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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.

Jan. 19. 1959.

Dear Arthur,

It is a shame that I have not written to you earlier thanking you for letters, the Geographical Magazine and many other things kindly sent to me. Accept my very best thanks for your kindness.

There is, however, a reason why I have not sent a word to you and it is simply that I have not been quite well. I have some trouble with my coronary arteries (beginning angina pectoris) so that I cannot walk more than 2-300 meters without stopping. Now I have got some more pills (nitroglycerine) and so I have been able to walk better. This is, as you know, a rather serious trouble and therefore I have been in a somewhat depressed state of mind. When you know <sup>that</sup> I hope you are willing to forgive my being so slow. My health has declined considerably. On account of this I have decided not to attend the Montreal Congress. It would be overrating my own health.

Today I have written to Hannah, as you asked me to do. As you surely have read in your newspapers our political situation has been rather troublesome and complicated. Our politicians are not the best ones of God's children. They are always quarrelling as bad little schoolboys and everyone wants to put in his pocket as many millions as possible. There is no one who should think of the country's welfare, only self, self! Our president too is a person whom I dislike very much and who is a ruthless and relentless man when his own position and his power are in question. And our neighbours, the Russians, and our own communists do what harm they ever can do.

But enough about such gloomy things!

Have you read Silva's proposal to the Congress 1959 that Ralfs 1848 as "starting point" should be rejected? Such a change would be quite catastrophic to the desmid nomenclature. I have voted against it and I know that the Czechslovak algologists do the same. I hope you too agree? I have also written a letter to Silva himself. Could you raise some opposition in USA?

Some days ago I had a letter from Mme Gauthier which I am enclosing. (To be returned!). Mme G. has made a new genus "Allorgeia" of our Micrasterias incredibilis. Of course, it is, as often, only a matter of taste but on the other hand this desmid "looks" rather like a Micrasterias. Sometimes I have thought - now, when we know a lot of asymmetrical desmids - that our Amscottia perhaps as well could have been made a Staurostrum - as

also Euastridium could be. Also your Ichthyodontum could have been made a Ichthyocercus without difficulty. There are two different opinions: 1) to make many small genera, such as Euastridium, Spinoclosterium, Spinocosmarium, Allorgeia, Actinotaenium, Nothocosmarium, Staurodesmus, Groenbladia &c. 2) to keep them as large as possible - i.e. as large as they have been and to add new species even if they are more divergent. Personally I like large genera, because, admitted that they become heterogenous, there is a better general survey of all desmids. When we create numerous smaller genera there will be no good summary of the system and there always remain borders where it is difficult or impossible to decide which is which. Such inconvenient borders are: Staurostrum - Staurodesmus, Actinotaenium - Cosmarium, Euastrum - Micrasterias, Micrasterias - Xanthidium (bifurcatum) &c. All earlier attempts to split up Cosmarium and Staurostrum have been unsuccessful. I should be glad to know your opinion in this matter?

You were quite right in your critic of the word "tessellatus". I am sure "cubicus" is the proper Latin word to be used. Only I could not find it in my dictionary of "classical" Latin.

I have not heard a word from Prowse since he sent me some samples from Malaya. Ha<sup>e</sup> has not told me if his inked drawings which I returned to him have arrived safely. I hope it will be interesting to you to meet him and Mr Sachlan too.

As to the problem Micrast. - Euastr. moebii - turgidum I am quite at loss what to do. Eu. turgidum typical is of course without doubt an Euastrum, but some of the varieties of Micr. moebii with the double apical processes are not much unlike Micr. americana (esp. var. lewisiana). So this border is very indifferent and unclear. It remains only for you to take the surgeon's knife and make a cut: and I think both E. turgidum and M. moebii will remain on the Euastrum side. (?) You have seen so much more of these desmids than I, so I think it would be too bold of me to give you any directions. I am surprized to hear that Teiling who never has seen a specimen of this group is giving you his counsel. But he is a typical writing table philosopher! He has seen very little - except Swedish plankton - but he has a very high esteem of his own ability notwithstanding. (I am here a little unkind, but Tg too has been very unkind in his letters to me and to Ruzicka).

Today (20.1) I received the Decemb. number of Amer. Sci. and also your letter containing some of my drawings. Thank you. I too have found some of your drawings which will be returned very soon.

The only difference between Desmidium and Bambusina seems to be the longitudinal striations in the apical part of Bambusina!

From handwriting of Prof. Ruzicka from Ruzicka's Personal Herbarium material 1888. Can you help me with this?

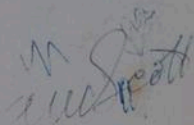
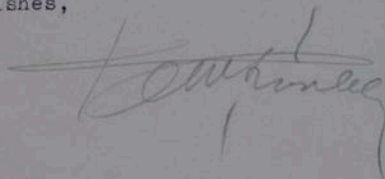
Karis 12.2.-59.

Dear Arthur,

I am sending enclosed a text intended for publishing in "Taxon" and I should ask you to correct the English. Would you perhaps join as author too? What do you think Prescott's opinion is? Now I believe it would be important to have this "article" published as soon as possible. So if you like to join and perhaps in this case to make some alterations, you may do so and then send directly to the editor of Taxon (International Bureau for Plant Taxonomy, 106 Lange Nieuwstraat, Utrecht).

Best wishes,

*I should be glad!*



Feb 17 1959

Dear Rolf,

Many thanks for the picture-book of Finland that you sent me; the photos are excellent, and have been much admired by several of our friends who have seen it. I would like to send you a similar one depicting New Orleans, but there seems to be nothing of the sort available here.

I am truly sorry to hear about the recurrence of your heart ailment, and I can appreciate what it means to you, for I have something similar myself, though apparently not so severe as yours because I have not had the symptom of angina pectoris. My doctor calls it "cardiac insufficiency" with atherosclerosis, the latter being, I believe, almost identical with arteriosclerosis. Your difficulty in walking any considerable distance would be quite a handicap, for I suppose you are not allowed to use your bicycle. Perhaps you will have to get a small automobile and learn how to drive it. That should not be hard in a small town like Maris where there is not much traffic.

Your decision not to go to Montreal is no doubt a wise one under the circumstances, but it is a great disappointment to me, and will be, I am sure, to your other American friends. I think, now, that I shall change my mind about going there. The trip would cost some \$500 or \$600 for me and my wife, who has accompanied me on all our travels during our nearly 40 years of marriage, and there is no way for me to get a grant to help with the expenses. I should like to meet some of the European phycologists who will be there, Bourrally for instance, but he does not speak English and my French is not good enough to maintain much of a conversation, so we should have to converse through an interpreter like Brunel.

Your mention of Silva's proposal was the first I had heard of it. I wrote to Prescott asking his opinion and saying that I am strongly against it. He has not yet replied; in fact he is so OVERloaded with work that he simply is unable to answer his correspondents promptly. Also I wrote to Brunel in the same vein, asking that he vote against it, and also that he use his influence with other members of the Committee on Nomenclature of Algae. Tonight, after 6 pm (when the rates are cheaper) I shall try to telephone Prescott at East Lansing and talk to him about it, and will let you know when I finish this letter tomorrow what he says.

Yesterday and today came your two other letters, with the copy of your and Huizick's paper, and the photocopies of the other Czechoslovakian phycologists' paper on nomenclature of the cyanophytes. From this I got the reference to Taxon 7, 1958, and I immediately phoned Prof. Joseph Bean in the Botany Dept. of Tulane University, with whom I am fairly well acquainted, and who subscribes to Taxon. I made arrangements to go there tomorrow, to read Silva's proposal and get a better idea of what his arguments are. This professor is a skilled taxonomist in the Phanerogama, though he has some knowledge of the Cryptogama also, and I have given him a number of blueprints of my plate drawings, which he uses as examples of good drawing for his students. He told me that he has already received the preliminary ballots for voting on the proposals to the Congress, so perhaps I can persuade him to vote against Silva's.

Later, June 21.

I spoke on the telephone with Prescott, and found that he had written me a letter which arrived two days later. On the phone he told me that he agrees fully with your opinion and mine about Silva's proposal. In his letter he wrote:

"I hope that Silva's proposal will be squelched. I am certain that the majority of phycologists - at least the desmidologists - will veto the proposal. I, for one, am quite satisfied with leaving the starting point of desmid nomenclature with Reife: it should start some place - and where else could it start would be my first question. I do not think Silva is in a very good position to have a sound opinion on it anyway, with all due respect to him and his delving into ancient literature. I think I should follow up on your suggestion to block Silva's proposal. I'll look into it. Maybe I can convince him to retrack, although it is probably too late now".

In the meantime I had visited Tulane Univ. and borrowed from Prof. Ewan the copy of *Taxon* with Silva's proposal. Ewan told me that he had already received the preliminary balloting booklet with all the proposals submitted to the Congress, and on my recommendation he said he would vote against Silva. He could not tell me what system is followed in selecting the botanists to whom the ballots are sent, but it would appear that specialists in the Palmaceae, or Cactaceae, or any other -aceae can vote on the whole list of proposals, even though they probably have no knowledge whatever of the Algae or Fungi. That doesn't seem right to me.

After studying Silva's proposal it seemed to me (and I hope you will pardon me for saying so) that your rebuttal is neither long enough nor strong enough. So I have taken the liberty of writing a longer and stronger one, going into more detail, which may help to convince non-phycologists if they think of voting on this proposal. I have not sent it direct to *Taxon* because I wanted you to see it first. You are at perfect liberty to make any changes that you think desirable, or to destroy it and use your own, which I am returning with some corrections. I shall not be offended. But even if it is published in an early issue of *Taxon*, it may not reach the botanists in time to do much good, because all educational institutions, in this country at least, are closed during June, July and August, and the professors are either on their vacation or teaching a summer course at some other place. So I have in mind to obtain a list of members of the Committee on Algal Nomenclature and send to each of them a mimeographed copy of the article. Do you approve of this?

I want to get this letter in the mail tonight, so will close now and write more tomorrow.

Sincerely,

26.2.-59.

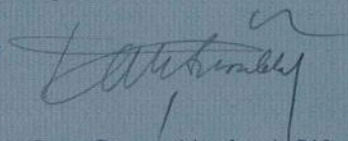
Dear Arthur,

If you have the "English Summary" corrected please send it kindly as soon as possible to me. The manuscript (typescript) of Grönblad & Ruzicka: "Zur Systematik der Desmidiaceen" has been sent to Sweden where it will be printed in "Botaniska Notiser". Only the English summary is lacking so far.

Kind regards

Today I got  
your letter.

Thank you very much!



I had a letter from Prescott about Silva's proposal concerning the "later starting point" in in the taxonomy of Desmids. He is against it.

28.2.-59 I have sent your version of the manuscr. to "Taxon" asking prof. Lanjouw to have it printed as soon as possible. Gblad & Ruzocka ms\_cr with with the English summary in your version is sent to Bot. Notiser. Thank you <sup>of</sup> again very much. When shall we do something with Sioli's desmids from Brazil???

Of course I think it very useful if you will send a copy of our article to the members of the Committee for Nomenclature of Algae.!

I believe I am the only member who has been working on Desmids I have already sent in my preliminary balloting paper.

March 2 1959

Dear Rolf,

I had intended to continue my letter of Jan 21 the following day, but each day since then something has occurred to prevent it.

The letter from Mme. Gauthier is really astonishing! It is almost incredible that M. incredibilis should turn up in three widely separated places in little more than one year. I can read her French easily enough, but her phraseology is such that I am not clear whether she found M. gudanica also; nor do I understand her references to M. crux-solitensis and St. clevei var. africanus. Are these also in her unpublished paper? Certainly St. clevei has no connection with B. furcatum; they have quite different structures.

As to Mme. Gauthier's new genus Allorveia, I don't quite know what to say. When I first saw M. incredibilis I thought it was well worthy of a new genus, because of its differing saccellae and because of the four processes of the "lower" saccellae radiating outwards and downwards from the isthmus, as well as other features. Yet Micrasterias is the only genus that it could be assigned to, if you don't make a new one, though there is no other Micrasterias of comparable structure.

On the general subject of the desirability of creating new genera I am also somewhat undecided. I recognize that Staurostridium could be placed in Staurostrum as Prescott thought; that Spinocosmarium could go in Xanthidium; etc. But I think that Spinoclosterium is definitely wrong, because in the preamble to the Code (1956, p.11) it says "The only proper reasons for changing a name are either a more profound knowledge of the facts resulting from adequate taxonomic study or the necessity of giving us a nomenclature that is contrary to the Rules". We don't know any more about this plant, except its distribution, than was known in 1848, though I have seen in one Indonesian specimen that the wall is extremely finely striate. So I intend to stick to Cosmarium cuspidatum.

Also I know that Amscottia and Ichthyodontum could have been assigned to Staurostrum and Ichthyocercus respectively, in fact I think I wrote that at the two discovery times. In the case of Amscottia personal vanity played a considerable part, of course. As you say, it is a matter of personal taste and opinion whether to make a new genus or not. I don't see any harm in making them; it is somewhat analogous to our decision not to add any more new varieties to such overcrowded and heterogeneous species as A. incus.

