results of the evolutionary research both in botany and, especially, in zoology. In this regard Borge's attitude is an exemplary pattern fashion.

A scientific amateur who does not publish anything, may use his own personal nomenclature, but a scientist still active in publishing new results, is obliged to ~~fix~~ use the nomenclature in accordance with the scientific results accepted by him, he must not act differently because of mental laziness. Everyone who is dealing science and publishes his results, is a member of the

international science; he must follow the rules, he is compelled to accept the scientist's conscience: to tell truth according to his possibilities of finding it, and, he must not suppress the results of his own or others' even if these would be inconvenient to him. If he acts in a different way, he must explain his deviating point of view.

Your emphasizing that morphology is the (hitherto) only criterion is true, but morphology implies not only the mere outlines (the chloroplast is an important feature of the morphology) and the so very varying elements of the morphology must be seen, apprehended and judged in the light of evolution, ontogenetical and phylogenetical. Otherwise a combination of coherent morphological elements will result in the "system of screws" previously mentioned. The coordination of morphological facts presupposes a penetrating knowledge of similar problems from other groups of organisms. The dogma of generic difference between the bi- and the pluriradiate is founded on morphology! But our experience shows that evolution has passed from pluri- to biradiation. You do not doubt that most of the monospinous desmids are near related because of the morphological agreement, but now, I hope, you feel obliged to restrict the very striking and previously absolutely dominating morphological character bi- and pluriradiate to a very small important ^{one}.

Your many examples on page two of your letter only prove that the present concept of genera is an antiquity from 100 years ago, founded on the very small material then known, Ralfs lists 37 *Cosmaria* and 56 *Sta-urastra* for instance. Our immense richness in forms does not fit in these extremely diffuse genera, parts of a "screw system". Of course there will always occur intricate intermediary forms that must be allotted to the one or the other of two adjacent genera conventionally, by expedient reasons. Nature has no limits, these are determined by Man in order to assign the organisms in groups of lower and higher rank for the sake of the international community and in order to find out the paths of the organic evolution. Remember also, that the difficulties of ^{my feeling} ~~finding boundaries~~ is quite as ~~xxxx~~ ~~xxxx~~ applicable to the species!

As to the morphology, the frontal thickening that you refer to, is an important diagnostic feature. In its most elaborate state it develops granules, verrucae and even spines. In my delimiting of *Std.* I must separate the incrassate species, *Std.* embraces only the smooth forms without thickening. The other belong to the *Cosmarium-Xanthidium* group. And, rub out of your brain the concept *Arthrodesmus*, please, you will procure more clear ^{cut-ways} ~~points of view~~ afterwards.

Your humoristic deliverance on Spinoc^{losterium}cosmarium is not relevant: "When a Closterium decides to grow a spine on each end" means that the spines are occasional, occurring or lacking in the same strain, eventually with dichotypical specimens. But if they are constant, and probably they are, this morphological feature may be reason enough to create a separate genus. You have arguments for creating a new genus, Amscottia, because of the vertical asymmetry, otherwise it has its natural position in the vicinity of the St. arcticon group, that has as yet not been distinguished as a separate genus. Your new genus, soon published I hope, agrees very much with Ichtyocercus and differs principally (as far as I can remember) by the vertical excavation. It seems as if you in certain cases are bound to tradition, in certain cases ^{quite} not. In cases of smaller deviation the limitation of a genus is a matter of taste.

I do not blame for your giving up because of the generic difficulties that you mention on p.2, but if you give up, you must not blame other desmidiologists if they do not give up. You do not see "much advantage in splitting off the small group ^{desmidiaceae} (32 species and 59 varieties and formae!) from Cosmarium" when I did not at the same time rearrange the whole heap of rubbish, called Cosmarium. Either you mean it humoristic, ironically or candidly, you seem to have a too high apprehension of my ability, even in USA it is impossible to build the Temple of Science in a hurry, one must place stone on stone and since most of the stones are quite different, the builder must puzzle out how they are to be joined; the architect Nature gives you no designs, only small hints often extremely difficult to interpret. New stones found may show that a part already built ^{is} wrong, then this part must be pulled down and its stones together with the new ones give new details of the Temple building.

I suppose that you find me somewhat conceited and anxious to get my desmid principles realized in a revision of the desmid taxonomy. The latter circumstance is very natural, nothing has been ^{an} done in this regard since Lütkenüllers classical work half a century ago and ^{an} immense heap of stones have been brought to light by the eager efforts of buzy desmidiologists (not at least be Grönblad and Scott), thus it is high time that something must be done. The application of Staurod. and Actinotaenium are successful and have been accepted by scientists of soundness in judgement; it is natural that I am anxious to see further application will be realized in a more extensive revision, a circumstance that must be favourable for the study and development of the desmidiology, a desire certainly approved by you.

You wonder why you have not found any Janus forms and supposes that this phenomenon is confined to plankton. Theoretically not, I think, but you must remember that a lake, even a small one, is an immense culture vessel, contain-

ningspecimens in number of an astronomical degree. When examining many thousands of specimens in a sample it is possible to find Janus forms even if their occurrence is only 0,01 promille. The factors favouring their coming into existence are unknown but sometimes they occur more frequent than in other cases. Certain desmids seem to be more disposed for Janus forming than other. I am sure that Janus and dichotypical forms have been seen not uncommon but they have been neglected as monstrosities and of no interest for the observer because they do not fit in his concept of true specimens. On the contrary, they are valuable hints of the Architect's intentions and show details of the phylogeny of the genera and also species. There are thousands of examples of important "monstrosities", i.e. deviations from the "normal", from animals and plants, when they are scientifically interpreted. Also this is morphology!

In your material of terraqueous desmids you seldom find strains great enough to contain the scarce Janus forms and these are not constant, a desmid 2+3 usually gives after the propagation 2+3 and $\frac{2+2}{2+2}$ but also very seldom you will find 2+3 and 3+4. But you have found both dichotypical and Janus forms sometimes, pictures of some of them you have kindly sent to me.

In this connection I enclose some copies of some variants of *X. antilopaeum*, which is variable as a plankton. Also a provisory sketch showing the probable phylogeny of some desmid genera.

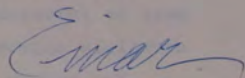
I have looked a little at some of the samples of this summer's, interesting from a planktological point of view but without any sensational finds of desmids except *Micr. tropica* (I have never found it before and I have not seen it ~~maxima~~ recorded in the plankton literature) and *St. sexangulare destitute* of the secondary processes.

At the congress I met Starr (Bloomington, Ind.) and Brooks (Scotland), who were invited to visit Grönblad but failed to appear. I was not sorry, G. and I had not been able to discuss our personal problems if they had been present.

Now, I fear that you are tired of my chat, at least am I. Perhaps you will receive a continuation later on. This summer I have spent only two days at our summer-cottage and I have not seen my wife since 22/VI, but next week we will be united there - for a week only.

My best greetings to your wife and yourself,

your sincere friend



Linköping 13/ VI/ 1956.

Dear friend Scott!

Many thanks for the careful and valuable work that you have spent on my asymmetry paper. I flatter myself by believing that your motives are not only your great interest of desmidiology but also your amability that you always have shown to me. But I must blame your extravagance, you should have sent it as recommended commercial paper, as a wrapper.

The paper will be improved by being stored to September, I have no time for making it ready for print now. My whole summer is engaged by my teacher ship at Aneboda, the congress and plankton collecting. I have had a tiresome time. My wife is suffering from an otitis since April, the first week she was unable to move her head and even to open her eyes because of the terrible vertigo and she must be nursed as a baby; our servant-maid is working at an other family half the day. When Tora became better I had a little vacancy in Lund and attended the promotion. I have worked hard with the investigation of the lakes at Stockholm and have just sent the report to the District Committee, by which a heavy stone fell from my chest, inasmuch some money will arrive. Therefore I am able to attend the congress, where I hope to meet Grönblad. I should have liked to visit Åbo and Kallio also but I fear there is no time for it.

All my correspondence has been postponed, this is the first letter since long and I beg you to excuse my ostensible impoliteness! Now I have only a poor little week for writing a lecture for the congress with pictures, and also a lot of urgent things that must be done before leaving my home for a long time.

In Lund, when lazily resting and listening the radio I reread your extensive letter and made many comments and explanations, but I fear most of them will not be sent. I think it is no use sending discussions but some elucidating facts will arrive later. Besides, I got a reminder of the rapid passing of the years, professor Du Rietz and Thunmark exacted my promise to put the desmid taxonomy aside until I have finished my principal work, viz. the Swedish fytoplankton. My material, consisting of lists and pictures from about 800 lakes must be dealt with and actually by me. Especially the ecological results, being the basis of the trophical spectrum and consequently the key of judging the trophical evolution of our lakes from oligotrophic in the direction of eutrophy is, according to these scientists, now more urgent than previously by the rapid contamination of the lakes. I got their promise that the paper would be printed immediately after the receipt, but there is much work until then.

My action of desmid morphology and generic taxonomy imply by-products of my distinguishing work on our planktonic desmids, but I could not avoid to treat them, not at least as an objection against the horribly petrified generic routine and as an attempt to bring up new and advantageous principles in desmidiology. This has taken more time than I had intended, but I have enjoyed it and I am quite convinced that the coming generation of desmidiologists will use them to the benefit of their science. Perhaps I may live long enough to return to this fascinating and grateful task. My monography of Staurodesmus is to its most part ready and I had intended to make it clear as my next job, but I have given my word.

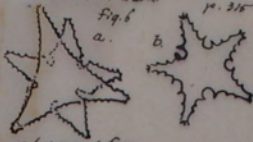
Up to now I have not had time to digest your valuable informations, comments, hints and pictures of the asymmetry paper, they will be an interesting lecture in Aneboda. Your proposal to delay the publishing and increase the material of facts by the many examples in your (and Grönblad's) paper is very tempting, but I think it will do in the present extent. Time is precious and I have extracted the principle and also, to a certain extent, the consequences, the former accepted by Kallio.

I do hope that you and your wife will have a good summer with health and recreation (without tiresome European sight-seeing and journeys in the "Polar regions" Sweden and Finland) and rich collections of interesting and new desmids. Please bring the kindest regards from ~~my wife~~ also to your wife from my wife

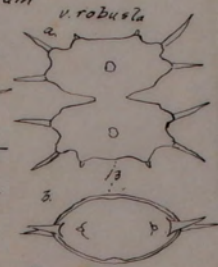
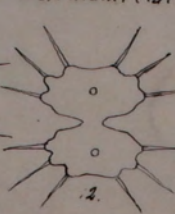
and your sincere friend

Eric

51. *meisena* Wall. p. 315
 Fig. 6
 a. b.
 abnormal form
 (Wastl)?: *Saxosina* Desmids
 of the United States.
 Journ. Linn. Soc. 33. 1895.

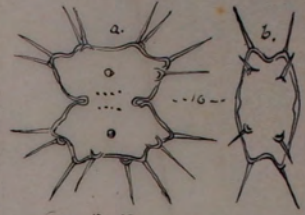


Lanxidium sexmammillatum



(Wastl)?:
 Fresh-water Algae from Burma.
 Annals Roy. Bot. Garden Calcutta
 p. 211. pl. XV

L. Burkillii



(Wastl)?: Fr. w.
 Algae Burma
 p. 210, pl. XV

Linköping 17/ II/ 1956.

Dear friend Scatt!

Many thanks for your letter, very welcome as ever. I am ^{Swedish Bot. Tidsskr} glad that your Wildemanni was printed so quickly, I had my thought on SBT when we were together but the Editor was absent very long.

As to my, perhaps very obtrusive, comments of your gratitude to Prescott, I only related the manner used by Nordstedt, Borge (and myself). Until Borge's death in 1932, Grönblad had practically most of his desmids controlled by Borge, the always altruistic friend, he lived at him now and then and there was an intensive correspondence. I know Borge very well, we lived for instance together one summer (it is ^a pity that you cannot read Swedish, I have ^{tried to} described his noble personality in a necrologue and already during his life, at a jubileum, I wrote a little paper on him as an algologist). ~~and he~~ would never have permitted Grönblad to attach his name on any paper, and the same attitude had Nordstedt taken vis à vis Borge.

As to your hesitation about Staurodesmus and Actinotaenium, I quite understand it, but you must permit me to examine your premisses. Grönblad is an old good friend of mine, already in 1919 I sent him literature, for instance Lundell's classical work, when I heard that he had begun to work with desmids. I have met him in Stockholm and here in Linköping and we have at times had an intensive correspondence in Swedish without restrictions not only in phycology. I think that I can say that I know him, his strong points as well as his foibles. We have discussed generic taxonomy thoroughly and he not avoided to demonstrate his averseness to new genera (except Amscottia and Groenbladia) and sometimes in words more impulsive than polite. He belongs to that category of scientists who are very good describers, they possess a sharp eye and are anxious to find the right box to place the specimen into, if the can not find the box, it must be a new taxon. Their arrangement of the boxes is unchangeable and every attempt to arrange them in a new manner is harassing, perhaps laughable, because they are quite unable of conceiving the frame-work of principles that is forming the taxonomical system. The system ^{while} that they are accustomed with from their earlier apprenticeship is enough for their boxes. They are quite logical in their treatment of their limited conceptions, but quite destitute of scientific imagination or intuition. When ^{when materials are} this is combined with ability of ~~making~~ **making** scientific collection of facts, you have the second category. These two kinds of scientific talents is discerned since long, already by Bacon 400 years ago. I have studied the scientists I have met and the difference ^{Grönblad is one of the best, his sharp eye is open also for other details, for instance the chloroplast, he sent me very valuable pictures for my paper}

For instance, he was the first who examined dried herbar specimens of mosses and water-plants and got in this way several new algal species and his method of swelling up dried algae and micro-fragments by means of milk acid is of great importance.

is easy to find out after having made their acquaintance and reading papers. For instance Lagerheim, a marvellous detail-observer in mycology and phycology with very good ideas but in spite of his enormous knowledge of species and genera he could not "grasp" higher ranges of taxonomy, some of his pupils ^{to instance the other side} did. A very fine representant of the second category was De Geer, my teacher in geology, a penetrating and critical investigator and a man with scientific imagination and intuition but always with his feet on the soil. Since long the geologists of Northern Europe had studied the "varved" clay, horizontal layers, brown, grey and black. De Geer came to the theory that the layers were annual sediments from the rivers of the melting Great Ice and the ^{ving} ^{was} ^{ing} ~~variation~~ thickness dependent on variation of the solar climate. This had proved right and his research has given astonishing results as to the chronology ^{and stratigraphy} of the quaternary age. ~~In many places~~ it is possible to count almost every year of the recession of the ice along Sweden and to give the situation of the ice-border at diverse epochs. Investigations are made in USA, the Alps, Central Africa, Himalaya and Australia, covered by ice during the Ice Age, (by his pupils). This is only one of De Geer's great discoveries, all of them made by a genial detail-scientist with creating imagination and intuition! Lack of this ability is to be regretted, but it is constitutional quite as colour-blindness. And further, like the colour-blind persons, the scientists of the first category are quite unable to comprehend the work of the other and according to the human conceitedness they deny it or make fun of it.

When scrutinizing this letter (I am a very bad type-writer), I found an example more convincing for you. Why has no desmidiologist observed the relationship ^{on the basis of} ~~or at least~~ the similiarity between the monosporous Staurastrum and Arthrodesmus. During a whole century, thousands of desmidiologists or students, many of them with a life-long experience, have seen the same as I did. Well, it took me many years to rub away the believe of the "authorities". When I showed it for Professor Du Rietz, he cried: "Oh yes, of course, ^{it is} I have seen it ~~but~~ ^{and} I have always been astonished of this remarkable similiarity! Grönblad was frightened when I set out this idea here in Linköping in 1951. Later he has admitted, that there perhaps, eventually, may be some grain of truth in it, but...

But now back to Grönblad. He suffers or suffered from a special kind of positivism. An example! When he was here, I asked him about a Staurastrum on which he had correctly depicted the apical verrucae in front view, but on the same specimen in end view the verrucae were omitted. Why? - I could not see them. - But you know that they were there, when you turned the specimen from front view into end view they became invisible on the background of the chloroplast! - Yes, but it is impossible to me to depict a thing that I can not see! This is no funny story, he was very earnest and quite convinced of the correctness. My objections left him impassive.

Obstinacy is a prominent element of the Finnish national character, it has become a phrase in Swedish language (Finland has been a brother country of Sweden during 800 years) All these things ^(not the whole thing!) have been discussed between us and I am quite aware of his attitude against me, I estimate his many good qualities and my friendship is not influenced by my knowledge of his ^{intellectual} ~~material~~ defects, even some of those he is unconscious of himself.

I am afraid that Prescott belongs to the same category as Grönblad, extremely energetic but ~~with~~ fairly moderate in his manner of thinking (evident in his survey of the Desmids in Botanical Review e.g.). He has a too strong confidence to his own opinion ^{a quality} to give a chance to others; he is dogmatical, always connected with intellectual narrowness. This is my impression, perhaps you can correct me.

Well, I don't like, for your own sake, that your opinion is determined or at least strongly influenced ^{mainly} by these two desmidiologists. It is a natural consequence, attached to them as you are by the friendship and gratitude of your own amiable personality. This is no flattery, I felt it every minute while we were together! I am sure that you, like me, are able to appreciate a person very much without being blind to his eventual deficiencies and I hope that your eventual estimation of me is based on the same ^{the} manner of measuring.

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As to yourself it is natural that you in regard to my activity as a "re-arranger" of desmid genera are dependent of authorities that you trust to. The foundations of my work are ^{only} principally my view-points of the radiation and the interpreting of the organelles, the latter according to the methods ~~since many years~~ since many decades proved successful in biology. On the whole, no morphological research has been performed in desmidiology except by Lütkenmüller, that is why it is so easy to be an investigator, even with my capacity. The great "guns" Brébisson, Nordstedt, the West e.g. have described new taxa, some of them, West e.g. often very superficially. Meneghini and Ralfs were excellent classifiers according to their view of the plants, remember, that Darwin's Origin of Species appeared in 1859, eleven years after Ralfs' standard work, with which ^{Ralfs} he ~~he~~ regrettably left the study of desmids. ^{revolutionizing} The new view of the living Nature caused by Darwin's evolutionism has now penetrated the biological manner of thinking, except those who prefer to follow West, G.S.

A complicate scientific apparatus is not needed in order to work scientifically, at first you must abstract the dubious and often erroneous

Scientists are not at all any rapacious, only in the beginning of the 19th century a German professor of medicine forced the circulation of the blood, 200 years after Harvey!

concepts of the old authorities, a task extremely difficult, but you must feel dubious against everything, and you must ^{use} Logic. A simple example! If you inspect a set of used screws, heridited from your beloved father, and you find a copper screw among the iron screws. Do you then think: "My father has put it among the iron screws, consequently it must be an iron screw." And if you find a worn nickel-plated copper screw instead, you will say the same, ^{or perhaps!} "It is so very similar to an iron screw, well, it may pass for an iron screw!", instead of putting it atleast in the box of the copper screws, or better in a new box for nickel-plated copper screws, of course provided that you will keep a strict system. The veneration for the authorities must not shackle the common sense and logic, even authorities can mistake! If Ralfs could be resurrected and learn the present biology, unconscious of his work in the previous existence ~~50~~ 100 years ago, I am sure that he would be the first to revise the Ralfs of 1848. I always read with emotion his book, written by a noble, in the best sense scientific personality who above all searched for ^{truth} ~~truth~~.

I must tell you Borge's opinion. Several times during our discussions he emphasized: I am not able to discuss genera and such higher taxonomy, I am describing what I find ⁱⁿ ~~under~~ the microscope and I use the generic names actual at present. He was humble in his view of his science and aware of his narrowness. If all desmidiologists were like him, this science would make better progress.

After this protracted elucidation of desmidiological work, I ask you: What is the purpose of desmidiological research? Of course you can say: The sport of finding marvellous desmids, it is enough for me. ^{That may be, you are doing a very good work, but if every desmidiologist did so, there would be no} ~~for~~ Science. For me it is to enjoy the varying artistical shapes, describe their morphology and physiology and, on the basis of the facts obtained, find out the hidden secrets of their evolution. In order to get uniformity and comparability it is necessary to use a non-ambiguous terminology in the morphology and nomenclature. ^{with} The generic nomenclature must coincide and reflect the system, built up by the results of the research devoted to interpret the present morphology, exterior as well as interior, and reconstruct the paths and mechanism of evolution.

The attitude of the leading desmidiologists, except Bourrelly, is a brake on the progress. Linnaeus, without microscope, called all freshwater algae Byssus (flos-aquae etc). During the 19:th century there was an eager creation of desmid genera, see Nordstedt's Index I, p.288, in many cases on the basis of starting-points now rejected, but this activity implies an admirable trend of creating a biological system. This was the dynamic period, now a petrified period has entered and, curiously, just during ^{the} ~~the~~ ^{epic} ~~period~~ in which all biological partial sciences have developed enormously, not only in the number of taxa but in the building of a system. The taxon-making is not the purpose (by all means for every person who is capable of it, but they must follow ~~the~~ Borge's example in their work), it is the necessary materi-

collecting of

al, quite as a brick-manufacturer makes the bricks, the architect finds out how to arrange them to a house. The progress of research makes the system to a dynamic conception but the vast majority of desmidiologists intend to make the whole to a "buttonology" or philately. Taxonomists of other biological sciences ~~will~~ ^{which is} laugh at the present state of the desmid system, guarded by a troupe of "scientists" ready to battle to the last gasp for it. The collective names Cosmariium and Staurastrum are quite homologous to Linnaei "Byssus", but these names are sacrosanct, it is almost a crime of lese-majesty to propose a partition, even on principles based on biological premisses.

Dear friend, this letter has grown out to a formidable paper, containing "mixed pickles": criticism of desmidiologists, dead and alive (the former are quite as dangerous as the latter), discussions about taxonomical principles (philosophical desk-work, according to Grönblad), anecdotes and selfconfessions (in confidence communicated ^{only} to my most intimate friends). I think you have fallen asleep several times, but politely, yawning, read it to the end. I have written, not in order to to relieve my sentiments but to give you an injection of serum against the influence of G. and P., because I consider you worthy of it. But, they are two ^{all} great authorities and I, what am I? I have no idea of having been able to convince you, but I would be glad if I have perturbed your dogmatic belief of authorities even in the slightest degree. Remember! Doubt ~~is~~ of dogmas and authorities is the first step on the steep and stony path to Truth! Perhaps, later, you once will give in the directives of your "desmidiological uncles" and go your own way.

I am sure that you will do full justice to me; if you feel displaced at some expressions, you must take into consideration my unfamiliarity with the more subtile gradations of a foreign language. And I repeat, I would never open my heart for an insignificant person, not even for a very good friend.

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Now something more easily digested as a dessert. Your Xanthidium Burkillii var. alternans Skuja seems to be X. sexmamillatum, ^{and actually} a forma with semi-radially displaced spines. About the concept "semiradial" you will read in a manus later sent to you (for inspection) by Hannah. I enclose a copy of this species, if you do not possess Wests' India book. Your drawing was signed Sachlan "X", lake, East Borneo.

As to your extremely interesting dichotypical *Staurostrum gyrans*, it is probably the one depicted by West, Some desmids of USA, under the name of *St. incisum* Wolle, a copy is included.

At the end of this month I probably shall be able to send my mscr of *Asymmetry* in the desmids to Hannah for scrutinizing and she will send it to you for inspection.

I got the first part of Kossinskaja's *Flora crypt., Conjugatae* from her, I did not know that vol.2 had appeared, I will ask for it in Stockholm.

Just now Grönblad's paper on Hannah's desmids from Woods Hole arrived. Very interesting but why is such a correct describer able to depict a *Staurodesmus* with hollow long spines? It is of importance since just the short-spined primitive forms possess hollow spine-like processes and it is valuable to define the difference between a spine-like process and a true spine.

Will you, please, leave a free margin on the back-side of your letters, about one inch. When I re-reading your letters I am obliged to take out the letter from the letter-gover.

At last my very best wishes to your wife and to yourself
from my wife and your friend

Digitized by Hunt Institute for Botanical Documentation

May 29 1956

Dear friend Teiling,

Sorrily I have had to keep your manuscript a little longer than I had anticipated, but it is only a few days since I finished drawing 37 plates of illustrations, and typing the Latin and English diagnoses and comments on something like 160 new desmid taxa from southeastern USA. The paper in an incomplete form was presented by Prof. Luther and Dr. Gedercreutz at a meeting of the Soc. Sci. Fennica a couple of weeks ago, and will be accepted for printing as soon as completed.

The manuscript with some enclosures that Hannah sent me is going forward under separate cover, and enclosed are 3 sheets of notes and comments that I have prepared. On the Ms. Hannah's notes are in green ink, mine are in pencil. I have made a number of changes or suggested changes in words, which would help to make the English read more smoothly and would eliminate some of the small peculiarities of expression that have been apparent in your former papers, even though they had been corrected by your English friends. Also I have caught several mistakes in spelling of names and in references, which I know you will want to correct.

I hope that my comments on the various species will be helpful, and you are quite at liberty to use them or not, as you see fit. If you could wait, and if I had the time to dig through my drawings from USA, Panama, Brazil, Peru, Australia, and Indonesia, I could almost double your list of desmids showing semiradial asymmetry for this phenomenon is much more common than has been thought.

I am sending a copy of the 3 sheets of my notes to Hannah, for I know she will be very much interested. She is, indeed, most kind and helpful; she is doing so much work for others that I hardly see how she gets time for her own studies.

Have just received from Prescott a new paper: "A guide to the literature on ecology and life histories of the Algae". Bot. Rev. XXII:3, 167-240, containing 1001 titles of papers in half-a-dozen languages, and covering both freshwater and marine algae.

Do you receive the Bull. Phycological Soc. of America? The last two issues have a very long list of literature on algae published in 1954, covering all phases, taxonomy, genetics, ecology, etc. etc. If you do not get it I would gladly send the copies for your inspection, but you would have to return them to me. Perhaps you can see it at Lund.

This is all for the present. I shall be glad to see your comments after you have received the Ms., which is going by airmail.

Best regards to you and your wife, from

Your good friend

Page 1. The word 'the' can be omitted twice in the title, and in many other places throughout the text, as I have indicated. This is contrary to usage in most of the European languages, but not in dear old English.

The word 'origo' should be 'origin', the mathematical term for the point of intersection of two or three coordinate axes. I suggest the following rearrangement of the sentence in which it first occurs: "The framework consists of the vertical axis, the isthmal plane perpendicular to the vertical axis and penetrated by it in a point called the origin, a number of radial axes extending outward from this origin in the isthmal plane, and the radial planes which are determined by the vertical axis and the radial axes".

'Isthmal' must be spelled either 'isthmial' or 'isthmian'. I prefer the former, though I cannot cite any authority. Prescott prefers 'isthmian', and refers to the Isthmian Canal Commission (Panama Canal), and Isthmian Steamship Company, both well-known in USA.

The word 'tribus' looks strange to me, though I know it is correct Latin. Many writers use the English word 'tribes', and I think you would use the English word 'families' if you were writing about them.

Page 2. Your expression "composed plane" should evidently be either 'composite' as suggested by Hannah, or 'compound'. I think 'composite' is better.

The revisions in your letter of Apl. 12 of the sentences beginning on lines 16 and 20 makes them more easily comprehensible and I have no changes to suggest.

Page 4. I find your notation^s for positions confusing, because you have employed the same (or very similar) notations for the numbers of radii in Janus-forms, i.e. 1 + 3, 2 + 4. I offer the suggestion to use the letters a, b, c and d for these positions, which would avoid the difficulty.

The word 'chaplet' in line 5 is unusual in this connection. How about 'ring'.

'Organel' is usually spelled 'organelle', I believe, and the latter spelling is used in the Encyclopaedia Britannica and A Dictionary of Biological Terms (Penguin Books, London, 1953).

On the subject of the small apical processes in *Miér. mahabuleshwarensis*, I think I may say that I have probably seen more different varieties of this species than anyone else, including two new unpublished varieties from N. Australia and Indonesia. I have never seen these small processes in any other position than 2&4. Though not excluding the possibility that they may also occur in the position 1&3, I am inclined to look with suspicion on drawings that show them this way, especially drawings by less experienced and less careful observers. Note that in Krieger's monograph at least one, and probably more, of his illustrations of *M. mahab---* has been reversed in the copying process, which reversed the position of the subapical processes.

Page 5. I think *M. ⁴alata* should be removed from this list. It has no subapical processes not spines, and belongs to a quite different group. Furthermore, it is one of the most consistently and regularly symmetrical desmids that I know of. Including two new and unpublished varieties, I don't think I have ever seen an imperfect specimen.

Irénée-Marie's figure of *M. muricata* var. *laevigata* shows the subapical processes in the position 1&3 on one semicell, 2&4 on the other. A mistake, I should think.

Page 6. In the comparatively few specimens that I have seen of *M. anomala*, the four subapical processes are always present, also in a new unpublished variety. You say that in this respect the species name is not 'valid' (inappropriate). But the species name was given because of the very strange appearance of the whole plant, not because of the subapical processes.

Page 6 (contd). Grönblad and I also think that *M. Moebii* ought to be transferred back to *Euastrum*, and he has suggested that I write a paper on this plant, of which I have several new forms. Perhaps I may undertake this at some future date. Have you noticed at that in Krieger's monograph, p. 624, he lists ~~MIXED~~ *M. Moebii* v. *javanica* Gutw. (1902, S. 603, Taf. 40, Fig. 58) as a synonym of *E. turgidum*, but on page 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 verrucae on the central tumour, which would be very unusual in a *Micrasterias*, but quite common in *Euastrum*. However, in all the forms of *M. Moebii* that I have seen the tumour has deep pits, not verrucae, and in well-developed specimens the pits are triangular and arranged in a hexagonal pattern.

M. muricata. I must request you to eliminate the reference to Prescott & Scott 1942, Pl. 1, Fig. 4. The drawings for this paper were made by Prescott, most of them copied from my sketches, but I believe this particular figure to be incorrect in showing only one small process on each of the apical lobes and in reversed positions. From the sketch of *M. muricata* that I sent you recently you will have seen that that it would not be quite correct to speak of 'subapical' lobes. The four lobules of the apical lobe are all of equal size and length, and nearly in the same plane, so that one of them is frequently hidden. In order to show the hidden lobule I have sometimes 'faked' the sketch a little. The apparent shortening of two of the four lobules in front view is caused by a twisting of the entire apical lobe around the vertical axis.

Page 7. *M. muricata* and *M. Nordstedtiana* "are known only from the same district in the eastern part of North America". Not exactly correct. Smith reported both from Wisconsin which is a middle western State, and I have both from southeastern USA.

Bottom of page. *Euastrum sphyroides* has a polar lobe which is exactly circular in top view, and thus does not belong with the other species that you mention, at least for the purpose of your discussion of the polar lobe. Cf. Krieger 1930, Figs. 10-12, and Prescott & Scott, 1952, Fig. 3:3.

Page 8. The notation in your tables I and II is confusing to me, despite your explanation that the symbol 1-4 means the occurrence of 4 apical spines. It would be clearer if you wrote 1,2,3,4 as you did previously, or a,b,c,d according to my suggestion. The zero not followed by a taxon name is not clear to me; what taxon does it refer to?

M. lux has 4 subapical spines according to the original description, and therefore should be removed from Table III to Table II. I have specimens from Indonesia in which 1, or 2, or 3, or all 4 spines may be missing, no apparent system to them. Also I have a new variety with spines on the face of the cell, but have not seen enough specimens to be able to discuss the subapical spines.

Page 9. *M. foliacea* is common here, also in Brazil and Indonesia, less common in N. Australia. It has two pairs of subapical spines of different sizes. I have seen a large number of specimens, in which the smaller pair of spines is always in the position 1&3 and the larger pair 2&4. The extreme apical points themselves (right and left) are not in the median plane, perhaps caused by the twisting of the right and left halves of the semicell, with the opposite semicell curved or twisted in the other sense. This twisting reaches such a high degree in Indonesian specimens that the filament, instead of resembling a ribbon of uniform width as in American specimens, has margins that are sinusoidally waved or deeply scalloped, giving a very curious appearance. I have tried to draw this, but found it too difficult to make an intelligible representation.

M. Borgei var. *multidentata*. I have specimens from Peru in which the subapical spines are entirely missing. The identification was confirmed by Krieger, who wrote "Der Scheitel trägt auf den Höckern keine Stacheln".

Page 10. The sentence beginning "Dichotypy signifies a desmid" is rather awkwardly worded.

I suggest the following: "Dichotypy signified a condition in which a desmid possesses two semicells which exhibit such great differences in important characters that they would be considered as belonging to two separate taxa if they formed parts of two monotypical cells".

Page 11. *Amscottia mira*, not *mirabilis*. The latter was proposed in correspondence, but changed to *mira* when published.

Enclosed is a somewhat better photo of *M. sudanensis* than the one G. sent you, also a poor photo of *M. incredibilis*. The latter is truly incredible. The lower semicell has two pairs of lateral lobes which diverge at about 45° from the vertical axis in both the ~~side~~ front view (as in the photo) and in side view.

After my experience with the new genus *Ichthyodontum* from Sumatra, first found only as dichotypical specimens, but which later proved to exist in two separate and distinct forms, I am inclined, that such desmids as *Amscottia mira*, *M. sudanensis* and *M. incredibilis* will eventually be found to exist in two different forms, though at present they are known only as dichotypical combinations. The most obvious explanation seems to be that they may be hybrids, though W. & W. discounted the possibility of hybridism.

Page 12. line 2. Hannah calls attention to the expression "an uniradiate desmid". This is grammatically correct, though I think it is obsolete or obsolescent. In speaking you say "a uniradiate desmid" because the next word begins with a consonantal sound, and that is the way I would write it.

Bottom line. It might be better to use the word 'transitional' instead of 'transitorial'. The usual meaning of 'transitory' is 'lasting for a short period of time', as in electrical phenomena.

Page 15. In a paper now in press on the new genus *Ichthyodontum*, I have called attention to the fact that there can be no real interlocking of the apical processes in any of the filamentous desmid genera. In describing the very large overlapping basal teeth I have used the word 'intermeshing' to exclude the concept of 'interlocking'. Prescott objected to this word because he could not find it in the dictionary, and doubted if it exists. But I have been familiar with it in engineering use for at least 25 years, so I used it. He will be surprised when he sees it in print.

Page 17. The specific form of *St. leptocladum* has no apical spines. The form with them is var. *cornutum* Wille. Grönblad called attention to this in his Brazilian paper, and I am giving an illustration of a spineless form from Lake Victoria, Uganda, in a new paper now in preparation. Note the illustration by L.N. Johnson, 1895, Pl. 211:3, where his vertical view shows the apical spines 2&4, but the front view 1&3. Yet Johnson was a careful observer and a good illustrator. For my part I have never seen the spines in any other position than 2&4, and I have seen plenty of specimens.

Page 15. (again). In *Onychonema filiforme*, *laeve*, *uncinatum* and a new unpublished variety of the latter, I have never seen the apical processes in any other position than 1&3, and am suspicious of drawings that show them as 2&4.

I have been under the impression that *Sphaerosma* always had 4 apical granules, but they are so very difficult to see that I cannot make any definite statement from my own experience. As a matter of information (not criticism) is it correct to apply the word 'process' to such minute granules? or to the apical connecting parts of *Besmidium*? I mean such species as *D. Swartzii*, not *apogonum* nor *Baileyi*.

I think all unpublished new names should be placed in quotation marks: "-----".

Linköping 12/ IV/ 1956.

Dear friend Scott!

Many, many thanks for your comprehensive letter that gave me so valuable indications of your feelings and view-points. These things are very difficult, some of them impossible to discuss in letters, I am sure that some hours discussion face to face would give more than a year's correspondence. I will come back to certain parts that contain mere facts later on.

Just now I will try to inform you about the little part of my paper dealing with the concepts and terminology of the desmid basic structure. I was not offended but very surprised at your comments. You find my disentanglement quite unnecessary and seem to have been both worried (big letters!) and surprised by the fact that I had made so little use of it in my paper. But, in my paper there are no descriptions, no anatomical discussions except the semiradial asymmetry, it is a mere list of desmids collected from a special point of view and the ~~xxxxx~~ morphological and taxonomical results that could be drawn. In any case you found it unnecessary and, what is worse, in reality incomprehensible. This implies two elements: 1) the justification of it, and 2) my treatment of it.

As to the former, 1). Every science must possess its basic elements clearly defined and unambiguously termed. Since there are no combination made about the fundamental concepts and terms of our branch, I have brought them together in a system, based on the radius concept, previously a dubious term also ~~xxxx~~ embracing radiating processes on the whole, the accessory processes of St.Arctiscon, according to G.S.Weste.g. I have used parts of the old terminology and only when necessary shaped new terms, only three, viz. radial, interrarial and isthmal, very easy to comprehend. I have rejected some synonyms in order to prevent misunderstanding, a basic logical rule. Is this unnecessary in your opinion?

As to the latter element, 2). I am very sorry that my explanation is impossible to understand, no one can regret more than I do, my bad English. As to the content I must admit that I am not quite ignorant of stereometry, on the contrary, I was very enjoyed in analysing the often intricate crystals during the exercises for my bachelor of science examination. Consequently my explanation must be unintelligible ~~xxxxxxfairxxx~~ since my opinion seems very simple, at least to me.

Together with the enclosed pictures I do hope that the following explication and complet~~ing~~ will make my opinion clear to you or at least clearer, the responsibility is mine, at any rate. I sent so many pictures to Hannah who had amiably promised to draw them (my hand is trembling and the result is very bad) but I did not send the pictures showing the planes and axes, regrettably, since I meant I could draw them myself by the aid of a ruler, but I ought to have sent also these pictures, I am sorry that I did not, such as the circumstances have developed.

Well, do you accept the concept of a radial planes, determined by a radius and the vertical axis, in the pictures signed a, and the double radial plane, signed A - A, i.e. one total section vertical plane, composed of two radial planes. Interrarial planes are signed b, double interrarial planes signed B+B.

The word "composed" seems to have been unexpedient; I mean one plane composed of one radial and one interrarial plane together. Evidently compound or composite is the correct word. I mean in the pictures the plane signed a - b in desmids with odd numbers.

As to your quotation, page 2, line 16: My sentence is misleading (sorry), it would be: "In biradiate desmids the double radial plane and the double interrarial plane form the two symmetry planes, the ~~xxxxx~~ latter is termed the frontal plane and the double radial plane is the lateral plane." And further, p.2 line 20: "In these each radial plane together with the opposite interrarial plane form a composed symmetry plane that on one side etc". Here I have used the reciproque expression "on the one side ...and on the other side" without thinking that the word side here will be apprehended

as on both sides of the plane and that it is, in this connection, misleading. It would be expressed "In these each radial plane together with the opposite interradiat plane form a compound (?) plane, that in the one direction divides a front side into two halves, in the other direction passes through a radius' angle".

You can say that my demonstration of the basic structure elements is "academical" and in certain details being necessary ^{only} for special desmid investigation. It is; however, well justified, you can be quite sure of it. The traditional terminology is based on biradiat desmids and I have made an universal combination of facts valid for all ^{desmids} basic forms of desmids on the basis of ~~them~~ ~~and~~ ~~the~~ ~~fact~~ ~~that~~ ~~the~~ ~~irradiat~~ ~~desmids~~ ~~are~~ ~~not~~ ~~the~~ ~~same~~ ~~as~~ ~~the~~ ~~desmids~~ ~~of~~ ~~the~~ ~~genus~~ ~~St.~~ ~~pinnatum~~ ~~var.~~ ~~floridense~~ ~~is~~ ~~marvellous~~ ~~both~~ ~~dichotomous~~ ~~and~~ ~~a~~ ~~janus~~ ~~!~~ ~~A~~ ~~preliminary~~ ~~explanation:~~ ~~the~~ ~~basal~~ ~~processes~~ ~~have~~ ~~not~~ ~~been~~ ~~developed~~ ~~only~~ ~~their~~ ~~(ordinarily~~ ~~two)~~ ~~end~~ ~~granules~~ ~~are~~ ~~developed~~ ~~and~~ ~~appear~~ ~~as~~ ~~rudiments~~!

I am very sorry that this could happen, I had not the slightest suspicion, that ~~xxx~~ this little detail of my paper would cause all this trouble, spoiling time, intelligence and temper of my two best friends. If you, after my elucidation not immediately comprehend my "stereographical" opinion, I beg you urgently to leave it ^{aside}; it is of so small importance on the whole, and is by no means worthy of this discussion between us.

Many thanks for your interesting pictures, your *St. pinnatum* var. *floridense* is marvellous, both dichotomous and a janus! A preliminary explanation: the basal processes have ~~not~~ not been developed, only their (ordinarily two) end granules are developed and appear as rudiments.

Your Gertrude Stein, if she had been a desmidiologist, would have written: "a plane, another plane, another plane and another plane, they are all different but they form together a rose, a rose, a desmid rose". You agree?

My excema is probably caused by formaline, I was obliged to supply formaline in my 300 samples from 1911, only a few of them were dried, fortunately. Now I have put a tube at the microscope table just at the slide and connected ~~it~~ to the vacuum-cleaner and I hope to avoid the inhalation of the formaline fumes.

Hoping that you and your wife are quite well I beg to express our best regards to you and your wife,

Your sincere friend

Emmer

one fixed plane, one fixed axis and four or more planes turnable at the vertical axis.

Linköping 10/11/56

Dear Friend!

After having read your words
of the previous part of your letter
I suspect that I, in the copy, don't
have expunged the sentence "The
starting point planus fides
by Alice", page 1, line 11 and 12.

This sentence is, as you have
said, superfluous.

Today, I got a letter from Givellén
in which he tells me that he will
attend the Congress and the related
excursion. It is a pity that you will
not attend for the sake of company,
I suppose very little is to be men-
tioned about mammals. My lecture
will probably be retained, but
I had missed to announce it
without the ruler part of time
best regards,

Yours sincerely
Gunnar

TELEGRAM

TELEPHONE

Apl 17 1956

Dear friend Telling,

Of course I knew you would be surprised when you received my last letter, but also I thought and feared that you might be offended despite my assurances. So it was a great relief to get yours of Apl 12 saying that you had not taken offense.

I can understand that from your academic point of view it is necessary to start with first principles and exact definitions, just as Euclid did. But that is where our viewpoints differ. I gather that your life has been almost entirely academical; mine, on the contrary, has been severely practical, so that probably I do not pay sufficient attention to theory.

You ask if I think your exposition and new terms unnecessary. Yes, I still think that you have too many species, subspecies and varieties of planes. I count eight of them; radial, double radial, interrational, double interrational, composite, frontal, lateral, and isthmal (not isthmal). I still believe that any case of desmid asymmetry could be adequately described by reference to the horizontal, frontal and lateral planes, and partial rotations about one of the axes lying in these planes, and in a few instances by reflections across a plane, (C. obliquum).

But enough of the argument. Your sketches have cleared up for me, as I thought they would, the meanings of your new terms, and you have explained the two obscure sentences whose exact meaning I did not grasp. Now I can make the necessary corrections in the English which will put your stereometry into proper shape. Already I have listed a number of comments on the various desmids that you mention, and I think you will find some of them interesting.

