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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.

27.IX.1961.

Dear Hannah,

I shall promptly give you my answer to your question about *Roya anglica* (- not *angelica* ! -). In my opinion they all are *R. obtusa*. In your drawings there is no cell with the apices so distinctly truncate as in my figures: the apices are "angulate", you cannot that any of your cells has an angulate apex? they are all more or less rounded, are they not? I hope you are not dissatisfied with my deciding your question in a negative manner! It is very good to have many figures and not only one or two.

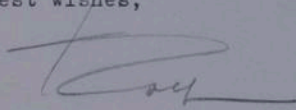
Two days ago I sent my Labrador drawings by air mail to you, and if you find something that you can use you are free to do so.

What is the matter with Scott???

Best wishes,

*The Russian papers you asked about I do not know
Where did you find references? I shall write to Kussinskeja
or Wasylik.*

*Krieger's copies of Hodgkett's figures
are not exact (as many of Krieger's cop'is)*



Ken's 28/11.61

Dear Kenneth

Now I can tell you that I have
examined your collection of samples
from Labrador. Some of them were
rather rich, in one I did not find any
diatoms. They were very little characteristic
for N. America, - by one species was *Spinococconeum quadrident* is strictly American (only
one cell was found). I have also made 68
pencil drawings and a list of each sample
these lists do not pretend to be complete, it
would have ~~needed~~ much more time. I shall
type these lists and send them together with
the drawings to you. I should be very much
interested to see your identification. There were
some species that I was not able to identify so far.
Best wishes Ralf

I think you are free on shells, but it was certainly not needed!

Dear Hannah,

Dec. 20⁷, 1961.

Thank you for your kind letters and for the list of species from Labrador. I have to forward to you Aili's sincere thanks for her X-mas gift and the letter which I translated to her. She was very proud, as you can understand to get things and letters from America! During my illness she has been a very kind and dutiful nurse to me. A year ago when I escaped from the hospital in Ekenäs - where the nurses were not kind to me - there was much trouble, but I shall not talk more of that hellish time. Never more shall I go inside Ekenäs hospital! Of course I was no angel, but I was sick, and a sick man desires a little kindness. Finis!

I have not yet my lists of species from Labrador in order and typed but I shall do this between Xmas and New Year when I am quite free from dentistry. (Now I have to make a manuscript ready for printing /Bahr-el-Ghazal/ and /Jämtland, Sweden, which I shall send you for inspection/. Besides there are other urgent "affairs" to be cleared. - I think I shall send my drawings first to you, because my are certainly not so numerous as yours. So you can criticize and tell your opinion about my identifications. As you must understand from the short time I have used, my lists are not complete (in fact every time when I take a proof and look at it through my microscope I find something "new", but I had to finish this examination once for all). Thus my work has been rather hasty and so there may be ~~some~~^{annoying} errors too. And I hope you are going to speak out your opinion quite freely and frankly. I am not going to be or feel offended whatever you say! And I hope I am allowed to speak without hiding my opinions to you. Such a manner of collaboration is the only useful and it worked excellently between Scott and myself. Einverstanden? I hope so.

From your lists of species which I have only very superficially compared with my own I see that there are many differences but you may observe that I have neglected a lot of "formae" also varieties and mostly only noted the species. But you will see. Wait with your drawings till you have got mine.

Then another question. You have been using Teiling's Actinotaenium /as also in your Alaska papers/ and Staurodesmus. I have as yet not been using them, not because I should not appreciate the idea of separating these new genera started by Teiling. Especially Actinotaenium seems a rather well defined group which can easily be kept as a different genus. But also here Tg's synonyms seem to me rather obscure. As far as I know Tg has never himself seen most of them (?), perhaps only very few (he has been working almost only with plankton algae, as far as I know), and he has been sitting at his writing table looking at and comparing a great many published figures, most of them bad, a few

good ones. I call this writing-table-products of the imagination. Even from the best of drawings you will not get the same "living idea" of a desmid as from studying a living or preserved cell. This accounts for many curiosities and obscurities in Tg's list of synonyms. I have tried to discuss - in a polite and friendly way - these questions with Tg who has been an old friend of mine, but it has not been possible because he gets furious instantly when you prefer an different opinion of your own which does not coincide with his ideas. That is absurd. I have given up every attempt in this way.