Prowse spent two days in New Orleans just before Christmas. He brought with him some small photocopies of his Malayan drawings, and we went through them rather hurriedly, and I identified some of them for him as we went along. On the second day we drove along the coast of the Gulf of Mexico to Biloxi, some 200 km from New Orleans. Both my wife and I were somewhat disappointed in him, because he showed so little interest in my laboratory, instruments, method of working, library, collections, etc., and in the sights that we showed him in this city and along the Gulf Coast. Then he went to East Lansing, Mich., and spent five days with Prescott, who also got a similar impression of him.

Sachlan is now in this country, where he will spend nine months, on a grant from the U.S. Government. He will be with Prescott for two weeks in April, and will come to New Orleans for four days early in June. Other places he will visit are Auburn College, for a course in hydrobiology and inland fisheries, and Dr. Starr

to learn about culture of algae. I guess that Uncle Sam will spend not less than \$5000 on him, a pretty large sum.

After considering the opinions of Prescott, Tailing and yourself on M. moebii and E. turgidum, I have decided to transfer the former back to Quastrum, but to make no change in the status of E. turgidum. However, in the paper I shall give a copy of Wallich's original illustration of E. turgidum and shall offer some remarks on this original form, and the forms which later authors have interpreted as belonging to this species, and shall try to show how turgidum may be distinguished from moebii.

Your drawings of desmids from the new Sudan material will be troublesome to identify, but I think I have seen a few of them in Miss Lind's Uganda material. I have been working on the latter for the last couple of months, off and on, and have nearly finished with them. So I would like to keep your drawings a little while longer, so as to compare them with mine, and then I will return them together with mine from Uganda.

Have just heard that a new short paper by Scott & Prescott has been published in "Reinwardtia", dealing with a number of new varieties of M. saltans and subsaltans. Hope to get the reprints in the next few weeks and will send a copy to you.

Sincerely,

March 11 1959

Dr. P. C. Silva,  
University of Illinois,  
Urbana, Ill.

Dear Dr. Silva,

For the last week or so I have been wondering where I could obtain the names and addresses of all the members of the Committee for Algae, and did not know just where to turn. Yesterday I received from Dr. R.C. Pollins, to whom I had applied for membership in the I.A.P.T., an acknowledgement and a copy of the January issue of Taxon containing a report of the Committee for Algae signed by you, from which I take it that you are the Secretary. Therefore I should be much obliged if you will give me the names and addresses of the other members.

This may strike you as a rather impudent request when I tell you the reason for it, which is, in fact, nothing less than to enable Grönblad and me to fight your proposal for the elimination of "later starting points for algae", insofar as it affects desmids. Our opposition, though quite serious, will be a friendly one, so I hope you will not take offence. Here's the story:

Some weeks ago Rolf Grönblad called my attention to your proposal, which I had not previously heard of. So I borrowed the copy of Taxon from Prof. Joseph Ewan of Tufts University and took it home to read. After studying it I was quite surprised, to put it mildly. The provision for later starting points has been in the Code for 25 years, maybe longer for all I know, and it simply did not occur to me that anyone could object to Balby 1848 as the starting point for desmids.

Grönblad was much perturbed, as I think he wrote you, and he asked me to join him in an article opposing your proposal, to be published in Taxon. Before agreeing I phoned Jerry Prescott in East Lansing to get his opinion, though I knew in advance pretty much what his answer would be. As I expected, and as he may have informed you by now, he is decidedly against it, and hopes it will not be accepted by the Montreal Congress. So I have joined Grönblad as co-author of the article, and have returned the Ms. that he sent me for correction of his English, and have added some comments of my own. It is now in Prof. Lanjouw's hands for publication.

But by the time it gets into print it may be too late to do much good. So I propose to have the article mimeographed and to mail it to the members of the Committee for Algae, and to a selected list of phycologists who are specially interested in desmids, from Grönblad's and my mailing lists. And that's the reason for my request. No hard feelings, I hope?

Sincerely,

W

ROLF GRONBLAD  
KARIS  
FINLAND

March, 14. 1959.

Dear Arthur,

Many thanks for your letters 1 and 2.

I see that you are of the same opinion as myself regarding the making of new genera.

I suppose the best thing is to do as you intend <sup>to do</sup> with *Eu. turgidum*. - *M. moebii*.

Has Miss Lind sent you some new material from Uganda? I had just thought of publishing my material, but perhaps it would be better to make one paper of all material from Uganda, or what do you wish? I am willing to do as you best like.

From Hans Luther I got some news concerning Krieger's desmid flora. There is a Dr. J. Gerloff (Botanischer Garten und Museum, Berlin - Dahlem, Königin-Luise-Str. 6-8) who is just finishing the *Cosmarium* part of the Flora. "Der Umfang der Publikation wird etwa 5 Hefte zu 180 Seiten betragen." Wich will make about 900 pages. Certainly it will be very useful though I do not know anything about this Dr Gerloff. (His name is to be found in the "Internat. Directory of Specialists in Plant Taxonomy. 1958 Utrecht.) I have written to him asking any news about its publication and he sent me a very nice letter in answer. There are some difficulties concerning the financial problems but Dr Gerloff thinks everything will be OK in a short time. If you have any communications with Prescott I am sure he would be very much interested in these news.

After having received our article against Silva's opinions Dr Stafleu wrote a very nice letter to me: "This is precisely the type of well-balanced and documented criticism we need." ... "You will understand that the chances that it /=Silva's proposal/ will be accepted as a whole are very small!"

I had a letter from Mr Prowse and he is rather enthusiastic about what he had seen in your home.

You did not tell me what you <sup>are</sup> intending to do with *Sioliá*' Brazil. desm.??? I have been told that they like him very little at Plön where he is arranging everything in a new way (This is from Hans Luther who is my "Fountain of Wisdom"!)

*Do not tell this slander to anyone!*

Best wishes,

*The Montreal Congress offered me \$ 100. - but I have told them that my health is not in traveling condition.*

*Why does Prescott publish so poor figures as his Brazilian *Algae* (1917)?*

*Digitized by the Hunt Institute for Botanical Documentation*

*I had a very nice and kind letter from Hammar. He and Prescott agree about *Chlo. N. S. S.**

UNIVERSITY OF ILLINOIS  
DEPARTMENT OF BOTANY  
URBANA, ILLINOIS

March 18, 1959

Dr. A. M. Scott  
2824 Dante Street  
New Orleans 18, Louisiana

Dear Dr. Scott:

I am happy to learn that some thought is being given the long neglected and misunderstood provision for a later starting point in desmid nomenclature. For this, and other reasons, I welcome your serious [and friendly] opposition to my recent proposal that later starting points in algae be eliminated.

The membership of the Committee for Algae, as established by the Eighth International Botanical Congress and as amended by a subsequent resignation, follows:

[the following are members of the Phycological Society of America; their addresses may be found in our membership list]

BOURRELLY  
BRAARUD  
DOTY  
FELDMANN  
GRONBLAD  
LUND, J.W.G.  
PAPENFUSS  
PATRICK  
SILVA (Secretary)  
TAYLOR

also: Dr. Søren LUND  
Danmarks Fiskeri- og Havundersøgelser  
Charlottenlund Slot  
Charlottenlund, Denmark

Mr. Robert ROSS  
Department of Botany  
British Museum (Natural History)  
Cromwell Road, Kensington  
London SW 7 England

Professor Dr. Y. Yamada  
Department of Botany  
University of Hokkaido  
Sapporo, Japan

Now, may I ask a reciprocal favor? I should greatly appreciate receiving a copy of the manuscript which will be published in TAXON in order that I may decide whether a reply will be in order. Thank you for this courtesy.

It would seem that opposition to the maintenance of Ralfs' The British Desmidiaceae (1848) as the starting point for desmid nomenclature falls into the same category as opposition to motherhood, matrimony, or some other sacred institution! As a matter of fact, the best case (in my opinion) for not using Ralfs as a later starting point was made by Nordstedt himself in his original proposal [Bot. Not. 1906], where he repeatedly pointed out the excellence of the work. Indeed, it is so excellent that to use it as a later starting point is to repudiate the very principle that Ralfs considered foremost and which guided him to the adoption of such a sound and fair system of nomenclature, namely, priority. I should like to call your attention to the third and fourth paragraphs of Ralfs' Preface [p. vi], where he stresses priority, and particularly to this part: "...for it is surely unjust that the credit due to the discoverer or first describer of a plant should be ascribed to one whose sole merit in regard to it has been to transfer it from one genus to another." Can you imagine how disconcerted Ralfs would be if he knew that solely by virtue of the fact that he had treated a particular species in his admirable monograph, this species should be eternally linked to his name rather than to the name of the describer? Ralfs' monograph was a milestone in taxonomy, not merely desmid taxonomy. In its thoroughness, in its superb documentation, in the attempts to ferret out existing names and to determine their application, in the consistent adherence to priority and the resulting logical nomenclature...in these and other features The British Desmidiaceae stands practically alone in its day; and the remarkable and regrettable fact is that Ralfs' standard of performance has seldom been matched in the algae during the succeeding century! By using The British Desmidiaceae as the starting point, later workers shirk their responsibility of maintaining Ralfs' standard.

There seems to be a vague fear that eliminating starting points will result in wholesale name-changing. Restricting the discussion to desmids, I can assure you this is not the case, the reason being that Ralfs was so thorough in incorporating the work of his predecessors and in following priority. Certain species of Corda, Ehrenberg, and Morren were not considered because Ralfs did not have access to the publications. Many of these entities are not capable of typification and must remain forever on the shelf. Two works published in 1847 were not seen by Ralfs: Focke's Physiologische Studien, Heft 1; and Kützinger's Tabulae Phycologicae, Band I (in part). It is particularly revealing to note that Closterium libellula Focke 1847 is used by contemporary desmidologists in preference to Closterium closterioides (Ralfs) comb. nov. [I don't believe this combination has been made previously], which is the correct name if you take 1848 as the starting point for desmid nomenclature [which desmidologists in practice do not]. Only one species was published in the 1847 Kützinger work, Entospira closteridia, sometimes identified with Spirotaenia. In summary, Ralfs overlooked or purposely omitted about 26 species, almost all of which are incapable of typification. Two species, which have been identified with some degree of certainty, are currently accepted, contrary to Ralfs' starting point: Closterium libellula Focke 1847 and Cosmarium dentiferum Corda 1840. It is doubtful whether more than one of these pre-1848 species will be found to displace a species recognized by Ralfs. In regard to later homonyms among specific names, there are about 22 pre-1848 binomials which are earlier homonyms of post-1848 binomials; many of these would require the proposal of substitute names. However, there are just as many later homonyms among post-1848 names, and eventually I will submit a list to the desmidologists for their consideration and action. Moreover, the fact that substitute names usually have not been proposed in

cases where a pre-1848 binomial is an earlier homonym of a post-1848 binomial is not due to any realization by desmidologists that because of the later starting point the later homonym is still legitimate, but rather it is due to lack of concern, especially because most of the earlier homonyms are synonyms rather than accepted names in current treatments. However, in at least two of these instances later homonyms have been renamed, and the substitute names [superfluous and illegitimate] are in current usage, contrary to the use of Ralfs as a later starting point!

On the level of the gems, the names used by Ralfs were previously safeguarded by conservation [four names]. In view of the 1848 starting point, these four conservations are unnecessary [I removed them from the Paris Code], but the fact that they were legislated again shows the lack of understanding of the meaning and application of the later starting point. Should desmid nomenclature revert to 1753, these four conservations would again be in order and could be reinstated.

In addition to the presence of unnecessary conservations in the Code and the current recognition of such pre-1848 species as Closterium libellula, there is abundant evidence that desmidologists do not use Ralfs as their starting point. The genera are usually attributed to their historically correct (rather than to their artificial) author, e.g., Xanthidium Ehrenberg 1837, Arthrodesmus Ehrenberg 1838, Staurastrum Meyen 1829, Micrasterias Agardh [I am now looking at a paper by Prescott]; species are usually in a similar manner attributed to their historically correct authors [e.g., Euastrum didelta (Turp.) Ralfs and E. elegans (Bréb.) Kuetz.]. Such citations are not only in violation of the Code, but more important they admit that Ralfs is not the starting point of desmid nomenclature.

Ralfs closely followed the type method. According to this principle, a name must be attached to a type, which in the case of a species or taxon of lower rank must be a specimen; the application of a name can only be determined with certainty [but still subject to differences of taxonomic opinion] by studying the type. The practice followed by many workers of basing their judgment solely on descriptions and illustrations is unsound, unless photomicrographs of the type are available. If a name cannot be typified [that is, if no original specimen can be located and the description and illustrations are not sufficiently detailed or accurate to permit a taxonomic judgment as to the identity of the taxon] it must be relegated to obscurity, not to be used unless and until someone typifies it [that is, type specimens are discovered] or is foolhardy enough to base a judgment solely on a [fragmentary] description and [sketchy] figures. The great beauty of the Code, shorn of unnecessary and unjustifiable complications such as later starting points, is that it provides a precise system of nomenclature to express any precise system of taxonomy that may be proposed. Like a tree in the wind, nomenclature is adaptable to changing taxonomic concepts.

You may ask then why I am anxious to have later starting points eliminated if, from the discussion preceding, there would seem to be little difference whether 1753 or 1848 were taken as the beginning date for desmid nomenclature. I should like to refer you again to my proposal in TAXON, where I point out some serious problems that would come to the forefront if desmidologists were to follow the type method.