I will return your Ms. during May, but until May 1st I must devote all my time to drawing my plates, in order to meet the deadline. In addition, most of the descriptions and diagnoses have to be retyped, because Hannah has made numerous small changes in Grönblad's Latin, and I have corrected his English. A paper this this, in which practically every desmid is new, requires a lot of work, but it will get into print practically all of the novelties that I have found in southeastern USA during the last 17 years. It's a pity you cannot wait for the appearance on this paper, you'll be astonished when you see some of the strange 'critters' that I have discovered.

Your explanation of the possible ~~xxxx~~ cause of the dichotomy in my example of *St. pinnatum* var. *floridense* is quite plausible. Only this one specimen has been seen, and we shall have to wait until it turns up again before much can be said about it. I have found many symmetrical specimens, and all of them were 5-rotate.

When you get a chance I shall be much interested in hearing your comments on some of the other subjects mentioned in my last letter. Until then, my best wishes to ~~you~~ you and your wife.

Your friend (still?)

Linköping, Easter 1956.

Dear friend Scott!

Thank you so much for your letter 20/III. I am very ~~glad~~ delighted at every contribution and improvement of my paper. If you critically comment my attempt of an exposition and terminology of the "desmid stereometry", I am sure it will approve, at least my reproduction of the ideas I have not seen any disentanglement of this subject and thus it is of great importance that this one is thoroughly discussed. Probably my verbal treatment is somewhat lapidary; it is a mistake to give way to the temptation of presupposing an immediate apprehension of the readers in subjects worked through by me during a long time, but are new for them, especially in foreign language. I have, however, too often laughed at certain authors who use too many words on simple things evidently in order to make the paper thicker and thus I have been anxious not to be looked upon as one of them.

Probably my paper will be printed this summer and appear in September perhaps only in December. You are now occupied with the revision of the plates of Scott-Grönblad's paper this month, probably you may have time to criticize and complete my paper in May ~~xx~~ and I can finish it in June. Your hints of additions are very promising and with your references to plate and figure numbers these additions would make the paper extremely actual.

Many thanks for the microphotos of St. Wildemani. Will you, please, give me the exact title of the Australian paper and the journal where it is to be printed, it would be a valuable addition to the list of literature cited. The MS with the pictures can you conveniently send as "Commercial papers" in a wrapper, for the sake of security with a twine round it, as a ~~mail~~ mail letter it is unnecessarily expensive!

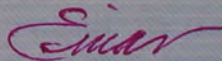
In the beginning of July I have the usual teachership at Aneboda and the 25/VII I shall go to Helsingfors for the Limnological Congress, it is a pity that you can not attend it. I should like to visit Åbo (Turku) at my way home, perhaps I have an opportunity of meeting Kallio. I think I must resist upon the long excursion to Lapland after the Congress. Bourrelly will be present and I am very delighted of meeting him and also many of the friends from the Congress in 1955.

I am very busy in investigating my samples of the lakes in the neighbourhood of Stockholm, collected in 1911 and 1956. Regrettably I have a too short time to make the report for the Sanitary Commission, that has economically supported my work, I just got a demand of sending it before summer, very unwelcome, previously I have not heard of any hurry.

I am suffering from an excema and have suffered from an eye injury that has checked the microscoping. The terrible coldness has decreased, fortunately, only these last days the temperature has passed over zero, spring is about three weeks delayed and this is rather much regarding our short summer.

Our best regards to you and to your wife,

your sincere friend



April 5 1956

Dear friend Teiling,

Your long letter of Feb 17th was delayed in reaching me because you had addressed it to 2428 instead of 2824 Dante St; it arrived in the same delivery with your airmail letter of March 27th. Today I have yours of 'Easter 1956' in which you permit me to offer a criticism of your paper on Asymmetry in Desmids. The latter will require a good deal of thought, so at present I will reply only to yours of Feb 17th.

It is a great honour and very flattering that you consider me, apparently both personally and as a desmidiologist, worthy of the considerable effort and time that your lecture or sermon, or homily must have required. Especially I appreciate your confiding to me some of your intimate thoughts about the science, and about other scientists. You may be sure that these will remain confidential; I have eliminated your reference to P. as you request.

But here is what I find rather strange. You seem to have revised your opinion of P. after he conceded the theoretical correctness of your two new genera. You have a high regard for Bourrelly, who has agreed to use these two generic names. You value your friendship with G. very highly, and recognize his great ability, but your estimate of him seems to have diminished somewhat because he does not see eye-to-eye with you on these two genera. Is it possible, do you think, that your attitude is very slightly biased because of your obsession with your theories?

Confidentially I may tell you that G. is by no means convinced of the feasibility or necessity of the genus *Groenbladia*. Here is what he wrote on Mch 19 "I don't know what to do with Teiling's 'Groenladia'. I am not convinced it can with good reason be separated from *Myalotheca*. What do you think yourself? *Gonatozygon* is known to have plate-shaped chromatophores, but already Delary has recorded specimens with 3-radiate coloroplasts. I myself have recorded specimens with 4-6 longitudinal ridges, (Gbl. 1943, I;1) but I think no-one would say it is not a true *Gonatozygon*. I have very much pondered over this question, I don't know?"

I had not answered this letter of his when I received another dated Mch 24, in which he says "Along the species not yet described are only very few: *Myalotheca* ("*Groenbladia*"), which I am inclined to accept on account of the very peculiar zygospores, not only to make Teiling glad, and myself proud".

As to the zygospores I am not quite sure what he means, because the drawings have not arrived. But so little is known about zygospores that I do not think they can be considered a generic character, merely a specific one, and that in comparatively few cases. For my part I have only had two occasions to use zygospores as a means of determination. One was a new *Anthrodesmus* from N. Australia which P. referred to an existing species as a new variety, until I pointed out that the zygospore which I had luckily found, was entirely different from any that had been described for the genus; so he made it a new species. Again, G. thought that one of my drawings, which I had labelled *Cosm. ocellatum* var. *incrassatum*, was identical with Borge's *C. luscum* from Brazil. But I was able to show that the zygospore is the same as the one described for *C. ocellatum*, and he revised his opinion.

What you tell me about Borge is quite surprising; I had thought of him in quite a different light. But his expression fits my case exactly. I am simply not competent to discuss the higher aspects of taxonomy (sense W.H.Camp), and I am too old to learn about it. My brain is not so active as it used to be, and no longer soaks up new knowledge the way it used to. I am well aware that in all branches of science

there are, and must be, different categories of workers. The lower ranks, in which I include in *Wae* *and* the *grade-guns* and provide the data which are identified or culled by the text of the next higher rank, and their results are made available to the 'big shots' (the 'great guns'), who can then play around with them to their heart's content, and shuffle and re-shuffle them into different combinations until a configuration is arrived at, after many arguments, that is more or less generally acceptable.

When other botanists laugh at the classification 'system' in desmids you could shut them up rather quickly, I should think, by asking them what the devil they would do if they had to classify their vascular plants on the basis of the leaves alone, without the aid of flowers, seeds, fruits, woody structure, &c, &c, &c. Morphology is the only criterion, I believe, that can be used for 95% or more of desmids. This is due to the negligence of Mother Nature, who originally created, a few million years ago, two or three desmid genera, but instead of keeping them ~~xxxxxxx~~ within the bounds of their little flower-plots she allowed them to run wild, with the result that there is now such an inextricable intermingling and intergrading of species and even genera that nobody, repeat nobody, knows where one genus ends and the next begins. In my own small experience I have had so many examples of disagreements between experts as to generic assignments that I have become more or less indifferent as to the genus in which my new desmids are placed. So long as I can attach a label to them so that they can be recognized and written about, they can be placed in any box that suits a particular expert's fancy. Here are some examples: Hannah would place a certain peculiar little desmid in *Xanthidium*, while G. has assigned it to *Cosmarium* on the basis of a slight resemblance to certain existing species in that genus, though he admits that it could just as well go in *Xanthidium*. The desmid for which I proposed the name "*cockeysidium*" was thought by Krieger to belong in *Euastrum*; G. has hesitated between *Cosmarium* and *Euastrum* for a few years, and it has finally landed in *Cosmarium*. Just the reverse is the case with another funny desmid that I found in Peruvian material; Krieger has assigned it to *Cosmarium*, while G. thinks it should have gone in *Cosmarium*. Krieger excluded a certain unnamed "*Euastrum* sp." found by Nordstedt in New Zealand on the ground that it was a *Cosmarium*, while Skuja, who found it in Rangoon, has named it *Euastrum diplostauron*; I have seen the plant and in this case I agree with Skuja. Krieger transferred *Docidium australianum* Borge to *Triploceras*, though its apical structure clearly shows that it cannot belong to the latter genus; I have seen the plant and have renamed it *Pleurotaenium australianum* (Borge) Scott. When I found the curious *Micr. ceratophora* Jogh. and believed it to be a new discovery, I hesitated between *Arthrodesmus* and *Micrasterias*, finally deciding on the latter for no very good reason. Then P. told me that old man Joshua had found it 70 or more years ago in Burma, and had placed it in *Micrasterias*. But it complies with every single one of the generic requirements for *Arthrodesmus*. As you and others have pointed out, *Arthrodesmus* is a purely artificial genus, serving as a convenient receptacle for certain forms not easily assigned elsewhere. But M. & W.'s generic diagnosis of *Arthrodesmus* would have to be changed very materially to include certain strange forms that G. and I have found. From USA I have an *A. impar* with a central protuberance and thickening, and also a new variety with a central spine just as long as the other spines. And from the Sudan G. found a remarkable *Arthrodesmus* of the same shape as *A. convergens* but much heavier build, that has an elaborate facial decoration of pits and granules, just as beautiful as *Cosmarium decoratum*, *C. cosmetum*, and similar ones. And there are certain desmidologists who think that when a *Closterium* decides to grow a spine on each end, it automatically transfers itself to another genus, *Spinoclosterium*, just as an electron jumps to a larger orbit when it gets a kick in the pants from a quantum of light energy. So, so, so, do you blame me for giving up?

As to *Actinotaenium*: Though I am not competent to judge them critically, your arguments seem correct to me. It is highly desirable to discover, if possible, the true genetic relations between the species, and to arrange them in genera that will each include closely related species, as has been done for the higher plants. But in the present state of our knowledge of desmids that

seems to me quite impossible, and I fear that it will be a very long time before it can be accomplished. Do you have any expectation of continuing with the re-arrangement of *Cosmarium*? If not, I don't see much advantage from the viewpoint of systematics (*sensu* Camp) in splitting off the small group represented by *Actinotaenium*.

Now some notes on *Staurodesmus*. As you know, I have a fairly keen eye though not quite so sharp as G.'s, who has discovered a few minor errors in my drawings, fortunately not important. Also I am constantly on the alert for unusual desmids. But my records do not show, nor do I ever remember seeing, either in my own collections from the USA or in other material from other parts of the world, A SINGLE SPECIMEN of dichotypical or Janus forms in the monosporous species of *Arthrodesmus* and *Staurastrum* that you have combined into *Staurodesmus*, and which you have found in considerable quantity in the plankton of Swedish lakes. Note, however, that I have made very few plankton collections, so I do not exclude the possibility that somebody taking plankton from the larger lakes in southeastern USA might catch such forms. Just now I tried to find your *Staurodesmus* paper so that I could see which forms I have recorded, but it has unaccountably disappeared temporarily. But I can mention such species as *A. incus* and var., *A. triangularis* and var., *St. dejectum*, *St. cuspidatum*, which I believe are among those you mention.

There are, of course, many differences between your Swedish lakes and the waters that I collect from, which are roadside ditches, small and large ponds, a few lakes, many marshes and swamps. Practically all my material is squeezings or strippings from submerged aquatics or mosses, because I get a much larger number and wider diversity of desmid species than with a plankton net. Your climate is cold, ours is subtropical; your rocks I think are largely granite or other acid types, in SE USA there are no acid rocks whatever as far as I know, the very few rock outcroppings are of the limestone type. In central Florida there are surface deposits of a soft phosphate rock, and incidentally there are no favourable desmid habitats in that locality. The whole of the Florida peninsula is underlain by limestone, but in many large regions the surface soil is sandy (siliceous) and so acid as to be useless for agriculture; the terrain is flat with poor drainage and consequently a high water table. Our annual rainfall is 1.50 m, but many of the small waters that I collect from dry up completely during droughts, which may occur once a year, or at any rate every two or three years, and this would prevent the development of a desmid plankton even if the waters were large enough.

Now what do you suppose is the reason for the TOTAL absence from my collections of the dichotypical forms that you have found in comparative abundance? I cannot see any plausible explanation. If you say that they are true plankters, what is the reason for their existence only in the plankton while the 'parent' species of both genera occur fairly frequently in my material?

You seem to think it strange that G. refuses to draw small details that he cannot see in vertical view though he has seen, or perhaps only glimpsed, them in front view. This is indeed a funny story, but the funny part is that you do not understand it. It is in fact my own practice, though I did not copy it from G. Indeed I should think that every careful worker would decline to draw details of which he is not certain. And though you can frequently see verrucae in front view, if you cannot see them in a true vertical view you cannot ascertain their exact position or arrangement or appearance. Just now I have a *Cosmarium*, possibly a new species, in which I have been unable to see the ornament in the center of the face, which the vertical view indicates is probably present. It is extremely rare, only 2 examples seen, so I have not found it practicable to use the chlorine method of bleaching or dissolving the chloroplast. G. has sometimes reproached me because I have noted "Central scrobiculae, if present, not visible", in certain species of *X. antilopaeum*. He says I should crush the specimens, even if the cover-glass is broken. He may be able to do this, but I cannot, because my method of mounting requires a shallow cell.

Yes, I have read of the remarkable results that have been obtained through study of varved clays in various parts of the world, and knew that the method originated in Sweden, but I had forgotten the name of the inventor of the theory.

I have Lagerheim's paper describing his method of separating desmids from dried specimens of *Utricularia*, etc., and though I know no Swedish there are sufficient words in your language similar to words in English or German to enable me to puzzle out the gist of his method. I have sometimes thought of trying to get from American museums samples of such dried water-plants, but it would be rather difficult because I have ~~know~~ no acquaintances with the museum staffs. And now I have on hand enough material to keep me busy for the next five years at least, and after that, who knows?

Your allegory of the iron and nickel-plated screws struck home to me, because ever since I acquired a home of my own, 36 years ago, I have been an inveterate saver of all kinds of screws, bolts, nuts, washers, springs, small pieces of steel, aluminum, brass, bronze, etc., so that when I want to make a small attachment or device for my microscopic work or other purpose I can usually find what I need in my various junk-boxes without having to go out and buy it. But they are only roughly assorted into the various kinds, so that just as with desmids it may take a considerable time to find what I want.

One of the difficulties inherent in the work you are doing in collating and comparing the various properties of desmids lies in the fact that you must rely so largely on illustrations, without having the advantage of seeing the plants themselves. This results in your making comparisons that seem quite peculiar to me. For instance you say that my *Burkillii* var. *aurantiaca* seems to be a *sexmillatum*. It is true that there is a superficial resemblance in the drawings you copied from W. & W.'s Burma paper (which I possess), but in vertical view the two lateral spines in *Burkillii* are widely separated, while in *sexmillatum* they are in the same vertical plane. In addition *Burkillii* is distinguished by the possession of an unusual feature, an arc of large scrobiculæ above and below the isthmus in a yellow band. In W. & W.'s illustration this arc is reversed; it should be convex upwards in the upper semicell, as is seen on a smaller scale in many *Cosmaria*, e.g. *C. obsoletum*. Enclosed is a sketch that I have tentatively identified as *X. sexmillatum* var. *pulneyensis* (sic) Iyengar & Bai, showing the lateral spines in the vertical plane.

Again, you say that my dichotypical *St. gyrens* is probably the one depicted by West as *St. incisum* (I also have this paper). But there is no possibility of comparing ~~of~~ these two species, even though there is considerable uncertainty as to what *St. incisum* really is because of Wolle's poor drawing. His vertical views show perfectly straight processes rather broad at the base and with sharp teeth. In *St. gyrens* the processes are always curved sinistrally, very slender with only a slight taper, and with minutely undulate or minutely denticulate margins, never with sharp teeth. Incidentally, Irene-Marie's drawing (1936, 54:12) of *St. gyrens* fa. *quadrata*, is a basal view, which reverses the position of the spine at the base of the processes.

Just a few days ago I found a single dichotypical *Staurastrum* which approaches closer to W. & W.'s figure of a dichotypical *St. incisum*, and I am enclosing a sketch. The 5-radiate semicell is *St. pinnatum* var. *floridense* Sc. & Gbld. (unpubl.); what the 4-radiate one is I don't know yet.

When I was searching for a particular form of *Tripl. verticillatum*

a few days ago, I found some nice specimens of *M. muricata*, a form which has unusually long apical processes of equal length. I was able to manipulate an empty semicell into exact vertical position, and I have drawn for you the three whorls of processes, with the frontal (vertical) plane accurately shown on each sketch. You will see that the 1st and 2nd whorls are quite symmetrical, but that the whole polar lobe with its 4 processes seems to be slightly rotated clockwise about the vertical axis, semiradial asymmetry. The vertical plane was determined in each case by focussing down to the base, making two marks on the paper at the extremity of the longer basal arms, then raising the focus to draw the second and third whorls, and then drawing a line connecting the two marks. I am not sure if the more usual form of *M. muricata* is built in this way, but if it is of interest I will investigate.

The title of the Australian paper is: Scott, A.M., and G.W. Prescott. Some Freshwater Algae from Arnhem Land in the Northern Territory of Australia.

It will be published in the botanical volume of a series of books describing the results of an expedition sponsored by the Commonwealth Government of Australia, the National Geographic Society and the Smithsonian Institution (USA). The publisher is the CSIRO (Commonwealth Scientific & Industrial Organization) but I do not know the exact title of the report. Other volumes will deal with anthropology, archaeology, ichthyology, etc. It is now $3\frac{1}{2}$ years since I sent the Ms and drawings to Australia, but it has been delayed by Government red tape, insufficient appropriations, &c &c &c. The latest information I have is that the printer is supposed to have the final proofs of the botanical volume ready by next July, so if there are no more delays it might be printed next year. There will be 20 plates of Desmids by me, and 8 of other algae by Prescott.

Now a few comments on your paper on Asymmetry, at which I hope very sincerely that you will not be offended. Hannah wrote me that after reading the third paragraph on page 2, she felt that I had a "stated plane". I have read your stereometrical discussion a dozen times, but I still do not understand the meanings of the special terminology that you have introduced, and I have had forty years experience in making engineering drawings and interpreting some fairly complicated ones made by dozens of other engineers and architects. No doubt it would be clearer if I had your diagrams with the various planes labelled, but why is it necessary to use all these special names. Surely everyone who has sufficient interest and knowledge of desmids to study your paper must have sufficient education to understand the use of the three principal planes of our 3-dimensional Euclidean space, the horizontal and two vertical planes. These are sufficient for describing the most complicated crystal structures, so they ought to be good enough for the rather simple symmetry and asymmetry of desmids. If anyone does not know the conventional system he most certainly will not understand your more complicated one. But what surprised me and caused some irritation was that after you have required the reader to spend some time and effort in trying to assimilate your new terminology, YOU MAKE NO FURTHER USE OF THESE TERMS THROUGHOUT THE REMAINDER OF THE TEXT, until you come to the penultimate page, p. 27 of the Ms, where the word 'inter-radial' is used just once. So my feeling is that the stereometrical part might just as well be omitted entirely. If the paper had been sent to me by anyone except a very good friend, I would read this part once or twice, conclude that it was incomprehensible, swear a little, and then jump to the taxonomical part, where there is some really interesting information.

Hannah passed on to me the job of correcting the English in this stereometrical part. I shall do my best, but there is one sentence in particular whose meaning I cannot grasp. On page 2 you say "In biradial desmids the two radial and interradial planes form each together symmetrical planes, the interradial is the frontal plane and the composed (? = composite) radial planes form the lateral plane". Or perhaps you mean the group of radial planes.

(6)

Please make a rough sketch showing and identifying these various planes. Also another sketch showing say a 5-radiate desmid, showing how "each radial plane together with the opposite interradiial plane form a symmetry plane that on the one side divides a front side in two halves, on the other side passes through a radius angle".

No doubt you have heard of Gertrude Stein, the poetess who lived in southern France, and who wrote "a rose is a rose is a rose". After reading your paper I feel like writing "a plane is a plane is a plane is a plane".

Your term 'semiradiial asymmetry' is good, and I intend to use it.

I have written frankly my opinion, as I would expect you to do in similar circumstances. Can you and will you forgive me?

Sorry to hear about your eczema and the eye injury, and hope they are better now. Your weather must have been about the same as in southern Finland: G. wrote 10 days ago that he had been fishing through the ice. Here the thermometer reached 30°C. yesterday, rather warm for this time of the year, today is a little cooler, about 25°.

I did not fall asleep while reading your long letter and since this even longer one will probably be delivered to you in the morning I believe you will be able to keep awake also. Please believe me when I say that everything I have written is in pure friendship, there are extremely few persons to whom I would express my opinions so openly and at such length. I do not regard your theories as 'desk-fancies', but as serious and valuable contributions to desmid knowledge even if I do not always understand them thoroughly, and may disagree with some of your statements.

With my best regards to you and your wife.

Sincerely,

Feb 26 1956

Dear friend Teiling,

Hannah has sent me the Ms. of your paper on Asymmetry in Desmids, together with some pencil drawings, and has asked me to return them directly to you.

I am very glad to have the privilege of reading your paper in manuscript form for several reasons. In the case of certain of the desmids that you mention I can offer comments based on my own observation, and perhaps you may wish to incorporate some of them in the paper. Also I can provide you with a good many more examples of various kinds of asymmetry. Then I would like you to eliminate your two references to Prescott & Scott 1942 in the instances of *M. muricata* and *St. gyrans* because I believe these two illustrations, which were drawn by Prescott, to be inaccurate. Although he presumably copied them from my pencil sketches, they do not correspond with any specimens that I have seen.

Hannah also asked me to comment on some paragraphs of your paper that she found puzzling. These are principally in the first few pages of the Ms. where you discuss your proposed geometrical framework. I also found them confusing and difficult to understand, though I have a liking for mathematics and geometry, and a fairly good knowledge of the subjects.

But I could hardly comment on these paragraphs without going into a detailed criticism, and since the result might not be to your liking, I would like to know whether you would like me to proceed. So if you will write me by return mail I will get the manuscript back to you as quickly as possible.

Just now I am in the midst of drawing about 35 plates to illustrate a new paper by Scott & Grönblad on New and Interesting Desmids from USA, describing about 160 new taxa. Included are several asymmetrical desmids that you have not seen, also some curious Janus-forms and examples of abnormal development of extra processes in *Staurastrum*. There is also a strange *Cosmarium* (which Krieger thought should go in *Duastrum*) that is asymmetric about all three axes and has asymmetric markings. For it I proposed (jokingly) the name *Cosmarium cockeyedium*, which G. has latinized into *G. strabo*.

I must get these plates to G. by May 1st, because the paper has to be presented at a meeting of the Soc. Sci. Fennica about May 19th, and it is going to take me every bit of the time to finish. I hope the paper will be printed in the *Acta* late this year, and if you could defer your paper until then you would have a good many more examples to offer.

After I get rid of these plates I shall gladly go through my collection of sketches and see what other cases of asymmetry I can dig out for you.

Wish best regards,

Sincerely,

Hannah and her mother are probably somewhere on the road in their car. I don't know how far they will get, for although they escaped two recent blizzards I am afraid they will not be able to get through the States of New York and New Jersey, where the roads are blocked with snow. Some months ago she thought of driving as far as New Orleans, but gave up the idea because of the great distance.

Dear friend Scott!

Linköping 19/ I/ 1956.

Now I have incorporated your rich and valuable collection of drawings in my collection of excerpts and I am working very intensively; I must cool my Parker pen in water in order to prevent melting. This because of the miracle hormones (Methandriol and Testosteronpropionate) remind these ~~is~~ ^{may} suspect the entrance into the declining years.

I have asked a friend, the Editor of Svensk Botanisk Tidskrift if he is willing to take your paper on *Staurostrum Wildemani*, you showed to me at your visit. I just got his answer and, if you like, you can get the paper printed here, the journal is a very good one, quite as Bot. Not. and printed by (the perhaps) best printer in Sweden (Uppsala). It can not be printed until the second number, in June, but the Editor wishes to get it about the 1. of March. In this case I will arrange that yours and mine paper (on *Ducellieria*) will be placed together, side by side. In any case, I repeat my objections that you must be regarded as the single author with your name alone on it. It is enough to express your gratitude in the text. I cannot understand how P. is able to see his name in this connection without laughing - or flushing, I could not. We call it to "get a lift" nearest to the Swedish expression but lighter. Of course, this is your business, I have only told my opinion, don't be offended.

Later, I shall submit you some questions about the names of your drawings, especially *Staurodesmus*, or have you handed over the determination to Grönblad? I have gratefully noted your hints about dichotomy into my excerpts. Your dichotomical *St. gyrans* is marvellous. "This is a damned lie" said the old man who saw a giraffe for the first time in the zoo.

A reconstruction of the chlpl of the new desmid from Sumatra is difficult as to the details, it may be something like this: *see the other side!* Your *Cosm. corrugatum* belongs to the omniradiate desmids that may belong to *Actinotaenium* quite as well as to *Cosm. Bourrelly* writes to me that he will use *Actinotaenium* and also *Staurodesmus* in the future, and Ruzicka has already described a var. of *Act. obtusum*. Some people can be convinced.

Your explanation of the apical asymmetry, depending on mechanical factors by the division of the cells is interesting and not foreign to me. In some cases it may be relevant, but asymmetry occurs in cases where is due to ^{the} failing of one part of a pairy organell, for instance the apical processes of of *Licr. mahab.* and similar species. If you like, Hannah can send you my manus after having scrutinized it. Probably you can make some comments.

We had a fine sejour in Sthlm, I increased more than one kilogram in weight! As an intermezzo I had a short but very pleasant trip to Uppsala, chatting and discussions with good scientific friends and fine finds in an archiv. The paper geological is sent away, my part containing a little contribution to the history of science. The 1. of Januar I was back to Linköping how extremely well my daughter and son-in-law took care of me, I longed to my studio and the waiting work; an old man has inveterate habits!

Here we have a bad winter, the temperature about zero and hardly any snow. Before Xmas we had a terrible snow-fall that caused much trouble in the traffic, our train to Sthlm the 22. Dec. was four hours delayed.

As to the common friends, from Grönblad no news, as a Xmas gift he sent me the latest volume of a very witty causeries of a beloved Fenno-Swedish chemist-professor. Grönblad seems to be very busy with your desmids. Ruzicka seems to be very active, he develops to a good desmidiologist, I think he has come to his right position. Hannah has sent me a collection of her fine drawings for examination.

In Uppsala I got the adress of Kossinskaja (a female desmidiologist). I sent her some papers and got some in return. The Russians now

only write in Russian, earlier they gave a little resumé in French, so that it was possible to get the title in legible language. Fortunately they are unable to change the Latin names, the names of the authors and the pictures in Russian manner! Her address is: O.C. Kossinskaja, Prof. Popov-str. 2, Ap. 20, Leningrad, U.S.S.R.

My best wishes to your wife and to you!

Your friend Einar Tulving

Jan 30 1956

Dear friend Teiling,

Thanks for your efforts with the editor of Svensk. Bot. Tidskr., but the paper on St. Wildemani has already been printed (I hope), in "Reinwardtia", a journal published by the Botanical Gardens in Bogor, Java. It is the successor to the former Bull. du Jard. Bot. de Buitenzorg. Formerly it was edited by Dr. M. A. Donk, a specialist on Fungi, whom you may have met at the Stockholm Congress. He appears to be a very thorough man, quite a stickler for precision in nomenclature and for compliance with the minutiae of the Rules. He has now retired and lives at The Hague, Netherlands, Prins Mauritslaan 18, where he may possibly continue to assist in editing the journal as regards the cryptogams.

I think you have slightly misunderstood the relations in my collaboration with Prescott. When my correspondent in Java sent me a considerable number of desmid collections with the request that I should work them up, I realized that it would not be possible for me to do it without assistance, for reasons which you can understand. So I asked Prescott if he would collaborate, to which he very kindly assented. It is true that I wrote the St. Wildemani paper, but I had consulted with him by letter and also in person when I visited him in 1954. Also I submitted the Ms. to him and he suggested some small changes, which I made. Now I have written another short paper describing the new *Ichthyocercus*-like desmid from Sumatra, and have sent it to him for approval. But when it comes to writing the larger paper on Indonesian desmids with some hundreds of species, many of them new or extremely rare, I shall have to get more active assistance from him in identifying them, for he is the only man in this country with an iconograph and a wide acquaintance with the literature, and the literature itself. Some years ago he graciously allowed my name to appear as the senior author because I had done most of the work in obtaining the material, studying it, making the drawings and tentative determinations. But without his help I should have made many mistakes, as other inexperienced authors have done when they have tried to reach out too far. Here's an example. Dr. Herman Silva is a young man who studied under Prescott and graduated from Michigan State College. Now he is Assistant Professor of Botany at the University of Tennessee, and about a year ago he published a small book on FW Algae from some of the southern States. I ordered the book from the publishers, but when I received it I found that the part dealing with desmids was so extremely poor that I returned the book and asked for a refund of my money.

I do not need many guesses regarding the questions that you wish to ask me; of course they concern your new genera *Staurodesmus* and *Actinotaenium*. My own position is that I am still undecided. I think that your reasons are probably more logical than those of other authors who have tried to divide the genera *Staurastrum* and *Cosmarium*, but despite your efforts to teach me both by letter and during our talks in Linköping my knowledge of the principles of taxonomics and genetics is too small to enable me to comprehend your arguments properly. So I must wait for someone else to take the lead, and then follow behind. If Grönblad and Prescott would adopt your system I should be quite willing to use it also, though the problems in nomenclature seem rather baffling. Soon after publication of your *Staurodesmus* paper I suggested to Grönblad that since he is such a good friend of yours, it would be a nice thing and a big boost for you, if he would adopt the new genus, provided he felt that it was correct. He replied that he was not entirely convinced, and for your sake I am sorry to report that this is still his opinion. Maybe I should not have told you this; perhaps it would have been nicer if he had told you himself and explained his reasons, but you had to know sooner or later.

Thanks for your sketch showing a possible construction of the chloroplast of the new Sumatran desmid. I am still finding more specimens, but they are either empty or with the chloroplast so deteriorated that I cannot ascertain its structure. Just two days ago I found a zygospore with three empty semicells of the new genus in close proximity, but the whole group was embedded in a mass of debris that also contained many empty cells and semicells of other genera, so it is not certain that the zygospore actually belongs to the new plant. Two of the semicells, if assembled, would form a dichotypical cell; the fourth semicell was missing.

Cosmarium corrugatum, of which I sent you a photo, turns out to be probably the same as *Calocylinthus costatus* Wolle. His drawing shows a much greater constriction, but his description fits my Florida specimens very well. It is really astonishing how many desmids old man Wolle saw, and what a pity he was not a better draughtsman so that you could be reasonably sure of the species he tried to depict.

As to the puzzling dichotypical specimens of *St. gyrans*, you will have noted the 5-radiate cells without the spine at the base of the processes. If these were found alone I think they could only be referred to *St. arachne*. West & West say that *St. arachne* occurs in 4-radiate form also, so probably it could also be 3-radiate. Now, if the basal spine were to be removed from *St. gyrans* there would be little left, if anything, to distinguish it from *St. arachne*. I have therefore suggested to Grönblad that perhaps *St. gyrans* should be considered as a variety of *St. arachne*, in which case all of the specimens in this collection, including the dichotypical 3- and 5-radiate cells, should be assigned to *St. arachne*. He has not yet had time to reply to this suggestion.

Hannah is busy checking the Latin diagnoses for more than 100 new desmids written by Grönblad, and there are still a good many more to come, for I counted something like 160 when I was in Finland. When Hannah has finished with your Ms. on asymmetry I should like very much to see it, so will you ask her to send it on to me?

Thanks for giving Loddinckje's address. I also shall send her some reprints and hope to get some in exchange. I have the first part of her *Flora Plantarum Cryptogamarum*, but not the second; Grönblad has it and tried to get a copy from the Akademische Buchhandlung in Helsingfors, but without success. Do you think it could be bought from one of the Swedish booksellers? From here I cannot send money to the USSR.

With my best regards to Mrs. Telling and yourself,

Your friend

Jan 5 1956

Dear Einar,

I am glad that my sketches of asymmetrical desmids will be of some help in writing your paper on this subject. I remember the drawing of two cells of *Euastrum gemmatum* that you mention, though I cannot put my hands on it just now. The upper cell was not asymmetrical, but was drawn in the oblique position in which it occurred, the obliquity being caused by the engagement of the four lobules of the polar lobe of one cell with the corresponding lobules of the other cell. *Eu. gemmatum* is a quite rare species in my territory of southern USA and I have not seen many specimens; as far as I know it is not asymmetrical.

It occurs to me that the phenomenon which I have just mentioned may be the cause of the twisting of the polar lobules in some species of *Euastrum*, or of the apical spines or processes in species of *Xanthidium* and *Staurostrum*. Let me see if I can describe it, using the well-known *X. cristatum* as an example. When the cell divides the new semicells are at first hemispherical and are attached to each other at the summit of the hemisphere. I do not know by what means they are attached (mucus? or the middle lamella if there is such a thing in desmids?). A little later the hemisphere becomes distended at certain points to form the lobes, and then the spines start to grow, the new semicells still being attached. When the lengthening spines from one new semicell meet those from the other new semicell they must cross each other, and one or the other set must be forced slightly out of position. The new cell-wall is still very thin and delicate, and the twisting force might be sufficient to twist the whole polar lobe somewhat, provided the means of attachment of one semicell to the other is strong enough to resist such a force. A similar line of reasoning would apply to any desmid possessing apical lobules, spines, or processes which project above the central point of the apex, which is the point of attachment of the two new semicells. However, this 'explanation' does not explain why the twist in a given species is always in the same direction; one would think that it would be a matter of pure chance whether the spines from the lower semicell would cross over or under those from the other semicell.

I see that I have failed to answer some subjects in your letter of Sept 20th which I received in England. The liquid that I use for preserving material is about 4 or 5% formalin and about 5% glycerine, as recommended by West & West. Fortunately I have used this from the time I started collecting, for I find that the water evaporates slowly through the screw caps, and some of my samples deposited in the Farlow Herbarium have dried up, but were restored to good condition by the same method that you used. However, I think that 5% glycerine is not enough, and in future I am going to use 10%, for obviously the amount of glycerine required to saturate the sediment after evaporation of the water will depend on the quantity of sediment, and some of my samples contain 2 or 3 cc of sediment.

As to your proposed paper on dichotypy, I think perhaps it might be advisable to postpone it for a year or so. By that time my paper on *St. Wildemani* will be available, and also another paper by Grönblad & Scott on the Sudan with two very strange new species of *Micrasterias*, of which all specimens seen are dichotypical. Also in the drawings of new desmids from USA, which G. is now working up, there are some cases of dichotypy, but I do not remember what disposition we made of them. In the paper by Prescott & Scott, *Micrasterias* II (1952), Pl. 8, Fig. 3 shows a dichotypical specimen combining *M. floridensis* v. *spinosa* with v. *subjohnsonii*. In the same paper Pl. 7, Figs. 8, 9, 10 show dichotypical forms of *M. pinnatifida*, which were listed as formae of *M. pinnatifida*. I had discovered them several years previously, and at that time

I did not realize what dichotypy meant; in fact I think you had not introduced it. Concerning these 'forms' of *M. pinnatifida*, it is a curious fact that I have found them in widely separated places, from western Louisiana to Florida, a distance of several hundred miles, but never any more than one or two in a collection, and there has been no evidence to suggest what may have been their origin, since the only other similar *Microsterias* in the samples was typical *M. pinnatifida*.

I discovered in 1941 another case of dichotypy involving *St. gyrans* with something else that neither Grönblad nor I could identify with certainty. I am to try to find additional specimens and make better drawings, and I will send you a copy. Unfortunately this habitat was destroyed several years ago in the course of highway relocation, so there is no possibility of getting any more material. (Sketches are enclosed herewith).

Lütkenheller's method of using optical sections is, of course, the same method that I have been using for many years in making my desmid drawings, but he must have been a far more skilled worker as regards the chloroplasts. The lens that I use when investigating the chloroplasts is a 60x apochromat, which has a theoretical depth of focus of only about 2 μ , so you ought to be able to get an optical section of only that thickness. But what you see is not only this optical section, but also the out-of-focus images of the overlying and underlying material, which obscures the details so badly that only in exceptional cases can I get a clear-cut idea of the structure. Enclosed is another sketch showing a possibly new species of *Cosmarium* that I first found in 1946, but my first sketches did not show the chloroplast. Grönblad asked me to try to obtain the chlplst details, so I have spent two days searching for it in two different collections. I found three empty cells, and only this morning did I find one with the chlplst, and then spent an hour before I could balance him on end for study. You may be sure that I have looked at it very thoroughly, and the sketch shows what I saw. There are 10 (or perhaps only 9) radial lobes, each with 'fingers' extending out to the cell wall. But the central part surrounding the single pyrenoid was merely a dark mass in which I could not see the structure, so that I am uncertain whether the chlplst belongs to your type S3, S4, or S5.

Also enclosed is a photo of one of the two 'basic' types of the new genus from Sumatra. This specimen was the best preserved that I have seen, but the chlplst was partly decayed so that I could not determine its structure. Can you hazard a guess? The basal view of the semicell is elliptical. There are 5 or 6 pyrenoids in each semicell.

The name of the glossary that I mentioned to you is "Petit Glossaire Limnologique", par Marcel Huet, Directeur de la Station de Recherches des Eaux et Forêts a Groenendaal. Extrait du Bulletin du Centre Belge D'Etude et de Documentation des Eaux, No. 3 & 4, 1949. It gives first definitions of some hundreds of limnological terms and phrases in French, followed by translations of them in French-German-English, German-French-English, and English-French-German. There is one amusing mistake: the author has translated the French word 'mouillable' into German as 'benetzbar' (correct), but into English as 'soft (able to be)', having confused mouiller and mollir. The English word is 'wetable'. Prof. van Oye obtained the book for me without any cost.

With best wishes from your friend,

Linköping 6/XII/1955.

Dear friend!

I got your thick letter yesterday and I must immediately firstly say my thanks and appreciation for your unspared trouble and kindness, and secondly reproach you for your uneconomical reckless; it was not so great hurry that you were obliged to spend so much of money to the post, "commercial matters" had been enough. But, I must admit that I was deeply affected by your consideration and amiability! By this, I am able to write the paper at home and make a fair copy of it during the idle days in Stockholm; we are to go to Stockholm before Xmas and I shall rest there over the turn of the year.

I have collected a good material, I think, not at least with your rich collection of contributions. Most of them follow strictly the law of unilateral symmetry that I have found in my previous material and thus your examples confirm my opinion. Just now I am very glad of all the work spent on my icdnotheque.

I had noticed Playfair's picture of St. Rosei where the vertical processes are obliquely inserted like St. clevei and laeve. Your picture of St. leptocladum var. sinnatum was very valuable. G.M. Smith has depicted it a latere but incorrectly. This species also has the apical small spines oblique. As to incorrections, Skuja has drawn his picture of Anth. Burkillii var. alterpans (Süsswasser-algen Burmas, Tab. 33:16 wrong. Your Streptonema could be the triradiate facies of Spond. pulchrum var. Scotti if not regarding the connecting processes, according to this it may be assigned to Sphaerocosma (?vertebratum) fac. triquetra. I have found the unilateral symmetry in your picture of Eu. Chlasonii var. aper-tosinuatum (Pre-Scott: Euastrum II, Tab. 1:6) and Eu. pingue, ibid. Tab. 1:7, in which the frontal scrobicle and the tunulus are asymmetrical.

Now I must beg you to inform me about your Eu. gemmanum, fig. 6 on the Plate 4 or the blue copy of "Desmids from Pearlington" sent separately. The upper cell is somewhat irregular, do you have any picture a vertical? asymmetrical?

I got a fine book from Hannah, viz. the textbook by Carl L. Wilson with her excellent drawings.

I am glad that you consider the chlpls, how difficult it may be to decipher the intricate cases. There is much to do in detail, though I think that I have caught the principles of their structure and phylogeny. The number of the pyrenoids is, of course, dependant of the largeness and the dissection of the chlpl, compare Cosm. connatum (my fig. 2-5 and the taenioparietal chlpl p. 284. Lutkemüller used a method of reconstructing the more elaborate chlpls. He depicted a series of optical sections of the chlpl and on the basis of these he worked out the 3-dimensional picture, quite as especially the zoologists do on microtome sections of small internal organs and their histological structure.

I understand that you and your wife were tired by the change of your new manner of living, the re-acclimatisation must be a tiresome time. Here the temperature only at some occasions has fallen below zero.

The hormone that has given me new forces is Methandriol (the trade-name, probably only used in Sweden) is Diandrin and is given in small pills with only 0,025 grammes of the hormone. It is a male hormone and stimulates all the human tissues and organs to regenerating. Thus, it is extremely valuable to people of our age when the destruction will become predominant of the regeneration.

Here we live in a time of eagerly searching for proposals of Xmas-gifts to four children and seven grandchildren. Every Xmas we decide to decrease the number of gifts, but my wife is not to be checked!!

The best regards and wished to your wife and yourself!

My wife agrees intensively.

Yours

Emat. W

Linköping 17/XI/1955.

Dear friend!

Very long since I heard from you, I do hope that you are all right and that no gangster has visited your house during your absence. As to me, I am quite well now, better than at any time since the past three years, the hormone cure, described by my son-in-law, has made miracles and I am now anxious to recover the lost time as much as possible. I am sorry that I was not so vital during your visit, then I could have made more of the unique occasion, at least for my own part.

I have got back the mahabuleshwariensis-paper from Hannah and am just working ~~with~~ out an idea deriving from it that seems very interesting, viz. Asymmetry in the desmids and I should be very glad for your aid. You showed me an example of what I call radial asymmetry: certain *Staurastras* have the axes of ^{their} processes not radiating from the exact center of the cell, perhaps you remember it?, you made a drawing of it. I have a habit of making excerpts of facts worthy of memorandum and have hitherto noted the following examples of asymmetrical processes in *Staurastras*: *gyrans*, *hystrix*, *mamillatum*, *arachne*, *pinnatum*(?), *margaritaceum*, *cyrtocentrum*, *forcipatum*, *triforcipatum*, *subgracillimum*, *controversum*, *oxycanthum*, *Sebaldi* var. *Raciborskii*, *vestitum* var. *subvestitum* and var. *tortum*. Also in the apical ornament this asymmetry may occur, *pingue* var. *tridentatum* e.g.

Another kind of asymmetry, oblique asymmetry, concerns the apical processes of certain *Microsterias*, *Staur. furcigerum*, *leptocladum*, *Rosei* and *Clevei*. A third kind is the constitutional asymmetry, for instance *Notocosmarium obliquum*. This kind of research is what Grönblad calls "desk-fancies" but if you can give me some further examples, it would ameliorate my paper.

