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

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

INTERNATIONAL ASSOCIATION FOR PLANT TAXONOMY

Office:
International Bureau for Plant Taxonomy
and Nomenclature
Tweede Transitorium, Uithof
Utrecht
(Netherlands)

Bankers: Messrs Vlaer & Kol, Utrecht

UTRECHT, 3 December, 19 71.

COPY

Dr. P. Legendre

6312, rue Saint-Denis

MONTREAL 326, Québec - CANADA.

Dear Pierre,

Thank you very much for sending me the article by yourself and Dr. Rogers. I shall be very glad to publish it in a coming issue of Taxon.

I shall try to send you two proofs but since this falls outside the normal routine I know that I might forget. In that case please just xerox the proof yourself.

There is a considerable delay in publication at the moment. I keep Taxon at ± 850 pp. for financial reasons but the number of articles submitted increases!

Can you review for Taxon the new book by R.G. Davies, Computer programming in Quantitative Biology? If so please let me know and I shall send it to your Lund address.

With all good wishes,

yours very sincerely,

F.A. Stafleu.

Dear

Thanks to both of you!

F

M

Genetiska Institutionen
Lunds Universitet

Institute of Genetics
University of Lund
Sölvegatan 29
S-223 62 LUND, Sweden

November 29, 1971.

Dr. David J. ROGERS,
Food Research Institute,
Stanford University,
Stanford,
California 94305, U.S.A.

Dear Dave:

Many thanks for your nice letter of November the 22nd, and for the accompanying documents. I am glad to hear that you are enjoying yourself in California, and that you find interested ears to talk to about sound taximetric principles. And you must have been lucky with your sample of French Canadians: our prime minister(s) would tell you that most of us are totally impossible some of the time, and some of us are so most of the time. But since you seem to enjoy impossible people, when will we have the pleasure to see you in Montréal ?

Your letter came just in time: I had mailed, in the institute's mail box, a letter to you the day before. But since they are not fast with mail around here, it was still in the mailbox when your letter came, and I got it back. I was sending to you a project of summary, since I had remembered that it was for another manuscript that I had prepared one. After looking at your summary, I put both yours and mine away, and wrote a third one, which is included with this letter. XXXX We can still modify it in the proofs, if necessary.

Also included are small copies of my fac-simile printouts. Will it do ? - actually, I took photographic pictures of all the illustrations, in case thet get damaged before reaching the editor. The printout we had for figure 1 did not come out nicely on the first pictures I took, so I decided to make a fac-simile for it too - O.K. ? Finally, I include a copy of the last page of the manuscript. I did not quite understand the section on "Availability of the programs", so I corrected it as shown. Is it what you meant ?

We are going home for Christmas, leaving Sweden on the 15th of December, and coming back a month later, after a visit at my brother's place, a few km east of Nice, on the Côte d'Azur ! We still enjoy very much our stay in Sweden, but the days are getting terribly short, and we envy a little bit those, like you, who live in the sun and far from the fog!

Our best regards to your wife and to yourself.

Pierre

Pierre Legendre

Fig. 1

CHARACTER I = 1 (6 STATES) COMPARED WITH CHARACTER J = 2 (5 STATES). D(1, 2) = .67534 S(1, 2) = .32466

OBJECTS NOT IN THE COMPARISON = NONE PROBABILITY OF GOOD OBJECTS = 1.00000

PROBABILITIES OF THE STATES OF CHARACTER 1, GIVEN THE GOOD OBJECTS ENTROPY IN CHARACTER 1 = 1.82257

I(1)	I(2)	I(3)	I(4)	I(5)
.10000	.48333	.30000	.08333	.03333

CONDITIONAL PROBABILITY DISTRIBUTIONS

	I(1)	I(2)	I(3)	I(4)	I(5)	CONDITIONAL ENTROPIES
J(1)	1.00000	0.00000	0.00000	0.00000	0.00000	-0.00000
J(2)	0.00000	1.00000	0.00000	0.00000	0.00000	-0.00000
J(3)	.02222	.57778	.37778	.02222	0.00000	1.23188
J(4)	0.00000	0.00000	.14286	.57143	.28571	1.37878

ENTROPY REMAINING IN CHARACTER 1 AFTER OBSERVING CHARACTER 2 = 1.08478
INFO. COMMON TO BOTH CHARACTERS = .73779 FRACTION OF INFORMATION IN CHAR. 1 ALSO CONTAINED IN CHAR. 2 = .40481

PROBABILITIES OF THE STATES OF CHARACTER 2, GIVEN THE GOOD OBJECTS ENTROPY IN CHARACTER 2 = 1.18773

J(1)	J(2)	J(3)	J(4)
.08333	.05000	.75000	.11667

CONDITIONAL PROBABILITY DISTRIBUTIONS

	J(1)	J(2)	J(3)	J(4)	CONDITIONAL ENTROPIES
I(1)	.83333	0.00000	.16667	0.00000	.65002
I(2)	0.00000	.10345	.89655	0.00000	.47983
I(3)	0.00000	0.00000	.94444	.05556	.30954
I(4)	0.00000	0.00000	.20000	.80000	.72193
I(5)	0.00000	0.00000	0.00000	1.00000	-0.00000

