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

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

29 April 1968

Dr. Nelson G. Hairston
Museum of Zoology
University of Michigan
Ann Arbor, Michigan 48108

Dear Dr. Hairston:

The enclosed map will get you out ϕϕ Denver. Coming into Boulder, stay on the Turnpike until you see on the right hand side the big sign for the Holiday Inn (where we have reserved a room for you). You will have to make a U-turn to the rightoon to the service road which parallels the freeway to get back to the Inn. It is very simply, easy arrangement, however.

My home phone number is 447-1819. Give me a call when you get in, if you care to. We might have a chance for a chat on Sunday evening. I look forward to seeing you.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

25 April 1968

Committee on International Exchange
of Persons
2101 Constitution Avenue, N. W.
Washington, D. C. 20418

Gentlemen:

Please send me application forms, a list of openings in the Biological Sciences and other information about exchanges.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

25 April 1968

Dr. Theodore J. Crovello
Department of Biology
University of Notre Dame
Notre Dame, Indiana 46556

Dear Ted:

Herewith, at last, your manuscript. It is so well done, and I am ashamed to say that it was just today that I got the thing started. I read it again; it reads very well, and I will say (when you are ready) to the editor of Brittonia that I am satisfied that you have done a fine job on it.

There are a couple of places in the manuscript that you can still do a little bit of editing on. I would very strongly suggest that you take yourself out of the third person wherever you have mentioned your own citations. I forget the page numbers on which it occurs, but I do recall one statement in there which says "this led Crovello to .." do so and so. Take it out and say "I did so and so" and then cite the place where you did it. The only other thing I would suggest is that since your paper with Ornduff on the Limnanthaceae is now out that under the discussion you can cite that as a more specific reference.

Turning from the subject of your paper to the IR project, we will be very, very pleased to provide you with our software. Most will be ready some time this summer. Since the input of the demonstration which we hope to have by the first of September has a great similarity to the information which you probably want to record about the Greene Herbarium it might be well if you can try to find some "scratch" to get out and take a look at it. If this can't be arranged let me know and we'll see if we can find some way to get together with you. We are most anxious to have data banks and give good tests to our procedure, and if you are willing to give us a data bank we will be happy to have it.

I just remember that you said that you were going to Chicago. Did that come through, or not? If so, I suppose that the E. L. Greene Herbarium will rest in peace without any good IR system for some time to come.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

24 April 1968

National Science Foundation
Division of Graduate Education in Science
Washington, D. C. 20550

Gentlemen:

I am interested in subbitting a proposal for a summer institute for biologists at the post-Ph.D. level and would like to have the brochure describing these programs and the necessary requirements for the application. I believe that the appropriate area is the college teacher program and particularly the short courses and summer institutes for college teachers.

Perhaps a description of our interest for an institute would give you a better idea of my desires. For the past several years we have been developing methodologies for using computers in the field of systematic biology. We have developed a course for students in this university which teaches biological, mathematical and computer methods for taxonomy. Since this relatively new field is becoming more and more significant in the biological community I feel that it is important to communicate the ideas to a body of professional teachers and researchers who have not had the opportunity to become familiar with these methods. My staff and I have concentrated on these efforts and have developed a curriculum which combines theory and practice to make biologists capable of going ahead on their own. The course could be taught in an intensive manner in about a month of eight-hour days.

I trust this description gives a sufficient insight to know which particular program I should address my application to.

I understand that the deadline for such applications for summer 1969 is July 1, 1968.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

24 April 1968

National Weather Records Center
Federal Office Building
Asheville, N. C. 28801

Gentlemen:

I have been told that you could provide information concerning actual rainfall records for Mexican localities.

I am interested to know the amount of rain (if any) that has fallen in the vicinity of Ciudad Victoria, state of Tamaulipas, Mexico, from February 1st of this year to the present time; and the same for the cities (towns) of Mante and Valles in the state of San Luis Potosi. The reason for my interest is that I want to collect some plant materials in these regions, and the plants are particularly dependent upon recent rains for their best growth and development. Without recent rains, the plants may remain in a dormant condition without leaves, and in this condition, they are next to impossible to locate.

Could you, at the same time, discover whether last year's hurricane dropped any rain on the above mentioned localities.

I will be most appreciative of any such information you can provide.

Sincerely,

David J. Rogers
Professor of Biology

DJR"gm

24 April 1968

Dr. Warren H. Wagner
Department of Botany
University of Michigan
Ann Arbor, Michigan 48105

Dear Herb:

Please send us a photo we can use in local publicity, and some "interesting" personal info to be used with it.

Looking forward to your visit - give me details, and I'll meet your plane.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

22 April 1968

Dr. Howard Irwin
New York Botanical Garden
Bronx Park, Bronx, N. Y. 10458

Dear Howard,

The copy of F. G. Brieger's paper just came. I read the English summary but did not spend any time on the text in Portuguese. If we can take some of the English summary statements as to what he believes in, then I think that Dr. Brieger should spend some more time reading before he does anything else. The first thing I object to is the fact that he calls numerical taxonomy a new branch of the biological sciences. This it is not. I am sure that Sokal would attempt to have use believe that it is, but under no circumstances can we claim anything really unique, so far.

Some of Brieger's criticisms of the statistical models that he finds displeasing are valid but I can't see a hell of a lot of reason for them. The more damning problem is the fact that the statistical models were established to deal with the kinds of work other than taxonomy. This seems to be the focal point of the difficulty; that the attempt to shove taxonomic data through the statistical models of which Brieger speaks is an incorrect approach. It behooves the taxonomist to establish the rules he wants and then to get a mathematical model which will be a reflection of those rules.

If Brieger is going to put on a seminar for you, I sure hope he does more reading in the literature and includes some of our works as well. I didn't see our reference at all, and I think we have just claim to be disturbed that he has not. Your own work on Cassia is a very good example of some of the things that should be mentioned.

Of course my comments are made on the basis of what I read in his English summary. There may be some other nuggets which go into these sorts of details that I have not read in the Portuguese section of the paper. If Brieger is travelling in this country at all, you might tell him that we would be happy to see him here for a while to let him know what is doing in this shop. He might be interested and might be able to arrange it on a return trip basis to his own country. Thanks for sending me this paper; I am always interested.

