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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.

FILE

TO Dr. David J. Rogers
Dept. of Biology, Taximetrics Lab.
Armory 101
University of Colorado
Boulder, Colorado 80302

FROM Mr Robert Breech
PUBLICATIONS OFFICE
THE NEW YORK BOTANICAL GARDEN
BRONX, NEW YORK 10458
(212) 933-9400

SUBJECT: Society for E.B. meetings in April DATE: 2/28/69

FOLD—

Dear Dave:

Are you planning to attend the meetings this year? You probably know that there is to be an historical review of the Society and that recognition is to be given to the founders of the Society.

Sincerely

Bob

PLEASE REPLY TO → SIGNED

Dear Bob: 3/4/69

I won't be able to come to the meeting. I suggest that you take a good hard look at any historical write-up about the society, because I am sure that the people in both Beltsville and Harvard will try to put themselves way ahead of NYBG and the contributions we all have made to that society, both in terms of time and money.

FOLD—

Sinc.

Dave

DATE SIGNED

Form RM—879 SEND WHITE AND PINK COPIES WITH CARBONS INTACT. PINK COPY IS RETURNED WITH REPLY.

MORTON COLLECTANEA
UNIVERSITY OF MIAMI
CORAL GABLES, FLORIDA 33124
Box 8204

MAR 11 1969

March 9, 1969

Dr. David J. Rogers
Taximetrics Laboratory
Department of Biology
Armory 101
University of Colorado
Boulder, Colorado 80302

Dear Dave:

I greatly appreciate your providing such a full and clear account of the events leading up to the formation of the Society for Economic Botany and its early history. My only regret is that you are sealing my lips by specifying that I "will not convey this message to anyone else". I would very much like to help make it generally known that the Journal was on its feet when it was adopted by the Society, and also who resurrected it.

It is probably late in the day now to alter the historical account which I asked Llewelyn to prepare. However, you will be pleased to know that I wrote him on December 30 especially to convey Dick Schultes' words: " . . .meaningful would be a 10-year history . . . starting with Fulling's pre-Society contributions -- and a word on the main officers in this period."

Dr. Hill was our President in 1967. Since Dr. Fulling has never held this office, and since both our Program Chairman and Llewelyn have suggested that the Council establish some kind of special award this year, I shall put forward the thought that it be an award (plaque or whatever) to Edward Fulling.

In view of your remarks concerning Bernice Schubert, you will be glad that she has been nominated as a Council member. I had to ask John Beaman to serve as Chairman of the Nominating Committee and take fast action, but I made no suggestions. He and his Committee came up with Schubert on their own and she has agreed to serve if elected. Their choice of President rather surprises me -- it seems fairly premature.

Would you care to write Llewelyn? He returned on the 5th from several weeks in Puerto Rico and it may just possibly be that he has not completed his little history.

Dave, it would be good to have your active participation in Society affairs, again. I hope that you will positively be with us at Texas A.& M. in 1970. We do not have an invitation for 1971, but are expected to be at Cornell in 1972. It was a great pleasure to have a part in your visit here with Richard. You both must come again!

Sincerely,

Julia

3 March 1969

Mrs. Julia F. Morton
Box 8204
Coral Gables, Florida 33124

Dear Julia:

In writing down my thoughts about the letter from Jones and about the write-up of the Society's formation, I am going to have some difficulty in separating my own feelings from the astigmatism and self-seeking which is apparent in Jones's interpretation of things. So let me give you some of the background, in the firm belief that you will not convey this message to anyone else.

I think the thing that bothers me most about Jones's writings is his failure to put the emphasis on the right people who really made it possible to even conceive of a society for economic botany. Two groups immediately come to mind, and two people particularly made it possible for such conception. These two were at work, and were strong movers in economic botany long before the group at Beltsville ever came into existence. The most famous, from the standpoint of education in the subject, is, of course, A. L. Hill at Harvard (and his predecessor, Oakes Ames). The other group was at the New York Botanical Garden, where H. H. Rusby had been Curator of Economic Botany since before the turn of the century, and whose traditions and good work stimulated the efforts of Ed Fulling, who established, financed and edited the only journal in the world with the title Economic Botany, and generally brought the rest of the world an awareness of economic botany as a useful, interdisciplinary subject.

A little background on the happenings at the New York Botanical Garden with respect to the journal Economic Botany is pertinent, particularly with respect to Jones's comments in his December 10th letter to you, that sentence which states "I was the one that suggested that we needed a society which would adopt the journal Economic Botany as its official organ, thus rescuing the latter from its then imminent demise." (Italics mine)

In 1947 Dr. Fulling brought his ideas for the journal to the Director of NYBG, W. J. Robbins, who encouraged Fulling to go ahead with the idea, but told Fulling that NYBG could not financially back another publication, but would be glad to lend the name of the institution to help Fulling in establishing it. Fulling, who was always in frail health, and who never received any salary or financial aid from NYBG, carried all the responsibilities for the journal until 1955, when his double responsibility for Botanical Review and Economic Botany became too burdensome, and he requested that NYBG take over the responsibility for editing and financing the journal. It took some time for the Director to get sufficient funds to hire an editor and to carry the financial load, and during this period Dr. Fulling became quite discouraged about the continued publication of Economic Botany. At one point, in 1956, he even sent out notices to subscribers, to potential authors and interested people that unless something transpired, the journal would go out of print. This notice seems to have stimulated the governing board of NYBG to provide the necessary funds, and they began searching for an individual to come to the Garden to be the editor, and to be curator of economic botany. I must admit that they worked very hard to find someone who would take the job, and only after several other offers were made and these rejected, was the job offerred to me. I accepted, and began work in July, 1957. At that point, the journal was evidently on the brink, particularly because Dr. Fulling had sent out his notice, and no authors were about to submit manuscripts to a journal with no future. But by dint of some very hard work, I was able to bring the journal back to life, and articles were coming in. But because economic botany had not become sufficiently well established in any other institutions than in the New Crops Research Branch, at NYBG, and at Harvard, there was no rush to put manuscripts in that journal, particularly so when authors would much rather put their papers in journals more closely allied to their own fields - of agronomy, forestry, pharmacology, etc. etc. and it was a continuous chore for me to keep good and useful articles coming in. But by the time Jones entered this picture, the journal had in fact already been saved from its "imminent demise." The journal would have continued, whether or not a society was formed. Indeed, had there not been a going journal to offer to potential members, it is highly unlikely that the society would have ever come into being.

carrying a very heavy work-load in Beltsville, and at the same time, she was also having difficulty with her eyes. She had to have very serious surgery during her tenure as editor, and obviously could not do the intensive amount of reading necessary for an editor. (At that time the editor wore about six hats, not only accepting articles for their scientific merit, but also doing all the work of copy-editing, dealing with the printer, etc. etc.) As a result, publication schedules got farther and farther behind, to one point where we were six mnnths behind schedule (in recent history this is still a problem). So, as managing editor, responsible both to subscribers not members of the society, and to NYBG, I had to be the devil to take back the editorial job until another, more active editor could be found. This was a very difficult chore for me, because I like Bernice Shubert, and knew her difficulties, and I tried to do it diplomatically, but I fear that diplomacy is not my long suit. I'm sure that the people at Beltsville, and at Harvard, have always felt some anger towards me because they think I "ditched" Bernice Shubert. I let you be the judge.

One or two more points are pertinent to this discussion, and I hope you'll bear with me while I lay these on the line. It is also true that when I became treasure of the society, I also got the onerous task of program chairman, to set up the first and third annual meetings. As a result of my activities, the form of the meeting which is still being used, was established. The idea for an inter-disciplinary symposium, the contributed paper sessions, the banquet and luncheon speaker, all came as a result of my planning of those first meetings. Another point - I have never been a council member - only a non-voting officer in the society. Why? Some of the reasons are obvious, others not so. The Managing Editor position (without vote in the council) was established to allow the NYBG to keep the finances of the journal (which is still its responsibility) on an even keel. This is not a society position - it is a requirement of any organization. But this has apparently stuck in the craw of the Beltsville and Harvard people. For some reason, they feel that either I or Bob Breach have too much control. But that journal is sold to many libraries, organizations both private and public to whom NYBG has an obligation, and the Managing Editor is the one on whom the responsibility immediately rests.

In connection with this offering to members, it should also be noted that NYBG had a history of helping societies out by providing a going journal to the society. This was true for Brittonia, which became the official organ of the Am. Soc. of Plant Taxonomy, and for Mycologia, the publication of the American Mycological Society, both of which were supported editorially and financially by NYBG for years before a society became involved. Thus, having this kind of background, NYBG felt willing to allow Economic Botany to become the official journal of a society, should one be formed. Dr. Steere, who succeeded Dr. Robbins as Director of the New York Botanical Garden, and long interested in economic botany himself, gave his official blessing to allow me to make an offer of the journal, in the event that the active groups of economic botany elsewhere were sufficiently interested in a formal society.

At this point, I began to ask prominent people around the country for their opinion. Not the least among this group of people were those in Beltsville. They had long been contributors to Economic Botany, and when I visited Beltsville to talk to them about continuation of Economic Botany, it was then that I discovered that they had already had some ideas about the need for a society. Up to that point, however, nothing had been done, but at their urging (which I had hoped for) I called together the group of 19 persons from around the country, the meeting mentioned in Jones's paper. The costs of that meeting were met by the NYBG.

The events for the formation of the society from this point on are reported fairly well in Jones's report. But from his letter to you, I feel that he missed a lot of other points which you should know about. One of the most important is that, after formation of the Society, one of the greatest contributors was Ed Fulling, who put far more money into the Society's kitty than anyone else. Another thing to mention is Bernice Shubert, who only acted as editor for a period of 9 or 10 months. She was invited to be the editor in the first place because we at NYBG did not wish the Society to feel that it was being pulled around by the nose by having all the control of editorial activity and financial control as well, in the hands of NYBG staff. When the society was formed, there were some 1,000 subscribers to the journal, and they paid a higher subscription rate than those who were members of the society. But the journal itself had an obligation to its subscribers. When Bernice accepted the position, she was

Finally, I completely agree with Jones's paragraph that states that we have no time to honor the Founders in some special commemorative program. I do think, however, that the council could find some way to recognize the significant contributions of Ed Fulling and A. L. Hill before it is too late. Clearly, the presidential role is not appropriate since it demands that the president given an annual lecture, and two very fine, reticent men, in ill health would not or could not accept such a task.

All the above is merely background information which I feel you need to have yourself. Now the question arises - what should be the nature of the writing which describes the ten year history of the Society for Economic Botany? Are you going to be the one responsible for writing the history? Or is it to be done by somebody at Beltsville? In view of the above, I would hesitate to let the Beltsville people do it myself; but then I suspect they would not be happy if I wrote it either. Let me know what your decisions are, and I will see what can be done to help out.

In closing let me say that I appreciate your tremendous efforts for the Society. Such an organization needs the dedication you have given it.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

Journal: ECONOMIC BOTANY

The Society for

ECONOMIC BOTANY, INC.

DEVOTED TO THE PAST, PRESENT, AND FUTURE USES OF PLANTS BY MAN

December 10, 1968

Mrs. Julia F. Morton
Morton Collectanea
University of Miami
Coral Gables, Florida 33146

Dear Julia:

I can best answer the questions posed in your December 5 letter by providing you a copy of the report I gave at the First Annual Meeting of the Society. Dave Rogers was present when this version was presented and he raised no objections. (Nor did anyone else)

I would have to say that Dave, more than any other one person, deserves the credit for being the moving force in assembling the group that launched the organizational leg-work that led to the actual founding of the Society. I was the one that suggested that we needed a society which would adopt the journal ECONOMIC BOTANY as its official organ, thus rescuing the latter from its then imminent demise. But it is obvious from the enclosed write-up that Dave played a key role throughout the several formative years of the Society.

All of this does not mean that I think the Society has turned its back on Dave. He was the Society's first Treasurer and its first Managing Editor. In the last 4 or 5 years a feeling has developed among the officers and councilmen that new faces are needed on these bodies. I agree with this thinking - as long as there is a thread of continuity on the Council and among the Officers.

Bernice Schubert gave a lot of herself to editorial chores in ECONOMIC BOTANY in the first several years of the Society. She deserves a good deal of credit and it would please me to see her as an active participant again.

We have so many timely and interesting subjects to occupy our limited meeting time that I would not be particularly in favor of using much of it in "honoring" the founder(s) of a still infant society. I

certainly agree with you that we need more peace and harmony and a general pulling together for the good of the Society.

I hadn't realized that Bob Breach had switched printers but I'm glad to hear it.

With best personal regards,

Sincerely yours,

Quentin

Quentin Jones, Botanist

Enclosure

New Crops Research Branch, USDA
Beltsville, Maryland 20705

COPY
12-10-68

Formation of The Society for Economic Botany

On occasions of Golden Anniversaries and Centennial Celebrations there is a human proclivity for exhuming the past. It is conceivable that in fifty or one hundred years from now members of this Society may be interested in the historical facts pertaining to the founding of the Society. In anticipation of their curiosity and in lieu of minutes of a previous meeting of the Society, your Secretary has attempted a factual recording of events leading up to this first annual meeting of The Society for Economic Botany.

The staff of the New Crops Research Branch, Agricultural Research Service undoubtedly represents the largest aggregation of economic botanists to be found in this country - and perhaps in the world. So it is not surprising that the idea of organizing a society of economic botanists was conceived by members of this group. An idea, however, may be a long way from a concrete accomplishment, even as a gamete is many obstacles away from becoming a mature, healthy plant.

Fertilization of our gamete-idea occurred at the New York Botanical Garden on the occasion of a national conference on the subject of economic botany. We in the New Crops Research Branch had suggested to Dr. Dave Rogers of the New York Botanical Garden, who was responsible for organizing the conference, that discussion of a society for economic botany be included on the agenda.

The conferees, 19 in number, favorably received the idea of a new society to serve the needs and interests of the broad field of economic

botany. It was recognized that economic botany is an interdisciplinary science and that any society organized to serve it adequately would have to be open to, and encourage memberships from, the disciplines of agronomy, anthropology, archaeology, chemistry, economics, ethnology, forestry, geography, geology, horticulture, medicine, microbiology, pharmacognosy, pharmacology, in addition to the established botanical disciplines; for the unifying interest among such a diverse group will be the broad subject of the past, present, and future utilization of plants by man.

With the appointment of three (L. G. Nickell, D. J. Rogers, and Q. Jones) to serve as a Committee on Organization, our newly formed zygote underwent its first cell division toward embryo-hood.

The Committee on Organization drafted a questionnaire to survey interest in such a society among potential members. This questionnaire was distributed (in September, 1958) to some 300 potential members, from all parts of the U.S. A very enthusiastic response was obtained. Three-fourths of those contacted returned questionnaires and, of these, about 85% indicated they would join such a society.

With this encouraging response the Committee on Organization took the next step of asking three older and wiser men (C. O. Erlanson, Wheeler McMillen, and W. C. Steere) to serve as an Advisory Council to the Committee. A Constitution and By-Laws were drafted by the Committee and reviewed and approved by the Advisory Council. Then a slate of

pro tem Officers and Councilmen was proposed to function in accordance with the provisional Constitution and By-laws. The Advisory Council plus L. G. Nickell, I. A. Wolff, and H. W. Youngken, Jr. were proposed to constitute the pro-tem Council. Their ratification of the provisional Constitution indicated their concurrence in the objectives, scope, and modus operandi of the Society. The pro-tem officers appointed were Ernest Guenther, President, D. J. Rogers, Treasurer, and Quentin Jones, Secretary.

Embryo-hood was now achieved and it was by this time Spring, 1959. If germination and further development were going to occur that season, planting had to be accomplished right away. A call for members was sent out on May 29, 1959 including a provision that if less than 100 applications were received all remittances would be returned and the Society had failed to germinate. Before July 1, 1959, about 150 had joined. The seedling was emerging. It has continued to grow in a healthy fashion until now, almost one year later, and with nearly 250 members, it looks as if it may flower and bear fruit.

Quentin Jones, Secretary

Presented at the 1st Annual Meeting of the Society
at Purdue University, May 21, 1960.

6 October 1967

Dr. Joseph A. Tihen
Council of Biology Editors, Inc.
c/o American Midland Naturalist
University of Notre Dame
Notre Dame, Indiana 46556

Dear Dr. Tihen:

Please remove me from your list of editors; I have not been connected with ECONOMIC BOTANY since the end of 1964. You can see from the letterhead that I am no longer in New York.

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm

Present Editor of ECONOMIC BOTANY:
Richard Evans Schultes
Botanical Museum of Harvard University
Cambridge, Mass. 02138

3-2
Econ. Botany

- Taxonomy Laboratory

November 4, 1965

Dr. K. J. Goering
Assistant Dean, College of Graduate Studies
Montana State University
Bozeman, Montana 59715

Dear Dr. Goering:

As I am no longer associated with Economic Botany, I am sending your manuscript to the editor of the journal, Dr. Richard Evans Schultes, at the Botanical Museum, Harvard University, Cambridge 38, Massachusetts.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/ec
CC - Dr. Schultes

October 15, 1965

Dr. Robert F. Raffauf
The Society for Economic Botany
Smith, Kleine and French
Philadelphia, Pa.

Dear Bob,

You should have received a letter from Bill Steere which de-connected me from the Journal and suggested that Robert Breech be put on as Managing Editor.

I seriously doubt that I can make the meetings of the Council in October or November, and I think that Bob Breech can very well handle all of the problems that have to be brought up in connection with the Journal's policy. I think that there is some precedent for the Society to pay for Thieret's transportation. I recommend very strongly that you get John there for this Council meeting. The guy is one of the most sincere boosters and workers for economic botany that we have got in the whole Society, and he sure needs to have somebody recognize his contributions. I very strongly urge that you go ahead and pay his way to the meeting of the Council. I am sure that those of you in the megalopolis area can handle the Council's activities. You will have my best wishes for good operations. I really have no official capacity in the Society any more. One thing that you may wish to take up at the meeting of the Council is a place to meet. I think that if you give me enough time, I can arrange for it here at CSU. Let me know immediately if this invitation is useful to you.

Sincerely,

David J. Rogers
Professor of Botany

DJR/ec

Journal: ECONOMIC BOTANY

The Society for
ECONOMIC BOTANY
DEVOTED TO THE PAST, PRESENT, AND FUTURE USES OF PLANTS BY MAN

October 4, 1965

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

Dear Dave:

While I am busy getting back into the routine will you fill me in on your continuing connection with "Economic Botany". Are you still Managing Editor? Will you be able to attend a Council meeting in October or November if we can tie it to a trip you might be making East anyway - if, indeed, you plan to make one? Is there any precedent for the Society paying for Thieret's transportation (ca. $150.) which he cannot justify for a Council meeting? If his, then how about everybody else's? The Council seems to be getting too scattered for these "spontaneous" meetings, though you will recall that this one had as its major objective the future publication policies of the Journal.