I have spent 10 days in Uppsala, a wonderful time among very good friends extremely noble persons and excellent scientists; the days at the microscope and the evenings up to the small hours of the night chatting phyceology and quaternary geology. A friend-geologist and I found that our diverse knowledge of an interesting natural landmark of memorable significance completed each other and we decided to make a paper of it: it concerns old shore-lines from the time when the immense fresh-water lake in the Baltic valley after the melting of the great ice changed into the salt-water sea, now decreased to the Baltic sea, at then 73 metres above the sea-level. This landmark, called the "Rubble-stone field" (*Stengårdet* in Swedish) is now quite destroyed by gravel-pitting and I took my way home over the place and examined "les beaux restes" and took photos, happily I have photos of it about 30 years ago. I have found a description of it from 1775, made by a nature-minded bishop and published in the Transactions of the Royal Academy of Science. Devotionally I read in it the list of the members of the Academy, many famous names, of which you know only Linnaeus!

My wife is well and enjoys her liberty as retired but she has worked as a supply teacher some weeks. She is a careful hen for her chicken-boys and so is our Ruth.

I should not be too surprised if there is a letter from you on its way to me just now, at a ~~any~~ ~~easy~~ rate I shall be enchanted to receive one, if ~~possible~~.

Possibly with your contributions to the desmid asymmetry. Grönblad has not answered on my two last letters, silence from East and silence from western. Hannah has sent me a wonderful text-book of Botany, in which a great many of excellent pictures derives from her skilful hand.

My best regards to your wife and to yourself

From your friend and accomplice in Desmidiology:

Tolling

Linköping 20/IX/1955.

Dear friend Scott!

Many thanks for your letters, of July 18. and Sept. 1. I am glad to hear that you are satisfied with your extensive Odyssey in Europe, also comprising the poor countries of Finland and Sweden. And I think that a resting month in Old England in the womb of near relatives will be a good preparation for your desmid research. I am convinced that your part of Prescotts team-work will be the most valuable for the whole; my well-founded opinion is that you are a much more clever desmidiologist than P.; it is very regrettable that you do not possess the necessary literary expedients.

I just have before me a thick dossier of letters and telegrams about Hannah's visit. Because of my daughter's sickness Hannah's visit at her house on the West-coast was inhibited and was meant to be substituted by a visit at Tebo in the beginning of August. Because of Hannah's peripatetic journeys and lacking service of her contact agent in London, she missed the opportunity until August 26., when I got a telegram that she would arrive to Stockholm by air. At that time my wife and I were in Stockholm and made excursions to the lakes that I had to take samples of there. We met her at Bromma, to her great surprise, and the next day we drove to Linköping, we examined her pictures, and made excursions in the southern part of Östergötland, the same district that we visited together. After having received several letters from her sick mother she felt obliged to go back sooner than she had intended.

Since then I have lived in Stockholm and finished the field work of my lake research there. Last Friday I returned and on my way home I visited several lakes in the province of Södermanland together with a good friend, an excellent female phycologist, (especially diatomist) who is examining a series of lakes for her dissertation. She followed me to Linköping and we have worked intensively the following days with the planktic desmids of the samples collected and ecological discussions. Very interesting for me, since I was able to clear up an intricate desmid, very similar to Staurastrum longipes. Several finds of its Janus specimens 2+3 revealed that it actually was the 3-radiate facies of Staur. Smithii (earlier St. contortum C.M. Smith), a facies hitherto not known. This desmid is pronounced eutrophic opposed to St. longipes that is decided oligotrophic. I am very glad of this: an other example of the taxonomic aid of the concept of dichotypy to which you showed me an excellent case (St. Wildemanni etc) at your visit here. I think it is now expedient to realize my intension of writing a little paper on the dichotypy in order to call the attation to this new, and as it seems to me, valuable principle in the taxonomical work. If you possess any more example of

this phenomenon, I should be very glad.

To day I looked over the fine collection of samples, formerly in the possession of Krieger, that you kindly sent to me. Regrettably I had not examined the packing of the cover of the vials and I found, to my great distress, that a couple of the samples were dried up. I gave every fial a few drops of glycerine and filled them with formaline liquid, I do not know which conserving liquid you had used. I always add a little glycerine after the examination, then a drying-up does not cause such fatal consequences.

I enclose some photos from my wife's as a memory of a unique situation.

We are all of us quite well. I am suffering from a too low blood pressure and is too tired, but I have begun a hormone cure according to the directions of my son-in-law, I do hope that it will give me more force for working.

With our very best greetings to your wife and yourself,

your sincere friend

Digitized by Hunt Institute for Botanical Documentation

Nov. 27 1955

Dear Einar,

I certainly owe apologies to you and Grönblad and several others, for not having written for so long. The fact is that since our return, nearly two months ago, I have simply not been able to get my mind down to business; not from ill-health, but just a disinclination to do anything except eat and sleep. Actually both my wife and I were thoroughly tired from our European visit. Also during almost the whole of October the temperature here was still in the nineties (32-34°C.), and we felt it very much after the cool weather that we had in England.

Yes, our house was OK after having been closed for more than five months, but our maid had come once a week to see that everything ~~was~~ was allright, and do a little cleaning. But my microscope room, which had been locked up, was a mess, with green mould (mildew) over everything. It even got inside my microscope and I had the devil of a job cleaning the mycelia from the complicated binocular prism system. That is one of the joys of living in such a hot and humid climate.

It is good to hear that you are in better health. Perhaps I need some of those hormone shots, but I stay away from the doctor as long as I can possibly do so. Please give my regards to your wife, and also to Ruth.

The phenomenon of asymmetry in desmids is one that has excited my curiosity also, and I have recorded many instances, so that I can add considerably to your list. The enclosed sketches which I have made especially for you, during the last few days since your letter arrived, show most of the instances that I have observed, but there are a few others in which the asymmetry is not constant. *Staurestrum* ornithopodum is portrayed by Wests as quite symmetrical in top view, and I have many specimens just like their picture, but in other cases the processes show a decided clock-wise curvature. Undoubtedly you have noticed that in many species of *Xanthidium*, like *X. armatum* and vars., and *X. antilopaeum* and vars., the group of apical spines or processes is not symmetrical with respect to the vertical plane; this occurs also in *X. subtrilobum* and var. *inornatum* Skuja. In some Indonesian specimens of *Xanthidium sansibarense*, one of the spines in the two lateral pairs is missing, making the top view asymmetrical; this might be regarded as an abnormality, but the condition is not infrequent in certain collections.

In *Cosmarium* asymmetry occurs very seldom. In addition to *C. obliquum* I have seen it in some varieties of *C. pseudotaxichondrium*, and in a Tasmanian plant which is probably the same as Playfair's *C. stenotum* v. *spirale*.

In *Euastrum* the condition is more frequent, as you will see from my sketches. In some of the more highly developed forms of *E. verrucosum*, the apical lobe seems to be slightly twisted with regard to the vertical plane, or the ~~three~~ four lobules of the apical lobe are unequal in size. For an illustration of this see Scott & Presc., *Euastrum* II, 1952, Pl. II, Fig. 6. Also I have a new variety of *E. turgidum*, probably the most highly ornamented desmid known, in which the same condition occurs.

Micrasterias foliacea has an asymmetrical arrangement of the apical 'horns', also a decided curvature of the lateral lobes as seen in top view; this is shown in one of my sketches of a new variety, var. 'quadrinflata', and also occurs in the specific form and var. *ornata*.

Some species of Desmidiium show a slight but unmistakable asymmetry in end view. I have shown three species in my sketches. In *D. Grevillei* also, the end view is sometimes slightly asymmetrical, the protuberances at the ends of the long axis of the ellipse are very slightly twisted counter-clockwise. *Spondylosium pulchrum* is asymmetrical in end view, in all cases that I have observed, and shows a remarkable similarity in this respect with *Streptonema trilobatum*. Not only are these two alike in shape, but also in the shape and ornament of the zygospore, and in the formation of the zygospore in a much inflated conjugation tube, and in the manner of vegetative division. I have commented on this in a paper now in press "Some FW Algae from Arnhem Land", and have suggested that perhaps *Str. trilobatum* should be transferred to *Spondylosium*.

You are at perfect liberty to use and publish any of these sketches. Some of them are new and undescribed species or varieties, so it would not be proper to use my tentative names, even in quotation marks; these names further are subject to change by Prescott or Grönblad, who might prefer different ones. But you will know how to handle this.

Here's another example that I have just thought of: *Spinocosmarium quadridens* and its varieties, Sc. & Pr. 1949. Note the unequal lengths of the two spines in the apical pairs, and the deflection of the lateral spine from the long axis of the ellipse in top view. This plant should have been placed in *Xanthidium*, as I think I have mentioned to you before.

Well, all these examples together with your own list ought to make a good paper, and I shall be especially interested in reading your opinion or theory as to the possible causes of constitutional as well as what might be called accidental or occasional asymmetry.

Sorry that I cannot appreciate your remarks about geology, I don't know anything about the subject; but I am glad that you have something important and interesting to write about.

Hannah has sent me a reprint of her first part of Alaskan desmids; very well written and nice illustrations. But I wish she would draw the desmids symmetrical as most of them are; some of her pictures are misshapen and distorted. When I was in Hanover last year I told her about a reflecting device that I have used occasionally for making exactly symmetrical drawings of such difficult things as *Docidium undulatum* and *Pleurotaenium burmense*, which are extremely hard to make appear symmetrical when drawn freehand. With this device you draw the left-hand side of the object first, then place the reflecting glass on the center-line, and by looking obliquely through the glass you see the reflected image apparently projected onto the paper, where it is easily copied. Hannah said that she had not thought desmids were perfectly symmetrical, but the fact remains that most of them are so, exactly. She also showed me some of her illustrations for the botanical textbook; very fine indeed, I do not think they could be improved on.

Well, I must bring this letter to a close, and go looking for the chloroplasts which Grönblad needs to complete some of my drawings. I don't like this job! My eyesight is keen enough for the shape and surface details, but it seems to fail me when it comes to deciphering the details of chloroplasts. And many of my collections, particularly those from far-off places, are in a poor state of preservation, with the cell-contents greatly deteriorated and the pyrenoids "gequillt", to use Krieger's word, which seems to mean 'liquefied' or 'dissolved'. We have a very large new *Xanthidium* which has 4 sector-parietal plates like your figure ~~Fig~~ F6p, and also similar to the 4th sketch in your fig. 31, but each of the 4 plates seems to have 8 or 10 scattered pyrenoids just beneath the surface, but I have not yet found a specimen well enough preserved to be certain of what I have seen, or think I have seen. Such an arrangement would be unusual, would it not?

Best regards and good wishes from your best friend in the USA.

Dear friend Scott!

Linköping 20/III/1955.

Many thanks for your letter all parts of which are very interesting, especially the latter is very joyful.

At first your planned visit. It suits me as the best if you will come about the 16. of June; in the first half of July I must be in Aneboda for my duties at the Limnological institute. I am sure that we, at least I shall have the very best proceeds of our personal meeting after eight years' correspondence. We can chat on desmids and look at my library, eventually also make a trip to Tebo before we drive to Stockholm. You must spend at least one day there, it is a wonderful town and well worthy of a look. In Stockholm you are welcome to live together with me. at my son-in-law and daughter, you will find them just as charming as they are, though, I fear, their English is not good, they always go to Italy instead of London. Then I must see you off on the steamer to Finland. To-morrow I shall send you prospectus of the steamer-lines London, Gothenbourg and Stockholm Finland.

Of course I am thankful for every discussion of Actinotaenium. By this I mean the points which I have not explicated sufficiently or in an understandable manner. I think, however, that the primary reaction of the desmidiologists depends on their inveterate habit of using the classic genera. In a smaller degree this is not valid for those who have their bent of mind open for the artificial extensiveness of such genera as e.g. Cosm. and Staur.

I am sorry that I omitted some details which ought to have been taken too, but the last retouch of the paper was made just before my entering the hospital, the paper must be printed in December. The one is the term punctate (punctae). This is used in two senses 1) a pore, 2) a granule. For the earlier desmidiologists with their bad lenses, a granule could give the impression of a small pore, probably because it acted as a small lens. In the cases where the protruding punctae, as we know, really were mucous, i.e. Lütke Müller's pore-organelles, the concept pore and granule coincide. There are, however, small granules in Penium that are built by the wall, i.e. the rows of extremely small granules, the size and density of which are specific characters. The occurrence of these pores in Penium is probable but until this is clear, the real nature of the actual specimens must be proved by careful examination of the structure of the cell-wall. The deciding demonstration is the manner of division. If you state intercalary bands (Gürtelbände) it is a Penium, but also if you find a series of dense thickenings round the middle part of the cell, each thickening formed by one division of the cell. The fissure of division is not constant, a new one is formed by each division. The similarity of shape and chloroplast between Penium and Actinotaenium makes it impossible to distinguish the genus merely by the aid of these characters. The small granules in Penium are also demonstrative, they are visible almost exclusively by staining of empty cells.

I have confined Actinotaenium to the smooth-walled species, partly according to the type species Act. curtum, but, above all, because of the fact that I am convinced that the granulataebaculifera species belong to a quite different line of evolution, deriving from cosmarioid ancestors. I have studied these species penetratingly: some of them are quite circular a vertice, but there occur also oval ones, even with dicentrical chloroplast (Pseudococconeum). Most of them have a distinct, sometimes deep and narrow inthmus furrow, a circumstance that at once proves their belonging to Anguloradiatae. The elaborate cell-wall indicates the same. I am sure that they are pseudo-omni-radiate and their similarity to Actinotaenium is a case of convergence, a phenomenon that is fairly common just at desmids of this very simple shape; the baculiform shape is a manner found by many originally different desmid genera.

I do not like authorities, but I must confess that I consider my opinion well founded. Ever after my youth I have been very interested in evolution and I have during my long life made extensive studies of it and taxonomy, i.e. our efforts to combine the groups of organisms in accordance with their relationship (often quite different to similarity) and, consequently the methods of distinguishing the different kinds of relationship. The best possibilities are to be found in zoology, where the organisms possess a

manyfold of organ-systems, giving numerous factors for connection. Also the higher plants give good possibilities in their structure of the flowers, external and internal. It is a pity that the pure desmidiologists, as a rule, do not possess the profound all-round biological education necessary for every taxonomist, on the assumption that he has the synthetic as well as the analytical sense. Consequently, they get out of hand all the taxonomical principles which must be foreign unfamiliar to their line of thought.

Of course, my arrangement of the many species, varieties and formae can be discussed, in several cases it is a question of taste and many desmids are very badly described. I am quite aware of, and had to choose between several alternatives, but I could not publish more that the one that seemed me most probable. When I made the paper, I hoped that it could incite the displeased desmidiologists to complete or correct our knowledge in order to prove that I am wrong; in this way truth will come to light, no matter that I am blamed, nevertheless I was the "provoker".

AEROGRAM

GÖKANT AV KUNGL. GENERAL-
BEHÖRSTJÖRNEN
ÖVER HJELA VÄRDE

Par Avion
Luftpost

Mr Arthur M. STEOTT
2824 Dante Street

NEW ORLEANS, LA.
Louisiana, U.S.A.

AVSKICKARE: **EMAR TELING**

Lektor
Klassiska språk, 10
LINKÖPING

OSTERBEHOVDAAS IKKE SOM AEROGRAM. OM NÅGON TACQERS INDI

ÖRNAS MÅR

As to your question on Whelden's *Penium margaritaceum* var. *papilliferum*, it is not quite sure that it is a *Penium*. The papillae are, no doubt, mucous, compare my trial with *Abtinotaenium palangula* var. *silesiacum*, p.405.

It is really extremely kind of you to send me the samples, previously in the care of Krieger. You may be convinced that I appreciate it very much, it is a pleasant change to look at the luxuriant tropical algae instead of the temperate one, how interesting they, nevertheless, may be from the view-point of the variability. Who knows, perhaps I can find something that has escaped your sharp eyes!

Now, I think I have made the most of the space of this aerogram, thus I have better to set point, with my very best greetings to you and your wife, Your best friend in Sweden

Emar Teling

April 22 1955

Dear friend Teiling,

Many thanks for your interesting letter of March 20th, and for the steamer schedules which arrived a few days ago. Our trip is now all settled and I am going to fly from London to Stockholm, via Amsterdam, arriving in Stockholm on June 20th at 4.00 PM on Flight 552 of the Swedish Airline; and I leave Stockholm for Helsingfors on June 25th at 12.00 noon on Flight SK 750.

On my arrival in Stockholm I propose to go directly to Linköping, and I shall have a representative of one of the Tour Companies meet me to help me through the Customs and put me on the train or autobus. Will you please reserve a single room with private bath at a good hotel in Linköping (perhaps the Frimurare ?) that is not too far from where you live. I appreciate very much your offer that I could stay with you at the home of your son-in-law and daughter in Stockholm, and I hope you will not think me ungrateful when I say that I should really prefer to stay at a hotel. I am quite sure that they are just as charming as you are, and I shall be delighted to meet them.

My principal object in going to Sweden, in fact almost the only one, is to make personal acquaintance with you, and I shall leave it up to you as to what we shall do. Of course I wish to see your library and particularly your iconotheque, and, if we have time, to check some drawings of Brazilian desmids that I am taking to Grönblad. If there is a possibility of meeting Prof. Skuja I should like to do so, for I understand that he may be the one who will undertake the completion of Krieger's unfinished monograph.

My addresses in England will be:

May 12, 13, 14, and May 25, 26, 27, 28, Mount Royal Hotel, Marble Arch,
London, WC 1.

May 29 - June 29, % Mrs. E. M. Nixon, "Greenhow", Roundham Gdns.
Paignton, S. Devon.

Between May 15 and May 24 we shall be on a tour of England and the southern part of Scotland. This will take us through the Lake District and we stay for one day at Grange-on-Sands, at the south end of Lake Windermere. I am going to try to leave the tour on that day and go to Wray Castle, to see your friend Dr. Lund. I have exchanged a couple of letters with him, and he has been good enough to send me several of his papers.

Hannah wrote me that she and her mother may go to Scotland during July or August, and that there is a faint possibility that she might make a quick trip to Sweden to see you and your wife, because she is so fond of both of you. A very nice compliment!

I am not writing any more about Actinotaenium at the present, but hope to continue the discussion when we meet in June. Until then, and with best regards to you and Mrs. Teiling,

Your friend,

Mch 8 1955

Dear friend Teiling,

Thanks for your letter of Feb 17th, and the reprint of your paper on Actinotaenium. I have already read the paper twice, and parts of it several times, but it is going to take a lot more study before I shall be able to assimilate it. Though you had mentioned the paper several times in your letters I did not what the subject was, for the name Actinotaenium did not register with me, and I thought it was something else than desmids. In fact I looked in Prescott's large book, and Smith's FW Alg. U.S., but could not find it. Of course if I had looked in the old reliable Nordstedt's Index I should have found that it was a sub-section of Dysphinctium! This shows how much I don't know about desmid history. If I had realized that you were dealing with the circular Cosmaria I could have sent you some drawings that I think would interest you. For instance, what would you think of a Penium rufescens with 16 shallow longitudinal corrugations starting just above the isthmus and dying out at the apex? This plant is so close to P. rufescens in size, shape, color, punctuation, that I proposed the name var. corrugatum. Grönblad, however, thinks it should be made a new species because of the unusual wall structure which is different from anything that I know of in desmids.

There are several things in the paper that puzzle me, and if I start asking questions no doubt I shall merely reveal my ignorance. You lay considerable emphasis on the fact that Penium is frequently punctate but has no large pores. But what is the difference between a pore and a puncta? Isn't it just a question of size? And since both of them are too small to measure, the distinction between them must remain a matter of personal opinion. I try to discriminate between them, when I can, by noting "porose", or "porose with smaller punctae" as in C. zonatum, for example. But what I consider a fairly large pore in a small desmid, I might consider as a puncta in a much larger one. And true pores, as in C. difficile and C. zonatum, may be so small that sometimes I cannot see them at all in spite of my utmost endeavours, though I know them to be present because I can see the internal papillae in optical section. It seems to me that both pores and punctae are simply different sizes of one and the same thing, namely, holes through the wall affording a communication between the inside and outside of the cell, to afford a passage for liquids and gases. What do you make of Penium margaritaceum v. papilliferum Whelden (1942)? I have seen a single example of a plant that I take to be the same as Whelden's, and I am fairly certain that the 'papillae' are mucus exudations; if I am correct then the holes from which the mucus is exuded would have to be true pores. Nicht wahr?

I do not understand why you confine Actinotaenium to the smooth-walled species, as opposed to those with sculptured walls. What happens to such cylindrical forms as C. elegantissimum, C. mansangense, C. inornatum, and such bigloboid ones as C. striolatum, C. glyptodermum, and Dysph. Lagerheimianum? C. suburgidum (or capax) and C. striolatum look very similar in all respects except the geometrical wall ornament in the latter, and to place them in separate genera merely because of this difference seems to me a very unnatural arrangement.

I can easily imagine that this paper has been a very difficult one to write, and that many persons will not appreciate the tremendous amount of work involved in trying to untangle the many obscure and incomplete drawings and descriptions of the older algologists, and to rearrange the species in a logical and comprehensible order. My comments are not in anyway as criticisms, which as you say are very easy to make, but simply requests for information on points that I do not understand.

Since you are now working on M. mahabuleshwariensis, I may mention

Linköping 17-II-1955

Dear friend Scott!

Many thanks for the letters, your previous and my last letter met on their way as has happened several times. I can hardly believe that almost 2 months have elapsed since then. - I am sorry that I missed your birthday, I have noted the date on the back side of your portrait, but I have, regrettably, an extremely bad memory of siphers: birthdays, phone numbers, prices etc I forgot ~~the~~ birthday two months after our marriage, she was in Stockholm and I at my sister in Western Sweden. My sister remembered it late in the ~~xxx~~ evening and I saved my head by means of a very late telephone conversation.

I agree with your opinion that a man is quite as old as he has the feeling of. After the operation I felt old but now I begin to feel young.

In May I got a letter from professor Starr in which he told me about his research of Cosm. botrytis. In the cultures he has found the 3-, and ~~xx~~ even the 4-radiate facies and he is hoping to get spores and geminating products of the different facies. Lefèvre has found the 3-radiate facies of C. ochthodes in his cultures, a desmid previously found by Schmidle and by him called Staur. alpicolum.

I have examined the interesting material you kindly sent me from Sumatra but as yet not found any specimen of the ardently coveted Euastrocer-cus. I am still hoping for success.

Many thanks for the fine coloured pictures, not at least my grandchildren were fascinated of them. I should have money enough to visit America thoroughly. As an old and trained geographer (I once wrote a textbook of physical geography) I am sure that I should make immense proceeds of such a trip. Many thanks also for the "meteor-paper" as it is called here. The origin of it, the floating layers may be composed by several filiform algae, in the phytogeography the Swedish name "flyt-ävjä" (flyt= floating) is internationally used. Your sample is now enclosed between glass and I shall give it to the botanical museum in Lund. I am to spend the end of April and the first part of May in Lund. Last year I witnessed the most marvellous wonder of spring there. Within a few days the beach forest changed from black to yellow-green and the soil was covered by the gaudy carpet of spring flowers. A friend of mine there permits me to dispose his car and I made excursions every day alternating with the often disappointing work with Nordstedt's slides. I do hope that the climatic conditions will be quite a good this time.

It was very interesting to hear of your visits at diverse phycologists. From Hannah's enthusiastic description of Prescott, I think that Taylor is his very contrast. In my correspondence with him he at first showed a very condescending manner that was almost offending. Brunel seems to be very amiable, he sent me Irénée-Marie's great flora and I have tried to revenge by sending him papers and portraits of old phycologists. Irénée-Marie seems to be a fairly eccentric person. I found his pictures of the chloroplast too "subjective" and sometimes incorrect or misleading and asked him about them. He admitted that he draws chloroplasts only in order to fill out the emptiness of the desmid contours, but he promised to devote more attention to them after having read my paper. He sometimes end his letters to me with the words: "your best friend in Canada" which I apprehend as a slight sign of humour at the same time an expression of his amiability.

Hannah is a very lovely girl. At her visit here we learned to appreciate her very much, even my wife who is somewhat retiring towards new acquaintances, fell for her charm and ingenious appearance and my poodle who has a sensible nose for really attractive personalities, jumped in her knees very soon, a very rare behaviour of him. Hannah xxx and we had a good and correspondence.

I understand that you are intensively revelling the rich collections from Lake Ambadi and the Gazelle river. It seems as if your patterned "Arthrodesmus" may be a Xanthidium; there are species of X. that possess only two spines, Staurodesmus (= the former Arthrodesmus p.p.) has a smooth cell wall.

During the Actinotaenium period microscopic studies have been very scarce. Now I have "peeped in the tube" for the little paper I mentioned just made

Jan 25 1955

Dear friend Telling,

Today was my 67th birthday; yours will arrive in a couple of months. Newspapers refer to people of that age as 'old', but I don't feel old, do you? In spirit that is, though my physical capabilities are not what they used to be.

By this time I hope that you have entirely recovered from your operation, which must have been a severe one, I think, and that your ulcer trouble will be entirely eliminated for the future. One of the penalties of increasing age (not old age) is the derangement of the body's internal mechanism in many ways. Two or three of my organs are not so good as they might be, but I refuse to worry about it.

I have arranged with Sachlan to get more of the material from Sumatra containing the funny Ichthyocercus-like desmid, and perhaps to send some living material to Dr. Paavo Kallio so that perhaps he can cultivate it and find out something about its reproduction processes. He expects to make the trip in the next few months, and I have asked him to make as many collections as possible from different places in the swamp, in the hope that the desmid may be more abundant in one of them.

A week or so ago I received from Dr. Richard C. Starr, Dept. of Botany, Indiana University, Bloomington, Ind., two papers concerning his discovery of a sexual strain of C. botrytis v. subtumidum, which habitually reproduces by sexual conjugation, while other strains of the same plant only propagate themselves vegetatively. In a letter he says that he has since isolated sexual strains of another Cosmarium and two species of Closterium. This is an entirely new concept to me, and I think it will be to everyone else. If you have not received the papers you should ask him to send them.

For the Christmas holidays my wife and I went to Florida, this time driving all the way down the peninsula to Cape Sable, in the Everglades National Park. At this extreme southern tip there is nothing but mangrove swamps, useless for desmids because the water is brackish, but with wonderful fishing (I am not a fisherman), and some of the rarest birds on the American continent (I know very little about birds). I made some collections on the way back, but the only novelties I have seen are a couple of varieties of Micrasterias that are new to me, though I believe they have been found before. On the east coast of Florida, between Daytona and St. Augustine, there is an establishment named Marine Studies, which has the most wonderful exhibition of marine fishes in the world, all the way from sawfish, sharks and porpoises down to tiny gorgeous tropical fishes only an inch long, and including octopuses (octopi?), sea-horses, lobsters, sea-anemones, corals, etc., all living under natural conditions in huge tanks (except the smaller ones which are in the usual glass aquariums). Around the bottom of the large steel tanks there are many glass portholes, through which you can watch the various species close-up, frequently within a few inches. Some of the smaller fish are eaten by the larger carnivorous ones, but the stock is constantly replenished by two fishing boats that are continually working. A most fascinating exhibit that I have seen many times. The porpoises are so intelligent that they have been trained to jump eight or ten feet out of the water to take a fish from an attendant's hand, and one in particular jumps through a paper-covered hoop three or four feet above the water. This one is now being trained to pull girls on water-skis!

The enclosed piece of brown paper is quite a curiosity. It is a natural paper formed by dried algae, in the following manner: A cotton field in north Louisiana containing the dead cotton plants was overflowed to a depth of four or five feet by water from a nearby river. The water remained for several weeks, perhaps months,

and an abundant growth of filamentous algae took place. As the water slowly drained off
sif ~~cuspevstpttdg~~ the ~~thesabgaalgeewraptad~~ fast ~~fastioned~~ over the ~~dead~~ cotton
plants, and also on the wire fences surrounding fields when the ~~theadige~~ they they
~~forerch~~ sheets of 'paper', at first greyish white in color, later turning to
brown. This was not a small phenomenon; the field covered many acres, and the whole
area must have been measured in square miles. If you place a small piece under the
microscope you will see that the structure is quite like that of lens paper. The only
recognizable genus is Oedogonium, but this and Spirogyra are present only in small
quantities, the principal component being Tribonema. This sample was given to me
by a friend who comes from that neighbourhood, and the occurrence was described with
photos by G. F. Moreland, Am. Jour. Bot. 24:9, 592-593, Nov. 1937. The sample is
therefore more than 17 years old. I have seen similar green sheets of algae draped
over water plants and dead branches, but have never seen the dried sheets.

I am still working on the Sudan material sent by Grönblad, and now have
about 150 desmids from this single collection, some of them the weirdest things you
could possibly imagine. But it is most exasperating stuff to work with; in many cases
I have seen only one or two or three specimens, and as you know it is seldom possible
to get all the information or to draw all the views from such a small number. For the
last couple of weeks I have been using a solution of sodium CMC (sodium carboxy-methyl
cellulose), more viscous than glycerine, and which has the advantage of being self-
sealing, so that no evaporation takes place and a mount can be used for several days.
The high viscosity helps in balancing the little devils on their side or end, though
sometimes it works in reverse and the specimens persist in drifting. When that happens
I add a little CMC around the edge of the coverglass and let it stand overnight, until
they have quitte down. I have now received a larger supply of the material, and I will
send you some, but you must not expect to find these strange critters all at once.

We are expecting to make the European trip this summer; if we go by boat
we shall probably leave in May, or in June if by air, and of course I shall go to
Sweden to see my dear friend Einar.

With best regards to you and your family, from Edith and myself,

Sincerely yours,

Linköping 16/XII/1954.

Dear friend Scott!

Our latest letters met in the air, as has happened several times, I think we can call it telepathy, cannot you? Since then I had an interesting experience month ago, and you have, too. I heard from Hannah that you have made your planned trip to Prescott, Brunel, and Hannah. I hope that the long trip did not leave any bad consequences! As to me, I was reminded of my stomach ulcer when working eagerly in order to finish the Actinotarium paper. This done, I went to my son-in-law for vacancies and to let him treat the ulcer. At first he thought it could be cured by medicine, but when he accidentally witnessed my spasms, he sent me to a Röntgen doctor for intensive examination. I returned home and some days later he telephoned that he had arranged everything for a necessary operation. He had chosen a celebrated surgeon who has no failure in his long list of cases. I got a respite of a fortnight in order to take part of our concert and the 23th of nov. the rearrangement of my stomach and adjacent intestines was done, in the presence of my son-in-law. It was a great success, the third day I took ^{in the room} promenades and in the 6th day I could sit 4 hours at the desk for reading the proofs. Some days later I was transferred to the house of my son-in-law and a week later I went home to Linköping in order not to be completely spoilt. I have been occupied with the proof-reading but I am afraid that there are still several errors.

Just now I am lacking force and also energy and I am longing for them both. Moreover, it is hardly an encouraging time for convalescence; only six hours' days and usually very lousy. But, after Xmas the sun will rise higher and the days become longer, then the zest for life will increase.

I hope that you have succeeded in your efforts of stimulating Sachlan to extended collections in order to increase the knowledge of your special desmid and to find more novelties, ^{and} why not, another new genus. I am glad that you apprehended my utterances about the name of the new genus as they were meant.

Now, I must beg you to excuse my chat and lamentations, the real intention of this letter is to wish you and your wife
A Merry Christmas and A Happy New Year
the latter including a fruitful trip to Europe and, last but not least, to Sweden and to your friend. My wife unites herself with me in sending all good wishes.

→ Einar Feltius

Dec 15 1954

Dear friend Einer,

A month ago we returned from our vacation, and since then I have been busy with many things.

First we drove to Michigan, and spent a week at East Lansing, staying at a beautiful hotel on the campus of Mich. State College. The Prescotts treated us wonderfully, and we thoroughly enjoyed our visit. Most of the daylight hours I spent in Prof. Prescott's office, talking shop, and I had the opportunity of using his iconograph, which seems to me to be very complete but arranged alphabetically, so that *St. O'Mearii* comes next (or close to) *St. Ophiura*. This is fine if you want to look up a desmid whose name you know, but when you have a bunch of totally unknown desmids, as I had, there is nothing else to do but wade laboriously through each genus, hoping to find something that looks like your drawing. It would take the better part of a week to go through the *Staurastrum* section alone, if it is done with anything like thoroughness, and I simply did not have the necessary time. I did identify some of my drawings, but there were a number that "passed through the sieve" as Prescott says, and they remain as probable new species. One Sunday we visited several small lakes about 50 miles from Lansing, including one that has a "quaking bog" margin. This is a floating mat of *Sphagnum* and other vegetation, that sinks as you walk on it. I would never have adventured on it if the others had not gone first, and even then I did not like it much, because of my game ankle which is still not too strong. But I got nothing worse than a "dunking" in very cold water, by stepping into an unsuspected hole that was deeper than my rubber boots.

On our way to Detroit I stopped in Ann Arbor and had a short chat with Dr. Wm. Randolph Taylor, whom I think you met in Sweden. He was very pleasant, but not so easily approachable as most other biologists. He is the one who introduced me to Prescott about 15 years ago, and I met him a few years ago at a meeting in New York.

Then we crossed into Canada, and drove through Toronto to Montreal, where we stayed for a few days. Of course I called on Dr. Jules Brunel, whom you also know, I think. He has a beautiful place to work, a nice new building, a spacious office with a private secretary, a large laboratory and a beautiful Zeiss microscope that I would like to own. Unfortunately his administrative duties as Director leave him little time for research, but he has been working on some marine algae for a volume on "Algae of Canada" that the Institute hopes to publish at some future time.

I had expected to go from Montreal to call on Frere Irene-Marie, who lives about 90 miles away, but the weather turned bad on the day we left Montreal, with heavy rain, and predictions of more rain and snow for several days. I don't like driving under such conditions, so I abandoned the idea of going to La Pointe-du-Lac, and turned south instead. Irene-Marie seems to be a peculiar kind of duck from what Brunel says, and apparently Brunel is the only one of our circle who knows him personally; nobody else whom I talked to has ever met him.

So we headed for Hanover N.H., to see our mutual friend Hannah. We got there early in the morning, and I had expected to spend only an hour or so with her, but she talked so persuasively that we stayed the whole day, and spent the night at another nice hotel on Dartmouth College campus. I think she enjoyed the visit; I know I did; we talked a lot of shop, and I showed slides of some of my curious desmids to a few botanists whom she rounded up, but I think she was the only one interested.

She has adopted my idea of separating the mirror of the camera lucida from the microscope, to get a much larger drawing, 50% larger than mine, but far too large I think. A plate of such drawings would be as big as a table-top!

Univ.

Then we went to Boston, passing Harvard/~~College~~ on the way, where I stopped to make the acquaintance of Dr. Lamb, Curator of the Farlow Herbarium where duplicates of all my collections are deposited. Some of my earlier ones had dried out, and he kindly agreed to have them restored by adding water and glycerine, and sealing the caps more effectively.

We by-passed New York, for it is impossible for a stranger to drive a car there, and I don't much like the city anyway; too crowded, too expensive, and too hard to get around if you can't walk very far, like me. So we returned home by the most direct route, for the weather turned bad in earnest, plenty of rain, and even the first snow of the season. One morning in New York State we awoke to find the car completely covered with a glazing of ice, a new experience for us. We did not strike warm weather until we were within 200 miles of New Orleans. All told I drove 4200 miles!, the wife does not drive at all.

Well, that's my news. What is yours? Grönblad has sent me a small quantity of algal material from Lake Ambadi, on the Bahr-el-Ghazal, a tributary of the White Nile, in the Sudan. The most amazing desmids I ever saw, and as you know I have a lot of peculiar ones. Three new species of *Micrasterias*, of which two seem to be constantly dichotomical with different semicells. Isn't it strange that four of these critters should have turned up in little more than a year, Amscottia from Brazil, the Ichthyocercus-like one from Sumatra, and now these two from the Sudan; and all of them found by only two men, G. and myself. In addition to these addities, the Lake Ambadi stuff has at least a dozen other new species in Staurastrum, Cosmarium, Arthrodesmus, and Desmidium. The Arthrodesmus has somewhat the same shape as A. convergens, but much more heavily built, and with a beautiful hexagonal pattern of pits and granules on the face! Sorry I can't send you any of the material; I got only a minute quantity, but perhaps I can catch some of these funny animals on a slide and send it to you.

Best wishes for Christmas and the New Year to you and your wife, from Edith and myself.

Your friend,

Linköping 19/IX/1954

Dear friend Scott!

back

Just coming from a week-end at Tebo (= Teiling's bo, that means "nest") I found your letter on my desk and write at once in order to send you a ^{copy} picture of N. Carter's picture. My son-in-law is very fond of planting and has found that the garden of Tebo contains places where more apple-trees ought to be planted. He and my beloved daughter were there a day before me and had dug and planned eagerly. They have a little cottage south of Stockholm and are working there almost every Sunday as a contrast and refreshment of his daily work with sick people. I think it was not so convenient for my foot (I feel it now) but I could not sit quiet in my Ebo (= Einar's bo), my special little cottage, when the whole family was working, digging and shaping down apples from an old giant apple-tree.

I am very sorry at Rolf's stomach ulcer, he has not told me about it but I have understood that he was not all right from his letters; he has not been so eager to answer my questions. I have just sent him my films of Brébisson's original excellent pictures, I hope he will enjoy in examining them. Of course I shall send him an encouraging letter.

I am glad that you have got a more extended apprehension of your new baculiform genus. I think that the first found is a var. or forma heteropolare of the quite symmetrical, though certainly of great interest. Many thanks in advance for your sample that probably will arrive in a near future. I should be extremely glad, if I was one of the first to take a look on this interesting desmid, especially if I could be able to find the form with vertical spines.

Nellie Carter's *Euastridium staurastroides* is unique, curious that she has been happy enough to discover two of the four species of *Euastridium*. Evidently dear Nellie (who is a very bad answerer of letters) has found only one specimen and she has not been able to get it in the exact position for the true end-view. I enclose a copy of it, in which I have omitted the granules of the lower semicell. Your fine pictures show a desmid resembling Nellie's species. The differences: the much larger size, the wider sinus, the concave apex, and the more slender neck of the apical part, seem to entitle a nova species of the section *Triberculatae*. Nellie Carter's findplace of her species is situated in the Assam valley, about 500-600 km northeast of Calcutta. Her front view is not quite comparable to yours, that is seen from an angle of the lower whirl, 60° to the side.

You will have a fine ~~xxxxxx~~ time together with Prescott, (please deliver him my best wishes!). I suppose he has a fine phycological library and iconotheque. He must be an intensive scientist, Hannah Croasdale has told me about him with great estimation. Your journey implies that your leg is much better now, I do hope that the recovery will continue, if possibly in a more rapid time.

At last my best wishes to you and to your wife.

Your sincere friend

Einar J.

Linköping 14/IX/1954.

Dear friend Scott!

The air-post is a marvellous thing, your letter was posted in the morning 23/VIII and I got it exactly 3 days later. I am glad that your leg is becoming better and more useful. I understand it very well just now I have got a very disagreeable wet eczema that seems to be rather lingering. My left foot is badly affected, swelled up and must rest on a chair, the arms and hands are not so bad -as yet- my chin and even the auditory conducts are affected. The doctor wants me to rest in the hospital, but I must be at home, at least now, in order to finish my paper on Actinotaenium.

Well, I congratulate you to your fine finds. It must be wonderful to work with such a material where lot of new desmids wait for being discovered! For me, the activity is quite different: to bring order in the manyfold of often incorrect or, more often, incomplete diagnoses and pictures, interesting but sometimes hopeless. Sorrowfully, I must have it ready for print in a month. I am glad that Hannah is an interested and willing scrutinizer.

I am sorry to hear that you intend to use the name Sachlania. I am quite aware that you are very grateful to Dr. Sachlan but I must say that he ~~may~~ not be worthy of such an honour. In the desmidiology only Grönblad and you are immortalized, not e.g. Brébisson, Nordstedt, Ralfs, After West, Lagerheim, Borge, Kirchner and some others genera of Protococcales have got their names, where also other algologists are remembered, e.g. Bohlinia, Chodatella, Cohniella, Golenkinia, Hofmania etc. I am sure that Sachlan, how good scientist he may be, cannot be compared with Grönblad and you as desmidiologists. His achievement in the desmidiology will be quite satisfied and honored by a species name, Sachlanii. Do find out a convenient expressive name and do not inaugurate a manner of naming, so foreign to desmidiological traditional nomenclature.

I am sure that you will comprehend my uncalled-for intervention and my motive of telling you my opinion as a familiar information from a friend to a friend.

Thank you for the picture of Microsterias alata var. parallela. She is so pretty that you could use it her as a charming motive in your ex-libris.

Krieger's death is a terrible loss. Probably the publishing of his flora will be delayed, though Dr. Helmcke wrote to me that there are piles of manuscripts and proofs for one meter or more in thickness in his rooms. I suppose that you know that he was struck by apoplexy. I am sorry that I had not ~~yet~~ written to him about a new genus comprising some desmids the facies of which are hitherto allotted both to Cosmarium and Staurastrum. I have intended to attach his name on it, he would be well worthy of this honour.

I am very sorry that your "bamboo" sample for me has gone lost. Next time I think you will prepare the surface of the bamboo with a file. It must be a strange experience for you not to have used a summer for collecting. I myself am sorry that I am unable to exploit the present fine weather, a delayed summer, for excursions.

I just had a letter from a very angry Grönblad. As he probably has written to you the name Scottia is already engaged (peculiar that the phanerogamistic editor of Bot. Not. did not observe it) and your desmid must be re-baptized to Neoscottia. This seems to be applied on the good desmidiologist Scott; a new Scott, loaded with energy and ready for new and numerous discoveries has arisen from the bed of sickness!! I hope you are now working without being hindered by the hurted leg.

My very best greetings to your wife (= nurse and guardian) and to yourself

from your sincere friend

TELLING

Musö, Fjällbacka, den 20 July 1954.

Dear friend Scott!

Many thanks for the letter with splendid drawing and microphotos. Your new desmid is very curious and is, as far as I can see, worthy of being assigned to a new genus in spite of its similarity to *Ichtyocercus*, the dichotomy of the cells is very interesting and also puzzling as in the case of *Scottia mira*, I think the former is more worthy of a nov. gen. after ~~the~~ name than the latter. ~~which~~ It is a pity that the desmid is so scarce in your sample, seven specimens are a very small material for stating the definite constancy; it may be a local mutation of a species with symmetrical vertices, The insertion of the vertex must be considered as a generic character, as in the case of *Euastrum*. I suppose that the desmidiologists now, when their attention has been attached on this desmid, will find more of it. This is an experience fairly often seen in science,

If I would venture a prophecy of the structure of the chloroplast, I think it is most agreeing with that of *Tetmemorus*, *Ichtyocercus*, and other elongate pseudo-omniradiate desmids, i.e. baculiform and circular in cross-section. Thus, a pseudo-stelloid chlpl is most probable; a core with several pyrenoids (already stated) and longitudinal radiating lamellae.

If you excuse, I must say that names ending in -astrum are not applicable in this case, this means star and something star-like is not visible. Thus, the connection to *Euastrum* must lie in the prefix. The resemblance to both *Euastrum* and *Ichtyocercus* is well realized in, for instance, *Euastrorcercus*; the determining part of a composed word is the suffix, actually this desmid is most similar to *Ichtyocercus*. I do not understand the species-name bipolare, this is valid to all desmids except *Scottia mira*, heteropolar would do better, decidioides is good.

