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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.

Univ.-Dozent Dr. Franz Buxbaum  
Professor  
Judenburg (Austria)  
Burggasse 35

To Mr. P. C. Hutchison  
Department of Botany, Univ. of California,  
Berkeley 4, Calif. U.S.A.

Dear Colleague !

April 1, 1955

I thank You so much for your reprints and I am on the way to send you some of my recent publications. As you are not only cactologist, I send you also such ones of other botanical content. I am very glad, that you are member of the IOS now, and that also so many American Botanists now have joined to our organisation.

As you are the USA- National Delegate I enclose to this letter the report on my publications since 1951 so far they are of interest for cactologists. You know, that the II. Congress at Monaco has given the advice to all authors, to send reports on their publications to the national Delegates for publication in their national Journals.

Since a long time I am in correspondence with Mr. Reid Moran. Although we do not agree at present in all point, I think we will do it in a near future. My researches are based on the most possible exact morphological examinations and the more materials I can get for examination the better we will be able to join our opinions. This is also the case why I should like very much to obtain lecturship at America Universities as a guest-lecturer and to come into direct contact with the American Colleagues. Thus I would be able to see the American Collections and to do some field-observations. I am sure, that my reform of taxonomic research, my "dynamic method" would interest also the American colleagues. Prof. Boke of the Oklahoma University called this an "excellent idea", but he has given me the advice to ask you, if this would be possible at the California University, because the Oklahoma University is only a small one and has not the possibility to do it. He has given me also the advice to write to the American Society of Plant Taxonomists, but I regret I have not the adress of this society. Can you tell me it, or, better mediate my connection? At present I do not know, when this would be possible, because maybe I will go for three years to the university of Kabul, Afghanistan. I am not yet sure, if I will accept this lecturship, it depends of the conditions.

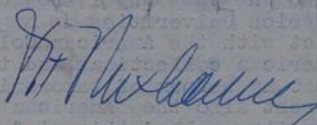
Mr. Krainz has informed me, that you have proposed for membership of the IOS also Mr. Backeberg. I want to tell you, that this is impossible. When we had the Congress at Monaco, many of the IOS-Members have declared, that they would left the room if Backeberg came in. Indeed, Backeberg knows many plants and his voyages had brought many new species. This was the case that I myself had proposed many years ago his election for Honorary member of the Deutsch Kakteen-Gesellschaft. But it is perfectly impossible to cooperate with him. You may ask Knuth, Werdermann, Baining Krainz or any other author who once has cooperated with him, and every one would tell you the same. Backeberg has published under his own name my drawings of the flowers of his *Thrixanthocereus* and *gymnanthocereus*, without any word about my help. He has published his "Haseltoia" without having analyzed it himself, when Krainz had told him his observations. As he had also had misunderstood Krainz the diagnosis of *Haseltoia* is as

wrong as this of his Neobinghamia and his "emended" diagnosis of Dolichothele, that he had taken without own researches and without having understood it correctla from Werdermanns description. Nobody can tell him anythink news, without the danger that he published it under his own name. At the other hand it is impossible to get any material from him. It is regrettable, indeed, but it is true. Whether or not he is aright with his opinion, is another point. Only exact researches can judge it. But personally it is impossible to cooperate with him. I think, nobody has tried it so many years to bring him to an exact and clean work, as I did - but it is impossible and the end was, that he became personally. I think, you will agree now with our standpoint!

Finally I want to ask you for support of my researches by sending me flowers of such Cacti that are not available in Europe, that means especially the big treelike genera. You may preserve the flowers for a fourtigh in 4 % Formaline and send them without liquid only in wet condition. In the near future I will send you a list of the genera and species that I have already examined morphologically. Perhaps you would be able to accomplish my researches by your own collections or from other American collections. Having analyzed the flowers I always send the Pollex to Dr. Kurtz for his examinations.

Hoping to hear again from you

I am, dear Colleague,  
sincerely Yours



2 June 1955

Dr. Franz Buxbaum,  
Burggasse 35,  
Judenburg, Austria

Dear Dr. Buxbaum,

Thank you for your good letter of 1 April 1955, recently received. Your reprints also arrived and I am most appreciative of them. Likewise your list of publications since 1951 will I am sure be of interest ~~of~~ to readers in this country.

With regard to the possibility of your visiting an American university to lecture I regret that here at the Univ. of California there is no budgetary provision for visiting professors in botany. Dr. Karl Rechinger (Vienna) and Dr. F. Ehrendorfer Jr. (Vienna) recently visited here on Fulbright fellowships I believe, and you might talk to them on the means of obtaining such a grant. It might also be good to visit the American Embassy in Vienna and talk to the cultural attaché. I believe that you will find it virtually impossible to visit here other than on an exchange fellowship of some sort, since none of the American universities I know of make such arrangements ordinarily, and indeed I know of no visiting foreign professors who were not on fellowships of one sort or another. If you are successful in your endeavor to obtaining supporting funds, this institution would be an excellent place for you to make your headquarters, since we have a large collection of Cactaceae, and of course the majority of the vast private collections in California are within a day's travel of here.

On advice of Mr. Krainz and others in the IOS I dropped the name of Mr. Backeberg ---- in the first place I had only inquired why he had not been nominated (along with several other dignitaries) and did not formally nominate him. I believe that nomination of members from specific countries already represented in the IOS should be left to the National Delegates and membership from those countries.

We will be glad to cooperate in any way we can in supplying material for your research. We would appreciate assurance that the source of this material would be credited and the source plants and data on them cited. Meanwhile I look forward with interest to receiving the list of materials you have already studied. In Peru and Chile I preserved considerable floral material in formalin and some of this material may be useful to you. Of Chilean genera, particularly, we should be able to supply additional floral material from my living collections now under cultivation here.

Sincerely,

P. C. Hutchison  
Senior Botanist

Univ.-Dozent Dr. Franz Buxbaum

Professor

Judenburg (Austria)

Burggasse 35

Mr. P.C. Hutchinson,  
Botanical dept. Univ. of California  
Berkeley 4, Calif.

November 24, 1955

Dear Colleague !

I thank you so much for your letter and I can tell you today, that it probably will bring a success. I was offered a scholarship for scientific researches in USA of 9,-Dollar daily grant for 6 month in 1956/57. For this purpose I need 3 letters of reference, one of which should be written from an American Botanist.

As I believe that you are very acquainted with my publications on cacti, I should like to beg you to write this letter for me.

The time of my travel probably will be September 1956 to February 1957. My travelling program contains a short stay at the Oklahoma University to see Prof. Boke, then I will go to the Desert botanical garden at Phoenix to Mr. Taylor Marshall and to the Arizona University at Tucson. According to your advice I would spend most time at the California University at Berkeley and during the winter-month at the Pomona College in southern California.

Thus, I think, I will be able to see and to forward my researches as much as possible.

Besides of this I am also very much interested for examinations of the Mariposa-Tulip-species, because neither the morphology nor the systematic (phylogenetic) position is cleared sufficiently.

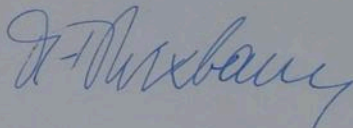
As I think that it will be of interest for the American Botanists I should like to lecture about my "Dynamic Method" of phylogenetical research.

Thus, I hope, this voyage will give me the occasion to get a close personal contact and co-operation with the American members of the I.O.S.

Please return the "letter of reference" by turning post because I must send it as soon as possible to the USA Education Commission at Vienna.

Very sincerely

Yours



Suggest  
Feb. Mar - Aug 1957

2 December 1955

Dr. Franz Buxbaum,  
Burggasse 35,  
Judenburg, Austria

Dear Dr. Buxbaum,

I enclose the recommendation for the Fulbright Scholarship, which I am pleased and honored to forward.

Permit me to most heartily recommend that you not arrive in this country in the fall, but rather in the spring so that you can count on examination and study of flowering and fruiting material of cacti. From March through September is the best time for such material. If you arrive in March you should be able to see flowering material of cacti in the desert as well as in our collections, both here at the University as well as at the other institutions you have mentioned.

I have noted in the recommendation that the U.S. Section of the IOS will meet in July, 1957 here at Berkeley in joint meeting with the biennial convention of the Cactus and Succulent Society of America. If your visit to this country can be so timed, you should plan to attend this meeting. Dr. Boke will tell you how valuable he has found the Conventions to be in establishing contacts for materials, and in addition you will find gathered here in Berkeley nearly all of the scientists in the US who are actively interested in research in succulents. Dr. Boke has already indicated that he will attend and will give two talks, and Dr. Kurtz also plans to attend. I am sure Wm. Taylor Marshall will be here if his health permits, as well as nearly all of the US members of the IOS and other visiting scientists. We would welcome one or more addresses from you and I, as Convention Chairman, and as Delegate of the U. S. Section of the I.O.S. cordially invite you to present at least one such talk (technical) to the I.O.S. and at least one talk of more general import to the Cactus & Succulent Society of America.

When you know for certain that you will be able to come here I will be glad to advise you further with regard to your forwarding a letter to the Chairman of the Department of Botany of this institution, requesting facilities for research while you are here. Please inform me on this point as soon as you can, since office and laboratory equipment is often reserved far in advance. Our collections of succulent plants, numbering over 5,000 accessions, and nearly all complete annotated with locality and habitat notes, will be available, as will my herbarium and seed and photographic records, and preserved-in-fluid material.

Very sincerely yours,

P. C. Hutchison  
Senior Botanist

My acquaintance with Dr. Buxbaum is by correspondence ~~quite~~ and by familiarity with his published research. I have a high regard for his personal integrity and productive capacity as evidenced particularly by his many papers on the anatomy of the Cactaceae and of related families of the Centrospermae, and the very considerable evidence this work has provided for an interpretation of phylogeny of the Cactaceae. Undoubtedly the provocative questions raised by Dr. Buxbaum in connection with his anatomical studies of the Cactaceae has stimulated more discussion and research into the systematics of the family than has the work of any other living botanist.

Dr. Buxbaum works at a tremendous disadvantage in studying a primarily American family of plants with only cultivated material in European collections available to him. His visit to this country would enable him to broaden the scope of his studies and by personal contact, exchange ideas, which would be as much desired by many botanists here as it is no doubt desired by him.

His planned itinerary includes stops with each of the most active research workers and institutions of pertinence. In addition it is hoped he will be able to time his visit to talk to the U. S. members of the International Organization for Study of Succulent Plants, of which he is a member and the Austrian national delegate. The United States Section of this organization will meet jointly with the Cactus and Succulent Society of America at Berkeley, California, in July, 1957 and an address to a joint meeting by Dr. Buxbaum would be considered an honor by both organizations. He would also be able to establish contacts at this meeting which would prove valuable in his research.

Paul C. Hutchison

Senior Botanist,

Dept. of Botany, Botanical Garden,  
University of California, Berkeley 4, Calif.

December 2, 1955

Dear Colleague !

January 26, 1956

To the first : I thank you so much for your excellent Reference Letter and for your advice with regard to the most favourable season for the voyage. I regret, it was not possible to postpone it for 1957 because for this year it would be doubtful if it would be possible to get a scholarship. But the US Education Commission in Austria has proposed a scholarship for me with beginning in March or April 1956!

Thus probably I will start around the end of March. My program is to stop several days at Norman to see Prof. Boke, then for about one or two weeks at Phoenix ( Tempe) and so at Tucson. From there I will go to Los Angeles and finally to Berkeley where - according to your advice - I will take Head-Quarter. There, and perhaps at the university of South California I should like to lecture a course about my "Dynamic Method of Phylogeny" for the Californian Botanists.

I do not yet know, how the voyage will be carried, whether the journeys in USA will be by railway or by Airway and how much time will be necessary for my program. But I think that I will arrive at Berkeley in May or beginning June.

As soon as I will know the details I will write you again. But presently I will be grateful to you for every advice with regard to my voyage.

very sincerely

Yours

J.F. Neuhauer

C  
O  
P  
Y

January 28, 1956

Dean M. A. Stewart  
Graduate Division  
Administration Building  
Campus

Dear Dean Stewart:

We should be pleased to accommodate Dr. Buxbaum in our Department during the spring and summer of this year. Since Dr. Buxbaum will be working on cacti that are growing in the Botanical Garden we would like him to be officially associated with Professor T. H. Goodspeed.

The Botany staff feel it would be appropriate to appoint Dr. Buxbaum a Research Associate without stipend upon his arrival.

Sincerely yours,

G. F. Papenfuss  
Graduate Adviser in Botany

GFF:aw  
cc Professor T. H. Goodspeed

5 March 1956

Prof. Franz Buxbaum,  
Sackgasse 13/114  
Judenburg, Austria,

Dear Dr. Buxbaum,

I am happy that your application for the Fulbright Fellowship has turned out so well and that shortly you will enroute to this country. Your planned itinerary as outlined in your letter of Jan. 26, ---to Norman, Oklahoma, then to Phoenix, Arizona, and finally here, sounds quite reasonable. For travel in this country you have several choices. If you go by air, travel by "air-coach" or "tourist", not first class, as the saving is considerable and the time about the same. You can also travel by train or bus, both quite fast. It will pay you to obtain a round-trip ticket from your point of entry to the U.S. to here and back to point of entry----a saving of at least 10% costs. There will be a considerable saving, again, if you take the train and can manage to travel "tourist" rate without a bed for night travel. The trains are quite comfortable. I am attaching a schedule of prices which I have obtained this morning from the various travel companies for a routing from New York to Norman, Oklahoma, to Phoenix, to Berkeley and return to New York.

I am informed that you will probably share my office facilities here in Life Science Building, which will be a great pleasure for me. There is ample space and my reference library on Cactaceae is here in the office. I am sure that such equipment as you may need can readily be supplied. However I am doubtful that there will be opportunity for you to give a course of lectures, since in June the vacation period begins here, and there will be no students about. This matter you can discuss with the department chairman on your arrival.

It would be a good idea for you to let me know your planned arrival and departure (approximate) at Norman and Phoenix. It is possible, although not sure yet, that I ~~am~~ may be able to meet you at Phoenix and drive you to Berkeley from there----a two day trip.

Since I last wrote to you it has become possible to plan another South American trip to Peru and Chile, and perhaps also Bolivia and Argentina. I am expecting to leave on this trip sometime between June and September, and of course my primary purpose will be study of S. American Cactaceae. .

If I can be of further aid to you, kindly let me know.

Very sincerely yours,

Paul C. Hutchison  
Senior Botanist

Your routing by train, bus or air will be New York to Oklahoma City  
Oklahoma City to Phoenix, Phoenix to Oakland or San Francisco if by air or  
to Berkeley if by bus or train. Dr. Boke can undoubtedly arrange to meet  
you in Oklahoma City which is 17 miles from Norman.

Air travel is about twice as expensive as train and I find only two air-lines  
which can route this way

TWA -  
American Air Lines. Costs on the latter are

\$359.26 air coach  
416.41 first class

By train you would take Southern Pacific. Costs are:

\$287.68 First class with lower berth  
156.64 by chair

The above quoted prices are round trip from New York to Berkeley and return  
to New York.

If you decide on train travel, which is what I recommend, you can leave  
New York at 6:15 PM to arrive in ~~Oklahoma City~~ ~~St. Louis, Missouri,~~  
about 18 hours later at 1:30 PM the next day. At 7 PM that evening you can  
take a train from the same station to Oklahoma City which will arrive a 8 AM  
the next morning. Dr. Boke could arrange your schedule from there to Phoenix  
and Mr. Marshall can arrange your schedule from Phoenix to Berkeley. I will  
meet you when you arrive in Berkeley.

Should you prefer to travel by bus there will undoubtedly be an additional  
saving in expenses, but it will take you longer. Both on trains and buses  
you will have to buy your meals, which can add to the costs considerably if  
you are very many days traveling. However as you will be only 48 hours  
enroute from New York to Oklahoma City (and Norman) and then 24 hours from  
there to Phoenix, and about 24 hours from Phoenix to Berkeley, I recommend  
train travel as your best and least expensive means of travel. If you wish  
I will check into the buses and see what I can find about their schedules  
and costs.

I find, further, that if you bought one-way tickets from New York to Phoenix,  
and I met you there, then drove you to Berkeley, and you then bought a one-way  
ticket from Berkeley to New York, that the total cost to you would be \$172.77  
or more than if you buy a round-trip ticket from New York at the beginning.  
So I definitely recommend that you buy a round-trip ticket from New York and  
return to New York, which will save you 25% in the long run.

Can I help you in any way in locating lodgings here in Berkeley?

To Mr. P. C. Hutchison,  
Dept. of Botany, California University  
Berkeley 4, Calif.

Dear Colleague,

March 31, 1956

As You see , I am already in New York and I had already very interesting and important speeches with Mr. Alexander. Monday April 2nd I will start first for Washington DC, where I will stay until April 5, going than to St. Louis Miss. . Monday April 9, I probably will come to Norman Okl. and start for Phoenix probably at April 11. , where I will stay about a week, may be longer. I thank you so much for Your kind advices . It really would be nice to see You already at Phoenix. But I got the ticket for the whole voyage from the Fulbright Commission and it was possible to get it from Phoenix over Tucson and Los Angeles to Berkeley withouth any expenses for me. As I will also stop at Tucson, I will arrive at Berkeley around the 25 th. The exact date I will tell you as soon as I can state it. Presently I do not yet know how long time I will need for My researches in the desert neither I know exactly how much time is necessary for the Railroad.

For every case I beg you to reseravate a lodging in the International House at Berkeley and to write me to the Desert Botanical Garden at Phoenix, if it was possible to get it.

Very glad to make soon your acquaintance,

Very sincerely

Yours

J. F. Schubert

5 April 1956

Dear Dr. Buxbaum,

I have reserved a guest room for you at International House for 5 days beginning April 25. This is a private room with bath and costs \$4.00 a day. Permanent residence for you at International House will have to be discussed and decided upon when you arrive----there are at present no single rooms available and probably will be none until about the middle of June. If by any chance you plan to arrive in Berkeley on some later date please inform me soon so that I can change the date of your reservation and save you the money involved.

It will be good to have you here---our cacti are now beginning to flower very heavily starting now with Chilean, Peruvian and Mexican material.

There will be a number of ~~ix~~ alternatives to lodging in International House which we can discuss when you arrive. But be sure to inform me immediately when you know your day of arrival here so I can confirm or change the reservation date at I. House.

Looking forward to meeting you with considerable pleasure,

I remain,

Very sincerely yours,

*Paul C. Hutchison*  
Paul C. Hutchison  
Senior Botanist

Papago Park, Tempe, April 21, 1946

Dear Mr. Hutchinson,

I have again changed  
somewhat my project and will take the route  
to' Line. Thus I will go over Grand Canyon  
Boulevard - Fresno and will arrive at  
Nubley one day later at April 26, at (I be-  
lieve!) 11<sup>50</sup> noon.

Please change thus my booking  
for the lodging!

Very sincerely yours

A. M. S. B. A. M.

Dear Mr. Hutchinson,

as I could not change my ticket  
from Southern Pacific to the T.O. line  
I have changed my project again and  
will arrive in Berkeley coming from  
Los Angeles at April 26 at  
6:22 pm

Sincerely yours

H. A. Gentry

FOLD SIDES OVER AND THEN FOLD BOTTOM UP  
MOISTEN FLAP WELL AND APPLY PRESSURE TO SEAL  
NO OTHER ENVELOPE SHOULD BE USED

Dear Paul,

Oct. 2, 1956

again in Vienna, I am hurrying to write you. I have been here in the Fulbright Commission and have spoken about your plan to go to Greece. The result is the following:

- 1.) The Fulbright Comm. is not competent for you.
- 2.) You must do a request to: International Institute of Education, 1 East 67 St. New York 21, N.Y. immediately, because the termin for 1957 was October 1, 1956. Maybe, you can be accepted at U. von Thun this institution you would get a full grant.
- 3.) You must have photo copies (microfiches) of all your University-Studies.
- 4.) For Austrian University you must have the BSc (corresponding our Certificate of Maturity).
- 5.) Your studies at American Universities will be counted partly only, but certainly in 2 years you can get the Austrian Dr. phil.

I hope this information will be helpful for you (I hope, you can read it).

Sorry, that you did not join our "J.O.S.-Team." Krausz will agree with us. I have informed him about our discussions.

With the best greetings to the whole "Antonie-Corden" ~ Sincerely yours, *Paul*

Dear Paul,

October 8, 1956.

I hope you have got my letter from Vienna and I hope that you may have been able to read my fine writing. The information about your voyage from the Fulbright Commission in Vienna will be followed by the information from the University next week, when I will have begun my lectures at Graz.

Now I am again at home and soon you will get my Bibliography.

But now I must ask you to send me my original drawings of my Morphology as soon as possible. For, I must continue the morphological part of "Die Kakteen" very urgently and I will use for this the original drawings of the monography.

With regard to the *Platyopuntia* with the terminal flower, I can tell you, that such a flower (abnormally) is illustrated in "Kakteen und andere Sukkulente" (DKG) 1937, issue 6 p. 89 (Opt. *Lemaireana* Web.). This has exactly the same shape.

My voyage has been very agreeable and in Zurich and Vienna I have given a talk in the two Societies. And in Vienna I found a fine job for you. The Vienna cactus Society will arrange an expedition to Chile etc. in about 2 years and has asked me to join. I think I might be too old for such an expedition and have advised the society to ask you for this expedition.

I think today my writing is somewhat confused. I am very much hurrying because it is highest time to start.

My best greetings to all of the Garden and Department.

Very truly Yours



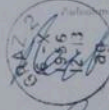
Graz, Steiermark, Mur u. Schlossberg

64-1871  
Karl Müller  
Verlag



Mr.

Paul C. Hutchins  
Dept. of Botany  
Univ. of California  
Berkeley 4 Cal  
U.S.A.



Greetings  
from Cross!  
Yours  
Nels

Postkarte und Verlag: Franz Knechtel, G.m.b.H.

Echte Photographie

# KAKTEENSPEZIALKULTUREN

IMPORT-EXPORT

SAMEN-EN GROS

## O. SCHMID-ZOHNER

WIEN XXII./148 ASPERNSTRASSE 119

TELEPHON F 10 7 31 R / POSTSPARKASSENKONTO 169.934

Vienna, Oktober 16, 1956

Mr. Paul Hutchison,  
Botanical Gardens  
University of California,  
Berkeley 4/ Calif.

Dear Sir!

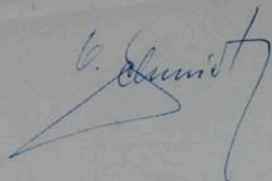
I received your adress Prof. Buxbaum, who was telling about his America-Trip in the "Gesellschaft österr. Kakteenfreunde" in Vienna. He told, that he everywhere found a hearty welcome, and that he was acquainted of many special collections. Because I love the Echinocereen, and also want to gather all these kinds, I asked Mr. Prof. Buxbaum, where I could get the missing kinds perhaps. So Mr. Prof. Buxbaum advised me, to ask you.

Till now I was in conjunction with the Pa. Schwarz and Wagner, but they also were not able to send me all kinds. In me collection now I have 80 kinds of Echinocereen, and now it will be allwith difficulter, to get the missing. Now I allow me to ask you, if you can send me some of these missing kinds. If little plants are not to be getting, I also want to have seeds. Solvency is net difficult from here. If you cannot send me something, so I ask you, if you will be able to send me the adress of Echinocereen-collectioners or Cactus gardens.

Now I ask you to excuse me bad English.

Readily I get answer and thank yuc previously.

Respectfully yours



And now the missing Echinocereus:

subinermis, pacificus, carneacanthus v. carnosus, barcena, adustus,  
ctenoides, rubescens, pectinatus v. tamaulipense, labretianus,  
Weinbergii, Baileyi v. brunispinus, - v. flavispinus, - v. oklahomensis,  
- v. roseispinus, hempelii, maritimus, pensilis, arizonicus, Bartolo-  
wanaus, Bolivianus, Boyce-thompsonii, Bristolii, caudicaas, Fobeanus,  
grandis, Jacobii, Haversmansii, Hildmannii, montereyensis, perbellus  
pfeifferii, platiacanthus, previspinus, roseanus, rueggii, schwarzii,  
spialberbis, staudleyii, stoloniferus, texensis, uspenskii, cermi-  
nosus, ferreirae, Gavisii.

16 Oct. 1956

Dear Buxy,

Thanks for both yours letters. I had looked into the potentialities of various fellowships and decided that only a Fulbright would do, but now I believe I shall wait one more year. There is much advantage in this---first in improving my knowledge of German, second, in being able to do much more writing. If, by chance, I decide to go ahead and go to Graz in 1957, then I shall have to do so without a scholarship---but I do not want to apply for one and win it then have to refuse it.

I have finally shipped all your belongings to you except the drawings and these will go shortly, as per your advice in your letter of 8 Oct.

Today finished Icones 3 and 4 and mailed them, together with "Comments on the Phylogeny of *Werckleocereus* and Its Allies." These for the Nov. journal. Haselton advises me, now, that he will not reprint these Icones, so I will have to search for another publisher, for reprinting them in serial or book form.

*Arequipa aurantiaca*, the Blossfeld plant, has produced flowers with wool rings-----. On close examination of those I pressed several years back (4 different times) I found incomplete wool rings. But our original drawing shows no wool ring. In this species the character is apparently phenotypically variable. What happens is that the primary stamens, which are coalescent at their bases, are in two series, and the inner series of these may abort several mm. above where they become free and then produce hairs all over the place. Myron and I have decided to call them *Borricactus* and our new species will be *Borricactus westii*. It, incidentally, is a *Matucana* with practically no hairs at all on the tube, very close to *A. aurantiaca*.

The proposal of the Vienna cactus society may be well-timed. Possibly I could leave here and go direct to Chile (there will be a Chilean expedition in Oct. 1957 from here) and return from Chile direct to Austria. I would have to complete my assignment for Dr. Goodspeed, resign from his expedition, then go ahead on my own for the Austrian Society.

Your bibliography: your latin diagnoses for the *Cere*. Don't forget them. I have looked over the manuscript and there will be much work to it to prepare it for the printers. I turned over your *Calochortus* paper to Dr. Mason and have heard nothing further from him. I haven't had a chance to look at the IOS paper but have one more Icones to do, then can turn to it. I will send it to you first for comment, and signature if you approve.

We very much miss you.

As ever,

Univ.-Dozent Dr. Franz Suxbaum  
Professor  
Judenburg (Austria)  
Sackgasse 13/1/4

Dear Paul,

October 22, 1956

thank you for your letter ~~3300/~~ of Oct, 16 and for the mailing of my many items. One parcel was already at the Botanical Institut when I came here last week.

Now I am hurrying to answer to hinder a mistake. According to your description of the hairs in the *Arequipa aurantiaca*-flower it is never a *Borzicactus*. For: the hairs in *Borzicactus* and *Bolivocereus* are true hairs, developing from the axial tissue. In *Denmoza* they are staminodes. Your description of the hairs of *A. aurantiaca* shows doubtless that this flower approaches to *Denmoza*, but not to *Borzicactus*. This is true, also, for the position of the flowers of *A. aurantiaca*. You better might join it (as a primitive subgenus) to *Denmoza*. Or even (????) to..... No, that would be fancy. I see the relationship in this group in the following: The tendency of development of staminodes is represented in *Denmoza* - hairs. In *Morawetzia* - the scale-like staminodes, in "*Arequipa*" *aurantiaca* - occasional staminode-hairs and (do you sit well? Than look): in *Cephalocereus polyanthus* - scales like these in "*Morawetzia*". *Morawetzia* probably is very near to *Oreocereus*, that is clear. Such it seems that there is a branch of evolution with this tendency to reduce the primary stamens which has again a division into a columnar and ~~into~~ a short-columnar (but massive) branch. The highest degree of this branch might be - at the one side: *Morawetzia* and far in the east: *Cephalocereus* (*Polyanthocereus*) *polyanthus*. At the short branch *A. aurantiaca* and *Denmoza*.

At the other side, I believe, that there might be also an eastern branch of the *Borzicactus* line (with true hairs) represented by *Zehntnerella*. Phantastic idea, is it not? I am sure, that Werdermann has made a mistake when he has united *Zehntnerella squamulosa* and *Cereus squamosus* Guercke. Or, if perhaps these might be really identical, than the position to *leocereus* might be wrong. For, *Zehntnerella* has wool at the base of the throat.

A Kingdom for flowers of *Polyanthocereus* and of *Zehntnerella squamulosa* Br. & R. And for seeds of these.

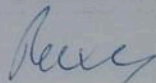
As to your PHD I have asked the Dean of the PH Faculty. It will be easy to help you, but it is necessary that you send me all your details on your studies in USA. But I would regret it very much if you would not come in 1957.

The Latin Diagnoses are in elaboration. I got my heavy luggage not before two days ago. Therefore I could not correct them earlier. In a few days you will get it. As to the names of the subtribes please correct the ending instead of *-inae* write only *-inae*. That's a new rule of the Nomenclature Commission. The other endings are correct. I regret it very much that you will have so much trouble with this manuscript. It would have been much easier, if we could do it together. Sorry!

Would you be able to co-operate in publishing articles in our new paper (connected German, Switzerland and Austrian Societies)? I willingly would translate it for you. Dr. Lindsay, also, has promised to send articles.

I myself, also, am thinking very often on our nice "Botanical Garden-Society". It was a beautiful time! Tell to all my best greetings!

Ever Yours



29 Oct. 1956

Dear Buxy,

Your letter of Oct. 22 is at hand. I hope we don't usually have a two week interim in our letters. I have at hand now a list of all the courses I have taken but must look up an old catalog to get you a description to go with each course number. When this is done I will forward the outline of work I have done.

I don't know what you said at Chicago, but evidently it has taken some effect. MacBride called today to say that Field Museum was interested in helping to finance some plates. A definite commitment and details have to be worked out now.

As far as Myron and myself are concerned, the structure of Arequipa aurantiaca is exactly like that of Bolvicereus, of which we have material in fluid and pressed, as you know.

We are still playing with phylogeny of this group but have a fairly clear and internally consistent scheme now, based much on the "pie-shaped" arrangement of the theoretical chart you presented in your seminar here in an attempt to show how convergent evolution may occur with end-products now having some characters in common, but having others. I enclose a rough sketch of our present changes in this scheme, but note, please, that we change this almost daily. We are aware of the differences from your concepts, and perhaps we will come around to your view, but for the present we do not accept it. Myron still wants Bolvicereus over on the Cleistocactus side of the picture, but I cannot accept this as of now. There is still so very much data missing.

All of your drawings I am shipping to you tomorrow.

I would appreciate it if you would not discuss our ideas of phylogeny of the Loxanthocerei with those working on this group in Europe, at least not at the present. We have lots of data but need much more before any of this information is broadcast.

I hope you have a copy of the Calochortus article. I very much urge that you reconsider the embryology section of it and at least consult with embryologists of stature in Europe before going ahead with publication in its present form. The present reactions here, including Foster's, are quite negative, and while I don't know myself, I feel certain that you might do yourself some harm if it is published and the people here are correct. What is involved is an entirely different philosophical approach to phylogeny-----what is considered here as making the facts fit a theory. They may be wrong, but if there is any chance that they are correct, then in its present form that paper will not reflect to your credit. Upon your neck be it!

Huhny for Hungary! And Poland! Yes I'll do my best to get a manuscript together for the new society journal over there. If you are still willing, I am anxious to supply you with seed for drawings of embryos of the Loxanthocerei. Interested? The resultant material could be published at the same time as our paper, but under your authorship.

Dr. Wheeler has given me seeds of Abies venusta, Pinus Balfouriana and Pinus monophylla, together with some cones. These I will send immediately in plastic---they must be sown soon.

AS EVER,

P.

from Buxbaum

Dear Paul,

November 13, 1956

I thank you very much for your mailing of my items as well as for the seeds and for the interesting letter of Oct. 29. Please tell Dr. Wheeler my thanks and best greetings. I intended to answer you immediately but just in these days my wife got an inflammation of veins and thus I had to do all housework and did not find the time to answer.

