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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.

MAX-PLANCK-INSTITUT
FÜR ZELLPHYSIOLOGIE

BERLIN-DAHLEM
GARYSTR. 32

Dr. H. David Hammond
410 West 110th St.
New York 25, N.Y.

March 5, 1956

Dear Dr. Hammond:

Thank you for your kind letter of February 5th, which was forwarded to me to Berlin, where I am spending a sabbatical leave of absence in Prof. Warburg's laboratory.

Your ideas about the possible role of carotene in the electron transport during photosynthesis are very interesting. I cannot express any opinion about them, however, until they can be experimentally tested. Despite my long interest in the possible role of carotene in photosynthesis, I personally have been unable to give it concrete shape, so far, by devising suitable experiments.

You might be interested in some recent views of Prof. Warburg on the role of carotinoids in photosynthesis (Z. fuer Naturforschung 9b : 667,1954)

Sincerely yours,

Daniel I. Arnon

Daniel I. Arnon

THE BROOKLYN INSTITUTE OF ARTS AND SCIENCES
BROOKLYN BOTANIC GARDEN
1000 WASHINGTON AVENUE
BROOKLYN 25, NEW YORK

March 25, 1955

Dear Dr. Hammond:

Yours of late February is before me and I have been trying to think of situations where your training and experience might be most helpful. As a matter of fact, we could well use your talents here but, unfortunately, we have no openings in your particular line.

Have you thought of writing to Dr. Ray Allen at the Kingwood Center, Mansfield, Ohio? I would think from what you tell me of your work that you would be extremely useful on Dr. Allen's staff. If anything else comes to mind you shall surely hear from me.

With my very best wishes.

Sincerely,

George S. Avery, Jr.

Dr. H. David Hammond
261 Harvey Street
Philadelphia 44, Pennsylvania

RUTGERS UNIVERSITY
The State University of New Jersey
COLLEGE OF ARTS AND SCIENCES
February 11, 1952

DEPARTMENT OF ZOOLOGY — NEW BRUNSWICK, NEW JERSEY

Mr. David Hammond
Department of Botany
University of Pennsylvania
Philadelphia 4, Pennsylvania

Dear Dave:

By all means send us an abstract of your thesis results and make it long enough to be informative in regard to the principle results obtained. We have no precise limits on space and would be glad to give you (and M.A.J. also) whatever space you need to call attention to this work.

Sincerely,

Alan A. Boyden
Director of the
Serological Museum

AB:jh

Dr. Siegfried Bellartz

Goethestraße 83
Köln-Marienburg
April 12th, 1957

Dear Dr. Hammond !

Please accept my sincere thanks for your writing and interesting publication I recently received. Immediately after the receipt of your letter I sent you a reprint of my paper in Planta 47. I hope you have got it meanwhile. I have not included the carotinoids in my investigations, for I didn't know whether those compounds play a part in pollen-tube growth, but I thank you very much for your precious hints. Perhaps the differences in pollen-tube growth really depend on those substances, and I shall go on researching on this way.

To my regret I haven't other papers on this domain of study, but I shall ask my teacher Dr. Linskens to send you some of his publications on the general subject.

Very truly yours,
S. Bellartz

UNITED STATES DEPARTMENT OF AGRICULTURE
FOREST SERVICE
PACIFIC NORTHWEST FOREST AND RANGE EXPERIMENT STATION

ADDRESS REPLY TO
DIRECTOR
AND REFER TO

423 U. S. COURT HOUSE
MAIN STREET AND SIXTH AVENUE
PORTLAND 5, OREGON

RS-NW, REGENERATION
Seed Studies
Collection & Distribution

Wind River Experimental Forest
Carson, Washington
July 16, 1948

Mr. H. David Hammond
U. of Pennsylvania Botanical Laboratory
38th St. and Woodland Ave.
Philadelphia 4, Penna.

Dear Mr. Hammond;

In accordance with my promise of last December, I have collected some Vancouveria seed as you requested. The Berberis seed is not yet ripe, but when it is I will get some of that also. Since you said that you were interested in all the genera of the Berberidaceae, I have gotten some seed of Achlys triphylla. There is not as much as you at first said you needed; but the seed crop has for the most part already fallen.

Hope there is enough here to be of use to you. We have had a peculiar spring and summer so far; and many of our herbs do not have good seed crops this season. However, there will be plenty of Berberis.

Sincerely yours,

W. E. Bullard

W. E. Bullard
Officer in charge

UNITED STATES DEPARTMENT OF AGRICULTURE
FOREST SERVICE
PACIFIC NORTHWEST FOREST AND RANGE EXPERIMENT STATION

ADDRESS REPLY TO
DIRECTOR
AND REFER TO

423 U. S. COURT HOUSE
MAIN STREET AND SIXTH AVENUE
PORTLAND, OREGON

Carson, Washington
August 24, 1948

Mr. H. David Hammond
Botanical Laboratory
University of Pennsylvania
38th st. and Woodland Avenue
Philadelphia 4, Pa.

Dear Mr. Hammond;

Enclosed are the thirty grams of seed of Berberis nervosa you requested earlier. These seed may still be somewhat green; but I hope they will be satisfactory. They run about fifty seeds per gram.

As to a favor in return, I would like a separate of your paper on this taxonomic problem when you write it up for publication.

If you do not know of the many wonderful properties of the fruit of B. nervosa; I will hasten to enlighten you. It seems to be full of pectin, for it sets a fine jell with apple or other fruits. The acid bleached the linoleum on our kitchen floor; while the pigment dyed our aluminumware a bilious maroon purple. Considerably diluted and with plenty of sugar added, the juice is a good substitute for grapejuice - which is why the common name of Oregongrape. It is also a good gin mixer. It peps up the flavor of canned fruit and berry jams to add B. n. juice, too. Now you know all.

Sincerely yours, W. E. Bullard

W. E. Bullard
Wind River Expt'l Forest

UNITED STATES DEPARTMENT OF AGRICULTURE
FOREST SERVICE
PACIFIC NORTHWEST FOREST AND RANGE EXPERIMENT STATION

ADDRESS REPLY TO
DIRECTOR
AND REFER TO

423 U. S. COURT HOUSE
MAIN STREET AND SIXTH AVENUE
PORTLAND, OREGON

RS-NW, REGENERATION
Seed Studies
Collection & Distribution

Wind River Expt'l Forest
Carson, Washington
15 December 1947

Mr. H. David Hammond
U. of Pennsylvania Botanical Laboratory
38th St. and Woodland Avenue
Philadelphia 4, Pa.

Dear Mr. Hammond;

As Mr. Briegleb stated in his letter to you of December 4, it is too late in the season to get any seed from Vancouveria hexandra or Berberis nervosa. It is too bad your request didn't come in August or September, as at that time I was collecting quarts of the barberry for jelly-making purposes and had plenty of seeds left over. However, if it will do you any good then, I will collect some seed for you this coming summer. Perhaps we had both better set up some promise cards to remind me.

There are several large clumps of Berberis Aquifolium growing on the campus of the U. of Washington in Seattle; and you might be able to get someone there to find you some seed. Possibly E. C. Moran of Stanford, Montana, who is a commercial seedman, could supply you. Before the war, the experiment station at Berkeley, California was doing quite a bit of work on native shrubs and had seeds of just about all of them. You might write to C. J. Kraebel, CF&RES, 331 Giannini Hall, UC, Berkeley 4, California, and see what they have.

Of course, it's none of my business; but I'm curious. Could you tell me a little about your project?

Sincerely yours,

W. E. Bullard

W. E. Bullard

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO

DEPARTMENT OF HORTICULTURE

Dr. H. David Hammond
Dept. of Biological Sciences
University of Delaware
Newark, Delaware

Dear Dr. Hammond:

There is a position open at the Un. of Nebraska - Lincoln, Nebraska in Horticulture but requiring a Plant Physiologist. In fact it is the job I had before coming to Colorado. Administratively things were not good at that time but within the past month a new Department Chairman has been hired & ~~~~ things may work out much better there. Good facilities were available including growth chambers and other control rooms, a laboratory etc. There are no strong Horticultural interests in the state to force work on any particular crop. You could write to Dr. H.O. Weiner, the present acting head of the department & mention my name.

Your comments about potato tuberization —

were most interesting. I shall look into the possibility of a connection between immunological data on various Solanums and the ability to produce tubers. Has your work been published?

Sincerely yours,

Harold W. Chapman

Harold W. Chapman

NEW YORK STATE COLLEGE OF AGRICULTURE
A UNIT OF THE STATE UNIVERSITY OF NEW YORK
CORNELL UNIVERSITY
ITHACA, NEW YORK

DEPARTMENT OF BOTANY
PLANT SCIENCE BUILDING

April 5, 1958

Dr. H. David Hammond
Department of Biological Sciences
University of Delaware
Newark, Delaware

Dear Dr. Hammond:

Thank you for your letter of April 3, expressing interest in our instructorship vacancy.