Sincerely,

David J. Rogers
Professor of Biology

DJR:cm

Taxlmetrics Laboratory

Armory 101A

19 April 1968

Prof. Frank White
Forest Herbarium
Department of Forestry
Commonwealth Forestry Institute
University of Oxford
OXFORD, England

Dear Prof. White:

Enclosed is my informal set of remarks for your inclusion or exclusion as you see fit in the introductory comments. As you can see there is little description but I feel that the commentssay about the kind of generalities you ought to make. Certainly the example (you and Pranco) will goavery far in making a meaningful illustration of computer methodologies.

As I said above, you may use this in any way that seems to fit with your own discussions. You may wish to put me on as junior author for the introductory paper. I trust that we can look forward to some more collaborative efforts.

Have you any more specific word about your student who was to join us for a while. We can foot the bill for his maintenance money while here. We look forward to having him with us.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

19 April 1968

Mr. Paul B. Holden
USDI - FWS
Utah Cooperative Fishery Unit
Utah State University
Logan, Utah 84321

Dear Paul:

Enclosed is the bill for your computer runs. I am glad to know that the program did some good for you, and I hope that we can help in the future. I will be glad to see your theses when you can send me a copy.

The new development here involves the development of a computerized information retrieval system. This IR system will be very useful to workers like yourself who want to keep track of large volumes of data, such as your fish. The first running program should be available late this summer. I think they would profit you and others working in your area to adopt them.

Let us hear from you as you go along.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

18 April 1968

Frank G. Hawksworth
Rocky Mountain Experiment Station
240 W. Prospect Street
Fort Collins, Colo. 80521

Dear Frank:

I received our manuscript and have looked it over. Your suggestion for a title page is good. I will fill in our half of the footnote. The minor changes you suggest are fine with me. I have a few additional suggestions myself. Whenever you are ready to get together for the final polishing, give me a ring.

Very truly yours,

George F. Estabrook

GFE:gm

17 April 1968

Dr. Ulysses J. Grant
The Rockefeller Foundation
Apartado Aereo 58-13
Bogota, Colombia

Dear Jerry:

On March 15 Dr. Rodenhiser pointed out several places in my report on yuca that he would like to have amplified. One of these required considerable search of literature which is not directly available to me. I have asked Miss Tersillo in Washington to round up some material, but she is apparently at work on some high priority job and has not yet had an opportunity to round up the literature that I requested.

Some of the other questions about the report can be answered more easily and I enclose herewith some supplementary material to be incorporated in the report now in your hands. In order that you can quickly refer to the questions asked by Dr. Rodenhiser I am including a copy of his letter requesting the information.

Rody's first question dealt with maturity. I think that the footnote included herewith will answer this question. The footnote can be inserted behind page 1 with an asterisk placed on the word preference" in the third line from the bottom of page 1.

The second request for an expansion of information concerning protein production by microorganisms has to be searched by Miss Tersillo. I can say without much research that the starch from cassava has not been used in any significant, large, scientific efforts for protein production. About the only modern effort that I know of has been stated in the present report (Gray, 1966). To my knowledge, Gray worked on a laboratory scale, and not on a production basis. I am continuing to work on this particular question.

The third question dealt with fertility levels. Again I believe that the information is best stated in a footnote and I have included this footnote to be inserted just behind p. 16 with an asterisk on "fertilizers," in the fourth line of the paragraph labelled Fertility-Levels, page 16.

The next request from Rody was that I delete the paragraph on information retrieval and the personnel to man a computer system. I understand the reason for this request and would merely suggest that, if this is amenable to you, those portions be merely struck from the present report. Since nothing needs to be said about computers for information retrieval, I think that this can be excised from the report without doing damage to the continuity. If you decide that I should rework this area I will do so.

On page 2 of Rody's letter he asks about storage requirements. I am a bit confused by the question as to whether it deals with the storage of the roots or the storage of germ plasm, but rather than to wait for clarification on this point I will assume that the interest lies in the storage of germ plasm. My reply is therefore directed to this point.

Germ Plasm Storage. Since the "germ plasm" for Manihot esculenta is a vegetative portion (stem cutting) there is little likelihood that we could maintain under the best of conditions viable material as cuttings for more than six months. Native farming practices indicate that if one places mature stems of Manihot under shade and under some light cover (leaves, for example) the stem will be viable for as long as 3 - 6 months.

In light of the paragraph above I would like to introduce a concept only recently being generated by Dr. Alfred Jones who has considered the problem of a "genetic bank" for sweet potatoes. It is Dr. Jones' opinion that the concept of a large living collection of sweet potato varieties is not a useful procedure (particularly because of loss to disease, confusion of labelling, and cost). It is possible in populations of sweet potatoes (whose methods of propagation are similar to that of yuca) to recover in a very short time any gene desired from a small sample of any group of cultivars. This possibility is based upon the fact that the sexual mechanisms involved in the production of seeds is of sufficient heterogeneity to contain any desirable properties. With this short discussion we can see that if we collect the actual seeds from Manihot populations there will be a high probability of having any recombination of genetic material needed for germ plasm banks. Sowing a random sample of these seeds will produce in the derived plant some particular useful gene combination. When the desired gene combination is found, it is cultured as long as is required by vegetative propagation.

Since this idea is probably one not entirely familiar to breeders of root crops such as Manihot esculenta it is likely that this concept will take some time to be accepted, but I am convinced from my knowledge of M. esculenta that it would not be profitable to maintain either living collections of all cultivars for breeding purposes nor to spend much time doing research and designing complex controlled environment chambers for the storage of the vegetative cuttings. We have no data on storage requirements for seeds of M. esculenta.

I trust that this satisfies the requirements, and that you can use the report to advantage.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

16 April 1968

Dr. Mason Hale
Department of Botany
Smithsonian Institution
Washington, D. C. 20560

Dear Mason:

I enclose herewith a letter to Bob Dressler. I hope you can forward it to his present address. I am not sure where he is at the moment, but my last recollection is that he worked for the Smithsonian in Panama.

Thank you for your efforts.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

P. S.: We are forging along on the IR system - my formal report on this later.

16 April 1968

Dr. Robert L. Dressler
Smithsonian Institution
Washington, D. C. 20560

Dear Bob:

I wonder if you could dredge up from your memory some knowledge of your collecting trip in the Sierra de Tamaulipas in 1957. We have a specimen of yours (#1913), Manihot Pringlei, collected 40 km NNW of Aldama on July 20, 1957. Were you on any sort of recognizable highway? If so, are there any landmarks which would tell something about what sort of habitat you might have found this specimen in? Was the 40 km a guess or an actual measure? Is the starting point, Aldama, a recognizable entity?