I think Schultes, Schmidt, Perdue and myself - along with Breach, Jones and Hodge - might make it easily; I don't know about Morton, Wall and yourself.

Suggestions will be welcome. The answer may be a phone conference or correspondence. What think you?

Best regards.

Sincerely yours,

BFR.

Robert F. Raffauf, Ph.D.
Chairman, Council

1500 Spring Garden

jm

Postcard reply 7/9/65
Letter 7/29/65

Department of Botany
University of Michigan
Ann Arbor, Michigan, 48104
June 24, 1965.

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

Dear Dr. Rogers,

I thoroughly enjoyed our discussions at the meetings at the University of Rhode Island last week. My only regret is that they didn't last longer, but that is the way of meetings. I promised to write you a brief resumé of my work on the Cypripedium calceolus complex in eastern North America. It isn't exactly brief, but it is a resumé.

My objective in doing this work is twofold. First, I wanted to work on a problem which was refractory in that such normal types of systematic work as crosses, etc. are precluded because the seeds don't germinate, because it is an excercize in inferential analysis. Second, I wanted to publish something to give me more security with respect to jobs and my grant. This is by no means a definitive treatment of the yellow-flowered Cypripediums because they are circumpolar and appear to consist of a series of races, species, phenons, things, or what have you. Before anything defintive can be said, or even a statement of what status the entities deserve can be made, the whole complex will have to be studied on a world-wide basis.

I recognize, as we discussed, the difference between taxonomy and biosystematics. So far I have done only taxonomy, i.e. I have tried to objectively describe the variation and to locate its discontinuities.

The complex, as treated by Fernald, consists of four entities and two named hybrids. Three of these entities are varieties of Cypripedium calceolus, varieties pubescens, parviflorum, and planipetalum. The fourth is a species, C. candidum. The two hybrids recognized are C. X favilleanum (= C. calc. v. pubescens X C. candidum) and C. X andrewsii (= C. calc. v. parviflorum X C. candidum). Variety planipetalum appears to be just a

spurious variant, differing from C. calc. v. parviflorum in having flat instead of spirally twisted lateral petals. (It is supposed to be a Gaspé endemic, and you know how Fernald felt about the Gaspé) Examination of the character ranges places C. calc. v. parviflorum intermediate between C. calc. v. pubescens and C. candidum. I found that what Fernald called variety parviflorum was intermediate in all of the characters I used except lateral petal and sepal color and lip color, lateral petal and sepal color being madder purple in v. parviflorum and green in v. pubescens and C. candidum, lip color being yellow in vars. pubescens and parviflorum and white in candidum. What is more, all of the character ranges overlap badly, including the lateral petal and sepal color, i.e. many plants are var. parviflorum in all respects but have green lateral petals and sepals, while nearly all candidums and pubescens have some degree of purple in their lateral petals and sepals.

I measured seven characters (I also measured lateral petal and sepal color, but did not include it because it did not correlate with the other characters. I think I can explain the appearance of a new character state in plants which are in all other respects intermediate between two entities by one of three hypotheses, but this is outside the realm of this discussion, Let it be said that this was just one of the infinite (almost) characters not used in the study.) on these plants, divided the total range into six equal segments, and assigned index values (I.V.'s) in the usual manner, using 0 through 5. I obtained summed I.V.'s for each plant and plotted the frequency distribution of the Σ I.V.'s. The sample consisted of all specimens in the U. of Mich. herbarium which had all of the characters I was using. This gave me 139 individuals. These were mostly from Michigan, but Michigan is one of the relatively few areas where all members of the complex grow.

The frequency distribution (see figure 3B) was then subjected to analysis by the TWOPOP program (see tables V and VI). The program was supplied by G. William Moore, one of my past Botany 101 students. I do not know

enough about programming or about statistics to understand the program completely, so my explanation will be at best superficial -- sort of a "biologist's mathematics". I do have a couple of extra printouts of the program, one of which I can send you if you're interested. The program finds the best-fit, normal unimodal and bimodal curves for the data. These are the one-population and the two-population hypotheses. It then uses an error function to determine a chi-square from which the probability of the data being two populations can be found. This chi-square can be looked up in a table to get the probability that the data fits a bimodal rather than a unimodal distribution. There are two assumptions which as far as I know are necessary and serious: (1) The assumption of normality. (2) The program tries only 5 different bimodal curves of the infinite number possible. It tries those with ratios of areas under the curves of 9:1, 7:3, 5;5, 3:7, and 1:9. This means that sometimes the data will be bimodal, but the program will tell you that it is unimodal. In my case, this is better than an error the other way.

In my data, the first run separated approximately the white-flowered (C. candidum) from the yellow-flowered (C. calceolus vars.)(see table V) with a probability of the population being bimodal of 80 - 90%. I then removed most of the white-flowered data by determining the point on the x-axis at which the left-hand ($\bar{X}$ = 17.7) curve contained less than 5% of the right-hand ($\bar{X}$ = 26.4) curve. The idea was to run the yellow-flowered ($\bar{X}$ = 17.7) curve to see if it too was bimodal, since the program cannot find more than two populations. This second run separated the yellow-flowered individuals into two populations also, with a 99.0 - 99.5% probability(table VI). Clearly this second probability is dependent on the first, therefore the real probability of the second separation is 99.0 - 99.5% of 80 - 90%.

Thus, the Σ I. V. frequency distribution is divided into three populations, one approximately corresponding to Fernald's var. pubescens, a second representing an intermediate "gemisch", and including Fernald's var. parviflorum, and a third representing approximately Fernald's C. candidum. Figure 3B shows only the two terminal curves superimposed on the whole frequency distribution.

This is how the data were analysed this spring. Bill is in Detroit this summer, working, but he has made several improvements in the program. (1) He added a null hypothesis test. (2) He has a program which handles up to four-population hypothesis, supplying the chi-squares between them and the null hypothesis test. (3) He has made the program more efficient. I feel that the null hypothesis test will "correct" for the normality assumption and for the fact that not all possible bimodal curves are tried.

If I am making myself understood, I feel that it will be clear that I am not doing numerical taxonomy. What I am doing is a statistical analysis of variation. I feel that we are using statistics in a valid way in that we are using it as a hypothesis test -- we propose the one- or two-population hypothesis and check it against the data. Through ignorance I have probably misused terms in my description of the technique, but I hope that we understand each other in spite of this.

As a check against our results, I find that the specimens can be sorted into three piles, but the overlap makes the assignment of many specimens rather arbitrary. This, incidentally, was done before the computer analysis was carried out.

Bill and I are now planning to publish this in tandem in the Michigan Botanist. He will publish his program as a pure technique, and use some of my results for examples, and I'll publish the Cypripedium work as an analysis of variation using the technique in the same issue of the journal.

I realize that you may need information about the specific mathematical techniques used. If you do, I can ask Bill to write to you about this. I

also have a 20-page paper which I wrote up as a report for Dr. Wagner on this which you may want to look at. I only have one copy here, and I am using it right now, but if you would like to see it, I can send it to you.

I will appreciate any criticizms and/or comments ~~xxx~~ you may have about this. I am a babe in the woods in statistics and, as such, welcome advice.

Sincerely,

Steve

Stephen D. Koch

P.S. I would be interested in reprints of your numerical taxonomy work, if you have any available.

TABLE V. Summary of the statistical analysis of the frequency distribution of Σ H.I.V. for the whole sample.

	Statistical parameter (Σ H.I.V.)				
	Range	Number of individuals	Arithmetic mean	Standard deviation	Probability of two populations
Yellow-flowered*	5-26	102	18.0	4.18	---
Taxon candidum*	21-33	37	27.9	2.66	
First curve**	5.00 - 28.75	97.3	17.7	4.37	80 to 90%
Second curve**	19.75 - 33.25	41.7	26.4	2.92	

*Parameters from data from all specimens judged to belong to the appropriate taxon according to Fernald.
**Parameters of ideal, normal, best-fit curves as determined by TWCPCP.

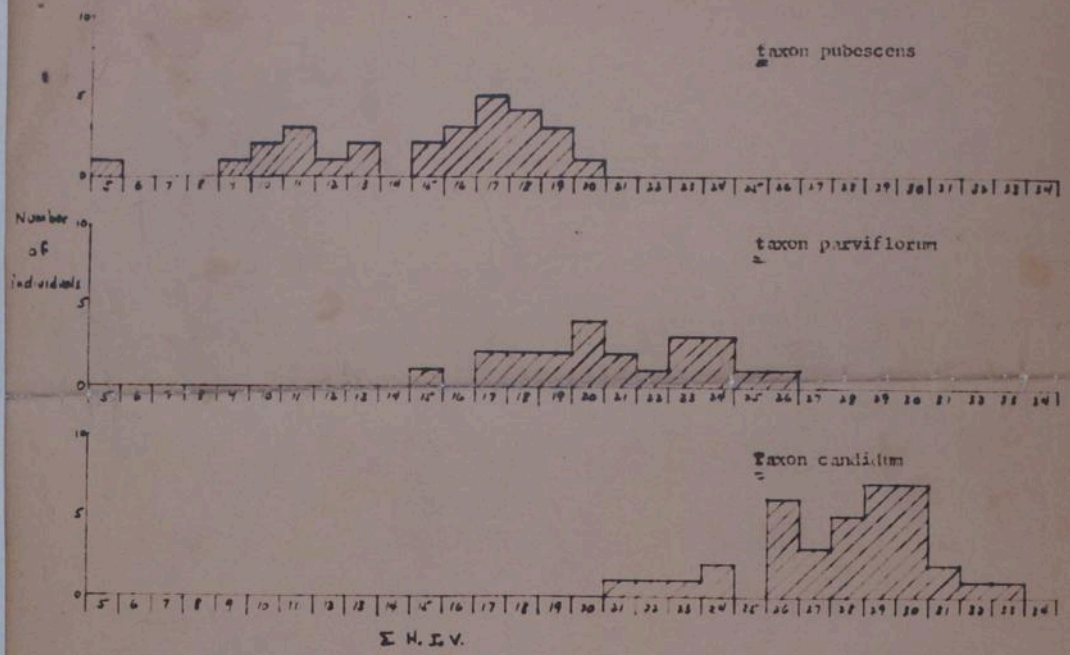
TABLE VI. Summary of the statistical analysis of the frequency distribution of Σ H.I.V. for the yellow-flowered part of the sample.

	Statistical parameter (Σ H.I.V.)				
	Range	Number of individuals	Arithmetic mean	Standard deviation	Probability of two populations
Taxon pubescens*	5-20	28	14.9	3.80	---
Taxon parviflorum*	13-26	22	20.9	3.06	---
Intermediates plus parviflorum*	10-26	74	19.1	4.44	---
First curve†	5.00 - 21.50	26.4	13.4	3.57	99.0 to 99.5%
Second curve†	13.50 - 23.75	61.6	18.7	1.79	

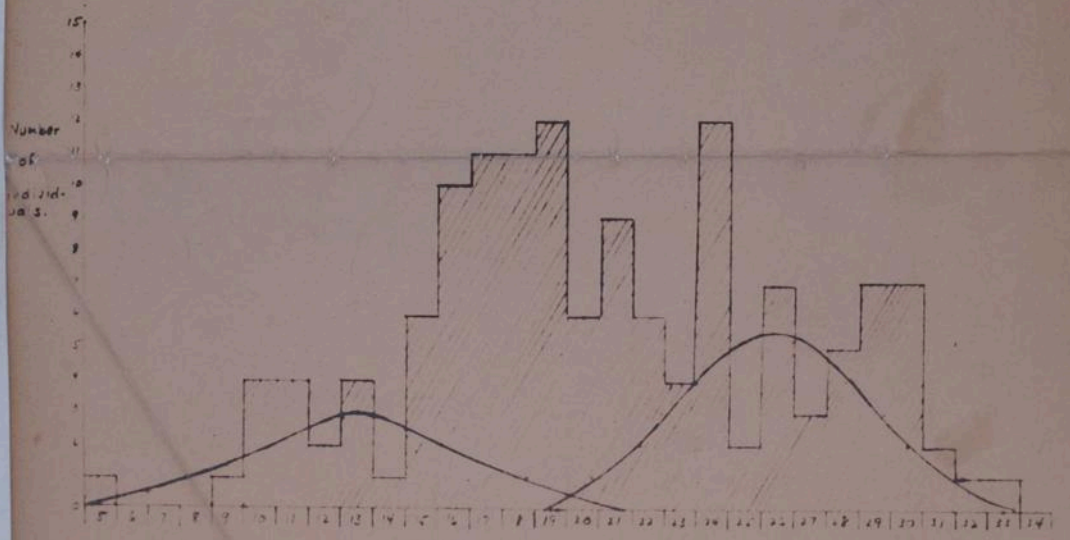
*Parameters from data from all specimens judged to belong to the appropriate taxon according to Fernald.
† Parameters of ideal, normal, best-fit curves as determined by TWCPCP. Only data through Σ H.I.V. = 22.000 included, therefore, second curve cut short.

FIGURE 3. Graphical summary of summed hybrid index value data and analysis.

A. Frequency distribution of specimens judged to belong to Fernald's taxa.



B. Frequency distribution of whole sample with idealized, normal, best-fit curves as determined by TAICOP. Smooth curves = best-fit, normal curves, histogram = frequency distribution of whole sample.



June 24, 1965

Dr. Max K. Hecht
Queens College
Department of Biology
Flushing 67, New York

Dear Dr. Hecht:

I am sorry to have delayed so long in answering your letter of June 10 requesting my acceptance of your kind invitation to speak to the Systematics Discussion Group of the American Museum of Natural History.

We have just accepted a position in the Department of Botany at Colorado State University, and unfortunately I will not be able to give the paper. I am sorry that we couldn't get together on it sooner.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

CC: Dr. Donn Rosen
Department of Icthyology
American Museum of Natural History
New York 24, New York

QUEENS COLLEGE
of THE CITY UNIVERSITY OF NEW YORK
FLUSHING • 67 • NEW YORK

DEPARTMENT OF BIOLOGY

June 10, 1965.

Dr. David Rogers
New York Botanical Gardens
Pelham Road
Bronx, New York

Dear Dave:

I am now making up the final program for 1965/66 Systematics Discussion Group of the American Museum of Natural History. We have you scheduled for the October meeting which takes place on the first Friday of the month at 7:00 P.M. If this is agreeable with you, could you send me* the exact title and the equipment that you require.

If the date is agreeable, please send the exact title to Dr. Donn Rosen Dept. of Ichthyology at American Museum of Natural History (New York 24, New York).

Sincerely,

Max

Max K. Hecht

* sending it directly to Donn Rosen for simplicity's sake.

June 24, 1965

Dr. J. C. Ogilvie
Department of Psychology
University of Toronto
Toronto 5, Canada

Dear Dr. Ogilvie:

I have just uncovered to my chagrin your letter of April 9 with references concerning the work on characters. I appredate your efforts on my behalf. Unfortunately, our paper describing the work on graph theory model has had difficulty in getting acceptance. It has been reviewed by a number of "classical" botanists who refuse to believe that the model can be based on such a few clear cut, simple rules and actually do any good. It is the opinion of most taxonomists that since the plant kingdom is complicated, a computer model therefore must also be complicated. Any suggestion that a model as simple as ours will actually have effective use for clustering of complex biological entities seems to disturb the reviewers.

We will shortly resubmit our paper to Systematic Zoology, a journal which I feel confident will not be so restrained. As soon as it appears, I have your name on file for receiving reprints and will send you one. In the meantime, I have sent a program listing of the graph clustering methods along with the development of our similarity measures to Dr. Soper and I am sure he will be glad to share these listings with you. Inasmuch as it is almost all in FORTRAN, I am sure that it will make sense to you. The only part not in FORTRAN is a sub-routine dealing with the input data and written in machine language that can be rewritten by any good programmer.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

University of Toronto
TORONTO 5, CANADA

DEPARTMENT OF PSYCHOLOGY

April 9, 1965.

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

Dear Dr. Rogers:

Here are the references I promised to send.

Shepard, R.N. Psychometrika, 1962, 27, 125-139 and 219-246

Kruskal, J.B. Psychometrika, 1964, 29, 1-27 and 115-129

The idea is Shepard's but Kruskal's papers give an improved numerical method. I obtained Kruskal's program MDSCAL which is in Fortran and is mostly compatible with FMS FORTRAN 2. This summer I expect to modify the program to IBSYS FORTRAN 4 for our 7094. In addition I want to obtain some empirical sampling distributions for Kruskal's index of stress.

Would you send me any reprints you can on the clustering technique you described in your talk. I very much enjoyed hearing about it. In addition, I would be grateful if you could send me a FORTRAN listing of the program.

I will look forward to hearing from you.

Yours sincerely,

J.C. Ogilvie, Ph.D.
Associate Professor of Psychology.

:em

June 23, 1965

Dr. J. M. J. de Wet
Department of Botany
Oklahoma State University
Stillwater, Oklahoma

Dear Dr. de Wet:

I have intentionally delayed answering your letter of June 3 because of our change in position and activities.

This fall I and my associates will be in the Department of Botany and Plant Pathology at Colorado State University. Until then we will be very busy wrapping up details before our departure from The New York Botanical Garden. It is our intention to continue our studies in more detail at Colorado where we have an opportunity to have students that we do not have here.

I trust that you can understand our position and will contact us again this fall.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

OKLAHOMA STATE UNIVERSITY • STILLWATER

Department of Botany and Plant Pathology
FRontier 2-6211, Ext. 550, 551

June 3, 1965

Dr. David J. Rogers
The New York Botanical Garden
Bronx, New York 10458

Dear Dr. Rogers:

We are definitely interested in having our data run with your program, and we are glad of the opportunity to collaborate with your research group.

We have a very large amount of data more or less ready to go. To give you an idea, in the complex genus Capillipedium - Bothriochloa - Dichanthium (Gramineae, Andropogoneae) we have studied some 50 characters in 1-10 specimens of each of approximately 1000 collections. These collections represent all known species and were obtained as far as possible from across the complete geographic range of each species. The collections were obtained as seed samples and were grown in a uniform nursery at O.S.U. We also have extensive studies to compare yearly variation, variation between field and greenhouse specimens, and between specimens from plants cultivated at Oklahoma and the natural maternal collections. We also have essentially the same types of data on 1-10 specimens of each of some 700 Cynodon collections (Gramineae, Paniceae) and some 700 Sorghum collections. Approximately 400 collections of Sorghum are cultivated types, all collected in their natural habitat and grown in a uniform nursery at O.S.U.

The characters are not all in a format that will fit immediate transfer to an IBM card, but they could be converted for this purpose without much trouble. I am including samples of our data sheets used for studying the various genera.

Financing such a vast study will present problems, but I have funds available for a trial run with Cynodon species.

Your suggestions will be appreciated.

Sincerely yours,

J. M. J. de Wet

J. M. J. de Wet
Associate Professor

JMJDW:DJJ

Enclosure

This is used for Bothriochloa, Capillipedium, Dichanthium and other Andropogoneae.