With good wishes,

Sincerely,

  
P. C. Silva

UNIVERSITY OF ILLINOIS  
DEPARTMENT OF BOTANY  
URBANA, ILLINOIS

March 18, 1959

Dr. A. M. Scott  
282½ Dante Street  
New Orleans 18, Louisiana

Dear Dr. Scott:

I am happy to learn that some thought is being given the long neglected and misunderstood provision for a later starting point in desmid nomenclature. For this, and other reasons, I welcome your serious [and friendly] opposition to my recent proposal that later starting points in algae be eliminated.

The membership of the Committee for Algae, as established by the Eighth International Botanical Congress and as amended by a subsequent resignation, follows:

[the following are members of the Phycological Society of America; their addresses may be found in our membership list]

BOURRELLY  
BRAARUD  
DOTY  
FELDMANN  
GRÖNBLAD  
LUND, J.W.G.  
PAPENFUSS  
PATRICK  
SILVA (Secretary)  
TAYLOR

also: Dr. Søren LUND  
Danmarks Fiskeri- og Havundersøgelser  
Charlottenlund Slot  
Charlottenlund, Denmark

Mr. Robert ROSS  
Department of Botany  
British Museum (Natural History)  
Cromwell Road, Kensington  
London SW 7 England

Professor Dr. Y. Yamada  
Department of Botany  
University of Hokkaido  
Sapporo, Japan

Now, may I ask a reciprocal favor? I should greatly appreciate receiving a copy of the manuscript which will be published in TAXON in order that I may decide whether a reply will be in order. Thank you for this courtesy.

It would seem that opposition to the maintenance of Ralfs' The British Desmidiaceae (1848) as the starting point for desmid nomenclature falls into the same category as opposition to motherhood, matrimony, or some other sacred institution! As a matter of fact, the best case (in my opinion) for not using Ralfs as a later starting point was made by Nordstedt himself in his original proposal [Bot. Not. 1906], where he repeatedly pointed out the excellence of the work. Indeed, it is so excellent that to use it as a later starting point is to repudiate the very principle that Ralfs considered foremost and which guided him to the adoption of such a sound and fair system of nomenclature, namely, priority. I should like to call your attention to the third and fourth paragraphs of Ralfs' Preface [p. vi], where he stresses priority, and particularly to this part: "...for it is surely unjust that the credit due to the discoverer or first describer of a plant should be ascribed to one whose sole merit in regard to it has been to transfer it from one genus to another." Can you imagine how disconcerted Ralfs would be if he knew that solely by virtue of the fact that he had treated a particular species in his admirable monograph, this species should be eternally linked to his name rather than to the name of the describer? Ralfs' monograph was a milestone in taxonomy, not merely desmid taxonomy. In its thoroughness, in its superb documentation, in the attempts to ferret out existing names and to determine their application, in the consistent adherence to priority and the resulting logical nomenclature...in these and other features The British Desmidiaceae stands practically alone in its day; and the remarkable and regrettable fact is that Ralfs' standard of performance has seldom been matched in the algae during the succeeding century! By using The British Desmidiaceae as the starting point, later workers shirk their responsibility of maintaining Ralfs' standard.

There seems to be a vague fear that eliminating starting points will result in wholesale name-changing. Restricting the discussion to desmids, I can assure you this is not the case, the reason being that Ralfs was so thorough in incorporating the work of his predecessors and in following priority. Certain species of Corda, Ehrenberg, and Morren were not considered because Ralfs did not have access to the publications. Many of these entities are not capable of typification and must remain forever on the shelf. Two works published in 1847 were not seen by Ralfs: Focke's Physiologische Studien, Heft 1; and Kützing's Tabulae Phycologicae, Band I (in part). It is particularly revealing to note that Closterium libellula Focke 1847 is used by contemporary desmidologists in preference to Closterium closterioides (Ralfs) comb. nov. [I don't believe this combination has been made previously], which is the correct name if you take 1848 as the starting point for desmid nomenclature [which desmidologists in practice do not]. Only one species was published in the 1847 Kützing work, Entospira closteridia, sometimes identified with Spirotaenia. In summary, Ralfs overlooked or purposely omitted about 26 species, almost all of which are incapable of typification. Two species, which have been identified with some degree of certainty, are currently accepted, contrary to Ralfs' starting point: Closterium libellula Focke 1847 and Cosmarium dentiferum Corda 1840. It is doubtful whether more than one of these pre-1848 species will be found to displace a species recognized by Ralfs. In regard to later homonyms among specific names, there are about 22 pre-1848 binomials which are earlier homonyms of post-1848 binomials; many of these would require the proposal of substitute names. However, there are just as many later homonyms among post-1848 names, and eventually I will submit a list to the desmidologists for their consideration and action. Moreover, the fact that substitute names usually have not been proposed in

cases where a pre-1848 binomial is an earlier homonym of a post-1848 binomial is not due to any realization by desmidologists that because of the later starting point the later homonym is still legitimate, but rather it is due to lack of concern, especially because most of the earlier homonyms are synonyms rather than accepted names in current treatments. However, in at least two of these instances later homonyms have been renamed, and the substitute names [superfluous and illegitimate] are in current usage, contrary to the use of Ralfs as a later starting point!

On the level of the genus, the names used by Ralfs were previously safeguarded by conservation [four names]. In view of the 1848 starting point, these four conservations are unnecessary [I removed them from the Paris Code], but the fact that they were legislated again shows the lack of understanding of the meaning and application of the later starting point. Should desmid nomenclature revert to 1753, these four conservations would again be in order and could be reinstated.

In addition to the presence of unnecessary conservations in the Code and the current recognition of such pre-1848 species as Closterium libellula, there is abundant evidence that desmidologists do not use Ralfs as their starting point. The genera are usually attributed to their historically correct (rather than to their artificial) author, e.g., Xanthidium Ehrenberg 1837, Arthrodesmus Ehrenberg 1838, Staurastrum Meyen 1829, Micrasterias Agardh [I am now looking at a paper by Prescott]; species are usually in a similar manner attributed to their historically correct authors [e.g., Euastrum didelta (Turp.) Ralfs and E. elegans (Bréb.) Kuetz.]. Such citations are not only in violation of the Code, but more important they admit that Ralfs is not the starting point of desmid nomenclature.

Ralfs closely followed the type method. According to this principle, a name must be attached to a type, which in the case of a species or taxon of lower rank must be a specimen; the application of a name can only be determined with certainty [but still subject to differences of taxonomic opinion] by studying the type. The practice followed by many workers of basing their judgment solely on descriptions and illustrations is unsound, unless photomicrographs of the type are available. If a name cannot be typified [that is, if no original specimen can be located and the description and illustrations are not sufficiently detailed or accurate to permit a taxonomic judgment as to the identity of the taxon] it must be relegated to obscurity, not to be used unless and until someone typifies it [that is, type specimens are discovered] or is foolhardy enough to base a judgment solely on a [fragmentary] description and [sketchy] figures. The great beauty of the Code, shorn of unnecessary and unjustifiable complications such as later starting points, is that it provides a precise system of nomenclature to express any precise system of taxonomy that may be proposed. Like a tree in the wind, nomenclature is adaptable to changing taxonomic concepts.

You may ask then why I am anxious to have later starting points eliminated if, from the discussion preceding, there would seem to be little difference whether 1753 or 1848 were taken as the beginning date for desmid nomenclature. I should like to refer you again to my proposal in TAXON, where I point out some serious problems that would come to the forefront if desmidologists were to follow the type method.

With good wishes,

Sincerely,

  
P. C. Silva

March 21 1959

Dear Rolf,

I have your letters of Feb 28th and Mch 14; also the package with my Sudan drawings came today. Many thanks for all of them.

I have not had any communication with Miss Lind; the Uganda material that I have been working on is what you sent me several years ago. My examination is now finished; though it might be possible to discover a few more forms by spending more time, I hardly think it worth while. Some of the desmids I have identified as I went along, but others will require considerable search through the literature. Anyway I will send all of them to you as soon as I can, and will return your Sudan sketches with them.

You told me some time ago that Dr. Gerloff would undertake the completion of Krieger's monograph. Dr. Krieger wrote me a few months before his death that he had completed his work on *Cosmarium*, and I suppose that Gerloff has been putting these notes and drawings in shape for publication. 900 pages seems quite a lot for *Cosmarium* even though it is the largest of the desmid genera. Do you know if he will continue with the other genera? I wonder if he has or will include any of the new *Cosmaria* that have been published since Krieger's death; there are quite a number of them in papers by you and me, Prescott, Hannah, Bourrelly, Behre, etc. I will send him copies of our USA paper, and the one from Arnhem Land; perhaps you can send him the Sudan paper.

It is quite curious that you should tell me about Gerloff's work just now, because only a few days ago I received pretty much the same information from Franz Eckert, of Ingolstadt, W. Germany. I don't remember if I have mentioned him to you before. I know little about him, except that he makes marvellous Dauerpräparaten of desmids, mounted in Caedax or Myrax, with 10 or 12 different desmids mounted precisely in parallel rows on the slide, just like type-slides of diatoms. In some cases he has stained the cell-wall yellow, the chloroplast in green, and the pyrenoids in blue; and on another slide he has arranged 25 desmids in a radiating circular pattern. Really wonderful work, which I should not have thought possible with such fragile organisms as desmids. I get the impression that he makes the slides for sale.

I am sending you Eckert's letter, which is quite interesting, but ask that you return it to me quickly, for I have not yet answered it. You will note he suggests that Kurt Förster might be able to finish Krieger's monograph, and asks my opinion. I know very little about Förster, who has written me only twice in four or five years, and then only when he wanted some of my papers. He seems always to be in poor health. He has been industriously collecting and copying illustrations of desmids for his "Monographie", but I have no idea what this may be. It seems to me very unlikely that he would be competent to complete Krieger's work, for it is necessary to have actual first-hand knowledge of thousands of desmid species from all over the world, in addition to looking at pictures; and also the resources of a very large library of desmid literature.

Eckert's reference to you came about as follows: In his last letter he told me that from a Dr. Pochmann (of whom I know nothing) he had some material collected in the State of Gojaz (Goiás) Brazil, by Lützelburg, who, according to Krieger (1950) died in den Wirren der Nachkriegszeit. Eckert sent me a slide with some of these desmids in glycerine, from which I made the 13 sketches that he refers to. Several of them are new taxa, including a new species of *Xanthidium*, and two very remarkable new species of *Euastrum*. I am enclosing copies of these sketches.

In my last letter to Eckert I suggested that if Dr. Pochmann had not made any other arrangements, he might send this Brazilian stuff to you for study; but as you will see from Eckert's present letter, Pochmann cannot make up his mind one way or the other. The Lützelburg material that Krieger worked up was collected in 1917! I don't know if Pochmann's stuff is part of this same lot, or if it was collected at a later date, but it cannot be less than about 20 years old, so it's about time he decided to do something with it.

In return for the beautiful slides that Eckert sent me, I sent him 24 samples from my best collections, with the result that he seems almost ready to burst with joy! This, however, is easily understandable, for even Krieger told me that some of my Florida collections were among the richest he had ever seen among the 8000 vials that he possessed, and for rare and strange and beautiful forms even these are surpassed by the Australian and Indonesian material. I shall tell Eckert that you also will be happy to receive some of his Typenplatten.

I have not written to Sioli since he returned to Germany; in fact I am ashamed of having neglected his material for so long. I have made about 200 drawings, but these represent only the new and rare and unusual species; there are many more of the common ones still to be drawn and/or listed. This will be my next project, but first I must get rid of the big Indonesian paper. The rough draft of this is finished, but it now has to be retyped in final form, and this is a long and difficult job that I think could hardly be entrusted to anyone else.

Glad to hear that Dr. Stafleu liked our reply to Silva's proposal, and that he thinks there is little chance of it being accepted. I have obtained a list of the members of the Committee for Algae, and find that there are three others besides you who have worked on desmids, Bourrelly, Skuja and Taylor, though it is 25 years since Taylor's Newfoundland paper. I will send mimeographed copies of the paper together with a personal note to those whom I know, and will also send you some copies for distribution.

Prescott's poor illustrations in his Brazil paper are as much of a mystery to me as to you. I saw the plates in preparation when I was in East Lansing in 1957, and was sorry to see the asymmetrical figures, the irregular arrangement of ornamentation, etc. He must surely be able to see this for himself, for as I have told you before, he is a really skillful artist. The only excuse I can think of is that he is so terribly overworked that he simply cannot take the necessary time to make good drawings.

Do you know if Kossinskaja has published any more parts of her Russian "desmid-flora"? I have the first two parts, up to and including Tetramorus, but these are dated 1951 and 1952. It would seem that more of them must have appeared by now.

Last year the head of the Dept. of Ecology at Tulane University (New Orleans) visited the USSR as a representative of the Am. Inst. Biol. Sciences, in an effort to effect a rapprochement between American and Soviet biologists. A summary of his report was recently printed in the A.I.B.S. Bulletin. He says that the Russians showed "consternation" at American lack of knowledge of their biological institutions, and that individuals were "irritated" that American biologists were unacquainted with their work. One Russian remarked that it was difficult for them to write in English so he proposed that they should write in Russian language to Americans, and that the latter could reply in English. In another article in the same issue of the Bulletin deals with the knowledge of foreign languages possessed by American scientists, and it shows that while a great many of them know French, German and Spanish, only about 2% of 127,000 American scientists have "some" knowledge of Russian, and that among this 2% it was largely because either because the individuals or their parents were born in Russia!