But now: there are the Latin diagnoses. Finally! It was quite a hard work. Please look for the newly corrected endings of the subtribe-names (inae instead inae!) and for Type-genus of the subtribe Coryphanthinae you must write *Coryphantha Lemaire* emend. F. Buxbaum. I hope that the printing really will start at January, because Backeberg's book really will start in early 1957 and it would be a pity, if perhaps he would bring there a "new system" with diagnoses, before mine.

To the *Loxanthocerei*! I am glad, that the scheme of my dynamic method has been of such use for you. As to your scheme of the *Lox*, it is not possible to give a judgement before having studied it very carefully and in connection with my former researches. That's clear that I will not speak to anybody before the whole question is settled. For the moment there are some questions which I cannot clear yet: *Clistanthocereus*. Binning has written to me, that it has a hair-ring in the receptacle. But what kind? *Borzicactus*-type (axial trichomes) or *Dennoza*-type (staminodes)? As to ~~Frax~~ *Bolivocereus* which I have examined very exactly, this has the *Borzicactus*-type. Exactly. Therefore your scheme is correct.

The second question: The inner hair of *Arequipa aurantiaca*. Please send me a flower of it, preserved in any way, even dried. First I must see myself which kind of hair-ring there are. According to your first description it seemed to indicate staminodes, but if really "the structure is exactly ~~like~~ like that of *Bolivocereus*, than the hairs would be trichomes and no staminodes. I will do the examination for you because I have more practice in these examinations. If you have no materials, please send me a copy of your drawings. But there is the great question: Does *Arequipa* really belong to the type species of *Arequipa*? I am not sure now, the more, as already the plant itself seemed to me to be quite different from all *Aequipas* I knew before. (See Oehme's publication in the German journal. Indeed: send me seeds of all kind of *Loxanthocerei*. I will see whether or not they can give any leading characteristic/ more.

As to my *Calochortus*. It is a fine idea! I will send the whole manuscript to Fagerlind. This man is doubtless the first embryology of today's Europe and specialized just for the questions on tetrasporic embryo-sacs. I am absolutely sure, that I am aright. Nevertheless: Fagerlind's answer will give a heavy weight at my side. - I will tell you the difference between the US colleagues and myself.

At UC as well as in most parts of the world my "dynamic method" is yet unknown. (Therefore, indeed, it will really be necessary to write an English edition of my book.) The way, you are going in USA - more than anywhere in the world - is absolutely static. This is caused by the technical facilities you have. Therefore the purely technical part of the research excellently developed, while the mental scientific part is quite lowly used and estimated. But the dynamics of evolution cannot be examined by experiments; they only can be found by conclusions, which are founded on a profound knowledge of facts. Otherwise it must enforcedly lead to an error, if the problem becomes more complicated. It is not possible, indeed, to get all conclusions in phylogeny "foolproof", because they need a higher degree of mental science. - I am not "making the facts fit a theory" but

I am - if you want to say so: "making fit the facts together". That's a

B-41-56  
To PCH

difference ! There is not anything of "theory" in my non-embryological part, but it is based on an absolute knowledge of the laws of evolution of the Liliaceae, a much more detailed, indeed, than anybody else has. I am not a " ~~the~~ Tabernist of Embryology" , indeed, that means, I did not make millions of slides and their descriptions. That's handwork - not science. Schopenhauer once has written : " In science it is not ~~essen~~ essential to see anything that nobody has seen before, but, seeing anything that everybody has already seen, to think what nobody has thought yet."

Nature very rarely is foolproof, indeed ! See : the Lobivleae, even the Loxanthocerei and all the Cereoideae at all ! I am very glad, that you are already going the scientific way ! For: the most interesting problems, the problems of the vegetable kingdom ( over the species ) need an higher approach of thinking, based - indeed - on most exact technical work. I think, you will understand what I mean.

I am sorry, the Hurrays" for Hungary have been much to early. It is not to be described, what cruelties, what a massacre is happening there now !

What are doing your german studies ? As soon as I will have got your dates I will speak with the Dean.

Anything more : I did not meet Dr. Just in the Field Museum, as he was in Holydays. But he had informed his Assistant Dr. Steiermark and therefore it was possible to speak about the Icones. I am glad, that it was successful.

In the next time I will send you the pictures of our party of the ~~Yas~~ last day. They are very nice !

Now : best greetings to all of the Botanic garden !

Always Yours

from PCT

19 Nov. 1956

Dear Buxl.,

I am enclosing your entire manuscript on revision of Ceroideae, including that portion of which I spent 8 hours typing up today. The task you have presented to me is totally impossible. I disliked the organization and presentation, which while it conformed to the Code of Botanical Nomenclature in most details, was to my way of thinking very disorganized from a readers standpoint. I feel it is incumbent on us, as research workers presenting our ideas, to present them in a form which conforms to highest standards of practice and at the same time ~~incumbent~~ makes possible a clear visualization, to the reader as he progresses through the article. Your article is broken up so that content of the subdivisions is not clear until one sees the summary at the end. The summary should be an appendix--- a means of quick review of your article, not an essential part of it. There are inconsistencies in spelling which I cannot clarify without referal. Style fluctuates greatly. There is no consistency in punctuation. The type-setter who would set up the article has no indication of how to treat the various categories of names, or how the article should look in print. There are many unsolved typographic problems, looked at from that standpoint. I simply cannot give one or two weeks of time to rewriting your article. I suggest therefore that you arrange to do this and then send me a final manuscript, which I would be glad to correct and make suggestions on. In its present form I would not think of forwarding it to an editor myself.

In the portion which I have typed up for you you will see incorporated into the manuscript my interpretation of what you have presented----as to style, typographic instructions, etc. In particular I attempted to incorporate as many of the long, discursive footnotes into the body of the article as I could, and to number footnotes with elevated numbers as this: Buxbaum<sup>3</sup> versus putting bibliographic citations in parenthesis Buxbaum (3). You can eliminate the bibliographic citations by making them all footnotes, but I personally feel this is not desirable. For such an important article a bibliography of the most important and pertinent books and articles should be included. Sometimes your meaning was not clear to me and I had to guess. You can judge how often I was correct!

Some comments: Britt. & Rose, not Br. & R. (the latter not legal); tubular, not "tubulate"---the latter, no such word; I don't believe you need a category "Introduction"---it is quite clear without it; I added included genera to each subdivision. For all new combinations, the first time I spelled you name in full; at all other times I abbreviated all names. I added citation of the type genus to the English version in all cases---simpler for the reader and costs next to nothing in typesetting. Thus the English and Latin are equivalent. Your citation of synonyms gives no indication of their original category---I would consider this before leaving them in this form. In particular your short-hand method for citing Backeberg's names, as for example, *Hylocereineae-Nyctohylocerei*, suggests this was a compound name, which it was not. Citation of an author not covered by your introductory comments (these were K. Schum., Britt. & Rose, Berg., Backeb.) such as Lemaire, should be complete, not just a name, but also the reference. Etc., etc.

For publication of the first installment Jan. 20 the manuscript must be in Haselton's hands by Dec. 15. I would have to read proof for you. The type-setter must have an original, not a carbon copy.

Because of the closeness of the time I suggest that you send me the first half of this article, up to the Echinocereidinae, as soon as it is ready. Mr. Haselton may be able to include that much in the January journal, but certainly no more than that as that issue of the journal already has much technical material, including an article by Kimmach and myself on "Comments on the Phylogeny of *Werckleocereus* and Its Allies". Because of its pertinence to this revision of yours I am enclosing a manuscript copy of that article for your study. It will appear in the Jan.-Feb. issue of the American Cact. Jour. (Cact. Succ. Journ. Amer. 29 (1):\_\_\_\_, 1957.

It is a terrible shame that we could not have completed this article of your while you were here. As I see it now I dare not rewrite it for fear that I may mis-interpret your meaning. This is particularly possible because I do not always agree with your conclusions----this being, really, a most healthy situation I think.

With regard to the *Denmoza* "staminodes" if all those hairs are really aborted Stamens, do you realize that the flower could not have contained them all if they were stamens. Is it not possible that structures homologous to staminodes can occur, which nevertheless are not equivalent.

Myron and I both believe that *Arequipa aurantiaca* is derived from *Borzicactus*, and that *Arequipa* may be from a different line. So far our evidence is incomplete----too much so for a final conclusion. We have studied and drawn many of the seed and do not find convincing support from seed for any hypothesis. Last week *Peruvocereus rubrispinus* flowered and it has a *Borzicactus* flower (red) so similar to others that I cannot see how they can be kept distinct. My remembrance of the white *Haageocereus* flowers, such as *H. acanthus*, is that they were *Trichocereoid*---very much so. I fear that no final division of these "genera" can ever be made. There seems to be complete intergrading, as in *Trichocereus-Echinopsis-Lobivia*, etc. Anyway we will send you our studies and some seed, etc., before we publish. Exception: those of the *Icones* which I publish will be under new combinations, for example, *Borzicactus aurantiacus*, comb. nov., and *Borzicactus westii*, sp. nov., instead of *Arequipa*.

I think you underestimate the status of taxonomy and research in general here, and don't believe you are qualified to judge it---not because you haven't the ability to do so, but because you haven't the awareness of the literature. For example, see Ledyard Stebbins "Evolution in Plants". See the periodical "Evolution." See U.S. periodicals publishing taxonomic and other studies, such as *Amer. Jour. Bot.*, *Bull. Torr. Bot. Club*, *Madroño*, etc., etc. You no doubt have a point in that there are several kinds of investigators---one who accumulates facts, and the other that uses them. Rarely, a person is both. If he is both, generally, nowadays, he is weak in one or the other or both. Such the danger of this age of specialization. Nowadays much of the type of work which you do is done here by specialists in, for example, Genetics. Goodspeed's "*The Genus ~~Nicotiana~~ Nicotiana*" or Bagcock's "*The Genus Crepis*" are not exactly "static" works. There are many others. Your statement that your "Dynamic Method" was unknown here at UC is a good indicator of what I mean by your lack of knowledge of what goes on here. I believe I told you that I was taught the principles of a dynamic approach to problems of phylogeny here in 1947, by Stebbins in his course on Evolution. The bibliography in Stebbins book, by authors from over the world, would probably surprise you, since many of those articles use dynamic approaches which you are apparently not yet aware of. The difference is in the conservatism with which they draw conclusions from their results. Theorists here are not conservative without reason, a fact which you do not seem to have investigated. An entire philosophy is involved in such conservatism, yet tremendous strides in the knowledge of mechanisms of evolutionary advance are being made. Again, the chart which I sent you of the *Loxanthocerei*---a modification of your technic, was first suggested by Berger, and was known to me from Berger's book. If there is something really new

in your approach to phylogeny in Cactaceae, I have yet to find it. You have come up with new facts and used them in a way now traditional here, but apparently under the impression that you are using them in an entirely new way. Hence your great surprise that I was gathering similar facts was even more of a surprise to me. Again your and my methodology differs in that you extrapolate from given or acquired facts much sooner than I will. Perhaps after I have had 25 years or more of experience in handling such data I will react as you do, but I cannot do so now. And hence my great curiosity as to the mechanistic aspect of your thinking. It is perhaps this, more than any other factor, which makes me want to study with you at Graz. To me the phenomena of your development and present thinking is that you have apparently acquired your present perspective of evolutionary mechanisms without the recourse to the training which I have been subjected to, and even without awareness that such training was available. The end results are about equivalent, but the application differs insofar as your and my personality differ.

Embryology.-----There are laws of evolution in Embryology too. I pointed out that your available evidence on phylogeny of the genus *Calochortus* allowed of at least two and possibly three conclusions as to their origin and as to the probable sequence of events. Dr. Cave pointed out, as an embryologist that from her standpoint the evidence of embryology favored one of these rather than the other two. Your conclusion is not supported by embryological evidence, but rather, primarily, by evidence from morphology and distribution. As such it is a tenuous conclusion, involving a reversal of known mechanisms of evolutionary advance, embryologically, in both animal and plant kingdoms. And that means evidence of all living things. I cannot believe, personally, that the available evidence admits of only one conclusion, and I find your fixity on this notion highly suspicious. On the other hand I think your conclusion highly ingenious. What I object to is your finality in its statement. In the past you have been equally final and have had to change your opinion. In the nature of our work this seems inevitable. Therefore why bother with such finality! All evidence is never acquired, and in the final analysis it is only necessary to suggest new ideas. They stand or fall on subsequent investigations. It is far better to have suggested a possibility ~~to be tested experimentally~~ and later verify it or have others verify it, than to propose a new idea as if it were definite and final and then be shown to be wrong. The former is the approach of men like Backeberg; the latter the approach standardized by experience of thousands of scientists.

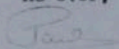
I have completed the typing of my outline of college work---and this I will forward together with a bibliography, outline of work in progress, and brief personal history. This is all confidential information to be used only for assessment by your graduate school Dean in determining my qualifications for matriculation at Graz. I will want to know what will be expected of me there in terms of course work and examinations and time to acquire a PhD in Botany. I might say that although I have had nearly 100 college units of science that much of it is long forgotten! At this stage it classes as "exposure", not as a background of remembered facts. Also costs to me for matriculation there.

Dr. Goodspeed wants me to go to Peru in Oct. 1957 to collect "All major subdivisions of the Cactaceae", which means covering the entire country. Do you think I should do this or come to Graz. If I stay here an additional year I can get in the expedition and get that much more work done here before leaving. Also Santa Barbara Botanical Garden has asked me to apply for their Directorship!

About your article again----to insert and correct as necessary would take me about 3 or 4 weeks 8 hours a day, and I would have to look up most of your names in synonymy to insert the category for each name. As I am not an author, this certainly is not my responsibility, and in any case I could never justify giving that much of University time to rewriting your article. You are quite

capable of this. I can correct your English and insert typographical instructions, the former only when I know your meaning and intent. I realize that procedure in preparation of an article for print in Europe may be different from here, but if for publication here, it must meet the standards here. Labor being so expensive no editor would devote the time to preparation in such detail of the article for the typesetter. Be sure you mail me the origin, not a carbon copy, as no editor will accept a carbon. I urge that you rush this if you want the first installment in January.

As ever,

  
Paul

PCI

5 Dec. 1956

Dr. Franz Buxbaum,  
Burgasse 35,  
Judenburg, Austria

Dear Buxl,

From O. Schmid I have a letter requesting Echinocereus. He was referred here by you. Of those he lists as desiring, we have E. pacificus, E. bailevi, E. maritimus, E. ~~maxim~~ bartelowanus, E. boyce-thomsonii, E. grandis, E. stoloniferus, E. ferreirae and E. davisii. However I cannot send plants to him, as a private-collector and dealer. I would be glad to send them to you or to an IOS collection in Europe, and he could then obtain propagations. What do you suggest. When you next see Herr Schmid will you inform him of this, please. Our only distributions to individuals and commercial firms are by exchange, and we do not wish, at this time, to expand our exchange program. Its already to big!

The French Journal surely messed up your Cleistocactus article. I think you will find that "Eucleistocactus" is an illegal name, if you check the Code.

Four ICONES are now published, and four more will be published in January. It looks like your "conception" will be realized, if this keeps up. I hope to publish about 24 of them in 1957.

You suggested that I forward an article for the new German-Swiss-Austrian periodical. What subject do you suggest? I don't know what the readers over there like. A purely technical article, or what?

Someone ought to write to Marnier and tell him that whoever edits the French Journal is doing a very poor job, which is a shame, considering the expense they go to on Beautiful plates, paper, etc. In #51 there are so many errors that it is absolutely ridiculous. I note that the beautiful drawing of Bally (the second drawing) isn't even labeled to species. I note your type of a Cleistocactus comes from Little Namaqualand. And in the Conophytum article in the title appears the name Cleistocactus. Heavens! Many more, too.

Also a whole series of comb. nov. nuda and sp. nov. nuda. Editorial policy should preclude acceptance of illegitimate names for inclusion in the magazine. Roux's article is full of them!

Hope you weren't too shocked by my reaction to the Cereanae article. But I just cant spend that much time on it. It looks like I may definitely go to Peru in 1957, and may extend to include the altiplano of Bolivia, and northern Chile. We miss you---everytime anyone says "Fantastic" around here, we all look at each other, laugh, then wish you were here.

As ever,

  
P.

Univ.-Dozent Dr. Franz Buxbaum  
Professor  
Judenburg (Austria)  
Sackgasse 13/14

Dear Paul,

Dec. 9, 1956

I hope you have got the manuscript at right time. The continuation will go by regular mail because air mail is too expensive. I will forward it in a few days.

Now to your studies at Graz. This week I was at the Dean as well as by Prof. Dr. Widder the department-chief for taxonomy. There will not be any difficulty for you, but your list of your college-work is not sufficient to imagine the development of your college work, because in Austria the valuation with points is unknown. Therefore I need the following from you: 1.) A "curriculum vitae". This must contain all your studies indicating the years when and the college where you have done the studies and your rank or class. In Austria we know the rank "ordentlicher Hörer" that means as much as "regular student" (I think this might be the best translation). For becoming "ordentlicher Hörer" in Austria the "Certificate of Maturity" is necessary, which in rank is equal to the bachelor in your country. For foreigners or even exceptionally for inhabitants which have not the Certificate of Maturity, nevertheless it is possible to become Ordentlicher Hörer if the reference of equal qualities can be brought. This is the case e.g. with your scientific publications. If you want to have more exact informations on this, maybe Mr. Bloß can tell you, as in Germany the Universities have the same organisation. Or, better, ask Prof. Dr. Albert A. Ehrenzweig, Prof. of Law at UB UC. He is an Austrian Professor and was former at the Vienna University. Probably he can tell you at best how to do it. To the 2.) I need reprints of all your publications. These will indicate to Widder your qualification.

Now to the question: Peru or Graz. That's a difficult question, indeed, perhaps you could begin at Graz in the Summer-semester 1958? I think, it would really be of great value, even for you, to continue your researches in South America. Perhaps it would be possible to join this expedition with the planned Austrian Expedition to Chile?

In every case: forward your studies in German and then try to read my book. This, already, will give you a way in your researches. I would regret it very much, indeed, if you would not come here, next year. But you are young and one year later, filled with such important researches, would not be a loss for you. And: keep open all doors. In Europe we have learned in the last 20 years not to project for a long time, but to be prepared for every eventuality. How would it be with the Santa Barbara Botanical garden? Better than Huntington?

The IOS-Congress now is proposed for the last week in August 1956. Would it be possible for you to be in Europe already at this time, if you would not go to Peru?

T Cacti: I am joining a picture from my *Erdisia squarrosa*. The flower shows, indeed, the same acrotony in the areoles as it is true in *Coryocactus*. I did not more remember to this. Thus you are really right to unite *Coryocactus* and *Erdisia*. The flower of my specimen has acute tepals contrary to the picture in Br. & R.

I regret: You did not joint your manuscript-copy of K & H, Comment on *Werkerocereus*. I should be very much interested for this.

Denmoza: The staminode hairs aboriginate very distinctly from a staminalbase, forming the nectar-glands as it is true for all stamen bases in Cacti. These are branching again and again finally forming the crowded hairs. Thus it is evident that in Denmoza all these hairs are staminodes. Contrary: in *Borzicactus* and *Bolivocereus*, in very young buds it is to be recognized very distinctly, that the hairs develop below the stamen-primordia directly from the axial tissue. The whole Group, indeed, is much more complicated than Ackeberg's description would imagine. At the seeds, I can imagine, that you did not discover too much for the phylogenetic research. Nevertheless: Denmoza, *Oroya* and *Oreocereus*

have rather dull seeds of the verrucose type with deep and large holes in the testa ( cf. Morphology of Cacti III Fig. 445) and perispermic genera te species Pl. Argent. ) Cladocactus seeds are glossy, the warts of the verrucose type appanate and between of the cells little spots ( spotted testa) ( cf. Morph. of Cacti III. fig. 421 E ). These seeds re-

resemble to these of Soehrensia and the Guascha -Group. Haagocereus and peruvocereus. Probably, indeed, the intergrading genus between trichocereus and Borzicactus. It was already Akers' impression, that Haagocereus resembles more to trichocereus; therefore he has stated the new genus, Heobinghamia Hackeb. also, belongs here. We do not know sufficiently exact details. Even here. As to Borzicactus you know : I do not like your connection of the globular and ( only in age ! ) brevicolumnar species with Borzicactus. But, maybe, you are aright. My examinations are not yet sufficient. and not yet finished. I did not get sufficient materials.

But there is an essential difference between A. Berger's diagrams of evolution ( also I estimate them very much, to be the first approach to true phylogeny) and my diagram of general use in my lecture. My diagram must be ( essentially! ) founded on the laws of progression which occur in the branch in question; i.e. it must differentiate the continuous lines and the equal steps of evolution. At Berger's time this knowledge did not yet exist, therefore these diagrams have used similitudes only and therefore very often have led into errors. (very often similarities of the terms only, like our example with the hairs in the tube.) That's "static".

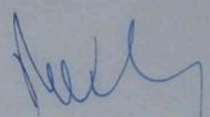
And there we are again at the question of "Dynamic method". It is, I think, rather hopeless to discuss this problem in letters. Nevertheless! Do you remember : When we went from the discussion with Dr. Gave (or was it while the discussion ?) you told me : " What might be more dynamic, than embryology !?" I did not answer. For , at this time I thought that maybe, the embryologists really use a dynamic method in MY SENSE. But later on I saw, they do not , indeed. What you call "dynamic" is not the same as the dynamic method in my sense. I have read many different american literature while i was at the UC. Indeed, I regret, I did not read Stebbins' book. I will see to get it from Vienna. It is a pity, indeed, that we have been isolated for such a long time and our libraries have only few of the american literature. But, all I know, from different journals, was very different from my approach to this problem. I saw in Huxley's book, as well as in a reprint of Daenicker, Zürich, that there is a striving after a way - but this way has not been reached, except in the taxa below of the genus - in the genetics. And what you call " conservatism in drawing conclusions" makes this fact any more evident for me. Our discussions just in the embryology of the Liliaceae have shown to me very distinctly this difference in thinking. But, indeed, it is scarcely possible to discuss these problems by letters. When once you will be at Graz, we will do so very intensively !

Finally : I join there the nice picture from our last Bot. Garden-Party. I think it will be a pleasure for the whole "Bot. Garden Company". The other one. with Mr. Blos will be mailed directly to the Blos.

Uff ! This has been a long letter ! Hope you will have time to read it.

In all case : My very best thanks for you help, my very best wishes for Christmastime and for the New year ! And tell my wishes also to all our Company.

Always truly Yours



14 Dec. 1956

Dear Buxl.,

Your letter of Dec. 9 is at hand. About my course work, I don't exactly understand the difficulties, but I will send you (enclosed) a transcript of my college record, official, from the University. I received my Bachelor's degree in June, 1949, but I have enough credits for a Masters degree. I don't think you will get much from the transcript except the approximate sequence in which I took my courses,----

About points these are based on grade values, A, being highest, and anything below a D not passing. A to D is passing work, both A and B are superior and C is average. Since I took  $154\frac{1}{2}$  units of work, if I received ~~xxx~~ a C or average grade in all this work, I would also have  $154\frac{1}{2}$  points. It is necessary to maintain at least as many points as units to graduate, i.e., a C-average grade. If I had acquired a perfect A-record, I would have acquired  $463\frac{1}{2}$  units. Actually I have  $256\frac{1}{2}$  units, which is somewhat less than a B average, but more than a C average ----closer to B. All my studies were done here at the University of California in Berkeley. Dr. Goodspeed says than no student he know of that has received a PhD in Botany here has had so much college work credit in science as I have. All of my credits are undergraduate, that is, I did no graduate work for credit here. My college work stopped with the Bachelor's degree, but I did six years work instead of the usual four years because I changed my major several times.

Since my Bachelor's degree is equivalent to the Certificate of Maturity I presume I would have status as "ordentlicher Hörer". If so, I want to know what I will have to do there for the higher degree.

I do not have all my reprints, and unfortunately, I think the latest papers are the best, so I will wait a little until I have reprints of them all, then send them all together. I will not be able to send "The Genus Nicotiana" which is a very large book, in which I was co-author of section 6, "The Taxonomy of Nicotiana".

I will shortly send you Stebbins book, so do not bother to look it up in Vienna. Your article received and already forwarded to Haselton. I hope he can bring it in the Jan.-Feb. issue of the journal.

On other points in your letter I will write you soon.

As ever,

PCH

1957  
3 Jan. 1956

Dear Buxl,

No word from you in some time, but I have news for you. Haselton rejected your article on the basis that it was too technical for his readers. I have been looking for a place to publish it for some time----there were several but all took much too long. Finally I went to Dr. Mason to see if he thought it appropriate for Madroffo. He said yes. They will of course send it out for review before accepting it and therefore it will be necessary to have all of it before I can submit it. Sp as soon as the remainder arrives it will be given to Dr. Mason for publication, probably all in one issue, in Madroffo. I won't know for about a month after that whether or not it will be accepted.

Both Myron and I wonder why Echinocereus, Wilcoxia, Rathbunia and Bergerocactus are not included in your revision. If you exclude these genera from the Cereoidae then I think you should mention the fact, and give some reasons.

I am not certain but you should check as to whether the prefix Su can be used for lines. I think its use, along with several other prefixes, has been banned for certain categories of taxa.

In your treatment of Gymnocereidinae you mention Hummelia Backeb. and Ebneria Backeb. These are only subgenera of Monvillea and need not be mentioned, so I have crossed them out. We were both surprised that Brasilicereus and Subpilocereus were reduced to Cereus. We don't particularly like the placement of Sulcorebutia in Lobivia. On the Loxanthocereinae you left Arequipiopsis Kreuz. & Buin. out, --it belongs in synonymy of Arequipa. Myron and I are reducing Matucana in the Jan. journal, and will reduce later Loxanthocereus, Maritimocereus, Seticereus, Oreocereus, Morawetzia and Arequipa. All to Borzicactus. We can't find the genus Vollia published anywhere. Under Neoporteria I also include Reicheocactus. Your remark on this generic name is not valid. One cannot just ignore a name. Bonifazia, not Bonifacia, which is a name in another family. Hattoria is conserved, you cannot use Hariota. Bonifazia may belong in Disocactus, -- we are checking this now. Gymnocalycium includes both Weingartia and Brachycalycium in my sense. Note that Bravo has already take up Stenocereus for Lemaireocereus, at least for some of them. You state that Acanthocereus doesn't have spines on the receptacle. But it does! ~~Saxifraga~~. I don't think numbers of letters for your taxa are necessary. Perhaps you could use these in the summary only.

Myron says to say Merry Christmas and Happy New Year and of course I join in these wishes for both you and your wife, whose fine cooking I am looking forward to sampling. It now appears that the S. American expedition will come off all right, and that I will leave for five or six months in next October. I don't think I can afford a trip to the IOS Congress if I go to S. America and later hope to get to Europe. It would cost almost \$1000.00 to attend the meeting---hopeless!

Best, as ever,



Dear Paul ,

Jan., 13 , 1957

Thank you so much for yours of Jan. 3 .

goodness, now I did hurry to finish the manuscript ! Fortunately I had it not jet. Otherwise I would have sent it by regular mail, as I thought, that is not pressant. Than it would arrive anywhen. For it seems that regular mail needs veryvery long time. Thus you did not get any word from me. I hope, now you have got it.

It, perhaps, it would not be possible to print the Cereoidae in Mardona, I can get it printed everytime in the Kew Bulletin which has asked me for publications two years ago. In this case pleas return the manuskript as soon as possible. I regrett that it now will not jet be printed in February ( at least at part, as packeberg's book seems to be soon printed at least the first part and it would be a pity if he wouldhave any validly published "system" even if it would not be valid in science. I think. it would also be regrettable, if this publication, made at UC would not be printed in USA but in England.

As to the special part of your letter: Tribe VII contains the genera, you missed in the first half of the manuscript. I have overlooked, that Hummelia and gbenria are only subgenera. Thanks for crossing it out. Brsilicereus and Subpilocereus are the most primitive and the most evolved parts of a clear line of evolution in Cereus you will se more about this in Succulentenkunde VI. Salcorebutia. Indeed, although the seed is equal to a typical Lobivia-seed and the flower a Lobivia-flower without hairs, shaving seen the habit in your adult plants, I am in doubt myself. Further examinations will be made and - I hope - claeer this question. Brchycalycium I never have accepted. Vollaia : I have seen the publication, I think in the french Journal ( I have now not the time to look for) it is a typicla ~~Spinthyl~~ Zygocactus-truncatus - branch, flowering from the lateral areoles. ( May be bugs had demolished the pseudoterminat meristemes ) Bu- I have not more the occasion to check, as I was not at Graz since having got your letter. Nevertheless, I think it is at maximum a recommendation not to use it, not a law! Euechinocactidinae is already in use since 1951 and would not be changed in any case. even purhipsalidinae is in use since 1951, although not yet published validly. As to the Loxanthocerei, see the appendix in the list. I have indicated, that you will contract the invalid genera. I will soon do the seed examinations of all these, to advise you in this point I cannot see how to accept a "genus" ( Reicheocactus) which has been published lacking any plant material, only from a very unclear and dubious description ! Bonifazia ( tahnks for correcting the spelling I have not yet seen it, therefore I do not judge about, Probably you are aright. Stenocereus: Probably Bravo used it in Berger's sense. The whole Lemaireocereus-problem will need perhaps a year more. For the present it is best doe as I did.

I am glad, if you will have occasion to forward your research in the high Andes, although I regrett, that we will not see one year longer. Indeed, I am missing all you myself and I am thinking very often . Answer to your letter before this comes later on. I am very much in trouble with about 2 lbs of unanswered correspondences !!  
Terribly L

All the best to you all and especially to you  
as ever yours

13-I-57 [??]  
B to PCH

Please Correct the following in the first part of the manuscript:

Trichocereidinae p. 22 first line

may be corrected as following:

" Subtribus 2. Rebutinae Donald emend. F. Buxbaum "

I had overlooked a publication of Donald, where a subtribe  
is made for only the subgenera of Rebutia ( taken there for genera.)

That please accomplish in the Bibliography the citation of  
Kimmach and Hutchison. ( You have not sent it to me ! )

Univ.-Dozent Dr. Franz Buxbaum

Professor

Jordanburg (Austria)

Sackgasse 13/14

March 16, 1957

Dear Paul,

every day I am waiting for a letter from you, but never it does arrive. Thus I am sorry, because it seems that you did not get the book, which I have sent you. It was - indeed - proposed for Christmas, but than I have seen, that I have mailed it much too late. Nevertheless, I think it must be at Berkeley now. May be, you still will get it. But therefore I did not write you earlier.

Now I have already such a long list of questions and other for you on my writing tabel, that I feel it necessary to write without waiting for you next letter.

First: As to your studies in Graz, there will not be any difficulties. In the contrary. If you would have had 4 years of studies in the graduate state you could do the examinations without any further inscription. Now, in this way this is not possible. But you can inscribe in the sate of a "ordentlicher Hörer" and your work in Berkeley as well as your publications will be counted for this and there will not be any difficulty to finish the studies in 2 years with the Ph.D. Nevertheless or better the more it is necessary to have a detailed "Curriculum vitae" tha means a biography containing your University work in the manner "from to" with the exact dates, your expedition work, and finally as more as possible reprints of your publication. It is clear that you cannot send Prof. goodspeed's book, but you will be able to indicate, which part (pages) is your work there. But - as you first will go to South America first, this point has time.

But in this connection any other question which may be is important for you: The Austrian cactus Society intends to send an collecting expedition in the Andes next year. They have asked me, whetere or not I could join as botanist, but I cannot. Therefore I told the Society, that may be you would do it. Perhaps you could join the Austrian expedition after your University-journey. This would be very useful for the Austrian society as well as for yourself, I think. Wouldn't it? Please tell me, how you think about this point.