ENTROPY REMAINING IN CHARACTER 2 AFTER OBSERVING CHARACTER 1 = .44994
INFO. COMMON TO BOTH CHARACTERS = .73779 FRACTION OF INFORMATION IN CHAR. 2 ALSO CONTAINED IN CHAR. 1 = .62117

Fig. 2 - First part

```
L= 1          C( 1)=  .86667

CLUSTER MEMBERSHIP                                 C-VALUE  CONNECTEDNESS      R( 1)
114  132
                                                   .86667       1    1         (132,114)(
MDAT =   .06667   NEXT PAIRS TO JOIN  (131,114) (

SINGLE MEMBER CLUSTERS (  8)
108, 131, 242, 281, 284, 330, 377, 454,


L= 2          C( 2)=  .82667

CLUSTER MEMBERSHIP                                 C-VALUE  CONNECTEDNESS      R( 2)
108  284
                                                   .82667       1    1         (284,108)(
MDAT =   .06667   NEXT PAIRS TO JOIN  (284,281) (377,284)

CLUSTER MEMBERSHIP                                 C-VALUE  CONNECTEDNESS      R( 2)
114  132
                                                   .86667       1    1
SINGLE MEMBER CLUSTERS (  6)
131, 242, 281, 330, 377, 454,


L= 3          C( 3)=  .80000

CLUSTER MEMBERSHIP                                 C-VALUE  CONNECTEDNESS      R( 3)
114  131  132
                                                   .80000       2    3         (131,114)(
MDAT =   .05667   NEXT PAIRS TO JOIN  (242,132) (

CLUSTER MEMBERSHIP                                 C-VALUE  CONNECTEDNESS      R( 3)
281  377
                                                   .80000       1    1         (377,281)(
MDAT =   .04000   NEXT PAIRS TO JOIN  (284,281) (377,284)

CLUSTER MEMBERSHIP                                 C-VALUE  CONNECTEDNESS      R( 3)
108  284
                                                   .82667       1    1
SINGLE MEMBER CLUSTERS (  3)
242, 330, 454,


L= 4          C( 4)=  .76000

CLUSTER MEMBERSHIP                                 C-VALUE  CONNECTEDNESS      R( 4)
108  281  284  377
                                                   .76000       4    6         (284,281)(377,284)(
MDAT =   .02667   NEXT PAIRS TO JOIN  (377,330) (

CLUSTER MEMBERSHIP                                 C-VALUE  CONNECTEDNESS      R( 4)
114  131  132
                                                   .80000       2    3
SINGLE MEMBER CLUSTERS (  3)
242, 330, 454,
```

Summary

The problem of producing a classification from data gathered on specimens has two main components: first the information about the specimens must be structured as characters and character states in such a way that it carries the most information about the taxonomic structure of the objects under study, the mathematical "noise" being eliminated as much as possible. Then this information must be handled in such a way that a hierarchical partitioning of the objects, called classification, is derived.

This paper presents computer-aided methods for the accomplishment of these steps. These methods were worked out to be both mathematically and biologically sound. The character analysis method (called CHARANAL) uses information theory to measure the amount of information common to pairs of characters, and derives from it various measures for the comparison of characters. The clustering technique presented here (entitled GRAPH), on the other hand, is based upon graph theory, and is intended to represent the thought process of the "classical" taxonomist. For each method are given a general explanation, a detailed explanation of the mathematics involved, an example, and a section on interpretation of results.

Résumé

Une classification produite à partir des données recueillies sur des spécimens résulte de deux manipulations successives de ces données: il faut d'abord structurer l'information que l'on possède sous la forme de différentes descriptions d'un certain nombre de caractères, et faire en sorte que l'information taxonomiquement significative soit préservée et que le brouillage mathématique soit éliminé le plus possible. Par la suite cette même information doit servir à produire une série hiérarchique de partitions des objets sous étude, ce qui s'appelle une classification.

Les auteurs présentent ici pour ce faire des méthodes d'analyse par ordinateur dérivées de principes biologiques et mathématiques reconnus. Pour l'analyse des caractères (programme CHARANAL), l'information commune à des paires de caractères est mesurée suivant les principes de la théorie de l'information, de qui permet de dériver différentes mesures pour la comparaison des caractères. La technique de groupement des objets (programme GRAPH) dérive d'autre part de la théorie des graphes et essaie de reproduire aussi fidèlement que possible le cheminement de la pensée du taxonomiste dit "classique". À la suite de l'explication générale, on retrouvera une explication détaillée de l'aspect mathématique, un exemple ainsi qu'une section sur l'interprétation des résultats, ce pour chacune des deux techniques.

and also other classifications can be compared with the preliminary structure. This process has been discussed by Rogers and Appan (1969).