Best regards from

The general conception of species and infraspecific taxa in the Desmidiaceae is at present very obscure. In most cases the extent of variability of a given taxon is unknown. Occasionally, however, evidence is obtained showing that the variability is essentially greater than generally has been thought possible. Thus it is always necessary to inquire into the total variability, even when we have to do with a seemingly narrowly limited form. So it is necessary in doubtful cases to allow a somewhat broader conception of the taxon, especially of the "species". This must not, however, be exaggerated in absurdum so that we get what are called "Sammelarten", large heterogeneous species which include elements with little relation to each other. Such species as Staurastrum polymorphum, St. gracile, and Arthrodesmus incus with its varieties, are very awkward in practical work, giving rise to such trouble. At present, when the limits of the taxa are not defined exactly, smaller modifications or variations may be annotated by the use of the special terms proposed below.

In order to avoid a continually increasing number of names and synonyms, and thus to avoid an obscurity of the whole system, a proposal is made as to certain limitations in the International Code of Botanical Nomenclature. First and foremost a strict separation of taxonomic and non-taxonomic variations should be kept. The conception of a taxon should be limited in such a way that only constant hereditary variations should be considered taxonomic units. Such a separation is not indicated or suggested anywhere in the Code.

As to the taxonomic groups, only those mentioned in Art. 4 of the Code should be used. In the Algae, especially Desmids, the use of only the following terms is recommended: species, varietas, forma, and only in exceptional cases subspecies. It is evident that forma is to be treated as a taxonomic term and not to be applied (as in older times often was the practice) to non-taxonomic groups. Theoretically there is no reason against making every slight variation a taxonomic category and thus giving to it a taxonomic epithet. But in order to keep a somewhat clear summary of the system it must in practice be limited to the most essential cases, i. e., when there is in question a sufficiently important variation. The judgement of the importance of the variations must, however, depend on the skill and experience of the author.

The hereditarily not stabilized and thus inconstant, temporary, and retrogressive variations are non-taxonomic. Such products of variations must not be designated by means of taxonomic terms (e.g., forma). The present authors recommend for use in such cases where it seems necessary to have a special "name" the term "modification" (modificatio), while the Czechoslovakian phycologists propose such terms as "morphe", "ekomorphe", "status". A special term, "facies", is, according to Telling, to be used for the variation in radiation of desmids, this also being non-taxonomic.

A non-taxonomic term, with a description of the variation in question, may be joined with the epithet of the species, or of the particular infraspecific rank of the species in which it occurs. The differing taxon should not be ~~fix~~ given an epithet in the nomenclat<sup>ural</sup> sense, and no author's name is to follow. The description is not to be understood as a diagnosis. If a Latin or Greek adjective is used its gender should not <sup>necessarily</sup> agree with that of the genus, but with that of the words "modificatio", "morphe", "facies", etc., these three being feminine.

The question as to the use of the term "facies" seems sometimes to be more obscure. There are "facies" which certainly are non-taxonomic and non-hereditary, (such as fac. 2 and fac. 3 found in the same filament of Sphaerosoma granulatum, communicated by Hůdicka in manuscript.). Also among the Staurastrum it is not unusual to find individuals in which the two semicells are not of the same degree of radiation, and belong to different facies. On the other hand there seem to be a number of constant "facies" which then should be marked as taxonomic categories, varieties.

In the present state of our knowledge it is sometimes very difficult, - sometimes, indeed, impossible - to know whether a taxonomic or non-taxonomic character is involved. There must remain a degree of uncertainty as to the real nature of such variations. In such cases ~~max~~ we are compelled to rely upon mere guesses and suppositions. This is, however, no reason why we should not try to use the nomenclat<sup>ural</sup> terms logically and build up a system that corresponds with our current knowledge. Otherwise there is imminent danger that the whole system will become monstrously swollen and quite confused.

Dear Arthur,

Enclosed I am returning the letter by Franz Eckert to you. It was most interesting. Perhaps you remember that I showed you some slides made by Homfeld where desmids were mounted "a fronte", "a latere" and "a vertice" under the same cover glass. The Germans seem to be very much interested in this sort of hobby, - I can scarcely call it scientific work. It is very curious that there have appeared at once and simultaneously three such algological hobbyworkers: F. Eckert, Förster and ~~Feckmann~~ Gerloff and not one of them has produced any scientific papers! Also Krieger was almost a "beginner" when he undertook the Rabenhorst's Flora. They have a phantastic ability to work I would rather say "to produce work", to write extensively. Perhaps you know Migula's <sup>his</sup> "Süsswasserflora" which is quite worthless but has been very much used. (Of course also other nations have produced poor work: Cooke, Hassall in England, Comère in France, Wolle in USA).

As to the continuation of Krieger's Flora I think Gerloff must finish it. I do not know if there is anyone better desmidiologist in Germany I know nothing about him but apparently he must have some skill with desmids. Of Förster I do not know anything either. I have sent him my separata but the last sending was returned! He has never written an answer to my letter nor has he said a word of thankfulness for all my separates. So I do not know if he has got them at all. If you write to him you may perhaps be willing to tell him this. (There were one man in USA and one in France who reported their interest in desmids, but never did they produce a scientific paper; Bullard and de Brébisson / grandson to the old man)

I do not know about Gerloff's further intentions: perhaps he is going to write the rest too?

Of course I am much interested to get some of Eckard's mounted slides, please tell him so.

I do not know about Kossinskaja's Russian desmid-flora. But I can try to write to her and ask.

Silva has sent me a copy of his last letter to you. I cannot quite see the force of his arguments. What I do see is that the Code is a Holy Book to him and the change of specific names is of no consequence. I cannot quite follow his arguments.

What you tell me about Russian biologists is quite characteristic for them. They are again following Mitchurin and Lysenko, and that is no science but pure humbug.

Kind regards,

April 4 1959

Dear Rolf,

Yesterday I sent you 55 copies (half of the total that I ordered) of the mimeographed article to be published in *Taxon*, for you to distribute to the same list of names to whom you sent the reprints of our USA paper.

However, something has developed that may make it advisable to postpone the mailing of these copies, perhaps even not to send them at all, or even to stop publication of the article in *Taxon* if it is not too late. I wrote Silva to obtain the names and addresses of members of the Committee for Algae, because I did not know where else to get them, and of course I told him the purpose for which I wanted them. In reply he wrote a long letter, which, together with the article by M.A. Donk, places the subject of later starting points in quite a different light, as you will see from the enclosed copy of my answer to him. I will send a photostat of his letter as soon as I can get them, within the next couple of days, and in the meantime you might read Donk's article.

I have certainly been under a misapprehension about Ralfs 1848 as the starting point, and I think you and Prescott and other desmidiologists have also. I thought it meant that any name used and/or recognized and/or originated by Ralfs in his 1848 book were automatically "correct", and that names published prior to 1848 which he did not recognize or which he cited as synonyms were automatically "incorrect".

According to the 1956 Code "Valid publication of names for plants of the different groups is treated as beginning at the following dates". According to Silva this means that pre-1848 names were not validly published, therefore they have no status under the Code, and in effect do not exist. Thus they would be in the same position as pre-Linnean names, of which my friend Prof. Ewan has given me an example: Oxys luteus was published by Johannes Bauhain in 1651, 102 years before the Linnean starting date; it is now known as Oxalis luteus Linnaeus 1753. The original author's name has been dropped, he gets no credit, and it is difficult to ascertain when and by whom the name was originated. There seem to be hundreds of other examples like this.

According to the Code the same thing would happen to pre-1848 names of desmids. Thus the genus Xanthidium Ehrenberg 1838 would become Xanthidium Ralfs 1848. Silva has cited the case of C. libellula Focke 1847 and has proposed a new combination. Conn. dentiferum Corda 1840 presumably would become C. dentiferum Nordst. 1886. There seems to be a process, about which I am not clear, by which these "devalidated" names can be "revalidated". It involves "typification", and Silva would like to insist upon an actual preserved specimen as the type, though he must know that this is physically impossible in the vast majority of cases.

All this looks like a horrible mess to me, and it is certainly not what desmidiologists want. Silva's proposal for the abolition of later starting points will not solve the problem satisfactorily, even if he does submit a list of names to be conserved.

In reading Donk's article one sentence caught my attention, and I recommend it to yours. It starts at the bottom of p. 249 (*Taxon* VI(9) December 1957), and reads: "In my opinion a radical change is needed, if one does not want, in the long run, to alienate mycologists and phycologists from the Code....".

Somewhere in the Synopsis of Proposals 1959 I saw a note by Lanjeu that the Bacteria have a Code of their own (and incidentally I see that the nomenclature of Bacteria starts on "1 May 1753" though their nomenclature was not systematized until somewhere around 1830). I have no idea what this Code for Bacteria looks like.

What about a separate Code for desmids??? Their taxonomy, systematics and nomenclature have little or nothing in common with those of the Phanerogams, nor with the other groups of Cryptogams, not even the Zygnemataceae, the only other family of Zygnematales. Such a Code could be written in a few pages, in reasonably simple language, free from much of the gobbledegook that clutters up the big Code. In case you don't know this word, "gobbledegook" is a slang word invented to describe the kind of language used in Government documents and orders, where 100 words are used to describe something that could be adequately covered in 15 or 20, and which merely succeeds in confusing the reader.

I am simply too old to be able to absorb all of the old jargon that is now in the Code and the new (and worse) things that are being proposed. In only three pages of the Synopsis of Proposals I find the following: holotype, lectotype, neotype, isotype, syntype, merotype, descriptotype, synonymotype, interpretotype; and in Donk's article the frightening term "monadelphous homonym"!!!

My suggestion about a separate Code for desmids is made to only a small extent in jest, to a much larger extent seriously. If you are at all inclined to consider such an action we can explore it further. Unfortunately neither of us can talk personally with other desmidiologists, but must depend on writing, which is much less satisfactory. You might discuss it with Hans Luther when you see him again. Nothing could be accomplished at the Montreal Congress, of course, and by the time the next one comes around I shall either be pushing up daisies or out of the desmid business.

Best regards from your friend

April 4 1959

Dr. F. C. Silva,  
Urbana, Ill.

Dear Dr. Silva,

Many thanks for giving me the names of the Committee members, and for your long and very interesting letter. Sorry to have taken so long in replying, but it seems that every day something happens to prevent me from doing what I had intended. However, the delay has been beneficial in that it has given me time to think about the subject, and I have done a lot of thinking.

From Prof. Bean at Tulane I borrowed the copy of Taxon containing W. L. Bond's article (Taxon VI(9) 245-256). I exchanged a few letters with Dr. Bond just before he left Bogor, when he published in Reinwardtia the first of the series of short papers by Scott & Prescott on Indonesian desmids. His article gives a lot of information about the actions of the various Congresses on later starting points that was quite new to me.

After putting together this information with that in your proposal #58 and that contained in your letter, and reading all of them many times, and devoting many hours of thought to them, it has finally penetrated into my consciousness that your conception of Halls 1848 as the starting point for desmid nomenclature and the ideas held by most desmidologists are vastly different.

If I understand the situation correctly, it appears that desmidologists who have intended to use Halls 1848, and have thought they were doing so, and in some cases have stated so in print, have actually been following Nordstedt's recommendations, and not complying with the later starting point as defined by the Code. Also the rules of the game have been changed several times, and I get the impression that the effect of the present 1956 Code differs radically from what the 1910 Congress intended.

The picture is so completely different from my former concept that I do not feel competent to discuss it any more at present. Accordingly I have taken the liberty (to which I think you will not object) of sending photostats of your letter and carbon copies of this one, to Prescott and Grönblad, and have requested that they communicate directly with you, with a copy to me.

Enclosed is a mimeographed copy of the article by Grönblad & Scott as you requested. In view of the above developments I have not distributed them, and have only sent a copy to Prescott. I have suggested to Grönblad that the copies that I sent him for mailing should be held up until further.

Sincerely yours,

CC to Prescott and Grönblad.

Dear Dr. Silva,

Thanks for giving me the names of the Committee members, and for your long and interesting letter. Sorry to have taken so long in replying, but it seems that every day something happens to prevent me from doing what I had intended.

Enclosed is a mimeographed copy of the article by Grönblad & Scott that will be published in Taxon. I am mailing about half of them, and have sent the other half to Grönblad so that he can mail them to the European phycologists, with whom he is better acquainted than I am. Also I shall send him a photostat of your letter and a carbon copy of this one. He sent me a small photocopy of the article by the Czechoslovakian botanists but asked me to return it immediately, which I did, so I cannot tell you anything about it, except that they are strongly opposed to your proposal as it affects the blue-greens.

Your reference to "opposition to such sacred institutions as matrimony and motherhood" suggests that you may have heard from other desmidiologists besides Grönblad and me! I don't think that Ralfs 1848 is quite that holy, but I do hold it in just as much esteem, and with just as much right, and for just the same reason that general botanists esteem Linnaeus 1753. What Linnaeus did for the phanerogams on a large scale is just about the same as what Ralfs did on a smaller scale for desmids. And the magic date "1 May 1753" is itself nothing but another arbitrary later starting point that involved I don't know how many violations of priority, maybe hundreds. In the Code I count about eleven hundred conserved generic names, with many more proposed, another large-scale violation. If the Code and botanists can tolerate such wholesale violations of the sanctified principle of priority, then the few that are involved in Ralfs 1848 as a starting point cut such an insignificant figure as hardly to be worth mentioning.