I hope you can jiggle your memory a little on this request because we are headed for that area and that species of Manihot particularly.

Thank you for your efforts.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

16 April 1968

Dr. Grady Webster
Department of Botany
University of California
Davis, Calif. 95616

Dear Grady:

On your Mexican field trip in 1962 collecting M. Pringlei (specimen #11229) 15 miles SW of Ciudad Victoria, please search your memory for additional locality information. We would like to put our hands on this plant on a proposed trip this summer. Any and all identifying features in the landscape that you can recall will be muchly appreciated. Were you on the road to Jamave?

When do we hear from you about information retrieval?

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

16 April 1968

Mr. Charles F. Rhyne
Department of Botany
Smithsonian Institution
Washington, D. C. 20560

Dear Mr. Rhyne:

I am sorry to report that we no longer have copies of the characters and attributes of Halimeda.

May I suggest that you write to Dr. Llewellya H. Collnvaux at the College of Biological Sciences, The Ohio State University, Columbus, Ohio 43210. She may still have this information.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

9 April 1968

Dr. Jonathan D. Sauer
Department of Geography
University of California
Los Angeles, Calif. 90024

Dear Jon:

I was pleased to get your reprint on Geographic Reconnaissance o of Seashore Vegetation along the Mexican Gulf Coast - I guess more technically it is not a reprint but a separate publication. As usual with all the rest of your papers I have read, it was very enjoyable. I was particularly pleased to see your comments on the origins of cotton and G. punctatum as a truly wild plant.

This leads me to ask if by chance you made any observations further back from the coast than reported here. As you know I have hypothesized that Manihot esculenta was first cultivated in the Yucatan and Meso-American regions. What I would like to know is how far north up the coast you could find, if you did notice, Manihot esculenta growing. About the most northerly representatives that I saw were at Boca del Rio, just south of Vera Cruz City. Were there any to the north of that that you ran into? Let me know your findings.

Thanks again for sending the paper,

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

9 April 1968

Mr. Mark I. Halpern
IBM, Dept. 234, Bldg. 062
Monterrey and Cottle Roads
San Jose, Calif. 95114

Dear Mark:

Here is a copy of the letter we sent to Herman Goldstine and a copy of his reply. If we hear anything from Dr. Porter, I'll let you know.

I'll be in touch with you.

Sincerely,

Robert C. Brill

RCB:gm

Taxlmetrics Laboratory
Department of Biology
Armory 101A
University of Colorado
Boulder, Colorado 80302
8 April 1968

Dr. Jorge Leon
Instituto Interamericano de Ciencias
Agricolas de la OEA
Apartado 478
Lima, Peru

Dear Jorge,

I have the summary of the paper you gave at the VII Reunion Latinoamericana de Fitotecnia. Have you a full copy of the text of that paper? If so I would be pleased to have one.

I have a student working on the genus Manihot and would like to have him do some field work collecting South American species. Have you thought anymore about sending collectors into the region of southern Brazil and northern Paraguay? Could you work up something for my student to go along?

Thanks and best regards.

David J. Rogers
Professor of Biology

DJR:gm

8 April 1968

Mr. David Rosenberg
David-Stewart Publishing Co.
3612 Washington Blvd.
Indianapolis, Indiana 46205

Dear Mr. Rosenberg:

I will be pleased to continue as a member of the editorial board for the new edition entitled "Natural Science ..."

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

8 April 1968

Prof. J. F. Danielli
Department of Medicinal Chemistry
School of Pharmacy
University of Buffalo
Buffalo, New York 14214

Dear Professor Danielli:

Please find enclosed the manuscript "A General Solution in Partial Orders for the Camin-Sokal Model in Phylogeny" which I am submitting for publication in the Journal of Theoretical Biology.

It might interest you to learn that my recent publication in the Journal of Theoretical Biology (12, 297-310) solicited nearly 200 responses from 24 countries.

Very truly yours,

George F. Estabrook

GFE:gm

4 April 1968

Dr. Russell B. Stevens
National Research Council
2101 Constitution Avenue
Washington, D. C. 20418

Dear Sir:

Please find enclosed the devised version requested.

Very truly yours,

George F. Estabrook

GFE:gm

DDDr. Reyment's comments are very relevant and appropriate, but I would like to share some further considerations with you. I do not feel that biology is a branch of statistics. Statistical Analysis is not the only mathematical technique which might be useful in systematics. To assume that it is is to eliminate, a priori, from consideration many of the advances of modern mathematics which might not only serve to analyze biological information but also to make more precise the basic principles of systematics. I would urge that an open mind be kept towards such branches of mathematics as combinatorial analysis, logic, and information theory.

I would like to emphasize Dr. Reyment's admonition that care be taken before employing a given statistical technique. We must be very careful that we do not make biology fit the statistics, but make sure that statistics fits biology. Dr. Reyment pointed out that certain statistical techniques tend to frown on redundancy. One of the effects of a principle component analysis, for example, is to summarize variation more efficiently by eliminating redundancy, i.e., correlations between the descriptors. However, many practicing taxonomists accept the idea that taxa are established with correlated characters. It is the redundancy in the descriptors which enables us to delimit, and later describe, taxa.

As biologists our primary concern whould be with the advancement of biology. Toward this end, mathematics may be used to help us to a firmer understanding of the principles of biology, as well as to help us process data. Thus, before any mathematical technique is used to analyze data it is essential that the biologist knows and accepts what biological principles the mathematical technique assumes to be in force. Care should be taken that the mathematics should be appropriate to its biological application, rather than making mathematics a "bed of Procrustes" for the biology it is designed to serve.

1 April 1968

Mr. Mark I. Halpern
IBM, Dept. 234, Bldg. 062
Monterry and Cottle Roads
San Jose, Calif. 95114

Dear Mark:

It was a pleasure talking with you Thursday. I appreciate the effort which you, Dan and Morty have made on my behalf. I am hopeful that with your comments and those of your colleagues, we (Bob and I) will be able to put our paper into acceptable form.

Please find enclosed the appendices which I said I would send you, If there are others who are willing and whose opinions you feel might be of value to us, I would be pleased to have their comments as well.

I hope your project receives the support you feel that it deserves and needs.