Name / No.	LEAF					SHEATH			NODES		STEM											
	Pubescence	L/W	Shape	Pungence	Position	Type	Pubescence	Ligule	Type	Pubescence	Size	Gr. Habit	Rhizomes	Stolons	Pubescence	Shape	Pigment	Internode	Thickness	Other

Shape	
LPA/LLR	Length primary axis / Length longest raceme
No. Nodes P.A.	INFLORESCENCE
No. Nodes L.R.	
No. P.R.	Primary Raceme
No. S.R.	secondary Raceme
No. Spik.	
Pubescence	
Other	
L/WGl.I	SESSILE SPIKELET
L/WGl.II	
Shape Gl.I	
Pub. Gl.I	
No. flor.	
Sex flor.	
4 lem.	
Shape Sp.	
L. Awn	
Pits	
Nervation	
Callus	
Other	

for each collection Number we study 10 plants if possible.

CYNODON

1-5. Number

6-7. Plant No.

8-9. Year

10. Field - Gr. H.

CYTOLOGY

11-12. Chr. No.

Meiosis

13. Regular-Irr.

14. Homog. - No

15. Structural - No

GROTH HABIT

16. Rhizome - No

17. Vulgarus - No

18. Laxus - No

19. Robustus - No

20. Suprarob. - No

21. Malagasus - No

22. Fine - No

23. Medium - No

24. Course - No

25. Afghanus - No

26. Africanus - No

27. Arcuatus - No

28. Barberi - No

29. Hirsutus - No

30. Transvaalus - No

31. Winterhardy - No

32. Strongly hardy - No

33. Weakly hardy - No

34. Disease Resist. - No

35. Very Suscept. - No

36. Weakly Suscept. - No

CULM

37. Nodes Pub. - No

38. Nodes Green - No

39. Nodes Blue - No

40. Nodes Red - No

41-43. Peduncle Length

44-46. 1st. Int. Node Length

PEDUNCLE LEAF

47-49. Length

50-52. Width

53. Pubescent - No

54. Sparsely Hairy - No

55. Pilose Above - No

56. Pilose Below - No

57. Pilose All over - No

PEDUNCLE SHEATH

58-60. Length

61. Pubescent - No

62. Pilose - No

63. Sparsely Hairy - No

1st INTERNODE LEAF

64-66. Length

67-69. Width

70. Green - No

1st INTERNODE LEAF (cont.)

72. Red - No
73-75. Sheath Length
76. Folded-Roled (Bud)
77. Arcuatus-shaped - No
78. Barberi-shaped - No
79. Other-shaped - No
80. Tubercled - No

LIGULE

81. Ciliate only - No
82. Membrane only - No
83. Cil. - membrane - No

INFLROESCENCE

84-86. Axis Length
87. No. Nodes
88-89. Raceme No. Total
90-91. No. at 1st. N.
92-93. No. at 2nd. N.
94-95. No at 3rd. N.
96-97. No. at 4th. N.

RACEMES

98-100. Length Longest
101-102. Length Shortest
103. Flexuous - No
104-106. Spik. No. L. Rac.
107-109. No. Spik. /L.L.R.
110. Red - No
111. Green - No

RACEMES (cont.)

112. Glaucus - No
113. Hairy - No

SPIKELETS

114-115. Floret No.
116. Rachilla Prod. - No
117-119. Length Gl. I.
120-122. Length Gl. II.
123-125. Length Flor.
126. Lemma Winged - No
127. Lemma Pub. - No
128. Glumes Pub. - No
129. Hairs Clavate - No
130-132. Gl. L/Fl. L.
133. No. Nerves Gl. I.
134. No. Nerves Gl. II

OTHER

135. Fertile - Sterile
136. Compl. Ster. - No
137. Part. Ster. - No
138. HCN Pres. - No
139. HCN High - No
140. HCN Low - No
141. Lemma Pub. Both - No
142. 1st. Int. Node L. Pub. - No
143. Sparsely Pub. - No
144. Pub. Above - No
145. Pub. Below - No.
146. Infl. Reflex - No

SORGHUM

1-5. Number	40. Red - No
6. Plant No.	41. Glaucus - No
7-8. Year	Ist INT. LEAF
9. Field-Gr. House	42-44. Length
CYTOLOGY	45-47. Width
10-11. Chr. No	48. Acuminate - No
12. Reg. Irreg.	49. Pub. - No
13. Homol. - No	SHEATH
14. Struct. - No	50-52. Length
GROWTH HABIT	53. Dist. Pub. - No
15. Rhizome - No	54. Sparsely Pub. - No
16. Cultivated - No	PANICLE
17. Weed - No	55-57. Axis length
18. Wild - No	58-60. Overall Width
19. Erect - Dec.	61. Open - Contr.
20. Robust - No	62. Strong-Mod. Br.
21. Slender - No	63. Br. Whorl. - Altern.
22. Tall - No	64. Br. Simple-Div.
23. Compact - No	65-66. No. Nodes Axis
CULM	67-68. No. Br. Node 1.
24-26. Height	69-70. Node 2.
27-29. Diam.	71-72. Node 3.
30-32. Peduncle L.	73-74. Node 4.
33-35. Ist. Int. L.	75-76. Node 5.
36. Node Pub. - No	77-78. Node 6.
37. Densely Pub. - No	79-80. Node 7.
38. Sparsely Pub. - No	81-82. Node 8.
39. Green - No	83-84. Node 9.

85-86. No. Br. Node others

87-89. Total No. Br.

90-92. Length Pr. Br.

93. Flexuous - No

94-95. No. Nodes/Br.

96-97. No. Sec. Br./Pr. Br.

98-100. No. Rac./Pr. Br.

101. Hairy - Essen. Glabr.

RACEMES

101-102. No. Ses. Sp./Rac.

103-104. No. Ped. Sp./Rac.

105. Pilose - Essen. Gl.

PEDIC. SPIKELET

106. Present - Abs.

107. Well Dev. - No

108. Male - No

109. Neuter - No

110. Deciduous - No

111. Pedicel Abs. - No

112-113. Pedicel Length

114. Ped. Unilat. Cil. - No

115. Ped. Bilat. Cil. - No

SESSILE SPIKELET

116. Lanceolate - No

117. Elliptic - No

118. Oblong - No

119. Ovate - No

120. Rotundate - No

121. Obovate - No

122. Rhombic - No

123. Closed - Open

124. Green - No

125. White - No

126. Red - No

127. Glaucus - No

128. Deciduous - No

129. Art. Freely - No

130. Art. Tardely - No

131. Art. with Rach. - No

132. Art. with Pedic. - No

133. Awned - No

134. Awn Prom. - No

135. Awn Small - No

136. Mucronate - No

137. No. Florets

138. Callus Acute - No

139. Pub. - Essen. Glabr.

GLUME I

140. Acute - obt.

141. Coriaceous - No

142-143. Length

144-145. Width

146-157. No. Nerves

148. Round - Flat

150. Depressed - No

151. Keel wing - No

GLUME II	GRAIN
152-153. Length	163. Longer than Gl. - No
154. Nerves	164. Much Longer - No
155. Pub. - Essen. Glabr.	165. Slightly Longer - No
LEMMA	166. Flat - Biconvex
156-158. Length + Awn	167-168 Length
159. Nerves	169-170 Width
LODICULES	171. White - No
160. Acute - No	172. Red - No
161. Nerved - No	173. Brown - No
162. Ciliate - No	174. Other - No
	OTHER

June 3, 1965

Dr. Frederick G. Meyer
In charge of the Herbarium
U. S National Arboretum
Washington 25, D C.

Dear Fred:

In answer to your letter of the 26th, after having read your description of the problem, it would seem to me that we definitely want to try to handle Coffea with the computer methodology.

I would rather talk to you directly than to try to set down a bunch of instructions as to what you should do. We have a number of techniques that are not written up that are critical to the problem when one is getting started.

If you find yourself interested in this project to the extent that you want to make a collaborative endeavor, I would suggest that you come and spend a day or two with me here in going over techniques with me and with my crew. Why don't you and Jean come up and spend a night or so at our house anyhow? We would like to see you.

Sincerely

DJR:MDF

UNITED STATES DEPARTMENT OF AGRICULTURE
AGRICULTURAL RESEARCH SERVICE
CROPS RESEARCH DIVISION

NATIONAL ARBORETUM

ADDRESS REPLY TO:
UNITED STATES NATIONAL ARBORETUM
WASHINGTON 25, D. C.

May 26, 1965

Dr. David J. Rogers
Curator of Quantitative Taxonomy
New York Botanical Garden
Bronx, New York

Dear Dave:

Thank you for your letter of 3rd May.

I have just received 50 reprints of my coffee paper, a copy which is enclosed herewith.

The problem of classifying in Coffea arabica is a puzzling and difficult one, to say the least. I am still not quite sure the kinds of discrete units that may be possible to identify in Ethiopian populations of C. arabica. This is a problem akin, I presume, with your own in Manihot esculenta. Of basic interest in C. arabica is to fathom the pattern of infraspecific variability and to sort out the variables that mean the most for classification purposes. It would be highly useful to know in what ways the patterns of variability are distributed geographically in Ethiopia. Also, it would be useful to know whether if, in fact, the many cultivars of C. arabica that have a genetic basis, are represented in natural populations in Ethiopia. We have the problem of tetraploidy involved in C. arabica vs. diploidy in all other known taxa of the genus Coffea. Ploidy is of the allo variety, presumably, but this has not been proved to complete satisfaction. What we now have in Ethiopia is a plant with a somatic complement of 44 chromosomes vs. 22 for the diploid taxa. Of basic interest, therefore, is the question how and whence did C. arabica originate. We know what the closest relatives probably are, or think we do. C. arabica is confined to the southwestern Ethiopian plateau in the montane rainforest. I am willing to say and I would emphasize this, that this region is the geographical center of modern dispersal of the Arabica coffee plant. I am not willing, however, to plot a distribution of the plant that will tell accurately exactly where the wild plant occurs. This, I feel, is impossible, because of the influence of man and the weedy nature of this plant. Whether man has caused it to become weedy, I wouldn't know, but in every way it acts like a weed should act according to the Andersonian definition. The closest modern relatives of C. arabica occur not in Ethiopia at all, but in central tropical Africa, the closest locality being perhaps

400 miles distant from an area where C. arabica is known to grow in a spontaneous state. Somehow, at some time C. arabica did get to the Ethiopian plateau and is now isolated there. Curiously, we cannot in any way associate this plant with any other geographical area of Africa as an indigenous species. Partly what this project means is to analyze to determine the variability, origin of C. arabica. My friend in India says he has been able to obtain a mutant sport identical to C. arabica by crossing C. eugenioides and C. canephora. Since he believes in Yoga, and I have not seen his plant, his alleged success is questionable to me.

To answer your question about possible cooperation. I am interested in solving some of the problems as outlined above. If your techniques would shed light, I have no objections to trying. What should I do? I have collected hundreds of leaves and mature fruits for whatever value they may be in trying to analyze variability. I collected this material because variation is most obvious in these organs. The flower may offer evidence, but I was not in Ethiopia during the flowering period.

With best regards,

Sincerely,

Frederick G. Meyer
In charge of the Herbarium
U. S. National Arboretum

Encl. (1)

University of Toronto
TORONTO 5, CANADA
DEPARTMENT OF BOTANY

June 2nd, 1965

Dr. D.J. Rogers,
New York Botanical Garden,
Bronx, New York 10458,
U.S.A.

Dear Dr. Rogers:

Your letter arrived about ten days ago but I have been away from Toronto attending meetings of the Canadian Botanical Association in Ottawa. Before leaving Toronto, however, I checked into the matter of your expenses and was informed that a cheque had gone forward about the same time that you wrote. I'm sorry for the delay, which was apparently not at our end but in the administration office somewhere. If by chance you have not received it, please write at once.

Thank you for the program listings which you sent with your letter. I have not yet had time to study them but hope we can test them on our Amelanchier data. I look forward to seeing your article in print in the near future.

Jean joins me in saying that we enjoyed your short visit at our home. We took the children on a trip to Harvard and Yale during the Easter vacation but didn't have time to add New York to the itinerary.

Yours sincerely,

Jim Soper

J.H. Soper
Associate Professor & Curator

JHS/cam

June 1, 1965

Dr. Llewellya Colinvaux
Department of Botany
Ohio State University
1735 Neil Avenue
Columbus, Ohio 43210

Dear Llewellya:

I am not sure if this is the ultimate or penultimate list of the characters and attributes. Whichever the case, they can certainly stand some modifications, best done by you and not by us.

Sincerely

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

THE OHIO STATE UNIVERSITY
COLUMBUS, OHIO 43210

COLLEGE OF AGRICULTURE AND HOME ECONOMICS
Resident Instruction—Research—Extension

BOTANY AND PLANT PATHOLOGY
Botany and Zoology Building
1735 Neil Avenue

Jaeger

25 May 1965

Dear Dave

Working on a new list of characteristics and attributes, I find, to my amazement, I have only a rather dated list of the ones Artemis compiled. I don't believe it dates back to before my first visit to you, for I note changes in my copy. I would like a copy of the latest - perhaps 3, if you have ample.

With best wishes

Sincerely

L Llewellyn

June 1, 1965

Dr. G. N. Lance
CSIRO
P. O. Box 109
Canberra City, A.C.T.
Australia

Dear Dr. Lance:

I am sending under separate cover the papers requested in your letter of the 5th of May. The reason for the delay is that I have waited to have a program listing for a completely new program that we have written. The new program has a completely new clustering technique and modifies the method of the development of similarities from that given in the BioScience article. We have found that the new program is much more satisfactory than the inhomogeneity program. Inasmuch as it is hierarchical, it is much more facile to run and takes out a considerable amount of guessing. In some respects it has similarities to Sneath's single linkage method.

I am including a program listing in hopes that you can decipher it. Most of it is in FORTRAN, and the only maching language is in the area of getting the Port-A-Punch cards read and into the computer. The machine language routine saves a considerable amount of storage space.

We have run the new program on several groups of plants. One is a genera of the Crassobalanaceae, the data provided to us by Dr. Gillean Prance who finished his work at Oxford two years ago where these data were first put up for factor analysis. Dr. Prance is very pleased with the results of this method and finds it an improvement over anything that has so far been done with his data. We have run it on a section of the genus Cassia for Dr. Howard Irwin with very fine results. We have run it on a small sample (some 68 objects) of the genus Streptomyces and find it very acceptable there. It has also been tested on a group of specimens representing the family of the subtribe Oncidiinae of the Orchidaceae, and again we are satisfied with the results.

I am sorry that we do not yet have a publication on this. We have submitted a manuscript to the Canadian Journal of Botany for publication, but it has not yet appeared in print, and we are not even sure that the paper has been accepted. I trust that you will find the program listing meaningful and that you will find that the program makes sense. Incidentally, the new program employs some of the principles of graph theory.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

CSIRO

P.O. BOX 109, CANBERRA CITY, A.C.T. TELEPHONE 40455. TELEGRAMS CORESEARCH CANBERRA

GNL:RAF 5th May 1965

Dr. D.J. Rogers,
New York Botanical Garden,
NEW YORK. U.S.A.

Dear Dr. Rogers,

I recently came across two of your papers on the classification of plants, (Science, October 1960, and BioScience, September 1964). Would you please be good enough to send me reprints of both these papers, and also a copy of the data which you extracted from the Hillis monograph. This is referred to in your Footnote 2.

I should perhaps explain that Professor Williams, of the University of Southampton, and I have been collaborating on work of this sort for some time. During a period which Professor Williams spent here a month or so ago, we developed a program for the Control Data 3600 computer, which deals with mixed data. By mixed data, we mean that which contains qualitative, quantitative and multi-state attributes. The similarity coefficient which we compute is a little different from yours, and our sorting procedure is also different. Thus, I would very much like to run your data on our program with a view to getting an assessment of the value of both our coefficient and our sorting procedure.

I should say that our sorting procedure involves replacing two individuals, which are combined, by their centroid, and then repeating the calculation. We have found that this technique is very powerful for purely qualitative or purely quantitative data, and this is why we used it in the multi-state case as well. However, the method of combining individuals is not easy, and our present one leaves something to be desired.

We have already written two papers on the subject, one submitted to Nature, and one submitted to the Computer Journal, and two more are in the course of preparation, and will be submitted to the Computer Journal. Meanwhile, we are trying to accumulate as much data as possible in order to get a good assessment of the merits of our technique.

Yours sincerely,

G.N. Lance,
Officer-in-Charge.

May 27, 1965

Dr. Jesse Fults
Department of Botany
Colorado State University
Fort Collins, Colorado

Dear Dr. Fults:

I am happy to report that I will be here during your visit in June. My daughter graduates on June 24 and that day will not be available to me, but on Friday, June 25, we would be pleased to see you.

The very best way to get to The New York Botanical Garden from midtown Manhattan, where I assume you will be staying, is via the New York Central Railroad, Harlem Division. I am enclosing a schedule of these trains for your benefit. You will see that it takes about 20 to 25 minutes from Grand Central Station to the Botanical Garden stop.

You can see the main entrance to The New York Botanical Garden from the station. Our office is in the Administration and Museum Building near the entrance.

I hope that we will be able to see you on the 25th.

Sincerely

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF
Enclosure

Botany Dept.,
South Road,
Durham, England.

May 26th 1965.

Dear Dr. Rogers,

Thankyou for your letter, & for the reprints which arrived only two days later - your letter took 10 days!

I have not yet published anything about my computer work on speciation processes. The work I described at Edinburgh was very much a ~~pi~~ pilot experiment on a slow machine. Since October I have been on sabbatical leave using a very much faster KDF9 machine at Newcastle. After a considerable period of experiment in search of biological sense, I am now in full scale successful production, & I hope to have a great deal of output in the next three months. I propose to have a paper ready this autumn, & I will certainly see that you receive an offprint.

Kind regards,

Jack Gray.

May 25, 1965

Dr. J. M. J. de Wet
Department of Botany
Oklahoma State University
Stillwater, Okla.

Dear Dr. de Wet:

I seriously doubt that your equipment has the storage capacity to handle the program for cluster analysis. We have been using a computer equivalent in size and storage capacity to the IBM 7090, the CDC 1604, and have had to do some machine-language programming in order not to over-load memory with FORTRAN type language. Actually, most of the program is in FORTRAN, but the input data are put in with the machine language programming subroutine.

I would suspect that you will be running very large numbers of objects into the program, and probably have a considerable amount of data (characteristics and attributes) for each object. If I am correct, I have an alternative suggestion that might save you time and money in the long run. Let me explain.

Since our write-up of our method in BioScience, we have developed a completely different clustering analysis program, one that we have tested on a number of problems ranging in size from intra-specific taxa to generic delimitation within a family. In each case we have been satisfied with the outcome, far more than with any other program yet devised. This program will handle up to five hundred objects with as much as 300 attributes per object. There are two major parts to the program: the first is the development of pair-wise similarities for all objects in the study; the second uses these similarity measures in the clustering analysis. Subsidiary to these is a plotting program that gives a modified dendrogram.