It is curious what different conclusions can be reached by different persons from one set of facts. I am quite familiar with Ralfs' book; have read and reread his preface and introduction many times. By some process of reasoning that is not clear to me you conclude that the very excellence of his work is the best reason for not adopting it as a starting point. Prescott, Grönblad and I (and I am sure many other desmidiologists) arrive at exactly the opposite conclusion: that its excellence is the very reason why it should be, and has been officially, recognized as the correct, and in fact the only possible starting point.

This provision has been in the Code apparently since 1910, almost half a century, and I, for one, have thought it was just as firmly fixed there as 1 May 1753. There has been no complaint about it to my knowledge; if you know of any I should be glad to hear about it. Ralfs 1848 as the starting point has been recognized, implicitly or explicitly, by many and probably most of the authors of monographs on desmids and of general phycological treatises. It is implicit in West & West's monograph, of which the first volume was published in 1904; explicit in Krieger (1937, p.114); implicit in Hirano's Fl. Desm. Jap.; I am not sure about Kossinskaja's Russian Desm. Flora, because I cannot read Russian; it is not mentioned in her introduction where it would have been printed in Roman characters instead of Cyrillic, but she seems to follow Krieger in general. The Czech desmid flora, now contemplated or perhaps in preparation under the direction of Ruzická, will use Ralfs 1848, as will also the North American desmid flora now being worked on by Prescott, in which he has invited Hannah Crossdale, Bill Wade and me to participate. General works on algae are of little value to specialists, so the only ones that I have are Smith's FW Alg. U.S., which recognizes Ralfs 1848, and Fritsch's Struc. & Repr. Alg., which is not a taxonomic work as he states in his preface.

You have a legalistic point in the instance of Cl. libellula Focke 1847, but I think you know yourself that it is not a very good one. If you were to publish Cl. closteroides (Ralfs) Silva comb. nov. it would be perfectly correct, legitimate, valid, and all the rest, but it would find few or no takers among desmidiologists, and you would succeed only in causing old man Ralfs to turn over in his grave.

I don't know why you mention Cosm. dentiferum Corda 1840. The nearest that I can find in Ralfs are C. margaritifera and C. Broomei, which are similar but different. I do not have Corda's work, nor do I know the plant itself. The only illustrations I can find are in Nordstedt 1888 and W & W Mon. Vol.3. Neither of these authors mentions Ralfs.

If you do not wish to stand as author for the article  
I am writing to take the whole responsibility on myself.

Dear Arthur,  
Please do not stop the  
article for Taxon. I believe  
it is good, even though the  
question is a very complicated one.  
Silva has sent me a copy of his  
letter to you. When I read those  
explanations by Silva and Co, it  
makes my head ache and I  
do not understand what I read,  
But I think this making of testimony  
to a separate and "deep" science  
is an altogether bad matter.

Best regards  
Hill Country

12. IV. - 59

I have written this with haste & I do not know whether it is correct or not.

You can be sure that if there were a special code for plants  
I should have written some. Silva to make whatever of it!

Many thanks for Scientif. America

Jan 8 Feb 1

ROLF GRONBLAD  
KARIS  
FINLAND

17. IV. -59.

Dear Arthur,

I do not quite see the point in Silva's argument, nor do I comprehend the intricate and complicated discussion by Mr Donk, which moreover seems to have nothing to do with Desmids. (I could not force myself to read through Donk's article from beginning to the end). Silva has very little knowledge of desmid taxonomy. Donk - as far as I know - no knowledge of them whatsoever. Is it more useful and necessary that the CODE should be made "BEAUTIFUL" (as Silva says) than that the nomenclature of the desmids should be stabilized as it has been during a long time (since Nordstedt, West & West &c.) Would it not be more useful to keep to the old good nomenclature as developed by wellknown desmidiologists: Nordstedt, Borge, West & West, Lütke-müller, than to try to resuscitate some old long forgotten names of authors who are more or less obscure. Is the priority of an author at all costs to be maintained? My answer is: no! but Silva's may be "of course"! Is it really so important that an old good name, such as *Cylindrocystis brebissonii* should be rejected and replaced by an obscure "*Cylindrocystis cylindrospora*"? (which may have been quite wrongly identified??) No *Cylindrocystis* with cylindrical spores is known, thus the name "*cylindrospora*" is Hebrew to me.

Of course there are some exceptions from Ralfs' nomenclature - everything is developing with time - but these exceptions have been generally accepted - is that what Silva means by saying that Ralfs is not used as starting point? - and these exceptions are very few in number.

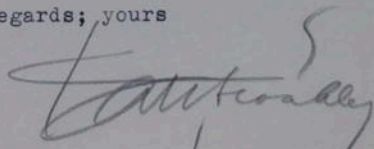
I have written and also spoken with Hans Luther about the Silva-question: he does altogether condemn Silva's action (with very bad words!)

I am still of the opinion that a stabilized and simple taxonomy in the desmids is much more valuable than the "beauty of the Code"!!

You write that Silva's and your own conceptions are quite different as to the starting point Ralfs 1848. Could you explain this difference to me in a few words and quite simply?

I have no intention to enter into a discussion with those codified pettifoggers: it would be waste of time and energy. But I believe that it is wholesome to the botanists to see that there is an opinion different from Silva's.

Best regards; yours



April 20 1959

Dear Rolf,

I have your two letters of Apr 12 and 17. I have made no move to stop publication of our article in Taxon; it was merely a suggestion. Also I am quite content to have my name appear as co-author, but I am not going to send out the mimeographed copies that I had made.

It is not surprising that you are unable to follow or understand Silva's and Donk's arguments. I thought I had a very good knowledge of the English language, but both these articles and many others in Taxon, and also in the Code, are written in such complicated phraseology that I had to read and re-read their articles many times and give many hours of thought before it dawned on me what they were talking about. The only important part (to us) of Donk's article is on pp 245 and 246, where he gives a brief history of the action taken by the several Congresses on later starting points. He says that the Brussels Rules (1912) merely state that 'botanical nomenclature' begins for different groups at different, specified, dates. If the wording had been left at that, everything would have been all right. But the wording was changed in the Cambridge Rules (1935), and in the Stockholm Code and the present one, where it now reads "Valid publication of names....is treated as beginning at the following dates". (1848 for desmids).

The difference between my conception (and yours and that of other desmidiologists) and Silva's is simply this: I had overlooked the change in wording while Silva had not; in fact he may have been partly responsible for the change. Under the present Code the name Closterium libellula Focke 1847 was not validly published. Ralfs 1848 published the same Desmid as Pendium closteroides Ralfs which was validly published. Ralfs had not seen Focke's work, as he states in his introduction. If he had known of it he would most certainly not have published the name that he did, and that is exactly what Silva means when he mentions Ralfs' extreme conscientiousness in giving proper credit to previous authors. Nevertheless, Ralfs' name is the first one that was validly published under the terms of the present Code; consequently if Silva formally publishes Closterium closteroides (Ralfs) Silva comb. nov., it would be the only legitimate name for the plant according to the Code, and Herr Focke would get no credit at all.

This also is what Silva means when he states that desmidiologists have not used later starting point. For instance, Krieger (Monograph p.114) states: "Als Anfangspunkt der Desmidiaceen-Floristik wurde Ralfs 1848 angesehen", and doubtless he thought he was doing so, but he actually did not, according to the Code. In order to comply with the later starting point he would have had to use the name P. closteroides Ralfs, though he would of course have transferred it to Closterium.

It seems to me that what Silva is trying to do is to improve this very bad situation, but the remedy that he proposes would be worse than the disease. It would require the artificial conservation of a considerable number of both generic and specific names, according to a list that he proposes to submit later to desmidiologists. How is he going to reach all the desmidiologists in the world? There are only half-a-dozen on the roster of the I.A.P.T.; the membership list of the Phycol. Soc. of Amer. is much larger, but there are many desmidiologists who are not members and never see the Bulletin. Even if agreement could be reached on such a list of conserved names it would be bad enough for experts, but far worse for new students, who would have to learn (how?) that the present Bibles of desmidology like West & West, and Krieger, are not Bibles at all, but simply out-of-date works subject to many corrections.

Anyway, it is obvious that neither you nor I can argue with Silva, for he employs a language that us foreigners both of us. Maybe Prescott could do it, with his professional experience of 30 years or more. He has not replied to my last letter of a couple of weeks ago.

So I guess the best thing we can do is to let the article appear in Taxon, and then drop the whole thing, and keep on writing in <sup>the</sup> same way that we have been doing. I have just created the name Sl. libellula var. elongatum (Krieg.) Scott & Presc. for an Indonesian desmid. If somebody wants to change it 25 years from now I shall never know about it.

My opinion about the Code and codifiers is just about the same as yours. All of my adult life I have been occupied with applied science as distinct from theory, and with practical ways of doing things. That accounts, in large part, for my impatience with theorists and the many kinds of special jargon that they invent. More than 300 changes in the Code have been proposed to the Montreal Congress, including 19 by Silva. This seems to me a very large number, though Lanjouw says it is not unusual. Many of these changes appear merely trivial to me, or even absurd like the one by Fosberg who would like to unite the parts of a split infinitive. Surely the brains of these people must work in a different way from mine and yours.

I am writing to Franz Eckert asking him to send you some of his slides, and I am sure you will be surprised and pleased when you see them. I think he is a preparateur and makes the slides for sale, and is not much interested in desmid systematics. The slides have a real professional look, with specially printed labels.

You are correct that the new Brazilian *Xanthidium* in Eckert's material can be made a new variety of *X. acanthophorum*. It differs considerably from the Indo-Malayan-Australian form that is the only other one that I know. Nevertheless the structure is the same. I shall not publish it, of course, unless he gives me permission to do so.

Best regards from

1. V. -59.

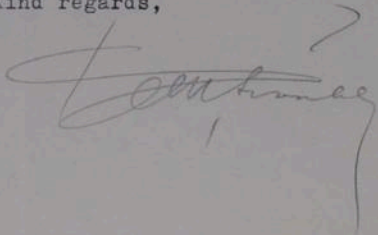
Dear Arthur,

I was very glad to receive your last letter of April 20th. It made the whole question at once quite clear to me. Of course I understand the English text, but really I had no patience to read it with thought and reflexion. So I was quite unable to comprehend the deep wisdom in it. Apparently Silva is a small "dictator" of his kind and he and his friends have very cautiously little by little transformed the Rules in the Code in a certain direction, these manipulations having been observed by nobody (by no desmidiologists). In Scandinavian countries the botanists do not accept such names as "Closterium closterioides" because it is senseless: translated into English it would be " a Closterium-like Closterium" which is nonsense. / The zoologists accept "Cossus cossus L" and some other peculiar combinations. / As the question stands today the name Closterium libellula and Cylindrocystis brebissonii must be made "nōmina conservanda". But as I told you, I do not wish to stir up this wasp's nest, and as to the arguing with Silva & Co, I am not fit to do it nor have I any desire to do so. I am glad to know that you ~~have=the=same~~ are of the same opinion. (Yesterday 30.iv, I had a letter from Kuno Thomasson, Uppsala, who had read our article in Taxon (I have as yet not got it) and he gives his full approval to our opinions.

Apparently you have not observed that Xanthid. acanthophorum has been recorded by me from Central Finland (Desm.Keuru 1920,pl.II fig.29-30 and furthermore I have seen it in an old sample made by Elfving from SE Finland (Imatra). Nordstedt reported it from Sweden (De algis et characeis I-II, 1880,p.12) and Raciborski in his "Nowe Desm.1889,pl.VI,f.18.

Do you have my papers Desm.Keuru (1920) and New Desm. (1921)? If you wish to have them I have got one copy of both of them which I have reserved for you.

Kind regards,



I had just finished this letter when I got your 14 gaudy drawings. There are quite a lot of them - My own material is all dead and useless.

ROLF GRONBLAD  
KARIS  
FINLAND

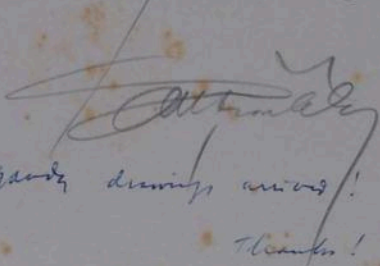
4. V. -59.

Dear Arthur,

I had just sent my letter to you when your drawings arrived together with my drawings from Bahr-el-Ghazal. Certainly your suggestions will be very helpful to me. As to the Uganda samples you had found a lot of species. I have not yet compared them with my list but I am sure there are there are such ones that I did not find. There is work to me for a long time because I am now very slow at work, not being able to sit working late in the night

Our article in Taxon seems to be something of a "success": I have got words of agreement and appreciation from Thomasson (you have not sent your last paper to him!), and from John W.G. Lund, Freshwater Biological Association, Ambleside and from Hans Luther. Dr Lund writes: "As one who is not a specialist on any group of algae but who equally read Silva's article with alarm, may I thank you and Dr Scott for your fine counterblast in Taxon. I feel hopeful that you and the Czech botanists have made the other side clear even to angiosperm taxonomists. I may say that as a member of the special committee of the British Phycological Society I have supported your view and the society have voted against abolition of later starting points. Kind regards John W.G. Lund". - I feel that we are fighting for the better case and I hope Silva has not too many American supporters

Not more this time. Best regards,



5.5 The additional Uganda drawings arrived!  
Thanks!

June 12 1959

Dear Rolf,

The reason for my long delay in answering your two letters of May 1st and 4th is that I have been extremely busy trying to finish my big paper on Indonesian desmids before the arrival in New Orleans of Mr. Sachlan. I just managed to complete it the day before he got here, but I still have to check it for correction of the numerous errors that I make in typing.

