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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.

- Taxonomy Laboratory

December 6, 1965

Dr. Charles B. Heiser, Jr.
Department of Botany
Indiana University
Bloomington, Indiana 47405

Dear Charley,

Since my letter to you, George Estabrook has gotten his schedule worked out, and now it appears that he will be showing up in Bloomington about 7:30 or 8:00 o'clock on the night of January 4th. He expects to be able to be there all day January 5th and leave in the evening. I hope this time is convenient to you and to anyone else who is to be included.

Since he will be staying over night, he would appreciate it if you could reserve a room at the hotel in the student union for him on the night of the 4th. His plane arrives in Indianapolis at six, and he will go on down to Bloomington in a rented car. I will tell him to go directly to the student union, and then contact you after he is set up there. I hope you can arrange the reservation for him.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/ec

- Taxonomy Laboratory

November 4, 1965

Dr. Charles B. Heiser, Jr.
Department of Botany
Indiana University
Bloomington, Indiana 47405

Dear Charley,

Thank you for your letter of the 25th. As it is now working out, it seems better if we send Mr. George Estabrook to see you during the week following the Christmas holidays -- that is, about January 4th or 5th.

George is able to speak our language, and he has been over all of the background information that you have given us about the Solanum complex. I will admonish him not to anticipate that you are a finished mathematician, but that you are looking for biological types of discussion rather than mathematical. He is perfectly able to do this, and I think that he will be good at explaining what is going on and some of the interesting results that you might get from this computer work.

I hope that this time is convenient for you. If not, please let me know.

Sincerely yours,

David J. Rogers
Professor of Botany

DJR/ec

INDIANA UNIVERSITY

BLOOMINGTON, INDIANA 47405

DEPARTMENT OF BOTANY
JORDAN HALL

AREA CODE 812
TEL. NO.
337-5822

October 25, 1965

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

Dear Dave,

I am sorry that I didn't get to talk to Henry Fleming at the meetings but the meetings, as far as I am concerned, are usually not a good place to carry on much of a discussion.

Our paper should be out in the next number of the American Naturalist and after that you are free to use our data in any way you want. If anything publishable comes out of it I see no need to do more than acknowledge your source of material. Since I have nothing to do with your analysis of the data I see no reason why my name should be included as an author.

We tried to make our understanding of the taxonomic relationships as clear as we possibly could and still keep the paper relatively brief. The results of the crosses are given as well as are all the chromosome numbers, and that plus my speculation is all that I know about the relationships.

Although I am not interested in appearing as a co-author, I will, of course, be glad to discuss the results with you if you think it will be helpful. Since I will not be going to the AAAS meetings, it would be necessary to take up your first suggestion. But if you send someone here be sure that he is able to discuss things in simple terms so that a taxonomist can understand him. Except for the period of Dec. 1-5, I should be here all the time until Christmas.

Sincerely,

Charby
Charles B. Heiser, Jr.
Professor of Botany

CBH/ljr

October 18, 1965

Dr. Charles Heiser
Department of Botany
University of Indiana
Bloomington, Indiana

Dear Charley,

Henry Fleming tried to grab you at the AIBS meetings, but was not successful. He had with him at the time the results of the computer run on your Solanum nigrum complex. We think the results are very interesting. We would like to go over these results with you in detail because it is obvious that, since we do not know the plant materials, we cannot judge the merit or value of the production. However, we think that we are, from our end, able to see things that perhaps you would not have been able to see if we gave you these results cold.

Charley, this may not be important to you, but it is tremendously important to us to put out a paper in which we demonstrate that the methodology has value. I fear that the result of your paper in the American Midland Naturalist, where you run your data using Sokal's method, may have a direct effect on our operations. The taxonomists do not know, I am sure, that we are not of the same "numerical taxonomy school" as is the Kansas group, and a paper that does not clearly indicate that there are several approaches to the use of computers for taxonomy may make it difficult, in the eyes of the granting agencies, to continue support.

For these reasons, I am willing to send one of my staff to Bloomington at your convenience to present the results, our interpretations thereof and a full explanation of the methodology. From my examination of the print-out, I think that you will be pleased to see the results that we have. I am sure that your schedule is very full, but I hope that you can arrange to have some time to go over these

Letter to Dr. Charles Heiser
October 18, 1965

Page 2.

results and publish them. We can suggest formats for publication which will simplify this matter for you. As far as interpretation is concerned, of course, this is your baliwick, not ours. Two ways may be possible to handle the business of bringing the data to you. I could ask my Assistant, George Estabrook, to bring them along with him just before Christmas. The other alternative is to invite you to stop here either going to or coming from the AAAS meetings in Berkeley, in the event that you are going to those Christmas meetings. I do not think there can be another good way to get together, but you may have some alternative suggestions which I will appreciate. But whatever way, I hope we can make it fairly shortly.

Sincerely,

David J. Rogers
Professor of Botany

DJR/ec

INDIANA UNIVERSITY

BLOOMINGTON, INDIANA 47405

DEPARTMENT OF BOTANY
JORDAN HALL

AREA CODE 812
TEL. NO.

June 11, 1965

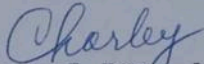
Dr. David J. Rogers
New York Botanical Garden
Bronx Park
New York 58, New York

Dear Dave,

I'm embarrassed to admit that I can't find the carbons of the final draft of our paper. I think that Don Burton may have them and since he won't be back until summer school starts I am sending you an earlier version of the paper. Some of the wording is changed in the final version and a sentence here and there has been omitted but the relationships and conclusions have stayed the same. I hope you can make some sense out of it. I found it a most difficult thing to write up. However, the reviewers for the American Naturalist were most kind (I thought that you might have been one of them.) It shall be most interesting to see what results you come up with.

We're looking forward to our visit to Colombia. The girls will stay here but Bix and Dorothy will be with me for two weeks. As I told you I am going primarily for *Solanum quitense* and *S. muricatum* and wild relatives (but there are also some *Helianthus* there and peppers of course). I have, I think, some rather exciting stuff from my hybridization studies of the *Solanums* which I hope to tell you about one of these days.

Sincerely,



Charles B. Heiser, Jr.
Professor of Botany

CBH/ljr

INDIANA UNIVERSITY
BLOOMINGTON, INDIANA

DEPARTMENT OF BOTANY
JORDAN HALL

AREA CODE 312
TEL. NO.

337-5822

February 8, 1965

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

Dear Dave,

I'm sorry that I don't have much to report. We compared your similarity values with the correlations values we had and I would say that in general the pattern of relationship that emerges is fairly similar with a few exceptions. I would say that your clusters told us pretty much the same thing we already knew from the dendrograms but at the same time gave some information not obtainable from the dendrograms. But I must admit that I haven't been able to devote a whole lot of time to your results. For one thing we have been short staffed this semester and I have had an extra heavy load of advising. What little research time I've had I've devoted to getting my Edinburgh talk and some Helianthus material ready for publication. My student, Don Burton, who is interested in statistical work, had a heavy semester of classes, and moreover the computing center has not been readily available since they have been installing a larger computer. So all in all, there is very little to report from here.

Sincerely,

Charley
Charles B. Heiser, Jr.

CBH/ljr

January 29, 1965

Dr. Charles B. Heiser, Jr.
Department of Botany
Indiana University
Bloomington, Indiana

Dear Charley:

We are sort of curious as to whether or not the similarity ratios and the graph theory methods for clustering of your material had anything meaningful to you.

If you get any ideas on the subject, we would be glad to have them. We would like to know whether we are on track with your data. If we are not, we would be glad to know that too.

Sincerely

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

INDIANA UNIVERSITY
BLOOMINGTON, INDIANA

DEPARTMENT OF BOTANY
JORDAN HALL

AREA CODE 612
TEL. NO.

November 4, 1964

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

Dear Dave:

Many thanks for the similarity coefficients and the results of our Solanum data. I want to study it more thoroughly before making any comment.

We are now writing up the results of our other study. I'm afraid it's going to be a long paper. At the moment I have no idea where I shall send it.

If I can work things out satisfactorily, I hope to spend some time in Colombia working with cultivated plants this summer.

Sincerely,

Charley
Charles B. Heiser Jr.

CEH/llt

October 29, 1964

Dr. Charles B. Heiser, Jr.
Dept. of Botany
Indiana University
Bloomington, Indiana

Dear Charley:

Here, finally, are some results that I hope will have merit and interest to you. These results have been determined by a completely new method for clustering which we are working with. It is the development of George Estabrook, Mike Wirth and myself in which we take a completely different idea for our mathematical model derived from graph theory. This is not a statistical model.

It is not fully programmed for the computer but will be shortly. The explanations given for your benefit are still unpublished. We would therefore appreciate it if you would consider this as a ~~privy~~ communication not to be yet exposed to the public eye. It might be looked at in terms of an unfinished research project.

From our standpoint this looks like the most promising method yet for taxonomic procedures. The method of illustration given here may or may not be the most appropriate method of illustration of the connections between objects. One might tack on to it something of a dendrogram, but we do not feel that a dendrogram can provide any more information than this technique does.

This will probably take some doing on your part to get it under your belt, but this can be done after you have had an examination of the enclosed diagrams. There is an explanation included for your benefit which may or may not fit your needs.

When I said just a bit above that this is a ~~privy~~ communication I did not mean by that that it is not something that you and your graduate students and colleagues should not examine, question and criticize. We will be happy for such criticism. We would also like to know whether the objects in this test are anywhere near realistic.

clustering of

Enclosed also is a copy of the similarity co-efficients for the objects. I think that the enclosed explanation indicates that some of the points have not been clustered because the hand manipulation which was done here would require an extreme amount of time but that when the program is written for the computer all points will be eventually tied together.

Dr. Charles B. Heiser, Jr.