We are looking for a man with strong teaching interests to take over the laboratory portion of our beginning course in botany. His duties will include the planning and organization of the laboratories, some record keeping, the supervision of the work of six undergraduate teaching assistants and direct participation in the program to the extent of two or three sections per week. The incumbent will work closely with Dr. H. P. Banks, who gives the lectures in the course. While we encourage research, I must say that this is considered to be practically a full time teaching job.

If the situation as I have briefly described it appeals to you I would be glad to have your transcripts and letters of recommendation. We have already received a number of applications for the position and I hope we will be able to arrive at a decision by June 1.

Yours sincerely,

Daniel G. Clark

Daniel G. Clark
Acting Head

DGC/kh

NEW YORK STATE COLLEGE OF AGRICULTURE
A UNIT OF THE STATE UNIVERSITY OF NEW YORK
CORNELL UNIVERSITY
ITHACA, NEW YORK

DEPARTMENT OF BOTANY
PLANT SCIENCE BUILDING

May 16, 1958

Dr. H. David Hammond
Department of Biological Sciences
University of Delaware
Newark, Delaware

Dear Dr. Hammond:

In response to our recent country-wide search for an instructor to teach elementary botany we received more than a dozen applications, a number of which were from men with truly outstanding qualifications.

After much deliberation we finally decided to choose a man whose particular background and teaching experience seem to fit our special needs most closely. He has been offered the position and has accepted it.

We greatly appreciate your courtesy in applying and assure you that we will have you in mind to notify should a vacancy of possible interest to you develop in the future.

Yours sincerely,

Daniel G. Clark

Daniel G. Clark
Acting Head

DGC/kh

MONTANA STATE UNIVERSITY
MISSOULA

DEPARTMENT OF BOTANY

March 10, 1958

Dr. H. David Hammond
Department of Biological Sciences
University of Delaware
Newark, Delaware

Dear Dr. Hammond:

Some time ago we hired Dr. Otto L. Stein to fill the position of cytologist in our department. He has been at the Brookhaven Laboratory for about two and a half years and is now on his way here to report for duty at the beginning of our spring quarter.

We are presently in the final stages of selecting another new staff member, who will probably be in the field of plant ecology.

I hope you will be successful in finding a position to your liking.

Sincerely,

R. A. Diettert

R. A. Diettert, Chairman
Department of Botany

RAD:sm

PHILADELPHIA COLLEGE
OF PHARMACY AND SCIENCE
43RD STREET, KINGSESSING AND WOODLAND AVENUES
PHILADELPHIA 4, PENNSYLVANIA

FOUNDED 1821

July 1, 1958.

Mr. David Hammond,
Univ. of Delaware,
Newark, Delaware.

Dear Dr. Hammond:—

I should like to thank you for your interest which you have shown in inquiring about the vacancy we had in our zoology staff.

However, the position of instructor in zoology at the Philadelphia College of Pharmacy and Science has been filled.

With best wishes for your future success,

I am,

Sincerely yours,

Marin S. Dunn

Marin S. Dunn
Professor of Biology

Philadelphia College of
Pharmacy and Science

TELEPHONES
SLOANE 7151
" 1716 (DEAN)
" 9433 (SECRETARY)

St. GEORGE'S HOSPITAL MEDICAL SCHOOL
(UNIVERSITY OF LONDON)
HYDE PARK CORNER
LONDON, S.W.1

Dr. S. D. Elek.

18th December, 1951.

Dr. David Hammond,
Botanical Laboratory,
University of Pennsylvania,
Philadelphia 4, Pa.,
U.S.A.

Dear Doctor Hammond,

Thank you for your most interesting letter of the 29th November. As a matter of fact you have given me an idea which may solve a difficulty with which I have been faced for some time. I have been working with various flocculating systems using the standard tube technique and I have been trying to obtain two different systems which give a single, narrow flocculation zone. So far in our hands, at any rate, the only system which has done this has been a diphtheria toxin-antitoxin system; all our efforts to find another system have failed. That is why I have been most interested to read about the results you obtained using an antigen from Delphinium consolida. I wonder if it would be presuming too much on your kindness to ask you if you could supply me with some of this antigen and its homologous antiserum. This would be of considerable assistance to me in enabling me to proceed with a projected line of research on the fundamental nature of the so-called alpha and beta optima in flocculating systems.

Yours sincerely,

Stephen D. Elek.

TELEPHONES
SLOANE 7151
" 1716 (DEAN)
" 9433 (SECRETARY)

St. GEORGE'S HOSPITAL MEDICAL SCHOOL
UNIVERSITY OF LONDON)
HYDE PARK CORNER
LONDON, S.W.1

Dr. S. D. Elek.

13th November, 1951.

Dr. H. David Hammond,
Botanical Laboratory,
The College,
University of Pennsylvania,
Philadelphia 4, U.S.A.

Dear Doctor Hammond,

I very much regret that I have no reprints left of the paper you wrote to me about. However I am enclosing some reprints dealing with various applications of the method in case it is of interest to you.

Yours very sincerely,

S. D. Elek.

EVANS & REEVES NURSERIES

"It's Different"

255 South Barrington Avenue, Los Angeles, 24 California
Telephones: ARizona 3-6528 -:- BRighton 0-4604

March 7, 1950

Univ. of Pennsylvania
38th & Woodland Ave.
Philadelphia 4, Pa.

Attn: Mr. H.Davis Hammond

Dear Sir:

In response to your letter of the twenty-seventh, I regret that we have no seed of Crossosoma, nor do I know where it might be obtained. I am forwarding your letter to Mr. Eustace Rush, 1014 West Olive Avenue, Burbank, California, who is a thoroughly reliable seed specialist.

Yours faithfully,

EVANS & REEVES NURSERIES

Morgan Evans

me/lmg

THE UNIVERSITY OF TENNESSEE
Memorial Research Center and Hospital
Knoxville

October 4, 1958

Dr. David Hammond
158 E. Main Street
Newark, Delaware

Dear Dr. Hammond:

You are long overdue my promised letter following our interview at Bloomington, and have undoubtedly made plans for the coming year before now.

Nevertheless, you might be interested in work at the Research Center next year. If so, please contact Dr. Stanfield Rogers, Director, sometime next summer.

The budget for this year did not allow the addition of another investigator.

With best wishes,

Herman S. Forest

Herman S. Forest

HSF/hb

UNIVERSITY OF ILLINOIS
DEPARTMENT OF BOTANY
URBANA, ILLINOIS

June 10, 1955

Dr. H. David Hammond
410 West 110th Street
New York 25, New York

Dear Dr. Hammond:

Thank you very much indeed for the reprint of your interesting paper which I had read when it appeared in SCIENCE. I am glad to have it for my files and I am most appreciated of your kindness in sending it to me. In reciprocation, I am enclosing a reprint in which you might find something of interest.

I have made note of your change of address.

Yours sincerely,

Harry J. Fuller

Harry J. Fuller
Treasurer, Botanical
Society of America

HJF:ems
Encl.

UNIVERSITY OF ILLINOIS
DEPARTMENT OF BOTANY
URBANA, ILLINOIS

February 4, 1955

Dr. H. David Hammond
261 Harvey Street
Philadelphia 44, Pennsylvania

Dear Dr. Hammond:

This is in reply to your long and interesting letter of January 17 about our shortcomings in the training of scientists. I agree obviously with all that you have written but I remain pessimistic about the possibility of achieving some of the ends which you have suggested. When one considers, for example, that in the average large American university such as my own, excellence in teaching is regarded as a distinctly second class activity and that the writing of research papers is regarded as the most important activity of faculty members, then I think a pessimistic viewpoint is not illogical. The excellent teacher of science must have the time, the leisure, and the opportunity to study and read and write on subjects outside his immediate field of research, for excellence in teaching is in part based upon a knowledge of the broad implications of his field and of the relations of his science to the areas of philosophy, economics, sociology, and history. With the premium placed upon research productivity, there are few scientists who can become the kind of broadly educated men who are needed in our universities to fulfill the moral and ethical objectives of science. This attitude is illustrated by the experience of a colleague on the grounds that "there is too much speculation in your paper; a scientific paper ought to consist of the reporting of facts". With this type of attitude so prevalent, how can we break out of the hollow sphere in which we find ourselves?