Then, Sachlan was with me for five days, and it was a rather strenuous time, for during the last few years I have not been accustomed to going continuously from early morning until late at night. He is a Malayan, and nominally a Moslem, but he did not decline to eat a pork sandwich when we were out on one of our trips. He reads and writes English very well, but it was sometimes hard for me to understand his speech, and in fact everyone to whom he talked had the same difficulty. I showed him as many of the sights of the city as I could, and took him on a desmid-hunting excursion, with rather poor results because of unfavourable conditions. One of the biologists of our Wildlife and Fisheries Commission took us in an airplane out to the string of islands off the Louisiana coast, where he wanted to count the pelicans to see whether they had been dispersed by a small tropical hurricane that occurred a few days previously. Over the islands we flew at only about 30 m. above the ground, which was quite exciting, but it was an amphibious plane and I suppose that the pilot could have landed in the swamp without damage if it had become necessary. I had never seen this coastline before because it is quite inaccessible except by boat or plane. We had lunch at the Fisheries Biological Laboratory on one of the islands, and then he took us in a speedboat out in the Gulf of Mexico to see some shrimp trawlers at work, in which Sachlan was specially interested. We boarded one of the trawlers, where they were sorting their catch on the deck. The fishermen were picking out only the shrimp, and discarding overboard all the small fish and crabs, which seemed to me to be more numerous than the shrimp. Sachlan was amazed at this waste of "protein", and said that in Indonesia nothing would be wasted; everything would be taken to the market and sold for food. But over here nobody would buy them.

On our way home from this trip we stopped at Sears Roebuck & Co., a very large department store of which you have probably heard. He wanted to buy a "cowboy suit" for his small son, and when this had been done I asked if he would like to walk through the store and look at all the merchandise that was on display, which is really astonishing even to an American. He replied, rather shortly, that he was tired of looking at "American riches". But that is understandable, I suppose, coming from an under-developed country like Java, as he does.

The comments on our article in Taxon are interesting, particularly the one from Dr. J.W.G.Lund. Since Lund is a member of the Committee of the British Phycological Society, and will probably attend the Montreal Congress, do you think it would be advisable to write and inform him about the true meaning of "Later starting point for Desmids", which he apparently has overlooked like everyone else? I have heard nothing more from Silva since I sent him the advance copy of our article, and Prescott is not inclined to argue any more about the matter, though he informed Silva some time ago that he was opposed to the change. Today I received the May issue of Taxon, and note that Silva's proposal received only 8 favourable votes, with 20 opposed, and 31 voting to refer it to the Committee on Algae. This will probably kill it, but the matter still will not be settled, for I believe that most phycologists are unaware of the real meaning of Article 13(g).

The reason I did not send Kuno Thomasson a reprint of the Arnhem Land paper is that his name was on Prescott's list, and we had agreed to split the expense and work of distributing the paper. However, I have sent one of my copies to Thomasson. His work on the Argentina lake district is a beautiful example of fine printing; the large photographs, reproduced by the photogravure process, are superb. He and his companion accomplished a huge amount of work in the short time of 10 days, most of it in the Phanerogams. It seems strange that he found not a single novelty among the desmids that he collected; I should have been much disappointed with such a result.

I have photocopies of the plates in your papers Desm. Keuru and New Desm., but no text, so I should be happy to receive the complete reprints if you can spare them. Yes, I had overlooked the fact that you had reported *Xanthidium acanthophorum* from Finland, and that Nordstedt had recorded it from Sweden. I had thought of it as purely a tropical species. The Brazilian specimen on Eckert's slide differs from the type in the proportions of length to width, and in the much greater stoutness of the spines, but it could easily be described as a new variety.

In the next few weeks I shall be able to get rid of the Indonesian paper, and then I will go back to work on Sioli's Brazilian material and complete it.

Best regards and good wishes from your friend,

29. vi. -59.

Dear Arthur,

I am glad to hear that you have finished your big paper on the Indonesian desmids. I always feel very happy when something is ready for the printer. The paper written jointly by Ruzicka and me became almost a nightmare to me. Perhaps I have told you its name "Zur Taxonomie der Desmidiaceer". The first outlines were written by Ruzicka but I had to shorten it <sup>with</sup> about the half. The language was very bad and sometimes impossible to understand (both of us can equally badly German!) but after numerous sendings to and fro the text became something that we could understand - both of us - and also accept. It will be printed in the "Botaniska Notiser 1959, fasc.2. But, really, it got on my nerves. From the beginning we had thought that also Teiling should join us, but very soon he was quite angry with R., and later with me too. I shall never more write such theoretical papers! The most important statement is that only fixed and hereditary variations should be treated as taxa - the nonhereditary being treated as "modifications" or "morphae" which cannot have taxonomical names nor author's names (to these belong for instance the various "radiations" of some Staurastras.). But you will soon see the paper when I get my copies.

I have not heard anything from Mr Prowse, have you? Was he disappointed with our Sudanese paper?

It seems to me a very good suggestion that you should write to J.W.G.Lund and inform him on the question about the "Later starting point for Desmids". Do it, please. - I have been reflecting upon the matter, and as far as I can see, - in spite of the presently ruling Code - such names as Closterium libellula could be made valid by using "nomina conservanda" also on the rank of species. Closterium closterioides is a very bad name, it is in reality senseless: "a Closteriumlike Closterium". But by using nomina conservanda specifica in this case as well as in some few other cases the situation would be acceptable. I have also given my vote for such a conservation of specific names. It would be very good to win such a phycologist as Lund against Silva & Co.

Kuno Thomasson's paper is fine indeed, but in such temperate regions I am not astonished that he did not find "new" taxa. However, I think there is much useful investigation on desmids besides new species. Perhaps he has not a keen eye for new forms. His figures are sometimes a little careless, they are more like mere sketches - but not altogether bad. As a personality I like him, he is full of youthful energy, but very modest and kind.

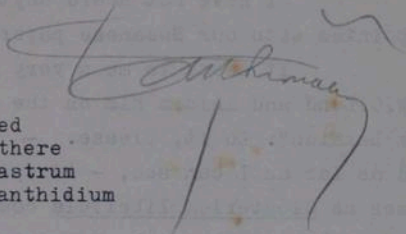
As I write this, I have already sent to you "Desm. Keuru" and "New Desm" together with a paper on *Cylindrocystis* (1959). (not by air mail!)

As to the material collected in Brazil by Sioli, you sent me also part of the samples and I have made some drawings. Would you like to have my drawings if you perhaps have better time to write than I have? You know my time is very limited, especially since I have been ill and I must go earlier to bed. And I have the Uganda paper, &c to write, and some collections for research.

Have you got any papers from Okada? There are two <sup>+</sup> papers 1953 that I got from Hans Luther. But I have not his address. Do you have it? The "New classification of desmids" (1953) seems to me obscure. Okada places Roya among the placodermae in "Closteriaceae". I do not think the system based on only the zygospores is quite "natural". What is your opinion?

Have you got the paper by Mme Gauthier-Lievre where she is describing our *Micrasterias incredibilis* as "Allorgeia"? (only with the difference that both semicells are like our "lower" semicell, thus a vertice quadrangular. Of course our name has priority and I do not think there is need for a new generic name (?). What do you say? It is a very interesting fact that such a more symmetrical (the two semicells being alike) or almost so) has been recorded from the French Sudan so soon.

My best and friendly regards,



+ ) In the second paper by Okada, entitled "Taxonomical studies on Genus *Euastrum*" there is on pl. II fig. 18 a desmid called "*Euastrum urniformis*" which is very much like *Xanthidium raciborskii*!

July 8 1959

Dear Rolf,

Many thanks for the films of so many of Schridde's papers. I had some of the papers already, but the others will be welcome additions to my library. Thanks also for the reprints of our article in *Taxon*, which I shall use only for extra-European correspondence.

I will write to Dr. J.W.G. Lund about Later Starting Points in Desmids, and will send you a copy. Your suggestion that the name Cl. libellula could be validated by the use of *nomina conservanda specifica* is correct, but there seems to be a very strong objection by most botanists to the conservation of specific names. I do not know what the reason is, unless because of the immense number of species, probably hundreds of thousands. The proposal for conserving specific names has been made to several Congresses, and has always been defeated by a large majority. I have heard nothing more from Silva, so we shall have to wait and see what he does at the Montreal.

Of course I shall have to read the paper by you and Ruzicka. In one of his letters a couple of years ago he attempted to explain his views to me, but unfortunately my reading knowledge of German is not very good when complicated arguments are involved, and I failed to comprehend the matter thoroughly. In some *Staurostrum* species the number of processes seems to vary more or less randomly, e.g. St. onhiura. On the other hand, St. sagittarium is known with "radiations" of 6 to 11 processes, but I have one Australian collection in which all specimens have 11 processes on each semicell, so that this "facies" is evidently hereditary. (Arnhem Land, Fig. 22, No. 6).

The only papers by Okada that I have are from 1935 and 1936; I have not heard of the two that you mention, nor do I have his address. As to classifying desmids solely on the basis of their zygospores, I should say that it is quite impossible for at least two reasons: 1) Zygospores are not known in a sufficient number of species; 2) Zygospores of the same type occur in different genera. It is surprising that anyone could mistake a desmid like X. raciborski for a *Buastrum*. The name uniformis should be changed to unifforme, according to the Code, and when that is done it conflicts with E. urnaeforme Wille, which Krieger rejected under the name uniforme, and which actually must be a form of E. pectinatum, perhaps var. lobuliferum Scott & Freac. 1952.

About two weeks ago I received from Mme. Gauthier-Lievre four reprints, two of them dealing with Rhizopoda, one on Algae from Tibesti, and the fourth on Asymmetrical Desmids. It is, indeed, astonishing that she should have found both of the asymmetrical Micrasterias, and even more so that her Allorgeia valiae is so much like, and yet so different from our M. incredibilis. I wonder what Thomasson's specimens from Lake Bangweulu look like; whether they agree with ours or Mme. Lievre's, or perhaps are ~~which~~ different from both. After my experience with Ichthyodontum sachlanii, where I at first found several specimens with unlike semicells, and later after considerable search I found the two symmetrical forms that gave rise to the dichotypical ones, I began to suspect that at some future time two symmetrical forms of Amscottia mira might be discovered. Now it is evident, from our Sudan specimens, and Mme. Lievre's from the French Congo, and Thomasson's from N. Rhodesia, that M. incredibilis and M. sudanensis can and do exist as entities in their own right. Mme. L. even says that sudanensis is "widely distributed in African equatorial and sub-equatorial regions", though she only lists one habitat for it, in Oubangi-Chari. I don't suppose that any geneticist can do any more than guess at the cause or origin of these asymmetrical desmids. My own guess is that the dichotypical cells of I. sachlanii ~~started~~ started as hybrids between the two symmetrical forms. This also may have been the origin of Amscottia and the two

asymmetrical Micrasterias species. Another possibility that has occurred to me is that they may have originated as "sports", caused by such a mechanism as rearrangement of the chromosomes and/or genes by cosmic ray bombardment, in much the same way as the abnormal variations of Drosophila are produced by exposure to X-rays. I wonder if any of these Drosophila variations are asymmetrical?

Both you are I have said before that the creation of a new genus is a matter of personal taste and opinion. M. sudanensis obviously belongs to Micrasterias, but if I had had the privilege of naming incredibilis I think I would have made a new genus, because it contravenes so many of the characteristics of the generic diagnosis of Micrasterias, and because it differs so radically from all other species. There would have been just as much justification for a new genus as with Amscottia, and even more than with Ichthyodontum. There is another reason: to prevent the genus from becoming too big, heterogeneous and unwieldy, as is now the case with Cosmarium and Staurastrum. The latter genus was founded on St. paradoxum, and the person who first discovered the orbicular forms like St. muticum, etc., could easily have justified a new genus. Likewise with the angular forms with a spine or spines at each corner, and the "hairy" ones like setigerum.

The nomenclature of incredibilis is somewhat confused. Our name has priority of date, but Mme. L's new genus has the priority of rank, according to an informal though authoritative opinion given me by Dr. H.W. Rickett to whom I wrote about the matter. Thus for those who believe the plant is correctly placed in Micrasterias our name has priority, and the Sudan plant should be cited as M. incredibilis, and her plant should be cited as M. valiae if it is considered to be a new species, or M. incredibilis var. valiae if it is considered merely as a variety. On the other hand, for those who think the new genus is justified, our plant has to be called Allorgeia incredibilis. But to make the confusion still worse, both Mme. L's generic and specific names are illegitimate, because they are not validly published in accordance with Art. 34 of the 1956 Code, which requires a Latin diagnosis for all taxa of Algae published on or after 1 Jan. 1956! Neither you nor I give a damn about this, but sooner or later someone like Silva will discover the omission. Would you care to point this out to her, very gently???