-2-

October 29, 1964

I hope by now that your broken arm is well healed or at least on the road to healing, and I hope also that you will forgive us for having delayed so long in the preparation of this data. We look forward to your comments.

Sincerely

DJR:MDF
Enclosures

INDIANA UNIVERSITY
BLOOMINGTON, INDIANA 47403

September 22, 1964

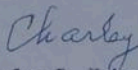
Dr. David Rogers
The New York Botanical Garden
Bronx Park
New York 58, New York

Dear Dave:

I have been meaning to write to you for some time to tell you that the similarity coefficients on our Solanum material has not arrived. As I recall, you told me at the meetings that this had been sent to us about a month ago.

Please excuse my delay in getting around to writing to you, but I broke my arm a couple of weeks ago and I'm a little behind with everything.

Sincerely,



Charles B. Heiser, Jr.

CBH/jek

June 24, 1964

Dr. Jorge Soria
Instituto Interamericano de Ciencias Agrícolas
de la OEA
Turrialba, Costa Rica

Dear Jorge:

Thank you very much for your letter of June 15. You should shortly be hearing from Mr. Sohmer who will give you specific information about his arrival and his address in San Jose.

In relation to the computer work, we have hopes that Charley's data will be run this week, but I am not sure whether we can do so or not. One of the problems was that we changed computer centers and have made a few alterations in the method of getting the data into the computer, and these have taken so much time that we have not done anything else except to improve the program statements. There is no modification in our model as a result of these changes. I am hoping that we will be able to have something specifically useful to you and Charley fairly shortly.

In response to your question concerning the article by you and your brother on the edible Caricas of Ecuador. I think that this would probably make an extremely fine article. However, I am not now the editor of ECONOMIC BOTANY, and all editorial decisions about the acceptance of manuscripts are made by Dr. Richard Evans Schultes at the Botanical Museum, Harvard University, Cambridge, Massachusetts. As soon as you have finished the article, I would suggest that you forward it directly to him. I think that he will probably find it acceptable. As far as I am concerned, I think that this type of information is very useful and would very likely be something that we would like to have in ECONOMIC BOTANY.

Thank you very much for sending all the information about the Hevea-Manihot cross. I am sure that this will be useful to the person who requested the information.

Sincerely

DJR:MDF



INSTITUTO INTERAMERICANO DE CIENCIAS AGRICOLAS DE LA OEA

El Instituto es un organismo especializado de la Organización de los Estados Americanos. Fue establecido por los Gobiernos de las Repúblicas Americanas en 1944 para promover su desarrollo económico y social a través de la educación y la investigación.

Centro Tropical de Investigación y Enseñanza para Graduados

TURRIALBA, COSTA RICA

Cable: AGROEA - Turrialba

June 15, 1964

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

Dear Dave:

This is in reply to three of your letters (April 10, May 25 and June 9, 1964). I am very sorry for my delay in answering, but this delay was due to my accumulated work here after four months of absence. I apologize.

The name and address of Mr. V. C. R. Henry was given to Dr. C. C. Moh, head of the basic sciences group here and who is in charge of the Manihot project. He says that he will try to see you on his next trip to the U. S. A. in the next fall.

The information about the hybrid between Hevea and Manihot was obtained from a clone inventory prepared by Mr. C. N. Hittle of U. S. D. A. (United States Department of Agriculture) in 1951. The information available here comes from a mimeographed booklet, "list of eastern clones in the western hemisphere with countries where the clones are now located." This list gives besides the parentage of each clone, the location where they are kept. Our case is clone Gx879 (Av-33x Manihot ult.) H.M. Gx means Goodyear cross pollination progenies and these were made in Sumatra; in parentheses are the parents of the cross pollination. It seems that there is an inversion of letters in the abbreviation after Manihot;

El Instituto tiene su Dirección General en Costa Rica: Apartado 4359, San José. Sus tres Oficinas Regionales abarcan los siguientes países: Zona Andina, Apartado 478, Lima, Perú (Bolivia, Colombia, Ecuador, Perú y Venezuela); Zona Norte, c/o Instituto Interamericano de Ciencias Agrícolas de la OEA, Apartado 4359, San José, Costa Rica (México, Istmo Centroamericano y Antillas Mayores); Zona Sur, Casilla de Correos 1217, Montevideo, Uruguay (Argentina, Brasil, Chile, Paraguay y Uruguay). Mantiene dos centros: Centro Tropical de Investigación y Enseñanza para Graduados, Turrialba, Costa Rica; y Centro de Investigación y Enseñanza para la Zona Templada en el Instituto Fitotécnico del Uruguay, La Estanzuela, c/o Instituto Interamericano de Ciencias Agrícolas de la OEA, Zona Sur, Casilla de Correos 1217, Montevideo. Administra los Proyectos 39 (Enseñanza Técnica Profesional) y 201 (Crédito Agrícola) del Programa de Cooperación Técnica de la OEA, patrocinados por el Consejo Interamericano Económico y Social (CIES). Mantiene también núcleos de investigación y enseñanza para graduados en instituciones de los países miembros.

it should be utl, and the clone Av stands for= Algemeene Vereeniging van Rubber planters tev Oostkuest van Sumatra. Finally, H. stands for Haiti and M. for Mexico, countries where this clone has been established. We do not have this clone here, but I would suggest that you write to Goodyear in the States asking for information about it.

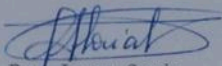
In regard to the visit of Mr. Seymour Sohmer in July to Costa Rica, I will be most happy to help him with the information and cooperation needed to make his trip most profitable. Piquin Córdoba is making a list of the places where he can find Manihot cartaginensis for me so when he arrives, I will help him by locating this material which is of our mutual interest, as the Institute is also interested in Manihot. I have talked with Dr. Moh and he is happy to let Mr. Sohmer use the material of the Institute's collection for his cyto-taxonomic work.

If Mr. Sohmer lets me know where he is going to stay in San José, I will be glad to look for him during the weekend of July 3 that he is arriving. If not, he can make contact with me through the Central offices of the Institute in San José, Edificio Mendola 3er. piso.

Did you already finish the analysis of our data with your system? I have not heard from Dr. Heiser in the past weeks, but I was under the impression that after correcting a clerical error in your program, the computation was ready. I am anxious to compare the results of yours and Sokal's methods with groups of plants with fairly well known genomes. Our computing programs worked nicely and gave us a good information on the use of standardized and unstandardized data. They were going to try Soria's standardized next, because I feel that the main problem resides in not yet having the best method of estimating the information (coding) which will reflect the actual value of a character without overweighing it or others.

How is your editing work of Economic Botany? I will have a short note on some of the highland edible Caricas of Ecuador. A brother of mine and myself are doing a little study on the relationships and possible evolution of the group. Do you think that you could accept this kind of material for Economic Botany?

Best regards,


Dr. Jorge Soria
Plant Breeder

cc: Havord
Moh
Madrid

JS/lis.

May 25, 1964

Dr. Jorge Soria
IIAA
Turrialba, Costa Rica

Dear Jorge:

In our last conversation you mentioned that you had heard of some breeding work where there had been a hybridization between Manihot and Hevea. If you can possibly recall the source of this information, would you please let me know?

I am sorry that we never did get through a run on Charley's data before you left. I don't know whether you saw the S_{ij} matrix that we did send to him. We found out that something was wrong with the program. Some of the cards that were part of the input data somehow got mixed up, and the output generally did not reflect accurately the kind of work the program should do. In the meantime, we have changed our programmer in hopes that we will have a better chance of getting accurate information in the future.

Best luck to you,

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDP

INDIANA UNIVERSITY
BLOOMINGTON, INDIANA

DEPARTMENT OF BOTANY
JORDAN HALL

AREA CODE 812
TEL. NO. 337-5822

April 20, 1964

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

Dear Dave,

Many thanks for the phone call. I am sorry that Jorge will be unable to come to New York on his way back but they are desperately in need of him at Turrialba so he feels that he must return as soon as possible. Even so he is planning to spend a few days at Davis before returning.

However, Jorge wouldn't have been of too much help to you, for he has purposely kept himself in the dark on the material that we are using so that our analysis would not be prejudiced in any way. He has worked solely with the numbers of our objects. Naturally, we hope to have a dendrogram in a few days and then we will discuss the results.

Don, Jorge, and I were wondering if it would be possible for you to come up with any results without knowing what the taxa are. As far as we are concerned it would be most interesting to see what sort of clusters would emerge if you simply adopt some arbitrary values and use them consistently in your clustering. I thought I would inquire into this possibility before sending you the list of species and hybrids which we have used. Of course, I shall be glad to send them, if you feel that you need them in order to proceed with the analysis.

I would like to come out to discuss the analysis with you but my busiest time of the year is coming up. In addition to the class I am now teaching, I take over the Survey Course for the last four weeks. On top of that I have student advising and all of my plants have to go into the field. So, I can hardly get away.

We would like very much to see your matrix of similarity values if you can send it to us. In some ways I feel that to a taxonomist this matrix is more meaningful than the dendrograms or other clusters. Of course, it can hardly be used to present results to the public.

We are very interested in what you will come up with on data but I hope you feel under no obligation to let it interfere with your own research.

Sincerely,

Charles
Charles B. Heiser, Jr.
Professor

CBH:ss

Dear Dave,

Your letter came in just as I was sealing this.

We have spent an hour or so going over the Sig matrices. Our cor. coef. give very similar clusters at the higher values (.5000 and over) although our values run higher. However, below .4000 there is considerable serious disagreement between the two matrices.

I'm afraid that we may have made a mistake in penning our entry on one of the cards. We got a cor. of 1.000 for objects 21 & 22 which means that the readings for the two are exactly the same. We would also expect 1.000 with your method but judging by the coef. value our item must not agree. The correct entries are given below

Charles,

Objects 21 & 22

Card 1

1, 7, 10, 12, 16, 18, 21
24, 27, 30, 33, 36, 48, 53
54, 63, 66, 68.

