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

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

4/24/15

New York Botanical Garden,
Bronx Park,
New York City.

April 24, 1915.

Mr. L. P. Gratacap,
American Museum of Natural History,
77th Street & Central Park West,
New York City.

My dear Mr. Gratacap:-

I return the enclosed letter from Mr. D. Le Roy Topping.

We have very large collections of Philippine Island plants here, but no special collection of seeds nor or fruits, and we do not have funds available at the present time to increase these collections.

Yours sincerely,

N. L. Britton

Director-in-Chief.

10/29/18

AMERICAN FERN SOCIETY

ORGANIZED IN 1893

THE OFFICIAL PUBLICATION (STARTED IN 1910) OF THE SOCIETY IS THE

AMERICAN FERN JOURNAL

A QUARTERLY MAGAZINE, AND THE ONLY PERIODICAL IN THE WORLD DEDICATED PARTICULARLY TO THE CONSIDERATION OF NATIVE FERNS

The Fern Bulletin, the former Society organ, ceased publication in 1912 with the completion of Volume XX.

Oct. 29, 1918

Mr. D. LeRoy Topping,
Bureau of the Treasury,
Manila, P. I.

My dear Mr. Topping:-

Mr. Winslow has sent me your letter in regard to the proposed reprint of the early volumes of the Fern Bulletin. Counting yours, we have had only three orders for such a reprint, so that it will be impracticable to attempt it now. Perhaps after the war we may try again.

Meantime, if you care to have me, I shall be glad to insert in the Journal a brief notice, stating that you are in the market for the missing numbers of your set. The chances of your getting them that way would not be very great, I fear; still, there would be a chance.

Very truly yours,

C. A. Weatherby

List of botanical collection made by D. Le Roy Topping at Vladivostok and vicinity during the Summer of 1919. (add 2000 to the number on original label)

No.				
2001-2005	May	4	1919.	Vladivostok
2006-2020	"	11		" - First River
2021-2026	"	18		"
2027	"	20		" [illegible] Park
2028-2046	"	25		Russian Island, near Officer's School
2047-2064	"	30		" "
2065-2074	June	1		Vladivostok - First River
2075-2076	"	7		" - Second River
2077-2100	"	8		Russian Island
2101	"	12		" "
2102-2108	"	15		Churkin
2109-2122	"	15		Russian Island
2123-2129	"	18		Vladivostok
2130-2159	"	22		Fortress No. 6
2160-2162	"	23		Vladivostok
2163-2166	"	27		"
2167	"	28		"
2168-2191	"	29		Evacuation Hospital
2192-2199	"	29		Russian Island
2200-2208	July	4		Vladivostok
2209	"	4		" Base Ball Grounds
2210-2241	"	6		Near sea shore (Monaco)
2242-2243	"	12		Vladivostok
2244-2253	"	13		Russian Island
2254-2255	"	19		Vladivostok
2256-2279	"	20		Okeansky - Bay of Amur
2280	"	22		Vladivostok
2281-2288	"	24		"
2289	"	26		"
2290-2309	"	26		Evacuation Hospital
2310-2322	"	27		Russian Island
2323-2324	August	3		Vladivostok
2325-2344	"	9		Near sea shore (Monaco)
2345-2358	"	10		Russian Island, Czech Hospital
2359-2360	"	20		Vladivostok
2361-2370	"	22		"
2371-2388	"	23		"
2389-2391	"	24		Russian Island, - Radio Station
2392-2409	"	29		Vladivostok
2410-2423	"	30		"
2424-2454	Sept.	6		" - Second River
2455-2459	"	7		Gornesty
2460-2462	"	12		Vladivostok
2463-2472	"	13		" - First River
2473-2485	"	20		Near sea shore (Monaco)
2486- 2497	"	21		Russian Island
2498-2515	"	27		Evacuation Hospital
2516-2529	"	28		Russian Island
2530	Oct.	9		Fortress No. 6

2/20/19

AMERICAN FERN SOCIETY

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Feb. 20, 1919

Mr. D. LeRoy Topping,
Bureau of the Treasury,
Manila, P. I.

My dear Mr. Topping:-

Since receiving your letter of December 18th last, I have heard of a set of the first twelve volumes of the Fern Bulletin which is for sale by one of the oldest members of the Fern Society, Mrs. Judth H. Coffin, 120 State St., Newburyport, Mass. The set, if as represented, undoubtedly contains the two numbers you want to get, but is perhaps more extensive than you would care to buy in order to get them. Mrs. Coffin wants, naturally, to sell it as a whole, and might not be willing to wait to hear from you if another customer turns up in the mean time. Her price is fifteen dollars.

If I ever get a chance to pick up the two numbers you want separately, I will, if you wish, buy and hold them for you. Such chances do occur occasionally.

By all means let me know if you come this way next spring. I shall be very glad to see you and help you renew your acqauintance with this part of the country.

Very truly yours,

C. A. Weatherby

10/9/21

O.A.C.,
Guelph. Ont.,
Oct 9th 1921

Dear Degener:

I intended writing you before, but on account of loosing your address had to wait untill I received your letter.

First of all I want to thank you and your sister for all that you did for me in New York. If it hadnit ben for you I would surely have ben stranded as far as seeing anything goes, besides helping me out with the other things. I am afraid that I seemed very rude when I left you so abruptly at the station, but I was more or less excited about getting to my train I expect.

I had a very pleasant journey on the train, Brown was there and I also met a man who knew a number of my Bermuda friends.

I arrived at the fruit farm at Grimsby just in time to get the last

of the fruit, mostly grapes with a few peaches and pears. While there I went in swimming in Lake Ontario, it certainly seemed cold to me after Grape Bay & Pink Beach.

I have been here at the College two weeks and am gradually getting settled down after having a week of lectures. Our year is still as large as ever, a number of new Scotians having come in it. There are probably over 150 of us the largest third year ~~ever~~ that has been here.

About your herbarium specimens, as yet I have been unable to get any, still things are a bit unsettled yet. However, I doubt if I will be able to get very many for you as I imagine that we do not have to make as extensive botanical collections as your College. And also most of our collections are of the common weeds most of which you will probably have.

The first and second years make only the small weed collections, the third year none while only a few of the options in the fourth year require collections and these men of course are not here when we come back in the Fall. However I will try to do my best and will let you know as soon as I get any.

I am glad to hear that you have changed your major to Botany as I think that that would suit you the best. As you know I am taking Biology and specializing in Botany but I will do very little specializing in Biology until next year. Thanks for offering to help me about that post graduate work, but that is still too far in the future.

I saw Porter's collection of Bermuda map shots they are very good. He asked to be remembered to you.

Yours sincerely,
Alan Dwe Ballan

4/7/22

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

April 7, 1922.

My dear Mr. Degener:

I am much interested in your Lycopodium prothallia. Those I found are L. complanatum. I have been intending to write a note for the Botanical Gazette telling of their occurrence. It is very interesting that a second station should be found in Massachusetts when there is only one recorded for the United States*. I can include your account with mine, or, what would be better we can send our accounts in together. Have you any data about the locality and the proximity of the parent plants? Mine were not less than 75 ft. in any case.

At the present time I am not planning any morphological work on the prothallia, but I did make some section to see if the usual endophytic fungus was present. It was present in every specimen which I examined.

I thank you very much for letting me know about your find. I hope to be over at ~~the~~ Clark Hall soon and we can talk over our material.

Yours sincerely,

Alma G. Stokey

*Bot. Gaz. 63:66-76. 1917.

101 Butterfield Terrace,
Amherst, Mass.
May 8, '22.

My dear Professor Stokey:

Mr. Martin, a fellow student of mine, found the gametophyte of L. clavatum a few days ago at Orient Springs. He has kindly given it to me.

I am planning, at Dr. Lovey's recommendation, to study the anatomy of the young sporophytes and to compare them with the mature plants. I will wait until I see you before

proceeding with the gametophytes.

It is very kind of you to offer to include my account of L. obscurum with yours of L. complanatum. I hope we will be able to work out these three species together.

Sincerely yours,
Otto Degener.

578/27

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

June 6, 1922.

My dear Mr. Degener:

I have been rather slow in acknowledging your letter with the interesting news of another stand of Lycopodium prothallia. Massachusetts is doing nobly. I would like to see your material and would be glad to learn about the Orient Springs collection. I can come over to Amherst almost any afternoon next week and would be very glad to talk over the prothallia. Is there any afternoon that you are free and what afternoon would be convenient for you?

Yours sincerely,

Alma G. Slothey.

6/6/22

6/7/22

UNITED STATES DEPARTMENT OF AGRICULTURE
FEDERAL HORTICULTURAL BOARD
WASHINGTON, D. C.

June 7, 1922.

Mr. Otto Degener,
101 Butterfield Terrace,
Amherst, Mass.

Dear Sir:

We are in receipt of your letter of June 3 requesting information in regard to the importation of dried and pressed herbarium specimens. Please note the enclosed copy of Quarantine No. 37. Since these regulations apply to plants or plant products capable of propagation, herbarium specimens may be imported without a permit as they could hardly be considered as capable of propagation. If you wish to make importations of plants as listed in Regulation 3 of Quarantine 37 you should make application on form 100. It is quite likely that you will wish to bring back some seeds or nuts and therefore will require a permit.

Very truly yours,

O. K. Courtney,
Plant Quarantine Inspector,
Foreign Plant Quarantines.

okc-gh
(Encl.)

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

June 9, 1922.

My dear Mr. Degener:

I had been intending to go over to Amherst anyway, but I think I shall not be able to do so next week, so I shall be very glad if you will come over to South Hadley, Monday afternoon. I shall be in my office in the "Temporary Science Building" which you can easily locate as it is over the hill from the gate, and just south of the Power House. I shall be very glad to see the lycopodium material.

Yours sincerely,

Alice Stokey

6/9/22

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

June 13, 1922.

My dear Mr. Degener:

Congratulations! I shall surely hunt up the spot.

You should have heard the operator wrestle with your message. It was almost as good as the message itself. She has learned a new word but she will never be able to use it.

Yours sincerely,

Alice Stokey

6/13/22

101 Butterfield Terrace
Amherst, Mass.
June 14, 1922.

My dear Miss Stokey:

While gathering a few plants for the Freshman laboratory yesterday, I came across another station of gametophytes. I am almost sure that they are L. clavatum. Since I wish to photograph the place, I took about twenty. In view of the fact that I have so many at this place and can get more accurate data, I would prefer that

you do not write to the "Botanical Gazette" for me. ~~I would rather send in my own account and do this at the same time as you send in yours.~~

~~I will try to photograph the place Monday~~ Sunday

I would rather send in my own account, ~~and do send it in the same time that you send in yours.~~ at the same time that you send in yours. I will let you know more definitely next week about this find and what I expect to say about them.

It certainly seems as though gametophytes ~~were regular weeds~~ ~~grow like~~ in this state grow like weeds.

Yours sincerely,

6/14/25

6/14/22

UNITED STATES DEPARTMENT OF AGRICULTURE
FEDERAL HORTICULTURAL BOARD
WASHINGTON, D. C.

June 14, 1922.

Mr. Otto Degener,
Massachusetts Agricultural College,
Amherst, Mass.

Dear Sir:

We are in receipt of your application and in response have issued permit No. 4938 authorizing you to import nuts and seeds in accordance with Regulation 3 of Quarantine 37. No permit is required for the importation of herbarium specimens which are dried and are not capable of propagation.

Very truly yours,

O. K. Courtney,
Plant Quarantine Inspector,
Foreign Plant Quarantines.

okc-gh
(Encl.)

6/22/22

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.

June 22, 1922

To Whom It May Concern:

Mr. Otto Degener has been a student at this College for four years and will graduate this month with the degree of Bachelor of Science. He has specialized in botany and has taken most of the undergraduate courses offered by this department. Throughout, he has shown marked adaptation to work in the natural sciences and has maintained a high standard of scholarship.

For two years Mr. Degener has been employed in the department as an assistant in connection with the teaching of laboratory courses in general botany. I consider him one of the best men we have had for this type of work and were he available I would be very glad to continue his employment for this purpose.

Mr. Degener is a young man of clean habits and high ideals. In training, experience and natural inclination and ability he is well qualified to teach general botany in either secondary school or college and I am glad to recommend him for any position which he may seek.

A. Vincent Osmun
Head of Dept. of Botany

AVO/M

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

July 7, 1922.

My dear Mr. Degener:

You surely are in luck. I congratulate you. But I don't believe that Lycopodium prothallia are very common for all that, as I have been looking for them ever since I came here and have only found one station. But I am not a very good collector.

Of course you ought to write your finds up yourself, but I think it would be a good idea if we could send in our notes at the same time.

Have you found if they are all infected with the endophytic fungus? I think it is almost certain that they are but it seems desirable to make sure before even sending in a note about them. We must have the right fungus in this region.

I hope to write up my note in a day or two and I will send it on to you to see if you would like it. I have been away and on my return my mother was ill so I have neglected to acknowledge your note.

Very sincerely yours,

Alice G. Storey

7/7/22

7/25/22

The Olympic Club
San Francisco

July 25th 1922

My dear Miss Stokey:

Your letter of July 7 has just been forwarded to me. It is very kind of you to wish to send me a copy of the note you are writing. I was going to send mine to you as soon as I had finished it as I have never done anything like this before and you might possibly be so good as to criticise the many mistakes I am bound to make.

I had intended sending my note to you right after Commencement but I was terribly busy with examinations; and then with packing up that I could do nothing about it. Then I left immediately after the Commencement Exercises for Canada and have been traveling ever since, not staying in any one place for more than a day or so. Now I am in San Francisco wondering how soon I can get to Hawaii. I have been having a little trouble these last few weeks and may have to have my tonsils removed in San Francisco before I do anything else. So I will have to wait until I get to the Univ. of Honolulu before I can ascertain the presence of mycorrhiza as you suggest. It might be of interest to you that the hemlocks growing in that general locality have rootlets with tips very much swollen with some endophytic fungus. I also noticed what is probably a Treme llodendron growing in 2 spots rich

with gametophytes. There may be a connection between this and there may not. I do not like to say.

I hope you do not mind that I am so slow-pokey about this. I do wish to get to a laboratory before I publish anything at all.

Very sincerely yours,
Otto Degener.

Did I tell you that I collected over 200 gametophytes of all stages in the few hours at my disposal? There are actually many thousands in that spot. Almost wherever I dug I found them "galore". Strange that no mature sporophytes are growing there!

8/7/22
1 – galore
2 – mature clavatum
3 – outhouse

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

August 7, 1922.

My dear Mr. Degener:

Lycopodium prothallia are on the boom. My brother and I have found another station; he found it while we were both hunting. This is a small patch on a small island in Forge Pond near Granby. My brother was inspired by my account of your telegram and he started out with the slogan "gametophytes galore." They were not galore, but I found nine prothallia without sporelings and 14 with sporlings of all ages. It is not nearly so large a patch as my first one.

The interesting thing about the last find is the type. I am quite sure that they are L. obscurum and that what Spessard called L. obscurum were L. complanatum. There are three species on the island with mature plants within 2-15 feet from the patch of sporelings: L. complanatum, L. clavatum, and L. obscurum. The prothallia are of the flattish more or less irregular type, often saddle-shaped. As I was collecting them I took it for granted that they were L. clavatum as Bruchmann has described the prothallia of L. complanatum as of the long turnip shaped type and my own collections which are unmistakable also show it, and Spessard described those of L. obscurum as being so like L. complanatum that there was difficulty in identifying them. He questions all of his L. complanatum collection except one sporeling with its prothallium and thinks they may be L, obscurum. Judging from my collections they are all L. complanatum and none are L. obscurum.

When I began to examine the sporelings I found that they did not show the slightest sign of the L. clavatum type of leaf (with the hair-like tip) even when the plant had got to the stage of branching four times. The leaves on the unbranched plants and on those with one fork are exactly like those on an unbranched sporeling of L. complanatum. The sporalings of L. complanatum do not show the slightest indication of the peculiar type of leaf until after the first branch and before the change in type of leaf occurs there is a change from a six rowed arrangement to a four rowed. By the time the plants have branched three or four times they begin to look distinctly like L. obscurum. I found several young plants on which I could see the shriveled remains of the prothallium still present after the plant had begun to develop the underground stem and the erect branches, characteristic of L. obscurum and very unlike that of L, clavatum. I am inclined to think that Spessard was fooled by the juvenile habit of L. complanatum, and the failure of the characteristic leaves to appear until the sporeling is fairly good sized so that he thought his sporalings were L. obscurum. L. annotinum has the flattish type of prothallium but we have none in this immediate region, and none of my specimens show any indication of the serrulate leaf.

I have written to Dr. Chamberlain about the identification of the new prothallia and have asked him if he would compare my prothallia and sporelings with the Bruchmann collection. As I remember thay have both L. annotinum and L. clavatum at Chicago.

8/7/22

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

I am inclined to think that identifying sporelings is not as easy as it might seem to be at first sight. Are you dead sure of yours?

What species have you found? Your last was L. clavatum, was it not?

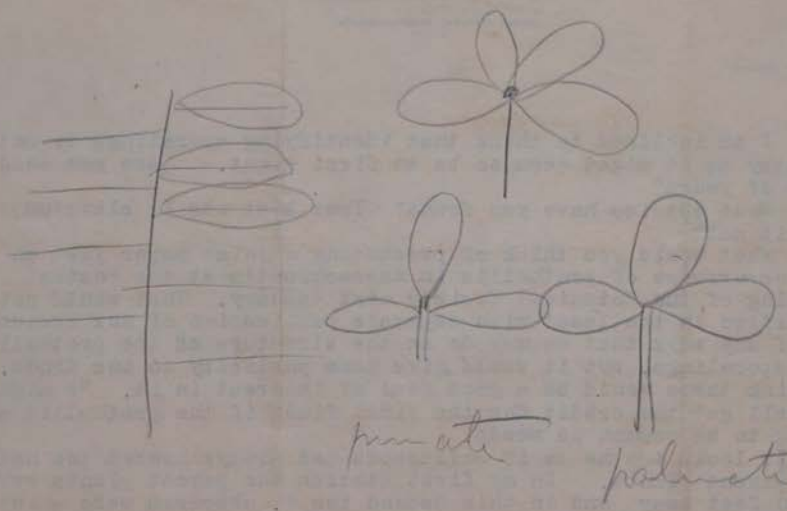
What would you think of presenting a joint paper just on the occurrence of prothallia in Massachusetts at the Boston meeting of the Botanical Society next January. That would not interfere in the least with separate publication of our accounts or of any work that we may do on the structure of the prothallia and sporelings, but it would give some publicity to our finds. I think there would be a good deal of interest in it. We might as well get the credit for the first finds if the prothallia are going to be common as weeds.

It looks to me as if collectors had always hunted too near the parent plants. In my first station the parent plants were 75-80 feet away and in this second the L. obscurum were about 15 feet away.

I almost forgot to say that I found in the earth among the others three prothallia of the turnip-shaped type but none with sporelings.

Yours sincerely,

Alma G. Stokey



8/11/22

HAWAII PUBLICITY COMMISSION

OFFICIAL INFORMATION BUREAU OF THE ISLAND OF HAWAII
HILO, HAWAII

POSTAL ADDRESS
HONOLULU, T.H.

OFFICES
ALEXANDER YOUNG HOTEL | HONOLULU | PHONE 1234
MOANA HOTEL, WAIKIKI | PHONE 2666

Honolulu, T. H. August 11th 1922.

Dear Miss Degener:

As a revised itinerary I would suggest the following trip:

Tues. Aug 22. Leave Honolulu for Island of Kauai.
Wed. Aug 23. Arrive Lihue. Visit Hanalei. Sleep at Waimea.
Thurs. Aug 24. Visit Waimea Canyon.
Fri. Aug 25. Visit Olokele Canyon.
Sat. Aug 26. Rest. Embark from Koloa at 6 p.m.
Sun. Aug 27. Arrive Honolulu Early.

—

Tues. (Aug 22)
~~Fri. (Sep 1.)~~ Leave Honolulu for Island of Hawaii.
Wed Sat. Sep 2. 8/23 Arrive Kailua. Visit Keauhou. Sleep at Wall's.
Sun. Sep 3 8/24 Visit Napoopoo – Honaunau. Sleep at Wall's.
Mon. Sep 4 8/25 Drive through to Volcano. Sleep Volcano House
Tues. Sep 5. 8/26 At Volcano. Sleep Volcano House
Wed. Sep 6. 8/27 To Hilo via Puna Coast. Sleep Hilo Hotel
Thurs. Sep 7 8/28 A.M. Railroad Trip. ~~P.M.~~ AM Rainbow Falls. asrd-
~~Fri. Sep 8.~~ 8/29 Embark 10 a.m for Island of Maui.

VISIT HAWAII THE "SCENIC ISLE" CONTAINING HIGHEST SNOW-CLAD MOUNTAINS IN THE PACIFIC
THE ONLY ACTIVE VOLCANOES
GREAT ACTIVE CRATER KILAUEA REACHED BY AUTO
MAGNIFICENT TROPICAL SCENERY
WONDERFUL WATERFALLS
UNIQUE LAVA FLOWS
MILES OF CAVES AND LAVA TUBES
ANCIENT TEMPLES AND CITIES OF REFUGE
GOOD AUTO ROADS THROUGHOUT THE WHOLE ISLAND AND GOOD ACCOMMODATION EVERYWHERE
COME OVER AND SEE US

Cont.

?8- -22

to make the whole journey in comfort with lots of room to spare. The ideal number would be four persons. This would reduce the cost of the car to $35.00 (£7.1.-) each + leave plenty of room for your luggage + your brother's specimens. The car would be a 7 passp. Cadillac.

I would prefer to issue you inclusive (all expense) tickets for all or any part of this itinerary. They will save you a lot of bother & will prevent any disputes. They cost no more and amply protect the passenger.

If the itinerary appeals to you I shall be glad to go over it in complete detail with you.

Yours very truly

Rodd Bis-Morton

Miss I. Degener.
Young Hotel.

? 8- -22

Sat. Sep 9. 8/30 Arrive Lahaina - Wailuku. Visit Iao Valley & Waihee.
Sun. ~~Sep 10 8/31 Rest & short local excursions~~.
Mon. Sep 11 8/30 Visit Kula & Haleakala. Drive to [illegible] evening
Embark Kahului for Honolulu at midnight.
Tues. Sep 12 8/31 Arrive Honolulu. 7.30 a.m.

The entire cost of these combined trips for both of you would be just about $450 66/100 - or - say - £90-4/5. It is made up as follows:

Island of Kauai.	Auto	51.-		
	Hotels	37.-	88.-	£17-12-6
Island of Hawaii	Auto	140.-		
	Hotels	60.-		
	Railway	5.66	205.66	41-2-9
Island of Maui.	Auto	30.50		
	Hotels	32.50	63.-	12-12-6
Steamers.	Kauai - return	36.-		
	Hon to Kailua. Hilo to Lahaina. Lahaina to Hon	58.-	94.-	18-16-8
			$450.66	£90-4-5

With regard to the Island of Hawaii section may I point out that I am quoting you an "inclusive" automobile rate, which means that a machine would come from Hilo to Kailua to fetch you & would stay with you until the conclusion of your trip.

Should you decide to handle it this way and fix your date definitely I would do my best to get others to go with you. They would then pay you (or me - if you so prefer) for their share of the car thereby relieving you of the burden.

I suggest this method because it is safer & better than relying upon Japanese driven jitneys & would enable you

9/1/22

Sept 1, '22.

My dear Miss Stokey

I received your letter of Aug. 7 in which you wrote of yours and your brother's find. I was not satisfied. We will have lots of material at this rate. I hope to equal you and find something there.

I was not. It is fine that we have so much material together, but best of all is that you may correct Spessard's mistake.

I was not satisfied with my identification and now I am still more puzzled. My material from specimens from Great Barrington, which you have seen, I thought to be L. obscurum while you find that a saddle-shaped prothallus belongs to that species. During Commencement this while I was getting the prothalli by wholesale, I went to a hemlock grove not far distant and found about a dozen sporelings only one of which had a respectable gametophyte attached. Many of the others have the little knot as yet showing that the gametophyte had just lately rotted away. The largest sporeling is about 15 cm. long. I showed them to Dr Lorey and told him that I thought them a different species but he squelched me. My reason for thinking this was chiefly due to the fact that they had a different appearance while growing. Just as all they were procumbent and seemed more delicate than the others I had found. Then again,

to me the leaves appeared ~~[illegible]~~ only to have setae although Dr Torrey doubted it. I hate to think # about the probability of who is wrong.

The prothallus, which is saddle-shaped just like the "galore" material, was but a fraction of a cm., below the surface ~~[illegible]~~ while the latter species was buried 2 to 9 cms (one was at 1 cm). Thus I felt almost convinced that these however 12 are L. clavatum. If I am correct in my "conjection" ~~conceit assumption~~ that these differs from the "galore" group, the latter are undetermined. So I will not answer your question until I have ascertained the sporophytes of these three groups. I can do this ~~inside of~~ in a few weeks.

Today I was able to see Dr James B. Pollock, the Exchange Professor from Michigan who just arrived, about my material ~~and~~ He will permit me to work upon it for my thesis. This, however, would not interfere with the presenting of a joint paper on the occurrence etc., at the Boston meeting of the Bot. Society. I certainly appreciate your kindness in this offer. I ~~[illegible]~~ only hope to be able to ~~get~~ collect more material for this ~~from~~ in Hawaii.

Sincerely yours,

A. L.

9/24/22

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.

Sept 24.1922.

Dear Degener:

By this time you must have attained your island paradise--palms and ferns and precipices, flaming volcanoes and maids with shredded wheat costumes. So I sit me down and scribe to thee since I now know where to direct a letter.

I heard yesterday that the tonsils about which you wrote me from the west turned out to be a broken nasal septum; I hope you were successfully repaired in SanFrancisco (Certainly some junk shop would have a perfect one to put in your nose). And now with the machine running smoothly we can expect great things from you.

You have my full sympathy concerning the Cambrian trilobites and brachiopods to whose heights you aspired but were unable to attain. But cheer up, son, they'll keep--they are non-perishable.

Naturally we are all interested in what has happened to you in Hawaii; we want to know what the University is like, whether you are able to get the supervision that you need for your work; whether Mr. McCaughey is a human being etc. How are the lycopods coming on, and last but not least how many thousands of endemics have you been able to add to your "Hortus siccus"? We read that Kilauea is acting up again and we hope that you wont let your scientific enthusiasm for vulcanism make a second Pliny out of you--after all a live scientist is often of more value than a cremated one.

I wont bore you with non-essentials about M.A.C. but I'm surethe

24-IX-22

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

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MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.

following facts cannot be left untold:

We havn't yet gone loose again but wild rumors are afloat that there are some 250 2-year Freshmen and only 175 4-year. My Lord, chickens and pigs will be draped all over the place.

Potter, Friend, Lindskog and Hunter are to assist in Bot 2s this Fall.

Woodworth is transferring to Yale Medic and tells me incidentally that he and Dorothy have decided that life apart from one another is impossible (he didn't use just those words but I infer that it is what they have aggreed). Anyhow they are engaged and he did tell me that he hopes the consummation may not be long delayed. So do I hope so for his sake, for his case is an excellent illustration of the lines:

"Love is like a giddiness,
It will na let a young man
Gang aboot his business."

This year we have four major students: Lindskog, White, Whitney and Woodworth the younger. The last shows lots of promise and I hope I shall be able to save him from hell.

As to other news of gret importance:

Lindskog is growing a moustache; his upper lip is strigose-hispid like a Hieracium aurantiacum leaf.

Mrs. Davenport isn't going to serve any more Sunday night suppers--Coises.

Jimmy Watts is getting fat so that he looks like a woodchuck.

The Chem Lab. burned down to the ground a few weeks ago midst great rejoicing among the natives (except for the loss of equipment). When Billy Hasbrouck saw the firemen trying to put it out he is reported to

24-IX-22

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AMHERST, MASS.

have said:"What they putting w ater on it for,Let the dam thing burn". S tockbridge basement and Micro, will be used for Chem.classes this fall

Mac's dog,Freckles, was run over a few days ago and snuffed out--Much gloom!

Miss Wallace is going to live with me at Mrs, Fearing's this year. I don't mean quite that but she is to have Joe Whitneys room.Do you suppose we can shave together in the bathroom mornings?