Nevertheless, I shall do what I can to bring to fruition your suggestion of a symposium at an AIBS meeting on this subject. I shall do a lot of effort-bending to bring about the institution of such a symposium, but I warn you at the outset that I am not overly optimistic about the possibilities of our success, for I believe that you and I are in a distinct minority in our views on the implications of science.

Nevertheless, I have enjoyed our correspondence, and I look forward to a meeting sometime with you over a bowl of mead to pursue this discussion. Thank you for taking the time to write me so interestingly.

Yours sincerely,

Harry J. Fuller
Professor of Botany

HJF/dp

UNIVERSITY OF ILLINOIS
DEPARTMENT OF BOTANY
URBANA, ILLINOIS

January 15, 1955

Dr. H. David Hammond
261 Harvey Street
Philadelphia 44, Pennsylvania

Dear Dr. Hammond:

Thank you very much for your letter of December 19 with its interesting and stimulating comments upon my recent paper in Science. I was glad indeed to learn of your support of my major thesis. I have been astonished by the flood of letters which have poured in on me as a consequence of the publication of my experimental results. Thus far 82 people have expressed agreement with me, three think that I am an outmoded old fossil. The score is fairly decisive so I am reasonably happy.

I agree with you entirely about the kind of questions which should be included in every Ph.D. examination, that is, questions concerning the broader aspects of a candidate's thesis work. One of the questions I usually ask of Ph.D. candidates is to give an explanation of the results in such a manner that the work would be understood by any intelligent layman who has not been a specialist in the candidate's own field. It has been a source of amazement to me to learn how few Ph.D. candidates can give a logical, non-technical, and simple explanation of their contributions to science. Part of the difficulty, of course, lies in the lack of acquaintance of many of them with their mother tongue, part of the difficulty stems from the fact that many of them have regarded science simply as the accumulation of facts and have never appreciated the broader relationships and implications of their work.

Your criticism that my questions did not get at the heart of the matter, of course, is a valid one and I accept it cheerfully. The idea of trying this came to me on the spur of the moment during a particularly dull Ph.D. exam. Once having started this experiment, I decided to continue it in the same fashion. Obviously it was not a carefully planned battery of questions, but I think that the results demonstrated are of some interest. I agree entirely with your suggestion concerning the inclusion of courses in philosophy in our graduate curricula. I am not certain that I agree with your approval of courses in the history of science. My experience has been that such courses are commonly organized on a merely chronological basis and that they are thus narrow and dull. I think perhaps that we should weave the historical aspects of our discipline into our science

subject-matter courses. Admittedly the most effective method varies with the professor.

Thank you again very much for your interesting and provocative letter, and for the trouble which you took to write to me. If you are ever in these parts, drop in and we'll go on from this point.

Yours sincerely,

Harry J. Fuller
Professor of Botany

HJF/dp

TELEPHONE: SELLY OAK 1881-5
- - 0340

UNIVERSITY OF BIRMINGHAM

DEPARTMENT OF EXPERIMENTAL PATHOLOGY,
THE MEDICAL SCHOOL,
THE HOSPITALS CENTRE,
BIRMINGHAM, 15.

PROF. JOHN R. SQUIRE.

PGHG./PG. 24th April, 1956.

Dear Dr. Hammond,

Thank you very much for your letter and reprints. I have already read your work in the Serological Museum Bulletin, but I am very glad to have a personal copy.

As regards our work here, I think we were attempting rather different things, in that we were able to measure ~~intrageneric~~ interspecific differences, while you were concerned with intergeneric differences. I do feel however that the quantitative aspect of the nephelometric technique is perhaps a little delusive, since it is never clear what one is measuring. We have found that our extracts always contain a number of different antigens and that the differences between species are partly due to quantitative differences in concentration, partly to cross-reactions, and partly presumably to the presence of quite independent antigens. This applies to tuber proteins, and also to leaf-proteins as far as we have tried: we have not worked with Solanaceous seed protein. Once one has identified the various components of the reacting system one can proceed to a quantitative estimation, either by a precipitation method, or by a gel-diffusion method such as that of Augusti and Hayward (Int. Arch. Allergy, 1955, 6, 134), or the recently published method of J.G. Feinberg (Nature, 1956, 177, 530), which we have found promising.

I will send you a reprint of the Nature paper as soon as ~~they are~~ it is available, and also of the more detailed publication when it appears.

Yours sincerely,

J. G. H. Gell

H.D. Hammond, Esq., Ph.D.,
The Long Island Jewish Hospital,
270 76th Avenue,
New Hyde Park,
LONG ISLAND, N.Y.

UNIVERSITY of PENNSYLVANIA
PHILADELPHIA 4

The College

BOTANICAL LABORATORY
38th and Woodland Ave.

October 6, 1954

Dr. H. David Hammond
261 Harvey St.
Phila. 44, Pa.

Dear Dave:

I will look over your paper, which is unfortunately at home, and give you my opinion concerning your figures.

Prints I am not certain that Roman will have time to make your ~~negatives~~ for you or not, but I believe that he can do them on an overtime basis. The Department will be quite happy to pay the costs involved.

If we cannot arrange to have Roman make the prints here we will pay for them being done outside the Department.

With best of luck.

Sincerely,

David R Goddard

David R. Goddard

DRG:dl

THE UNIVERSITY OF WASHINGTON ARBORETUM
SEATTLE 5, WASHINGTON

November 4, 1947

Mr. H. D. Hammond
Assistant Instructor in Botany
University of Pennsylvania
Botanical Labratory
48th St. and Woodland Ave.
Philadelphia, Pennsylvania

Dear Mr. Hammond:

We are pleased to receive your letter of October 13 regarding seeds of the Berberis Genera.

Under separate cover I am sending two of the Mahonia, one is Mahonia bealei and the other is Mahonia nervosa.

We do have one or two Berberis species in the nursery and could send you seed, however, that may be hybridized naturally so will not send any of those at this time. If you would care to have some of the seed we will be glad to send it on to you.

Yours very truly,

Robert J. Hansen

Robert J. Hansen
Assistant Superintendant

RJH:P

UNIVERSITY of PENNSYLVANIA

PHILADELPHIA 4

The College
Division of Biology

June 6, 1958

Dr. H. David Hammond
Department of Biology
University of Delaware
Newark, Delaware

Dear David:

I have this day recommended you to Dr. L. R. Crane of the State University of New York for appointment as instructor in floriculture and ornamental horticulture.

Perhaps you should drop him a note and a vita.

With my best regards.

Sincerely,

David R. Goddard

DRG/rds

BARD COLLEGE
ANNANDALE-ON-HUDSON
NEW YORK

Sept. 3, 1958

Dr. David Hammond
158 East Main St.
Newark, Delaware

Dear Dr. Hammond,

I regret to report that we have filled the position in biology at Bard. The decision was made last friday. I held off making a decision until the last minute in the hope that someone at the meetings such as yourself might be interested and call me. Classes begin at Bard Sept. 8, so I had to make a decision at the end of last week.

I trust you will find a suitable position in the near future.

Sincerely

George B. Hooper

George B. Hooper
Asst. Professor Biology

RUTGERS · THE STATE UNIVERSITY

GRADUATE SCHOOL June 20, 1960 NEW BRUNSWICK, NEW JERSEY

Mr. David Hammond
8802 Glenville Road
Silver Spring, Maryland

Dear David:

I was indeed pleased to have your recent letter and to know that you are in good health. It is my hope that you have completely escaped from the cancer difficulty.

Mrs. Johnson and I are embarking on a trip around the world next Thursday. We will have an opportunity to visit many of the places that botanists read about but never have an opportunity to see. Some of these are Honolulu, Japan, botanic gardens at Hong Kong, Bueitenzorg, Java, also Peradeniya in Ceylon. I am bursting with half truths and undigested information from guide books and the usual sources that one consults for such a trip.

Write me some time next fall and it might be possible to arrange a visit to Washington and a seminar for your graduate students. We have a son in Washington so we frequently come down to see how the grandchildren are making out.

I shall touch up Boyden in regard to the Serological Museum Bulletins and also send you the single brief reprint on the grasses. Unfortunately, we are experiencing considerable difficulty in immunizing rabbits but results from the antisera which we have are very interesting and I believe we will have something to contribute to taxonomic relationship.

Sorry to write you in such haste. As you can well imagine, I am trying to get a very cluttered office in shape before taking to the air.

With best wishes,

Sincerely,

M. A. Johnson
Dean of the Graduate School
and Professor of Botany

MAJ:ah

Feb. 22, 1957

Dear Dr. Hammond:

I am grateful for your interest to my trifling treatise to the Fam. Solanaceae. Because I am studiing on the pharmacognosy in the pharmaceutical institute, I have been interesting to the Solanaceae for the relations between the plant classifications and the components of this family, which include many medicinal or edible plants, and had tried to introduct my own plans ~~in~~ to the classification of this family.