As a "soulagement" for your disappointment to rest in USA this summer, I can tell you that we have had a bad weather, coldness and rain as almost in the whole of Europe. I think, if you are not especially fond of coldness and rain, much of the pleasure would have gone lost for you. Next summer will be better, surely. I have been in Aneboda and Lund since last summer, rain and coldness! Now I am at my son-in-law at the West Coast, a district usually favoured climatically and especially this summer. My arrival, however seems to have a bad influence, rain and coldness! During this last month I have enjoyed warm and sunny weather, but 5 days! The first week in Aneboda the temperature in my room in the morning never exceeded 13° Celsius. = 55° F.

To day an astonishing fine weather. My son and daughter and two guests departed yesterday and consequently I and my wife are alone here with four children and an excellent servant-maid. My wife and I rudded(?) out in order to take up a net that had been lying some days. The result: two fine cods and - five great *Homarus*, you know the great crustacean animals, very delicate. Life still possess some pleasures!

Happily I have brought the figures, excerpts and the manuscript of my *Actinotaenium* investigation with me. I should like to have Hannah here in order to inform me about the marine algae, she is a skilful diver, and collector.

I do hope that your leg soon will be ready for use, it is a horrible feeling to be a prisoner, I know it!

At last the best greetings to your wife and yourself from my wife and your

sincere friend

Emar.

Sept 21 1954

Dear friend Telling,

I am terribly sorry to hear that you are laid up with eczema. This is one affliction that I have not experienced myself, but two of my close friends have had it, and I know that their doctors had to treat them for a long time before the cases were cured. Nevertheless, I hope very much that your case will prove to be an exception and that you will be able to get rid of the eczema within a reasonable time.

Well, your prognostication proved correct. The funny little desmid from Sumatra actually is a 'mutation' of something else, or rather of two other somethings. After more hours of tedious searching I have landed a single specimen of the fish with two tails, one on each end, and have him safely, though temporarily, shut up on a slide. From the enclosed sketch you will see that even though the two poles are alike, the two semicells are still different; the 'lower' one (in the position in which I have drawn all the specimens) is more strongly curved than the 'upper' one, and the peculiar curvature of the whole cell is common to all examples, with not much variation. In all previous cases of dichotomy that I have encountered, the combination form has always been much rarer than either of the basic types; in the present case I have found 13 and Sachlan has seen 10 dichotypicals, or a total of 23, against one each of what would seem to be the basic types. Another puzzle! I have asked Sachlan to try to get more material, and have offered to pay his expenses if he can find an opportunity of visiting the habitat at Benggala, which is probably no more than a day's journey by plane and auto. I suggested that he get 10 or 20 collections from various parts of the swamp, and separate squeezings from each of the three aquatic plants that he mentioned, *Nydrilla*, *Ceratophyllum*, and *Utricularia*, and any others that probably occur there like *Myriophyllum*, *Gabomba*, and aquatic mosses, for it is possible that each one of them might harbour a different association of desmids, and that the fish-tailed one might be more abundant in one than the others. So we have to wait and see what occurs.

I am not at all offended, but instead quite glad for your remarks on my proposed use of the name *Sachlania*. Prescott had also objected, not very strongly, because he said that some persons frown upon the use of patronymics; but he did not say who did the frowning, nor why they contracted their eyebrows. You, on the other hand, have explained the matter quite clearly, and I can now see that the honour should be reserved for persons who have made important contributions to the science (I hope you consider that I belong in that category). I had one good precedent in the genus *Burkilla*, named by the Weste after Mr. I. H. Burkill (Burma 1907), and this probably influenced me, for I have been studying all the literature that I possess on Indian and Indonesian desmids. Sachlan is an ichthyologist, with some knowledge of desmids and a great interest in them. Recently he has written to Dr. Paavo Kallio asking for information on their cultivation which as he is anxious to try, he may succeed, for he has the laboratory facilities and the time to devote to it.

Yes, Grönblad was much put out to find that the name *Scottia* is preempted. On checking he also found *Scottea*, *Scotia* and *Schotia*. So he wrote suggesting *Scottiella*, which he thought preferable to *Neoscottia*, and I replied that I also liked it better. A couple of days later I was glancing over some new papers from Japan, and I came across the name *Scottiella nivalis*, which apparently is a well-known snow-alga. I suppose that it would be undesirable to have two such similar names as *Scottiella* and *Scotiella*, though there are *Westella* and *Westiella*, the former belonging to the same group as *Scottiella*. So perhaps G. will have to go back to *Neoscottia*. Your interpretation of this last name is very nice and flattering.

Prescott wrote me that your friend Prof. Fritsch died recently; another great loss to algology. Miss Croasdale said that her visit with Fritsch was the

best that she had in England, and strongly recommended that I go to Cambridge to see him, which I had fully intended doing.

Now, in return for the good advice that you have given me on many occasions, may I offer a friendly comment on two words that you use not infrequently? 'Sorrily' is an unusual word; in fact I looked in the dictionary to see if it really existed. It does, but the dictionary defines it as "meanly, despicably, pitifully, in a wretched manner", which is not exactly what you mean. However, the way you use it, your intent is quite clear, and it adds a characteristic Tellingness touch to your always charming and entertaining letters, so please continue using it. However, Hannah is not a 'skilly' diver; she is a skillful one; the word skilly is not in my big dictionary.

You are quite correct that my wife is my nurse and guardian. As a small reward for the excellent care that she took of me this summer, and the extra work imposed on her, and in partial recompense for our missed trip to Europe, I promised to take her for an automobile trip somewhere, as soon as my foot was well enough. Prescott was in Mexico during August, and I invited him to return via New Orleans and offered to drive him over to Florida where the Phycological Society held its meeting early this month. But he was unable to come, so I have decided to go to Michigan and spend a week with him, to see how many of my strange desmids from Indonesia and North Australia he has in his iconography, and to get his assistance in determining them. So next week my wife and I shall start for Lansing, a trip of 1900 km. I hope it will go all right; for the last three weeks I have had a 'bobo' on my posterior that makes sitting rather uncomfortable, and driving a car all day for four or five days will probably do it no good. But I don't want to postpone it any longer, for bad weather will be starting sometime in October. Depending on how my 'bobo' behaves, and how the weather turns out, I have in mind the possibility of going to Montreal to see Dr. Jules Brunel, and Irénée-Marie lives somewhere around there; I can't find his place on the map.

So wish me well, and in return I hope for you as speedy recovery as may be. When you recover you will have to take your nurse and guardian for a nice trip somewhere. Tell her I said so, please!

With my best regards to both of you,

Sincerely your friend,

The Japanese papers that I mentioned are:

Les végétations algologiques des Shima, source chlorique et amère.	1950.
Les algues d'eau douce observées pour la première fois au Japon (1).	1951.
Ibid.	(2) 1951.
Ibid.	(3) 1951.
Ibid.	(4) 1953.
On the red and green snow newly found in Japan.	?
Studies on the Cryoalgae of Japan. Notes on <i>Oocystis lacustris</i> fa. <i>nivalis</i> .	1953
Diatoms Flora of Oze.	1954.
Desmids Flora of Oze.	1954.

The author (with collaborators on some of the papers) is Hiroshi Fukushima, Biological Institute, Faculty of Science, Yokohama Municipal University, sixth & 7th Kanazawa-ku, Yokohama. He asks for an exchange of publications. The first, ~~some~~ papers are written in Japanese, with English or French resumes, very brief. Mostly they are simply records of species found, with dimensions and some ecological data, apparently no novelties. The illustrations of diatoms seem to be well-drawn, those of desmids only so-so.

Sept 12 1954

Dear friend Teiling,

In your letter of July 28 you remarked, concerning the new Ichthyocercus-like desmid from Sumatra, that 7 specimens constitute a very small number for stating the definite constancy; "it may be a local mutation of a species with symmetrical vertices". I have now received a somewhat larger supply of the material and have found 13 specimens, all alike and with differing poles. But the fourteenth specimen 'upset the apple-cart' by possessing asymmetrical apices, just as you thought. Since this discovery I have been searching assiduously for more examples, but so far have not been able to turn up any more symmetrical ones.

From the enclosed sketch you will note that the poles of this 14th specimen are of the slightly elevated type belonging to the 'upper' semicell in my original drawings. It seems to me that there must be another type having both poles of the 'fishtail' shape belonging to the 'lower' semicell of my original drawings, and that the dichotypical specimens are combinations (perhaps hybrids?) between the two species.

The situation has me quite confused, and I don't know what to do with the peculiar little beast. It is still a new genus; there can be no doubt of that, but I shall have to abandon the specific appellation 'heteropolar' which I had intended to use. At the end of this month my wife and I are going to drive up to Michigan and I shall stay in Lansing for a week or so for a visit with Prof. Prescott, in order to use his iconograph and library in the determination of some hundreds of desmids from Indonesia and Australia. This will give an opportunity for a viva voce discussion of the problem presented by the new genus, which is far better than an argument by long-distance correspondence.

Last week I sent you a small quantity of this material, in which you will find a lot of interesting algae, and if you are lucky you might find one or two examples of the 'heteropolar' plant. If you have still more luck you might come across one with symmetrical poles, and if you do I wish you would make a sketch of it, or better yet, capture it on a slide if possible and send it back to me.

Another sketch enclosed shows a very rare desmid that I take to be Euastridium staurastroides. I do not possess Nellie Carter's illustration, but there is a diagrammatic top view in your paper 'Radiation of Desmids', which corresponds with my sketch. This plant occurs in another collection from the neighbourhood of Palembang, Sumatra; I am sorry the quantity is so small that I cannot send you any of it.

Grönblad has had a stomach ulcer for some time, that has worried him a good deal. Now he writes that the doctors say the ulcer is gone, but he is suffering from a 'hypersensibility' that is very unpleasant. I can appreciate this, for I have had a similar feeling of debility after my attacks of cholecystitis. Write him one of your cheery letters, please, and try to help 'buck him up'.

So long, and good wishes,

Sincerely,

Aug 23 1954

Dear friend Teiling,

Thanks for your letter written from Musé on July 28th, with the interesting comments on the new desmid genus from Sumatra. There have been a number of suggestions as to the proposed name, and I think I am going to adopt yours of heteropolare, making it Sachlania heteropolare, though Prescott seems to object to the use of patronymics. Just half-an-hour ago the postman brought me a letter from Sachlan, saying that he has sent me more of the material and that he has seen at least 10 specimens, all alike, and identical with the drawings and photos that I sent him. Let us hope that I shall be able to find some specimens with good chloroplasts, so that I can determine their structure.

Apparently you are correct about this summer's weather in Europe. Grönblad wrote about the bad weather in Finland, and my sister in Devonshire, right on the English Channel, wrote that early in July they had one day with the temperature down to the freezing point, and that afterwards they were miserable because of the continuous rains. Such a climate is no place for me or my wife; we are used to hot weather, and well-heated houses in the winter.

I don't know much about the lobsters (Homarus) that you get in your northern waters: they are not found down here, and the few that are shipped alive and by aeroplane from the State of Maine are so expensive that I have never bought one. Another kind of lobster, Homarus, is different from the one I bought in Florida, but it is about the same size but lacks the large front claws, which are replaced by very long 'feelers' or antennae. It is not so good as Homarus, and I don't care for it much.

A couple of weeks ago I sent you three samples of desmid material, for your enjoyment. They were contained in a section of bambo, a very convenient and safe way of shipping that was taught me by Minoru Hirano of Japan. But unfortunately I stuck the paper label with library paste, which refused to adhere to the highly glazed and siliceous surface of the bamboo, so the postman brought back the label to me, which is enclosed herewith. Where the bamboo tube is, goodness only knows. However, it is of no consequence, and I shall send you another lot in a day or two.

I wish to call your special attention to the Australian sample X-104, from Campbell in Arnhem Land, about 100 miles east of Darwin. I have seen many very rich desmid collections, but this is one of the most extraordinary. I have examined at least 50 slides from this lot, which would be sufficient to exhaust any ordinary sample, but just yesterday I found two new species of Euastrum on one slide, and I found another a couple of weeks ago.

Here's another example of my amazing luck! Microsterias alata was described by Wallich 94 years ago, and has since been found in eight different countries by a number of workers. Perhaps you have seen it in some of the material I have sent, and if so you will feel better acquainted with it than if you know it only from pictures. For myself, I know it quite well, for it occurs in several of my Florida collections, and I have also seen specimens from Brazil, Sumatra, Java, Borneo, and North Australia. In all these years no valid variety has been described (Turner's var. depressa is not a good one, as Krieger remarks). The biggest variation I have seen, until recently, is that between the two forms shown by Grönblad from Brazil, which also occur in one of my Brazilian samples. But in the Australian collection X-104 there occurs, very rarely, a truly astonishing new variety of which I am enclosing a sketch because you may not be lucky enough to find it. When I first saw it, I recognized at once that it belonged

to M. alata, but my first impression was that it must be teratological. However, this impression was contradicted by the exact vertical and lateral symmetry, and since then I have found three more specimens, showing that it is a true variety. Furthermore, in another collection from Unawalerke Lagoon, in the same neighbourhood as Campelli, I have found a new forma of M. alata, which retains the general shape but differs in having much wider sinuses that give it a quite different appearance.

Want another example? Micrasterias lux was described by Joshua 68 years ago, and since then has been seen only by Turner and Minoru Hirano (1943). I have seen plenty of specimens of the specific form from Indonesia, and this same collection X-104 contains an abundance of a new spinous variety. You will find many specimens, but you will have to look closely to see the tiny spines on both sides of the sinus, and along the margin of the upper lateral lobe adjacent to the polar lobe; no spines, strange to say, on the polar lobe except the apical ones, which may or may not be present.

Another example? Micrasterias Torreyi. I have seen actual specimens of both the species and all of the six named varieties shown in Krieger's monograph, and in addition another variety found by Sachlan in Java. None of these forms possess any spines. In this same collection X-104 I believe I have found a spinous variety, but this requires confirmation, because so far I have seen only one slightly damaged empty semicell.

No doubt you have heard of the sudden death of Dr. Krieger. This is a deplorable loss to all desmidiologists throughout the world, and even more so to me for I had hoped to meet him in person. His death must have been very unexpected; only two weeks previously he had writtch me about the new desmid genus, and expressed the hope that we could meet in Europe in 1955.

My ankle is getting much better, and I can now walk without crutches, though not for any considerable distance. The foot still swells after it has been on the floor for an hour, but my doctor says it may be gone in a few more days before the swelling ceases, but everything seems to an end in time.

With best regards from Mrs. Scott and myself to you and your wife,

Sincerely yours,

Linköping 29/V/1954.

Dear Friend Scott! I am very sorry that my answer is so extremely delayed. For the planning of my summer there has been several incertainties of the point of time; my teachership at Aneboda and also the summer-program of my son-in-law and daughter. During the summer we two and our three daughter-children live at our summer-cottage but we spend about July at our son-in-law who possess a fine house on a very beautiful island at the West-coast (he has built a fine little cottage for us there, too). This gives rise to transports to and from, and our own cottage has a very inconvenient situation as regards railway communications. Sorrowfully two of the children are ~~invaid~~ partially disabled and I must help my wife with the transports.

According to these factors, I propose the following plan. Since your discussions with Grönblad is the most important job and he is independent of considerations as to a family etc, you go directly to Åbo or Helsingfors where Grönblad will meet you. You spend your time there, and when your collaborations is as clear as it can be, you go to Stockholm and further to Linköping in August, when I am quite free from considerations to anyone ~~ni~~ else. You stay at me as long as you like. You have then cleared up the intricate things with Grönblad and, with a good conscience, you is able to enjoy your freedom. Afterwards, if you have any interest of seeing Sweden from another view-point than from the air, I drive you to Gothenbourg. From this town, on the West-coast, very fine steamers go to London, taking one night+one day, one night. You will arrive to London in the fore-noon. If you find this voyage takes too much time, you can take the aero Gothenb-London.

If you, like me, avoid expences that do not give you a corresponding advantage, I submit a list of the parts of your journey London-Karis and back with the diverse communications, time and ~~prices~~ cost.

London - Stockholm, by aero: 5 hours, I. class-481 Swed. Crowns, Touring class 410:- Cr. The difference is: no free alcohol and 20 kgrams bagage (30 kg. in the I class) in the touring class!

London-Gothenb. by steamer: 36 hours, II. class (cabin for 2 or 3) 195 Cr. III. class (cabin for 3 or 4), 140:-Cr. Very good, I have used it, the difference depends on the extremely fine meals in the II. class.

Gothenb. - Stockholm, train: 5-6 hours: III. class (very good) 35:-Cr. Stockholm - Åbo, by aero: 2½ hours, 89 Cr. to Helsingfors 132:- Cr. Karis is situated just at the middle of the distance Åbo-Helsingfors!

Åbo - Stockholm, by aero: 89:- Cr.

Stockholm - Linköping by railway: 3 hours, 22:- Cr.

Gothenb - London, by steamer, see above, by aero: I. class 377:- Cr, Touring class 316:- Cr.

I do hope that this delay of my letter has not given you any suspicion about my feelings towards you, it depends on factors beyond my possibilities to arrange. I assure you that I am seeing forwards to your visit with the utmost interest and pleasure. Besides the personal contact, I think you may have interest of my library and also my iconothèque.

It was interesting to hear of Prescott's mentality. I have observed his predilection for *Spinoclosterium* and *Spinocosmarium* and his selfsufficiency when he argued for his own *Staur. polonicum* var. *coronulatum* (instead of *Euastridium verrucosum*). I have spent three weeks in Lund studying Nordstedts library and archive. I also looks at a proof-sheet of Grönblad's paper on *Scottia mira*, the editor asked me for advice in a couple of questions. The number of *Bot. Not.* will appear about at Midsummer.

At your stay here we will have valuable discussions on desmids and desmidiologists, I do hope that you feel that you are so very welcome!!!!!!!

With my best greetings to you and your wife and wishes to a happy journey!!

Yours affectionately

Ernst J.

Linköping 26/VI/1954.

Dear friend Scott!

What a bad luck!! I deplore your broken tarsus bone, so little and so disastrous, with all my heart. I fully understand your disappointment of missing the planned personal collaboration with Grönblad and the valuable discusses with Krieger. Against this my own disappointment is too little to be mentioned, I am only one of the smaller prophets of desmidology, that is why I apprehend your intention to visit me as a sign of personal amiability that I estimate very highly.

In a manner, I am a pessimist, a convenient attitude to life: a pessimist is only met with good surprises! My pessimism, however, seems not to be as adrk as yours, though yours is understandable, since you, at least at present, look x on life through the dark goggles of the illness! You can be sure that the circumstances will be quite as favourable in the next year you will be able to realize your plans though you will be obliged to use the calendar of 1955 instead of 1954!

I do hope that the doctor's prediction of 4-6 weeks in the bed and several months' crutch-walking will prove too pessimistic. Fifty years old I broke my right key-bone when my horse broke down on a fence, it took four weeks for healing (a complete) and ten years ago I broke a metatarsal bone, it took three weeks and yet I was able to attend to my school-job. of course, the ability of healing is somewhat diminished in increasing age, but many doctors are anxious to keep a suffer longer time under treatment than necessary, for the sake of the fee.

I look forwards your pictures of the new desmid genus with the greatest interest, and I am flattered that you include me in the circle of your nearest "desmid" friends. I am so glad that your eager desmid work has given so fine results and that you, moreover, have had such a good luck in your so rich and virginous district of research. Actually, it is curious that the discovery of a new genus is regarded to be much more glorious than that of a new species. The difference lies in the material, a sharp eye and also the scientific intuition; Be' transpiration and cathodes are the same. I remember how proud (I was then a very young man, mentally) I was, when I found a new genus of Protococcales, viz. Tetrallantos. G.S. West was eagerly searching for new genera, he found three real desmid genera, but several of his creations have been mistakes, in certain cases terrible mistakes. He eagerly rejected my genus, possibly a sort of envy; he was always extremely critical against new scientists, for instance Lütkenmüller's first papers. West was the uncritical species-maker, extremely industrious but I was a genius, superior to all desmidologists of this century; in the past century I must mention Brébisson and Ralfs and the kings.

I am now in Linköping preparing my trip to Aneboda and the West-coast for a month's duration. To-morrow I shall drive to Aneboda, passing through the belt where the true shadow of the moon sweeps along the surface of the earth causing the total solar eclipse. Next time this occurs here only after 127 years, that is why I must use the opportunity. Probably there will be thousands of cars on my way, I think I have better to start very early.

I have not heard anything from Mr Chlorophyll (translated exactly = Grönblad) since long in spite of two letters, probably he has been pre-paring for your visit. There are many ex friends who regret your misfortune, the crack of the small bone extends his influence over the Ocean!

At last I can only hope that your recovery may run as quickly as possible, so that you soon can enter your laboratory and resume your work with our beloved small darlings.

Whatever you do, be cautious not at your promenades, in order not to break a hand, that would be even worse.

My best greetings to your careful wife and yourself from

your sincere friend

Ernst Sjöström

July 7 1954

Dear friend Telling,

Your cheery letter did much to dispel the gloom that I have been under for the last three weeks. Actually, of course, my accident is not a very serious one; it is just that it had to happen at this particular time, three weeks before the start of our European trip. Today we were supposed to arrive in London!

My foot and lower leg have been in the plaster cast for two weeks, and it must remain for two weeks more. Then the doctor will remove the cast, and perhaps take another X-ray picture to see if the bone has knitted. If everything is well I shall have to walk on crutches for two or three weeks more, and even after that I expect it will be a month or so before I shall be able to walk unaided for any considerable distance. It would be foolish to go to Europe on a pleasure and sight-seeing trip if I were unable to do a lot of walking.

But that's enough of my troubles. I am surprised that you think there is no more glory in the discovery of a new desmid genus than a new species. It seems to me that in addition to being a pessimist you must be rather blase; perhaps that is one result of 40 years of teaching the same subject to pupils many of whom are probably unappreciative and not too much interested. I too must confess to being a pessimist, and to a certain degree of 'blase-ness'; it takes something out of the ordinary to give me a kick. But I certainly get a thrill when I find a new desmid species, and a much greater one for a new genus. This is as it should be, for there are more than 150 times as many species as genera of desmids. I guess that I have something like 100 new species in my own collections from USA, and those that I have received from other collectors in Brazil, Australia, and Indonesia. And when you consider that both of my new genera possess the strange feature of unlike semicells, I think it is almost incredible that an amateur like me should have the amazing good luck to find them both within a space of nine months.

The enclosed sketch and photos will give you a good idea of the new plant. I am sorry that I cannot send you any material, for I have only a minute quantity, and specimens are extremely rare. Nevertheless I have found 7 whole cells and 2 detached semicells; all of them are alike, with only minor variations in size. The two ~~cells~~ semicells have different curvatures, different degrees of basal inflation, and especially the structure of the poles is quite different. All the chloroplasts in the collection are badly deteriorated and I have not been able to determine the arrangement, but I believe there are 5 pyrenoids in each semicell, in an axial line. The shape of the plant gives an obvious suggestion of Ichthyocercus, but the large intermeshing basal teeth, the two large mucus-pores, and the different shapes of the apices, are all features that belong to Euastrum, though the basal teeth also suggest Docidium. Prescott, Grünblad, and Krieger have seen my sketches and I also sent each of them a single specimen on a slide. When I can use my microscope again I will try to find another specimen for you. P. and G. agree that there is sufficient justification for a new genus, but both think it could be placed in Ichthyocercus. Here is Krieger's comment: "Nur zu der merkwürdigen Desmidiacee aus Sumatra. Sie haben wirklich grosses Glück, gleich 2 neue Gattungen zu finden. Die sumatranische Form hat den Habitus von Docidium und die Zellenden von Euastrum. Der Name 'Docideastrum' (which I suggested) erscheint mir sprachlich zu kompliziert. Vielleicht können Sie die Bezeichnung 'Cercastrum bipolare' oder 'Cercastrum docidioides' verwenden. Ich bin neugierig, was Grünblad dazu sagt".

My own opinion is that the single feature of unlike semicells is enough,

by itself, to justify a new genus, to say nothing of the other curious characteristics which do not fit into any existing genus. It will be written up by Prescott and myself, and published probably in 'Reinwardtia', one of the journals of the Botanical Gardens at Bogor (Buitenzorg), Java.

I suppose that the reason you have not heard from Mr. Chlorophyll is that he has been too busy corresponding with me regarding the large number of collections that I have sent him. During the last couple of years I have had many letters and drawings from him, and I have had several interesting arguments with him concerning some of the puzzling forms.

That's all for the present. Now I must hobble upstairs and rest my foot, which swells and becomes painful when I place it on the ground for more than a few minutes.

With my best regards and good wishes to you and your wife,

Sincerely your friend,

Just now it is fairly intricate to place this paper in the typewriter and take a copy of it. Besides, I am afraid that you may fancy me feeling myself unappreciated. On the contrary, 45 years of intensive teachers' work and studying literature and people I am too well aware of the difficulty of changing deep-seated opinions. It may succeed at young people, at aged, not.

I have offered some marine papers to Professor Doty, now on Hawaii. I was very imposed that our aerograms went the long way during only five days.

I am sending you two samples of the Isdlite under separate cover.

At last my very best greetings to Mrs Scott and to yourself from your friend

Emnar T.

OPNA:HR

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ÖVER HELA VÄRDEN

AEROGRAM

Mrs Arthur M. Scott

2827, Dante St.

NEW ORLEANS, LA.

Louisiana, U.S.



Linköping 5/III/1954

Dear Friend Scott!

Many thanks for the letter and the papers with extra pictures, some of which were incorporated in my iconotheque a few hours after their arrival. For instance your gracile, Oh this gracile! Every desmidologist perpetrates, once at least, most very often, the special sin against gracile. Your gracile, fig 5:1, is St. planctonicum, see Teiling 1946, f 30, 32. The two or four or even lacking rudiments of verrucae are quite different to the granulation of gracile, as it is amended by G.M. Smith 1924, p 88. The form of the cell of your gracile is quite typical and the spines of the ends are no specific difference. I must give you a special clapping for your impression that your gracile f. minimum should be compared with pingue. Actually it is pingue, a species very varying as many of the plankton desmids reduced from more elaborate littoral forms. The variability concerns the isthmal inflation and also the ornament(lack-

~~king~~ in your forma. Both these desmids are exclusive planktons and of about the same ecology, just as you have found them.

I am glad for your correct and elucidating picture of *C. cucurbitinum*. I think you have compared your *C. multigranulatum* with *C. mansagensis* West and especially its var. *africanum*, see Skuja: *Birna algae*, tab 27:7. The cells are there somewhat longer and the granules arranged in longitudinal and vertical rows.

Your photographs are very good considering the depth of the area that the object embraces. It is a pity that microphotos suffer from the small depth of focus, what an extremely fine aid it would be with a more favorable depth of focus! Now it is only to be used on the most flattened species of *Micrasterias*. You must have used a very small iris!

I am very grateful for samples from your rich "Archivum Desmidiacearum Scottianum". The few vials I have kept for me and not sent to Grönblad have given me much pleasure and valuable drawings for my *Staurodesmus* paper. I have also seen peculiar species of *Peridinium* in their content.

As to your new desk's surface, I must attach your attention on a board covered by "Isalite". It is used here (Swedish made) for purposes requiring extremely firmness and resisting power. I think I can send you a sample of it to use when you will recover your desk the next time.

As to my library, I am afraid that you have misunderstood me. Of course, I do not sell my phycological library as long as I am able to use it and after that I will try to sell it to an institution, where it can be kept as a unity, its value lies in the manyfold of valuable papers now not obtainable as abstracts. No, my sale comprises other books and reprints, botany, geology, geography and Arts. I possess *Wolle* 1884, I did not know of a second edition, but I must say, there are many pictures in my edition that need an improvement!

I am glad ~~that~~ to hear that you under all circumstances intend to visit me. I do not know anything about *Krieger's* linguistic ability, probably he is a real German. I must say that these giant congresses do not give, at least to me, the real profit proportionately the expenses. For professional academicians and people fond of international acquaintances, for phycogeographers and laboratory men who have a real profit of seeing selected vegetation or laboratories, it is a quite different story. Many attendants are genuine tourists who like to visit Paris and attend the solemnities. In 1950 most of the people were ecstatic of delight by being invited to a garden-party at a royal castle and to shake hands with a crown-prince (our present King). Well, crown-princes are very rare birds, only four of them at that time, except the coloured, of course.

Thus, the great botanical congress in Stockholm 1950 gave me very little of personal contacts, the more intime Limnological congress in England more valuable. The lectures are, as a rule, published and can be read with more profit than heard, often delivered by bad speakers in a foreign language. I think there will be thousands of attendants in Paris. I do not think you will have much pleasure of *Bourrelly*, he is a secretary and I fear that he will have very little time for the phycologists who he would prefer to discuss with. Even if I had plenty of money, I ~~don't~~ think I would go to Paris at the congress

when I can meet you anyhow. These are just my personal opinions,

take them from your own point of view.

I have been sick for some weeks, my stomach again! Have I told you that I am working out the *Clevei*-section, mentioned in my chloroplast paper, to a new genus. It is quite laughable that clear-thinking men do mean that for instance *cucurbitinum* belongs to the same genus as for instance *Botrytis*. May be that the principles are not clear, or have been clear, but there have been ~~no~~ efforts to find them. I know that the desmidiologists do not like new genera, they become irritated, they are accustomed to say and write *Staurostrum defectum* instead of *Staurodesmus defectus*, they maintain the "genus" *Arthrodesmus* in spite of its clear absurdity. They work with ~~genera~~ species, the genera are mere names necessary for the recognition of the species. I have heard that *Prescott* admits that there may perhaps exist something of value in my theories of desmid radiation and the phylogeny of the chloroplast. This seems to be regarded as a great progress of a professional phycologist.

April 12 1954

Dear friend Teiling,

Many thanks for your long and interesting letter of March 5th, and the two samples of Isolite. There are similar materials made in this country, but I don't know much about them except that they are rather expensive, and since they are made only in certain sizes you have to buy a much larger sheet than you require. I did not want to spend any money on the old desk, which was a gift from the company when I left them, so I merely removed the old varnish with a liquid preparation, sandpapered the top surface, and then applied a coat of clear lacquer. However, I should have done better to use a board similar to Isolite, for there were many scratches and cigarette burns that I could not remove with sandpaper. But at least it gives me a working space where I can spread out my drawings and reference works, and six drawers for storage.

My application to the National Science Foundation for a grant to attend the Paris congress was refused, because of insufficient funds. Just imagine! The USA is spending many billions (milliards) of dollars all over the world, much of it completely wasted in my opinion, but they could afford a total of \$5000 or \$6000 for this congress, enough to allot about \$450 each to 12 or 15 botanists. My cost for the trip to Paris, with field excursions but excluding my visit to England Sweden and Finland, was estimated at \$950.00. So I am not going to Paris for the congress, but the remainder of my trip remains as planned.

There was so much delay and uncertainty in trying to book a passage on one of the freight-passenger steamers from New Orleans to England that I have made up my mind to fly instead. So the plan at present is for us to leave New Orleans on July 5th, arrive in London July 7th, spend a week in London, and then to my sisters in Devonshire. About the end of July I shall ~~fly~~ fly (alone) to Stockholm and thence to Linköping to spend a few days with you, and then to Finland for a visit of about 10 to 14 days with Grönblad, and a day with Kallio at Turku, to whom I have recently sent a trial shipment of living desmid material. I have asked G. to let me know if the last week in July and first week in August will fit in with his plans, or if the first two weeks in August will be better. Also I should like you to tell me if the last week in July will be convenient for you. Please do not let me interfere with any vacation plans that you may have made; my schedule is somewhat flexible, and I could go to Finland first and thence to Sweden if that would suit you better.

I appreciate your comments on Staurastrum gracile, planctonicum and pingue in the South Australian paper, but your gentle reprimand should have been administered to Prescott, for these were his determinations, not mine. I did not agree with them and told him so, adding the remark "Teiling will not like this". But I am not sufficiently familiar with this puzzling group of Staurastrum to tell exactly where these specimens should be placed, and so I could not put up much of an argument. Prescott has a much higher standing as a phycologist, and far more experience than I have, so I must defer to his judgement. Also, as I think I have told you before, he is quite stubborn and adheres tenaciously to his views. A couple of years ago I wrote a long dissertation on Spinoclosterium, attempting to prove that the presence of terminal spines on an otherwise typical Closterium was not sufficient justification for establishing a new genus. I used what I considered very pertinent analogies, such as Fleurotaenium Kayei, Pl. spinulosum, Pl. (Docid.) australianum, Cosm. denticulatum, etc., with all of which I am quite familiar, to show that the original discoverers of these plants had no hesitation in assigning them to genera in which spines were unknown previously, and that these assignments

have stood the test of time. (Krieger's transfer of Docid. australianum Borge to Triploceras is an error; it is definitely a Plaurotaenium). But all my arguments had no effect on Prescott, and my Australian specimens will be described as Spinoclosterium in our North Australian paper to be published next year, though I have added a dissenting footnote.

The photographs of Scottia mira were taken with a 10x objective and 10x ocular, in a 35 mm camera provided with extension tubes to increase the focal length by 45 mm, and the negative was enlarged about 3 times in making the prints. I think I could do better with a 5x objective and a longer extension for the camera, but I do not possess such an objective and it would have taken too long to buy one. Reducing the iris opening of the adjustable diaphragm caused diffraction lines around the profile of the object, and to avoid this I tried something that seemed to work satisfactorily, and which might be useful to you sometime. I unscrewed the two parts of the 10x objective, and on the ~~upper~~ lower surface of the upper lens I pasted temporarily a circle of black paper 10 mm diameter with a 5 mm hole in the center, and then screwed the two parts of the objective together again. This reduced the effective opening of the objective by one-half, and did not give the diffraction lines as a similar reduction by means of the iris would have done. ~~Apertural~~ with a 3 mm hole in the black paper did not work, because the image on the ground-glass was so faint that I could not tell when it was properly focussed. I don't know why this method should eliminate the diffraction lines, but apparently it did.

I am quite in agreement with your contention that the genus Cosmarium ought to be split up, and I hope that your effort will be more successful than previous ones have been. Another genus that requires revision is Staurastrum; it is perfectly absurd to place such species as St. muticum and orbiculare in the same genus as St. leptodladum, leptacanthum, elongatum, to say nothing of the strange and highly elaborated forms from tropical lands.

By the way, if you would like to see more of these tropical desmids, just pick out anything that strikes your fancy from Krieger (Sunda), Grönblad (Brazil), or Borge's and Norrstadt's Australian papers. I do not have all of them, of course, but there are quite a lot that I can supply, including Streptonema trilobatum and Gymnozyga armata.

En attendant le plaisir de vous lire, veuillez agréer Monsieur, l'assurance de ma plus haute considération et mes amitiés sincères.

Feb 19 1954

My dear friend Teiling,

Just as I was preparing, this morning, to answer your letter of Jan 24th, the postman arrived with your second letter of Feb. 15th, so I can answer both of them together.

First of all, many thanks for your information about the hotels in Paris, and the excellent small map of the city. This will be of great assistance in planning our trip, if I actually go to Paris; for I must confess that some small doubt is beginning to creep into my mind as to whether it will be possible (and worth while) for me to attend the Congress. There are several reasons for this. First, I learn that the amount of the grants to be awarded by the National Science Foundation is only \$400, just about 40% of the estimated cost of the trip to France. Second, the number of the grants will be "between 10 and 15", and since there will undoubtedly be hundreds of applicants it seems that my chance of getting one is very small. Third, at Prof. Wm. R. Taylor's suggestion I submitted an abstract of a paper to be presented to the Congress, entitled "Some new and little-known Desmids from Indonesia and North Australia", to be illustrated with projected slides of my drawings and some photomicrographs. But Bourrelly replied that the programme was almost filled with about 100 papers that had been submitted, and suggested that my paper be presented at an informal gathering after 5 p.m., after the close of the day's formal business. This is not attractive to me, because by 5 o'clock everyone is tired from sitting and listening all day to papers, some or even many of which are of no particular interest to the individual, and everyone is anxious to go home or to his hotel. Fourth, none of my special friends in Europe, you and Grönblad and Krieger, will be there, and even Prescott has now given up hope of being able to make the trip.

However, even if I do not go to Paris I shall certainly take my wife to England to see my sisters in Devonshire, and then continue alone to Finland and Sweden, perhaps stopping in Berlin on the way to see Krieger. By the way, can you tell me whether Krieger speaks English? He writes in German, which I can read with some difficulty, but I cannot speak nor understand spoken German. So we should have some trouble in communicating if does not speak English.

It is indeed very kind of you to offer me your hospitality, and I shall gladly accept it. But my stay with you will be for only a few days; in fact my only reason for visiting Sweden is to see my good friend Einar. I could not go to Finland without also seeing you. That, I think, will show you how much I value our friendship. No, I am not going to take my car; I am not a millionaire!! In fact, this trip will be possible only by spending part of my capital; my income is greatly reduced, and this will be the last long voyage that we shall ever take.

Of course I shall be delighted to read your paper if I go to Paris, and I think it is quite a compliment for you to ask me. If I don't go, Bourrelly will no doubt be able to get someone else to read it.

The new Brazilian desmid seems to have caused a little excitement in Finland. Dr. Paavo Kallio, Univ. of Turku, is greatly interested, and will write some notes on the cytogenetic problems raised by the differing semicells, as a supplement to Grönblad's paper. ~~HE~~ Kallio wants to get some living specimens from Brazil for culture experiments, but I see no possibility of this, especially as

Brazilian correspondent has left the Amazon region, and is now living in Belo Horizonte, which is not far from Rio de Janeiro. But it will turn up again, one of these days, perhaps in Florida, where I believe I saw a single specimen a few years ago. This Florida specimen was so badly entangled in mucus and debris that I was unable to determine its structure, but from what I could see I came to the conclusion that it was not a desmid, and reluctantly I had to abandon it.

I am afraid that I cannot agree with you that this desmid is 3-radiate; I have seen perhaps a hundred individuals, some of them no doubt more than once, and the only structural variation that I have seen is in the number of apical lobules or teeth. There is no variation of the number of 6 processes in the upper whorl and 10 in the lower whorl. The apical lobules are usually 8, but a few specimens have 9, and I have seen two cases with 8 lobules and an open space for a ninth lobule, which was not present. It seems to me that this strange plant must be classed as an exception to the general rule of radiateness, at least until more knowledge is gained.

My new way of living in retirement is going very well so far. I am working just about as hard as I did at the office, and am gradually cleaning up some accumulated arrears of work. During the last couple of weeks I have sent out 192 vials of algal collections; 72 samples from Indonesia and Australia to Prescott; 48 from Brazil to Grönblad; 24 vials of tropical desmids to Ruzicka for his delectation; and 24 empty vials to my friend in Brazil so that he can make gatherings in his new location. In addition, I have sent duplicates of all of them to the Farlow Herbarium at Harvard University. That's a good-sized piece of work, making the wooden boxes, transferring the original collections into vials of uniform size, dividing each one into three parts, labelling them, and checking each one with the collector's list. Would you like to see some of these tropical desmids? Just say the word and I shall gladly send you some more.

My next undertaking will be the enlargement of my microscope room, which means a week's work with hammer, saw, and paintbrush. Then I have an old desk to be repaired and refinished; all the old varnish must be removed, the wood sandpapered, and new coats of varnish applied. So you see that I can find plenty to occupy my time.

It is surprising to hear that you are going to break up your library, and it cannot be a pleasant job to let go of your old friendly books. But perhaps it is best to do it while you can direct their disposition. If you have a copy of the second edition (1892) of Wollé's Desmids of USA, and wish to sell it, I should be glad to buy it at any reasonable price. For the last 10 years I have tried to get this work, but all I have been able to find is the first edition (1884). Prescott says the 2nd edition is much better than the 1st.

Enclosed are a few photos of the new desmid, the best I have been able to make so far. As you know, it is extremely difficult, perhaps impossible, to get all the processes in sharp focus at the same time. I have tried several combinations of microscope objectives and oculars, with blue, green, and red filters; I even mounted one specimen in potassium mercuric iodide, K_2HgI_4 , which had a refractive ~~index~~ index of 1.65, and is supposed to give a greater depth of focus, but the improvement, if any, was not noticeable.

I shall write you again about my trip, when I am able to make up something like a definite schedule. In the meantime, au revoir and best wishes.

Your friend

Linköping 15/II/1954.

Dear Friend Scott!

It was very interesting to hear of your changed manner of living. I am sure that you are right in your opinion that your future days will be quite filled up by your science and hobbies. I have tried it now in more than half a year and I must say that I am busy every day, writing, mending and arranging books. I have a lot of them, collected during 50 years. Most of them are valuable and it is now my desire to get them placed on the right place by selling and giving them to those persons and libraries where they will come to real use. I am so busy that I am afraid I am neglecting my body's need of movement. Just now the weather is very cold and not inviting for going out.

I am glad that you intend to go to Paris. I hope you will agree to my proposal to deliver my little lecture, only ten minutes. Especially I am glad of the hope of meeting you, I think we will get well together quite as well as per correspondence. I sketched a plan of our being together and I hope you can take your time about it, how anxious you are of meeting Grönblad as soon as possible. I wish I could follow you to him.

I think you are right in ordering an hotel room as soon as possible, there will be an enormous immigration to Paris this summer. When in Paris 1950, I lived in Hot l Slavia, Boulevard de Marcel 13(7), Paris 5^e, on the little map I am sending you it is marked 1. It is situated near the wonderful Jardin des Plantes with the biological institutions. I can recommend this hotel, it was clean and comfortable, elevator and hot and cold water and fairly cheap, I think I paid about 2 dollars a day, now I think it is somewhat more expensive. No restaurant but you can have coffee and the extremely delicate "croissants" with butter in the morning. Small good restaurants are very common, I used one just near to the hotel, nice and cosy. The price-class of the restaurants you can see from the outside-hanging menus, if you observe the ^{charfe} ~~price~~ of the plate! A speciality for Paris? You have also a wine-dealer in the nearest corner! I think you best write to your friend Grönblad to order it for you, he did so for me, I do not know if the landlord is capable of reading English, silent and unobtrusive as he was.

My son was very content with Hotel St Pierre, rue de Médecin, Paris 7^e, (marked 2), fine situation at Jardin de Luembourg but some way from the battle-place, and my wife and daughter recommend Hotel Florida (3) address is seen on the hotel-label in the wrapper. It is situated in the most busy part of the town and the going with the "metro"-tube is no pleasure. I am very sorry that I can't join you, I am sure we would have a pleasant time together, and I am "in measure somewhat at home" in Paris after two visits.

Many thanks for the picture of your interesting desmid, I hope it will be published in the next number of Botaniska Notiser. The most characteristic, in my opinion, is the lapse from the vertical symmetry, common to all desmids hitherto found. The direction of the branches must be seen in connection with this character. I agree that the number of the radii is somewhat intricate to discern, but it seems to be 3-radiate, especially your statement of the sometimes occurring π 9 apical lobules affirm this. This shows that the emergencies may be varying in number. Desmids are no crystals with mathematical structure but living organisms subjected to the mutative activity. Deviation from normal number are found for instance at St. leptocladum var. tetroctocorum, where two branches of the upper whirl are omitted, Nolle: Desm of U.S.A. p151, f 30. See also St. furcigerum in Lowe: Canad. arct. Exped. pl 4, f 9 and Beck-Mannagetta: Algen Riesengebirg f 33. Such deviations are labelled "abnormities" or even "monstrosities" but they exist, only a more penetrating research, for instance culture, would prove its abnormality, perhaps it is constant, but scarce!