Again in connection to your planned South America trip: Please send me soil samples from anywhere where you collect cacti! A teaspoon ful or a hand tablespoon ful is sufficient. It might be taken between the roots and if possible a little stone of this soil to recognite the mineralogical origin of it. Naturally to each sample: Name of the cactus which has grown in it, locality and so on (if possible also a geological date) as for a herbarium voucher. This would be of greatest importance for further cactus culture. The soil from Cap Gabrillo between *Bergerocactus emoryi*, and *Hammillaria dioica* had pH 5, tending towards 4,5! All samples from the Arizona desert (Organ Pipe Monument) have pH 6, only one was pH 6,5, but no one was neutral or even alkaline! The cause, why the plant physiologists of the Oklahoma University did not find any acid reäction (oral communication of Prof. Boke) was (probably) that they did examine the soil in an agricultural method, i.e. they did dilute the soil with 100 ccm of water and examine the water. This in the case of an extreme xerophyt is wrong, because the important reaction occurs in a very concentrated solution. That is clear, that such a very much diluted solution has a very little concentration of H-iones and therefore seems to be neutral.

Prof. Mason has written to me, that Madrono will print both my publications. First they must be examined by the staff but this is a formality. Pegerlind wrote to me, he is not sufficiently familiar with the Liliaceae to judge. He was interested very much for my manuscript. But, he wrote, in every case, "homology" in embryological research must not be taken in the way as it is taken for outer morphology. "Homologies" in embryology are questionable even with regard to the character of the tetraspores: Thus this is, what I meant.

To my Cereoidae-manuscript: I have discovered, that I did overlook Islaya. Please put it in into the manuscript behind Notocactus.

And another change : I have now studied all details of *Pseudorhipsalis* and I see that you are aright: This genus belongs to the *Discoactinae*, convergent to *Wittia*, representing another way of reduction than *Wittia*. Nevertheless, do not unite *Pseudorhipsalis* with *Discoactus* (*Ps. macrantha* indeed). They differ too much in the androecium. But please correct this too in my manuscript, putting *Pseudorhipsalis* between *Discoactus* and *Wittia* with the footnote : " First indicated by oral communication by P. C. Hutchison."

Today I got the *Cactus & Succulent Journal* 1957/1. I was much interested for your *Hylocereoidae*-article. Its very fine written! Thanks! That is a fine way of Co-operation. If you would unite all the three genera, *Wercklerocereus*, *Weberocereus* and *Eccremocactus*, as subgenera, than I can agree with you. There is only the question, wheter or not it will be useful. ?

My English Book: *Cactus Culture Based on Biology* " now soon will go in print. Mrs. Higgins has already translated a great part. For this book a have an important question to you : Which cacti can be used for ~~stomach~~ grafting as a nematode-proof stock ?

Probably the british editor will permit very many pictures for this book. Therefore I join a list of genera of which I have no very good photos and I should be very grateful if you could send me any photos of these genera. But I dislike for the book cacti which are distinctly grafted.

For my researches :

Please , give me dates of the origin of you *Notocactus Haselbergii* BGUC Nr. 49.1764

of *Zygocactus truncatus* BGUC nr. 52.1856

*Rhipsalidopsis rosea* BGUC nr. 51.554

*Rhipsalidopsis Gaertneri* BGUC 52.1109

*Zygocactus* (*Epiphyllanthus*) *obovatus* BGUC 51.596

Please can you send me seeds of all genera of *Hylocereidinae* and *Loxanthocereinae* so far you have them. 2-3 grains will be sufficient and mixed fertilisation is not important, as I need them only for morphological research .

For my lectures at the University I should like very much to have cuttings of *Cryptocereus*, *Epiphyllum chrysocardium* and *Eccremocactus*. Oh yes, also from *Weberocereus* and from *Pseudorhipsalis macrantha*. But please send them to my home adress, because Prof. Widder likes to make difficulties because the douane.

Riviere will be very glad to follow your intention with regard of plant materials. Will you now write him directly ?

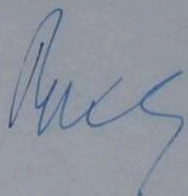
I should like to send reprints to Mr. Macbride but I have not his exact address. Please tell me it.

Krainz has forwarded your IOS- concept to Byles, but he did not hear anything more about it. Did Byles already write to you ? It might be very urgent to write to the Secretary about your proposals. Why - otherwise - we would have smoked so many cigaretts this night ! ?

For the moment , I think, this is all. It might be sufficient!

Monday it will be a year when I started for the USA ! Near to half a year since I have leaved from Berkeley ! I am missing you all very much. it was such a wonderful time . "Fantastic"! Is it not Pleas tell my cordial greetings to the whole Botanic Garden -Company ! Myron, May, Peter, Mrs Wheeler and last not least to Prof Goodspeed, Mason and all the others.

Especially to you ! As ever Yours



Photos wanted for my Book "Cactus Culture based on Biology" from the following genera:

- Acanthocalycium
- Apocactus
- Ariocarpus ✓ + Roseocactus
- Aztekium
- Coryphantha sensu stricto
- Cylindropuntia + Austrocylindropuntia + Tephrocactus
- Dolichothele
- Echinofeslanocactus
- Echinocactus
- Epithelantha
- Eriocyce
- Eggestoa
- Frailea
- Gymnocalycium
- gaugeocereus sens. lat.
- Harriaia + Eriocereus
- Helicocereus
- Leptocladodia
- Leuchtenbergia
- Mahacarpus
- Mammillaria ( Subgen. Chilita, Elegantes-group and others)
- Mamillopsis
- Melocactus
- Monvillea
- Neobesseya
- Neolloydia
- Mammilloidya
- Oehmea
- Pelecyphora
- Selenicereus
- Stromboactus
- Wilcoxia

All pictures wanted on glossy white paper

Univ.-Dozent Dr. Franz Buxbaum  
 Professor  
 Judenburg (Austria)  
 Sackgasse 13/14

Form PCT

21 March 1957

Dr. Franz Buxbaum,  
Sackgasse 13/1/4,  
Judenburg, Austria.

Dear Buxb.,

Your very fine letter of March 16 arrived today. The delay in your hearing from me is due to many things, but largely my negligence. You will understand more from details below. However I did send you from my home a certified copy of my "Transcript of Record" of the University, showing the date of all my matriculation here. Did you not receive this? That, combined with the ~~xpik~~ explanation of my courses which I first sent to you should give a most complete idea of my background. Your most thoughtful gift arrived several weeks ago and make me itch to see that country. Also I have a friend traveling in Europe who visited Austria and wrote me in some detail of the great beauty of the country and friendliness of the people. All conspires to favor. But I am not yet positive that I want to leave my work for all that time. I am so swamped, even as you always are, that I don't know how I can call a halt. And now, more than ever, I am beginning to produce significant papers and don't want to stop them. Well, we shall see.

As to a "Curriculum vitas" I will prepare a copy organized in the way your suggestions imply and send it. I have already sent you a complete set of reprints, one for yourself and a second for the University, about a week ago. I will obtain also for you, Dr. Goodspeed's book, as a gift, and you can then show it to them. In it I worked on pages 326 to 536 most extensively, and on earlier sections I did only small contributory work. For the taxonomy section I rewrote it in its present form entirely----it took two years. The early research for this section was done by Dr. Wheeler (also co-author of it), but its present organization and format are mine. The single exception is that many taxonomic decision were made by Dr. Goodspeed concerning the validity of certain species and recognition or not of certain varieties, with which I did not agree. Hence the authorship on all new names is his, as I would not author them, or even co-author them. Some day I may have something to write about these few taxa. I consider at least five of the recognized species to be suspect!

Also the copy of Stebbins book is now enroute to you. The bibliography I sent you is now out-of-date as I have finished many more articles. I will check and send you a new bibliography when I complete the biographical statement.

My work in S. America will be primarily in Peru. I hope to extend to northern Chile. It is possible I may be able to collect in Chile for the Austrian society. What do they want and under what conditions to they propose that the material be collected for them? It is not cheap to do such work. Perhaps I could receive credit in schillings to use if and when I am in Austria, at least in part, for sharing seed and plants. As for your soil samples I shall be glad to oblige you. I will also take care of the corrections in your Madroffo articles.

No cacti are nematode-~~xxx~~ proof. For grafting when it is known there will be nematode infestation, Myron says it is better to use species with big root-systems, like *Cereus peruvianus*, as they are stronger feeders and better able to survive with nematode.

- 49.1764 *Motocactus haselbergii*, a plant of horticultural origin received in 1949. This same plant was studied by Kurtz---for pollen morphology.
- 52.1856 *Zygocactus truncatus*, I call *Schlumbergera truncata* cv. ORANGE ROYAL. Imported from Brazil, without field data, in 1952. The cultivar name has not yet been published.

Rhipsalidopsis rosea UCBG 51.554. The number changed to 50.1258, so change the number on your records. It is of horticultural origin, without data. It also has been used by Kurtz.

Rhipsalidopsis gaertneri UCBG 52.1109. This may be a variety of the species. It also came from Brazil in 1952 without data.

Zygocactus obovatus UCBG 51.596. We received this from a nursery in southern California in 1951 as Epiphyllanthus obtusangulus (Schum.) Berg., but changed the name to E. opuntioides (Löfg. & Dusén) Backeb. It definitely belongs in Zygocactus. We don't think, however, that it is obovatus.

The request for seeds is a big one, but I will do what I can. Don't draw these seed yet as we have already drawn many of them. But especially, look at the embryos and draw them if you are going to draw anything, as we haven't done the embryos. I may not be able to package many of these seed for several weeks, but I will get some off to you soon. Incidentally we find that mixed pollination sometimes does make a difference in morphology, and also that some species have highly variable seed on a single plant, and others vary on different plants.

Cuttings of Cryptocereus, Echynocardium and Eccremocactus---yes. We must wait for warmer weather however. The Cryptocereus is now covered with buds and I will pickle some for you. Not really necessary as we are drawing it, but I am sure you will want them anyway. We think it belongs near Hylocereus. As to the others---Weberocereus and Pseudorhipsalis, yes on them too. We will send them to your home address, by air. Do you have to have a permit first, or shall I just send them direct.

You mention Riviere, but I have forgotten what I proposed with regard to plant materials in connection with him. What is it?

Mr. MacBride no longer keeps reprints and he will just give away what you send him, so I recommend against your doing so. You can, however, send anything to him by addressing to J. Francis MacBride, c/o Dudley Herbarium, Stanford University, Calif.

The IOS thing. Byles sent it to me and he and I have had an intensive correspondence on it. I enclose the present revised copy of it. Please send it immediately to Krainz if you approve and have Krainz send it back to me. Both Byles and I feel that your suggestions about taxonomic matters cannot be included, since such matters are governed by international rules (the Code) and cannot be legislated by a society. First things first---and the first thing is to get an organization that will do something and will gather prestige for itself. I think the present format in a simple way outlines steps necessary to this end. Details will have to be worked out only if the principle is approved. Interesting you should have mentioned this matter, because in today's mail, with your letter, came the last letter from Byles on the same subject, and a return of my manuscript in the form of the enclosure. We must rush it now, as it must go to the Secretariat very soon.

I have transmitted your greetings to all and they all send you their best wishes. Here, when you are mentioned, all faces light up with pleasure. We miss you too, Buxl, very much. How terrible it is that travel is so long and expensive today. Yet it would have been far worse 50 years ago. I will check on photos for you and let you know. (for your book). I may be able to take a few to fill in.

As ever,

512, 1831  
July 1900

Dear Paul ,

April 14, 1957

we need for our common "Die Kakteen" very very urgently transcripts of the original diagnoses of following species:

- Mamillaria ~~166~~ mainae Brandege in Zoe, 1900: 7 5, p. 31
- Mamm. oliviae Orcutt in West American Scientist 1902, 50 and 163
- Mamm. dioica in West American Scientist 1884, 9

I think these diagnoses will be available in the UC library. Would you, please, be kind and transcript the and send the ~~manuscript~~ transcriptions directly to Krainz.

Thank you for your nice letter. Indeed, the dates on your studies are in my hands. about all other in a later letter.

I am too much hurried today.

Always Yours

QH1  
W42

Ergebnis 115- 130  
Duck

sent air mail Apr 23, 1957

Dear Paul ,

May, 10, 1957

Just I have got the March-April issue of the Journal and have seen the *Borzicactus aurantiacus* Leon. I regret it very very much, but you are in error with this new combination on the whole ! The first view to the picture has shown to me, that the combination with *Borzicactus* is wrong. This plant is very closed to *Denmoza*, but very far from *Borzicactus*. In *Borzicactus* the hairs in the base of the receptacle are true hairs, arising from the epidermis of the axial protrusion ( you remember that I have examined and discovered this fact at Berkeley ) while in *aurantiaca* and in *Denmoza* these are true staminodes. The other is the seed. I have seeds of *Borzicactus morleyanus*, collected by Rimbach at the type locality. These are very different in type of this of *aurantiaca*, belonging to the same type as the nearly smooth spotted seeds of *Cleistocactus*. The seed of *aurantiaca* is exactly the same type as this of *Denmoza*. I would not be able to distinguish these both seeds. Thus, even in the shape of the plant the *aurantiaca* approaches so much to *Denmoza*, that I would say it is an open-flowered *Denmoza* !

I regret this mistake very very much, because I dislike that you make a mistake. Therefore : please make mikrofilms of all the drawings of *Loxanthocerei* so far you have and send them to me. I will give you any advice you need for the further work on *Loxanthocerei*. Our knowledge of the plants belonging here is still insufficient to judge the validity or invalidity of these genera definitively. And we must not put water into Backeborg's mill ! Just in the *Loxanthocerei* he would be very sensible and it would be a pity if he becomes able to prove any mistake of an professional botanist.

Therefore : Send me all material you can send me before publishing anything on this way. It is not of importance whether or not you will cite my name, but it is of greatest importance, that your publications do not contain any mistake !

Anything for me : Would you be able, to send me some of the pictures for my book soon ? It seems that it soon will become urgent to revise the pictures. The book will be available in 1958.

Best greetings to "the whole company" of the BGUC !

Always Yours

*Alcyon*

17 May 1957

Dear Buxl,

Thanks for your letter of May 10. I have very few pictures from your list, but will forward such as are at hand, plus a few others which may be useful although not on your list, within a day or two. I am leaving for Peru July 18 and will not return until about Dec. 1. The question of the Loxanthocereinae should be resolved by the collections taken on this trip. However, both Myron and myself feel the question to level of genus is resolved now, excepting for one thing. That is the question of separating *Cleistocactus* and *Borzicactus*, which we find very difficult. We sink all genera into *Borzicactus*, excepting *Oroya*, *Denmoza* and *Cleistocactus*. Nearly all of these segregate genera we have given subgeneric or sectional rank. When you "judge" our taxonomy, be sure to do it in terms of a much broader concept of genus than you have been using. We have examined flowers, x fruit and seed of at least one representative of each segregate genus, and often of more than one. A paper presenting our results will be given to the U. S. Section of the IOS meeting here in Berkeley July 17, entitled "A Revision of the Genus *Borzicactus*".

If you believe that I am going to recognize a genus on the basis of seed character, you are mistaken. My taxonomy will reflect opinion of the sum total of all characters of a species, and of comparative data with other species. We found in *Borzicactus* and its allies a more or less complete intergradation of nearly all characters, when all species were taken into account. Indeed no separations seem possible that can be based on more than one or two characters. Unless we can demonstrate that the assemblage is polyphyletic as ~~xxx~~ currently conceived, there is no recourse but to reduce the segregates. Indeed it is with much doubt that I have not reduced them altogether, under *Cleistocactus*. If you think this problem is simple, then I suggest you try to draw up "Generic" descriptions of those segregate genera and see what you come up with. None of the characters hold up, as you will see. Now, as to *B. aurantiacus* and its hairs. It may be true that the species is closer to *Denmoza* than to *Borzicactus*. But this doesn't both me, under the present taxonomic scheme, for one would expect to find a gradient of some sort leading to *Denmoza*. As to seed of *B. aurantiacus*, perhaps you will be interested in morphology of seed of the other "Matucanas"----which is quite different and quite variable within that so-called genus. Also see seed of *Arequipa* and *Oreocereus*, particularly of *Oreocereus fossulatus*, which is very aberrant. In spite of seed variation the *Matucanas* form a nicely integrated and easily defined group---a good section at least. You do not seem to realize that we have most of the plants you have seen and many others as well. The difference of opinion probably reflects a difference in judgement as to the significance of certain characters, within certain lines in this family. That this difference is major will become more apparent to you. For example, I suggest you reconsider your origin of the *Rhipsalidinae*. Myron is preparing and will publish a paper proving most conclusively that they originate from *Corryocactus*-like ancestors. That ought to shake you up a bit. But please do not release this information. I believe the documentation necessary to support this thesis will take us about two years. The point is that the concept of a "mistake" depends on how one uses evidence available. It is a two-pronged sword.

Will write further shortly.

Best, as ever,

Univ.-Dozent Dr. Franz Buxbaum  
Professor  
Judenburg (Austria)  
Sackgasse 13/11<sup>a</sup>

Dear Paul ;

June 10, 1957

Oh goodness, You are a difficult man ! But I am at present too much in business to be able to write you more detailed and answer your letter of May 17, 57, Nevertheless, something of importance. If you follow the concept of your ~~meaning~~ opinion of the category "genus", there is not any reason not to unite also the genera Cleistocactus and pennoza into your sample genus Borziocactus. Only : than Borziocactus is wrong and the genus must be Cleistocactus because it is much older.

I only regret, that finally, I see, you will reach the Linnean genus Cactus for all cacti. And in the same way one genus Crucifera for all Cruciferae and one genus "Umbellifera" for all Umbelliferae. For the differences between the genera of these families are much less important than these in Cacti. And these, also, are differentiated by seed differences.

And I regret very much, that you are not yet able to read my book. But : Myron may read it, and tell you the most important facts of it. I think it will be of great use for both.

And perhaps the lecture enclosed to this letter may help you too. I will read this lecture at the IOS-Congress at Holland this year and I think it may be of interest also for the US-Section. Therefore I beg you to read it at your meeting for me. And ~~it~~ it would be of great interest for me, if you or any other member would inform me about the discussion which might take place after the lecture. And if you publish ~~it~~ the lectures of your meeting, you may do it too; otherwise I would publish it elsewhere.

I am waiting very hardly for the promised pictures, because my editor is already waiting urgently. I hope they may arrive soon.

For your expedition, all my best wishes will accompany you and I hope you will come back with a lot of important results. And do not ~~to~~ forget, to send me soil samples of cactus-soils !!

And do not forget, before starting : My requests for different seeds of the Berkeley collection : Bergerocactus, Werckerocereus, Weberocereus, Mediocactus, Lecremocactus and any other seed of Hylocereidinae. Especially Wilmattea would be of greatest importance for me. And do not forget even other materials as seeds of different Loxanthocerei ( although I have quite the most myself. It may be important to see seeds of exactly defined species ! ) I can imagine that you are much hurried now with the meeting and your expedition. But perhaps Myron can help me and send me the seeds. 1-2 grains of each would be sufficient, if they are rare. I do not need them to grow them, but only for the inner and outer examination.

Myron may excuse me, that I do not write him personally. But what I am writing to you, means always even himself, because I have too less time. But all my best greetings always mean him too.

I hope to hear again from you before your start.

Sincerely, always Yours

2 July 1957

Dear Buxbl----I mean Buxl,

From my silence you must have deduced that I am also in a very bad position with regard to time. I am trying to organize the two meetings, scheduled for Berkeley next week, and last week finally shipped truck and supplies to Peru, and the day after the IOS meeting here on July 17 will leave for Panama, Ecuador and finally Peru. You can contact me hereafter c/o American Consulate, Lima, Peru. I am aware of your interest in soils and will send you samples of same, as well as samples of seed.

Pictures enclosed. I regret that for most of the genera you listed I had no pictures. If it had been any year but this one I could have taken them for you. But this last six months has been impossibly difficult for me and even by working late, up to 2 AM at nite, I haven't been able to keep caught up. With regard your interest in seeds, most of those I have are Ritter collections, which I presume you have, or have access to by Krainz. There are some few of my own 1951-52 collections. I think it best to send you seed from Peru and let the others wait until I return, as I am far behind on preparation of my field notebooks for work in S. America, and that, at this point, must come first. I can only prepare them once, and must finish this in the next few weeks, of which 10 days go to convention. I do enclose seed of *Bergerocactus*, which I just collected from our plants.

With luck I shall get a few items in Ecuador, Bolivia, Chile and Argentina. In any case you will hear from me very briefly from places in S. America, but mostly I will just send you material of soil and seed for your work, under collection numbers and with brief notes on probable names. I have so much ground to cover in Peru that I won't be able to do much writing. Will be back in the U.S. about December 31.

Your paper arrived just in time to include in the printed program of the IOS meeting here, and I will read it for you. It needs corrections, but I shall not have time to type it over, hence I will hold it until my return from S. America before submitting it for publication. Hence it will not be published until 1958.

Myron and I will present a paper "A Revision of *Borzicactus*" which you would find of much interest, as we have seen I think much more material of all *Loxanthocerei* than you have, one or more species of every "genus" including all characters down to seed. We have not examined embryos however. I think maybe this is a good place for cooperation and perhaps a joint paper---on morphology of this group. We realize that *Borzicactus sensu lato* can scarcely be separated from *Cleistocactus*, but nevertheless want a separation, and we are hard put to find characters on which to base a separation. Your help here would be invaluable. For those wanting to recognize the segregate genera of *Borzicactus* they must prove their case. Your argument to me is unconvincing as you are talking of phylogeny, not genera; you and I agree as to the probable phyletic sequence in the *Loxanthocerei*. From my point of view it is equivalent to that of many other genera in many other families of flowering plants. The differences which have evolved also equate. As to *Umbelliferae* and the several other difficult ~~gen~~ families you mention, the world authorities cannot agree, ---especially on *Umbelliferae*, and Constance last week told me the divisions are certainly arbitrary, although traditional. For the *Cactaceae* we cannot yet call them traditional, and unless someone takes up an alternative, they might become

traditional. This, to me, would be too bad, as the present divisions do not convey much sense as to concepts of genera. Indeed, many of the segregate genera don't seem to me to be much more than species. Take *Maritimocareus* for example, and *Digitolobivia* or *Pygmaelobivia*. *Morawetzia* is a good species, but a ridiculously weak genus. I haven't much appreciation of the attitude that just because someone has given generic names to certain names, that they should be upheld until the evidence is so overwhelming that they have to be rejected. In physical science such an idea would be considered ridiculous. New genera and new species can be considered to be proposed theories or hypothesis, insofar as their newness or their category are concerned. As such they should be always considered hypothetical unless evidence supporting them is overwhelming and is fully documented at the time of description. Not a single one of the many segregate genera of Backeberg is convincing to me, nor are any of them fully described. Hence once cannot assess them. If someone proposed a new explanation for gravity of the earth, we would not accept it just because it was the latest explanation; rather we would assess it, and if not convinced would continue to follow the older explanations until convinced otherwise. By using new theories one encourages by example others to use them, and thus traditions are born. Use + time gives tradition. I refuse to use names that I am unconvinced about. There is a borderline area where one cannot be sure, that is, where the evidence seems good, but where one may not know enough to judge. In such cases an option exists to use one or the other system, theory, or name. But for the groups I have studied and on which I have expressed opinions in print, I no longer feel the necessity for arbitrariness----I feel the evidence balances in favor on the system I am using, and further, I can document that evidence very fully, both factually and theoretically.

Backeberg's recent paper (Dec. 1956) describing the Rauh and other collections has added 190 new taxa to Peruvian cactaceae. I have never seen a more disreputable paper in my life, either by him or by any other author. I thought of reviewing it then decided that it would be better not even to give him that courtesy. I have documented over 200 errors in it already., which I can prove.

Vatrican and others are much upset about the IOS proposal, probably especially to Part A which criticizes the IOS. Most of that I did not write as you know---- it is mostly a combination of Byles and your remarks. Although they are not my ideas, ~~xxx~~ I want you to know I fully agree with them and that I stand behind them. Vatrican has written me a long long letter in French and I haven't even had time to read it. I am asking him to forward all future correspondence to you, as I can answer no more letters for the next six months. Having presented my ideas in Part B, and having achieved the agreement on these of yourself, Krainz and Byles, I toss in my hat so to speak and turn my back. The ideas are there and if the IOS wants to do something to revitalize itself they can do so, but my part of it all is done. I urge your strong support of the proposal in its entirety as both Byles and myself will not be present. You, at least, know much detail about the reasoning behind the criticisms and the proposals. Meanwhile, dear Buxl, know that I am thinking much of you and warmly so, and that we miss you very much. We simply have to get together again.

P.C.  
As ever,

P.C.

Dear Paul ,

July 26, 1957

I just have got the issues of "Sukkulentenkunde VI" containing an very interesting article of Prof. Raunk on his Peruvian voyage. As I think this might be of interest or even importance for your voyage, I send it by "Air Mail Reprint" to your adress. Thank you so much for having read my lecture. It would not have been necessary to send it because I have a copy myself. When you will be back I will send it again for reprint.

When sending seeds for the Austrian Cactus Society, please tell me a price.

As to our circular for the IOS there has already been some trouble. Nevertheless, I will do my best, and I am sure, Krausz will do it too. In every case no decision can be expected at this congress. I think there will be statuated any Commission to elaborate new Statutes.

Please do not forget, that I am extremely aware for flowers (at least dried) of Neoraimondia, Haageocereus, Armatocereus, Espositog and any others of the rarely cultivated genera. It is not urgent, but I beg you to preserve some more for me. Even if you make drawings of any genus in Mrs. Elos's excellent manner, it is always important for me, to make my own drawings and examinations. My principle: What I have not drawn, I have not seen correctly.

I am much in pressure, because in a few days I will go for two weeks in the mountains.

All the best for your voyage!

Always yours

Dear Buxl,

I am taking this brief respite to write you in detail, as I don't expect to get another chance until I get back to Berkeley. The trip is going well in that I have found all the plants I intended to get up to now, but I am behind schedule and will not be able to get to all the places I want to visit. Have covered the coast from the Ecuadorian border to Chala, also Piura-Huancabamba and Olmos to the Rio Maranon, and the route from Nazca-Puquio-Coracora-Incuys-Chala in the south. Now heading from here to Balsas via Cajamarca. We are in Chiclayo for repairs to truck---broken springs, from the Huancabamba trip. Thanks for the Rauh reprints. The Rauh & Backeberg new names for Peru published in Backeborgs Dec. 1956 paper are gradually being cleared. Most of them not new! So far. I think I will be able to supply what you need of flowers. Already have *Neoraimondia*, *Armatocereus*, *Corryocactus*, *Haageocereus*, *Thrixanthocereus*, *Rauhocereus*, *Mila*, etc., in fluid. Can't seem to find *Espestea* in flower, though I've seen thousands of plants. To me all the *Espesteas* so far seen are a single species. I think we have been misled by recent workers into believing cacti have narrow distributions. Actually my concept now is that some species cover the length of Peru, others are narrow endemics, and others are inbetween, with somewhat restricted ranges. Backeberg would have us believe all species are narrow in range. My idea supported by distributional ranges of associated plants.

Will be glad to send you seed upon my return to US, but not from here---too complicated. This will send direct to you in such quantity as I can spare, and if you wish to distribute to Austrian Society, give it to them. Suggest they sell it and use proceeds to support their journal or your research. Or perhaps you can sell it.

Am happy to report am clearing up the older species for Peru---even *Cereus serpens* HBK. I found it near Sondorillo, the type locality, and also on the Olmos-Maranon highway. It is a *Borzicactus*. Backeberg described the Olmos-Maranon one as *Gleistocactus* (?) ~~name~~ *crassiserpens*, but is same thing. Also on the Maranon below Bagua found a new *Borzi* (*Matucana*) which looks like a *Gymnocalycium*---practically nude on some plants. Often I find no seed and have to take living plants---in such cases will hope to get fls. and seed in Berkeley.

*Armatocereus* fruit suggest *Corryocactus*---investigate this. Both have spiny fruit---spines growing as fruit matures. Spines become loose while fruit is still green, and easily come off. Fruit drops, then colors, then spines come off completely---and no dehiscence. Just rots. *Armatocereus* fruit turn reddish; *Corryocactus* (what I have seen) turn yellowish. *Weberbauerocereus* puzzling---have fls and frt in fluid. May be a good genus, certainly as distinct from *Trichocereus* as is *Haageocereus* and *Borzicactus*, superficially at least. Have few soil samples---just no time. Already 6000 miles covered over impossible roads, and as much to go. How did the Congress go. I heard of the trouble with the suggestion for revision of Constitution, but at this point, couldn't care less what they do. US Section meeting in July a success, but members think IOS in present form is superfluous---action in Europe may be critical to continued US participation. For my part, the fewer organizations, the more work I can get done! Best wishes and fondest regards,

Paul  
PG

[Kinnach]

Dear Myron,

Sept. 6, 1957

first : please excuse me, that I did not yet write directly to you; as I had always any discussion with Paul, but very little time for writing, all my letters have been adressed to Paul. But I think, Paul certainly will have always forwarded my greetings to you. Be sure, that I am missing you all !

But today I have some very urgent requests to you. I intended to write already since a long time, but these hollydays I was nearly never at home and the few days I was have been filled with business. Thus my request finally became urgent.

Paul had promised me to send me materials for my researches as well as for my lectures at the University, but I can imagine how he was troubled before the meeting and the start to Peru. Thus these materials have not been sent and now I beg you to do it.

For my lectures on Morphology I should like to have cuttings of:

- 1 Epiphyllum chrysocardium
- 3 Weberocereus tunilla and others
- 1 Werckleocereus imitans
- 1 Ecceumocactus bradei
- 2 Disocactus eichlamii and macranthus.

8 spp. seeds  
6 packets need

For examination of the inner structure I should urgently need seeds

of :  
Mediocactus megalanthus,  
Cryptocereus anthonyanus

→ Werckleocereus imitans

" tonduzii

→ Weberocereus spec. *tunilla*

→ Ecceumocactus bradei

→ Disocactus macranthus

These seeds do not need to be fertilized with equal pollen, as I do not intend to cultivate them but only do examine them. 1-2 grains of every species might be sufficient. As I know that you have fertilized these species I hope it will be possible to get them

And any further request, if it is not already too late for this year. In consequence of my researches at Berkeley and with the materials brought home, I would still need young flower buds of Morawetzia doelziana to examine the development of the staminodes and of Cleistocactus baumannii ( or any of its varieties) to examine the development of the "nectar holder". These buds may have a length of about 5-10 mm. Maybe that these plants still might bring some, although it is really rather late in the year.

If possible I should also like to examine very young buds ( about 5 mm) of Denmoza species. All these buds must be preserved in liquid, but it is possible to mail them than only on a wet condition without superfluous liquid, when in a glass well closing glass or tin.

Yesterday I came back from the IOS-Congress at Den Haag. It was very fine again here. The lectures were of a very high standart and on very different matters. Relatively few taxonomic ones. The most important resolution of this congress was the conclusion that all members of the IOS become members of the International Association of Plant Taxonomists (IAPT). In this way

6-18-57  
to Kimmick

it will not only be possible to publish purely scientific works ( which are of no interest for the popular Journals of the different Societies) in "Taxon", but it will also be possible to get money for publishing special works ( thus probably also for the "Icones"! ) from IUBS. Thus we have made a great progress at the way to reach our programm of 1950 of Zürich.

At the other hand, our common proposals of Paul, Krainz Byles and myself, have been accepted only as base for further discussions. By the ~~fusion~~ junction with the IAPT, indeed, we have reached more than we would have ~~reached~~ reached by opening the membership to everyone. For, the interest of the ~~many~~ amateur-cactophiles for science is very low, and therefore we probably would not get many more members, but probably some not wished ones. Now, I think, the IOS comes on the right way for further progresses.

Please tell my best greetings to the whole "BGUC-Company" !

Always Yours

*W. G.*

P.S. If perhaps you feel it necessary to ask Prof Goodspeed for my requests, please do it in my name or write my by turning post to do it by letter.