Card 2

1, 6, 11, 14, 18, 20, 22,
25, 28, 31, 33, 36, 38, 40, 45,
48, 52, 58, 63, 66, 69

Card 3

2, 4, 8, 14, 17, 20, ~~22~~, 24,
~~28~~, 31, 33, 35, 37, 39, 41, 43

April 17

Dear Charley:

Here's the S_{ij} matrix. Sorry it has taken this long to get it to you. I think you can read it without difficulty.

As I told you on the phone, we ran the clustering technique, but didn't get all the way through the analysis, even after 3 hours of running time. Fortunately we didn't get charged for the run because it was "dumped" for another program that had priority, and we have the print-out without cost. I think I mentioned that we used the same constants used for the Halimeda analysis, and we knew in advance that these constants would probably not be satisfactory for your data, yours being of any entirely different sort than the algal data.

I will repeat the clusters that were formed, in case you didn't get them down from our conversation. They are: objects (cases) 64-65; 21-22; 66-67; 47-21; 27-68; 23-22; 13-12; and 72-71.

In the T-ranking, the most typical case is 57, and the least is object 1. For your information, the T-ranking, list from most to least is as follows: object 57, 64, 21, 65, 67, 60, 66, 47, 26, 25, 27, 49, 68, 22, 41, 43, 61, 52, 45, 48, 20, 63, 56, 23, 13, 59, 74, 51, 44, 50, 53, 54, 14, 15, 72, 40, 12, 37, 32, 46, 38, 10, 11, 31, 73, 71, 58, 42, 36, 17, 28, 33, 18, 75, 39, 19, 69, 70, 35, 24, 62, 34, 16, 30, 15, 29, 7, 9, 8, 2, 4, 5, 3, 6, 1.

It might be interesting to see what the specimens look like when line up according to the T-ranking, in case you want to. I have no idea what it would mean, but would be interested to know.

The δ used in this run was .04. By examination, we found that if we had set the δ at .005, we would have clustered more objects.

The point, as we see it, is that much more analyses of the distribution of the objects will have to be made in order to set a satisfactory δ for the group. As a matter of fact, we intend to write a subroutine to be added to the main program that first determines what constants should be used for that particular group under study. Then there won't be so much guess-work in the clustering. More work, more time.

Incidentally, we have informal word on a successful NIH grant application to hire our own mathematician-programmer. Do you have any suggestions from there as to proper applicant? How's the math department--do they have any interested in applied mathematics? Let me know if you have any suggestions.

Sincerely,

INDIANA UNIVERSITY
BLOOMINGTON, INDIANA

DEPARTMENT OF BOTANY
JORDAN HALL

AREA CODE 412
TEL. NO.

April 14, 1964

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

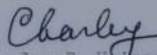
Dear Dave,

Glad you enjoyed your visit here. Jorge, Don Burton and I all found the talks with you most profitable.

Sorry that I didn't respond to your comments about Seymour Sohmer, but we're always interested in getting good students. However, we have already filled all of our assistantships for next year so we wouldn't be able to offer him any financial help.

We're pleased to hear you are running the Solanums and shall look forward to receiving the print-out.

Sincerely,



Charles B. Heiser, Jr.
Professor

CBH:ss

April 10, 1964

Dr. Charles B. Heiser, Jr.,
Department of Botany
Indiana University
Bloomington, Indiana

Dear Charley:

The opportunity to visit you and Jorge was certainly welcome. Altogether it was a profitable experience--I think more so for me than it was for the Conference, but that is a question for them to decide.

I was very much in hopes that I would be able to bring out with me a print-out of the similarity ratios for your Solanum materials, but we didn't get to it. However, we are now running the data. There are going to be two questions that we will have to ask you once we have run it. Our first clustering analysis was with the Delta set at .004. At this level the clusters are forming with only two objects per cluster. What we hope to do is set the Delta at a finer splitting point, perhaps half of this figure, and see what clusters are formed then. This should be done some time this week--today hopefully. If it is, we will send out the print-out to you as soon as we can.

I didn't get a clear cut response or reaction about a student that I have known who wants to come and work with you next year. I mentioned him previously--his name is Seymour Sohmer. He is now doing his Master's with Jack Sharp but wants to get to work where more bio-systematics is done. He is very interested in continuing his research in the bio-systematic problems involved in Manihot. We are already having cytological material collected for us by three people in South America--one, Jorge Leon, and the other two may or may not be known to you. They are in Recife and Sao Paulo. If he gets this material, he will have something to work on. We have mailed off reagent bottles and reagents with a set of instructions to each of the three people mentioned and hope that they will come through with something for us. I think Sohmer is a very intelligent young man dedicated to systematics. I believe he would make an acceptable graduate student under you. I am merely bringing this up so that when he does apply you will know that I have given him my official blessing.

Sincerely

DJR:MDF

April 10, 1964

Dr. Jorge Soria
Department of Botany
Indiana University
Bloomington, Indiana

Dear Jorge:

I just recalled that I was to let you know if there were anyone who might be able to work on the Manihot breeding program at Turrialba. There is a faint possibility that Mr. V. C. R. Henry, who is a Trinidadian, might be interested in the project.

The last address that I had for him was P. O. Box 97, Macdonald College, Province of Quebec, Canada. I do not know where he is at the moment. Our last correspondence with him was two years ago, and very likely is back in Trinidad by now. I hope that if you write to him at the address above they will forward the letter to his most recent address.

I enjoyed seeing you and discussing some of our mutual problems. I am writing to Charley now.

Sincerely

DJR:MDF

December 10, 1963

Dr. Charles B. Heiser, Jr.
Department of Botany
Indiana University
Lafayette, Indiana

Dear Charley:

The punch cards arrived in good shape yesterday. It will be helpful to us if you will send us a list of the characteristics and the numbered attributes that go with them.

Have you heard of Paul Allen's death? We received a Christmas card from his wife yesterday with a note that he had died on November 14. Concerning MBG, it seems to me that we all need to do something to try to make this organization continue. I have suggested as an interim measure to Hugh Cutler that I would be willing to act as an editorial board member to continue the Annals. This is one small stop-gap measure to keep things rolling a little bit. I suspect that some manuscript has accumulated for the Annals and that probably they need to be worked on.

I have also suggested to Hugh that it would seem to me to be appropriate that the Board of Trustees have more contact with the alumni of MBG in order to try to settle upon some policies that would satisfy both the Trustees as well as the needs of the Garden. This is such a fundamental place and its history is so tied up with taxonomy in this country that we simply must not let it die.

If you have any suggestions or comments that we might carry out together or as a group of MBG graduates. I would be pleased to help out, organize, or do whatever is necessary.

Sincerely,

DJR: MDP

INDIANA UNIVERSITY
BLOOMINGTON, INDIANA

Dec. 6, 1963

Dear Dave,

The punch cards are enclosed. We have 75
populations (OTU's) and 3 cards for each. We used
58 characters with ^{from} 2 to 6 states, needless to say, we are most
interested in seeing how these come out.

184 all 246

I was certainly sorry to learn of Bob's death. With
Went resigning, no replacement for Andy, and Henry leaving W.U.
for Conn. things would appear to be in a very sorry state there.

Our mycologist here committed suicide two months ago so
I've had to take over part of his teaching which helps explain
why it took so long to get these cards to you. However, I must say
the punching went very rapidly once we got started.

Sincerely,

Charley

March 10, 1964

Dr. Charles B. Heiser, Jr.
Department of Botany
Indiana University
Bloomington, Indiana

Dear Charley:

I have to be in Bloomington for a meeting on the weekend of the 3rd and 4th of April. I expect I will be pretty busy on Friday and Saturday; but if you have any time on Sunday morning, I would like to have a chance to just chat with you for a bit--nothing really critical but just a review of our present status on the computer business to bring you up to date on it.

If this is convenient^t, I look forward to seeing you. I have to leave Indianapolis at 1 P.M. to return on Sunday.

Sincerely

DJR:MDF

October 11, 1963

Dr. Charles B. Heiser, Jr.
Department of Botany
Indiana University
Bloomington, Indiana

Dear Charley:

Your manuscript entitled "Yellow, Red and Black" came yesterday, and I have commenced the reading thereof. After I have gone over it, Dick Klein will take a look at it, and then we will send it down to the Doubleday editors. So far as I have read (to page 72), it sounds real good. I have no comments to make yet about it except some curiosity factors which are not editorial problems but concern the plant materials themselves. On page 56 and 57 you talked about ragweed as a possible Indian food and mentioned that it never seemed to have caught on. You did not mention this, but I wonder if the reason it did not catch on as a cultivated plant could have been the same for the Indians as it might have been for us--namely, that it is a terrific hay fever plant. It would be interesting at least to think about that.

My second question is could you supply the seeds of H. maximiliani? You sold me on this species, and I want to raise some in my garden next year. If you don't have them, who has?

Now about the second part of your letter--on the ordering of punch cards. If you will tell me how many cards you need, figured by the number of specimens times the number of attributes, we can probably supply you with these cards from our own stock at a much better rate than if you ordered them directly from IBM (unless you are so sold that you are willing to buy about 10,000 of them). The point is that for small quantities the expense is high, and we will be glad to let you have some of our stock and let you pay at the rate that we did when we got them. Just let us know how many you need, and we will send them off to you. The cost should not be more than \$10 or \$15 for about 1,000 cards.