I have had only one letter from Dr. Frowse since his return to Malaya, and it was just a thank-you note for the courtesies we showed him while he was here. So far as I know he was well satisfied with our Sudan paper; at any rate he said nothing to indicate the contrary. In the Phycological Bulletin No. 37, Silva lists a paper by Frowse in 1957, "Distributional relationships of freshwater algae in Malaya", Abstr. 9th Pac. Sc. Congr., p. 51. This seems a little premature, for in 1957 he could not have been in Malaya for more than a year or so.

Have you received the announcement of the new journal of cryptogamic botany "Nova Hedvigia"? The first number is out, though I have not yet received it; it contains papers by Skuja, Ettl, Gerloff, Hortobagyi, and Pott. The publisher is H.R. Engelmann, Box 166, Weinheim/Bergstr., Germany. It is to be issued in 4 to 6 numbers a year, each of about 160 pp and 20 to 30 plates. The price per issue is DM 20 to 25.

The same firm will also publish Die Gattung Cosmarium, by Krieger & Gerloff, in about 7 issues of 160 pp, including 16 to 32 plates, each. Price per issue is also DM 20, with a discount of 10% to subscribers to Nova Hedvigia. These are the prices quoted to American subscribers, and I should like to know if the European prices are the same. Sometimes I think the European publishers make special prices for the rich Americans.

Best regards and good wishes from

Dear Arthur,

The paper by Gblad & Ruzicka is already printed in Bot. Notiser 1959, fasc.2. Ruzicka's German language is rather bad and complicated - and so is mine too! - and we had sometimes difficulties to understand each others. But now the German is much simplified and made correct by Dr Hans Krieger (who is in Helsingfors but who is not identical with Dr w.Krieger's son). I had to cut the text to about the half of the original because R. likes to say the same thing with many words and many times. Otherwise our cooperation has been quite amicable and easy. At first it was thought that Teiling would have been in collaboration too, but he was angry with R. because both of them had their own views, and then he was also angry with me because I supported R's opinions. That is childish and old men should not quarrel about such matters.

Staurastrum ophiura, as far as I have seen it in Finland, is mostly 7-radiate sometimes 8-radiate, but never different radiations are found from the same locality. So there is a certain amount of stability and heredity. But only pure cultures can clear up such questions.

Do you think it would be possible to get Okada's address? If you wish I can send you photocopies of these two papers? In my opinion the classifying of Desmids on the basis of their zygospores is not a happy method.

Mme Gauthier-Lievre's "Allorgeia valiae" is certainly very closely related to our Micr.incredibilis. I should think they are best treated as two varieties of one species. These species would be most interesting subjects of critical cultivation. (e.g. for Kallio!). Whether the name Allorgeia or Micrasterias should be used is only a matter of personal taste. (There is Micr. muricata which has its processes in 3 different planes, or rather 3 + 2). Perhaps it is correct to treat Allorgeia incredibilis + var. valiae - or Allorgeia valiae + var. incredibilis as a separate genus? // It would perhaps be quite consequent to do so when we are treating Amscottie as a separate genus and not as a Staurastrum - which would also be possible. // I do not know what to do with the illegitimacy of Mme G.L.'s diagnoses? Perhaps it would be advisable to write to her. Bourrelly does never use Latin diagnoses (the French people think that their language is the foremost language in the world as it was once in the times of Louis XIV and XV and Napoleon. It is very thoughtless because some day in the future the Russians, Japanese or Chinese can do the same thing and write their descriptions only in their own languages.

What would then happen?

Is the paper by Prowse the same as I have: "An introduction to the Desmids of Malaya" - Malayan Nature Journal, vol. 11, 1957? This is a paper of no special importance and I think Mr P. simply had to write something on Desmids in order to show his knowledge. Do you have it? (There are many wrong identifications).

I too have received the announcement of "Nova Hedwigia". The first supplements will be "Die (Desmidiaceen-) Gattung Cosmarium by W. Krieger and J. Gerloff. About 7 issues of 60 pages (16-32) plates included at DM 20,- (\$ 5,-). I have written to Akademiska Bokhandeln and asked them to procure for me this big work. So the costs for us poor Europeans are alike heavy. But certainly it is a book worth while having. I am very curious to see what "J. Gerloff" has made.

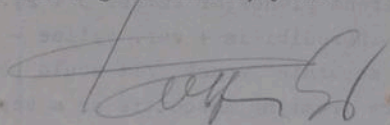
I do not know what my opinion today of *Staur. saltans* is. It is so long ago I wrote those papers. Perhaps you are right in what you are saying in your paper Notes Ind. frw. alg. III p. 5. Don't you think that my *St. saltans* (1936) I:18-20 and II:26-30 really belong to *S. saltans* ?? I should like to know your personal opinion about them.

To morrow I am going "by air mail" to Stockholm where I shall stay a few days with Lisa Borge, the daughter of old Dr O. Borge. We have always been friends since my first visit to the Borge family.

We have had a rather good summer this year and I have been collecting desmids in some lakes and ponds nearby using my moped. There are hundreds of lakes quite nearby. Yesterday I found *Micr. verrucosa* which is rather rare. I believe it is very closely related to *M. angulosa* (?). Most of the desmids here are well known, but sometime it may happen that a new or interesting desmid is found.

I shall send you reprints as soon as I have got my reprints from Bot. Notiser.

Best wishes for a good summer, yours



*Today I received the Scientific American for June. My best Thanks!*

July 24 1959

Dear Rolf,

Enclosed is the copy of my letter to Dr. Lund; there has not yet been time enough for a reply, of course.

In my last letter I forgot to mention the Brazilian material. I shall be glad to write the paper if you wish, but I think it will be better for you to hold your drawings until I have completed my study. Then I will send my drawings to you for comparison with yours and for a check of my identifications, so that we may reach an agreement on them. After that you can send my and your drawings here, and I will proceed to draw the plates and write the descriptions.

I agree that Mme. Gauthier-Lievre's plant differs only varietally from our M. incredibilis, and since our specific name, at least, has precedence, here should be known as Allorgeia incredibilis var. valiae, for those who accept the new genus. She made the collections about 30 km. from Fort Rousset, which is about 425 km. in a straight line NNE of Brazzaville, in the French Middle Congo. There are several small towns in the neighbourhood as shown on my map, though probably they are no more than native villages. However, she might know someone in Fort Rousset, or in Brazzaville who could make some new collections, and send them by airmail to Kellio in a living state. If you write to her you might mention this possibility, and I think you might also mention the necessity for Latin diagnoses. Bourrelly also maintains some algal cultures, and it might be that he could undertake to culture this desmid; undoubtedly they have a quick airmail service between Brazzaville and Paris.

The paper by Prowse is not the same as "An introduction to the desmids of Malaya", which I have. It is a second paper, and he has not yet sent it to me.

Yes, I think your St. salians var. midgyracoenas (1936) is correctly assigned to St. salians, though it seems to be either a primitive or else a depauperate form. The reason I think it correct is that the plant has 4 apical spines horizontally directed, as in the species and in the several differing forms that I have illustrated from Indonesia, and that there are no apical verrucae. The American forms (Cuba, USA, Canada) on the other hand, have two asymmetrically placed vertical spines, and usually two large emarginate verrucae, though Borge (1899) mentions three and shows five verrucae. The "consensus of opinion" that I mention means that West & West (1902), Lagerheim (though he later changed his opinion), G.W. Smith, Taylor, Irene-Marie, Wölle, Prescott and myself, believe that these plants should be and have been properly assigned to St. grallatorum. It is possible that the two species ought to be united because they show evident similarities; if and when this is done the name grallatorum has priority. But nobody knows enough about them to do this. I have probably seen more different forms than anyone else, but even I have seen only varieties, not either of the two specific forms. Your var. brasiliense seems to be intermediate between the two species, because it has the ring of minute denticulations around the base of the semicell, which, so far as I remember, have been shown only by Soshua and West & West.

I shall try to get Okada's address for you, but it will take a little time. Prescott is in Montana, and then will go to Montreal, and in September he is scheduled for another trip to Alaska. So I shall have to try elsewhere, but all the colleges are closed for the summer. I think my best bet will be to write to Minoru Hirano, and at the same time ask him to get Okada to send me the two papers that you mentioned.

A few months before his death Dr. Krieger wrote me that he had just about

finished his work on *Cosmarium*, and began to have hopes of getting it published. 30  
I suppose that what Gerloff has done is to put Krieger's material in shape for  
publication, and that he has probably not added anything of his own. I see that  
Gerloff is listed as a specialist on *Cosmarium* and *Staurostrum* in the Internat. Directory  
of Plant Specialists, p. 131.

A week ago I received from a young man named George J. Schumacher, who is with  
Dr. L.A. Whitford at the North Carolina State College, a slide containing numerous  
examples of a peculiar *Staurostrum* that he asked me to identify. He said it resembles  
*St. cingulum* var. *ornatum* J.-M. (1949), and also that it resembles "in part" *St. johnsonii*  
and *St. gracile* as depicted by Skuja in his Swedish Lake Plankton, which I do not have.  
Of course it has no connection with *gracile*, nor with *johnsonii*. In fact it has some  
unusual features of its own, that make me suspect that it is a novelty. I am enclosing  
copies of my sketches, which you may keep, and I would like to have your opinion on it.  
In the vertical view you will notice that the arcs of bifurcate verrucae are not  
parallel or subparallel with the triangular margin of the body, but are strongly curved  
and intersect the margins, so that the first and last of the verrucae in the arc  
project beyond the margin. All of the areolar verrucae are of approximately the same  
size and shape, instead of diminishing in size as they approach the bases of the  
processes, as is usual in other *Staurostra* with similar apical ornament. The small  
sharp spines on the processes are not continuations of the arcs of verrucae; they are  
arranged in rings around the processes, and there are two large rings of these spines  
on the upper part of the body also. In the basal view, the basal inflation is not quite  
circular, but slightly triangular, and at each of the angles there is a group of  
2 plus 3, or 3 plus 4 small sharp denticulations, one group under each of the processes.  
In *St. cingulum* and var. *ornatum* these basal teeth are described as forming a complete  
circle of 12 to 14 teeth, about 6 or 7 being visible, so the new plant does not belong  
to that species. Notice also that the ends of the processes are slightly inflated  
and rounded, with four long slightly curved spines arising from the periphery of the  
inflation, the angle between opposite spines being about 90°, sometimes even more.

I have not seen anything quite like this in my own USA collections, nor do  
I recall any illustrations having this combination of characteristics. It evidently  
belongs to the group comprising *St. manfeldtii*, *lustkennelleri*, *pseudobaldii*, etc.,  
which Gilling discussed in his 1947 paper, and it seems closest to your figure of  
*St. manfeldtii* var. *plancticum* in your 1942 Lapland paper. I am not well acquainted  
with these European forms, but my guess is that the plant in question might be described  
as a new variety of *St. manfeldtii*. What do you say?

*Micro. angulosa* was made a variety of *M. denticulata* by West & West (1902),  
which seems reasonable, and I don't see why *M. verrucosa* could not also be transferred  
to *denticulata*, from which it differs only in the possession of the facial verrucae.  
*Verrucosa* is quite rare over here, too, and I have found it only a few times. The  
"verrucae" in my specimens are not of the same type as occur in *Staurostrum* for example;  
they seem more like small hemispherical vesicles or pustules with very thin walls  
instead of being solid.

So far our summer has not been too bad. We have had a few days with the  
thermometer up to 94 and 96° F., but fortunately the nights are cool. Almost every  
day this month we have had heavy thunderstorms and plenty of rain which keeps the  
temperature down. As I write this, the concrete floor of my "basement" (which is  
actually at the ground level) is halfway covered with water which has seeped up through  
the crevices between the floor and walls, and through the joints in the floor.  
Probably the drains outside are stopped up, and I shall have to get a plumber to come  
and clear them out. Drainage is a serious problem in New Orleans, for the ground is  
almost perfectly flat and the city is surrounded by levees to keep out the water of  
the Mississippi River and Lake Pontchartrain, and every drop of rain that falls here  
has to be pumped out, except of course what evaporates or is absorbed into the ground.

Best regards and good wishes from

July 27 1959

Henry K. Wampole & Co. Inc.,  
Philadelphia, Pa.

Gentlemen,

• Please send me your literature on Vastran Forte.

Very truly yours,

Aug 22 1959

Dear Rolf,

Many thanks for the postcard sent from Bromma Airport in Stockholm, with your and Miss Borge's signatures. When I was waiting at Bromma for my plane, I took a photo of a miniature locomotive pulling three or four very small cars around the grounds, but I could not find anyone who spoke English to tell me what is its purpose. Did you see it, and do you know what it is used for?