One has to be a biologist to make a biological classification, or a sociologist to make a sociological one, since only a specialist (sensu lato) can interprete in the correct way the various attributes of the objects under study. An advantage of biology over, for instance, geology, is that one knows that there is a genetic basis for the similarities and dissimilarities observed, and thus there is hope to find a "natural" classification. But one does not have to be a mathematician to understand and use the methods explained here; by applying them, he will realize that they are intended to help him to work according to his own mental process as a taxonomist.

Availability of the programs

Fortran IV

Available: listings, CHARANAL and Graph flow-chart; ~~CHARANAL~~

At IBM: Graph, version of the program written for an IBM 7044 is available from the Program Information Department, IBM, 40 Saw Mill River Road, Hawthorne, N. Y. 10532. Library #3501.

November 22, 1971

Dr. Pierre Legendre
Institute of Genetics
University of Lund,
Lund, Sweden

Dear Pierre:

I finally did what should have been done much sooner. I plead that I have had to give too many seminars in and around Stanford to get much else done.

Enclosed are: a copy of a graph printout as you ask. I think the one level contains sufficient information to let you make a facsimile; a copy of the title page, with some suggested changes in the footnotes, and a suggested summary. I think the summary could be expanded to give some more precise information about the contents, but I wasn't sure what you feel to be the most important to be included in a summary.

If we had a summary, I could not find it. Perhaps you are recalling some other paper of yours.

We are enjoying life quite a bit. Interestingly enough, there is French Canadian graduate student and his wife here who are almost as nice as the Legendres. From my sample of people from Quebec, they must be the most intelligent people in the world!. In other areas, I have lectured before the biosystematists of the San Francisco Bay area (Berkeley, Stanford, and U. Calif.-Davis and a few other schools represented) on taximetrics, and have had all kinds of nice things said as a result. I've lectured on economic botany here at Stanford, and on Manihot before the people of this institute. We are enjoying the tremendous variety of good restaurants around here, and getting fat as a result. But in some ways, I still like Boulder better than here.

Hopefully, I have done all needed to get this manuscript out of our way. If not, let me know, and I'll try again. Thanks for all your efforts.

Sincerely,

Genetiska Institutionen
Lunds Universitet

Institute of Genetics
University of Lund
Sölvegatan 29
S-223 62 LUND, Sweden

October 18, 1971.

Dr. David J. Rogers,

Food Research Institute,
Stanford University,
Stanford, California 94305.

Dear Dave:

Thank you very much for the manuscript and your nice letter. Both arrived this morning, to my surprise: I did not expect that you would have time to go through the manuscript and send it that quickly !

Today, I made a couple of chromosome fixations, so that I did not have time yet to go through the manuscript. But I will, before I get your reply to the present letter.

Dave, we have a problem. Or rather, three problems:

1) I did not get the title page. So, I don't have the title which we had chosen, nor the reference that you would like to make for your name. We could proceed by numbers, like 1 and 2 for me, and 1 and 3 for you, 1 being the Tax Lab in Boulder, 2 being the Institute here in Lund, and 3 being perhaps the Food Research Institute where you are now ? Anyhow, I would like to know what goes on that page. Also, I need the title badly for another reason: I would like to refer to this publication in another of my papers.

2) I believe that we had a summary all prepared (had I not translated it in French, also ?) If you cannot find it, please give me at least a sketch of what you think should go into it, and I will write it and send it back to you for corrections.

3) About the fac-simile printout of Graph: to produce it, I will need some sort of an example, so as to know what to write, and where to write it on the page. And all my papers on Graph are in Montréal ! So, could you send me a couple pf pages of printout of Graph, making sure that I get at least one example of "Internal connections after level ..." Photocopies of a couple of pages would do as well.

By the way, there are some people here who could be interested to use Graph to classify clones of chromosomal types obtained by prolonged tissue culture: just as you did classify the clones of M. esculenta ! I will tell you more about this later.

Sincerely,

Pierre

Pierre Legendre

Summary

We describe two computerized programs developed in the Taximetrics Laboratory which have been previously described separately, but which are intended to be used together for improvement of taxonomic classification. The two programs are entitled "CHANAL" and "GRAPH", the first being a set of procedures to evaluate taxonomic characters, and the second a clustering procedure. It is our hope that this paper will give the reader more understanding of the programs, since we attempt a description meaningful to biologists but with sufficient discussions of the algorithms to be meaningful to other disciplines as well.

MS sent Oct. 12

Genetiska Institutionen
Lunds Universitet

Institute of Genetics
University of Lund
Sölvegatan 29
S-223 62 LUND, Sweden

September 30, 1971.

Dr. David J. Rogers,
Food Research Institute,
Stanford University,
Stanford,
California 94305,
U.S.A.

Dear Dave:

Many thanks for your letter of September 23. If I may come right to the point: I have never had to complain about you, and you have always been helping me a lot. So, I am enclined to believe that if you had to put the manuscript in the files, you had good reasons to do so. And indeed, the circumstances you mention are quite explanatory by themselves ! So, we will just have to cope with the situation.

