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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.

- Taxonomy Laboratory

January 3, 1966

Dr. Walter Hodge
Program Director for
Systematic Biology
The National Science Foundation
Washington, D. C.

Dear Walter:

I have just received an invitation from Bob Dressler to participate in the Symposium on the Amazonian Biota. Specifically, he wants me to talk about the origin and distributions of cultivated plants.

The Symposium, of which I am fairly sure you are aware, is to be held from June 6th to the 11th, 1966. As usual, he has invited me to attend, but has no funds available for my transportation. I would like to know if it is possible to have a travel grant for this purpose, and if so, will you please send me application forms?

Best wishes for the new year.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 3, 1966

Dr. William P. Anderson
Chief of User Services
Western Data Processing Center
Graduate School of Business Admin.
Los Angeles, California 90024

Dear Dr. Anderson:

This is to express my appreciation of your generous reception last Thursday afternoon. It was indeed a pleasure to meet you, to see the facilities, and to discuss the general problems of computing. I particularly appreciate your giving me a lift back to the Los Angeles airport.

We are certainly pleased with the efforts of the Western Data Processing Center in all activities in which we have collaborated so far. We are more than happy that things have worked out so smoothly as they have. We look forward to the continued happy relationships in the new year.

According to your instructions and your admonitions we are preparing all of our programs so that they may be broken down into component units and run as separate things in order not to take up too much time on an extensive run. It may be, however, in the future that we will have to make some special arrangements for lengthier time consumptions. I trust, however, that we can manage to keep ourselves within a reasonable time period.

We are also pleased that you are willing to act as our sponsor for incorporating various programs of ours in the SHARE library.

Once again, thank you for your fine reception. I was pleased to meet you.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 3, 1966

Dr. Robert L. Dressler, Executive Dir.
Organizado pela Associacao de Biologia Tropical
Museu Nacional,
Quinta da Boa Vista
Rio de Janeiro,
Estado da Guanabara,
Brasil

Dear Bob:

I am interested and pleased to have received your invitation to present a paper on the origin and diffusion of cultivated plants. At the Symposium on the Amazonian Biota my acceptance of the invitation is dependent, of course, upon whether or not the National Science Foundation will foot the bill. If they will then I can make it. Otherwise, I have no other funds for travel.

I am definitely interested in talking about this subject and hope that we can make a go of it. I have written to Walter Hodge and asked him if he can foot the bill through the National Science Foundation. As soon as I have heard from him I will give you a definite yes or no.

Once again, thanks for the invitation and best wishes for the new year.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 3, 1965

Dr. Carl C. Moh
Inst. Interamericano de Ciencias
Agrícolas de la LBA
Centro de Enseñanza e Investigación
Turrialba, Costa Rica

Dear Dr. Moh:

Thank you for your letter of December 22. I have just now had the opportunity to answer it.

Concerning the various questions you pose: I don't think that the shipment will be much damaged by cold, particularly if, on the outside of the parcel, the statement is made "Perishable - Do Not Allow to Freeze, Deliver Immediately." I think that we included in our materials such a statement to that effect. Another question that you had was that dealing with the cost of the mailing from Miami to here. According to the regulations that I have, after the plant material has cleared the plant quarantine the material is returned to the mails and goes forward to its destination here at Fort Collins under the original postage. Therefore, I assume that the cost of the mailing should be figured at your end as though you were sending it directly to Fort Collins. But, the material will obviously pass through the plant quarantine at Miami because of the labels on it. That is, the yellow and green labels which you will put onto the outside of the package.

I am not in any particular rush for this material and I would suggest that you do it at your convenience whenever it is possible.

Thanks again for your questions and attention to this request.

Best wishes for the new year.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR7pam

- Taxonomy Laboratory

January 3, 1966

Dr. David P. Lillinger
Associate Curator
Division of Ferns
Smithsonian Institution
United States National Museum
Washington, D. C. 20560

Dear Dave:

Thank you very kindly for sending me a copy of your dissertation, or at least the relevant parts of it. I am a little beyond where I can use it specifically in the paper that I was hoping to do. But, I am glad to have it for a full exposition of your ideas, anyway. They will certainly come in handy.

I had a chance at the Berkeley meetings to talk to Herb Wagner and he told me that you were definitely interested in going ahead with us on the collaborative project. He said that you hoped that you might put in for funds from the National Science Foundation to foot the bill. I told Herb that the situation has changed considerably since we left New York and that it was very likely that we could run the material here if it didn't get too big, without any cost to you whatsoever. If this is the only stumbling block that you have, that is, the cost, then I think that we can probably make it go here. I am not sure yet, but I think that the funds would not be at all unreasonable. Probably less than one hundred dollars. I am not even certain how much this charge will be because we have not yet had any experience with running our programs here at C.S.U.

As you may know, C.S.U. is one of the Western Universities that works with the Western Data Processing Center at Los Angeles. We have a teleprocessing line from here which shoots the data right into the 7094 in Los Angeles and we get the print out back sometimes within 24 hours by the same line. We have still to complete the transfer of our programs from the Control Data Corporation's machinery that

we used in New York to the I.B.M. hardware in Los Angeles. However, this is progressing nicely and I think that we should have ourselves in operation for our Clustering Methods and so forth within the next two months.

I hope you can go ahead and prepare your data now with the relief that you won't have to pay such a wampun big bill as you might have had to do if we were still on the east coast.

Best wishes for the new year.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 7, 1966

Hertz Rent a Car
205 N. College Avenue
Fort Collins, Colorado 80521

Dear Sirs:

The enclosed Rental Agreement is the only copy I have in regards to the car I rented from the Stapleton Air Field in Denver, Colorado and returned to Fort Collins, Colorado on December 30, 1965.

Sincerely,

David J. Rogers
Professor of Botany

DJR/psm

, Taxonomy Lab. 1/8/65

Dear Steve:

We were glad to have your X-mas card. As usual, the Rogers didn't get around to sending any this year. We've been hearing a little about you and your activities from Bill Klein, but hadn't heard that they were about to pull the rug out from under you there at Occidental.

I am surprised to learn that an institution which is supposedly devoted to teaching undergraduates would get so specialized in their desires in biology that they couldn't accomodate several disciplines, as long as the course-work were offered. So many places make the mistake that if you aren't riding a bandwagon, you aren't teaching the right subjects. If they are switching to molecular biology, they are behind the times, for the new kick is now "behavioral biology," whatever that may be! Are the powers that be all biochemists? That may explain it. Or, even better these days, you ought to be a "mathematical biologist."

We came here because we were asked to come, a very important aspect to me. While at New York, I had to keep fighting my way in, and I felt that I was more tolerated than desired. Bill Steere never lifted his finger to help, though when I would ask him, he'd say "sure, Dave, I like it, but you've got to do the work and get the money." I felt, during the last six months of my stay in NY that I had made some inroads through the antagonism barriers on the second floor, even to the point where BM even wanted to know how it worked! His approach was, I think, dictated by the fact that someone finally pinned him down to do a monograph of Clusia, and he is up against it. Now he needs all the help he can get.

We also came here because we literally hated the New York area. I have very little good to say about our living conditions there, and we are much happier in the environment of a land-grant institution ~~xx~~ in a relatively small town. We all suffered from the extreme competitiveness in our suburban community. It wasn't that I mind competitiveness per se, but the kind they had there, where you had the feeling that "I'm going to get to the top, to hell with whoever gets in my way." While you get competition here, it's not that sort, and there are many people who would rather cooperate than compete.

^{me}
I have with ~~xx~~ Henry Fleming, whom you know, George Estabrook, a mathematician, and Bob Brill, a programmer. While we're in the Dept. of Botany, we actually are quite independent, both in work and in housing. We occupy the next to oldest building on campus, the old music building. It may not look like much, but it suits us fine. We've got lots of room, and noone to bother us. We still support ourselves with grants, but that's the common way of life in science these days.

I have often wondered what you were doing in regards to the business of "teaching machines" for botany. Have you ever written anything about this? What are your thoughts about how to proceed? Have you ever applied for funds to develop something? Personally, I always thought that much of comparative morphology could be done better by teaching machines than by an instructor, but never got much farther with my thinking than that. I was sort of waiting for you to do it for me.

The idea has passed through my mind several times that we ought to get some money to develop such a technique, or do research on it, and each time I get this idea, I think of you. Would you be interested in pursuing this here? It occurs to me that now is the time to begin working on the idea, and if you are interested, we should put some thoughts together about what you mean in this area, and what I mean.

But before we could actually get started, we'd have to have money, and that takes time to get. I don't have a budget which will let me hire anyone of my own accord now, but have wondered where to go to get some. I noted that Time-Life has just started a new venture in collaboration with General Electric to enter the field of new types of teaching techniques in the sciences, in education in general. They might be a prime target for funds, but I don't know whether they will be willing to fork over for research, but this has to be explored. There are all kinds of governmental agencies supporting this type of research, but again, I haven't explored them individually.

I would suggest that if you are interested, you ought to do the following: (pardon me for offering advice--it's worth what you're paying for it) (1) stay at Occidental for the next year and get those publications out, (2) during that time also begin the exploration of procedures for teaching machines, (3) think of whether you want to live on grants for a while.

Well, I've said about what I wanted to say, and I hope that this strikes a responsive note with you. Let me hear from you, and don't wait till next Christmas.

Sincerely,

- Taxonomy Laboratory

January 12, 1966

Dr. Burton Atkinson
National Science Foundation
Washington, D. C. 20025

Dear Dr. Atkinson:

In response to your phone request for suggestions or guidance in the support of procedures for biological data retrieval, particularly for nomenclatorial problems, I submit herewith a memorandum written to Dr. Steere in September, 1963.

The history of this memorandum is that it was prepared for Dr. Steere shortly after a very severe criticism of Gould's work appeared in *Taxon*, the official journal of the International Association of Plant Taxonomists. I do not know whether or not the contents of the memorandum were given any consideration, or whether it was forwarded to Gould.

While the enclosed memorandum does not spell out in detail any particular procedure, it does lay down what I consider the minimum guide lines to approach the problem. Let it be said that no one at the moment has come up with the "correct" solution to the problems.

Perhaps the most critical consideration in any such operation is that of the personnel. Having worked in the area of taxonomy and computers, I have come to realize that this type of work is one requiring team effort, a type of effort that taxonomists are little disposed to participate in. But a procedure not including them is bound to fail. The team should include top-notch taxonomists, a systems analyst, a programmer, and at least on a part-time basis, a librarian knowledgeable of both retrieval systems and biological literature. The taxonomist need not be expert in the computer field, but he should have enough speaking acquaintance with computers that he will not be baffled by the computer specialist's jargon. On the other side, the systems analyst and programmer must recognize that they cannot use a program package already designed for other work, and that they must really become immersed in the

biological aspects of the work.

On a larger scale, whatever team is put together will need to keep in touch with the biological community. This must not be an informal arrangement, but one of very specific connections. I recommend highly the relations that Biological Abstracts has with biologists. I also recommend that Phyllis Parkins be invited to participate in the design of the program or programs.

Lastly, whatever type of program is being designed in museums, or institutions where cataloguing is a major goal, the most critical element comes down to the names of the organisms. Therefore, this aspect must be tackled first, and done well.

I hope that this letter and the enclosed memorandum serve a useful purpose. Without further instruction, I hesitate to add more. In the event that more is needed, however, I will be glad to assist.

Sincerely,

David J. Rogers

Professor of Botany

DJR/pam

Taxonomy Lab.

January 12, 1966

Mr. Robert Lauer
1310 24th Street, Apt. 16
Boulder, Colorado

Dear Mr. Lauer:

Dr. R. Z. Norman, Dartmouth College, has recommended that we write to you concerning your possible interest in a position here at CSU.

We are a special research group in the Department of Botany with the objective of developing mathematical procedures and computer programs for certain areas in the biological sciences. Our work is supported largely from federal grants, but the University supports our endeavors as far as teaching is concerned.

Our needs are for a mathematician who can work in collaboration with biologists, who can discover the appropriate mathematical expressions for certain rules in biology, and can in turn design algorithms to be programmed for the computer. The areas in biology of greatest interest to us are in classification (taxonomy) and environmental studies (ecology).

If you are at all interested in changing activities, I will be glad to give you more details. Please let me know at your earliest convenience. If you care to phone collect, my phone is 491-5201.

Sincerely yours,

David J. Rogers
Professor of Botany

- Taxonomy Laboratory

January 13, 1966

Dr. Thomas P. Myers
University of Illinois
Department of Anthropology
137 Davenport Hall
Urbana, Illinois

Dear Dr. Myers:

Thank you for your letter and inquiry on Manihot esculenta. Enclosed are the two reprints you requested. I do not understand your comment: "You mention that you know of no manioc from coastal Peru." In re-reading the paper I find no such statement. I have collected M. esculenta near Lima myself, and have other botanical vouchers for the coastal areas. It is unlikely that I would have intended such a statement under any circumstances. I also know Margaret Towle personally and have her book in my collection.

In answer to your question concerning botanical reasons for Sauer's assertion that bitter manioc drops out at extra-tropical latitudes and altitudes, I have the following comments. Since we have no botanical information to differentiate between "bitter" manioc and "sweet" manioc I cannot make any reasonable responses. Unless Sauer defines what he means by "bitter," there is no way to argue the point. He is going under the mistaken opinion that some differences do exist. I have no evidence that they do. For example, we know that soil and climate have a marked effect on the CN⁻ content of the roots, and that in the same general area, we can modify the concentration of CN⁻ in the same cultivar merely by growing it in two different habitats.

As you are aware, we have to treat statements by natives about the bitterness, or lack of it, with utmost caution. Some cases have been found where the processing of "sweet" manioc followed exactly that for the "bitter", merely for reasons of habit. Or, the practitioner was really aware that he couldn't distinguish one cultivar from another, and would rather treat the roots as though they were poisonous just to be on the safe side.

Dr. Thomas P. Myers

Page 2

I must say that I am far from knowledgeable of the total botanical problems of these plants and recognize that many more studies will have to be made.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

P.S. I've added two more reprints on M. that may interest you.

- Taxonomy Laboratory

January 13, 1966

Dr. Sidney R. Galler
Assistant Secretary for Science
Smithsonian Institution
United States National Museum
Washington, D. C. 20560

Dear Sid:

I talked to Burt Atkinson Wednesday morning, and was relieved to find that he had already made a decision about the project for nomenclature since my conversation with him at Berkeley, and that I was not required to write a history of that effort. What he wanted instead was to have some suggestions for guidelines in dictating their policy in support of various data retrieval projects in systematic biology.

I have written down for his benefit some very general principles which seem to me critical for the development of an adequate program. Essentially, I said that the most important element in the whole operation is the selection of the personnel to work on the job. In this respect, I suggested that it must be a team operation, where appropriately trained biologists are put in contact with appropriately trained computer experts. I also suggested that taxonomists are the last hold-out for prima-donna studies and that it is difficult to find the taxonomist who was willing to bare his chest before a non-taxonomist. Furthermore, the programs to be put into operation first are retrieval systems for biological nomenclature because, even if one is interested mainly in cataloging museum collections, inevitably the problem gets to the point where nomenclature becomes primary.

For any such program, I suggested that it have a real connection with a group outside the institution where the project is carried out, much in the same way that Biological Abstracts has a real tie to many biologists. I suggested that librarians and other information retrieval types be at least on a part-time basis with the working staff.

I made one or two points about the general problems in getting nomenclature into some sort of retrieval system, suggesting that the hardware aspects be thoroughly investigated before setting up any system. Being certain of an adequate system is critical, and this is tough because all the manufacturers and their salesmen indicate that their hardware is best. But the hardware should still remain secondary to the business of appropriate algorithms and programs.

As you suggested, I told Dr. Atkinson that you had invited me to consult on the project you outlined, but I didn't make much of a point of it. Nor did I say anything about a meeting with him. I leave such as that up to you. All that really interests me is that the project be undertaken. I am happy that you are forging ahead with some procedure, because it is most appropriate at the Smithsonian. Actually, I had been toying around with a small pilot system here, but would not be as happy about it as I would with one at an institution that has the size and scope that you do. Just let me know if there is any way I can help.

Sincerely yours,

David J. Rogers

DJR/pam

- Taxonomy Laboratory

January 13, 1966

Dr. Sidney R. Galler
Assistant Secretary for Science
Smithsonian Institution
United States National Museum
Washington, D. C. 20560

Dear Sid:

I talked to Burt Atkinson Wednesday morning, and was relieved to find that he had already made a decision about the project for nomenclature and that I was not required to write a history of that effort. What he wanted instead was to have some suggestions for guidelines in dictating their policy in support of various data retrieval projects in systematic biology.

I have written down for his benefit some very general principles which seem to me critical for the development of an adequate program. Essentially, I said that the most important element in the whole operation is the selection of the personnel to work on the job. In this respect, I suggested that it must be a team operation, where appropriately trained biologists are put in contact with appropriately trained computer experts. I also suggested that taxonomists are the last hold-out for prima-donna studies and that to find the taxonomist who was willing to bare his chest before some other non-taxonomist is hard to find. Furthermore, I suggested that the program to be considered first were those that attacked a retrieval system for biological nomenclature because, even if one is interested mainly in cataloging museum collections, eventually the problem gets to the point where nomenclature becomes the primary problem.

In connection with any program, I suggested that it have a real connection with a group outside the institution where the project is carried out, much in the same way that Biological Abstracts has a real tie to many biologists. I suggested that librarians and other information retrieval types be at least on a part-time basis with the working staff.

I made one or two points about the general problems in getting nomenclature into some sort of retrieval system, suggesting that the hardware aspects be thoroughly investigated before setting up any system. Being certain that one has an adequate system before grabbing any one is critical, and this is tough because all the manufacturers and their salesmen indicate that their hardware is best. But the hardware should still remain secondary to the business of appropriate algorithms and programs.

As you suggested, I told Dr. Atkinson that you had invited me to consult on the project you outlined, but I didn't make much of a point of it. Nor did I say anything about a meeting with him. I leave such as that up to you. All that really interests me is that the project be undertaken; I am happy that you are forging ahead with some procedure, because it is most appropriate at the Smithsonian. Actually, I had been toying around with a small system here, but would not be as happy about it as I would with one at an institution that has the size and scope that you do. Just let me know if there is any way I can help.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 14, 1966

Dr. Frederick L. Wellman
North Carolina State University
School of Agriculture and
Life Sciences
Dept. of Plant Pathology
Box 5397
Raleigh, North Carolina

Dear Fred:

I have your letter of the tenth requesting information and ideas for a retrieval system for plant pathology, using computers. Having been in the field a little bit longer than I was when I last talked to you about such operations, I have naturally gotten a few more ideas about them.

It seemed to me that your needs fall into more than one category. Some of the categories are things that you can take care of at any university and can be done at regional centers. Other problems are ones that need to have the facilities of a very large collection of mycological materials and at the same time a very fine library. This latter part deals specifically with the names of the organisms. As you know, this is critical to your operation and you've got to have some sort of a national or international system where the names of the organisms are studied, worked on, catalogued, and disseminated. With this available to you, then it would seem to me that regional centers could be established. Just exactly how this would work would take a tremendous amount of endeavor before one could approach a computer.

If this becomes something of particular interest to you and you can see it generate enough interest amongst folks to do anything about it, I would be glad to work with you on some more specific ideas that might make headway with whatever funding agencies are available.

In this connection you would want to certainly look at what's going on in oceanography these days. I know of at least two data retrieval centers, one at Pascagoula, Mississippi and

the other one is at Woods Hole in Massachusetts. There they have data retrieval centers in operation for various marine organisms. Another place comes to mind, and that is the Department of Oceanography at the University of Rhode Island. If they can get the money for the oceanographic work it would seem to be profitable to explore the possibilities of getting money for plant diseases. I concur that some sort of computer system is absolutely essential, but, yet cannot decide which system is most appropriate to the needs. The workers themselves have to define the problems and the project and set up their necessary outline of procedures before any kind of computer work can be done. Let me see what we can do here at this institution to back up your ideas and interests. I know that biology is getting a push here at C.S.U. and maybe we can interest some of the powers that be in the same kind of a project. I must explore this with the various heads of departments and so forth. I am glad to hear of your interest in this area and hope that we can continue to collaborate until something is actually in the works.