5818 Jordan Ave.  
El Cerrito 9, Calif.  
USA

Oct. 10, 1957

Dear Dr. Buxbaum,

Tomorrow I am sending to you, by air, cuttings of all the species you wanted. I am sorry that you will be receiving them so late in the year, but Paul had said nothing to me about your wanting plants. Things were very confused around here during the Convention and making preparations for the Peruvian trip. If you fail to root the cuttings I will send them again in the spring. I have also included seeds of all the species wanted except *Cryptocereus* -- our plant did not flower this year, and we still do not know what the fruit looks like. As to *Mediocactus*, I have suspected it to be more closely related to *Harrisia* or *Acanthocereus* than to *Selenicereus* or *Hylocereus*, but I have not seen seed of the former genera.

I have also sent buds of *Cleistob. baumannii*, but *Denmoza* has finished blooming. I have still not seen the "Nectar holder" in *C. baumannii* -- the diaphragm forms a tube but the tip is not curved down into the tube -- however I suppose this character varies, as do others, among different clones or varieties of this species. Incidentally, on a horizontal stem of this plant I saw last week an erect flower with a perfectly straight tube and regular limb -- this also occurs in *Bolivocereus* and, I suppose, potentially in any genus with zygomorphic flowers.

It is good to know that the IOS favors belonging to the IAPT. I have belonged for two years now and find "Taxon" often of much interest. However, it publishes only articles on nomenclatorial matters. I heard today that your long paper is being worked on in the editorial office of "Madrona", so it should not be long before it is in print; they estimate that it will comprise 32 pages, or a whole issue.

W. T. Marshall died several weeks ago; Hubert Earle, formerly the superintendent there, is now Director. Haselton wrote me today that Howard Gates just passed away.

I have just written another "Icones", this time on *Epiphyllum cartagense*, a most interesting species morphologically. I believe I now have the phylogeny of *Epiphyllum* fairly well worked out, both as to species and as to the origin of the genus. I am certain (!) that it has nothing to do with *Mediocactus*, but rather that the genus is derived from ancestors resembling *Cryptocereus*; the more primitive the species of *Epiphyllum* the more conspicuously lobed are the stems and the more bristly the pericarpel. *E. chrysocardium*, the most primitive, grows near *Cryptocereus*, in S. Mexico.

Too bad you couldn't have attended the IOS meeting here. I worked on the *Borzicactus* paper all during the Convention trying to finish it, but finally got it done and delivered. I still prefer to recognize only *Borzicactus*, *Croya*, *Denmoza* and *Cleistocactus*.

Paul has sent back two shipments already, of *Haageocereus*, *Lobivia*, etc. As expected, he is finding that most of Backeberg's new species (from Rauh) are minor variants. He does not expect to visit Chile, and is behind on his schedule already. -- Dr. Goodspeed retired and our new Director is Dr. Baker, formerly of the Gold Coast.

Hoping you receive the plants in good condition, I remain,

Sincerely, Myron Kimnach

10 October 1957

Univ.-Dozent Dr. Franz Buxbaum  
Judenburg  
Sackgasse 13/4/1  
Austria

Dear Dr. Buxbaum,

We just learned that it is necessary for us to have a permit from the Austrian Agriculture Department before we may legally send plants to your country. Without this Austrian permit, we cannot obtain the necessary U.S. permit for shipping out of this country. However, since there is so little time before it would be impossible for plants to arrive in Austria without damage, we will send the plants to you without any permits, either American or Austrian. They will be mailed within the next two days, so we suggest that you try and make some arrangement with your own agencies to permit their entrance into the country. We, of course, are not sure that they will be able to leave this one, but we shall try. Just in case that package does not arrive, we will send the seeds you wanted with this letter.

I hope that you will be able to manage things at your end of the shipment, and that everything works out to your satisfaction.

Sincerely yours,

G. B. Newcomb  
Staff Botanist

~~515~~

16 Jan. 1958

Dear Buxl.,

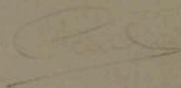
To let you know I am home again I am forwarding by post a small package containing four fruit of *Bergerocactus emoryi* (Engelm.) Britt. & Rose, UCBO 51.550. I enclose the annotation label for it.

Completely swamped with the expedition materials and backlogs of mail. I will shortly be able to send you some of the materials collected in Peru, and will have many subjects to discuss with you. *Neocardenasia* is beyond any doubt a *Neoraimondia*, and I think both, together with *Armatocereus*, go into *Lemaireocereus*.

That should shock you more than does my concept of *Borzicactus*.

All my best,

As ever,



Dear Paul ,

23.1.1958

thank you so much for your letter ! Fine that you are back again and I can imagine that you have now a lot of work to do. I got your letter just when coming back from Zürich where I was for 2 days to see Krainz and to discuss with him the further issues of "Die Kakteen".

For Krainz is very heavily blessed in the Hospital ! He was crushed by driving over with 80 km speed. Both legs heavily broken and two vertebrae too ! It will need many month until he will be able to leave the hospital ! It would be nice, if you would write to him a few nice words ! His Address is : Hans Krainz, at present Chirurgische Abteilung, Kantonsspital Zürich, Rämistrasse 100.

It is a very hard time for a man of Krainz's activity ! Fortunately he is very active in mind and thus he will overcome the time easier.

Naturally : Now I have double work to do, because the continuation of "Die Kakteen" for the following time will be my own work.

Besides of this I have presently very much pressing items : My book "Cactus culture based on biology" goes in the press in February ( I just have to correct the last correctings, and it will be in sale in April. Further I must very quickly perfect the Cereoideae-System for Madrono. This, also, will come in the April issue. And besides of this different other pressing works. Terrible !

Thus this letter is a little short, though I would have to tell you a lot of news !

But essential : Keep hand off of Lemaireocereus ! This genus probably is invalid, as the type species - so far I have already seen, is a Pachycereus ! The " usual " Lemaireocereus have such a stigma :

I will be very glad for your materials and see to elaborate it as soon as possible!

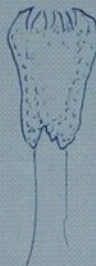
Please tell Myron my best greetings and many thanks for his wonderful work in edition of my Cereoideae ! I will write him as soon I will have finished the most pressing businesses.

Than : Welcome at home !

As ever Yours



OK areole



weakly evolved areole

higher evolved areole

Dear Paul ,

April 1, 1958

although I can imagine that you are in great trouble yourself with your materials from Chile I must beg you very very urgently for a copy of a Diagnosis . I need very urgently the diagnosis of

*Cereus giganteus* Engelman in Emory, Mil. Recon. 159. 1848.

If not at p. 159 than on p.158 ( see footnote in Britton & Rose Vol. II, p.164)

As I must deliver the manuscript for which I need this diagnosis in about two weeks, I beg you to send the copy by turning post. I think it will not be very long.

After short <sup>+</sup> will send you a longer letter, today I am too much in trouble.

All the best to you and the BGUC-fellowship !

Yours

29 April 1958

Dear Buxl,

Very sorry about the delay on this. I was sick for about 10 days and then on return to the garden, had to do quite a little research to find this very obscure publication. I have enclosed a copy of all pertinent information.

I hope this contains all of the information which you need.

Very sincerely,

P. C. Hutchison

29-10-58  
P.H. to C

Citation should be: Engelm. in Emory, Notes Mil. Recon., 159, 1848.

"Notes of a Military Reconnaissance from Fort Leavenworth, in Missouri, to San Diego, in California, including parts of the Arkansas, Del Norte, and Gila Rivers."

by W. H. Emory, Brevet Major,  
Corps, Topographical Engineers

made in 1846-7 with the advanced guard of the "Army of the West."

[begins on bottom of pg. 158 (one line) and immediately continues to pg. 159]

pg. 158. A gigantic cactus was observed along the Gila River about the middle part of its course,

pg. 159. at an elevation of from 2000 to 4000 ft.; it is frequently mentioned in the report from the ~~ix~~ first to the ninth of November, an figured on several plates. (P. 72-79) [This is a page reference, the plates are not numbered]. It most probably is a true Cereus. I judge so from the seed, which fortunately has been preserved. This is obovate, obliquely truncate at base, black, smooth, shining, small, (only about 0.7 lines long;) the embryo is hooked, the cotyledons foliaceous, incumbent; no albumen. If it is a constant fact that the cotyledons of the seed of the genus Pilocereus are thick and globose and straight, the plant in question cannot belong to that genus, which comprises the most gigantic of the cactus tribe.

The large Cereus, C. Peruvianus, is vastly different from our plant, which I would propose to name Cereus Giganteus. Unfortunately I can say but little about the character of this species. The stem is tall, 25 to 60 feet high, and two to 6 feet in circumference, erect, simple or with a few erect branches; ribs about 20, oblique or spiral, (?) no spines, (?) (Emory's Notes; probably only below without spines), fruit produced toward the top of the stem or branches. (None of the fruit was procured, being too late in the season, but the molasses expressed from it by the Indians was procured in abundance at Pimos village).

It is called Pitahaya by the Californians, but this appears to be a general name applied in Mexico and South America to all the large columnar cacti which bear an edible fruit; especially Cereus variabilis, which is common on the eastern coast but is widely distinct from our California giant.

Very truly,

Yours,

G. Engelmann

Dear Paul ,

June 21, 1958

at first, I thank you so much for the copy of Engelmann's diagnose. I regret it very much, that you have been ill but I hope you are now healthy again.

Now my first question : What is the matter with your coming to Graz ? It was already necessary to announce the lecture-programm for the winter-semester 1958/59 and for every eventuality I made my program with regard to your probable coming, such, that if you come you can get a whole course of my methodics. Maybe your collections of South America will fill your time very much and you would not have the time to come here. but I would regret this very much.

My secon question is this for materials. You have promised to send me seed materials and soil sampes from the roots of you collected *acti*. Besides of this I want to beg you very much for at least one preserved flower of an : *Armatocereus*, *Browningia*, *Weberbauerocereus*, *Neoraimondia* and *Eulychnia* ( incl. *Philippocereus*). These materials would be of greatest importance for my further researches. If you cannot spent me th them, I would return you the materials after examination including copies of my analyses ( drawings) and notes. This would be of value for the collection itself. Prof. Raub has promised me to send me from his collections and has already sent me - long ago - photoprathy of flowers and flower-sections. But these photographs are not sufficient for phylogenetic conclusions, and since a long time I did not more hear from Raub, not an answer to my letter nor any preserved material. Therefore I believe that Backeberg might be behind this fact. Therefore I want to be independent from Backeberg's friend . i.e. form Raub's materials.

As to *Armatocereus* : A few days ago I have got preserved flower materials of many species of *Lemaireocereus* and relateds form Helia Bravo and conferring these with Raub's pictures of flowers of *Armatocereus*, I am absolutely sure, that both are not related ! *Armatocereus* is so far I know *Leptocereus*, near to this genus as well as somewhat less closed to *Samai-paticereus*.

At the other hand : *Lemaireocereus* does not exist as a valid genus. My examinations have proved that *Lem. hollianus*, the *typ-spec.* of the genus is a typical *Pachycereus* ! So far my examinations have shown hitherto it seems that all former *Lemaireocereus* can be transferred into *Stenocereus* ( Berg.) *Rivcob.* which is the next validly published name.

Some days ago I have got a very interesting self-edited publication from Friedr. Ritter "Die von Curt Backeberg in /Descriptiones Cactacearum novarum/ veröffentlichten Diagnosen 'Neuer' peruanischer Fakteen nebst grundsätzlichen Erörterungen über taxonomische und nomenklatorische Fragen ". Ritter has published there his observations on his many voyages in Peru which have visited even all the Ways where Raub has been and many others. So far I have rwead the issue it seems to be very important. I will tell Ritter's sister Mrs. Winter to send a copy to you, if not perhaps she has done it before.

In connection with this, I should like very much to have your *Borzicactus* publication although - of even because - I do not agree with you in all point.

Myron has sent me last fall cuttings of different *Hylocereae*. It was a little too late in the year and as my conditions for winter time have not been satisfactory ( this year they are better ) I had little succurs

in rooting them. The *Weberocereus* spec. and *Disocactus* spec. did not and even *Epiphyllum chrysocardium* did not. Only *Eccremocactus bradei* and *Werckercereus imitans* rooted well, but although these have made big roots they do not grow and it seems that the areoles are already too old and not more capable to make shoots. Therefore I should beg you to send me again cuttings if the following: *Disocactus eichlamii and macranthus*, *Weberocereus* all spec., *Epiphyllum Chrysocardium*, and *Cryptocereus*. If possible it would like to have also a new cutting of *Werckercereus imitans* and *Eccremocactus bradei*, although I still hope that one day these may make shoot too.

Also the seeds of *Hyllocereae*, which Myron has sent, have not been of value, because these were unfruitful, and therefore I could not examine the inside. If perhaps you can send me fertile seeds of any *Hyllocereae*, it would be of great importance for me. especially I would like to examine seeds of *Mediocactus* and of *Cryptocereus*. From the latter I should like to examine myself also a flower.

Many requests, indeed, isn't it!?

These materials must not arrive in August, as I will not be at home this month. I will go to the Cote d'Azur and the Italian Riviera to examine at San Remo, Italia the only flowering specimens of "*Vatricania*" in Europe. Last not least, I want to congratulate you and Mrs. Blos to the wonderful recently published "*Icones*" which are the best cactus-drawings and most complete analyses that exist anywhere!

Krainz is still very bad. Presently he makes his first 10-minute attempts of walking. I myself am very much overworked and not in the best condition. But now soon the holidays will begin and this year I will use them for a radical recreation! (Supposed, I can exist without working!)

Now, tell my best greetings to the whole BGUC-fellowship, especially to Myron, who will get soon a little surprise.

Hoping to hear a little more from you,

Always Yours

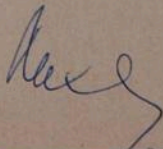
Dear Paul ,

July 4, 1958

some days ago I have got from Prof. Rauh his  
Book : Werner Rauh, Beitrag zur Kenntnis der peruanischen  
Kakteenvegetation, Sitzungsberichte d. Heidelberger  
Akademie der Wissenschaften, Math.-Naturw.Kl.,  
Jahrgang 1958, 1. Abhandlung. This book of 542 pages  
is most important for your work. Helas, the systematical  
part is made " in co-operation with C. Backeberg  
and it looks like this. You will have to do correcting  
it.

Hoping to hear again from you,

always Yours



July 25, 1958

Dear Buxl.,

Yours of June 21 and your recent postcard are at hand. You will get some idea of what I am up against here from the fact that I am a month in answering your letter.

As things stand now there is no possibility of my coming to Europe either in 1958 or in 1959. Firstly I cannot afford it, and secondly I would not be allowed to leave, as the new Director here does not want me to be away at this time, due to the many changes here and the necessity that he finds of having me supervise certain programs. I have about given up hope of advanced study in Europe. In addition I am deeply committed on research projects----the Cactaceae of Peru, Cactaceae of Chile, and the genus *Adromischus*.

I have received Ritter's paper from Mrs. Winter, and found it very interesting. Ritter cites our treatment of *Borzicactus aurantiacus* in his paper, but from his remarks it is apparent that he did not read the article.

We will attempt to get a shipment of living cuts off to you but we cannot now do so before August, therefore we will ship at the end of August to arrive with you in September.

As to the pickled materials, I can supply some of those you request. Actually I have only fruits of *Browningia candelaris*, no flowers, but I have flowers of *Azureocereus* (= *Browningia*) and of *Cereus microsperma* (= *Browningia*) (one flower only) and several others I consider belong in *Browningia*. I have also flowers and fruits of *Neocardenasia* which I consider belong with *Neoraimondia*. Myron feels that *Neoraimondia* belongs in with *Armatocereus*. *Eulychnia* flowers I have, but I thought you examined them when you were here. *Philippocereus* hasn't flowered for us yet. *Weberbauerocereus* flowers I have, and also *Neoraimondia* and *Armatocereus*. It is simply a matter of getting the stuff together, which I hate to do right now as I am so fully engaged on material which is flowering in the collections now. Probably it would be better if I tried to mail these to you this fall, after the living material stops flowering.

I don't know how to get Rauh's book, as it isn't listed in any catalogs I know of. Eventually I'll probably see it listed and then will order it.

Nothing published on *Borzicactus* yet. We will of course send you reprints of anything as soon as available. I will definitely reduce *Erdisia* to *Corryocactus*, and the only question remaining is whether or not *Corryocactus* belongs in *Eulychnia*. I haven't yet examined the Chilean material collected six years ago!

Myron is preparing a monograph of *Disocactus* and of *Epiphyllum* which are excellent treatments. *Cryptocereus*---we haven't ever succeeded in setting fruit. It has only flowered twice! *Mediocactus*, seeds enclosed.

Best wishes as always,

PCH

Sept. 30, 1958

Dear Paul,

First: The seeds of "Mediocactus" as well as these of PCH.nr. 1521 arrived totally powdered by pressing! These of Mediocactus seem to have been sterile, as in the envelop was only a powder of testa cells without any embryo-tissue. It is a pity! PCH 1521 contains also embryo tissue-powder. It really is not possible to ship seeds except in a solide box which cannot be pressed. Even the seeds of different *Hylo-cereae* which Myron has sent me formerly have been powdered with few exceptions - and most of them sterile too!

Besides of this, I should need a little...an idea, what kind of cactus it might be: a short one or a big one. Maybe, than, that I can identify the genus. But I am not absolutely sure that it is possible. Within the fragments of PCH 1521 I have discovered a somewhat less destroyed embryo which seems to have been highly succulent with highly reduced cotyledons. This fact indicates that it probably was a seed which does not contain any perisperm (this - maybe - might have been visible within the fragments). The testa fragments show a slightly verrucose testa of ovate cells - hela: the most frequent type of testa! - and it seems from some fragments that the seed might have a large crater-like hilum, maybe somewhat similar to *Astrophytum*, which has a glossy testa of a *Frailea* which - contrary to *Astrophytum*, has small separate tubercles on it. The Testa fragments, indeed, are different even from the *Frailea*-type. But the fragments are broken so totally that it would be a hopeless beginning to confer them with my about 1300 seed-slides. For I am still far from having examined all my slides and to KNOW all kind of seeds. If you can send me at least one grain more, I would try to confer with my collection.

Now to yours of July 25. I regret it very, very much, not to see you in the next years! It would have been a fine co-operation again. But I felt it, that now your presence at BGUC would be necessary and your researches on the Chilean and Peruvian cacti, indeed, is extremely important - as Rauh-Baskeberg have shown once more.

Rauh's book: Rauh W., Beitrag zur Kenntnis der peruanischen Kakteenvegetation, Sitzungsberichte der Heidelberger Akademie der Wissenschaften, Math.-Naturwiss.Klasse 1958, 1. Abhandl. " is printed by Springer-Verlag Berlin-Göttingen-Heidelberg and costs DM 113,40. It certainly will be available by any book-shop.

As you can see in "Madrono", I have already united (with "?") *C. microspermus* (*Gymnanthocereus* Backeb. *Gymnocereus* Backeb.) with *Browningia* and *Neocardenasia* with *Neoraimondia*. Having seen now Rauh's illustrations, I am sure that even *Azureocereus* belongs to *Browningia*, you are aright.

As to *Armatocereus*, *Neoraimondia*, I would not unite these genera. There is a relationship, indeed, between *Leptocereus*, *Armatocereus*, *Samaipaticereus*, *Neobabottia* and, also, *Neoraimondia* (the relationship following this order of names). But in this very old group of cacti the genera have differentiated too much to unite them, except by forming very inhomogenous "sample-genera" again. This is true also for *Glychnia* and *Corryocactus*, which have a very different morphological type of receptacle.

Presently I am working on *Pachycereae* with very fine materials which I have got from Helia Bravo and Hertrich. Even this tribe is very complicated, but my present materials probably will allow me to clear up its evolution. One point is already clear now: *Lemaireocereus* does not exist validly: The Type-species, *L. hollianus* is a *Pachycereus* whether or not all other species of former *Lemaireocereus* can be united with *Stenocereus* Riccob. is not yet sure.

A very interesting result of my recent examinations: *Vatricania* Backeb. is invalid and absolutely identical with *Anrixanthocereus*

Backeb. ( he did not know the structure of the flower neither of the one not of the other, otherwise he would not - maybe - have " fabricated" the genus *Vatricania* ) Rauh does not agree with Marshall's ( and my ) opinion, that even *Thrixanthocereus* is synonym to *Facheiroa*, but now, having discovered the indentity of theperuvian <sup>hrix</sup> and the Bolivian *Vatric.*, I am more than before, that Marshall was aright. But : There is the difficulty : There doesnot exist anywhere flower material of ~~the~~ *Facheiroa*! Ule's materials ( preserved in alcohol ) at Berlin had been destroyed in the war and Werdermann's picture of it in his book ist too small to recognizeany sufficient detail. Dont you know anywhere a flowering plant of it ? Or has'nt the UC a collector in Bahia, Brazil ? There is nowher anybody, to whom one could write. The Horto Florestal at Joazeiro seem not more to exist, as it is impossible to get any answer from there. And just~~y~~ near Joazeiro another very much important plant is living : *Micranthocereus polyanthus* ( *Cephaloc. polyanthus* Werderm. ) According to the description and Werdermann's photo of the seed, I am now sure, that thisvery interesting plant is a cephalium-bearing related to *Cleistocactus* ! But the livin g and preserved materiqls at Berlin ~~is~~ have been destroyed too.

I did not examine your *Eulychnia* materials , when I was at Berkeley, as always any living material was more urgent, and finally, you know, the time was gone too quickly ! If you will send me parts of your materials, I would send you than copies of my drawings ( although they will not be so excellent as Mrs. Blos's drawings ) and of my notes on it. You than can use my observations an can get your materials easier together. Our nice Co-operation must not be hindered by the - helas - so far distance !

As to cross fertilization for getting the seed structure: As a fact , such cross fertilization is only possible , if you use the pollen from thespecies with the longer pistill and bring it on the stigma of the species withthe shorter pistill. Otherwise the pollen-tube most times doesnot reach the ovules , because~~it~~ it cannot live~~x~~ and grow so long. In the German Journal Clarence Kl. Horich writes about his expedition which he has~~y~~ made for the UC to get *Wilmattea*. It would be extremely necessary to have seeds of this genus. ( at least one ). Have'nt you one for me ? But please , in a very solide box ! Marnier has ~~with~~ *Wilmatteas* ( so far I remember two plants, and as I wrote him for seeds he has fertilized one~~y~~ flower, but~~y~~, there is the question, whether or not the fertilization will have an effect, as~~y~~ the only flower he had was already two days open.

One thing more 'What are doing your *Discocactusses* ? Did one of them already flower ?

And do you know anybody who would be~~y~~ able and willing to send me materials of *Leptocereus* from *Hispaniola* ?

Your plant materilas did not yet arrive. I believe itneeds more than 6 weeks. Nevertheless I hope I will get it in good condition, for there is not yet the~~y~~ danger of frosts.

So much for today ! I, also, have now to do very many things ( for the school ) which I hate to do ! "his is everybody's "kismet" !

All the best

Always Yours

BUXBAUM, Dr. Franz, 13/1/4 Sackgasse, Judenburg, Austria (for research)

Sent air-mail Oct. \_\_, 1958, the following cacti.

- 52.1091 *Weberocereus biolleyi*
- 52.1090 *W. tunilla*
- 52.1086 *W. trichotus* CLONOTYPE
- 52.11083 *Werckleocereus imitans* CLONOTYPE
- 52.1079 *W. tonduzii*
- 57.239 *W. glaber*
- 57.705 *Cryptocereus anthonyanus*
- 51.953 *C. anthonyanus* CLONOTYPE
- 54.1242 *Epiphyllum chrysocardium* CLONOTYPE
- 51.552 *Disocactus eichlamii*
- 51.951 *Disocactus macranthus* CLONOTYPE
- 56.771 *D. macranthus*
- 56.767 *D. macranthus*
- 52.1084 *D. himantocladus*
- 57.235 *D. (Bonifazia) quezaltecus*
- 52.1087 *D. acuminatus*
- 57.049 *D. ramulosus (Rhipsalis coriacea)*
- 57.244 *D. biformis*
- Rhipsalis leucoraphis (=R. aculeata?)*, PCH, Bolivia.

8 Oct. 1958

Dear Buxl,

Thanks for your letter of Sept. 30. We will try to get together a batch of seed to send to you boxed. Your deductions about 1521 are remarkable---it is a short one, Peruvian. I'll send more seed.

The prices on both Rauh's and the Backeberg series are very high and I have delayed buying, but UC library will get them so I'll at least have them available. Glad you agree about *Browningia*---I predict the discovery of at least two more species on the eastern slopes of the Peruvian Andes, and I think I know where they will be found. I think there is only one *Azureocereus*, contrary to Johnson, who believes there are several. Myron has a plant of *Facheiroa*, but a young one. We do not know how long it will take to flower it, but when it does, we will preserve the material for you to examine. No contacts in Brazil except through their Dept. of Agriculture and, of course, the botanical garden at Rio de Janeiro. I will try to get some *Eulychnia* and other materials off to you this winter, in fluid---I have not even had an opportunity to open the vast quantity of pickled material I took in Peru. We have not yet sent your plants. Yesterday we prepared a shipment of 19 items and these we will send to you air-mail within the week, as soon as they are inspected.

- 52.1091 *Weberocereus biolleyi*
- 52.1090 *Weberocereus tunilla*
- 52.1086 *Weberocereus trichotus*, sp. nov. (prob. by Kimm. & Johnson) CLONOTYPE
- 52.1083 *Werckleocereus imitans* CLONOTYPE
- 52.1079 *Werckleocereus tonduzii*
- 52.239 *Werckleocereus glaber*
- 57.705 *Cryptocereus anthonyanus*
- 51.953 *Cryptocereus anthonyanus* CLONOTYPE (a frt. is developing.)
- 51.552 *Disocactus eichlamii*
- 56.771 *Disocactus macranthus*
- 56.767 *Disocactus macranthus*
- 51.951 *Disocactus macranthus* CLONOTYPE
- 54.1242 *Epiphyllum chrysocardium* CLONOTYPE (a frt. is developing)
- 52.1084 *Disocactus himantocladus*
- 52.1087 *Disocactus acuminatus*
- 57.235 *Disocactus (Bonifazia) quezaltecus*
- 57.049 *Disocactus ramulosus* (= *Rhipsalis coriacea*)
- 57.244 *Disocactus biformis*
- Rhipsalis leucographis*, leg. P.C. Hutchinson, Bolivia (= *Rhipsalis aculeata*?)

Your work on *Vatricaria* sounds good and comes as no surprise to us. We have no *Wilmattea* seed, but should get our first flowers in 1959, and seed. Will send if we get. One of our *Disocactus* flowered and we have it in fluid. We will send it with the others. No contacts in Hispaniola and no sources yet for *Leptocereus*. Please see that Marnier gets cuts of all of the above after they develop. We today prepare for him, also, a shipment, different from the above. I must side with Myron that *Rhipsalis* is probably not derived from the above assemblage, but rather from *Corryocactus*-like ancestors. Your arguments were not convincing, and certainly did not refute our evidence. 1521 is a *Borzicactus* (*Matucana*) which looks like a *Gymnocalycium* or *Lophophora*-with-spines. Floral structure is a shocker, and the seed is a strophiole. Hope all is well with you---am very rushed now on *Adromischus*, drafting the revision, and preparing diagnoses of 30 new species.

15 April 1959

Dear Buxl,

Luck seems to be with me, for the gamble I took in choosing to go to S. America and stay with UC seems to have paid-off. I have won a Guggenheim Foundation fellowship to cover a year of study here and in Europe on the Cactaceae of Peru, and will leave for St. Louis, Washington, D.C., and New York on July 1 and probably will arrive in Europe sometime in August. My round-trip ticket will be to Vienna, so that I fully expect to be able to spend some time with you. I have not written to anyone else in Europe yet, but I am hopeful that I can spend some time at Les Cedres, or near there so that I can work on the plants there. Is there any possibility that we could be there at the same time? My schedule, at this point, has to be open, as I am unsure how long herbarium work at the various institutions I will visit will take. I note that the IOS meetings will be at Kiel from 31 Aug. to 4 Sept., and it is possible that I will be able to attend, although to make it will require very speedy work at a number of institutions, on my part.

Before I leave I'll get off to you the promised pickled material. You have no idea of how difficult it has been to do anything at all on cacti since my return to Berkeley, for I have been swamped with assignments of all sorts. This is just a quick note to let you know how the Guggenheim turned out, and to thank you for your support, which was important in making the award possible for me. So we can now get together again, and I cannot possibly say to you with what relish I anticipate our reunion.

All my best,

As ever,

P. C. Hutchison

Univ.-Dozent Dr. Franz Buxbaum  
Professor  
Judenburg (Austria)  
Sackgasse 13/14

Dear Paul ,

April 26, 1959

that's a fine new ! Just coming back from Graz where I had the first lecture of this semester , I have got your letter. I am very, very glad that you have got the Guggenheim fellowship and I congratulate you to this success !

How quickly time is going ! Since Octobre I "am writing" to you and now April goes to the end. Finally it was my fixed intention to write to you this week ( or at least the next ). But now it goes more quickly than ever .

I do not know your detailed program, but in any case you must stay at least several days as my guest here at Judenburg. I cannot offer you a splendid lodging, but you can sleep in my living room at the chouch and we will have a fine time.

I have the intention to stay just in August perhaps for a fortnight at Arma di Taggia-San Remo, Italy, not too far from Monaco and Les Cèdres. The time is not yet fixed definitively. I will stay there in "Pensione Helvetia", Arma di Taggia, Italia. There is only one difficulty which might make it impossible : The conflict between Austria and Italy because Southern Tirol. It happened several times that neofaschistic Italian teenagers and rowdys have demolished Austrian cars. I hope these actions finally will become hindered by the Italian police, because they have diminished the Austrian visits in Italy very much. Otherwise I would not go to Italy this year and there is no other way to the Côte d'Azur, except by a too far detour.

The most important collection which you must see in Europe is the Städtische Sukkulentsammlung at Zürich. Krainz at this time probably will be again in the hospital because he must again undertake an operation of his leg. I believe you know, that he had a very heavy accident in December 1957 and was in the hospital 11 months. But he will be very glad to see you and his assistant certainly will help you in any way.

If you can do it , it probably would be important to see also Rivière's collection "Pinya de Rosa" at Blnes, Gerona, Spain (Catalonia, near Barcelona !) and his neighbouring Foundation Carlos Faust "Marimurtra", which nevertheless, is not so important as Pinya de Rosa. Riviere has the largest collection of Opuntia in Europa.

The Botanical Garden and Herbarium of the University of Vienna probably will not be much important for you. I have not been there since several years and therefore I do not know how it has developed in this time. It has been heavily damaged by the war. The Herbarium of the State Museum does not more contain the cacti, as just this part of the herbarium burned in the war. Nevertheless, the State Botanical Garden Schönbrunn at Vienna is worthy to be seen and there are some good private collections.

This year, indeed, I have not the intention to go to the congress to Kiel. It would cost me much money and Kiel is the centre of other succulents but not of cacti and therefore not so interesting for me. But it would be very ~~mann~~ good if you would go there. You could join this trip with the necessary visit of Prof. Rauh at Heidelberg and with a visit of Dr. Cullmann's fine collection. Not too far from this you can see Ritter's plants at his sister H. Winter, Frankfurt/Main-Fechenheim.

I, also, have some interesting news for you. First: I will retire from the educational service and remain only at the University. At February 25th I will finish my 60th year and after this time I can retire ( after 65 I must). Therefore I will " become ill" in September and take an illness furlough at beginning new school-year, until I will have reached the retiring age. Thus I will be free from middle of July and can do what I want to do.

There is a reprint on Espostoa on the way to you by regular mail or, perhaps you already have got it. It certainly will be much important for you.