It just happens that I could perhaps do something from here. Let me explain: here, I am quite free of my time. Of course, I keep busy, since I am in contact with a different world, with unbelievebly good facilities. Right now, I make cultures of cells of cyprinid fishes, and I will use this material to learn about the technique to study the fluorescent banding pattern of chromosomes. This could provide me with the numerical data to put in my models of genus and species, and it could also help finding the origin of natural hybrids, which are common in cyprinid fishes, among other groups. But, I repeat, I can do whatever I like, whenever I like. Including working on this manuscript.

a) For the manuscript itself: if you could add the fancy mathematical symbols (which would take little time), and then send me the whole thing (including the illustrations), I could proof-read it before sending it to Dr. Stafleu.

b) For the fac-simile printout of the graph run, I don't remember what was in it ! And I don't have my file here. But could you provide me with the following information:
- the raw data of the run (perhaps they are already in the text?)
- a copy of the printout produced by the 6400: I believe that we had such a printout, did not we ?
- if not, at least what should be on the printout (is it not all in the text, already ?), with an example of the format, so that I could produce a fac-simile. (By the way, had we gotten a fac-simile printout for Charanal ?)

My ideas are the following, by order of applicability, as I see it now:

1) We have here a Univac computer: perhaps I could use it to produce the fac-simile.

2) Perhaps Dr. Stafleu could ask his printer to make the fac-simile.

3) I could ask my sister-in-law, in Montréal, to run it from the data. Indeed, I have left a source deck of Graph in Montréal, and there, they have fast printers which do an acceptable job.

4) I could ask her to produce instead a fac-simile printout from cards that I could punch here.

5) I could try to find a character ball for an IBM Selectric, with a character comparable to the one of the computer (square letters without serifs), and produce a fac-simile that way.

By the way, when I look at what I have just been writing, perhaps it could do the job ? What do you think of the present character ? Like in

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NEXT PAIRS TO JOIN THE CLUSTER: (101, 103) (203, 308)
SINGLE MEMBER CLUSTERS: 0
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So, there is still hope ! I am anxious, as you are, to get rid of this manuscript. I just hope that you enjoy your stay in Stanford, and that you live in a pleasant environment (I understand that they got 21" of snow in Boulder, already ?) And our best regards to your charming wife.

Sincerely,

Pierre Legendre

September 23, 1971

Dear Pierre:

By now, you must certainly think that I am the world's greatest procrastinator, and you have good justification if you do. I am sorry that it has been so long before I communicated with you, but there have been extenuating circumstances which have prevented my completion of the work with our manuscript. I did have the ms retyped, but haven't even proof-read the retyped work, nor put in the remaining fancy math symbols. Perhaps my worst error (or ommission) was not getting the graph run on the 1401 to be used as an illustration. Something happened on the run, and we got only a series of zeros after the similarity tables were printed, and this happened just before I left for Africa. I told the people at IBS to hold onto the whole thing until I returned from Africa, but they didn't, and I don't know know where either the graph deck or the data cards are.

We tried to get back to it after I returned from Africa, but both Appan and Henry had left by that time, and I was left with no help whatsoever. Most important, however, was the fact that I had a fantastically complicated job getting all my Manihot specimens properly labelled and returned to the 32 different herbaria from which I had borrowed them. Using all my family, and two volunteer assistants, we finished the job on the very day that the movers came in to move me from the Armory over to Hale Hall. This was, without doubt, the most chaotic summer I have ever spent, working 16 hours a day, trying to get things done so that I could be away for one year. And our manuscript had to be put in the file until I could get around to it.

Now, I am sure that you must be very busy getting started with your work there, and probably have very little time for anything extra. But I wonder how we should proceed. Do you think that you could build up a facsimile print-out of the graph run that would satisfy? I know that you had worked very hard to get a decent computer print-out to use as an illustration, but I don't see how we can do that now. Would you like to see the manuscript that I had retyped to be certain that it is as it should be? I can send it to you, if you decided that you can.

Unfortunately, I find my obligations are more pressing to finish the manuscript for my big monograph of Manihot, and then write the book on cassava that I came here to do. These must be my first and only preoccupation, simply because I am committed to these first, and not to the work that you and I put together (mostly you). I am sorry to put things this way, but that's the way it is. I await your decision before sending anything on.

I hope that life in Sweden is enjoyable and profitable. My best regards to Ghislaine.

Sincerely,

Taximetrics Lab. Nov. 7, 1969

Dr. H. S. Irwin
The New York Botanical Garden
Bronx, New York 10458.

Dear Howard:

Your are very persuasive. I will tentatively accept yours and Art's invitation to do a survey of taximetrics for BR, but only tentatively. This is an extremely complex area, with many different aspects in many different fields to be considered. I personally really cannot do it, and will require the collaboration of others, particularly Estabrook, to go after the mathematical aspects. A review is one thing, my own feelings about the field another.

You may recall that Fleming, Estabrook and I did something similar in a chapter in a book of which Steere is an editor. That chapter, by no means a review, took three months to get together, and we said just about what we wanted to say there. If I confine my work to that which has been done in botany alone, there won't be much of a review. I can't stop, if it is to be broadened, with just the work in zoology, but must also include work in anthropology, psychology, sociology, and all the various branches of applied mathematics. These were the reasons I had when I told Art that I wasn't up to it, intellectually or otherwise.