Almost of these plans resulted my experimental datas are not yet published, but as soon as I carry out, I will be present to you with pleasure all my heart.

If you need to the studiing on the classification of this family, I will prepare to present some of the living seeds or blotting plants

of this family which collected in Japan, especially natural in this islands.

sincerely yours,

Hisakichi Kimura
木村久吉

assist. Prof.
of Kanazawa Univ.

Seeds of Ranunculaceae and Berberidaceae

collected in 1948

by

M. Kumazawa
Biol. Lab. Daihachi-Kôtô-Gakko
Mizuhoku, Nagoya, Japan.

Hisakichi Kimura,
Laboratory of Pharmacognosy
Institute of Pharmacology,
Faculty of Medicine,
Tokyo University,
Hongo, Tokyo, Japan

Apr. 16, 1948

Dr. H. David Hammond
Botanical Laboratory
University of Pensylvania

Dear Sir :

By your letter I was informed that you have expanded your field of study to include the Ranunculaceae. At present the seeds of any species except Nandina are not in my hand nor in any seed supplier's, but on one hand I will endeavour in this summer to collect the seed of some species myself and on the other hand I will let the suppliers do this.

I believe that you have already received the seed of Nandina domestica, which was sent to you the other day via an American civilian.

Among the genera cited in your letter, Myosurus and Knowltonia are neither found in wild nor cultivated in Japan, and Hydrastis is an American plant, being rarely cultivated in this country.

As stated in my former card, almost all my reprints were lost by war in my laboratory, but seven kinds of reprint were got back again from my collagues and were sent to you at the same time with my first card. The envelope containing reprints has not been sent from any office back to me till now. But it seems to me very doubtful whether you have received them, because I sent

VIA

them to you, being ignorant of the fact that the personal exchange of printed matters between occupied Japan and foreign countries had been prohibited by the directive of U S Army. The prohibitation ~~is~~ was, however, ~~now~~ removed in Feb. 26, so that I will send you once more, if some of my reprints are got back again from any of my collagues.

Sincerely

M. Kumazawa

Masao Kumazawa

Professor in Biology

UNIVERSITY OF RHODE ISLAND
COLLEGE OF ARTS AND SCIENCES
KINGSTON, RHODE ISLAND

DEPARTMENT OF BOTANY

September 26, 1958

Dr. H. David Hammond
158 E. Main Street
Newark, Delaware

Dear Dr. Hammond:

I am sorry that a series of incidents have prevented me from writing to you sooner.

We decided not to hire a man for the first semester. We will, therefore, consider applications for either the second semester or for next year.

I shall keep the resume which you provided for me and will consider that you wish to continue as an applicant.

Sincerely yours,

Robert Lepper, Jr.

Robert Lepper, Jr.
Head--Department of Botany

RL:p

THE BROOKLYN INSTITUTE OF ARTS AND SCIENCES
BROOKLYN BOTANIC GARDEN
1000 WASHINGTON AVENUE
BROOKLYN 25, NEW YORK
TELEPHONE: MAIN 2-4433

November 20, 1947.

Mr. H. Ward Hammond
Botanical Laboratory
University of Pennsylvania
38th St. and Woodland Ave.
Philadelphia 4, Pa.

Dear Sir:

Your letter to Dr. Avery has been handed to me for reply.

We do not have seed of the genera which you request in your letter. We have not been in the practice of collecting seed recently, except when we know in advance and can collect it before the seed is dropped or the plant tops destroyed in our fall cleanup.

The three following companies may be in a position to supply some of the seed that you want - certainly the Mahonia and Nandina are commercial plants, and surely would be available:

F. W. Shumacher
Jamaica Plain 30, Mass.

Herbst Brothers
92 Warren St.
New York 7, N. Y.

Rex D. Pearce
Moorestown, N. J.

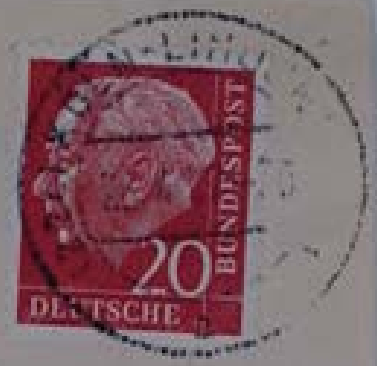
Yours very truly,

Conrad B. Link

Conrad B. Link
Horticulturist.

CBL:evm

5M-4-45



Mr.
Dr.H.David H a m m o n d
41o W 11o th st
N e w Y o r k 25 N.Y.

USA

Prof. Dr. Linskens

BOTANISCHES INSTITUT
DER UNIVERSITÄT KÖLN
KÖLN-LINDENTHAL
GYRHOFSTRASSE 15-17
FERNSPRECHER NR. 41 12 21 / 475 und 468

Sehr geehrter Herr Kollege !

Soeben höre ich von meinem Kollegen und Mitarbeiter Dr. Bellartzki, daß Sie an Problemen der Physiologie der Inkompatibilität interessiert sind. Mit gleicher Post schicke ich Ihnen meine einschlägigen Arbeiten. Ich wäre sehr daran interessiert von Ihnen die dieses Problem betreffenden Publikationen zu erhalten.

Mit vorzüglicher Hochachtung

Ihr

H. Winkler

25.4.27

Dr. Alicia Lourteig D. L. L.
MUSÉUM NATIONAL D'HISTOIRE NATURELLE
LABORATOIRE DE PHANÉROGAMIE
57, RUE CUVIER, PARIS-V^e · Tél. : GOBELINS 30-35

Paris, 6th november 1955.

Dear Dr. Hammond,

Your reprint on Systematic Serological Studies in Ranunculaceae for which I am very much obliged to you has been kindly forwarded by the Gray Herbarium.

Although I knew it (through your unpublished Thesis) I read it attentively. I must say that it is a pity it has not been published in full. Many of most interesting details are missed.

I am sorry, I cannot offer to you a reprint of my 2nd part of my South American

Ranunculaceae with two genera you do not know, because it has not been published as yet.

I am working in this huge museum since the beginning of the year and shall stay here for a time.

If I can be useful to you from here do not hesitate to let me know.

With my thanks and best regards and wishes

Sincerely yours

Felicia Courtois

Smithsonian Institution

Department of Botany,

Washington, D.C.

18th. july 1953.

Dear Dr. Hammond,

Thank you very much indeed for your kindness. I was longingfor reading your Dissertation for it is an exciting approach of the subject.

I have read it thoroughly . As I got my Doctor 's Degree in Biochemistry, in the Medical Sciences Faculty of Buenos Aires, I am able to understanding your experimental work since I am familiar with this sort of research.

Sorry indeed that it has not been published, for I would like very much to have a copy for my own use. Although I have made a card and some notes of the work you are perfectly aware of the many details that though interesting cannot be retained by heart.

I have sent to you a reprint of my first paper on Ranunculaceae. The second, dealing with the taxa of the rest of the South American countries is now in progress and practically finished. Surely, I shall keep your name in mind for letting you have a copy of that second part. There you will find my treatment of the big Ranunculus of the highest Andes of Perú (that have been described under other genera) as well as of Laccopetalum.

Dr. G. Erdtman has studied pollen samples of almost all the South American species at my requirement and with the samples I have sent him named by me. It has been a real interesting experience since in general his results would group the taxa in about the same way I would do taxonomically. It was of particular value with the Barneoudia and Oreithales genera for which I had strong reasons to keeping then separated from Anemone. Laccopetalum has a different pollen from Ranunculus too. I am dealing with ten genera since Delphinium is only cultivated.

For Ranunculus (your paper p. 56 and 58) I do not think that under

the modern criterion the swarm of segregates does subsist. All these for South America are being reduced by me. Batrachium is only a section according to the modern papers (only one occurs in South America that is as Ranunculus in my paper).

Certainly, Laccopetalum must be kept immediately next to Ranunculus because of the many characters that relate both genera. However, the numerous nectaries scattered on the petals as well as the absence of a scale and the structure of the pollen grains give the evidence of a different genus. At the moment, I have neither fruits of it nor of Hamadryas but their obtention is not out of my possibilities. However, I must say that that will take a long time for both genera occur in localities not easily reached. So that, I wonder whether you will continue interested on the subject.

For Anemoneae (p. 70) I should suggest you the inclusion of Barneoudia and Oreithales. Nevertheless, for the former you might have the base of my paper already published, whilst for the latter (overlooked by the monographers and the Index Kewensis) you have not arguments as yet. This are included in my present draught. One of its synonyms is Capethia Britton . Yet, according to Dr. Erdtman's studies (unpublished!) the pollen grain of Barneoudia would be "polyporate". In the case of Oreithales is 3-colpate but different from those of Anemone.