We have the program for these methods well worked out, but so far, it is necessary for the programmer to set dimension statements at several steps during the program, and we have not yet decided on a formal way of instructing others on how to set the dimension statements.

If you are interested, and have the data well in hand, we would be willing to run your data on the machine we rent (we do not have our own) provide you with the appropriate instructions, set the dimension statements, and give you the print out, if you are willing to meet the actual costs of the machine rental. I cannot give you a hard and fast figure for the costs until I know the number of objects you're interested in running and the number of defining attributes. Given these we can get a close estimate of running time and machine charges. To give you some idea, 31 objects run completely through all the programs in three minutes. 125 objects ran in 15 minutes, and the largest input yet, 350 objects, ran in an hour and a half. Machine costs are $215.00 per hour.

We have spent considerable time studying the problems of setting up characteristics and attributes to do the best job of clustering. Most of this is not published, but has been written up informally, more for the use of those who actually collaborate with us than for formal publication. We cannot emphasize how critical the input data formats are. Given a well-defined, well-constructed set of characteristics and attributes, we get a good classification. With less well-prepared input, we obviously get lousy clustering.

If you wish, we can work with you to get the most back from your data. I am enclosing two papers that have some bearing on this subject, but these are not complete, by any means. The informal write-up mentioned above is only for those who will actually work with us. Our interest in your work is that we want to test our methods on as wide a range of classifications as we can get, and we want to try various worker's data. So far, we have had good luck in that our collaborators have each contributed something more to our understanding of the working of the program, at the same time receiving back a very useful classifiying procedure.

I will be pleased to hear whether you wish to work with us.

Sincerely,

David J. Rogers
Curator of Quantitative
Taxonomy

Encl.

OKLAHOMA STATE UNIVERSITY • STILLWATER

Department of Botany and Plant Pathology
FRontier 2-6211, Ext. 550, 551 74075

May 21, 1965

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

Dear Dr. Rogers:

We have been working on the taxonomy of the Andropogoneae for the last six years, and have now come to the conclusion that the only possible way to analyze the accumulated data is by computer.

Our statistical laboratory is equipped with a 1410, 7 takes, 1 channel computer, and a Fortram IV and II compiler. Will the program you have written for your taxonomic studies work in our computer, and if so, is this program available to other taxonomists?

Your cooperation in this research project will be appreciated.

Sincerely yours,

J. M. J. de Wet

J. M. J. de Wet
Associate Professor

JMJdeW:bds

May 20, 1965

Dr. Brent Berlin
William James Hall
Department of Social Relations
Harvard University
Cambridge, Massachusetts 02138

Dear Dr. Berlin:

Your local IBM supplier should be able to find Port-a-Punches for you. In the event that your local supplier can not handle this, the last place that we ordered some from was IBM, c/o Mr. Kegler, 2 Broadway, New York, New York. The price for the board and the stylus is $5.50, and you will need clear templates to go with it. These come at the rate of six for $2.50.

As far as the cards are concerned, if you have your own design, you will have to pay for making up the special design which runs to about $75. If there is no particular concern for a design on the card, then you may probably order the blank Port-a-Punch cards very, very cheaply. If you want, we can order the cards of our design which IBM keeps for us at less than the cost you would have were you to make up your own design. IBM has to have our permission before you can use our design. In the event that you have not seen our card, I enclose one for your examination. If your need for cards is not more than, say, 2,000 or 3,000, we could let you have that number from here at $3.00 per thousand.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

HARVARD UNIVERSITY
DEPARTMENT OF SOCIAL RELATIONS

William James Hall
Cambridge, Massachusetts 02138

May 18, 1965

Drs. D. J. Rogers and H. S. Fleming
The New York Botanical Garden
Bronx 58, New York

Dear Drs. Rogers and Fleming:

I have recently read your report on your Port-A-Punch in the April, 1963, AIBS Bulletin. Professors A. Kimball Romney, Peter Raven (of Stanford University), Dennis Breedlove and myself are conducting ethnobotanical work among the Tzeltal of Chiapas, Mexico where I think your device will be of significant value. Could you send me information on where I might secure most quickly two of the devices with appropriate cards and instuctions?

Thank you,

Brent Berlin

Brent Berlin
Instructor in Social
Anthropology

May 20, 1965

Dr. R. D. Gibbs
Department of Botany
McGill University
Montreal, Canada

Dear Dr. Gibbs:

We are in the market for a chemotaxonomist. We would like to have a person who has had some experience in the field, if possible, and wonder if you have any recent graduates who might be interested in working here at The New York Botanical Garden.

We will attempt to make a chemotaxonomic study of the genus Cinchona and will be pleased if you could recommend someone who might be interested in this job. Clearly, a Ph.D. is preferable.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

May 19, 1965

Dr. John R. Reeder
Department of Biology
Osborn Memorial Laboratories
Yale University
New Haven, Connecticut

Dear John:

Could we impose on you again for an arrangement to sit down and talk as we did earlier this year? I would like, if possible, to meet with Dr. Youngken at about 10 A.M. on May 26. I hope this is okay.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

MEMORANDUM

1/4/65

TO: Mr. Kolkebeck

FROM: Dr. Rogers

Starting today and for the months of January and February, Miss Donna Estabrook will be working on an hourly basis (at the rate of $2.00 an hour) in the Department of Quantitative Taxonomy. The salary is to be charged to account #4396.

MEMORANDUM

5/18/65

TO: Mr. Kolkebeck

FROM: Dr. Rogers

Starting today and for a period of about a month, Mrs. Shirley Fearon will be working on an hourly basis (at the rate of $2.00 an hour) in the Department of Quantitative Taxonomy. The salary is to be charged to #4396.

May 18, 1965

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

Dear Heber:

The 26th is fine with me. I will arrange to have a space for us to meet at Yale in the morning. I think we can probably make it a bit earlier this time than we did last. I believe I could get there by 10 A.M. If that time is all right with you, we will go ahead and arrange it.

The application is now in rough draft and is in the hands of one of our assistant directors for his approbation. The budget is going to be a lot bigger than I expected, but I think I will leave that as is until you and I go over it. We may have to do some justification of this thing in person; I think it would be good if we could direct ourselves personally to Jelliff Carr or perhaps to Coatney or to Huff.

It turns out that a considerable part of our project hinges upon a large amount of spade work in determining whether or not the various reports of folk medicine have given us the correct botanical name, and once having determined the correct botanical name whether or not we can actually get the material in the quantities that you need. Some of the plants will no doubt be growing only in localities where allied people are not welcome. This will obviously restrict our endeavors, and we may have to make a substitute of a nearly related species. I can not judge beforehand whether or not the plant material can be obtained and part of the grant will be used in this determination.

I too was pleased with the meeting at Bethesda and feel much encouraged to go ahead with the submission of the grant.request.

Sincerely

DJR:MDF

UNIVERSITY OF RHODE ISLAND • KINGSTON

Office of the Dean *College of Pharmacy*

May 17, 1965

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

Dear Dave:

Thanks for your letter of May 14 and I am glad to know that you have completed your material for our joint project.

The earliest that I can get down to New Haven would be May 26 or 27. Unfortunately, I have a Ph. D. examination scheduled for one of my graduate students this Friday and possibily Monday next week and so cannot possibly get away.

I hope the delay in our getting together for another week will not seriously inconvenience you. Perhaps in the meantime you might wish to send me what you have so that I can look it over before we meet. If the 26 and 27 are not suitable dates, I believe I could get away early on Friday, May 28. Please let me know.

I think our meeting in Bethesda a week ago was most profitable and hope you feel the same. With best regards.

Cordially,

Heber

Heber W. Youngken, Jr.
Dean

HWY:BW

May 18, 1965

Miss Judy Tate Morgan
Department of Biology
P. O. Box 704
Princeton University
Princeton, New Jersey

Dear Miss Morgan:

I have an alternative suggestion to make to you concerning your interest in classification problems for Boltonia. It would strike me as a good deal better if you could come and spend some time with us going over the project so that we might give you the best possible advice on it.

It has been our experience that the input data needs very careful attention, and we have some new methodology here which may be more meaningful to you. Since I am not certain what computer hardware is available to you at Princeton, it would seem to me wise to have this information as well.

Why don't you drop in and see us some time? Let meknow when you are coming, and we will talkeit over.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

PRINCETON UNIVERSITY
PRINCETON, NEW JERSEY

Department of Biology

Reply To
P.O. Box 704

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

Dear Dr. Rogers:

I am currently engaged in a monographic study of the genus Boltonia (Compositae) and am interested in constructing a classification of these plants with the aid of the computer. All the data has been assembled and the Princeton University computer facilities and personnel are available to me. However, I have had very little experience in using the computer.

Would you consider loaning me a copy of your program? Certain modifications will have to be made in the program for use on our machines and for my material but with the aid of the computer personnel I think this can be accomplished. At any rate, I am most eager to try.

If possible, I would prefer a copy of the program and not the flow chart. But if you find it inconvenient to send the program, the flow chart will be appreciated. I will make a copy of the program and return it to you immediately.

Thank you for any attention you are willing to give this request.

Sincerely,

Judy Tate Morgan

Judy Tate Morgan

Hudson Laboratories of Columbia University | 145 Palisade Street
Dobbs Ferry, N. Y. 10522

Telephone Code 914, OWens 3-5800

called Thursday
20th

Serial No. 2778-LR
May 17, 1965

Dr. David Rogers
New York Batonical Garden
Bronx Park
New York City, N. Y.

Dear Dr. Rogers:

I have heard from Dr. C. S. Clay that you have made significant progress in your research on classification. I would appreciate it very much if you could send me any papers which you have published since your work with Dr. Tanimoto, becuase I too am interested in the problem. If nothing has yet been published, would it be possible for me to come to see you sometime to discuss your work?

Sincerely yours,

Lloyd Rosenberg

LR:dd

Lloyd Rosenberg
Research Scientist
Contract Nonr-266(84)

May 17, 1965

Dr. J. H. Soper
Department of Botany
University of Toronto
Toronto 5, Canada

Dear Dr. Soper:

At last I am sending herewith a copy of the program listings for our various activities. Inasmuch as it is in FORTRAN, I think you will be able to make something of it. There is only one part that is missing, but it would not be useful to you now with the 7090 but can be developed for the 7090, I am certain. The part that is missing is the program for ordering the similarity ratios from highest to lowest. Any good FORTRAN program written for the 7090 should do this job. ordering

You will see that there are three separate parts here. One part for computing the S_{ij} also gives you a print out of the input cards. We found this very useful to check to see whether the input data is correct or not. The ordering of the S_{ij}'s should follow the computation of the S_{ij}'s. What you get from this S_{ij} program is a triangular matrix. The fourth step, which we call "skyline" is done on some sort of graphing maching, the name of which I am not certain ~~of~~, but I think it is fairly common to most computer installations. Let me know how you come along with this; I will be happy to see if it works for you.

Incidentally, we have had a rough time getting the write up about this program accepted in the Canadian Journal of Botany. One draft of the paper was rejected, and I think we can understand that part of the rejection was due to the fact that we had overblown the descriptive parts in our effort to give an understanding to botanists about what is going on. After the rejection we reworked the paper completely and returned it to Dr. Michael Shaw, the editor, on the 14th of April but have not heard its fate.

It is very difficult for us to write papers trying to express in essentially laymen's terms what is going on in a mathematical model. All I know is that this program has come closer to doing the job of clustering according to the desires of the taxonomist of anything that we have ever seen. You will note that the word Chrysobalanaceae occurs on the cover of the printout. We actually

ran the program on 125 species selected to represent the various genera of the Chrysobalanaceae. The data was prepared for us by the expert in the field, Dr. Gillean Prance. Although we had very near minimal data for each of the objects that went into this study, the output, according to Dr. Prance, is the most exciting that he has had yet in the way of a computer run. He has had several different runs made on his data. Ours comes closer to reflecting his ideas of the group than any that has so far been done.

I would appreciate it if you would not publicize this program to anyone until we have word that the paper has been accepted by some journal for publication. I will keep you informed.

Incidentally, I wonder if there has been any effort for repaying my expenses up there. I have not seen any reimbursement although I submitted the expense account shortly after my return. Please give my regards to your wife; I enjoyed the short time we had at your home. I still remember the friendliness of your cat. It isn't often that folks have such animals around. I like to see that.

Sincerely

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF
Enclosure

Biological Sciences Information Services

OF BIOLOGICAL ABSTRACTS

3815 WALNUT STREET PHILADELPHIA 4, PA. U.S.A.

Telephone: EVERGREEN 6-0414 Cable: BIOABS Philadelphia TWX: 215-569-8821

Editorial Offices: PHILADELPHIA • MEXICO CITY

May 12, 1965

Dear Dave:

Thank you for your enthusiastic response to the taxonomy editorial--it's always a joy to have one's efforts praised. It often seems that people are more interested in passing on caustic comments than kudos, and while constructive criticism is also appreciated, I must admit I frankly welcome a word of approval now and again.

You have my permission to quote as much or as little of the editorial in question as will serve your purpose, and I wish you well in promoting the cause of taxonomy.

Perhaps one of these days we can get together to take a closer look at our respective systems for handling taxonomic information. Perhaps they have more in common than appears on the surface. I'd like very much to know in more detail how your system operates.

At any rate, you have our very best wishes in keeping up the good work.

Sincerely,

Phyllis

Phyllis V. Parkins
Director

P.S. I'm not really sure, but I don't think our 1440-model computer is that smart. Maybe our next one--the IBM-360 model may well be so equipped. I'll check on it.

Dr. David J. Rogers
The New York Botanical Garden
Bronx, New York 10458

PVP:nmp

Form Approved: Budget Bureau No. 99-R217
Approval Through: 12-31-65

NATIONAL SCIENCE FOUNDATION

5/5/65

CURRENT RESEARCH AND DEVELOPMENT IN SCIENTIFIC DOCUMENTATION, NO. 14

Project Description Form

- If you have more than one project to report for Current Research and Development in Scientific Documentation, No. 14, please describe each one on a separate form.
- Attach extra sheets of paper if more space is required.
- Mail completed form to: Project: CRDSD No. 14, Herner and Company, 2431 K Street, N.W., Washington, D.C. 20037

A. Institution at which work is being done:

The New York Botanical Garden

Address of institution:

Bronx Park, Bronx, New York 10458

Title of project:

The Use of Computers in Classification

Person directly in charge of project:

Dr. David J. Rogers

Person to whom inquiries may be addressed, if different from above:

Source of financial support:

National Institutes of Health and Office of Naval Research

Project began: 1962 Project scheduled to end: ?

B. Objective or purpose of project:

1. To provide computerized mathematical models for the purposes of general classification (cluster analysis). The major emphasis is for classification of biological entities in an hierarchical manner, with maximum information concerning each of the classified entities, the amount of connectedness to other entities. The classification should be useful to those interested in any type of classification problem, be it taxonomic, ecologic, or other.

2. To develop computerized models for the determination of the value of defining data to be used in the process of classification. Involved in this project are several levels: (1) how to recognize patterns (or characters); (2) how to optimally divide patterns into discrete states to define the objects under study; (3) how to determine the value of each pattern as it affects the classification; (4) how to deal with the situation in which not all desirable information is available for an object of importance in the study.

3. Develop retrieval systems to make the classifications useful for various purposes: correlations for various users; identification of unknowns; entry to the classification from any point.

C. Methodology and/or approach:

1. Two cluster analysis methods have been completely programmed and are available. The first, using an inhomogeneity factor is non-hierarchical and is particularly useful in determining, within clusters, the amount of variability, "noise," tightness of the cluster, etc. This model assumes that all clusters are essentially spherical. The second cluster analysis based on graph theory, gives an hierarchical classification and is more appropriate to biological taxonomy in that clusters may assume any shape.
2. Both clustering methods start with a developed pair-wise measure of similarity between all the objects in the study.
3. The model for the determination of the value of input data in the clustering analysis (character correlation) is based on information theory. This model is intended to be a complement to the clustering analysis method. In any classification scheme, the classification is only as good as the descriptive data used to make the classification. We must know whether the input data will indeed be useful in constructing clusters. The method will indicate the value of the states within a character, give the value of the character over the number of objects it will define, and give a measure of predictive ability for each character in relation to other characters.

D. Results or progress to date:

Some of the results are combined in part C. As indicated, two cluster analyses are immediately available. The inhomogeneity clustering method is described in BioScience 14(9): 15-28. 1964. In the second clustering method, a descriptive paper has been submitted for publication. The graph theory clustering method has been tested on a complex group of tropical legumes of the genus Cassia, and on the genus Manihot (Euphorbiaceae). In both cases, the results have been extremely valuable in that a classification was produced which could not have been otherwise achieved. The classification, based on a large number of descriptive states (over 400 in the case of Cassia, 250 for Manihot), is by far the most objective classification of a group of plants yet accomplished.

E. Current and future plans and activities:

1. Studies in pattern recognition. Botanical and zoological characters are "patterns." Symptoms of diseases are "patterns." It seems reasonable to use the methods used by taxonomists to establish characters as a take-off point to study the basic problem of how patterns are recognized. No immediate model suggests itself, but if this group is given grants to continue its work, this will be attacked.
2. As many different applications of models for character correlation and clustering as can be adequately handled, will be run. This will be done on a collaborative basis, in which experts in various fields will be asked to provide data, and the Department of Quantitative Taxonomy will provide the expertise on best methods of handling. The areas of immediate interest are botanical, but others will also be attacked, such as various types of disease, anthropological classifications, etc.

F. Specify name, brand, and model number of all equipment used or developed in project. (If none, so specify):

CDC 1604 32K memory computer

G. List any specialized dictionaries or thesauri, machine-readable texts, recordings, programming documentation, or other research materials generated in connection with project. Specify physical form (book, punched cards, magnetic tapes, etc.) and conditions of availability.

Two program decks (or program listings) are available to qualified workers. (Qualification means--has problem similar to ours, is willing to work with this department to develop models of more general and useful nature.) The programs are written largely in FORTRAN II. (Some machine language included.)

H. Definitions of unique, specialized, or local terms used in this project description (if not defined in Glossary, pp. 407-409, of the 13th edition of Current Research and Development in Scientific Documentation):

I. Definitions of abbreviations or acronyms used in this project description (if not defined in list, pp. 410-413, of the 13th edition of Current Research and Development in Scientific Documentation):

J. References to publications or reports resulting from project:

Rogers, D. J. and T. T. Tanimoto. The IBM Taxonomy Application--an experimental procedure for classification and prediction purposes. IBM Publication. Data Sys. Div., Math. & Appl. Div. 74 pp. 1959

Rogers, D. J. and T. T. Tanimoto. A computer program for classifying plants. Science 132(3434): 1115-1118. 1960.

Rogers, D. J. Recent endeavors with computers in taxonomy. The Garden Journal 11(6): 201-204. 1961.