But the most compelling reason for my first decline was that I doubt that we will be together as a team much longer. This university has failed to pick us up, and all our grants have expired. We haven't found a place which will pick up the whole team, and I simply can't operate this sort of activity alone. No matter how I try, I can't seem to get my ideas and concepts across. I'm tired and fed up. Let me give you an example of

my frustration. NSF has given AIBS a big grant to do a systems analysis for the Flora of North America. Much of that money is to be spent on a high-powered computernik to come in and put together a set of computer programs to do the data-processing for the Flora. They have set the requirements for this fellow such that he must know nothing about botany, floras, or anything in systematics. For some 15 years I've been working to get ready for such a task as the data-processing to be done in FNA, spent half a million dollars of Uncle Sam's money developing the know-how and programs which will do the job, and now find, that for political reasons, I can't get that job.

So, much as I'd like to ask for your help, I fear that the kinds of things above keep me from suggesting ways that you can help. If somehow we do manage to survive as a group, then we can turn our attention to such things as a thorough-going review of the field. I know that it is needed, and I would rather be the one to do it than someone else. Provided we survive, and we are working on it, I will give you more than a tentative acceptance.

Sincerely,

THE NEW YORK BOTANICAL GARDEN
BRONX • NEW YORK 10458 212/933-9400

November 3, 1969

Dr. David J. Rogers
Taximetrics Laboratory
Department of Biology
University of Colorado
Boulder, Colorado 80302

Dear Dave:

Art has told me you turned him down for a summarizing article he invited you to do for BOTANICAL REVIEW. I am sorry you feel unable to do the job, for I think you are the person to do it and we all hope you will reconsider your position. Although there may be reasons you cannot undertake the review article now, can you see your way clear in the next year or two? Both as a statement of the present state of the art as well as a means of presenting to an increasingly interested but as yet largely uninformed (and therefore often fearful) audience, this paper is needed, Dave. Could you give us a little better idea of whether you simply haven't time, or whether on other grounds you cannot or do not want to do the paper? If there is any way I can help, please say so, as I, perhaps more than any other here, have propagandized for this review.

Please let us hear from you.

With best regards,

Sincerely,

Howard S. Irwin

HSI:dz

OCT 9 19[illegible]

Yale University *New Haven, Connecticut 06520*

DEPARTMENT OF BIOLOGY

Osborn Memorial Laboratories

October 4, 1967

Dr. David Rogers
Department of Biology
University of Colorado
Boulder, Colorado 80304

Dear Dave:

Thank you very much for your critical comments on Crovello's paper. This sort of help from colleagues is greatly appreciated, and in fact, is absolutely necessary if one hopes to publish a journal of reasonably good quality.

Sorry for the delay in acknowledging receipt of the manuscript and your comments. After the Texas meetings, we went to Mexico for a time and returned to New Haven only last week.

Sincerely,

John

John R. Reeder
Editor-in-Chief
BRITTONIA

JR:sc

- Taxonomy Laboratory

August 5, 1966

Dr. William T. Stearn
Department of Botany
British Museum (Natural History)
Cromwell Road
London, S.W. 7, England

Dear Dr. Stearn:

I have just now had an opportunity to spend some time with "Botanical Latin." I feel required to thank you for writing it—the title does not do justice to the comprehensiveness and value it has for me and for most other taxonomic (even physiologic) botanists. Having been exposed to the precision (and vagaries), joys, and exasperations of Latin descriptions by the late Robert Woodson, himself a classicist, I can appreciate the great usefulness of this one volume.

But I must ask: Aren't you swimming against the tide? Note the length of Latin diagnoses found in the latest botanical works. Most of them barely fill the bill of the International Rules. There is a growing recognition that, unless one spend an inordinate amount of time becoming really well-founded in a language, it is not possible to carry one's precise meaning across in another tongue. Is not your argument for botanical Latin one of personal experience, past inclinations, and education in the public schools? That must be a unique experience, not common to the rest of the world, no matter how we try to emulate it.

No matter how I argue, however, I will attempt to make any Latin statements in keeping with your fine publication. I shudder, though, when I think of submitting my work to any publication of which you must be the arbiter of the included Latin!

Sincerely yours,

David J. Rogers
Professor of Botany

DJR:ch

AMHERST COLLEGE
Amherst, Massachusetts 01002
DEPARTMENT OF BIOLOGY

February 1, 1966

Dr. David J. Rogers,
Taxonomy Laboratory,
Department of Botany,
Colorado State University,
Fort Collins,
Colorado 80521

Dear Dr. Rogers,

Thank you for you letter of January 14, in reply to my enquiry about Crepis. I should certainly appreciate any achenes of Crepis (particularly so if the species is identified) that you may be able to get for me. However, I must admit that my needs are limited to species that have few chromosomes. I believe that all native Crepis species are polyploid apomicts, which would be of no use to me, and that the most likely foreign species would be C. capillaris, which I already have.