Recently I am working on the Pachycereae and I am near to finish the phylogeny of this tribe. I had got fine materials from H. Bravo and from Hertrich and thus I was able to do it. There are only a few materials which I am still missing. The Mexican I will get this year from Mrs. Bravo. But I would need very necessarily flower and seed material of "Pachycereus" gaumari. There was a flowering specimen of this species in the Knickerbocker Nursery at San Diego (photographed in Marshall-Bock). But G. Lindsay wrote to me, that this nursery does not more exist and the plants have been sold to Poindexter. Since Poindexter's death he does not know where these plants are. Do you perhaps know, who has the Poindexter collection now?

I thank you very much for the living plant materials. But there has happened a misfortune! Just when these materials have been on the way, we had - in October !!! - a very heavy frost period! Thus some of the plants have taken frost and I was not able to save them. The others are doing very well. It was the funniest winter I have seen in this country. Heavy frosts in the early autumn and then a winter without any snow, except in the high mountain, not too cold but absolutely dry! Contrary, Tirol had very much snow. In other years the snow can reach in our country more than 3 feet. Thus I must ask you again for some of these species. These are:

- 52.1090 Weberocereus tunilla
- 51.552 Disocactus eichlamii
- 56.771 " macranthus
- 52.1084 " himanthocladus
- 57.235 " (Bonifacia) quezaltecus
- 57.049 " ramulosus
- 57.244 " biformis.

Can you send me specimens once more? But please: Do send them directly to Krainz (for me!). The First Gardener in our Bot. Garden is stupid and Prof. Widder does not like me, and therefore there is no space in the greenhouses. Therefore I will bring the last materials too to Krainz (I will go to Zürich end of May) where they will be in the best hands and I can get it whenever I need it.

I hope you have got my book "Cactus Culture Based on Biology" and I think you may be pleased with it. The book has got excellent, really enthusiastic reviews, the English edition as well as now the German which was edited in March. I hoped and I would appreciate it very much, if you or Myron would give a report on it in the CSJA. I wondered that Haselton has made only once a short note without citation of the complete title. Just the words "Based on Biology" show the difference to other cactus books.

Finally, please tell Myron my best greetings. I will write to him soon. For the first: Generally I can agree with his concept of Borzicactus, but I would propose to elaborate the subdivision into Subgenera as soon as possible. Otherwise it brings too much trouble into the cactus-trade as well as into the cactus-friends, and it is too difficult to work phylogenetically, if the whole is one genus without grouping. Although, indeed, the whole group is a phylogenetic unit, it is necessary to subdivide. Only with regard to Morawetzia I would prefer to separate it. The staminodial scales above the nectary chamber are a characteristic which does not exist except in Morawetzia, Micranthocereus and Zehnerella squamulosa. In connection with the cephaloid inflorescence (I regret, I had not yet the occasion to examine it morphologically) it might be a good generic characteristic. - As to the Rhipsalide-problem, I cannot judge about, as I do not know Myron's arguments. Maybe, he is true, but I am very sceptic in this point.

Please tell me your program for Europa, at least approximately - Maybe I can give you some advise and maybe I can join to you for one or the other trip. This depends on how much Blandford Press ~~man~~ will pay this year. One think more: Would you be able to give lectures

to show slides on your Chile and Peru expeditions? I was able to do it. There are only a few materials which I am still missing. The Mexican I will get this year from Mr. Bravo. But I would need very carefully flower and seed material of "Pachycaulis" generally. I have a photograph of this species in the Kew Herbarium (photographed in Kew's Herbarium - Book) but G. Lindley wrote to me that this nursery does not more exist and the plants have been lost to the world. Since Lindley's death he does not know where these plants are. Do you perhaps know, who has the best collection now?

So much for today!

Yours always

*Handwritten signature*

I speak a very much for the living plant material. But there has happened a catastrophe! Just when these materials have been on the way, we had a winter!! - a very heavy frost period! This some of the plants have taken frost and I was not able to save them. The others are doing very well. It was the funniest winter I have seen in the country. Heavy frosts in the early autumn and then a winter without any snow, except in the high mountains, not too cold but absolutely dry! Contrary to the fact that in other years the snow can reach in our country more than 10000 feet. Thus I must ask you again for some of these species:

- 52.100 Pachycaulis (Lindl.)
- 51.522 Diacoccus (Lindl.)
- 52.111 " "
- 52.1084 " "
- 52.1085 " (Pencil) (Lindl.)
- 52.1086 " "
- 52.1087 " "
- 52.1088 " "

Can you send me specimens once more? But please: Do send them directly to Kew (for me!). The first specimen in our Herbarium has in a label and Prof. W. H. H. does not like me, and therefore there is no space in the greenhouse. Therefore I will bring the last material to Kew (I will go to London end of May) where they will be in the best hands and I can get it whenever I need it.

I hope you have got my book "Genus Oritaria based on Biology" and I think you may be pleased with it. The book has got excellent really enthusiastic reviews, the English edition as well as now the German which was edited in Kew. I would appreciate it very much if you or Nixon would give a report on it in the GSA. I understand that Nixon has made only once a short report without mention of the complete title that the words "based on Biology" show the difference to other genus books.

Finally, please tell Nixon my best greetings. I will write to him soon for the first time; generally I can agree with his concept of the genus, but I would propose to elaborate the subdivision into subgenera as soon as possible. Otherwise it brings too much trouble into the genus-trade as well as into the genus-trade, and it is too difficult to work phylogenetically, if the whole is one genus without subgenera. Although, indeed, the whole group is a phylogenetic unit, it is necessary to subdivide. Only with regard to Kew's collection I would prefer to separate it. The anatomical evidence above the necessary character are characters which does not exist except in Kew's collection. Kew's collection and *Leimnasteria pumilum*. In connection with the cephaloid inflorescence (or I regret, I had not yet the occasion to examine it morphologically) it might be a good generic characteristic. - As to the Kew's program I cannot judge about, as I do not know Nixon's arguments. Maybe, he is true, but I am very sceptic in this point.

Please tell me your program for Europe, at least approximately. Maybe I can give you some advice and maybe I can join to your trip on the other side. This depends on how much Lindley's time will be for this year. I think you will be able to give lectures

11 May 1959

Dear Buxl,

It was good to hear from you again. I don't yet know what my *sheddã* will be for certain, although I plan to leave by air around 1 July. My provisional plans were to go to Scandinavia first for herbarium work, then to work south, so as to take advantage of the better weather that time of year in the north. All of the places you have mentioned are on my list for visits. I expect to have a full six months for Europe and my entire time on leave will be one year. It seems to me to be best to plan on northern Europe first, then to work south as winter approaches, but above all, I do not want to have an inflexible schedule. At present finances will be tight, but there is a small chance that the Univerzãtý here will provide some financial support in the form of some sort of sabbatical; this would be most unique, but then the whole situation is unique, for I'm the first non-academic employ of UC to get a Guggenheim, so far as I know. They simply have no provision for such an occasion in their set-up here; so anything they do will be improvised. I have a letter from the Chancellor saying that they will do something. So I'm not laying final plans until they do.

News of your retirement is wonderful. Will there be any chance for you to come to the U.S. again. This is something we must talk about in some detail when I see you. I consider Graz the most important goal of my trip. Your suggestion to visit Gullmann and H. Winter is excellent—I had not planned on this but certainly I shall now do so.

The *Espositoa* paper received and excellent. Your conclusions match both Myron's and mine—in fact I was reducing *Thrixanthocereus* in my present work on Peruvian cacti on the basis of material I studied and collected in Peru in 1957. Myron has a juvenile *Facheirosa*, incidentally, which has the bristles which are of such interest to you. With regard your remarks on *Weberbauerocereus*, I note your footnote on pg. 155. My present attitude on this genus is still unresolved, but less firm in support of it. I do not think that I will recognize it in my treatment, for I have intermediate species now, and the differences from *Trichocereus* are in characters which are not basic ones. The affinity with *Trichocereus* is unquestionable, and recognition of it seems to be arbitrary. In such a circumstance I ordinarily do not recognize a group of species as a genus. However, I will have a firmer attitude after this year of intensive study.

When creating a subgenus, or subspecies, which contains the type of the genus, or type of the species, it is to be cited without author. So you should cite *Espositoa subgenus Espositoa*, not subgenus *Espositoa* Buxbaum. In this subgenus, at the present time, I recognize only the one species—*E. lanata*.

We will get the living material off to Krainz in about a week, along with several other items for his collection. We are just sending a shipment of 40 items to ~~Krainz~~ Vatrican, and this to be followed with a large shipment to Barcelona of *Opuntias*. I wish that we could meet in Zurich. Since I go there after the visit with you at Graz, perhaps you will be able to accompany me. *Morawetzia* is so close to *Oreocereus*, that I cannot see how you can separate it on the basis of a single character. My own reaction to cephaloid inflorescences is that they may be present or absent within a genus. The staminodial scales are perhaps unique in *Morawetzia* (and the *Loxanthocereus* group) but then one can expect species to differ from one another in different degrees, depending on age, degree of isolation, etc.

Yes, I can talk to the Congress, but it will probably have to be off-the-cuff, rather than a formal presentation of a paper. Several subjects occur to me: Adromischus, the genus concept in the Cactaceae with particular reference to cacti of Chile and Peru, and my two trips to S. America. The last can be slide-illustrated. I shall have all of the drawings of the Peruvian cacti with me, and I'll make an effort to have a duplicate set made for you, as soon as I receive some of the grant money. These must not be used in publication at all.

Lectures elsewhere depend on time available to me---in general I don't want to be tied down by obligations as I have a great deal of work to do and limited time and funds to do it in. I certainly shall not permit being put to any expense (travel) in order to give talks as that is not the reason for my trip to Europe. On the other hand, where a talk will fit into the schedule, well and good! The trouble will be that I won't, for the most part, be able to say when I will be where. And I would always have to talk in English.

It is unfortunate that I cannot meet you at San Remo, but of course I do not leave here until July. Can you tell me at what time I might expect good flowering (if at all) in the places I will visit---Zurich, Heidelberg, Les Cedres, etc.---or is the season about what it is here? If so I'll miss flowers altogether in most places. Perhaps I should consider not going to Europe until late winter so as to see the flowering material in Europe this time next year; but then I would miss the IOS meeting, which, though it is of little significance for my project, would give me an opportunity to meet all of the persons interested in the technical side of succulent plant study.

Hope this finds you well. I am swamped now with a 16 hour day, and working week-ends too. Write soon and help me plan my routing and schedule. Your knowledge of European travel will be an invaluable aide to me. I at present plan to fly to Oslo, Stockholm, Copenhagen, Hamburg (Kiel), London and home, then return from London before using the last part of my ticket to spend 3 or 4 months in Europe traveling by train. I can buy for \$125.00 a ticket good for 60 days on all trains in Europe; but I think that if I travel 2nd and 3rd class that I can go a long way on European trains, without the necessity to hurry because of the 60-day time limit on a ticket. So I at present think I will not buy such a ticket. What is your advice on this? Do you think it would be better for me to hit southern Europe first (in July).

Your cactus culture book is ~~the~~ one of the finest, if not the finest, I have seen, and I may review it---I've already recommended it to many persons. The taxonomic portion was especially interesting to me. We seem to be influencing one another, and that is certainly a healthy thing.

We have had a long, early, and very dry spring. Today the cactus garden is a glistening blaze of flowers, and Tricho. schickelantail is a mass of over 100 flowers this morning with hundreds of excited bees at work on it. I must now mail this and pick up my mail, then get to work pressing flowers. All my best to you.

As ever,

PCH

Dear Paul ,

June 22, 1959

Really in the last moment I can find the time to write you a last letter to USA and I hope it will reach you before your start.

I had very businessful weeks since your last letter. As I had written to Vatrican and to others to ask them about to flowering time of the "Peruvians" - and did not answers at right time, I have lost the time to write you earlier. In this time I was in Switzerland to speak with Krainz ( who again is for about 2-4 months in the hospital ) and to have a discussion on a phylogenetical-chemical problem which I will publish commonly with a Chemist of the Zürich university, and , finally had a lecture at Bern: When I came back, I found a letter that I must give a lecture, which was proposed for October, already last week. This was a lecture on my recent researches on the phylogeny of the tribe Pachycereae, and the same lecture will be read on the congress at Kiel. But I had not yet the drawings perfected and for the Congress I must have slides of them. Thus I am now continuously occupied on my writing table, so far I am not occupied by my official service ( which I will finish definitively in a month. All this is the cause that I did not write you earlier.

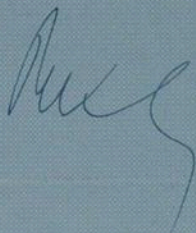
I can tell you only, that you will be very welcome everywhere - but no one did answer my question, which time would be the best. Generally, it will be the same as in California.

But, does no matter ! There very much will be to be done, although the time will not be the flowering time.

This week we have the final examinations of the highest class of our school, corresponding ( not exactly ) to the bachelor. Thus I am hindered more again in my work for nearly the whole week.

Thus, finally : have a good trip ! And I am glad to see you in a short time !

Always yours



30 June 1959

Dear Buxl,

All factors considered, I have decided to reverse my trip, and visit Europe after I have first spent four or five months on my manuscript here. This means that I will miss the IOS meeting, which is too bad, but everything else works out much better. I will arrive knowing exactly what to look for, I can expect to arrive in Europe when material is beginning to flower, and to be there during its season. At present I go on leave 1 August and will work here until I have covered all materials at hand. Then to the eastern U.S. herbaria, and finally, by about March, 1960, to England. I will then arrive on the continent around the middle of April. Thus, perhaps, I will be able to see something of the spring there.

I have yet to hear from the University about my leave----this is one large part of my decision to delay departure, for they are considering providing some financial support.

Thank you for yours of 22 June. I think you are right that I had best count on flowering time in Europe as about the same as here, namely, from May through July and even into August. I will plan to stay on the continent until about 15 August, providing my money holds out long enough.

I have been moving my books and everything home and setting up a laboratory there, as well as supplementary space at the herbarium. It has proven impossible for me to work here at the garden----too much interfering. I am anticipating this year for research tremendously----I once calculated that my total amount of research on cacti came to no more than 6 months full time; for I have always had to crowd in research in little bits and ~~pieces~~ pieces after other work. It should be a wonderful year and a rich experience.

Best as always,

PCH

*Chil*

Dear Paul,

it is now a long time, since I have written a longer letter to you and I have not yet answered different questions of your letter of May 11. And now we have August!

I was extremely much occupied with the Pachycereae, and as this was my intention to have a communication at the Congress on this problem, I was hurried very much to elaborate not only the communication in a short report on the researches but also to perfection the more than 100 drawings for it. These had to be photographed for slides. It was rather a hard work, but it is finished now and, as I will not go to the Congress this year myself, Dr. Cullmann now can read it for me as well as if I would be there myself.

The phylogeny within the tribe Pachycereae now is ~~clear~~ cleared except of *Cereus* (Polaskia) *chichipe*, *Lophocereus* and *Myrtillocactus*. *Chichipe* probably is the missing link to *Lophocereus* and to *Myrtillocactus* which I now am sure to belong to this tribe. (I believe, this now was a funny English, isn't it?). Over tomorrow I will start for the Mediterranean coast, especially Sanremo, Italy and, for perhaps two weeks to Monaco. There, in the Jardin Exotique I will get materials of *Myrtillocactus* and *Chichipe*, and I have already materials of *Lophocereus* (brought with me from Arizona and cultivated ones) and of *Myrtillocactus* which I had not yet sufficient time to examine.

In connection with these researches I was able to clear definitively the problems of the cephaloid inflorescences as well as of the polyflorous areoles! Yes, I agree with you, that it is possible that the same genus can contain cacti with or without cephalium! The cephalium can develop to a different degree, from none over the pseudocephalium to a true cephalium within one genus. This is true also in the *Cephalocereus*.

Although my report contains "only" 20 pages, the final publication will be a work of at least 200 p.!

Thus you can imagine that I was "gone" for several months but now to you: Although it is regrettable indeed, that you, also, will not come to the Congress, I am glad that you will come to Europe in spring. This is a much better time and, maybe than we can go together to Monaco and to Marnier. For, then I already will be free from my official service.

For the trip through Europe, I believe you will be best advised by Cook & Sons (in Europe called Compagnie des Wagon-Lits, Cook) or by the American Express co. I do not believe that such a 60-day's ticket would be opportune for you. In the second class (there is no third in Europe since several years) you will do better with separate tickets; at the other hand you can get e.g. in Switzerland and so in Austria less expensive round-trip tickets (about 30% less) I did so when I was visiting Krainz in May. In this way you can see very much of the beautiful countries. Probably in Germany, also, such round-trip-tickets will be available. Coming from England you can (and must) see Baining's collection (many species of Akers!) and then passing Rauh at Heidelberg and Cullmann at Martheidenfeld go to Mrs. Winter at Frankfurt a.M. (no: first winter, then Cullmann is easier). At this way you can come to Munich (München) where, maybe you will be interested for the botanical Garden of Nymphenburg. (I do not know whether or not it will be important for your researches. And then you have a direct train to Villach and from there to Judenburg. This is a very wonderful journey, and if you have time a day you might leave the train at Bad Gastein and see this very beautiful health-resort. Your most important goal will not be Graz, but Judenburg; this is 80 miles from Graz. For I go to Graz only for my lectures at the University every Monday and the other part of the week I am at home at Judenburg. The cactus collection of the BG of Graz is a very desolate one. There is nothing of any importance. (Neither the chief professor!) But in my home you will find all we need for a great number of things! It will be a very pleasant time for both!

letter 1957

H.P.H.

Thank you for the communication on the Facheiroa-seedling. Thus, once more, Rauh is badly blamed by his "Backebergisme". It is a pity! Why he did not follow my advice to keep hands off from Jackeberg!

As to Weberbauerocereus, this genus indeed belongs to the Trichocereinae. But the inner structure is so much different from Trichocereus, that I easier would unite it with Haageocereus than with Trichocereus. Thus I believe, your first impression to recognize the genus was correct.

As to Morawetzia, it would be necessary to examine many flowers of many different specimens, to know whether or not the staminodial scales are ~~more~~ constant. If so, then I would believe to separate the genus or at least make it a subgenus. For ~~much~~, such staminodial scales do not exist except in Morawetzia, Micranthocereus and Zehnerella (according to the literature - I had never the occasion to see these flowers). Both the latter genera I believe now to belong to the Porzicaetinae!

Yesterday I got the interesting mail that the German edition of my cactus book already is out of sale! Within 5 months! This was very much quicker, than the English edition, with still is available. It really would be very nice - and useful for me - if you soon would review it in the Cactus & Succ. Journal!

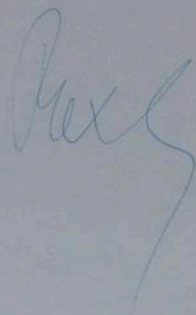
And - finally - do not forget the promised materials. And if possible: Send me please preserved flower-buds of Morawetzia in very different age, from the littlest ones; you can imagine my intention: Examine the development of the staminodes! And for my researches on the polyflorous areoles I should like to have a preserved branch of a flowering Disocactus macranthus with solitary and with polyflorous areoles.

I am joining to this letter a declaration which I have sent to Patrican for the congress. I think it will be of interest for you too.

Now, that's all. Overtomorrow I will start and I will come back towards ~~them~~ middle of September.

ALL THE BEST TO YOU AND TO THE "Botanical Garden  
fellowship!

As ever Yours



To the  
V. I.O.S. Congress  
at  
K i e l

n.l.  
12 May 1959  
Firm B + IOS

Dear Friends ,

it is the first times since the memorable foundation of the I.O.S. at Zürich, that I do not participate personally to an I.O.S.-Congress. There are different personal causes which have hindered my coming to Kiel. But Dr. Gullmann will read my lecture, as well as he is authorized to vote in my name. As Kiel is the center of succulents of other plant families, this congress probably will firstly engage in these, as more as also my friend Mr. P.C. Hutchison cannot come to Europe before the next spring.

When I am able to show you in my lecture a new very essential progress in Cactus research, you may take it especially as an success of the I.O.S. itself. For, my researches would not have been possible, if not our Mexican member, Mrs. Prof. Dr. Helia Bravo-Hollis would have don all possible to send me the necessary materials as well as to help me by communications by letter.

Therefore I want to beg the Congress to thank Mrs. Bravo officially by resolution for her excellent co-operation. I am sure that Mr. Rivière de Caralt will write a fine Spanish letter, although Spanish is not an official congress-language. And, it is true, Mrs. Bravo certainly will be pleased on this.

There is one point , because of which it would have been useful if I would be at the congress personally. It is an unpleasant point, but fortunateln caused outsidess of the I.O.S. Nevertheless ~~it~~ it seems necessary that the I.O.S. engages in it.

In his book "die Gactaceae" Vol. I. p. 83 , C. Backeberg has written in a footnote to Neobuxbaumia, that he cannot acknowledge my emendation of Neobuxbaumia, because I have joined to this genus his Rooksbya, the flower of which I did not know and probably do not know yet. He cites in this connection not only my publication in "Die Kakteen" 1957 but also the fundamental publication in the french Journal "Cactus" 1953, where I have published two full page tables containing all details of the flower as well as of fruit and seed. You will see these ~~the~~ tables in my lecture.

There cannot be any doubt, that Backeberg knows this publication in "Cactus". For, at 1953 he still was in Les Cèdres and besides of this, the same issue of "Cactus" contains a publication of himself.

July 1959

Dr. Barbaun to the ICS-Congress 2

Thus it is evident, that he intends to prejudice my scientific reputation by this aggression as well as by the many others published in this book. It is an aggravating fact, that he did it in a book which is not only read by cactologists, which know me and my publications, but also by other botanists.

I must enter an official protest against this insult of my scientific honour, here, at the international Congress. And I beg the Congress to join my protest and to condemn officially these Backebergian methods.

In this way, indeed, Backeberg now has disqualified his book himself.

Besides of this, I declare officially, that in future I never will react against any of the backebergian aggressions. I will not give the honour of an answer to a man who uses such dirty methods.

It is regrettable, indeed, that the German Cactus Society has refused to print my defense against this aggression in the Journal "Kaktusen und andere Sukkulenten". This fact will have consequences.

With regard to this accident, I had heavy hesitations against the election of Prof. Rauh. For it was he, who has given new impulses to Backeberg co-operating with him. I am feeling a high esteem for Prof. Dr. Rauh as a prominent morphologist. Therefore, finally, I decided to authorize Dr. Cullmann to vote also in my name for the election of Prof. Dr. W. Rauh. We may confess: Firstly we all have been disappointed to Backeberg; Werdermann, Kupper, Graf Kruth, Krainz, Marnier and - myself. It was later, that we have seen through Backeberg's tricks.

By electing Prof. Dr. Rauh, I hope, we might lead him to a more closed co-operation with the true cactus-scientists. In this way he will turn away from Backeberg by himself.

And if we can reach this effect, then the I.C.S. Congress at Kiel already will have been a success.

And this is the effect that I wish you with my whole heart  
as yours always truly

Dear Paul ,

Dec. 20, 1959

as I did not hear anything from you, I have no idea, where you presently might be. Therefore I will try to reach you by the address of the Bot. Dept. at Berkeley and I beg you to write to me if you have got the letter.

Now there are a few months only, until you will come here and I am very glad of this fact. At the University I have announced for the summer-semester a Collegium privatissimum on the Dynamic Method of phylogenetical research. As the semester begins after easter time, you can immatriculate as an extraordinary student. May be it might be of use for you in a later time. I have left time and place of the lecture open and we can do it wherever.

Since I have left the official service at school, I have much more time and I was very busy. A very great and important publication on the Caryophyllales just is in print. I was able to settle definitively the origin of this order and to clear some much discussed, but nevertheless not yet cleared morphological problems of this ~~minute~~ Order.

My researches on the Pachycereae are ready except the one line which seems to lead from Polaskia chichipe to Lophocereus and Myrtillocactus. For this part I had not yet the materials. I am just preparing the manuskript for print. The results are very interesting and absolutely clear now! Mrs. Bravo had sent me so much materials, that it was possible to elaborate this somewhat confused tribe. At the congress at Kiel, where I was not personally, Dr. Cullmann has read a short extract of my results with slides of my drawings.

Everyone was extremely estonished about your idea to propose Backeberg for the IOS. ~~Oh~~ You do not know Backeberg, otherwise you would not have made this mistake. For it is not Backeberg's "Botany", why he is refused by us all, but his absolutely dishonest character! You may tell Backeberg anything new about your researches - next day he will have published it as his own result. Another example: In his book (!) he writes "Buxbaum has not yet seen the flower of C. euphorbioides and probably he does not know it hitherto and therefore he did amend Neobuxbaumia and has included this species" - although he knows that my publication on this species has two pages of drawings of all details of the flower, fruit and seed! In this way he tries to found undermine my scientific reputation! Everyone, who has tried to co-operate with him has made the same bad experiences: Eric, Knuth, Kupper, Werdermann, Krainz and myself. Finally Rauh never has given the permission to misuse his name by the "New species Rauh et Backeberg"!

Now, dear Paul, all the best for Christmastime and to the New Year which will bring a new, happy co-operation!  
As ever Yours

Dear P a u l ,

April 2, 1960

I am very glad to have - finally - an address, where I can write you! When I have got your letter, I just had the intention to try to write to you by address of the New York Botanical Gardens, as I thought, that you will start for Europe at end of April.

Now to your schedule :

The summer-semester at the University begins officially April 20, practically I will begin with my lectures about May 1. Thus I am at Judenburg, respectively at two days the week at Graz from beginning May until August 3.

At the University I have announced a "collegium privatissimum" " Introduction of the methods of the dynamik taxonomy" for you! It will be held in English and time and locality of the lecture is free according our intentions. I think, it might be useful for you, to have a certificate on a lecture at the University of Graz, even if you are not here the whole semester. If you agree, I will speak with the Dean of our Faculty and do the inscription for you. All other are formalities of no importance.

As to your Europe-trip : It is essential for you, to see the collection of Dr. Willi Gullmann, Marktheidenfeld /Main, Germany, because he has the most complete collection of old specimen of *Haageocereus*, *Eupostoa* and other South American species. From there you can go with him to Mrs. Winter ( cactus Nursery ) the sister of Ritter, not far from Marktheidenfeld.

The most important collection of Europe is the Städtischer Sukkulentengarten at Zürich ( Krainz ). It is the most important Protective Garden of the IOS, and there you will find also dry materials and notes from Ritter also.

Contrary to this, La Mortola (Ventimiglia), Berger's Garden. is not more of importance for cactologists. After Berger's death it has lost all the importance, especially by the war.

When you are here at Judenburg, we will do a trip with my car to Linz, where in the Botanical Garden of the Town there is an excellent collection of Rahn's peruvian "species" ( Matucana a.s.o )

Now to your arrival here :

Give attention, where the train stops. Most of the international trains do not stop at Judenburg, but at Zeltweg. Therefore you must write me exactly the day and the train of your arrival and I will await you with my car at the station, equal Judenburg or Zeltweg. There is no use for you to stay definitively at Graz, because most time I am here at Judenburg, and to Graz we can go together with my car. Besides of this the lodgings at Graz are extremely expensive.

Now, I must hurry because the Post office soon will be closed and it is essential to mail the letter still at Saturday.

I am very glad to see you soon !

All the best for your trip

warmest Yours

Best greetings to Mr. Denvor !  
and compliments to Mrs. Denvor

July 1960  
Sackness 13/1/6  
Dear Paul ,

April 16, 1960

at first : Please excuse me , that I did open your letter. As I am waiting for an answer from Haselton, I did not look for the address and therefore I did not see that it is for you.

Now to the seminar : It costs - nothing. Besides to this we can do it in my home at any time, because it is a " Collegium privatissimum" and I did announce it as " time and locality according to agreement". In ~~this~~ this way we can join it with our discussions on cacti, which, certainly will fill our time, and you will not lose any time for your work. I will speak about this with the Dean, and as I have your University-card, I am sure that it will be possible to do the immatriculation for you. I am very much interested to inform you more detailed on my "dynamic method" as I know that it is the only way to a phylogenetical system. In the last half year I was able to finish two big publications, one on the phylogenetic position and division of the Centrospermae (Caryophyllales) which is already in print and now recently the phylogenetical development and division of the Pachycereae which just is going in print and will be available end of 1960. Both problems never would have been possible to be cleared except by the dynamic method, and both are so clear now that nothing will be necessary to be changed in future.

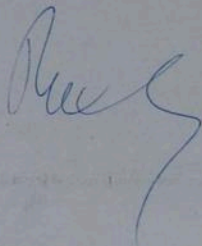
But now I have a request to you. Perhaps you will have a little time to see Mr. Denver once more. Probably you know that Mr. Denver is my licentiate for my fertilizer-salts for the Commonwealth and the USA. But it is very hard to get any answer from him. Therefore, if you might have the occasion once more, please ask him : 1.) for a settlement of account that means I want to know how much of the fertilizer he has already sold how it my fee aso. 2.) Whether or not he has edited my booklet "From A to Z with Buxbaum's Fertilizers" and if he did, to send me at least one issue of it. 3.) ~~to give me~~ to give you the last licence-fee for me. It would be more practical for me if you can bring it than if he would send it by bank.

Finally : Please tell me from time to time your addresses, where a letter can reach you and tell me your arrival at Judenburg about a week before.

Have a fine and successful trip !

all the best !

Yours



Dear Paul ,

June 20, 1960

As you have indicated, that you will come here in the first week of July, I have arranged for you a lecture for the Austrian Cactus Society at Vienna for July 9, 1960.

The Vienna cactologists will come for you here with a car and bring you to Vienna and will pay for you also the hotel. In this way you will have the occasion to see also Vienna. There is a cactologist ( amateur indeed!) who was many years at Chicago and he will make the interpreter for you.

But for preparing any kind of " programm" ( to get any income for the lecture ) ~~the Society would like to have any dates on your expeditions to Chile and Peru indicating where you have been and so on. If it would be possible, please send such short dates directly to the secretary :~~

Gesellschaft Österreichischer Kakteenfreunde,  
c/o Herrn Fritz Habacht,  
W i e n XIX, Heiligenstädter Straße 157.

Please tell Mr. Vatrican my best salutations, I will write to him in the next days ( to morrow I will start for a trip to Gmunden and Linz and that for my lecturing to Graz ). I have got his letter and I am not angry that he ~~did~~ not write earlier. But for any case I beg him to preserve several flowers of "Lemaireocereus" ( Polaskia ) chichipe for me, in the case that the plant flowers this year.

All the best !

Auf Wiedersehen !

Yours

Dear Paul ,

Dec. 22. 1960

It is allways the same ! First one thinks, there is still a lot of time for Christmas-correspondence, and finally there come anythink between and it is too late !

Nevertheless : My wishes are not less heartly and I and Friedl send you all the best for Christmas time and for the new year. Excuse me, that it comes too late. There just in the most disagreeable time I have got many galley-proofs ( of my great work on Centrospermae as well as of my Pachycereae-book and finally Friedl has got a heavy angina and thus I must be the "allround-man and houslady" dooing all my work as well as this of her.

Continued Dec. 26th. You may not believe it, but today it is the first moment to continue ! And so it happens so me very frequently ! Last summer I had a great misfortune. Just in San Remo I got a very little luxation of my right knee and the effect was a very heavy exsudation, such that I needed to lie 4 weeks ! In consequence of this, I was not sufficiently recovered and we made very often trips in the mountains in fall. But in this way I have lost much time and now I am missing it.

Recently I have got the second galley-proofs of my work on Centrospermae (Caryophyllales) and just now I am working with the galley-proofs of my Pachycereae-book. It will be available in the first quarter of 1961. - I regret it very much, that I did not hear from you more than the Christmas-greetings and the reprints. But I can imagine, how you are working now on your many dates brought back from Europe. (Friedl asked, whether or not you did think on her shoes ?!) We, both are "working animals", thus writing letters is a unwelcome interruption. Is it not ? But to day I want to send you all our best wishes for Christmas time ( a little too late) and for the new Year ! You may have all successes you want to get ! And please tell my best wishes also to the whole "Botanical Garden Society" especially to the Hoses ! But, as I saw that you did not give the UC-adress at your Christmas Card : Are you perhaps not more at the BGUC ?