Very truly yours,

George F. Estabrook

GFE:gm

28 May 1968

Miss Frances Tersillo
Biological Sciences Communication Project
The George Washington University
2000 P Street, N.W.
Washington, D. C. 20036

Dear Miss Tersillo:

Your references and "reprints" have been arriving like spring showers. I appreciate all the time and effort you have spent. My sincere thanks. If what I am looking for is in print, I ought to be able to find it now.

Sincerely,

David J. Rogers
Professor of Biology

DJR:GM
CC: Dr. Dorothy Parker

To Whom It May Concern:

Mr. Subramanian Ganapathi Appan is a research assistant under by direction whose annual salary is guaranteed by my in the amount of $4,166.55. Mr. Appan's employment is guaranteed until September 1, 1969.

Sincerely,

David J. Rogers
Professor of Biology

24 May 1968

Inter Documentation Co.
Researc h Publishers N.V.
Rijnsburgerweg 177
Leiden, The Nederlands

Gentlemen:

Can you tell me, please, if microfisches of the actual specimens of the De Candolle Prodromus Herbarium exist? I am particularly interested in the specimens representing the genus Manihot. Is it possible to buy separate parts of the Prodromus Herbarium microfische?

Will you please let me know the answers to these questions, and the prices?

Sincerely,

David J. Rogers
Professor of Biology

DJR:cm

23 May 1968

Dr. Franklin W. Martin
USDA - ARS - CRD
Federal Experiment Station
Mayaguez, Puerto Rico 00708

Dear Frank:

Thanks for the character coding sheets. It occurs to me that we will have several useful programs in addition to the one already known to you which will be useful when we go into the origin and evolution of sweet potatoes. The information gathered on these coding sheets you have sent can serve as "input" to these other programs without modification, but used in different ways.

Some day, either going or coming from Hawaii, why dontt you stop in and we'll give you a complete run down on what we can do with various pieces of our equipment. Most of our efforts turn out to be quite useful for the kind of thing you and Al and I want to do.

Have a good time in Hawaii.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

23 May 1968

Dr. Phyllis Parkins
BioSciences Information Service
2100 Arch Street
Philadelphia, Penna. 19103

Dear Phyllis:

I have been made a member of a committee to investigate possibilities of an updated bibliography and potential IR system for for tropical root crops. You may remember that I have done considerable amount of work on tapioca plants, Manihot esculenta). It strikes me that one of the best things we might do would be to discover if through BIOSIS we might achieve a very much better search of the literature than any of us as individuals could accomplish.

I am therefore writing this as a preliminary inquiry to know how you have set up special project relations at BIOSIS. Could we establish some procedure for the biological data on the five or six major tropical root crops? What arrangements for funds and staff do you need to make such a bibliography go? What would we need to put into the collaborative effort?

I will be pleased to have your response to these questions.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm
CC: D. G. Coursey
Tropical Products Institute

22 May 1968

Dr. R. S. MacNeish
Department of Archaeology
University of Calgary
Calgary, Alberta

Dear Scotty:

A recent letter from Reichel-Dolmatoff prompts me to inquire whether any further activities concerning travel to Tokyo have developed. Any news would be appreciated.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

P.S.: There is a chance that our department may be able to get University funds for travel, to participate in the meeting. However, the announcement you sent with dates, topics, participants and sponsors has been mislaid. Could you send us another one, please?

17 May 1968

Dr. John Reeder
Osborn Memorial Laboratories
Yale University
New Haven, Conn. 06520

Dear John:

You will recall that I reviewed a manuscript by Ted Crovello, University of Notre Dame, and turned it down. This was last fall, I think. Since then, I've seen the reworked manuscript, and from my standpoint, it is in good shape now. He is in the process of retyping it, and either has, or will shortly, resubmit it to you. It has my official blessing, for what that's worth. Since there are no taxonomic decisions (revisions, new taxa, etc.) but rather, deals with methodology, I should think that you could let it run.

Do I hear that you're moving to Wyoming? It would be nice to have you for neighbors, and I hope we'll see you often.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm
cc. Ted Crovello

UNIVERSITY OF COLORADO
BOULDER, COLORADO 80302

TAXIMETRICS LABORATORY
DEPARTMENT OF BIOLOGY
ARMORY 101

PHONE: 303-443-2211
Ext. 6712

14 May 1968

MEMO TO: Mr. Jim Stein, University Services
FROM: David J. Rogers, Dept. of Biology
SUBJECT: Relocation of telephone

The secretary of the Taximetrics Laboratory is going to move from Armory 118 to Armory 101. This will necessitate a change of telephone, extensions 6712 & 6078 (A), between rooms.

This will also involve moving out the Artic-Alpine Institute phone which is in Armory 101, extension 6387-6388. Could both moves be made at the same time?

David J Rogers

14 May 1968

MEMO TO: Mr. Jim Stein, University Services
FROM: David J. Rogers, Dept. of Biology
SUBJECT: Relocation of telephone

The secretary of the Taximetrics Laboratory is going to move from Armory 118 to Armory 101. This will necessitate a change of telephone, extensions 6712 & 6078 (A), between rooms.

This will also involve moving out the Artic-Alpine Institute phone which is in Armory 101, extension 6387-6388. Could both moves be made at the same time?

15 May 1968

Mr. Ted Tedesco
Municipal Building
Boulder, Colorado 80302

Dear Mr. Tedesco:

This confirms our telephone invitation to you to join us at a seminar on Tuesday, May 21, at 4:00 P.M., in Hale Hall 102. Dr. Warren H. Wagner, Director of the University of Michigan Botanical Garden will speak to us on "The Place of a Botanical Garden in a University and Surrounding Community." We're also having a cocktail party after the seminar at the clubhouse of Mountain Shadows, 1501 S. Broadway, to which you are invited for informal conversation with Dr. Wagner.

As you are probably aware, some thoughts are being generated about the most appropriate use of green belt land of the city and the possible University role in development of biological, educational and recreational activities. I have used the words "botanical garden" in a very broad sense, for want of a better term to indicate a location which can serve a multiple set of needs in keeping with the greenbelt idea. Dr. Wagner is very well equipped to speak to such points. He is one of the country's leading biolgists and has much experience in developing such a multipurpose facility.

We hope to have a number of seminars by various types of indiv viduals who can help us consider needs and objectives along these lines in order to find the most useful combination. We hope, therefore, that you will be able to join us in these considerations and look forward to seeing you on Tuesday.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

P.S.: Can you let me know if you can make it to the cocktail party. Thanks. Phone 443-2211, extensions 6712 or 6078.