When I was working in Kew I had the opportunity of hearing about some African plants. In general, the idea is to keep Knowltonia as a separate genus whilst some other minor genera would be reduced.

I must congratulate you for your paper and the way it has been carried out.

I am in contact with a young student in New Zealand who is doing research on the Ranunculaceae of his country. He started, as far as I can understand,

from the citological point of view. When I shall hear that he has published anything, I shall suggest your name in order he will get in touch with you.

I am returning your dissertation under a separate cover, next monday.

I am leaving Washington in a few days for coming back to the Gray Herbarium of Harvard University where you can reach me anyway.

I shall be delighted to supply you with any data you might think of any value. If you need the translation into English of any part of my paper, just let me know.

With my thanks for your courtesy and kindest regards,

I am

yours sincerely,

Alicia Lourteig

/A licia Lourteig

UNIVERSITY OF SOUTHERN CALIFORNIA
UNIVERSITY PARK
LOS ANGELES 7, CALIFORNIA

DEPARTMENT OF BIOLOGY

April 9, 1958

Dr. H. David Hammond
158 East Main Street
Newark, Delaware

Dear Dr. Hammond:

Thank you for your letter of April 2, 1958 in regards to a position on our staff. At this time we have nothing available.

We appreciate your interest in our university.

Sincerely yours,

W. E. Martin

W.E. Martin
Professor and Chairman

WEM:nw

UNIVERSITY OF CALIFORNIA

DEPARTMENT OF BOTANY
BERKELEY 4, CALIFORNIA

3 February 1948

Mr. H. David Hammond
Botanical Laboratory
University of Pennsylvania
38th St. and Woodland Ave.
Philadelphia 4, Pennsylvania

Dear Mr. Hammond:

Somehow, your letter of December 4th became buried and has just now come to my attention again. There are several people in California who specialize in native plants and who might have seed of the berberidaceous genera which you wish. I don't know how accurate the field data would be, however. Following are the addresses of the main ones:

Carl Purdy, Ukiah, California
(A son is now carrying on the business)

Theodore Payne
2969-99 Los Feliz Blvd., Los Angeles 26, California

Lester Rowntree
Rural Route No. 1, Box 170, Carmel, California
(Probably your best bet)

We will add the genera that you wish seeds for to our "Want List," and pick up any that we are able to during the coming season.

Sincerely,

Herbert L. Mason
Professor of Botany and
Director of the Herbarium

HLM/e

Yoshiharu Matsumura,
Nikko Botanical Garden,
Nikko, Tochigi-Ken,
Nippon.

Nikko, Nippon. April 21, 1950.

Mr. H. David Hammond,
University of Pennsylvania,
Botanical Laboratory,
Philadelphia 4, Pa.
U. S. A.

Dear Mr. Hammond, Your joyable letter was just at hand. And I will send you three species of seeds which you request: Diphylleia Grayi Fr. Schm., Ranzania japonica, Ito, and Caulophyllum robustum Maxim.. Praying they reach to you safely.

The literatures which I wish is the Journal of Ecology, the American Magazine(?),at present. But any ecological literature which is published in your university or other university and society is good for us.

Sincerely yours,

Y. Matsumura

Yoshiharu Matsumura.

東京大學

Nikko Botanical Garden,

March 6, 1950.

Dear Mr. H. Hammond, I am very sorry I can not spelling your name correctly as it was so unformal but beautiful writting! Excuse me please.

Your desire is scarecely obtained. We had very little volume of the seed of Glaucidium this year and it was all of them ~~to~~ which offer ed to you. We have no ~~nee~~ need the money, If you dont mind, you Please send us some recent literatures concerning Plant ecology or Teratology instead of sending money.

Sincerely yours, Yoshiharu Matsumura.

YOSHIHARU MATSUMURA, the director.

WAYNE STATE UNIVERSITY
COLLEGE OF LIBERAL ARTS DETROIT 2, MICHIGAN

DEPARTMENT OF BIOLOGY

February 26, 1958

Dr. H. David Hammond
Assistant Professor
Department of Biological Sciences
University of Delaware
Newark, Delaware

Dear Dr. Hammond:

Thank you for yours of February 18 inquiring about our opening for a plant physiologist. Our Personnel Committee has already made a preliminary selection of candidates who have applied earlier and have selected the top three individuals from the list of names thus compiled. At this late date, I do not believe your application would be considered.

However, I am turning your letter over to our Personnel Committee with instructions to keep it on file and to notify you in case of any further developments regarding our employment of a plant physiologist.

Thanking you for your interest in Wayne State University.

Sincerely,

William V. Mayer

William V. Mayer
Chairman

WVM:klg

BOYCE THOMPSON INSTITUTE FOR PLANT RESEARCH, INC.
YONKERS 3, N. Y.

GEORGE L. McNEW
DIRECTOR

March 1, 1955

Dr. H. David Hammond
261 Harvey Street
Philadelphia 44, Pa.

Dear Dr. Hammond:

We have no openings for a morphologist-systematist at this time. We were very impressed with your ideas and background but will not be able to create a position for you. I can assure you we will keep your name in our active file in case we hear of a suitable opening or should have a change in our plans.

Some time ago I heard rumors of a change in the herbarium at Michigan State. It might pay you to contact Professor Drew.

Very truly yours,

George L. McNew

George L. McNew
Managing Director

bb

RANCHO SANTA ANA BOTANIC GARDEN
OF THE NATIVE PLANTS OF CALIFORNIA
(Founded in 1927 by Susanna Bixby Bryant)
22831 ESPERANZA ROAD: ANAHEIM, CALIFORNIA

March 2, 1950

Mr. H. David Hammond
Assistant Instructor in Botany
University of Pennsylvania
38th and Woodland Avenue
Philadelphia 4, Pennsylvania

Dear Mr. Hammond:

Inclosed please find a package of seeds of Crossosoma californicum in response to your request of Feb. 24. We hope that this will be an aid in your study of the group.

Yours very truly,

P.A. Munz

Philip A. Munz

gc

DEPARTMENT OF BOTANY THE ROCKY MOUNTAIN HERBARIUM

THE UNIVERSITY OF WYOMING
ENGINEERING BUILDING
LARAMIE, WYOMING

July 9, 1956

Dr. H. David Hammond
410 West 110 Street, Apt. 614
New York 25, New York

Dear Dr. Hammond:

I thank you for responding to our ad in Science about a position open in our department. In the interim between the placing of the ad and its appearance, we managed to hire a promising young botanist. Hence the position is filled.

Sincerely yours,

Henry T. Northen

Henry T. Northen, Head
Department of Botany

HTN:nbm

UNIVERSITY OF MINNESOTA
DULUTH BRANCH
DULUTH 5

THE SCIENCE AND MATHEMATICS DIVISION

September 3, 1958

Dr. H. David Hammond
158 East Main Street
Newark, Delaware

Dear Dr. Hammond:

I have your letter regarding a position in botany in this department for the coming year. The position, however, has been filled. Thank you for your interest.

Sincerely yours,

Theron O. Odlaug

Theron O. Odlaug
Head, Department of Biology

SEEDS, PLANTS AND BULBS
CALIFORNIA WILD FLOWERS
2969 LOS FELIZ BOULEVARD

THEODORE PAYNE
SEEDSMAN, NURSERYMAN
LANDSCAPE ARCHITECT
LOS ANGELES ~~26~~ 39

TREES, SHRUBS AND VINES
CALIFORNIA NATIVE PLANTS
Telephone: OLympia 3609

March 17, 1950

Mr. Howard Hammond
Assistant Instructor in Botany
University of Pennsylvania
38th and Woodland Avenue
Philadelphia 4, Pennsylvamia

Dear Sir:

Your letter to the Evans & Reeves Nursery in regard to seed of Crossosoma was passed on to Mr. Eustace Rush, who in turn referred it to me as I make a specialty of growing California Native plants.

I used to grow Crossosoma californica, but not have had any for several years. I do not have any seed on hand except a little old seed of which I am enclosing a small packet. I doubt very much whether this will grow but if you want to try it you can do so. I can probably collect a little seed of this for you next summer when the new crop is ready.

In regard to Crossosoma bigelovii, this grows on the desert, and I do not think I will have an opportunity to collect any seeds this coming season.

By mail under separate cover, I am sending you a copy of my latest catalog which may be of interest to you.

Yours very truly,

TP-ABP

Theodore Payne

University of Arizona
TUCSON

COLLEGE OF AGRICULTURE AND
AGRICULTURAL EXPERIMENT STATION

April 7, 1958

Dr. David Hammond
Asst. Prof. of Biology
University of Delaware
Newark, Delaware

Dear Dr. Hammond:

I have your letter of March 28 saying that you heard we were looking for a Botanist for the coming year.