Thanks for writing.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 14, 1966

Dr. Howard Irwin
Department of Botany
Universidade de Brasilia
Brasilia, D. F. Brazil

Dear Howard:

This is a long overdue response to your letter of the 8th of November and also to the fine Christmas card which we received from Marian giving some later data about your folk's operation there in Brazil. Marian mentioned that you still hadn't received at Christmastime the needed bus. In other-words, we can paraphrase this and I'm sure you must have by now, by calling it the "wayward bus." I'm sure you have be- come an expert at stack blowing by now. I wonder how you make out when you're doing it in Portuguese. We read in the papers about the terrific rain storms that they had in Rio. I suppose that the same general rains are occurring somewhere around your area and I can imagine that some of those roads back there must be real beauts by now.

I'm certainly glad to hear that you are getting some field work in and that Manihot is being looked after very nicely. I certainly look forward to your fine material. It does me a great deal of good when I pull out your specimens and can see the whole cotton-picken plant right there in front of me so that I know what I'm dealing with rather than little ole scrappy pieces of junk that I've been trying to work on for so long. You mentioned the problem of sending seeds. I would guess that the best thing to do is to hang on to seeds that you may collect until, say, about the first of May. When you send them it seems to me that probably the easiest is to send small amounts and send them air mail directly to me. This way we can circumvent any problems that might occur when dealing with the introduction of these things. Just give me a little notice ahead of time when the seeds are being sent and I will be on the watchout for them.

After much searching about here in Fort Collins we finally found a house that we liked very much and that we could afford.

Strangely enough, after living near great bodies of water in the East for so long we finally have a place in the semi-arid West that looks out over the water. There is a reservoir right behind our house and so we have now, a room with a view and also a guarantee that we're not going to have a hundred houses built right next door to us. This place, like all other university centers is growing like a weed and the typical pattern for house building is to buy up huge tracts of land and then plunk down as many houses as you can jam onto them. The place that we got has about three fourths of an acre and on one side faces up to a highway and on the other side faces the lake (reservoir). The neighbors are sufficiently distant and have a sufficient amount of land around their property that we feel nicely isolated. The address, incidentally, is

Route 2, Box 44-H
Fort Collins, Colorado

Our work on computer classification is finally getting into high gear. It took us alot longer to get moving than we thought it would. We're still in the process of rewriting the programs that we had in New York and still haven't yet got the whole process of clustering running on the I.B.M. 7094 computer that we have available to us. I might say that we are very pleased with the financial arrangement. Our bills, so far, have run something like \$30 and \$40 each for computer work per month. You should remember in New York any one bill that we got would be somewhere in the hundreds and more likely in the thousands. This is a great relief. Within the next two or three years this institution will have in its own shop a computer which will very nicely handle the kind of work that we want to do. Therefore, we look forward to collaboration again on the genus Cassia and any other projects that you may wish to give us. Incidentally, have you heard any more about the application to NSF to publish our joint efforts in the Memoirs? Clark Rogerson wrote to me and asked back last fall for some words to be said about why the particular paper should be supported and I hope I gave him what was needed. Since then, however, I have heard nothing and wondered if you had.

Well, Buddy, I hope that you are able to get on with your work as you had planned it and that everything is finally smoothed out. Incidentally, did you ever think about becoming the director of the university there?

Best wishes,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 14, 1966

Dr. Hugh Wallace
Amherst College
Amherst, Massachusetts 01002

Dear Dr. Wallace:

In answer to your request for achenes of various species of the genus Crepis, I am afraid that I will not be able to give you much help immediately. I am sure that Dr. Schuster did not remember that I have transferred my activities from the New York Botanical Garden and am now a staff member here in Colorado. If it interests you I will alert some of the botanists on the staff here and next summer when there is an opportunity to get out into the field we will attempt to get whatever species are available.

Most of the genus Crepis occurs on the western slope in Colorado, considerably removed from our present locality. However, some of the botanists and extension people are frequently on the western slopes and it may be that we can get some of them to collaborate with us to get achenes for you.

It occurs to me that you may wish to write to the various state universities, those that have departments of agriculture, particularly to the department of weed-seed analysis. These people may or may not have collections of Crepis achenes that would be viable. I do not believe that Crepis ever becomes a weed problem and, therefore, you may or may not have success by writing to the weed laboratories. This is just one suggestion. I hope that it is helpful.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 14, 1966

Dr. Robert L. Dressler, Executive Dir.
Organizado pela Associacao
de Biologia Tropical
Museu Nacional,
Quinta da Boa Vista
Rio de Janeiro
Estado da Guanabara,
Brasil

Dear Bob:

I have just received a negative answer to my request for funds from the National Science Foundation to travel to Belem for the Symposium. I have one more slim hope through a U.S. Aid project which supposedly has something to do with Brasil and particularly with Manihot here on the campus at C.S.U. This institution has some sort of a collaborative project with the University of Bahia. This is one of those crazy commercial endeavors where big stick engineers and business types from the campus go down there to Brasil and tell the folks how to make a Manihot manufacturing plant. They seem to have more money than they have sense and it may be that I can spring some dough from them.

Do you have any suggestions for funds from such organizations as UNESCO? It might be that they would be more willing. Let me know if there are any ideas that occur to you where funds may be derived.

Sincerely,

David J. Rogers
Professor of Botany

DJR/ppm

- Taxonomy Laboratory

January 14, 1966

Mr. Robert Gulick
BioSciences Information Service
of Biological Abstracts
3815 Walnut Street
Philadelphia 4, Pennsylvania

Dear Bob:

Thank you very much for your letter of December 29, offering to try to get in my request for a literature searching. Unfortunately, or fortunately, I don't know which, my immediate demands for the search has passed and at the moment I am not pressed for a particular literature search. Incidentally, BioSciences Information Service, with B.A.S.I.C. and BioScience Research Titles, and so forth was very useful for me when I was looking for information.

I don't believe that the kind of information I was looking for could be searched for against one or two parameters, if I understand the meaning of parameter as you use it.

What I was trying to do was to run down all the information about the use of computers in the studies of evolution and taxonomy, and some of the areas where I have to search have to deal with all kinds of different titles. For example, human evolution, anthropological studies, studies of social problems, etc., etc. When I can define these areas better, then perhaps we can get a better research for the literature going.

Once again, thanks for your offer and I'm sorry I got you to the point where you had to bother to answer that query. I'll try to plan ahead a little bit more in the future so that it won't be one of these hurry up and then no go jobs.

Best wishes to all at B. A.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

To: D. Lellinger
Smithsonian

Taxonomy Lab. Jan. 17

Dear Dave:

Let me try to unconfuse the present confusion.

We can only run your data on our program. We cannot run your programs.

Now that I have laid it out, let me say why. I hope this explanation doesn't add another factor of confusion. There is a very good chance that we would have to reprogram your programs for the specific configurations of the 7094 at the Western Data Processing Center at UCLA, the installation we use. This institution is one of the western universities tied in to the WDPC by a teleprocessing line. We have to pay only for the cost of the ~~xxxxxxxx~~ transmission of the data, but pay nothing for the actual computing machine time in Los Angeles. Because of the contract terms, we can only work with our own programs.

In addition to these contractual complications, there are also the problems that we'd have in doing the reprogramming, even though you might have everything in FORTRAN IV. If you and your programmer were not around to tell us what was going on, it would be a long drawn-out process, as I'm sure you are well aware.

May I ask why you can't continue to use your program at U. Mich? Can't Herb get on there without it costing him anything? Sounds kind of funny to me that a program developed there can't be run there.

But Before I get completely off the topic, be assured that we will run your data with out any charge to you.

Now that I've said that, and hope that you are still interested in collaborating with us, let me ask you something entirely separate from it. I heard via the grapevine that someone at the Smithsonian is interested in running a retrieval program for nomenclature. Have you any information on who this might be, and is it any more than scuttelbut? I'd be interested to know. I hope that something will be done, but that it will not be the same unfortunate type of operation that has slowly died at NYBG. I can think of no better place for such work than the Smithsonian, provided the right people are involved.

Looking forward to receiving your input.

Sincerely,

- Taxonomy Laboratory

January 19, 1966

Dr. William C. Steere
New York Botanical Garden
Bronx Park
Bronx, New York 10458

Dear Bill:

Herewith is the manuscript, "Use of Computers in Studies of Taxonomy and Evolution," as our contribution to Vol. I of EVOLUTIONARY BIOLOGY. I am sorry that I did not meet the deadline, but in my letter of Nov. 20, I explained the difficulty in meeting it. Hope that this is not too late.

I have included a separate page which suggests one way the subsections of the chapter may be set up. This is only a suggestion, and may be modified to fit the style of the book if the suggestions are not appropriate.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam
cc. Mr. Richard van Frank

- Taxonomy Laboratory

January 20, 1966

Mr. Robert M. Lauer
1310 24th St., Apt. 16
Boulder, Colorado 80302

Dear Mr. Lauer:

Thank you for your prompt reply. I am motivated to write again, not so much to try to get you to change your mind as to comment upon some of your interesting observations. I might say, however, that we were precisely interested in your type of mathematics, having had your background explained to us by Dr. Norman.

If one examine the printed record of mathematical applications in biology, one does, as you have, discover that one branch of mathematics, statistics, is heavily relied upon. However, such analytic tools do not "pay off" in some areas of biology, indeed, in many of the disciplines. Our area, for example, has had a number of statistical models developed, and these are not doing the job.

You mentioned that many biologists shied away from the physical sciences for reasons other than their ability to understand mathematical principles. You are correct, and I am one of those who barely succeeded in escaping from poor teaching of math in high school. However, I suggest that other reasons may have caused such individuals to discover a science that did not require the full treatment of formal analytical mathematics in college. I suggest that these people by some unexpressed intuition discovered that what they wanted from math as a contributor to their understanding of their disciplines was not found in the content of the math courses which seemed to be offered more as a hurdle to be jumped than as contributors to the education of the biologist. (This is no fault of the mathematicians who had not investigated the field of biology).

Your observation that biologists respond very well to propositional logic and finite probability theory is certainly a true statement -- I did. But, not for the reasons given. Had I followed, as you suggest, into formal analysis, and then into statistics, I would tell you again that what I wanted for

my biological interest did not come from calculus, nor differential equations. These disciplines simply do not address themselves to my problems.

If I have accomplished anything in the field of botany, it has been because I consistently sought the advice of a person well-trained in mathematics, rather than rely on my own limited ability to derive the needed methodologies. I think you will agree that it is impossible for one individual to be really competent in two disciplines. I have, therefore, attempted to be as good as possible in botany, and expect that the mathematician will be so oriented in his own field. But, given this, we have to spend much time in attempting to understand thoroughly what is needed in each of the disciplines. Our method of operation is to discuss thoroughly the problems, being certain that each individual (mathematician or botanist) really understands the position of the other, then when we have reached this understanding, attempt to put together some rules in botany that the mathematician needs to discover the appropriate model in mathematics.

Our present thinking is that the mathematician, if he is to be happy in our work, should have an opportunity to work with, and in, mathematics, to offer courses in mathematics, and spend at most 50% of his time working with biologists. We would only ask that one of his courses be the sort that many biologists need -- something in finite mathematics. We have the backing of the math department here at C.S.U. in this. They would welcome such an arrangement.

Now, if the above discussion strikes a responsive note, and I have indicated that the nature of our work really is striking off in a new direction, I hope you will wish to reconsider our offer. Since you stated definitely in your letter that you weren't interested, I have small hopes that this reply has any effect. However, in considering this discussion, perhaps you can think of an individual who might be so oriented and so interested.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 20, 1966

Mr. Harold K. Voris
Division of Reptiles
Chicago Natural History Museum
Roosevelt Road and Lake Shore Drive
Chicago, Illinois 60605

Dear Harold:

I have looked over the coding that you sent me. It looks good. If you finish the coding as you have started, then I think we will have no problem with the structure of the characters. Of course, what descriptive information to code remains your decision.

I think we can be sure that \$50.00 will be an upper bound on computing costs. I am sure that you need not apply for more than that amount.

A "program graph" is still being rewritten to run on the equipment available to us here and is not quite finished. I cannot say exactly what field on the card will correspond to object numbers but I'm sure that that field will contain columns 76 77 78 79. If you put your object numbers in these four columns you will be safe. If you use more than 72 characters and thus require more than one card per object the 80th column is then used to indicate the first (1) and second (2) cards by punching 1 and 2 in column 80 respectively. Otherwise, column 80 is blank.

Tell your "office mate" to write us if he is genuinely interested in running some of his data with us.

Give my regards to the rest of the staff at the Museum.

I hope your work progresses well.

Very truly yours,

George Estabrook

GE/pam

- Taxonomy Laboratory

January 20, 1966

Dr. J. G. Field
Zoology Department
University of Cape Town
Rondebosch, C. P.
SOUTH AFRICA

Dear Dr. Field:

I am sorry not to have answered your letter of December 1, 1965 any sooner. With respect to your request for information concerning our classification program and your interest in the development of a system for storing and handling data for marine invertebrates, I have the following comments.

First of all, our programs are intended to take discrete characters describing the organisms, use the characters to determine the over-all similarities in a pair-wise manner between the specimens included in the study, and then to take the similarity measures generated to cluster the organisms for the purposes of classification. Just on the surface of it, and from your descriptions of your interests in your letter, I think that you would be better advised to look for information retrieval systems along the lines developed by Dr. Harvey R. Bullis, Jr., whose address is

The Bureau of Commercial Fisheries
Pascagoula Fishery Station
Pascagoula, Mississippi

Or, along the lines of the methodologies developed by Dr. Robert H. Parker whose address is

The Systematics Ecology Program
Marine Biological Laboratory
Woods Hole, Massachusetts.

Both of these institutions are actively in the field of marine biological data retrieval and I think you will find a considerable number of similar objectives between your interests and theirs.

In the event that you are indeed interested in methodologies of classification I will put you on the mailing list for some of our more recent publications -- those that are still in press, as a matter of fact. As soon as they are available I will send them to you.

Sincerely,

David J. Rogers
Professor of Botany

- Taxonomy Laboratory

January 20, 1966

Dr. Alick Elithorn
Medical Research Council,
J Hut,
Royal Free Hospital,
Lawn Road,
Hampstead,
LONDON, N.W. 3.

Dear Dr. Elithorn:

Your secretary requested the address for Dr. Tanimoto who co-authored a paper "A computer program for classifying plants." You may reach Dr. Tanimoto as follows:

Dr. T. T. Tanimoto, Chairman
University of Massachusetts
Boston Branch
Boston, Massachusetts, U.S.A.

I am not certain that Dr. Tanimoto will be much concerned about the project in which you are presently interested in as much, as his interests have diverged from this endeavor since the writing of that paper. In the event that you would like to continue interest in the study of clustering methods, I am enclosing a paper which expands upon the model originally published by Dr. Tanimoto and me in 1960. Furthermore, we have continued our efforts in this field of taxonomy and have developed programs for classification which we feel are much improved over the one reported upon in the enclosed reprint.

Papers describing our most recent efforts in classification are now in press and as soon as they appear I will forward copies of them to you.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam
Enclosure

- Taxonomy Laboratory

January 20, 1966

Dr. R. C. Jancey
C/- Chapman's Boatshed,
5A Phoebe Street,
Balmain
Sydney - NSW.,
AUSTRALIA

Dear Bob:

I am sorry it has taken me this long to acknowledge receipt of your letter and the copy of the paper. They arrived in good time.

First of all, let me say that I think the paper has some very useful comments to make and should, with some modifications make a fairly good paper. The modifications I suggest particularly are: that there be added to it documentation for some of the statements. In other words, references to the literature. This can probably be done without too much difficulty. Secondly, it needs sub-headings stuck in at various locations when you change your topic and also to make the paper flow more smoothly.

I strongly recommend that the paper be submitted to a journal of botany or biology there in Australia where it will do the most good for you. The only thing I would suggest is that you type my name on the end of it and acknowledge the fact that the work was done during the tenure of your stay in New York and under the aegis of the National Institution of Health which forked over the dough for your coming, staying and going.

With these slight modifications I think the paper is probably okay to be used. I will give it a more thorough investigation in the next few days and will try to discover whether or not I am perfectly satisfied with some or all of it.

If you have as much luck at getting this paper published as we have had in getting all our other output in print, you may or may not ever see the time when this actually gets before the people who should see it. We think that the paper developed by Mike and George on the graph method is finally going to see the light of day in Systematic Zoology in the March issue of

January 20, 1966

that journal. I am not certain that it will make it yet. Every paper that we write seems to be under the thumb of Robert Sokal, and unless we satisfy Robert Sokal we don't get it going.

I am glad to see that you are back in Australia. Has Claire joined you yet? I only found out about what happened to you from George who visited Mike at Christmas time in New York. That must have been quite a hairy situation there before you left.

Keep me informed of your activities and interests, and if you should need any kind of recommendation let me know.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 20, 1966

Dr. Walton C. Galinat
Associate Professor
University of Massachusetts
Waltham Field Station
Waltham 54, Massachusetts

Dear Walt:

I am sorry that it has taken me so long to respond to your letter of December 13, 1965. I have been submerged for the last two months trying to put together a paper which gives something about the philosophy and background and history of the development of the use of computers in biology and particularly for the studies of evolution and taxonomy. Without a doubt this was the hardest paper I ever had to write, involving as it did the disciplines of mathematics with the computer field as well as taxonomy and evolution.

The papers that are enclosed are the only ones that are now available in print. We have two more that we hope to have out -- one to be published in the March issue of Systematic Zoology and another to be published in the Journal of Theoretical Biology. Just as soon as the Systematic Zoology paper appears I will send a reprint. This paper describes our latest methodologies. We feel that finally we have discovered a method to use the computers in a way that will do some good for taxonomists.

If you ever decide that you'd like to run some of your corn data on a computer, I would be very interested in helping out and collaborating with you if you so desire. Frankly, my whole objective in developing computer methods for taxonomy was to work on the really complicated problems of cultivated plants, and I think this is where the payoff will be the greatest. So, keep this in mind and if you are willing to go through some of the difficulties involved, I will be glad to go along with you.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

P.S. I have also inclosed ~~an~~ ⁱⁿ ~~manuscript~~ ^{reprint} of the paper ~~which~~ ^{was} given at the AAAS mee

of your paper may I have one? Any other papers that you have available, I would also like to have them. spares

2nd letter -
follow up

- Taxonomy Laboratory

January 20, 1966

Dr. David B. Dunn
Department of Botany
University of Missouri
Columbia, Missouri

Dear Dave:

At the suggestion of Edgar Anderson, I am writing to request the loan of material representing the genus Uvularia in your herbarium to this department, and specifically to me. The reason for this request is that I would like to have a good clear-cut genus to run as a test case for our computer methods. I recall that this particular genus has several fine examples of clear-cut species as well as good examples of introgressive hybridization. If all goes well, the need for this material will be completed in the next nine months. I will be happy to have any specimens you have.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam
CC - Dr. Harold Harrington, CSU Dept. of Botany

P.S. I recently received a set of your reprints, and I am very appreciative of them.

- Taxonomy Laboratory

January 21, 1966

Dr. George W. Argus,
Assistant Professor,
Plant Ecology (Plant Taxonomy)
University of Saskatchewan
SASKATOON,
CANADA

Dear George:

Thanks for your letter of the 13th.

We are real glad to be here in the West. Seems as how we're closer to nature and life is simpler, and this I like. In addition to the physical comforts of the place there are a number of other features that are much better for us. For example, we have practically free use of the 7094. All we have to do is pay for the transmission of data from our 1401 here to the 7094 at the Western Data Processing Center in Los Angeles. This is such a minimal charge that we practically can count on doing our work free.

We are in the process of converting all of the programs that we had written for the Control Data Corporation's 1604 machine in New York to the 7094 in Los Angeles. This is a big problem, as you know, in as much as the CDC Fortran is a much more scientific oriented language than is the Fortran 4 of the 7094. If all goes well we should have all of our programs running on the 94 sometime within the next couple of months.