Your Geology friend Rich. Whitney came to see me last week;I guess he has decided that the only thing for him is business.I shifted him into Organic Chem.in place of Histology and French.

Clark Hall has just recieved a new coat of paint inside and we have all been cussing about the turpentine smell. Lindskog the younger is taking his entrance exams and is working for us a few days;the small lecture room(B) is being fitted up as a laboratory for Papa Davis and his mikes.

The marigolds planted by Prof.Waugh in front of Clark Hall have waxed prodigious strong and make two yaller streaks running up from the road. Perhaps it would be better to say that they form a golden pathway along which aspiring youth shall travel to the portals of learning(Please read that with the proper touch of emotion).

And now; How about Miss Grace Smith.She must surely have found you for she was on your trail and I was urging her on.You are bound to like her and she would prove a companion to your sister too(That "too" is a bit ambig uous but it means of course,as well as to yourself) Give my best regards to her if she has not already left Hawaii.And remmmber me to your sister.I wonder if you fully appreciate her loyalty and self-sacrifice i

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.

following you to the ends of the earth and making you way straight. I only wish I might have deserved such a favor from the Gods.

Now, Son, I will be glad to get a letter from you and I will share it with others so you wont be burdened with writing to us all. We shall expect to see you soon again, for time goes mighty fast and in a year or so you will be back covered with glory.

Mit luf

Ray E. Torrey

9/29/22

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

September 29, 1922.

Dear Mr. Degener:

I am enclosing some photographs of our Forge Pond material - L. obscurum, and also two of my Moody Corner collection. You will notice that the lower part of the L. complanatum sporeling is exactly like that of the other type and you can see how difficult if not impossible it would be to be sure of L. complanatum in an early stage of the sporeling if there were no other check on it than the type of leaf.

I have seen the material that you left at Amherst - our assistant brought it over for me - and I feel pretty sure that both Mr. Martin's material and your June collection are L. obscurum. As for the Great Barrington material, it must be L. complanatum. I deduce that by the process of elimination. The sporeling is too young to show the characteristic type of leaf; it never shows up until after the first forking and often not then. You can see in No. 1 that it would not have shown in that specimen . I have such a good series of the Forge Pond material that I can show every stage up to plants almost ready to fruit. My reasons for calling it L. obscurum are: 1) the series leads up to L. obscurum; 2) there are none of the bristle-like hairs terminating the leaves such as are characteristic of L. clavatum and it branches too much to be L. annotinum - the two of our native species which are reported to have the saddle-shaped prothallium; 3) it develops a deep-seated rhizome like L. obscurum and never is superficial or runs near the surface. 4) the woods where it grows has but three species - L. complanatum. L. obscurum and L. clavatum. It can't be the first or third so it must be the second. I really feel dead sure of it - as sure as I ever felt of anything.

Did I write to you after my brother and I found the third station - our second this summer? We set out to locate some more and we established another station; this one is on Toby, on the west face not more than half a mile from where we began to enter the woods. All the plants are L. obscurum. I hope I shall be able to establish some more stations before the season is over. I know of only one place where L. clavatum fruits abundantly and I am going to do some hunting there. It is fairly common in most of our woods but it does not fruit much, not so freely as the other species. We also found a plant that looked like a sporeling near Enfield, but we did not get any prothallia. Our system is to hunt a fruiting patch and then look for young colonies around it, and then to hunt in the environs of the young colonies for depressions where humus and moisture collects. The colonies are most likely to occur in the depressions. I am not sure how much significance there is in the difference in depth of your two collections. In most cases it seems to be related to the nature of the substratum and the depth of humus, but it seems plausible that L. clavatum would germinate nearer the surface than the other species. I wish I could see that collection that you suspect of being L. clavatum. My L. obscurum material is all much more robust that the L. complanatum - the prothallia grow to be much larger and persist much longer after the sporling develops,

29-IX-22

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

but that may be because they had a more favorable location; the Moody Corner habitat is not so rich as the other two. The sporelings are also much more robust in the case of L. obscurum. No. 5 still has its prothallium.

I am about to start out making a set of cultures to see how long it takes for the prothallia and sporlings to develop. That will occupy some years, I presume. I have selected a place on the campus where I can have a set of cultures which can be examined when necessary, once a year, I presume.

In a short time I shall sent you my sketch about the occurrence of the gametophytes so that we can make up our paper for the Boston meeting. Do you think it likely that you could direct me to the place where you found the L. clavatum (?) material? I would like to verify that if possible.

Yours sincerely,

Alma Stokey

10/10/22

Oct 10. 1922

Dear Miss Stokey:

I just got your letter and those
pictures. Picture No 1 (L. complanatum) has
cleared up things for me. My and your Great Barrington
specimen though not showing the L. complanatum type of leaf
as well as yours approaches it somewhat and
the stem has a tendency to become flattened. This
material then is L. complanatum. As I remember
the gametophytes you showed me were much
smaller than mine. My smallest is 2.5 × 7
mm. while one more typical of the others is 4 × 10. The conditions
for growth may have been better in my case, or the
conditions for fertilization inferior, so that
sexual stage had longer time to growth before
sporophyte developed to sap its strength. Was
the locality in which you found yours very dry?

Mr Martin's as well as my Amherst "galore"
material is without doubt L. obscurum. I
had thought it to be L. complanatum because
I called the Great Barrington material
L. obscurum. Martin brought some L.
clavatum with his specimen telling us that
it was the same kind. This proves that
proximity to mature plants is of no value at
all. This seems evident to me when I consider
the great distance the spores will blow. It
is now definitely settled Martin's and
"galore" specimens are L. obscurum.

As to the material I found in Amherst near the orchard to which I referred in a former letter saying that Dr. Torrey "squelched me" in calling it L. clavatum, a different species from the gabre type, I was correct. So this is settled to be L. clavatum.

Thus of the three species I was fortunate to find I but determined the easiest, L. clavatum; you determined the other two for me. ~~although~~

I will send you a mess of material as soon as I have finished writing up this report. But please send it back to me insured because if it were to become lost ~~any mate~~ I would have nothing to work upon for my Thesis and I must start on this soon.

Dr Davis was ~~so~~ kind as to photograph the gabre locality for me ~~so please ask him to show it to you.~~ If in case you should go to M.A.C., ~~when~~ Dr. Davis is the ~~one~~ that knows exactly where it is. I am enclosing a crude map that might help you in locating the two localities. It might interest you to see the pictures of the ~~plac~~ spot with sporelings which Dr. Davis took for me. They are still at the college. I would prefer that one little spot noted with sporelings be not disturbed because it is interesting to follow out their development. To get material most easily, ~~use a~~ put the earth in a sieve and move it up and down in pail of water until nothing but sand etc. are left. Then you ~~can easily~~ find the gametophytes. I find that even when I think I am through looking over the remains in sieve, specimens are contained in it. I guess [illegible] your method

10/12/22

Oct 12, '22

I just wish to let you ~~know that~~ my suspicion as to finding L. clavatum at Amherst has become a firmly established conviction. There is no doubt about this because of the large sporeling I found with it and the resupinate habit of ~~with~~ them all. I am afraid you ~~cannot~~ I will ~~not be~~ have a difficult time to verify my L. clavatum find in that locality ¿marked upon the map. It is not a favorable place at all. I will send you my L. clavatum material, however, and you will become convinced directly.

Have you read Bruchmann's article I have. In it he describes his culturing gametophytes in flower pot under bell jar. He could get spermatozoids at any time merely by watering them. Can't you get some of my L. obscurum material, cut out all the ~~sporan~~ archegones and then ~~wash them~~ or place sperm derived from other species ~~with~~ upon them. It seems the gametophytes are not delicate at all, and stand hard treatment. If you won't do this I will some day if possible. It would be interesting to develop a hybrid

10/23/22

Oct 23, '22

Dear Dr. Lowry:

I am awfully glad to hear about the Chemistry Building and Lud's hog's emblem of manhood. I ~~hope~~ only hope it was not scorched at the fire.

I saw Ralph Borden when I arrived and now it is up to him to advise me about "World Aggie Night" here. You do not know with what pleasure I anticipate that day.

Now as to Botany — I do what I want and at the time I want to do it. There is no botanist here except Dr. James Pollock, ~~of~~ exchange professor from Michigan. He is a good physiologist I suppose ~~and~~ but is not acquainted with systematics to the extent that I might wish. So in this matters I use my own poor judgment instead of bothering a man who has a family on his hands. ~~The botanis~~ The regular botanist of this institution is of course in Michigan; while Rock, a man who has written a lot upon the plants here, is somewhere ~~x~~ in the South Seas. Possibly he is married to shredded wheat ~~now~~ — they have expected him here about a dozen times for the last two years, but he does not appear. Up to now I have accomplished nothing much. I have been sharpening a microtome razor with a curved edge on ~~a glass~~ plate glass with diamantine for at least ~~35~~ 35 hours ~~until now~~ Now it ~~is~~ has a straight edge but will only cut at 20 μ. I have imbedded a little of my L. material in celloidin and have tried cutting it. I do not know how I will manage. The microtome

here is the same as the one in Clark Hall which Dr. Davis thinks is fine, but which you said Jeffrey advised nobody to get. It is a huge thing in which the knife slides, not the stage. ~~It is therefore~~ Miss Stokey has identified the gametophytes - a think I should have done myself but didn't. All she did was to look at the leaves of the sporelings for the determination, while I was planning elaborate slicing ~~and stelar~~ operations to discover stelar differences. Anyway, I know that the material which I found near the orchard at college is L. clavatum, no matter what you say. Miss Stokey says my Great Barrington material is L. complanatum, and my ~~Amherst~~ Car M.A.C. Campus material is L. obscurum. Now that she says so, I see it too. I have all three species. So please change the labels on the material you have.

Since I cannot get any help here in morphology I have decided to write up L. obscurum since the gametophyte ~~is~~ is unknown. ~~It~~ I am I am afraid ~~that~~ it won't be any good, merely ~~enough for~~ a degree catcher. ~~I think~~. I see Prof. Pollock about once a week for an hour or less, that is all.

But I am collecting plants and tracing them down. The collecting is not as good here (~~I~~ Island of Oahu) as ~~it~~ on Hawaii so I will take ~~a~~ leave in a month or so for Hawaii

23-8-22

and live for two mo[illegible] Kob[illegible] and collect. Besides, I will see the volcano in action. It is getting better daily. There is no danger of imitating Pliny. But one poor fool some time ago took a close up picture of the lava which was overflowing. He was surrounded by a little rivulet and to escape put his camera upon it and used this as a stepping stone to dry rock. He could not quite make it and burned the foot terribly. I must grab plants and off the path of lava flows.

I have very many Canadian plants and quite a few from Mt. Rainier. I have been identifying these. I [illegible] The only books of the [illegible] West here is Coulter & Nelson "New Manual of Rocky Mt. Botany" 1909, a wonderful list of plants collected in the Canadian Rocky Region & through which I passed by Edith Farr in "Contributions from Bot. Lab. of Univ. Penn.," and Piper's "Flora" of Washington "from Contributions from the National Herbarium", Vol. XI. This last just came today. Farr and Piper are merely lists without descriptions so I have lots of trouble in running around for a possible description in Coulter & Nelson or Gray of the plants listed. It is most disagreeable when only a few are described. I will send some of these my duplicates to M.A.C. in a month or so

So you see I need help on this Cyperaceae problem. Any advice will be gratefully received.

Another trouble is that there is no herbarium at the College. ~~The best book~~ They sent it to the Bishop Museum. ~~Mr.~~ Dr. Ball, the former M. A. C. ~~zoologist~~ assistant zoologist seems to be curator of it. ~~During his absence~~ ~~While he was out, I had some~~ Not I asked ~~a~~ a young Entomologist whether I could look through the herbarium sometime ago and he unlocked it for me. ~~Nothing~~ There must be ~~100~~ 50 times as many plants there than at M. A. C. Hardly any of them are mounted and they are merely stuck in just as they were dry ~~enough~~. ~~Not th...~~ Not even Ferns or Mosses were separated from the higher plants. Everything is being eaten by insects. Later I saw Dr. Ball ~~and he must~~ He must have had a fight with the entomologist for allowing me to see it. He told me that he was fumigating the collection and that ~~it~~ it would take a month or possibly two or even three ~~#~~ before I could see the plants. In other words he wants them undisturbed. I was shocked to see them like that. Miss Wallace, I am sure, would have fainted in righteous horror. Plants from Hawaii, of all places should be preserved — many of them are dying out.

You see I found a plant, a nice little plant, with frail ~~creeping~~ creeping branches, and tiny ~~sp~~ strobili. In fact it ~~comes~~ is like no Lycopod described in the books I can find here. Dr. Lyon of the Sugar Planter's Association, a botanist who has

23-X-22

taught before hell got him and who, ~~is one of the best man I have seen anywhere~~, seems to be a very good scientist, tells me that I should make perhaps a new ~~it is probably a new~~ species of it. He advises me to collect all the L. cernuums which I run into most so as ~~to~~ to determine their variations because, in this place things vary more than anywheres else. At any rate it is a new variety varying a lot from L. cernuum. It is for this reason that I wished to look at all the Lycopods previously collected and stored away in the Bishop Museum.

Running down the plants here is not like tracing the Bermuda flora in Britton's book. Since Hillebrand's time in 1888, many new plants have been found, but I cannot get the literature on them. It is all scattered in more or less individual accounts.

Now the only thing that is going well beside the ~~fa~~ mere collecting of plants here is my zoological work. I am working on the hermit crabs of Hawaii. This means drawing every species I can get. Nobody has done anything about them as yet. Here I am again in trouble because 8 months of work at 8 hours a week on this problem is too little. I don't see how I can finish.

¶ Up to now I think I could do just as well ~~if~~ marooned on one of the South Sea Islands with half a dozen ~~books~~ Manuals as far as botany is concerned.

¶ McCaughey is not approachable since he is head of the entire Dept of Education. He has no time for botany. He is one of the most important men here because of the school question. They have a lot

of trouble with Japanese language schools
here now.

11/25/22

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

November 25, 1922.

My dear Mr. Degener:

At last I am sending on the article which I promised last summer. Many things have interfered - teaching, of course - but also the pursuit of more gametophytes. Gametophyting is a great game. As you will see from the enclosed manuscript we are still finding stations, and if the snow holds off we may find a few more before Christmas. We now have nine altogether. Miss Starr of the Botany deparment here who was in at the first discovery has been continuing the game and we are writing this up together. The important thing is to select a good place to hunt; we are making our heads save our knees; you may remember that Mr. Spessard crawled 150 days on his hands and knees for his collections.

I have not yet visited your "galore" station but I hope to do so next week. I want to incorporate a description of it in the report I give at the Boston meetings. I am in hopes that I shall receive your account before that time. I think it must be more like our last station, Aldrich Mills, than any of our others. If I get a chance I want to hunt up another hemlock station.

What do you think of the points I have made about the type of habitat? Do they agree with your finds? I don't feel dogmatic about any of it, but it has worked in hunting up new stations.

I wish that man Spessard would publish his paper so that I can know where he stands. Dr. Chamberlain said he had a more extended paper on L. obscurum and L. lucidulum which was to appear in the September Gazette. It did not appear in that or in the October one and the November has not arrived yet. I shall write to Dr. Chamberlain to find out what he is doing as it is necessary to know before the Boston meeting. L. obscurum ought to be written up properly and it cannot be done until we know whether or not Spessard is going to correct his mistake himself.

As you will see from the list I finally found L. clavatum sporelings and prothallia; they are unmistakable. It is interesting that mine should also be near the surface - they were nearer the surface than any others that we found. Mine are young erect plants, not old enough to have become resupinate, but the bristle hair on the leaves is distinctive.

I have been reading Bruchmann's four articles with a good deal of interest. I want to raise some cultures even if it will take years. I am getting two places ready - one near the patch in the Upper Lake woods. I would like to get germination time and stages for L. obscurum. The prothallia and sporelings seem to be more vigorous than those of L. complanatum; I wonder if it means that they grow more rapidly or more slowly. I am inclined to think they are quicker.

My brother was much interested in the hybridizing idea. I am afraid I shall not have time to take it up this year, and you may have a chance long before I shall. I have a long paper on the prothallia of the tree ferns which I have been working on for years and I do want to finish that. I would like to work up the morphology of Lycopodium obscurum prothalli for a paper. I should be glad to join with you on it if you would like to do so. That is, if Mr. Spessard does not do it himself, and I doubt if he does. I have in mind also a short paper on the gametophyte of L. complanatum. I have been looking over

25-XI-22

Bruchmann's article in the Bot. Zeit. for 1908 and while he has found a great many thing which I have not, he does not describe a thing which seems to occur frequently in my Moody Corner material. I have not sectioned any of the other. It is rather uncertain just at present but it is a point I wish to work out. The prothallium seems to act like a sympodium; after the production of an antheridial crown, a lateral branch will shove it aside and continue the growth. The crowns shrivel and collapse after the antheridia are empty and do not make much of a show, but I have several prothallia sectioned in which it seems as if this has happened several times. It does not seem to be the same type of thing which Bruchmann has described in his article on Lycopodium complanatum. It may be that it happens only when growth is very slow. You remember that my Moody Corner prothallia were very small and rather irregular; that causes part of the irregularity. The soil there is harder and less rich in humus than at any other station; doubtless that is why they are small.

I hope to visit your station this week, and I assure you I shall not disturb any of your plants. I thank you for the map.

I hope you will keep a copy of any paper you send me, for although I shall take the best of care of it and ship it with all possible precautions, I prefer not to take any chances. I am sending you a carbon copy of our paper, which is messy to be sure, but not valuable. I hope we can have our articles ready to publish soon after the Boston meetings.

If you think the material is too diffusely written up please say so. I tried to be sufficuently detailed to give anyone a basis for selecting collecting regions. I shall not give so much material in the Boston report, probably the first page, a brief summary of the stations, and the discussion.

I hope you are enjoying your sojourn in Honolulu. It does sound mighty interesting. Do tell me if that really was a new species of Lycopodium. What type is it?

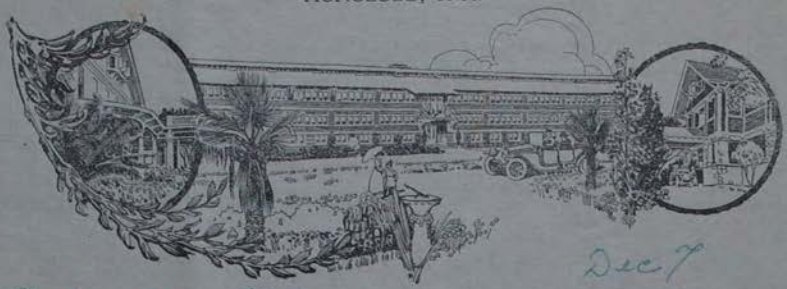
Yours sincerely,

Edwina Stokey

12/7/27

Cable and Wireless Address:
"PLEASANTON, HONOLULU"

PLEASANTON HOTEL
HONOLULU, T. H.



Dec 7

My dear Miss Stokey:

I am delighted to hear of the great number of stations you have worked with Miss Starr. That will put an end to the popular notion for ever that Gymnopodium hardly ever reproduce sexually.

I guess our letters have crossed and you must have my report by this time. If you have not, you had better send me a night letter charged to me. I am terribly afraid that my report will not be in presentable form before the meeting that you can get the necessary data from it.

I notice that you say little about the characters of the gametophytes and of the young sporelings. Can you a brief account of both. Do you think that that is perfectly alright for me to do so? I may give you my plea that my "pièce de resistance" is among the fact, probably an unimportant one, that gametophytes can grow to fair size. And as possibly an account of the characters of the gametophytes may not cause comment. Another plea is that I believe Martin's R. obscurum is the first one ever found, after your explanation of Spessard's mistake, and since I had material "galore" I thought I would briefly describe them as they are unknown outside the botanical department of M.A.C., and Mt. Holyoke.

Another fact I would like to mention is that you do not correct Spessard's mistakes in

calling the L. complanatum specimens he found L. obscurum. Must you not correct that remark before referring to our finds? I took for granted that you were going to do this and consequently wrote in my article Page 6 L. 6. "But since his L. obscurum specimens happen to belong to L. complanatum as Professor Stokey has shown, it is evident that the gametophyte of L. complanatum can grow in several positions."

Then again, you say in your letter Dr. Chamberlain said he had a more extended paper on L. obscurum and L. lucidulum which was to appear in the September Gazette. It did not appear in that or in the October one and the November has not arrived yet. I shall write to Dr. Chamberlain to find out what he is doing as it is necessary to know before the Boston meeting. L. obscurum ought to be written up properly and it cannot be done until we know whether or not Spessard is going to correct his mistake himself. Don't you think that since you discovered the mistake and since you are writing up all your finds that you had better mention Spessard's error in your article? Has Spessard really gone into Zoology or has he gone into Medicine?

My thesis subject at the University of Hawaii is to work up my L. obscurum material, and I have already begun. But if Spessard has found L. obscurum material, and I have already begun. But if Spessard has found L. obscurum gametophyte previous to Mrs. Martin's find, I feel he should have priority in publishing on his discovery. I will of course work upon it for the sake of my M.S. thesis, but since I have never done any original work before and have plants (and Hermit crabs also) to identify there will be lots undone after my thesis is presented at the University. Then will be the time for us to go into the matter exhaustively with the aim of finishing L. obscurum. This would mean spore germination, gametophyte growth and morphology, development of embryo, stelar structure of a sporeling, of old sporophyte

7-XII-22

Cable and Wireless Address:
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PLEASANTON HOTEL

HONOLULU, T. H.

and comparison of the two. My thesis is thus but a
preliminary of the work to come just as your article
of the stations may be considered a chapter of the
finished monograph. If Spessard does nothing
more, I think we are perfectly justified in carrying
out such a program.

Your also say in your letter that you may visit
my "galore" station and assure me that you
will not disturb any of my plants. I hope
you have not misunderstood me but will dig
around and verify my statements. It is only that
one little spot I would like to have remain as a
"National Preserve of L. obscurum gametophytes
and sporelings" so that we can watch this
development under normal conditions. I want to
discover whether the sporelings can survive and
grow to maturity in that hemlock covered spot.
It may give us a clue as to why no mature plants
are found in places of that type. If no forest
fire has occurred, and I believe none has, I
cannot understand why thousands of gametophytes
of all stages and sporelings of all stages up
to a few years of age grow so well in that spot
and not a single mature sporophyte. There is
some important reason for this.

On the first page of your article under station 4
you give "Campus of Mass. Agricultural College".
Please be sure to change that. That is what I
was going to say myself but Dr Torrey did
not like the idea, because he said people might
come and root around for them without having
scientific justification for doing so. Please

change it to "Amherst hillside." Then for station 2 giving "Pittsfield" change to "East Rock Mountain, Great Barrington." That will mean that you must change "Pittsfield station" on page 2 to "Great Barrington station". This change is absolutely necessary since it is more accurate in the second instance (and purposely less accurate in the first) and it will then coincide with my article.

I am not quite sure whether it is correct to say that the "galoo" locality" and the clavatum locality are the same station. Personally I believe they are distinct because of the distance of one from the other, the different type of wood between the two places in question, and above all because not a single obscurum specimen was found in the neighborhood of the clavatum place and vice-versa. If you think this but a technicality which it may be, it had better stand the way it is.

I do not know the procedure that occurs at the January meeting nor what you intend to do about publishing the articles in the Bot. Gazette. Will you give an abstract of my localities at that time? And would my article be sufficient for you to make it? I was under the impression that the report from which I had written was for that meeting, but I realize the impracticability of doing that. I would of course like you to include my stations in your summary.

Now as to publishing our facts in the Bot. Gazette. Do you think that what I have sent you is suitable? Please be absolutely frank and brutal in your criticisms. I know nothing about these affairs nor what to do. I think it were best to have both our reports published as soon as possible. Do you think we can manage? I would be awfully much obliged to you if you would send my article in to the publishers at the same time with yours. It would avoid the possible danger of having them printed in different numbers.

I really think that it were best for me to have my report printed separately from yours because if a beginner like me were to collaborate with a botanist people would of course think that the botanist had entirely attended to the "writeup" while in this way I am entirely responsible for what I say.

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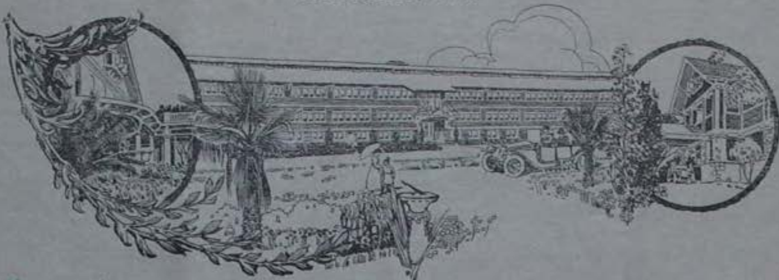
HONOLULU, T. H.



Since I am now resuming this letter at a late hour, I do not wish to wake everyone with my typing, so you must please excuse this writing.

I have been thinking about your statement to me when I was at Mt. Holyoke that Chamberlain had said Spessard had gone into medicine. Then you say his paper is to have appeared in the Sept. Gazette and there is no sign of it. You no doubt have sent Chamberlain some of your specimens of L. obscurum and told him of your discovery of S's mistake. It seems to me that it is your discovery and that you have a right to correct him. I do not mean to say anything against Chamberlain or Spessard but could it not be that you have given them a clue to their mistake, something that they had not dreamed of and that now they are quickly trying to correct their mistake, I think that is perfectly correct for them to do so provided they give you credit for it. But even if they do it, why can't you write up that fact? What difference would there be if you do it or they give you credit for the discovery in their publication. The trouble is that I am stuck this way. I must explain in my article that Martin's find up the gabore material is L. obscurum and how can I do so unless I give you credit for it? I think it only fair that Martin should be the discoverer as he is the first one that actually went out to hunt for gametophytes and was successful. With us it was accidental that we found anything the first time.

Since my article is now finished for the Bot. Gaz. I have changed some things after having gotten your letter and report. Instead of saying as before "that [illegible] all his obscurum

specimens suffer — as Professor Stokey has shown"
I have put "But — as Professor Stokey has informed
me" and I leave upon the development of the proof to
you in the footnote No 2 "The writer considers it only
just that Prof. Stokey give his own version of this
fact."

Then in the summary I mention the characteristics of
L. obscurum, sporeling and gametophyte briefly and in
3-3 state "The gametophyte of L. obscurum is not of
the conical shape as previously reported but of a
saddle shape similar to that of L. annotinum
and L. clavatum." In this I feel that I have
decidedly overstepped my rights because you have
discovered this fact. I hardly doubt that my
mentioning that "Prof. Stokey has informed me" is
sufficient excuse for doing so. Please tell me
what to do.

Another questionable thing is this. I would prefer
to have my article published separately at the
same time with yours but I fear it is awkward
for the reader to refer to a different page for my
stations in your outline. I like the idea of that
outline itself and your method. My objection is
in enumerating my stations as "2, 3, 4"
since it gives (me) no beginning nor ending
to my article and it chops up your account.
How would it be if you were merely to mention
the characteristics of my stations (as to locality
and environment)? Then people do not feel
that they must refer to my article when reading
yours. Thus both our articles will be complete
in themselves. Then it might be sufficient
for any one especially interested to note your
statement of my fuller account of my stations.

I like your "Discussion" very much. The
best part about it is that you know nothing
about my Great Barrington locality and
it fits into your Type II. The Great Barrington
station is essentially relatively dry with

7-8-22

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PLEASANTON HOTEL

HONOLULU, T. H.

4

brambles growing [illegible] [illegible] it had been a forest of mixed [illegible] woods. That [illegible] kept the place from [illegible] drying out excessively as I have noted [illegible] without knowing anything of your types. And the ground did slope slightly in toward the [illegible] though the general locality happened to be a [illegible] side.

Another fact [illegible] might have mentioned in my article is the total lack of mature sporophytes in the "gālore" region. All of them are quite a distance away and [illegible] no connection with the station.

I do not quite agree with your idea that leaves check the ability of spores to percolate into the soil. It seems to me that since the spores are so minute that the slightest amount of water would be sufficient to cause them to become stuck among the soil particles. Another fact is their great length of viability and the tremendous number produced yearly. That is merely my opinion at this moment.