I do not know where you got this information as my staff is complete at the present time and I know of no one who is planning to leave.

However, I shall keep your letter in my active file in case anything comes up in the future.

Sincerely,

Walter S. Phillips, Head
Department of Botany

WSP/mjd

ELMER C. PURDY, MANAGER
MARY P. ROBINSON
MABEL P. MAHURIN

CARL PURDY GARDENS

Founded by Carl Purdy 1879

Bulbs - Plants - Seeds

UKIAH, CALIF., U.S.A.

SPECIALISTS IN
CALIFORNIA NATIVE BULBS
(SINCE 1879)
RARE PERENNIAL PLANTS

Feb. 18, 1950

Prof. H. David Hammond
Botanical Labratory
38th & Woodland Ave,
Philadelphia, Penn.

Dear Prof. Hammond:-

Crossosoma ?californica(Nutt) in so far as I know has never been in cultivation in Calif. unless in one of two botanical gardens. It was mentioned years ago as in cultivation in Europe.

It is found in one location, Catalina Island which is a private presserve and I believe no collector would be allowed there unless from a scientific institution.

It is possible that the shrub ,(actually a small tree as I saw it on Catalina Isl.) is among the plantings of either or both,-

Rancho Santa Ana Botanic Garden, R #3, 23831 Esperanza Rd.
Anaheim, California.

Santa Barbara Botanic Garden, Mission Canyon, Santa Barbara, Calif.

Dr. Munz heads the first named, is the most authoritative botanist in southern Calif. and it is possible that the gardens may have some seed either collected or saved in the gardens. He is most cooperative and if he has the seed I am sure who share with you.

I am sure you would find this true at Santa Barbara if they have the seed. I have the comple te lists of everything in each garden but unfortunately cannot now locates them.

The suspicion that this may be related to Paeonia seems rather far fetched, especially if Jepson is correct in putting this in a separate family, "Crossomataceae". I have'nt a copy of Munz to see what he has done with it and the genus is not included in several other botaniss at hand. This family includes just the two species, Crossosoma California and C. biglovi.

Crossosomataceae does not appear in Jepson' key to the families but in his system of arrangement is quite well separated from ranunculaceae to whic h paeonia belongs. It is quite possible Dr. Munz has pursued the lines you propose and can give you some data.

Cordially yours,
Elmer Purdy

BARNARD COLLEGE
COLUMBIA UNIVERSITY
NEW YORK 27

DEPARTMENT OF BOTANY

March 2, 1955

Dr. H. D. Hammond
261 Harvey Street
Philadelphia 44, Pa.

Dear Dr. Hammond:

I wish I could give you some encouragement as a result of your recent letter concerning a position here, but the fact is that we do not anticipate any additions to our staff in the foreseeable future. With a research background as interesting as yours, you should be able to find something that will be suitable to your talents.

With best wishes for success in your work, I am,

Sincerely yours,

Donald D. Ritchie

Donald D. Ritchie

DDR:lm

NEW YORK STATE COLLEGE OF AGRICULTURE
A UNIT OF THE STATE UNIVERSITY OF NEW YORK
CORNELL UNIVERSITY
ITHACA, NEW YORK

DEPARTMENT OF FLORICULTURE AND
ORNAMENTAL HORTICULTURE
PLANT SCIENCE BUILDING

May 26, 1958

Dr. David Hammond
Dept. of Biological Sciences
University of Delaware
Newark, Dela.

Dear Doctor Hammond:

At the present time we do not have a position open of the type for which your background would be suitable. We had hoped to have a position this year in the field of plant materials but the new position did not develop as we had hoped. I will keep you in mind, however, because it may be that in the next year this type of work would open up again and we would be glad to talk with you about it.

I see that you mention Benjamin C. Blackburn. I had my woody plant materials course work with Dr. Blackburn at Rutgers in 1936.

Sincerely yours,

John G. Seeley

John G. Seeley
Head

JGS:L

ESTADO DE SÃO PAULO
SECRETARIA DA AGRICULTURA
DEPARTAMENTO DE DEFESA SANITÁRIA DA AGRICULTURA
INSTITUTO BIOLÓGICO

N.°SFV/106

São Paulo, August 24th, 1955.

Ilmo. Snr.
Prof. H.D.Hammond
Botanical Laboratory
Rutgers University
NEW BRUNSWICK, N.J.

Dear Prof. Hammond,

With a great interest I have read your article: A study of taxonomic relationship within the Solanaceae as revealed by.... the Serological method, published in the Serological Museum 1955.

Many years ago I became interested in the taxonomical relation between various genera of this family and have studied principally the graft compatibility between various representatives of the Solanaceae. In my own experiments, f.i., I found, that it is very easy to graft Petunia hybrida on Nicotiana tabacum and vice verse, whereas Krenke N.P.,(Wundkompensation, Transplantation und Chimaeren bei Pflanzen, Berlin, Springer, 1933) on pg. 588 states, that Nicotiana and Datura take much easier than Nicotiana and Petunia. My own results agree more with the close relationship, which you found between Petunia and Nicotiana. By this letter I wanted only to tell you that your studies are also very interesting for people, who, like myself, are concerned with the graft-compatibility between the different species of the Solanaceae. One of the studies, published more recently in this field, and which I found very interesting, is by W.C. Whaley and has the title: The growth of reciprocal tomato-tobacco grafts (Bull. of the Torrey Bot.Club, 80, 26-32, 1953).

Unfortunately of many of my earlier papers on grafting they are no reprints available, but I shall send you at least a little note on graft experiments, published in portuguese.

With friendly regards

Yours sincerely

Karl M. Silberschmidt

Dr. Karl M. Silberschmidt
Chefe da Secção de Fisiologia Vegetal

SÃO PAULO — BRASIL
CAIXA POSTAL, 7119

Göteborgs Botaniska Trädgård.

Stora Änggården, Göteborg V den December 18th 1947.

CB/FS

Mr. H. David Hammond
Philadelphia.
U.S.A.

Dr. Mr. Hammond,

I am sorry that you came too late for the 1946 harvest. It seems that 1947 yielded very little of the Berberidaceae except Berberis, but we shall be glad to send you our seed-list which will be out after a week or two. As our exchange with other gardens is rather extensive, we cannot always send you as large quantities of each species as you desire.

Sincerely

C. Skottsberg
Director

UNIVERSITY OF DELAWARE
NEWARK, DELAWARE

SCHOOL OF AGRICULTURE
INSTRUCTION - RESEARCH - EXTENSION
OFFICE OF DEAN & DIRECTOR

TELEPHONE: ENDICOTT 8-2373
ENDICOTT 8-8511

March 18, 1958

MEMORANDUM

TO: H. DAVID HAMMOND
DEPT. OF BIOLOGICAL SCIENCES

RE: EMPLOYMENT POSSIBILITIES

I wish to thank you for your letter of March 14th concerning your availability for employment. I would appreciate it very much if sometime in the near future you would take the opportunity to come in and see me so we could get better acquainted. I regret that during the past year I have seen rather less than usual of the people in the Department of Biological Sciences and hence I don't feel I have come to know you very well.

The more I know about your background and capabilities the better I will be able to discuss it with anyone who inquires.

Signed: G. Fred Somers

G. Fred Somers
Assoc. Dean and
Assoc. Director

GFS:jvp

UNIVERSITY OF UTAH
SALT LAKE CITY 12

DEPARTMENT OF EXPERIMENTAL BIOLOGY

June 25, 1958

Dr. H. David Hammond
Assistant Professor of Biology
Department of Biological Sciences
University of Delaware
Newark, Delaware

Dear Dr. Hammond:

I regret my delay in answering your letter of June 6, 1958; however I have been attending meetings in the east for the past two weeks. Your letter and vitae have been turned over to the Executive Committee of our Division of Biological Sciences which handles appointments and promotions.

We have taken the liberty of writing several of the references you listed for letters of recommendation. We are very much interested in your application, and I will write again in a few days after we receive replies from your references.

Sincerely,

John D. Spikes

John D. Spikes,
Professor

JDS:ag

COLLEGE OF THE PACIFIC
STOCKTON, CALIFORNIA
DEPARTMENT OF BOTANY

March 7, 1958

Dr. H. David Hammond
Department of Biological Sciences
University of Delaware
Newark, Delaware

Dear Dr. Hammond,

I have your letter of March 2nd referring to the matter of a position here concerning which we had some correspondence last year. Arrangements have recently been made to award this position to an individual who has done some work on this campus in previous years and is, therefore, well known here. That, therefore, closes this possibility for the present, and I do not forecast any other appointments in our area in the foreseeable future.