We are just putting the finishing touches on our program. It has taken much longer than we had anticipated as nothing like this has ever been done before. We are much more confident now that our model of the taxonomic method more nearly reflects the procedures

Dr. Charles B. Heiser

-2-

October 11, 1963

that a taxonomist uses in thinking about his species than does the model proposed by Sokal. Essentially the difference, as I see it, is that Sokal uses a statistical procedure which requires essentially a normal curve fitting, and this of course forces some rather odd fellows together, whereas the procedure that we follows allows more freedom and reflects the differences between taxa better than does that of Sokal.

As soon as our program has the bugs worked out of, which will be soon, we will hope to make a very thorough publication of this information and try to spell it out in such terms as biologists will be able to understand. This will then be a more useful publication than a purely mathematical one which usually frightens people off.

At any rate, we are glad that you are still willing to prepare the data for us because this will prove to be one of the best tests of our procedures. I doubt that anyone really knows the taxa better than you and your students do, and this essentially is the proof of our operation--does it satisfy your feelings about the plants?

Sincerely

DJR:MDF

P. S. Enclosed our instructions for illustrations.

INDIANA UNIVERSITY

BLOOMINGTON, INDIANA

DEPARTMENT OF BOTANY
JORDAN HALL

October 7, 1963

AREA CODE 812
TEL. NO. 337-5822

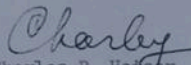
Dr. David J. Rogers
The New York Botanical Garden
Bronx 58, New York

Dear Dave,

Under separate cover I am sending you a copy of "the book." I have held it up, hoping to find some time for revising but the way things are going it doesn't look like I'll have time for it in the near future. I am not enclosing any illustrations (drawing of sunflower, photos of sunflowers and peppers, and maps). Some of these are finished and some are yet to be done. Incidentally, you promised to send me instructions about preparing the illustrations.

All of the *Solanum* data are not collected and we're ready to start punching the cards - that is, if you are still willing and interested in handling it for us. However, we will need more cards. You said I could buy these through you. I wish you would send me details as to cost, per hundred or thousand, etc. so that I can send in a formal order.

Sincerely,



Charles B. Heiser, Jr.
Professor

CBH:jh

July 5, 1963

Dr. Charles B. Heiser, Jr.
Department of Botany
Indiana University
Bloomington, Indiana

Dear Charley:

I have your letter of the 2nd. Would you please address your manuscript to Dick Klein here at the Garden? I will be in Mexico when it arrives. He will take care of it.

The work on the Solanum sounds good, and I hope we will be able to do some processing of data for you some time this summer, although at this very moment I am very pessimistic because of the slowness of our operation. Anyway, we will do the best we can to handle your data when you get it to us.

Sincerely

DJR:MDF

INDIANA UNIVERSITY

BLOOMINGTON, INDIANA

DEPARTMENT OF BOTANY
JORDAN HALL

July 2, 1963

AREA CODE 212
TEL. NO. 337-5822

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

Dear Dave,

Thanks for your letter of June 20. I, at last, have someone typing up the manuscript and I am working on the illustrations. I lengthened the book in line with your suggestions. I'll send the mms. to you in a couple of weeks or so, although I won't have all of the illustrations ready by then.

We have almost finished the measurements on the Solanum and I have been devoting all of my time to this work the last week or so. I should be ready to start punching cards sometime this summer.

Sincerely,

Charley
Charles E. Heiser, Jr.
Professor

CEH:jh

April 24, 1963

Dr. Charles B. Heiser
Department of Botany
Indiana University
Bloomington, Indiana

Dear Charley:

If you will tell us approximately how many cards you need, we will send them to you. Inasmuch as the card format is ours, IBM has to have our permission before they can be sold. What we will do is charge you at the rate per card and then send you the cards and you can pay for them whenever you feel that you have the funds available.

In answer to your question as to how to indicate which is card 1 and card 2, you are correct in assuming that the column marked as "add card code" is the point of indication of which is card 1, card 2, etc. Merely punch the 1 for card 1, then the specimen number. If you require more than one card, punch row 2 in the "add card code" column, but be sure to keep the same specimen number on the second card to indicate the relation. The same sequence is followed if more than two cards are needed--column 3 punched out, etc.

Sincerely

DJR:MDF

INDIANA UNIVERSITY

BLOOMINGTON, INDIANA

DEPARTMENT OF BOTANY
JORDAN HALL

April 16, 1963

AREA CODE 812
TEL. NO. 337-5007

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

Dear Dave,

Sy - for your info.

I am always glad to hear of potential graduate students, but I'm afraid it's too late for me to do anything about it for this year. We have already made offers for all of our assistantships for next year. I'm sorry that I didn't hear about Mr. Sohmer sooner.

Thanks for the note about the punch cards. We're still collecting data but hope to get around to punching the cards early this summer. Since I'm sure that we'll need at least two cards for each population I'd appreciate it if you would tell me where I can order more. Also I'd like to know how I indicate which is card 1 and card 2 for each population. Is this done in the "add card code" column?

Sincerely,

Charley
Charles B. Heiser, Jr.
Professor

CBH:jh

P.S. I still don't have the book typed but hope to get to it next month. I have all my help measuring Solanums.

February 25, 1963

Dr. Charles Heiser
Department of Botany
Indiana University
Bloomington, Indiana

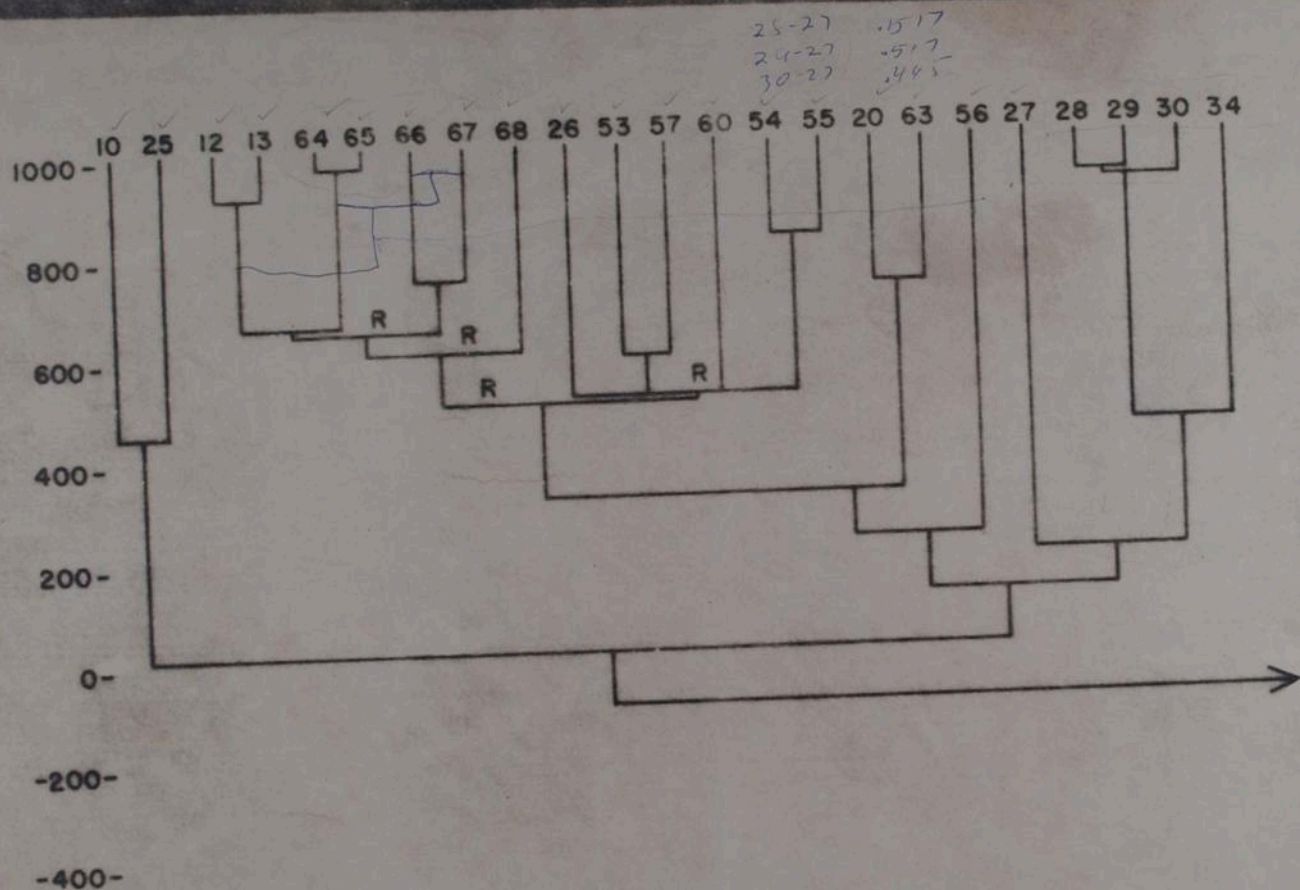
Dear Charley:

I enjoyed my visit with you tremendously. I hope that we can soon get going with some data for you. I am fairly sure that the cards that I left with you will not be enough; and when you are ready for more cards, please let us know.

As usual, it was a pleasure to see your family, and I enjoyed myself tremendously.