Tomorrow I go to the Volcano for two or three weeks to collect plants. L. [illegible] is all over there and grows to a height of 5 feet when aided by ferns, [illegible] and bushes.

a fern forest is also in the neighborhood. I am wondering what your tree fern problem is and whether I could not be of help to you in collecting spores and keeping them in moist vial on the frond and sending them to you if you study their germination. If there should be anything that I can do, please let me know.

Did my specimens of L. gametophyte arrive safely. One of them is a typical variety g. from which the sporeling developed nevertheless. What do you think about that paragraph of mine?

My new (L) Lycopod is very different from any form here as far as I know. It shows closest affinity to L. cernuum.

I believe I have found something new in Canada but with few books on northern plants at the library here, I cannot say for sure.

Please let me know in what respects you would like to see my article changed and let us try to get it published as soon as possible.

I hope you will have a very merry Christmas. I cannot accustom myself to Christmas in midsummer. It seems ridiculous.

Yours sincerely

12/22/22

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

Dec. 22, 1922.

My dar Mr. Degener:

Your letter of Dec. 7th arrived yesterday and I hasten to answer it although I am swamped to my neck in work - and my typing is sure to be a mess.

I finally heard from Mr. Spessard and am feeling most annoyed at either him or Dr. Chamberlain, whoever was responsible for my not hearing sooner. Mr. Spessard had discovered his mistake about L. obscurum for himself and is writing about the real form. He did publish a note about it in 1918 in the Gazette and referred it to the correct type. His paper is to appear in the December number of the Gazette and I am curious to see it. I don't think it will exhaust the subject at all as he says the L. obscurum is not as abundant in Michigan as L. clavatum. I don't know how abundant any of his material is but I am sure it is not as abundant as ours and probably not as luxuriant. Anyway it is not amiss to have two accounts. You know Lang had an account of L. clavatum in the Annals of Botany very shortly after Bruchmann's account appeared in 1898. I have a feeling that we can write up a better article than Spessard anyway - modesty! I am sorry that I never happened on Spessard's note - I don't know how I happened to miss it - it would have saved me some energy. Spessard is now teaching in Joliet, Illinois.

I have not time to go into the details of your letter as I have not finished up my short article for the meeting on L. obscurum - I am going to give that anyway. I have finished the account of the stations - it is also brief. I don't want to empty the room as people sometimes do; it has happened more than once at the meetings. I have been busy with that material and then most inopportunly some fungus material which I had been trying to get for a year come along just this last week - along with Spessard's letter. I have been trying to get a certain stage of the fungus - Leptomitus - for a year and it turned up just when I wanted it least, but I had to attend to it.

I agree with you about the separate articles and giving the list of stations separately. I really think it is better as I did not see yours for one thing, and you would not get the credit you deserve for another. I hope to send the material to the Gazette shortly after Christmas. I did change the Pittsfield to "Great Barrington," as I discovered from your paper which arrived safely. So did the specimens in perfectly good condition. I'll be careful not to specify the Campus of M.A.C. - it might be dangerous for the preserve.

I started some cultures just before Thanksgiving; some are out of doors both in pots and in the open, in regions where prothallia had been growing; two are in pots in the greenhouse. I want to check up on them every year to see how they are coming on - if at all.

Now I must go to work on the article. I shall write you more fully after Christmas.

I am much interested to hear that the East Rock station fits into the category. As for the explanation of the abscence of sporelings where there are dead leaves, I have no real conviction - it is merely a suggestion. I may test it experimentally.

Yours sincerely, Alice Stokey

12/25/22

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.
Dec. 25, 1922.

Dear Otto D:

It would have been much better had I written this letter some two weeks ago that it might have reached you on Christmas — and that was my intention but sometimes one's intentions fail to materialize. But I have thought of you many times recently for things keep constantly arising to remind me. For example when lecturing in Bot 58 I am always using "a specimen of the plant which Mr. Degener obtained in Bermuda"; or "a sheet of Colocasia which Mr Degener sent me from Hawaii" Thus you realize that your name will go sounding down the ages and that generations yet unborn (and still in the dream-paradise of Devachan) will come forth to call you blessed. Your latest call to fame will sound next week in Boston when a paper is announced "On the gametophyte of Lycopodium ?" by Miss Alma Stokey and Otto Degener. Thus even after you leave the country, your fame continues to mount.

We were much pleased to receive the shipment of plants from Canada and Hawaii. Miss Wallace has mounted them all and they are safely tucked away. I have sent a communication about them to the "Collegian." We can't properly thank you for such things; I realize the amount of thought and work you have put into their preparation and I wish we might make a greater recompense but I guess your reward will have to be one of the intangible sort - gratitude and good will.

And how are all the "dear little hermit crabbies"? And will the "diamantine" microtome knife get cut classes at 50 μ. Have you started on the reproductive anatomy of the palms? Has Kilauea been vomiting any more? You see that you reside in a land of wonder and have a chance to lead us bleak New Englanders into the world of romance.

I'm just thru with the Sophomores in Bot. 25; only 7 failed and six were conditioned. I attribute the low number to the text-book which allows them to get down and dig and if need be almost commit it to memory. It is written in tabloid form and they seem to get along better than with the lecture method. 32 have elected Bot. 26 for next term - it is the biggest class I ever had and Julian has had to section it.

Potter has been a treasure to me and much of the success of Bot. 25 I attribute to him. He is stable and firm; he doesn't get rattled easily and best of all he has a clear idea of what I

25-XI-22

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.

am working for and is in accord.

Tomorrow I'm going to Boston to attend the meetings of the A.S.S and to see Dr. Jeffrey about next summer's work which I'm to handle alone. The papers are likely to be a bore but one meets many old acquaintances. I intend to steer clear of all papers on Plant Pathology and as far as I can of those who present them.

I'm wondering what sort of human society you find in Hawaii. After all, Lon, science can only carry us a short way. As I have told you it comes dangerously close to being an opiate at times. I wish you might find some friend there - one who was wise (not a second Whetzel) one who knew that Life was the "Great Adventure," one who was a bit unstable perhaps, and would follow intuition and faith rather than the solid realities of matter and energy. I want you to find someone to play with, someone with whom you can go off for long rambles at night; someone with whom, even in the most conventional hotel dining room you are in telepathic contact.

All of which sounds as if I was in love and was advocating a similar state of felicity for you. But I'm talking about a real friendship

between two men, and there I find more enjoyable than any affair with women.

If an opportunity comes your way don't fail them diffidence and coldness; the personality always steps in to mix things up. Remember that others like yourself are rather lonely and that you must go part way. What you need is faith in your own Inner Self and faith in the same Self of others.

There, I've sermonized enough and so I will not bore you any more. Be good, and don't get eaten by a hermit crab, or by anthropophagi; and don't sacrifice yourself to Kilauea or to a shredded wheat. In short, continue to walk in the straight and narrow path that you were taught by your Alma Mater.

Faithfully yours

P. E. Torrey

1/1/23

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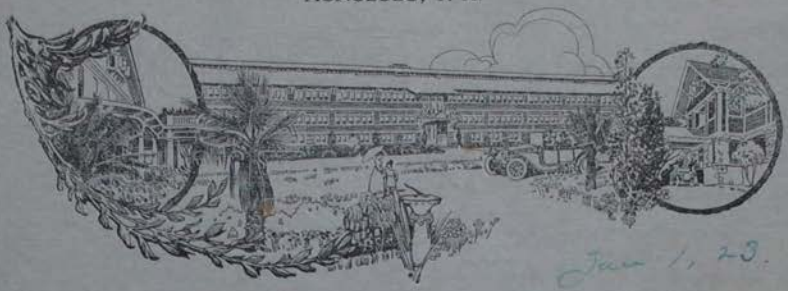
HONOLULU, T. H.



Jan 1, 23.

Dear Dr. Lorey:

I have just gotten back from a three weeks stay at the volcano. The Volcano House is but three miles from the active crater of Kilauea so that I am quite familiar with Kilauea having seen it half a dozen times both by day and by night. It was very interesting to see the two everlasting fountains of liquid lava 20-30 ft into the air while there was an overflowing red hot flow of lava (at about 2000 ft.) flowing towards me out of the crater and carrying pieces of black hardened lava crusts along with it. It reminded me of a stream carrying cakes of ice besides it made a similar sound.

But the important part of this trip is that I collected much too many plants. Although they are badly fermented since they did not dry quickly enough in that damp region, still they are good enough to identify and I guess that in the long run is the only value of herbarium specimens anyway.

As I have mentioned to you before, I found hundreds of sporlings of Lycopodium cernuum in all stages of growth. But I am

quite make out the gametophyte. It seems very "gooyey" and without definite structure as far as I have looked into the matter. ~~But that I heard them~~ in great numbers is certain. The region around the hotel is covered with a tangle of L. cernuum which grows to a height of 5 ft in places, Gleichenia dichotoma clambering up Metrosideros polymorpha for 15 ft, and many ferns (Sadleria and Cibotium sps.) as well as several shrubs with the habit of heaths. Not more than a few hundred feet from the hotel are a series of earthquake crevices out of which hot steam continually escapes. At the border of these cracks I noticed some sickly L. cernuum and on further search found some sporelings. Then walking along an abandoned logging railway track, I found many sporelings and gametophytes upon the roadbed. I shall try to find out when this track was built. Then on my walks I found a few sporelings almost daily, usually on perpendicular moss-covered embankments and in one case quite a number on the side of a woodroad. This must frequently have been escaped by the wheels of wagons and so I have a sneaky feeling that the development of L. cernuum, and of the lycopods in general, is not as long as commonly accepted. Getting an idea of the type of locality suited to them I investigated more steam-heated earthquake cracks and here found gametophytes just pepered over their whole sides (almost). Even on horizontal rocks which were near a tiny hole from which steam escaped, did I find a little clump of moss with many sporelings growing upon it. The sides of these steam cracks are of rock or rather of firmly cemented volcanic ash or clinkers, but no real soil.

1-I-23

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HONOLULU, T. H.

2.

Upon this material a layer of fine roots has grown so as to form a spongy mass perhaps half an inch or more in thickness. Upon this the gametophytes are growing, often imbedded in a greenish, slimey zooglœa (?). I am not quite sure whether this "zooglœa" may not be a multitude of prothallia that have fused more or less. That is my second sneaky feeling.

On the perpendicular sides of the steamcracks no sporelings higher than 5 mm or so can be found. While in places on these walls where slight bumps occur, the sporelings grow to a greater height and even branch profusely. When you get to the top of the cracks the sporelings gradually increase in size the farther they are from the cracks. In taking the temperature of the substratum upon which the gametophytes grow best, I find that their optimum temperature is about 88° F. In places above 95° they do not occur. It seems then that the gametophyte and the "pro-sporeling" are adapted to a temperature to which the mature or even older sporeling cannot accustom itself. This may be the

although I am not sure whether the following cannot be the correct (idea) The steam rising from the depths of the creek surrounds any sporelings that have grown to such a size that they stick above the substrate for more than a few minr. and boils them to death. The smaller sporelings survive since they are almost flat with the side of the creek. Now I am wondering whether there may not be a yearly "crop" of sporelings developing along these walls and that as soon as they reach definite size they are killed — or does the steam stunt them and keep them at a definite height. Of what seems more probable to me, both occurs? I would like to determine whether the lower deeper parts of the moss may not have many dead 5 mm. sporlings imbedded in it.

It almost seems to me that in the case of our Lycopods the gametophyte and the sporophyte (or the sporophyte alone) is running away more and more from the environmental conditions of the other generation. They are no longer able to thrive as well under conditions that are more or less favorable to both. Thus the gametophyte thrives in steam heated soil which is absolutely fatal to the sporophyte while the sporophyte thrives under relatively dry conditions and in this region at an average temperature of 60°F. And under these conditions, I find the gametophyte but rarely and evidently not as thrifty as the "last house" crop. With this running to environmental extremes, the Lycopodiales are tending to die out or eclypse the alternating generation. Thus in the sporophyte generation asexual reproduction is almost the rule according to the commonly accepted opinion, while in the gametophyte generation possibly asexual reproduction (budding) is

1-I-23

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PLEASANTON HOTEL
HONOLULU, T. H.

3

occurring to form my "zooglea" if that happens to be a mass of g., or if that be not true, it is true that the g. generation is but able to develop a sporeling that cannot, but in rare cases, approach asexual maturity.

(I forgot to mention that while collecting the material every hair on my hand was covered with minute droplets of water while my head was soaking wet. Evidently the air is saturated with moisture. Sometimes I felt as though I were in a Turkish bath while on some occasions I was obliged to give up gathering the sporelings because of the great heat ~~[illegible]~~ when the wind blew the steam in my direction.) You know Kunkelmann mentions the fact that mature sporophytes are never found with the g. Miss Stokey noticed the same fact, and in my case it seemed the same.

If we let our imagination wander we can theoretically derive two organisms, one X and the other 2 N, each one of which no longer has anything to do with the other. Might not such a step have developed among some of the fungi with their chromosome mix-ups?

Since ontogeny recap. phylogeny we might say that the prothallus just emerging from the spore has characteristics that were common to its ancestors. If this be true then why is it not perfectly plain that "ancestral environment" and conditions will be best suited to the young gametophyte and sporeling

and consequently if spores are exposed to them they will germinate more readily and the same gametophyte and young sporeling will have optimum living conditions. Then under the older sporeling is antogenetically similar to a later ancestral stage. It succumbs. Consequently I have another sneaky feeling that the carboniferous or an age when the Lycopsida were in the making had an atmosphere practically saturated with moisture, and that the temperature was probably quite uniform and usually around 88° F.

The following fact also points to this theory: Near the volcano in another steam crack of a different character as far as moisture is concerned I found hardly anything but Psilotum and Ophioglossum. They just covered every crack in the walls. In no other place have I seen Ophioglossum in these islands and it is not even reported from the Island of Hawaii (different from Hawaiian Islands which includes Hawaii, Molokai, Oahu, etc.) by Hillebrand. Everything around that region is covered with rather recent lava upon which no plant grows as yet. There is no doubt but that the gametophyte developed in that spot quite readily. The soil in which they grew was heated volcanically. Not a single Lycopodium grew there, and I believe the reason for this is that the place is much too dry for those gametophytes that grow on the surface. (There was but little water vapor.) Of course gametophytes of Ophioglossum and of Psilotum are subterranean and thus not exposed to the sunny and dry surface.

What do you think of my badly stated theory and of my "sneaky feelings"?

There is another thing I would like to ask you about. Green algae are supposed to grow in shallow water, then come the brown, and

1-I-23

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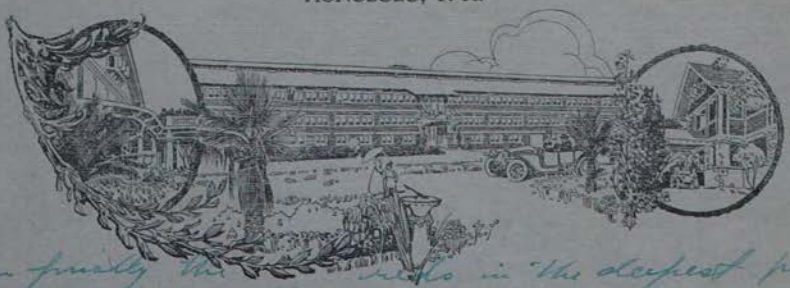

then finally the reds in the deepest part of the sea. Here in Hawaii green algae seem very rare (except Ulva) while the browns predominate and grow in water suitable for wading. There is no "color-line". And here in the tropics the light is more intense and the water clearer. Can the property of sunlight here differ from that further north so is this "color-line light" theory ~~based on the wrong~~ incorrect?

I have heard from Mr. Whetzel quite often and he has decided that it is best to postpone the St. Lucia trip for about a year because he cannot finish a text book which he is writing in times. This is of course disappointing for me in one way although it suits me nicely in another as long as I am out this far anyway. I thought it would be interesting for me to stay in California for the next year until Whetzel is ready. What do you know of Leland Stanford for Systematic and do you think it advisable for me to go there? In fact do you think it is any use for me to try to study further. Monographs who such as those just out by Standley interest me. Would it be well to get training in such work, and if so where should I go (the East is out of the question, I wish to become familiar with another part of the country).

I have finished my Mass Lycopod report but Miss
Stokey holds me back by not finishing hers. Germany
report if worth while will be separate. I shall send you 3
some of my material.

1/5/23

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

Jan. 5, 1923.

My dear Mr. Degener:

The little period of leisure which I wanted so that I could do some work on the paper has not arrived, but I really think I ought to tell you about the Boston meetings.

I took up your jar of material which was much better for young stages than anything I have so that it was a valuable exhibit I also took a jar of large specimens - my huskiest ones, and a dish of fresh specimens - six or seven with and without sporelings and of a wide range of size and type. There was a great deal of interest in the specimens and a similar interest in how to find them. There was a good deal more interest in the joint paper on the stations than in the one on L. obscurum, and it was given in the better meeting. There is a demand for material, even from people who were not at the meetings. I gave some to Dr. Nichols of Yale, and I am going to send some to Dr. Jeffrey, both of whom have been very good in giving us material. You will find it is a fine bait for exchanges.

Dr. Eames, of Cornell, said that after seeing our material he was sure he had found some several years ago on the edge of a woods, I think, where there were young plants of Trailing Arbutus. He brought some in and gave it to some one in the Botany department who was so sure that it could not be prothallia and sporelings that the material was allowed to dry up. I wish I could see his place as it sounded rather unlike ours, but it may be more alike than it sounds.

I am enclosing copies of the papers as read so that you can see just what I said. I abridged everything as much as possible, as people are more likely to be interested in conclusions than in data; the data is interesting for research workers and ought to come in a published paper.

You see I changed the statement about the number of your stations - I had not realized that the two Amherst ones were distinct. If it only would thaw I think I could find another one and make a dozen. We are having real Christmas weather - not your type.

I shall re-write the account of "The Prothallia in Western Mass." shortly and send it to you, following some of your suggestions. There will have to be a few changes, perhaps, in view of Spessard's note on L. obscurum, but not many, as that really has very little to do with the question of finding stations. We can't write up our account of L. obscurum very well until his paper appears but that ought to be very soon if it is coming out in the Dec. number of the Gazette. I am sure that a second account will be of value, as supplementing or confirming his work.

I m sorry that you could not be at the Boston meeting. It was a fine one. I saw Dr. Torrey and Dr. Osmun there.

Yours sincerely,

Alma Slotkey

1/6/23

Jan 6.

My dear Miss Stokey:

I had just sent off my ~~last~~ (?) article in the morning to you when your letter arrived telling me about Mr. Spessard's account in the Botanical Gazette and of his paper being in press. ~~At that I had~~ It is certainly discouraging to have to ~~change and type all this over again especially if you are a one-finger typist.~~ This must be at least the 5th time. I am glad Spessard has corrected himself and did not leave this mistake stand ~~very long~~ for years.

No doubt you ~~must~~ have noticed that my report after this news was unfit ~~to~~ for publication and have not forwarded it to Coulter. ~~So that both our articles may come in the same number, I shall send my slightly revised manuscript to him direct~~ ~~with~~ along with yours. At Christmas I shall send my slightly revised manuscript direct to him thus saving time and increasing our chance for having them published as soon as possible. I ~~am~~ only hope that it can appear in print before I go up for my M. S. so that it may count as part of my work for it.

I am writing Coulter that we had had considerable communication together and that ~~since our articles do not deal with the same phase of the~~ since in some respects our articles supplement each other ~~i.e.~~ as you mention my stations and because in my description of them they automatically fall into your habitat groups, they should come in the same number. ~~I also said that~~ I will also say that we do not deal with the same phase of the subject.

If Spessard has found many gametophytes and sporelings of L. obscurum, I am afraid my measurements are not very significant but if he has

only a dozen or so, my figures based on greater numbers will be important.

~~Possibility it would be~~ Since it takes ~~about~~ not quite a month for ~~a letter~~ ~~the answer of a letter~~ for a question ~~answer~~ from the publishers to me to be answered, I will take the liberty to ask them to send any urgent questions about my manuscript to you. ~~Then~~ They may be forced to get permission for ~~doing something~~ before doing something that is self-evident because of red-tape. I would be ever so much obliged to you if you would then tell them "yes" or "no", ~~But if there should be~~ If there should be any bother involved in it, please merely forward the letter to me. Anyway I do not know whether there ~~may~~ will be any urgent questions after all.

I think I told you I had found the gametophyte of L. cernuum. I am out for L. pachystachyon now, as I think it is more or less epiphytic, an endemic L.

I think that you must limit your habitat types only to ~~super~~ subterranean species. I guess you imply that anyway. L. cernuum, of course would not fit.

I wonder when Spessard's article will get to Hawaii. I wonder what he will say.

1/16/23

Pleasant Hotel,
Honolulu, Hawaii.
Jan. 16,1923.

My dear Miss Stokey:

I just received your letter of Jan. 5 and you probably have received mine yesterday since we sent ours off at practically the same time. I had thought that that 10-page article that you sent me the latter part of November was your first draft for the Gazette and that you were going to send it to Coulter with my article shortly after Christmas. Now I sent my article,with a few revisions necessary in view of the knowledge of Spessard's correction,direct to Coulter and yours is not there. That they will not be printed separately is sure because of the letter I enclosed with the manuscript. In it I said: "I have had considerable communication in reference to this matter (MS on Four New Stations of L.g.)with my friend, Prof. Alma. G. Stokey of Mt,Holyoke College,who is writing upon Lycopodium gametophytes for the Gazette as well. Since these articles supplement one another and do not deal with the same phase of the subject,we are hoping that it may be possible for you to have them printed in the same number." But since we had wanted to submit our manuscripts to Coulter at the same time, I am enclosing a letter to him which you could mail to recall my manuscript if you think it desirable. Then I can change the wording a little and it will be an entirely new article which can then be submitted to Coulter through you along with your finished manuscript as it should have been done.

From the article read at the Botanical Society,I see that you have found giant gametophytes of 10 X 18 mm. and that your common size is 9 X 10 mm. I measured mine shortly after collecting them and have in my notes 12 mm. as the largest diameter of my largest specimen. This specimen I cannot find among my material but one of 10 mm. so I give that as my largest size. I may possibly have given away my 12mm. gametophyte. My commonest size for the gametophyte ranges from 4 to 5 mm. It is interesting that your specimens have a different range for the average. I wonder what the reason can be. I just wish to call your attention to this because you may be able to explain it. Of course an ecological factor may be the cause of it or just chance. Did you use a sieve and thus get almost all the gametophytes in that locality or did you pick them out by hand. If it was hand-work,it may be possible that you have so many large ones because of the greater chance of finding the bulkier material.

In your other article you say that old plants of Lycopodium seem to be unfavorable for the development of prothallia. This fact has been noticed by almost everyone so there must be a very strong reason for sporelings and old plants being found in different localities. I cannot imagin that Bruchman is correct in suggesting that the old Sporophytes cause the disappearance of the fungus necessary for the gametophyte. There must be another factor. From my observations on L. cernuum at the volcano at Christmas,I almost think that moisture is the chief reason with possibly warmth in the case of L. cernuum being a very important contributing reason. I shall tell you fully my observations with probabl a lot of repetition of what I have already told you. The trouble is that I have entirely forgotten how much I have written you about it.

I will give you my notes on this: The region is covered with a tangle of L. cernuum which grow to a height of 5 ft. in places,Gleichenia dichotoma clambering up Metrosideros polymorpha for 15 ft., and many ferns(Sadleria and Cibotium). Not more than a few hundred feet from the

FOUR NEW STATIONS OF LYCOPODIUM GAMETOPHYTE

hotel are a series of earthquake cracks out of which hit steam continually escapes. At the border of these crevices I noticed some sickly L. cernuum and on further search found some sporelings. Then walking along an abeandoned logging railway track, I found many sporelings and gametophytes upon the roadbed. Then on my walks I found a few sporelings almost daily, usually on perpendicular moss-covered embankments and in one case quite a number on the side of a woodroad. This must frequently have been scraped by the wheels of wagons so that L. cernuum no doubt does not have such a long spore-germination and gametophyte stage as the others. Getting an idea of the type of locality suited to them I investigated more steam-heated earthquake cracks and here found gametophytes just papered over their sides. Even on horizontal rocks which were near a tiny hole from which steam escaped, did I find a little clump of moss with many sporelings growing upon it. The sides of these cracks are of rock or rather of firmly cemented volcanic ash or clinkers but no real soil. Upon this material a layer of fine moss and sometimes a zoogloea has grown so as to form a spongey mass perhaps half an inch or more in thickness. Upon this the sporelings grow in great numbers. In fact they are ~~XXXXXXXXXX~~ entirely the only plant higher than a moss (phylogenetically I mean) growing in such spots.

On the perpendicular sides of the steam-cracks no sporelings highe than 5 mm. or so can be found. While in places on these walls where slight humps occur, the sporelings grow to a greater height and even branch profusely. When you get to the top of the crack the sporelings gradually increase in size the farther they are from the crack. In taking the temperature of the substratum upon which the gametophytes grow best, I find that their optimum temperature is about 86° F. In places above 95 they do not occur. It seems then that the gametophyte and the very young sporeling are adapted to a temperature to which the mature or even older sporeling cannot accustom itself. But possibly the steam rising from the depth of the crack surrounds any sporelings that have grown to such a size that they stick above the substrate for more than a few mm. and boils them to death. The smaller sporelings survive as long as they are almost flat with the side of the crevice.

It almost seems to me that in the case of our lycopods, the gametophyte and the sporophyte (or the sporophyte alone) is running away more and more from the environmental conditions of the other generation. They are no longer able to thrive as well under conditions that are more or less favorable to both. Thus the gametophyte thrives in steam-heated and SATURATED (possibly this is the only reason as far as all L. are concerned) soil which is absolutely fatal to the sporophyte, while the sporophyte thrives under relatively dry conditions (in case of L. cernuum I think) and in this region at an average temperature of 60° F. And under these last conditions I find the gametophyte but rarely and evidently not as thrifty as the "hot-house" crop. With this running to environmental extremes, the Lycopodiales are tending to die out or to eclypse the g. generation.

What do you think of my theory? It has not quite matured but possibly some such reason is responsible for finding sporophyte and gametophyte always in different places. I may develop this theory further and include it in a future article on the finding of L. cernuum. Do you think it would be worth publishing? The readers of the Botanical Gaz. will soon be sick of seeing the word Lycopod with all these articles upon it. Would this theory be disproved by your observations or strengthened?

I bet that after your reports at Boston, many people will be on the lookout for gametophyes and that many finds will be announced next year. It has shown people that they can be found. I think it was very kind of you to include my finds in with yours.

A Dr. Harold Lyon, formerly Prof. of Bot. in Wisconsin and discoverer of Ophioglossum gametophytes has shown me photograph of saddle-shaped L. gametophyte that he found years ago out West. He does not know the species.

I have added on two photographs of L. obscurum

sporulings to my article. They are not especially good but I think in spite of that they will give people a better idea than my brief description. If Dr. Davis would only send me those photographs me took of obscurum in the fall I would show that too. That I do think with showing since the location was so rich in plant.

Only two pictures even though poor, I believe it add interest to an article.

Yours sincerely,
Otto Degener

Mr. John Merle Coulter
Editor Botanical Gazette
University of Chicago

Dear Sir:

In fact view of the fact that Prof. Alma G. Stokey of Mt. Holyoke College has made some changes in her article on Sphenodium gametophyte which affects the manuscript I had sent you Jan. 6, I request that it be returned to me in care of Prof. Alma G. Stokey, Mt. Holyoke College, Smith Hadley, Mass.

I trust the enclosed stamps will cover the return postage.

Very truly yours,
Otto Degener

1/18/23

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

January 18, 1923.

My dear Mr. Degener:

You have doubtless read Mr. Spessard's article by this time, and probably agree with me that L. obscurum is not yet a closed subject.