With this situation I have an alternative to offer to you. In as much as you would have to do some re-programming for the 7040 there from our 94 programs, how would it be just to send your data here and let us run it for you? The turn-around time would not be much longer, I guess. And in as much as some of our programs have quite a number of sophistications in them you might run into difficulty with your programmer in converting them. I can't say how much the charge would be because the only bills that we have so far had were so small that we can't really get an indication of the costs off of them. Actually most of

our computer time and expenses have been for de-bugging runs, and these don't really give you a clue about the final costs.

We have changed our way of data coding to make it easier on the investigator. We have cut out the PORT-A-PUNCH entirely. Enclosed you will find the data sheet that we use at the moment. The columns are used for individual characters. The rows are used for specimens or objects or whatever you wish to code. Each character can have up to about 35 states. That is, all the alphabet plus nine more. You can use any combination of letters or numbers that you care to use. It isn't likely, however, that any one character will have anywhere near the number of the total number of states possible. That is, it has been our experience that most characters will run between three and seven or eight character states. Please do not infer from this, however, that we are limiting the number of states that you, as the investigator, want to use. The number of states per character is strictly the investigator's choice.

I will not go into some of the refinements of character coding that we have made available to the investigator until I hear from you as to whether or not you are interested in running your stuff with us. If you are interested I'll give you most of the details of what you need to know.

I don't know how this guy Löve hears about all these things because we haven't officially said anything to anybody about the need for a taxonomist. Strangely enough, we do have need for a taxonomist, but we don't have any money at this moment. I have a big stick grant request in which will be used for, in large part, the hiring of taxonomists. However, until that grant comes through there is no use in saying anything about it. If it does, I might write to you and suggest the needs that we have for a taxonomist and, if you are interested, then maybe we can get together. What the Department of Botany here needs is another matter. As you know, Dr. Harrington who has been here for a long time will soon be retiring. We will need to get a replacement for him. However, this is a matter that is not in my hands but in the hands of the chairman. Dick Ward is now the acting chairman and we are in the process of looking for a man to fill that post. Until we have a chairman I don't think much can be said about filling Dr. Harrington's place. Whatever the developments are I'll let you know in sufficient time so that you can plan for it.

I'm looking forward to hearing from you.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam
Enclosure

- Taxonomy Laboratory

January 21, 1966

Michael P. Dacquist
Lt. Colonel, MC
Special Assistant to the Director
Dept. of the Army
Walter Reed Army Institute of Research
Walter Reed Army Medical Center
Washington, D. C. 20012

Dear Colonel Dacquist:

I am pleased to accept your invitation to participate in the one day meeting on Monday the 21st of February on the subject of natural products for malarial activity.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 21, 1966

Mr. Robert Breech
New York Botanical Garden
Bronx Park
Bronx, New York 10458

Dear Bob:

My number four (#4) issue of BRITTONIA, 1965, has never showed up. Would you forward me a copy?

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

P.S. I am still waiting for a reply to my inquiry to you several weeks back. What's with it Bud?

- Taxonomy Laboratory

January 24, 1966

Case # - Gn 567039

Mr. Arthur Rowan
Associated Hospital Service of
New York
80 Lexington Avenue
New York 16, New York

Dear Mr. Rowan:

I am in receipt of your letter of January 17, 1966 in which you claim that some "previous request for information was not received." I and concerned parties such as the hospital and attending doctor know of no such request.

I have also received a reply from the local medical association concerning an inquiry they made for me through the Denver office of Blue Cross. In regards to your payment of \$30.00 for the doctor's fees submitted to you from Dr. A. S. Rumley, they, Blue Cross, know of no plan which also would not have honored at the minimum the additional \$50.00 for emergency surgery work at the hospital. I ask you to reconsider this matter as you state you are willing to do in your second paragraph.

Since we have been enrolled in Blue Cross through (P & S) Columbia University, New York Zoological Society, and the New York Botanical Garden since sometime like 1938 or 1939 and you have never yet had a bill from us, largely because of our affiliations with the medical professions, I consider your attitude extremely cavalier. As a matter of fact the present hospital bill was scaled down about \$40.00 because of our familiarity with hospital charges.

You will find in your records, as has been supplied to you by both the Poudre Valley Hospital and Dr. A. S. Rumley, an account for Mary E. Fleming
New York Botanical Garden
Bronx Park
New York 58, New York
122 796 16F 01

The account was carried here for something like 8 years.

Sincerely,

Henry S. Fleming
Professor of Botany

HSF/dam

- Taxonomy Laboratory

January 26, 1966

Dr. Herbert Friedmann,
Director
Los Angeles County Museum
Exposition Park
900 Exposition Boulevard
Los Angeles, California 90007

Dear Dr. Friedmann:

This letter is in response to your request for my comments on Dr. Stephen Tillett.

First of all, I knew Steve as a colleague at the New York Botanical Garden for approximately one year. Steve was at that time a member of the herbarium staff under a grant to Bassett Maguire. His major function was to assist in the collection of materials from the Guianas, the area of interest to Dr. Maguire. Therefore, I was not able to directly observe the results of Steve's work and only through personal conversation with him was I able to discover what, indeed, he had done and this is the basis for most of my comments to follow.

To my way of thinking the job that Steve filled was not at all one that he should have been working in. Dr. Tillett's training is mostly in the area of modern types of taxonomic work and the job called for an individual who was largely a collector for work in classical taxonomy.

Secondly, Dr. Tillett had a very difficult time because of the fact that he was not free to spend the funds required to do a very good job when he was in the field collecting for Dr. Maguire. He had his hands tied and on several occasions arrangements for the work had been made which turned out to be completely unsatisfactory and made it difficult for Steve to do anything appropriately. Under these circumstances it surprised me that as many good collections of plant materials were made by Dr. Tillett. The experience gained by him on this expedition and his subsequent return to New York seemed to me to be very valuable ones in ways not to run a botanical expedition and,

of course, this gives him the experience necessary to do such jobs as administration of funds more appropriately.

I discussed with Dr. Tillett some of his own interests in taxonomy and in botany in general, and discovered that in his areas had he been allowed to employ his talents he would have been an excellent staff member. Not only could he accomplish a considerable amount of classical type of taxonomy but he would be able to bolster this with a number of modern disciplines. I think that he has demonstrated a considerable interest in modern techniques to educate students. His interest in "teaching machines" has led him to investigate the requirements of teaching various botanical subjects and I think that this demonstrates one of his positive contributions.

I really am not in a position to determine directly his ability to carry on administrative work. I do think that if his job is clearly outlined that the type of administration expected is known, and it is suggested to him exactly what is desired, that he would have as much capacity for carrying on such work as the next botanist. By this I mean that nowhere in anyone's graduate training is there given a clear picture of the requirements of administration. To my knowledge all botanical training is bent in the directions of increased knowledge about botany and I doubt that anyone has any particular skill to begin with. That such skill can be developed is obvious.

Frankly, I do not believe that Steve is the type of individual who will be particularly successful in personnel administrative relationships. I do not think that Steve has achieved a considerable amount of maturity outside of his own work. I certainly would not recommend him for any job where he must spend considerable amounts of time in the process of administration. If, on the other hand, the majority of his time is to be spent working with plant materials and teaching, then I recommend him strongly.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 26, 1966

Mr. S. H. Sohmer
Department of Botany
The University of Tennessee
Knoxville, Tennessee 37916

Dear Sy:

In response to your letter of January 17, 1966 and your sad tale of woe on your Mexican trip - all I can say is 'tis ever such with the study of Manihot. There are more rocks in the road than just the study of the plants. A

Are you sure you still want to stay at Tennessee for the PhD?

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 26, 1966

Mayfair Nurseries
R. D. 2
Nichols, New York 13812

Dear June:

Yes, we are out here collecting lichens and studying alpine plants. However, we still have the house in Upper Nyack and my mother is there. Our daughter, Mary, is taking care of the plants. We have sent three boxes of cacti from here which have been planted there.

Tell Walter that a few minutes from here at elevations starting at 7,000 to 8,000 feet there are just acres of bear berry. It is the dominant understory plant.

You might send several lists out here to us as Mary has given several talks on rock gardens and she has been trying to interest people in building them. She has just attended two days of conferences with the local nursery people and is at some turf conferences today.

Now that Walter is an author is he walking about with a lounge jacket on and acting like the squire of the manor?

Our local address is - 905 West Laurel Street
Fort Collins, Colorado 80521

Best to all - come out and see us.

As ever,

Henry S. Fleming
Professor of Botany

HSF/pam

PAPER PROPOSED FOR THE _____ SCIENCES SECTION
SOUTHWESTERN AND ROCKY MOUNTAIN DIVISION
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
42nd ANNUAL MEETING
and the
NEW MEXICO ACADEMY OF SCIENCE
ANNUAL SPRING MEETING
LAS CRUCES, NEW MEXICO, MAY 1 - 4, 1966

Author(s):

(If paper is of joint authorship, underline the name of the one who will make the presentation at the meetings.)

INSTITUTIONAL CONNECTION OF AUTHOR(S):

TITLE OF PAPER:

TIME REQUIRED FOR PRESENTATION:

PROJECTION EQUIPMENT NEEDED:

ABSTRACT
(Must not exceed 200 words)

CONFIDENTIAL

- Taxonomy Laboratory

January 27, 1966

Dr. Clifford M. Wetmore
Department of Botany and
Plant Pathology
Wartburg College
Waverly, Iowa

Dear Dr. Wetmore:

We have received your thesis and it is certainly very well done. I am enclosing a check for fifteen dollars (\$15.00).

By the way, I notice that you carry Heppia despresauxii as a synonym of H. lutosa. Mrs. Fleming sent you the Heppia material from the New York Botanical Garden sometime ago which included some specimens I had carried under the name H. despresauxii, collected about 30 - 35 miles north of New York City within sight of the Hudson River. It covered an area of 9 - 12 square yards on lime stone. It was the first time we had found it in the East.

I hope you will put our name on your reprint list. We have a copy of your The Lichen Genus Nephroma in North and Middle America.

As I wrote before, we are going to build up the herbarium collection of lichens at C.S.U. and intend to concentrate on northern Colorado and southern Wyoming initially. Weber and Shushan have done very little work in this area. If you have any duplicate specimens, they would be greatly appreciated. It is a bit too early for me to promise any to you.

I thank you again for the copy of your thesis. It will be very helpful to us.

Sincerely,

Henry S. Fleming
Professor of Botany

HSF/pam
Enclosure: check for \$15.00.

- Taxonomy Laboratory

January 27, 1966

Mr. Theodore W. Hurst
Worthington, Hurst and Associates
333 N. Michigan Avenue
Chicago, Illinois 60601

Dear Ted:

We finally have a printout of your information. Anytime that you are interested in coming out we'd be glad to go over it. We are already making some preliminary examinations so that we will be better able to go through the data when you get here.

Looking forward to seeing you.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 27, 1966

Dr. Paul F. Knowles
Department of Agronomy
University of California
Davis, California 95616

Dear Paul?

I have just received the copies of letters from S. M. Dietz to you. They are very interesting and I'm very glad to have this summary of the activities with relation to safflower and the information retrieval aspects of things.

Unfortunately, we did not write down a summary of our meeting at Berkeley. As a result, I cannot remember whether I was supposed to send you something or not. As I recall the situation, you were going to have a student code up information about one species as a trial run on our methodologies. Would you mind jogging my memory a little bit by telling me whether I suggested that I would send you a list or a set of instructions that would tell you how to get the data ready for computing. Please forgive me for my nebulous memory.

Have you been able to determine the most convenient time for going to Pullman next summer? I would like to begin to schedule time for the summer, and certainly want to have the time free that is best for safflower. Let me know when this time is as soon as you can determine it.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 27, 1966

Dr. Gerhard Van Biema
Geismar & Company, Inc.
10 East 40th Street
New York, New York 10016

Dear Dr. Van Biema:

May I apologize for not having answered your letter of November 23, 1965 any sooner. I am glad to hear that you are interested in contributing to the monograph suggested by Dr. Normanha. I have no notion as to whether or not this work will be published in any time that will be very useful to any of us. I have not yet submitted my own contribution on the systematics of manioc.

In your letter you requested information on the use of manioc leaves as animal feed. I enclose two reprints of my own that will indicate about as much as I know about this possibility. Unfortunately, I cannot give you the information that you asked for, namely, the approximate weight of leaves on a dry basis which could be obtained per plant growing. I cannot supply this information either for Thailand, Central America, or for Brazil. May I suggest, however, that there are a number of variables involved herein. There is no way to determine the amount of leaf per plant unless we specify the age of the plant. In the individual plant's development, it would probably have more foliage per plant at an age of 3 - 5 months, long before the plant is ready for harvesting for its roots. As the plant approaches maturity (for the roots) much of the foliage has dropped off and there are usually only leaves near the apex of each of the stems. However, the greatest variability is not on the individual plants, but between different varieties. That is, there will be a great variation from one type of manioc to the next and we would have to make a considerable study in order to determine that variety most suitable for the production of leaf material.

I suspect that you are interested in getting as much from

January 27, 1966

one plant as possible and you would, therefore, be interested in harvesting the leaves at the same time the root is harvested. This probable condition would require that you investigate the different varieties to determine that one which has the most potential interest to you.

As you can see from the enclosed reprints, there is a wide range of variation in the crude protein content of the leaves. This would be another factor of consideration when you go to select the most appropriate ones for the purposes you have in mind.

I hope that these comments are useful to you.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam
Enclosures

JAN 27 1966

Since digital computers with their capabilities for making comparisons and logical decisions are being applied as a taxonomic tool, there has been a reappraisal of the aims and purposes of systematics. The Adansonian concept of so called "over all similarity" has been emphasized as the "new approach." While it is not a new approach as is easily demonstrated, the characters used in constructing a classification based on 'over-all similarity' must be defined. Certain types of biological information is of second or third descriptive level and it is improper to incorporate such descriptors with primary descriptors in any taxonomic computer program that is presently available. These other variables are geographical distribution, allopatory and sympatry with their associated characters, and isolating mechanisms of various sorts.

Dr. Fleming

- Taxonomy Laboratory

January 28, 1966

Dr. R. K. Oldemeyer
P. O. Box 539
Longmont, Colorado

Dear Dr. Oldemeyer:

Please accept this as my reservation for dinner at 6:30 p.m.
on February 4, 1966 in connection with the Colorado-Wyoming
Interstate Genetics Seminar.

Thank you.

Sincerely,

Henry S. Fleming,
Professor of Botany

HSF/pam

1/28/66

DOCUMENTING HERBARIUM SPECIMENS OF MANIHOT ESCULENTA

Directions for Collecting for Herbarium Specimens

Fill out the label (sample attached) as nearly completely as possible.

1. LOCALITY. Give the country, the state, and the immediate locale. Add information about the nature of the soils in the vicinity.
2. COMMON NAME. If more than one common name is known for a particular variety of manioc, put down those that are known.
3. STATION NUMBER. If the plant is being grown at an experiment station give the local station's number. Add the age of the plant.
4. ORIGIN. If the origin of the plant (that is, from what particular part of the country it was procured) is known, add this.
5. STEM:
 - a. Color External. I have used the following designations for other collections that may be adequate in describing the external color of the mature stem. Silver (gray); brown; red-brown; yellow; or orange. These colors will encompass most of the variations in external stem color.
 - b. Color Internal. The color of the woody portion of the internal part of the stem may be white or some shade of yellow, or perhaps a whitish green. Record this color.
6. BRANCHING. This is a very important attribute of the plant to be known and is difficult to include in herbarium specimens. In order to determine the branching pattern, perhaps it is well to make a scaled drawing which indicates the point at which the plant branches and the number of branches at each point. Put a scale to indicate the height of the plant along side the branching pattern. It is very useful to have a photograph of the plant if this can be arranged.
7. LEAF:
 - a. Color of Young Foliage. This is an indication of the color of the young foliage at the apex of any stem and may be recorded as reddish, reddish blue, bluish, or green.
 - b. Petiole Color. Record the colors of the upper portion

of the petiole of a mature vegetative leaf at about a center point between the insertion of the leaf on the stem and the insertion of the leaf blade.

8. **ROOT.** An important feature of the plant for purposes of taxonomy is to make a note on the -
 - a. **Surface Condition** of the root which may be recorded as follows: If the external layer of the root is dark brown it will most frequently have a very rough epidermis. If the surface of the root is a light tan or grayish color, most frequently this root's surface will be smooth. Therefore, surface condition of the root can be recorded as either rough or smooth.
 - b. **External Color** of the root should be recorded as light brown, dark brown, tan, or any other appropriately designated color.
 - c. **Color of Subepidermis.** Immediately below the epidermal layer, which can be easily peeled off the mature root, one may note the color of the subepidermal layer which may be either white or pink, or in some rare cases, light yellow or tan. One should add, as well,
 - d. **The Color of the Flesh of the Root**, most frequently white, but sometimes yellowish.
9. **REMARKS.** Include any specific qualities of the individual plant: (bitter or sweet), age to maturity, etc.
10. **RECORD THE NAME OF THE COLLECTOR OF THE PLANT.** If he has his own collecting numbers be sure to record them. Be certain that the date of the collection is given.

Portions of the Plant to be Included in the Herbarium Specimens

We have standardized the parts of the plant to be included in the herbarium specimen. First, the collector should examine the variability of a particular variety as it grows in the field and try to select from the set of plants representing that variety, one plant which typifies or seems to be representative of all the plants of that particular variety. Such an examination should include the colors to see whether or not the leaf and stem colors are fairly standard. Look at the branching patterns of the variety and discover one plant that seems to represent most of the branching patterns for that particular variety. After making a selection of one plant one should also check the mature vegetative leaves on the plant and count the number of lobes. Usually, the mature vegetative leaves

will have a fairly standard number of lobes. The collector should examine several plants and count the lobes on the mature vegetative leaves and discover one leaf which represents the average number of lobes for that particular variety. This leaf that he selects should be in as near perfect condition as possible.

After having made a survey the individual specimens should be taken up, roots and all, and photographed, or if a camera is not available, then, as suggested above, the branching pattern of the plant should be made into a simple diagrammatic sketch. After taking up the plant the specimens to be taken should include the following:

1. A mature leaf should be cut from the plant. Cut the stem with the leaf so that there is a portion of the stem attached to the mature vegetative leaf.
2. A portion of the apex of a vegetative shoot. That is, about a fifteen or twenty centimeter sample taken at the apex of one vegetative branch. If the variety also has flowers and/or fruit on it, then the specimen should include samples of these with their colors given.
3. A thirty centimeter length of the mature stem of the variety. This stem may be cut in half lengthwise in order to facilitate drying.
4. One mature section of the root. Make a longitudinal section, approximately one centimeter thick through the median portion of the length of the root. This section of the root may be attached to the stem sections in order to prevent the root section from breaking. These may be immersed in a weak solution of formaldehyde, about a five or ten per cent solution will be satisfactory to kill the stem and root tissues and prevent fungus attack.

The leaf and flower materials should be dried between newspapers in a standard manner of herbarium specimen preparation. The stem and root sections may be dried in a hot air oven. Each variety should have a sample made in the same manner.

The collector's number, the label and any sketches of the branching pattern must be kept with the specimens AT ALL TIMES.

- Taxonomy Laboratory

January 28, 1966

Mr. Sidney R. Galler
Assistant Secretary (Science)
Smithsonian Institution
Washington, D. C. 20560

Dear Sid:

Thanks for your letter of the 24th. I have to be in Washington for a one day meeting on the 21st and would like to stop in and see you that night if you will be in your office. I recognize that the 22nd is Washington's birthday. Is this a holiday for you? If so, I might arrange to stay over until the 23rd. Let me know if you are available on the 22nd and if so where does the assistant secretary sit? Do they have you in a tower?

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

P.S. That is the 21st and 22nd of February.