Best regards to all,

DJR:MDF



Heiser -- Solanum

184 attributes
75 objects

<u>Character- istic</u>	<u>Last punch attribute</u>	<u>Character- istic</u>	<u>Last punch attribute</u>
1	102	30	230
2	108	31	232
3	111	32	235
4	115	33	237
5	117	34	239
6	120	35	244
7	123	36	247
8	126	37	249
9	129	38	255
10	132	39	260
11	135	40	265
12	139	41	268
13	143	42	270
14	147	43	302
15	150	44	306
16	153	45	309
17	156	46	314
18	161	47	318
19	165	48	320
20	167	49	323
21	169	50	327
22	203	51	330
23	209	52	332
24	213	53	334
25	216	54	336
26	219	55	338
27	221	56	340
28	224	57	342
29	226	58	344

Nov. 1965
first draft

A numerical taxonomic study of
Solanum species and hybrids ¹

Charles B. Heiser, Jr., Jorge Soria V., and
Donald L. Burton

*Sokol
manuscript
this one*

Numerical taxonomy is the determination of the similarity of taxa by numerical or statistical methods and the erection of a classification based upon the similarities obtained. The numerical taxonomic method has been developed largely by Sokol and Sneath, whose recent book (1963) on the subject brings together the literature as well as a treatment of methods. Although much has now been written about numerical taxonomy, both pro and con (for example, see Ross, 1964), relatively few studies have been made employing numerical taxonomy, and so far as is known, none has utilized material of known relationship. Therefore, it seemed appropriate to carry out such an investigation utilizing artificial hybrids, antepolyploids and allopolyploids as well as species.

Materials and methods

Several members of the section *Morella* of *Solanum* were chosen for the study since seed of a large number of species and hybrids was available. Although this group is generally recognized as a difficult one taxonomically, there is

¹ This work was carried out with the aid of a grant from the National Science Foundation. Such a study as this is not possible without the help of a number of people. We would like to thank the students who helped with the measurements with particular thanks to Mr. Alvin Reeves III. Some of the hybrid data given in Table II is from the crosses made by Mr. and Mrs. Hortono Wirjodermodjo. We should also like to acknowledge the program furnished by Dr. James Rohlf which we modified for our use.

now fair agreement on the taxonomic treatment of many of the taxa (Stebbins and Paddock, 1949; Wessely, 1960). The plants of this group offer several advantages; (1) the plants are small enough so that a large number can be accommodated in the greenhouse and they have a fairly short life cycle; (2) the plants are largely self pollinated, thus providing population samples that are fairly uniform (as well as) assuming an abundant fruit set in the absence of pollinators; and (3) both artificial hybrids and polyploids are fairly readily secured. A total of 75 operational taxonomic units (OTU's) were employed. Seventeen different species, six of them with more than one "race", representing diploids, tetraploids, and hexaploids were included, and four antopolyploids, six allopolyploids, nineteen artificial homoploid hybrids, and one volunteer plant of unknown origin made up the remainder of the OTU's, (Table I.) The polyploids were obtained from colchicine treatment of seedlings or from spontaneously doubled plants that had appeared in our cultures in previous years. Since under natural conditions members of this group of Solanum are readily modified by environmental condition, all plants were grown in the greenhouse at the same time in order to minimize ecological or non-genetic variation. Because of the limitation of greenhouse space only 10 plants each OTU were grown, although in some cultures because of poor germination less than 10 plants were available.

Seventy "characters" were used for each plant. Some of these could not be scored or measured accurately or consistently and others proved to be redundant, thus a total of 58 were used for correlations, 30 of which were scored and 28 were measured. Two to several states were recognized for each character. The characters used and the number of states recognized are as follows:

corolla lobe color (2); corolla star color (6); corolla lobe shape (3); corolla length (4); corolla sinus index (2); corolla diameter (4); corolla texture (3); style length (3) style pubescence; (3); anther length (3); degree style exceeds

anther (3); anther color (3); filament pubescence (4); calyx length (4); calyx sinus index (4); calyx expanding or not (3); calyx reflexed or not (2); presence of light area between calyx lobes (2); symmetry of calyx (3); calyx with purple spot (2); pollen diameter (5); inflorescence type (3); *inflorescence* simple or branched (2); number of flowers (4); peduncle length (4); angle of peduncle (2); length of pedicel (6); pedicel pubescence (3); angle of pedicel (2); pedicels hooked (2); stem diameter (3); stem ridged or not (2); plant height (4); "rosette" present (2); plant habit (3); presence of basal branching (2); days to flower (4); fruit color (5); fruit shape (3); fruit luster (2); fruit diameter (6); fruit hard or soft (2); number of sclerotic granules (5); number of seeds (5); diameter of seeds (3); color of seeds (2); leaf shape (2); blade length (4); leaf index (3); length of teeth (5); number of teeth (4); blade-petiole ratio (2); basal leaf angle (3); number of hairs on leaf (3); length of hairs (4); presence or absence of long glandular hairs (2); short glandular hairs (2); simple hairs (2);

A full discussion of these characters, their choice, how they were divided into states, justification for considering them as separate characters, and their significance in previous taxonomic work, could well occupy several pages. Only a few comments will be made here. The use of states for characters which were measured may not be justified since the actual measurements could well be transposed for uses on the computer as we did in an earlier study (Soria and Heiser, 1961). However, the use of other types of coefficient of similarity, such as that used by Rogers (1964), requires states.

It is obvious that hybrid vigor or hybrid weakness influences the expression of some of our characters. Thus it was found that some hybrids had average heights greater than that of either parent; and one hybrid combination produced plants shorter than either of its parents. Partial sterility in some of the hybrids also sometimes resulted in a given hybrid producing fewer seeds than either parent.

A number of hybrids which failed to produce ^{fruit} were not included in the study since several of our characters relate to the fruit and seed. Although it would have been possible to have used a NC (no character) state for missing characters we have avoided this. *not with Pearson Regression.*

Product moment correlations were secured at the Indiana University Computing Center on the IBM 709 with both standardized and unstandardized data. The matrices are too lengthy for inclusion here but are on permanent file in the herbarium of Indiana University and available for consultation. Clustering was then done with the computer using the variable group method of Sokol and Michener. ^{This method} The variable results unfortunately in some "reversals" (for example, in fig. 1 the cluster OTU 66-67 link with the cluster OTU 12-13-64-65 at a higher value than the clusters OTUs 12-13 and 64-65 join to each other) and such reversals have been corrected in the manner suggested by Sokol and Sneath (1963). ^{is it not 114 111 112 113 114 115 116 117 118 119 120 121 122 123 124 125 126 127 128 129 130 131 132 133 134 135 136 137 138 139 140 141 142 143 144 145 146 147 148 149 150 151 152 153 154 155 156 157 158 159 160 161 162 163 164 165 166 167 168 169 170 171 172 173 174 175 176 177 178 179 180 181 182 183 184 185 186 187 188 189 190 191 192 193 194 195 196 197 198 199 200 201 202 203 204 205 206 207 208 209 210 211 212 213 214 215 216 217 218 219 220 221 222 223 224 225 226 227 228 229 230 231 232 233 234 235 236 237 238 239 240 241 242 243 244 245 246 247 248 249 250 251 252 253 254 255 256 257 258 259 260 261 262 263 264 265 266 267 268 269 270 271 272 273 274 275 276 277 278 279 280 281 282 283 284 285 286 287 288 289 290 291 292 293 294 295 296 297 298 299 300 301 302 303 304 305 306 307 308 309 310 311 312 313 314 315 316 317 318 319 320 321 322 323 324 325 326 327 328 329 330 331 332 333 334 335 336 337 338 339 340 341 342 343 344 345 346 347 348 349 350 351 352 353 354 355 356 357 358 359 360 361 362 363 364 365 366 367 368 369 370 371 372 373 374 375 376 377 378 379 380 381 382 383 384 385 386 387 388 389 390 391 392 393 394 395 396 397 398 399 400 401 402 403 404 405 406 407 408 409 410 411 412 413 414 415 416 417 418 419 420 421 422 423 424 425 426 427 428 429 430 431 432 433 434 435 436 437 438 439 440 441 442 443 444 445 446 447 448 449 450 451 452 453 454 455 456 457 458 459 460 461 462 463 464 465 466 467 468 469 470 471 472 473 474 475 476 477 478 479 480 481 482 483 484 485 486 487 488 489 490 491 492 493 494 495 496 497 498 499 500 501 502 503 504 505 506 507 508 509 510 511 512 513 514 515 516 517 518 519 520 521 522 523 524 525 526 527 528 529 530 531 532 533 534 535 536 537 538 539 540 541 542 543 544 545 546 547 548 549 550 551 552 553 554 555 556 557 558 559 560 561 562 563 564 565 566 567 568 569 570 571 572 573 574 575 576 577 578 579 580 581 582 583 584 585 586 587 588 589 590 591 592 593 594 595 596 597 598 599 600 601 602 603 604 605 606 607 608 609 610 611 612 613 614 615 616 617 618 619 620 621 622 623 624 625 626 627 628 629 630 631 632 633 634 635 636 637 638 639 640 641 642 643 644 645 646 647 648 649 650 651 652 653 654 655 656 657 658 659 660 661 662 663 664 665 666 667 668 669 670 671 672 673 674 675 676 677 678 679 680 681 682 683 684 685 686 687 688 689 690 691 692 693 694 695 696 697 698 699 700 701 702 703 704 705 706 707 708 709 710 711 712 713 714 715 716 717 718 719 720 721 722 723 724 725 726 727 728 729 730 731 732 733 734 735 736 737 738 739 740 741 742 743 744 745 746 747 748 749 750 751 752 753 754 755 756 757 758 759 760 761 762 763 764 765 766 767 768 769 770 771 772 773 774 775 776 777 778 779 780 781 782 783 784 785 786 787 788 789 790 791 792 793 794 795 796 797 798 799 800 801 802 803 804 805 806 807 808 809 810 811 812 813 814 815 816 817 818 819 820 821 822 823 824 825 826 827 828 829 830 831 832 833 834 835 836 837 838 839 840 841 842 843 844 845 846 847 848 849 850 851 852 853 854 855 856 857 858 859 860 861 862 863 864 865 866 867 868 869 870 871 872 873 874 875 876 877 878 879 880 881 882 883 884 885 886 887 888 889 890 891 892 893 894 895 896 897 898 899 900 901 902 903 904 905 906 907 908 909 910 911 912 913 914 915 916 917 918 919 920 921 922 923 924 925 926 927 928 929 930 931 932 933 934 935 936 937 938 939 940 941 942 943 944 945 946 947 948 949 950 951 952 953 954 955 956 957 958 959 960 961 962 963 964 965 966 967 968 969 970 971 972 973 974 975 976 977 978 979 980 981 982 983 984 985 986 987 988 989 990 991 992 993 994 995 996 997 998 999 1000} Places where reversals occurred are indicated with an "R" in the dendrograms (fig. 1-3). The dendrograms as have been pointed out by others (Sneath and Holm, 1963; Michener, 1963) may mask considerable information contained in the original matrix but at the same time there seems to be no other convenient way to summarize the results obtained. ^{the 4-way clustering the network} The dendrograms drawn from the standardized data are presented in fig. 1-3. The correlation coefficients with ^{from} unstandardized data are much higher than those from standardized data and the dendrograms ^{secured} ~~secured~~ from them in general show the same pattern of relationships with a few major exceptions, some of which we shall call attention to in the Results. We have chosen to use the standardized data since in general it seems ^{should} to give slightly more "reliable results" than unstandardized data, a conclusion ^{because it is based on standard deviation, a statistic more reliable than sample averages} previously reached by Rohlf (). All in all, the method we have used is very similar to that used in entomological numerical taxonomic studies and is fully explained in Sokol and Sneath (1963)