13 May 1968

Dean Thurston E. Manning
Regent306
Boulder Campus

Dear Dr. Manning:

We in Biology invite you to hear Dr. Warren H. Wagner, Director, University of Michigan Botanic Garden, speak on Tuesday afternoon, May 21, at 4:00 P.M., Hale Hall, Room 102. Dr. Wagner's topic, The Place of a Botanic Garden in a University, is particularly pertinent to the development of our own ideas with respect to potential collaboration with the City of Boulder, and the use of some of the greenbelt.

I hope that you can attend the seminar, and also, that you can join us for a "Happy Hour" (at 6:00 P.M.) following the seminar at the clubhouse of Mountain Shadows Condominium, 1501 S. Broadway. This would give you an opportunity for informal discussions with Dr. Wagner, an extremely knowledgeable botanist.

We look forward to seeing you.

Dave Rogers
Biology Department

DJR:gm

P. S. Could you let me know if you can make the Cocktail Party?

Same letter to: Dean William E. Briggs

Taximetrics Laboratory

Armory 101A

13 May 1968

Dr. U. J. Grant
The Rockefeller Foundation
Apartado Aereo 58-13
Bogota, Colombia

Dear Dr. Grant:

This belated addendum to my report on Manihot esculenta refers to the potentiality of using cassava as a substrate to cultivate microorganisms for protein production. This goes with page 7 of the report. Under any circumstances, I strongly recommend that thvestigations of this potential be made part of the cassava work.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

13 May 1968

Dr. Shirley Tucker
1022 Baird Drive
Baton Rouge, Louisiana 70808

Dear Dr. Tucker:

In the last few weeks I have been attempting to discover a term in descriptive morphology and wonder if by chance I might enlist your aid in finding the appropriate terminology.

In my studies of the systematics of Manihot esculenta I have used a character with three states which essentially measures the distance on the mature stem between one node and the next node immediately above it. This is a convenient measure but I have no term to describe the character: that is, the term for this distance. I understand that the term for nodes directly one above the other, in a line, is the orthostichy - this term, however, does not define the actual distance. My plants have a 2/5 phyllotaxy.

I wonder if I may impose on your time and knowledge to suggest an appropriate term.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

13 May 1968

Dr. Elisabeth Cutter
Botany Department
University of California
Davis, California 95616

Dear Dr. Cutter:

In the last few weeks I have been attempting to discover a term in descriptive morphology and wonder if by chance I might enlist your aid in finding the appropriate terminology.

In my studies of the systematics of Manihot esculenta I have used a character with three states which essentially measures the distance on the mature stem between one node and the next node immediately above it. This is a convenient measure but I have no term to describe the character: that is, the term for this distance. I understand that the term for nodes directly one above the other, in a line, is the orthostichy - this term, however, does not define the actual distance. My plants have a 2/5 phyllotaxy.

I wonder if I may impose on your time and knowledge to suggest an appropriate term.

Sincerely,

David J. Rogers
Professor of Biology

DJR:am

8 May 1968

Dr. Jorge Leon
I.I.C.A. de la O.E.A.
Zona Andina
Apartado 478
Lima, Peru

Dear Jorge:

I was very pleased to receive your paper yesterday and I am glad to see you are progressing with studies of yuca. In as much as we both have some similar objectives I wonder if I could not provide you with some assistance with your studies there. If you are interested, we can provide a computer-based classification of your cultivars. We could also provide you with the information which tells how your varieties are related to varieties all across the distribution of the species.

Assuming that you are interested in collaborative work, I am taking the liberty to suggest some procedures for gathering the information necessary to make a computer-based classification. Specifically I am enclosing with this letter a list of field data which should be consistently gathered for each cultivar. This list of data can be expanded as you like but the pieces of information are the ones that we have found useful so far in our classification.

We feel that it is vital to study the herbarium specimens which are the documents on which we will have to base the classification. We would therefore like to have a herbarium specimen from you for each of your cultivars made according to the enclosed recommended procedures, including the listed field data.

I hope that we can collaborate in this endeavor. If you find these suggestions meaningful and interesting, let me know.

I hope that you will have some information about the projected trip into Paraguay and southern Brazil soon. I believe that the Rockefeller Foundation is interested sufficiently to aid in supporting such a collecting expedition. I would be glad to contact them in your behalf (and mine).

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

6 May 1968

Prof. J. F. Danielli
Department of Medicinal Chemistry
School of Pharmacy
University of Buffalo
Buffalo, New York 14214

Dear Professor Danielli:

In rereading my paper "A General Solution in Partial Orders for the Camin-Sokal Model of Phylogeny" which I submitted for publication on April 8, I discovered two typographical errors. The changes are underlined in red on the enclosed Errata sheets. Would you be so kind as to forward copies of the corrections to the reviewers who have copies of the paper.

Thank you very much.

Very truly yours,

George F. Estabrook

GFE:ge

3 May 1988

Dr. Richard A. Howard
Arnold Arboretum of Harvard University
Jamaica Plain, Mass. 02130

Dear Dick:

m As a member of the Plant Records Center Committee of the A.H.S. my vote on the proposals are as follows:

Item 1: Approved.
Item 2: Accepted
Item 3: Approved.

I am pleased that the American Horticultural Society has accepted this activity and I will be happy to assist wherever possible.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

2 May 1968

Dr. Robert L. Dressler
Smithsonian Tropical Research Institute
P. O. Box 2072
Balboa, Canal Zone

Dear Bob:

Thank you for your prompt answer to my query about M. Pringlei. We will most likely visit your locality and we will certainly give a greeting to Don Ignacio.

I am sorry that you didn't give me a run-down on your presentactivities. I would like to hear what's cooking with the orchids, etc.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

2 May 1968

Dr. Grady L. Webster
Department of Botany
University of California
Davis, Calif. 95616

Dear Grady:

Thanks for the information on Manihot Pringlei. I hope we can get back to your locale for it.

We would be pleased to have your graduate student come and spend time with us. It would be preferable for him to take our course in Taximetrics. He could at the same time do some preliminary work with either Croton or Cnidoscolus. The only problem that I can see is that we are on the semester system. Could you spare him for two quarters - say winter and spring? Let's try to make some accomodation for this fellow. Could you let me have a little of his past experience.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

2 May 1968

Dr. C. C. Gotlieb
Institute of Computer Science
University of Toronto
Toronto 5, Ontario

Dear Dr. Gotlieb:

Thank you very much for forwarding the comments of the reviewer. We appreciate his time and effort on this work. We agree with his comments in large measure.