- Taxonomy Laboratory

January 28, 1966

Dr. Ernest R. Sohns
Head, Science Information
Coordination Section
National Science Foundation
Washington, D. C. 20550

Dear Dr. Sohns:

I have to be in Washington for a one day meeting on Monday, February 21st. I wonder if it would be profitable to stop in and see you on the day following. I recognize that that is Washington's birthday, but I am not certain whether this is a holiday for you or not. Please let me know if you are interested and available sometime on the 22nd.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 31, 1966

Mr. Henry Mabie
Control Data Corporation
575 Lexington Avenue
New York, New York

Dear Henry:

I'd appreciate it if you would forward to me the port-a-punch cards for the last run on the computer. Also, if you have the S_{ij} 's for that run, I'd like to have them too.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

January 31, 1966

Mr. S. H. Sohmer
Department of Botany
The University of Tennessee
Knoxville, Tennessee 37916

Dear Sy:

Herewith are my comments and your manuscript returned. The work that you have here really is a contribution. I take it from your summary and conclusions that you do not feel that it is much of one. Let me assure you that it is. Having said this, now let me make a few comments about the way the paper is written as much as anything else.

First, I would suggest that you get together with a buddy, a graduate student who is majoring in English and see what he can suggest to you about the proper use of the English language. As many young and budding biologists, I can sense that you have not much use for the niceties of the language, but believe me, it is a critical matter and one that you can certainly profit from by going and getting your head knocked against a wall by an English major who can tell you lots and lots about the way to set up your sentence structures, your paragraphs, and so forth. This is something we all constantly need assistance with. I do and I've been at it considerably longer than you. There are far too many popular expressions included in this paper and they do not serve it well. You may not think that this work will ever see the light of day in print and it may not in its present form, but don't forget that such manuscripts as this have a way of being stashed away by librarians, and sometime, we can't tell when, someone may pick this paper up. With this in mind be careful how you express yourself.

Secondly, I made a few comments which I think are important in terms of the biological interpretation of the organism. You will see notes along in the margins where I have made such comments. I have not caught all of them. You will have to review it again. Consistency in terminology is very important in this field and you will do well to get a reference source

which gives you a good background in morphological designations of parts of the organism.

As far as the content of the content of the paper is concerned, I think that you have included enough material to more than satisfy a master's thesis. You will note, however, that I rather strongly disagree with your summary and conclusions. It is my opinion that you are doing yourself a disservice to make the types of negative statements that I have underlined. If you can't say good about your own research, don't say anything. It isn't required. I think you must have been at the bottom of some psychological cycle when you wrote that end part, and I think that you can do better by yourself and others with it.

Don't forget that you have all kinds of learning to do at this stage of the game, not only about your science, but about the way you talk about it. Any comments that I make are intended to give you some extra in both the content and how to say what you mean.

I hope that I haven't rubbed too much the wrong way and that you will let me see the paper after you have revised it once more.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 31, 1966

McGraw-Hill Book Company
330 W. 42nd Street
New York, New York 10036

Gentlemen:

Please send descriptive literature on the series of
reading texts by Maurice William Sullivan, recently
discussed in Time magazine.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

January 31, 1966

Park Sheraton Hotel
2660 Connecticut Avenue N.W.
Washington, D. C.

Gentlemen:

Please reserve a single room for me at the lowest rates available for the nights of February 20th, 21st, and 22nd. I expect to arrive at noon on February 20th.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 1, 1966

Dr. Robert E. Gordon
Head, Department of Biology
University of Notre Dame
Notre Dame, Indiana 46556

Dear Dr. Gordon:

I have a copy of Dr. Steere's letter to you concerning your interest in developing a new and vital program of taxonomy at Notre Dame.

One person you may wish to consider is a recent Berkeley graduate under Lincoln Constance. His name is Dr. Theodore Crovello. At the moment he is occupying a rather nebulous position under Dr. Robert Sokal, Department of Entomology, University of Kansas. Dr. Crovello indicated to me at the AAAS meetings in Berkeley this past Christmas that he was interested in locating a position and asked if I had anything for him. Dr. Crovello, who did his thesis work on Salix, is much interested in "numerical" taxonomy.

While the University of California department gives excellent training in some aspects of taxonomy, they have absolutely no interest, as a staff, in quantitative taxonomy, (a term I much prefer to numerical taxonomy). In spite of this, Crovello attempted to discover ways and means to get interested in it. He read the literature that was available to him and, unfortunately was taken in by the Sokal "School of Taxonomy." This "School" unfortunately began making noises before they had learned much about what was right and wrong with their own techniques. However, I think that Crovello is not too much indoctrinated yet by that "School" and could probably swing out on his own investigations if given the proper opportunities and the proper assistance.

I have not, myself, looked at his background, his curriculum, nor his record at the University of California and,

February 1, 1966

so, I cannot advise on the man's real ability. This would be something that you would have to check for yourself. I do know that he has a tendency to be somewhat brusque in his remarks and comments.

I wish that we were turning out students who might come and fill your position. Unfortunately, there are no students yet ready and it may be a couple or three years before we have one who has gone through a sufficient amount of training to be "modern" in his approaches.

I note that Bill Steere mentioned a chap named Dave Lellinger, spelled by Steere as Lullinger. Dave is a very competent and interested young man, but you might have to go higher than an Assistant Professorship to get him.

I hope this is useful information to you.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 1, 1966

Dr. Marshall C. Johnston
Department of Biology
S&ll Ross State College
Alpine, Texas

Dear Dr. Johnston:

I am trying to get more precise localities for specimens of the genus Manihot. I have a specimen which you collected on April 26, 1960, number 5363B which has been identified as M. walkerae. Your locality indicates that the specimen was collected in the state of Tamaulipas, Mexico on the Rancho Loreto.

I would appreciate it very much if you could give me a more precise locality for this particular plant.

Thank you for your efforts.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 2, 1966

Bausch and Lomb, Inc.
75828 Bausch Street
Rochester 2, New York

Gentlemen:

Would you be so kind as to send me information regarding the use of the Inclined Photo-Binocular Scope, PBV-S, (31-20-83-54)? Any other information you may have regarding this particular scope would be greatly appreciated.

At the present time I am borrowing the use of the above scope, but hope, in the near future, to purchase one of my own. Consequently, I would like any information, in the form of pamphlets, etc. that you have on your entire selection of scopes.

Thanking you in advance, I remain

Very sincerely,

Patricia Maurer

- Taxonomy Laboratory

February 2, 1966

Mr. Jack Sheean
116 North Main
Bloomington, Illinois

Dear Mr. Sheean:

Due to the whole series of clerical errors, it took until January 25th before three offices in this University got through processing the bill. As I understand it, on January 25th the appropriate bill was sent to Denver and I am advised that it takes from two to four weeks before you will receive payment.

I will have the apologies of the whole state of Colorado for this delay, but the payment should be forthcoming within the time mentioned.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 3, 1966

Dr. William B. Drew, Chairman
Department of Botany and
Plant Pathology
Michigan State University
East Lansing, Michigan

Dear Bill:

I would like to have a loan from the Herbarium of all of the specimens representing the genus Uvularia. In as much as there has been a good monograph and a number of studies of this genus, I would like to run a computer analysis of the group. I expect that we will be finished with this work by June of this year. The loan should be made to me in care of Dr. ~~J.~~ D. Harrington, Curator

Herbarium
Department of Botany
Colorado State University
Fort Collins, Colorado 80521

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 3, 1966

Dr. C. Ritchie Bell
University of North Carolina
Department of Botany
Chapel Hill, North Carolina

Dear Ritchie:

I would like to borrow the specimens of the genus
Uvularia. The loan should be made to me in care of Dr.
H. D. Harrington, Curator
Herbarium
Department of Botany
Colorado State University
Fort Collins, Colorado 80521

Thanks for your consideration.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 3, 1966

Mr. S. H. Sohmer
Department of Botany
The University of Tennessee
Knoxville, Tennessee 37916

Dear Sy:

Will you please check in the herbarium and determine the number of specimens and species of the genus Uvularia? If there is a good representation of this genus in the U. T. Herbarium I may wish to borrow these. *Please* let me know at your earliest convenience about this question. I will send a letter later with a formal request for a loan, depending on your answer.

Thanks.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 4, 1966

Dr. P. F. Knowles
Agronomy Department
College of Agriculture
Agricultural Experiment Station
University of California
Davis, California 95616

Dear Paul:

In answer to some of the questions in your letter of the 27th, the following is the best I can do for you. First of all you wanted to know the name and source of the color chart. The name of the color chart is a Nickerson Color Fan and you can get it from the American Horticultural Society, 1600 Bladensburg Road, N. E., Washington, 2, D. C. The price is, I believe \$6.50 plus postage.

You asked about people and addresses of persons interested in the symposium on "Wood." Are you expecting to develop the symposium yourself or is this something that the program chairman has to take care of? The most important person that I can think of in connection with wood is Dr. B. F. Kukachka at the Forest Products Laboratory, U. S. Forest Service, Madison, Wisconsin. I think he is by far the most outstanding well-rounded individual with respect to wood science of anybody in this country, and perhaps, in the world. I suggest that you contact him and tell him the plans for the symposium and ask him for a set of names. I would emphasize the inter-disciplinary nature of our society and that we are looking for people who may or may not even be called wood technologists. I would suggest that you contact your own forestry school if it is there on the Davis campus. They would very likely have a list of west coast scientists and companies interested in various aspects of wood. Certainly Crown Zellerbach would be one of the major places and you might write to their director of

February 4, 1966

research. Then there is the Georgia-Pacific Plywood Company. I'm sorry that I don't have the addresses of these people in my hands. Most of them are members of the Society for Economic Botany, however, and a letter to Bob Breach at the New York Botanical Garden, Bronx Park will get you the addresses of these people if they are members of the society or if they are subscribers to the journal without being members of the society. Another person who comes to mind whom I'm sure would be very much interested in this is Dr. William Stern, The Division of Botany, U. S. National Museum, Smithsonian Institution, Washington, D. C. Bill is very much interested in wood anatomy and I would even guess that he would be willing to talk about this as a part of the symposium if you have to organize that too.

I am still going over your format for information retrieval and will have some comments to make on it, but I'm not fully finished with it at this stage of the game. More will come on that later.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 4, 1966

Joseph Felsenstein
Department of Zoology
University of Chicago
Chicago, Illinois 60637

Dear Joe:

I have been meaning to write ever since I returned from Chicago. It was a real pleasure talking to someone whose mind was in gear.

I would appreciate it if you could find time to write me a short note explaining again that measure which you feel should be optimised to most successfully establish machine computed suggestions for phylogeny. "Heterozygosity", I believe, was the term you used for the thing being measured.

I hope your admittedly more interesting work in Ecology is progressing well.

Very truly yours,

George F. Estabrook

GFE/pam

- Taxonomy Laboratory

February 4, 1966

Dr. Richard Evans Schultes
Botanical Museum
Harvard University
Cambridge, Massachusetts

Dear Dick:

I am disturbed that I have not received back the original copies of the Manihot kodachromes sent to you earlier this past year. These slides are extremely important to me and I do not know how to tell you of the grievance of the loss that this imposes upon me.

Have you any suggestions for their replacement?

Sincerely,

David J. Rogers
Professor of Botany

DJR/pqm

- Taxonomy Laboratory

February 4, 1966

Dr. Harold C. Conklin,
Chairman
Department of Anthropology
Yale University
New Haven, Connecticut 06520

Dear Hal:

I am sending you two separate packages: one, the kodachromes and two, three specimens. The specimens are being sent express, collect. Will you please note the care that I have used in packaging these materials and return them to me in the same type of packaging. I need not emphasize how important these specimens are. Since we are still working with these and other specimens of Manihot esculenta, I will appreciate it if you could use them and return them to me within, say, a month.

Amongst the kodachromes you will find a series of slides numbered 1 - 5 which represent the processing of Manihot as it is done by Indians on the Upper Mazaruri River of British Guiana. The slides that you have here are copies of ones that I, unfortunately lost through the malpractice of Richard Evans Schultes of Harvard University. This is the last record of these slides.

The other kodachromes give some pictures of various structures of Manihot - flowers, leaves, roots - and variations in habit of the species. There are seven of these slides. I will also appreciate their return as soon as you have had an opportunity to use them.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 7, 1966

Dr. Roger A. Anderson
Botany Department
University of Denver
Denver, Colorado

Dear Dr. Anderson:

The date February 18 will be impossible as we have a special faculty meeting for that day. Let us try to choose another date for the get together.

Sincerely,

Henry S. Fleming
Professor of Botany

HSF/pam

A re-working of letter dated Feb. 1
Also, new address

- Taxonomy Laboratory

February 8, 1966

Dr. Marshall C. Johnston
Department of Botany
University of Texas
Austin, Texas

Dear Dr. Johnston:

I am trying to get more precise localities for specimens of the genus Manihot. I have a specimen which you collected on April 26, 1960, number 5363B which has been identified as M. walkerae. Your locality indicates that the specimen was collected in the state of Tamaulipas, Mexico on the Rancho Loreto.

I would appreciate it very much if you could give me a more precise locality for this particular plant, and other Manihot specimens which you collected on the same trip.

Thank you for your efforts.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 8, 1966

Dr. Robert L. Wilbur
Department of Botany
Duke University
Durham, North Carolina

Dear Dr. Wilbur:

I note on specimens of Uvularia that I have just borrowed your annotation label dated 1962. Have you published a monograph of Uvularia? If so, I would be very pleased to receive a reprint or at least a reference to it.

Thank you for your help.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 8, 1966

Mr. J. Morningstar
Morningstar-Paisley, Inc.
630 West 51st Street
New York, New York 10019

Dear Mr. Morningstar:

Since our last contact I have moved my operations from New York to the present address. This has not diminished my interest in the study of tapioca plants. Strangely enough I am able to pursue these studies with more intensity here than when I was in New York.

I wonder if I may call upon you for advice and possible assistance. I have been invited to participate in a symposium on the study of tapioca plants to be held at Belém, Brazil in June of this year. Unfortunately, I find myself without any financial assistance available to cover my travel expenses. Is there some fund available to you or can you suggest some resources that might permit me to participate in this very important meeting?

I will appreciate any advice that you may have on the subject.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 8, 1966

Dr. Gerhard Van Biema
Geismar & Company, Inc.
10 East 40th Street
New York, New York 10016

Dear Dr. Van Biema:

I wonder if I may call upon you for advice and possible assistance. I have been invited to present a paper on Manihot esculenta at a symposium on the Amazonian Biota to be held at Belém, Brazil in June of this year. I would like very much to participate in this symposium but find myself without any available travel funds. The regular channels that I would normally request assistance from have indicated that this year, because of tightening federal budgets, they cannot fund travel to this symposium.. I would like to inquire if you have available any funds to assist private workers, and if so, how should I apply? Or failing in that, if there are no funds available to you is it possible for you to suggest other sources of money to pay for my travel to Belém and back?

I will appreciate any advice or assistance that you can give me.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 8, 1966

Dr. George W. Argus
University of Saskatchewan
The W. Fraser Herbarium
Saskatoon, Canada

Dear George:

Enclosed is a data sheet we use as our input to card punching. The columns are used for your characters and character states (attributes) and the rows are for the objects that you wish to input. You probably are already going ahead with some sort of arrangement similar to the one we use for breaking your characters into states. There is nothing very precise about any instructions that we have and you, as much as any taxonomist, should be able to decide which characters are critical for your analysis. The number of characters used is not a particular factor. It is interesting to have as many characters used as possible, but there is no sound way that I can tell you how many are needed.

We have had some interesting experiences with Charlie Heiser on a study of Solanum nigrum complex. You may have seen his publication, "A numerical Taxonomic Study of Solanum Species and Hybrids" published in the American Naturalist, Vol. xcix, page 471, 1965. Charlie gave us his data and we have run it for him and gotten different results from those which he indicates in this study which was run on a Sokal model. Our results, which I think reflect very well the information as he gave it, doesn't please Charlie because it does not indicate precisely what he thinks ought to be the species and what ought to be hybrids. Our opinion is that we have done the very best we could with the data that he presented. Had Heiser not been so certain that he knew that he had a species I think perhaps he would be able to indicate a little bit more than he thinks he knows.

All this is stated here because I want you to be sure to know that the information given is the source and eventual

direction of what comes out, that if you do not include data concerning the genetic information you will not get an output that tells you anything about the genetic information, but I suggest that it will give some clues that may open your vistas a little wider than had been seen before. One of the greatest errors that taxonomists make in connection with the use of computers is that they expect the computer to tell them something about something that they gave the computer no information to work on.

Since you have not had an opportunity to read the methodologies that we use in our clustering I can understand how you might feel. The clustering technique will, I think, help you. Our clustering technique is one which is a part of the total procedure, and can be run directly, without any intervention on the operator's part. One result of the clustering technique provides what we call "articulation points" between clusters. An "articulation point" is an object or a specimen or a species which fits between two clusters and has connections to both clusters. Biologically interpreted these seem to be hybrids. I would suggest, therefore, the clustering technique that we have would be directly applicable to the problem that you present for Salix.

Inasmuch as we have the programs more or less keyed together would you object if we ran your data the whole way through and developed the clusters as giving you the similarities, which I think is what you meant by the term "Distance Matrix?" We expect these programs to be ready by the end of this month and I would guess that it will take you sometime longer than that to get all of the data prepared. I think that we will be ready to go before you will have finished up recording your data. At any rate go ahead. If you need more data sheets let me know and we can provide them. However, mimeographed sheets can serve just as well.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

Enclosure: Data sheet

P.S. I have just had an inquiry from two different people looking for taxonomists, preferably people interested in taximetrics. Would you be interested? One is from the University of Notre Dame and the other one is from the University of Western Ontario. If you'd like, I can recommend you to them or at least suggest that they contact you.

- Taxonomy Laboratory

February 8, 1966

Dr. Pierre Dansereau
New York Botanical Garden
Bronx Park
Bronx, New York 10458

Dear Pierre:

I wonder if I may ask your advice on phytogeographic maps of South America, Mexico, and Central America. What I'm looking for is a generalized map on about a one to a million scale; a map which would give me both the geographic distributions and, on the same map an outline of the countries. This is an attempt to discover some way of doing geographic coding on my Manihot materials and I would like to do it on the most rational basis possible. If you have any suggestions will you please let me know the source and the availability of such maps.

We are well set up here in Colorado now and would be very pleased if you could drop by to see us. We would look forward to a visit.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 8, 1966

Dr. Jonathan D. Sauer
Department of Botany
University of Wisconsin
Madison, Wisconsin

Dear Jon:

I wonder if I may ask your advice on phytogeographic maps of South America, Mexico, and Central America. What I'm looking for is a generalized map on about a one to a million scale; a map which would give me both the geographic distributions and, on the same map an outline of the countries. This is an attempt to discover some way of doing geographic coding on my Manihot materials and I would like to do it on the most rational basis possible. If you have any suggestions will you please let me know the source and the availability of such maps.

We are well set up here in Colorado now and would be very pleased if you could drop by to see us. We would look forward to a visit.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 8, 1966

Mr. George S. Bass
37 Mt. Vernon Street
Boston, Massachusetts

Dear George:

I am sending this letter to both of your addresses in hopes that I may be able to catch you somewhere between your various flights round the world.

I'm in a spot and I need some help. I have been invited to participate in a big stick symposium down in Belem, Brazil this coming June. Like most such invitations, they do not come accompanied with greenbacks. Could I tap you for some finances to make that trip? The thing I was thinking about was that if I could make that trip to Belem it would be no sweat to stop off in Colombia. Let me know if you can afford such as this. I don't have any idea at the moment about the total figures, but the base round trip air fare from here to Belem is around \$650.00. I hate to beg like this, but there is nothing left for me to do. All of my travel funds have either been used up or are ear-marked for some other expedition.