Rogers, D. J. Taximetrics--new name, old concept. Brittonia 14(4): 285-290. 1963.

Rogers, D. J. and H. S. Fleming. Data recording: an inexpensive and efficient method. AIBS Bulletin 13(2): 42. 1963.

Rogers, D. J. and H. S. Fleming. A computer program for classifying plants II. A numerical handling of non-numerical data. BioScience 14(9): 15-28. 1964.

In addition, five articles are now in press or in preparation.

K. Copies of the publications cited above may be distributed by the National Science Foundation through the Clearinghouse for Federal Scientific and Technical Information. (Please check box below)

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

Office of Science Information Service

April 16, 1965

Gentlemen:

The National Science Foundation has contracted with the firm of Herner and Company of Washington, D.C. for the preparation of the 14th edition of Current Research and Development in Scientific Documentation (CRDSD). The publication is issued for the purpose of disseminating, on the widest possible basis, information regarding ongoing research and development projects in scientific documentation and related fields. Illustrative of the scope of CRDSD is the following list of headings and explanatory notes:

Information Needs and Uses - studies and analyses of the information needs of scientists, of the uses made of scientific and technical information, and of communication problems in science and technology, including publication studies and experiments with new publishing formats and techniques.

Information Storage and Retrieval - studies of methods, systems, and procedures for analyzing, organizing, encoding, storing, and searching subject matter, including theoretical studies of information storage and retrieval.

Library and Information Center Functions and Networks - analyses of library activities (acquisitions, cataloging, serials, circulation, document storage, user services, etc.) through systems studies, data processing or computer applications, cost or operations analysis, and behavioral studies or user requirements; also library simulation studies, experimental or demonstration prototype libraries, and economic or statistical studies; this includes interlibrary projects such as network design or modelling, shared resources, communication or data transmission linkages, or other cooperative developments.

Mechanical Translation - research on problems of automatic translation from one natural language to another.

Equipment - development of devices for the processing of scientific information including devices for reading, storing, searching, transmitting, and translating.

Related Research - work on problems whose solution is likely to have a direct impact on documentation, including such fields as character and pattern recognition, speech analysis and synthesis, linguistic and lexicographic research, artificial intelligence, and pertinent psychological and sociological studies.

In order to make the 14th edition of CRDSD as complete and up to date as possible, we are most anxious to have descriptions of all current research and development projects in documentation being conducted by your organization. We would therefore appreciate your completing the enclosed Project Description Forms, which have been assigned Bureau of the Budget Approval Number 99-R217, using one Form for each discrete project your organization has underway. All completed Forms, requests for additional Forms, and inquiries should be addressed to:

Project CRDSD No. 14
Herner and Company
2431 K Street, N.W.
Washington, D.C. 20037

Because of severe deadlines, we would appreciate your returning your completed Forms to the address indicated as soon as possible, but in any event no later than May 14, 1965.

In completing the Forms, please be as detailed and explicit as possible. Please use single space typing throughout. Wherever necessary, do not hesitate to use additional sheets to complete your response to the various categories of information requested. Where additional sheets are used, please indicate by means of headings from the Form the categories of information involved.

In listing publications or reports resulting from each project described, please include the following, in the order stipulated below:

Books and/or Reports: author(s), title, place of publication, publisher, date, report number

Papers and Articles: author(s), title, journal, volume, pages, date

Non-periodical Symposia, Proceedings, Compendia: author(s), title, name of publication, pages, place of publication, publisher, date

In regard to names, brands, and model numbers of equipment, and definitions of unique or specialized terms and abbreviations and acronyms, please be as detailed as possible in selecting what requires defining and in the composition of the definitions themselves.

When the 14th edition of CRDSD is completed, copies will be mailed free of charge to all organizations who have contributed to it by completing and returning Project Description Forms.

Sincerely,

Burton W. Adkinson

Burton W. Adkinson
Head

Form Approved: Budget Bureau No. 99-R217
Approval Through: 12-31-65

NATIONAL SCIENCE FOUNDATION

CURRENT RESEARCH AND DEVELOPMENT IN SCIENTIFIC DOCUMENTATION, NO. 14

Project Description Form

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- Attach extra sheets of paper if more space is required.
- Mail completed form to:
  Project: CRDSD No. 14
  Herner and Company
  2431 K Street, N.W.
  Washington, D.C. 20037

A. Institution at which work is being done:

Address of institution:

Title of project:

Person directly in charge of project:

Person to whom inquiries may be addressed, if different from above:

Source of financial support:

Project began: Project scheduled to end:

B. Objective or purpose of project:

C. Methodology and/or approach:

D. Results or progress to date:

(OVER PLEASE)

E. Current and future plans and activities:

F. Specify name, brand, and model number of all equipment used or developed in project. (If none, so specify):

G. List any specialized dictionaries or thesauri, machine-readable texts, recordings, programming documentation, or other research materials generated in connection with project. Specify physical form (book, punched cards, magnetic tapes, etc.) and conditions of availability.

H. Definitions of unique, specialized, or local terms used in this project description (if not defined in Glossary, pp. 407-409, of the 13th edition of Current Research and Development in Scientific Documentation):

I. Definitions of abbreviations or acronyms used in this project description (if not defined in list, pp. 410-413, of the 13th edition of Current Research and Development in Scientific Documentation):

J. References to publications or reports resulting from project:

K. Copies of the publications cited above may be distributed by the National Science Foundation through the Clearinghouse for Federal Scientific and Technical Information. (Please check block below)

May 14, 1965

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

Dear Dr. Rogers:

I was the graduate student who called to ask when you would present the panel discussion at Queens College. I have enclosed a self-addressed postal card so that you could let me know when you might have this or another one in this area.

Thank you.

Yours very truly,

Beryl Robichaud

Beryl Robichaud
2 Tudor City Place
New York 17, New York

Attachment

Ack. Post card sent 5/17/65 Cancelled - now scheduled

May 12, 1965

Dr. J. L. Crosby
Department of Botany
University of Durham
Durham, England

Dear Dr. Crosby:

I was very pleased to hear the presentation that you gave last August entitled "Computers and the Origin of Species." This is one of the first times that I have seen an actual methodology for testing various evolutionary processes.

I wonder if you have published the paper following this presentation. I would be pleased to see a copy or to have the cited reference if such is available. I am sending by separate mail several papers of our own which may or may not be interesting to you.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

THE UNIVERSITY OF MICHIGAN
DEPARTMENT OF BOTANY
ANN ARBOR, MICHIGAN 48104

May 10, 1965.

Dr. David J. Rogers
Curator of Quantitative Taxonomy
The New York Botanical Garden
Bronx, New York 10458

Dear Dave,

Many thanks for your good letter about the computer work and Dave Lellinger's evaluation of the Adiantaceae. I am glad that you are helping him; he feels that your methods make considerably more sense than the others which have been published.

I am more and more convinced that Step 1 in any study of this nature is to produce a good taxonomy. I do feel, though, that the matter has not yet been sufficiently tested in enough laboratories to assure that there is good agreement on Step 1. My own interests are less taxonomic and more systematic, in the sense that I don't much concern myself with classification per se; I am more worried about character-trends and evolutionary relationships. In other words, I am interested in Step 2 -- the evaluation of relationships. The latter depends on taxonomy, or at least that is the way it seems right now.

Dave's thesis represents an experiment, namely to see whether by evaluating characters he can come up with something approaching a general purpose taxonomy. It will be extremely interesting to see whether, after he has programmed the data according to your pattern, another classification will result which is similar.

We have a fellow here named Jim Farris, who has come up with a classification technique that simultaneously combines evolutionary and classificational methods. It is based on degree of divergence determined entirely by quantitative techniques. He gave a number of seminars on it last spring. I shall keep you posted on how his work is going.

I want to thank you for taking an interest in Dave's problem. I know that Dave is delighted to have this contact; and I hope that some interesting results will ensue.

With all best regards, sincerely,

Warren H. Wagner, Jr.
Professor and Curator.

SMITHSONIAN INSTITUTION
UNITED STATES NATIONAL MUSEUM
WASHINGTON, D. C. 20560

April 15, 1965

Dr. David J. Rogers
New York Botanical Garden
Bronx Park
New York, New York 10458

Dear Dave:

Many thanks for the help, encouragement, cards, puncher, offer of computer time, etc., etc. On the basis of what I learned about recoding my data, I have been able to procede, although the job is far from finished.

This coming week I'll write to Dr. Wagner, telling him that this new work will have to be kept out of the thesis, since there is no sense comparing two methods with two data sets. He will understand, I am sure, and will be pleased that this decision can bring my thesis closer to conclusion.

Another thing he needs to know more about is the great progress and worthwhile focus of your recent work. I, too, was unaware of exactly what you are doing; flying to New York was worthwhile if for no other reason than talking things over. Which brings up a possibility for the future. Although we did not get funds for fiscal year 1966 (ending June 1966), the Smithsonian hopes to expand its education and training functions. Among the programs which have been suggested is an interdisciplinary scientffic group, large or small, and the subject is very much open. Perhaps some time we could organize a two- to four-day taxiometrics session. We have a nice country house (mansion) which was given to the Smithsonian, and it is ideal for such gatherings.

Thanks again for everything; I'll write again soon.
Best regards to all,

Sincerely,

Dave

David B. Lellinger

April 26, 1965

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

Dear Herb:

I hope that you have had a report on my collaboration with Dave Lellinger by now. There are a couple of comments that I would like to make so that you understand our relationship and also to help clarify the thoughts about the use of computers.

First of all, our method will not classify if one is given the wrong sort of input data. It is an absolute requirement that all of the attributes of the organism be given. In Dave's case he had left out certain kinds of characters because his model did not require that they be used. Spore characters, for example, were not included. In a measure of over-all similarity between two things, if one leaves out information, obviously one is not going to be able to make any judgment about them no matter whether you use a computer or not. In Dave's case there was a heavy emphasis on characters of the rhizomes. This will be reflected in the classification that you make. These may sound like truisms, but it is something that is frequently overlooked. I should also like to point out that this is not a valid test of the method to determine whether it gives back the same conclusions desired. Dave had the idea when he first started here (now modified fortunately) that he wanted to run his information unaltered in our program and then make a statement in his thesis whether or not the method worked. This is certainly not pleasing to us because unless the data is prepared in a way we recommend we can guarantee that it will not give a good classification. I might say that this would probably be the case for Sokal's method.

It is further not possible to make a contrast between Sokal's method and ours for very fundamental reasons. Sokal's method employs statistical procedures in which the characteristics must have "well ordered" attributes. This means that each attribute must have some specific quantitative relationship with each other attribute. This is certainly not possible when dealing with qualitative characters. Furthermore, in Sokal's statistical method each attribute must be averaged against a mean and again taxonomic data can not be validly averaged against a mean. What you are interested in is a color of a particular organism. How can you average that color? Or you are interested in the shape of a leaf. How can you average the shape of a leaf? These inherent difficulties are the ones that we have tried to get away from completely in our method. We employ a non-statistical

method which comes closer to reflecting the way a taxonomist uses his informat on than does any statistical method known to us. If Dave had used all linear measurements or weight or chemical tests where concentrations are linearly ordered, then this might have some meaning in a statistical model, but most taxonomic data can not be so ordered. Therefore, the same sets of attributes can not be simultaneously utilized in both methods and have any meaningful comparison.

We have spent a very great amount of time studying character selection and the most appropriate taxonomic methodology for use of characters and attributes in a computer method. It must be recalled that there are different ways of looking at characters. In one sense a taxonomic character is intended to be a statement by which the plant can be reconstructed. In our sense the character is a bit of information which is directly comparable to other bits of information for the purposes of establishing similarities or differences between plants. If one prepares characters and attributes by the first of these two models, he will not succeed well in preparing his data for the computer. Dave had used the former method. Fortunately, it is possible in many cases to re-state the characters such that they can be used in the second sense. We went over most of Dave's characters and attributes with him, and we left him to set them up for appropriate runs on our ~~data.~~ method

I hope that you understand what I am trying to say--that there are many approaches to the use of computers in taxonomy and that these are not necessarily overlapping approaches. It is not the case that Sokal's method and ours are identical. Both are looking for clusters of organisms, to be sure. The way you get to those clusters is clearly different between theirs and ours. This is not appreciated by many folks in taxonomy today. One has to spend a considerable amount of time in this field before this understanding is reached.

It is also a truism that there is a difference between the studies of taxonomy and classification and the studies of evolution. What we have attempted to do is provide the best possible over-all phenotypic classification methods. If one wishes to study evolution, one has to have a model therefor, and this model should by all means be one that is theoretically sound before one attempts to make use of it. To insist that classification methods are identical to evolutionary methods is obviously wrong. I am not saying that you can not study evolutionary methods. I am merely saying that our model and all other computer taxonomic methods known to us are not intended to be evolutionary models. It is a classification model. This distinction should be kept in mind and should be known by taxonomists although I am afraid that such is not the case.

I should very much like to be able to talk to you about these difficulties. I want very much to help Dave out in his work. We are, as you probably know, going to run his data when it has been reorganized so that it will be meaningful for our method. He must know and you must know, however, that if all information is not included you will get a classification based on that which is. There is no magic in the computer, and it can not read something that is not there. Let me know if you are going to be in the New York area some time soon. I would very much like to talk to you about these things.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

CC: Dave Lellinger

SMITHSONIAN INSTITUTION
UNITED STATES NATIONAL MUSEUM
WASHINGTON, D. C. 20560

February 19, 1965

Dr. David J. Rogers
The New York Botanical Garden
Bronx Park
New York, New York 10458

Dear Dr. Rogers:

Dr. Wagner has asked me to try a "purely" taxiometric method as a comparison with the ground plan method. I have six sets of data, ranging from 5 to 19 taxa, each coded for 108 characters.

If I could submit the data on cards exactly in your format, if I can get money from the University of Michigan to bankroll the computer and staff time, and if you have time to see the thing through the computer, would you be interested in doing so?

Unfortunately, my access to a computer is now virtually nil. My programmer at Ann Arbor has left the University, and there is no computational facility available to me here yet, nor any staff assistance.

I have tried to follow your technique "by hand" using my smallest data set, but got bogged down in computations. But perhaps the stickiest wicket is the money. How much would be necessary?

By the way, I have found out an easy way to calculate s_{ij} with data in which there are no absences:

$$s_{ij} = \frac{\Sigma M}{2m - \Sigma M}$$, where m is the number of characteristics, not attributes.

You have doubtless known about this for a long time, and in attempting to accomplish a "by hand" analysis, it has been a real time saver.

Best regards to all; many thanks,

Sincerely,

Dave

David B. Lellinger
Associate Curator
Division of Ferns

P.S. If for any reason you don't want to get involved, do not hesitate to say so, and I'll try my best to dissuade Dr. Wagner from his present plans.

Ans 2/23
OK - Come with to see
us about collab.

Thomas J. Watson, Jr.
Old Orchard Road, Armonk, New York 10504

May 5, 1965

Dear Mr. Rogers,

Thank you for your recent letter requesting our financial support of The New York Botanical Garden.

Since matters of this nature are the responsibility of Mr. H. M. Sibley, our Assistant Treasurer, I am passing your letter along to him. After he has had an opportunity to review it and our requests of your staff, you may be sure that you will be hearing from us.

Sincerely yours,

T J Watson Jr

Mr. David J. Rogers
The New York Botanical Garden
Bronx, New York 10458

May 3, 1965

Mr. Thomas J. Watson, Jr.
International Business Machines Corp.
590 Madison Avenue
New York, New York

Dear Mr. Watson:

I should like to tell you about some of the work being accomplished at The New York Botanical Garden and some of the activites concerning which various members of IBM have called upon our experience to assist them in their own internal operations. In the Department of Quantitative Taxonomy we have specialized in the broad area of classification--the science of taxonomy. Our skills are such that we have generated many data useful in testing clustering techniques. Clustering techniques are of interest to science in general and of specific interest to medical diagnosis. The need for well structured data to test various clustering methods is critical, and because we have been able to provide appropriately structured data, some of your mathematicians come to us for advice. I might point out that although our data concern plants, an experienced biologist can determine that the type of data is nearly identical to that found useful over a great range of biological problems, whether they be medical, agricultural or social.

I bring this situation to your attention because, while we are willing to assist in some of the projects in which your staff is interested, it is a time-consuming endeavor and one which calls upon our expert knowledge, perhaps not immediately available within the framework of your organization.

While contributing to the work your staff members are accomplishing, we are being taken away from our own endeavors. The research and educational activities of The New York Botanical Garden are supported by private funds, and it is

in the nature of a private, non-profit research foundation. It is therefore not possible for us to supply the needs of the public except insofar as the Director of The Botanical Garden requests that we, for the good of the public, participate in certain activities.

Considering these factors, it occurs to me that if we are to be of some use to staff members in several divisions of your organization, then it is not unreasonable to request that some financial assistance be made available. In illustration I can point out that several industrial and commercial organizations in the New York area who use our facilities, library and advice have adopted this method of financial assistance. I trust that this appeal is reasonable and that you will understand our position in making this request.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

CC: Dr. William C. Steere

May 5, 1965

Dr. Howard Pfeiffer
Department of Botany
University of Connecticut
Storrs, Connecticut

Dear Dr. Pfeiffer:

Herewith are the papers we have so far published. The most exciting paper that we have is having a hard time getting into print because of a bunch of nuts who find it difficult to accept this as being worthwhile.

This is a manuscript not yet published and not, therefore, for distribution, if you please. It will, however, be the closest thing to a model for classification done anywhere by anyone. Please forgive the modesty.

Come down and see us some time.

Sincerely

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF
Enclosures

May 4, 1965

Dr. Frank G. Hawksworth
U.S.D.A., Forest Service
221 Forestry Building, C.S.U.
Fort Collins, Colorado 80521

Dear Dr. Hawksworth:

In answer to your query on the Port-A-Punch cards, you should fill out each card even if you break a character in two. The computer program does not recognize the differences in cards but goes ahead and counts up each character because we set into each run the number of attributes per character and the computer reads all cards for each object and does not make a distinction between card 1 and card 2, etc.

I can understand your busy season coming up and will not expect any cards from you until after the summer season. I frankly do not think that there is much use to make an analysis of the composite cards that you now have. However, if this is of great importance to you, send them along and we will see if we can get a run on them. Be sure when you send them that yougive us a table which indicates the number of objects, the number of cards per object, the number of characters and the number of attributes for each character. It would be wise if you would send us a listing of the characters and attributes if you have them. I hope this is not too much trouble. If it is difficult, it can wait until you have returned from your trip.