Then *Staurodesmus*. Also this genus may be - perhaps - a good idea. But I am not quite convinced about it. And it is more difficult to define its limits. (Tg wishes to include also all species with thickened angles: that I am not willing to do? Most *Arthrodesmi* have a very flattened body. (there are exceptions: *A. convergens* &c). You can do a good deed if you will take trouble to make *Staurodesmus* clear or clearer to me! Have you a complete list of all species, with author's names of all species that you include in this genus? and the synonyms? Because Tg's systematical review is completely impossible to use. You cannot divide a species into subspecies on account of 2-, 3-, or 4 radiate cells. It is no taxonomy. The radiation of *Staurodesmi* and perhaps also of *Staurodesmi* has no taxonomical signification, that every one must admit? The different radiations are no taxonomically useful characters (cf. Gblad & Ruzicka). What is *Stdesm. vulgaris*? What do you make with *Arthrodesm. incus*? that Tg declares for a nomen delendum (he writes "nomen desertum", but he may not be too sure) after having been in common use for more than 100 years in perhaps several 100 of publications. Should not such an old and widely used name be a nomen conservandum? better than rejected?? I have not quite recently studied the Internat. Code, so I cannot what is correct and what not - but at least in Borge's time he often said to me that both he and Nordstedt were not willing to change names that have been universally used and accepted for a very long time. - Perhaps this is not the standpoint of our days? - But would you call *Cylindrocystis Erebissonii* by the name of "*Cylindrocystis cylindrospora* Drouet & Daily"??? I hope not? I have now here above been telling you some of my views, and I hope you do not feel unpleasantly because I have criticized our friend Teiling too much. I wish not to harm him, but I must have a right to tell my opinion. Also if you cannot and will not accept them!

Now I hope you will tell me your opinions quite frankly. If you think I am in error, perhaps you can convince me? I should be glad to change my mind. I hope we shall have a very interesting and large correspondence!

Tomorrow I am going to my sister at Kuusankoski in E. Finland but I am returning to Karis to the new year. Happy X-mas! (Sorry for the work of your neighbour!)

My sincere greetings to your mother

1.2.1962

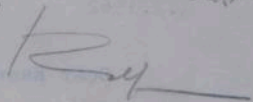
Dear Hannah,

I wish to tell you that everything has safely arrived in Karis, your letters all drawings, your comments your funny book. Many best thanks for this. Today I have been the whole day in my bed because of influenza or something that is coming. I did not sleep last night at all. But I hope this will be very soon over. When I am better I shall

write more. My best wishes so long.

I have a long letter for you but it
must be typed!

Yours,



Fernand Analla

Dear Hamel,

Your last letter received. Was?
Have my flu N°2. The latter
truth is that I never found
between the two notes, so I am
rather down at this moment. But
I am not intending to ~~write~~ write to
you a "persuade", but on our beautiful
Dresden

[I am glad to know that you got the
small Munich-book, and that you found
it funny]

Who can pretend to understand much
of the evolution theory concerning
small invertebrated organisms, a little of
"Guessewille" where everyone who
has every possibility to clean his nose, has
as the best ones. If you know the book
"Der Alpenkuck" by ^(from local sources) ~~Stoll~~ and other
nature books for students you may soon not
find that every writer has his own evolution
theory concerning these organisms - the original
being to be found only in the writers own heads. ~~That~~
is so far as I can judge a rather good and
modern book. I admit: I do not believe
we can have any ^{what} ~~of~~ ^{evolution} ~~evolution~~
any ^{what} ~~evolution~~ ^{evolution} where only few
of any ~~two~~ ^{individuals} have been preserved
during geological periods of any length. (see
Darwin etc.) Or you know Oskar's opinion of
Dresden

I gladly admit that Prof. Loewen
and Hamel are far too shy and heterogeneous

about how two can be clear the sit-
uation. There have been many attempts
to make an orderly system in both German
and French. As you will have seen, but the
system has never been good. One such instance
of substance was Wattson, and what is there re-
maining that you could use? (Quite recently
Dr. Joyner, Chadwick, has made a "phylogenetic
system" based solely upon the position of cytopro-
pores. It is only a comparatively small part of cytopro-
pore position and is not really described, and I believe
it is entirely false to state as foundation of a biological
system only one character i.e. the cytoprotopores. As
you see he will put Boys among the Urosalpinx be-
cause the pleurostoma group. I have tried to understand
where he got with his arrangements, but I have failed.
This came to me no systematical any more.

In ^{a letter of} ~~the~~ J. B. - 62 Dr. Chadwick is writ-
ing to me: "Unsere Ansichten an Substratium und
Stomatodermis nehm sich offensichtl.ich immer mehr
das Sie schreiben, der biolog. Sinne. Ich empfindet Teilung
hat zwar gute Ideen, wir sind aber taxonomisch
und nomenklatorisch fehlerhaft beherstet und
alt ganz unbrauchbar. Sie konnt offenbar nicht
einmal die einfachsten Vorschriften des Trot. Code."

I cannot write more this time. I am hanging
in bed, and I can do no work. I believe my
dentistry work must be finished for ever.

9, 3. - 62 Best greetings

Ruf

Karis Febr. 5 1962.

Dear Hannah,

This letter is written in many parts because I have been (and I am still) more or less ill, influenza? or something like it. It is nothing very serious but you know one is not quite like oneself and one's activity is nearly ± 0 .

25.I. I got your returned manuscript with corrections. Of course you are correct: it should be *processibus* (I have looked in my old old schoolgrammar that also says "artus, tribus, -cus, e.g. lacus). I know my -ubus is wrong but I forget it every time. In some curious way I got it into my head, I suppose from "lacubus"? Very bad!