I have given my impressions of your wonderful desmid to Grönblad, but they are of course no news for him. *Send my best greetings to your wife and yourself and my hope to be a bad people I would say "ou vaiz" Louis* *Emil T.*

Linköping 24/I/1954.

Dear Friend!

Long since I heard of you, I hope you have got my letter of 19/X/1953. I hope ~~too~~ too that you have not been annoyed with me because of my letter in which I told you my disliking of receiving too much gifts from you. Of course, I did not mean letters. I sincerely wish that our friendship will continue; I appreciate you very highly, you must be fully conscious of this fact.

Well, my days are trickling away in the empty nothingness. My health has not been so good and all the banal everyday's business waste my time, actually very little of value has been done. I just finished a little lecture on Staurodesmus ^{defectum} for the Paris-congress. Bourjelly, who is the secretary of the phycological section wished a contribution from me, ^{even} if I could not come. He said he would have somebody to deliver it and he wanted it for the transactions. Now I have heard that you will be a member of the congress; I am glad for this, you are the most convenient person I know to read my lecture. This does not in any way trespass your own lecture.

Sorribly I cannot come myself; at that time I am occupied with my teacher's work in Aneboda and possibly I am to take part of an investigation of the lakes in the neighbourhood of Stockholm. I examined most of these lakes in 1911 and a comparison will probably be of value. There has arisen a lot of suburbs that have contaminated the lakes horribly. It will be the first time in my life I may make some money of my science. I do need it; my income has decreased to 60 % after my retire.

Grönblad also told me that you intend to go further to Scandinavia. I really hope that you will take your way over Sweden. Please tell me in good time your itinerary so that I can arrange my summer plans according to it. I enjoy very much the possibility of meeting you "corporeally", a seven years' acquaintance by means of correspondence ought to be completed by such things that are not visible in letters and pictures, e.g. voice, gesture and the glint in your eyes. I suppose that you should like to look at my library. Probably my wife is occupied with our grandchildren on the ^{at that time} coast, but I am quite able to lodge you and your wife, I hope your wife will accompany you, in our flat here in Linköping, and also in Stockholm in the flat of my son-in-law, he and my daughter have vacancies from Midsummer to about the end of August. This, if you and your wife do not possess too exaggerated demands of comfort and attendance.

I suppose Grönblad is your especial destination. Linköping is situated on the main roads, railway as well as highroad, from Scania where the most used entrance-port Malmö, is situated, to Stockholm. Do you take your car with you? - From Stockholm you can fly to Abo or Helsingfors every day or, if you can spend a more day, I should propose you to take the steamer. Then you have the possibility of seeing a unique landscape: the marvellous "skerries", the archipelago of millions of islands of all sizes, it reaches with a little interval of pure sea from Stockholm to Helsingfors. I think we shall plan for the best in order to give you memories for life.

With my very best greetings to you and your wife,
Your friend

Einar Nilsson

Dear Friend,

I must thank so much for two letters, the first one was written the same day 27/VI, I wrote to you, and the letter of 23/VIII.

The treatment of the plates of your paper in Australia is almost incredible, the editor must be everything but a scientist. I hope that he now has changed his mind int this respect.

My British journey was extremely interesting but fairly tiresome. It was my last chance of seeing the British islands, thus I was anxious not to omit any opportunity of learning. I had no holydays, every day in work from breakfast at 8 o'clock to about midnight, when I had finished my diary. A unique experience was the stay at Christ's college, a memory for life! The extensive bus journeys were more valuable for geographical apprehension than for collecting plankton samples, for which I had got a grant from ^{out} Natural Science Research Council; I missed a lot of fine lakes in Scotland because of the long day's tours and consequently very few stops. Actually, I felt like Tantalus! In Ireland we had more time and I got a lot of samples. After the excursions I spent my stay in London mainly indoors, happily I had been quite healthy during the Congress and the excursions.

When at home, I had a three days' parleying with Hannah Croasdale. She is a very nice and clever girl, my wife and I are very fond of her (even our black poodle jumped surprisingly up on her knees) and it was a pity that she had no more time for us. She had had an instructive time at Grönblad, then a couple of days in Upsala. From here she went to Denmark (Nygaard) and over Oslo and Bergen to Newcastle. Just now she must be at home.

Many thanks for your interesting drawings of Staur.Bibrachiatum et formae. Such variations are extremely valuable for our comprehension of the concept of species, a concept which is archaic and not agreeing with modern results.

In Cambridge I had a chat with Fritsch. He regretted his incapability of sending you more abstracts, he meant that his abstracts, as yet left, ought to be sent to institutes and libraries where they are available for most scientists and I stressed your difficulties in this respect. After the great war when enormous quantities of abstracts were burnt, for instance in Leipzig, it is very difficult to get abstracts and second hand books, and those for sale are too expensive. In England I was seriously blamed by a Fennish botanist, who had heard of my sendings to botanists abroad. Evidently he had an exaggerated apprehension of it, but my elucidation was rejected, apparently he was a grumbler (he blamed a lot of common friends). Actually, he had also received valuable abstracts from me.

It is a pleasant feeling to be home from School every day, since 1895 I have

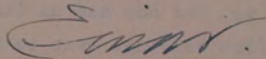
been obliged to go to school, with the exception of two years! Sometimes I pay a short visit at the "place of my crimes" in order to be quite aware of the contrast. I have not as yet properly started with my work, I have the Actidesmium paper unfinished since a year and I am curious about the summer harvest. I am just making an arrangement like yours for the mirror, in order to get larger drawings. I am very glad to have every morning free (I am a decided night-owl!) but it will tempt me to change night into day.

My samples from Victoria lake are very poor in desmids, the part of the enormous lake, where the samples are caught, contain mainly Microcystis aeruginosa, even the endemic protococcales are very scarce. I have, however, promised to examine samples taken by a Dr Ross from British museum. He is a diatomist and attacked me at the Congress. On the whole, it was very interesting to meet people well known as correspondents or in the literature.

In the parlours there was a more or less discreet looking at the badges with our names and one often heard expressions of joy or delight at surprising meeting. I made many pleasant acquaintances, curiously enough especially amongst Germans. An American fishery-man, Mr Nelson, gave me a pronounced "Swedish" impression, that I said to him he must be of Swedish origin. "Yes, my father was born in Stockholm," he said.

Now, at last, my best greetings to your wife and you,

tuus



Jan 4 1954

Dear friend Telling,

Well, this is the start of a new year, and the beginning of a new way of life for me, for at the end of 1953 I retired voluntarily from partnership in the small business firm with which I have been connected for the last 44 years, from office boy to Vice-president and finally partner. How it will turn out remains to be seen, but I think I shall enjoy it as much as you seem to be doing. Some of my friends have prophesied that I shall be looking for a job within six months, or a year at most, from boredom or something like it, but I don't believe them. My wife and I have enough money to keep us, in moderate circumstances, for the rest of our lives, though I shall not be able to spend money as freely as I have done for the last 10 years. And I have other hobbies like woodworking, metal-working, and photography, besides my desmids. In desmids alone I have enough on hand to keep me busy for the next five years, and I am getting more all the time, from distant parts of the world.

The account of your visit to England, Scotland, and Ireland was most interesting, and I can easily imagine your disappointment at having to pass up many of the Scottish lakes. On some of my long trips by automobile I have felt the same way, because there simply was not enough time to stop at all the attractive ponds and lakes in Central Florida and other regions.

I am very grateful for your efforts in inducing Prof. Fritsch to send me the nice collection of his works, and I can easily understand his feeling that the scarcest ones ought to be reserved for libraries where many persons can see them instead of just a single individual. You are certainly not to be blamed for your distribution of old reprints, and I can assure you that I have made very good use of those that you sent me, and shall continue to do so in the future. The person who reproached you must certainly be very "disgruntled" (an American word meaning a chronic grumbler).

Yes, Hannah Croasdale is a very lively, energetic and likeable girl. I have met her on only one occasion, a few years ago, when my wife and I went to New York to attend a meeting of the AAAS and the Phycological Society. But I have corresponded with her for several years, and she has made all the Latin diagnoses for Prescott's and my papers. At Christmas I received a card from her, with a brief note concerning her visit with Grönblad, at whose house she said she had eaten some rice that I had sent him. He must be hoarding the rice as a treasure, for it is some years ago that I sent it. Would you like some rice and coffee for your birthday? I should be very happy to send it.

Now here is another piece of news. I am seriously thinking of going to the 8th International Botanical Congress at Paris next July, and have applied to the Nation Science Foundation for a grant. But I think I shall go even if I do not receive the grant, and I hope very much that you will be able to go and that I shall have the great pleasure of meeting you there. Grönblad and Krieger have written me that they will not be able to attend, because of the expense, so it is my present intention, after the Congress, to visit K. if he can meet me in West Berlin, and then continue on to G. in Finland. And if I get as far as Finland I shall certainly visit you in Sweden, that is if you do not get to Paris.

Since you are well acquainted with Paris, perhaps you can tell me the name

of a moderate-priced, but modern, hotel that is not too far from the place where the meetings will be held, which I assume will be the Museum National d'Histoire Naturelle.

Now, here's another news item. A few months ago I received a nice lot of collections from Brazil, some from the upper Rio Negro which unfortunately are poor in desmids, and others from the Rio Arapiuns and its tributary streams, not far from the locality where Grönblad's Brazilian collections were made. In fact I have found many of the beautiful and highly-elaborated desmids that are illustrated in G.'s work. And in one of the samples I have found what I claim to be THE MOST WONDERFUL DESMID IN THE WORLD, and I think you will agree after you have examined the enclosed drawing. When I found the first specimen I could not decide whether it was a normal desmid or a monstrosity, or even a desmid at all, but after seeing several specimens, all alike, I slowly came to the conclusion that it is not a freak, that it is a desmid, and that a new genus will be required to receive it. So I sent this drawing and actual samples to Prescott, Grönblad, and Krieger, all of whom agree that a new genus is warranted, and G. is going to write it up in Botaniska Notiser.

I think this new desmid is going to give you a headache in trying to fit it into your radiation scheme, for as you will notice, there are 10 processes in the lower whorl, 6 in the upper whorl, and 8 tooth-like lobules in the apical corona on the upper semicell only. This combination of numbers is divisible only by 2, so you might say that it is biradiate. BUT, ~~but~~ ^{there are} occasional specimens with 9 teeth in the corona instead of 8, and how you can take care of that, I do not know.

The original sample was a plankton collection, and contained only a very small quantity of material, more Crustacea than anything else, but I can spare a minute amount for you, and am sending it under separate cover. After you have seen some specimens I shall be greatly interested to hear your comments on this strange plant, and I know that G. will be interested also.

With best regards and wishes for the New Year from my wife and myself to you and Mrs. Teiling,

Your friend

Aug 23 1953

Dear friend Teiling,

I am sorry that you feel as you do, concerning the journals and the few books that I have been sending you. Since I receive the journals "Science" and "Scientific American" regularly, it costs me nothing but a few cents postage to mail them each week to one of my friends instead of throwing them in the waste-basket after reading them. I cannot send these journals to Grönblad, Krüger, or Ruzicka, because they sometimes contain articles that criticize Soviet science and politics.

The last book that I sent you, by Tiffany & Britton, was purchased for my own use, but after finding that the section on desmids was completely without value, and since I am not interested in any other algae, I resolved to send it to you. But you need not feel under any obligation! You have given me books of at least an equal value to those I have sent you, - Nordstedt's Index and Supplement, his FW Algae of New Zealand, W&W's Madagascar paper, to say nothing of the numerous smaller papers that you have kindly sent me and that can only be obtained by the kindness of a friend. Perhaps you do not realize the monetary value of some of these works, so I may say that quite recently I have paid Harvard University Library \$16.70 for a photocopy of W&W's FW Algae of Ceylon. To my way of thinking, our financial account is pretty well balanced, so please stop worrying about it, and in return I have stopped the sendings unless I find something in the Scientific American that may be of special interest to you.

I am quite sure that you have enjoyed your attendance at the Limnological Congress in England and your trips to Ireland and Scotland. Did you know that Dr. Hannah Crossdale is visiting our friend Grönblad in Finland? By the time you get this letter she will probably be there, and perhaps she will get an opportunity to go to Sweden and meet you.

Grönblad has sent me samples from two Italian lakes, each containing numerous specimens of St. bibrachiatum Reinsch, which probably belong to var. gymnatum W&W, and also "combination" forms in which one semicell has only 2 arms instead of the 4 which characterize bibrachiatum. But the 2-armed form also exists in both samples as a separate entity, with 4 arms on the whole cell, and in this state it closely resembles St. Smithii (G.M. Smith) Teiling, being strongly twisted at the isthmus, and St. excavatum var. planctonicum Krieg., which is also twisted. So the "combination" forms might be regarded as either dichotomical of two species, or Janus forms of one species, depending on the viewpoint. In this connection it is noteworthy that similar "combination" forms occur in all the collections, from several countries, where bibrachiatum has been found. I have them in my own collections from southern USA; Bohlin from Azores; Krieger from Sumatra; G.S. West from Yan Yean Reservoir in Australia; Grönblad from Italy; and W&W found a single 7-armed specimen in Madagascar. So apparently a change in nomenclature is desirable, and no doubt G. will have something to say about it when he publishes the results of his Italian trip. He wrote me that he found it "not quite easy" to draw these tiny and delicate desmids, and asked me to try my hand at it. My own opinion is that it is very difficult to make acceptable delineations of these multiply twisted little devils, but after many struggles I have made a number of sketches that I am sending him. I am enclosing a copy of one of them so that you may see what they look like.

With my best regards and good wishes,

Sincerely your friend

Dear Friend Scott!

Time runs too quickly and this school year has passed, I am very sorry to confess, without any productive work. I envie your industriousness.

I had a letter from Fritsch to whom I wrote (9/IV) an intimation that you were in want of his papers on tropical desmids. I hope that you now has got a good deal of them, he regrets, however, that the copies of several of his papers are quite consumed. I myself ~~am~~ am sorry that I could not procure more of my earlier papers on planktonalgae 1912 and 1916, my first paper of 1909 (on fossile brown algae) was too generously spended on friends by the very, very young author. Many thanks for the doublets of Staurastrum Ophiura, I had, however, copied its figures previously, thus I have sent the copy to you in return.

Sampaio's pap e is beyond my powers of comprehension. I believe that the cellwall is a result of the activity of the protoplast and not the reverse. I got a letter from Fritsch where he tells me that he has written a third paper on the phylogeny of desmids and that he has used certain view-points on mine which ~~he~~ seem plausible to him, very nice, indeed. My correspondence has decreased because of my inactivity. I cannot understand that my former energy is so exhausted for such a long time; smaller periods have occurred previously but this time it is horrible.

So long I had, in May, made a draft of a letter. Now the continuation. The end of a school-year is always a buzy time and now I was obliged to wind up my transactions with the college, especially my institute. I was honoured by the students and by the Inspector, I hope it was not exclusively due to the fact that they now get rid of me. I have spent a fortnight at my summer cottage, mainly occupied with the dammed job in the garden and painting the houses.

To morrow I shall ^{drive my car} go to Aneboda Limnological Laboratory for lecturing and demonstrating plankton for the students from the University and then I shall visit lakes previously not examined by me. Sweden possess^{es} more than 96000 lakes and I have visited only 700 of them. Two weeks I shall spend at my son-in-law on his charming island on the West Coast. Not until the middle of August I shall go to the Limnological Congress in England. Because of a grant from the Swedish Natural Science Research Council I shall be able to take part of an excursion in Ireland and also Scotland.

Yesterday I received Ruzicka's latest paper on Cosmarium ochthodes, his special darling among the desmids. The most important contribution to my library, however, I received from you: Tiffany and Britton's great book. Actually, I don't know how to recompense you. I am extremely grateful to you for your unaltered kindness and friendship, but I am too well aware of the fact that I am the receiving part. I have in my whole life got into the habit of paying everything what I have received. That is why I beg you not to send more gifts. I do hope that you understand and forgive me; my heart is warmed by your altruistic friendship which have resulted in a lot of valuable gifts. Sorrowfully, I am unable to express my feelings in good English but I think you understand my clumsy words. I am quite aware that you have no thought of gifts in return, but even gifts from my relatives prevent me to enjoy as they are worthy of, when they are expensive. You have spent a good deal of money also when sending me the journals, please stop it. If you find specimens of Scientific American the content of which you suppose may interest me, I should be glad to get it. It is a marvellous publication, its illustrations seem me to be unsurpassed.

I hope that your house now is your well-founded castle again and a fine setting for a skilly and famous desmidiologist. Here we have a very fine summer, the temperature is often very high for these latitudes, even in Lapland it surpasses 30 centigrades, with a sun shining about 23 hours a day valuable vegetables are harvested on the open ground which not occurs every year. I think that my plankton algae are thriving and ^{will} give me good results.

I am glad to hear that you and your wife are all right and I send the best wishes for a healthy and successful summer.

Your friend

Einar Telling

New Orleans 18, La.
June 27 1953

Dear friend Teiling,

Many thanks for the interest that you took in telling me that some desmid works were shown in Asher's latest catalogue. However, it happens that I have both of those that you mentioned, the Dunda paper by Krieger, and Part I of his Monograph in Rabenhorst's Kryptogamenflora. Both of these are extremely valuable to me, and as a matter of fact I keep them on my microscope table where I refer to them constantly.

For the last 2 or 3 years I have been sending an occasional C.A.R.E. food parcel to Dr. Krieger; he is very grateful for them and says that many of the foods are simply unobtainable in the Soviet Zone. But the last parcel that I ordered from C.A.R.E. a few months ago could not be delivered in East Berlin because of difficulties with the Soviet officials, and my money was returned to me. So now Kr. Krieger has given me the name and address of his son who lives in West Berlin, and I have sent another parcel to the son, hoping that he will be able to get the articles across the border in some way. His address is:

Dr. Kurt Krieger,
Berlin Steglitz,
Grunewaldstr. 29.

So if you want to write to Dr. Willi Krieger you might address the letter to his son, and you need not be so careful about what you say. His letters to me are always very brief but I gather that he is getting along as well as can be expected under the Soviet rule. I hope that the recent uprising in East Berlin and the consequent police counter-measures and reprisals have not had any effect on him. A year or more ago he wrote me that he had just about completed the next part of his Monograph, on Cosmarium, and that he began to see some hope of having it published soon. I am eagerly awaiting this, and am very curious to see what he has done with this unwieldy and very confusing genus. Also he has promised to send me a few African samples, from Lake Victoria, that he says contain some interesting desmids. Have you any material left from your Lake Nyassa samples? If so I should be very glad to have an opportunity of examining it.

I have had several letters from our friend Grönbald this year, and in return I have sent him several dissertations on some of my desmids, on which I ventured to disagree with him. In some instances I think he will have to admit that I am right!!

A few days ago I received an advance copy of a reprint of a paper by Prescott & Scott on South Australian desmids, published by the Royal Society of South Australia. But I was horrified to see that the editor, without permission, had reduced four of the five plates to such a minuscule size, 5.5 x 9.5 cm, that the figures are almost useless for identification or comparison. I have written him an angry letter of complaint, and have asked him to have additional reprints made with full-page plates. If he won't do it in Australia I intend to have the paper reprinted here, and shall then have additional copies made of the plates, for you and other compilers of iconothèques.

Prof. Fritsch has sent me, no doubt at your suggestion, seven reprints of the papers by himself and Florence Rich on South African algae, even digging up some as far back as 1914. These are a very welcome addition to my library, and I have written him a letter of thanks. Please accept

Linköping 5/II 1953

Dear friend Scott,

many thanks for your wrappers very often coming to me. Especially I enjoy your latest paper on Euastrum, in all respects an excellent paper, very fine biosystematix in the words as well as in the beautiful drawings. In this you have once more demonstrated your capacity of research, previously well known to every desmidiologist. I congratulate you to the membership of your Academy of Sciences by all my heart. I am glad that the academy has become aware of the sharp-eyed and prominent micrologist, hiding in the business man Arthur M. Scott. My wife joins in this congratulation.

As to me, little has been done during the last months. I have suffered from a mental depression but I am now getting into full swing with the description of a new genus. - Ruzicka has sent me a lot of drawings of the intricate series of St. Manfeldtii-Sebaldi-vestitum etc. I am very glad that he is so industrious and enthusiastic, it seems as if his conditions of living are fairly good, as good as the may be in that damned sovjet "paradise". - Grönblad has been quite silent, except a Newyears-card. - Messikommer and I are discussing St. Manfeldtii and Sebaldi and their reduced forms, from his side in his fairly peevish and supercilious manner.

I should like to submit you a proposal. Several desmidiologists are making iconotheques, containing copies of all (possibly!) the reproductions of the desmids published in the numerous papers of all parts of the world. The copies are pasted on paper-sheets, each species on one or more sheets and arranged like a card-system. The value of such a "herbarium pictum" can not be overestimated! Nordstedt had built up a fine collection and Borge another, even more extensive. The owners of iconotheques send douplets of the figures and plates published in their papers in order to reduce the toil of making India ink copies. As far as I know, at present Fritsch, Nygaard, Ruzicka, Teiling and probably Bullard and Messikommer are active in making desmid iconotheques. At least Nygaard and I include all fresh water algae. That is why it would be very acceptable if you, in the future, procured separate prints of the plates and distributed to these algologists. Your delicate drawings can hardly be done full justice to, when copied by drawing.

My very best greetings to you and your wife from

your friend

Erwin Teiling

Good Friday, April 3 1953

Dear friend Telling,

No doubt you have been thinking that there was something wrong ^{with} me, perhaps ill again, because I have not written for so long. Fortunately that is not the case, it is simply that I have been spending almost every minute of my spare time in getting the additional information on my desmids that Grönblad has asked for, and in trying to untangle the confusion that exists in the nomenclature of certain forms, e.g., *St. gallatorum* var. *forcipigerum* versus *St. saltans* var. *forcipigerum*.

Thanks for your kind words regarding my papers, which are highly appreciated coming from an expert like you. Several others have complimented me on my drawings, but few have commented on the text. Sorry to say that there is no particular distinction in being a member of the New Orleans Acad. Sci. Anyone can be a member if he can spare \$2.00 per year and knows somebody who is already a member. I was presented for membership 10 or 12 years ago by a man (now dead) who was one of the first to give me a helping hand at the beginning of my serious microscopical work. Prescott seemed to think that mention of membership lent a certain amount of prestige and that is the only reason for inserting it in the papers.

Your proposal that I should get additional copies of the plates for my papers is a very good one, and I shall try to comply with it in future. Some years ago Nygaard asked if I could send him ~~the~~ second copy of the papers, so that he could cut out the illustrations for his iconotheca, and I have been doing so ever since. Probably he is also receiving a third copy from Prescott, in the case of joint papers. A couple of weeks ago I sent you some extra copies of my papers for this purpose, and also sent them to Ruzicka. I have had no correspondence with Messikommer, but he has kept me supplied with reprints of his papers for some years, so I shall send him copies of whatever papers I can spare. I do not know Bullard's address, and was unaware that he is still active; the only time I have seen his name mentioned is in G.M. Smith's paper (1924), and in an old paper by Bullard on mounting desmids. As to Prof. Fritsch, I have sent him reprints of all my papers, but he has never taken the trouble to acknowledge them, nor has he sent me anything of his own. I should like very much to have some of his South African papers, but they are probably not available now. Undoubtedly Prescott sends him copies.

There was an article in *Mikrokosmos*, March 1952, "Ein neues Einschlussmittel" by Horst Kaudewitz, that might be of interest to you. It concerns "Kaurit 65", a formaldehyde-carbamide condensate, water-soluble, which hardens to a solid. It is worth reading.

No doubt you saw Ruzicka's paper on the mysterious phenomenon of two or more desmids enclosed in a thin transparent membrane. I wrote him that I had seen similar cases, and suggested that the desmids had been swallowed by some animal and later evacuated through the digestive tract. A week or so later I found two specimens while hunting for something else, so I made sketches and sent them to Ruzicka. In one case the membrane enclosed one whole cell and one semicell of the same species, both empty or nearly so; in the other case there were three desmids, all of different species. My suggestion seems to be the only one that would fit this occurrence. I don't know, of course, what kind of animal it could be; perhaps tiny fishes, or tadpoles (Kaulfrosch).

Also you have probably received Sampaio's latest book, in which he attempts to show that the various kinds of spores formed by desmids, i.e., zygospores, parthenospores and aplanospores, are caused by changes in the desmid cell-wall, induced by changes in the mineral content of the water. Since I know nothing about this subject I cannot criticize his theory intelligently, but perhaps I may express an amateurish opinion. It seems to me that his theory is rather far-fetched, and that the minerals in the water

can act directly upon the cytoplasm and nucleus of the cell, without having to go through the roundabout method of mineralizing and hardening the cell-wall, which according to Sampaio prevents the two semicells from separating. It can be shown very easily that the water is in direct contact with the cell-contents, by the almost instantaneous speed with which the nucleus, pyrenoids, and chloroplast take up a stain such as fuchsin or iodine that is added to a preparation. What do you think of this? and how do you regard Sampaio's theory? I have read only the summary in French; I did not think it would profit me much to wade laboriously through the Portuguese version, even though ~~it~~ no doubt it explains his theory in more detail.

I thank you very much for sending the photo of John Raftis, which is a valuable addition to my collection. I have a great admiration for him and for Francis Wille; both of them did pioneer work under what must have been very difficult circumstances.

My own work is coming along slowly but surely. I have nearly finished with the drawings that Grönblad returned to me, and then I shall be able to resume work on the highly interesting material from Borneo, Java, and Sumatra that Sachlan sent me. Just now I am having some extensive (and expensive) repairs done to my house, which was being damaged by settlement of the foundations. This is caused by shrinkage of the alluvial soil (formerly a cypress swamp), on which the whole city of New Orleans is built.

I hope sincerely that you and your wife are in better health now than when you wrote last year. Glad to say that Mrs. Scott and I are quite well now, and both of us send our best wishes.

Your friend,

New Orleans, La.
Dec 14 1952

Dear friend Teiling,

Many thanks for your letter of Nov. 3rd and the copy of your paper on the chloroplasts of desmids. No doubt this is the paper that you had to revise and curtail at the request of the editor of Botaniska Notiser. I have never seen any papers by Lütkenmüller or Nellie Carter, so yours is the first extended treatment of the subject that I have read. Previously I had formed the opinion, based I think on W. & G.S. West's monograph, that the chloroplast was comparatively unimportant in desmid taxonomy, and that in only a few cases could species be separated because of their different chloroplasts. But friend Grönblad has been fussing with me for the last couple of years because my drawings do not show the chloroplasts, and I can see now that I shall have to try to include the chloroplasts and pyrenoids in my drawings. This is not always easy, especially with collections that are sent to me from distant parts of the world, in which the chloroplasts are frequently so much deteriorated that I can form no idea of their original appearance. Also in many cases I have had to destroy the chloroplasts, in order to distinguish the wall ornament, which seems to me much more important for taxonomical purposes. By the way, I have found a new variation on your method of using sodium hypochlorite for this purpose. I add the NaClO to the water containing the desmids and allow it to stand over night; then I add hydrogen peroxide (H_2O_2) which causes a brisk reaction with evolution of nascent oxygen (and Chlorine?). Usually one treatment will remove all of the cell contents, leaving the completely empty cell for examination. Mr. Paul Conger, the well-known diatomist of the Smithsonian Institution, to whom I communicated the method, wrote me that he finds it valuable for cleaning delicate diatoms which would be subject to damage by the usual treatment with strong acids, and also for breaking up diatomaceous deposits of earth.

I have read your paper attentively three times, and though I seem to understand each of the separate parts, I must confess that I cannot get a clear grasp of the whole paper. This, of course, is because of my lack of training in any branch of biology; all my life, until I started playing with desmids, I have been mechanically inclined and interested in tools, machinery, engineering. But I shall continue to study your paper and am sure that it will help me to an understanding of the various types of chloroplasts and their application to desmid taxonomy.

Grönblad wrote me a short account of his trip to Italy, which appears to have been profitable as well as enjoyable. He received plenty of assistance from the Italian professors, and made some good collections of desmids, which, however, will probably turn out to me mostly hard-water forms, which are not so interesting (to me) as the acidophiles. Before he went to Italy he had done a lot of work on my USA material, and sent me a batch of his own drawings with his tentative determinations, on which he wants my opinion, and in many cases he wants more information on my drawings, chloroplasts, pyrenoids, figures a vertice, etc. ~~MEXICO, SEVERAL CASES~~ But it is a curious fact that in several instances I am now unable to find a single specimen of the required desmid in the same collection from which I made numerous drawings some years ago. There are plenty of other desmids remaining in the vials, but the particular one that is wanted cannot be found by either Grönblad or myself. That reminds of Charles Bernard's account of Spinoclosterium curvatum, of which he found plenty of specimens when he collected it in Singapore, but could not find a single one on re-examination of the material after his return to Bogor (Buitenzorg).

A few years ago Grönblad loaned me some old slides, made 50 years or more ago by such masters as Lütkenmüller, G.S. West, Nordstedt and others. But many of them were in the condition you describe, and of little value. I should think that some of the modern synthetic resins (plastics) could be used for making permanent slides of desmids. There is one made in this country, and no doubt also in Europe, which is sold in a liquid form, and by the addition of a liquid catalyst it sets to a solid in a short time.

The solid is quite insoluble in water, but dissolves in acetone and similar organic solvents. The process is used for embedding flowers, etc., to make small ornaments, and also for scientific purposes, preserving animal and plant specimens. For desmids it would have the disadvantage of requiring dehydration of the material, which of course is also required for mounting in balsam or Venetian turpentine. I have thought of trying it, but I have no aptitude for such work, and I don't seem to get sufficient time.

Dr. Krieger wrote me recently that he has retired on a pension, as I think you also have done, or will do shortly. He said that he now has the next *Lieferung* of his monograph, dealing with *Cosmarium*, nearly ready for the printer. I am very anxious to see this and to know what he has done towards straightening out this very complex and confused genus. It must have been an enormous job.

I am truly sorry to hear about the illnesses of you and your wife, and sincerely hope that things may have improved somewhat by this time. I believe that gastric ulcers can be controlled, even cured, by following a strict dieting regime, and I hope this will prove to be true in your case. What you tell me about your wife's eye appears to be quite serious, but I can cite a somewhat similar case which turned out happily. After our automobile accident in Cuba two years ago, my wife suffered from "double vision" because the muscles of the two eyes did not co-ordinate, which was caused by some injury to the optic nerves. She went to one of the best oculists in the city, though I could not see that he did much in the way of treatment. But the trouble cleared up in the course of a few months and now her eyes are all right again. Also I am glad to say that the herpes zoster has disappeared after running its course of six weeks, exactly what the doctor predicted.

So now we are both in fairly good health again, and we are leaving next Saturday, Dec. 20th, for a two weeks trip through Florida. I hope to get as far as Key West at the extreme southern tip of the State, and along the way I shall try to find some new collecting places and perhaps pick up a few new desmids.

My wife and I both send our best wishes to you and Mrs. Telling for Christmas and the New Year. Glædelig Jul og godt Nytaar. Yes, I know that is Danish, but I can't write it in Swedish!

Sincerely your friend,

Linköping 3-XI- 1952.

Dear Friend Scott!

It is a very long time since you heard from me, the 4/VI. Summer is gone, a bad summer from climatic point of view; the warm and sunny days have been very scarce. I think I have told you that I was very anxious to finish a paper on the desmid chloroplast before summer and I was very busy in my efforts of succeeding this task. A copy of it is just now on its way to you. I hope that ~~it~~ this paper will give an explanation of the factors which have formed the main types of the chloroplast as well as of the desmid shape and also the relations between the varying types of the chloroplast. I also hope that the desmidiologists will not grumble about my new terminology, especially the ~~concept of~~ ^{concept of} parietal chloroplast contains several genetically different types. New terms are always unpopular among the desmidiologists but they are inevitable in order to prevent confusion of similar but essentially different concepts. Of interest are also several cases of convergency, which have caused confusion in the biosystematy. On the basis of my results, I found it necessary to erect an own genus for Hyalotheca neglecta (it is curious that it has been neglected so long) in spite of its name), I called it Groenbladia, since Grönblad has given the most valuable informations of this species.

After the traditional teachership at the limnological laboratory of Ångström, I visited some lakes in the south of Sweden and then I stayed some weeks at my daughter and son-in-law on the Westcoast. Already before summer I had some trouble with my digestion and an examination in the middle of oktober stated irregularities in the large intestine and a fairly great gastric ulcer. That is why I now is away on sick leave, how long the next röntgenogram will decide. At present I am, during my better hours, occupied with a paper on the Clevej-section of Cosmarium (see my paper, page 292). Microscoping is a little too trying just now, unhappily.

Grönblad is back from his Italian journey, I am sure he has done a good work, probably he has hunted in Lago di Candia, the classical place of research of Delponte. Delponte's new species need a confirmation. It is remarkable that there are no active Italian fresh water phycologists just now, succeeding De Toni and Forti.

In order to change the milieu I made a trip to Lund, where a room is at my disposal in the Botanical Museum. I took a glance at Nordstedt's collection of slides, the vast majority of which made by him but also a great many sent to him by other desmidiologists. I have promised to bring them in order and select

original

the type specimens. I am sorry to say that a very great part of the slides now are of little value. During the 50 - 70 years the mounting medium has disappeared and the "sealing" (gold-size or something similar) has grown in and in too many cases filled up the room between slide and cover-glass. I hope that a future more detailed examination will give a more encouraging result. At all events, it is of greatest importance that an inventor must give ~~x~~ very exact and detailed figures of his new species. Likewise, the inventor ought to be obliged to assign the characters which discriminate his new form from the nearest relatives, a procedure now very often wanting. I think that a proposal in this direction will be set out at the next Botanical congress, in Paris.

Later: Just now your letter arrived, many thanks. All the time I have got kind proofs of your friendship in the wrappers containing Science and Scientific American. I am glad that your silence has been caused by intensive phycological writing; I was afraid that your gall-bladder was troubling. I am well aware of the intensive desire to bring a job to its end and to get rid of it, and I congratulate you to your success and the "happy end". The samples from Arnhem Land must have been veritable full hits, extremely seldom found. Bohlin once collected a sample in Swedish Lapland, containing 139 desmid species (described by Schmädle, unhappily with his bad drawings).

I regret your wife's illness, herpes zoster usually being of fairly long duration. My wife has got a paralysis of a muscle of her left eye, which at present is unable to follow the movements of the right eye. The doctor fears that she will be practically one-eyed, though there is a faint possibility of sanity. Well, when getting old, one must run the risk of ailments! I hope your wife will recover very soon, illness is depressing not only for the suffering but also for the family.

My wife sends her best regards and good wishes to your wife and you,
and so does your sincere friend

Telling

Linköping 4.VI 1952

Dear Friend Scott!

I am very much ashamed that you now, I am sure, are obliged to place me in the same category as is Bourrelly. In his latest letter, of 10. Nov. 1951, he told me that he would "consacrer une année ou deux à terminer la thèse sur les Chryso-Phytas". I think he is quite occupied with this task, but of course he ought to acknowledge and be glad of your technical methods.

I myself just found a foul copy of a letter to him (in French, Pohl!) which I have forgotten to send away. Probably that is why I have not heard of him this year. It is terrible how time runs away, the faster the older I become. In the beginning of May I sent my MS to the editor and got the amusing letter that he must cut away two chapters; the paper was too long for the Bot. Not. After getting rid of this job, I had to devote my whole attention to my neglected school-service.

Because of the writing there have been only few hours at the microscope. Many thanks for your interesting samples from Borneo. I have only looked at them superficially. I am sure that Insulinde hide a great treasure of unknown desmids and other algae as well as Australia. (Owing to you I have got connection with Ophel who has promised me to send plankton-samples) What a harvest a skillful desmidiologist as for instance you, would carry in there. In 1918 I was engaged as a botanist in Jawa for studying rubber diseases but the war made it impossible to go over there and I am glad of that. The next year I was appointed lektor and I am sure that my own country is the best for me.

Just now I have West's paper on the Algae of Ceylon and of Burma borrowed at home. I think I shall have opportunity of copying them for you this autumn. Now I am quite occupied with the school and immediately when the vacancy begins, the 12. June, I go to my summer-cottage; I am extremely tired and my intestines have begun to trouble. I must procure vigour for my teaching job at Aneboda and then a week's amusing trip to unvisited lakes in South Sweden, ending at the West Coast on the marvellous island where my son-in-law has built a special cottage for us.

In this autumn begins my last year of service. I have been offered to continue but I have refused. It will be marvellous to have the whole day for research!

I am extremely glad of your find of the dicho-, tricho-, and tetrachotypical specimens of St. Wildemanni! For me, who in manner, have indicated this method of biosystematy, it is very gratifying to see this method realized. The conservative and orthodox desmidiologists, supporters of the "Old Testament of desmidiology" need every torpedo in their incorrect position. Your method of giving an extensive material of the variation, e.g. Spinocosmarium, is very elucidating and progressive; a true "desmid-philatelist" had created a lot of species and many variations and formae. Already in 1894, Wildemann regretted the tendency of the desmidiologists of making describing specimens instead of real species.

As to the supposed (by me) St. gemelliparum, I judged from the number of the processus. Your name leptacanthum var. dodeacanthum agrees with West's in N. American desmids t 18:6. I think that their view-point has been the long forked processus, they mention that the semicells are more triangular than leptacanthum and accept Wolle's var. tetroctocorum which is quite as well a reduced leptacanthum as a supernumerary tohopekaligenae. For me the structure of the angles is decisive, as you have seen in my Radiation-paper p 310. If you call the lateral processus P and the apical ones p, the angle of leptacanthum is: PpPpP, quite as Arctison, which is a counterpart of it in the series with dentate processus. Irregularities of the disposition of the processus are not too scarce among the multibranchéd Saurasträ. The size is no specific character, a natural species is not exactly identical to the specimens occasionally found by the inventor. You mean, I suppose, that your form has missed the P at the right xxxxx side of each angle? This is very possible, quite as in your St. furcatum. Such loss

of symmetry may occur as well as redoubling. Your opinion of its belonging to Tohopekaligense (with an angle structure: pPp) as a sectorial reduction of leptacanthum. And more, the losses are more common than new creations! If you are looking for a convenient name of it, I should propose scaevola, i.e. left-handed. The phenomenon of reduction ~~xxxx~~ gives a view-point of the biosystematy of these Staurastra, see for instance St. inaequalis var. triceps in Grönblad's Algae Brasil. p 212. and a leptacanthum in Wests' Frw. algae of Ceylon, a copy of it enclosed.

Many thanks for the splendod coffee! But, you shouldn't send me such things now. My heart is delighted at your amiability, but my sense is ever more weighed down by the burden of dept. If I may be able to make good copies of the wanted papers on tropical desmids, my burden would decrease, at least in some degree!

Ruzicka has sent me intricate questions on Staurastra and Cosmaria. The former I have tried to answer, the latter I have transferred on Grönblad. I have also given him a description of the arrangement of my iconotheque, based on the more extensive built up by Nordstedt and Borge. Nygaard is, I suppose, occupied with his new wife and, being a teacher, he has little time for far friends. Your interesting picture book from Japan is sent to Grönblad, I hope you will have it back in course of time. It seemed to be a book especially intended to limnologists.

Which part of the world are you going to visit this summer? I should not be astonished if you want to the swamps of Amazonas, I think there are many fine things waiting for exploration in spite of Grönblad. I am sure that your ^{coll. of} exploration will give very fine results; Grönblad has hardly time to write to me, so he must be eagerly working with your collections.

In the last months I have corresponded with Irénée-Marie who calls himself "your best friend in Canada". He seems to be a bit of a humorist under the cover. I asked him about certain details of his chloroplast pictures, and he answered that he sometimes draws them in order to fill up the space; the chloroplast being of no diagnostic value. He is very industrious, the advantage of being a bachelor (?). Do not tell this to your wife, please! Yesterday I got a thick paper from him, published in "Hydrobiologia". In an other paper he rejects Epinoclosterium on the basis of a very extensive justification. He seems to belong to the desmidiologists of the Old Testament, mentioned above. For him Ralfs' book is the Holy Bible, for him a biradiate monospinous desmid is an Arthrodesmus even if it is full of warts like triradiate Staurastra!

At this point of my letter, I think that you are weary of my chit-chat.

Thus, I finish with my best regards and do hope that you and your wife will have a very good and refreshing summer (without gallstone intermezzos!!).

Yours friend

Einar Ruzicka

P.S. your drawings are
and as wrapped.

Oct 27 1952

My dear friend Teiling,

For the last three months I have spent almost every minute of my spare time on preparing the MS for a paper on freshwater algae from North Australia, to be published next year (I hope) by the Australian Government in one of the volumes of reports on the 1948 Expedition to Arnhem Land. This is the northernmost peninsula of Australia, between Darwin and the western coast of the Gulf of Carpentaria, an almost unexplored region.

A couple of years ago I received five vials of material collected in this locality, and in them I found no less than 251 different desmids, and Prescott has listed 71 other algae. Although the samples were taken by the botanist of the Expedition, following directions which I gave him, he is not an algologist, and therefore I think that the large number of forms found in only five vials is quite remarkable. In the desmids there are quite a number of new species and varieties, and also many rediscoveries of species not seen since their original description half-a-century or more ago, by Borge, Nordstedt, Joshua and Playfair. Some of the species of *Euastrum* and *Micrasterias* are so strange that they were either omitted or excluded by Krieger in his monograph, as being either doubtful or possibly teratological.

Prescott made the determinations, or verified mine, and also wrote the descriptions, but the work of assembling the MS fell to me, because Prescott has run away to Point Barrow in Alaska, where he is spending several months. And this work turned out to be a much larger job than I had bargained for, involving a considerable amount of research in verifying names, spelling, original descriptions and dates, arguments by mail with Prescott regarding some of his determinations, etc., etc., etc. I wrote the entire paper three times, and even the third time the copy is far from perfect, but it is not finished and I shall mail it tomorrow to Australia, with the 26 plates of illustrations.

So that is the reason why I have written almost no letters for three months, and I hope you will understand and accept my apologies for neglecting you.

I heard from Bourrelly, no doubt after some gentle prodding from you. He wrote that he and some of his associates are much interested in my method of drawing on a horizontal surface with the microscope in an inclined position.

Friend Grönblad wrote me some postcards from Italy, and said that he was having a nice holiday, but that he had not been able to collect many desmids. This agrees with my experience in Central America a year ago. I think that you would have to be exceptionally lucky to visit a new country, and be able to find good desmid habitats quickly. It took me several years to find the best places for desmids even in Louisiana and Mississippi and Florida, which are close to home. Grönblad has sent me a lot of his drawings of my USA desmids, and I am to examine them and tell him my opinion as to his determinations. I can see that he is having considerable trouble with some of them, because as West & West remarked, the American desmids frequently show close similarity with the corresponding European forms, but in other cases they differ enough to make it difficult to decide whether they should be described as new varieties.