Best regards,

David J. Rogers
Professor of Botany

DJR/psm
CC - Apartado Aereo 13768
Bogota, Colombia

FEB 8 1966

During my recent trip (24 December 1965 - 7 January 1966)
I did indeed visit the several places mentioned and paid for
lodging at each of them. In my ignorance I did not save
receipts. However, please find attached hereto a copy of a
canceled check indicating that I paid for lodging at the
Indiana Memorial Union, University of Indiana

George Estabrook

FEB 9 1966

10 Weeks 2 Lectures One long lab. or discussion per week

Lecture Number	Subject Speaker	Contents
1. Introduction	Dave	Purpose of course, how we got into this ourselves. (Anderson's Exp.)
2. Computers	Bob	Simple exposition of how they work, what they can and cannot do.
3.		Examples of uses. (Anthrop. first part)
4. Mathematical concepts	Me	Sets, Arithmetic, logic. Definitions, unique determination, functions, and correspondences.
5. fundamental to objective		
6. thinking and modeling		
7. Review of the history problems and objectives of bio-class	Dave	What is classification for and what have others thought about it. (Davis & Heywood: <i>first 4 chapters</i>)
8. How do you describe plants	Hen	Coding and characters keys
9. and what can be done with the subscriptions	Dave	identification information preservation.
10. How do you measure similarity	Me	Discuss similarity measures
11. Problems biological with similarity	Dave	How does similarity measure influence coding decisions. What problems arise.
	Hen	
12. Solutions	Me	How can math. solve problems.
13. How should we define a	Dave	Discuss various definitions
13½ biological cluster	Hen	
14. How can we use these definitions	Me	Discuss efforts on cluster analysis
15. Other measures and methods	Me	
16. The role of the machine in using the method	Bob	Preparation of data formats for printout, etc.
17.	Dave	
18. Interpretation of results	Hen	Case studies
19.		
20. Presentation of results	Dave	Case studies, keys, diagrams
	Hen	
21. Recap.	Dave	Restatements of objectives and attitudes

Labs. and Discussions 10

1. Discussion of anything - D H B G plus class.
4. Davis-Heywood critical discussion of first few chapters.
3. Problem sessions in support
of lectures 4,5,6.
5. Case study - discuss how to subscribe some particular group of plants.
Re-describe case study with a similarity measure in mind
7. Test definitions of clusters on the case study.
6. Actually code the case study
- 8/ Prepare data for machine
9. Interpret the results
10. Present the results.

- Taxonomy Laboratory

February 9, 1966

Dr. Paul F. Knowles
Department of Agronomy
University of California
Davis, California

Dear Paul:

You asked me to give my judgment of the proposed uniform format for information retrieval given by S. M. Dietz. About all I can do is give my general impressions, for without knowing the philosophy behind the methods and without knowing what is intended or what is expected from the information retrieval system, I cannot make useful judgments.

I would have to know in some detail or have explained to me what was intended, who will use the system, who will want to know "what" from it, what is the size of the problem. Knowledge of this type of question precedes any ability to make any meaningful responses to it. Clearly what is needed is a well thought out set of objectives from the biological standpoint. Once these objectives have been established then some consensus can be reached on how to best utilize some piece of equipment of other to handle the problem.

In reading the Memorandum from Dietz, dated July 27, 1965, I get the impression that a considerable amount of time has been devoted to the set up of the data on the computer cards. I would consider the set up of the cards as the last problem that the biologist should have to worry himself about. Beyond the actual details of card punching described in the Memorandum there is very sketchy or no description of the program the computer is to follow. Without this program I cannot make any heads or tails of what's coming out and how it will be used.

In our own experience with one limited activity, namely classification, we have discovered that the most work is involved in setting up such a program, and is done long before one con-

siders anything about card punching or otherwise. It is usually the case, as it was with us, that the biologist begins in a very naive way to approach the use of the computer and, only after having his head banged against the wall does he discover that the computer does only that which it is instructed to do. All biologists should realize that they need very strong assistance from people in the computer milieu. But, getting the right assistance from people in the computer milieu is dependent upon the ability of the biologist to search out and discover the right person. You can find all kinds of computer salesmen and others interested in the machinery or hardware aspects who are willing to do anything as long as their computer is spinning, and this is a very serious failure for the biologist. In other words, it comes down to two real problems: one, the biologist himself and his willingness to think through his problem, and two, the right individuals knowledgeable of the computers to carry out his wishes.

I'm sorry to be so negative about this, but I will have to discuss with Dietz himself or with others exactly what he intends and I suspect that he cannot really give me the full intent of his methodologies.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 9, 1966

Mr. George W. Byers, Editor
Society of Systematic Zoology
Department of Entomology
The University of Kansas
Lawrence, Kansas 66045

Dear Mr. Byers:

The halftones for our paper are suitable.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 10, 1966

Dr. R. Z. Norman
Department of Mathematics
Bradley Hall
Dartmouth College
Hanover, New Hampshire

Dear Dr. Norman:

I was sorry not to have seen you personally during my recent trip east, even though our phone conversation was profitable. We contacted Robert Lauer but he seemed to be not interested in the position.

I have been told that I am accepted at The University of Massachusetts. However, there is some question still of the availability of financial aid. I would appreciate it if you would write to Wayman L. Strother (Chairman of the Dept. of Math.) at U. Mass. indicating to him why U. Mass. should provide me with financial support.

You may remember my telling you of a course offered by the math. dept. of U. Mass. for students in business. This course uses "Finite Math With Business Application" as a text. Some members of the business faculty are interested in having me associated with this course and have spoken to W. L. Strother on this point.

It is important that I receive some kind of financial aid if I attend U. Mass. next fall and I will really appreciate anything you can say on my behalf.

I wish you success with your own investigations.

Very truly yours,

George F. Estabrook

GFE/pam

The first operation as in normal systematic work is to assemble the data which is considered to be of taxonomic use. Since in the great majority of plant taxa this data will consist of taxonomic characters based on plant morphology, we shall discuss this aspect of character selection first.

As we have a classificatory frame work inherited from Linnaeus and refined subsequently by the labors of over two centuries of systematists, the work of present day taxonomists largely consists in correcting past errors and improving the classification by the inclusion of new data for selected predetermined and delimited groups. The erection of a higher taxon in the angiosperms such as a family, is unlikely to occur because of the discovery of a new species that cannot be placed in the present classification but may well occur as a result of adjustments within the present classificatory frame work. Thus, working with previously established taxa, the taxonomist inherits from previous workers very useful taxonomic data. He adds additional data of his own and attempts to make a new independent appraisal of the taxa in the light of any new taxonomic basis, procedures, principles, or rules.

The diagnosis, either of new taxa or redescriptions of previous inadequately described taxa by competent contemporary taxonomists (see Barnaby, 1964), consist not of complete descriptions of the taxon which would make inordinately long if ever really complete descriptions, but of a series of either quantitative or qualitative information that is comparable between the taxa being studied. Each item or specification in the diagnosis expresses a mode or condition for a distinct organ or portion of the plants

contained in the taxon. The differential diagnosis are selected items or key specifications that are useful in distinguishing a particular taxa from the other usually closely related taxa. The point being made here is that since we already have reference points within the structure of the present classification, it is not necessary nor desirable to have exhaustive and detailed descriptions of taxa which includes descriptive matter held in common by all the taxa under study. Just as genetic characters are not properly included in descriptions of species so characters that do not have comparable, differential value for the taxa under consideration should not be included.

These same conditions are also true when coding data for taximetric analysis. If the investigator is studying specimens, he first indexes the categories or characters that are to express the relations between the taxa. These characters that may be either qualitative, such as "color of corolla" or quantitative such as "length of sepals" are used as vehicles to limit or define the explicit private properties which specify the individual. A character is an abstract of a group of logically related properties, attributes, or states of the objects under consideration. The smooth and logical flow of good descriptions in the literature is a result of the ordering of characters so that the particular states for each character assumed by the various species, for instance, occur in a logical sequence. One commences with the *facies* of the plant which may include several characters such as light, branching pattern, etc., and proceeds next to some vegetative considerations such as shape of stem, pilosity of stem, etc. In

coding for computer analysis the same rational ordering of characters should be followed even though it is of no consequence at all to the digital computer.

The number of characters necessary to adequately describe any group of objects is dependent first, on the nature of the material. The nature of bacteria severely limits the number of morphological characters that may be used. Fortunately, in bacteria physiological characters have been useful. In the blue-green algae the relatively few characters that have been thus far discovered that have comparable value has made the group difficult for taxonomists. On the other hand, a genus such as Astragalus possesses many characters, some of which are restricted to plants only in particular geographic areas. Aside from the large number of species described, the large number of characters and the uneven distributing of these characters geographically has made this genus difficult for the taxonomist to study.

The second consideration in regard to the number of characters is dependent on the perspicacity and skill of the taxonomist. All the valid taxonomic characters should be included in the analysis.

- Taxonomy Laboratory

February 10, 1966

The Geographical Press
Columbia University
New York, New York

Gentlemen:

I would like to determine if the physiographic maps published by A. F. Lobeck are still in print. If so, will you please send me a catalogue and a price list. Any other catalogues of maps that you may have will also be appreciated.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

February 10, 1966

MEMO TO: Dick Ward

FROM: D. J. Rogers

SUBJECT: Travel funds for my trip to Brazil

George Olson advised that travel funds might be available by submitting to Hehn (Dir. of the Budget of Institutional Research) on Budget Form #3, a request for "indirect cost budget increase." He suggests that we support the request by a statement of the need. Herewith, a statement of the need: I have been invited to present a paper in a symposium entitled "The Amazonian Biota" before the Association for Tropical Biology. The Association for Tropical Biology is a new organization and has no funds within it to support travel for invited speakers. This symposium, however, is a very important one, and is international in scope. That is, there will be speakers from all over the world at this meeting. The meeting is to be held from June 6 - 12, 1966 at Belem, state of Para, Brazil. I feel that it is an honor to me to be invited to participate in this meeting and it certainly will reflect good upon Colorado State University as being represented at such an international meeting.

I hate to say this, but you might stick in some such dazzling type of phraseology as, "Dr. Rogers is the world authority on the genus Manihot, a food crop of fundamental importance to tropical peoples." Also, you might state in this same request that I have attempted to get federal funds for travel from the National Science Foundation. However, the National Science Foundation has not recognized this particular meeting as one of the major scientific meetings which they are supporting this year. Their budget has been apparently curtailed in support of travel by scientists. A budget for this trip would amount to the following:

1. Round trip economy jet to Belem \$655.90
2. Ground transportation
 - a. Car rental for travel from Fort Collins to Denver, rental price..... \$20.00
 - b. Ground transportation at Belem..... \$10.00
 - c. Ground transportation at intermediate stops in Miami and Baranquilla..... \$10.00
3. Per diem, \$15.00 per day, 10 days, ~~400~~.....\$ 150.00
4. Total budget request.....\$855.90

- Taxonomy Laboratory

February 11, 1966

Dr. William Bridge Cooke
1135 Wilshire Court
Cincinnati, Ohio 45230

Dear Bridge:

So far I have had no luck whatsoever in getting a student interested in the problems that we discussed at Berkeley. Unfortunately, the majority of the students here in mychology, at least at the moment, have interests other than in classificatory processes.

I am sorry to report this because I am vitally interested in the type of data that you have for yeasts and really do want to do something to help out if at all possible. I will keep stirring around to see if some student does not show some glimmering of interest in this area. In the meantime, it might be interesting to consider continuation of your vital studies on yeast with a separate grant request to the National Institute of Health. This strikes me as a very fundamental need and if anybody can handle the problem, certainly you can. If you wish to explore a joint application let me know and I will prepare some basic materials to support your request.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 11, 1966

Dr. Heber W. Youngken, Jr.
Dean, College of Pharmacy
University of Rhode Island
Kingston, Rhode Island 02881

Dear Heber:

According to Dacquist's request I am making a reservation
at The Motel Georgian. I hope this meets with your approval.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 11, 1966

Park Sheraton Hotel
2660 Connecticut Avenue, N. W.
Washington, D. C.

Gentlemen:

Would you please be so kind as to cancel the reservation
I made for a room in my letter to you dated January 31, 1966.
The room was reserved for the nights of February 20th, 21st,
and 22nd.

Thank you for your consideration.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 11, 1966

Professor H. N. Metcalf
Department of Plant & Soil Science
College of Agriculture
Montana State University
Bozeman, Montana 59715

Dear Professor Metcalf:

We will be glad to talk about the problem you present on
Iris. We would prefer some time during the week of May 7 - 11.
Any day will be okay.

Looking forward to seeing you.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 11, 1966

Dr. Jack Sharp
Department of Botany
The University of Tennessee
Knoxville, Tennessee 37916

Dear Jack:

This is a formal request for the loan of the Uvularia specimens. We expect to use this material only for a period from now through June to test some of our computer methods.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 11, 1966

Mr. S. H. Sohmer
Department of Botany
The University of Tennessee
Knoxville, Tennessee 37916

Dear Sy:

Thanksyfor your info on Uvularia. I have sent a letter to Jack Sharp requesting the loan of this material.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 11, 1966

Dr. C. J. Hickman
Department of Botany
The University of Western Ontario
London, ONTARIO,
CANADA

Dear Dr. Hickman:

I am sorry to report that I have no students as yet available who might fill the bill that you request in your letter of January 28, 1966 written to me at New York.

You will note that I have changed my address to the one above. This move was largely dictated by the fact that it was practically impossible for me to have graduate students at Columbia University who could collaborate at the New York Botanical Gardens. We hope to improve that situation here at Fort Collins. Incidentally, if you have undergraduates interested in graduate training in this area, you might let them know that we have instituted a well-rounded graduate program in systematics.

My only suggestion to you at the moment is that you might wish to consider a recent graduate of the University of California at Berkeley, Dr. Theodore Crovello. Dr. Crovello is at present a post doctoral student, working under Dr. Robert Sokal in the Department of Entomology, University of Kansas, Lawrence, Kansas. Dr. Crovello took his degree in systematic botany, working on the genus Salix. He seems to me a very intelligent young man and one whom you may wish to consider. I do not have any pertinent data concerning Dr. Crovello, but he may be interested in your position. I am sending a copy of this letter to Dr. Crovello.

Thank you for your inquiry.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

CC: Dr. Theodore Crovello

- Taxonomy Laboratory

February 11, 1966

Dr. Carl Berkley
MEDICAL ELECTRONICS
645 Stewart Avenue
Garden City, New York

Dear Dr. Berkley:

Thank you kindly for your letter of February 4, 1966. We appreciate the invitation to prepare papers which may be of interest to the readers of MEDICAL ELECTRONICS. There are several in the mill, some of which may or may not be applicable. At any rate I shall give you an opportunity to read the papers and decide whether or not they are suitable for your audience. Some of them have to do with basic biological problems, but ones specifically oriented towards the proper use of electronic data processing equipment.

I note that in your address to me you have me in The School of Medicine of Colorado State University. This is not exactly accurate in as much as I am a botanist and I am in The Department of Botany at this institution.

Most of our endeavors take examples from the botanical field rather than from animals. However, we feel that the endeavor is sufficiently fundamental that the type of organism employed has little total effect on the value of processes for classification. At least we hope so.

Thank you for your interest. We will probably submit some papers to you shortly.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 11, 1966

Dr. David D. Keck
National Science Foundation
Washington, D. C. 20550

Dear Dave:

I am remiss in not letting you know of my move to Colorado. The reasons for it are largely my interest in teaching. There was no opportunity with Columbia's situation, to do much in that respect in New York.

Since I plan to be in Washington on Monday, Tuesday, and Wednesday, February 21st through the 23rd, I might stop in to see you if you are available on either the 22nd or the 23rd. Do you work on Washington's birthday? I'm not sure. I'll give you a call when I get there.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 15, 1966

Information Office of the Organization
of American States
Washington, D. C.

Gentlemen:

I would like to have catalogues with details of maps available through the O.A.S. or other agencies known to you concerning small scale maps of South America and the countries south of the United States. I would like to know if there are topological and other types which I may purchase.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 15, 1966

The Office of Information
The United Nations
New York, New York

Gentlemen:

I would like to have catalogues with details of maps available through The United Nations or other agencies known to you concerning small scale maps of South America and the countries south of the United States. I would like to know if there are topological and other types which I may purchase.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 15, 1966

American Geographical Society
Broadway at 156th
New York 32, New York

Gentlemen:

I would like to have catalogues with details of maps available through the American Geographical Society or other agencies known to you concerning small scale maps of South America and the countries south of the United States. I would like to know if there are topological and other types which I may purchase.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 15, 1966

American Geological Society
231 East 46th Street
New York 17, New York

Gentlemen:

I would like to have catalogues with details of maps available through the American Geological Society or other agencies known to you concerning small scale maps of South American and the countries south of the United States. I would like to know if there are topological and other types which I may purchase.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 15, 1966

Map Information Office
Geological Survey
Washington 25, D. C.

Gentlemen:

I would like to have catalogues with details of maps available through the U.S.G.S. or other agencies known to you concerning small scale maps of South America and the countries south of the United States. I would like to know if there are topological and other types which I may purchase.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

2/15/66

Our work in taxonomy has two aspects:

1. Investigation of theoretical aspects of systematics.
2. Applications of methodologies derived from the theoretical investigations.

While we use botanical materials as the source-data for our own internal investigations, the results of our studies carry far beyond the purely botanical endeavors, into all biological fields (including medicine and agriculture), and into sociology and psychology. Examples given below indicate to some extent the scope.

= Our work involves, for the theoretical aspects, (1) above, both biological and mathematical investigations, and the flow of the work is from the investigation and discovery of the basic thought processes and rules of the systematic biologist, determination of the logic (or lack of it) involved in the rules, through the discovery of appropriate mathematical models which precisely reflect these rules. It is important to point out that most of the mathematics used in this operation are of a type not statistical, and also not analytical (e.g. differential equations, calculus, etc.).

Under the second heading above, there are two categories (at least). The first is the development of practical computer programs which carry out the biological rules, following the mathematical procedures discovered to reflect the rules precisely. The programs are tested using our own data, and then the second category of applications deals with the collaboration with other scientists, attempting to discover as broad a group as can possibly adequately use the computer programs. Some of the more interesting applications in which we are collaborating are:

1. Biological:
 - a. botanical: classification of strains of Streptomyces griseus,

with Dr. William Trejo of Squibb Institute of Research; classification of yeasts, with Dr. Wm. Bridge Cooke, U.S. Public Health Service; classification of lichens with Dr. Roger Anderson, Denver University; classification of genera of the tropical angiosperm family Chrysobalanaceae with Dr. G. T. Prance of the New York Botanical Garden; classification of sections of the genus Cassia, with Dr. Howard Irwin, the New York Botanical Garden; classification of the Solanum nigrum complex with Dr. Charles Heiser, University of Indiana; classification of the genus Oplonia, (Acanthaceae) with Dr. W. T. Stearn of the British Museum; classification of the genus Arceuthobium, with Dr. Frank Hawksworth, the Rocky Mt. Forest Experiment Station; classification of Orchids of the tribe Oncidiinae, with Dr. M. Wirth; and, hopefully, classification of interspecific hybrids of western species of Juniperus, with Dr. D. F. Van Haverbeke, of the Rocky Mt. Forest Experiment Station, Lincoln, Nebraska and several others.

b. zoological: classification of the marine isopods of the arctic and antarctic oceans, with Dr. Robert Menzies, Duke University; classification of marine snakes with Dr. H. K. Voris of the Chicago Natural History Museum.

2. Psychological: classification of job applicants and job categories with Dr. T. Hurst, of Worthington, Hurst Associates, Chicago.

3. Sociological: classification of job categories for the state of Colorado, with Mr. R. Ruff, Department of Labor, Denver; classification of native folk music, with Mr. Alan Lomax, Columbia University, New York. Historical. All of the above endeavors grew out of my own need to make a sound classification of the numerous varieties of

Manihot esculenta. The first investigations were carried out in collaboration with the I.B.M. Corporation, working with a mathematician Taffee

T. Tanimoto. Support for this research has come from The National Science Foundation, The Office of Naval Research, The National Institutes of Health, and from I.B.M.

The staff presently involved in the work include the following:
Dr. D. J. Rogers, Mr. H. S. Fleming, Mr. George Estabrook, and Mr. Robert Brill. Rogers and Fleming are systematic biologists, Estabrook is the mathematician, and Brill is the computer programmer.

We presently have grants from NIH and from ONR, and are anticipating soon the receipt of a grant from the Army Medical Service to study the classification of Cinchona, and other plants reputed to have anti-malarial drug derivatives.