Krieger and the Germans, also Fott in his "Algenkunde 1959" are using "aerophytische", "atmophytische Algen" (Fott's book has figures made by Růžička) so I thought there might be such a word in English too. I have not seen it in any dictionary!

"latius, hians - latiori, hiante". I have seen names used in their inflexible state and I like it more so! perhaps not quite correct?

Thank you very much for all corrections. As you see your help is very useful and helps me to avoid bad looking mistakes both in Latin and in English.

I had 25/1 a first letter from Arthur. He has apparently been very seriously ill and his letter is still very depressed and pessimistic. So I have written to him trying to find some consolation for his mind. But it is very difficult, one must be very careful with words and for me with my incomplete English it is not very easy. When I myself was ill a year ago I also was deeply sunken into the darkest pessimism. I really did not care for anything at all - and now I hope I am changed very much for the better.....

Your letter with comments upon my drawings arrived. There were not so many or important differences. But later more on them. A book about Russian emigrants which you sent me I have read. I think it is not very much exaggerated - they are funny people. Arthur told me he had got a book for X-mas present which was intended for you. He said it was childish, but everyone here enjoys it very much. So I bade him send it to you.

Arthur writes that he thinks he will no more be able to work on desmids and will send all his drawings to Prescott. I told him how downhearted I felt when I was in the Hospital in Ekenäs. I had lost all my hope of recovering. "Voi che entrate, lasciate ogni speranza" was my slogan. I know very well that ~~anything~~ ^{everything} can happen at any moment but I do not think of it so much anymore. Everything can happen to anyone. We will hope he will still do desmid work.

Now I am examining a collection of plankton samples from N. Zealand I do not quite agree with Thomasson in his identifications (although I like him as a pleasant and agreeable fellow). With Brook's opinions I cannot agree. He certainly is exaggerating the relations between some Staurostrum (I believe Teiling is approving of this kind of taxonomy-evolution? I am not unkind nor malevolent towards our friend Tg but our views are are so very different: "West is West and East is East and never will the two meet" I believe Kipling said?

30.1. The drawings arrived, everything in good condition. I have had only a very slight look at them and I can see that there are not very important differences in our opinions. You have seen and drawn much more Closteria and Metria than I have. / I must confess that I am very unthrifty as to the very many varieties of Metrium and Cylindrocystis and I believe that one must have a very rich material with plenty of "different" cells to be able to decide between the very doubtful varieties and species of such genera. / Here is a good example of what is emphasized in G. Glad & Ruzicka (1959), quoted from Ernst Mayr: "The first rule which cannot be emphasized often enough is that we should name populations, not individuals.." This is a rule very often neglected I can tell you a story? There is a German desmidiologist Kurt Förster (do you know him?). He is working in the following manner: he has a friend who is interested in making microscopical slides, to mount desmids in some stable media after having stained the cells. Now these slides are quite excellent (I have got some of them with Brazilian material) but there are single or very few cells of every species, and they lie steadily mounted in a fixed position. It cannot be good to make identifications from such slides, especially you cannot get any idea of the variation of species. I should say it is really dangerous to use such methods. I have also written to Mr Förster on this. / He makes good identifications and good drawings/.---

It is curious that although there are lots of desmidiologists making pure culture research, there nobody who should make it from a taxonomic point of view. And I believe much could be obtained by this method. (if only strict criticism is always following). This is of course very easy to say by one who has never tried pure culture methods, but I believe it is our only remaining way.

When we find in a very rich plankton sample a lot of nearly related Staurostrums, or we need not be talking of "relations" which is somewhat obscure and doubtful - we say Staurostrums that have a somewhat similar appearance - it is certainly thoroughly unscientific to believe and to try to make others believe that in plankton there is the very place for development of new taxa. This is the idea originating from Teiling (Bot. Congr. 1950) and followed with great fidelity by Brook. What development in Desmids really is,

what phylogeny in desmids (and other algae?) is, notwithstanding several authors have made wonderful "trees of succession", that is something entirely unknown and all those trees are phantastic inventions - nothing more. It is a fault, a scientific crime to try to make others believe that we really know something of the phylogenetic evolution among desmids. And this is the very point where Teiling and I entirely disagree. I believe I know enough about evolution in general so that Teiling is not the man to teach me. His tone in his letters to Arthur is not gentlemanlike. Even Borge, who was my first true friend and in spite of his wide knowledge does not be approved by Tg's critical "evolutionary" eyes. I who knew Borge during nearly 20 years I can't tell you that he was not only a pidgeonholing unscientific "taxonomist" but he did never boast of his knowledge but he was an most unpretentious man.

Dear Hannah, excuse me all this above, Teiling and myself we are both good friends of yours and it is perhaps unsympathetic to you to read such stuff. But I must, as far as it goes, make you see the real difference between Tg and me. It is not only Staurodesmus and Actinotaenium, it is the whole scientific look at everything. I shall try to avoid this question in the future

Now I have come to the end. This letter has been written in my bed on small sheets, so it is certainly most disjointed?

Best wishes to your mother and yourself.
I hope your health is in good state.