Finally I want to beg you, if it would be possible to send to me copies of the following diagnoses, which I am searching already several month without any success:

Mammillaria corbula Herrera in Rev. Univ. Cuzco 8:61, 1919

( it is really Lobivia corbula as you know.)

Battora Britt. & Rose in St. Pailey standard Cyclopedia of Horticulture 3 : 1432, 1915

Zygocactus Schumann in Martius Fl. Brasiliensis 4/2 223, 1890

( just this volume of Fl. Bras. is not available in Austria ! ) and

~~Mammillaria~~ No, thats all for today ! I hope it will not give you too much work ! But please : as soon as possible !

I also have to send you the best greetings from the Baroness Brosch. She told me, that when you left Judenburg, the whole family was missing you very much ! She told me : "This never has happened to me before !"

Now, dear Paul, all the best also from Friedl ! We like to remember the fine time when you have been here and we allways hope that any day we will see another anywhere again !

Allways Yours

PS. By regular mail I send you the confirmation of the University of Graz. May be it might be useful for you!

3 Jan. 1961

Dear Buxl,

Thanks for yours of Dec. 22-26. I will check on the descriptions for you this afternoon and mail them on within a day or two. I am still at the B.G. but am doing quite a little work at home now so sometimes address my letters from there. I am sorry to hear of the bad luck you and Friedl have had this past year with health, but hope that things will be much better for you both this coming year. It was very good to hear from the very kind Baroness Brosch and her family. They were wonderfully good to me and I certainly very much enjoyed the pleasure of staying in their home during my short visit in Judenburg. And also, my thanks for the confirmation of Univ. Graz, -- how did you ever manage to get this? Anyway I am most appreciative and it will certainly be a wonderful souvenir of my stay with you. And who knows, perhaps it will someday be very useful.

I got home in September, and have been swamped since; in fact, I think almost everyone I saw in Europe has wondered what has become of me. I go nowhere, just work here, late hours, and even week-ends. I had intended to do some articles on the European trip for the U.S. Journal, and the English journals also want some, but there has been little time. Also much of my film is not yet printed, because it costs so much for so many prints! I have found the notes on Friedl's footwear and it is on my desk at home, but no chance up to know to hunt for shoes. Perhaps I'll be able to do this soon on a Saturday. I cannot on week-days as the stores close before I get home. I cannot guarantee that I'll find what she wants, nor that they will be the right size or fit or anything, for I have never bought womens shoes before, but I'll try anyway.

It has been a good winter here. There was plenty of rain in November, which was badly needed, and December has been very dry which was fortunate for holiday festivities. The winter has been mild, no frost until last-nite, and so far, no damage to our plant outside. Nearly all the plants (some 2000) which I sent back from Europe have survived the trip very well. Our biggest single acquisition was the J. T. Bates collection of stemless Mesemb., from England. Of course we couldn't grow all the plants I sent back, so I have farmed-out many of them to Al Irving, Dodson, and Kimmach. But much of the more important material, including types of Rauh & Backeberg species, are here. They grow well. I hope to have the Cacti of Peru nearly finished by next winter. It will be a very busy time for me this spring, for I have to send much exchange material to all those who gave me plants in Europe---Marnier, Cullmann, Rauh, Andreae, Krainz, Vatrican, and many collectors in England. This will be very expensive and very time-consuming, but it is necessary nevertheless.

Work has indeed been so hard for me that I am rather tired since I got home. I lost about 20 pounds before I got back, and got off the plane completely exhausted, then began work immediately the next day and have been going ever since. A very busy time for me. In addition to all else I am now considering editing and publishing a new journal, technical in nature, to contain critical research materials on succulent plants, including facsimile reproduction of rare works, translations, bibliographies, biographies, collations, and original research papers. This will be a good place for longer or technical papers which amateur journals are reluctant to publish. Nothing final about this yet. It would be impossible to print the journal here, so am checking on possible foreign publication.

Glad to hear your papers will soon be out--- will certainly be happy to see the Pachycereae one in print. My very best to Friedl---I have not yet tried to make the strudel, but will do so one day soon. I fear there is little chance that it will taste anywhere near so fine as hers.

Best as ever,

FCH

January 5, 1961

Dr. Franz Buxbaum  
Sackgasse 13/1/4  
Judenburg, Austria

Dear Buxl,

Herewith is the requested description from Bailey, L.H., Standard  
Cyclopedia of Horticulture 3: 1432, 1433, 1915, which, incidentally appears  
unchanged in later editions of the same work.

"HATTORA (an anagram of Hariota). Cactaceae.

Upright cacti, allied to Rhipsalis.

Plants erect, branching; branches short, arising in 2's or 3's from  
tops of older branches, smooth and spineless, bearing several abortive  
areoles along their sides and each a large wooly terminal one from which

pg. 143

arise the fl. and succeeding branches: fls. terminal; ovary globular, naked  
or nearly so; sepals usually in 2 rows, the outer ones broader and short, the  
inner ones larger and more petal-like; petals distinct, narrow toward the base;  
stamens distinct, erect, borne on the disk; stigmas 5, erect, white. Closely  
related to Rhipsalis, with which as Hariota it is often united, as it was in  
the Cyclo. of Amer. Hort. — Some 6 or 7 species of Hariota have been de-  
scribed, but most of these are true species of Rhipsalis; 2 were recognized by  
K. Schumann. The following is in cult. Hariota, DC. (1834) is a homonym of  
Hariota, Adans. (1763), and hence the name Hattora has been substituted.

salicornioides, Brit. & Rose (Hariota salicornioides, DC. Rhipsalis  
salicornioides, Haw.). Plant upright, reaching a height of 18 in., richly  
branched; areoles hardly setulose or lanate; sts. cereiform, with cylindric  
or oblong-elliptic joints; mature or fruiting branches with verticillate,  
club or flask-shaped joints, with slender base, all apparently, as well as  
the fls. and fr., growing from the tops of joints; fls. yellow, funnellform,  
1/2 in. long; berry small, whitish. Brazil. B.M. 2461.

J. N. Rose."

The description in Flora Brasiliensis by Schumann proved to be too long  
to copy and I have ordered a photostatic copy today which I will mail to you  
tomorrow night. The description by Herrera of Lobivia corbula will follow  
shortly.

Very sincerely,

16 Jan 1961

Dear Buxl,

Just a note to say that we do not have Rev. Univ. Cuzco for 1919, and I find that there are only four copies of it in the U.S. I am writing today to Harvard University for a photocopy of the section on *Mammillaria corbula* Herrera, and as soon as I receive it I'll forward it to you. Meanwhile the other two items you requested have been sent and should be in your hands by now.

Sincerely,

PCH

16 January 1961

Librarian, Biology Library,  
Harvard University,  
Cambridge, Mass.

Dear Sir,

I find in the Union List of Serials that you have the following reference at Harvard, and I would appreciate it if a photocopy, in duplicate, using any form of reproduction (preferably cheapest), of it be forwarded to me together with invoice for costs.

Revista Universidad Cuzco (Peru) vol. 8, pg. 61, 1919. [L45 C95]

The text needed concerns description of the species ~~in~~ Mammillaria cortula Herrera, and if this text runs over to page 62, this should be included in the reproduction. I do not believe there is a figure, but if there is, this should also be included.

Thank you for any assistance you can give in this matter.

Yours truly,

Paul C. Hutchison  
Senior Botanist

HARVARD · COLLEGE · LIBRARY

CAMBRIDGE 38 · MASSACHUSETTS

DSG: 4159  
25 Jan 1961

Mr Paul C. Hutchison  
Senior Botanist  
Botanical Garden  
Department of Botany  
University of California  
Berkeley 4, California

Dear Mr Hutchison:

Your letter of January 16th has been brought to my attention by the Biological Laboratories here. Each issue of Revista Universitaria, Organo de la Universidad del Cuzco, is paged separately; your reference appears in whole number 28, June 1919 (volume 8), page 61. It is in "Contribución a la Flora del Departamento del Cuzco", by D. D. Fortunato L. Herrera, which extends from page 3 to page 98. Mamilaria corbula is number 408 in the list. "IV Orden. - Dialipétalas inferovariadas" begins on page 60 with "Fam. Cactáceas" under which four "Opuncieas" are listed (nos. 404-407); Mamilaria corbula is the first of the five "Equinocactéas" listed on pages 61-62.

The minimum charge for photostats is \$3.50, but since the description is so short, this transcript of the entire entry may meet your needs.

"408. - Mamilaria corbula,  
De Flores rojas. Cultivable como planta de adorno.  
También se menciona el M. senilis. Quebrada del  
Apurímac."

Italicized words are underlined in the above. If you require a photostat for verification, by all means order one, by writing directly to me.

Yours sincerely,

Dan S. Greenwood  
Photographic Reference Assistant

DSG/p

31 January 1961

Mr. Dan S. Greenwood,  
Photographic Reference Assistant,  
Harvard College Library,  
Cambridge 38, Mass.

Dear Mr. Greenwood,

Thank you very much for your letter of 25 Jan. 1961 (ref: DSG: 4159), in which you quote for me the information I desired from *M Revista Univ. Guzco* 8: 61, June, 1919 (whole no. 28), pgs. 61, on "*Mamilaria corbula*." Apparently no previous botanist bothered to check on this "description" which I consider to be insufficient and the name therefore not validly published. I have copied your notes verbatim and forwarded them on to Dr. Franz Buxbaum of Austria who requested the information, and I certainly thank you for saving me \$3.50 which you quote as the price of photostats there. That price was a shocker to me---here they are .75.

Very sincerely,

P. C. Hutchison  
Sr. Botanist

31 Jan. 1961

Dear Buxl,

I have from Harvard University a quotation of the description by Herrera in Revista Universitaria, Organó de la Universidad del Cuzco, 8: 61, June, 1919 (whole number 28. In his article "Contribución a la Flora del Departamento del Cuzco" by D. D. Fortunato L. Herrera, pages 3-98, "~~Mamilaria~~ Mamilaria corbula" is numbered 408 in the list. "IV Orden. - Dialipétalas inferovariadas" begins on page ~~60~~ 60 with "Fam. Cactáceas" under which four "Opuncieas" are listed (nos. 404-407); Mamilaria corbula is the first of the five "Equinocactees" listed on pages 61-62.

"408. - Mamilaria corbula.  
De flores rojas. Cultivable como planta de adorno.  
También se menciona el M. senilis. Quebrada del  
Apurímac."

This deserves some comment. First the description is inadequate and therefore I would treat this publication as invalid. It is not possible to identify a Lobivia from such a description. However it is possible to deduce that what Britton and Rose treated as this species is not the same. ~~Mx~~ Lobivia corbula Britt. & Rose, Cact. 3: ~~43~~ 56, Plate V, fig. 2, 1922, should be treated as a new species, and this is the plant that Krainz has that I told him was L. corbula, not L. pentlandii. It is L. corbula Britt. & Rose, non Mamilaria (sic) corbula Herrera, Rev. ~~Mx~~ Univ. Cuzco 8: 61, 1919. I will put a note about this in the U. S. Journal and meanwhile I suggest you hold publication until this appears. Britton and Rose cite "Near Cuzco, Peru" as the type locality but as you see Herrera gives "Quebrada del Apurímac" as the place of collection. This is ~~probab~~ a vague locality, but probably it was collected between Cuzco and Abancay or Ayacucho, an entirely different floral province from the highlands of Dept. Cuzco with practically no Lobivias, so far as I know, common to the two areas. L. corbula Britt. & Rose is from above Cuzco on the road to Puno, and even around the Peruvian side of Lake Titicaca (Juliacs, etc.), all these localities at very high altitudes south of Cuzco. The Apurímac is east of Cuzco and much lower, an entire days journey. Also Herrera gives the flower color as "rojas" or red, and L. corbula is never pure red, but always has yellow or orange in the flower, and sometimes is pure yellowish-orange with no red. Therefore Herrera's name applies to some other species, probably one of those now named by Backebérg, but in any case the matter is academic, since Herrera's name is not validly described.

I trust that this gives you the information you need. I suggest that you not publish this species in Die Kakteen until my note on it appears.

As ever,

PCH

Dear Paul ,

July 22, 1961

it's a very long time since I have got the last letter from you. But as Krainz let me read your last letter, I know, that you are very much in pressure and have not the time to write unimportant letters. Thugh it is excused !

Even I myself am very much occupied and do not write, except it is very necessary.

This case happened now. Although I have been chosen for Honorary-member of the California Cactus & Succulent Society and was rold that in consequence I will get the Cact. & Succ. Journal every times, I did not get any issue of this year ! When I was at Zürich I looked a little in an issue of this year and I saw, that there was an article of great importance for my work. Thus I discovered the missing of the Journal in my home. Would you, please, be kind and arrange this again so that I will get even the issues of 1961 . I think , you can do it by telephone.

Another thing : When I had mailed the reprints of some of my last publications ( containing my pub. on the Centrospermae ) I got back one of the enveloppes of which the address adnex had been swapped off by water. I am sure, that this ~~paper~~ envelope had been sent to anyone interestet on cacti in USA, but I do not know who this may be. Did you get these papers ? do you know anyone of my friends who~~ch~~ did not ?

My book on the evolution lines of the Pachycereae is printed now and soon you will get one issue of it. A followin work now will bring the clear ed up morphology of the cephaloid " inflorescences". I spoke about this already at the IOS congress and will write it as soon as I will find time. For this purpose I beg you to observe, whether or not Discocactus has really a cephalium. So much I was able to examine on an specimen ( herbarium ) which I had got from Brasil, it is not, as the areoles later ~~marginate~~ get ~~marginate~~ spines and become normal areoles so as it is in Malacocarpus ( Eriocactus ). And please do not forget to send me a flower of Discocactus ! You have promised it

The german edition of my book is nearly out of sale and I am preparing now the 2 nd edition. Contrary to this, the sale of the british edition is rather low, except in the first year when had been sold 1000 books. It seems, that in the USA the book is practically not sold, although you have written such an excellent report. Also I do not see any advertisement in the Cactus & Succ. Journal. How ist this ? Is Haselton angry to me ? I cannot get any answer from him . Can you do anything in the Society to forward the interest for my book ! ?

The best greetings from Friedl !

All the best, as ever Yours 

P.S. I have a ~~man~~ Menue-Card of the final bankett of the congress which I let underwrite by all for you. I will send it soon by regular mail.

PS! Please can you send me a seed of ~~him~~ Zygocactus opuntioides? ( obovatus ) One grain is sufficient, but please send it in a sufficiently hard box, otherwise it becomes pressed by mailing ! This would be much urgently !!!

16 Aug 1961

Dear Buxl.,

With this letter I am sending, by separate post, a flower of *Discocactus tricornis*, a plant imported by Dr. Meredith Morgan of Richmond, California (who is now dead) from S. Rocha, Rio de Janeiro, Brasil, without exact locality of collection. The name to species is not definite, and the original plant is dead so that I can't check it. It is the only entire flower we have ever been able to get from a plant of *Discocactus*. I'll try with our own plants when next they produce. Usually we find it nearly impossible to cut the flower below the ovary, as you can't see what you are doing. Myron has examined the cephaloid apex in some detail and says it is nothing but masses of hairs such as you find in *Malacocarpus*. This has been my impression also, from herbarium material I've examined here and in Europe. We have not cut up one of our plants yet and I'm reluctant to do this until we have good series of flowers and fruits. We have a few seeds on file, if these would be of any use to you.

I'll take up your membership in the Society here next week when the President of it returns from vacation. I have not been active in the local society for years, so don't know what has occurred. Their membership is now over 100.

I didn't receive the *Centrospermae* articles you referred to so that it is likely that the package returned to you was the one destined for me. Looking forward very much to your *Pachycereae* book.

Haselton would not be likely to advertise any book unless the publisher paid for the advertisements, or some special arrangements are made. You should contact him about this. Of course your book competes with his own, in one way, though probably he doesn't think of it that way. Most collectors want always the latest thing and thus buy all the good new books. In England the problem is probably that there are already so many cactus books on the market, and more being produced all the time. Thanks very much for the menu card from the Congress, which has not yet arrived, but which I'll look forward to receiving. I got a card from the Cullmann's with many signatures on it, but most of them I couldn't even read! About *Zygocactus opuntioides* we have no seed on file. Myron says he can set some seed next time if flowers, but that may not be for some months.

It has been a terribly busy year and I've done no writing to speak of. Perhaps this fall I shall find a bit more free time for that purpose. I have been unable to find anything like the sketch you gave me for Friedl's shoes, although I must admit I've only spent two afternoons (on Saturdays) looking. The old pair must be completely worn out by now. Finally, Haselton has been away for the summer months on vacation, so this may explain why you have had no reply from him. I saw him 4 or 5 months ago and there was certainly no indication then that he was angry at you or anyone else. He has been terribly busy however as the result of the fire which destroyed so much of his stock of books and of the journal. My warmest regards to that remarkable cook Friedl, as always.

as ever,

Paul

Dear Paul ,

Oct. 25th, 1961

it is a pity with our correspondence , is it not ?!  
at 16 Aug. you have written "With this letter I am sending, by separate post, a flower of *Discocactus tricornis*..." but up today it did not arrive.

So I have repeatedly promised to send you the certificate of our University as well as the dinner-card from the congress at Barcelona - and up today I did not send it. But finally, today I ~~then~~ send these items just with this letter and in this way , I hope, I will mail them really.

And so I hope , you might not mote forget to send me the *Discocactus* flower as well as the flowers of *Eulycnias* for my researches. Even these you have promised to send me, but I never got them and I would need them extremely necessary !

The California Cactus Society has send me last week the invitation to the meeting. I regret very much, that it was not possible to take a plan and to come there. But I see, taht the society has not forgotten me or - you already have told me that I feel forgotten. But nevertheless, I did not yet get the ~~Journal~~ Journal of this year.

besides of all this, I have today some questions to you and I beg you to answer as soon as possible.

1.) Whe I was at Berkeley, I made a drawing of a *Corryocactus* Nr. 53.509. Please : Have you already determined this plant, which in 1956 only was called *Corryocactus* sp. ? *melanostichus* ?

2. I would need exceedingly necessary a flower ( or the ~~cross-section~~ drawing by May Blos of the section ) of 1. *Gymnanthocereus*, 2. *Browningia* 3. *Armatocereus* and 4. *Acraimondia*. Perhaps - I hope - you have some from you Peru-voyage. I need them only for examination of the inner structure and I would send alle these flowers ( even this of *discocactus* and *Eulycnia* ) after having made my drawings, except, if you would tell me, that you do not more need them. In this case I would send them to Krainz for the Zürich-Herbarium.

To the next :

I think that now might be the right time to send me fruits and seeds of *Bergerocactus emoryi*, from the seeds if possible a greater portion of living ones, indeed. But please send seed always in a firm box, because otherwise they never arrive un-démolished.

Finally : How do you do ?

What are you doing ?

How is ~~it~~ it with your work on the chilean and peruvian cacti !? I would be very much interested on the clearing up of the *Neoporteria*-group !

This summer we have spent a very fine time in Spain ( 7 weeks ! ) and than I was 3 weeks in our mountains to elaborate there the 2nd german edition of my book *Cactus culture*.. .

Finally We have used the wonderful fall for a fine trip by car in our beautiful mountains ( even the famous Gross-Glockner road, 2500m)

All the other time , I am very busy and very much occupied with my researches and with lectures.

Hoping to hear and to see soon anything from you and with the best greetings from Friedl,

as ever  
Yours

sent Pch  
327 *E. acida*  
*Discocactus tricornis*

Univ.-Dozent Dr. Franz Buxbaum  
Professor  
Judenburg (Austria)

Dear Paul ?

Oct. 31, 1961

Today I have a very urgent question and I beg you to answer by returning post !

I wrote a new publication on the phylogenetic position of the genus *Corryocactus*, which soon will go in print. After all examinations it is clear, that *Erdisia* must be included into, *Corryocactus*. Even Rauh ( 1958 ) is of this opinion, although he still used the two genera separately.

As you have been the first, who told me, that these two genera must become united, but as you did not publish this fact and the new combinations, I beg you to allow me to publish the new combinations for you in my work as follows:

- ~~Corryocactus squarrosus (Vaupel) Hutchison comb.nov.~~
  - ~~Corryocactus philippii (Regel et Schmidt) Hutchison comb.nov.~~
  - ~~no. Corryocactus meyenii (Britt. & Rose) Hutchison comb.nov.~~
  - ~~Corryocactus spiniflorus (Philippi) Hutchison comb.nov.~~
  - ~~Corryocactus maximus (Backeberg) Hutchison comb.nov.~~
  - ~~Corryocactus apiciflorus (Vaupel) Hutchison comb.nov.~~
- (This species has been put into *Erdisia* by Backeberg.)

Have you any new combination more ? Or is anyone of these wrong ?

Rauh has published new *Erdisias* :

*Erdisia erecta* Backeberg in Fedde "ep. LI, 1942 S. 62

*E. tenuicula* Rauh et Backeb,

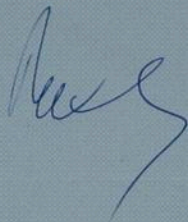
*E. quadrangularis* Rauh et Backeb. This species has been transferred to *Corryocactus* already by Ritter. The others he does not accept. How do you think about these ! *Tenuicula* seems to be a forma of *squarrosus*, as even Rauh himself says that it differs only by the more tender spines.

Please tell me also the name and origin of the *Corryocactus* BGUC Nr. 53.509.

Hoping to get your agreement very soon !

As ever

Yours





15 Nov. 1961  
Berkeley, Calif.

Dear Buxl,

In mine of 7 Nov. 1961 I missed spelling on my number 5, which should read Coryocactus tenuiculus (not tenuicula).

I have packaged the following to go-(air-mail) in plastic bags from pickled materials:

Discocactus tricolor <sup>None</sup> (without data, from a local collection) a flower lacking all of the ovary, the only material we have in fluid. Give this to Krainz.

Eulychnia acida, PCH 327, collected in habitat in Chile, a series of sections from young bud to flower to fruit and including seed. Give this to Krainz.

Browningia altissima (Ritter) Hutchins., ined. Ritter's name is good if this is not Cereus chlorocarpus HBK. I put both Gymnanthocereus and Azureocereus in Browningia, and the combinations are in my manuscript. Return all of this to me. A drawing of this is in your hands I believe, but if not, let me know and I'll send a photocopy.

PCH

~~1633x~~

~~1157~~

1476

Browningia microsperma (Werd. & Backeb.) Marsh. PCH 1653. This is my only flower and must be returned urgently as I've not yet had it drawn, or perhaps you can send me your drawing when you have done it so that I can use it after you have finished with it.

Browningia candelaris, a series of buds to flowers from Rauh from which I made our plate---don't you have a copy? No, of course not, as we completed it after my return. I'll get a photocopy and forward it. Please return this material. ---I'll send plate only at first.\*

Armatocereus cartwrightianus Br. & R. Photocopy of our drawing will be enclosed in the package.

Neoraimondia macrostibas (Sch.) Br. & R. Photocopy of our drawing will be enclosed in the package.

I have several other Eulychnias if you need more material. Bergerocactus emoryi has no fruits or seeds at this time of year. This will have to wait until next summer.

Still shopping for shoes for Friedl---yesterday finally my mother found a pair of correct design except for two things---the heel was only about an inch wide, and the front, while cut like your sketch, had transparent plastic between the interspaces, thus ceiling all but the toe which was open. \$11.00. I decided not to take them. Mother says this type of shoe is usually only available here in our summer and spring.

\*Since we have the drawing of Browningia candelaris, I'll send it before sending the pickled material, as this may be sufficient for you.

I also reduce Neocardenasia under Neoraimondia. I am uncertain where Armatocereus belongs, as I've not gone enough into the Mexican material, but I suspect it is not a good genus. I've had very little time for research and none for writing since my return. Perhaps next year will be better. Next fall I hope to go to Peru again, but am still uncertain about this. My best to that fantastic cook Friedl and to the Baroness and her daughters.

Sincerely,



27 Nov. 1961

Dear Buxl,

Sorry for the delay, but I found that we had no photographs of some of the drawings so that I had to take them to a photographer to have the work done, and this time of year that is always slow. I enclose photocopies reduced to 4 x 5 of the drawings which will be used in the Cactaceae of Peru, of:

*Browningia amstutziae* (Rauh & Backeb.)  
*Browningia altissima* (Ritter)  
*Browningia candelaris*  
*Corryocactus brevistylus*  
*Armatocereus cartwrightianus*  
*Browningia hertlingiana*  
*Neoraimondia macrostibas*

A fifth *Browningia* remains to be drawn -- microsperma, of which I forwarded flowers. For *altissima* fruit and seed are unknown. For *altissima*, I have not seen mature fruit or seed. I hypothesize the existence in Peru of at least two more species linking those known, geographically and morphologically. The relationship of the line to Caribbean and Yucatan species seems unquestionable.

The *Corryocactus brevistylus* drawing is, of course, of the type of the genus. The only remarkable character seems to be the bent six filaments of the lowermost series. This character is consistent in all material examined. Some of Backeberg's new species are of course identical to this one.

*Neoraimondia macrostibas* I consider to encompass the entire coast of Peru -- in other words I recognize only one species. The other is *Neocardenasia* of Bolivia and adjacent Argentina.

*Armatocereus*----I haven't worked out the species to my satisfaction. There are at least three in Peru, perhaps more, but I think not so many as Backeberg records.

I hope these will be of some use to you. Kindly do not reproduce them.

Sincerely,

P. C. Hutchison

Dear Paul ,

Dec. 3, 1961

I thank you very much for the pickled flowers as well as for the photographs, which arrived today. Helas, the heavy other flowers of the pickled materials had shivered to pieces the Discocactus-flower. Only the receptacle remained one piece. But my hope, that thus it would be possible to examine at least the inner structure was vain, because this fragment did not miss only the pericarpell but also the lower part ~~including the~~ of the receptacle containing the insertion of the lowest stamens. In this way this flower was a great disappointment, and I will try now to get flowers, at least herbarium materials from Brasil. My new method to soak herbarium materials with a solution of chloride of lime - I have sent you a reprint of this publication - allows me to refresh and bleach even dry flowers very well. But for the future, I would counsel to use the alcohol not more than 60 % !

The more the other materials and the photographs are extremely useful for my work. I see now with full clearness an excellently straight line of evolution, which I have already discovered in connection with my researches on the cephaloid formations, that means of the "cauline zone" of the flower. There is a continuous line of evolution from *Rauhocereus* ( according the pictures in Rauh's book), which has nothing to do with *Trichocereus*, over *Castellabosia* to *Browningia* in your sense which also is the mine. In the evolution of the cauline zone *Br. candelaris* has reached the highest state while in the seed *B. hertlingiana* and *altissima* seems to be higher evolved. As soon as Prof. Rauh comes back from Madagascar in December I hope to get a flower of *Rauhocereus* and than I will be able to confirm my theory. I hope than I will, may be, be able to discover also the origin of this line.

As to *Armatocereus*, I am already absolutely sure, where it belongs. This genus is the missing link connecting *Leptocereus* and *Corryocactus*. The connection to *Leptocereus* in habit and geographically is given by *Arm. humilis*, in the flower by *A. laetus* and *procerus*. The flower of *A. matucanensis* leads to *Corryocactus*. As the *Armatocereus*-flower is distinctly acrotonous as it is in *Leptocereus* too, while acrotony never occurs in the *Pachycereae*, *Armatocereus* never can be directly related with *Stenocereus* ( former *Lemaireocereus*) but only with *Leptocereus*. Even the seeds of *tenocereus* and *Armatocereus* are distinctly different, although in these rather primitive *Cactaceae* there is always a greater similarity in the seed than it is in the higher evolved genera.

*Neoraimondia* ( incl. *Neocardenasia*, which "genus" I never had accepted ) have acrotonic flowers too, but, as I can see in your photographs a very different arrangement of the stamens and therefore seems to arise from a very primitive pro-type of *Armatocereus*.

*Leptocereus grantianus* ( illustrated in Marshall-Bock ) is not a true *Leptocereus* but a *Samaipaticereus*, as it has spineless flowers.

As the "morphological Typus" of *Corryocactus* ~~is~~ is a pro-type of this of the *Notocactaceae* and nearer to these than to the *Leptocereae*, I put now *Corryocactus* and *Austrocactus* as the "subtribus primordioides" to the *Notocactaceae*. Contrary to this, *Armatocereus* is morphologically very closed to *Leptocereus* and thus a typical member of the Tribe *Leptocereae*.

"*Erdisia philippii*" is not a *Corryocactus* but - according to the very good description of the flower and the emended Diagnosis of *Austrocactus* a typical *Austrocactus*.

I do not understand, what you meant with a connection

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of Browningia with "Caribbean and Yucatan species". Do you mean Pterocereus?

All about Leptocereus-Corryocactus-Austrocactus-other Notocactaceae is already in print (Sukkulantenkunde, Switzerland). The different spination and the nude areoles of the Browningias is shortly cleared in the extract of my Congress-lecture in I.O.S.-Bulletin No.1 1961, on p. 8. I am preparing the detailed publication on all cephaloid formations, where this also will be treated exactly. But it still will need any time to its publication. Nevertheless, if you want and need to use my phylogenetical indications, you may do it. Certainly you than will indicate my authorship and send me a reprint of your publication.

When do you think, that your Peruvian Cacti will go in print?! and the Ghilwan?

The proofs of your new combinations in Corryocactus will be sent to you directly from Krainz. I think, this was an excellent way to publish them as soon as possible, is it not?

In a few days I will return the pickled flowers you want back. I would beg you still for flowers of Eulychnia castanea or another species with spiny flowers. Except the fact, that Eriocysce is near related to Eulychnia, this group still is a puzzle to me.

Finally, dear Paul, all the best for Christmas time and for the New Year from Friedl and from myself! And you may not more have any further trouble with shoes for Friedl as, finally, she discovered a mark which is adequate also in Graz! Also I have to tell you the best greetings from the Baroness Bros and her family. The oldest daughter is already teacher on a highschool!

And please tell all my best greetings and wishes also to all of the BGUC-fellowship!

Very sincerely as ever

Kours



Form PCH

14 Dec. 1961

Dear Buxl.,

Thanks for yours of 3 Dec. I don't see how you can tell much from Rauh's figure of *Rauhocereus*--I couldn't. I have the type specimen of *Castellanosia* here, and am uncertain of its affinity. I look for the origin of *Browningia* in a line connecting to *Escontria* and *Pterocereus*. In fact I don't see much difference between them. I flowered *Pterocereus gaumeri* last summer, and have the beginnings of a plate on it. In 1963 I expect to study all of these in the field, on my way back from South America. Present plans call for departure for Peru about a year from now, with a circuit of So. America, then return by surface from Panama in a jeep to collect Central America and Mexico. Hopefully some generic problems can be resolved during this trip, where I hope to get material in flower, fruit and seed of most genera. I expect to find more, connecting species.

I have never seen good material of *Leptocereus*, but wonder if you have compared it in any detail with *Samaipaticereus*. *Neoraimondia* seems to fit in here somewhere. The differences between *Neocardenasia* (with spiny flowers) and *Neoraimondia*, with spines merely delayed in development to fruiting stages, is paralleled in the *Pachycereae*. I told you in Judenburg that I felt that you gave undue emphasis upon what has become traditional thinking with regard geographical separations (North versus South America, etc.) and presence or absence of spines. I find in many of the *Pachycereae*, that the development of spines is only delayed to later stages of the flowering or fruiting. Hence I cannot find so many genera to be needed, even though you may be correct about evolutionary sequences. To take other parallels, you can find in the *Loxanthocereinae*, in *Borzicactus sensus Kimmach*, developments parallel in some respects to the *Pachycereinae*, and I find no reason to treat them differently in one group than in another. Not to say that I've reached any decision in the matter. I've not studied the *Pachycereinae* in any detail. However I'm suspicious of your taxonomic framework for this group from what I do know, even if your phylogeny is correct. I expect to go into this in more detail later---a few years, certainly not before I've done some field work on them. I won't expect to publish *Cactaceae* of Peru until this is done.