*what is Standardization
what are product moment*

Results

I. Species and species groups.

The first major cluster (OTU 10 through 56, fig. 1) brings together the samples of S. nigrum, S. villosum and various hybrids involving these two species. Some of the allopolyploids (OTU 53, 57, 60, 54, 55) have a diploid parent from another cluster (S. americanum or S. nodiflorum in fig. 2) but ^{their} the clustering with the polyploid parent is not unexpected. The three races of S. nigrum come out as only distantly related; OTU 10 and 25 are linked together ^{but} OTU 26 which was thought to be quite closely related to OTU 25 is rather far removed. The closeness ^{of} the two samples of S. villosum (OTU 12 and 13) is apparent, but OTU 20 which may be nothing more than a geographical race of S. villosum does not tie in closely with the other two. Another species, S. roxburghii (OTU 27, fig. 2) which is also thought to be closely related to S. villosum does not even fall into the same cluster with that species in the present analysis, although it did so in our previous study (Soria and Heiser, 1961).

To the far right of fig. 1 is a second cluster composed of three species. The first of these, S. burbankii (OTU 27) we feel is more closely related 2 to S. villosum. Solanum sarachoides (OTU 28, 29, 30) was thought to be only very distantly related to the other species included ^{and here, the dende. missed.} and we had predicted that it would show its nearest relationship to S. excisiorrhombum ^(OTU 3A). The rather remote affinities of S. sarachoides ^{to other species} and its relationship to S. excisiorrhombum ^{seem to be} ~~be well~~ portrayed by the dendrogram. ^{poorly}

But not to (27) with
which it shares 4, 11, 12

2 It should be obvious that we are using "relationship" in more than one sense. Numerical taxonomy gives us a phenetic relationship based on overall similarity for the characters used. Our concept of relationship involves morphological resemblance with conscious and unconscious weighting of what we regard to be important characters and also may take into account the reproductive isolation or lack of it. The "true" relationship in terms of origin may or may not coincide with either of the above. It should also be apparent that we have defined our species nor have we made any attempt to distinguish between systematics and taxonomy.

A large cluster comprising S. americanum, S. nodiflorum, some hybrids involving these species, S. roxburghii, S. interius and unknowns are shown in figure 2. The position of S. roxburghii has already been commented upon. Two of the strains of S. americanum (OTU 3, 2h) come out very close and OTU ⁶⁹_^ which is simply a dwarf form of OTU 2h falls into the same small cluster. On the other hand, OTU 3l which we also identify as S. americanum falls into the cluster with S. nodiflorum. Hybrids of OTU 3l with OTU 2h gave OTU 7₀ which falls into the cluster with the latter.

Two of the geographical samples of S. nodiflorum (OTU 7, 8) are linked closely but the other samples of this species (OTU 5, 2l) do not come out particularly close to each other or to the previous. ^{samples of this species} Solanum nodiflorum as now interpreted is a very wide spread and highly variable species and it might be argued that we are dealing with more than one species but for the present we prefer to consider our samples as members of a single species. OTU 2l and 22 which show a correlation of 1,000 are samples grown from the same lot of seed but under two different numbers. Their identity was not revealed until the completion of the study and provide a check on our methods. Although the means for all characters of these two numbers are not the same it so happened that the state scores came out the same for every character. OTU 47 was volunteer plant that appeared in the greenhouse and it was thought that its inclusion would finally show its relationship to one of the other OTU's. Such was not the case, however. Possibly if we had more than one plant the results would have been different. We would identify OTU 47 as S. nodiflorum on the basis of the characters usually associated with that species.

OTU 19 from Florida could not be precisely identified as any known species.³ Our tentative hypothesis was that it might be hybrid origin derived from plants similar to S. americanum (OTU 3) and the other unknown from Florida (OTU 32).

³ Unfortunately, numerical taxonomy does not provide us with a name for all our species.

However, as showed by the correlations it does not appear to be closely related to any species included in the study but would have its nearest affinity with S. nodiflorum. To the far right of figure 2 are the two strains of S. interius (OTU 14, 38) and their low level of linkage with other clusters shows them to be most distinct, which is of interest since we had not been convinced that such a species actually existed at the beginning of the study. Re-examination of our specimens reveal that in so far as these two samples are concerned this is a very distinct taxon.

Figure 3 reveals two major clusters. On the right are found a number of species, which we have loosely designated as the "large-flowered" group, and hybrids involving these species. These same species clustered together in the previous analysis (Soria and Heiser, 1961) but the internal arrangement was somewhat different. The two races of S. gracile (OTU 35), ^{16, 17 which are quite far removed from me} ~~are~~ shown as more closely related. ^{another geographically, are shown as as closely related. The Canadian species show an interesting derivation.} related to two strains of the tetraploid S. interandinum (OTU 37, 39) than is the third strain of S. interandinum (OTU 36) to the other members ~~to the members~~ of its own species. However, on the basis of two morphological characters it has been found possible to distinguish the diploids from the tetraploids (Heiser, 1963). It is not too surprising, however, than when a great number of characters are used and given equal weight that S. soriae should prove to be more similar to S. interandinum than are some of the races of the latter species to each other. — OTU 32, another large flowered species, links to the other numbers of this cluster at a fairly low level. It has not yet been satisfactorily identified but appears to be close to the "S. gracile Link" of Small's manual (). As the dendrogram indicates it is a fairly distinctive species.

To the far right of figure 3 stands a cluster of composed of S. melanocerasum (OTU 11) and various hybrids with that species. Solanum melanocerasum stands along with S. sarachoides as one of the morphological extremes in this section of the genus. In our previous study (Soria and Heiser, 1961) its uniqueness is not so readily apparent.

Solanum soriae, a diploid (OTU 33)

In the dendrogram made from unstandardized data S. melanocerasum stands by itself and its hybrids are attached in the first cluster with hybrids of S. nigrum and S. villosum. The arrangement as shown in fig. 3 we feel is the more satisfactory one.

Homoplaid hybrids

It is quite obvious that by inspection of the dendrogram one can ^{not} tell which OTU's are hybrids nor can this information be gained directly from the original matrix of correlations. In general, the hybrids cluster fairly near to one parent. Thus the hybrids of S. villosum x burbankii (OTU 64-67, fig. 1) link to S. villosum (OTU 12, 13); the hybrid S. villosum x aff. villosum (OTU 63, fig. 1) is tied closely to the latter parent (OTU 20). One significant exception is provided by the hybrids of S. nodiflorum x amethystinum (OTU 52, 40, fig. 3) which are rather far removed from both parents. It can be seen that any hybrid involving a species from fig. 3 with either S. americanum or S. nodiflorum always fall into the cluster on fig. 3. To understand more about the position of the hybrids in the dendrograms it is necessary to examine their correlations values in the original matrix. Many of the hybrids show their highest correlations with other hybrids involving one or both of the same parents. However, if we examine the correlations of the hybrids with the species and not consider other hybrids we find that four of the hybrids show their two highest correlations with their two parental species; these involve for the most parts intraspecific hybrids or hybrids between very closely related species. However, because of the nature of the dendrogram they can link only to the species with which they have their highest correlation. Thirteen of the hybrids were found to have their highest correlation with one parental species and their second highest correlation with some species than their second parent, and one hybrid (OTU 41) did not show its highest correlation with either parent.

Reciprocal hybrids were grown for only four combinations (OTU 64, 65; 66, 67, fig. 1; 72, 73; 58, 59, fig. 3). Other reciprocal hybrids were not included because they were not obtained or the reciprocal failed to produce fruit. The reciprocals of a given combination as expected ^{showed} a high correlation but ^{with each other} according to the values secured they were not identical. Whether this reflects an ^{if accurate} actual difference or whether it is due to ^{or would be equal in measurements} sampling errors resulting from the small number of plants used is not known. However, it is possible that cytoplasmic inheritance could be involved which would account for some of the differences. Cytoplasmic factors have been found to operate in many interspecific hybrids involving S. americanum (Heiser and Ungerer, unpubl.).

Antotetraploids

As would be expected the four antotetraploids are closely linked to their ⁴diploid progenitors. Those of S. americanum (OTU 4, fig. 2) and S. douglasii (OTU 2, fig. 3) are associated with the diploids at high correlation levels, whereas those of S. nodiflorum (OTU 9, 23, fig. 2) show lower levels of correlation. It should be clear that the differences we are measuring here apparently stem solely from polyploidy and do not reflect the genic difference which is responsible for the differences among the diploids.