His material is not sufficiently abundant nor vigorous for as full an account as we can give. You have much smaller specimens that he has evidently, and I have much larger not to mention the fact that we have many more. It seems to be desirable to give an account of the younger stages, and photographs, or drawings showing the range in size and habit. It seems not improbable that by a careful examination of the soil in the "galore" region that one might get some very early stages. None of my patches seem especially promising for that; they are better for old ones. I shall collect some of the soil in the spring and go over it with a lens and perhaps a microscope, if that seems promising, as it would be a good thing to get some of the early stages. I am going to the Mt. Toby station again as that had very lusty sporelings and large prothallia. There certainly is more to be said on the habit of the prothallium than Spessard has said.

Probably there is more to be said on structure, too, as he has done very little with that. (By the way did you find any constancy in color in the different species? I did not. I did not have enough material of L. clavatum to have any idea about it, but there is considerable range in L. obscurum and L. complanatum.) I have sectioned only a few specimens and have not had time to make much of a study of them. Some of them are fine for antheridia but I have not had any that are good for archegonia, although several show archegonia.

I can give him some pointers, too, on L. complanatum. I am making a series of sketches of L. complanatum (one specimen) to trace the development of the antheridial cushions - the rate of development. I wish I had begun it when I first brought the specimen in, but I did not realize that things would happen so rapidly.

Of course your writing up of the paper on the habitats will have to be modified in view of the proper identification of L. obscurum by Spessard. As L. complanatum is the only species on record with the carrot-shaped prothallium, the points about its identity are not essential. Spessard's habitats for the most part seem to agree with our analysis of conditions, except that they are more in the open. Probably there is some difference in climatic conditions between northern Michigan and Massachusetts. He has evidently not seen as great a range as we have, or as many stations; he seems to have stuck pretty closely to a few spots.

Shall I mail you specimens to you? They came through here all right, but they had lost practically all their alcohol; however, they

had not dried out. I suppose it depends on just how rapidly they travel. I can mail them to you now or on your return.

You were good enough to suggest that you might be able to get me some fern material. I have been working on the prothallia of the tree ferns for ten years and have studied all the genera except Balantium. I have Dicksonia antarcticum (Balantium antarcticum). I just got some Thyrsopteris this last summer from Kew and have cultures of it going now. I have Hemitelia horrida and would be glad of other species, if you can get any. I might enclose a list of what I have and if you can get me some good fruiting material of any other species of the Cyatheaceae, I shall be very grateful. If they can be identified there, so much the better; it takes more material and a description of the habit to identify them here. I have found an unpleasant discrepancy in the determinations which are made here.

My method in handling them is especially designed for green house material, where there are many species growing and one is in danger of getting impure cultures. That may also apply to material collected in the open. I collect material that is just about to shed its spores, and then wash the leaves in running water with a brush stiff enough to remove spores. (Very likely the water has more effect than the brush.) After washing the material I put it on waxed paper loosely wrapped or in some way protected from dust and contamination with other spores. The whole point is to give the sporangia a chance to dry sufficiently to discharge their spores before the material becomes water-soaked and liable to decay, and yet not have them exposed to contamination. they have to be carefully wrapped for shipping so that the spores will not sift out from one package and into another. They should be shipped in tin small tin boxes; one for each species is desirable. All this is not as elaborate as it sounds and I hope that it will not discourage you.

I shall be very glad to get any Pteridophyte material that you can send or bring.

How is your other Lycopodium find coming on?

We are having a very snowy winter - snow almost every day; the ground has not been bare since the first week in December, but for the most part it has not been cold.

Yours sincerely,

Chas. G. Slotey

1/27/23

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

January 27, 1923.

Dear Mr. Degener:

I am just going out to post THE PAPER. No doubt you think I might have had it finished long ago, and so I might, but there were too many untoward events. The Rockefeller fire (did you know that one of our dormitories burned almost to the ground the morning that college closed for the Christmas holidays) took some of my time as I was on the salvaging committee and was put on a Faculty Fire committee. I had less time than I had counted on during the Christmas holidays and almost no time since then to re-write the paper. Didn't Spessard's mistake make us a lot of unnecessary writing? It is in the same general form as when I sent you a copy but I had a few bright (?) ideas which I inserted later. Mid-years began Thursday and I have spent the time going over the paper.

I am curious to hear what your opinion is of Mr. Spessard's paper be. We have so much more material that we ought to be able to add a good deal to it. By the way have you plenty of old material for you sporeling paper? I have considerable and will be glad to give it to you. I have several good specimens of stout old plants with the foot - I mean relatively old with two erect branches. I can't remember whether I ever heard if the "galore" path was as good for old stages as for young ones. I am stronger on old than young.

I wrote to Dr. Coulter saying that our papers ought to appear together and that if yours had not yet come it soon

would. More than likely it is waiting for mine.

It continues to snow and there will be no gametophyting for some time.

Congratulations on the gametophytes of L. cernuum. I hope you find those of the endemic species.

Yours sincerely,

Alma G. Stokey

P.S. I should be very glad if you would collect some habit material of L. cernuum for me. My brother sent me a specimen from Cuba which I thought was L. cernuum, but it was burned in the Williston fire. I shall be very glad to get any tropical fern material which you want to bother with. I noticed in "Die Geographie der Farne" that there are no Alsophilas or Cyatheae in Hawaii, but there are other members of the Cyatheaceae, so I shall hope for some.

A. G. S.

F6–3 hrs

Cambrian scenes $\frac{1}{25}$ sec F 11

Volcanic Scenes $\frac{1}{25}$ F 12

Waterfalls $\frac{1}{25}$ F 8

Water scenes $\frac{1}{50}$ F. 11

Canyon Scenes $\frac{1}{2}$ F 11

Tonic Expression

Cambrian scenes $\frac{1}{2}$ sec F 16

Volcanic Scenes $\frac{1}{2}$ sec F 16

Waterfalls $\frac{1}{2}$ sec F 11

Water Scenes $\frac{1}{2}$ F. 16

Canyon Scenes $\frac{1}{2}$ F 11

A.G.Stokey. 27-I-23

Cyatheaceae. Prothallia cultures.

Alsophila Cooperi.
A. nitens
A. aspera
A. excelsa

Cibotium glaucum
C. Scheidii
C. Barometz
C. regale (?)

Cyathea muricata
C. Tussacii
C. arborea
~~Cxxxxxxxxx(?)~~

Dicksonia antarctica
D.squarrosa
D. fibrosa (?)

Hemitelia horrida

Lophosoria quadripinnata

Thyrsopteris elegans

1/28/23

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.

Jan 25 1923

Dear Degener:

You certainly handed me a whole magazine of science in that last letter. My suggestion in regard to your oleanic Lycopod gams is that you should write up the matter in not too extensive form presenting the facts with tables of temperatures etc and then drawing your conclusions concerning a former Paleozoic climate. The hypothesis is ingenious and would support the Bowerian idea of a superior antiquity for the pantophyte which evolved on an earth rent with steam fissures. The "Zoögloea" which you speak about sounds like blue green algae to me and these notoriously love hot water and are ancient forms. You may have a case of symbiosis. Nostoc lives with Anthoceros - why not with Lycopods. Another article on the flora of these steam fissures to include the cryptogamic flora would also be valuable.

Don't doubt your own abilities re. Any man who has the interest in, and enthusiasm for Botany which you have is bound to produce results of value. I'll be glad to go over any articles you may work up and see about getting them in press for you.

As to your green alga problem and your lack of "color line" I'm as ignorant as a fish. Perhaps your algologist who is wandering in a proa among

the St. Lea islands will eventually inform you.

You ask about Leland Stanford and Systematic Botany. Now of course I dont know much about N. M. Smith of Smith College was there last year and didnt like the Botany Dept. She thought the members far from cordial or helpful.

Here is a suggestion however. At Occidental College somewhere in the same State, I have a personal friend Dr. Frank J. Smiley who is a straight systematist, a graduate of Harvard under Fernald and whose thesis work dealt with the distribution of Flowering Plants along the Sierra Nevada. Frank is a fine fellow. Why not write to him at the Bot Dept of Occidental College.

Your description of St. Lucia reminds me of a sort of Lower Hades. I have no use for the fer-de-lance or for "mosquitos and damp miasmatic mists."

Did I write you that you became famous at the recent Boston meeting of the Am. Assoc. for Adv. of Sc. without knowing about it. Miss Stokey had your name on the program with Miss Stark and her own for her short report on Lycopod gams.

By the way I'll be glad to write Smiley about you when you have time to consider the matter. Let me know.

Best wishes

P. E. Torrey.

1/30/23

Pleasanton Hotel,
Honolulu, Hawaii.
Jan. 30, 1923.

Dear Miss Stokey:

I just received your letter of the 18-th. It is miserable that our letters have crossed again. The December Number of the Bot. Gaz. has just arrived a few days ago. I was a little disappointed in Spessard but am glad there is something left for us to do. I think he has some fine drawings and has done a lot of work. His statement that the gametophyte of L. obscurum is of an intermediate shape between that of the saddle type and that of the asrot type makes me mad, especially since he mentions the fact that possibly his figure of L. complanatum, designated obscurum in his first paper, may be L. obscurum after all. There is no possibilty of its being that nor that the gametophyte of L. obscurum is intermediate between those two shapes. It may be true that it is a little deeper than that of L. clavatum but he stretches his point much too far. My specimens do not substantiate his view.

What do you think about sending in our two papers to the Gazette practically the way they stand? The locality of find would have no bearing on the morphological work. It would mean that I would have two papers in the Gazette instead of having but one. My measurements would detract little from the value of a later paper because you have found far larger specimens and if we pool our specimens as far as measurements are concerned, the average would be different. I am working upon the anatomy of the sporeling of obscurum at present. That might go well with a complete acount of the gametophyte. I am making drawings of transverse sections at different heights. There is a striking differenc in the number of poles of the stele at different heights.

As you will notice from the letter that probably reached you around the 15-th of January, L. cernuum grows in the greatest abundance under hot saturated conditions. Can you possibly dig up some strobili of obscurum and expose the spores under similarfconditions? For instanc with a slight amount of steam from a radiator escaping into an aquarium with strips of firmly packed earth from gametophyte stations and pieces of filter paper placed in almost horizontal positions. If you dust thes with spores, you might possibly be able to find out the length of time for germination and speed of cell division. I think that exposing the spores to a considerable amount of moist heat may cause them to germinate if they are at all like those of L. cernuum.

As to the color of the gametophytes: I call that of L. complanatum "dirty gray in color" in my report. For L. obscurum "the color is of a more yellowish tinge." As far as I remember the gametophyte of L. clavatum was the lightest of all inwolor. But because I had but one specimen from which to judge, I said nothing about it. I don't see that L. obscurum is reddish the way Spessard describes it. I do not think the color can be an important consideration. In fact my L. obscurum material varies a little in color, but dirty gray with a more yellow tinge is broad enough to cover it.

I have not yet sectioned any gametophytes - only the sporeling. There is a difference between two of the species in cortical development by which they can be told apart at an early stage. I noticed that in August but have at present forgotten to which species that applies.

Please send me my alcohol specimens. I may have to section one of those L. complanatum specimens, while that L. clavatum gametophyte is the only one I have.

Have you any specimens that show growth in an inverted position?

30-I-23

That photograph that I added to my report I shall replace by a better one. I think a figure in the text of a few sporelings in the field will be worth adding since it will show the type of locality as far as number of ~~xxx~~ hemlock needles, etc., is concerned. Then I thought of adding two plates. One would show a mess of sporelings with gametophyte or foot attached, while the other would show nothing but gametophytes. I doubt the value of showing any figures of any specimens except of obscurum. I shall send you my other two photographs as soon as they have been developed. Then we can decide what should be included in my report. I do not wish to show so many figures on L. obscurum that you cannot show any without merely duplication. We shall have to see what is best to be done.

As far as ferns are concerned, the following species are represented in Hawaii: MARATTIA, SCIZAEA, HYMENOPHYLLUM, TRICHOMANES, ACROSTICHUM, GLEICHENIA, POLYPODIUM, PHEGOPTERIS, GYMNOGRAMME, VITTARIA, ASPIDIUM, NEPHROLEPIS, CYSTOPTERIS, ASPLENIUM, SADLERIA, DOODYA, LINDSAYA, MICROLEPIA, ODONTOLOMA, CIBOTIUM, PTERIS, SCIZOSTEGE, PELLAEA, ADIANTUM.

According to Hillebrand of 134 species of ferns and Ophioglossaceae in Hawaii, 75 are indigenous. Now being in Honolulu, I will not be able to get at the different ferns as easily as when I staid at the volcano. I know I can SADLERIA, a treefern, peculiar to Hawaii.

Do you think my report needs any further ~~m~~odification in view of Spessard's correction in his 1917 article?

Sincerely yours,

2/3/23

Pleasanton Hotel,
Honolulu, T.H.
Feb. 3, 1923.

Dear Dr. Torrey:

I received your letter of Dec. 25, and incidentally mine has crossed yours. It is very thoughtful of you to have an announcement printed in the "Collegian" about the reception of those duplicate herbarium specimens that I collected. You must not forget that I collect for the fun of it and that it is no trouble at all for me to send miserable "seconds". I must have something to do. As fas as becoming "famous" or rather infamous is concerned, I shall escape both extremes before I die. To be sure, everyone has the tendency toward that desire but it is a filthy, brutish instinct which should be repressed as you realize more than others. So when I send you more plants please forget from where they came and keep it dark. I do not know what else to do in my spare time. It gives me a certain pleasure to think that plants in a college herbarium are well cared for and that possibly in years a few out of hundreds may prove interesting scientifically.

Your "sermon" I liked very much. That is what I need, and it gives me something else to think about. To this I can answer that I know one man a little better than an acquaintance. He is an old judge who reminds me of Sir Roger de Coverley. Every Sunday I take a walk with him. He dresses like a dandy, wears a wide black ribbon to his glasses, bows to all the ladies in the dining room, and treats the Japanese the way a typical old Southerner treats negroes. At breakfast he looks around the dining room and takes a flower from the table for his button hole. If the flowers on his table are not to his taste, he merely takes it from an other. Since we are both conceited and BE eccentric, I enjoy his company. If it were not for him, I would almost forget how to talk since all I say are my dining room orders and ask for trolly transfers. To what extent I exaggerate is up to you to guess. So much for that.

Judge James Parker Kinsley

Now as to my saying that all lycop. gametophytes are normally adapted to a warm environment is stretching a point too far when we consider the thousands of L. obscurum gametophytes that drive is arctic Amherst. This applies possibly to L. cernuum though moisture I believe to be more important.

Please tell Dr. Davis that the photographs arrived. They are fine. A photographer here even said so. I had some pictures taken of my sporelings and gametophytes here but they are not much of a success. I shall pose them again. In spite of the fact thatva photographer did it badly, it is cheaper for me to have him do it thannfor me to do so. The only picture that I have taken that was not light-struck, after a ma had fixed my camera, is the volcano at night. I shall have the picture that Dr. Davis took of the 3 sporelings reproduced in the text and then have 2 plates; one of sporelings, the other of gametophytes. I hope my report will be accepted.

I made a fool of myself. Miss Stokey told me she was going to send her report to the Bot. Gaz. shortly after Christmas, and she had my manuscript to forward also. Then I discovered that I must change my paper due to Spessard's article and so I sent my revised MS direct to Coulter to save time instead of sending it to Miss Stokey. Now she has not finished hers and I must recall mine until she is ready.

The following I must decide soon: Since none of the family are any good as Botanical alma maters, I am asking you. I hear Leland Stanford is weak in Systematics, and since I wish to keep West of Chicago fo

next yearuntil Whetzel is ready. I don't know where to go. Nelson is in Wyoming. Should I go there? Where on Earth West of Chicago on the North American Continent do you advise me to go. If it is best for me to try to take a part-time assistantship, I must decide soon. Please tell me where to go for Systematics (Hell excluded). I do prefer escaping another winter if possible.

To day I caught a young octopus in my hands. Its body is about the size of an egg. I may pickle it.

I fear Mrs. D's zoo is deteriorating. Dr Gage and you must be the last remnant. How many young faculty additions does Dr. Gage see in Devachan? I am wondering how Moody is and especially of what has become of Irish.

After Commencement here I shall go for half the summer to Kauae. That is the farthest island of the group and the oldest. There is a wonderful swamp in the interior. I shall try to get there. I have asked about it and it seems to be unapproachable almost. I just heard from the photographer that it is a Baptist's Hell with fine deep mud. That will be ideal. The arboreal violet grows there.

Yours sincerely,

2/14/23

Pleasanton Hotel,
Honolulu, Hawaii
Feb. 14, '23.

Dear Miss Stokey:

I received your letter of Jan. 27 this afternoon and sent off my finished (I do hope this is really the last time) manuscript on my report this morning. I have sent it to you with the intention that you would forward it to Coulter with your own report. But from your letter I see you have just mailed yours to Coulter.

Dr. Davis was so kind as to send me the photographs that he took with me at the Salvia station. This time I was obliged to cable him — the censor thought "hybrid gametophyte pictures" was a message in code and first did not like to send it. But that was the only way I could get them in time. Then since the pictures that I had taken here of sporelings and gams. were not clear, an inspiration came to me. I went to a professional photographer and had them photographed and printed on paper that will readily be reproduced in the Gazette. It was lucky I had done this.

Coulter's letter dated Jan. 27 arrived here a few days ago. He accepted my article for the Gazette, tells me your MS has not yet been received, and advises me to have my photographs on glossy paper. This I had luckily anticipated. Now I was forced to rewrite my article as you can see from the copy which I sent you to forward to him.

I just wrote Coulter again that he might destroy the copy of my manuscript of Jan. 6 if you had not recalled it to me before this time. I also wrote him that I had sent you my revised report which you would send him with your own article. I only hope there will be no misunderstanding.

Sunday I collected a specimen of Doodya media but fear the spores are shed. I also prepared three plants of Asplenium nidus. One specimen had two fertile fronds; the fertile part I have cut off and put in a bottle. As soon as it has dried unstoppered, I shall send it to you. I fear if I corked it now and sent it the material might start to decay before it arrived

I knew that you would not get many spores but since I have never seen this fern before and since it appears to be rare in my neighborhood, I thought I had better send what I might happen to get. I hope it may be satisfactory if I wrap the fronds in newspaper as I find them so as to avoid contamination with other spores. I caught Ophioglossum pendulum, an epiphyte, in any quantities, but not with dry sporangia. How would it be if I were to send you that type of material between blotters sealed in a vial? I think the spores are practically ripe. Lycopodium Chamissoi is easily gotten in quantity. This I will be able to send without difficulty. L. cernuum is also easily procured.

As far as "mature sporelings" are concerned I have quite a lot. So I will not need any of yours. But I have two or three gametophytes of 3 mm or so which you might not have. Would they be of any use to you?

Sincerely yours,

It is a great relief to know the report is off and accepted. It might be a good idea to plant mature L. obscurum plants in the gallow place and see whether they die out quickly or not.

2/21/23

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

February 21, 1923.

My dear Mr. Degener:

I seem to be somewhat behind hand on my correspondence. The trouble is that all my spare time lately has had to go into planning details of the new science building. I think of nothing but sinks, gas, electricity and tables. I am running up some of my prothallia and hope to embed them this week, but I have no idea when I shall cut them.

It looks as if our papers would not appear for some time. I shall enclose Dr. Coulter's note. I notice that the Journal of Botany is about as far behind. I had hoped for earlier publication as last year they (the Bot. Gaz) were within four months of their manuscripts - the closest they had been for many years. I have sent your letter on to Dr. Coulter but have not received the manuscript yet. When I send it on to you I shall enclose a copy of mine as it went to press. I did not send in any photographs but I am glad you are going to do so. That one of the habitat is fine!' I tried some but did not get anything as good. I shall enclose a print of the only habit photograph which I have of L. obscurum showing groups of plants.

Your large photographs came yesterday. The one with nothing but prothallia seems to be a very good one for a paper on L. obscurum But not so good for the paper on the habitats. It would be more satisfactory if you had a centimeter rule in the photograph to give an idea of the size as they must be magnified somewhat. I judge that the others are life size as mine are.

I think I have some specimens that show growth in an inverted position, as well as upward and sidewise.

Your find of L. cernuum is very interesting. The relation to heat is an interesting point. I think it is quite possible that the gametophyte and sporophyte are adapted to different temperatures. You know the optimum for the stem and root are not the same - at low temperatures roots grow faster than stems and at high, stems faster than roots. I don't know whether that applies to Pteridophytes as well as to crop plants and bulb plants. Is there any indication of the higher temperatures being more favorable for one organ than another or one stage of development?

You mention several ferns as growing in Hawaii which I would like very much to try to raise for a study of the prothallia, and if it is possible to send any spore material I shall be delighted to get it, Vittaria, Lindsaya, Sadleria, Cibotium and Schizaea.

I shall send your specimens on at once - there is a slight let up in the rush about the building and I shall make the most of it.

When your paper comes from Dr. Coulter I shall look over it to see how it fits in with mine, and also to see how it will fit in with a paper on the prothallia of L. obscurum, which I think we ought to do jointly. Then you will also have a paper on the sporeling. I thing I would better send you some of my larger sporelings as you apparently did not get anything as big as my largest.

I suspect that most of my stations were older than yours or perhaps in regions where the chance of fertilization was less. Of coure if there were fewerx prothallia the chance would be less as there would be a mmaller chance that sperms would be on hand at the few times when there would be an egg ready for fertilization. In noe of my stations were the prothallia as abundant as in your galore patch. I want to visit Toby as soon as the snow thaws (7 ft. of snow this winter and never a thaw!), as that was the best place for large ones. I did not find many small ones. I tried sifting the material and also mulling it through between my fingers; I did betterx that way than by sifting through a sieve either when wet or dry.

Have you read Treub's article of L.cernuum? I do not have the article but I have some notes on it. Apparently it is the easiest and the quickest one to germinate of the Lycopodium species.

I shall try your suggestion of using moist air or steam to start spores. I have some material - soil with spores and small pieces of xxxxxxxxxx strobili mixed. By the way what are the spores of L. cernuum like? Do they have a netted coat? Is there any superficial character to indicate why they would germinate more quickly than our subterranean species?

This letter is a hod-podge. I have been interrupted so many times that it has no coherence. I hope the can extract some of my ideas (?) from the mess.

I was over at M.A.C. a short time ago. Dr. Torrey told me that you are going to Santa Lucia (?) next year. I am glad to hear that you are planning to protect yourself from xxxx the fer de lance and other dangers.

Very sincerely yours,

Alice Stokey

3/14/23

Cable and Wireless Address:
"PLEASANTON, HONOLULU"

PLEASANTON HOTEL
HONOLULU, T. H.

March 14,1923.

Dear Dr. Torrey:

I have written for catalogues of the different California colleges or universities. That of Leland Stanford has come. What they offer in Botany does not appeal to me. Many of the courses are not given certain years. That looks bad in itself. You know I met their Systematist,a Mr.LeRoy Abrams,who is nice enough to be sure but seems mediocre. To study under a man like Standley who is actually doing things would appeal to me. Then also Stanford is situated near San Francisco - I want to get farther South if possible.

I just received a letter from Smiley. I had written to Occidental College,which happens to be in Los Angeles,but did not say where I had studied so that I would hear nothing but unbiased facts from him. His letter is as follows: "[illegible] I have asked the registrar to send you a copy of the college bulletin for the current year. You will note that no provision is made therein for graduate work in botany. It so happens that subsequently to the issue of this bulletin we have decided to admit a limited number of graddate students in several departments, including botany,and now the situation is somewhat different from what is indicated in the catalogue. I judge,however,that you will be more likely to find the kind of work you are seeking in one of the universities,either California or Stanford."

From the Univ.of Calif.,also near San Francisco,I have received announcements of fees and deposits and all other nonessentials but have no idea of what they offer. Their catalogue is out of print and so they will send me a copy of the new edition which is in course of preparation. So I should get this in a year or two.

The question is whether I should concentrate entirely on Systematics or whether I might not try to train from a pedagogical standpoint in Botany for a year. If the latter were advisable a small college woul be best for me. Then again,before getting any teaching positioj at all, a person must have taken a certain number of credits in Education I believe. Then after St.Lucia I might go to Buitenzorg for a year or two to study,fuss around with plants,and keep warm in winter. It seems the Western colleges are not strong in Systematics. Have you ever heard of a college in New Mexico? I want a hot place for next winter.

Not long ago I sent Dr.Davis his "hope chest" with a few fungi and some tubers of DIOSCOREA sativa L.,that I found growing wild in the mountains. The tubers were borne in the leaf-axils six feet above ground,while the plant arose from between a double tuber each one of which is 5 inches in diam. Please plant these in the greenhouse if you have room. The development of the aerial tubers should be interesting while you will be able to get flowers for dissection for next year.

14-III-23

Cable and Wireless Address:
"PLEASANTON, HONOLULU"

PLEASANTON HOTEL

HONOLULU, T. H.

I have been very busy with my hermit crabs. I have collected about 14 species and just lately a single small beast that is different from anything I can find in the literature. I think it is probably new. I have painted it. First I killed it in formalin but it came to life ag again so I could not kill it a second time. That would be vile. During that "killing" procedure it lost a claw. Yesterday it moulted and regener ated a new one. NowXXXXX I have two specimens of this new (?) species since the moulted skin is as good as a specimen. That is my method for multiplying rare specimens.

Coulter accepted my Lycopod report for the Gazette. It will be published in the same number with Miss Stokey's article sometime next year. For the last paragraph I wrote: "To Dr. James B. Pollock, Exchange Professor from the University of Michigan, the writer wishes to express his indebtedness for valuable suggestions and criticism in the preparation of the mabuscript." This I sent Coulter long before I added the photographs to it. Dr. Davis certainly helped me almost as much in taking those pictures as Dr. Pollock has done. But I reproduce only one of them and shall have printed in agate type under it "Dr. W. H. Davis & C. Degener". I do not think that gives sufficient credit to Dr. Davis for his help. Should I not add something to my concluding paragraph in regard to him. But I must make it proportional to the actual amount of work in my report attributed to him otherwise it will not be fair to Dr. Pollock. Would you add: "The ability to photo-graph the sporelings in the hemlock grove is due to the kindness of Dr. Wm. H. Davis of Amherst, Mass"?

I am beginning my L. cernuum report. So far I cannot find any accou at all of the station in which the gametophyte has been found. Treub says he grew it on tree fern stumps. In another place he merely states that the gametophytes prefer clay soil. I can find no account in Just's Bot. Zentralblatt of any other L. cernuum find. XXXX So I think a brief description of my normal stations would not come amiss for a contrast to the account of the steam-crack stations.

I find it is impossible to state that the gametophyte is always the result of prog. steril, of pot. sp. tissue for according to Bower, even both the antithetic and the homologous theory are true in certain plants. The latter being the case in many algae, etc. That I should think im-plies that the gam. in the algae is but analogous to that in higher plants and not homologous phylogenetically. I think my "theory" would be a risky statement to make.

I AM PRETTY SURE THAT L. OBSCURUM SPORELING HAS MESARCH XYLEM. This is always the character of the leaf, a conservative organ according to Jeffrey. If ont. recap. phyl. in sporeling and this is mesarch, a characte not at all typical to the Lycopodiales; and leaf, a conservative organ is mesarch, are these two facts merely a coincidence? The tracheids for

14-III-23

Cable and Wireless Address:
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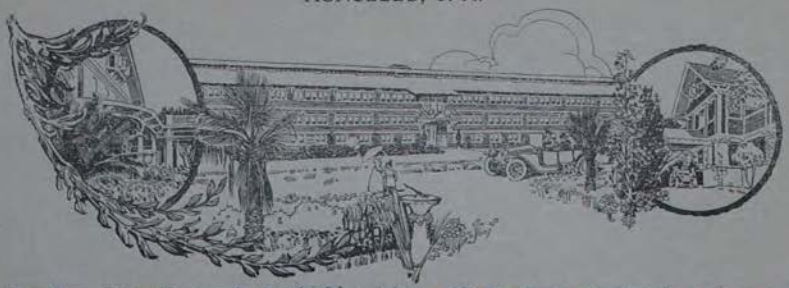
PLEASANTON HOTEL
HONOLULU, T. H.



the leaf arise through modification of cortex without being connected a all with central stele in many cases. Xylem also arises from nowhere in particular near middle of foot. The foot is nothing but parenchyma otherwise, and I do not see why cortex should be anything more than sclerenchymatized parenchyma. This origin of central cylinder and origin of xylem in leaf may then be similar. Furthermore the sporeling of L. cernuu is like PHYLLOGLOSSUM and I believe that the xylem of the leaves in phylloglossum are not all connected with the dentral stele. They are probably unconnected since they are so near the source of food. A continuous mass of xylem fromrroot to leaf tip would not be necessary. Then possibly the simplest Lycopod would be nothing but a corm-like structure - namely the foot - with many leaf-like organs upon it (as in case of L. cernuum). Then further growth is produced merely by additional leaf formation centrifugally with axillary sporangia. Why would not this be exemplified by a Phylloglossum in which the sporophylls are not reduced but like the cauline leaves. Then do away with the ######## stem upon which the sporophylls are heaved up into the air. The stem is surely nothing but an elongation of recent development ####### for ai in spore-dissemination. I did nor ealize up to now that I get nothing more than an isosporous ISOETES by this slight transformation. This brings together the Lycopods as shown by the normal undifferentiated aerial gametophyte of L. cernuum, it brings in line Phylloglossum which resembles it so closely, and it brings in Isoetes which is primitive probably because it is aquatic. Then ALSO I remember that Bower says Isoetes resembles a Lepidodendron or Sigillaria or something like that. You will know which fossil I mean.