This year my wife and I have not even left the city of New Orleans. Partly this is because I have been very busy with my desmids, and partly because money is not so plentiful this year. I spent more money on that Central American trip than I ought to have done, so I have been trying to make up for it by staying at home.

We shall probably go away for Christmas, but since both my wife and I dislike cold weather, the only place we can go is south. And that means either southern Texas where there are no desmids and where you run the risk of encountering a cold wave, or Florida; so it no doubt will be Florida again. We don't want any more airplane trips for some time. I should like very much to visit Brazil, but it is too expensive, and I cannot spare the time for a visit that would be worth while. Recently I have exchanged a couple of letters with a limnologist in Belem, Para, Brazil, and have hinted that I would like to get some collections from his region, and have even offered to pay for them, but so far he has not taken the bait. I have hopes of getting some desmid material from Uruguay, from Dr. Müller-Melchers, who seems to be principally interested in marine diatoms. And two friends in Australia are trying to get more material from Arnhem Land and other parts of Australia. So together with Sachlan's Indonesian collections I have more than sufficient to work on for the next few years.

Glad to say that I have been in good health all this year, but my wife has been laid up in bed for the last two weeks with a very painful ailment, Herpes zoster. She is slightly improved today, but the doctor says it will be some weeks more before she recovers entirely.

I hope that you and your family are all well, and conclude with my best regards and good wishes.

Sincerely your friend,

April 19 1952

Dear friend Teiling,

It is good indeed to hear from you again. I had actually begun to think that you must be ill, as perhaps our friend Bourrelly is. On Jan 25th (my birthday) I sent him a long letter, together with photographs of my microscope set-up, describing my method for making horizontal drawings through the camera lucida although the microscope may be inclined. But he has not acknowledged it; maybe he is away on a trip to Madagascar!

Please do not bother to make copies of Wallich's drawings of Streptonema trilobum. I expect that they are no better than Turners, which are inaccurate and show the plant much too regularly geometric in shape. Actually the end view of a cell shows that the lobes are not circular as he depicts, but more or less elliptical, with a tendency to a "rotary twist" which is seen in many species of Desmidiium. I have sent you three samples from Indonesia, in one of which you will find specimens of Streptonema.

A couple of weeks ago I received a new lot of collections from my friend Sachian in Bogor, Java, who has written to you, I believe. These and the others that he sent last year contain the most bewildering assortment of unusual desmids that you could possibly imagine. Truly he is right when he says that Borneo is a "Desmid Heaven". But so are Java and Sumatra also. I have been working, or rather playing, with them three or four hours every night, and half the day on Saturday and Sunday, making drawings of the most interesting, and unfortunately passing by those that are well-known. Last night I was at the microscope until 1.45 am. It is fortunate that I have had plenty of practice in drawing these complicated Staurostroma and Euastrum, and that I know their anatomy pretty well. It is not too difficult to draw a desmid correctly when there are plenty of specimens that you can examine, but when you find a single example of one that is totally strange, and which must be drawn regardless of unfavorable conditions, the problem becomes much more difficult. Many of them I can identify from Krieger's Sunda paper, of which I have the plates, and from Skuja's Rangoon paper; but there are quite a lot that are quite unknown to me, and that I simply have to put aside for Prescott's attention, whenever he can get time to do it.

I now have copies of both of Bernard's papers, but am disappointed that his drawings are not very good, and many of his determinations are inaccurate. I checked in his 2nd and shorter paper those genera that have been treated in Krieger's monograph, and find that Krieger has changed the names of nearly all of them. In his introduction Bernard admits that when he was unable to find a desmid in the literature available to him, he gave ~~it~~ it a new name, expecting that in many cases these new names would have to be changed.

I am in need of additional literature on Indo-Malayan-Indonesian desmids, and if you ever see any of the following works offered for sale, I wish you would buy them for me. By the time I get catalogues from Brill, or Asher, or Wepf, and send an order by airmail, the books that I want have already been sold.

- W. & G.S. West, 1897a, Desmids from Singapore.
- 1901, Flora of Koh Chang.
- 1902b, Freshwater Algae of Ceylon.
- 1907, Freshwater Algae from Burma.

In this Indonesian material I have come across a most extraordinary case of dichotypy, or rather "tetrakotypy" (good word?), in which Staurostrom Wildemanii forms dichotypical cells with three other forms. Each of the three forms also exists as entire cells, and each of them would be classed as distinct species. In fact

one of them is St. subtrifurcatum var. major. The other two are unknown to me. I intend to write a separate paper on this interesting case after I have studied it more, and after I have ascertained whether the two unknowns have been published before. It will require a considerable number of drawings to illustrate all the different "species" and their combinations.

Among the drawings of Multiredioste Staurostria that you returned to me some time ago, there was one showing a single semicell that I thought might belong to St. leptacanthum, but which more likely is a variation of St. tohopekaligense. On the drawing you have noted "St. gemelliperum var. longibrachdatum fa. retroflexum, nor. var. Teiling". I intended to write you about this, but the drawing became mislaid and has just turned up again. I hope you will forgive me for saying that I think you are mistaken in this identification. It is much too large for gemelliperum, and further it is asymmetrical with regard to the three long lateral processes. In my view it is to be regarded as an aberrant (not abnormal) specimen of topokaligense, either of the 3 plus 6 type with one extra lateral process on each of the three sides, or of the 9 plus 6 type in which lacks one of the usual two lateral processes on each of the three sides in the lower whorl. I am returning the drawing, on which I have marked in red the radii of symmetry, and in blue the axes of the lateral processes. You will see that the red and blue lines do not meet in a common center, as would have to be the case with gemelliperum.

I searched the collection in which this specimen occurred, but could not find any more examples, either of the normal or the aberrant form. But an exactly parallel case occurs in several forms of St. furcatum. Many years ago I found my first example, and suggested that it might be named St. furcatum var. semifurcatum, by analogy with St. vestitum var. semivestitum. It might equally well be named St. furcatum var. semifurciculatum. Since then I have found several more examples in my American gatherings, and in the Indonesian material such cases are fairly common. I am enclosing three sketches of these Javnese forms, and ask that you return them after you have examined them and compared them with the sketch of the Florida plant.

Grübled has written me several times lately. He is actively working on my material, and is now examining the Florida collections. I'll bet they are giving him a lot of fun, and also a lot of trouble in trying to decide of names for many of the new and strange species and varieties.

As you no doubt are aware, Ruzicka is now working at the Central Biological Institute, Sedlice u Blatne, Czechoslovakia. I am very happy to know that his troubles seem to be over, for the present time at least.

A few days ago I sent you a package of coffee that I hope will arrive safely, and will give you a lot of pleasure.

With best regards to yourself and your wife, from Mrs. Scott and me,

Sincerely your friend,

P.S. I am sending a little book from Japan, sent to me by Minoru Hirano, and I think you will enjoy seeing some of the desmid habitats of that country. After you have looked at it, please send it to Grübled, and ask him to return it to me.

Linköping 30/III 1952.

Dear friend Scott,
I think you believe me dead and buried since I have not given any sign of "letterary" activity. I am, however, quite well but I am so busy in writing my chloroplast-paper that everything else is put aside, only the viola-playing is maintained as a salubrious recreation, it is the only movement of my body except cycling to and from school and eating.

In Februar we had a one week's vacancy for ski-running and other athletics of winter. I spent it in Lund where I studied in the archives of Nordstedt. Very interesting to read the letters from Hütkenmüller, one of the most prominent desmidiologists ever lived, not so much as a biosystematist but he has, in his excellent investigations of the desmid cellwall, given a solid basis for the taxonomy. This lead to his remarkable revisions of Penium and Cylindrocystis, genera previously forming a conglomeration of species, quite different as to their origin. It was a regrettable loss when he was struck by a sudden death in 1913. I was especially interested in his examinations on the chloroplast. His delicate drawings are now in the possession of the University of Vienna. He had, however, sent copies to G.S. West and I had hoped that there would also be copies in Nordstedt's archives. There were sorry only a few of them, it should be of very great importance for me to study them but I suspect that an opportunity of going to Vienna is out of my possibilities.

I have a bad conscience of not having copied the figures and diagnosis of Streptonema which I have promised you, but it is a laborious entertainment to change my studio (with four windows!) into a dark-room.

I am glad that you captured Bernard's paper. A monotypical genus is in itself quite entitled. There are several desmids worthy of such a fate. I have just erected Hyalotheca neglecta to a separate genus. This quite correct because of its laminated chloroplast, quite unique beyond the saccodermas desmids and the Zygnemaceae. I consider for instance Staur. insigne and Cosm. annulatum worthy of separate genera. The desmidiologists are almost exclusively interested in species, many of the taxonomic changes in the traditional list of genera seem to provoke them; Notocosmarium is rejected and Euastridium is not generally accepted, G.M. Smith does not mention it, but he swallows Spinodlosterium without hesitation!

I am glad that your samples from Central America are better than you thought at first.

My thanks for your remembering my birth-day, I should be glad to know yours! I am longing for my next which gives me the right of becoming a pensionary. I do hope I will be still alive then and capable of working some years after, so that I can finish the investigations which now are waiting for its getting finished.

Very nice of you for your care and attention of my coffee vice. Now coffee is free for sale, but I must confess that a rest of your delicate coffee still remains in our house-keeper's treasury, she ~~uses~~ uses it to give a more full-flavoured taste in our usual decoction.

Our common friends are silent, probably my fault. The other day I received some papers from a Mr Thompson in Kansas, on new interesting Protozooids and Pyrrophyta. The contributions to my library are very scarce, except the fine volumes you have sent me. The check on my delight at it is, that my dept of gratitude to you will become too great and increases with every sending of the "Science" which arrives.

Here is still winter, it is cold and some snow, it is true that March is labelled the "spring-month" in the calendar. This is a wrong optimism, May is the real spring-month and not seldom cold nights with a temperature below the freezing-point occur at mid-summer.

With my best regards to you and your wife,

Your sincere friend

TELLING

Feb 24 1952

Dear friend Telling,

Immediately upon receipt of your letter of Jan 21st, I wrote by airmail to Wepf & Co. in Switzerland, asking them to send me the two papers by Bernard and Krieger that you mentioned. I have just received Bernard's paper from them, but the one by Krieger had already been sold. However, that does not matter so much, because I possess photo copies of the plates of Krieger's Sunda paper, and a microfilm of the text. But it is so inconvenient to project the microfilm that I very seldom use it, though there is a lot of useful information in this text.

It was very good of you to take the trouble of telling me that these two papers were listed in Wepf's catalogue. I am glad to get Bernard's work containing the original description of his Spinoclosterium curvatum, which you were kind enough to copy for me some time ago. Recently I have a lengthy argument, by correspondence, with Prescott, G.M. Smith, Whelden, and Hirano, trying to convince Prescott that the correct name for this plant is Closterium cuspidatum Bail., but Prescott, Smith and Hirano hold to their opinion that the possession of spines is a character sufficient to justify separating the plant from the genus Closterium, and establishing a new genus to receive it. Smith lists it in the 2nd. edition of his F.W. Algae of U.S., and wrote me that his failure to mention Bailey's name as published by Reifs was due to ignorance, but is of the opinion that Spinoclosterium is correct. Hirano has made a new combination, Spinoclosterium cuspidatum (Bail.) Hirano, which Prescott accepts. Whelden is undecided, and thinks that perhaps my contention is justified, that it is unnecessary to create a new genus to receive only one desmid, whose only difference from Closterium is the addition of terminal spines. Grunbled and Irénée-Marie are strongly of the opinion that it should remain in Closterium, and I gather from your letters that you agree with them.

Bernard's work establishes a number of new species and varieties, but in checking the genera Closterium, Penium, Docidium, Pleurotaenium, Buastrum and Microsteries with Krieger's monograph, I find that with three exceptions Krieger has reduced all of them to synonymy. The three exceptions are Buastrum elegans var. brevius Bern., which Krieger regards as doubtful, and Microsteries lux var. Grameri and M. rotata var. Treibii Bern., not mentioned at all by Krieger. If Bernard knew of this I imagine that he would be turning over in his grave.

There is nothing new with me. I have almost finished examining my Guatemala and Panama collections, which turned out somewhat better than I first thought, with something like 85 desmids total in 19 collections, and most of them are so scarce that it has taken me two months to find the 85. I could list that number or more in one hour from some of my rich Florida gatherings. I have one new species of Buastrum, and two Cosmaris that may be new species, and quite a number of additions to Prescott's list from Panama.

With my best regards, and kind wishes for your 64th birthday, I am,
Your sincere friend,

P.S. Would you like me to send you some more coffee?

Linköping 21/1 1952.

Dear Friend Scott,

many thanks for your letter the other day. Now I write because of a catalogue just arrived from Switzerland. Now it is very good times for antiquarian libraries after the general destruction during the war. If you shall order something, no delay. Well, the catalogue contained two papers, probably of interest for you, viz. Bernard and Krieger, both dealing with tropical desmids. Bernard contains 186 figures of which about 125 are desmids. Krieger has lots of figures on the 21 plates and also the text is very extensive.

If you should like to procure them, the librarian is Wepf & Co. Eisengasse 5, Basel. The catalogue is nr 146, Botanik and the books:

581 Bernard, Ch. Sur quelques Algues Unicellulaires d'Eau Douce, récoltées dans le Domaine Malais. Buitenzorg 1909, 4^o
94 p. 6 pl. broschiert 10:- Schweiz. francs

618 Krieger, W. Die Desmidiaceen d. Deutschen Limnolog. Sunda-Exped. Stuttgart 1932, 102 Seiten, 21 plates 18:- Schweiz. francs.

My best greetings to you and your wife!

Just in a hurry, yours sincerely

Einar Tulving

LIBRARY
MICHIGAN
UNIVERSITY

MASSACHUSETTS
HARVARD
UNIVERSITY
VERONA

Jan 12 1952

Dear friend Teiling,

This is the first opportunity I have had to reply to your friendly and chatty letter of Dec. 16th.

On our plane trip through Central America we visited the following cities: Guatemala City, Guatemala; San Salvador, El Salvador; San Juan, Costa Rica; Panama City and Colon, Panama; Medellin and Barranquilla, Colombia. In addition the plane stopped at Tegucigalpa, Honduras; and Managua, Nicaragua; but we did not stay at these places.

For many years I have looked at these countries on the map, and wished to visit them. Since they are so close to the United States it seemed very easy to go there, but it was not until last Autumn that I could spare the time (and the money) to do so. It has been a wonderful experience for me and my wife, but now that it is over, neither of us has any desire to go back there. There are several reasons for this. For my wife's part, she is rather "finicky" about many little things, such as hotel rooms and beds, restaurants, and shops, and constantly compares them with the fine hotels and meals that we get in the United States. These Central American countries were settled by the Spaniards long before this country, but they have not developed so rapidly nor to such a high point. So the hotel accommodations, by American standards, are distinctly second-rate, and the shops resemble those in some of our small country towns.

For my part, I should have enjoyed the trip much more if I knew more of the Spanish language. I can read enough of it to get the gist of the news in the daily newspapers, or to understand a scientific paper on desmids, but I cannot speak it nor understand the spoken language. Consequently, everywhere we went, it was necessary to engage an English-speaking guide and an automobile to take us around. This proved to be very expensive, much more so than I had anticipated, because all the guides in all the countries were lying in wait for American tourists. In San Salvador, for instance, (a dirty little town), the guide wanted \$35.00 U.S. to take us on an automobile trip. After I got the hotel manager to argue with him in Spanish he reduced the price to \$20.00 at one jump, which I figured was still much too high, and therefore we say very little of that town, and left the next morning.

Again, I thought we should be able to live more cheaply in Costa Rica, where the rate of exchange is 7.40 Colones to the U.S. dollar. But the hotel room still cost ten dollars, and everything else was in proportion.

It was a great disappointment to me that my desmid collections are very poor. The reason, of course, is the scarcity of habitats, and the unsuitable soil and water conditions of those that I did find. The regions that we visited are highly mountainous, and more or less arid, with very few lakes, and none of the roadside ditches, ponds and swamps that I am accustomed to finding in southern U.S. It appears that the parts of these countries that are best developed, and that have an adequate road system, are the upland valleys and plateaus, at elevations of 2000 to 5000 feet, no doubt because of the more comfortable and healthier climate. The large lowland areas between the mountains and the Caribbean Sea have no roads at all, and can be reached only on horseback or muleback. There are a very few roads running down to the Pacific coastal plain, and I made a special trip from Guatemala City to a little town named San Jose on the Pacific coast of Guatemala, (not the same San Jose that is the capital of Costa Rica). This road ran for 25 miles or so across the coastal plain, and I made a few collections from roadside

ditches and ponds. But I could tell at the first glance that the waters were unsuitable for desmids, and this was confirmed when I examined the material after we returned home. They contain only three or four of the commonest Closteria and Cosmaria, typical of hard waters.

We visited two large lakes, Amatitlan near Guatemala City, and Ilopango near San Salvador. Both of them seem to be of volcanic origin. Amatitlan has a very meagre growth of aquatic plants, from which I made squeezings at two locations, with the same result as above. Lake Ilopango had no visible aquatic plants of any kind, just a bare stony bottom and shore, so I was unable to get a collection there, not having a plankton net with me.

Prescott has given a very long list of algae and desmids from Panama, most of them from Getun Lake, which was formed by damming the Chagres River when the Panama Canal was built. I had high hopes of getting some nice desmids there, but it turned out that Getun Lake is inaccessible by road, and could be explored only by boat, and I did not have time for that. I made a dozen collections from the few ditches and ponds that I found, all of which are very poor, and five collections from five different lily-ponds in the Experimental Garden at Summit, mentioned in Prescott's paper. Some of these contain a fair assortment of desmids, as rare and scattered individuals, and I shall be able to add a few names at Prescott's list, perhaps even a couple of new varieties. An unusual one is Cosmarium hexalebum, previously known only from Northern latitudes, I believe.

Medellin in Colombia is at an elevation of 5000 feet, in a valley between two of the lower ranges of the Andes. I found literally no waters there except a few rapid mountain streams. Flying from Medellin to Barranquilla on the northern coast of Colombia we passed over a very large swampy region, that must cover thousands of square miles, but it would be extremely difficult to reach this almost unexplored area. I had intended to stay in Barranquilla for a couple of days, to explore two large lagoons that appear to be accessible from the road that runs from Barranquilla to Cartagena. But I was so disgusted at having to pay \$25.00 per day (with meals) for a room in a second-rate hotel, with unappetizing meals, that I left by plane early the next morning, and never saw the lagoons except from the air.

United States citizens can travel in any of these countries on "tourist cards", without needing a passport. The one exception is Nicaragua, which refused to grant me a tourist card because I am a naturalized citizen, instead of native-born. So I could not visit that country. But our plane flew along the shores of the two large lakes, Managua and Nicaragua, and I got an excellent view of them. These lakes are not of volcanic origin, but are said to have been connected with the Pacific Ocean a long time ago. The water near the shore appears to be quite shallow, and I saw a belt of aquatic vegetation parallelling the shore; also I saw a number of lagoons near the City of Managua, which might be favorable collecting places. Some of these lagoons can be reached by road, while the lake shores would require a boat in most places. So if you know of anyone visiting that country, you can tell him about this.

So that's the story of our trip. Both of us enjoyed it immensely, but we are glad to be home again, and I doubt if we shall ever return there.

With best regards from Mrs. Scott and myself,
Sincerely,

Linköping 16/III 1951

Dear Friend Scott,

many thanks for your letter, your pleasant cards, wrappers and, last but above all, Smith's big book, extremely valuable to me, I have had the first edition borrowed from the limnological institution of Lund but it had been requested back. Tell me what I can do for you in return! At present there seems ~~not~~ to be little opportunity of getting papers on tropical desmids, but I am keeping my eyes open for it.

I have sent the photos of Brébisson to Brunei and also to Grönblad. I hope I may be able to take copies of a picture of Ralfs whose necrologue I have read. I have always been curious why Ralfs did not publish further investigations on desmids was ~~his~~ "The British Desmidiaceae" his swan-song? Actually he lived right on to 1890, 83 years old! Since 1831 he suffered from insanity of his lungs and often spent years in the bed of sickness, a tenacious boy like field-botanists in general. He was a successful surgeon in the prime of his ^{days}. He worked with phanerogames, cyanophyceae diatoms, and marine algae. His studies of desmids occurred in 1841-48. He is described as an extremely noble and unselfish, silent man, well known and much respected in Penzance where he lived in the most convenient climate. Nordstedt visited him and this noble man here met a congenial personage, he spoke about him with the utmost appreciation.

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I think Bourelly would like to get your device for drawing on a horizontal surface. He is an ingenious man, apparently keen on methods simplifying his work.

Prescott's book is of great value as a comprehensive text-book on non-tropical algae. Except Pascher's "Süßwasserflora" and, of course, Rabenhorst's new edition, it is the only comprehensive text-book and, being of moderate size it is very convenient, especially for elementary use. I think, it will successfully promote the study of freshwater algae; new-beginners hesitate when brought up against the difficulties of identifying the manifold of their subjects. It is horrible that Prescott's immense output of work is "sold for a mess of potage". Scientists are obliged to be idealists and work for the sake of honour. I understand your indignation and I supporte to the full. I am sorry that bad circumstances prevented P. to be present at the botanical congress, he wrote to me with a tinge of sadness that he was obliged to give it up.

I am very glad to hear that you have another paper in the press. I am not a subscriber of Hydrobiologia, it is a little too expansive, the zoological papers are of little interest for me and the most part of the algological ones are sent me by the authors.

Last week I went to Lund in order to attend the defence of a doctor's dissertation by a female doctorand, a pupil of mine. The investigation dealt with the vegetation of Scania lakes (Scania is the most southern part of Sweden). The treatment of the algae, littoral and planktonic, was purely ecological and the biosystematical content was fairly poor, of course no new or critical species. She has succeeded by the aid of her energy and does not possess the real scientific mentality. She is, however, the first female doctorand of botany in the University of Lund. It was interesting to be present and not at least, at the following "doctor's dinner" an excellent opportunity of meeting, chatting and gossiping with possibly many botanists within a limited space of time.

I have just sent the most part of a paper on the desmid chloroplast to Mr Lund at the Laboratory of Fresh Water Biological Association in Westmorland for scrutiny. Have I told you that he and his wife, an excellent specialist on water fungi of fresh water algae, have a little son, bearing the name of Einar. They have evidently been infected by a predilection of this name and I think the little boy is unique of this old Nordic name, once born by a famous wiking, especially known as an eminent archer.

I hope you had a good journey in Central America with a successful hunting of algae besides the business. It is a pity that our time needs iron more for war than for useful things, but when will the evil be so great that it is a good thing that U S is capable of resisting.

At last I heartily wish you and your wife A peaceful and Happy Christmas and A Good and Successful New Year.

Your friend

Einar Östling

2 = Soviet

Linköping 27/X 1951.

Dear Friend!

Thousand thanks for the thick volume of Freestott. I have not scrutinized it yet but it is a very good work of energy and industriousness very welcome to every student. I intend to write a review of it in Bot. Not., a very great deal of it is applicable to the Swedish algal flora. We have never had a text-book on fresh water algae, the public interest of this subject is too small for a text-book in Swedish. The good work of Pascher's Süßwasserflora is out of print, several parts are out of date and the desmid part has not appeared. Lütkenmüller died before his finishing the task and Grönblad who promised to finish it backed out. Now, Messikommer is occupied with it.

You had found many small incorrectnesses and incompletenesses in Smith's book. As far as I have seen such things are not uncommon in Prescott's book. It is inevitable, such a compilation involves an immense literary knowledge, more than one man can perform in a reasonable space of time, it presupposes years of excerpt-making. I have got a specimen of Manual of Phycology, sent by the Editor (Chronica Botanica) for reviewing. It contains nothing of biosystematy. As to Smith's Fresh water algae of USA I have not received any answer of my letter to him. I know that he is a lazy correspondent. In a more urgent matter several years ago I must, finally, write a rather reproaching letter to him. Then he answered very courteously and sent slides. During the Congress I met him at a dinner arranged by Borge's daughter.

I have borrowed the paper of Walllich on Desmidiaceae from Lower Bengal with the original description and figures of Streptonema. Just now I have a terrible hurry but soon I hope to make photocopies for you.

I hope you are all right, that you are still alive I have seen from the fine journals you kindly are sending me here we have had a marvellous weather, an ideal "Bridget summer" or "Indian summer". The first name alludes to Sainte Bridget, our famous female saint and poet, who was celebrated in the middle of september. The latter alludes to the red autumn leaves.

We are suffering from the economic inflation but the coffee is free now, happily. I have been engaged in the civil defence organisation which takes much of my time, sorrily.

I hope you have got the picture of Erbsisson which I sent the 2. of September and a letter. I just have found a specimen of Wittrock and Nordstedt: Desmidiaceae and Oedogoniaceae from Tyrol. Have you any use of it? It contains a lot of Cosmarium species and some Staurostraa.

My sincerest greeting to you and to your wife,

Your friend

Einar Teiling
Einar Teiling

Linköping 2/IX 1951

Dear Friend! Many thanks for your letter of 30/VI and for the frequent and valuable sendings of Science and Scientific American. I suppose that you have understood that my long silence only is due to my long absence from Linköping. I am from an English dictionary! Now I am back in the town and school has begun.

I thank you very much for the pamphlets on Eastmann's special photopaper. I have since long made such photocopies and have also built an apparatus for it but the positive copies will not be rich in contrasts enough. I had an old camera and made an oblique mirror in front of the lens, your copies were the first I made in this manner. This method is simpler and also cheaper; there is need of one paper only. I would be more glad if I were able to render you a service in return. At present I have no doubts, but it is a slight possibility of procuring a specimen of Lundell's classical work if you do not possess it. It seems to be rather rare now, the biggest libraries of second hand books do not have it. I have not got any answer from Smith, but I know that he is a bad correspondent. The new edition of his Algae is very desirable and later on I should be very grateful if you sent it me, provided that I am allowed to pay for it.

Now to your problems dealt with. It is a peculiar manner of the desmidiologists to "strain at gnats and swallow camels". On the one side they distinguish certain forms as genera which show very small differences from adjacent genera, Phymatodocis: Desmidium, Docidium: Pleurotaenium e.g., on the other side they deal with pure names as if these were real genera, for instance the veritable heaps of rags: Cosmarium and Staurastrum. These "genera" are taboo and must not be touched upon! As you are well aware of it is impossible to give any limiting diagnose of Cosmarium, it contains a lot of phylogenetic branches very different, but also some groups very similar due to convergency, for instance the ones which are circular a vertice. As to the value of Staurastrum it is enough to observe that such a clever man as Skuja labels a genuine Cosmarium as a Staurastrum because of its triradiate shape. As I hope to have showed Artispedanus is no genus at all, I think I have at least 30 of all figures published by conspicuous desmidists in my collection and there are certain forms with a thickening of the frontal cell-wall. Such thickenings, often with ornament, are to be found in "Cosmarium", Ranthidium, Staurastrum and (Micrasterias), thus no exclusive character.

You are right in your suspicion that Streptomena may be a good genus. At Turner's time the connecting processes were considered significant characters, and the satisfaction at having discerned a new genus has always been a creator of new genera (as well as species!). This tendency is dangerous in that way that it checks the criticism of the inventor, but it has also good effect: it directs the attention on the novelty and stimulates other scientists to control it. This is often the way to truth. I have ordered the original paper of Wallich from the library. When I get it I will send you copies of the figures. I don't know any more original figures except Turner's.

You are also quite right in your observation that the desmidiologists, even the more prominent, best e.g., have omitted to consider the mucous connections. We say in Sweden something like this: "What is not in eye, is not in mind." I think you have a corresponding proverb. It is astonishing how elucidating mucous staining or adding of India ink is on the apprehension. The extremely excellent scientist Lütkenmüller, one of the best in the world, has given a good lesson in this respect in his "Der Zellmembran der Demidaceen" p 365-367. I enclose a figure copy of his figure of Onychonema filiforme.

My samples from Lake Victoria are very poor in desmids: Staur. limneticum, leptocladum, muticum, planctonicum, setigerum, Tohopekaligena, leptodermum var. subcorniculatum and some other, not yet named. These are very scarce and are hardly to be seen in

the enormous porridge of *Micromyctis aeruginosa* and *Protococcoides*. Several endemic diatoms are there also. When I am to re-examine the material I will select specimens for you. At present I am writing a typology of the desmid chloroplasts, a very interesting study. The results confirm my theory of the phylegenetic development of the radiate desmids in a flattering manner.

I beg your pardon for my idle expression "unguilty". In English it should be "unvalid". My wrong word depends on a verbal contamination, the word unvalid is in Swedish written *oviltig* (o-vil-tun).

Some weeks ago I had a card from Dr. Lund in England. His wife had reached just give him a boy and he hurried to tell me the name: Richard Einar. Apparently they are fond of my old Nordic name. Dr. Lund is a very clever

scientist, specialist of parasitical water-fungi on fresh-water algae. He wrote that he had been down in his mind, he intended to divorce. His wife has always been ill in health.

I am very glad to hear that your gall-bladder trouble is better. I hope that the recovery will continue.

My best greetings to you and to your wife.
Sincerely yours

Emil Sjöström

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...the connecting processes were considered...
...the inventor, but it has also good effect...
...the inventor, but it has also good effect...
...the inventor, but it has also good effect...

...the more prominent...
...the more prominent...
...the more prominent...

...samples from late...
...samples from late...
...samples from late...

Nov 4 1951

Dear friend Teiling,

I owe you an apology for not answering your letter of Sept 2nd, and I now have yours of Oct 27th. The fact is that I have been very much preoccupied with business worries. Our business depends on steel which we fabricate, and this metal is in such demand that all supplies are allocated by the Government. I had to fly to Washington, D.C., and spend a week there arguing with the government officials, trying to get our allotment increased. I did get an increase, though not as much as I had hoped for, but now it proves almost impossible to get steel from the mills which roll it. They are literally swamped with defense work and with orders from the very large users like the automobile industry. A small concern like ours has very little chance of getting the steel that they need, and the result is that we shall have to close our plant, or at least reduce our operations very much, by the end of the year.

The photograph of de Brebisson, and those of Bourrelly and yourself arrived safely, and I thank you for them. I must confess, however, that I like better your picture taken at Wray Castle, which is now framed and hanging on the wall of my microscope room, with those of Grönblad and Nordstedt. Did you send a copy of De Brebisson's photo to Jules Brunel? He has a collection of photos of a great many algologists. If you did not, I can have it copied here and send it to him. I was glad of Bourrelly's picture; it is always good to know what your correspondents look like. He has a very nice Reichert binocular microscope, but I see that he is using an inclined drawing board, which is a nuisance. Do you think he would like to know about my device for drawing on a horizontal surface with an inclined microscope? If so, I shall gladly tell him about it. In my opinion it is much easier to use than the inclined drawing board.

Many thanks for your offer of Nordstedt & Wittrock's paper on Tyrol, and your offer to try to obtain a copy of Lundell's classical work. But these are so old that I do not think they would be of much use to me, and further they deal with European desmids, while I am interested in tropical forms. These works would be much better used by a taxonomist, to whom of course such old papers are quite necessary. If you ever have a chance to get such papers as Gutwinski, Java, 1902; Joshua 1885 and 1886; G.S. West, Yan Yean Reservoir, 1909a; W. & G.S. West, Singapore, 1897a; Ceylon, 1902b; Burma, 1907; Wille, Samoainseln, 1914, I should be delighted to have them, and will pay any reasonable price for them.

Glad that you like Prescott's book, and hope you will give it a good write-up in your review, of which I should like to have a copy when it is published. I cannot criticize it, since I know very little about any of the algae he describes. Of course, there must be errors in it, as can be found in any large work of this nature, and there will be disagreements with his identifications. I was somewhat surprised at the excellence of his drawings; I did not think he could draw so well, because some of his desmid drawings are not particularly good. He has spent an immense amount of time and labor on it, and I know that he has been trying for the last few years to get it published. He was unsuccessful in finding a commercial publisher who would produce the book, so he finally had to agree to Cranbrook's terms. You will have noted the statement that neither the author, publisher, nor printer derived any profit from the book, which is a fixed policy with Cranbrook. But Prescott tells me that his total reward is FOURTEEN FREE COPIES, with the privilege of buying some more at an author's discount, whatever that may mean. This seems highly unfair to me. The typesetters, printers, and bookbinders certainly had to be paid their hourly wages of not less than \$2.00 per hour. Is the author's time alone not worth that much? And considering that nobody made any profit, that many of the

zinc cuts for the plates were furnished by the Univ. of Wisconsin, and that publication was aided by a grant from a multi-millionaire, the price of \$11.00 seems unreasonably high.

I ordered a copy of Smith's 2nd. edition of F.W. Algae of the U.S. to be sent to you, and have just been notified that it was despatched on Oct 29th, so you should receive it soon after this letter. If you review this, I should like to have a copy of the review.

By the way, the photo of Prescott on the jacket of the book is a very good likeness, but it has been retouched, and makes him appear as if he had received two black eyes in a fight! Prescott wanted badly to attend the Congress in Stockholm last year, to meet you and other European algologists, but was unable to afford the expense, which was a great pity.

I have just returned to Prof. van Oye the corrected proof for a new paper on Eusstrum by Prescott & myself, so it ought to appear in the next issue. If you subscribe to Hydrobiologia perhaps you may see it before I do, since I am not a subscriber, though I see the journal through the courtesy of a friend in the Louisiana Dept. of Wildlife & Fisheries.

The best wishes and regards to you and your wife, from

Your friend,

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Dear friend Teiling,

It was very good of you to copy for me the pages of Bernard and his plate showing Spinoclosterium curvatum. But his remarks make the question somewhat more complicated. I was not aware that this plant had been referred to Ophiocytium and to Reinschiella, until I looked it up in Nordstedt's Index. So Bernard had some justification for establishing his new genus, because of the confusion caused by Rebenhorst's and De Toni's belief that Bailey's plant was not a desmid. But Nordstedt apparently accepted it as a Closterium, since he lists it under Bailey's name. So we have Bailey, Ralfs, and Nordstedt among the old-time authorities, and at least Grönblad and Irene-Marie among the moderns, who think it should be included in Closterium, and only Prescott (so far as I know) among the moderns who thinks that the terminal spines are sufficient reason for excluding it from Closterium and establishing a new genus for it.

There are parallel cases in other genera. Archer found Fleurotaenium (Docidium) Keyei in 1865, but did not establish a new genus for it, though the generic diagnosis of Fleurotaenium did not at that time contemplate any spines. Again, the original generic concept of Cosmarium did not provide for spinous species, I believe, but today there are several species possessing spines, from C. ovale with short blunt teeth, through C. Kloiseanum Wolle with long conical teeth, to the C. denticulatum Borge from Australia which has true slender spines, and var. perapinosum Grönbl. from Brazil which is spiny all over. I have just seen some examples of Grönblad's variety in new material recently received from Peru. In Micrasterias there is Joshua's M. ceratophora, totally different from any other known species, with long slender spines like those of Arthrodesmus incus var. validus. When I first saw this plant from Australia I did not know it had previously been found by Joshua, and I hesitated between Arthrodesmus and Micrasterias, finally deciding on Micrasterias. I had tentatively named it as a new species, until, Prescott found that it had been described by Joshua in the 1880's. Again, Arthrodesmus is not supposed to have any differentiation of the wall in the center of the face, and G.M. Smith in his 2nd. edition of his F.W. Alg. of U.S. still quotes this, but there are several species of Arthrodesmus with internal thickenings of the wall, similar to those in Xanthidium.

It seems to me that with increase in our knowledge of desmids, and the discovery of new species that do not fit exactly into the established generic concepts, it is necessary to make revisions in these concepts from time to time, and that it is unnecessary and undesirable to found a new genus merely to receive a single non-conforming species. I regard Spinocosmarium Presc. & Scott in this way, and have always thought that it should be placed in Xanthidium. Streptonema trilobum Wall. is another case, on which I shall have more to say later on. It shows a remarkable resemblance to Spondylosium pulchrum (Baill.) Arch. both in the form and size of the plant, and in the method of formation of the zygospores. In fact, the only difference between the two plants is that Streptonema has three apical swellings which secrete the mucous connectors between adjoining cells, while Sp. pulchrum has only one swelling, which during development is infolded inside the cell, like Streptonema, Desmidium and Bambusina. Do you know of any other illustrations of Streptonema besides those in Turner's F.W. Algae of East India? I mean original illustrations, not copies.

I note that you made your copies of Bernard's paper with your camera. Perhaps you are unaware, as I was until a couple of years ago, that there is an easier method, called reflex copying, by which contact prints, full size, can be made of any drawings or printing or writing, even on opaque paper, or even pages of books printed on both sides. Ruzicka is the one who told me about it, and he sent me several papers that were copies in this way, without any camera. I have sent you some pamphlets of the Eastman Kodak Company, describing the special papers that are

Your experience with Iyengar agrees exactly with mine. I sent reprints of out last two papers to Iyengar and three other Indian algologists who are members of the Am. Phycological Soc., with a request for exchange of papers, but have not received anything from them, not even an acknowledgement.

Prescott has in press a new book, *Algae of the Western Great Lakes Area*, and I have ordered a copy to be sent to you by the publisher as soon as it is issued. Enclosed is an advertisement, from which you will note that he has excluded the distoms and desmids, much to my regret.

If you do not get a copy of Smith's 2nd. edition from the publisher, let me know, and I will have it sent to you. Do not worry about the cost; I can easily afford to buy some presents for my friends. Here are some more criticisms of statements in his book. On page 319 he says that *Pl. subseronulatum* var. *detum* has the cells permanently united in filaments by an interlocking of the apical tubercles. This is not correct. The tubercles on adjacent cells intermesh like the teeth of gear wheels, but they do not and cannot form an interlock. The adhesion between cells must be by means of mucus secretion, as in other genera like *Sphaerocapsa*, *Spondylium* and *Phymatodocis*.

Of *Onychonema* he has repeated a statement made by several authors, that the apical processes overlap the adjoining cell "and so firmly unite the cells one to another". Mechanically this is impossible, since the slightest counterclockwise twist of one cell with respect to the next would immediately "unlock" them and the cells would fall apart. I think it would be found, if one cared to investigate, that there is a mucus layer covering both the cells and the apical processes, and in that case the processes would greatly strengthen the bond by increasing the resistance to twisting of the filament, and the capitate ends of the processes would strongly resist any longitudinal pull caused by movements of the surrounding water, or by animals. Even with the complicated apical processes of *Microsterias foliacea*, there would be no resistance to a longitudinal pull, though there is a true interlock against a twisting force. In Smith's illustration of *O. laeve* var. *letum*, the position of the apical processes is reversed. With the cell in a vertical position, the upper process is always (according to my observations) on the right-hand side of the upper semicell, and the left-hand side of the lower one.

It is no longer true that *Phymatodocis* "is one of the rarest of desmids found in this country". Though never plentiful in any one collection, it occurs in so many of my gatherings from Louisiana, Mississippi and Florida, that I regard it as more common than *Triploceras* and *Cosmoecidium*, for example. In 1942 W.C. Frohne published a paper "Notes on *Phymatodocis Nordstedtiana* Wolle from South Carolina and its var. minor Børgesen from Georgia", Trans. Am. Microsc. Soc., (61)4; 438-441, in which he gave a figure of conjugation between two filaments, not separated into their individual cells. I have a rather poor sketch of a similar occurrence, the only case that I have seen.

As regards his statement that walls of *Desmidiaceae* are "smooth and without striae, punctae, or spines", there are at least four species that are prettily decorated with concentric rings of large to small pores around the semicells. Three of them are common in U.S.A., *D. Grevillii*, *D. quadratum*, and *D. coarctatum*. The fourth, *D. bengalicum* I have seen from Australia and Indonesia. In *D. Grevillii* the pores are large and easily visible, in the others more difficult.

Smith continues to use the name *Gymnozyga*, though *Bambusina* is the *Nomen conservandum* for this genus, according to the International Rules of Nomenclature.

"Cells of *Arthrodesmus* are always small", page 329. *A. maximus* is a large species, more than 100 μ with spines, and I have found it in several collections in USA, though Smith does not ~~know~~ know it yet.

"Cell division in *Desmidiaceae*, as well as in *Gymnozyga* and *Streptoneema*, is

different from that of other Desmidiaceae", page 334. This is by no means certain for *Streptonema*, but requires more investigation. In the few cases of division that I have seen I believe that it occurs in the typical manner, and that only the apical swellings are infolded. But I have not yet seen the very early stages of division, and the one collection in which dividing specimens occur the filaments are badly infested with the epiphyte *Peroniella*, so that observation is difficult. Note that Turner's illustration of division in *Streptonema* merely shows the apical swellings infolded. This occurs also, as I have mentioned, in *Spondylosium pulchrum*.

Thanks for your interest in my gall-bladder trouble. Now I have almost recovered, and am back on a normal diet, but with a warning not to eat too rich food. The doctor keeps me on a "no-fat", or rather "low-fat", diet for four or five weeks after an attack, in order, I suppose, to give the digestive organs a chance to recover from the inflammation. I am taking two kinds of medicine that he prescribed, of which I am enclosing the labels which might interest your son-in-law. The Methiscol supplies additional cholesterol (?) to aid in fat digestion, and the Octin is intended to relax something, I don't know just what. In a medical book that I borrowed from the doctor, nitroglycerine was recommended to relieve the pain of an attack, but he says that it is not suitable for my particular case. He has given me another medicine for this purpose, called Donnatal; some kind of a narcotic, but not a morphine derivative, which is contra-indicated for gall-bladder trouble. As you say, operations for removal of the gall-bladder are quite common; several of our friends have had the operation, and in some cases they claim to be able to eat anything, but others have to be very careful, and occasionally they have recurring attacks.

In one of your recent letters you mentioned that you were examining some plankton material from the central African lakes. If there are any desmids therein, and if you could spare a small quantity, I should like very much to have an opportunity of looking at it. I have never seen any desmids from Africa.

You used a phrase in your last letter that is not quite clear to me, i.e. that synonyms older than Ralfs' names are "unguilty". Unguilty is not the correct English word; can you give me the French or German equivalent?

With my best regards and good wishes,
Sincerely yours,

Linköping 14/VI 1951.

I am sorry that you have recurrences of your gall-bladder trouble, probably there is no help but an operation. My wife had, long ago, gall-stones with terrible torments when the stones squeezed through the narrow end-hole of the bile-duct into the duodenum. Unhappily this sickness was combined with gastric ulcer which made an operation absolutely necessary. The gall-bladder was taken away, the end-part of the bile-duct was enlarged and a fistula was opened joining the stomach with the small intestine. This happened in 1922 and she has not had any trouble since then. And now the operative technic is advanced enormously, my son-in-law who is just here, tells me that ~~such~~ gall-bladder operations are very common now. Thus, I think that your apprehensions of recurrence even after an operation are groundless. My son-in-law says that he has tried nitroglycerine in several cases with good results, as well as against angina pectoris. Cramp-relieving remedies cause also good effect. On the other hand, he considers that fat-free diet is of no use; normal food, of course without excesses, is sufficient. When using nitroglycerine it is, however, necessary to observe the hydrochloric acid in the stomach; very often this substance will diminish with digestion troubles as a consequence, a lack very easy to eliminate.