We did not intend to imply that all parts of the paper were new, but used some of the descriptive methods at the beginning to carry forward the argument to the new parts. We will attempt in the rewriting to indicate clearly these points.

I trust that we may resubmit the paper and that we have not been rejected entirely.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

2 May 1968

Dr. J. H. Camin
Department of Entomology
University of Kansas
Lawrence, Kansas 66044

Dear Dr. Camin:

As you may know I have been fascinated by the interesting and challenging problems which your paper, "A Method for Deducing Branching Sequences in Phylogeny," Evolution 19, presents. Please accept the enclosed confidential manuscript wherein is presented a mathematical solution to these problems. This solution gives the most parsimonious cladograms in the sense in which you originally defined them. I have made no attempt to discuss the biological validity of the assumptions on which the method depends. Rather I have accepted your assumptions in the spirit in which you presented them in the above paper, and have formulated my solution in their context.

Very truly yours,

George F. Estabrook

GFE:gm

2 May 1968

Dr. Robert R. Sokal
Department of Entomology
University of Kansas
Lawrence, Kansas 66044

Dear Dr. Sokal:

As you no doubt realize, I have been very interested in the challenging problems posed in your paper, "A Method for Deducing Branching Sequences in Phylogeny," Evolution 19, ever since you first explained them to me at our meeting at Colorado State University in Fort Collins. I would be pleased if you would accept the enclosed confidential manuscript wherein is presented a mathematical solution which gives the most parsimonious cladograms in exactly the sense in which you originally defined them. I have made no attempt to discuss the biological validity of the assumptions on which the method depends. Rather I have accepted yo your assumptions in the spirit in which you presented them in the above paper and have formulated my solution in their context.

Very truly yours,

George F. Estabrook

GFE:gm

1 May 1968

Dr. Rolf Sattler
Botany Department
McGill University
Montreal, Quebec

Dear Rolf:

I wonder if you can help me with a problem in descriptive nomenclature. In my study of Manihot esculenta I have found that a useful characteristic is the distance on the stem from one leaf scar to the next scar directly above it. I have not been able to find a term which describes that distance.

Knowing of your interest I thought that perhaps you might be able either to tell me the word used to describe such a distance or to give me a reference wherein such may be found. I believe that the phyllotaxy of M. esculenta is a 2/5 phyllotaxy (alternate leaves) Different cultivars of Manihot may be differentiated on the basis of the distances from one scar to the next directly above it. I am aware of the fact that one describes the lines passing through the vertical of the leaf scars as orthostichy but this does not tell me the descriptive word I need. I hope that it will not be too difficult for you to help me with this problem.

Thank you.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

1 May 1968

Monsieur le Professeur J. Miege
Conservatoire et Jardin Botaniques
Route de Lausanne 192
Geneve, Switzerland

Dear Professor Miege:

I would be pleased if you could tell me whether microfisches are available for the Manihot species of the DeCandolle Herbarium. If microfisches can be obtained would you please let me know their cost.

Thank you for your attention and information.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

27 June 1968

Dr. Raul Martinez-Crovetto
Faculdad de Agronomicoly Veterinaria
Universidad Nacional de Nordeste
Sargento Cabral 2139
Corrientes, Argentina

Dear Dr. Martinez-Crovetto:

We would very much appreciate receiving a separate (reprint) of your article entitled "Una nueva especie de Manihot (Euphorbiaceae) de la flora Argentina," which appeared in BONPLANDIA REV. Vol. 1 #4, 1964, if it is still available.

Any other articles on Manihot would also be appreciated and read with interest.

Sincerely yours,

David J. Rogers
Professor of Biology

DJR:GM

27 June 1968

Dr. A. G. G. Menon
Division of Agronomy
Agriculture College Research Institute
Vellayani
Kerala, India

Dear Dr. Menon:

It would be greatly appreciated if you could send us a reprint of your article in the Agr. Res. J. Kerala, Vol. 4, No. 2, pp. 95-99, which appeared in 1967.

Any other articles which you may have copies of, on the subject of Manihot esculenta, we would be interested to see.

Sincerely,

David J. Rogers
Professor of Biology

DJR:GM

25 June 1968

Dipl. Phys. E. Winkler
Deutsche Akademie der Wissenschaften zu Berlin
Forschungsgemeinschaft
Permoserstrasse 15
D.D.R. 705 Leipzig

Dear Dr. Winkler:

I have left no more copies of J. Theor. Biol. 12(3): 297, 1 1966, which you requested. However, I hope you find the enclosed helpful in your work.

Yours truly,

George F. Estabrook

GFE:GM

20 June 1968

Dr. James F. Danielli
Theoretical Biology Center
State University of New York at Buffalo
4248 Ledge Lea Road
Amherst, New York 14226

Dear Dr. Danielli: Re: Ms B. 867

The May 20 reviewer has so little to say that I will speak to the May 24 reviewer only. The first point, concerning the notion of Parsimony, is indeed well taken. However, it is evident that the reviewer is unfamiliar with the J. Camin and R.R. Sokal (1965) ppublication upon which this present work is based. In that paper, the notion of parsimony and the equality of evolutionary steps is discussed. At any rate, it is not the purpose of the present paperto rediscuss this. This is expressly stated (cf. p. 1, Introduction, ¶2) at the outset. I agree with the reviewer that these points need to be discussed further, but not by me in the present paper.

The second point may be explained easily. Firstly, it is true (and even proved in the appendix of the present ms.) that in general (i.e. in every last case), including the "counter example" of the reviewer) the the lengths of all complete chains connecting two fixed points in are the same. It is evident, from his comments where he misuses the concept "cover," that the reviewer did not read closely the definition of the concept "cover" as it appears on page 3 of the present manuscript; for in his counter example" (p_2, t_2) does NOT cover (s_1, t_1). I am confident that if this reviewer were to master the concept of cover, as it is presented on page 3, that he would find considerably less ambiguity in the subsequent results.

I would prefer not to change the present manuscript unless sounder criticism can be brought to bear on it.