Thank you for your willingness to look up the matter of those cedars. When I wrote you about that some weeks ago, I assumed that you were still located in New York City. Through some other correspondents I have now learned sufficient facts concerning these for my present purpose and hope that you will not have incommoded yourself in the matter too seriously.

Yours truly,

E. E. Stanford

E. E. Stanford
Professor of Botany

EES:g

COLLEGE OF THE PACIFIC
STOCKTON, CALIFORNIA
DEPARTMENT OF BOTANY

December 13, 1957

Dr. H. David Hammond
410 West 110th Street
New York 25, New York

Dear Dr. Hammond:

Perhaps you may remember our correspondence of last spring concerning my interest in Sequoia and Cedrus as planted in the United States.

Since then I have run across an article by George V. Nash which appeared on Page 392-4 of Volume 9 (1913) of American Forestry. This centers about a tree then growing on the estate of Mrs. Colis P. Huntington at Throgg's Neck in New York City. The tree was alleged to have been planted by Philip I. Livingston in 1790, and was said to be, at the time of Nash's writing, in good health. I am much interested to know whether this tree still exists, and have written to several individuals in that neighborhood who I thought might be able to inform me, but have not obtained any positive information. It is, of course, possible that the estate may have been developed for real estate purposes and the tree may be gone.

As it would seem to be probably the oldest specimen in the United States I should really like to know something about its fate. From your interest in such matters expressed in your last letter, it occurred to me that if you are still in New York you might be kind enough to make some inquiries for me and let me know what you can learn about it. Anything of this sort would be most heartily appreciated.

There are said to have been other plantings of cedars in the neighborhood of Flushing, but I doubt if any of the older ones are left.

Yours very truly

E. E. Stanford

E. E. Stanford
Professor of Botany

EES:C

UNIVERSITY OF CALIFORNIA
COLLEGE OF AGRICULTURE
AGRICULTURAL EXPERIMENT STATION

DIVISION OF GENETICS
DAVIS, CALIFORNIA

September 29, 1950

Mr. H. Hammond
Botanical Laboratory
University of Pennsylvania
38th and Woodland Ave.
Philadelphia 4, Pa.

Dear Mr. Hammond:

Since writing you on September 14, I have looked at the papers on serology of which you sent me reprints. I was particularly interested in that by Baldwin, Fred and Hastings on legumes. It bore out my previous opinion on the way in which serological characters will fit into the general systematic picture. If you compare their grouping with the system developed by Taubert in Engler and Prentl as well as with the cytological information gathered by Senn and Tischler, you will find much agreement between the three systems of classification, but also some major discrepancies. For instance, the lima bean and garden bean belong in different groups according to their proteins but are closely related according to the two other systems. In another place Lupinus and Ornithopus have closely similar proteins according to the serological test, but are far apart in both external morphology and chromosomes. My experience with systematics leads me to believe that more study of this and other families would bring to light still more discrepancies. I, therefore, retain my previous belief that protein similarity as determined by serological tests is just another systematic character. Its value may be great in many instances but it cannot be relied upon as an absolute guide to relationships any more than can any other single character.

Yours very sincerely,

G. Ledyard Stebbins

G. Ledyard Stebbins, Jr.
Professor of Genetics

GLS:jlc

UNIVERSITY OF CALIFORNIA
COLLEGE OF AGRICULTURE
AGRICULTURAL EXPERIMENT STATION

DIVISION OF GENETICS
DAVIS, CALIFORNIA

Seprember 14, 1950

Mr. H. Hammond
Botanical Laboratory
University of Pennsylvania
38th and Woodland Ave.
Philadelphia 4, Pa.

Dear Mr. Hammond:

Many thanks for your interesting letter of September 6. I was very glad to get the series of references on serological work analyzing the relationships between closely related varieties and species. As I stated in my book, this is the type of work that is needed in your field at present.

My chief criticism of Mez's school is that they tried to do too much in too short a time. Eventually, serology may be of great importance in plant systematics and phylogeny , but it needs first to be developed by scientists well trained in both systematic botany and protein biochemistry ; such people are now practically non-existent. I am very glad to hear that you are doing work in this field, and shall be most anxious to hear of your progress in it. I should very much appreciate an exchange of reprints when you have them available.

In regard to your question about the haploid vs. the diploid state, perhaps I didn't emphasize enough on page 174 the importance of genes which under the present environment of the species have a slight to moderate slective disadvantage . Mutations of this nature are certainly numerous, and perhaps constitute the majority of those which occur naturally. Under new conditions of the environment these mutations, or combinations of them , might be expected not infrequently to have a selective advantage in a new environment. And the fact that they must pass every generation through the haploid gametophyte would not , in my opinion , be any detriment in the case of the vascular plants. Remember that most genes produce their significant effects only during certain stages of development. For instance, one would not expect that genes affecting the structure of leaves or flowers would do much to the gametophyte, since it does not have such organs. The difference is not only between haploidy and diploidy , but also between entirely different developmental stages. The important fact is that in the higher plants one of these stages , which is the most important one, can harbor recessive genes, while the other, which is relatively insignificant, cannot. In Algae and Bryophytes, this situation is reversed. (many

I hope that we shall have a chance to meet again before long and discuss these problems.

Yours very sincerely

G. Ledyard Stebbins Jr.

THE GEORGE WASHINGTON UNIVERSITY
WASHINGTON 6, D. C.

DEPARTMENT OF BOTANY

April 15, 1958

Dr. H. David Hammond
Department of Biological Sciences
University of Delaware
Newark, Delaware

Dear Dr. Hammond:

I appreciate having your letter of April 7, inquiring whether there were any openings in the Department of Botany here at George Washington.

We have only a very small department, and much of the work is carried by part-time staff who teach in the evening classes. For the time being I see no likelihood that this situation will alter appreciably. Under these circumstances, therefore, we do not have need of any full-time members on our staff.

With all best wishes,

Cordially yours,

Russell B. Stevens

Russell B. Stevens
Executive Officer

UNIVERSITY OF PITTSBURGH
PITTSBURGH 13, PENNSYLVANIA

DEPARTMENT OF
BIOLOGICAL SCIENCES

March 5, 1958

Dr. H. David Hammond
University of Delaware
Newark, Delaware

Dear Dr. Hammond:

I read your letter with much interest and am enclosing a personal record blank which you can fill out and return to us if you are still interested in the position. I must tell you, however, that the appointment is to the Instructor level only and that the salary is to be within the range $4,100 - $5,300, the exact salary being a matter of agreement.

Sincerely yours,

Ian M. Sussex,
Assistant Professor
of Botany

IMS:amg

Enclosure

March 28, 1955

Dear Dr. Hammond:

I am very much interested in your serological work and should be grateful if you would send me reprints.

Sam F. Trelease

Columbia University
in the City of New York
NEW YORK 27, N. Y.
DEPARTMENT OF BOTANY

March 28, 1955

Dr. H. David Hammond
261 Harvey Street
Philadelphia 44, Pennsylvania

Dear Dr. Hammond:

Thank you for your recent letter inquiring about the possibility of a teaching-research position, and giving the details of your scientific training and experience. We are sorry that there is no opening in the Department now and it seems unlikely that there will be one soon. We are circulating your letter among our staff members, and if we learn of any opening that might interest you, we should be glad to write to you about it.

Yours sincerely

Sam F. Trelease

Sam F. Trelease

SFT/fj

SANTA BARBARA BOTANIC GARDEN
SANTA BARBARA, CALIFORNIA

MAUNSELL VAN RENSSELAER, DIRECTOR

March 3rd, 1950

Dear Dr. Hammond:

In response to your request for seeds of Crossosoma californica, or C. Bigelovii, I am sorry to say that our plants here in the Botanic Garden do not bear fruit.

However, I am sure that it would be possible for you to obtain seeds of C. californica from the Rancho Santa Ana Botanic Garden at Anaheim, California, or from Mr. Herman Schroeder, Box 1278, Avalon, Catalina Island. For seeds of C. Bigelovii, I would suggest that you write to Mr. Lloyd Mason Smith, Director of Palm Springs Desert Museum, Palm Springs, California.

Will you kindly give our personal greetings to Mr. Henry Skinner when you see him again.

Very sincerely yours,

M. Van Rensselaer

Director.

Dr. H. David Hammond
Asst. Instructor in Botany
University of Pennsylvania
Philadelphia 4, Pennsylvania

UNIVERSITY OF MICHIGAN
ANN ARBOR
DEPARTMENT OF BOTANY

October 7, 1954.

Dr. David Hammond
Morris Arboretum
9414 Meadowbrook Avenue
Philadelphia 18, PENNSYLVANIA.