Your critic of the new books with Smith's name is interesting. The Manual of Phycology will be sent me, according to the Editor's promise, for reviewing in Botaniska Notiser and I have proposed Smith to stimulate the Editor of Fresh water Algae to send it me for reviewing, but I am not convinced that Smith will accept this proposal. I am still obliged to avoid expenses and this manner of procuring books is good, favourable to the Editor as well as to the receiver.

Yengar has not answered my repeated proposals of interchange of reprints, nor has he, in any way given any sign of life in consequence of my sendings of reprints. If I am to make a review of the "Fresh water Algae" your hints and points of view are very valuable and I should like to get more of them. Smith is, I think, too much occupied with his own affairs to take any interest in his former maintenance of his formerly very extensive knowledge of the desmids and other fresh water algae. He deplores that all pools in his region are covered of petrol.

Actually, I have missed Ralfs' note of *Closterium cuspidatum* in his "British Desmids". The justification of *Spinoclosterium* as a separate genus is, as often ~~was~~ the desmids, a question of taste. The conclusive character is the thickening of the cell-wall but such thickenings are also to be seen on other Closteria, e.g. *Cl. Neanthodes*. Bernard's *Spinoclosterium* is described in his paper "Sur quelques Algues unicellulaires d'eau douce recoltées dans le domaine Malais" 1909. Ralfs' *Cl. attenuatum* is an interesting intermediate stage, showing the tendency of attenuation; all solid processes are developed from hollow processes.

Bernard mentions Bailey but not Ralfs, it is easy to overlook the Appendix in Ralfs' book. You have made a good deed in bringing *Cl. cuspidatum* into the circle of attention. It seems me too absurd if Prescott is inclined to neglect Bailey-Ralfs' description because of its lack of notes of the dimensions. Ralfs' book is the starting-point for all desmids, described or figured in it, it is the law, but even if Ralfs had not taken up the *Cl. cuspidatum*, it had been valid, nevertheless. I have ~~xxx~~ apprehended the thing in that way that synonyms of genera and species, older than Ralfs' names, are unguilty. In the beginning of this century there was a tendency of taking up generic names based upon very mis-leading objects. Nordstedt made a detailed analysis and forced the Botanical congress in Bruxelles to accept a proposal based on this analysis. By this a great many of generic names were eliminated; such as *Binatella*, *Colpopelta*, *Phycastrum* etc. In your case it is quite evident that *Cl. cuspidatum* is identical with *Spinoclosterium-cuspidatum*, and the species-name *cuspidatum* must be maintained, even if the new generic name is accepted. I suppose that Bernard possessed very little of literary sources, he is, in his books he is very generous with N.sp.!

I shall make copies of the pages and the plate of Bernard. I am sorry that my copies are not very beautiful because of the provisory lightning-applicances. but ~~at~~ at least for me they are sufficient. I have placed a mirror in an angle of 45° before the objective and take the figure on bromid-paper, white on black. I hope that

181/177

Linköping 3/VI 1951.

Dear Friend Scott,

Time is running too rapidly for me. Spring is very late this year but just now the apple-trees are flowering and something like summerheat is coming. School is over and we shall move to our summer-cottage. After mid-summer I shall go to Aneboda as a teacher at the laboratory and my wife goes to London. Then we shall meet in Gothenburg and drive to our daughter. Her husband possess an island at the West-coast and we shall have a good fortnight there, fishing, bathing, and idling, sailing, and motorboating. Of course I shall have my micro with me, in the small rock-pools there may be pure-cultures of algae.

I have, at last, got a letter from Ruzicka. He is now at home since a month, but he does not at all know how long he is allowed to be staying there. He is anxious to get a job as a scientific worker, but he says that he has rather small expectations of it; his juridical training is no particularly weighty merit for such an employment. He commissioned me to deliver his very sincerest and amical greetings to you. He is very glad of the copy of the review in *Hydrobiologia* you sent him; a ray of light in his darkness! I also got a letter from an other Tjeckian Friend, a musician, from whom I have not got a letter since 10 months. He writes: "Be not disappointed that I did not write sooner. It is better for me not to write very often now. Hope you understand."

Many thanks for the fine journals which often find their way to my letter-box. The reprint of H.A. Camp's *Biosystematics* is very interesting. Many thanks also for the film of my diffident person, almost in natural size! After another hint to Bourelly I got a letter from him where he enumerates all the causes of his delay (except the principal one!): the darkroom is under reparation, Mr Lami is absent (for a fortnight!) and the picture of Brébisson in on a glass-plate, "très fragile". He now offers to make the photos in their photo-lab, if I tell him the size wished. As I want copies for you, Bruneel, Grönblad, and myself, I am hesitating to accept his original propose. And, if I do at what time will the copies be made, I should prefer to get them when still alive. I repeat: it is a certain difference between French mentality and our. I am sorry that I told you the possibility of getting the photos (especially the one of Brébisson, probably unique, is valuable) but I could not expect that this little commission should take almost a year. Life is short!

I have got a little sign-of-life from Grönblad with your *Penium*. Sorrowly I have not had any time to examine it yet. The other day I got a collection of very interesting samples of plankton from Lake Victoria in Central Africa. From this lake I now possess samples of its plankton of all months through the year. It seems to be a very small seasonal difference probably depending on the uniform temperature.

I have on his request sent your *Eurodesmus*-figures to Grönblad and told him that I had your vow of getting them back when he does not need them any longer.

To-day it is a very fine Sunday and I have promised ^{the} two boys of my daughter who are visiting their grand-parents to give them a little trip to the foresty parts in the Northern of our town. The dark and serious forests of pine and spruce on rocky terrain with narrow valleys and clear oligotrophic small lakes are the very best recreation for my kind mind. I should wish to have you in my company!

At last my best regards to you and your wife,

Affectionately, yours

Einar Tolling

Linköping 14/IV 1951

Dear Friend Scott,

Many thanks for your letter of 1/IV and the very valuable gift "Culturing". I have always considered not least culturing of desmids as a very valuable method to follow changes of shape, ornament and other characteristics. I think, however, that this is a very intricate job with extensive and time-consuming laboratory work. In order to get results in mutations and not merely teratological forms it is necessary to arrange hundred and thousands of cultures during many years. That is why my longing has rested platonically.

I am glad that Ström has taken consideration to my request. The honey-bee poison treatment is rather often used here, I think, it derives from the experience that bee-cultivators very seldom suffer from rheumatism.

It is astonishing to look at the extensive and expensive menu you sent me. I read it when eating my lunch and, being a gourmet of very small degree, I could not imagine the Columbia restaurant dishes more delicious than the bacon-gratin I was just eating. Our maid is a specialist in every respect, a true jewel who has been at us during 14 years in spite of the fact that she is a serious Baptist. I think that even you could revel her culinary skill.

It is a pity if you have exhausted the possibilities of finding more desmids in your district, but your lists of interesting forms already found by you will give you a prominent place among the desmidiologists. I don't know the geology of your district, but I have the impression of mere soil with ponds and rivers. Are there any mountains with rocky pools and wet moss on rocky slopes or even bogs? Of course, certain habitats of plants (biotopes) must be exhausted by very intensive research, then you must try new biotopes, for example in the West of Texas, montaneous with damp climate? 266000 29 m.

According to your kind information I have requested Grönblad to send me copies of Penium rufescens var. corrugatum. I have not heard from him since long, perhaps he is somewhat surly, since I was obliged to pull him by the ear after a letter in which he in a superciliously scornful manner spoke about my comprehension of desmid radiation. Even if a person is unable to apprehend evolution he must respect a serious attempt of solving an important problem.

I have not yet got the French films of the algologists Brébisson and others but I have written to Bourelly to stimulate the owner to send them to me. I think the French national temperament contains a bit of the Spanish "manana": to-morrow. I enclose copies of Gymnozyga arnaya and Desmidium curvatum, these are the only desmids dealt with in the cited paper of Nordstedt. I am just writing a letter to our poor friend Ruzicka with your extract of the recitation. Very kind of you, I think he needs every encouragement in his life. My best compliments to you and your wife. Yours Emil Rellier

May 28 1951

Dear friend Teiling,

Once more I have been on the sick-list with gall-bladder trouble; this time two successive attacks within a week. That kept me in bed for ten days, and I am back on the "low-fat" diet for a month. My doctor says that another attack will mean an operation for removal of the gall-bladder. I am not afraid of that, but there is no guarantee that the operation would eliminate the trouble. Previously I have recovered within a week or so, but this time it has left me in a run-down condition, and I don't seem to have the energy or ambition to work, even at my beloved hobby of desmids.

A couple of weeks ago I received a long 4½ page (!!!) letter from our friend Grönbald, mostly relating to the collections and drawings that he has been working on. He has completed his examination of all my Louisiana gatherings, and has sent me all his drawings and notes. These will require much consideration, and there are many questions of procedure to be discussed with him.

In your last letter you mentioned the possibility of collecting desmids in the mountains of West Texas. I have been through that part of Texas, many years ago, and though there are some small mountain ranges, the country is very dry, and it would need considerable time, more than is at my disposal, to seek out the few lakes and reservoirs where desmids might be found. But Texas is a very large State, almost as large as Sweden and Norway combined, and it is quite likely that somewhere there may be suitable desmid habitats, but so far I have failed to find them.

As you have surmised, the territory in which I have collected is mere soil, without any rocks of any kind, and no bogs such as occur in northern climates. The nearest mountains are 400 miles away, and when you get there you find few suitable habitats, since the formations are larry limestone. In Florida, on the other hand, there are large areas, many miles in extent, covered with a few inches to a few feet of water; some of them are densely populated with small slender trees, others have no trees but thick growths of reeds, rushes, and small shrubs. In such places the soil and water are usually acid, and there is generally plenty of open water at the edge of the swamps, or in the ditches along the roads that have been cut through the swamps. That is where I have obtained so many beautiful gatherings, and I am afraid, in fact, that they have spoiled me for less rich regions. But through correspondence I am gradually acquiring material from some of the far-off places of the world. Already I have collections from Tasmania, South Australia, North Australia, Sumatra, Java and Borneo, and these will keep me busy for quite some time. In addition I am going to draw the plates for Grönbald's papers on my desmids, which will be quite a task itself.

Recently I have received two new books by Gilbert M. Smith. The first, entitled "Manual of Phycology", is a smaller book than his "F.W. Algae of the U.S.", yet he has attempted in it to include all of the algae, both freshwater and marine, with the result that everything is so compressed and condensed that I cannot imagine what class of algologists can find much for it. The various chapters are written by specialists; for instance the "Chlorophyceae" is written by Iyengar, the Indian algologist, and desmids are barely mentioned, with only a few sketchy illustrations of them. I was so much disappointed that I sent the book to a friend in Australia who has specialized on marine algae. I do not think it would be of any use to you.

The second book is the 2nd. edition of Smith's "F.W. Algae of the U.S.". Much of the text and many of the illustrations are the same as the 1st edition. The section on desmids is practically the same, the principal change being the addition of the genus Spinoclosterium Bernard, without any mention of the fact that it was

first found by Bailey more than a century ago, and described and figured by Ralfs in 1848. He does not list the genus Spinoclosterium Presc. & Scott, which is quite agreeable to me because I think this plant should have been assigned to Xanthidium; not does he list your genus Staurodesmus. I am quite surprised at the incompleteness of his bibliography. Though for some American authors he lists papers up to 1948, and even 1957 for Transeau's "Monograph of the Zygnemataceae" which is still in press, he has omitted or disregarded many important papers. For instance he lists Presc. & Scott 1942, but omits our 1943 and 1945 papers on Microsterias and Eusstrum. For you he lists only two papers, 1916a and 1916b. For Grönblad he lists only up to 1934. He lists the first volume of Krieger's monograph, but omits the first part of the second volume (1939 Microsterias). The names of Nygaard and Messikommer are missing entirely; etc. etc. Also he has perpetuated several erroneous statements that appeared in the first edition, such as "Walls of Cosmeria are without spines", that "walls of Desmidium are smooth and without striae, punctae or spines", that "conjugation of Phymatodocis probably takes place between isolated cells", etc. Somebody ought to call his attention to the serious omissions in his bibliography and to the errors mentioned, but I do not feel that I have sufficient standing to do so.

In the other orders of algae treated in the book, there have been numerous changes and additions, according to the publishers' statement. Therefore I think it would be of value to you. Let me know if you want it, and I will order a copy for you.

When you get time, I wish you would do me the favour of looking up Ch. Bernard's publication of Spinoclosterium in his Protozoococcees et Desmidiées d'au douce, recoltees a Java, 1908 or 1909, and tell me whether he seemed to be aware of the previous publication of this desmid in Ralfs. If he was not aware of the fact, then Bailey's name must stand, Closterium cuspidatum, until someone deliberately and with sufficient reason transfers it to another genus. I have called Prescott's attention to this, but he seems fixed in his idea that it is not a Closterium, and furthermore seems to think that Ralfs' publication has not valid, presumably because of an insufficient description and lack of measurements. But Ralfs' British Desmidiaceae is taken as the starting point in desmid nomenclature under the International Rules, and therefore it seems to me that any name published by Ralfs is valid, with or without a description of the kind that would be required today. The matter is of some importance to me, because I have found this plant in Florida, in North Australia, and in Indonesia. The Florida specimens will be described by Grönblad, who insists that it is Closterium cuspidatum, while the North Australian plant has been described by Prescott as Spinoclosterium, in an unfinished paper. All of these plants, so far as I can see, are identical, and the same as that figured by Ralfs. Minoru Hirano has also found it in Japan, but I have not seen his drawing.

Many thanks for the copies that you sent me, of the illustrations of Gymnokyge armata and Desmidium curvatum; I had not seen them before. Did I tell you that I have found just two semicells of D. curvatum in one of my several hundred gatherings from Florida? They were almost identical with Nordstedt's drawing.

With my best regards,

Sincerely yours,

Linköping 18/3 1951.

Dear Friend!

I am now writing in order to tell you that your valuable gift has arrived and has excited great delight at me and my wife. Many, many thanks!!

During these last weeks there has not been so much of algological work, sorrily, my service (we have a very energetic rector) and also the music have taken much of time. On the 21, the vernal equinox, our orchestra will deliver a concert with rather intricate pieces, Tjarkowski's fifth symphony e.g. We shall have a dinner for the female conductor, the only woman here who is able to conduct a symphony orchestra. She is a friend of ours and married with a very prominent leader of orchestras. Afterwards we shall drink the fine Louisiana coffee and send a thought of gratitude to the noble giver.

In Eastern with its many holidays I hope to be able to write a good deal of my paper on the desmid chloroplast. I am sorry to say that I am a rather strange boy. Investigating is a source of intensive enjoyment to me, when, however, the result is clear I almost lose the interest of it. The working out of a paper is not attractive at all. I am longing for setting about a new problem or investigation. To some extent this fact may depend on the translating to a language which I do not master.

From our common corresponding-friends I have not heard anything. Grönblad, I think, is eagerly occupied with your samples, he told me that he finds them marvellous, they represent a paradise of a desmidologist.

My best wishes of a fine Eastertide! (Do you eat Easter-eggs in America also?)

Yours very sincerely

Ernst Peltier

April 1st 1951

My dear friend,

I have your two letters of March 11th and 18th, and am glad to know that the several packages that I sent you have arrived safely. The book on Culture of Algae is a gift, not merely for your inspection. I have sent a copy to Grönbld also. This book is of no value to me, since it tells nothing about desmids.

I have received the several reprints that you sent, and thank you very much for them. The one by West and West on Madagascar is especially valuable. Prof. Ström has been kind enough to send me not only his paper on Desmids from North Australia, but also eleven other papers of his, and I have written thanking him. It was a little disappointing, however, to find that his North Australian paper merely lists 24 desmids that were found among algae growing on the shell of turtles. What a pity that some algal collections were not obtained from the lagoons where these turtles were caught.

The cure of your lumbago by injections of honey-bee poison is rather astonishing. Is this treatment used regularly, or is it experimental? So far as I know it is not used in this country.

My own gall-bladder infection has cleared up, and on my last visit to my doctor he permitted me to return to my normal diet, though he warned me to be careful and not to over-indulge. So now I can have my cocktails, and beer, and some of the rich foods that I like so well. At Easter my wife and I drove to Tampa, Florida, where we dined one night at the Columbia Restaurant, which has, I think, the finest food that I have ever tasted, and I know many excellent places in Mexico, Havana, New Orleans and New York. I have sent you, as a souvenir, one of the menus from this restaurant, to show you the large variety of foods (and drinks) that they offer.

On this trip I made a number of collections, but to my regret they contain practically nothing that I have not obtained before. It seems that I have now collected practically all of the desmids that grow in southeastern U.S., because the results of my last few trips to Florida have been pretty much alike, merely one or two novelties that hardly repay the time and trouble and expense of these long trips.

I do not possess a drawing of *Gymnozyga armata*, Nordstedt & Löfgren, and should be glad if you would send me a sketch of it. I have seen it referred to in the literature, but had assumed that it was the same as *Haplozyga armata* Racib.

Here is something that might be of interest to you regarding rediation in desmids. I have a new variety of *Penium rufescens*, for which I have suggested the name "var. *corrugatum*". This has a series of corrugations extending from the isthmus almost to the pole, where they die out. In vertical view it is circular with sixteen shallow crenations around the perimeter, one for each of the radiating plates of the chloroplast. Grönbld has my original sketch and could copy it if you would like to see it, and he also has the two collections in which it occurs.

Enclosed is a copy of a review of Ruzicka's paper on *G. hornavannense*, that appeared in "Hydrobiologia". If you write to R. you might send this to him. I do not know who wrote the review.

I should be glad to have the photographs of Brebisson and Bourrelly,

if they are of a size suitable for framing and hanging on the wall of my room. And if you want to do something for Dr. Jules Brunel, you might send him copies also. I think you met him at the Botanical Congress last year, but perhaps you do not know that he has a collection of pictures of all the algologists that he can get. I have sent him your picture and one of Grönblad, from enlargements of the small pictures that you and he sent me.

Just like you, I do not like the writing of papers; I much prefer my observations and drawing. But now I must get busy on two papers that are almost ready for the final writing. I am a long way behind on my work.

With my very best regards,

Your friend,

Linköping 11/III 1951.

Dear Friend Scott,

it is now five weeks since I received your last letter and I have not written to you though I have received many fine things from you; Science, Scientific American Bull. biogeogr. Soc. Japan with Okada's paper and now the valuable "Culturing of Algae" Many, many thanks!! I am sorry that I have so little to give you in return. Gutwinski has arrived now, I hope, I sent it 31/XII, 50. West's Madagascar Algae is probably still on its way, I sent it about 1/II.

I have been somewhat sick, at first lumbago and afterwards influenza, but I had a fine week in Stockholm at my daughter and son-in-law who cured me from the lumbago by means of injections of poison from the honey-bee. The effect of penicillin in your fingers and toes is curious, it is a good luck that you didn't get anything worse, the allergic consequences of chemical influences on our body are extremely varying and quite individual.

It was interesting to hear your experience of Grönblads categorical taxonomical decisions. I think Krieger is something like him and I know that Skuja is absolutely categorical: he has his opinion and afterwards - no discussion!

I have got a photoreproduction of an old daguerrotype of Brébisson, safe in the archive of Laboratoire de Cryptogamie, perhaps the only picture of B. except ~~some~~ some I have sent to my friend, the extremely variable marine algaologist, Harv. that made it for me. I have required the film in order to have a better and retouched copy made. I also asked for the film of Bourrelly. I have not got it yet, the French have no hurry! If or when I get it, have you any interest of a picture of Brébisson? or of Bourrelly?

I have written to Moloszynska a month ago but yet no answer. No hurry in Poland.

I agree to you that Raplozyga armata ought to be regarded as a Desmidiium, I had placed it in that genus in my iconotheque. You know the Gymnozyga armata Nordstedt and Löfgren (Nordstedt, De Aëgis and Characeis Æ 3. De duabus novis speciebus Desmidiarum e Brasilia. 1839) This one seems to be something else than Raciborski's armata. The former is regularly sixradiate with one spine in each radius, " " is triradiate with pairy spines. If you don not know Nordstedt's armata, tell me and I will send you a drawing. Many thanks for your completing statements as to radiation, they are noted in my working-copy.

When looking through this letter I find that I have connected the Activation-book with your other gifts, of course I regard it as a book "for kind inspection".

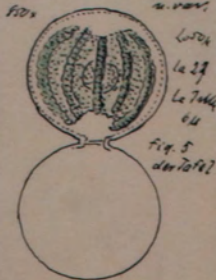
I hope that you now are quite well and have cleared off every kind of ill health for many years.

The very best greetings to you and to your wife,

tuus

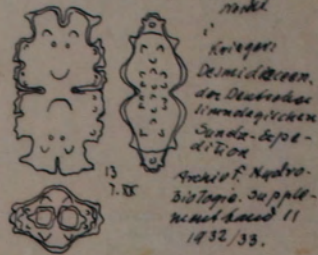
Emmanuel Pelling

C. moniliformis Reife
 var. *pubescentissima*
 n. nov.



Fr. Steincke: Die
 Algen des Zehlauer-
 Sees
 "Schriften der Physika-
 lisch-ökonomischen
 Gesellschaft" zu
 Königsberg, Jahrg. LVII
 1916.

Eu. rostratum var. *proemortuum*
 n. n. sp.



Krieger:
 Desmidiaceen,
 der Deutschen
 Mykologischen
 Gesellschaft
 Archiv f. Natur-
 hist. Bot. Supple-
 mentband II
 1932/33.

Jan 28 1951

Dear friend Teiling,

Many thanks for your letters of Jan 5th and 12th, with the sketches of *Cosmarium moniliforme* var. *pulcherrimum* Steinecke, and *Euastrum rostratum* var. *praemorsum* Nordst. The latter is a synonym of *E. praemorsum* (Nordst.) Schm., and I have found many examples of it from North Australia and also from the East Indies. There is a certain resemblance to the unnamed *Euastrum* of which I sent you a sketch, but they are not the same plant.

I am glad to have the illustration of Steinecke's variety of *C. moniliforme*, which seems to be almost identical with my North Australian plant, particularly since the arrangement of the chloroplast is so similar.

The package of *Staurastrum* drawings that you returned last September was received safely, and I am sorry if I forgot to acknowledge them. The papers by Gutwinski and Allorge which you are sending have not yet arrived, but I suppose they will come next week. I should be very thankful for a copy of Wests' F.W. Algae from Madagascar, which I have long wanted to possess. Bourrelly has just sent me a copy of a paper by Bourrelly & Mangin on Madagascar (1948) that is very interesting. Also Dr. Krieger has been kind enough to send me a number of duplicates, some of which deal with African algae, so gradually I am building up a collection of works on tropical and subtropical desmids which are my special love.

It is strange that friend Grönblad should not show more understanding and sympathy for your effort to show the meaning of the various degrees of radial symmetry in desmids. But I think he is of a rather obstinate disposition and that his ideas are so firmly fixed that it would be almost impossible to make him change them. He once wrote me that *Microsterias decemdata* var. *expansa* (Turn.) Krieger was the same as my *M. piqueta* var. *lata* var. nov. (unpubl.) which is quite unthinkable. I attempted to convince him by enlarging Krieger's illustration (Zef. 104, fig. 12) to the same magnification as my drawing of *M. piqueta* var. *lata*, showing that it is only one-half the size, and placing both drawings on the same sheet for easy comparison, but I never heard any more about it. Another time he wrote that my *Cosmarium ocellatum* var. *incrassatum* was the same as *C. luscum*, but here again he was wrong, because I have the zygospores of *C. ocellatum* var. *incrassatum* which correspond closely with those of the species as illustrated by Krieger and Homfeld.

By the way, in your paper on radiation, p. 304, you show *Desmidiium* as being 4-, 3- and 2-radiate. This can be extended to 5-radiate. Grönblad shows a quinquangular form of *D. graciliceps* in his Brazilian paper, and I have found 3-, 4- and 5-radiate forms of *D. pseudostreptonema* in Florida. And have you noticed the very strange *Hoplozyga* (= *Bambusina*) armata in Raciborski 1895 *Desm. des Tapskoomasees*? It seems to me that this plant is out of place in *Bambusina* and should be transferred to *Desmidiium*, by analogy with *D. curvatum* (= *Meteola scuticobum* Salisb.)

The address of Mlle. Woloszynska is:
Prof. Jadwiga Woloszynska,
Institut Botanique,
ul. Krupnicza 16, parterre,
Krakow, Poland.

She must have been out of touch with other European desmidiologists for several

years, because Krieger also asked for her address, and said that formerly he had corresponded with her.

A second edition of Smith's F.W. Algae of the United States has recently been published. It is said to be a revised and enlarged edition, with the addition of a number of genera that have been added since the publication of the first edition in 1933 (?). It costs \$10.00 against the \$6.00 for the 1st edition, but all scientific books are now horribly expensive. I think I shall have it sent for examination, on approval, and if it contains sufficient new material I shall probably buy it.

Shortly after I wrote you on Jan 6th, I had an attack of laryngitis and bronchitis. The doctor kept me in bed for nearly two weeks, and it is only within the last few days that I have been able to get up for a few hours a day. He gave me some injections of penicillin, which cleared up the bronchitis quickly, but they have had a peculiar after-effect. My fingers and toes are swollen, and the skin on them is hardened, just as if I had spilled strong formalin on them. I am still on the no-fat diet for the gall-bladder infection, which is by no means pleasant, since I am something of a gourmet, and am very fond of the rich foods for which New Orleans is famous. So this winter has not been a good one for me, but I suppose everything will straighten itself out in time.

For two weeks I have had your coffee ready to be packed, and today have finished the parcel, and shall mail it tomorrow, Monday, so it should reach you in time for your birthday.

With my best regards and good wishes,

Your friend,

Linköping 5/1 1951.

Dear Friends Scott,
1000 thanks for your charming and comprehensive letter which reached me in the evening in Stockholm. My wife and I were guests at our daughter and son-in-law and had pleasant days with them and with my son and his family who also lives in Stockholm. He had just entered a profitable contract with a firm on some letters patent and was very glad; a captain in the army is badly salaried. This was the our first Amas in Stockholm since 1919, previously we lived there.
Interesting to hear that you have a temperate climate, though this temperature for you, accustomed to tropical heat, seems somewhat coolish. We are obliged to heat ten during six months. Now the mild climate, before Amas scarcely below the freezing-point is changed into a good winter. When looking through the window I see everything covered by snow, xxx the trees have even their smallest branches quite white, a charming view!

I am very glad that you and also your wife enjoy my formal letters, I like to chat in letters to my near friends, I am a busy letter-writer. I do feel that you and I are in the same way-length in many respects, my wife is flattering me saying that I must be alike you. Thus I can continue without thinking that you are critically on my letters.

I am glad to hear that you have finished the first part of your Australian Algae and sorry that you intend to delay the publication of it. I understand that this is desirable from the Australian point of view but I must beg your to think it over. Of course, greater and more extensive papers are valuable but in the meantime there is a risk that new descriptions of yours will be anticipated by other scientists, and, my opinion is that even a scientist must use the opportunity. The situation may change and especially in the present time a delay may be extended longer than you may like. If you publish the paper now you can, in a later paper, published in Australia, arrange the the species from the earlier paper which are not found in the later collection. I beg your pardon that I put my nose in your affairs, is exclusively for the sake of niggology. When I was a young man my dear friend Borge advised me not to publish short papers but to collect the investigations to a bigger paper. Of course, this is a preferable thing from certain points of view, and convenient to him who worked with his algae almost every evening. For me, a young student, who had my time occupied with teaching every day to 15 o'clock and afterwards very extensive studies for my examination and only now and then was able to look in my microscope, it was very inconvenient. Thus, several very good things, found by me, were published by others and I am very glad of the few cases in which I offended against Borge's rule.

Many thanks for the drawing of the chloroplast. It is very interesting and valuable; a good illustration of the axile star-like (a vertice) chloroplast with a central pyrenoid and laterally extended edges of the plates. As to the name of the species, I can't tell you, there is a lot of *C. moniliforme*-like desmids of all sizes. There is a *C. moniliforme* var. *pulcherrimum* of which I enclose a copy. I am very glad of your copy, it shows that a primitive radiate chloroplast can exist even in a strongly constricted cell. Ordinarily this type of chloroplasts appear in elongated, cylindrical cells.

As to Krieger and consortes, it is a pity that this very skilful species-specialist, (as well as Skuja, Grönblad and others) is destitute of higher points of view on taxonomy. He is a very good compiler, though he never gives reason of his combinations, a fact already complained by Borge and also by Grönblad. I had a letter from G., in which he is completely ununderstanding of my analyse of the radiate element of radiation. He finds it "arbitrary and hypothetical, not founded on real facts etc". It is a pity that several of the most busy desmidiologists are not initiated of the elementary taxonomical points of view, procured by intensive and also extensive study of botanical and zoological organography xxx on the basis of evolution. They are sharp-eyed speciesmakers and collect an excellent material for taxonomy, but they are incapable of discerning the general aspects, necessary for creating genera and taxonomical conception of higher rank, the worst is that they do believe that they are.

Linköping 12/1 1951.

Dear Friend Scott,

your letter of 6/I and mine of 5/I have passed in the air, a synchronism of thought! You are right in your elimination where Euastrum Borgei is described, but I can't avoid calling your attention to Nordstedt's Index; on page 65 you will read: "Borgei, description (18)95 Euastrum, Schmidle in Hedwig. 34 p.79, t.1, f.11" and on page 29: (Schmidle in Hedwig.34) - Weitere Beiträge etc.

Please excuse my finger of a school-master, it is meant as an information of the astonishing servicableness of this eminent work. I have written to Ström, begging him to send you of his papers if his, yet available.

Eu. Borgei is a strange form, extraordinary broad. I think, however, it is too early to consider it "teratological" or immature until new finds give comparison. There is a lot of records of Eu. binale with the same proportions! I enclose a copy of Eu. rostratum var. premorsum, somewhat of the same type as yours desmid, which has an elongated apical lobe.

I regret your deplorable misfortune with your gall-bladder, I hope you will have it removed with good success! This ill-ness was a very bad Xmas gift and you will have a dreary time without culinary nutriment and luxuries. I hope you ^{don't} possess a gastronomic temperament, otherwise your cheerful spirits will suffer under the childish diet.

Many thanks for the book-catalogue. When looking it through I was astonished at the high prices (at least in Swedish crowns). I myself, ^{being} a very bad man of business, fixed the prices of the papers on marine algae sold to American algologists, according to the prices of 1914 in German book-catalogues. No wonder that a Californian algologist engaged a whole list per wire!

I hope that you possibly soon will be all right for business and algology!

My best wishes to you and to your wife who now is more filled with tender feelings than usual. Women are happy when having their husbands to bed, like children.

Yours sincerely

Einar Telling

Jan 6 1951

Dear friend Telling,

I am enclosing herewith a sketch of a peculiar *Euastrum* that I found in some material from North Australia. It is evidently the same plant as the unnamed *Euastrum* figured by Borge, 1896, Austr. Süssw.-Chlorophyceen, page 14, Pl.4, fig.63. Of course it should be named after its discoverer, but the name E. Borgei has already been used by Schmidle, in one of the three papers that he published in 1895. I have the first two of these papers, but not the third, "Einige Algen aus Sumatra", so it is probably in that paper that E. Borgei is described.

There is little chance that Schmidle's plant is the same as Borge's, since Borge's paper was not published until a year later than Schmidle's. Nevertheless the reference must be checked. I have asked Prescott to do this, but he has so many other things on his mind that he frequently neglects or forgets to answer my questions, and he has not done so in this case. Would you therefore be kind enough to compare my sketch with Schmidle's illustration, and see if they are the same? Krieger says (Desm. Eur. p.654) that Schmidle's plant is "deformiert", and Homfeld (1929 p.9) intimates that it is an immature form. I should be grateful if you would make a pencil copy of the illustration of *E. Borgei*.

Would you be willing to lend me the paper by Schmidle mentioned above, and also a paper by K.M.Ström (1921a) Algological Notes I. Some desmids from North Australia. Nyt. Mag. f. Naturv. LIII? I would guarantee to return them promptly and in good condition. I also asked Prescott for them, but he has failed to send them, and though I could get them from one of the large libraries on an inter-library loan, I would not be permitted to take them home.

Christmas-time has been unlucky for me the last few years. In 1948 my wife and I were hurt in an automobile accident in Cuba. In 1949 we were in New York and nearly froze to death. Last week, instead of going to Florida on a collecting trip, I was laid up with a severe attack of biliary colic, caused by gall-bladder infection. The doctor sent me to the hospital at 5 O'clock in the morning, and kept me there for two days. Now I am on a "no-fat" diet; no butter, fats, or oils; no fried foods; no pork, ham nor bacon; no alcohol [!!!] and little smoking. I have had these attacks before, so I know that I shall have to adhere to this diet for a few months until the infection disappears. One of these days I shall probably have to get the gall-bladder removed.

I hope that you and your family had good Christmas and New Year, and I send you my best regards.

Your friend

Linköping 28/10 1950.

Dear Friend Scott,

many thanks for your promising me to have the Staurodesmus-deawings still some time. I have made a little pause in my Sturodesmus job and have worked out some, as it seems to me, very interesting consequences of my radiation-theory concerning the development of the chloroplast the results of which have importance of the coming revision of Cosmarium. Don't mind my request of Cosmarium drawings, what I need are good drawings of the chloroplast, especially a vertice, a detail mostly neglected, often very difficult to examine on spiritus material and quite impossible on material from exciccated material.

Many thanks for your often arrived sendings of "Science". The "Plants of Bikini" is still on its way over the Ocean. Your Ophiura paper arrived to day and I have sent the doublet to Ruzicka. Yesterday arrived the "Scientific American", one of the most valuable scientific journals of the world, at least the finest with regard to the illustration.

After having received your letter I immediately wrote to the editor of Botaniska Notiser and have just got his answer. The last number of this year is quite engaged and so is the first number of 1951, but there are still space in the following numbers. He proposes you to send him the manus as soon as possible, the contributions are printed are printed in the sequence the arrive to the editor: Fil. Dr. Hakon Hjelmqvist, Botaniska Muséet, Lund.

I have, since many years, collected material for an obituary notice of P. M. Lundell, the famous desmidiologist, who died in 1930. I have never met him and thus it is difficult to get a characterization of him as a private person. Some weeks ago I had, however, the extremely good luck of getting hold of very valuable notions, in several indirect ways. A prominent journalist, now deceased, has carefully described his memories of Lundell, at first as a pupil in 1877-1882, and afterward as a personal friend of Lundell right up to L.'s death. He has given a very good character-revealing portrait of L., which completes the concept that I have shaped from notices given by other contemporary colleagues of Lundell.

From the photos of your laboratory and from the fine boxes with samples I have understood that you are a very dexterous man. I am glad that you have removed your interest on the desmids, actually there is an evident coincidence between marquetry and desmid research, isn't it?

Now the best regards from Your friend

Emar Tilling

Linköping 3/12 1950

Dear Friend

Time runs too quickly, only some little time ago I went back to my school-job and now Christmas is near. I think it is a common experience that time is running ever faster with our increasing age. I see from your latest letter that you are occupied with algae from insulinde. I just found a doublet of *Cutwinski*: *De Algisch Doctore M. Macfiborski anno 1899* in insula Java collectis, with 5 double-plates. Do you possess it? If not, I shall send it to you. Also *Alborge et Linguin: Algues d'eau douce des Pyrénées, basques, contenant 30 figures de desmids*.

I am so sorry that so little real work has been done by me this autumn; there has been too many distractions. After an intensive summer with Congress and studies in Paris, I was very tired and was obliged and was obliged to recover on my sofa several hours every day after my come-back from the school. My daughter got married and such things vast time and interest. This event, however, is very joyful; it is a good thing to know her safe in the care of a good husband in this bad world, with its darking future. My son-in-law is a very skilly and successful doctor in Stockholm, lover of music and nature, humorous and, above all, a real good man. My daughter is a partner of a taxidermyxxxxxxx ladies' tailoring-trade and a very skilly creator of attractive models.

Have I told you that I am, occasionally, examining phytoplankton from Lake Victoria in Central Africa, very interesting. I am hunting good figures of desmid chloroplasts they are very scarce, most of such figures are useless, being too "objective" and very seldom seen ~~in~~ a vertice, which is necessary. The end-view is on the whole often neglected, perhaps because of the difficulty of getting the specimen in good position, and such sins of omission are annoying, since I am especially interested of *Cosmarium*-species with circular a vertice in order to erect a genus, broken out from the extremely unnatural bulk of *Cosmarium*. I and Staurström are the immense publishers of species, which cry for division, of course on the basis of real principles.

I hope you and your wife are well in all respects. Your climate must not be so tropical at this time? Just now it is falling snow here but we have not winter, only cloudy and wet weather, the sun is visible but about one day a week. I enclose a Christmas card for you to look at when you are suffering from heat. It shows a fishing methode, very common and popular in Sweden. Sometimes it will happen like this; the perches are laughing at the fisher's efforts. To the right in the background you see a fisher cutting a hole in the ice and the third fisher is heating himself by means of a "coachman's fire", swinging his arms violently. The central fisher seems to be quite content, but he is smoking his pipe, for me this is a plausible explanation.

A friend of mine has prevailed me to act as an opponent at ~~his~~ the public ventilation of his dissertation, which is necessary for obtaining a doctor's degree in Sweden, except for a few happy old boys who like me obtain it "honoris causa". The preparations, however, take much time, especially as my task is to be the author's "friend". One of the professors, acting as judges, is unfriendly to my friend, thus I must avoid such criticism that may be used as "fuel" for the judger's opinion and opposition. Sorry, I am a bad quibble-solicitor, I like better to say my real opinion. It is, however, pleasant to come to Lund and meet many friends and discuss algological and limnological problems.

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Dec 16 1950

Dear friend Teiling,

I have been very busy for the last several months, and still am far from having finished; so that must be my excuse for not having answered sooner your letters of Oct 23th and Dec 12th. Both my wife and I greatly enjoy reading your friendly, chatty, informal letters, giving the news of what you are doing.

Winter has started early in New Orleans this season. Already we have had two freezes, though on both occasions the temperature barely went below the freezing point. Nevertheless all the banana trees and the poinsettias have all been killed. For the last month the weather has been chilly; today the maximum temperature is 55°F (12°C) which though not cold is somewhat uncomfortable for us, and our automatic oil-burner has been going on and off all day. This automatic heat is quite a joy. Our house is what is called here a "raised bungalow". The living rooms are raised about 2.75 metres above the ground, and the "basement" (really the ground floor) is paved with concrete and contains my workshop and laboratory, a very large storage room, another room where the washing is done, and the automatic oil-burning boiler which heats the house. This works entirely by itself, regulated by a thermostat upstairs. It starts at 6 am and by the time we get up the house is comfortably warm; then it maintains a temperature of 70°F (22.5°C) all day; and shuts itself off at 11 pm; or I can push a button and let it run all night at a somewhat lower temperature on real cold nights. The oil-man comes around and puts in 150 or 200 gallons of oil in a large outside tank about once a month, and all I have to do is pay the bills. Yes, it is a little more expensive than coal, but there is no bother whatever.

My wife and I hope to go on a trip between Christmas and New Year, but though the time of departure is less than a week away, we are still not quite sure if we shall be able to go, because one of the members of our firm has been in the hospital for three weeks. He is next-in-command after me, and I cannot go away unless he is able to return to the office. But it looks as if he will get out of the hospital in the next day or two. Fortunately all we have to do is pack our bags, lock up the house, climb into the automobile, and drive away. I should like to visit Mexico again, but there are no desmids to be found there, so I suppose it will have to be Florida once more. Certainly I shall not drive North; I had enough of cold weather in New York last winter.

I have been working on three desmid papers, by Prescott and myself. One of them, on *Micrasterias*, has just been accepted for publication in *Trans. Am. Micr. Soc.*, but it may be a year or more before it is printed. The second, on *Euastrum* is waiting for Latin translations for the new species and varieties, which I cannot do myself. The third, on South Australian desmids, was finished only this week, and I was just about to rewrite it in its final form and send it to the editor of *Botaniska Notiser*, when just this morning the postmen brought an airmail letter from my friend in Australia saying that he would prefer to have it published in Australia. Further, he is sending more collections, and suggests that we wait two or three years for still more, and then consolidate all of them into one large paper which would then be published in the *Journal of the Royal Society of South Australia*. I agree with him that it would be much more satisfactory than several shorter papers, but it means that much of the work that I have done will be wasted, and God alone knows what may happen in the next few years.

I do not have either of the papers that you offered to send me, by Gutwinski, and Allorge & Mengin, and should be very grateful if you would send them. In return I am sending you a Japanese book, containing Okada's "Preliminary list of Desmids in the Northern Kurile Islands", which was sent to me by Dr. Minoru Hirano, of Kyoto. The paper on desmids is not of very much value, since it is only a list, and contains only well-known cosmopolitan species, but it can take its place in your library.

of

I am enclosing a sketch showing a large circular desmid, similar to *Cosmarium contractum*, showing the chloroplast arrangement, which is the only one that I have made of the chloroplast in any desmid. The arrangement is somewhat unusual, consisting of seven bands, with a slight spiral twist. I do not know if it is *C. contractum* Kirchn. or not, and should be glad if you would tell me your opinion.

Grönblad has written me several times recently, but his letters are not like yours. Mostly they are merely acknowledgements of the many boxes of samples and parcels of drawings that I have sent him (525 tubes of material and more than 3000 drawings), which will be enough to keep him occupied for a long time. In one letter he says that he has commenced work on them.

I have also received several letters from Krieger this year, thanking me for the CARE food-parcels that I have been sending him, and acknowledging a box of *Aggl.* collections from USA that I sent at his request. He wanted specimens of American *Cosmaria* so that he can make original drawings for the next installment of his monograph that he is now working on. He does not have very much to say about desmids, but I have asked for his opinion on a very strange new species of *Euastrum* in one of my gatherings. I am not greatly surprised to hear that he does not like your new genus *Staurodesmus*; on the other hand I do not like his high-handed fashion of arbitrarily changing the names of other people's desmids. For instance, in 1934 Taylor described *Triploceras gracile* var. *bispinatum* var. nov. from Newfoundland, and gave in detail his reasons for not assigning it to Nordstedt's subspecies *bidentatum*. But Krieger calmly lists Taylor's plant as a synonym of var. *bidentatum*, without any explanation, and certainly without having seen the plant. Several of my collections contain a variety of *Tr. gracile* which I take to be the same as Taylor's, and they certainly are not the same as var. *bidentatum* which I have also seen from Australia.

Further, he has transferred Borge's *Docidium australianum* to *Triploceras australianum* comb. nov., but the plant is not a *Triploceras* at all; it is most certainly a *Pleurotaenium*, because it has a smooth rounded apex surrounded by a circle of stout spines, quite analogous to *Pl. Kayei*. I have seen several specimens from North Australia.

Again, on p. 115 of his *Lieferung on Microsterias*, he excludes *M. ceratophora* Josh. as "whol teratologisch", but it is a perfectly normal desmid that I have seen from both North Australia and Borneo.

He quotes *Euastrum denticulatum* var. *stictum* Børges. as a synonym of *E. Turneri* W. West, but the plant, which I have found in Florida and also in the South Australian material, does not check with *E. Turneri* nor either of the two varieties shown by Krieger, nor with West and West's drawing. It has smoothly undulate lateral margins without an incision dividing it into apical and basal lobes. Prescott and I are going to describe this as *E. denticulatum* var. *stictum* (Børges.) P. & S.