So, if any of your colleagues or extension people happen to find Crepis, I would like to have a sample of achenes. I am not in so desperate a position as I was last month, having obtained another three European species since then.

Thank you also for your advice on the possible sources in weed-seed analysis laboratories.

Sincerely,

Hugh Wallace

Hugh Wallace

31-W

- 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

AMHERST COLLEGE
Amherst, Massachusetts 01002
DEPARTMENT OF BIOLOGY

January 5, 1966.

Dr. David Rogers,
New York Botanical Gardens,
Bronx Park,
New York, N.Y. 10458

Dear Dr. Rogers,

I am attempting to collect a variety of the annual species of Crepis, in order to make a cytogenetic analysis of their hybrids. So far, I have only managed to obtain C. capillaris and the ornamental C. rubra (and learned that the extensive collection at Berkeley no longer exists).

Dr. Schuster of U. Mass. suggested that I might ask you for advice on how to get achenes of other species. Do you happen to have any recently collected specimens with fertile achenes that you could spare, or could you please suggest anyone who may be able to provide me with some?

Yours sincerely,

Hugh Wallace

Hugh Wallace

Assistant Professor

- 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 mmment 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

MOSS HERBARIUM

THE NEW YORK BOTANICAL GARDEN

BRONX • NEW YORK 10458 LU 4-8500

FEB 21 1966

February 17, 1966

Dear Dave,

Here is the page that you want from Pohl's Plantae Brasiliensis. I'm sorry it is in pieces but I guess you know that the Xerox machine will not do an entire folio page at once.

I am glad to hear that things are going well with you at Colorado State. I'm sure you must be happy to be teaching again. The country there must be magnificent and, I should think, a wonderful place to live.

It has taken me some time to get used to being in the moss herbarium again and to pick up where I left off. But I am glad to be back working at the Bryophytes.

The new Librarian, John Reed, is a first-rate man and he is doing a splendid job. The books are pretty well moved into the new wing but apparently there is going to be a wait of some weeks or perhaps months until all the furniture is delivered.

Please call on me again if I can be of further help here in any way. My regards to the Flemings.

Yours,

- 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 appempt 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 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 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 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 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 conderning 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.

Inasmuchas 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.

UNIVERSITY OF SASKATCHEWAN

THE W. P. FRASER HERBARIUM

SASKATOON, CANADA

January 28, 1966

Dr. David J. Rogers,
Taxonomy Laboratory,
Colorado State University,
Fort Collins, Colorado 80521

Dear Dave:

I am very pleased with your response to my letter and your suggestion that I send my data to you and to have it run off on the 7094. It would be very time consuming for us to get your program reprogrammed for our computor for programers seem to be few and far between and they take a long time to do whatever they are doing.

I will briefly describe my problem so that you can see what I am after. I am studying a Salix hybrid from a fen about 150 miles north of Saskatoon. The species involved in hybridization are Salix pedicellaris and S. planifolia. What is suspected, so far, is that sterile hybrids are being produced (possibly triploids) as well as fertile polypoids which seem to be hexaploid. Some chromosome counts have been made and others are being made so that the problem may be attacked from that point of view. Pollen stainability tests also seem to support that hypothesis. I have analysed the hybrids by use of the Anderson hybrid index but I am not pleased with having to select "typical" manifestations of the parents nor am I satisfied with our methods of arranging characters in linear series. I have used your method of scoring characteristics and I am convinced that this or some similar method is more objective than the Andersonian method and hence more reliable. There is also the possibility that a hybrid swarm (not the one in question) may involve more than two species and this would be obscured by the hybrid index type of analysis. I would like to use your method up through the computation of the Distance Function Matrix. After that point I am not sure that a cluster analysis is the best way to handle the data since the hybrids could not be expected to be clustered around any centre.

In order to analyse the data I have been using an ecological ordination method similar to the one devised by Curtis at the University of Wisconsin. This method has the advantage of being readily understood by non-mathematicians and that the object can be arranged on axes and graphed in three dimensional space. I would not object to trying a cluster analysis on some of my data but I would like your opinion to whether or not you think it would be appropriate for a hybrid study.

..... 2

What I would like to do is to calculate a Distance Function Matrix for my specimens and then to take those values and run them through our ordination program. I really do not understand enough about the mathematics of clustering to go beyond this point right now. I seem to feel a lot better when I have a graph to deal with rather than just a series of numbers.

If you would let me know when you expect to have your program ready and just how to transcribe my data onto your general coding work sheet I will send you my data as soon as possible.

I was interested to hear about the taxonomic situation at Colorado State and I would appreciate being informed about developments as they take place.

Sincerely yours,

George

George W. Argus,
Assistant Professor,
Plant Ecology (Plant Taxonomy)

GWA:rmf

- 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 PORTA-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 Love 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 anytbing 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

31-A 1-17-66

UNIVERSITY OF SASKATCHEWAN

THE W. P. FRASER HERBARIUM

SASKATOON, CANADA

January 13, 1966

Dr. David Rogers,
Department of Botany,
Colorado State University,
Fort Collins, Colorado.