Fleming, Estabrook, and Brill are paid exclusively by grants to me. The University pays nothing for their work.

- Taxonomy Laboratory

February 15, 1966

MEMO TO: Mr. Ed C. Chapman

FROM: D. J. Rogers

SUBJECT: Travel advance for trip to Washington, D. C. to attend
Anti-Malarial Convention.

ESTIMATED EXPENSE:

Plane fare	\$178.10
Bus fare	5.00
Taxi Fare	10.00
Hertz-Rent-a-Car	20.00

TOTAL TRANSPORTATION. . . 213.10

Per Diem: 4 days @ \$15 60.00

TOTAL ESTIMATED EXPENSE: . . . \$273.10

The source of repayment is the Walter Reed Army Institute of
Research, and, the actual re-imbursement will be by check soon
after returning to Fort Collins.

The dates of the trip are: Sunday, Feb. 20 - Wednesday, Feb. 23.

- Taxonomy Laboratory

February 16, 1966

Dr. Roger Anderson
Department of Botany
University Park
Denver 10, Colorado

Dear Roger:

March 4th will be all right. Suppose the three of you arrange to arrive about lunch time and we can all go out and have lunch together near by. Dave Rogers will give us all dinner that evening at his house. Any talking we have not finished in the afternoon we will continue in the evening.

We are in the old music building on the corner of South College and West Laurel. Thus, we are on the extreme northeast corner of the university campus. We are across the street from the Campus Shop (commercial book and coffee shop). I suggest going north on route 25, turning west on route 14 to second traffic light which will be South College and then south two blocks on College. Turn right and make a left on the first possible driveway back of an old red brick building. You will see a parking place near some greenhouses. By not wasting any time but still not picking up any traffic tickets, I reach the Denver airport in one hour.

Sincerely,

Henry S. Fleming
Professor of Botany

HSF/pam

Programafor the CDC 1604 based on the math model explained in Science (1960). This model efficiently grouped similar objects on the principles expressed in "Information Theory." It aggregated both the objects as defined by the attributes and the attributes as defined by the objects. The model was very sensitive and extracted those groups of objects that were the best of all possible groups and were non-equivocal. This program has been used by several other workers in various fields.

We also developed another program using part of the above program that discovered the same aggregatjions of objects and some additional aggregations that operated much more rapidly on the computer.

Various tests with the above programs indicated that while indeed these programs were useful and efficient, they were not adequate. It was desirable to develop clusters in which the interrelationships between the objects were much more intricate and in which these relationships were not greatly obscured.

Thus, a program was developed for the CDC 1604 using "information Theory" to detect the dependence and predictability of the attributes possessed by any set of objects. Thus, for this program has been tested on one group of objects and has been successful in producing the pbesence of attributes given a different set of attributes

Another more generalized classification program for the CDC 1604 was developed based on the mathematic disciple of graph theory. Graph Theory has been developed by the mathematician in the last ten to twelve years and we have found that

the application of some of the rules of graph theory has partitioned the objects to be classified in very meaningful clusters and has indicated the relationships of the objects both within the clusters and between clusters. It also constructs a hierarchical system which is pleasing and convenient to a classifier. Incidentally, it has freed us theoretically from spatial considerations, whether multi-dimensional, Euclidian, or non-euclidian.

We are now in the process of reprogramming this classification model for the IBM 7094 with complete documentation.

We are now developing the algorithm for determining how to specify, define, and separate determined groups of objects on the basis of the attributes of these sets of objects. The program will give the investigator lists of properties of the sets of objects at various classification levels that will partition and identify his objects. This is the problem we were approached with by Hudson labs.

Abstract for the Las Cruces Meeting, SW AAAS

Title: The Teaching of Taxonomy

David J. Rogers

Most of the teaching in taxonomy focusses on presentation of attitudes, and customs, not on methodologies. Methods for the aspiring taxonomist must be discovered by the inefficient process of imitation of older taxonomists, some of whose methodologies leave much to be desired. A new course at CSU focusses on the methods involved in the process of classification. The discipline of systematics is divided into component parts, namely: (1) discovery of a "taxonomic problem"; (2) decisions as to methods of attacking the problem; (3) developments of measures of similarities and differences; (4) methods of clustering the objects; (5) the development of diagnostic keys; (6) the presentation of formal classifications; (7) and, methods of presentation of results. The major emphasis in the course is placed on areas 3, 4, and 5, for these areas leave most to be desired in "orthodox" taxonomic work. With the aid of computers, and with the development of certain types of mathematics, much can be done to dispell the aura of mystery in classification.

- Taxonomy Laboratory

February 18, 1966

C. S. Hammond & Company
517 Valley Street
Maplewood, New Jersey

Gentlemen:

I am in search of a source of physical maps of Latin America and the Caribbean on a scale of approximately 1:10,000,000. I would like to be able to purchase these maps individually and not in atlas form. If available, I would like to have physical maps combined with ones which have the major political boundaries drawn in. If you have such maps available may I please have a catalogue describing them, giving prices. If you do not have such maps available, could you suggest a source?

Thank you.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 18, 1966

Rand McNally & Company
124 West Monroe Street
Chicago 3, Illinois

Gentlemen:

I am in search of a source of physical maps of Latin America and the Caribbean on a scale of approximately 1:10,000,000. I would like to be able to purchase these maps individually and not in atlas form. If available, I would like to have physical maps combined with ones which have the major political boundaries drawn in. If you have such maps available may I please have a catalogue describing them, giving prices. If you do not have such maps available, could you suggest a source?

Thank you.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 18, 1966

Dr. R. L. Wilbur
Department of Botany
Duke University
Durham, North Carolina

Dear Dr. Wilbur:

Thank you kindly for your reprint of Uvularia and the others enclosed with it. I am very pleased to receive them. Under separate cover I am sending several of my papers.

You may be interested to know that we will be teaching a course this year in which we will attempt to duplicate your monograph using computers. This is a classroom endeavor and we picked Uvularia because it represents a number of interesting problems and is still not too large to be managed by a group of advanced students.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 18, 1966

Local Board No. 16
48 Webster Avenue
Town Hall
Goshen, New York 10924

Gentlemen:

I wish to make the following statements about Mr. George Estabrook, who has just been classified 1A.

He is a valuable member of a research team, he plays a vital role in the teaching in the Department, and in contributing to the research activities of a number of allied scientists both at this institution and in other research organizations in the country.

For the past year and a half, Mr. Estabrook has been working as a research mathematician on a program entirely supported by federal government funds. The first year was spent at the New York Botanical Garden, Bronx, New York. In August 1965 he, and the three other members of the research program moved to Colorado State University, Fort Collins, Colorado, to continue the program under more nearly optimal conditions. In this new position, Mr. Estabrook has, as added responsibilities, the teaching of graduate students in the biological sciences. He will commence his teaching responsibilities in March, this year.

This research and teaching program has as its objectives the discovery of better means to diagnose diseases of plants, animals and man, the discovery of better drugs to treat man's ailments, and to produce better scientific methods to aid in such fundamental work. Mr. Estabrook has become one of the key men in the research team, and he cannot be replaced without losing most of the effectiveness of this team. In addition, his teaching activities will be directly related to the training of young men and women along the lines of this research activity.

Sincerely,

David J. Rogers
Professor of Botany

2-21

	<u>Mar. 23</u>	<u>Mar. 25</u>
	Introduction	Introductory Lab.
<u>Mar. 28</u>	<u>Mar. 30</u>	<u>Apr. 1</u>
History of Problems and Objectives	Computers - I	Davis & Heywood Lab.
<u>Apr. 4</u>	<u>Apr. 6</u>	<u>Apr. 8</u>
Computers - II	Math. I	Math Problems
<u>Apr. 11</u>	<u>Apr. 13</u>	<u>Apr. 15</u>
Math. II	How to Describe Plants	Examine Plants (Uvularia)
<u>Apr. 18</u>	<u>Apr. 20</u>	<u>Apr. 22</u>
How to Set Up Characters	Problems in Finding Similarity Measures	Code the Study
<u>Apr. 25</u>	<u>Apr. 27</u>	<u>Apr. 29</u>
Solutions to Similarity Problems	What is a Biological Cluster?	Test Clusters With Specimens
<u>May 2</u>	<u>May 4</u>	<u>May 6</u>
Development of Numerical Method	Brief Review of Other Methods	NO CLASS
<u>May 9</u>	<u>May 11</u>	<u>May 13</u>
Role of Computing Machines in Bio Modeling	How to Interpret Results	Prepare Data for Input
<u>May 16</u>	<u>May 18</u>	<u>May 20</u>
Presentation of Results-I	Present Results - II	Interpret Results
<u>May 23</u>	<u>May 25</u>	<u>May 27</u>
Future Applications, Implications, etc.	Recap	Make Classification

- Taxonomy Laboratory

February 22, 1966

Mr. W. V. Schmidlin, Pres.
William Tricker, Inc.
Saddle River, New Jersey

Dear Mr. Schmidlin:

Since we will be in Colorado for some time please send a catalogue to the above address. Keep sending your catalogue to the Upper Nyack address as that house is still open and selections will still be made from there.

On the way west this summer we stopped at Missouri Botanical Garden and visited for a couple of days with the Cutlers and Prings.

Thanking you for your consideration.

Sincerely yours,

Henry S. Fleming
Professor of Botany

HSF/pam

Regardless of the purpose of a classification, the structure of the classification will be dependent on the properties selected to describe the organisms. The method of selecting these properties in taximetrics do not differ in kind from those that have been customarily employed by taxonomists but do differ in that the method is more formal and explicit. The expression of the similarities and differences between the specimens will be dependent on:

1. The kinds of information used
2. The number of groups of logically related properties
3. The number of properties per group or character.

Some kinds of information that a taxonomist uses are not pertinent to the evaluation of the organisms similarities or differences. In addition, while the attributes or properties of the organisms are obviously either discreet or continuous, in both types some non-evident and non-trivial decisions must be made by the taxonomist to reflect the similarities of the organisms.

- Taxonomy Laboratory

February 23, 1966

Dr. H. N. Metcalf
Dept. of Plant & Soil Science
College of Agriculture
Montana State University
Bozeman, Montana 59715

Dear Dr. Metcalf:

I am answering your letter of February 19, 1966 in lieu
of Dr. Rogers who is in Washington, D. C. We certainly did mean
March 7 - 11. Sorry for the error.

Sincerely,

Henry S. Fleming
Professor of Botany

HSF/pam

Notes for WRAIR Natural Products (1) Meeting From D. J. Rogers

Comments before the meeting.

I. Basic botanical aspects.

- A. Quinine - the only known anti-malarial so far remaining effective against resistant strains of the organisms causing malaria is a plant derived product. The sources of this product (of several related alkaloids) is from a group of species known in very incomplete way - Cinchona. The plants of this genus may have other alkaloids (or combinations thereof) which
- 1) have more potential as a prophylactic
 - 2) have more curative abilities
 - 3) can be used as models for synthesis
 - 4) may be proved to be the only effective anti-malarial.

Therefore, we must, under no circumstances, ignore the basic studies of this group of plants. A method for studying the enormous biological variability to be found in this genus is by techniques developed in our laboratories (descriptive papers are appended to this report).

- B. Studies of other plant materials as potential sources of anti-malarial drugs.

There are numerous reports in folk-lore and more sophisticated publications of plants derivatives which are (or have been) used as agents either directly mentioning malaria, or other descriptive terms which may infer

malarial infections. These publications record substances from most tropical and subtropical areas where malaria is most prevalent.

If one records the plants mentioned, then arranges them in some systematic way, one finds that the reports are of a remarkably similar group of organisms. That is, the same families of plants are found to produce the extracted substances and that they are definitely not randomly distributed in the plant kingdom.

We must, to be sure, accept the word "cure" in these reports with much skepticism, discount those sources which are evidently copied from older publications, etc., but even with this, there are still some intriguing leads from such sources. Let us remember that plants still provide us with some of the most sophisticated curative agents, and that many of them were already known to primitive man.

II. Post conference comments.

- A. Testing - there is need for a more sensitive battery of tests, and this problem should be attacked by basic biological methodologies, by studying metabolism rates, and requirements of the various components of the life cycle of Plasmodium falciparum. Such in vitro tests should probably eventually, replace the mouse tests. The effectiveness of various agents as inhibiting to the malarial organism in one or another stage of its cycle could then more effectively be measured.

- B. While analysis of such basic testing procedures are under way, there should be a selective screening of natural products, first to determine the similarities of known anti-malarials (both synthetic and natural). Their mode of production (the metabolic pathways) in the plants should be investigated in order to discover methods of synthesis.
- C. While screening of the selected natural products is under way, there should be concurrent botanical studies to correlate any positive candidates with the plants from which they were derived. The obvious purpose is to make it possible to determine the most effective means of harvesting the plants (or plant parts) and deriving the desirable anti-malarial drugs.
- D. Once effective testing procedures have been worked out, then the screening of natural products on a larger scale could profitably be considered. However, pilot studies of natural products should be done first in order to establish an orderly flow of the work, train technicians, and get the various aspects of the work "in gear".

III. Summary

There seems no alternative to natural products screening.

The testing procedures must be improved by basic biological research.

There should be a step-wise development in the natural products anti-malarial search, proceeding from hints given in the literature to broader screening when the testing procedures are perfected.

- Taxonomy Laboratory

February 24, 1966

Joe Felsenstein
The University of Chicago
Department of Zoology
1101 East 57th Street
Chicago, Illinois 60637

Dear Joe:

I read your very prompt and complete reply with much interest. I also read Lynn Throckmorton's article "Similarity versus relationship in *Drosophila*" Systematic Zoology, Vol. 14, No. 3. Comparing his article with your explanation of your procedures, I would not give Throckmorton too much credit for what you have done. His influence is certainly recognizable, but what you have done is non-trivial.

I would like to make a few comments on what you have said. (I will assume that you have kept a copy of your letter to me).

There are two considerations (at least) in the development of a procedure such as yours: a) a conceptual or theoretical one which is confined to the context of the discipline in question, in this case the study of evolution from a biological point of view, b) a procedural one which concerns itself with the methods and procedures (algorithms) for discovering the results with the conceptual considerations indicate to be desirable. These two considerations can, and I feel should, be separated. However, it has been my experience that in most discussions in biology of processes (as opposed to discussions wherein results per se were presented) this separation is not well made. In your discussions to me of your method this dichotomy was not well preserved (I realize that it may not have been your intention to preserve it - you may not have even been aware of the consideration).

What I find genuinely interesting about your contributions here is the first consideration, (a), namely, the biological considerations of evolution. These well formulated and defined statements of evolution provide the biological substance on which an evolution model can be built. It is the primary function of a student of biology to concern and *address* himself to his biology. I think this obvious point has been tragically over-

looked by some other who are working in "numerical biology".

I don't find particularly interesting or impressive your efforts with the procedural considerations. I don't mean to indicate that they aren't as good as some other procedures that have been proposed, but I do not consider this phase of your method any real contribution. You realize, of course, that your method will not always find the best schemes of evolution according to your criteria (which speaks to the procedure, not the criteria), that your procedure is a pragmatic, "best I can do" approach which is not atypical of the work which is being generally done in "numerical biology." These arithmetic *peregrinations* are, however, not interesting from a mathematical or numerical analytic point of view and I contend are also basically uninteresting to a biologist. The development of the specific procedures and the programming and debugging of your program may have been interesting, challenging and rewarding to you (maybe not) but they don't really constitute much of a contribution in my way of thinking (for similar reasons I think much of Camin and Sokal - EVOLUTION, Vol. 19, : 311-326., does not constitute a contribution). Detailed descriptions of how you handle tables in the computing machine are not of particular interest to biology even though they may be very clever solutions to computing problems.

I would enthusiastically suggest that you expand the arguments presented in the "P.S.", i.e. the biological basis for your methodology into a short statement (a few pages) and mention therein that you have written a computer program which attempts to discover desirable evolutionary schemes, and submit this to, for example, the "research *notes*" section of "Systematic Zoology or Bio Science.

Very truly yours,

George F. Estabrook

GFE/pam

- Taxonomy Laboratory

February 24, 1966

Dr. Sam Shushan
Department of Biology
University of Colorado
Boulder, Colorado 80304

Dear Sam:

Enclosed are abstracts of two papers that Henry and I would like to present at the AAAS meetings in Las Cruces.

If at all possible we would like to have these two papers back to back, mine being presented first and Henry's second. This will make a good sequence and I think will be more meaningful if put together like that.

You will note that we have asked for 20 minutes for each paper, but if you feel that we cannot afford that much time then we will run each paper 15 minutes.

Hope this is satisfactory.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam
Enclosures

- Taxonomy Laboratory

February 24, 1966

Dr. J. F. Danielli, F.R.S.
Chief Editor, Journal of Theoretical Biology
The University Health Center
414 Michael Hall
Buffalo, New York 14214

Dear Dr. Danielli:

Thank you for your letter of February 14. We will be glad to make the necessary corrections. However, Dr. Sokal must still have the manuscript, for we do not, and apparently he has not returned it to you. Would you be kind enough to instruct him to mail it to us?

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 24, 1966

MEMO TO: Dr. Richard Ward

FROM: Henry S. Fleming

SUBJECT: Application for cryptogamic cases for herbarium.

Two interlocking card cabinets are required for the cryptogamic herbarium. These cabinets will be used to store lichen, moss and algal specimens. This manner of storing these materials is an innovation in herbarium custom. The advantage of preserving these plant groups in this fashion are:

1. smaller capital equipment cost in comparison to conventional herbarium cabinets
2. less space required for equivalent number of specimens
3. considerably better preservation of the specimens
4. smaller cost in both materials and time in the preparation of the specimens
5. the better filing system facilitates the study of the material
6. makes the recording and control of specimens loaned to other institutions much simpler.

Since the GSU herbarium has virtually no study or teaching collection of specimens in these cytogamic areas, no alteration of herbarium procedure will be involved, but the cryptogamic collection to be built up will be in the best possible arrangement.

John's Office Equipment, Inc. of Fort Collins carries Steelmaster interlocking card cabinets that are proper for this manner of storage and preservation. They supply units of 15 drawers which are $36\frac{1}{2}$ " high, 16" deep and $27\frac{3}{8}$ " wide for

MEMO TO: Dr. Richard Ward

-2-

February 24, 1966

\$99.95 (ninety nine dollars and ninety five cents). The University is granted a 20% reduction on this list price. The equipment is free on board at Fort Collins.

Two cabinets #5-3463 @ 99.95 ea. = \$199.90
20% discount = 39.98

Total \$159.92

These cabinets will be placed initially in the Taxonomy Laboratory.

- Taxonomy Laboratory

February 24, 1966

Dr. Annetta Carter
University of California
Department of Botany
Berkeley, California

Dear Dr. Carter:

Thank you for your offer of Manihot collections from Baja California. I would be very pleased to see this material as I am sure that it will fill out some gaps I have in that area. May I caution, however, that the identification may take a little time. I still have to sort out that which should receive names.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 24, 1966

Dr. H. C. Cutler
Missouri Botanical Garden
2315 Tower Grove Avenue
Saint Louis, Missouri

Dear Hugh:

Thanks for the reprint on "Corn and cucurbits from
Wetherill Mesa.

How's it going with MBG?

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 24, 1966

Dr. Walter Hodge, Program Director
Systematic Biology
National Science Foundation
Washington, D. C. 20025

Dear Walter:

Herewith is the expanded critique of the proposal by Rohlfs. Hope this is satisfactory. We feel that these comments are very important to be considered by the panel.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

Supplimentary Evaluation Sheet for Proposition B 6 1251 R (SYST)

Preliminary statement made on February 19, 1966.

At the outset we would like to clear up some confusion surrounding Dr. Rohlf's use of the term "on-line." On-line means "done by the computer" and off-line means "done by auxilliary equipment." As an example, data on cards may be entered into the computer in two ways: (1) the cards are placed in the computer's card reader and read directly into main storage (on-line), or (2) the cards are first put through a card-to-tape conversion via auxilliary equipment (off-line), then the tape thus prepared is read directly into the computer (on-line). From the foregoing it is clear that there is no such thing as off-line computation as mentioned on P. 3 of Dr. Rohlf's Research Proposal. All computation is done by the computer and is therefore on-line. The terms on-line and off-line have significance only when referring to the mode of input or output. However, from the context of Dr. Rohlf's proposal it is apparent that he means "on-line computing" to be synonymous with "computation under manual control of the operator" as opposed to the usual method of operating the machine at top speed under automatic program control.