Now force up the central growing point of L. cernuum sporeling. The farther you remove the leaves from the foot with its close contact with food from gametophyte, the more need will develop for special tissues to bring this food up to the higher leaves. Thus tracheids develop for the higher leaves. I forgot to mention that the lower scale-like leaves in the sporelings of L. obscurum have NO xylem at all.

I know of a man who wishes to get together about ten people, each one of which should pay $500 for the purpose of buying a boat. Then he wants to cruise around the South Sea Islands and when through sell what remains of the boat to get back some of the money. I am watching how this will develop. If he can get a decent boat that wont sink too easil I might consider something like this. As yet it is nothing serious with me. I had better stay on dry land for the summer. Butthere is another expedition to the S. S. islands leaving England. These people expect to collect material, have a little adventure, and trade with the natives. Their chief cargo is going to be dinamite and oil. If anything happens, it will happen very quick.

An old ichthyologist ate with me for a few days. He fimly believe in all these psychic phenomena and told me a lot about them. I lent him Crawford's book and he drew my attention to the Scientific American. The are publishing their investigations conducted by the leading men in that field. Don't forget to look at Jan-March numbers and keep it up

3/20/23

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

March 20, 1923.

My dear Mr. Degener:

This is the fourth attempt to write to you about the papar but I never get far enough to count for much. This was a bad year for me to attempt to collaborate with any one. The new building takes every minute and for the last two months the amount of sickness has made an enormous amount of extra work. I haven't been sick but my assistant has and almost all of our students - perhaps it is not as bad as that but it seems pretty bad when there is a lot of make-up work.

I went over your paper very carefully when it first arrived and made some notes; then I compared it with the copy which Dr. Coulter sent back. I did get so far as to write to him that I had your copy and to ask him not to hold up the publication (which must be some months off according to his letter) as I could send it on at once but would rather write to you about a few points. As I have gone over the paper I felt inclined to make a few suggestions. I am not sending back the paper as the changes can be made here if you agree with me - if not I shall send it in as it is.

On page 2, I would leave out "or thallophyte" on the first line; it seems pedagogic to me and not necessary. In the second paragraph I don't quite see the force of phrase "in spite of the fact." L. obscurum as we find it growing has erect shoots from an underground stem which is horizontal, so that it seems quite natural for a prostrate stem to have three erect shoots and to continue horizontal. I am not sure whether or not I am misunderstanding your description, but it seemed to me that on the bottom of p.6 where you speak of "one or two subterranean shoots which seem arrested" the shoots could perfectly well be the underground rhizome which is just beginning. In my material the first shoot is always erect; by the time this is 2(?) years old or at least has begun to spread the lower part sends out 1 or 2 horizontal runners underground and then these runners give rise to erect stems. I should think that they were hardly arrested but merely of later development. From my material I would say that the clause "which seemed arrested in growth" should be dropped out.

I am enclosing page 6 of your old copy which is in part identical with page 6 of you later copy. I should favor a different statement with regard to the position of the prothallia. I would not leave out anything that helps the statement. As you referred before to Spessard's early statement and as he himself corrected it - it seems better not to rub it in. For that reason it seems better not to put in part of the material in the early part of the paragraph. It sounds better and not as if you were trying to hit a man who had given in. Perhaps the reference given on that page (Spessard, 1918) would do just as well on page 2 at the end of the second complete paragraph where you mention the gametophyte of L. obscurum discovered in 1917 by Spessard.

If you think that these are unnecessary quibbles don't hesitate to say so. I am giving my impressions both on first reading and on re-reading.

Your photographs are splendid. I am not sure whether or not

20-III-23

you gave me your idea of writing up our L. obscurum material - the prothallia. I am sure that we have a good deal to add. I think that plates Y and fig. 2 would do equally well if not better in a paper on the structure of the prothallia, but that X and ~~xixtx~~ fig. 1 are particularly well adapted to thispaper. I would like to collect material this spring and write up the paper this summer. I shall want some of your young material unless I collect some at Amherst at your patch. We can combine our observations and data as well as photographs and make a really authoritative paper.

The Cibotium spores which you sent arrived last week and I had them planted. I saw yesterday that some of the spores on distilled water had begun to crack. I had a little C. glaucum once but did not get much from it. I have never had C. Chamissoi. Many thanks.

How is L. cernuum coming on?

It is still winter - down to 8° last night, but there are a few babe patches on the ground and it begins to look as if we might have spring some day.

I hope that the worst of the rush about the science building is over and that I can get to work on Lycopodium. I hope to be able to collect some after spring vacation.

Very sincerely yours,

Alma Stokey

4/4/23

Cable and Wireless Address:
"PLEASANTON, HONOLULU"

PLEASANTON HOTEL

HONOLULU, T. H.

April 4, 1923.

Dear Miss Stokey:

I just received your letter of March 20. It is very lucky for me that you drew my attention to my paragraph on Spessard's mistake! By all that would be a terrible blunder. That would be an awful thing to do. Thank you ever so much.

Since that paragraph beginning with "Because of a question ---" on page 3 likewise emphasizes that mistake in determination by Spessard I think the enclosed version will be alright. Then I discuss the position of the gametes of both species more fully than am. Your correction would be fine if I left that paragraph on page 3 as it stands, but I think same of this had better be omitted.

In regard to adding "or thallophyte" on page 2: I noticed that you generally use the term "prothallus" while I always say "gametophyte." For that reason I meant to add, "or prothallus" to make my article more like yours. How I got to put the "thallophyte" there instead of prothallus I do not know. Anyway that would be just as bad. So please scratch out "or thallophyte" for me.

I really think that those shoots are arrested in growth. Lycop. is supposed to dichotomize, theoretically the rhizome and the spring shoots should be of equal length. This is not so. I think that in growing, the rhizome and that first shoot develop dichotomously but that the shoot develops quickly and gains nourishment for the plant by photosynthesis since now the gametophyte furnishes nothing more. Thus the other branches

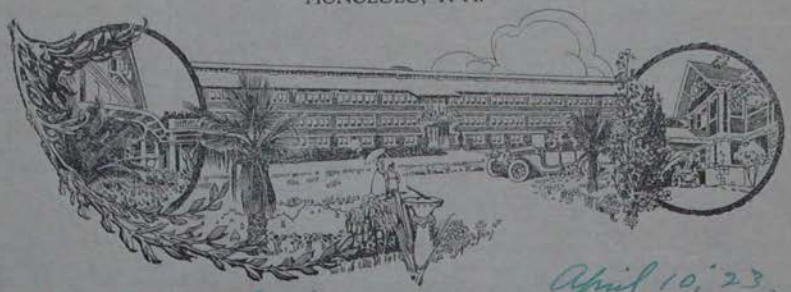
of the dichotomy lags behind until food is at hand whereupon it forges ahead in growth. That is my reason for thinking that it is arrested in growth.

According to Goebel's "Organographie der Pflanzen II" p. 1018 adventitious buds cannot be formed in the Lycopods at all except only at very young shoots in case the tip has been removed. He also says "— — — that some shoot 'anlagen' do not develop right off but remain in a resting stage at the base of the plant. Then when they later develop, it appears as though adventitious buds had been formed". Do you not think that for this reason my way of stating it growth would be correct? Possibly I am merely making things seem more involved by saying that these shoots appear dormant because when you come down to it, sympodial growth of which this might be a certain type, is due to a state of dormancy in a dichotomy. I would prefer to let my statement in regard to this stand as it is. But in case you are convinced that I am very wrong after hearing my reasons, please send in Coulter my second paper (under separate cover) in which I have changed this statement to agree with your view.

4/10/23

Cable and Wireless Address:
"PLEASANTON, HONOLULU"

PLEASANTON HOTEL
HONOLULU, T. H.



April 10, '23.

My dear Miss Stokey:

I am writing up my thesis on L. obscurum now as it must be in before May 26. I wish to find out the range in size for the gamus of obscurum. Not a single one of mine approaches the carrot shape at all. But Spessard in his last article gives fig. 70 as L. obscurum! I cannot see but that it is practically typical of L. complanatum. I would like to know from you whether a single one of your L. obscurum gamus has a tendency to become carrot-shaped. Furthermore do you think that figs. 76-79 on Plate XIII are abnormal? I do since I have nothing like that probijection of antheridia. Do you find the fungus near the antheridia? Spessard on page 406 states that the fungus decumbide the antheridia. I can hardly think this is correct or rather that it may. He too is certain of his cases but that this is an abnormal condition. Of course the thing for me to do is to look at the antheridia of my specimens to see whether there is any sign of the fungus, but as yet I have not done so.

I think Spessard is incorrect in calling fig. 70 obscurum and for that reason I would like to know just to what extent your specimens approach that type. I also believe figs 76-79 are absolutely abnormal, in fact I am practically sure. I think

this proliferation of antheridia might be due to the entrance of the fungus to the tissue close to the place ~~which~~ where the antheridia are normally situated and that this stimulating or irritating (meristematic) effect has caused a "witches broom" at that point (or gall).

I think that when we write up L. obscurum we will be obliged to revise Spessard's statement in his summary on p. 411 that "the prothallium of L. obscurum shows a form transitional between the L. annotinum and L. complanatum types" and also correct figs. 76-79, saying that they are abnormal.

I just heard from my sister that you know Miss Sturtevant of Barnard College. ~~She is a good friend of my sister.~~ is a friend of yours.

Sincerely yours,

Otto Degener

I shall correct these two mistakes (? a very faint question mark to my mind) in my thesis since there it matters little what I say. Nobody will read it anyways so ~~that~~ it does not harm Spessard when I criticise some of his statements.

4/12/23

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

April 12, 1923.

My dear Mr. Degener:

Doubtless long before this you have received my letter about the manuscript and found that it arrived perfectly safe. Foreign mail often is arratic, I find, and things mailed together may arrive several days apart. I had a note from Dr. Coulter saying that it would make no difference if I held the manuscript for a few weeks, as it would not be used for several months anyway. I wanted to be sure that it would not delay publication if I stopped to write to you.

The packages of spores - Sadleria and Cibotium Meziesii - were forwarded to me last week as Woods Hole where I was spending my spring vacation, and the bottled material of Lycopodium cernuum arrived this week. Very many thanks for all of them.

I got out the Torrey Bulletins to make an attempt to verify the determinations and found that habit and gross characters were used so extensively that I did not feel at all sure of my determinations. I infer that you have not the Torrey Bulletins on hand so I am sending you a copy of the descriptions so that you can make them in the field. I hope the plants are sufficiently common so that this does not involve much work. I thought that some of them would probably be common. I am very glad to have the material and am particularly glad to have so many new Cibotiums, as my work was rather weak on Cibotiums. Everything has germinated well. The specimens which you sent last month - C. Chamissoi or C. glaucum - have already made a noticeable green covering on the peat, and the last material which I planted on distilled water last Saturday on my return has begun to germinate; Sadleria shows rhizoids in five days and C. Menziesii has begun to crack. I certainly am delighted to have them. I notice that Miss Robinson says that Cibotium is the only genus of the Cyatheae found in Hawaii.

I have examined the bottled material which you sent. I am very glad to see the prothallia of L. cernuum, so much more delicate than our subterranean forms.

The material was badly shaken up when it arrived, or course, but the slimy covering on the soil held great masses of it together. I examined it and found that it was a blue-green alga, probably Gloeocapsa - a unicellular colonial form with a very heavy slime sheath. There was only one thing in the bottle which suggested a moss a creeping plant with many closely packed leaves. It is a vascular plant with true roots containing a vascular system, so it must be some sp. of Lycopodium. I don't know whether that was what you meant or not in your reference to a moss.

It looked like this →

I would like to do some experimental work on the question of the conditions for germination of lycopodium spores and take up the question of the rate at which they are carried down into the ground. However,

12-IV-23

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

at present I seem to have as many problems on hand as I can manage. I may be able to interest my new assistant in the problem - she is a physiologist and keen on research. In Bruchmann's article on the three prothallia which he raised from spores he attributes their slow germination to the presence of raised ridges forming a network on the coat, the compartments between the ridges holding air and keeping the spores from wetting. That doesn't seem to me to be an adequate explanation - many fern spores are roughened and repel water for a while and the somewhat greater roughness doesn't seem to be equal to several years delay; it may be that he is right in that it helps to carry them down into the soil. Puff-ball spores resist wetting similarly and they also would need to be carried down into the soil for favorable conditions for growth. Our three species of which we have the prothallia - L. clavatum, L. obscurum. and L. complanatum all have very rough spores with a very heavy ridged network. L. lucidulum has sommther spores, probably more like L. Selago as described by Bruchmann. I find that Bruchmann describes the spores of L. cernuum.

I think it is of considerable interest that you find mesarch structure in the stele of the sporling of L. obscurum. Didn't one of Dr. Jeffrey's students (Eames or Sinnott or someone else) find mesarch xylem in a mature stele of Lycopodium?

It is just beginning to be like spring. Such weather as we had during the spring vacation! It went down to zero the morning of April 1st here in South Hadley and at Woods Hole it was only 6° - the coldest weather they had had all winter and the wind was blowing a gale such as we had not had all winter. Miss Bonnell and I went down to do some inside painting on my new summer cottage and we managed to do considerable inspite of the cold. We had only a fireplace and an oil cook-stove for heating but we managed to keep fairly comfortable when we were in sunny rooms.

How long are you to be in Hawaii? I shall be very glad to get pressed specimens of any ferns, especially endemic ferns. If you can hire some one to do the work I shall be very glad to pay for it.

I want to thank you again for the excellent spore material of Cibotium and Sadleria.

Very sincerely yours,

Alma F. Slosson

4-(12-14?)-23

Robinson: Pteridophyta of the Hawaiian Islands.
Bull. Torr. Bot. Club. 39: 242-243. 1912.

CIBOTIUM Kaulf. Jahrb. Pharm. 21: 53. 1820.

Pinonia Gaud. Ann. Sci. Nat. 3: 507. 1824.

Rootstock usually arborescent with numerous large, chaffy scales at the apex; leafstalks not articulate; blades deltoid-ovate, bipinnate to quadripinnate, the pinnules stalked: sori valvate capsules, consisting of a cuplike outgrowth from the margin of the lobe of the pinnule and the indusium, which forms a lidlike covering. Sporangia stalked.

The leaves of all Hawaiian species are tripinnate.

Type species: Cibotium Chamissoi Kaulf.

Caudex 4-8 m. high; leafstalk 3-4 m. long, covered with soft, brown scales at the base, with black, hairlike scales above; blades 3-4 m. long; sinuses between the segments of the pinnules broad, shallow, often margined with sori; fertile veins mostly simple, usually 7 or less to a segment. C. Menziesii

Caudex less than 3 m. high, usually about 2 m. high; leafstalk clothed at the base with soft, brownish scales, upper portion naked; blades less than 3 m. long; sinuses between segments of pinnules narrow, acute; fertile veins simple or once forked.

Lower surface of blade dull glaucous, for the most part strongly tomentulose; veins 6-8 to a segment, simple or forked. C. Chamissoi

Lower surface of blade conspicuously glaucous, for the most part slightly tomentulose, becoming glabrous; veins 9-10 to a segment, usually 1-2-forked. C. glaucum.

Robinson: Pteridophyta of the Hawaiian Islands. 4-(12-14)2-25
Bull. Torr. Bot. Club. 40:224-225. 1913

Sadleria.

Sadleria, an endemic Hawaiian genus, is frequently found as a pioneer on disintegrating lava rock. It differs conspicuously in its greater size, rigidity, and the number of its scales, from Blechnum.

Caudex erect, often arborescentm more or les s clothed with scales, or "pulu"; leaves bipinnatifid to bipinnate, 60-180 cm. long in arborescent species, 25-50 cm. in herbaceous forms, usually coriaceous; sori linear upon intercostal arches on either side of and parallel to, the midrib of the pinnule, covered by a coriaceous indusium; sporangia short-stalked.

Caudex arborescent, 60-300 cm in height.

Leafstalk scaly at base, naked or slightly furfuraceous above: sori usually extending nearly the length of the segment.

Leafstalk 60-90 cm long, sulcate, densely clothed at base with light brown scales, 3-4 cm.x 5 mm., slightly chaffy above; blades chartaceous to subcoriaceous, furfuraceous when young, nearly glabrate when mature, oblong-lanceolate, 150-200 cm. long, bipinnate; pinnae linear, 30-60 cm. x 4-7.5 cm.; pinnules 3 mm. apart. S. Souleytiana

Leafstalk 30-60 cm. long, not sulcate, paleaceous at the base with scales 4-5 cm. x 3 mm., naked above; blade coriaceous, glabrate, ovate-oblong, 60-90 cm. long, bipinnatifid; pinnae linear-lanceolate, 15-20 cm. x 1.5-2 cm.; segments of pinnae 2 mm. apart. S. cyatheoides

Leafstalk scaly throughout, 20-45 cm. long, sulcate; basal scales ribbed, and revolute, upper scales ribless, chaffy; blades coriaceous, oblong, acuminate, 45-60 cm. long, bipinnatifid: pinnae oblong, falcate, 10-16 cm. x 15 mm. chaffy along the midrib; sori usually not as long as the segment, often less than half as long. S. Hillebrandii

Caudex not arborescent, 2-15 cm. in height; leafstalk densely paleaceous as are mirribs and costae.

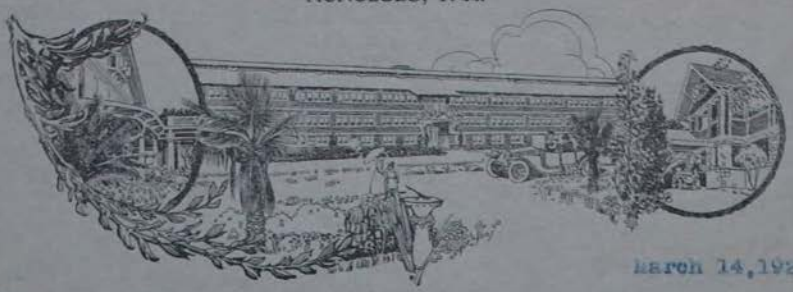
Leafstalk 15-25 cm long; scales dark brown 10-15 cm., long; blades coriaceous, oblong-lanceolate, 25-50 cm. long; pinnae 5-10 cm. x 12-25 mm.; pinnules crowded; margins crenulate. S. polystichoides.

Leafstalk 12-15 cm. long; scales reddish brown, 6-10 cm. long; blade sub-coriaceous, ovate-lanceolate, 28-30 cm. long; pinnae 2-4 cm. x 0.75-1 cm., reduced below, not crowded; margins entire. S. unisora

1/1/23

Cable and Wireless Address:
"PLEASANTON, HONOLULU"

PLEASANTON HOTEL
HONOLULU, T. H.



March 14,1923.

My dear Miss Stokey:

I received your letter of Feb.21. I have not looked at the spores of L.cernuum but I have those of L.pachystachyon here,an endemic to Hawaii,and similar to L.phlegmaria. These spores I have put in a hanging drop Feb.3 and also in a petri dish with organic matter in which the sporelings had grown. About a week ago (or 2) a few of these spores have changed in appearance. Normally they show a homogeneous content of protoplasm with one hyaline mark in the midale which may be a nucleus although it looks like an oil drop. How many of these germinating? spor es show hardly anything but oil drops of different sizes as contents. They tested for oil with Sudan III. But I am not sure of that nucleus or oil drop.

From Treub's account in Ann.de Buitenzorg which I am reading, L. cernuum spores develop oil drops after a few weeks before germinating. Thus I hope to germinate these spores before long.

Now as to your question about the nature of the L.cernuum spore, and its quick germination. I know nothing about that. But L.pachystachyon appears to be germinating and these spores are PITTED but otherwise entirely smooth. I know they are finely pitted on the wall that is external when they are in tetrads. It appears to me that the other three sides between the 3 scars due to tetrad origin are not pitted. I am not quite certain as I should really use an oil immersionand I lack the oil. None is in Hawaii at all. This pitting may cause early germination. I think this epiphyte must germinate its spores quickly because bark of trees in which they are caught and in which they germinate, will not resist decay for many years. I will let you know about character of L.cernuum spores. Could you tell me whether the Mass. species have pits?

L.pachystachyon I have found common on the windward side of the mountain back of Honolulu. On this side it does not grow since the leeward side does not intercept the rain. I have found scores of sporelings but cannot find the gametophyte. though I am quite sure I have it in the fibrous aerial fern roots and debrees that I have collected on trees. I think the gametophyte is filiform and very brittle.

Have you received my MS for Coulter? I am worried about it. I sent it to you to forward to him. I do not see why it did not arrive at the same time as my letter.

I enclose herewith part of the frond of a CIBOTIUM of which I had sent you to vials of spores. I believe it to be C. Chamissoi but hope that you will verify this by referring to Bul. Torr. Bot. Club p. 242 for I think 1918. or thereabout. Enclosed is also vial of C. Menziesii with parent fronds. No doubt at all for this determination.

In the footnote of the MS I give the magnification for the photographs .

When I have time to pick out the specimens from the substrate, I will send you a mess of L. cernuum game.

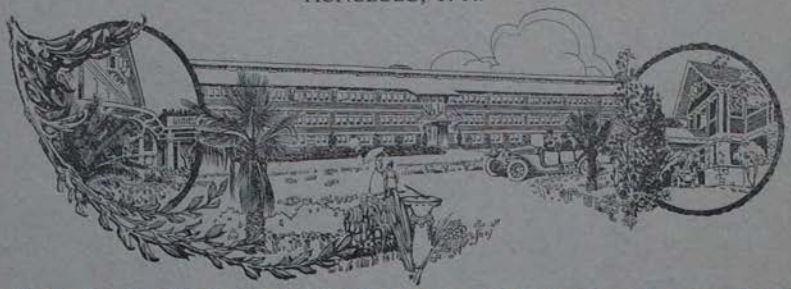
I have sectioned sporelings of obscurum and find the stele generally radially arranged with 5 poles. But the important thing is that I have found it to be mesarch, not exarch. This I think significant when you consider that the leaf of the Lycopods is mesarch, and this is a conservative organ according to Jeffrey. And no doubt the base of the sporeling should be considered conservative if ontogeny recapituates phyllogeny. As far as stelar arrangement is concerned, I thing that is purely arbitrary as far as we can tell. All the L. differ in stelar arrangement in different regions of the same individual. I do not see that I can get any results beyond stating the fact that stelar arrangement is very variable just as in the other species worked out by others Your telling me that you have sporelings with exceptionally large feet (badly expressed but true) has made me observe that the foot swells with age probably for the purpose of pressing against the gametophyte to aid in transllcation of food. I notice that the cells of the gametophyte adjoining the foot are collapsed as though their contents had been removed to large extent. I believe the food has not mechanically crushed them only, but that the chief action was the absorption of the contents causing them to collapse.

I did not know that the optimum is different for stem and root. Thi fact steengthens my belief that the gametophyte and the very young sporeling of L. cernuum find their optimum conditions in those volcanic steamcracks while the old sporophyte is killed by it.

Sincerely yours,

Cable and Wireless Address:
"PLEASANTON, HONOLULU"

PLEASANTON HOTEL
HONOLULU, T. H.



I am sorry that my copy of my ms. is not spaced exactly like the one that you have. I am therefore not sure whether you can merely put one of the enclosed pages into the place that the other had occupied.

Please take those pictures out of the article as you have advised. That will leave the one by Dr. Davis and the plate of sporelings. Would you then kindly letter those two inverted prothallia with gametophytes and that gem. that is standing on end? I think it is on the plate that is to be omitted that I have lettered them to fit the text. I am very sorry to give you all this bother. Then we can keep the other photographs for a collaborated article on the gams.

Since we have not really collaborated as yet I would suggest the following heading in the Bot. Gaz:

STUDIES IN THE GENUS LYCOPODIUM

A.G. Stokey & O. Degener

\- - - - - - - - -

I. LYCOPODIUM PROTHALLIA IN WESTERN MASSACHUSETTS

Alma G. Stokey

(Then your article)
mine following:

II. FOUR NEW STATIONS OF LYCOPODIUM GAMETOPHYTES

Otto egener

(My article)

In a diferent volume the following year:

STUDIES IN THE GENUS LYCOPODIUM

III. THE GAMETOPHYTE OF LYCOPODIUM OBSCURUM

A. G. Stokey & O. Degener

This could then be followed under the same heading [illegible]

by part IV "The Gam.[of] L. cernuum in Hawaii."
O.D.
by part V "The Sporeling of L. obs."
(A.G. Stokey (?) (?)) O.D.

Such a method would combine these different articles on Lycopodium in a way, and it would also show what articles are collaborations.

I am very inquisitive about your article. You wrote a long time ago that you had gotten a bright idea, and to this you added a question mark.

I am not quite sure whether I told you that my gametophyte arrived and your long sporelings. The foot there is much larger than in my specimens. I do not quite understand why the foot should grow any more after the gametophyte has rotted away.

Sincerely yours,
O. H. Degener

I have decided to send you those corrected pages under separate cover.

4/17/23

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

April 17, 1923.

Dear Mr. Degener:

Another station for Lycopodium - two species, L. complanatum and L. obscurum. The spring season is on and I landed these while out with a class. You should see the L. complanatum ones - such whales, 16mm. x 6.5 mm., nice light tan and clean looking. I found three of them and three L. obscurum but I did not hunt very long. It was not a thick patch and perhaps that is why the prothallia are so large; the chances for fertilization are less and the gametophyte is not checked so soon. The patch was on a slight slope on the west side of the Newton Smith woods, close to the remains of an old chestnut, where there were some red maples and a few young hemlocks - indications of a good mesophytic habitat. They had unusually long stems, the sporophytes of course; one was over 15 cm., owing to a winding course to the surface. They were all rather deep - from 7 to 10 cm. I think I shall have to make an addition to my paper. This place is at least 1 ½ miles from any other habitat and probably 2 miles.

I want to look in another place before I change my paper; the foot of Mt. Holyoke looks like a good region and I have done no hunting near there.

Probably one region why the Gazette is so slow in publishing is that is is cutting down on issues. Do you notice that there are to be no issues in Feb. and March, and only 8 a year? One of my former students has a paper in the March number on the prothallia of Lygodium. She is at Vassar.

Yours sincerely,

Alma Stokey

4/26/23

MOUNT HOLYOKE COLLEGE
SOUTH HADLEY, MASSACHUSETTS

DEPARTMENT OF BOTANY

April 26th.

My dear Mr. Degener:

I have received your letter about the paper for the Gazette and hope to get it ready to send in a few days. Your letter asking about Spessard's pictures and my data has just arrived and I hasten to answer it.

Like you I have felt sceptical about his fig. 70. It looks to me like L. complanatum. I doubt if he has ever had enough ~~L. complanatum~~ material of L. obscurum to be really sure of it. I almost think I can tell the sporelings apart from the first leaves above the ground. I have not analysed the difference but there is one That seems to be very constant in all the material I have seen, no matter where it was gathered or how large the prothallium. I have never seen anything that suggested in the slightest degree that it is a transition form and I doubt it. I have seen various kinds of bulging antheridial cushions - none quite like his 77 and 79 - but not unlike 78. I have not observed any fungus in the antheridia but I have not gone after it. I have had many prothallia like 75 with much bigger cushions. I shall enclose some drawings which I made of a few. The drawings are not much but I would like them returned as I want them for record.