Dear Dr. Rogers:

When Dr. Dick Ward was in Saskatoon this fall he mentioned that you had moved from the New York Botanic Garden to Fort Collins. How are you enjoying your stay in the Rocky Mountains? I spent some time at Laramie, Wyoming and I appreciated being a short drive from the mountains.

As you may know I have been spending most of my time studying the systematics of Salix in North America. Your paper on a numerical handling of non-numerical data was very interesting to me and I have used your method of arriving at similarity values in two small preliminary studies, one on Salix and the other on Pinus. I would like to expand my study of Salix and this will require machine handling of data. I would very much appreciate it if you could give me some help in two aspects. First I would like to know exactly how the data were placed on the IBM cards, I have used the method for collecting data which was described in your 1964 paper in Bio-Science. Secondly, I would like to know if the computor program for your method, up through the determination of the distance function, is available. I would like to be able to determine the distance function matrix for my specimens which will then be analyzed by a modification of the ordination technique. The computor we have is the IBM 7040 and I am sure that your program could be converted to our computor without too much difficulty.

The problem I am studying is a case of hybridization and polyploidy in Salix. I have already ordinated the distances which were obtained in the preliminary study and the results have been very encouraging.

If there is any cost either for the IBM card method or the computor program I am in a position to cover that cost from my research grant. I hope that you will be able to help me with my problems.

Sincerely yours,

George Argus

George W. Argus,
Assistant Professor,
Plant Ecology (Plant Taxonomy)

GWA:rmf

over please

I have heard from Dr. Löve at Boulder that your department is looking for a plant taxonomist. If so I would appreciate hearing ~~and~~ about it. I would be interested in returning to the US after a four year stay in Canada.

Send my best wishes to Dick Ward.

George Argus

- 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. R. 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

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

31-F

DEC - 6 1965

UNIVERSITY OF CAPE TOWN

(WITH WHICH IS INCORPORATED THE SOUTH AFRICAN COLLEGE)

Zoology Department,

RONDEBOSCH.

1st December, 1965.

Dr. David Rogers,
State University of Colorado,
FORT COLLINS,
Colorado,
U.S.A.

Dear Dr. Rogers,

Recently I came accross reference to two computer programs which may be of interest to us. They are: "Taxonomy program for classification of data" and "Classification for similar characteristics of data". I have have written to Dr. Melvin Klerer about this and he has referred me to you.

Our department is looking for a system of storing and handling data on the abundance and distribution of bottom-living marine invertebrates obtained by dredge and grab sampling and the program titles mentioned looked as though the programs might be useful to us.

I would very much appreciate it if you could give me some information on what sort of data these programs are designed to handle and what they are designed to do. If you have them handy flow charts would be helpful to us if the programs are suitable because our computer is an I.C.T. machine operating on Mac autocode and it is unlikely to be able to handle your programs as they are.

I hope that this will not inconvenience you.

Yours sincerely,

J.G. Field

J.G. Field

31 - F

- 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

31-E

DEC 10 1965

DR. ALICK ELITHORN

Medical Research Council,
J Hut,
Royal Free Hospital,
Lawn Road,
Hampstead,
London, N.W. 3.

3rd December, 1965.

Dr. David J. Rogers,
Curator of Economic Botany,
New York Botanical Gardens,
New York.

Dear Dr. Rogers,

Dr. Elithorn is very anxious to get in touch with Dr. Tanimoto who was the co-author of your paper "A computer program for classifying plants" published in Science in 1960. We have a large amount of data on 119 epileptic children to be analysed and Dr. Elithorn thinks that an analysis based on the taxonomic program described in your paper would be very useful.

We have been unable to get in touch with Dr. Tanimoto through I.B.M. and if you have his present address or could suggest how he might be found, I should be most grateful if you would let me know.

Yours sincerely,

Mary Norris

Secretary.

31-G

- 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 our 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 included a manuscript of the paper which was given at the AAAS meeting. Any spares of your paper may I have one? Any other papers that you have available, I would also like to have them.

31-G

DEC 16 1965

The Commonwealth of Massachusetts

University of Massachusetts

Waltham Field Station, Waltham 54

December 13, 1965

Dr. David J. Rogers
Dept. of Botany & Plant Pathology
Colorado State University
Fort Collins, Colorado

Dear Dave:

Congratulations on your new position in Colorado. I received your address from Richard Schultes and have placed a slip for you in my mailing list file.

I do not seem to have a reprint of yours on numerical methods of evaluating plant relationships. Please mail such to me if available.

With best wishes for, and expectations of, your continued leadership on the 'botanical frontiers'.

I am.....

Sincerely yours,

Walt

WCG/m

Walton C. Galinat,
Associate Professor.

31 W

- Taxonomy Laboratory

November 5, 1965

Mr. Hayden R. Williams
Orange Coast College
2701 Fairview Rd.
Costa Mesa, California 92626

Dear Mr. Williams,

I am sorry to report that our interest in the systematics of marine algae never really got off the ground and that there is no paper of the type which you mentioned in your letter. Dr. Levin is no longer with us, but is at the Department of Botany, Notre Dame University.