Allopolyploids

The artificial allopolyploids or amphidiploids, unlike the antotetraploids, are morphological quite distinct from their parents as well as being reproductively isolated from ^{them} ¹thus can well be regarded as distinct species. The taxonomic placement of allopolyploids often poses problems. A numerical taxonomic treatment, ^(see discussion) of course, places the allopolyploid nearest to the plants to which it is most similar, which as well be seen, may or may not include one or both of the parents.

Three of the artificial allopolyploids combined parents which were both diploids. Two of these (OTU 50, 62, fig. 3) involved different races of the same species.

They come out in the same minor cluster as do the diploid hybrids of the same combination. The other allopolyploid involving two diploid parents, S. nodiflorum x americanum (OTU 48, fig. 2), however, clusters nearer to S. nodiflorum than it does to the diploid hybrid of the same combination.

Those allopolyploids involving a diploid parent and a polyploid parent, as would be predicted, show a much closer relationship to the polyploid parent than to the diploid. Two different polyploids (OTU 54, 55, fig. 1) derived from different F₁ plants of the terraploid S. villosum x the diploid S. nodiflorum are found in the same cluster as the tetraploid parent. A allopolyploid (OTU 56, fig. 1) derived from the reciprocal hybrid, ^{S. nodiflorum x villosum}, while falling into the same general cluster, shows a surprisingly distant relationship to the previous hybrids. While the plants are different the differences seem to be overemphasized by standardization. The dendrogram derived from unstandardized data shows a much closer relationship of these three allopolyploids. A third allopolyploid involving the other strain of S. villosum and S. nodiflorum (OTU 60, fig. 1) also shows a rather low level of linkage with the previous hybrids although clustering with them. If one goes directly to the original matrix of correlations it is found that three of these allopolyploids of S. villosum x nodiflorum show a higher level of correlation with S. nigrum rather than S. villosum. This is of some interest since it seems possible that S. nigrum, itself a hexaploid, might have originated from a hybrid of this combination. The close relationship of the allopolyploid of S. roxburghii (4n) x nodiflorum (2n) (OTU 14, fig. 2) to S. roxburghii (OTU 6) is shown and that of the two different octoploids of S. melanocerasum (6n) x sarachoides (2n) (OTU 74, 75, fig. 3) to S. melanocerasum (OTU 11).

Hybrid Fertility

Although not all possible crosses have been attempted with OTU's under study, a number have been and the results are of some interest in connection with the relationships shown by the dendrogram. In general, if two plants produce a fertile hybrid

they are considered closely related, whereas if they fail to hybridize or if the fertility of the hybrid is impaired they are considered more distantly related, although, of course, exceptions are known. Morphological similarity is often correlated with crossability, and perhaps the generalization can be made that morphological similar plants will cross more readily and produce more fertile hybrids than morphologically dissimilar ones, although many exceptions are known (see Davis and Heywood, 1963). It is also obvious from biosystematic studies that the acquisition of sterility barriers and morphologically dissimilarities do not necessarily go hand in hand (Stebbins, 1950).

In measuring degrees of ~~fertility reduction~~ ^{receptivity for hybridization studies} several factors have to be considered, among them the ease with which a hybrid can be obtained, the number of fruits set, the number of seeds per fruit, and the pollen fertility. Primary emphasis will be placed on the last here for the sake of brevity. Seed set in most of the Solanum hybrids with 90% pollen stainability or better was equal to that of the parents and those plants with more reduced pollen stainability generally showed a seed set slightly lower than that of the pollen stainability. The ~~pollen~~ ^{plants} stainability was measured in from five to ten hybrids in each cross with two exceptions where fewer than five plants were obtained. The parent species gave 90 to 100% with the exception of S. amethystinum which ranged from 82 to 88%. ^(this species is sensitive to daylight variation) At least 10 attempts have been made to secure hybrids for all the crosses marked in Table 2. Some of these secured, however, required more than that number of attempts, however, and not too much significance should be attached to the failure to secure hybrids in any particular cross. Certain species, S. nodiflorum, S. americanum, and S. interius, offer some mechanical difficulties for the small size of the flower makes it difficult to accomplish emasculation without damaging the stigmas.

Results of crossing of the diploid species are given in Table 2. It can be seen that a completely clear cut pattern of relationships does not emerge. However it is clear that S. sarachoides which is well separated from all the other species of morphologically is well isolated from them reproductively. We regard the failure

to secure crosses of this species with others as significant in that we can fairly

ability averages 96% or higher and the seed set falls between that of the two parental species. However, the hybrids involving S. melanocerasum with S. nigrum also give fairly fertile hybrids with pollen stainability averaging 91 to 95%. The seed number in the hybrid⁵ however, was less than that of either of the parent species. Thus we are faced with somewhat of a dilemma. We use the fertility data to support our claim that the three races of S. nigrum are in reality members of a single species, but we reject the fertility data as indicating that S. nigrum and S. melanocerasum are closely related.

Discussion

✓ According to Sokol and Sneath (1963) their methods of numerical analysis should result in a taxonomy that is repeatable, objective, and stable. Obviously our results with Solanum are repeatable if one works with the same data and uses the same methods. However, it is clear that our results are not the same as that we secured in an earlier analysis (Soria and Heiser, 1961). ^{likely due to the wrong method and data} Moreover, it is apparent that slightly different arrangements ^{will} may result depending upon the method used as indicated by the differences we found using standardized and unstandardized data.⁴ Thus we are still faced with a value judgement in deciding the "best" method to use. Our study has been as objective as possible, but it should be realized that some subjective ^{element} enters in the selection of characters and their coding into states. As to stability of classification there is some question here as to what is meant. The stabilization of names is a matter of nomenclature and numerical taxonomy has nothing to contribute to this problem. If on the other hand,

⁴ Dr. David Rogers has kindly run our data using his similarity coefficient. The matrix resulting shows some disagreements with our results, and although we have not constructed a dendrogram from the matrix it is clear that it would give a slightly different arrangement of the taxa from the ones we secured.

"stability" ~~may~~ refer to agreement on the number of taxa that should be recognized in a group and their relative positions, we may ask if this is as desirable as some people assume. As long as taxonomy is a living science, utilizing new data and new methods, we may expect changes in classification as a consequence. (see Mason, 1950)

✓ In many of the previous numerical taxonomic studies it has been pointed out there is good agreement between the treatment from the numerical method and the orthodox method. The numerical taxonomists have used this as evidence for the soundness of their method, and the non-numerical taxonomists might use this as evidence for ~~support~~ that traditional methods are fairly objective and repeatable. It should be emphasized that in the present study that, although we have found some agreement with our previous ideas of relationships, there are some rather serious disagreements. However, if one wants to construct a "phenon line" to recognize "species" we would suggest ^{for this group} drawing it at the 500 level of correlation. This does considerable violence to our previous concept of S. nigrum, S. americanum, S. nodiflorum and S. roxburghii. No phenon line can be constructed to show these species as we had previously recognized them. To this it might be said that our previous ideas were in error and that numerical taxonomy gives the "correct" interpretation. This possibility exists, particularly in regard to S. nodiflorum, but we do not feel that the numerical taxonomic ^{treatment} result, in any great improvement on our earlier ideas.

It is worthwhile perhaps to speculate as to why the numerical analysis failed to show ^{some} these species as we had previously interpreted them. One obvious reason for this is that we have apparently attached more importance to certain characters whereas in the numerical treatment all characters are given equal value. There is, as Rogers has pointed out (1963), no reason why a numerical analysis could not include weighing. Rogers suggests that biochemical characters may be given more weight than morphological. But how does one determine whether the presence or absence of a ^{for example,} chalcone is more or less important than whether a leaf is entire or

lobed and if one is more important than the other do we give it twice or three times as much weight as the other.⁷ It is obvious that once weighing is done the numerical taxonomic analysis loses much of its so called objectivity. One point that has frequently been overlooked is that the taxonomic importance or significance of a character may change from one species to the next and from one group of species to another. The whole subject of characters, how to choose them, how to score them, and possible methods of weighing them, needs more study than the mathematical or statistical methods to be followed in erecting classifications which thus far has received the most attention. *

The foregoing discussion has been mostly concerned with the species, but turning now to the hybrids and allopolyploids it is seen that the dendrogram reveals little about their origin. Since numerical taxonomy does not claim to be phylogenetic this was not unexpected. Although sometimes hybrids have been maintained as species by taxonomists without knowing their true nature, today with modern taxonomic methods it is usually possible not only to recognize a plant as a ^{hybrid} ~~species~~ or allopolyploid but to point to its actual ancestors. To accomplish this the taxonomist has not only used morphological characters but given emphasis to other lines of study, including plant distribution. For example, a hybrid is usually found where its parent species have an overlapping distribution. To the objection that we are confusing classification with ^{evolutionary studies} ~~evaluation~~, our answer is that for a taxonomic system to have the maximum amount of content we should know as much as the origin of taxa as possible and with some groups it is possible to ^{obtain} ~~have~~ such information and in turn to use it in our classification.

However, the numerical analysis did reveal some things of interest about the hybrids. For example, ^{some of them} were far less intermediate between their parents than would be expected. Thus, as Peter Raven has pointed out (oral communication, 1963) that the numerical analysis provides a tool for analyzing overall morphological expression of hybrids. The finding that the highest correlation of allopolyploid of S. villosum x nodiflorum was with S. nigrum also indicates a potential value of the method in

studying origins.