Dear Dr. Hammond,

Having inquired of Prof. D. R. Goddard where I might obtain a copy of your publication on chemotaxonomic studies of Paeonia, I was advised to write you at this address.

For use in my course in Evolution and Systematics, I should like to have a copy of your paper, which I am informed is not yet published.

But, if you have some mimeographed or printed résumé of your work, I should be grateful for a copy which I could use in teaching this fall.

Please give my regards to Prof. Fogg.

Sincerely yours,

Warren H. Wagner Jr.

Warren H. Wagner, Jr.
Assistant Professor.

lm

UNIVERSITY OF MICHIGAN
ANN ARBOR
DEPARTMENT OF BOTANY

February 10, 1955.

Dr. H. David Hammond
The Morris Arboretum
Chestnut Hill
Philadelphia, PENNSYLVANIA.

Dear Dr. Hammond,

The time is long tardy for thanking you for all your help in guiding me to references, and in lending me your theses in connection with chemotaxonomic work.

Botany 251, Evolution and Systematics of Vascular Plants, is now over, and both the teacher and the students, I believe, profited greatly by it. I began my taxonomic work largely as a collector and a hobbiest and gradually evolved from this to a study of hybridization and the phylogeny and taxonomy of ferns. My largest taxonomic stimulation came from my experiences at Berkeley and at Harvard, where new approaches such as detailed anatomical studies, statistical studies, and cytogenetic studies, were being exploited. When I came here I found that the taxonomists were still interested in local flora primarily, and this was the length and breadth of it.

I long ago proposed that it might be desirable to present a course emphasizing the new approaches to taxonomy, and my colleagues took kindly to the proposition, even to the extent of relieving me entirely of my other teaching responsibilities last fall while I undertook to teach the new course. I had to depend greatly on correspondence with various researchers, like yourself, for help, and in many cases I was enabled to present their actual research materials.

Ernest Babcock wrote me just before his death that slides were on their way to illustrate the use of chromosome morphology in Crepis evolution, and prior to that T. Harper Goodspeed had already sent fine materials of Nicotiana to bring out the use of chromosome pairing behavior as a source of evidence of relationships. I. W. Bailey most kindly sent a complete set of the sections of different primitive woods, as well as the flowers of a variety of primitive angiosperms, and Jim Canright sent a collection of his stamen material. Tom Morley presented me with a series of slides illustrating various aspects of sclereids in connection with his taxonomic investigations. A.C.Smith kindly presented me with some examples of Degeneria vitiensis for classroom study. I cannot enumerate all of my friends who came to my aid here, but the result was that we were able to discuss in a very "personal" way the work of a large number of pioneers in taxonomic research.

I do not know whether you remember meeting me when you were with Dr. Wherry at the A.I.B.S. meetings at Ithaca. It was there that you arose during the Symposium on the Genus, and pointed out the glaring omission of the various speakers, and, incidently, made your point very well. And in presenting my new course I had hoped to take up chemotaxonomy in more detail than I actually did. What time we did spend on it was most profitable, and I hope the next time to to devote more time to it.

The introduction to your Ph.D. thesis was especially useful as background material for my lecture on the use of serology. I notice that you are going to publish a short paper soon in the Serological Museum Bulletin, and I hope that you will send me some copies if you get some extras.

I had intended sending you an outline of the course, but such is still not available, simply because I haven't had a chance to prepare it. This spring I present (as I do every year; the advanced course is only presented every other year) the regular Elementary Taxonomy course, and I have 46 students and 5 auditors, making a total of three laboratory and field sections. One of the students doing research with me is planning to work on circular chromatography in connection with his plant genus. This new technique seems quite promising as another chemotaxonomic approach, and I would be curious to know what you think of it. This student, along with seven others who are doing research with me this spring, takes a lot of time, and these people, plus the Elementary Taxonomy course, are slowing any research plans of my own down to practically a standstill. And I don't see my way clear to writing up the outline of Evolution and Systematics for a while yet, but when I do I shall send you a copy.

On the long chance that you may be interested, I am sending you some reprints of papers which illustrate the kind of work I have been doing in the ferns. I feel now, of course, that if I had also chemotaxonomic methods in my approach it would add a great deal. It is going to take me some time to catch up properly in this respect, and I am going to have to learn the techniques. I strongly suspect that my students will beat me to it.

I have both of your theses here, and would like to have them for a couple of weeks longer if this is satisfactory to you. Let me know, and I will send them on.

With best regards,

Sincerely,

Herb Wagner

Warren H. Wagner, Jr.
Asst. Professor of Botany.

UNIVERSITY OF MICHIGAN
ANN ARBOR
DEPARTMENT OF BOTANY

November 15, 1954.

Dr. H. David Hammond, Jr.
261 Harvey Street
Philadelphia 44, PENNSYLVANIA.

Dear Dr. Hammond,

Thank you very much for your letter of November 8th.

I shall be glad to send you an outline of my new course -- when I get one! I have a broad outline which I am following, but I am deviating from it opportunistically, depending upon new literature I find or have my attention called to.

Roughly, though, this is my pattern: I am taking up all the groups of vascular plants as a sort of backdrop. Then, in each group, I take up the modern research and theories that are most pertinent. For example, in the Lower Groups I take up Telome Theory, the Use of Teratology, in the Gymnosperms, Extreme Reduction and Modification of Organs (e.g., Podocarpus, Taxus), in the Origin of Angiosperms, we take up the famous Bailey-Tupper base-line of tracheary length and all the associated changes. All the while the students have to make their own slides or other preparations. The study of pollen grains, for example, came in the Ranalian Complex, and the students had to prepare research slides of pollens. The chemotaxonomic approach has been introduced only once so far, but will be the subject of more work as we go along. For each general subject -- tracheary elements, origin of angiosperms, cytology, etc. -- we give a complete summary of subject matter at some point during the work, usually bring in new material at the time. (The only chemotaxonomy so far is Mover on Euphorbia latex.)

Have you seen Kirk, Main, and Beyer 1954, The use of paper partition chromatography for taxonomic studies of land snails, Biochemical Journ. 57 (no. 3): 440-442? It sounds promising.

With best regards, sincerely,

Warren H. Wagner, Jr.

Warren H. Wagner, Jr.

INSTITUT FÜR SYSTEMATISCHE BOTANIK
UND
BOTANISCHER GARTEN
DER UNIVERSITÄT GRAZ (ÖSTERREICH)
HOLTEIGASSE 6

Graz, am 5.1.1950.

Herrn
H.David HAMMOND, Assistant Instructor
in Botany, Botanical Laboratory, University
P h i l a d e l p h i a, 4 (Penns.)

Sehr geehrter Herr!

Indem ich auf Ihr Schreiben vom 14.2.1948 nocheinmal zurückkomme, sende ich Ihnen anbei einige Samen von Ranunculaceae.

Gleichzeitig mache ich Sie darauf aufmerksam, daß 1949 eine eingehende Arbeit erschienen ist, die sich mit den Sie interessierenden Gruppen befaßt: JANCHEN E. 1949: Die systematische Gliederung der Ranunculaceen und Berberidaceen. Denkschr.Akad.Wiss.Wien, math.natw. Kl. 108: 1-82.

Ich hoffe, daß Ihnen dieser Hinweis willkommen ist und bleibe

hochachtungsvoll

Ihr sehr ergebener

Widder

Beilagen (Samenproben)

(Univ.Prof.Dr.Felix WIDDER)

CITY OF ROCHESTER

NEW YORK

DEPARTMENT OF PUBLIC SAFETY
BUREAU OF PARKS.

November 12, 1947

David Hammond
Assistant Instructor in Botany
University of Pennsylvania
Philadelphia, Pa.

My dear Mr. Hammond:

In reply to your letter of November 4th. we regret to state that we have no seeds of Epimedium or Podophyllum at the present time. The Podophyllum ripens in July and the Epimedium long before that.

The following commercial dealers in native plants might be able to supply with the desired seeds:

J.J. Nudd, Exter, New Hampshire
E.C. Robbins, Ashford, N. Carolina
Gillett Fern & Flower Farm, Southwick, Mass.

Sincerely regretting our inability to assist you, I am

Yours very truly,

Wilbur E. Wright

WILBUR E. WRIGHT
Assistant Superintendent

Yampolsky

Cornwall Bridge, Connecticut

Dear Dr. and Mrs. Hammak,

May I introduce myself in two capacities: as a friend of Gus and Ethel Macnapol, and as a fellow botanist? I have heard your praises sung and I hope we can meet. I am a botanist of old standing but I still retain a youthful interest in that field.

Sincerely

Cecil Yampolsky

Feb. 4, 1956