There is also an unnamed species of *Euastrum* figured by Borge (1894) Taf. 3, Fig. 39, which Krieger considers "unsicher". But I have several good drawings of the same plant from North Australia, quite normal in every respect. We shall probably name it *E. Borgeanum* P. & S., since *E. Borgei* is already occupied.

I could name several other instances; for instance, *E. Wolllei* var. *cuspidatum* Wollb. and *Pl. spinulosum* (Wolle) Brunel, which has been so ably treated by Jules Brunel in a paper that I suppose he has sent you. I have sent Krieger some material containing this plant, so that he can see it for himself.

Now, if I have found all these errors in my very limited experience, there must be many more in his monograph. So it cannot be relied on unreservedly; but it is invaluable to workers like myself who do not have access to much of the world literature.

I have also noted that Krieger does not provide a Latin translation of the diagnoses for his new species and varieties, so that 25 years from now somebody will declare all of them to be invalid on this account. I asked Grönblad what he thought about this, and he wrote that he had called Krieger's attention to it, but had not received any satisfactory reply. Grönblad added that K. is a very good man, busily engaged in a tremendous task, and he is not going to bother him any more about it.

A few days ago I received a paper on fossil desmids from Prof. Jadwiga Wołoszynska, of Krakow, Poland, but it is written in Polish of which I know nothing whatever. The very short English resume is not enough to give an idea of the scope of the paper. She has a large number of excellent photomicrographs showing not only the ornament of the cell-wall, but also the pore-systems in some cases. I should like to know what method she used to get such beautiful pictures.

Another recent paper is "Fresh-water Algae of the Maritime Provinces", by Dr. Elwyn O. Hughes, Dept. of Botany, University of Oklahoma, Norman, Okla., listing a large number of desmids and other algae from Nova Scotia and Prince Edward Island in Canada. There are 56 pages of text, and four plates of fairly good drawings. Some of the identifications are incorrect.

For some time I have intended, but have forgotten, to ask if you would like me to send you some more coffee and rice. If you desire it, just say the word and you shall have it for your birthday next March, since I cannot get it to you for the Christmas and New Year celebrations.

With my best regards and good wishes,

Your friend,

Oct 3 1950

Dear friend Telling,

On my return from Columbus I found your long and very interesting letter of Sept 12th, for which many thanks. I also thank you for your conversations with Grönbled, which no doubt influenced him in his decision to work with me. I am now in the process of sending him all of my original drawings, which I have had copied by photography, and in the next couple of weeks I shall have finished the despatch of more than 3000 drawings. The samples will take longer; I have already sent him three boxes containing about 190 samples, some of which I had on hand. There were about 300 more to go, and these will have to be prepared specially from my larger bottles. So he will have plenty to work on for some time; and it will be O.K. for you to keep the Staurodesmus drawings until you have finished your new paper. As to your request for "good and clear drawings of Cosmerium and other biradiate species", I am not quite clear as to what you want. None of my drawings show the chloroplasts, but I have several hundred drawings of Cosmerium, Euastrum, Micrasterias, Xanthidium, biradiate Desmidiium, and many biradiate Staurostrum, the latter including a puzzling series of 2-, 3-, and 4-radiate forms which I think are all closely related to St. Johnsonii. I suggest that you write Grönbled in more detail what you want, and tell him that you have my permission to borrow them.

Recently Dr. Krieger asked me to send him some samples containing typical American Cosmeria, so that he could make original drawings of them for the next installment of his great work which will deal with the genus Cosmerium. So I had the pleasure and honour of preparing for him a number of samples specially selected for their content of Cosmerium species which occur only in this country, such as C. dentatum, coemetum, novae-terrae, refringens, sublobetiforme, quadrilobum, etc., and I also sent him sketches of several zygospores that have not been published previously.

Thanks for the elucidation of the meaning of "meristem" and "organelle". I have one book on general botany, which I bought for the purpose of learning more about the subject, but I am unable to work up any interest in it. All that I know about botany is the little knowledge that I have acquired of desmids, and the study of these beautiful little plants will be quite sufficient to last me for the rest of my life. I have just received a shipment of material from Java, Bali and Borneo, with a great many strange and beautiful forms.

The meeting of the Phycological Society at Columbus was a complete fizzle, as far as I am concerned. Neither the President Brunel, nor the Secretary Prescott, was able to attend. Of all the men and women whom I hoped to meet again, I saw only Teft and Trenseau, and these in fact were the only people to whom I spoke, so I had no chance whatever of discussing your paper on Desmid Radiation. I have sent you a copy of the programme, from which you will note that eight papers were scheduled for the morning session. Only three of these, Nos. 5, 6 & 7, were presented. The other authors neither put in an appearance, nor delegated anyone else to read their papers, nor offered any explanation for their absence, so far as I know. I was so greatly disappointed that I did not go back to the University for the afternoon session. But my wife and I made a pleasant 10-day vacation out of the trip, and we returned through the mountainous regions of Virginia and Carolina, which neither of us had seen before. Also I found a new and rich desmid-bearing region in the neighbourhood of Savannah and Brunswick, Ga., where I made a number of collections that I have not yet had time to examine except for looking ~~for~~ a drop or two out of each bottle. The desmid-flora is quite
at
similar to that of the adjoining region of northern Florida.

A week ago I sent you a book "The Plants of Bikini" by Wm. Randolph Taylor. I ordered this from a pre-publication notice, because I thought it would tell something about the fresh-water algal flora, though I was aware, of course, that it could be only very scanty, and that few if any desmids could be expected in these isolated coral islands of the Pacific. But he dismisses the fresh-water flora in half-a-dozen words as "trivial". The book is of no use to me, so I am passing it on to you, and if you don't want it you can hand it to somebody else.

Also I have sent a reprint of a small paper, my first, on some new varieties of *St. Ophiura*, with an extra copy that I would like you to send to Ruzicka. I did not write anything on Ruzicka's copy, since I think under his present circumstances that it is best it should not be known that he has any communication with the USA. I was greatly distressed to learn of the conditions under which he is working, though he says nothing of the kind of work he is doing. His paper seems to be very good, and I am only sorry that I do not understand a single word of the Czech language. It is surprising that a newcomer can write in such a detailed and thorough manner on what appears to me to be an extremely difficult group of desmids. Please give him my best wishes and remembrances when you write to him again. I am sending copies of his letter to Prescott and Brunel who have had some correspondence with him.

I had intended to write a series of short papers on new desmids from my collections, but since Grönblad is going to work up all the remainder of my stuff I have dropped the idea. However, Prescott and I have three joint papers that are nearing completion. Two of them, on *Bustrum* and *Microsterias* respectively, will be published in this country. The other one, which is only waiting for the Latin diagnoses, deals with some collections from South Australia, and it seems to me that it would be better to have it published in Europe, where there are many more desmidiologists who are interested in, and familiar with, the desmids of the far East. I had in mind offering it to the *Acta Hydrobiologica*, or perhaps *Botanische Notizen*. Some time ago you mentioned that you thought the editor of the latter journal would accept a paper from us, and I wonder if you would be kind enough to ask him if he will consider it. If he will, please tell me his name and address. There will be five plates, illustrating three or four new species and a number of new varieties, and a corresponding amount of text. I do not know the number of pages of text, because I had to send the only copy of the paper away for the Latin translations.

I am glad that you spent such an interesting time in Paris with Bourrelly, who wrote me a nice letter a few weeks ago, explaining once more the reasons why he could not send me any of his Madagascar material, but saying that perhaps he will be able to do so when he has finished his study of it. I do not find it strange or laughable that you purchased pistols and a violin, since I also like to indulge myself when I see anything that I like. Thus I have a large assortment of woodworking tools and machinery which I never use now; but 15 or 20 years ago my hobby was the making of fine furniture, cabinet work and marquetry. I still have a collection of fancy veneers of tropical woods from all over the world.

That's all for the present. Best regards from

Your friend,

Linköping 12/9 1950
Dear Friend Scott,

Many thanks for your letters. The first one, of 27/VI, I got just in the busy time of the finishing term and then I went, very tired, to my summer-cottage and wrote a lecture for the Congress, in English. In the last days of June I went to Anaboda Limnological Station for lecturing and then directly to the Congress and at last, even before the end of the Congress, I went to Paris. In August I had only 1 1/2 week for recreating and a short plankton-trip before my teacher-ship began.

I am glad that you had such a success of the Australian desmids. I hope that Prescott soon will sum up your results. I associated frequently with Grönblad during the Congress, and I submitted all the weighty reasons for a valuable cooperation between you and him. I should be glad if I had succeeded in influencing him in the right direction.

Many thanks also for the interesting figures of *St. sagittarium* and *Sexangularis* and for the 15 dollars. I fear that you have spent your own money for it haven't you???? It was not necessary to ~~have~~ exchange the dollars to francs because I did not buy any expensive things. Now I intend to buy better lenses and some valuable things for my research.

I must say that I was rather tired when I must enter in my service. I can not remember that I have previously begun my teacher-ship with greater dislike.

I am glad that you like my Radiation paper, I think it will be useful because of its new points of view. I have received estimating letters from Irénée Marie and Lund. The former says that such papers are just what the desmidiologists need and supposes that coming generations will appreciate it more than the present one. I fear that parts of the content are not immediately apprehended viz. ~~the~~ those facts which have their fundamentals in a more extended biological knowledge than many desmidiologists are accustomed to.

Meristem is a term indicating an embryonal tissue which gives rise to shoots, leaves, roots etc. A meristem is the beginning of all buds and all growing phenomena depend on the activity of cell-division. That is why I used the term meristem in order to signify the growing activity and more rapid rate of growing activity of the processes and outbulgings of desmids. These active parts are not tissues but parts of the protoplast. Within the unicellular organisms we don't speak of "organs", which always are composed of cells, i. e. tissues. Thus, the biologists speak of "organelles" in order to signify the homologous activity of certain parts of the protoplast within one cell.

Organelle is a term seldom used in Botany (unicellular plants are not so specialized as unicellular animals, thus the zoologists have created the term which is very often used on the Infusoria, the differentiated parts of for instance *Paramecium* are "organelles". If you like to know more about meristem, you can find it in any textbook of botany, for instance Arthur W. Haupt's *An Introduction to Botany*, published by Mc Graw-Hill Book Company in 1946. I think you will find it in the Public Library of your town.

My paper was written in a hurry. I ought to have remembered your great experience and sharp eye, both of things that make you an excellent desmidiologist. I am sure that you were able to give me valuable material for the subject dealt with in my paper and I should be glad to receive by and by what you can give me. I ~~xxxx~~ was obliged to have the paper printed for the Congress but I am sure that it had been more valuable if I had collected more material and ~~thoughtxxxx~~ reasoned out the results more. I was anxious to have it out in order to stimulate the desmidiological research as soon as possible. I am old and delay is dangerous. Some conclusions which appeared after the paper was written, were, however, put in to the paper, though the Editor quarrelled of the length which had become greater than I had promised. I am now writing a supplementary paper, mainly dealing with the light-ecological point of view. At present I am examining the literature for collecting most of the figures of the chloroplasts. It seems to me as if it now is possible to give a firm basis for a separation of the Cosmaria into two phylogenetically different groups: the one with pluriradiate chloro-

plast with one single pyrenoid, for instance minutum, ^{and} subtile, and an other part with biradiate chloroplast, originally, from which a lot of more developed and complicate chloroplasts have arisen, ending with the parietal ones. If you have good and clear figures of Cosmarium and other biradiate species, I should be very glad to hear of you.

I am very sorry to give them back now. There are so many which I should like to copy. I must find out a method of taking clear photos of them, graphit-drawings are difficult to get uniform. I hope to be able to begin the definit paper of Staurodesmus at Christmas; all preliminary work, figures, systematic, principles etc. are ready, but the final writing is, nevertheless, an intricate job. I shall do my best in sending you the figures as soon as possible and I am very grateful to you for your permission to use them scientifically. I don't know the details of your agreement with Grönblad, but if his task is to scrutinize the figures, and, if necessary, control or complete them by means of the samples, I think he has job enough for a long time with the material you send him as the first? Thus, I make the proposal of keeping the Staurodesmus figures at me during the nearest time. This is only a very modest proposal, of course I comply with your decision such as I have apprehended it in your letter. I am sending the figures of the polybrachiate Staurastras soon enough after having copied the extremely dichotomous forms. In your collection there are a series of Std. maximus, very interesting because of the reduction of the spines, a process previously known from Std. convergens and leading over to "Cosmarium". I want to use it in my paper.

I had a good fortnight in Paris. Bourrelly is a nice fellow, he did everything in order to make my stay there as pleasant and successful as possible. I spent most of my time in the laboratory and in the classical place of "Jardin de Plantes", where the lab is only a little part of a lot of institutions of Natural History. I procured a very fine microlamp, manufactured by the old factory "Machet". I have been longing for such an effective lamp since long but it has been too expensive. I also bought two things, which must seem very laughable to you: two kind old pistols and a very nice shaggy in the Librairie.

I enclose a copy of a letter from Ruzicka, very depressing. It gives the impression that he is a convict. Supposingly he is not a sympathiser of the special form of "democracy" of sovjetian model. It is dreadful that this fine, good man will have his future destroyed for the sake of liberty. It should be very interesting to hear of Prescott or other Desmidiologists that you have met with, have mentioned their opinion of my paper. Irénée-Marie wrote me spontaneously a very appreciating letter, and also did Lund. I was surprised; I thought that Irénée-Marie was a pure species-maker!

With my best regards and wishes,
Sincerely yours,

Erwin Schulze

I ought to have remembered your great experience and sharp eye, both of things that make you an excellent desmidiologist. I am sure that you were able to give me valuable material for the subject dealt with in my paper and I should be glad to be helped by and by what you can give me. I was obliged to have the paper printed for the Congress but I am sure that it had been more valuable if I had collected more material. I was anxious to have it out in order to stimulate the desmidiological research as soon as possible. Some conclusions which I gathered after the author's paper, though the author had been rather than I had promised. I am now working a supplementary paper, mainly dealing with the light-ecological point of view. At present I am examining the literature for collecting most of the figures of the chloroplasts. It seems to me that it is now possible to give a firm basis for a separation of the Cosmarium and Staurodesmus groups: the one with parietal chloroplasts and the other with one single pyrenoid.

Sept 5 1950

Dear friend Telling,

I received the postcards which you sent from Stockholm and Paris, many thanks for remembering me. Also I have a note from Bourrelly saying that you had spent two pleasant weeks working with him. I am quite sure that you enjoyed the trip, and I hope that the rate of exchange was not too unfavorable for you. I spent two days in Paris back in 1919, when I was in the U.S. army, but all that I remember is that Paris is a beautiful city, and that the girls at the Polices-Bergres were very friendly.

Thanks also for your paper on Rediation of Desmids. Some time ago you told me that you ^{were} working on this subject, and it is a pity that I did not have a clearer idea of the scope of your work, for I could have furnished you with many more interesting examples of both dichotypical and Janus forms, some of which are very puzzling. Already I have read your paper twice, and shall read it many times more. It gives me an inkling of the reasons for the widely differing shapes of desmids, and if I knew more about botany in general I could understand it better. For instance, I do not know the meaning of either "meristem" or "organelle", but from the context I get the vague idea that the meristematic organelle may be something like an organized group of molecules of such chemical composition that they can synthesize cellulose, pectose, and whatever else the desmid wall is composed of. Similarly I suppose that diatoms have organelles which can take silicates from the water and transform them into SiO₂ or perhaps organic silicon compounds to form the valves. Perhaps, also, animals like Arcella have organelles which form their chitinous (?) shells. I have long wondered how such a mere blob of jelly as an Arcella could manufacture such a beautifully decorated shell, with its intricate hexagonal pattern and its symmetrical arrangement of cusps around the opening. Now, I get from your paper at least the hint of an idea of how it could be done. But I suppose that nobody will ever be able to explain what forces control the arrangement and location of the organelles, so that in a typically 6-radiate St. Bphiura, for instance, they can produce only 6-radiate forms, save under exceptional circumstances. Here is an item which I think I am the first to notice. In St. radians, top view, the 7 or 8 processes are not arranged exactly on lines radiating from the centre of the circular body, but form a slight angle, maybe 4 or 5 degrees, with such radial lines. The first time that my drawing showed this, I thought it was "cockeyed" and that the specimen had not been quite level. But further investigation showed that the drawing was correct, and I have since found this feature in all specimens of this plant where the processes are long enough to draw their center lines accurately.

I am glad to say that I am now able to accept Grönblad's offer of collaboration in publishing the remainder of my desmids from southern U.S., and I am writing him today to this effect; also I am sending him all of my drawings and a complete duplicate set of all my collections. For this reason I must ask you to return to me, as soon as you conveniently can, the drawings of the polybrachiate Staurastrum, also the Arthrodesmus and Staurodesmus drawings, which I sent you some time ago. I cannot find my drawings of Arthrodesmus maximus and varieties, and think they may be among those which you have. You are at perfect liberty to make photographic copies of any or all of these drawings, and to publish any of them that may be useful to you.

The day after tomorrow my wife and I are going to drive to Columbus, Ohio, (900 miles) for the meeting of the Phycological Society. The meeting itself

will probably be as boring as the one in New York last winter, but I wish to renew my personal acquaintance with Prescott, Taylor, Taft, Brnel and others. I think that Irene-Merie will be there, and I am anxious to meet this Frenchman, who must have some peculiar mannerisms, judging from his correspondence. A few months ago I sent him some rich collections, but he is afraid to examine them, for fear that he may find in Canada some of my new desoids and that he may anticipate me in publishing them. I told him, politely, that he is crazy.

With my best regards and wishes,

Sincerely yours,

June 27 1950

Dear Friend Telling, (Is that better?)

Please accept my very sincere congratulations on your honorary degree of Ph.D. That is something to be very proud of, and more so in your country than in the USA, where honorary degrees are conferred upon many people who have not the slightest right to them, even politicians, and benefactors who donate large sums of money to the universities. I cannot resist the temptation to place your full title on the envelope, just this once. Your description of the ceremony was very interesting even for me, who knows nothing of college or university life.

I also have been very busy for the last few months. I have finished my examination of the North Australian material, and found a total of about 240 species and varieties of desmids in the five vials. Many of them are cosmopolites, but also there are many great rarities, and undoubtedly some new species, which will have to be determined by Prescott. All of the desmids are to be illustrated, by special request, even the common and well-known ones. This means that I have to draw 15 or more plates. Already I have made the pencil lay-out for five plates, but for the last week I have been unable to work on them because of the great heat (today 96° F. and about 96% relative humidity)!! My hands are sure perspire so much that it is impossible to work on a finished drawing, so I am writing letters instead.

Here are the addresses that you asked for:

Dr. Lewis H. Flint, Dept. of Botany, Louisiana State University, Baton Rouge, La.
Dr. M. Sechlan, Laboratory of Inland Fisheries, Tjikeuseuh 99, Bogor, Java,
United States of Indonesia.

Dr. C. S. Nielsen, Associate Professor, Dept. of Biology, Florida State University
Tallahassee, Fla.

In the next few days, if the weather becomes a little cooler, I shall make copies of my sketches of *St. Sagittarium* and *sexangulare*, with their variations, but they will be for your private information only, since I cannot grant you permission to publish them before the appearance of the official report on the Arnhem Land Expedition, which may not be for a couple of years.

Last week I received from Minoru Hirano, University of Kyoto, Japan, a lot of 38 samples of algal collections from his country, in return for some of my own collections. But I was disappointed to find that nearly all of his samples are of the diatom-rich type, with few other algae, and especially poor in desmids. Nevertheless, there are a few collections that contain some very curious desmids that are completely strange to me. Some of them would be of special interest to you, viz: a very large and stout *Staurastrum* which from its size and general appearance I took to be *St. arctiscon* var. *glabrum*, but from further examination and more examples I think it must be an undescribed (?) variety of *St. leptacanthum leptacanthum*. It occurs in the same gathering with a much smaller and more slender form of *leptacanthum* with 9 and 6 processes. He sent me a generous quantity of material, and I shall ask him for permission to distribute it to some of my friends, including you, with the understanding, of course, that nothing must be published without asking his consent. If he is accustomed to working with such poor desmid material he will get a big surprise when he looks at the very rich collections that I sent him.

I have heard nothing from Grönbled since last January, and he has not told me definitely whether he will or won't, can or cannot, agree to my proposal of collaboration in the publication of my USA desmids. If you see a chance, please give him a gentle prod. But the developments of the last few days in Korea may set

the whole world on fire once more, and if that happens (which God forbid) I don't think that anyone will have time to worry about desmids, either in Europe or on this side of the Atlantic.

Prescott had wanted very much to attend the Botanical Congress in Stockholm next month, but could not afford the expense. Instead he is teaching a summer course at the Univ. of Montana Biological Station, Flathead Lake, Big Fork, Montana. I have never visited that part of the USA, but from the map it appears that the location is at an altitude of 3000 or 4000 feet, so it ought to be cool.

Recently I read in "Time" that the unofficial rate of exchange for French francs had changed considerably, and is now not very far from the official rate. You had better look into this, since it may affect your plans.

So far this year I have done no collecting. Next week-end is a holiday, and I could get away for about five days, and had thought of visiting a new region in southeastern Georgia, near Savannah, which I think ought to be productive of desmids. But driving 1000 or 1500 miles in this kind of weather is no joke, and I haven't made up my mind whether to go or not. If the war news gets worse in the next two or three days I may make the trip, since if war actually develops it will put a stop to travelling for a long time.

With my best regards,

Sincerely yours,

Linköping 12/VI 1950.

Dear Friend,

many thanks for your letter of 2/V. There has elapsed a long time since then, but May has been a very busy time, it ended with an inforgettable day in Lund. I had some days vacancies before the Promotion. Perhaps you are interested of hearing how it is arranged in Sweden? A brilliant day: sunshine and wind enough to keep the flags stretched, the Swedish, the Danish and the star-striped ones. There were created also five Danish and two American doctors honoris causa: Ira Sprague Bowen and Milton Lasalle Humason, two famous astronomers from Mount Wilson and Palomar observatories. I am sorry to tell you that they were absentes. The Danish, one jurist and four medecins were present, they had a much shorter journey than I. The ceremony is in most parts from the Middle Age, only Latin except the introducing lecture. This was the biggest promotion since 1853, (68 doctors) and was taken place in the old Cathedral, consecrated in 1145. Each doctor received the ring and the diploma, the ones of the Philosophical Faculty were crowned with a wreath of laurel and the doctors of the other Faculties got instead of it doctor's hats. At the banquet 318 persons were present, but many more were refused because of lacking space. Sorrily I was obliged to go home early next day, ~~the~~ duty was calling!

Before my name I now have the right to place Fil. Dr. h.c., but I hope that you don't feel obliged to use it and, if you want to confer another benefit on me, don't write "Dear Dr Teiling". If you are persisting, I am ashamed of my more intimate introduction of my letters to you. I feel, that we are members of the great fraternity of Scientists, and, to judge from your proved kindness, I hope that you also have the same feeling.

Your notes on the two forms of St. sexangulare are highly ~~fasting~~ interesting. I have since long attached my attention on St. Arctiscon and leptocladum, which only differ in the surface of the processes. These desmids are very near allied. If this difference is to be regarded as specific or not is a matter of taste. Every contribution to the amplitude of radiation is very welcome to me. I should be very thankful if you kindly send me drawings of sexangulare and sagittatum. Of the latter only Nordstedt's figures are known (?). I have considered it in a discussion on St. Arachne and allied species.

I should also be very thankful for adresses of Vaas & Sachlan, Lewis Flint and Nielsen & Madsen.

I have not heard anything from Bourelly since I sent a letter to him a month ago. Perhaps he is searching for a lodging for me in the neighbourhood of

the Laboratoire. I am very anxious of my planned collaboration with him.

In some days I am going to my summer-cottage where a hard job is waiting: weeding the garden plots, a tedious and tiresome job, and, for all, writing a lecture in English for the Botanists' Congress.

Many thanks for the valuable journals and the papers on Myxophyceae and culture methods.

As to your kind offer to send the money of the Miami university, I think it is more convenient that you take it as a fund for the postage expenses to me.

From a "microscopical" friend I have got the refractory index of coumarine: 1,56.

I think you hat a rather hot weather in Louisiana. We have had a hot time, up to 27 ° Celsius, not much for you. Ordinarily we have a little over 20° at madday time, rain has not fallen during several weeks and the soil is very hard, when weeding last sunday I must vigorously use the hoe.

I hope that you will have a good summer with many new interesting finds of our lovely desmids.

Sincerely yours

Emory Tilling

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May 2 1950

Dear Dr. Telling,

I now have several of your letters to acknowledge, and also to thank you for the various things you have sent me:- the two photographs of Dr. Nordstedt, the copy of his *Symbioses et flora Brasiliæ*, and the two papers in French on mounting algae. I like the pictures of Nordstedt very much; he was a very noble, wise, good, and kind-looking man. Rather strangely, he resembles the photos of my father, who died 35 years ago. My father also used to wear a long grey beard and spectacles; but he was far from being a scientist, in fact he had comparatively little education. I am having the larger picture framed, to hang in my laboratory, and I should like to have a larger photograph of you, to accompany it.

The "Symbioses" I already possessed, so I have sent it to Prescott, with instructions to hand it to one of his students if he already has it. The two French papers on mounting are interesting, but they are simply variations of the glycerine jelly method. I cannot find the refractive index of coumarone listed in any of the works in my library, nor is it listed among the gums used for mounting. I do not see what part the chloral-phenol plays in DeFlandre's process. It would be simpler to gradually replace the water with acetone, and then replace the acetone with the solution of coumarone in xylol. In any event these methods would be useless in the case of a single rare desmid found in searching a mount, and Nygaard's method seems to me the only one adopted. But when you search a mount it is always covered with a cover-glass, and if you remove the cover-glass it is 10 to 1 that you will not be able to find the specimen again. At least that has been my experience. So when I find a rare desmid that I want to preserve, I simply seal the edges of the cover-glass with melted vaseline, and let it go at that. Such water mounts probably will evaporate in a comparatively short time, but it is the best I can do at present.

Both Skuja and Nygaard have very kindly sent me their large works, and I have written to thank them. I had a nice reply from Skuja commenting on some sketches that I sent him of some slightly differing forms of his *Staurastrum octodentum*. I am glad you told me something about his personality and mannerisms, so that I may be a little circumspect if I have occasion to write him again. I can hardly believe that his drawings are made without the use of a camera lucida. They are very good but I think they could be improved if he would make his lines not so heavy, and use less shading. Nygaard's work is a masterpiece of laborious collections, observations and tabulations, and it will take a long time for me to read and digest it. His conception of the frequency factors of the various microscopic organisms as an index for evaluating the type of habitat is quite new to me, though I had, of course, recognized in a rough way the differences between Myxophyceae and desmid habitats. In fact I can now tell the favorable desmid habitats from the unfavorable ones merely from a quick glance at them while driving along the road in my automobile. Naturally, I am frequently fooled! Sometimes the most unpromising waters yield surprisingly good collections, and vice versa.

Please accept my congratulations on your degree of *Doctor honoris causa*, and tell me what letters I should add after your name! I am sure that your family is delighted, and that it will add to your renown in Linköping, as well as among all those who know of your work.

Your description of the conditions under which Bourrelly is working explain quite clearly why he was unable to let me have any of the Madagascar material. It is a little difficult for me to realize such conditions; for I have been my own boss for so long that I should never be able to work under such abusereretic regime.

A couple of weeks ago I sent Bourrelly a box with half-a-dozen vials of my best collections, just to let him see what a large assortment of "fancy" desmids we grow in this country. When you see him this summer you can tell him something about me, and perhaps at some future time he can get permission from the Director to exchange material with me.

Prescott was not able to come to New Orleans for a visit as he had hoped, much to my disappointment. It was one of the many things that he plans, but which never turn out as he expects.

With regard to your paper on desmid radiation, here are some disconnected facts which may or may not be of value to you:

In the material from Arnhem Land, North Australia, I have found *Staurastrum sexangulare* in two distinct forms, one with smooth processes, and the other with granulate or denticulate processes. Each of these forms occurs in 4, 5, 6, and 7-radiate forms. *Staurastrum sagittarium* Nordst., previously known only in 7 and 8-radiate forms, also occurs in 6, 9 and 10-radiate versions. I should be glad to send sketches of these if you want them.

Enclosed is a list of algal literature which I have received recently from various sources. Most of the titles will be familiar to you, but there may be some of which you have not heard. If you do not know the addresses of the authors I can send them to you. I have added some comments on the value of the papers.

I am glad to know that my recent air-mail letters have reached you promptly. Do you wish me to forward the material which was sent to me by the University of Miami, in addition to that from Stanford University?

This is all for the present. With my best regards,

Sincerely yours,

Apl 2 1950

Dear Dr. Telling,

Your two letters of Mch 26th and 28th arrived almost simultaneously. The first one had a long ride; it was sent by mistake to Los Angeles, and thence returned to New Orleans. The second arrived the following day.

Prof. Skuja has very kindly sent me his large work on Burmese algae, at your request, and I thank you very much for having asked him to do it. The book will be of very great help to me in connection with my Australian collections and I wrote immediately to thank him for it. Also I sent him sketches of two curious desmids illustrated in his work, which I have found in the Australian material; namely, *Euastrum diplostauron* Skuja, and *Staurostrum octodontum* Skuja, both differing somewhat from his figures. I have also found several others, better known than those above-mentioned.

Two weeks ago I received a friendly postcard from Nygaard, saying that he was sending me, by the same mail, his large work and also the paper by him and Osterfeld on Algae of the Getun Lake, and was also returning the English magazines which you sent him. But none of them have arrived yet; I hope they are not lost in the mail. I shall wait another week and then write him enquiring about them.

Prescott's paper on "Desmids" was disappointing to me, also. There was very little new in it; in fact I had read practically everything it contains previously in other authors. I have not seen Irene-Marie's paper in criticism of the Wests', but should like to see it; he does not send me his papers unless I write and specially ask for them. The Wests, of course, were not infallible, any more than other algologists. I find myself in disagreement with them in the case of *Cosmerium obsoletum* var. *sitvense* Gutw. They say that the protuberance at the basal angles is hyaline, and that the apparent spine is really a pore. I have recently found this desmid in considerable numbers in the Australian material, and I cannot see it the way they did. The protuberance is not hyaline, in fact I can see a thin but distinct "skin" around it, and the spine really looks like a spine to me, though why a spine should be surrounded by a membrane is more than I can say. I can see no evidence of an opening at either end of the spine or pore, though I have looked carefully for it.

I shall be very glad to receive the photograph of Nordstedt, and the figures of his Brazilian algae, though I think I have the original paper. If this turns out to be the case I will pass them on to Prescott, who can give them to someone who does not possess them. It is always interesting to see photos of the authors whose works you read, and especially of those who were or are great authorities.

Ruzicka also wrote me a letter, received a week or so ago, giving the same news that he sent you. I am very much afraid that Czechoslovakia is going the same way as Hungary and Bulgaria, and that diplomatic relations between the U.S. and that country will be broken off, and that U.S. citizens will be forbidden to travel there. Ruzicka, and millions of other Czechs, will have to submit to the Communist rule, since it is probably impossible for them to get out of the country.

A couple of months ago I wrote to Bourrelly, asking him to send me some of his reprints, which he kindly did; but his supply of the Madagascar papers was exhausted, and that was what I principally wanted. I also suggested that I would be glad to send him some material from my rich Florida collections in

exchange for some of his material from Madagascar. He replied that he could not do it, for the following reasons:

- 1) The material is not his property, but belongs to the Laboratoire.
- 2) It was not collected by him.
- 3) It is not yet fully studied.
- 4) It is in very small quantity.

Any one of these reasons is sufficient, and together they are overwhelming, BUT, if he really wanted to -----

Nevertheless, I am going to make up a batch of my samples and send them to him, and perhaps at some future time he may get other tropical desmids that he CAN send me. Jules Brunel speaks very highly of Bourrelly, and considers him one of the best of the French algologists.

I know nothing whatever on the subject of redistria in desmids, and shall be glad to receive your paper and see what you have to say about it. In the Australian material, *Staurastrum sexangulare* occurs in 4, 5, 6 and 7-radiate forms, some of which have been given varietal names. In my Florida collections I have an unidentified *Staurastrum* which, I believe, occurs in 2, 3, 4, 5 and even 6-radiate forms. I think all of them belong to the same species, but it will require somebody with more knowledge and experience than I have to straighten the matter out.

I was quite amused by your very cautious approach to the "extremely delicate" financial proposal; and what a lot of worry you wasted on it. I have not the slightest hesitation in agreeing to exchange your dollar checks into cash. Send me the checks, endorsing them if they are payable to you, and I will send you the dollar bills at once. Of course you may keep the five one-dollar notes that I sent.

With my best regards,

Yours sincerely,

Dear Friend,

Linköping, 28/3 1950.

I just got a card from Skuja, that he has sent you a copy of his paper on the Burmese Algae. Thus I have cancelled the order at the book-seller. According to what I wrote in my letter of yesterday, I hope that you agree to my retaining the five dollar-notes and you utilize five dollars from my money in your safe. Do you agree?

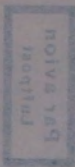
In my paper I have been obliged to criticize the Wests, who actually had no or little grasp of Taxonomy above pure species-making. I am somewhat afraid that certain Englishman, Fritsch e.g., will feel hurt; West is their great name. I have chosen the kindest words not at all in the manner of Irénée-Maxie. Have you read his pamphlet against the Wests, especially G.S. West? It is rather malicious, and unhappily, his premisses are often wrong. He has not sent it to me but I have read a copy of Grönblad's.

Kind regards!

Sincerely yours

Edvard Tellberg

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QAKR HBY A VYSEY
SROKREK LOKRE OET HROSLAC
KOPHILANTIES HIL HAY 281
CROONDI A V KINCTI CHEKENT
MAYPOCPEA
BEBOKOBYM

Linköping 26/3 1950.

Dear Friend,

many thanks for your letter. I have ordered Skuja's book. The distributor writes to me that any specimens for the market are not yet available but he has noted my order. Then I wrote to Skuja and told him your desire, perhaps he will send you a copy, I hope so. I also wrote to Mygaard and got a letter, where he told me that he had sent you a copy together with His and Ostenfeld's paper on the Algae from the Gatun Lake. Many thanks for the paper of Helen Jean Brown. It is peculiar that she the rather intensive examination has not resulted in any new Desmid forms. A paper on Desmids and nothing but pure known forms! Her paper and the one of Lowe are now copied and sent back, many thanks!

Prescott wrote to me that you have paid my annual subscription to Am Phyk Soc. I am overwhelmed by all the favours that you are doing to me, moreover, always connected with expenses, that I must suggest you to take of my dollars in your care, in the first hand the fees to Am. Phyk. Soc. I am so glad and I am appreciating your kindness so much, but when I now possess dollars, you must use them! Besides, will you, please, change the check into dollars, I remember that the check has a limited duration. As to the Phyc. Soc. I have written to Prescott, that my membership will cease with this year. From P. I also received, inter alia, his survey of Desmids. It is interesting but not so valuable as I had hoped and imagined. This opinion, of course, will rest between you and me. I am afraid that he was not glad when I told him that he had forgotten Staurostrum in the list of Desmid genera.

I have sent you in wrappers, the figures of Nordstedt's Brazilian Algae. I had a specimen of the plates only (Nordstedt printed such ones for the desmidiologists) and I had used the plate IV for my iconotheque. Afterwards I got the figures in pieces from the literary remains of Börgesen. If you do not possess the paper, the figures, nevertheless, may have any interest. Also I sent a little picture of Nordstedt. Perhaps I shall be able to procure another, better, Nordstedt was a very good man, an altruistic man, quite as Borge, they were congenial spirits, he was not married, he lived exclusively for his Science and for the botanists. I am sorry that I never met him. When hearing that I was not rested of Desmids he sent me his index, wasn't it nice?

To day I have just written the literature-list of a paper on Desmid radiation, where I have collected the little, known of this subject, and I have tried to give a hypothesis of the nature of this important feature of Desmids. Peculiarly, no one has in abstracts on Desmids tried to examine the concept of radiation, they have used it in distinguishing genera, species and varieties but nothing more.

I just got a letter from Ruzicka. He has serious anxieties for the future. In some months he will lose his appointment. He has just been registered as a "scientific worker", that is why he is hoping that he may get an appointment suitable for him. I do not think he is fit for manual working. But I fear that the Sovjet Gouvernement has no use for Scientific work. Apparently the small kings of Gouvernement have no use of Law and Justice, their own personal bad opinions are law enough, and in the Tjeckian national character there is a good deal of animosity, envy and persecuting, this was clearly manifested by the English agent, a member of Parliament, who studied the intricate question of the Sudeto-Germans and their difficulties before the latest War. Ruzicka, however, was very glad that he had been able to write his paper on Cosmarium bornavanense.

At last, I dare, with the utmost indecision, ask you about a transaction, extremely delicate. So delicate, that I shall not, by any means, be deceived, if you refuse. Please, remember this. This summer, I intend to go to Paris for studies at the Laboratoire de Cryptogamie. Dr. Bourrelly is a correspondent friend of mine since several years, I met him some days last spring, and we found that we got on very well with each other, we hold agreeing opinions and he was very eager to show me his fine methods and mounted things. A stay there would be very instructive, he has promised to give me every possibilities of a profitable work. I am also anxious to photograph some pictures of Desmids, drawn and coloured by Brébisson 100 years ago. Inter alia there are Brébisson's original figures of Staurodesmus dejectus, a species, afterwards quite misinterpreted, at first by Ralfs and later on by West and others.

March 26, 1952

A stay in Paris means money, even with my very modest habits of living, and I don't
it permits me some extravagances. I have, however, near relatives to whom I must give maxi-
assistance. I have some dollar checks, payable everywhere, I change them into fra-
France I get a certain amount, if I instead have dollar notes, I get much more. The
difficulty is to get the checks changed into notes. Thus, I wonder, if you are enter-
by us. Perhaps I am not so careful, but a question is a question, and if you refuse,
we are quite as good friends as before.

As to *Spinocosmarium*, I have just mentioned in my paper, that the dichotomous
specimens had made the connections between the many forms more clear. If every des-
midologist would publish such an extensive material of his new forms, Taxonomy would
be very easier. I have spoken very loud against the apprehension of "*Cosmarium*" and
"*Staurastrum*" as genera. Every Cosmidologist knows or should know that they are arti-
ficial, an absolutely no genera in taxonomical sense, but names possess a mighty pres-
sure on the opinion, especially on young persons.

At last my best regards,
Sincerely yours,
Einar Telling

I have just written the literature list of a paper on Desmidia radiation,
where I have collected the little known of this subject, and I have tried to give a
synopsis of the nature of this important feature of Desmidia. Regularly, no one has
in Desmidia tried to examine the concept of radiation, they have used it
in distinguishing genera, species and varieties but nothing more.
I just got a letter from Kautsky. He has been registered as a "scientific worker"
months he will lose his appointment. He has just been registered as a "scientific worker"
ker", that is why he is hoping that he may get an appointment suitable for him. I do
not think he is fit for manual working. But I fear that the Soviet Government has
no use for botanical work. Apparently the small things of government have no use of
law and justice, their own personal bad opinions are law enough, and in the Russian
national character there is a good deal of animosity, envy and persecuting. This was
clearly manifested by the English agent, a member of Parliament, who studied the inter-
state question of the Gubeto-terranus and their difficulties before the latest war.
Kautsky, however, was very glad that he had been able to write his paper on *Cosmarium*
before the war.

At last, I dare, with the utmost indecision, ask you about a translation, extreme-
ly delicate. I shall not, by any means, be deceived, if you refuse.
Please, remember this. This summer, I intend to go to Paris or at least to the Labor-
atory of the Gubeto-terranus. Dr. Boury de la Motte is a correspondent friend of mine since several
years, I met him some days last spring, and we found that we got on very well with
each other, we hold opposing opinions and he was very eager to show me his fine metho-
d and his every possibilities of a profitable work. I am also anxious to photograph some
pictures of Desmidia, drawn and colored by Boury de la Motte 100 years ago. Later this year
I will send original figures of *Staurastrum* desmids, a species, afterwards this mis-
interpreted, at first by Kautsky and later on by West and others.

Please return to A M Scott

Linköping 24/1 1949.

Dear Sir!

Many, many, most hearty thanks for all benefits: coffee, rice, algae-samples, drawings and the cloud of journals!!!! The coffe-pack arrived just on the last day of the year, exactly in order to give the new year a splendid introduction. On the 1st of Januar I went to Dr Nygaard in København and spent some days in his nice home. He has an excellent wife and two nice, very well educated girls who are very good friends of "Uncle Einar".

Nygaard has constructed an ingenious "micromanipulator" by the aid of which it is rather easy to catch a single alga from the microscope-slide for mounting. Do you know the simple mounting-method by means of solid parafin-wax? If not, tell me and I will give you a description. Nygaard and I intend to make a collection of type specimens mounted on slides, we think it would be valuable to many algologists. Figures are in the most cases more or less subjective, and, if incorrect or drawn not good enough, rather misleading.

I have studied your excellent figures and I hope that you permit me to take copies of those which are of the most interest, there are lots of them, both the monospinous and the polybrachiata ones which are very valuable for my taxonomical work. I got your samples when I was in Lund, a university-town where Nordstedt lived and worked. His immense library is in the possession of the University as well as his extensive collection of slides. I am now back to Linköping and diligent at my work of my lectorate which takes too much of my time. Since september my research-work has been rather neglected because of the illness of my wife. She has been operated twice for cancer in the thyroid gland and during several months the peril of a disastrous spreading of the disease was very impending. That's why I have been a bad correspondent.

I have only taken some looks at your sample La 67 but it seems to give the solution of several questions on the Staurodesmus arebassoni arebassoni which presents a real stroke of luck, extremely rare, equivalent to the sample in which I found the varying forms of Staurodesmus arebassoni figured in my paper, fig. 12 - 24. The dichotypical specimens of your sample show that the different length and number of end-teeth of the processes are of no specific value. Do you permit me to publish some of your excellent drawings??

I have also received a number of very fine blue-prints, showing desmids of new or rarely known radiations. There was no letter in the envelope, are the drawings meant for my private use or is there any questions about them on which you want my diffident opinion??

I have been pondering upon your rich and elucidating collection of figures of Impar, I am not yet, however, clear with my opinion. When I am looking at your drawings, I find it as a good luck, that the superfluous rich area of Louisiana possesses such an energetic and perspicacious Desmidiologist with such a prominent cleverness of drawing as You!! This is no flattery, when I confide to you the expressions of my well-founded estimation.

The genera of the desmids are flowing, several species are impossible to place irrevocably in a certain genus. I have ~~some~~ troubles by limiting Staurodesmus towards Staurastrum and Cosmarium. Many other examples could be referred. Several species of intermediate character must taxonomically be placed in separate genera, for instant Epinocosmarium; there will always be found over-limiting forms. Most of Desmidiologists are conservatively disinclined for new genera, although they must be convinced of the fact, that the "good, old" genera only are crude generalisations. It seems to me a mental laziness, this obstinate persistence in genera, created long ago on basis of minimal knowledge of taxonomical principles. I am writing this, because I am sure that you are parting my opinion.

Yours sincerely
Einar Nygaard

St. Johannekaligens

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