We have two lines leading from *Corryocactus*, one through *Pfeiffera* to *Rhipsalis*, and the other through *Austrocactus* to the *Notocactineae*. I'm afraid that I cannot agree with you that *Erdisia philippii* is an *Austrocactus*, at least not yet. I've collected this in the field and have good drawings. I now have living *Austrocactus*, but haven't flowered it yet. Up to now, I'm inclined to keep it in *Corryocactus* and to draw the line with it. It may prove necessary to place *Austrocactus* differently, but that too can wait, as next year I'll study them.

The carryover from Britton and Rose, expanded by Backeberg, of the concept of genera delimited by spiny versus spineless flowers, or by geography, is totally unacceptable to me. My concept of genera is too much influenced by what is done in other families with similar distributions, to accept such narrow delimitation for this one family. Hence I find *Leptocereus grantianus* acceptable, and as a corollary, *Samaipaticereus* unacceptable, at least on the grounds of presence or absence of spines on the flowers. Whether there are other grounds for reducing *Samaipaticereus* remains to be seen. I still have not examined living *Leptocereus*. Until I have seen flower, fruit and seed of several species, including the type, I will hold this question open. It seems obvious, however, that new species connecting the Bolivian and Peruvian materials with the Caribbean ones, will be

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found, eventually, since there seem to be few missing links in this family, PCH + B  
 Apparently primitive forms have survived even up to today, with all stages connecting  
 them still existing---a most convenient circumstance for the evolutionist.  
 My S. American venture, beginning about a year from now, and to last over a year,  
 will be aimed at investigating some unexplored (and very difficult) regions to  
 get more of these missing links. I expect to find some of them in Central  
 America, and even have an idea about where to look. Northern S. American will  
 provide additional ones I think.

Areoles of *Browningia*, in my sense, are not always nude. However it is  
 more or less typical to find at least 2 and usually three types of spination  
 on the plants in the columnar stages---juvenile, mature, and flowering stems.  
 The latter in *B. candelaris* are nude; in *B. microspermus*, nude except at the apex;  
 in *B. altissimus*, nude; in *B. amstutziae*, spiny, and in *B. hertlingianus*, spiny.  
 For *B. amstutziae*, only the flowering zone of the ~~stem~~ stem is known---Rauh never  
 saw this plant as it was brought to him by a native. I'll try to get it again  
 when I go down. I've a piece of Rauh's type, but it is still not rooted, after  
 over a year.

*Eulychnia castanea* flowers for us know, from my collections of seed of 1951.  
 Next summer I'll forward a flower from it. You saw the fruit of this species when  
 you were here, in pickle, but up to last summer I never had seen flowers. We are  
 drawing it, of course.

No, I don't particularly want to publish the *Corryocactus* combinations in this  
 manner, but since you want to use them it is all right. Generally, when I make  
 new combinations, or reduce a genus, ~~ix~~ the argument is presented at that time.  
 It is a bit unusual for one person to present the argument, and the other to make  
 the combinations, not knowing the argument, for my reasons may be quite different  
 from yours for all I know. But this is of no great concern, since I expect to  
 get a longer article out in 1962 discussing genera in this family.

Our *Discocactus* will probably flower again this year, as all plants are growing  
 exceedingly well, and if they do, I'll forward a fresh flower immediately. We want  
 to try to get seed this next time, if two of them will open at once.

I suppose that all of the above discussion really reduces to a few simple  
 statements of small differences in your and my methods and opinions about genera  
 in this family, largely reflective of different background and attitudes about  
 taxonomy. In the main we may find agreement about phylogeny. You are inclined  
 to publish opinions on far less evidence than I demand to reach similar conclusions.  
 Also there is often the question of what species you really had when you report  
 morphology of flower, fruit and seed, for vouchers are not cited. Citation of  
 vouchers is considered an absolute necessity nowadays to verify the names used.  
 There is some evidence that some of the things you have studied were not correctly  
 named. I. W. Bailey and N. Boke are now attacking the whole problem of leafy  
 cacti, with somewhat different results from yours, and this is probably attrib-  
 utable in part at least, to the fact that some of your material was wrongly  
 named. The only alternative to that would be that it was wrongly interpreted.  
 In the long run, though, your contributions to knowledge and to stimulation of  
 thought are so large, that other considerations are minor ones. I shall, meanwhile,  
 continue to plod along my own set and rather slow course and to come to my own  
 conclusions. Where these agree with yours, I am sure there will be great cause  
 for rejoicing. But you will know that I will never agree in the expression of  
 phylogeny in this family with the use of innumerable splinter-genera. The reduction  
 of a few Backeberg names, here and there, will not bring order out of chaos, even  
 if we know the relationship of these entities. Somehow some other solution has to  
 be found and with time and plentiful material it should become evident what that  
 solution will be. All my best to you both,

as always,

Dear Paul,

Jan. 22., 1962.

I thank you very much for your long and interesting letter. I just had already the intention to write to you, because I have made an observation which is very much important also for your work. But as your letter needs a longer answer, I had not the time to write it earlier.

At first my observation: To bleach the black flower of "Azoreocereus nobilis" (Brown. hertlingiana) which I have got from Johnson, I have put it - as usual - into a fresh solution of chloride of lime ( $\text{Ca}(\text{OCl})_2$ ). This is a very fine method which I am using for all equal bleachings, as it also removes the mucilage and even herbarium materials (dry!) become practicable again. But with this flower anything very extraordinary happened. The inner parts of the flower (it was dissected into two halves) became wonderful light blue after a short time! This never happened in any other flower, I have bleached! In consequence I have made the same with your half flower of Brown. altissima and finally with this of B. microsperma, always with the same result! (I did not bleach your flowers totally, therefore the outer layers remained black). For control, I made the same with a ~~Stenoc.~~ Stenoc. aragonii-flower, because it might have been possible that the becoming blue might have been caused by any impurity of the chloride of lime. But neither this flower nor others I bleached later became blue. Thus this change of colour is a new and very unusual chemical characteristic especially of this line of evolution, perhaps of the genus Browningia alone, and thus this proves the fact, that all these species really belong to Browningia. I think, this will be welcome for you!

As to the materials of Br. microsperma you have sent, I regret to tell you, that the whole inner part of the adult flower was totally demolished by any "worm" (larva); you can discover this bad animal between the stamens. There is not anything more to be recognized of the inner morphology! Even in the flower-bud, there was already such a beast, but this still was little and had not yet done much demolishing work. I have put this younger one into a little box and send it added to the flower material in the next days. Thus, I have made a drawing only ~~from~~ of the longitudinal section of this flower-bud. I have not yet made an ink drawing. ~~Wahh~~ Do you want an ink-drawing (you know my big drawings!) or is it sufficient if I send you a copy in lead-pencil, which can become fold? The latter I would send by Air mail, the large ink-drawing by regular mail.

The section of the bud was much important in connection with Rauhocereus and thus I am coming to your letter.

I was not too much astonished, that you could not see much in Rauh's pictures of Rauhocereus, although just these pictures show much more than others of Rauh's photographs. Probably - indeed - it needs 25 years of research of cactus-morphology, to recognize more. But I will try to lead you, and, I believe, than you will see it too.

There is already the first impression: The fruit (Fig. 175 / V). Imagine as absent the hairs at this fruit and you will see a fruit of Browningia candelaris (confer Rauh, Fig. 131 left on p. 278). You never will find a similar Trichocereus-fruit! - In Fig. 175 III, the outside-view of the flower-bud, also, the hairs imagined as absent, the bud becomes very similar to my flower of B. hertlingiana. Such large scales I never have seen at any Trichocereus-flower. The largest I know, in Trichoc. bridgesii, are far different from these! The open flower in Fig. III a does not show any details, but it shows, that the bud of fig. 175 III is near of anthesis, as the open flower does not differ from it in the dimensions. Finally, the longitudinal section has the same large nectary-chamber, the same long anthers and the same position of these in the bud as I saw it in the bud of B. microsperma. In Trichocereus short-tubed flowers never have a nectary-chamber, only long-tubed flowers have a diffuse chamber. Finally, the pericarpell and receptacle is very similar to this of a Browningia, but dissimilar to this of any Trichocereus species, and the development of hairs at the receptacle is forwarded acrotically, while in Trichocereus basitubically. If you read the descriptions of Castellanosia (the drawings are awfully bad!) which, also, has at least wool in the axils of the

scales and if you see, that the flow ers of this genus develop in the same position as in the Browningias, than you will confess, that there might be a direct connection from *Rauhocereus* to *Castellanosia* and *Browningia*. There nothing is proved yet, indeed, but therefore it is necessary to do all necessary examinations to prove this opinion or to reject it.

Now the aberrant or lacking spination of the flowering branches. As the juvenile spination very often differs from the adult one, especially if the areoles continue developing spines for many years, I only will speak of the spination of the flowering branches and of the vegetative adult branches. In all cacti the vegetative areole is bilateral. It develops the spines ( transformed cataphyllary leaves !!) of the axillar bud (=areole) only in the lower - abaxial - part, while the upper - adaxial - part remains meristematic, producing immediately or later on a flower or another lateral branch ( even the flower is a whole lateral branch which develops with radial symmetry. In the morphological view, this is a beginning serial division, which becomes totally in *Mammillaria* and others . Therefore, if vegetative spines are developed, the flower never develops in the centre of the areole, but always above the spines. Cardenas describes the flower : " flower buds naked, arising from the center of the mammillium bristle-bearing areoles" and this is to be seen very well at his Fig. 38 ( act. & Succ. Journ. Am. 1951 p. 90 ). This , also, is true for *Brown. hertlingiana* - excellently drawn by Mrs. May Bloss ! ( Please tell her, that I admire her wonderful drawings, where really all details are as exact as to be recognized as if one would see the living material ! And tell her my best greetings ! ). When I was at Juckeberg in 1936, he gave me a dry flower which he called *Gymnanthocereus chlorocarpus* (HBK) Backeb., to draw. I believe it was identical with your *B. altissima*. There was also a part of the flowering rib with a bud. You can see my drawings in "Juckebergs Blätter für Kakteenkunde Nachtrag 15, 1937-7" where Juckeberg has published my drawings in his own name . The bud-bearing areole shows at the abaxial part some spines ( developed first ) in a normal ( vegetative ) position, and around the bud some bristle-like spines. All this is the same : The spines ( or bristles ) around the bud are not more spines of the areole itself, but aboriginate from the "cauline zone" of the flower bud itself, the spine bearing scales of this zone mostly remaining microscopical or even reduced at all. This is the same development as it is in a cephalium ! Confer this with your drawing of *B. hertlingiana* ! In your picture of *B. altissima*, also, sometimes one areole-spine does exist, the spines of the cauline zone being extremely reduced or lacking at all. In *B. candelaris* the areole most times does not more produce spines at all, therefore the flowering branches are totally naked. Thus, the flowering parts of the stem really could be called an " inflorescence ", as they probably cannot more produce vegetative branches ( exceptions even can be in true cephaliums ). These flowering parts are the same as a cephalium of *Melocactus* - but without hairs. This, I believe , might be the cause, why your piece of *B. amstutziae* does not produce roots. Try to get rooting a cephalium of *Melocactus* ! Even flowering branches of *Myrtillocactus* do not !

The cauline zone , indeed, has been neglected hitherto, although it occurs much more frequently in cacti than one would think. But its very similar development in *Browningia* and *Castellanosia* is too equal to be only a convergence !

You see : My conclusions are absolutely not " speculatives " but based on a profunde knowledge of the dynamics of morphological evolution. It was not easy to get this knowledge and I needed about the half of my life for this ( not yet being at the end ! ), but in a plant family with so many convergences and therefore convergent similarities it is the only way to get phylogenetic conclusions and to avoid errors ! Imagine, that my " Morphology of cacti " is only a short extract of my knowledge of 1950 ! And I did not therefore my own opinion on that I might publish phylogenetic conclusions before having this knowledge ! Similarities - especially in the cacti - are " Will-o'-the-wisps " which lead on the wrong track. At any point of the morphological analysis of any line of evolution you must ask : How, according to the laws of evolution of this familia, the evolution can

continue and how it cannot? For "nature does not jump" ( natura not facit saltus !).

Therefore your opinion, that I give undue emphasis to the " traditional " use of geographical separations or to presence or absence of spines at the receptacle and pericarpell is wrong ! Remember our discussions on the question whether the " imitans " is a Werckleocereus or a Ecremocactus ! It was I, who did not hesitate to call it Werckleocereus, although the flower has no spines. The only argument for me is the degree of loss of the axial character of the receptacle and pericarpel and the degree of general reduction. Even in the achycereae I was not induced by the spination, but only by the dynamics of evolution. It is a pity, that you do not know German and it would be essential for you, to learn it ! Thus you cannot read my publications in detail and very often you misunderstand my conclusions. Otherwise you would recognize, that we could agree much better than you believe it now !

As to the origin of Browningia : Pterocereus seems to me to be still very closed to the most primitive Cereoidae, perhaps having been similar to the recent Leptocereus. For I believe Leptocereus to be the most primitive genus of cereoidae. The Browningia-line, also, might aboriginate not very far from these ; but I do not believe a nearer connection to Pterocereus, of which I know only the pictures. And Escontria certainly stand far from this line. Confer my pictures and you will recognize it yourself. The seed is essentially different and more primitive in Browningia than in Escontria.

To Leptocereus : I, also, have not yet seen flowers of Leptocereus ( in Britton & Rose's sense ) and know only pictures and descriptions. In December, I have spent 300,- \$ for mail, to write to different institutes in the hope to get materials of Leptocereus and others which I am still missing. But in any case the spination of the flowers in Br. & R. shows a characteristic similar to gnathia, that means, the receptacle which is not yet separated from the pericarpel has still absolutely axial character. This is not more the fact in Lept. grantianus, in Samaipaticereus and in Neobabbottia. These flowers, all, would induce a connection to neobabbottia but not to Leptocereus sens. strict. But - I have not yet seen fresh or preserved flowers ?

Austrocactus has been emended by Castellanos and belongs in "Los Generos de las cactaceas Argentinas " Ann. Mus. Argent. de Ciencias Nat. 39, 1938, where a very good drawing shows distinctly the inner structure. This drawing has been badly redrawn in Descoles Gen. et Spec Arg. Austrocactus contrary to Corryocactus, has only the basal stamens around the pistil and another series at the base of the perianth. This, exactly corresponds to the description of Erdisia philippii. I would not disagree an union of Corryocactus and Austrocactus although Austrocactus is more advanced in the stamen-character.

I do not know your " connecting link", but, indeed, I am extremely sceptic about any connection of Corryocactus to Pfeiffera ! For, in the seed characteristics, there is not the least morphological connection ! Would you be able to send me seeds of: Willmattea, Weberocereus, Werckleocereus and other Hylocereae ! ? When I will send you your Browningia flowers, I will add some little boxes ( these are boxes of fire-stones ) for mailing seeds. I hope these boxes will not become crashed by the post offices !

Finally, dear Paul, I am very happy for you, that you will have the occasion to do such a big expedition this and next year ! I am sure that you will do very fine work ! All my best wishes will go with you !

Now, this has become a very long letter ! All the best !

as ever ? Yours

Walter Rauh

Paul

2 March 1962  
Berkeley, California

Dear Buxl,

At last I can take a few moments to answer your long and interesting letter of 22 January. How exciting is your discovery of the chemical reaction of the various *Browningia* flowers. Our *Castellanosia* plant is getting sufficiently large that it may provide flowers sometime soon, so we may be able to check it out. I confess that you may be right about *Rauhocereus*, and certainly your summation of evidence is impressive. I've never been much impressed by the way this plant has been tossed about by various people, and certainly can't accept it as a *Trichocereus*. But I need flowers. Perhaps I'll get some next year, if our plants don't produce. It may be noteworthy that neither *Rauhocereus* nor *Castellanosia* provide the various spination types I've noted for other *Browningias*.

I would of course be delighted to receive any of your drawings, and they can come surface mail. If in ink, then I would plan to frame them for my study. But please sign them. They would be a wonderful souvenir.

We had a bad frost 3 weeks ago and many of my Peruvian plants were killed, especially the *Corryocactus*. It has been most sad, but then I may be able to get them again next year.

Enclose for your comments is drawings (2) of *Borzicactus madisoniorum* Hutchis., found in the area transitional to jungle and only about 5 or 10 miles above the rain-forest where the Marañon drops through a deep gorge in northern Peru. It has flowers of *Loxanthocereus*, Fruit of *Matucana*, Seed of *Thrixanthocereus*, Stems like *Lophophora* or *Gymnocalycium*, and the epidermis is papillose and viscid. The spine-clusters become easily detached by being jarred, or will easily pull off. In nature many specimens were spineless, but in cultivation from seed, all produced spines. In nature up to 3 spines were produced, ~~xxx~~ rarely four. In the seedlings most have five. A good new genus for Backeberg. For me a *Borzicactus*, but if I were to segregate it, I'd call it *Matucana*. Will publish it this year. *Vatrican* has a plant of it and perhaps you have seen it there. I expect next year to find species transition between this and *B. aurantiacus*, somewhere further upstream along the Marañon. Backeberg knows the *Vatrican* plant and will include a photo of it in his vol. 6. Incidentally, he writes that he has contracted to do *Die Cactaceae* all over again!

I still await the seed boxes you will send when you return the flowers now on loan to you. When they arrive, I'll do my best to send specimens to you. I am ready also to send Krainz herbarium specimens, and most anxious to have the official abbreviation of ~~his~~ his herbarium. I wrote to Holland to the IAPT to call attention to the fact that *Index Herbariorum* has no listing for the Krainz set-up, and they have contacted him, but I've not heard further about it. If you see him, find out what letters will be used for this abbreviation. We can send herbarium materials if he is prepared to provide exchange materials of as good quality as what we send to him. He knows, of course, that I will be especially interested in Andean cacti.

Best to you both,

As ever,

PCH

*Paul*



of *Br. microsperma* which you have send to me ( I hope, you may already have got it again !). The fruit is the same typus - except that it has hairs - and even ~~the~~ seeds are equals to *Castellanosia*. Although there is no dimorphisme of the spination, the "cauline zone" of the flower is well developed and bears spines, ~~around the flower-basis.~~ This can be taken as the first step leading to the dimorphisme of *Castellanosia*. Thus the line : *Rauhocereus* - *Castellanosia* - *Azureocereus* - *Gymnanthocereus* - *Browningia* s. str. now is proved. I am now preparing the publication. But as your publication on the connection of *Browningia* *Gymnanthocereus* and *Azureocereus* is not yet made, I will only indicate the near relationship and only acknowledge Marshalls connection of *Gymnanthocereus* with *Browningia*, to save your authorship. Is it right so?

Finally to the Dimorphisme of the spination.

The examination of the weakened rib of *Castellanosia* have proved my opinion, that the fine spines around of the base of the flower do not belong to the areole itself but are developed from reduced areoles of the cauline zone of the flower itself, even if the flower did not continue to develop (Confer: *Burbaum*, *Spaltcephalum* von *Espostoa* ).

As I am recently preparing a compendious publication on all kinds of cephaloid formations, I have a request to you : Would you be able to send me at least one areole of the flowering parts of all species of *Browningia* you have, for examination !? Just the spine-less areoles of *B. candelaris* would interest me extremely much .

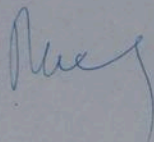
The wanted drawing of your *Browningia microsperma* you will get a little later. For presently we have both very much to do. For in the first days of June we will start for Spain and remain there until middle of August. (My adress here will be "Jardin Botanico Mar y Murtra, Blanes (Gerona), Spain).

We had a terrible long winter this year. It began to snow already in October, and with intervals of frosty but snowless periods we had big snow ~~until~~ near above the town until last week ! Then, just <sup>down to</sup> Fryday before Easter, it suddenly became very warm - but only for one week. Today we again have a cold rainy ~~weather~~ weather! Only at Graz the trees already become green ! 7<sup>th</sup> of winter ! Brrrr!

All my best greetings to all of the BGUC !

Especially to you, also from Friedl !

All the best Yours



21 Sept. 1962

Dear Buxl,

I've just received Die Kakteen, Jdef. 21 and hasten to write concerning the altered phyletic outline for the Cactaceae. In the first place, if *Pyrrhocactus* is to be recognized, then the synonyms you give for *Neoporteria* all belong under *Pyrrhocactus* rather than *Neoporteria*. Recently we flowered a true *Pyrrhocactus*, and I concluded that this was the case. *Neoporteria*, *sens. strict.* remains very weakly separate, but at least separable, with no known intermediates.

Concerning the *Thelocactus* line cf. Benson's recent treatment of the *Pdiocactus* group, with reduction of *Utahia*, etc. The placement of *Chiapasia* and *Wittia* under *Disocactus* is by Kinsch, not by Hutchison. I can see practically no difference between *Samaipaticereus* and *Leptocereus*! Also I believe *Mediocactus* and *Wilmatea* belong in *Hylocereus*. For *Wilmatea* a genus can be recognized, but it is a weak one. For *Mediocactus*, there is practically no case at all.

For the *Borzicactus* assemblage I think things are not improved: *Arequipa* is almost certainly derived from *Oreocereus* or something from which *Oreocereus* is derived; on the other hand, *Matucana* is derived from ~~*Borzicactus*~~ *Loxanthocereus sens. strict.* When you put *Arequipa*, *Matucana* in the same genus, the genus resulting is unnatural and cannot stand. If you recognize *Matucana* as distinct, it would be O.K. but then all you have to use to distinguish it as a genus is number of ribs, stem form, and fruit dehiscence. Since my new species *Borzicactus madisoniorum* has only 5 ribs, and would have to go into *Matucana* if into a segregate genus at all, then the number of ribs for *Matucana* would no longer separate it from *Loxanthocereus*. Each new thing that comes along seems to make the whole *Borzicactus* assemblage weaker. Either you have to recognize the weak genera or lump them. There is practically no case at all for lumping only some of them, because when you do, the weak genera then become even weaker.

I remain undecided how *Haageocereus* and *Weberbauerocereus* will be kept out of *Trichocereus* (as subgenera). Both of them are weaker genera than the segregates of *Borzicactus*. Then you have to separate *Haageocereus* from *Espostoa*, and that is no easy task either. Only the cephalium really separates them, and there are intermediates in this character too.

I cannot at all accept placement of *Pygmaocereus* in the genus *Arthrocareus*, up to now anyway. I think it is directly derived from *Haageocereus*, and worth generic status. There is no conceivable explanation for an isolated species of this type on the west coast of S. America if it is an *Arthrocareus*. Morphologic similarities I think are convergent. I still am unconvinced that *Mila* may not bear the same relation to *Haageocereus* that *Pygmaocereus* does.

In the synonymy of *Borzicactus gracilis*, *Lox. nanus* Backeberg should appear without the parenthetical (Akers), since Akers never published and Backeberg is the only author. Had Backeberg published it in *Maritimocereus*, it could have been "Akers ex Backeberg" but not (Akers) Backeb. Akers' epithet *nanus* thus has no nomenclatural standing.

If you would like to use some habitat Kodachromes of cacti for Die Kakteen, I will be glad to provide some for you.

I see from the folder that I did not answer your letter of 27 April. How quickly time passes. Thank you for your support for the Fulbright. I still have no word, but it will be most remarkable ~~xx~~ if I get it, for there were no openings for research

for Peru for 1963. Even so the application was approved by the Fulbright committee of selection, and waits now for the state department to decide whether arrangements can be made at this late date for a research fellowship. I hope to hear in a week or two.

Concerning *Borzicactus radisoniorum*, Backeberg has known of the plant for two years. He first saw it in 1959 at Vatrancans. He has been kind enough to not publish it up to now. Concerning Matucana and Arequipa, as I've said above, I don't think these have the same origin in the Borzicactineae, and in any case my plant has the fruit dehiscence of Matucana. No other Borzicactineae have this. One simply cannot deduce phylogeny using single characters, Buxl, hence you must not use amount of hair, only, (hair on tube). We know so many groups where characters have disappeared and reappeared later in the line, too, so that it is dangerous to assume that once lost a character does not reappear. That might be more safely deduced if the genetic evidence seemed to point out that only a single gene controlled the character in question. In cacti, I think, this is seldom the case.

Delighted to hear about the stem dimorphism of *Castellanosia*. You might make a list of S. American cacti you wish material of, including what type of material. I will include this in my collections from Peru next year.

Very sincerely,

P. C. Hutchison

From B

Dear Paul ,

Nov. 9, 1962

I thank you very much for your letter of Sept. 21, where you try to teach me phylogenetical research. It is very nice, that you do so, but you may remember that my teacher in taxonomy was R. v. Wettstein, Europe's most famous phylogenetical botanist of this century, and that I did not stop on the way and have developed new methods, which have given me very great successes in plant families in which rather famous other authors had none (Liliaceae, origin of the Caryophyllales aso.; all my statements in Liliaceae have been proved by all later authors to be right, finally even in the chemical way by Prof. Santavy a chemist in C.S.R.). Thus my methods are used already in Switzerland and Germany too.

But, as you cannot read German, you never can follow my conclusions and you only see the sometimes striking final results without knowing the very very long and careful way to reach these results. Otherwise you would know, that even in cacti, I never use a single "characteristic". All my conclusions in cacti are the result of a 35 ~~years~~ year's research on morphogenesis, that means on the laws which rule the whole evolution of ~~nature~~ every part of the plant. Each part of the plant develops independently from each other in evolution as well as in heredity.

I am very much astonished, that you tell me, that in "so many groups" lost characteristics reappear and thus Dollo's law of irreversibility would not be true, I myself do not know such a case - except if the taxonomic range is wrong!

As to my provisional systematic order: There, also, it is necessary to read the footnotes! This order, or - if you want "System" shows the standpoint of published knowledge at 1962/62. I cannot use unpublished "opinions", and ~~that~~ I have expressed that further researches possibly might bring changes, e.g. in the Tribe Notocactaceae, Cereae etc.

In summary: I feel it extremely necessary for you:

1. to learn perfectly German
2. to learn the methodics of examination the inner structure of cactus-seeds.

The first, to be able to read my book on the "Dynamic method" and my different phylogenetical publications. Cacti have a very funny way of evolution. There are only few ~~named~~ tendencies in evolution which are combined in very different manner thus causing the immense many-foldness. Therefore all characteristics repeat in very different lines of evolution and it is essential to follow the evolution of every part of the plant in the dynamic "step-by-step-method". Otherwise you always will mix convergences and relationships. Only in this way I was able to clear up the Liliaceae and the origin of the Caryophyllales. In cacti, the inner structure of the seed is extremely important (See e.g. my publication "Once more Phylogeny of Pelecypora, Solisia, encephalocarpus and Epithelantha, Nat. Cact. & Succ. Journ 15, 1960)

But now to your remarks:

Pyrrhocactus: Former I was of the same opinion, that Pyrrhocactus belongs to Neoporteria. (See: Buxbaum, Vorträge zu einem phylogenetischen System der Cactaceae, Vortrag gehalten auf dem 1. Internationalen Kongress d. Sukkulantenforscher: Kakteen u.a. Sukkulanten 4, 1953, p. 2 ff). But now, having examined the seed of all these "genera", I needed to change my opinion. For, the inner structure of the (true!) Pyrrhocactus-seed, differs essentially from the other "Neoporterias" and is equal to the seed of Coryocactus. It would be better to unite the true Pyrrhocactus (not the Horridocactus!) with Coryocactus and than also Austrocactus than with Neoporteria! My researches still are in the first state, but so far I can see now, Pyrrhocactus seems to be the connecting link from Coryocactus to all other Notocactaceae. (As to Coryocactus: My publication on its phylogenetical position is already in print).

Continued Nov. 17, 1962

It is terrible not to have sufficient time to write a letter at once!

We have living materials of all species of this line at Vienna and I already have made some examinations. I feel, that this publication L. Benson's is a "retreat-battle". In 1950 he did not yet accept any more genera than *Opuntia*, *Cereus*, *Echinocactus* and *Mammillaria*. Lyman Benson is an excellent florist - but he is not a taxonomist. Samaipaticereus and Leptocereus. You do not see the differences, because you accept *Leptocereus grantianus* really for a *Leptocereus*. But the type-species, *Lept. assurgens* is extremely more primitive. You may unite "*Leptocereus grantianus*" with *Samaipaticereus* and even with *Neocabbottia*. And I would say "o.k.", if you would unite *Leptocereus* s. str. with *Calymmanthium* Ritt., perhaps also with *Armatocereus*, but these two lines are different, so far I can judge without having fresh materials of the true *Leptocereus*.

Willmattea and Mediocactus. As to "*Mediocactus ~~mona~~ megalanthus*" it seems really that this plant is very near to *Hylocereus*; *M. coccineus*, in contrast, is rather different - so far the illustration of *Castellanos* shows. *Willmattea* seems to me to be a good genus. I have examined the flower. But I am still missing seeds of all these plants. Especially these of *Willmattea* would be extremely important!

Borzicactinae. Former, the union of *Maticana* and *Arequipa* was YOUR idea and opinion and thus I followed uniting these genera. I myself have not done much in this part of the system, because it is more your ressort and your special research. Therefore I made a footnote about this. As to *Borzicactus madisonorum*, I do not like it within the genus *Borzicactus*! For, this plant unites tendencies of different lines, in a very uncommon way: Flowers primitive, fruit opening of *Espositoa blossfeldiorum*, even so the seed, habit: different of all other *Trichocereae*!

Perhaps you will understand my standpoint, if I give you an example of another plant family, the *Roseaceae*. Besides of others, you can see in the family the following tendencies:

1. development of a receptacle
2. connection of the receptacle with the carpels
3. flower axis becoming fleshy
4. loss of carpels (diminution finally to one)
5. augmentation of the ovules (primitive 1) in each carpel
6. the ectocarp becoming fleshy
7. loss of the petals

These tendencies are realized in:

*Potentilla* - no one (most primitive genus)

*Fragaria* - Nr. 3

*Rubus* : 6, sometimes also 7

*Rosa* : 1 and 3

*Sanguisorba* 1, 4 and 7

*Alchemilla* even so

*Spiraea* 4 and 5

*Prunus* 1, 4, and 6

*Pirus* 1, 2, 3, 4, 5, and 6 (this genus thus being the highest deduced of its line)

This fact means: A genus very rarely is characterized by characteristics which are specific for the genus and do not exist in any other. This only occurs in extremely old genera. The generic characters are special combinations of realized tendencies which - in other combination also can occur in other genera. The more a genus is primitive, the less of the tendencies have become realized (e.g. *Potentilla*), the more of the tendencies have been realized in a genus the higher degree of evolution has been reached (*Pirus*). Thus this way allows to a certain degree to recognize the relative age of the genera of a certain branch of evolution.

And now look to your *Borzicactus madisonorum*: The flower is e

9-XI-62  
B to PCH

extremely primitive for the subtribe ( only a slight pseudozygomor-  
phisme, many internodes, especially at the pericarpel, wide opening  
of the flower, diffuse nectary chamber), like *Loxanthocereus* - I am  
using this name to have a contrary to the " true" *Borzicactus* which  
have a non- staminodial hairs above the nectary chamber & the fruit  
opening is more similar to *Espostoa blossfeldiorum* than to *Matucana*.  
to the contrary : *Arequipa* as well as *Matucana* have a ~~no~~ closed nectary  
chamber (diaphragme !!). The seeds , also, resemble these of *Espost.*  
*blossfeldiorum* and develop from equal ovules by equal growth. *Matu-*  
*cana* - contrary to *Arequipa* - has a much reduced nomer of internodes  
at the pericarpel and ( therefore ?) most times no hairs. *Matucana*  
( your *Borzicactus* ) *surantiaca* has staminodial hairs ( in some flowers)  
above the nectary chamber; this seems to occur in different genera  
( also in a *Espostoa*). In the habit the *madisonorum* is absolutely iso-  
lated, repeating a tendency which only occurs in the *Echinocactaeae*  
( e.g. *Lophophora*). Do 'nt you feel the discrepance ?! The outway to unite  
all plants of this line into *Borzicactus* maked this genus more and  
more ~~wash~~ proteus-like and impossible to give a defintion of it and  
finally leads to the same result as was the "Genus *Crucifera* " ofr all  
plants of this family !