I am sending a reprint which may be of interest to you on the use of computers.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/ec
Encl. Reprint "A Computer Program for Classifying Plants"

ORANGE COAST COLLEGE

2701 FAIRVIEW ROAD
COSTA MESA, CALIFORNIA 92626
KIMBERLY 5-1151

October 29, 1965

Dr. David J. Rogers
Curator of Economic Botany
The New York Botanical Garden
New York, New York

Dear Dr. Rogers:

My field of research as a graduate student at the University of California, Irvine is the systematics of marine algae. The title and short review of the paper that you presented with Dr. Levin sounds like what I have been looking for.

If you have a reprint or offprint of A Taximetric Analysis of Genera and Families of the Nemalionales, please send me one to the above address.

Respectfully yours,

Hayden R. Williams

Hayden R. Williams
Instructor in Botany

31-R

- Taxonomy Laboratory

November 18, 1965

Dr. Lloyd Rosenberg
Hudson Laboratories of Columbia University
145 Palisade Street
Dobbs Ferry, N Y.

Dear Dr. Rosenberg,

I have just received a copy of your letter to Mr. John G. Field. I am prompted to write to you because of the information related in your letter to Mr. Field. I have not seen the paper referred to in your letter "Taxonomy Program for Classification of Data". If you have a copy of this paper, or if you can find out where I may obtain one, I will be pleased to have this information.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/ec

Hudson Laboratories of Columbia University

145 Palisade Street
Dobbs Ferry, N.Y.

Telephone Code 914, OWens 3-5800

November 16, 1965

Mr. John G. Field
University of Cape Town
Zoology Department
RONDEBOSCH, C.P.

Dear Mr. Field:

Dr. Melvin Klerer has requested that I reply to your letter of October 6 which was addressed to him.

The "Taxonomy Program for Classification of Data" and related programs were taken from the work of Dr. David Rogers, formerly of the New York Botanical Gardens, now at the State University of Colorado, Fort Collins, Colorado. Since we have merely copied this work I would suggest that you write to Dr. Rogers directly for further information about his programs and for some of his new work which he has been doing.

Very truly yours,

Lloyd Rosenberg

LR/bks

cc: Dr. David Rogers

- 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. Herold Harrington, CSU Dept. of Botany

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

31 - B

- Taxonomy Laboratory

December 6, 1965

Dr. Jack L. Beal, Professor of Pharmacognosy
The Ohio State University College of Pharmacy
1958 Neil Avenue
Columbus, Ohio 43210

Dear Jack,

Your letter addressed to me at the New York Botanical Garden was forwarded to me at my new address here I thought that I had circulated enough information about my change to Colorado, but apparently have not done so. You can see my address on this letterhead.

With respect to your request for seeds of Thalictrum, I had thought that our National Seed Storage Laboratory here at Colorado State might be of some help. However, I have checked with them and find that they have not any possibilities. They suggest, however, that you write to the following address, and maybe they will have some information for you there.

Dr. L. M. Pultz
Oil Seed and Industrial Crops Research Br.
Plant Industry Station
Beltsville, Maryland

If you write to anyone at the New York Botanical Garden, perhaps the best man to address your letter to is Mr. Louis Politi who is in charge of seed exchange. I do not know whether you will have any success with him or not.

I have a couple of other suggestions, but unfortunately they are not complete as far as address is concerned. You might try some of the European Rock Garden Society places. One is the Scottish Rock Garden Society in Edinburgh. I am sorry I don't have a specific address for these people. Another you may wish to address

is the American Rock Garden Society. You will probably be able to find in your library a publication by the American Rock Garden Society, and in there you will probably find an address for seeds exchange.

Other than these, I'm afraid I'm not going to be much help to you.

Sincerely,

David J. Rogers
Professor of Botany

DJR/ec

THE OHIO STATE UNIVERSITY
COLLEGE OF PHARMACY
1958 NEIL AVENUE
COLUMBUS, OHIO 43210

November 24, 1965

Dr. David J. Rogers
The New York Botanical Garden
Bronx Park
Bronx 58, New York

Dear Dave:

I am carrying out a comparative study of the alkaloids of Thalictrum species. We have the following species on hand: aquilegifolium, dasycarpum, dioicum, dipterocarpum, fendleri, minus var. adiantifolium, polygamum, revolutum, rochebrunianum, and rugosum.

I am sending a letter to about 85 botanical gardens throughout the world requesting seed of any Thalictrum species we do not have. In the letter I offered to exchange seed with them. Actually, I am not very optimistic about obtaining seed by this method, but I know of nothing better to do.

Any help that you can give me in providing seed of Thalictrum species we do not have would be very much appreciated.

Sincerely,

Jack

Jack L. Beal
Professor
Pharmacognosy

Scottish Rock Garden Soc.
Edinburgh,

JLB:tem

Am Rock Garden Soc.

L. M. Pultz
Old Seed + Indus. Crops Lab
Pl. Industry Sta.
Beltsville