Very truly yours,

George P. Estabrook

GPE:gm

17 June 1968

Dr. F. A. Stafleu
106 Lange Nieuwstraat
Utrecht, Nederlands

Dear Dr. Stafleu:

Enclosed is our manuscript, "Application of an Information Theory Model for Character Analysis in the Genus Arceuthobium (Viscaceae)." This paper is a follow-up of the paper by Estabrook you were kind enough to publish in TAXON 16, 1967, and we think it illustrates nicely the value of the procedures given in the first paper. Your consideration of this paper for TAXON is appreciated.

Sincerely,

David J. Rogers
Professor of Biology

DJR:qm
Enc.

12 June 1968

Editor, Endeavour
North Block, Thames House
Millbank
London, S.W.1, England

Dear Sir:

It would be greatly appreciated if you could add my name to the mailing list for the Endeavour.

Sincerely,

David J. Rogers
Professor of Biology
Director of the Taximetrics Laboratory

DJR:qm

11 June 1968

Dr. J. S. Farris
Zoological Museum
University of Michigan
Ann Arbor, Michigan 48108

Dear Steve:

I will arive on United Flight #240 at Detroit Metro. at 5:50 P.M. on Saturday, June 15.

See you then.

George F. Estabrook

gm

Taximetrics Laboratory
Department of Biology
Armory 101
University of Colorado
Boulder, Colorado
11 June 1968

Dr. A. Schreiber
Botanische Staatssamlung
Menzingerstrasse 67
München, Germany

Dear Dr. Schreiber:

Dr. Howard Irwin, Curator of the New York Botanical Garden, has asked that I report to you the status of the Manihot specimens which I borrowed in 1961. I am sorry that the material has not yet been returned to you, but it has been possible to interpret your excellent material only within the last two years, as a result of recent collections made by Dr. Irwin.

As you know, I have been actively engaged in the revision of the genus, using methods of classification developed in my laboratory using computers. This now aids us to speed up the process of classification, and to give much more accurate interpretations of the species. I am sending separately several reprints which will give you some ideas about the computer methods which we employ.

We hope to complete studies of specimens, and return the Manihot material with one year. Your patience in this respect has been much appreciated, and I will forward the material as promised.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

11 June 1968

Dr. H. A. Rodenhiser
Rockefeller Foundation
Apartado Aereo 58-13
Bogota, Colombia

Dear Rody:

Enclosed is my bill for preparation of the report on cassava.

Since I last wrote directly to you, I have added some few remarks to the report, and as you requested, sent them directly to Dr. Grant. Unfortunately, I could not amplify my remarks on the production of protein from cassava very much. I had Miss Tersillo make quite a thorough search, and while she did an excellent job, there really was very little that could be uncovered. As I suspected, very little work has been done on cassava in this direction, though many other substrate sources have been investigated. Certainly research from the ground up will have to be done, if this is to be made a part of the Center's work.

I will be very interested to hear the decisions concerning the Center's activities. I certainly hope that cassava, my main interest i in botany, will be on the list of crops to be included. I intend to continue my own work with the classification of the species (and the whole genus), and hope that it will be contributory to any work Rockefeller does in Colombia.

In the event that cassava is not included in the research efforts of CIAT, I would like to go to FAO, and perhaps other international bodies, to see if they cannot support the work. I hope that that will never have to be done.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm
Encl.

over

To: The Rockefeller Foundation

From: David J. Rogers

For: Preparation of study report on Cassava for CIAT

Date: June 11, 1968

Following are costs for preparation of the study report on cassava submitted earlier to Dr. U. J. Grant/

1.	Typing expenses	$25.00
2.	Consulting fees . . . . 8 days, $75/day .	600.00
3.	Miscellaneous phone calls	5.00
	Total	$630.00

10 June 1968

Dr. G. Reichel-Dolmatoff, Chairman
Depto. de Antropologia
Universidad de los Andes
Apartado Aereo 4976
Bogota, D.E.- Colombia

Dear Dr. Reichel-Dolmatoff:

It was a pleasure to hear from you again, after a number of years. I trust that we will have an opportunity to meet in Tokyo, although I understand that Dr. MacNeish is having some difficulty (or the Japanese are) getting sufficient funds to support the symposium.

With respect to your request for latest information on botanical studies of root and other crops, I am sorry to say that we have not progressed very far recently. I do have a suggestion, however, which will probably give you as up-to-date references as are available. Dr. Alvaro Montaldo, whose address is:

Head, Instituto do Agronomia
Facultad de Agronomia
Universidad Central de Venezuela
Maracay, Venezuela

has been working very hard to put together an extensive bibliography of tropical root crops, and probably could supply a copy of his most recent accessions. I also suggest you contact Mr. D. G. Coursey, whose address is:

Tropical Products Institute
56/62 Gray's Inn Road
London, W.C. 1, England.

I also suggest that you call Dr. Jerry Grant, of the Rockefeller Foundation there in Bogota. Dr. Grant probably has in his files most of the reports of the Symposium on Tropical Root Crops held last year in Trinidad. Many reports of that symposium would be useful to you, although most of the information concerns agricultural procedures, rather than aspects of particular interest to yours and my specialities.

Unfortunately, I personally, have been out of contact with the anthropological and ethnological studies for the past three years. Since I left New York, there has not been an opportunity to continue my interests in these directions. I am sorry that I cannot be of more help to you.

Sincerely,

David J. Rogers
Professor of Biology

DJR:dm

MEMORANDUM June 10, 1968

To: Dr. Askell Löve, Chairman, Department of Biology

From: David J. Rogers

Subject: Request for travel funds to Tokyo, Japan, to present invited lecture to the Eighth International Congress of Anthropological and Ethnological Studies.

1. Dr. R. S. MacNeish, The University of Calgary, Canada, chairman of a symposium entitled Incipient Agriculture and the Development of Civilization in the New World, has invited me to present a paper on the Origins of Manihot and other lowland plants.

2. Dates of the meeting are September 3 - 10, 1968.

3. Participants in this symposium are outstanding scholars who have studied various anthropological, ethnological and biological problems which shed light on the subject. It would be an honor to me to be able to participate in this symposium, and I trust, an honor to the University as well.

4. Funds needed:

Air travel, round trip, to Tokyo, Japan	$814.00
Ground travel, United States	20.00
Ground travel, Japan	100.00
Per Diem, at $16 per day, Sept. 1-12 (12 days)	192.00
Miscellaneous costs	25.00
Total cost	$1151.00

June 5, 1968

Dr. Frederick Wellman
Department of Plant Pathology
North Carolina State University
Raleigh, N. C. 27607

Dear Fred:

I hope that Dick Klein answered your inquiry satisfactorily: he told me that your book is now in the process of being printed.