Sincerely yours

David J. Rogers
Curator ofQuantitative Taxonomy

DJR:MDF

UNITED STATES DEPARTMENT OF AGRICULTURE
FOREST SERVICE
ROCKY MOUNTAIN FOREST AND RANGE EXPERIMENT STATION

MAIN OFFICE
221 FORESTRY BUILDING, C.S.U.
FORT COLLINS, COLORADO 80521

IN REPLY REFER TO
4600

April 28, 1965

Dr. David J. Rogers
Curator of Quantitative Taxonomy
New York Botanical Garden
Bronx, New York 10458

Dear Dr. Rogers:

Thank you very much for your letter of April 5 and the reprints on numerical taxonomy. I have read your papers, and I'm anxious to see how our Arceuthobium data will work out. Unfortunately, however, I have not been able to transcribe the ~~species~~ SPECIMEN data onto Port-A-Punch cards; and with the field season fast bearing down on me (I leave for an expedition to Mexico next week), it doesn't seem likely that I will be able to do so until fall.

I would, however, probably be able to send you composite-cards based on several collections for each taxon. I gather that you don't think such an analysis would be so useful. In this case, though, it might be of interest since I'd say we know that we have about 25 taxa. The problem is deciding on their relative similarity as an aid to assigning taxonomic rank.

I do have one question regarding setting up the system for the Port-A-Punch cards. For example, one characteristic ends at 167. The next character has 8 attributes. I assume that 3 should go on the first card (168-170) and the other 5 on the second card (201-205). Or should all 8 go on the second card?

I will write you later after I have had a chance to enter our specimen data onto cards. This summer's collecting should enable us to get a better picture of variation within taxa.

Thanks again.

Sincerely yours,

Frank G. Hawksworth

FRANK G. HAWKSWORTH
Forest Pathologist

April 5, 1965

Dr. V. G. Hawksworth
U. S. Forest Service
221 Forestry Building, C.S.U.
Fort Collins, Colorado

Dear Dr. Hawksworth:

It was a pleasure to have been at Fort Collins and to see your interesting problem in Arceothobium. Enclosed is the reprint which I promised to send you to give you the process for recording the data.

In addition to the instructions given in the reprint, the following will give you additional methods of data scoring: The reprint supplies the necessary basic information for punching the cards used in our program. We have added several refinements since this paper. First, under (a) if a specimen is damaged, lacks information because of the period in the life history when the specimen was collected, etc., it is not necessary to punch out the lower row for that specific characteristic. The computer is programmed to disregard any unpunched characteristic in pairwise comparisons. Regardless, some investigators still punch out the lower row so that they may have a record of the amount of information missing for particular specimens.

Secondly, under (b) when by its nature a specimen can not have an attribute in a particular characteristic, we add an additional attribute within the characteristic specifying its absence. All specimens not having petioles would be punched for an attribute specifying that petioles are absent for each characteristic concerned with petioles.

An additional refinement is our manner of handling data which is orderable. For instance, in data which is divided up into a series of measurements, it is often undesirable to have complete mismatchs on adjacent or nearly adjacent attributes. The closer relationship of a specimen that is 10u long to another specimen that is 20u long as contrasted to one 50u should be reflected. It is only necessary when coding such characteristics to place the attributes in the desired order and to inform us which characteristics you desire to have handled in this fashion.

I am also enclosing for your convenience one of our specially designed Port-a-Punch cards which may give you the necessary information so that you may continue with our work. The BioScience article is included as well. Between these pieces of information it should be possible for you to process the data for Arceothobium in a manner which would be suitable to run in our program.

Sincerely yours

Enclosures
DJR:MDF

David J. Rogers
Curator of Quantitative Taxonomy

UNIVERSITY OF OREGON

INSTITUTE OF
MOLECULAR BIOLOGY

EUGENE, OREGON 97403
telephone (code 503) 342-2411

April 15, 1965

Dr. David J. Rogers
Curator of Quantitative Taxonomy
The New York Botanical Garden
Bronx, New York 10458

Dear Dr. Rogers:

Thank you for your letter of March 23rd which arrived just a few days before I left Milano and please excuse my delay in answering it. Thank you also for your suggestion to process our data with your new program based on graph theory.

If I understand correctly from your letter, you have made two main progresses: the first is represented by a new clustering method and the other by a more efficient coding.

We have also tried to exploit graph theory for clustering purposes, but we have not continued in this direction because the clustering was exactly what could be expected by a single linkage method. It also had the typical defect of that method: to form very elongated and tread-like clusters. Moreover, we were interested in a clustering method which had a test of homogeneity of the clusters and comprised instructions to reject those OTUs which lower the homogeneity.

The main reason for our desire to process our data with the method you have described in BIOSCIENCE is that this method comprises a homogeneity test; but we would be glad to test your new method if this also comprises some criteria for rejection from the clusters of OTUs which lower the homogeneity.

It seems very interesting to me that, by adopting your coding, Dr. Trejo has found a perfect correlation with chemical test data. I suppose indeed that this means a perfect correlation of morphological with chemical test data. I am particularly interested in this result as we have found that an Adansonian classification of streptomyces (which is mainly based on chemical tests) does not confirm the traditional classifications of this genus, based mainly on morphological data.

I shall write to Dr. Trejo to suggest that we confound our data, if this is possible, and process them together. We have data on almost 200 different strains, and he, I suppose, has also studied a large number of strains. It could be worthwhile to fuse (if it is possible) the two sets of data.

Thanks for your kindness.

Sincerely yours,

Luigi Silvestri

LS:mm

March 23, 1965

Professor L. G. Silvestri
Universita di Milano
Istituto P. Stazzi
Via Celoria 10
Milano 4, Italy

Dear Professor Silvestri:

The long delay in answering your letter of the 22nd of January is unforgivable. In defense of this delay, may I say that one of the reasons is that we have found a new methodology for clustering which has a great deal more merit and value than the method which we announced in BIOSCIENCE. The development of this program and its subsequent testing with various groups of organisms has indicated that we are closer to ideal clustering techniques than we would likely have achieved had we pursued our information theory model.

The present model, based on graph theory, is an extension of and improvement on the single linkage method proposed and later discarded by Sneath. To date, we have tested this new program on a small group, Streptomycetes, the data provided by W. Trejo of the Squibb Institute; on a group of taxonomically complicated tropical legumes; on cultivars of Manihot esculenta; and on several other groups of organisms. In each case it has been our pleasure to find that the new graph theory model is truly a step forward.

We would be pleased if we might collaborate with you on a test of this procedure with your information. I will describe below some of the activities required before submission of the data to our program. Inasmuch as the program is written partly in machine language and partly in FORTRAN for the CDC 1604 computer, it is unlikely at this time that it would be feasible to rewrite this program for any other machine including the 7090 series, and it would therefore seem more reasonable if you can find the additional funds to run your data through on the facilities that we rent from the Control Data Corporation.

The enclosed reprint (Data recording: an inexpensive and efficient method) supplies the necessary basic information for punching the cards used in our program. We have added several refinements since this paper. First, under (a) if a specimen is damaged, lacks information because of the period in the life history when the specimen was collected, etc., it is not necessary to punch out the lower row for that specific characteristic. The computer is programmed to disregard any unpunched characteristic in pairwise comparisons. Regardless, some investigators still punch out the lower row so that they may have a record of the amount of information missing for particular specimens.

Secondly, under (b) when by its nature a specimen can not have an attribute in a particular characteristic, we add an additional attribute within the characteristic specifying its absence. All specimens not having petioles would be punched for an attribute specifying that petioles are absent for each characteristic concerned with petioles.

An additional refinement is our manner of handling data which is orderable. For instance, in data which is divided up into a series of measurements, it is often undesirable to have complete mismatchs on adjacent or nearly adjacent attributes. The closer relationship of a specimen that is 10u long to another specimen that is 20u long as contrasted to one 50u should be reflected. It is only necessary when coding such characteristics to place the attributes in the desired order and to inform us which characteristics you desire to have handled in this fashion.

Note also that our procedures require that two-state characteristics have an attribute for each state such as 'produces lactose'--'does not produce lactose.'

Our experience with W. Trejo in Streptomycetes leads us to believe that if a critical and careful logical analysis of morphological characteristics and attributes is not made previous to the coding the proper affinities of the specimens will not be indicated. We are apparently much more familiar with morphological data in our milieu, and when we indicated to him how we would code his data, he obtained perfect correlations with chemical test data.

If you desire, we can supply Port-A-Punch cards required for this data preparation. The device necessary for punching the cards, the Port-A-Punch, is probably available to you through some IBM supplier in Europe. If you decide to prepare your data for running in our program, we will need (1) a list of the number of characteristics and the number of attributes in each characteristic, and with this listing it will be necessary to know (2) which characteristics are to be handled as ordered characteristics, and we will also need to know (3) the number of specimens. We trust that since we have a very limited budget you can select a meaningful small group of objects for the test case. It would be well if you could to keep the number of objects under 100.

We certainly hope that you will want to prepare data and have the test by our methods. We have at the moment submitted manuscripts for publication, but these are not yet in print. Therefore, it is difficult to describe the details of the methods at this stage of the game.

Very sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

January 26, 1965

Prof. L. G. Silvestri
Universita di Milano
Istituto P. Stazzi
Via Celoria 10
Milano 4, Italy

Dear Professor Silvestri:

We received with pleasure your manuscript, "Automatic Classification of Staphylococci by Principal Component Analysis and a Gradient Method," and are in the process of digesting the information therein.

We would like very much to try the program, modified since the one published in our last article, on your data. Inasmuch as our input is in the format of Port-A-Punch cards and inasmuch as our program to read this data in is written in machine language, it would seem to me more practical if you were to send us your lists of your data rather than having it already punched on to cards. However, if you do have it on cards and it is difficult for you to put it on to a coding sheet, then we will attempt to use the cards as they are.

We hope that we will be able to give you some results that may interest you.

Sincerely

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

UNIVERSITÀ DI MILANO
Istituto "P. STAZZI"
Via Celoria, 10 - Tel. 23.61.528
MILANO, 4

Milan, 22 January, 1965

Progetto Sistematica Attinomiceti
PSA/LGS/pd

DR. D.J. ROGERS
New York Botanical Garden
Bronx Park
NEW YORK 58, N.Y., U.S.A.

Dear Dr. Rogers,

Thank you of the reprint of "A computer program for classifying plants" published in Bioscience, 14: 15-28 (1964).

Could you send us the cards of the program? If the program was written in FORTRAN we could utilize it to process our data, with a computer as 7040 IBM or 7094 IBM which accepts programs written in FORTRAN.

Alternatively, could you process our data if we send them? We have a matrix of 80 characters (with three attributes: 1, 0 and NC) and 49 organisms. We could send you our data already punched on cards.

Please, find enclosed a preprint of our most recent paper, that we have sent to Jour. Bacteriology for publication and which will be published in the first months of 1965.

Truly yours.

Silvestri

Prof. L.G. Silvestri

SMITHSONIAN INSTITUTION
UNITED STATES NATIONAL MUSEUM
WASHINGTON, D. C. 20560

March25, 1965

Dr. David J. Rogers
The New York Botanical Garden
Bronx, New York 10458

Dear Dave:

Thanks much for your letter of March 22. I am glad to know that costs can be reduced somewhat, as Dr. Wagner hasn't come up with a nickel yet. But I've been hooked now, and a few hundred dollars won't stop me. Besides, I am trying to get an IRS ruling that will allow me to deduct out-of-pocket research expenses, the way university professors can.

I have asked for permission to come on Tuesday, April 13. I'll take a shuttle flight up and rent a car to drive directly to the Garden and back to the airport. Is this date okay? If not, it can be changed. Also, if you think that more than one day will be necessary, please advise me, as that will mean a new travel request will have to be prepared and approved. I do expect to be able to put in a full day up there, by leaving here very early and arriving at the Garden about the same time it opens; is 8:00 when you start your day?

The data, as I have them at hand, are in the form of a "raw" i x m table (up to 9 states coded 0--8) for 108 characters; also in the form of a "binary" i x m table with about 550 characters. There is a code book which will translate either table back into the biological information. I hope this will be sufficient. Card decks of data are back in Ann Arbor, and are apparently in use, but I could repunch the "raw" table if this proves useful. The Fiscal Division man who is in charge of the IBM 1440 has proved to be very cooperative; so a certain amount of keypunch use and computer time for re-ordering card decks, etc. can probably be arranged without much fuss.

Best regards; see you soon,

Sincerely,

Dave

David B. Lellinger
Division of Ferns

ans by post card 3/29

March 22, 1965

Dr. David Lellinger
Smithsonian Institution
Washington, D. C. 20560

Dear Dave:

In answer to your letter of the 15th, the best I can say from the provided information is that it would cost approximately $250 to $300 for the full run. I can also say that we are going to have a lot of talking to do before you are ready to run with our procedures.

We have developed a whole new set of coding methods and feel that we are on track toward some very sound methods that will make your information do what you want it to do in the clustering process or in the development of the S_{ij}'s.

When you come up, why don't you bring with you not only the coded data but also the raw data itself. This would help to to get together on appropriate punching techniques.

I won't be in the office on the 1st and 2nd of April, and I won't be here on the following Thursday and Friday, but I should be working during the week of the 12th. If you can make it up during that week, why don't you let me know?

Sincerely

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

Smithsonian Institution
Washington, D.C. 20560
March 15, 1965

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

Dear Dave:

Please excuse the long delay in answering your letter of the 23rd. It doesn't indicate a lack of interest on my part, but I've been busier than a segregationist in a Selma. I started a letter to you last week, then quit, when I got some time to see the fellow who is in charge of the Fiscal Division's IBM 1440. It looks like we could use it to change my data cards from my format to yours, if the work would warrent developing a short program to do this.

Your new program certainly sounds exciting. Does it retain the entropy idea and the expanding and collapsing spheres? It seems to me there is something inherently sound in those concepts. By the way, a zoology student at U. Mich. thinks he has a deductive mathematical proof to indicate that Wagner's and Camin & Sokal's dendrogram constructions are the most parsimonious possible.

Because of the fiscal quarter system that we use, I won't be able to come up before April 1. Dan Nicolson (Araceae) wants to spend a few days in your herbarium, so we may fly up together. At any rate, we can come whenever after April 1 is convenient to you--just let me know a couple weeks in advance so we can line up permission and money.

I'll bring my data along, but do you want it on cards or as a printout? Either way it is currently multistate, but with the IBM 1440, we could perhaps do some tricks, according to your specifications. Perhaps this explanation of my data will be of some help in your planning. The subfamily (represented by 78 taxa) is divided into six tribes:

TRIBE	NO. CODED FOR 108 CHARACTERS	NO. CODED FOR <108 CHARACTERS	TOTAL NO. CODED	SIZE OF i x m TABLE
#1 Onychieae	9	0	9	972
#2 Cheilantheae	19	2	21	<2268
#3 Pelleae	11	0	11	1188
#4 Gymnopterideae	13	2	15	<1620
#5 Gymnogrammeae	17	0	17	1836
#6 Adianteae	5	0	5	540
	74	4	78	

Where coding is incomplete and information is missing I had only literature or literature plus incomplete specimens. In every case these taxa are "perkipheral," specialized ones, so if your program isn't adapted to incomplete data (my programs aren't), there is no worry--we can just put them on the diagram at the end of the computer work, marked with a dotted line to their most probable place of affinity.

If this gives you a better idea of costs, I'd appreciate hearing about any possible reduction, as it will please Dr. Wagner. If you want to fill me in on the coding scheme so that I can try to have things prepared before the visit, that would be fine with me.

Many thanks; best regards to all,

Sincerely,

March 22, 1965

Dr. Llewellya H. Colinvaux
Ohio State University
Department of Botany
1735 Neil Avenue
Columbus, Ohio 43210

Dear Llewellya:

Thanks for your letter of the 17th. Before I read it, I had hoped that it would convey the news that you were supplying us with data tomorrow. I understand, however, that this is not so, and certainly we can't expect that you will be able to do so before the time you indicated.

Something that you will want to think about is that now we are working with specimens rather than with species. This gives us an opportunity to rework our conceptualization of what a species is. It also gives us an opportunity to determine the affinities of highly variable populations in which it is difficult to recognize the taxa.

As you know, we had always considered that our methods should be applied directly to the specimens rather than to take an already pre-conceived notion as to what the taxa are. When you are thinking of the data preparation, it would be well to consider using specimens directly and let the program generate the various taxa that you may wish to consider.

Inasmuch as our staff has been augmented by three members, we have had a much greater opportunity to investigate many of the aspects of the computer program. One of the most critical areas that we have worked on is the actual data preparation in which a considerable amount of effort has been made in the process of setting up our characters and attributes. We think we can derive considerably more information from the characters and attributes than by the method which we used originally.

It would certainly be well for you to begin to develop your ideas about the characters and attributes that you wish to use and for you to let us have an opportunity to go over the character and attribute listing before you proceed to collect large amounts of information, it being easier to modify before than after. If, in the near future, you can provide us with the listing of characters and attributes you would like to use now, then I think that we can advise you upon techniques which will make a much more meaningful array of input data.

Sincerely

David J. Rogers
Curator of Quantitative Taxonomy

DJR:mdf

THE OHIO STATE UNIVERSITY
COLUMBUS, OHIO 43210

COLLEGE OF AGRICULTURE AND HOME ECONOMICS
Resident Instruction—Research—Extension

BOTANY AND PLANT PATHOLOGY
Botany and Zoology Building
1735 Neil Avenue

March 17, 1965

Dr. D. J. Rogers
N.Y. Botanical Garden
Bronx Park
New York 58, New York 10458

Dear Dave:

I have been slower than anticipated in sending this letter. My apologies. Your last letter helped me to visualize some of the future work. A somewhat awkward feature is that, with the exception of the new species H. cryptica for which you already have the data, extensive additional data will not be ready for probably a year. This delay was not anticipated when I visited you last winter, but N.S.F. support for my study of the Indian Ocean Halimedas has only just come through. Thus last year was spent on somewhat physiologic instead of taxonomic aspects of Halimeda.

It might be useful to you if I indicated the sort of data that is likely to come out of my current study. There will be at least some increase in the number of attributes. Furthermore, some attributes can now be applied more rigorously. This new usuage began with the cryptica paper. It will be continued for all those species occurring in the Indian Ocean, but will not, in the immediate future, be applied to non-Indian Ocean species (except H. cryptica).

It is likely that Taylor's new species H. velasquezii is in the Indian Ocean collection. If so, its description will be made parallel to the others, and a number of gaps in its known characteristics and attributes thereby filled. And some of its characteristics and attributes as understood from Taylor's description may be modified, for his description is based on a single plant.

Since considerably more will be known about Halimedas in the Indian Ocean after this study, it may be possible to now apply the characteristic of geographic distribution. I have fertile material of at least two species for which gametangia were previously unknown, but the characteristics of reproduction will probably still not be sufficiently known to use.

It is conceivable that I may incorporate into the study one or

more closely related genera for comparison with Halimeda. The most likely is Tydemania which has two known species. This genus which is uniaxial is of particular interest now that a uniaxial species (H. cryptica) has been placed in the formerly exclusively multiaxial genus Halimeda.

I hope the delay will not create undue problems, and that this sort of data will be useful for joint work.