The principal advantage of an electronic computer over a hand-operated desk calculator (and the sole economic justification for its use) is the extreme speed with which it can perform. Dr. Rohlf's so-called "on-line" approach to computation removes this advantage and effectively turns the computer into

an expensive desk calculator. It is true that the statistical routines of the Culler-Fried system run at top computer speed. However, between routines the machine idles while the investigator examines the results and decides which routine to try next. The time gained in using programmed routines is frittered away while figuring out what to do next.

We feel that the additional expense of Dr. Rohlf's manual approach brings with it no compensating advantages. On the contrary, we feel that the investigator who does his thinking on-line is under a disadvantageous pressure as he tries to work out his problems while an expensive machine is idling. In the usual approach to computation, an investigator runs his data with a program specially written by him or for him or with a regular package of routines such as the Bio-medical series mentioned by Dr. Rohlf on P. 3. He submits this program and data to the computation center and goes on to other tasks while the job is being processed. The job is run under the control of an automatic monitor which runs job after job in a highly efficient manner. When the results are returned, the investigator may give them a thorough examination without pressure, call in specialists or other colleagues to consult,^{1/} and plan his next computer run after a well thought out decision, during all of which time he is not paying for the use of a computer.

The above should demonstrate that the same man-machine dialogue can be established under automatic control as under manual control, but with savings in computer time and benefits in additional thinking time. It may be argued by advocates

of the Culler-Fried system that overall real time is saved by their method, since the closed shop turn-around time^{2/} is avoided. This observation is true. However, real time is not a critical factor in biological research (as it is critical in, for example, a radar defense system). Therefore the justifying criterion must be economical and one should choose the system which minimizes the expense of operating the computer.

Dr. Rohlf tries to justify the use of the Culler-Fried system in the first paragraph of his proposal. He criticizes the standard packages of statistical programs (such as the Bio-medical series) by saying, "It is all too easy for investigators to simply run their data blindly through standard programs." This may or may not be a true observation, but in either case, it is not a criticism of the packaged programs but of the slipshod methods of certain hypothetical investigators. Anyone who runs data "blindly" through a program is in trouble to start with and can certainly not be rescued by the Culler-Fried system or any other system. If we can assume that an investigator knows what he is doing, then we can assume he can solve his problems by a judicious use of standard programming techniques.

Dr. Rohlf criticizes the standard statistical packages also by saying that these programs give either too little output or too much. This too may or may not be a true observation, but it is not a criticism directed at the automatic vs. manual question under debate. If these programs do provide inadequate or overabundant output they must be corrected by modifying the

programs. Better output cannot be provided by establishing manual control of the computer.

Now we would like to turn to the three specific research projects enumerated in Dr. Rohlf's proposal.

(1) Development of operations for general statistical computations. The task that Dr. Rohlf has set himself here belongs properly to the domain of mathematics and statistics, and although biologists can sometimes usefully apply statistical operations, the development of these operations is not a part of biology. Therefore, we feel that this section of Dr. Rohlf's proposal is improperly submitted to the Systematics Panel.

(2) Computer assisted identification of organisms in numerical taxonomy. It is difficult to evaluate this section of the proposal as no explicit description of Dr. Rohlf's methods is given. We suggest that he expand this portion of his proposal and resubmit it.

We can say this much, however. Inasmuch as the taxonomic identification problem is one of information retrieval and file maintenance it is not suited to Dr. Rohlf's "on-line" approach. Appendix A of Dr. Rohlf's proposal is the User Manual for the Culler-Fried system. On P. 3 is a check list for judging the appropriateness of using this system for any particular application. By means of this check list we discovered that the taxonomic identification problem fails to be appropriate on four out of five counts.

(3) Computer assisted instruction. In this final area

of the proposal we feel that the Culler-Fried system comes into its own, for here in the teaching of the uses of statistical analysis the instant feedback of the manually controlled programs, together with the graphic display feature, are perfect pedagogical tools. We feel that this equipment for this usage should not be under the administration of a life-science department, but should properly come under the jurisdiction of the mathematics department or statistics laboratory where it can be of service to all who are teaching and studying statistical methods. Moreover, the equipment should not be installed so that long-distance communication is required to a computer in Santa Barbara, California. It is probably the case that the existing data processing equipment at the University of Kansas can be modified to handle this procedure. Some standard version of a remote keyboard inquiry station, together with some standard cathode ray tube output device, together with whatever computer already exists at the University of Kansas could be made to serve very well for manually controlled teaching exercises by writing a special control program around any standard statistical package already running on the machine in question. This not only makes more sense from the point of view of centralizing control of the operation and also simplifying it, but it would certainly save considerable expenditure. Dr. Rohlf's proposal calls for \$55,950 for long-distance transmission of data along and this solely for his own use of the equipment.

Moreover, Dr. Rohlf informs us (P. 7) that a new and faster computer is scheduled for Santa Barbara. This, in a

manually controlled system, confers no benefit whatever. On the contrary, if the new computer is not completely compatible with the existing model (and such incompatibility is typical of hardware changeovers), a nasty conversion problem could arise. On the other hand, we read in this proposal (footnote, p. 15) that the University of Kansas is firmly committed to move in the direction of on-line computation. If so, then let Kansas initiate, develop, and operate such a system of their own.

We suggest that if Dr. Rohlf wishes to use the techniques he describes for teaching statistical methods to his biology classes at the University of Kansas, he might try to interest the mathematics department in submitting a grant request (but not to the Systematics Panel) for the necessary equipment and programming. If this should bear fruit, Dr. Rohlf could then rewrite his own grant request to include funds for computer time on the Kansas machine.

In section XI of his proposal, Dr. Rohlf sets out his proposed budget. From this we learn that all three research projects proposed are to consume one day a week of the investigator's time during three nine-month periods out of the three years. In view of this light time commitment we feel that the budget proposed is high, particularly when it could be reduced by the suggestions made above.

We can summarize as follows: Project 1 - not for the Systematics Panel. Project 2 - not well defined. Project 3 - not under the right administration and not for the Systematics Panel. "On-line computation" not applicable to the first two research projects.

FOOTNOTES

1/ It is generally agreed that programming a computer is as much a full-time specialty as being a biological investigator, and most biological investigators whose work involves computer usage rely on the training and talent of professional computer programmers and/or mathematicians. The "free" time between runs permits the proper division of labor among these specialists before they put their heads together for a decision. Under the Culler-Fried system the team of specialists at the console must have a phenomenal quick-wittedness and rapport, or else, and this is rarer, the sole operator must combine in himself the talents of all those who should be helping him.

2/ ^{Turn} ~~Turn~~-around time: the time that elapses between submitting a job to a closed shop computation center and receiving the results. This varies from shop to shop and from day to day, but it is generally several hours minimum and can be as long as several days under poor operating conditions.

- Taxonomy Laboratory

February 25, 1966

Dr. Ernest R. Sohns
Head, Science Information
Coordination Section
National Science Foundation
Washington, D. C. 20550

Dear Ernie:

It was indeed a pleasure to discuss, under such very fine circumstances, the problems of information retrieval for biological sciences. Please tell your wife how much I appreciate the very fine lunch we had.

The next day I went over to the Smithsonian and talked for awhile to Sid Galler and to his new programmer, a man by the name of Suszynski. I did not have an opportunity to talk to the biologist most interested in this program, a fellow by the name of Don Squires, who was in the Antarctic. I understand that Squires is the one who would be most likely to head up any group at the Smithsonian interested in these projects. He is a systematic zoologist I believe, in the invertebrates. I also had a chance to go to the U. S. National Herbarium and there talked for awhile to Dave Lellinger. Dave strikes me as being the most knowledgeable (in the botanical group, at any rate) about retrieval programs. He, himself, apparently is not willing to devote full time to such a project but definitely should be kept at the top of the list as far as advisors on the program is concerned.

One of the things I emphasized with Sid Galler was the absolute requirement of collaboration between the biologists and the programmers. It's well that this emphasis was made because when I talked to the programmer I found that he was far from clear about what he should be doing, through no fault

of his own, of course. I also emphasized that one of the best ways to work would be with the collaboration of Biological Abstracts and I hope that they will take this advice to heart.

As I told you, we do not wish to take on the responsibility here, indeed we could not for a retrieval system for nomenclature in biology. However, we feel that there are several levels of retrieval systems and would like to try to investigate some of the newer programming techniques that we know to be available for the various parts of a retrieval activity. For example, there are several institutions whose collections are of about the same size as Colorado State University's. That is, they have herbaria with one, two, or three hundred thousand sheets in them and they have one, two, or three botanists interested in the systematic procedures. We could probably put together a decent project set up as a pilot program. If we can make a good information retrieval program for a small collection this may have some use across the country. Whatever program that we had would obviously have to be tied in closely with the nomenclatorial project at the top. However, the method of handling specimens, of collections, of names on specimen sheets, and so forth, we could also incorporate because of my interest in the monographic work with the genus Manihot. I have several ideas that I wish to get going just in terms of how to retrieve data from specimens which would probably be quite useful for others in getting rid of some of the hard and tedious jobs involved in preparing monographic treatments.

Would you be interested in a proposal along these lines? If so, we'll try to firm up our ideas and make a relatively straight forward project out of it.

Once again, thanks for your fine hospitality. I certainly enjoyed myself. Looking forward to seeing you again.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 25, 1966

Mr. George S. Bass
301 East 66th Street
New York, New York

Dear George:

Thanks for your letter of the 21st. I have some very faint clues about other potential utilizations of yuca. I have run across a group that is attempting to develop a "yuca chip" which is nothing more than a chipped up root dried and sent to Europe for cattle or animal food. I have not been able to get my hands on the exact group that is involved in this inasmuch as it came to me second-hand, but here is another use for yuca, that is, for a chipped dried product that can be used by various people for cattle feed. That is the only other use that might have any potential size involved in it. One other little thing that came to my mind recently has an intriguing possibility, although you will probably say, as I have to myself, that it doesn't have one heck of a large potential market. But, let me mention it anyway. When I was in Jamaica I noted that they made a very nice crisp little cocktail wafer from yuca. This is a product marketed locally in Jamaica and, to me, the product was certainly a delightful one. It was made from ground up root which had had the juices extracted and the wet product formed into a loaf about the size of a small loaf of bread. This was then sliced cross-wise through in thin layers, dotted with butter, shoved into the oven and baked until dry and crisp. There may be other aspects to the manufacture of this wafer or biscuit or whatever you wish to call it, but it sure looked like a good deal to me if it could be put out in any size or quantity. Does this sort of thing grab you?

This is about the only way that I have heard of any new uses for yuca. I hope this does you some good.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

February 25, 1966

Dr. Roger Anderson
University of Denver
College of Arts and Sciences
Department of Botany
University Park,
Denver 10, Colorado

Dear Rogers:

I have just received the notice from Sutherland that the next genetics meeting will be on the 4th of March.

Since both Dave Rogers and I would like to attend this meeting, perhaps you would allow us to renege on the dinner and we can all go over and have dinner with the genetics crowd at the Student Center.

Hoping to see you all on the 4th.

Sincerely,

Henry S. Fleming
Professor of Botany

HSF/pam

- Taxonomy Laboratory

February 25, 1966

Dr. William A. Weber
Curator of the Herbarium
University of Colorado Museum
Boulder, Colorado

Dear Bill:

I am glad to see your publication on the bibliography of Cockerell is out. I thank you for the copy you sent.

It certainly represents a tremendous effort on your part and you are to be complimented on it. On the negative side, it must have taken time from your museum and lichen activities and this, with the small staff (no staff) you have, leaves me with mixed feelings.

I knew old Cockerell, albeit, casually. Thirty years ago at CU I recall him telling me that when he was very young he decided to publish as many papers as possible in as many different journals as possible. He must have made this statement to other people as well because Will Beebe told me that the same statement in effect was made by Cockerell to me probably 20 years earlier. I know from personal experience that if I sent him one he would be very apt to publish on it. I really think you did a tremendous job.

I expect Roger Anderson on the fourth of next month. He is interested, along with Porter, in what we are doing. I met him at the last genetics meeting at Longmont. I also met Sam at the last couple of meetings. He is as entertaining and as enthusiastic as ever.

I have ordered through the Botany Department two cabinets of 15 drawers each for lichens. There is a very good chance that I'll get them.

Hope to see you soon. Very best to the family.

Sincerely,

Henry S. Fleming
Professor of Botany

- Taxonomy Laboratory

February 25, 1966

Dr. James D. Monk
University of Colorado
Department of Mathematics
Boulder, Colorado 80304

Dear Dr. Monk:

It was a pleasure speaking with you last Tuesday about the Graduate Program in Mathematics at the University of Colorado. Since that time I have read the printed material provided me by the department describing the program and there are some additional questions I would like to ask you. I learn from The Graduate School University of Colorado Bulletin, P. 89 that quite recently the Department of Mathematics and of Applied Mathematics have become one. The six page leaflet entitled Graduate Study in Applied Mathematics indicates that a Master of Science is awarded by the Department of Applied Mathematics. I would be interested in working towards a Master of Arts degree in Mathematics. Is such a program offered by the "new" mathematics department?

I was most interested and encouraged by your descriptions of the course work and seminars offered by the Department of Mathematics. I was further encouraged by the descriptions provided in The Graduate School Bulletin. I was discouraged by the descriptions of the Prerequisites and Qualifying Examination as presented in the six page leaflet earlier mentioned. I am not interested in differential equations and I don't mean to study them. Dartmouth College did not even offer a course in differential equations when I was there and, aside from my independent reading on the topic I have never formally encountered the discipline. I hope the "new" Mathematics Department does not continue to stress this topic so heavily

February 25, 1966

in terms of prerequisites. I feel my undergraduate preparation as a mathematics major at Dartmouth College should meet the prerequisites for any Masters Program in Mathematics that I should want to pursue. I am, in general, very enthusiastic about the program offered at C.U. but I am anxious not to find myself in a position where immutable rules, prerequisites, precedents, etc. prevent us from discovering a course of study for me which is satisfactory to both the Mathematics Department and myself.

I appreciate your consideration of these questions.

Very truly yours,

George F. Estabrook

GFE/pam

- Taxonomy Laboratory

February 25, 1966

Dr. R. L. Dressler, Executive Director
Organizado pela Associacao de Biologia Tropical
Rio de Janeiro
BRASIL

Dear Bob:

Thanks for your letter of the 15th. I have one more slight lead to get funds to come down in June. This is a private source of funds and may or may not come through. I'll let you know when and if I can make it.

I appreciate your suggestion of Louis Long, Jr., of the U.S. Army Natick Laboratories. Unfortunately, I know Mr. Long. Indeed we attempted to get together for funding some collections of Manihot, but for some reason or other Mr. Long seemed to think that we should subsidize his work by doing the collections for free, and this we found unpalatable.

You asked for Mike Wirth's address. The last address known for Mike is: 4507 20th Road, Long Island City 5, New York. I hope you can reach him and I also hope that he hasn't pitched his copy of the thesis out. Knowing Mike, I wouldn't be surprised if he has.

The other day in conversation with Tom Soderstrom I found that we can get maps of Brasil at incredibly low rates through you. I think that Tom said that if I sent you a check you could ask the Instituto Brasileiro de Geografia in Rio to send me all of their maps of Brasil on a millionth scale. Is it true that you are willing to perform such marvelous services? If so, please let me know and I will forward appropriate check. This would be a very fine service and I'm sure the best way in the world to get it.

Nothing else of importance now. I hope that we'll get some positive reactions on the various things above.

Sincerely,

David J. Rogers
Professor of Botany

- Taxonomy Laboratory

February 25, 1966

Mr. Mulford Martin
The New York Botanical Garden
Bronx Park
Bronx, New York 10458

Dear Mulford:

Thank you kindly for your prompt attention to my request for a Xerox of Pohl's publication. This is much appreciated. I have nothing else at the moment that I would like, but most likely in the future there will be such items as this past request, coming up.

Thanks again.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

Summary Remarks

In order to develop the present model for biological classification, it was necessary to choose some basic principles for a theoretical foundation and then define them carefully. The three principles chosen for the classification model are:

1. A biological classification for a collection of objects is a series of partitions for this collection, classes under later partitions consisting wholly of classes under earlier partitions. (For example, genera consist wholly of species.)
2. For any given partition in this series, two similar (see P. 8, No. 2) objects should not be placed into different classes (remembering that no knowledge of evolution is available).
3. The classes of a given partition should be isolated from one another; that is, there should be some phenotypic discontinuity between members of different classes.

The choice of these principles is extremely important, for they necessarily constitute the core of the model which embodies them. The principles chosen for the basis of a mathematical model should have at least three properties:

1. They should be consistent. Can they all be applied at the same time without leading to a contradiction?
2. They should determine a model. Do they provide enough constraints so that a unique (within the bounds of known parameters) simulation can be defined?
3. They should embody at least some of the practices of the discipline to be simulated.

The main text of this paper demonstrates that the principles chosen for this model have the first two properties above. The third property warrants some discussion. Clearly, all aspects of biological classification (even if the influences of evolution are not considered) are not summarized in the statement of these three principles. Some precise statement of principles is necessary to develop a model, and it is not always possible to make this statement reflect a consensus of biological thought, especially when no consensus exists. A model based on well defined principles has several advantages. Because it is rigorously developed from well defined principles, its results can be unambiguously interpreted in terms of those principles, and the information presented to the worker by the model can be retrieved in a straightforward manner. Because relatively few basic principles are employed and the model is logically derived from them, any worker is capable of understanding why the model works and can thus apply it with more confidence. This model does provide some means of using high speed digital computers as an aid in biological classification.

The general investigation of mathematical models for classification (biological or otherwise) is still in its early stages. The "taxonomy problem" is far from solved. The approach developed in this paper for one aspect of the taxonomy problem, an hierarchical clustering procedure simulates closely the traditional approach and has been successfully applied to a variety of problems. These are: delimitations of

genera within the family Chrysobalanaceae^{1/}, differentiation of several genera within the subtribe Oncidiinae of the Orchidaceae^{2/}, differentiation of the species of the Section Apoucouita of the genus Cassia^{3/}, selected strains of species of Streptomyces^{4/}.

1/ Prance, G., publication in preparation.

2/ Wirth, M., Estabrook, G., and Rogers, D. J., publication in press.

3/ Irwin, H. S., publication in preparation.

4/ Trejo, W., publication in preparation.

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review copy

- Taxonomy Laboratory

March 24, 1966

Dr. Peter H. Raven
Division of Systematic Biology
Stanford University
Stanford, California 94305

Dear Peter:

I am sorry I delayed answering your letter of the 8th for I am really excited to hear of your interest in the ethnobotanical work in Chiapas, and your findings on Manihot there.

The enclosed reprint gives some ideas I have about the early cultivation of M. esculenta, and indicates that the area you are working in may be very significant in this connection. There was an earlier paper with other details in the Bull. Torrey Bot. Club (90:43-54. 1963) but I am out of reprints.

Also enclosed are a set of instructions for the collection of data and specimens of Manihot cultivars. I have evolved this procedure over a number of years of study, and it covers the main points for the study of the morphological variability of the cultivars. For an ethnological study, I have not developed any clear set of instructions for data gathering, but can suggest a few items that might be useful. You should discover the cultivation techniques employed: when planted, whether interplanted with other crops or single-crop culture; any local superstitions about planting (dark of the moon, etc.); when harvested (month, or after the maize is harvested, or at irregular intervals); do they plant a single cultivar, or do they mix several together (I know you found only two, but look around--they may have more that they lump together under a single epithet). Take note of the way they use the roots: simple boiling, or more complex preparation; do they use any special utensils, baskets, or griddles. Note any specification they may have for the places of planting; close by their dwellings or removed; is the facing of the planting on an east-west, or north-south side of hills, or in valleys, etc. Any folklore should be investigated.