Thanks also for the reprints of your 1920 and 1921 papers, *New Desmids*, and *Keuru*. Some time ago I told you that I had photocopies of the plates of these papers, but that was only partly correct. I had copies of the *New Desmids*, but of *Keuru* I had only two small photos of Plates 1 and 2 that you sent me. I had never seen the text of either, and I wish that I had had them long ago, for I think they would have saved me a good deal of time and worry in identifying some of my specimens. The 147 figures on your *Keuru* Plate 3 must surely be the largest number ever assembled on one plate!! and I wish you had written the text in German like the introduction, instead of Latin. I have picked up a sufficient smattering of Latin to be able to read the dog-Latin used in desmid diagnoses, but I don't know the grammar or syntax, nor the conjugations nor declensions, so I am unable to understand some of your comments. For instance in *Keuru* p. 86, referring to *G. delicatissima*, you ask: "An sint hae duae species formae tantum eiusdem speciei?". I don't know the exact meaning of this sentence, but it seems that you are questioning whether *G. confervacea* and *G. delicatissima* are the same, or perhaps you are asking whether Wolle's figures 23-24 represent *G. confervacea* rather than *G. delicatissima*. As I pointed out several years ago, *Norstedt's* plant checks with Wolle's figures 23-24, but not with his fig. 22, but that there actually is a plant agreeing very closely with Wolle's fig. 22 and with his description, which states that there ~~is~~ are two narrow, thick bands around the middle, twice the diameter of the cylindrical body. This is the true *G. delicatissima*. *Norstedt's* plant and Wolle's figs. 23-24 may be identical with *G. confervacea*, which I do not recall having seen.

On Monday, Aug. 24, my wife and I will leave on an automobile trip that we hope will take us as far as San Francisco, though whether we shall get there is by no means certain. Both of <sup>us</sup> are old, and I especially tire very easily, yet I have to do all the driving, and it may prove too much for me. It is sort of a last fling, for in another year neither of us will be able to stand such a trip. If it proves too much we can always turn round and come home again, but I want to see the Sequoia National Park and Yosemite Valley.

Just had a letter from Hannah, who has had a miserable time with a bad case of shingles (*Herpes zoster*) with complications, that has partially paralyzed her face. She is now recovering and hopes that in another month her face will be all right again. This is an extremely painful ailment; my wife had it a few years ago, and there seemed little that the doctor could do either to cure it or to relieve the pain.

If all goes well on our trip we shall return about October 1st, so I shall not be able to write you again until then. In the meantime, best regards and good wishes.

Sincerely,

21  
Sept. 27,  
~~XXXXXX~~ 1958.

Dear Arthur,

I beg you to excuse the very long delay with my answer. I began this letter already a month ago but I have always had so much to do.

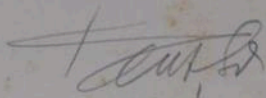
As to Micr. - Euastr. Moebii - turgidum I entirely agree with your opinions. The central tumour<sup>noy</sup> of those cells of this species that I have seen is not granulate, only covered with triangular pits. I have made a drawing from var. javanicum from Malacca which has a simple ring of rather large granules surrounding the central tumour. I am enclosing the drawings which I beg you to return.

In the summary a complete list with full names and author's names would be useful to avoid mistakes and misinterpretations. So it even would be good to have a short declaration why *Eu. turgidum* is taken as the type, the year of publishing of all these names and a complete list of literature. You will observe that according to the Code there is no "typical form" of a species but the species is divided in several varieties &c, ~~ea~~ each of the same rank and value. The "typical variety" should, however, be quoted without author's name, thus: *Eu. turgidum* Wallich var. *turgidum*. (I do not like this arrangement very much, but I believe it must be followed?)

A propos Playfair: did I send to you my copies of some of P's papers? I ~~cannot~~<sup>do not</sup> remember, nor can I remember if they were returned. Do not consider this question a rude one, perhaps I am quite wrong. But I cannot find my Playfairs anywhere. Forgive me!

Perhaps I told you that I got a small collection from Sudan (Bahr el Ghazal) from Rzoska. There were very few Desmids the greatest part of the material consisting of Cyanophyceae. There are a few Desmids which I have not yet identified. Could you give your opinion if I send them to you I should very much appreciate it. The material is not worth while sending to you. (I think so).

With best greetings,



7.X.-59.

Dear Arthur,

I suppose you have returned from your long journey and perhaps you are happy to be in your own home again? So I am always when I have been away. I should be glad to know anything you have seen and experienced. I hope you will write when you have time enough.

What about the Montreal Congress? Any news? They have again put me into the special committee for algae. I do not wish to put my head into the vasp's nest, but Hans Luther said to me that I must not give up. I am not much interested in the nomenclaturists' rant.

The paper by Grönblad & Ruzicka I am sending to you. What do you think of the question of the definition of a taxon? I think that some of the taxa described by us are to be given up because there are dichotypical cells which indicate that the vegetative cell is not quite stable.

I got two samples of algae from Sierra Leone collected by a teacher in biology near Karis. One of them is rather interesting and I am very much disappointed that she did not fill all the 20 vials I gave her. (4 of them were filled but 2 only with sand and debris!). There is a Finn student in geology working in Sierra Leone and I have asked him to collect for me. Are you interested in this small "collection"? (I could send the material and my drawings if you wish).

I had a letter from the "Verleger J. Cramer" (=Nova Hedwigia) telling me that the first part of the Rabenhorst-Krieger-Gerloff Flora will appear later this fall. I was asked to join the work with the following parts but I do not feel myself fit for it. Besides my health would never stand such work. So I have declined and proposed Skuja or Prescott. I believe they are reflecting on Skuja but I do not know if he has time and feels inclined to such work. He is, I think, older than myself, and he has made an immense amount of work with other algae so he may not wish to have more work.

The *Staurostrum* sp. drawn by you from material-G, J. Schumacher is a little like *S. cingulum* v. *obesum* in Skuja : *Phytopl. XVIII:9*, but not quite the same. My *S. manfeldtii* v. *planctonicum* (Gblad, 1942 textfig 1) which by Teiling is called *S. sebaldi* f. *planctonica* (Gbl.) Teil. is still more different. But there are formae of *S. pseudosebaldi* Wille with some resemblance to your fig. not, however, identical by any means. I believe I should have called it *S. pseudosebaldi* var. - or perhaps "nova spec." Perhaps *S. manfeldtii* var. - would do, but I do not quite like it. All this is of course very little helpful to you, but I cannot give you anything more valuable!

I am almost certain that *Micr. verrucosa* is only a variety of *M. denticulata* or *M. angulosa* which are both rather variable. I have sent living specimens to Kallio for cultivation.

I did not see any "small locomotive" at Bromma. So do not know what you have seen!

You are asking what "An sint hae duae species formae tantum eiu- dem speciei?". I was asking whether *G. confervacea* and *G. delicatissima* are only two formae of one species. But this is long ago, and I have changed my opinions many times since then. At present moment I believe that pl. IV fig. 32-33 and fig 30-31 both are *Hyalotheca* (*indica* v. *javanica*), they are as far as I can see not *Gymnozyga* (= *Bambusina*).  
or *H. neglecta*??

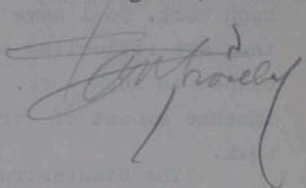
I am very sorry to hear about Hannah's illness. I have not had a letter from her in a long time but I shall write. I have too my "angina pectoris" which makes one feel just quite helpless sometimes, especially when I shall walk. Otherwise I do not feel anything seriously bad, only I am always too much tired. I know that I am old. But I did not realize it because it came so suddenly upon me. And I am something of a pessimist. This is of course quite wrong and I am trying to get wiser!

Did you see *Sequoia*? I wish I could have been with you.!

I hope your health is in good condition so that you can continue with your desmid work.

I am returning the letter by J.W.G Lund. I am glad to see that he does not like the "nomenclaturists" more than you or I do. I believe he has a too high idea concerning my qualities!

Best regards, sincerely,



Oct 24 1959

Dear Rolf,

Yes, it is always good to get back home after such a long trip; to sit in your own comfortable chair; to sleep in your own bed; and to eat the good food that you are accustomed to instead of the miserable stuff that is served in most of the roadside restaurants. It was the longest trip we have ever made ~~in~~ by auto, 6000 miles which we covered in 31 days of actual driving, or less than 200 miles per day average. Neither my wife nor I had any trouble with our health, though I had half-way expected that the altitude might have some effect on my heart and lungs, for we had to cross several mountain passes at elevations of 8000 feet or more.

Oct 31 1959

I started this letter a week ago, as you will see, and then I ran into troubles. From Java came a letter saying that the editor of Reinwardtia has lost the Ms. of my fourth short paper, so I had to send him a duplicate that first has to be checked for numerous small typing errors. He also informed me, after a delay of four months that Reinwardtia will not be able to publish my large paper on Indonesian desmids with 63 plates of illustrations, because their budget has been drastically reduced. I had more or less expected this on account of the political troubles in that country. But it meant that I had to write a letter to Mr. M. Sachlan who will leave the U.S.A. in a few days on his way back to Java, and another letter to Prescott asking for suggestions as to other journals that might print it.

Then I have a bad fungus infection of the skin between and under my toes, colloquially called "athlete's foot". This is a common disease that I have had for many years, and hitherto I have kept it under control by using a fungicidal ointment, but this time it had gotten out of my control, so the doctor is treating it with  $\text{KNO}_3$ , with not much perceptible improvement so far, and it is rather difficult for me to walk.

Well, these are minor troubles that will clear up themselves in time. Getting back to our trip, I can say that my wife and I enjoyed it immensely, because the country and scenery are so greatly different from anything that we have in Louisiana or any of the southeastern States. As soon as you get about 100 miles west of the eastern border of Texas the country starts to get dry, trees become scarce, then the soil becomes arid though still productive when irrigated; and finally, in Arizona and southern California there are vast expanses of true desert, sandy and stony wastes with only a scanty and scattered vegetation of xerophytes. I can recognize only a few of the larger plants from having seen them illustrated so often, like the giant cactus (Saguaro); the prickly pear, cholla and ocotillo, all three of which belong to the same genus (*Opuntia*) though they are vastly different in appearance; the Joshua tree (*Yucca brevifolia*) and two or three other species of *Yucca*; the century plant (*Agave*) which strangely enough grows well in New Orleans. These deserts must be highly interesting places for naturalists who can afford the time to explore and study the many curious plants and animals that inhabit them. It was pretty hot in the desert, but our car is air-cooled with a small machine similar to that in an electric refrigerator, so we simply closed the windows, turned on the cooler, and rode in complete comfort at a temperature of about 80°F., while outside it must have been about 110°F. in the shade, but there is no shade whatever!

Of course we visited many of the famous sight-seeing places, most of whose names you will recognize, such as the Meteor Crater, the Petrified Forest, and the Saguaro Forest in Arizona. In California we went to Yosemite Valley, Lake Tahoe, two different Sequoia forests, and Death Valley. Driving up and down these steep

mountain roads, with their innumerable sharp curves and hairpin turns, was an exciting experience, for I have done no mountain driving for the last 30 years or so.

Yosemite Valley is a very wonderful place, with enormous granite domes, half-domes, and sheer cliffs that were eroded and shaped by glacier action millions of years ago. Unfortunately last summer had been unusually dry, so the many waterfalls were almost completely dry; even the famous Yosemite Falls had not a drop of water, and the equally famous Mirror Lake, close by, had shrunken to a mere pond, with people walking over much of its exposed sandy bed.

Death Valley was a little disappointing to me. There are some spectacular views from the mountains on each side, but the floor of the valley is little different from the other deserts that we crossed. At its lowest point the valley is 285 feet below sea level, the lowest point in the U.S., while less than 100 miles away is the ~~highest~~ highest point in the U.S., Mount Whitney, about 14500 feet high. It gets extremely hot in the valley during summer, and there is no water whatever, so tourists are warned not to venture off the main roads.

I have seen many photographs, and no doubt you have also, of the famous redwood trees, the Sequoias, showing people standing close by, from which the size of the trees can be judged. Yet the sight of the trees themselves is literally astounding, breath-taking. It seems incredible that the largest specimens have diameters at the ground line that are greater than the entire width of my house (and probably yours). Yet it is a fact. My house is 32 feet (9.8 m.) wide, while the largest redwood tree has a mean diameter at the ground of 10.3 m., and largest diameter of 12.3 m. These dimensions are for *Sequoia gigantea*, which grows only in a few places in the Sierra Nevada mountains at elevations of 4000 to 8000 feet. There is another species, *S. sempervirens*, that grows in a long belt near the Pacific coast of California and Oregon, at elevations up to 2000', and you will see a good picture of it in the November issue of the National Geographic, illustrating some trees in the Muir Grove just north of San Francisco, which we visited. This species does not attain quite such large diameters as the *gigantea*, but grows even taller.

November 2.

My doctor has told me I must keep off my feet for a week or more, to allow the lesions under my toes a better chance to heal, so I shall not be able to write any more for that time. I am returning your review of Borneo & Flahault's book, with some corrections. This work deals only with certain Cyanophyceae, so I think it would be out of place to refer to desmids, and that your remarks on desmids should be saved until the appearance of the reprint of Ralfs 1848. It would be desirable, I think, to give a list of the "Starting point books" that Cramer expects to publish. In all reviews I like to see information as to the size of the book, the binding (whether cloth or paper), the number of pages, the price, and the complete address of the publisher, which in this case is H.R. Engelmann (J. Cramer), P.O. Box 166, Weinheim/Bergstr., West Germany.

Will write more as soon as the doctor permits me to walk again.

Best regards, from