You see : I know much more about this subtribe than you thought.  
Bit I do not like to publish on the special research of a colleague.

One genus more : *Pygmaeocereus*. is exactly intermediate between  
*Arthrocereus campos-portoi* and *Arthrocereus* ( *Setiechinopsis*) *mirabilis*.

Thank you for the Kodachromes offered for "Die *Kakteen*". Please  
offer them to Krainz, who is the editor and manager.

My publication on the "*Auhocereus-Castellanosia-Browningia-Line*  
already is in print.

But now : Finish for today ! I mus prepare my University-lecture  
( Morphodynamic as the base of phylogenetic research) and I still  
have ca one lb. of un-answered letters of the summer !

If, perhaps, I do not more the time to write :

All the best for Christmas-time and the more for the  
coming Year ! also from Friedl indeed !

AS EVER YOURS

Best wished also for the whole "BGUC-fellowship!"

27 Nov. 1962

Dear Buxl,

Thank you for your letter of 9 Nov. Your reaction to my letter of 21 Sept. is amazing to me. It also somewhat angers me, and it is in this state that I am replying.

You must be aware that Wettstein's work is well-known over the world, and that, in fact, even I have heard of him. That you were a student of him is of no significance to me at all. The only significance of any man's work is what he publishes, and the methodology by which he works. When I was a student, 15 years ago I was taught as a matter of routine the methodology which you have given a new name as "your" dynamic method." You seem totally unaware that the techniques you are using, most successfully I must add, are in use over the world and have been for twenty years or more. If you want abundant examples, then look at the theses published in U. C. Publications in Botany for the past several decades, and in any other major U. S. University, to say nothing of many European schools. In these works not only your methodology but many additional approaches to systematic study have been utilized, and with fortuitous results.

It is your right to consider yourself a taxonomist. I have never understood your grounds for this presumption, and do not now. Morphology as you have utilized it can be a most useful tool to the taxonomist, but it is still a separate field, just as is genetics, physiology, and all the other disciplines. It's application to taxonomy by the morphologist is most questionable, particularly when the person involved is subject to the kind of absolutism and positivism which you take as your prerogative. I was taught that whenever a scientist is absolute he is most probably wrong. You always give the impression that you are always absolutely right. I know that this is not possible, for any man.

1. You must certainly be aware that multigenic characters may appear, disappear, or reappear depending on the combinations of genes in a particular individual. There are thousands of examples of this in the animal and plant kingdom. I studied many of them in genetics and evolution under Stebbins some years back, and, incidentally, that course is available for your perusal in the form of a text on plant evolution by Stebbins. Dollo's law would, therefore, seem to be contradicted not once, but many times. Perhaps at issue here is what I mean by "lost" and what you think I mean by it.
2. If your system, which is provisional (as are all systems!) is based on published knowledge, then how can you utilize my unpublished opinion of 9 years ago, when I was first acquiring information on the *Borzicactus* assemblage, that *Matucana* and *Arequipa* are the same genus. I abandoned that idea once morphology of the various genera of the assemblage was known, years ago, and of course if you are to utilize a published opinion, Kimmach published one. In my mind none of these things are settled and probably they never will be. But I do object to polyphyletic genera, and the problem of lumping part of the *Borzicactus*-assemblage is simply how one can do so as to distinguish the groups resultant and also be sure that they are not poly-phyletic. *Matucana* and *Arequipa* are a single genus only if included in *Borzicactus*, sensu Kimmach. There they would be in different branches of the genus, as I understand them now. I don't object to anyone's recognizing them as distinct, or to lumping them in *Borzicactus*, but merely to sink one in the other is highly questionable and I think results in a mere form-genus in the old Schumann sense.

3. "Each part of the plant develops independently from each other in evolution as well as in heredity." From your letter. I have always thought of all of the parts of a plant as being rather closely integrated in their evolution. You would certainly have to put some kind of limitation on this idea of yours.
4. You state that you never use a single criterion in coming to phyletic conclusions. Then you proceed to suggest uniting *Pyrrhocactus* and *Corycoactus* on the basis of seed character.
5. Don't forget when you make deprecating remarks about Lyman Benson, who has considerable reputation as a taxonomist in several groups other than cacti, that he has your work available to consider when he comes to conclusions. The differences in taxonomic expression of conclusions express differences in conception of taxonomy itself, not in knowledge of morphological facts. Of morphological and other kinds of data on *Pediocactus* Benson had much more data than you could possibly get. This doesn't mean that he is necessarily correct. But it does mean that he finds it possible to express taxonomic ideas in a framework entirely different from yours. It is this sort of difference which morphologists, geneticists, and others never fully understand. H. L. Mason has dwelt in considerable detail on the subject and is currently doing a book on it. He contends that the taxonomic system should not be the medium of expression for specialized disciplines, but that specialists in fields other than taxonomy should erect their own frameworks for expressing ideas, leaving to taxonomists the job of adapting the system to new knowledge as it comes along.
6. I have no feeling of possession about any area of taxonomy in which I work, Peruvian cacti, or other groups. So don't hesitate to publish ~~xxx~~ whatever you wish on my account.
7. *Borziocactus madisoniorum*. You may be right and I must admit I am in doubt about this plant and how to handle it. Unlike Myron I have never made up my mind about *Borziocactus* either. I can see how the single genus can be made meaningful, but only by creating sections within it, and my system then would be equal to yours (except for *Matucana-Arequipa*). This could just as well be expressed with genera as with sections. But when there is an option I incline to take the conservative view. Phyletic ideas can be expressed within genera as well as by them!
8. On *Pygmaeocereus* I pass for the time. I have still to examine fruit and seed. I find it difficult to believe this coastal Peruvian endemic has anything to do with the tropical eastern South American highly advanced species of *Arthroocereus*, in spite of the floral resemblances. We have *Arthrocereus* here and I'll examine them more closely.
9. German. Yes, my greatest deficiency. As for my own work on cacti, my present feeling is so negative that I may give it up altogether. Not only am I unable to spend the appropriate amount of time, but the literature is so rapidly filling with names that it is scarcely possible to keep up with it all, and takes far more time than it is worth to show the errors of much of it. I don't object to faulty conclusions, but the methodology of Beckeberg, Ritter, and other taxonomists, so-called, in this family, makes me sick.

In other words, at the present time I am very depressed about the literature and the situation here, and I may leave botany altogether very shortly. Which will leave you with one-less taxonomist to argue with. In spite of all this, and of our relatively unimportant areas of disagreement, I send both to Friedl and you all my love and warmest regards for the Christmas and coming year.

Very sincerely,

Univ.-Dozent Dr. Franz DuxRUBIN  
Professor  
Jedenburg (Austria)  
Sackgasse 13/14

Dear Paul ,

June 25ht, 1963.

Now it is more than half a year, that I have not your last letter. Nevertheless, it is not, because because this letter has been somewhat angry, that I did not answer you earlier, but only because I am extremely much engaged with different scientific work and other publications, my lectures at the University and much other. The last two month I worked daily about 16 and more hours. Finally now I am for short holidays at Opatija on the beautiful Adria sea and this is the first time when I can begin to answer my correspondences of 1962 !

I do not know whether or not you have a copy of your letter but nevertheless I will try to answer the more important points of it.

As to Dollo's law, I feel that you have another mind of the sense of the word "loss" than I have. There is a great difference between characters of specific rank, which, indeed can disappear and reappear and therefore can be observed by genetic experiments and such ones which have been lost on a very long way of evolution, even in the possibility to develop. I do not know whether I have expressed it correctly enough, such that you can understand what I means. But I would say: any spination characters or similar can change, appear, disappear and reappear again as long as the possibility to develop an areole at all. For alle spines are transformed leaves. Contrary to this, in the Cerecoidae the loss of leaves is a definitive one because the transformation to spines ~~from the base of~~ or of the leaf-base to the podaries is not reversible. From this point of view - not from the genetic standpoint - Dollo's law is not contradicted; but seen from the genetics is is misinterpreted. In the Liliaceae I myself have seen and examined cases, where in the course of evolution an organ (the tube) seemed to have reappeared while really it was a reversion by a true progress, the secondary tube being a very other typus than the primary (This is the case in Ixiolirion).

As to Corryocactus and Fyrrocactus you did misunderstand me totally ! I never had the intention to unite Fyrrocactus with Corryocactus. In my letter I meant: If, perhaps, you would have arguments to unite these genera, I would be able to agree. But I do not more agree to unite Fyrrocactus with Neoporteria because Fyrrocactus is much more primitive and - so far my recent researches show - that Fyrrocactus might be the point of origin of several lines of evolution, one of which are the "Neoporteriinae" (not yet published name).

When - next week - I will come home, I will give you the reprints of my publication on the "Browningia-line" (Rauhoceraus-Castellanosis-Browningia sens. Burttinsson) and will send it to you. Even this letter a will mail from at home. I am sure that you will be very much interested for it and that you will agree with my results. Only - indeed - it is a pity that you cannot at least read the german texts ! Really: it will be essential to learn at least so much the german !

I am consterned of your idea to leave cactology and the more, botany at all ! This would be a very heavy loss not only for myself but also for botany at all ! You are one of the most excellent young botanists and especially cactologists I know and I am of the opinion that finally, when I will not more be alive, you would be the only man who would be able to correct and finish my own work on cacti. Although I hope to live 20 years more: I am 63 and who knows whether or not I will be able to finish all my researches. You must not leave your excellent work, you must continue it ! E. Roeder once wrote: "Every cactus has hooked spines" that means, once working with cacti one cannot more leave them. I hope, that this will be true for you!

But finally: I have some requests to you. I gave some drawings made at Berkeley and I would like to have more detailed dates of these plants or in some cases I would need to know whether or not the names are correct or have been revised.

→ the 3 addresses, I have read!

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C.H. PCH

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These plants are :

*Notocactus submannulosus* - pampeanus BGUC Nr. 32.344  
*Brasillicactus haselbergii* BGUC nr. 49.1764  
*Copiapoa coquimbana* or *cinerascens* PCH nr. 325  
*Neochilania* sp. PCH nr. 378 BGUC nr. 52.577

Besides of this I would need flowers of  
*Islaya islayensis* BGUC 51.655

*Neoporteria jussieui* PCH 399, BGUC nr. 52.584 -4

( do you believe *Islaya* to be a valid genus ? I do not ! )

and flowers of any species of

*Discocactus*, *Austrocactus* and *Neowerdermannia* (especially *vorwerckii*)  
( the other "Neowerdermannias", I believe are none and the genus is  
weak ! )

Finally : I already once asked you for seeds ( and if possible a fruit )  
of *Bergerocactus emoryi*

Have you any species of *Leptocereus* ? I wrote letters for 700,- Sch  
to get materials from the caribeans - without any success !

Now, dear Paul, I hope you are not more angry and I hope you will con-  
tinue your researches ! How will it be with the results of your voyages  
to Chile and Peru ? These are of such importance that we cannot miss  
them !

The best greetings also from Friedl, and all the best  
from your

Best greetings also to the whole fellowship of 1956 at the BGUC!

5 Sept. 1963

Dear Buxl,

Your letter 25 June has sat in the pile-up on my desk now for months and I've been so swamped I couldn't get to it. I leave 3 Nov. for South America as Director of the UCEG 7th Expedition, 14 months, mostly Peru, but also adjacent Ecuador and Bolivia.

Pyrrhocactus again---haunting me! I also think Neoporteria sens. strict. is good. But I sink most of the rest of the segregates of echinocactoid Chilean species in Pyrrhocactus, since this year we finally flowered a true Argentine Pyrrhocactus. I find the presence or absence of wool or spines on the flowers of *Horridocactus*, *Neochilenia*, etc., etc., unimpressive in face of structural similarities of the flowers. As for *Austrocactus*, this too may be sinkable, but I've not yet flowered our one plant of this genus. I agree with you that *Corryocactus* leads to the *Pyrrhocactus* group, but is distinct. The real question to resolve is the relationship of *Pyrrhocactus* to the Argentine and other species east of the Andes now in different genera, including *Austrocactus*, *Malacocarpus*, *Notocactus*, *Parodia*, etc. and *Islaya*. *Islaya* seems to me to be *Pyrrhocactus* also, but with convergent similarities to *Parodia* in flower only. In fruit and seed and geographic range it is *Pyrrhocactoid*. In spite of the existent variation I am provisionally calling all the *Islaya*s one species. About this, however, I feel I am on very unsure ground, and perhaps this next trip will help to clarify the matter.

Your *Browningia* paper arrived and is indeed impressive. Congratulations. We have, since I last wrote, flowered *Castellanosia*, and it certainly belongs in the group, but as I was unable to spend any time on the flower and have not studied the fruit and seed, I can't say whether or not I would reduce it.

49.1764 is *Notocactus leninghausii*, so you must have wrong number. Probably it is 49.1763, *Notocactus haselbergii*, or horticultural origin and still here

32.344 is *Rhododendron maddenii*! The only card on *Notocactus mammalæsus* var. *pampeanus* is 38.349, but the plant is dead and I can't vouch for the name.

PCH 325 is det. *Copiapoa coquiabana* (Karw.) Br. & R., but this is a somewhat decumbent form and probably Ritter has given it a new name. It is from Quebrada Amoland in ~~Extremadura~~ Prov. Coquimbo, Chile. I had wild-collected flowers and fruit in pickle, and probably you drew these.

PCH 378 is det. *Pyrrhocactus kesselringianus* (Döhlz) and is from Prov. & Dept. Coquimbo, in the canyon east of Coquimbo at Los Chiches, 3 km. west of Vicuña. As with many of Beckeberg's and other European author's names, I'm not certain of the application of the epithet, but it seems to match. I had wild-pickled fruits!

As to your desiderata, I can't do much about them now. *Bergerocactus emoryi* doesn't fruit here. We have no *Leptocereus*, although I'm trying now to get the Puerto Rican one. We have the other things you want, but no flowers now (*Islaya*, *Neoporteria jussieui* (= *Pyrrhocactus*), *Discocactus*, *Austrocactus*, *Neowerdermannia*). *Discocactus* flowered a week ago but I missed the flower, unfortunately, as I've been waiting for it for 3 years! I've two of them on large grafts now, so that we will certainly get more flowers.

For the forthcoming expedition I will have a 3/4 ton 4-wheel drive truck fully equipped for this type of work, with a steel cab on the back with 6 feet clearance inside, many cabinets, a roof-rack, and many other extras. The best we have had up to now for any expedition. I must give up a year's salary, but let us hope that the gain to science will be in proportion. Honolulu Botanic Gardens, Strybing Arboretum Society (San Francisco), Chicago Natural History Museum, and the University of San Marcos (Lima) are the co-sponsors. I will take a zoologist with me as assistant (C. K. Wright) and make some studies of pollination in both orchids and cacti, and make a special effort to catch for identification the birds, bats and insects associated with cactus flowers.

While I have been able to function for science the past 3 years it has been depressing that I've been unable to do any writing. Perhaps when I return from this expedition things will change a bit. For one thing my house will be paid for in March, 1965, which will relieve the financial pressure, and I can perhaps then spend more time on writing. I have nearly 200 new species to publish, in all groups, not just cacti!

As to your being 63 and having only 20 years more of active publishing, that is ridiculous. You are incapable of growing old. And probably you will outlive me! I'll be 40 in January.

Martin Cardenas writes that he is back in Vienna after the IOS Congress, which apparently did not impress him. He finds tourism in Europe very distressing, as well he might, and indicates that he will have to go home to get a rest! I have not had a vacation for two years and very badly feel the need of one. When I leave here 3 Nov I'll fly to Washington DC for 3 days then to Panama for a week on Barro Colorado Island, which will be my rest, at the Smithsonian Research station there. Then to Cali (to collect Dagua Valley cacti) and Quito, about 4 or 5 days each, and on to Lima by Nov. 24 to meet the truck, (arriving by boat.) With luck we will be in the field 48 hours later. The first objective will be Chachapoyas, a region rich in cacti in the north-Peru interior, but which no-one has collected.

My best to Friedl.

Sincerely,

P.C.Hutchison

Sept. 29, 1963

Dear P a u l,

I was very glad having got your letter, especially to hear about your great expedition. In this way, indeed, you are safed for botany and cactology, and all my best wishes will be going with you!

The IOS-Congress at Vienna was a great success for us austrian members. At the final dinner Prof. cardenas ( who was Congress-Persident) has called especial attention to our three lectures: The new Austrian Members Dr. Kladiwa on the Toumeya-Utahia-group, and Dipl. Ing. Frank on the Peñio-cactus-group, both cooperating with Anderson and well trained in my methods of research and finally the mine on origin and lines of evolution of the tribe Notocactaeae. specially Dr. Kladiwa's lecture was excellent! He has shown color-slides of the plants and seeds, drawings of the seeds, excellent maps and also color-slides of the type localities of each species.

As to my lecture I will give you a short report, because, I believe, this will be useful for your expedition. The probable origin of the tribe is already published in theSwitzerland "Sukkulentenkunde VII/VIII, which just came out at the congress days. (Kladiwa's and Frank's lectures, also, are published there.) It will need about one year more to examine and clear all detailed questions within the Notocactaeae, but at present I have already reached a very clear outline of the evolution and especially of the subtribe Neoporteriinae.

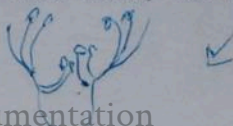
It is necessary to anticipate a few essential points. The flowers indeed, give very few characteristics to separate groups, except the sub-tribe Gymnocalyciinae. But they ~~have~~ have a very good characteristic together: The two groups of stamens, which can be distinguished at least in the separate vascular strands, mostly even by their position. The lower group is inserted radially, the upper one tangentially to the receptacle. Such:

Each group can become multiplied or reduced. But each has a separate vascular innervation.

Austrocactus is different from Corryocactus only in the fact, that the upper group of stamens is reduced to a few rows at the margin of the receptacle. This characteristic occurs also in Gymnocal. mihanovichii, contrary to the other species of Gymnocal. Thus it would be possible, to unite Austrocactus with Corryocactus; but in such a very primitive group as it is represented by the subtribe Corryocactinae, this highly derived characteristic seems to me to have a much greater phylogenetic value than in Gymnocalycium, which is a ~~rather~~ relatively young genus in a state of wide-spread evolution. Therefore I would prefer to keep Austrocactus separated, the more, as I do not yet know the inner structure of the seed. In no case Pyrrhocactus can be ~~sim~~ derived from Austrocactus-like ancestors but only from such ones which had the characteristics of the recent Corryocactus species. This origin must have been very early. For the seeds of recent Corryocactus - so far I have already examined them - have not more a perisperm, while Pyrrhocactus ( in the ONLY true sense of A-Begger, with P. straussianus as Type-species, has the same character and inner structure but still contains a little perisperm with some amyllum. The hilar-region, including the micropylar porus is more or less ovate (at the seed of Corryocactus). Between the micropylar part and the chalazal part of ~~it~~ it the hilar tissue forms a somewhat prominent " bridge", i.e. a transversal ridge; this shape of the hilum is rather common in different Cerecoideae, but it is a quite primitive characteristic. In Corryocactus the bridge is more typical than in other genera ( i.g. <sup>U</sup>obivia a.o.)

In Pyrrhocactus the micropylar part becomes smaller than the funicular part, but even in the same species we find different variations and some of these show distinctly the same character as it is in Corryocactus. (See the drawings!)

Thus, if in any genus of the Notocactaeae the hilum characteristics of any species still show the united chalazal and micropylar region and the bridge, than this species is more primitive than these, which already have a deduced shape of this " proto-type".



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*Pyrrhocactus* Berg. s.str. (Typ spec. *P. straussianus*, See drawing has a somewhat pear-shaped ~~seed~~ laterally appressed seed. The hilum is situated laterally. The testa is covered by a brownish aril-skin. The hilum includes the micropylar porus, the "Bridge" is more or less distinct. The micropylar part can be much smaller ( sometimes in the same species different) The embryo is slender, hooked around a small perisperm and has rather great cotyledons.

Within the genus *Neopterteria* ( including all later "genera" except *Pyrrhocactus* ) we can distinguish two lines. I call them, only to distinguish them, the "*Neopterteria-Typus*" ( Type spec.: *Neop. subgibbosa*) and the "*Nichelia-Typus*" (Type spec.: *Neop. jussieui* )

~~Neop.~~ *Nichelia* Typus: ( I avoid the name *Neochilenia* because in its present use it is extremely confusing. *Nichelia* Bullock, although used for a genus it is a homonym, can be used in the sense of only a line within the genus *Neopterteria* and is defined by its Typ- species *Neop. jussieui*).

Hilum surrounded by a broad swollen collar. Micropylar porus separated from the Hilum and situated within the collar. Embryo very succulent but ~~more~~ more or less hooked or at least bent, with distinct, rather large cotyledons. No perisperm. This type therefore is more primitive in the inner structure than the *Neopterteria-Typus* but less primitive by the ~~margin~~ displaced micropylar porus.

*Neopterteria-Typus*: Hilum surrounded by a small margin, micropylar porus closed to the Hilum, included into the common margin. Embryo straight, extremely succulent, cotyledons extremely reduced.

Thus it seems to be possible to distinguish two genera or subgenera. But it is not so because these two types are connected by ~~many~~ intermediate links. ( See drawings)

You see, I made my researches at first only with regard to the seed-characteristics, without regard to habitual and flower characteristics. I did so, to be not prejudiced by habitual convergences, and I think, it was good so. Co-operating with ~~Quining~~ *Quining* who has a great collection of these species I will try to be able to examine seeds of all species and than I will compare these results with flower and habitual characteristics. A lot of work ! But it is necessary.

The second line that aboriginates directly from the *Pyrrhocactus-typus* begins with *Islaya*. The near relationship to *Pyrrhocactus*, that you have seen by other characteristics, is excellently proved by the seed characteristics ( see drawings) in this line the "cauline zone" of the flower, which in *Neopterteria* sometimes causes up to 3-flowered areoles ( in dichasial character), bears very many very woolly "micro-areoles" and thus the top of the plant becomes so woolly. This leads to *Copiapoa*. Probably Ritter's *Pilocopiapoa* ( which I have not yet seen ) will be the connecting link. This relationship already was supposed by Rauh. I am shure he is aright.

The most interesting line that has its origin already in the *Pyrrhocactus-typus* is the subtribe *Gymnocalyciinae*. This subtribe is connected with ~~the~~ *Pyrrhocactus* by ( genus or subgenus ) *Weingartia*. *Weingartia*, also, has a very well developed cauline zone, although less woolly than it is in the *Islaya*-line, and therefore it bears sometimes several flowers on each areole. The character of the receptacle is nearer to this of *Pyrrhocactus* and *Neopterteria* than to *Gymnocalycium* s. str. The seed-characteristics, especially the very large hilum ~~micropylar zone~~, also, are very near to very primitive *Pyrrhocactus*-types. There is a very striking characteristic at the seed: The inner integument protrudes out of the micropylar porus! ( see your picture of *Gymn. westii* ) The stamens of *Weingartia* are ~~very~~ strikingly short, all of equal length and only in the innervation we can observe the two groups. These two characteristics are extremely important for a later observation. The seed of *Neowerdermannia chilensis* Ritt., also, has the same primitive characteristics as *Weingartia* has. But I do not know, whether or not it is a "*Neowerdermannia*, because I have not yet seen a flower of it.

*Neowerdermannia Vorwerckii*, indeed, is highly deduced in its

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flower with the long scale-less receptacle and in the position of its stamens. But I do not yet know its seed.

In *Gymnocalycium* s. str., *G. netrelianum* and *G. guercaneanum* have the most primitive seed and even in the flower these species seem to be the most primitive species. (Very interesting: In the even primitive *G. baldianum* sometimes spines at the flower can be observed!) *Gymnocalycium* s. str., indeed, is in a state of recent evolution, which goes somewhat similar as this of the genus *Notocactus*, ~~convergent~~ convergent in the differentiation of the seed as well as in the insertion of the stamens. I have not yet done much with this genus.

But: Just a week before the congress I have got flowers of *Discocactus alleolens* and of *D. heptacanthus* and was able to examine them very exactly. The result: *Discocactus* is very near to *Weingartia*! It has short equal stamens, the equal, rather unusual, stigma which is covered around with very fine papilles and even *Discocactus* has the protruding inner integument at the seed, although not in such a striking manner. Thus the descendance of this genus now is cleared too! And - a fine thing more: In the Mexican Journal II, Issue 2, H. Bravo has given a very exact description of *Melocactus delessertianus* with good drawings, and just this species is the missing link between *Discocactus* and *Melocactus* in the inner structure of the flower. I have examined flowers of other *Melocactus* species with shorter flowers and these, also, have shown equal shape of the nectary-tissue, equal stigma structure.

As to subtribe, *Notocactinae*, I have not yet discovered the origin. May be, that *Notoc. graessneri* might be an intermediate link, as its seed still has a distinct micropylar porus. But many further examinations will be necessary. Only one thing is clear to me: *Parodia* must become reduced to the microspermic species. Otherwise it would not be possible to separate it from *Notocactus*.

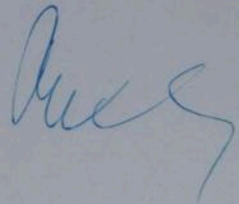
You see: although I am now working with this extremely confused tribe about half a year, I am still on the beginning, but nevertheless I have already got rather important results. Thus I hope to get the work finished in about one year more. But I think, my present results already will be useful for your own researches.

Finally: Excuse the very bad prints of my photographs; They have been made at our local drugstore and are terrible, although the negatives have given very good dias. But you can see all that is essential.

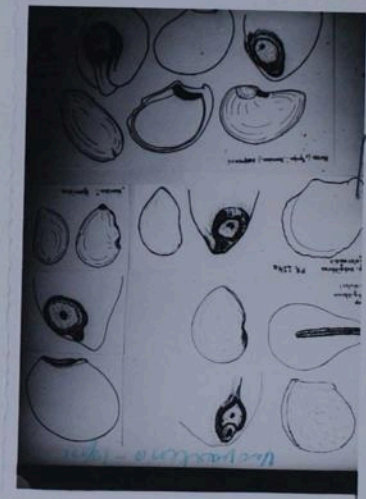
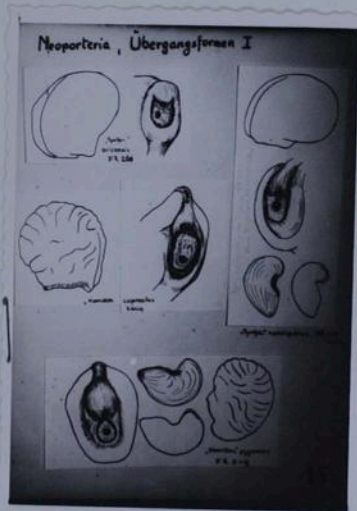
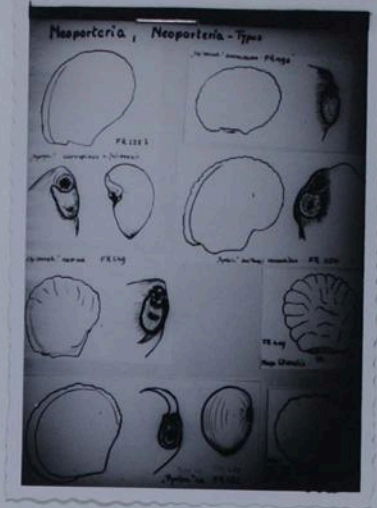
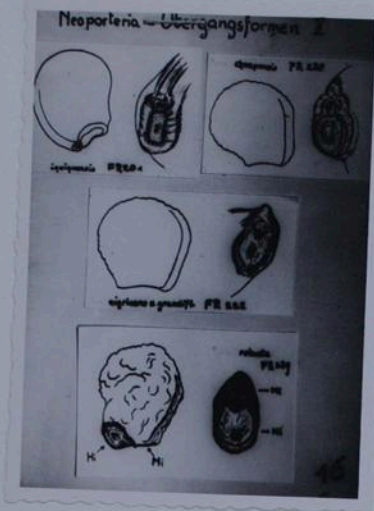
Just yesterday I have got the soil sample of *blossfeldia* soil. Thank you very much. When I have examined the soil, I will tell you the result.

Now, this letter became just a scientific lecture! I hope it will interest you. And I hope, you may have any day a little time for a letter. For I would be much interested for your answer!

Best greetings from Friedl! and all the best from yours



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B to PCH





8 Oct. 1963

Dear Buxl,

A rather fantastic letter---long, crammed with information, and fascinating. I find it very stimulating.

I haven't seen Sukkulantenkunde 7/8, and I certainly hope it arrives soon. I asked Krainz to send at least 6 of them as soon as they were off the press, but they haven't come. Probably they will arrive after I leave. Too bad. I'm pleased to hear about your findings on *Austrocactus*, for on this basis I can retain *Pyrrhocactus* in Ritter's sense and expand it as well. For I believe that *Neochilenia*, *Horridocactus*, *Reicheocactus*, and all these new things of Ito and Ritter, belong in *Pyrrhocactus*. I would keep *Neoporteria* sens. str. separate. This does not disagree with your findings. I don't think you should use "*Nichelia*" as the name is not valid. Of course at a new level you can use it, but maybe resurrecting that old name will just add to the confusion of names already in use. Your option of course. I am most delighted by your findings on *Islaya*, *Weingartia* and *Gymnocalycium*. Particularly the latter has puzzled me, although I have never had occasion to go into the problem, since *Gymnocalycium* seemed adequately distinct to me, just as *Copiapoa* does.

After having looked over your drawings and compared to those we have on file for the Chilean species, I think that more material should be studied in the hope that a clearer organization of the *Pyrrhocactoid* things can be arranged for. You and I seem to agree to go back at least to *Pyrrhocactus*, and to separate *Neoporteria* (the pink-flowered ones with erect inner perianth segments) for the time being. Except for *Copiapoa*, I put all the rest of the Chilean globular cacti in *Pyrrhocactus*, and there it is a question of how to organize them. Then, is *Islaya* to be treated as a genus or subgenus, or put into *Copiapoa*. The last I doubt! I think we can include it in *Pyrrhocactus* though, as a subgenus, and perhaps some of the very woolly-flowered things of Chile belong with it. *Corryocactus* we keep distinct, as we do *Austrocactus*. How to separate *Notocactus* and *Parodia* present a real problem, and perhaps they are not worth separating! At generic level at least. Fortunately I don't have to face that problem with Peru. Later perhaps. *Neowerdermannia chilensis* and *N. peruviana* we have but have not flowered, so their position will have to wait until we get flowers and fruit. My present inclination on this group seems to coincide with yours, that *Weingartia* and *Neowerdermannia* can be fitted into *Gymnocalycium* as subgenera, or maintained as weak segregate genera, but the recent additions to *Neowerdermannia* are questionable. We have always agreed I think that *Discocactus* and *Melocactus* were *Gymnocalycioid*, but it is good now to have some evidence from morphology in support of this idea. They rank as genera in any case, and I think no one has seriously challenged this contention. Probably we are going to find other missing links in this group, perhaps things not yet discovered.

As usual I hope you can integrate studies of seed with those of flower and other morphological features of the plants. I wish it were possible for you to use our materials. But I can't possibly get a shipment to you before I leave as am swamped in final preparations for departure.

On the whole you have done a remarkable study for only six months work, and I hope you can continue with it. Meanwhile I'll be in the Andes trying to find new problems for you to solve. Best wishes to you and to Friedl.

As ever,  
P.C.H.