These are but suggestions for your data gathering. They are intended to discover whether the natives have a long history of cultivation, or whether rather recent. If they have a long history of using this crop, they will probably have a number of specific requirements. Your own interests will dictate the types of information

required for the project. You are fortunate to be working with anthropologists in this work--they can suggest good means of gathering useful information which will have meaning to botanists. Whatever you do, when you make up your labels of herbarium material, include the ethnological data along with the other interesting information. This way, the specimen becomes a source-document for information retrieval.

You mentioned that the plants you saw were not in flower. There may be several reasons for this: remember that these plants are selected for their root production; they are mostly reproduced by stem cuttings, (at least in cultivation) and unconsciously the natives may have selected some variant that doesn't flower frequently; you may have seen the plants during a vegetative flush, or conversely, during a dry period. I have so little information on the course of development of an individual plant that I can't be sure about any of these factors. I do know that there are varying times to maturity of the cultivars, and you may have found varieties that take much longer to mature (or flower) than others.

But whether the cultivar is in flower or not, take specimens of it, because we have to classify the plants. We can do quite a bit with the vegetative characters, indeed we must, because we are dealing with a plant used largely for its vegetative parts.

Manihot is complicated in meso-America. While you are working amongst the indians, ask them if they know of any wild species of M. in the neighborhood. There are some, I know, and the Indians may be aware of them. Be sure to make specimens of the wild ones, and include in the data any comments of the natives.

I hope these comments are useful, and that you will keep me up-to-date on your findings. Thanks, and good luck !

Sincerely,

David J. Rogers
Professor of Botany

DJR:ch

Enc.

- Taxonomy Laboratory

March 23, 1966

Mr. Bob Wright
Consulting Service
Western Data Processing Center
University of California
Los Angeles 24, California

Dear Mr. Wright:

The enclosed deck is the one which I have been trying to teleprocess to you since the 11th of March. As I have left instructions with our teleprocessing operator to continue trying to transmit this deck, you may end up with two copies. (Please forgive the non-Fortran character interpretation on the deck.)

Let me remind you of our problem with this deck. Twice now it has compiled without going on to a run of the object program.

Our phone is 303-491-5201 in case you have questions. If you can manage to squeeze some output out of this deck, please mail it to me (with a binary deck, please) at our address above.

Yours truly,

Robert C. Brill

RCB:ch

cc: Dr. David J. Rogers

- Taxonomy Laboratory

March 9, 1966

MEMO:TO: Mr. Bill Ault
FROM: Henry Fleming
SUBJECT: Requisition

Dr. Fleming would like a requisition for the following:

"The Genus *Lecidea* (Lichenized Fungi) in Rocky Mountain
National Park, Colorado."

The address where it is to be ordered from is:

f University Microfilms, Inc.
 313 First Street
 Ann Arbor, Michigan

The order no. is 65-4174. He would like the xerography
of this dissertation which costs \$9.90.

Charge to 403 . Thanks very much.

Henry S. Fleming

3/9/66

I have known the applicant for somewhat better than two years. At this time we had searched the mathematics departments of approximately twenty universities for a mathematician, preferably with a PhD who would mathematize biological principles of classification. Mr. Estabrook was one of the recommendations made to us (then, Dept. of Quantitative Taxonomy, New York Botanical Garden) by the math. dept. of Dartmouth. That his work has been satisfactory to us is attested by the fact that when we moved the operations to CSU, he accompanied us.

While he has been with us he has generated new approaches to establishing the basis of biological classification in the areas of botany, zoology, microbiology, medicine and allied areas. He has also performed had to define and delimit concepts in an area that has largely depended on intuitive methods before applying mathematical methods to resolve the problem.

He has written or been co-author on several papers while he has been with us. This and the work he has performed while with us makes me confident that he can do advanced study and research. He has given several lectures for us at various universities competently which gives me assurance that he has teaching ability. At all events we are having him teach some classes in a course we are giving this spring for graduate students and advanced undergraduates.

As regards character, I believe the expression is "he is of good character."

March 9, 1966

LETTER TO SUPPORT APPLICATION FOR GRADUATE ADMISSION AND/OR APPOINTMENT

I have known the applicant for somewhat better than two years. At this time we had searched the mathematics departments of approximately twenty universities for a mathematician, preferably with a PhD who would mathematize biological principles of classification. Mr. Estabrook was one of the recommendations made to us (then, Dept. of Quantitative Taxonomy, New York Botanical Garden) by the Math. Dept. of Dartmouth. That his work has been satisfactory to us is attested by the fact that when we moved the operations to CSU, he accompanied us.

While he has been with us he has generated new approaches to establishing the basis of biological classification in the areas of botany, zoology, microbiology, medicine, and allied areas. He has also perforce had to define and delimit concepts in an area that has largely depended on intuitive methods before applying mathematical methods to resolve the problem.

He has written or been co-author on several papers while he has been with us. This and the work he has performed while with us makes me confident that he can do advanced study and research. He has given several lectures for us at various universities competently which gives me assurance that he has teaching ability. At all events we are having him teach some classes in a course we are giving this spring for graduate students and advanced undergraduates.

As regards character, I believe the expression is "he is of good character."

- Taxonomy Laboratory

March 9, 1966

Mr. G. H. Kisbany
Office of Naval Research
207 West 24th Street
New York, New York 10011

Re: Contract Nonr-3640(00)

Dear Mr. Kisbany:

On the Final Technical Report, Contract Nonr 3640(00), the reports sent were incomplete and we are sending completed copies with this letter.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

Enclosures

- Taxonomy Laboratory

March 9, 1966

Mr. Robert Thacker
CNR Resident Representative
University of Denver
Columbine Hall, Room 118
2030 E. Evans
Denver, Colorado 80210

Dear Mr. Thacker:

On the Final Technical Report, Contract Nonr 3640(00), the report sent was incomplete and we are sending a completed copy with this letter.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

Enclosure

- Taxonomy Laboratory

March 9, 1966

Dr. Roger Reid
Biology Branch
Office of Naval Research
Washington, D. C.

Re: Contract Nonr 3640(00)

Dear Dr. Reid:

On the Final Technical Report, Contract Nonr 3640(00), the reports sent were incomplete and we are sending completed copies with this letter.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

Enclosures

- Taxonomy Laboratory

March 9, 1966

Dr. Helen L. Hayes
Biology Branch
Office of Naval Research
Washington, D. C.

Re: Contract Nonr 3640(00)

Dear Dr. Hayes:

On the Final Technical Report, Contract Nonr 3640(00), the report sent was incomplete and we are sending a completed copy with this letter.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

Enclosure

- Taxonomy Laboratory

March 7, 1966

Dr. Roger Reid
Biology Branch
Office of Naval Research
Washington, D. C.

Re: Contract No. Nonr-3640(OO)

Dear Dr. Reid:

Enclosed please find copies of our Final Technical Report for the above named contract. We have submitted other copies and the remaining details to the ONR New York City branch, specifically to Mr. Kisbany.

I thought you might be interested in the results obtained and our outlook for the future. Let me say that we wish to express our thanks to the Office of Naval Research for its fine cooperation and support of our work.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

Enc: Final Technical Report

cc: Dr. Helen L. Hayes
Biology Branch
Office of Naval Research
Washington, D. C.

- Taxonomy Laboratory

March 7, 1966

Mr. Robert Thacker
CNR Resident Representative
University of Denver
Columbine Hall, Room 118
2030 E. Evans
Denver, Colorado 80210

Dear Mr. Thacker:

Enclosed please find a copy of a completed contract technical report that was sent to the New York representative. I am sending this report merely as a matter of information for your benefit. I trust that if it raises any questions in your mind you will feel free to contact me. Our telephone number is: AREA CODE 303 491-5201.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

Enclosure

- Taxonomy Laboratory

March 7, 1966

Mr. G. H. Kisbany
Office of Naval Research
207 West 24th Street
New York, New York 10011

Dear Mr. Kisbany: Re: Contract Nonr-3640(00)

Enclosed please find the following:

- 1) Final technical report
- 2) A statement of property purchased with grant funds
- 3) A Statement that we have not borrowed or generated classified material
- 4) A statement that we have complied with requirements concerning personnel.

I trust that these are in order.

Thank you very much for your kind administration concerning our activities on this contract.

Sincerely,

David J. Rogers
Professor of Botany

DJR/ppm

cc: Mr. Robert F. Kolkebeck
The New York Botanical Garden
Bronx Park
Bronx, New York 10458

Dear Bob:

Finally I have sent the required reports to Mr. Kisbany. I am sorry that it has taken so long to prepare, but I suppose that you are used to that sort of thing.

How are things going in New York? We are all well here, have a new home on the edge of town where we overlook a big reservoir and the mountains. It's quite a setup. Give my best to all.

- Taxonomy Laboratory

March 7, 1966

J. Donald Monk
Chairman, Graduate Committee
Department of Mathematics
University of Colorado
Boulder, Colorado

Dear Dr. Monk:

Thank you for your encouraging reply. I hope to be given
the opportunity to study at University of Colorado next year.

Very truly yours,

George F. Estabrook

GFE/pam

- Taxonomy Laboratory

March 7, 1966

Mr. Bill Anderson
Chief of Data Processing Services
Western Data Processing Center
Graduate School of Bus. Adm'n.
University of California
Los Angeles, California 90024

Re: Our job order No. 2633

Dear Bill:

Here we go again! Last week I called asking how you could help us with the running of a program that is having trouble. You gave us a set of instructions. We followed those instructions explicitly and resubmitted. Again, absolutely no response. Nothing happened.

Today I called again and, not being able to reach you, I got a chap by the name of Penzales. From him we get a different set of instructions. He tells us that there are a series of published and circulated instructions for using the teleprocessor and the 7094 with FORTRAN IV. The last communication from WDPC received by Colorado State University came in March, a year ago. No other instructions - no nothing has been sent in order for us to keep up to date on procedures or changes in operating instructions.

How do we get off the ground with this program? All I can say so far is that I'm out about \$15.00 worth of telephone calls. No other results.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

March 7, 1966

Mr. James K. Page, Jr.
The Natural History Press
Central Park West at 79th Street
New York, New York 10024

Dear Jake:

I am finally answering your letter of the 14th. I am glad to know that work is going ahead with Wellman's plant pathology book. I suppose you expected that the first volume would be one where you would have to do alot of the suggestions on how the book should be edited and taken care of. Now that we have more of an idea of the process, we can do a better job on the succeeding works.

In this respect, it would please you to know, I hope, that I have about two more chapters to go and I will submit a manuscript to you on economic botany. I am stil bugged about a title for the thing, but hope that maybe something will jell before too long.

We really are enjoying ourselves here in Colorado. The place is just fine. The mountains are really beautiful, although I must say that we have not done much more than look at them from a distance. We really haven't had a chance to do much exploration. I bought a jeep as a second car and this should give us a chance to really do some back country investigations.

I was looking forward to a visit from you back last November. What happened? I will continue to look forward to a visit and will not take no for an answer.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

March 4, 1966

Dr. B. L. Shaffer
Continental Oil Company
P. O. Box 1506
Shreveport, Louisiana 71102

Dear Dr. Shaffer:

Your request for reprints and information on our computer programs was forwarded to me at the above address. We have moved our operations to this institution where we are continuing investigations on computer-aided classificatory procedures. At present, my group consists of two biologists, a mathematician, and a programmer.

The reason I explain this is to let you know that, if you are interested, we can provide competent advice on a wide range of related endeavors. Also, we are interested in a varied set of applications of our more recent programs, some of which you may find interesting. Your endeavors sound interesting to us, and different from other applications we have had

One of our programs, devised since the publication of the paper in BioScience, is described in a paper to be published in the next issue of Systematic Zoology. The mathematical basis for this particular program is in press in the Jour. of Theoretical Biol. The date of appearance of the latter paper is not certain. However, the program described in these two publications has largely replaced (in our lab.) the procedures we described in BioScience. While the methodologies of the information theory model have much to recommend them, we found a sufficient number of fundamental errors in them to cause us to shelve that program (the program decks are still available) in favor of one based on an entirely new mathematical model. Having tested this new program on a number of different problems, we find ourselves much better satisfied with it than the other. If you are interested, program decks of this new model can be made available for FORTRAN 63, CDC 1604, and will shortly be reprogrammed in FORTRAN IV for the IBM 7094 (which we are using here).

While the above described program is in operation, using as

input much the same types of data (formats are almost the same as those described in BioScience), we have, within our own group, learned to be much more sophisticated in the preparation of the data. We are now in the process of describing some of these new sophistications and modifications of data preparation. The ms. is through one rough-draft. This means, of course, that the paper probably won't appear for at least eight to ten months. If some of these newer ideas are incorporated, one's work has an even higher chance of giving the desired results. If these sophistications and modifications interest you sufficiently, we could tell you about them at some mutually convenient time.

Since I am not certain of your desired application, and whether or not you are interested in other pieces of the classificatory process, I will not mention other, equally interesting procedures which are now available, or soon will be. I shall be interested in your response.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

Encl. - reprint

- Taxonomy Laboratory

March 2, 1966

Refer to: MEDEC-ZM

Lt. Colonel Michael P. Dacquisto
Special Assistant to the Director
Dept. of the Army
Walter Reed Army Institute of Research
Walter Reed Army Medical Center
Washington, D. C. 20012

Dear Col. Dacquisto:

Enclosed herewith are comments that might be useful in your summarization of the meeting held on the 21st of February. In addition to comments that I had prepared before the meeting I have included the post-conference comments which strike me as being interesting to say. I have likewise included herewith two papers that have come from this laboratory describing our methodologies.

Shortly after the conclusion of the meeting I talked to Dr. Jacobus who seemed to be very much interested in our procedures.

I would be very pleased if, after you have had an opportunity to read these for yourself, you will pass the two papers over to Dr. Jacobus. Both of these papers are now in final stages of publication and should appear in print shortly. I trust they will have meaning to Dr. Jacobus and to you.

I want to express my appreciation for having been included in a very exciting meeting and I trust that the meeting had meaning for you as well.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

Enclosures

Taxonomy Lab.

Mar. 8, 1966

The Commercial Paper Corporation
The Herbarium Supply Dept.
300 Brannan Street
San Francisco, California 94107

Gentlemen:

This will constitute an order for the following

1,000 genus covers, Univ. South. Calif. style, manila
(Bonestell & Co. Durable Genus Covers), at \$93.00
\$9.30/100

We have placed a request for payment for these genus covers through our business office, on Purchase Order no. 9873, dated Feb. 21. This should appear in your office soon.

I trust that you can make shipment on the basis of this letter.

Sincerely yours,

David J. Rogers
Professor of Botany

- Taxonomy Laboratory

March 2, 1966

Ing. Agr. Julián Cámara-Hernández
Botanical Museum, Harvard University
Oxford Street
Cambridge, Massachusetts 02138

Dear Mr. Cámara-Hernández:

Thank you for your letter inviting my participation in the Symposium on the Ethnobotany of the Americas. I would indeed be pleased to participate.

I cannot answer your question whether I can obtain a grant for this travel until I have requested from the various granting authorities. I am at this moment writing to the usual sources for funds to determine whether or not this particular symposium is one of those to be sponsored by the National Science Foundation. If it is officially sponsored by the National Science Foundation there is some likelihood that funds will be forthcoming. Until I have had an affirmative or negative reply, however, I cannot definitely accept. You will hear from me as soon as I have any information.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

March 2, 1966

Dr. Ernest R. Sohn
Head, Science Information Coordination Section
National Science Foundation
Washington, D. C. 20550

Dear Ernie:

As I promised, I am sending you copies of two papers from this laboratory which describe some very exciting methodologies for computer classification. The paper by Wirth, Estabrook, and Rogers will appear in the March issue of SYSTEMATIC ZOOLOGY. The paper by Estabrook will appear sometime later this year in THE JOURNAL OF THEORETICAL BIOLOGY. Both papers are now in the hands of the printers. I hope that these are useful to you.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

Enclosures

- Taxonomy Laboratory

March 2, 1966

Dr. William J. Riemer
Biological and Medical Sciences
National Science Foundation
Washington, D. C. 20550

Dear Dr. Riemer:

I have been invited to attend and participate in a symposium at the XXXVIIth International Congress of Americanists to be held in Argentina in September 1966. I should like to determine whether this congress is one of those recognized by the National Science Foundation for which travel support may be obtained.

I will be pleased to hear from you.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/pam

March 1, 1966

Mr. Mark Halpern
Dept. 52-40, Bldg. 201
Lockheed Missiles and Space Co.
Palo Alto, California 94304

Dear Mark:

Regarding the subject matter of our two phone conversations, I'd like to summarize our position here at the Taxonomy Lab, with respect to our interest in XPOP.

(1) We are now running our programs (and continuing to write them) in FORTRAN IV and MAP for the IBM 7094 at the Western Data Processing Center in Los Angeles. Colorado State University has an order on an IBM 360, model 44, which we can expect to be using after the spring of 1967; Since this will give rise to a conversion problem even if we stick with FORTRAN, we are considering the possibility of converting at that time to XPOP and gaining the benefits of a more powerful and more flexible system than FORTRAN; I speak here of our continuing series of experimental programs designed to yield biological classifications.

(2) We are also planning to design and implement an information retrieval system for use with our herbarium collection, and which would ultimately be expanded for the use of other biological institutions for retrieving and correlating data from catalogues, museum collections and other compendia of biological information. For this application also we are interested in XPOP with its facilities for revising and extending both notation and data processing powers.

(3) Before we reach a final decision on the feasibility of converting to XPOP, we would like to clear up a number of questions concerning details of using the XPOP system, such as the necessary handling involved in hooking up XPOP to

March 1, 1966

IBSYS for the 360, using and modifying MPL-1 language, etc. At the moment we are all too busy here with our current effort to give much thought to our future with XPOP, but I'm hoping that we can get together later on, perhaps this coming summer, to iron out these details and hopefully clear the way for our change-over to XPOP.

Best regards to your family. Sylvia sends love.

Very truly yours,

Robert C. Brill

RCB/pam

P.S. I just received your communique with the Sneath reference and the New Scientist article on Numerical Taxonomy. Many thanks.

- Taxonomy Laboratory

March 1, 1966

Coast and Geodetic Survey
Room 1125
14th and Constitution Avenue, N.W.
Washington, D. C.

Gentlemen:

Will you please provide a list and prices of available GNC's,
1:1,000,000 and GNC's 1:5,000,000 for Latin America (Mexico,
Central America, South America, West Indies).

Any descriptive information will be appreciated.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

- Taxonomy Laboratory

March 1, 1966

Dr. Jacques Barrau
sous. directeur au Museum National d'Histoire Naturelle
Laboratoire d'Ethnobotanique
57, RUE CUVIER
PARIS, 5, FRANCE

Dear Jacques:

I am interested in your new address and position. Are you in charge of the Journal D'Agriculture Tropicale and Et De Botanique Appliquée? Could you give me information concerning the cost of subscribing to these journals? What will be your duties in the laboratory? I always wondered what a person interested in ethnobotany was supposed to do in a museum. My own experience as curator of economic botany never provided me with an answer.

I would be interested in your program, if you have time to write me about it.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam

March 1, 1966

TO: Mr. Bill Ault

FROM: D. J. Rogers

SUBJECT: Requisition forms for purchasing maps for use in the
Taxonomy Lab.

Mr. Ault, we need two requisition forms - one, for the following maps: 1A, 1B, 1C, 1E. The cost is \$4.00 per sheet and we are sending for four sheets, thus \$16.00 in all. We have also asked that they send us two more maps when available, but no money is needed for them. We are purchasing them from the American Geographical Society
Broadway at 156th Street
New York, New York 10032

The other requisition form is to be for three GNC charts Nos. 9, 18, and 24. They sell at 50¢ each plus 20¢ postage; thus, \$1.70 in all. That address is:
Rand McNally & Company
124 West Monroe Street
Chicago, Illinois 60603

I think Dr. Rogers needs these maps as soon as possible, so could you put this at the top of your "list" of important items for the day? Thanks so much

David J. Rogers PAT.