Some of my L. complanatum have done all sorts of things with their antheridial cushions. I have a set from Moody Corners where the soil was hard and not favorable in which they are irregular in shape - not nice smooth carrots - and they seem to show a monopodial development, as if they had produced a series of crowns and had continued with the main axis at the same time or in between times. There is no question of their identity, either.

As to size. I have none as small as your smallest; the smallest with a sporling was 3mm. and I had scareely any as small as that. My largest was 18mm. in its longest axis. It was the [illegible]-shaped type

In regard to the abnormality, I am not sure. They do look queer - those drawings of Spessard's, but it is merely more of the same kind of thing that a healthy prothallium does. It looks however, as if such a development would stop the growth of the thallus. It seems too much, so perhaps it is due to some unusual stimulus.

As for anybody reading your thesis - you never can tell. More than likely some one will stew over it and weigh every word to know just what you had. I am always surprised when I find that anybody has read anything I have written, but it does happen.

I am going down to New York this week-end to make out examination questions for the College Entrance Examination Board, and I hope to have an opportunity to see Miss Sturtevant. We have been friends ever since she taught at Mt. Holyoke.

Your suggestion about naming the series of articles seems to me to be a good one. I shall write to Dr. Coulter about it.

Very sincerely yours,

Anna M. Starkey

577/23

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.
May 7,1923

Dear Degener:

I have been very slow in replying to your letter but I have been turning over in my mind a proposition which I am going to make to you.Have you settled on what you are going to do next year?If not I have Professor' Osmun's authorization to offer you the graduate assistantship in this Department.It carries $600 and you know pretty well what the work is.I am not in the least concerned about your ability to do all that we shall ask and to do it well.

Now .Son, I want you to go carefully in this. If your real interest is in Systematic Botany and you want to go forward as a Systematist then I would seriously adviseyou to go either to the Missouri Botanic Garden, or to New York or to Harvard. If on the contrary you want to go in for teaching and research you probably won't go far wrong if you come back to us for a year.

I am contemplating an advanced course in Morphology which may be given next year.It will cover the first two terms anyhow and would serve as an outline on which you could fit your special work.

Your Lycopod problem would seem to offer possibilities.I do not feel however that at present you are in a position to generalize on a very sound basis concerning any theory of phylogeny whether homologous or antithetic. Your imagination is very active and your letter sounds as if your thoughts were feeding upon one another.I don't know what they are doing to you in Hawaii but it seems to me that you are drifting alone too much.Do you

7-I-23

receive a degree this year or would another year be necessary? If the latter I see no reason why your credits could not be transferred and you could receive the MSc degree from here in June 1924.

I'm not keen about taking on graduate students for it means lots of extra work but I guess I understand your case as well as anyone and if you think you can stand another New England winter we shall be glad to have you come.

[Dave]
Potter has been working with me this year and has turned out a very creditable thesis on Morphological and Physiological Correlations in the Solanaceae.

Professor O. is anxious to learn soon if you will accept the position so will you cable us Yes or No.

I don't want to influence your previous decisions or mould an embryo systematist into a morphologist--so use your own good sense in acting upon this proposition.

The lycopod problem isn't necessarily the only one which you could tackle. Perhaps you have discovered something else in the Hawaiian flora that excites your greater interest. I can conceive that a comparison of Pandanus with sparganiums and cattails might be of interest. The question of the supra-axillary inflorescence might receive elucidation. Roots stems and leaves in 50 % alc. would be allright if one doesn't want protoplasm. Use chrom-acetic if you do.

Very truly yours

R. E. Torrey

6/1/23

HALEIWA HOTEL
HALEIWA, HAWAII

June 1, '23.

Dear Dr. Torrey.

As I have a few days before Commencement, I have come to this place to hunt hermits. All I am doing here is collecting them upon the reef during the day, and putting my Thesis into shape at night. My ideas will stand but I have a few misspellings and some horrible sentences.

In your letter you say you are going to give two new courses. If you could stand one extra student in it, I should like to take it. I have been thinking about myself in a purely impersonal way and think that some plans of mine may be dangerous. One year's freedom is enough. If I continue my collecting of plants without any definite

immediate purpose, and without any real deep thinking forced upon me by some one else, I may become nothing more than a tramp. It is easy to get into a rut and still easier to ~~get out of~~ remain in it. Before collecting material in St. Lucia I must break this routine of collecting and I believe ~~that~~ your course would be about the best thing. I will not only be forced to think again but I will learn things that I should know. If I were to go to Tulane, I don't know what they teach nor what kind of person does it. Then also, I need cold weather before going on another tropical trip so as not to become "washed out" with continuous warm weather.

To come to Hawaii for a year was an excellent thing for me to do. But the longer I stay the less profitable can I spend my time here. I have learned a lot about plants from an ecological and systematic standpoint. Now it is time that I study

6/2/23

Cable and Wireless Address:
"PLEASANTON, HONOLULU"

PLEASANTON HOTEL
HONOLULU, T. H.

June 22,1923.

Dear Dr. Torrey:

I received your letter of June 9 in which you say that the assistantship is still open. A "guileless person" has finally been trapped provided this position has not been filled while your letter was traveling to me. I have just cabeled Prof. Osmun.

I received a letter from Whetzel stating that he was still planning upon this St. Lucia trip a year from now. He procrastinates too much so that I feel I should not depend too much upon his plans. I hope he can wait a month or so for me if he is really serious about going on this trip around April. I can easily enough follow him, or go alone if our plans differ too much. I certainly prefer to go with him but I cannot waste too much time. It is best for me to get a thorough review of Botany, finish my L. obscurum anatomy since Miss Stokey must wait until I have completed this work, and study L. pachystachyon. I should be able to publish these two papers by the end of next year under your direction.

It is very kind of you to correct my L. cernuum report. It certainly needs it but unfortunately I have rewritten this before submitting it as m part of my thesis so many of the very worst mistakes have been changed. I was too eager and so sent you my paper as soon as my ideas had been fully set down. Although some of these mistakes have been corrected, I am sure that many are left. I can now compare your correction of my old mistakes with my rewritten paper to see how I differ from you. It was very good of you to take all this extra trouble for me. I can also realize that a paper in such form will make a very bad impression upon Miss Stokey.

I sent this same paper to my sister. She says that I should study English during the summer instead of collecting plants. She also told me that my writing was a mixture of German and English. I wonder whether I am especially rotten this year because I never talk to anyone except to my Japanese waiter. I am completely free and isolated in this hotel. It is fine.

The L. cernuum report has not yet arrived. I guess it just missed the boat at San Francisco. It should be here next week.

I am glad that there is nothing especially wrong with my idea. I added my observations on PSILOTUM and OPHIOGLOSSUM because they were growing in a volcanically heated crevice. They could have reached this place only by spo dispersal. Ophioglossum cannot be found growing anywhere except in this one heated crevice. The gams. of both O. and P. are thus "present but not visi in that crack. I am going to that place again in a few weeks to verify ran of temperature, etc., to take photographs and to try to find the gams. of O. and P.

I have received my M. Sc. from the University so that all my future work will have to count toward a Ph. D.

I have receved a letter from Coulter stating that my photographs wil not reproduce at all satisfactorily. It cost almost $10 to have this mate photographed by this professional plumber. I shall try to get at least p

of my money back again. I will try another photographer. To get decent photographs here will take a lot of time and money. But that can't be helped.

I sent you some living PSILOTUM triquetrum. It should grow well if planted in sandy soil and kept in full sunlight. Kindly let Miss Stokey have some of it. I would advise you to pot this P. and place the pot on a bench of sifted a mixture of sifted soil and sand. Dr. Lyon t old me that he found many sporelings in his hot-house in the neighborhood of adult potted plants. He is sure that they arose from gams. You might raise some gams. of Psilotum by leaving the soil undisturbed in the neighborhood of the plants that I have sent you. If sporelings should show up, all that will be necessary to get the gams. will be to sift the soil.

Now that Commencement is over, I am determining my plants again. I sent off a package to you about a week ago. I do not know how I will ever get away from here. My wardrobe trunk is overflowing with herbarium specimens. I am getting rid of as many clothes as possible.

Yours sincerely,

6/6/23

~~FOUR NEW STATIONS OF LYCOPODIUM GAMETOPHYTES~~[1]

Otto Degener

June 6, 1923.

Dear Mr. McCallan:

Hawaii is very much like Bermuda. Many of the plants that I learned to know while I was at the Station are found here. So I can see no reason why most of the plants growing here will not grow just as well in Bermuda. It is for this reason that I think Rock's book may be of interest to you.

Lantana is a terrible pest in the dry regions and grows in almost impenetrable thickets with Guava. So I think the Guava would be a very dangerous plant in Bermuda if it ever got a start there. Remember that I found a plant growing in Dagat Marsh but this was not the same species as the pest here. I have noticed a rust and also an insect that attacks the Lantana and possibly keeps it in check in certain regions. These two things might be of use to you.

The Mesquite or Algaroba, Prosopis juliflora, was introduced here many years ago. It has spread like your Liddlewood and is found all over in dry regions even to the waters edge. It is a very valuable tree because it grows where other plants will not grow.

It bears a lot of beans that are rich in protein and an excellent cattle feed. This tree, however, is thorny and for tourists not pleasant. I should think it would grow well in places like Tuckers Town.

The kukui is a native of Hawaii. I think it is very picturesque growing normally in the mountains where its light green foliage is in striking contrast to everything around it. They grow this tree in gardens. I should think it would be fine in Bermuda

There are very many plants here that would be excellent in Bermuda from an ornamental standpoint, and there are very many that in Bermuda that they should grow here. Your cedar, if it did not grow so slowly would be valuable in the drier regions. But I guess if one does not get quick results with plants people will not try to introduce them for the sake of the next generation.

I have just finished my course here and feel relieved that this is over. Now I will concentrate upon collecting for a month or so and then probably go back to Mass. Agri. College to study under Dr. Lowry as I know of no one that is better than he. He is awfully nice and as near a genius as any man I have met. What is Alden doing? Is he going to continue his studies? I am. I shall try to get a Ph.D. degree if possible. If Alan is going after more than M. A. C. can be one of the best places for him as far as Entomology is concerned and also as far as Botany is concerned if he gets under

with me. They did not pay a cent although they were foreigners. Here in Hawaii, I had a separate room to work in in the Botany Building and a separate room in the Zoology Building at the beach. I used a lot of chemicals and slides in my work yet I paid not even a laboratory fee. All I paid was a matriculation fee of $5 to cover the expense of printing a diploma etc. I guess that the government realizes that it does not matter what a persons nationality may be. So long as they are able to experiment and possibly discover things, nationality is immaterial whether they are Chinese or Indians. All knowledge is international.

I heard from Prof. Whitzel that you had another fair at the station. I wish I could have been there to see it. I guess when I am in New York again I shall have to go to Bermuda again for a visit within the next ten years. If you have once been to a place for any length of time, it is hard to keep away. It was really my summer in Bermuda that drove me to Hawaii. I liked the subtropical so much that I wanted to get to a place that would be still farther south.

Please remember me to Mrs McCallan and to Miss McCallan. I guess she is studying medicine in New York now. I guess Mr. Winterbridge is back again and married and that Young is still with you. Please remember me to all.

Sincerely yours,

Otto Degener

I do not think I would go to any of the institutions that are not upheld by the state. I have seen too much of Amherst College to know that type of place. They are a fast crowd that go to college merely to keep from going to work for four years more. Williams I hear is nothing but a grand country club from the father of a friend of mine, who has just graduated from there. The state institutions being upheld by the taxes of the people force the student to study because otherwise the investment does not pay. Cornell of course is good but I think M. A. C. surpasses it as far as entomology is concerned because there we have Dr. Fernald teaching there. Should Alan go to M. A. C. please let me know. If he wants to work his way through to a certain extent I am sure that Dr. Farrey could give him a part time assistantship in Botany. He offered me the entire graduate assistantship in the Department for this year but as you no doubt know Prof. Whitford wants to go to St. Lucia this year and so I had to refuse if I am to go with him. I hated to do it. I hope he will offer me the same position for next year. M. A. C. has cut so much to keep the college going that they have decided to require students to pay some tuition but graduate students do not pay anything. Last ~~My last~~ In my last year there a few Chinese and Indian graduate students were in the same of the same classes

6/9/23

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.

June 9. 1923

Dear Degener:

I am returning your article today and as you will see it is much corrected but almost entirely from the standpoint of the English. There are still a few obscure points which you ought to fix up. These I have noted.

I am delayed in returning it ever since it came just when Final Exams were on and I couldn't get to it. I did not send it to Miss Stokey for I thought it would be better if you corrected it first and then transmitted it thru her to the Gazette.

I have little quarrel with the matter contained or with the conclusions. It seems to be a real contribution. I can't see the exact pertinence of including the Peltotum and Aphanocoenum material, since you didn't find the Gametophytes and only found adult sporophytes.

Have you finally decided what to do. It would seem to me that you had best remain where you are till Whetzel is ready. You can surely do more collecting and spend your time

profitably on your crabs etc.

You really should try to get more training in clear expression. I think the careful reading of good English models such as Huxley's essays would help. Experiment with descriptive writing taking Huxley as a model and see too it that everything is clear cut and that there can be no possible ambiguity. Look out for words like you use "amphibious" when you mean semi-aquatic or very moist. You spoke of silica and alumina as "elements". I question too the advisability of using "node" ~~for~~ as a term for the place of anchorage of a rhizome. Of course a real node in botany is a place where a leaf is borne.

You have the stuff and a good scientific brain. Now you must master a style that shall be sharp, clear and free from padding or diffuseness. Say what you have to say in the fewest possible words. Look out for your verbs. You have the German trick of holding them over to the end of long sentences.

Potter has received his M.Sc. — He did a good piece of work.

As yet we have no one to take his place but I don't worry much. Some "guileless Yankee" will finally be trapped. You were too clever.

Best wishes

P. E. Torrey

6/18/23

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.

June 18. 1923.

Dear Degener-

Your letter of June 1 is here. Now as you know I don't like to give advice particularly when it involves your coming back to M.A.C. I agree with you that you need a more definite regimen after this 'Bummeljahr' that you have had. Of course one always likes to have an enthusiastic student in one's classes and so I would like to have you come back but I question the wisdom of such a move.

If you have definitely decided on the St Lucia trip then I should think your best move is to get in touch with Britton this Fall, get his interest enlisted and become familiar with his methods. Or I could recommend you to the Pray Herbarium. You would find conditions for work almost ideal and I would be glad to give you a letter of introduction. I'll write you a letter of recommendation to Britton whenever you say.

It seems to me that this scheme would be much better for you than to come to me for more morphology which would have little bearing

on your subsequent movements.

It appears from your letter date that it takes something over two weeks for a letter to reach you. Before you get this you will receive your connected thesis. I should like to see it in print. Either the "Gazette" or the "Annals of Botany" would probably take it. If you care to try the latter you could send the manuscript to Prof. Roland Thaxter, Harvard Univ. He is the American editor of the Annals.

I shall expect to see you as Fall draws near. Now think seriously about either N.Y. or Harvard. Either will prepare you for your St. Lucia trip much better than would anything which I might give you.

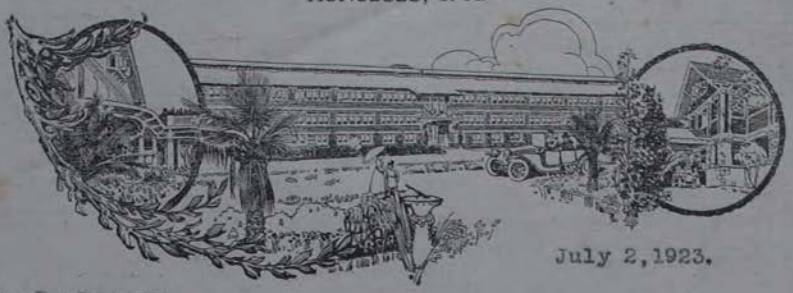
Very truly yours

C. E. Torrey

7/2/23

Cable and Wireless Address:
"PLEASANTON, HONOLULU"

PLEASANTON HOTEL
HONOLULU, T. H.



July 2, 1923.

Dear Dr. Torrey:

I just received your letter of June 18 in answer to mine of the first. I am sorry that our letters have crossed. I sent a second letter on the 22nd explaining "new developments". This letter gave my reasons for changing my mind and cabeling my acceptance for the assistantship. Possibly this letter is crossing one of yours again. I think you had not received my cabel at the time you wrote your letter of June 18. I believe my present move is for the best as things now stand. You may possibly agree with me upon reading this letter and my preceding one.

Before cabeling Prof. Osmun, I cabeled Whetzel asking him to wire date of St. Lucia trip. He did not answer. I am now certain that he is not ready to go this winter. I received a letter from him a few days before I cabeled, saying that he still planned on the St. Lucia trip a year from now. So I can ignore the trip as far as this coming college-year is concerned.

Miss Stokey and I have planned to write up L. obscurum thoroughly. I wish to work out the morphology of the stele of the sporeling as part of this work. This work should be done quickly. First, because Miss Stokey must wait for me. Second, because L. obs. gams. are at present imperfectly known (in spite of Spessard's article - he gives shape of gam. as intermediate bet. saddle- and carrot-shape !!!!). Third, because Spessard may work out sporeling morphology at any time, or anyone else may get the gams. and work out that which Miss Stokey and I should do.

I wish to work out gam. and sporeling of L. pachystachyon. Unfortunately I have not a superabundance of gam. material.

I wish to finish my hermit-crab article so that the Bishop Museum can publish it. This will be more or less routine work. I wish to draw the limb of all the species and paint the entire animal of my new species. Incidentally I have rediscovered my lost species. I had painted my only specimen and was worried no longer about it as I had it on record. It died sometime during examination week and decayed. Later I discovered that a cockroach had licked off all the paint from the picture. Thus the species was lost. A few days ago I collected coral heads and rediscovered the beast in them. I have 7 specimens now - all from coral. There is some symbiotic relationship. This should be interesting.

If I go to M.A.C. for a year, it is a guarantee that my L. obs. work will be authoritative. I have your supervision, and I have any amount of material growing near the college. Then again, I will be better able to collaborate with Miss Stokey in producing a paper fit for publication. If you could supervide my L. pachystachyon report. I would finish my hermit

during my spare time and identify the plants that I have not yet determined. I will also get training in teaching by being your assistant, an opportunity not easily duplicated. I don't doubt but that you will be able to keep me busy.

I do not quite understand what you mean by saying that Britton or Fernald could prepare me for the St. Lucia trip much better than you could. What preparation do I need? I should think that all I need are the plants. Then I would go to New York to study them under Britton. If I do need some kind of preparation for this trip, I could get that next summer- thereafter move to St. Lucia.

I think it would be inadvisible for me to go to the Gray's Herbarium/ Britton seems to specialize upon that region. I would also be able to live with my brother. He has just bought a house near the N. Y. Bot. Garden. I shall rent his garage. The chaufeur's quarters would be ideal for me. There I would be undisturbed.

My L. cernuum paper arrived a few days ago. That correcting is a great help to me. Would I had corrected does not amount to anything. It merely made it passable as far as my degree was concerned. I will write this over after I have been at the volcano. I must verify the temperatures, and try to find the gams. of Ophioglossum and Psilotum. I hate to waste so much of your time with a miserably written thesis and innumerable letters.

I can get no one to photograph my gametophytes again. I have tried 5 photographers. I have written Coulter about it. If he is not in too great a hurry I might be able to have them photographed in Los Angeles in Aug., or by his own photographer in Chicago in Sept. It might be a good plan for me to meet Coulter anyway.

I found a ~~bushy~~ shrubby violet last Sunday. There was only one flower.

Sincerely,

7/31/23

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.

July 31, 1923.

Mr. Otto Degener,
Clark Hall,
Amherst, Mass.

My dear Mr. Degener:-

As recommended by Professor Osmun, I am glad to appoint you as graduate assistant in the Department of Botany; compensation for half time service will be $60 per month from September 16, 1923, or as soon thereafter as you arrive, to continue while you are in service, but not to exceed one year from that date.

Very sincerely yours,

Kenyon L. Butterfield
President.

8/29/23

The Commonwealth of Massachusetts

DEPARTMENT OF EDUCATION

PAYSON SMITH, COMMISSIONER OF EDUCATION

MASSACHUSETTS AGRICULTURAL COLLEGE
KENYON L. BUTTERFIELD, PRESIDENT

AMHERST, MASS.

Aug. 29, 1923.

My dear Boy:

Your letter bowled me over and I dont need to tell you how sorry I am to hear about this piece of ill luck that has come to you. After your letter came I wandered around Clark Hall like a lost sheep all the rest of the afternoon, but then I got to thinking more sanely and began to realize that you are not gone by a long shot. You have had the same thing before and come thru all right and you will do so again. It is quite remarkable how the old physical machine will repair itself if you give it half a chance.

Remember Son, your mental attitude will have a lot to do with the situation. I know this is boot-strap advice. It's so easy to say - dont worry - but it isn't easy to carry out

But really you have little need to worry. Even if you do "shuffle off" a few years earlier than the rest of us, you have to go thru it sometime the same as I have. It is but an incident in life and I believe it is an interesting experience. You have done more good, lackadaisical work in the last few years than most men do in a lifetime.

Believe me - nothing is lost - you have triumphed mightily over a weak physical machine, and over a diffidence and shyness which make social life mighty uncomfortable for you. No young soul could have done it!

There, I'm not writing your obituary - for I am thoroughly convinced that you are coming through all right but it will demand common sense; you must lie low for a time -

I won't permit you to take the assistantship; it is too strenuous and there is too much nervous tension connected with it. Why even our delicate friend, Dick, used to go off the handle trying to educate some of the blockheads. He has actually been known to swear at them right in Lab.

I want you to come back and just go your own pace, work on your plants - you can have the whole herbarium room; section anything you like, read what you will, write up your articles; play with Miss Wallace, take my lectures you want to and if you feel equal to it come to the Lab. sometimes as student assistant. I could put you in nicely to help me out with the boys in Histology - for I have outlined a full year's work starting with staining and mounting Algae and going thru the groups. We shall need your enthusiasm. There won't be any of the disagreeable compulsion and rushing for chapel etc. which attended your undergrad. work.

There the whole proposition is so attractive that I'm inclined to take it myself and give up teaching - the part about Miss Wallace particularly appeals to me!

Dont concern yourself about the graduate assistantship. All write to the Fisk Agency and also set all our department people working on their professional friends and we'll find someone.

A big box came in yesterday for you and also the "Scientific American" arrived with more plants. You have certainly immortalized yourself thru the herbarium. Generations yet unborn shall rise up and call you blessed.

Potter has gone to Maine with his parents on a vacation but he is coming back about Labor Day and will be here till the middle of Sept when he goes to Albion.

I wish I might think of a good plan for you to room and board. It would be fine if you could come to Mrs. Fearings but all her rooms are taken. The Davenport is probably the best place after all. You can eat in the Zoo; it is better than the big noisy dining-room.

What you need is a life free from disturbance and excitement. I'm going to start a flower garden in Mrs Fearings yard this Fall - you can come over and sit out under the trees in a deck chair and watch it develop.

There, you see the situation isn't half as dark as you were inclined to paint it. I can see lots of things for you. For instance we need a good Flora of Amherst with full descriptions, economic notes etc. Perhaps we could collaborate sometime on it.

Dont try to come back this Fall till all your immediate trouble is over.

Yours very sincerely

P. E. Torrey

9/30/23

Roselawn Annex
2517 Kalakaua Ave.
Honolulu, T. H.
Sept. 30, 1923.

My dear Mr. Degener,

Dr. Edmondson brought to me in the laboratory the little book on Dr. Osborn's reply to Mr. Bryan that you kindly sent me. It would indeed be interesting to have the whole controversy in book form, for it may become of historical importance, as Mr. Bryan is probably the last notable opponent of evolution.

I want to pay you for the book or return it to you. Which do you wish? I would rather pay you for it and keep it.

I hope that you arrived home all right and that you are well, and happily engaged at your new occupation.

I am now becoming accustomed to the regular routine of school work and enjoy it as much as ever. We have 56 freshmen in Zoology — the same number we started with last year, hence two laboratory divisions as before. A Histology class of 14 meets Tuesdays and

Fridays. With this I also assist Dr. Edmondson.

Dr. Edmondson upon his return expressed his regret at not having been able to see you before you left. I believe he would be glad to hear from you sometime.

When I passed through the zoo a week ago, I observed the orang-utan clapping its hands, and also slapping the soles of its feet with the palm of its hands, finding delight, apparently, in the loud reports produced. I asked the keeper about that, and he told me it had learned it recently. It is very convincing to me that by watching this beast one can learn much about the infancy of our own race — away back in Miocene times probably.

I was much interested in finding an article in the June number of the "Scientific American," which you gave me before you left, with the heading, "Timing an Ice Sheet." You should read it, if you have not read it. It is on page 387. It tells

as how Swedish scientists by means of annual glacial deposits have established an exact chronology, with the year as its unit, of the recession of the last great ice sheet that covered Scandinavia at the latter part of the Glacial period, giving the time as 13,500 years ago when the uncovering of the most southerly part of Sweden began. Dr. Osborn wrote an article in the March - April num. of "Natural History," emphasizing the importance of the discovery, and stating that these scientists have set the date of Scandinavia's first inhabitants as between 10,000 and 11,000 B.C.

I received a good while ago a kind invitation from the American Museum of Nat. History to become a member, which I immediately accepted by sending in my annual dues. I thank you very much for your indorsement. With the invitation a sample copy of "Natural History" was sent me, the number which contained the above mentioned article of Dr. Osborn.

I have some fresh-water pond snails, Limnaea columensis, I believe it to be, in the laboratory, which have laid a number of eggs. I have not yet observed their whole development but I hope to do so soon. I found that at the time of the larva's release from the jelly in which it develops, it has past through its whole metamorphosis and is almost entirely like the adult, creeping along on the bottom. These snails are air breathers, as you once suggested to me. I am desirous to know whether a rudimentary velum does not occur at sometime in the larva. That is the structure for swimming.

I saw in the papers a few weeks ago that Hawley hotel burned down to the ground.

I told you how industrious with language study I had become. My French which I take at the University takes so much of my time that I have had to drop almost entirely Latin and German. Those French idioms are fierce.

With the best of wishes for your health and success, I remain Yours very truly, J. M. Stagg

11/25/23

The Commonwealth of Massachusetts

Department of Education

Copy c/o Brewer [?] Trust Co Honolulu

Dept. of Botany,
Mass. Agri. College,
Amherst, Mass.
Nov. 25, '23.

My dear Mr. Topping:

Several months ago I was staying at the Pleasanton Hotel in Honolulu. Here I met Mr. Reed Robinson who told me that you had collected plants extensively and had a very large herbarium. I was very eager to meet you and to see your herbarium but as I had but a few more days in Honolulu I was unable to do so.

On my way to Hawaii last year, I passed through the Canadian Rockies. Here I was able to collect quite a few plants. Then I also collected on Mt. Rainier for a few days. I believe I must have duplicates of at least 150 different species that would be fit for exchange. I have also quite a number of species from Amherst. Should you be interested in plants from these regions, please let me know.

I ~~am very eager~~ wish to enlarge my collection of Hawaiian species and I imagine you would prefer to send me those in exchange.

I hope to work out the stelar structure of the Hawaiian Lycopods some time and have therefore collected plenty of material of L. cernuum, L. pachystachyum, L. remotulum and L. polytrichoides. I have none of the other species. Should you have any duplicates of these, I would be very much obliged to you if you would

I am especially interested in the Lycopods and would like to get some of the species that I have been unable to collect. I have only L. cernuum, L. pachystachyum, L. remotulum and L. polytrichoides.

Since it takes so long before ~~I~~ one can receive a reply from ~~you~~ Honolulu, I shall send you a list of my duplicates as soon as possible. ~~Should you desire to trade specimens, you~~ ~~If you do want any of them, you might check them and return the list with~~ Then if you do wish to trade you might check those that you wish and return the list to me with one of your own if that is agreable to you.