I am glad to hear of the continuing interests that you and others there have with regard to an information retrieval system for plant pathology. As you probably already have heard we got a grant from NSF which began last July and runs for a two year period. This has allowed us to pursue our development and at the present time we have made such progress that we can see the end of the initial phases of writing a computer I.R. system for biology by the end of this summer. We are sufficiently optimistic to think of putting on a demonstration of the procedure for interested people some time next September.

The demonstration will be given for a variety of interested biologists who represent several disciplines, and the demonstration for them will clearly show what our machines and programming can do with regard to their specific interests.

If you and your colleagues care to join us we will put you on the mailing list and let you know of developments as they occur. I believe that our methodologies are sufficiently general to accomodate almost any kind of biological data, and at the same time be capable of answering specific requests for information from its data bank.

We will be pleased to collaborate with you in the organization of an information retrieval system tailored to your needs. We hope that you will consider this invitation in the light in which it is intended. Our goal is to aid and assist biologists with such fundamental problems as those you have already outlined. If you need to know more about the system in advance, let me know.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

4 June 1968

Dr. Billie Turner, Chairman
Department of Botany
University of Texas
Austin, Texas 78712

Dear Billie:

We have just reviewed yours and Flake's proposal to NSF, and a copy of the "confidential" review is enclosed. I am prompted to write this letter for two reasons: (1) We want to see you funded for this work, and (2) we feel that some aspects of your proposed research might prove infeasible. George Estabrook (the mathematician who has been working on these problems with me since 1964) and I have spend considerable time wondering how we might provide you with a useful back-up for your work. In the spirit of trying to be helpful, the following comments about the proposal are made.

1. Research in the areas of cluster analysis has been active for a number of years now. Much meaningful progress has been achieved (in Kansas and Colorado). We should begin to pass out of the phase of empirical development and "after the fact" evaluation of the several extant methods (on the basis of how well the various computer programs fit some a priori opinions of the taxonomic specialists whose groups have been analyzed), and into the phase of the development and recognition of biological principles involved.

2. The areas of research which are in desperate need of investigation now are the points of interface between biology and the mathematical methodologies employed to link biology to computing machines. Efforts should be made to abstract principles from biology which may be used as objective yardsticks to evaluate (and improve) mathematical methods. An ideal objective would be to abstract sound enough principles from systematic biology such that these alone might constitute the axiomatic base upon which the appropriate mathematical methodologies might be built.

3. Perhaps the most elusive area of interface is the recognition, es establishment, evaluation, modification, etc. of characters. Here is where biological information is being carried to the mathematical methodologies. There are presently very few principles governing this procedure and yet we must be confident with this aspect of the interface if we are to ultimately adopt these new methodologies as ones which deserve our confidence.

4. I would suggest that the statement of objectives under the heading "Characterization" appearing on pp. 3 and 4 of the Grant Proposal be expanded and made more precise. This will give you a clearer picture of what your objectives are. The statement (p. 4) including "... may not require much investigation to obtain a satisfactory approach..." certainly indicates an optimism on your part - I hope it does not reflect your failure to recognize the importance of this phase of your work. Although a little progress has been made, this merely provides you with a starting place.

5. In my opinion Mayr's "prediction criterion" is a misrepresentation of the problem; it stands too close to the current issues. One needs to stand further from the problem, disentangle oneself from the conventional points of view, view the problem in more generality and bring more imagination to bear. I think that the results (still outstanding) on the first part of the research plan (Characterization) are needed before meaningful work can be done on the second (Prediction). I would strongly urge that the substance (90%) of your effort go into a collaborative effort between yourself (and your students) and your methodologist to shed some light on the very knotty problems of characterization which underlie all considerations of methods further along in the taxonomic process. If you can contribute to the solution of these fundamental problems in characterization you shall have made a profound contribution to the art. Fiddling around with ad hoc classification methods is easy work for methodologists; shedding light on characterization is hard work for biologists. We need the experience, professionalism, and willingness of men such as yourself to address this difficult area.

These points, said out in the cold, may not seem entirely relevant, but they indicate some very fundamental points of view. Perhaps the only way that their relevance can be made meaningful is by face-to-face talks with you and Fleke, and perhaps with your graduate students. We do not always come across with our intentions intact until such time as we can be certain that our terminology and yours coincide. Even with a complete rapport on the words used, we need to give some more explanation of our attitudes about the complex interrelations between taxonomist, mathematician and computer programmer. Some of our ideas appear in print, but since they do not all occur in one paper, it is difficult to gain from our published papers what is intended here in this letter.

With this background, let me suggest that we put our heads together some time this coming summer, with sufficient time to gain an insight into our respective problems, and some means of attacking them. We are entering a very significant phase in what I prefer to call taximetrics, having been through a few years of empirical efforts and some more fundamental problem solving techniques.

I trust this suggestion is meaningful to you, for we all need as much intensive effort on the work as we can get.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

June 4, 1968

Dr. Howard Irwin
New York Botanical Garden
Bronx Park
Bronx, New York 10458

Dear Howard:

We're sending a package of root and stem specimens back to you, as follows:

Specimen 14193, root - Gutiferae
" 10930, stem - fern
" 9772, root - Cassia
" 15576, leaf - Cyperaceae
3 root specimens with undecipherable numbers

We're also sending the following, for which we have no leaf specimens:

root - 9625, and stem - 2389.

Hope this clears things up.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

20 May 1968

Dr. George W. Fischer, Exec. Director
XI International Botanical Congress
University of Washington
3917 15th Avenue N.E.
Seattle, Washington 98105

Dear Dr. Fischer:

Will you please direct this letter to the appropriate organizing secretary? I am vitally concerned to have a day's symposium, if at all possible, on biological information retrieval systems. This significant and important activity has generated a sufficiently large number of workers in botany to make it feasible to explain our various operating procedures.

Since information retrieval is a fundamental underlying activity for all botanical science I believe we must put on some symposium which tells the community where we now stand.

I will be glad to organize this symposium in collaboration with whomever I should collaborate.

Thank you for your consideration.

Sincerely yours,

David J. Rogers
Professor of Biology

DJR:gm