Sincerely,

L Lewellya

Llewellya H. Colinvaux
Assistant Professor

LHC/cl

March 12, 1965

Mr. Theodore J. Crovello
Department of Botany
University of California
Berkeley 4, California

Dear Mr. Crovello:

I am sorry it has taken us so long to get around to answering your letter of February 25. Part of the reason for it is that we have been spreading the word by seminar technique in various eastern universities. So far we have not had any takers or interest from the west coast for seminars, and I am not sure that I will have an opportunity to speak in California anywhere inasmuch as there are no botanists other than yourself who have either evidenced interest in our work or have done anything of their own. I will try to answer the questions you raised in your letter but will probably not answer them all.

You have the right idea concerning my work with Manihot. I have not published anything on this work for several reasons. Although the BioScience article indicated that we had a working program, we have discovered that the model that we had for it had some serious flaws in it. These most specifically center around the idea of a cluster having to be spherical. The mathematical nature of the model insists that the cluster be spherical. This is obviously a restriction unbearable in the business of taxonomy where clusters should be considered as having no shape at all. As a result of our discovery that this was the case, we have written a completely new program, which in some of its aspects looks like Sneath's single linkage method published in his paper in the Journal of General Microbiology in 1957. However, the new model goes considerably further than the model proposed by Sneath and gives a considerable amount more information. We have found our new program very acceptable in that we have now run the data on the Manihot cultivars and on the series of specimens selected from the subtribe Oncidiinae of the orchids, and a section of the genus Cassia from tropical America. These have indicated a satisfactory method, and therefore it should not be too much longer before we will publish something on Manihot, Cassia, and the orchids.

It is practically meaningless to publish matrices of similarities when large numbers of objects are under study. Because of the inter-relatedness among the similarities in pairwise analyses, there is very little that can be seen from a cursory examination of such a table. We have found that the number of defining attributes for the study of Manihot is much too small a number to give the best kind of classification. There were 15 characters and 48 attributes used. These were selected by me at a time when I was not aware of the need for using a larger number of characters and attributes; but in defense of my selection of this small number,

I had to use characters which would be applicable to the wide range of Manihot cultivars which do not under normal circumstances have floral characters when the sample is taken. Only infrequently do I find floral characters available to me. We must, therefore, to be practical, stick with the kinds of characters that the plants most generally will present to the worker who is interested in the classification; so in spite of the small number of characters, we will have to find some way to deal with them. Interestingly enough, the small number of characters do provide us with a classification, but we will have to think very hard about the classification of cultivars anyway whether we use computer methods or not. This is true because no one has really set down as yet what should be the nature of subspecific characteristics of cultivated plants. We are working completely in the dark as you can well understand.

We do achieve clustering of the cultivars. What is the meaning of these clusters? At what level can we describe them in the usual taxonomic hierarchy? Are they meaningful in the same terms as the usual subspecific epithets? My answer to most of these questions is that we simply do not know. Working in wild plant taxonomy is "duck soup" as related to working with cultivated plants. This is not to say that there are not many complicated problems in wild plants. Salix is a good example; Cassia is another, and there are a large number of other "refractory" groups. This is about all I want to say about Manhot at this stage of the game; maybe this answers some of your questions, and maybe it does not.

Now to tell you something about the Department of Quantitative Taxonomy. It was the decision of the administration of the Botanical Garden that since my interest lay in this direction it would be wise to give me the title to go with it. I more or less had a free hand in deciding how such a department would operate. I am the only staff member on the permanent staff of the Botanical Garden. There are four men working with me who are paid out of grant funds. Research Associate in our terminology indicates a person who has finished his degree or who is not now actually working toward a degree but is a person working on a research project. Our goal is to study the dynamics of taxonomy and to discover some of the models intuitively used in the field. You must remember that taxonomy is a very useful science and that its practitioners have evolved a beautiful technique for the process of classification. What we want to do is to see if we can establish some generalized rules by which the process goes on, quantify these, and put some systematic basis to them so that anyone who wishes to be trained in taxonomy may examine these rules and learn procedure by other than trial and error technique. Involved in such studies are the problems of pattern recognition. To rephrase this, one might say that we are trying to find out whatis a character. Given this, how does one go about the business of defining the attributes of the character

in such a way as to give the maximum amount of information about the selected character as it pertains to the classificatory process. Then if we use this character and we have a good method for establishing the kinds of information required for classification, how do we use this information to actually go through the process of classification? The whole classification process involves many different methodologies, only some of which are our immediate concern.

We have not worked on the logic inherent in the processes of nomenclature. This is a valid area for examination and needs a considerable amount of study before we can say anything about it. How do you establish good keys? What is a key in the first place?: What is the process by which a taxonomist establishes a key? When does he know he has a useful one? We will not even consider at the moment the problems that are involved in the development of a species. These are too hairy even to be thought about until a considerable number of actual plants are run through the program and we have a battery of data built up to discover the relationships among the clusters considered to be a good species.

Lastly, concerning your data sheet for Salix. I am very pleased to see that you have given considerable thought before actually making an analysis of Salix to the actual business of giving finite statements to your intuitive feeling about what characters are. This is a very good approach. If consistently applied, you will be much farther along even though you never approached the computer with it. Would that all people working on taxonomy would do such as this! I am sure that most people have given it up after some trial because of the fact that they did not have the advantage of a computer to correlate from the immense number of data sheets that will be generated when you work with such a complex set of information as you will achieve.

I will not make any specific comments about the form which you have employed. You obviously know more about Salix and of the need for it than I do. This must be always emphasized--that the taxonomist is the guy who should generate the data. You knowledge of the biological problems with Salix is more critical than anything else in this kind of work.

May I say that I am very pleased that you are working on this thing. I think I know that you are having to fight an uphill battle in the department there, but let me also say that anybody who does this sort of thing these days is fighting an uphill battle. But hang on because I think we will be seeing a remarkable change in a fairly short length of time.

Sincerely

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

Department of Botany
University of California
Berkeley 4, California
February 25, 1965

Dear Doctor Rogers:

I trust the enclosed data sheet and procedure will interest you and you will feel free to comment on it and suggest improvements.

There are a number of reasons for writing today. I trust you will be able to help me.

The first deals with the study that you and Doctor Tanimoto did. Currently I am writing a paper on my procedures and in the beginning I compare the work of others already completed. As I see it, this is what you did. Working within one species, 300 different cultivars were found or grown by you. Each cultivar consisted of a group offifteen or more plants from the same site(or same seed set ?) . From each cultivar you selected one plant and scored that plant. The data from this plant was then used for comparisons with the other cultivars. Please let me know if this is not correct. Also, have the actual results, showing the various matrices, OTU x Character and OTU x OTU, ever been published? And finally, how many characters were used and how many readings of each individual character were made per plant? It would be a great help if you could supply this information.

For the same reason, I wonder if you could fill me in on what is happening at the Garden in the field of quantitative taxonomy. I noticed in Bioscience that two research associates had joined the "Department of Quantitative Taxonomy". Have you now a formal department there and are your research associates graduate students like myself or are they at a more advanced level? Needless to say, whatever information you can give me will serve to make more people aware of what is going on under Doctor Rogers at the Garden, and communication at this stage of the development of quantitative taxonomy is most important for its future.

I hope I have not requested too much of you, and trust you will feel free to criticize my data sheet and procedure.

Sincerely yours,

Ted Crovello

Theodore J. Crovello

Feb.9,1965

Procedure For Use Of A Morphological Data Sheet For Numerical Taxonomy

by

Theodore J. Crovello

The attached data sheet, consisting of four pages, records variation in external, morphological chafacters. When subsequent information of chromosome number, wood anatomy and paper chromatography is added, the combined knowledge will form the basic data for a numerical taxonomic study of the genus Salix in California. As of January 1965 the data sheet scores over 140 characters, including both two-state and multi-state characters. The individual organism is taken as the Operational Taxonomic Unit. Thus, only one plant is scored per four page data sheet.

For convenience the characters are grouped according to organs. Space is provided for recording the state of each character. Some are recorded only once, as Growth Form of Twigs. This occurs when by past experience the character state is known to be constant on the entire individual. However, as seen on the sheet, most characters have five small spaces to the right of each character name, which form five columns down the sheet when combined with other character space rows. The point is that all the measurements down any one column are from the same mature leaf, the twig that bears it and the proximal leaf blade on that twig. The same is true for the other organs. Thus all the measurements in the first column of ament characters are from the same ament, those in the second are from a second ament, etc.

Hence, from one plant information will be recorded on the data sheet from a minimum of 5 twigs, five buds, five leaves, five aments, etc. Some characters are scored from more than five instances. For example, pollen size is measured from 50 or 100 pollen grains. The larger column on the right end of each row is reserved for the value of the character state (mean or range) that will be recorded on IBM cards for processing.

The method used to decide which five instances of each character are to be scored is as follows. The available material is scanned with regard to each character, eg. bud size. Two or three buds exhibiting extremes are selected for scoring and then the remaining material is scanned again to make sure the range has been covered. If so, then the remaining number is selected with a view to completing the continuum of variation exhibited by the organism. This procedure succeeds in describing the range of variation. It makes no claim to provide an adequate sample to obtain a frequency distribution of the states of each character on an individual plant.

Some characters are obviously redundant. These will not be processed in this study.

This list includes all morphological characters previously used in the taxonomy of the genus, and all that have come to mind outside of that. Suggestions of additional characters will be appreciated, as will constructive criticism on any stage of the study.

The following characters have been added as of January 1965.

Page 1. Plant height and width.
Bud scale fused or open.
Bud [illegible] cross sectionshape.
Bud Apex.

Page 4. Nectary width.
Stigma revolute.
Stigma pubescent.
Stamen filaments divided.

Data Sheet For Numerical Taxonomy

The Genus Salix

Theodore J. Crovello

Collection no. _____ Location: _____

Herbarium no. _____ _____

Date collected: _____ Habitat: _____

Date tallied by TJC: _____ _____

Given epithet: _____ _____

N.T. epithet: _____ Water: Standing__ Running__ Not evident__

HABIT CHARACTERS

Erect_____ Dwarf shrub_____

Trunk: Simple____ Many branched at base_____

Bark: _____

Branches: Erect_____ Pendulous_____

TWIG CHARACTERS (last year's) Fl____ Fr_____ Veg_____ Age_____

Growth Form: Straight_____ Crooked_____

Color: _____

Lenticels: Present_____ Absent_____

Shape: _____

Pliability: Flexible_____ Brittle: _____

Length of Twig: (cm)	___ ___ ___ ___ ___	_____
Diameter: (mm)	___ ___ ___ ___ ___	_____
Cross section shape:	___ ___ ___ ___ ___	_____
Pubescence:	___ ___ ___ ___ ___	_____
Flakiness:	___ ___ ___ ___ ___	_____
Pruinose:	___ ___ ___ ___ ___	_____

BUD CHARACTERS This year's buds ____ Last year's buds _____

Color: _____

Two at each leaf scar: Yes_____ No_____

Length:	___ ___ ___ ___ ___	_____
Position: (alt. vs subopp.)	___ ___ ___ ___ ___	_____
Position: (Appressed or not)	___ ___ ___ ___ ___	_____
Pubescence:	___ ___ ___ ___ ___	_____
Surface: (smooth or wrinkled)	___ ___ ___ ___ ___	_____

LEAF CHARACTERS Flr_____ Fr_____ Mature____ (Characters from most mature)[2.] (leaf.)

Stipules:
Present_____ Absent_____

Deciduous or Persistent: ___ ___ ___ ___ ___ _____
Length: ___ ___ ___ ___ ___ _____
Shape: ___ ___ ___ ___ ___ _____
Margin: ___ ___ ___ ___ ___ _____
Glandular: ___ ___ ___ ___ ___ _____

Petiole:

Length: ___ ___ ___ ___ ___ _____
Glandular: ___ ___ ___ ___ ___ _____
Cross section_ ___ ___ ___ ___ ___ _____

Blade:

Length: ___ ___ ___ ___ ___ _____
Shape ___ ___ ___ ___ ___ _____
Margin: ___ ___ ___ ___ ___ _____
Margin Glandular: ___ ___ ___ ___ ___ _____
Margin revolute: ___ ___ ___ ___ ___ _____
Base shape: ___ ___ ___ ___ ___ _____
Apex shape: ___ ___ ___ ___ ___ _____
Texture: ___ ___ ___ ___ ___ _____
Veins Prominent below: ___ ___ ___ ___ ___ _____
Color of veins below: ___ ___ ___ ___ ___ _____
Abaxial side glaucous: ___ ___ ___ ___ ___ _____
Abaxial side pubescent: ___ ___ ___ ___ ___ _____
Adaxial side pubescent: ___ ___ ___ ___ ___ _____
Venation pattern: ___ ___ ___ ___ ___ _____
Number of leaves from tip: ___ ___ ___ ___ ___ _____
Width ___ ___ ___ ___ ___ _____

THIS YEAR'S TWIG CHARACTERS

Length of twig: ___ ___ ___ ___ ___ _____
Number of leaves on twig: ___ ___ ___ ___ ___ _____
Diameter: ___ ___ ___ ___ ___ _____
Pubescence: ___ ___ ___ ___ ___ _____
Flakiness: ___ ___ ___ ___ ___ _____
Pruinose: ___ ___ ___ ___ ___ _____
Color:_____
Growth Form: Straight___Crooked_____

PROXIMAL LEAF BLADE

Length: ___ ___ ___ ___ ___ ___ _____
Shape:

Width
Margin:
Margin glandular:
Margin revolute:
Base Shape:
Apex Shape:
Texture:
Veins Prominent below:
Color of Veins below:
Abaxial side glaucous:
Abaxial side pubescent:
Adaxial side pubescent:
Venation pattern:

AMENT CHARACTERS Flr_____ Fr_____

Time of flowering: Precocious____ Cotaeneous____ Serotinous_____

Number per lateral branch: _____

Ament length:
Ament width:
Peduncle length:
Peduncle leaf number:
" " length
" " width
" " margin
" " pubescence
Direction of flowering:

Ament dense or lax: _____

Rachis cross section: _____

COMMON FLORAL CHARACTERS Flr___ Fr___

Scale length:
" shape:
" adaxial pubescence:
" abaxial pubescence:
" color:
" margin:
" persistant or deciduous:
" veins at distal end (no.):

Nectaries (adaxial)
Number:
Shape:

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Psychological Services for Industry

March 3, 1965

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

Dear Dr. Rogers:

A progress report. I should have known better when I talked to you on the phone and estimated a week or two to get my data on the Port-a-Punch cards. I have been plagued with the delays and problems that one always is plagued with on these projects.

The latest word from the girl who is doing the punching is that the proofreading should be completed in another 6 or 8 hours. But, since this is a part-time operation for her I can only hope that they will be ready to go by the end of this week.

I'll call you when I have them firmly in hand.

Sincerely,

Theodore W. Hurst/bpm

Theodore W. Hurst

TWH-bpm

IBM

590 Madison Avenue
New York, N. Y. 10022
PLaza 3-1900

International Business Machines Corporation

February 26, 1965

Dr. D. Rogers
New York Botanical Gardens
Bronx, New York

Dear Dr. Rogers:

This is to confirm and clarify our conversation of February 24, 1965 concerning the publication of the results obtained by processing Dr. Prance's Chrysobalanacae data with the new taxonomy program I am developing. My understanding is that the following were the most pertinent points discussed:

1. Dr. Prance will provide me with the taxonomic interpretation of the results which I will incorporate in my discussion of the results obtained with my program. It is understood that you will have a chance to review and approve this section of my paper before publication.

2. It is IBM's policy not to use an individual's or an institution's name in connection with any IBM publication or advertisement without previously obtaining their approval. This policy naturally also applies to this case.

3. I appreciate your interest in the journal to which my paper will be submitted and I will give careful consideration to any suggestions that you care to make. However, I wish to reserve the right to submit it to the journal of my choice, including as a possibility the IBM Journal of Research and Development.

I trust that if any of the above do not reflect your understanding or meet with your approval, you will let me know.

I want to thank you for your continued cooperation with us. It is always a pleasure to consult with you and your staff.

Yours truly,

Arnold Rubin

J. Rubin
Advanced Engineering Applications
Mathematice & Applications

okd by phone
3/10

JR:mk

cc: Mr. S.M. Matsa

February 10, 1965

Treasurer, Systematics Association
c/o British Museum (Natural History)
Cromwell Road
London, S.W. 7, England

Dear Sir:

Enclosed herewith is my check in the amount of $3.00 to cover annual membership fee for Dr. David J. Rogers and for myself, both at the above address. We shall appreciate your taking the necessary steps to enroll us as members of the Systematics Association and to put us on the mailing list for your publications.

Sincerely yours

Henry Fleming
Research Associate
Department of Quantitative Taxonomy

HF:MDF

March 1, 1965

Dr. William S. Benninghoff
Department of Botany
University of Michigan
Ann Arbor, Michigan 48104

Dear Dr. Benninghoff:

Enclosed herewith is a Xeroxed copy of our SCIENCE article and a reprint of the BIOSCIENCE article.

You may be interested to know that we have developed a new clustering program that is much more satisfactory to us than the clustering techniques described in BIOSCIENCE. It is a much simpler procedure with much more information and has the advantage that it is much less time-consuming when run on the computer and is capable of handling much more data than we had been able to do with the clustering program in BIOSCIENCE.

We would indeed be pleased to have a copy of your program for the ordering of phytosociological tables. We would prefer the FORTRAN II program and would be pleased also to receive your manuscript describing it.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF
Enclosures-2

THE UNIVERSITY OF MICHIGAN
DEPARTMENT OF BOTANY
ANN ARBOR, MICHIGAN 48104

February 22, 1965

Dr. David J. Rogers
New York Botanical Garden
Bronx Park
New York, N. Y.

Dear Dr. Rogers

Your papers on applications of computer techinques to methods in plant taxonomy (SCIENCE 132: 1115, 1960, and BIOSCIENCE 14: 15, 1964) are useful to me because of parallels with work I have been doing on the ordering of phytosociological tables. If you still have reprints of your papers, I would appreciate having copies of them.

W. C. Southworth and I demonstrated some of our results with the 7090 Computer at the Botanical Congress in Edinburgh, and a copy of the abstract is enclosed. If you think any aspects of our techniques might be adaptable to sorting out morphologic characters for taxonomic uses, I would be glad to send you a copy of our two best programs in either MAD or FORTRAN II and a copy of the full manuscript we expect to send off for publication shortly.

Sincerely yours,

William S. Benninghoff

William S. Benninghoff
Professor of Botany

Enclosure
WSB:bh

Copied from: Tenth International Botanical Congress, Edinburgh, 1964,
Abstracts of Papers, Part 2, pp. 331-332.