Very sincerely yours,

Otto Degener

1/19/24

The Commonwealth of Massachusetts

Department of Education

MASSACHUSETTS AGRICULTURAL COLLEGE AND EXPERIMENT STATION
DEPARTMENT OF BOTANY
AMHERST

C 9/14

Jan. 19, 1924.

Dear Mr. Osterhaard:

I received your letter of Sept. 30 - a terribly long time ago. Please do not worry about that book by Osborn which I sent you. It cost surprisingly little and I have the original article myself. That book may come in handy for you when you talk to students on your good friend Wm Jennings Bryan. I only wish they had printed his ridiculous article in the same book.

Last term I was very busy since 140 Sophomores took Botany and I assisted in all the laboratory sections. There were 4 sections, each one meeting twice a week for two whole hours. I could do hardly anything on my Hawaiian things as I was taking several courses myself. One of them is Vertebrate Zoology. This will continue throughout the year. This term, however, I have no assisting to do at all. I shall get my herbarium

into shape and work on my Lycopods. I have plenty of things to do. Those hermitcrabs with all those drawings would take me over half a years to finish. And I would like to imitate you with your nudibranchs by painting a picture of each one! If I do that I will have to beg you to send me freshly pickled specimens that you happen to pick up on the reef. Mine have already faded to quite an extent.

For Christmas I was in New York. I went to the American Museum of Natural History and saw those Dinosaur eggs. They are about eight inches long and about two in diameter. They are remarkably well preserved. The texture of the ~~egg~~ shell is almost as plain as that of a fresh egg. One egg that was partly broken showed the vitelline membrane very plainly. Another broken egg showed a few fine bones. I guess they were determined in that way.

I must have told you that I expected to go to St. Lucia this coming summer with Prof. Whetzel of Cornell. I cannot go, however, as I must live carefully for a year or so. I wrote him that

The Commonwealth of Massachusetts

Department of Education

I just received a letter from him telling me ~~He just wrote me that~~ that it did not matter as he could go the following year just as well ~~and that~~ He would then go this year to Porto Rico instead. So that satisfies me a lot. I hope I can go the next year. I will try to get fixed up physically this summer. A thorough overhawling; which means the removal of my tonsils and possibly fixing up a sinus in my nose. Shall I send you some adenoid tissue for dissection? I should have some human material at my disposal this summer. Then you can label your microscope slides. "Glandular Tissue from University of Hawaii Student, 1923."

I saw Nathaniel L. Britton during Christmas at the New York Botanical Garden. He is the director. I spoke to him about my wishing to study more taxonomy. He says that I could study under his direction at the N. Y. Bot. Garden and get credit for my work at Columbia College. I think that would be quite a good plan. I could either Monograph one group of plants or work up the flora of one particular

region. When he heard that I had collected a few plants in Hawaii he suggested that I work those up. I think that should be my first problem. Then I could tackle the Hawaiian plants deposited in the New York Botanical Garden herbarium and possibly at Harvard. Of course I could only revise a few small groups at a time. Then the following year I might go with Whetzel to St. Lucia if I am lucky. With those St. Lucia plants I would go back to Britton and there determine them and write a Catalogue of the flora of St. Lucia. That should give me plenty of practice. After that I might be able to work out things by myself more or less. Possibly that could be my Doctor's Thesis. Of course I do not know whether I could ever become smart enough to get that. What I eventually would like to do is to go back to Hawaii and live there for ever and forever. They might accept me as their laboratory assistant in the Department of Botany when I have studied some more. (In fact I do not think I am quite as foolish as I look - but I do look terribly stupid). Then for my spare time I might work up the flora of Hawaii, revising Hillebrand. That book needs it. Why should I not be able to do that if I take plenty of time for it. I can

always go back to Harvard and get help on the most critical work. I guess these are mere castles in the air.

This term I am taking two courses in Botany, a continuation of last terms Zoology, and a course in Historical Geology. Last week I gave a two hour talk on fossil plants in that course. That is the nearest thing to a lecture that I have ever given. That was not so hard as there were but twenty students and I showed forty lantern slides of fossil plants. Then I also had plenty of herbarium specimens of lycopods, of Horsetails, and such other ancient types of plants. Another factor that acted in my favor was that the students were supposed to have a quizz as soon as I had finished. So you see I was the lesser evil.

I am wondering how many queer new things you and Dr. Edmondson have found since I left. Any hermits? It is horrible to live in the north and be cooped up all winter. I never go out except for lunch and dinner. That is all. As long as I cannot collect plants, ~~then~~ I have no incentive to roam the ~~north~~ snow-covered fields. What is Doctor Pollock doing. I wonder whether he has visited the volcano. Please remember me to all. I always meant to write but never get down to do it. Please

remember me to Goto and Hope also. Are they doing any special zoological work? I think I know what those sponge-like animals are in the tanks of the aquarium. Could they not be Tunicates?

Just before Christmas some ministers started to argue about whether Christ was born of a virgin or not. This started quite a war in the Church. I am sorry I could not follow it. But it shows exactly what fools those people are. The enclosed clippings will give you a little idea about it.

How is your pet? (Your vicious finger-biting eel that "never bites".)

You should read the introduction to George Bernard Shaw's "Back to Methusalah." It is quite long, but excellent. You will like it. It is on Darwinism and evolution in general.

I was greatly surprised this evening by seeing on the bulletin board of this building that four scholarships are offered at the Bishop Museum at $1000 a piece. That is fine. If I could not afford to go to Hawaii myself I would try my hardest to get one. But as those that cannot manage to get to Hawaii by themselves go with those scholarships. I will reach Honolulu again some day. You just wait.

Sincerely
Alb Eggleston

2/11/24

Clark Hall,
Mass. Agri. College,
Amherst, Mass.
Feb. 11, 1924.

Dear Dr. Edmondson:

I received your letter ofJan.12. You must have had a wonderful time on those islands east of Hawaii. You wrote that you had sent hermits to Schmitt or Miss Rathbun. As I thought that they would describe and write up any new species from Oahu that might be among that number, I imitated you and sent Dr. Schmit my specimens. I wrote that I was working upon the hermits that I had sent him and asked him if he would allow me to publish on the material that I had assembled. I also asked them to verify my identifications and to let me know whether my "new species" were actually new. I thought this would be the best thing for me to do. Thus I will be able to let you know the names of a few species that I have with me and which you have not got at the Lab. because there is only one specimen of each. I think the one species is P. punctulatus and the other P. spinimanus.

I have just received the answer from Schmitt. Hewwrites that he will determine my material next month. Also that he will furnish me with any additional information or records that might be pertinent. He is going to look over what there is on Hawaiian hermit crabs at the Smithsonian Institution. So you see I am nicely fixed as far as this paper is concerned. He says "It might pay you to look through the Zoological Record subsequent to the date of Alcock's Monograph, though I think very little has been done on these forms since that time."

I am enclosing my hermit key. I must really add another Calcinus to it. The species that is always found in coral heads and resembles C.Herbstii to some extent. There is also another specimen belonging to another genus. This hermit is smaller than a pea and is found in dead coral heads quite commonly. I have left lots of duplicates with you. I should be able to finish my hermit paper this summer and then be able to send you the proof for correction.

The Prof. of Bee-keeping atbM.A.C., a Mr.M.Cassidy whom I know quite well, has become quite enthusiastic about Hawaii. He has read this month's "National Geographic Magazine" on Hawaii and has pumped me a thousand questions about the University, Honolulu,etc. He graduated from here several years ago and then taught school in winter and kept an apiary during the summer. He is now teaching at the College and at the same time working toward his M.S. He wanted to know what opportunity he and his sister would have if they went to Honolulu to look for a position. His sister is a school-teacher. He has been thinking of going to Honolulu for the winter term next year to look for a position. I have advised him to try for that Yale scholarship at the Bishop

3/29/24

Clark Hall,
Mass. Agri. College,
Amherst, Mass.
March 29, 1924.

Dear Dr. Pollock:

My article has just appeared in the March No. of the Botanical Gazette and the editor or some similar person has made an awful blunder. Instead of putting at the end of my paper "University of Hawaii, Honolulu, Hawaii", he has put "Mass.Agri.College, Amherst Mass." I do not know how they could have made that mistake as they sent me a proof and it is correct on that. They sent one proof to me here and one to Hawaii because they did not know where I was staying The proof that was forwarded to me from Hawaii, I am enclosing now a as I have not yet received reprints. As soon as they come, I will send you a copy. I would like to deposite one with my thesis in the University Library.

What should I do about?it? It is part of my thesis at the Univers sity and not my M.A.C. work, although I had gathered the material for it in Amherst. Could I mention this article of mine in my L. cernuum paper and therein say something as follows:"----- as determined at the University of Hawaii and described in my previous paper Then I could give the reference of this article at the bottom of the page. This is a nice blunder. There isxalso the mispelling "bowlder" although it is spelled correctly in the proof. The dictionary gives both boulder and bowlder so that is not really incorrect. But I do not see why they changed those things.

I have written to Dr. Edmondson a few times about my hermits and also to Mr. Ostergaard. I guess they told you what little news there was in my letters. I have decided upon a new move for next year sinc I am unable to go with Whetzel to St. Lucia. Two weeks ago I went to Woods Hole, Mass., to the Biological Laboratory. I looked at the place, and my sister and I looked at some cottages that are for rent for the summer. We have one in mind and will rent it (provided the landlord will come down in her price). Then I will take the course i in Cryptogamic Botany at the Laboratory. Since this takes about six weeks and I expect to stay there for three months, I will spend the rest of my time finishing my hermit crab drawings and mounting the rest of my Hawaiian plants. In October I will go to Britton at the New York Botanical Garden to get training in Systematics. My first w work there will be the identification of my Hawaiian plants. Then I will try to work them up critically under Britton's supervision. I will probably stay at the New York Botanical Garden until the following June or July. Thereafter I will look for a position.

This term I am taking a course in Geology, one in Zoology and odd and ends in Botany such as the History of Botany and the History of Pathology. I am continuing my course under Dr. Torrey on the Evolution of Green Plants. This is the best course of all. As I am graduate assistant here, I am supposed to help in two courses this term. The one is Freshman Botany and the other Cryptogamic. Since there ar

are so many Freshmen and about 40 repeaters, Dr Torrey has two lecture sections and four laboratory sections. Each section has two lectures and two two-hour labs per week. This means 20 clock hours per week for Dr. Torrey. It must be quite exhausting for him. In this course I do very little because Dr. Torrey is trying a new method. He is sick of accepting miserably drawn pictures in lab. notebooks and believes the students waste too much time in drawing. He therefore gives them practically a lecture during their entire laboratory period on the ordinary laboratory material such as seedlings, etc. All I must do up to now in this course is to correct the notebooks and tests, and ofcourse take the role. This is lucky for me as Prof. McLaughlin is teaching Cryptogamic at almost the same ho hours as Dr. Torry has his Freshmen. So after I have taken the Fresh man role I rush in to him and assist in that course just as Brown us used to do with you. We are using Coulter. In Cryptogamic we have on one lecture and three two hour labs. per week. For the lab., we have three sections- in some cases, one lab. follows the other immediately so that we have four steady hours some afternoons. Sometim I feel as though I were getting a permanent kink in my back. A corse should help. In this course I will have to correct notebooks and qui quizzes. Up to now, neither Dr. Torrey nor Prof. McLaughlin have giv en a single test nor required the notebooks to be corrected. I find little time to do anything on my own work. I have not even looked at my gametophytes of that epiphytic Hawaiian Lycopod. I must admit that I am not quite as industrious as I was in Hawaii. I am so swamped with Hawaiian plants that need granting and identifying that I feel hopeless inffinishing them. My trunk has overflowed with specimens so that I was forced to order another Herbarium trunk" ten days ago. It has not yet arrived.

Have you gone to Kilauea? Dr Edmondson wrote me that you often came to the lab. because you were working on your reef problem. I hope you have been at my steam crevices at Kilauea and looked at my gametophyte mine. I want all the suggestions you can give me. I have found that there is a L. vulcanicum. I went to the Gray Herbarium on my way to Woods Hole and looked through the literature and found that this is the synonym of a form of L.cernuum that is wont to grow near fumeroles. So I am finding more facts that I must incorporate in my L. cernuum paper. But I am terribly worried about my "Theoretical Considerations". This is all based on the anti thetic theory of alternations and Dr. Torrey says that the majority of people now believe in the homologous theory. He even says that Bower himself has repudiated his "Origin of a Land Flora" and has accepted the Homologous Theory. I am not yet convinced. I shall send my paper to Miss Stokey of Mt.Holyoke of whom you know, and probably to Rev. Holloway in New Zealand whose articles on Lycopodiu I have quoted. I think I can be on the safe side if I say in my paper that my theoretical considerations would probably follow if we accept the antithetic viewpoint. I must not be too sure in my article. Dr. Torrey's opinion on this has rather quieted me down otherwise I would have been ready to send this paper to the Bot. Gaz. a long time ago.

29-III-24

You I know I would very much like to get a position at the University of Hawaii and stay there for ever, growing up with the department. I dont know whether my acquaintance with the university people will be a help or a hindrance to me when I ask Dr, Dean whether he has use for a neophytic botanist in the Botany Department. The University is growing and a student is required to help Dr. Bergman. It might be just as well to substitute for student help an enthusiastic fool like me, who has no interests outside of Botany, who has no strong family ties, who is not greedy for money, and who should eventually know enough to teach. I have assisted in Freshman and Sophomore Botany so much that I know those courses by heart and could easily conduct the laboratory. I am now assisting in Cryptogamic and will take the course in Cryptogamic Botany offered at Woods Hole. After this intensive training I should know something at least.

What I would like to do is to revise the flora of Hawaii.and try to get it up to date. I shall start with my little collection of plants and at the same time get training under Britton for this type of critical work. What do you think about my plans? Have you any advice to give me?

Kindly remember me to Mrs. Pollock and the other members of your family. My L.obscurum article has at length forced me to write this letter to you which I have been putting off from day to day.

Sincerely,

4/1/24

Clark Hall,
Mass. Agri. College,
Amherst, Mass.
April 1, 1924.

Dear Dr. Pollock:

I have asked Prof. Osmun, head of this department, what I should do about the mistake in my paper. He advises me to send in a note stating the fact. I thought of sending Coulter the following note. This will not appear like an Gerrata" but merely as an addition to the former. Please let me know how this seems to you and write anything else that you think is better.

The paper on "Four New Stations Of Lycopodium Prothallia", was printed in the ? (March Number) previous issue), was written in partial fulfillment ? (for) of the degree of M. Sc., from the University of Hawaii under the direction of Dr. James B. Pollock, Exchange Professor from the University of Michigan. Otto Degener

I shall do nothing until I hear from you about it. There is no special hurry. I do not think it matters if one number comes between my paper and the correction.

Yours,

5/7/24

Clark Hall,
Mass. Agri. College,
Amherst, Mass.
May 7, 1924.

Miss Josephine Tilden,
Dept. of Botany,
University of Minnesota.

My dear Miss Tilden:

Enclosed herewith are two vials containing Bluegreen Algae. I found the dry material in Dec. 1922 growing in steam crevices at a temperature of about 40 °C. near the Sulphur Banks of Kilauea, Hawaii. The Gloeocapsa in the other vial grows in the same type of station except that the maximum temperature is about 55°C. As I am writing a short paper on some lycopods found in the same locality, I am very eager to have these blueggreens identified.

I have sent the same material to Miss Snow of Smith College aand to Dr. Hazen of Barnard. Both are a little hesitant about their identifications and have therefore advised me to send the specimens to you for verification and further identification.

Miss Snow writes that the dry material is probably Hapalosiphon pumilus Kirch. The other vial according to her letter probably contains Gloeocapsa sanguinea Kg., and an interesting form which is either Schizothrix or Dasygloea.

Dr. Hazen, in verifying Miss Snow's tentative determinations, writes "Hapalosiphon is good, but does not appear to me to be H. pumilus for which the accepted name seems now to be H. fontinalis (Ag.) Bornet." With it he finds something that resembles Gloeo-

capsa sanguinea, but as that species is not reported from hot water, he is doubtful about it.

I would be very much obliged to you if you would be so kind as to let me know whether you will have time to identify these specimens.

Very sincerely yours,

5/11/24

Honolulu, T. H.
May 11, 1924.

Mr. Otto Degener,
Amherst, Mass.

My Dear Mr. Degener:-

I am afraid I have not been very prompt in answering your letter of March 27, which, however, was not received for nearly 3 weeks afterward. In any case you said there was no special hurry.

I think the suggestion you made in the second letter, as to the statement correcting the mistake is as good as any and approve it wholly.

If you wish to read me the copy of the reprint to put with your manuscript here, I shall be glad to see that it is done. Make the correction in your reprints before you send them.

I have not yet gone to the volcano, but hope to do so this coming summer. Recently there was a very disconcerting series of earthquakes in Puna on Hawaii, and the earth rose as much as 20 feet or more in places over a strip of country 3000 feet wide and 4 miles long, down to the sea—here, where now some coconut and pandanus trees are in 10 feet of water that were on shore before the subsidence. Following this the lava in Kilauea Iki has dropped down 800 feet and appears to be running out, probably in the direction of the subsidence, but the lava has not appeared at any known place on the

as a result of the fall of lava in the fire-pit great avalanches are falling from the walls, and a fine dust rises from the pit, miles high, falling on everything in the vicinity of the volcano.

I will not offer you any advice as to your "theoretical considerations" for your paper on Lycopodium cernuum, believing that in Dr. Faxey you have a safe mentor. I suppose it would not be possible for you to germinate any of the spores, and try them at different temperatures, to see what is their optimum.

I think your plan for next year is a good one, and think that at the N. Y. Bot. Garden you ought to find all the literature on systematic botany available anywhere. If you go there, look up H. A. Gleason, and remember me to him. He was assistant director, but I believe his status has changed within a year, and I do not know just what it is now. He is interested primarily in some of the Compositae.

If you are inclined to get a "kink" in your back because of so much stooping over to lab students, make it a practice to sit down whenever it is possible, and make it possible nearly all the time. I know from experience that it can be done.

Has your heart action improved since returning to Massachusetts?

Did Metzl give up his trip to St. Louis altogether?

Let me hear from you again, and occasionally in the future.

Yours truly
Jas. B. Pollock

6/30/24

June 30, 1924.

[James B.]

Dear Dr. Pollock:

I am now at Woods Hole. I arrived Friday with one box full of clothes, one compound micrpscope, one portable typewriter, one parcelpost package of blotters, one box full of books and my two herbarium wardrobe trunks full of plants. I have so many plants that I was forced to store a stack between two and three feet high at College. I really need a curator. Luckily my sister is with me. It will be a blessing in disguise if she finds time lying heavily on her hands. The course at the Laboratory begins on July 2. The staff of instruction is composed of Ivey F.Lewis of the Univ.of Virginia, Tracy E. Hazen of Barnard College, and Wm.R.Taylor of the Univ.of Penn. Who really conducts the course and what it will be like, I have no idea. At any rate, I should be strengthened physically as well as mentally because we must catch our algae as well as study them. Another advantage of this locality from a physical standpoint for me is that the food is excellent.

I was really extremely interested in your letter. I read it sever al times when it arrived and then read it to Dr.Torrey and another m man as we walked to the hotel for lunch. With face parallel to the g ground, I was reading your account of the subsidence of the coast as a lady was coming toward us. Not until she was about to pass me did I notice her. I was so startled at this approaching body that I jerked together. This movement and the consequent rattling of your letter scared that lady to such an extent that she squeaked in frigh I have been dreading to meet her on the street ever since.

I have not put any corrigenda in the Botanical Gazette for that mistake. Dr Davis says that most papers have mistakes in them and that I should not draw anyone's attention to it. I do not exactly like to say that I received an M.S. It sounds conceited. What I have done instead, is to have a short paragraph printed that corrects this error. This I can paste into my reprints. You have noticed it no doubt.

For L.cernuum I have changed my concluding paragraph to read as follows:

"This paper was begun at the University of Hawaii in partial fulfillment of the degree of Master of Science. The writer wishes to express his great indebtedness for encouragement and invaluable advice in the preparation of the manuscript to Dr. James B.Pollock, Exchange Professor from the University of Michigan at the ~~institution~~. Thanks are due for similar facors to Dr. Harold S. Palmer of t the ~~University of Hawaii~~ as well as to Prof.A.Vincent Osmun of the Massachusetts Agricultural College. Only through the kindness of Dr. T.A.Jaggar, Director of the Hawaiian Volcano Obervatory, was it possible to illustrate the paper with photographs of the stations. The writer alone is responsible for the views expressed in the text and for all possible errors."

There are two definite questions I wish to put to you.
1. The Bot.Gaz. will not write "University of Hawaii, Honolulu, Hawaii" at the very end of my paper because I am not now at that institution. It will either be "Woods Hole, Mass." or "New York Botanical Garden", according to how soon my paper is published. As I wish to emphasise this article as being essentially a Hawaiian product, could I not place an asterisk at the end of my title, drawing attention to the following footnote at the bottom of the page: "# Contribution from the Department of Botany of the University of Hawaii". Do you think Prof. Bergman would object because my paper does not strictly adhere to facts?
2. I have read several accounts in the eastern newspapers about "Daily Eruptions of Kilauea Present Spectacular Scene". It states that "desert lands of Kau are taking on a new appearance, as is the floor of Kilauea proper." The town of Pahala, some 25 miles away, is covered by ash to a depth of 1/16 of an inch. From reading the Honolulu paper, can you tell me anything more definite about the are in which my stations are located? I believe they must be smothered by ash or boiled to death by additional volumes of steam. I wish to add the following footnote:

"Since the writing of this article, eruptions of such magnitude have occurred at Kilauea that the stations probably have been destroyed. It is reported that a blanket of ash ? inches thick is covering the region for miles around. Thus it should not be difficult to determine more accurately the length of time that must elapse ~~before the~~ for the development of the gametophyte of L. cernuum"

I have sent the bluegreen algae that I found growing with the gams. in heated crevices to about half a dozen botanists. They are not yet determined. One is Gloeocapsa and another probably Hapalosiphon. I would like to mention these in the text correctly.

Another bothering fact is synonymy. I mention several ferns and flowering plants. Hillebrand is out of date so I should really use Miss Robinson's determinations for the ferns. But she unfortunately uses the American code. I do not want to use Dicranopteris for Gleichenia! Then what can I do for Metrosideros, for instance? In Engler, M. polymorpha of Gaud. is not reported from Hawaii. He gives another authority. Rock in his monograph gives an entirely different name. To get my paper off to Coulter I will probably have to say that the determinations for the Vascular plants are according to Hillebrand. I hate to do it.

I have discovered some more references to L. vulcanicum. I am having photostats of the articles sent to me from Washington. This will mean a few more additions to my historical part.

If I can find a photographer at Woods Hole who can take pictures of my gametophytes, I will add two plates. One will show the gams. and another the minute sporelings. This would require a photomicrographic apparatus.

I can get no binocular at Woods Hole so I may have to wait with my hermits until I return to ~~Hawaii~~ New York.

Whetzel is not going to St. Lucia. He is now in Porto Rico.

I am still a little under paar from that infection. I must alway avoid mountains and be careful. That is quite a handicap for an inveterate collector of plants such as I. That means the life of a museum pest rather than a tramp. Sincerely,

When you visit Kilauea, don't undertake climbing

8/25/24

The Commonwealth of Massachusetts

Department of Education

MASSACHUSETTS AGRICULTURAL COLLEGE AND EXPERIMENT STATION
DEPARTMENT OF BOTANY
AMHERST

Aug. 25, 1924.

Dr. William R. Taylor;
Marine Biological Laboratory,
Woods Hole, Mass.

Dear Dr. Taylor:

I have resurrected my few Bermudan algae that I had put away in formalin, and am now sending you duplicates under separate cover.

Would you be so kind as to determine these specimens for me some time this summer. You could then write me the results along with the herbarium number of each specimen. As I have duplicates of all the algae excepting No. B 101, please do not bother to return any except that one. I am sorry that I have such poor specimens to offer.

I hope Dr. Lewis and you will reach Bermuda some day. It is certainly a very interesting place.

Yours,

9/7/24

Jonathan Court,
Rochester, N.H.,
Sept. 7, 1924

My dear Mr. Nyquist:

It is a shame of me to wait all this time, before replying to your long and interesting letter of June 19, which was duly received. I also received your publication on Lycopodium protostelia, on which I congratulate you.

Tomorrow we have registration at the university. How time flies! I hardly realize that our long summer vacation is over. I hope you have had a good summer and done and observed a lot of interesting things; I also hope that you are in good health, and that the physical repair you wrote that you were to undergo this summer, has proven profitable. My health is excellent and I feel full of energy for the coming schoolyears work.

You must have had your hands full during last years last semester, assisting in botanical class work. I expect you must have found the experience profitable. I see that besides botany subjects you took a whole year in vertebrate zoology.

You are surely getting a good foundation in the whole field of Biology. Such is also my hope and ambition, but it will take me many years, for I must go slowly — for the present anyway.

So you don't like the winter, I see; Hawaii must have spoiled you. I can assure you I would be glad to have you back with us again. There are so few, don't you know, that one can really talk to. No doubt you could find botanical work here that would keep you busy; and I should think the University of Hawaii could find a place for you in the botany department.

You are probably looking forward to going to St Lucia next summer. How I should like to make a collecting trip like that.

Dr. Edmondson returned about a week ago from one of the usual expeditions sent out by the Bishop museum. He was gone a little more than a month and the party visited Fanning, Palmyra, Christmas I. and Jarvis I. and did all the collecting time would permit. The party, who made the trip on a government boat, had a very successful and pleasant trip. I mail you the evening paper, so you can read all about it.

Probably you think that I stayed in the laboratory all by myself this summer painting Nudibranchs as usual. Not at. During the latter part of May and the early part of June, Dr. Joe Hartman of Stanford University, assisted by his wife, made use of the laboratory for the purpose of collecting and studying Nudibranchs! This looks like an encroachment on my territory, and so it was. Here was a trained anatomist, who had studied the Nudibranchiate molluscs and their allies for many years and made notable publications on California species and others; compared to a novice like me, without experience and ability. His wife is a skillful artist and has painted in water colors many California Nudibranchs. From her I received some good hints in that sort of work. I compared her water colors, Newton's Winsor, with mine and found them far brighter. She uses a heavy water color paper which she wets and stretches before applying the colors. She did no painting while here, as time did not permit, but she made color notes. I resolved to do

no more painting till I should be able to get a supply of these colors. I could not get them here and perhaps not in San Francisco. They are made in England and there is a duty on them.

I decided to let Dr. McFarland do the taxonomy of the Hawaiian Nudibranchs and I turned over to him the preserved material. I will supplement his work with color plates and notes and observations on the mollusks, as these things have to be done from the living animal. Besides that I have planned to do considerable on the embryology of the species obtained, i.e. that is to get histological sections of all stages of the larva, in order to trace the germ-layers.

You wrote about wishing to paint the hermit crabs and have me send you fresh specimens. It would not be much trouble for me to do that. I have two specimens of the rare Aniculus alive in the lab., both of which I found close together on the town side of the Diamond pool, and both

of them were occupying One shell.

During July and part of August, Dr. W. H. Dall, professor of botany of the University of Calif., and specialist on marine algæ, made use of our laboratory for collecting and drying specimens, his wife accompanying him and assisting him with his work. They spent the earlier part of their vacation in Tahiti, collecting.

Dr. Dall was keenly interested in reef formation — coral reef ecology — ancient and recent — and devoted much of his time to the study of the limestone formations of Oahu.

He secured my services, much to my delight, for collecting and determining the fossil mollusks. By auto we reached all limestone exposures on the island and made collections of the fossil plants and animals, which in several places were very abundant. I finished my report on the work assigned me about a week ago. I collected 61 species of Gastropoda and Pelecypoda, and identified them all but 3. One is known to be extinct, and the 3 unidentified, which I am

sending to a specialist, may also prove extinct. I don't think these elevated reefs are older than pleistocene. You have no doubt noticed them on your trips.

The other day the university sent out to the laboratory two fine steel cabinets for me — just what I needed. I am now going to have our laboratory collections placed in these cabinets and labeled up according to the latest taxonomic changes.