BENNINGHOFF, W. S. & Southworth, W. C. (The University of Michigan, Ann Arbor, USA) Ordering of tabular arrays of phytosociological data by digital computer-----The eye-and hand sorting procedures used by phytosociologists for ordering plant species lists or releves in tabular arrays can be programmed for digital computers, at least to a point at which botanical or ecological criteria become too complex for economical programming. Computer techniques, such as are demonstrated by our results with the IBM 7090 computer, save considerable amounts of time and effort in the purely mechanical aspects of tabular ordination and permit more extensive empirical testing of working hypotheses.

The basic algorithm employed in our programme is a precise test for similarity between combinations of two species, performed by the expedient method of choosing a row in the original table to become the first row in the rearranged table, then comparing that row with all other rows in the original table, the most similar being chosen as the second row in the rearranged table. The third row is selected by comparing the second row of the rearranged table with all the remaining rows of the input table, and so on. To complete the reordering of rows $1/2m^2-1/2m-1$ tests are made by taking all possible combinations of m species two at a time. The row-wise rearranged data are then subjected to a mechanical matching process in which each column is scanned and the position of the centroid of the longest vertical segment of consecutive entries is recorded. The column with the second lowest level segment becomes the second column, etc.

The output of this process is influenced to some extent by the first row(species) and first column(releve) fed into the programme; therefore a species suspected of having value as a differential species or a releve suspected of being characteristic of a group can be tested in relation to the remainder of the array. As a second, refining programme the output of the first programme can be subjected to screening by a series of 'templates' of selected species with designated presence value limits, so that differential tables based on various criteria can be quickly and economically produced.

March 1, 1965

Dr. Ralph J. Edwards
Department of Agronomy
Agricultural Experiment Station
University of Illinois
Urbana, Illinois

Dear Dr. Edwards:

I am sorry to say that our reprints of the SCIENCE article have long since been exhausted; we are, however, enclosing the requested three reprints of the BIOSCIENCE article.

I am quite interested in your development of a program for a classification of maize into race types. As you may have gathered, it was my interest in the cultivars of Manihot esculenta which lead me into working on this whole problem.

Since the publication of the BIOSCIENCE article we have developed a new clustering methods, one which I think is much more to the point and more useful. A description of this method is now in the hands of the editor of the Canadian Journal of Botany and should appear fairly shortly. I am testing the new program against my 200-odd Manihot esculenta cultivars and will know shortly the success of this program for the fine differences between races of cultivated species.

We have tested the program against a complicated section of the genus Cassia and have been pleased with the results, but we have not yet received the print-out of the Manihot esculenta which should be due any day now. I would recommend very strongly that you take a good look at this procedure as soon as it appears in order to see if it would not satisfy your needs for classification of maize.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF
Enclosures-3

University of Illinois
Urbana, Illinois

COLLEGE OF AGRICULTURE
AGRICULTURAL EXPERIMENT STATION—EXTENSION
SERVICE IN AGRICULTURE AND HOME ECONOMICS

Department of Agronomy

February 24, 1965

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

Dear Dr. Rogers:

It would be greatly appreciated if you could send me three reprints of each of your two papers on computer programming in plant classification. "A Computer Program for Classifying Plants" from Science and part II from Bioscience, "A Numerical Handling of Non-numerical Data". Since I am attempting to develop such an approach for use in the intraspecific classification of maize into race types, I would also appreciate any other information that you could furnish such as flaw charts or other such material.

Sincerely,

Ralph J. Edwards

Ralph J. Edwards
Research Fellow

RJE:mf

February 18, 1965

Dr. John R. Reeder
Department of Biology
Osborn Memorial Laboratories
Yale University
New Haven, Connecticut

Dear Dr. Reeder:

We will be glad to come to Yale on the 11th of March for the graduate seminar. I suggest as a title "Some Dynamics of Systematic Biology." I would rather use these terms than "computers" in the title because it inevitably turns out that our considerations are not with the hardware problems but with some of the basic tenets in taxonomy itself.

I will bring with me Dr. Michael Wirth, Research Associate in the Department of Quantitative Taxonomy, as well as Mr. George Estabrook, also a Research Associate in this department. If I may, I might ask to come along, but not necessarily to participate, two other members of the staff, Mr. Henry Fleming and Dr. Robert Jancey. Wirth, Jancey and Rogers are plant taxonomists, Fleming is an entomological taxonomist, and Estabrook is a mathematician interested in applying his mathematics to systematics.

This may sound absurd to bring so many, and I would like to have your opinion on it. Frankly, we are concerned about the possibilities of getting more people interested in this field and thinking about some of the problems that we will present. It would also have an advantage for us to have you know some of our people. I hope this is satisfactory to you.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDP

Yale University *New Haven, Connecticut*

Department of Biology

Osborn Memorial Laboratories

February 15, 1965

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

Dear Dave:

My colleagues and I, who conduct the Graduate Seminar in "Evolution & Systematics" are delighted that you will be with us on March 11 to talk about the use of computers in biological classification. I should tell you that in our seminar we are not concerned just with plants, and the faculty consists of one botanist, one zoologist, and one paleontologist.

The customary procedure is to meet for a "bag lunch" at about 11:45 in the Faculty Lounge, and proceed to the amphitheatre for the formal lecture at 12:15. Since some of the people have classes coming up at 1:45, we try to have the lecture about one hour so that there will be time for questions and discussion.

When I talked with you on the phone, I should have asked for your title. Will you please send it soon so that we shall have ample time to get the notice into the faculty bulletin?

If you think it would be helpful to bring some members of your team with you, we should be very pleased. In that event, perhaps you would like to give us their name or names so that they could be included on the notices.

I shall look forward to receiving your title, and to having you with us on March 11th.

Sincerely,

John

John R. Reeder

Some Dynamics of systematic biology -

February 16, 1965

Prof. Richard T. Ward
Department of Botany
Colorado State University
Fort Collins, Colorado 80521

Dear Professor Ward:

Thanks for your letter requesting information on candidates for a job in quantitative Taxonomy. I am sorry to say that we have no one available, and I am not certain whether any place in the country is preparing students in thisdirection. It is a much needed addition to the curriculum, and I would love to see such a curriculum instituted.

The best thing I can suggest is that you contact Billie Turner in the Department of Botany at the University of Texas. Some two years ago at Amherst he outlined a program for training taxonomists not only for chemical taxonomy, but he also mentioned that they were giving part of their curriculum over to certain interested students who wished to combine mathematical studies with taxonomy. Whether this has actually gotten off the ground or whether Turner has any students is something I don't know.

If you are successful in locating someone, I would be pleased to know how you made out.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

COLORADO STATE UNIVERSITY

FORT COLLINS, COLORADO 80521

DEPARTMENT OF BOTANY AND PLANT PATHOLOGY

Dr. David J. Rogers
New York Botanical Gardens
New York, New York 10458

Dear Dr. Rogers:

We are to add two staff members this fall, one of them in the area of plant taxonomy. Can you recommend names of recent PhD's who are familiar with the numerical approach to taxonomy and who might be interested to apply?

Yours truly,

Richard T. Ward
Associate Professor

RTW:jd

February 11, 1965

Mr. John H. Wilson
Head, Systems Identification and Analysis Section
National Referral Center for Science and Technology
Library of Congress
Washington, D. C. 20540

Dear Dr. Wilson:

Thank you for sending the data sheets for the National Referral Center. As you will see, we have only partially filled out the data sheet inasmuch as our activities are not describable in terms of pages 2, 3, and part of 4.

Actually, we are a very small group; as can be seen, there are five of us. Our capabilities are largely in terms of advice to biologists, using this term in a relatively broad sense to include agricultural people and medical people as well but also in a rather restricted sense in that we tend to stick to those interested in the processes of systematic biology. Systematic biology would here be used to include medical diagnosis, a process of quite similar nature to classification and taxonomy.

Our specific abilities in this area lie in our knowledge of mathematical models and various electronic data processing devices in the areas of systematic biology. We restrict our endeavors to people who are genuinely concerned with the problems of quantitative methodology and taxonomy, but it is quite surprising how many people are interested in this field. We can advise scientists about the applicability of one method or another and the value of certain kinds of hardware for the jobs desired.

While we are working under two grants, neither of these grants provides funds at the moment for such information service, and we only do the information service as a general service to the scientific community inasmuch as this can become rather time consuming. We try to be rather selective of the types of people whom we are actually giving advice to although we may consider many problems.

I trust that this information is useful in categorizing our endeavors and our abilities. We have four systematic biologists and one mathematician. The four systematic biologists are broadly trained in systematics generally and have considerable knowledge of mathematical methodology, both statistical and non-statistical. It is our expectation that our endeavors will increase in size and our

staff will be enlarged to provide a more rounded organization and to provide a sizeable enough staff to make it possible to engage in educational methods (short courses, primarily).

I might say that the form that you have sent me seems to be very much appropriate to be filled out by The New York Botanical Garden in general inasmuch as this institution is of the sort that could very well fill out the requested data on pages 2 and 3 of your data form. I will take this up with the Associate Director for Botany to see if he wishes to complete or have completed this form for the whole institution.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF
Enclosure

NATIONAL REFERRAL CENTER FOR SCIENCE AND TECHNOLOGY
of the
LIBRARY OF CONGRESS
Washington, D.C. 20540

A Division of the
Reference Department

February 5, 1965
Code 10674

Mr. David J. Rogers
Curator of Quantitative Taxonomy
The New York Botanical Garden
Bronx Park
New York 58, New York

Dear Mr. Rogers:

Thank you for your offer to register with the National Referral Center as an information resource, as volunteered in your letter of January 26, 1965, addressed to Mr. Trew.

Enclosed are the materials we are using to gather information--a covering letter, brochure, and duplicate copies of our data form, one of which you may retain for your files. If you prefer to describe your activities in your own words rather than use the data form, please do so.

If you have any questions, do not hesitate to call on us.

Sincerely,

John H. Wilson
Head, Systems Identification
and Analysis Section

Enclosures

February 4, 1965

Miss Amy Jean Gilmartin
Botany Department
University of Hawaii
Honolulu, Hawaii

Dear Miss Gilmartin:

I am glad to hear of your interest in taximetric methods for the bromeliads of Ecuador. We have completely exhausted our supply of the Science reprints, but I'm sure that you can get a copy made there which will serve as well. At any rate, you would not gain much from that paper inasmuch as we have covered all the salient features, with amplification, in the Bioscience paper. Indeed, the instructions given for coding data in the Bioscience paper ~~was~~ were not included at all in the Science article. There were a number of corrections made in the original model of 1960 in the later paper, all of which grew out of our experience and trials with the old program.

You may have discovered independently that no computer methods so far written, by me or anyone, give an accurate reflection of the "intuitive" process at work in taxonomy today. The reasons for this discouraging state are that the intuitive process is so poorly understood (even by the practitioners) that we have not yet ferreted out the rules by which the game is played, and that the rules that we have established have not yet been completely programmed for the computer.

The paragraph above may sound very discouraging, but I hope you will not take it to be so. First, I think that even if you do not succeed in getting a computer program running, you will have gained a tremendous insight into the taxonomy of your group if you will follow the rigorous procedures of data gathering required for the computer method. You will gain much insight into the problems by attempting to make a logical coding system for the characteristics and attributes that will be needed to define the organisms using quantitative methods. If all taxonomists would follow even this much of the process when gathering their taxonomic information, then we would have a more objective taxonomy, without any program.

I hope you will continue to be interested in the methods to be programmed for computers, but I also hope that you will have a realistic attitude about the present state of the art in what we call quantitative taxonomy, or taximetric methods. You should not expect that the present computer programs will give the answers you seek, and for this reason do not base your whole research program on these methods.

We will soon publish our third go at reflecting the taxonomic process. This attempt breaks drastically with the present model, and hopefully, is an even better measure for taxa. We will keep your name on our mailing list, and when this paper appears, you will receive a reprint.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF
Enclosures-3

31 January 1965

Dr. David J. Rogers
New York Botanical Garden
New York, N. Y.

DearDr. Rogers,

Would you be so kind as to send me a reprint of your article co-authored with Tanimoto, A computer program for classifying plants, Science 132 (3434): 1115-1118?

I have read with great interest your article with Fleming in BioScience. I anticipate using the methods in my PhD program on the systematics of the bromeliaceae of Ecuador with the view of comparing the results obtained from processes of numerical taxonomy with those obtained from classical methods. I think that perhaps the importance of using many of the approaches of numerical taxonomy are especially valuable when dealing with the plants of the tropics that have been neglected for so long. There are so many undescribed species and so few qualified botanists tackling the flora that the usual benefits of having other botanists check the classifications is not available. One person becomes the authority and is checked only by his own development. Perhaps with the methods of numerical taxonomy and the greater degree of objectivity the flora of the tropics can be resolved much more quickly and accurately.

Thanking you in advance, I remain

Sincerely,
Amy Jean Gilmartin

Amy Jean Gilmartin
Botany Department
University of Hawaii
Honolulu, Hawaii

Put on mailing list for computer reprints.

February 3, 1965

Dr. James E. Dunn
Department of Mathematics and Astronomy
University of Arkansas
Fayetteville, Arkansas

Dear Dr. Dunn:

Thank you for your letter of January 28. I enclose herewith a Xeroxed copy of the Science paper which you have requested. I also enclose a later model and description printed in the last Auguat issue of Bioscience.

We have, since the publication of the last-named article, prepared an entirely different model for clustering based on graph theory. The description of this model is now in press, and we hope it will be available within the next two or three months. I might say that we have found that, although the models previously employed were quite useful, they had several difficulties which we hope are now clarified. We shall keep your name on the mailing list and when this paper appears will forward one to you.

We will be interested to hear of any results that you have had in the classification of Indian relics. Inasmuch as you have 4000 objects, it would seem to me that our latest program would be of value to you, because it will be much better able to handle this much data than were the previous programs where the poor memory of a CDC 1604, 32 K machine would not permit any more than the handling of 200 objects.

I hope that these comments are useful to you.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF
Enclosure

UNIVERSITY OF ARKANSAS
COLLEGE OF ARTS AND SCIENCES
FAYETTEVILLE

DEPARTMENT OF
MATHEMATICS AND ASTRONOMY

January 28, 1965

Dr. David J. Rogers
New York Botanical Garden
New York, New York

Dear Dr. Rogers:

With respect to the article "A Computer Program for classifying Plants" (Science (1960)132:1115-1118) by T. T. Tanimoto and yourself:

1. Have you since performed other studies using the information theory approach? What is your present opinion of the method?

2. Where could we obtain a copy of the computer program which you used at that time (or now use). What are the hardware requirements?

Specifically, we are trying to develop a "natural" taxonomy for some 4000 Indian relics collected in Arkansas. Having examined the bibliography of measures of association and methods of classification recently appearing in Principles of Numerical Taxonomy by R. S. Sokal and P. H. A. Sneath, we are forced to conclude that your approach is about the only one which has a reasonably good theoretical justification. Hence, any helpful comments which you can make will be appreciated.

Sincerely,

Jim Dunn

Dr. James E. Dunn

February 3, 1965

Dr. David W. Goodall
CSIRO, Western Australian Regional Laboratory
Private Bag
P. O., Nedlands, W. Australia

Dear Dr. Goodall:

I am sorry that you were not able to get a reprint of our first paper in Science. I am sending herewith a Xeroxed copy; we have long since run out of the actual reprints.

It is interesting to note your comments concerning the development of a new similarity index. We are in the process of doing exactly the same thing, recognizing the shortcomings of our own. It sounds as though your procedure and our and at least the thought processes are similar. It will be interesting to see your results.

Incidentally, you might be interested to know that Dr. Robert Jancey is now working here in this department. He wishes to be remembered to you.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF
Enclosure

CSIRO

WESTERN AUSTRALIAN REGIONAL LABORATORY

PRIVATE BAG, P.O., NEDLANDS, W.A. TELEPHONE 86 2451. TELEGRAMS CORESEARCH PERTH

26th January, 1965.

Dr. D.J. Rogers,
The New York Botanical Garden,
Bronx Park,
NEW YORK 58,
N.Y. U.S.A.

Dear Dr. Rogers,

I was delighted to receive, a week or two ago, your paper from "Bio-Science" last year. When your first paper on the subject was published in "Science", I wrote to your co-author and mentioned my interest in the subject, expressing some doubts as to the way in which semi-quantitative attributes had been treated, and asking for a copy of the paper. I never received a reply - perhaps my letter did not reach him - and would still greatly appreciate a copy of the earlier paper, if one is still available.

I am myself working in the field of numerical taxonomy, and in fact have recently been applying this to the classification of plants - notably to a study with H.T. Clifford of Brisbane of the inter-relations between the tribes of Gramineae. This is now nearing completion, and I hope we shall be able to write it up during the coming months. In this work, I have developed a new similarity index based on probability theory which treats qualitative, ordered and quantitative attributes in different ways. So far this seems a very promising approach, and I am now developing a method of cluster analysis based on it.

Best wishes,

Yours sincerely,

(DAVID W. GOODALL)

COMMONWEALTH SCIENTIFIC AND INDUSTRIAL RESEARCH ORGANIZATION, AUSTRALIA

DWG:SH

January 28, 1965

Mr. Theodore W. Hurst
Worthington, Hurst and Associates
333 North Michigan AVenue
Chicago 1, Illinois

Dear Mr. Hurst:

Just a question in our mind as to whether or not you are proceeding with the preparation of punched cards for us. We are ready to do some runs and would be very pleased to have your cards to process for you.

Sincerely

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

January 26, 1965

Mr. James R. Trew, Assistant Head
Referral Services Section
National Referral Center for
Science and Technology
Library of Congress
Washington, D. C. 20540

Dear Mr. Trew: Re: Your Ref. 3160

Thank you for your letter of January 22, 1965, and for the enclosed brochure on the National Referral Center. We have read the brochure and would appreciate your sending us the necessary materials so that we may have our activities included in your inventory of information resources.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

NATIONAL REFERRAL CENTER FOR SCIENCE AND TECHNOLOGY
of the
LIBRARY OF CONGRESS
Washington, D.C. 20540

A Division of the
Reference Department

January 22, 1965
Ref: 3160

Mr. David J. Rogers
Currator of Quantitative Taxonomy
The New York Botanical Garden
Bronx Park
New York, New York 10058

Dear Mr. Rogers:

Your letter of January 4, 1965, addressed to the Documentation Center of the Library of Congress, concerning computer programs for the process of classification of plants, has been forwarded to the National Referral Center for Science and Technology for reply.

I'm sorry to report that there isn't any organizational element of the Library known as the Documentation Center and I can't think of any other Library activity specifically concerned with maintaining the files you refer to. The only organization that comes to my mind that is similar in title to the one you addressed is:

Defense Documentation Center
Cameron Station Building 5
5010 Duke Street
Alexandria, Virginia 22314

I am enclosing a brochure describing the functions of the National Referral Center and if, after reviewing it, you feel any of your activities are suitable for inclusion in our inventory of information resources, please advise me and I will send you the necessary materials.

If there is any other way we can be of assistance, don't hesitate to call on us.

Sincerely,

James R. Trew

James R. Trew
Assistant Head
Referral Services Section

Enclosure