Perhaps you read in the papers about the Pan-Pacific Food Conservation Congress held in Honolulu this summer. Dr. Josephine Tilden of the University of Minnesota, expert on marine algae, made use of our laboratory with a group of co-workers, during the latter half of the summer vacations. She started to work out an economic problem — the food supply of food fishes. You will therefore see our laboratory has not been standing empty this summer.

You were asking about the finger biting eel. What do you think the last thing did? He crawled out

of the trough once too often. One morning last December, when I came to the lab, I found him rigid and dead on the floor. I could have cried, for I had become so attached to him. Dr. Edmondson also felt bad about it. We have him preserved in a jar.

Mr. Hoyt went away to Oregon Medical College this fall. He was a bright lad and a hard worker, too.

Dr. Pollock [James B.] left for his home college [Instructor] by the middle of August. He took his year off to the study of nest-building agencies. I expect he will publish the results.

Your newspaper cuttings concerning the "Virgin Birth of Christ" were interesting. These poor ministers have their hands full. Some time ago Rev. Palmer of the Central Union Church of Honolulu, in one of his newspaper sermons stated the attitude of the modern Bible student, simultaneously defending the recognized truths of evolution. This brought forth an array of attacks from the fundamentalists, some denouncing him as unfit to be a minister of the Gospel, others attacking the "evolutionists" with bitter sarcasm. But others advanced

good arguments, considering their ignorance against evolution. It was a one-sided argument, until one day a brief and very appropriate reply appeared from a geologist on the staff of the Bishop Museum, in which he complimented the Honolulu people for having the services of a man with so broad a vision and so clear an understanding, who had the foresight to discern what steps were to be taken in order to save Christianity. This ended the attack; nothing more was said.

I believe I told you once how many languages I was studying. Well, I have come down to only one - French. I took it last year and shall continue this year. I should rather spend all my time on zool., but I want French.

Mammalian anatomy will be given this year, so I have an opportunity to review the muscular system of the cat. Lots of fun!

Dr. Edmondson sends his regards to you, and receive from me the best wishes and sincerest regards.

Very truly yours,

Jas. M. Ostergard

Pickwick Arms,
Grand Concourse,
New York City.
Sept. 27, 1924.

Dear Mr. Ostergaard,

I was shocked to receive the announcement of your vicious finger eating pet's death. Will you ever be able to get a su substitute? If you do, you will have to make your tank fool proof.

I know exactly how you must feel about your Nudibranch work. Dr. McFarland's has both its advantages and its disadvantages for you. May I be so bold as to say what I believe the disadvantage to be? If you and Dr. McFarland do not take proper precautions in publishing your work, your own researches on the Nudibranchs will be entirely eclypsed by Dr. McFarland's name. The trouble is that he has already become known through his articles, while you are unknown to the Nudibranch-loving public in spite of the fact that you are the one that has been piling up a huge amount of data and has been painting the Hawaiian species for years. What I don't like in your letter is that you say "I will supplement his work with color plates and notes and observations." Please do nor suppliment his work but be his collaborator. Some day I want to read Ostergaard and McFarland on "The Nudibranchs of the Hawaiian Islands." Your colaboration with Dr. McFarland will mean that your article will becocompleted much earlier, you will be able to check up each others errors, and you will have help in the terribly difficult task of writing a large book.

You seem discouraged about your painting. You know perfectly well that your illustrations are as accurate as anyone can make them. The difference between your painting and a professional artist's painting is that it takes you more time to do the same thing. I spoke to a Mi Miss M.E.Eaton about your work. She is the lady that paints flowers that are beautifully reproduced in the National Geographical Magazine She uses the colors you wrote about. She told me that you should use paper called "Imperial." The best size for you, I believe, would be 22 by 30 inches and it should be "Cold Press." She also told me that for the work that you and that she is doing, it is merely waste of time to wet and stretch the paper. You need only do that if your pan painting is comparatively large. The stretching and wetting is to prevent the paper from buckling and wrinkling when it becomes wet when you paint. I enclose a sample of the paper for you.

I have been spending my time quite profitably. When college close[d] my sister and I went to Woods Hole, a town on the tip of Cape Cod. The Marine Biological Laboratory is situated here. At this institution, you can rent offices for research, buy supplies of all kinds, or take courses in Zoology or Botany. I took the course on the Morphology and Physiology of the Algae. The class consisted of about 20 students and Dr. Hazen of Columbia, Dr Ivey Lewis of Virginia, and Dr. Cleland of Rhode Island as teachers. These last are all excellent men in their respective fields. Class would begin around half past eight with an hour's lecture followed by laboratory work until 12. At 2 o'clock we would continue the laboratory work, and end at half past four. I then returned to my room and wrote up my lectures or mounted my plants. Quite often my sister would help me to mount them. So now I have almost all my Hawaiian plants mounted, excepting my duplicates.

The course was very thorough and therefore probably somewhat elementary. The trouble is that I had taken two courses on the algae at college several years ago, and assisted in an Alga course during the Spring before going to Woods Hole. But by taking this course I have a more comprehensive idea about the algae and have also become familiar with many more species. I was also able to collect quite a lot of algae and mount them for my herbarium. We went to different islands for collecting on the laboratory steamer. We took such a trip about once a week. I enjoyed the course very much.

When the course ended at Woods Hole, I went to Martha's Vinyard, an island in the neighborhood, for a week. It was very difficult for me to find lodgings here. Finally, however, I met an old Indian lady and gentleman who took me for the week. They were very nice people and I would recommend everyone to go there to stay. During that week, I collected quite a lot of plants, including Poison Oak. In carrying it home in my vasculum, it must have touched my wrist. From there I got the poison on my face. The result was that I was a mess and felt messy and dirty too. I could not shave and did not dar go to a barber to have my hair cut.

At Martha's Vinyard I could have collected any amount of fossil wood from the Cretaceous Period. I gathered a vasculum full of it. Some day I hope to section some of it just to see what it is like. I also found two shark's teeth enclosed in a calcareous matrix, and a bone.

After my Martha's Vinyard escapade, I returned to Amherst for a few weeks. There I strai htened up my affairs and completed the copying of my notes. From Amherst I returned to New York and moved into an apartment near Bronx Park with my sister. This is about ten minutes' walking distance from the New York Botanical Garden where I am now studying. I have decided to devote this entire year to my Hawaiian plants and hermits. I have therefore registered at Columbia University as a candidate for a Ph.D. My thesis will be the revision of the flora of Hawaii. To accomplish this I must compare all my own Hawaiian specimens with the material in the herbarium of the New York Botanical Garden and, in general, study all the Hawaiian material that I can lay my hands on. Last week I checked up Miss Robinson's revision of the Hawaiian Ferns. This was herethesis from Columbia at the New York Botanical Garden. In comparing her specimens with those that I had collected, I discovered that I had two ferns that have new never been recorded from Hawaii before. I

I am afraid that I will not be able to finish this revision in on one year. You know how slow I am and how little experience I have on such work! Naturally since the number of Hawaiian specimens at the Garden is somewhat limited, I am unable to study the range of variation of the different species. What I must do here, then, is to get trained in how to approach my subject . Later when I am back in Hawaii, I will have access no doubt to the Bishop Museum collections.

This question of knowing the range of variability of species has just bothered me today. I collected a liliaceous plant at Kilauea. It is Astelia veratroides. In the Garden herbarium they do not have this species but two plants that were collected by Heller. One of these was labelled Astelia argyrocoma n.sp. I found, however, that this A. argyrocoma is the same as A. Menziesiana. Now in looking at my Kilauea specimen and Heller's argyrocoma, I have obviously two different species before me. Hillebrand in his flora, however, says that Menziesiana is a variation, not a variety even, of veratroides. How can I check this up definitely without more specimens? To complicate matters still more, Heller's second specimen, which comes from the top of Konahuanui, is not labelled at all. It differs entirely from all other Hawaiian species and must be new. I am going to describe it provisionally as new and then verify my interpretation of these plants when I am back in Honolulu.

I am getting along with my hermit work rather slowly. I finished the drawings of another. Thank you very much for offering to send me hermits to paint. For the present, I have given up the idea of painting them. It would take too much time away from my botanical work. I must see to it that Columbia will count this year's work as a full year's residence work. If they count it as that, I can leave for any place on earth to work "on my own hook" and still receive my degree from Columbia. But if they do not count this year as my residence year, then I cannnever get a degree no matter how much original work I may do later unless I come back to New York.

Why don't you write a more or less popular article upon the Hawaiian Nudibranchs? Imitate Miss Eaton. Write something about their protective or warning coloration, about some of them giving off ink, or about some that grow living plants in their mantle. Then illustrat

your paper with your paintings. I am sure a magazine such as the National Geographical or the Natural History would accept it. The National Geographical would pay a better price. You might also write a more or less popular article upon your boxing crab, sponge crab, Holothurian intestine-inhabiting crab, etc. I am sorry I have not a similar opportunity in the botanical line.

I am looking forward to seeing you, Dr. Edmondson, Judge Banks, a and the others a year from now (if everything goes well). I plan to return to Hawaii as soon as I have completed my work at the Garden, whether Dr. Dean can use a second man for Botany or not.

Please pickle any of the hermits that you have and that happen to die. Just throw them all into a jar. They have very little Hawaiian hermit material at the Smithsonian and it might be just as well that I send your crab contributions to them when I have worked up my mater ia and labelled it correctly.

Since you are now a fluent French scholar, I shall close with

Au revoir,

9/27/24

12/10/24

Pickwick Arms,
3224 Grand Concourse,
New York City.
Dec. 10, 1924

Dear Dr. Lossey:

I received your letter of Nov. 5 and quite agree with what you wrote. I did not get Kempis' book as I still feel "fleshy" and like to sniff around at Material Nature. But when I have sniffed my fill and have grown up I really think I would read it as well as similar works and absorb & amalgamate a little of them into me. I did get "Tertium Organum" as you let me get it - so to speak - the last time I saw you. I have read a third of it and rather felt it was not entirely new to me but just common sense. The reason for that is your talking at and sometimes with me on a part of that subject. But after the first third of the book I became confused, did not follow it as before or felt as though I were disagreeing with part of it. This last is probably because I could not quite follow. I showed it to one of my sister-in-law's sisters, the only one in either of our two families that really thinks. She grabbed it eagerly. She is now in Michigan to stay for an indefinite length of time at some religious convention. She is a Christian Scientist, but a free-thinking one. Before leaving she made sure that my s-in-l. would return Tertium Organum to me. We reminded each other of the book when last I was at my brother's house, but at the last minute of my stay both of us promptly forgot about it.

I wrote President Dean again, and received the reply that a position in Botany at the University of Hawaii was a certainty for me but that he "cannot tell yet, of course, what the finances are going to be like for the next biennial period." He also wrote Dr. Gregory of Yale, the Director of the Bishop Museum, about my plans. Dr. Gregory saw me at the New York Bot. Garden yesterday and blushed to the effect that the Bishop Museum is now working upon a revision of Hillebrand's Flora to bring out a Flora Hawaiiensis (exactly the opposite of what I learned from Elmer Merrill of the Philippines whom I met here and who according to Gregory is cooperating in the revision of the Flora.) Then I learned from Gregory that cooperation with the Bishop Museum for me would no doubt be desirable, etc., etc., that exchanges with the N. Y. B. Garden are much to be desired, etc., etc., it might be a good plan to send someone to Berlin to look at Hillebrand's types etc., etc., and whether that should be done immediately or should be postponed until near the end of the study etc., etc., that Yale has scholarships with the Bishop Museum etc., etc., these scholarships amount to $1000 per year etc., etc., too bad that there is such a dirth of young men interested in Botany and that these scholarships have not been filled, etc., etc., could I go immediately or would it be more profitable for me to work at the Garden until summer etc., etc., did I ever get to New Haven where he is living etc., etc., et cetera - - - - - - - - - →

7/24

Honolulu, T. H.

Mr. Otto Degener,
Woods Hole, Mass.

Dear Mr. Degener:

Your manuscript on Lycopodium cernuum was received, I don't know exactly how long ago, and your letter of late June 30, not very long ago, perhaps a week.

Dr. Birgenau offers no objection to your "contribution to the Dept. of Bot. of the Univ. of Hawaii," so there is no reason why you should not do as you suggested on that score. I cannot tell from the newspaper reports just what has happened to your stations, but suggest that a letter to Dr. Jaggar, who is back in Hawaii now, would give you reliable information.

I find in looking in Rock's Indigenous Trees of the Hawaiian Islands that he used the name Metrosideros polymorpha Gaud. with 12 synonyms, 3 sections and 10 forms, distinguished by Greek letters. He also recognizes 4 other species and one variety of Metrosideros. If you do not have this book you ought to have access to it.

I have a feeling that you build a rather large structure of theory on a rather small

-1-134

paris of observed facts. How does Dr. Torrey regard the theoretical discussion? Your observations are suggestive of the reason why the sporophyte and gametophyte are not generally abundant in the same localities, but as you do not really know what either generations can endure, outside of a probable optimum environment, I question the value of so much theory as you bring into your article. However, it is yours, and if you can get it accepted for publication it is up to you.

By the way, I will return the manuscript to you.

Have you definitely decided that you will be at the N. York Botanical Garden next year?

I have had to let up on the pace I have been going, and it looks now as if I would not get to the volcano at all, though I have a chance to go on the "Kukui" with the superintendent of lighthouses on his inspection trip around Hawaii. [an lay-off work completely for a week is inserted above the line]

This P.M. I saw Edmondson & Bergman, and a group of other men off on the mine sweeper Whip-poor-will, for a "Tan. Expedition" to Jarvis, Fanning, Christmas and Washington Islands, the first named being south of the equator. I did not envy them much.

Yours truly, Jas. B. Pollock

1/15/25

Waikiki Aquarium

(1)

New York Botanical Garden;
Bronx Park, New York City.
Jan. 15, 1925.

W. deC. Ravenel,
Smithsonian Institution,
Washington, D. C.

My dear Mr. Ravenel:

A few days ago I mailed five vials of hermit crabs to Dr. Schmitt, four of which Dr. Schmitt and you had kindly lent me for study April 25. These hermit crabs, I believe, belong to Dr. Charles H. Edmondson of Honolulu. He has just written me that he would like to have them again as soon as possible because he has just about completed his manuscript on his "Tanager Expedition" catch except for description of these specimens.

In case Dr. Schmitt is away from Washington and does not know of Dr. Edmondson's need, could you arrange matters with him so that Dr. Edmondson must not postpone the publication of his paper. The package should be sent to Dr. Charles H. Edmondson, Bernice P. Bishop Museum, Honolulu, Hawaii.

I am very much obliged to you for lending me this material.

Very truly yours,

13125

R. E. TORREY
AMHERST, MASS.

Feb. 3, 1925.

Dear Degener:

For days and weeks I have kept intending to write to you and yet the deed never before got done. First of all I'm grateful for the Gnetum specimen which came through as fresh as a daisy. At first I was inclined to press it and add it to the herbarium but later I decided to keep it in formol so that the "kids" may see the seeds in place and not wrapped up in a paper packet for they probably would tumble off. By the way what is the species?

Well, I trust you are well and happy in your own way - getting much desirable butter (cane finger prints) rich cream without sediment in it), tropical fruits, poi, pate-de-foie-gras, caviar, mint juleps, roasted grouse, etc. etc. In addition to that I picture you seated into a room with mountainous piles of Carices, Junci, Crataegus and Composites (I'll throw in a few Astragalus and Paronchia for good measure) just squealing with delight.

Well, let me tell you the news:

Twelve young innocents out of 25 weathered Bot 58 and proceeded with 59. Slowly but surely they are being warped and twisted into those peculiar creatures known as systematic botanists. I saw Prof. Goodale of Amherst College yesterday; he is just as crazy as any of them. He dashes up to a metal case, flits gaily among the shelves and lifts some a genus cover with a flourish - opens it

with a graceful sweep and steps back to get my reaction. There lies a corpse of Saururus cernuus collected in Amherst in 1860±. And of course I have to play with the little boy so I say: Gee, that surely is a find - why even Prof. Fernald doesn't know that it comes north of Rhode Island. "Oh yes he does", says this dear little man "I wrote him about it"

Then he hops to the cabinet again, stands on tiptoe and yanks out more mummies. He runs his finger lovingly over a yellow label: "Francis Boott" he breathes in reverent tones. Well, we played like that a good half hour and when I left I had the promise of a lot of Cakile duplicates (the poor fellow alas, fell off the East Boston Ferry and was drowned) Really O.D. they should never go out without a keeper - lucky you have your sister.

I can't seem to escape the disgrace of Systematic Bot. Even Sid Hackell sneers at me as a Systematic Botanist - me - Me - ME. who am a really honest-to-god Coniferoxylopalaeontologist.

Miss Cyner is mounting the Crane collection as fast as I can fasten names on the specimens. I have dumped your friend Rydberg into the dust-bin and use only Coulter and Nelson who have the credit of never making 2 spp. where one could possibly do. When you become well versed in the 250 000 spp. of plants you can come around and split up all our species.

John Perry is trying to carry on though the poor chap is far from well. and ought to go to Arizona. Today

he is in Boston to see a doctor. He had a haemorrhage from the lungs a few days ago - nice isn't it?

Today has been a day of visitors for me. This morning Sammy White dropped in. The firm he was working for went into bankruptcy so Sam is out of a job. He intends to teach. (It is the final resort of all derelicts - like the Sargasso sea - where one accumulates barnacles and seaweed).

This afternoon Bob Woodworth appeared. He likes Williams fine. Kept full both he and Church are going to Jeffrey to be made into real ~~fighting~~ guys. Bob was here today to get his orchestra to go to Williams to play at the "Prom".

Last of all Ashley dropped in so my day has been, as you see, a round of hectic pleasure.

I've left the Davenport and Mrs. Fearing has taken me in hand trying to fill out the hollows in my "wicket." My gizzard is improved; I no longer use cracked oyster-shells but find sharp pebbles much better to triturate my pabulum.

I hear you took Woody to the opera - I guess you wrote me yourself that it was light and "flirty."

Little is seen of "Theodora" any more. He now works downstairs under Andy's direction raising onion smut. Dr Davis rushes around with tubes of agar and Petri dishes just as ever. His room is full of rotten cabbages and other dead things lying in state under glass jars and wreathed in blue penicillin halos. It's a wonderful science. One thing about systematic botany - it doesn't stink when properly handled.

Orton still reads German books and talks about food; and his students extract chlorophyll from leaves and watch the bubbles come off Elodea (not Ella-dear please understand - she has left us along with the other Sophomore "Schwalbens".

Saw Thompson yesterday - the same rolling, smiling happy-go-lucky. He is studying Zoo. now. He said he didn't see everything he was supposed to and the smell of those pickled earthworms made him sick. That was the main thing he remembered about the work. Williams is much distressed because his grades have been falling - no more women for him. To hear him you would think he was a young Don Juan repenting from his sins.

Forgot to say that another visitor today was "Pinkie" Futterman who visited with me an hour during Orton's Lab. Pinkie has no job yet (he is waiting for the Pathologists here to find one) - he is taking Crampie's Ent. 90 and is quite thrilled. Crampie has decided that we came straight from Chimpanzees - I'm dreadfully glad it is finally settled so conclusively.

Did you follow the "Margani" case to its sad finish? Houdini is an ass apparently and didn't even play the part of a gentleman. He accuses them now of using "black magic" to destroy him.

Well son, I've gabbled merrily on till it's time to go to bed -

Pax vobiscum and write

Some day. We all enjoy your letters

P.S. Big Chief "Rain-in-the-Face" P. E. T.

Dept. t. Walter King the library

9/17/41

Hvon f the chew - Ho. O. P.

Tel Elk 0802 .

22 Fairfield Ave
San Francisco
Calif.

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Dear Mr Degener (C D)

Very pleased - proud was I to receive your fine book yesterday , t,is so handsomely set up & on starting to read it very soon found that was the product of a man who loves his work . Most thoughtful of you to send me so desirable a gift . It is now on my shelves at - Singalang where on days of leisure i can so greatly enjoy what one may rightly call Treasures .
Your book came some time ago - but I have been working out of town - polishing up professionally for what maycome , could not have access t mail till I returned.
When the Junk docked at Honolulu I remained there for four days before taking the Lurline - h ad some drives thru the island & hear tha you were still absent , your house was pointed out to me .
John saw Dr Coulter on the Junk & after the 2d day no one except the ships company could come aboard .
You will have heard the aftermath of the cruise , because the beans tha the Piccaroon did,nt spill the Skipper did .
The outstanding man of the Junk was Mr Raval - he lost he loyalty , in fact the control of his men because of his patience with & apparent devotion to Mrs A .
For my part the cruise was a success - geographically . Rarotonga was a corking island to see , we allhad a jolly time there , How John revel led in the Hupa- hu pa (shimmyings) & very hearty was the Europ hospitality accorded to us . Would say that the Maori admixture is s something to write home about .
The only real achievement of the Expedition was your plant- gather ing on the islands of Fiji , or so Mrs A said .
WE were in Tahiti for six weeks - I took the 8- mile walk to the famous Falls of Fatua & on Sundays usually went with Mr Raval & the " boyce " to replenish the Junk galley .
Now & then I hear from Tucker , John & I becam,me very good friends he is now on a newspaper job in Washington . Soon after you left us a gret many letters came for you ,John took particular care to re post them in one parcel - we h ope that y ou received them all .
All of tthe Philippino crew were en route to t heir islands the day I sailed , & the Samoan sailors accompanied myself & baggage to th liner & played- blew a fine Conch shell till Aloha ...
Again - my pleasure at receiving Plants Hawaii National Park is indeed great - when I see tucker I shall lend it to him .
Must say a word about our fine Pey (Admirable Chrichton) at Pago he left the Junk & returned to his people but not before giving us a days memorable entertainment at his village ofAallo , his uncle is a Chief there .
Very special greetings to your Emilio & always the best of everyth thing to yourself.

Mary Keegan

Mary Keegan

5/20/30

Volcano, Hawaii,
May 23, 1930

Dear Mr. Degener:

I had not forgotten your request for that kukui but because of too many reasons to write here, have not had a chance to go to the place in Kona where I heard my plant came from. It meant searching along a road. But I suddenly got inspired as to whom the woman was who knew of their presence over there. (I guess it is whom after all!) I rang her up and she told me she had a tree in her yard grown from nuts she got in Kona, and that I could have all the specimen material I wanted. She said she had a tree from nuts from Kauai that was different from the common ones here. Anway, I finally got to her place. She was absent but I got some specimens from two trees, which I am sending you this same mail. There was a third tree in a gulch and not so accessible but it looked like the Kona type with its indented leaves. I can get some from it if you wish it. I'll see her again some time.

I am in an awful rush to catch the mail, so if I do not get the nuts started, you'll get them later. Do the male and female flowers grow in a separate cluster? I read it up after I got home, so didn't look for different flowers. I was hurried then, too.

Haven't had a chance to make an expedition up the side of Mauna Loa from Kumuula to get the exocarpus. That means a two day trip and getting horses.

The nuts are numbered 1 and 2 like the leaves. Excuse haste. Much aloha to you and your sister.

Isabel Jaggar

[Wife of Dr. Thomas Jaggar, vulcanologist.]

We believe we mailed you years ago a sheet of Aleurites remyi (or A. moluccana var. remyi), Euphorbiaceae. This xerox may be appropriate for inclusion in sheet folder.

O. & I.

11/5/24

R. E. TORREY
AMHERST, MASS.

Nov. 5. 24

Dear Theo

We all laughed over your very characteristic letter and fully appreciated the white elephant piano, the Swedish Professor who stammers, and the artistic lady with the misanthropic mind. Yes, most botanists (pro botanists) are freaks if we consider that "normalcy" is manifested by the 90,000,000 morons who constitute the backbone of this glorious democracy (which last is one of those half truths so dear to the Mencken school).

"Theodora" is correcting papers this afternoon; I have finished my "stint" and have been averaging grades. As usual we can give a prominent contribution to the Deans Board; there are thirty "below" and 12 "low". But these marks come from a quiz on wood anatomy: that explains much.

Your present employment doesn't sound wildly exciting but here is where the German in you will have to assert itself. Such work ought to be done and I know that it can hold one's interest as a mass of troublesome facts and species begin to flow into an organized system.

Please remember me to your sister. I trust she is

developing that much needed calm and does not get upset over your perturbing characteristics.
Some Sunday she might like to go to the Chapel at 10 Horatio Street for the service. I still feel that in some way her road lies in that direction. To be frank, you are not ready yet - you have much to learn about yourself. But I think your sister has earned the right to something very great.
You asked for Rev. Woodworth's address. It is 110 Manhattan Ave; why not look him up?
Dr. Crampton wants to be remembered to you. He amuses me by his conventionally polite attitude. Poor man, I haven't the least shadow of dislike for him; he is just a hot-head and a bit dogmatic.
Potter was up for the Amherst game. In spite of the fact that he is struggling with vertebrate zoology which he knows "nothing about" his rotundity is increasing and he looks prosperous.
Pue Lindskog comes on well at Harvard Medic and I gather that he has begun to stand out already.

Was up at the Stones last Sunday eve. Dr. is well but Mrs. S. is suffering from conjunctivitis due to infection from the dust which has been swirling up the Amherst streets lately. When one stops to consider what that dust represents one

wonders that anyone escapes infection.

I see that the article on Lycopod seedling anatomy (which you were to prepare) has appeared in the Oct. "Bot. Gaz." It was done under Chamberlain and is of his usual stone-counting character.

Well now — what ought I to say to you more. Most of the above is relative and temporal. The small study group of seven meets with me each Friday night (when mass-meetings etc don't interfere). We are working on Plotinus and neo Platonism. The boys do extremely well and I am pleased with their ability to intuit a region where the mind simply can't enter. It is far more important that they should understand than that they should load the mind with encyclopaedic information. As Thomas à Kempis says: "What value is it to dispute learnedly of the Trinity if thou art displeasing to the Trinity?" Or again: "In the last day it shall not be asked of thee how much thou hast read, but how well thou hast lived." Perhaps you would find Thomas à Kempis' "Imitation" valuable, though I'm afraid you would be repelled by its Christian terminology.

In conclusion let me quote you a fine verse of Swinburne's I ran across lately. It is a little

but rescinnetic but I like its rythm and alliteration.

"From too much love of living,
From hope and fear set free,
We thank with brief thanksgiving
whatever gods may be
That no life lives forever,
That dead men rise up never,
That even the weariest river
Winds somewhere safe to sea.

Pax vobiscum

P. E. Torrey

The New York Botanical Garden's stupendous importance reflecting upon me seems to have wrought such a favorable impression about me to Dr. Gregory, a geologist, that I feel like a counterfeit coin which will be eagerly handled until a real botanist inspects it, shows its value — then I'm dropped. It were far better if they thought that I knew less than I really know. At any rate I can see the in for with dimension the offer of a scholarship, especially since the Bishop Museum does not pay it but Yale while the work will be purely Bishop Museum clerico-botanical work. With the filthy lucre from this, from the University and from my own canned supply I will at least be able to climb the highest mountains by taxi or Rocky Mountain Canary if I can't use my legs to carry myself plus impedimenta like blotters, an oil stove, a press, a 20 pound rock to put upon it, those same mountains. Thus I can continue to sniff at Nature until its present delightful perfumes through constant contact with my senses seems to change to an abominable stench. Then the pendulum may swing back and I may set a match to my herbarium collection containing every plant on earth. With all these foreign entangling chains gone, I might be satisfied to anchor myself out on a raft in the center of the Sargasso Sea and meditate upon our embryonic soul.

As I still have one or two jars of pickled material some place in Clark Hall and as I still owe Mr. Ball something for laboratory supplies and genius covers, I will be forced reluctantly to return to Amherst before moving with bag and baggage to my Paradise of the Pacific.

In the meantime I wish you Merry Christmas and the other conventional wishes — I also hope you will accept Mrs. Davenport's next invitation to have Christmas dinner with her and her relatives. Is there any more pitiful sight than a college professor with a dislocated stomach eating his sparse and lonely Christmas dinner? Nevertheless I hope you are eating your victuals again.

I saw Woody Woodworth a few weeks ago. As I had two tickets for the Opera, we went. It was the Rosen kavalier — rather stupid, flirty play.

Yours,
Alb Degener

I hope Eladora is still single. If not, please remember me to both her and Theadora.