Michener (1963) has discussed allopolyploids in connection with an organism belonging to more than one group. As seen from our dendrograms an allopolyploid having its progenitors from different clusters has to be linked to only one of the clusters. Actually this is not dissimilar to present taxonomic practice. An allopolyploid from an intergeneric hybrid is usually placed in the genus with the species to which it is most similar. The tetraploids wheats combining a genome of Triticum and one of Aegilops are placed in Triticum. That the hexaploid wheats with two genomes of Aegilops and one of Triticum are placed in Triticum hardly seems logical, however, but offers no great problems. No system that permits overlapping clusters in numerical taxonomy has yet been successfully designed but the ramifying-linkage method of Cattell (1964) works fairly well for a small number of taxa (Eshbaugh, 1963).

Although our study doesn't bear on this point, attention should be called to the fact that numerical taxonomy may give a rather misleading representation of the relationships of cultivated plants. Eshbaugh (1963) in a study of several wild and cultivated species of Capsicum found that the numerical taxonomic results for the wild species gave fairly good agreement with the relationships he thought existed and agreed closely with the results of relationship as indicated by hybridization studies. Yet he found that the cultivated species, C. pendulum, showed a most distant relationship to the wild species, one of which C. microcarpum, which from other lines of evidence is thought to be its progenitor. Capsicum pendulum he considers merely a variety of C. microcarpum, even though the numerical analysis indicated the ^{was} closest relative of the cultivated species ~~to be~~ with C. frutescens, another cultivated species.

✓ ^{optimistic?} It seems evident to us that numerical taxonomy does not live up to the extravagant claims made for it by Sokol and Sneath (); nor is it as bad as some of the critics have claimed. We feel that it might be a valuable adjunct in taxonomic studies and it may have particular appreciation in groups of cultivars

of complex origins, apomictic complexes and the like when the type of "natural-artificial" ^{classification} which numerical taxonomy gives ^{is} all that can be hoped for. One of us (Heiser, 1963) has previously pointed out that it has been realized that taxonomists should use more characters in their work and that computers will be a great aid in analyzing the large bodies of data which result. Perhaps the greatest good that has come out of numerical taxonomy to date is that it has forced many taxonomists to give more thoughts to their methods and objectives. Our hope in the present paper is, that by presenting actual data rather than discussion alone, ~~that~~ certain issues might be clarified.

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Table 1. Source of species and hybrids of Solanum[†]

Species	Accession Number	Locality or Parentage	Collector	Ploidy Level	Number of Plants
1. <u>S. eschweylei</u> (Funtia) Heiser	(S106)	Costa Rica	C. B. Heiser	2n	7
2. <u>S. eschweylei</u> (colchicine tetraploid of no. 1)	(CS106)			4n	2
3. <u>S. americanum</u> Mill.	(S118)	Florida	W. F. Stoutania ^{YF}	2n	9
4. <u>S. americanum</u> (colchicine tetraploid of no. 3)	(CS118)			4n	2
5. <u>S. nodiflorum</u> Jacq.	(S122)	Peru	C. Rick	2n	10
6. <u>S. roemerianum</u> Don.	(S126)	India	A. Sawant	4n	10
7. <u>S. nodiflorum</u>	(S127)	India	A. Sawant	2n	10
8. <u>S. nodiflorum</u>	(S128)	Ecuador	J. Soria	2n	10
9. <u>S. nodiflorum</u> (colchicine tetraploid of no. 8)	(CS128)			4n	5
10. <u>S. rigens</u> L.	(S133)	Denmark	C. A. Jorgensen	6n	10
11. <u>S. melanocerasum</u> All.	(S132)	cultivated	C. A. Jorgensen	6n	10
12. <u>S. villosum</u> Mill.	(S135)	Denmark	C. A. Jorgensen	4n	10
13. <u>S. villosum</u>	(S136)	Denmark	C. A. Jorgensen	4n	10
14. <u>S. interius</u> Rydb.	(S140a)	New Mexico	R. C. Jackson	2n	10
15. <u>S. douglasii</u> Donal	(S143)	California	K. Maller	2n	9
16. <u>S. grave</u> Otto	(S144)	New Zealand	G. T. S. Baylis	2n	10

17. <u>S. gracile</u>	(S206)	India	Saier Seed Co.	2n	10
18. <u>S. gracile</u> (colchicine tetraploid of no. 17)	(CS206)			2n	3
19. <u>S. species</u>	(S211)	Florida	Heiser	2n	10
20. <u>S. aff. villosum</u>	(S839)	Africa	R. Polhill	4n	10
21. <u>S. nodiflorum</u>	(S251)	California	J. McCaskill	2n	10
22. <u>S. nodiflorum</u>	(S251)	California	J. McCaskill	2n	90
23. <u>S. nodiflorum</u> (colchicine tetraploid of no. 21)	(CS251)			4n	1
24. <u>S. americanum</u>	(S264)	Kansas	G. E. Morley	2n	10
25. <u>S. nigrum</u> esp. <u>schultzei</u> f. <u>humile</u>	(S276)	Germany	I. Wessely	6n	30
26. <u>S. nigrum</u> esp. <u>schultzei</u> f. <u>laridum</u>	(S277)	Germany	I. Wessely	6n	8
27. <u>S. wartschii</u> Bitter	(S282)	Africa	Gle ^c pler's Seed ^e man	4n	10
28. <u>S. sarachoides</u> Sendt.	(S283)	California	C. F. Smith	2n	10
29. <u>S. sarachoides</u> (colchicine tetraploid of no. 28 treated but undoubted)	(CS283)			2n	9
30. <u>S. sarachoides</u> (colchicine tetraploid of no. 28)	(CS283-13)			4n	1
31. <u>S. americanum</u>	(4769)	Missouri	C. B. Heiser	2n	10
32. <u>S. species</u>	(S210)	Florida	C. K. Wood	2n	8
33. <u>S. scirae</u> Heiser	(S295)	Ecuador	C. B. Heiser	2n	10
34. <u>S. excisicolumbum</u> Bitter	(S297)	Ecuador	C. B. Heiser	4n	10

35. <u>S. paracasii</u> Heiser	(3298)	Ecuador	G. B. Heiser	6n	10
36. <u>S. interandinum</u> Bitter	(8299)	Ecuador	G. B. Heiser	4n	9
37. <u>S. interandinum</u>	(8300)	Ecuador	G. B. Heiser	4n	5
38. <u>S. interius</u>	(8265)	Kansas	G. E. Morley	2n	9
39. <u>S. interandinum</u>	(6020)	Ecuador	G. B. Heiser	4n	3
40. <u>S. nodiflorum</u> x <u>amethystinum</u>		5 x 1		2n	7
41. <u>S. ~~americanum~~ nodiflorum</u> x <u>amethystinum</u>		8 x 1		2n	10
42. <u>S. douglasii</u> x <u>amethystinum</u>		15 x 1		2n	9
43. <u>S. americanum</u> x <u>nodiflorum</u>		3 x 5		2n	10
44. <u>S. roxburghii</u> x <u>americanum</u>		6 x 3		6n*	10
45. <u>S. gracile</u> x <u>americanum</u>		16 x 3		2n	10
46. <u>S. americanum</u> x <u>nodiflorum</u>		3 x 21		4n*	2
47. unknown volunteer				2n	1
48. <u>S. nodiflorum</u> x <u>americanum</u>		21 x 3		4n*	5
49. <u>S. gracile</u> x <u>nodiflorum</u>		17 x 5		2b	9
50. <u>S. gracile</u> x <u>nodiflorum</u> (colchicine tetraploid of no. 49)				4n	1
51. <u>S. nodiflorum</u> x sp.		5 x 32		2n	10
52. <u>S. nodiflorum</u> x <u>gracile</u>		7 x 16		2n	6
53. <u>S. nodiflorum</u> - <u>villosum</u> x <u>nigrum</u> (F2)		56 x 10 (see no. 57)		6n	10
54. <u>S. villosum</u> x <u>nodiflorum</u>		12 x 8		6n*	10
55. <u>S. villosum</u> x <u>nodiflorum</u>		12 x 8		6n*	10

56. <u>S. nodiflorum</u> x <u>villosum</u>	8 x 12	6n*	10
57. <u>S. nigrum</u> x <u>nodiflorum</u> - <u>villosum</u> (F ₁)	56 x 10	6n	10
58. <u>S. nodiflorum</u> - <u>villosum</u> x <u>melanocerasum</u>	56 x 11	6n	10
59. <u>S. melanocerasum</u> x <u>nodiflorum</u> - <u>villosum</u>	11 x 56	6n	8
60. <u>S. villosum</u> x <u>nodiflorum</u>	13 x 8	6n*	8
61. <u>S. nodiflorum</u> x <u>gracile</u>	8 x 16	2n	8
62. <u>S. nodiflorum</u> x <u>gracile</u>	8 x 16	4n*	1
63. <u>S. villosum</u> x <u>aff. villosum</u>	12 x 20	4n	10
64. <u>S. villosum</u> x <u>barbankii</u>	12 x 27	4n	10
65. <u>S. barbankii</u> x <u>villosum</u>	27 x 12	4n	10
66. <u>S. villosum</u> x <u>barbankii</u>	13 x 27	4n	10
67. <u>S. barbankii</u> x <u>villosum</u>	27 x 13	4n	10
68. <u>S. villosum</u> x <u>barbankii</u> (F ₃)	12 x 27	4n	10
69. <u>S. americanum</u> - dwarf form of no. 24		2n	5
70. <u>S. americanum</u> x <u>americanum</u>	31 x 24	2n	9
71. <u>S. nigrum</u> var. x <u>melanocerasum</u>	25 x 11	6n	10
72. <u>S. melanocerasum</u> x <u>S. nigrum</u> var.	11 x 26	6n	8
73. <u>S. nigrum</u> var. x <u>melanocerasum</u>	26 x 11	6n	9
74. <u>S. melanocerasum</u> x <u>sarachoides</u> (F ₃)	11 x 28	8n*	10
75. <u>S. melanocerasum</u> x <u>sarachoides</u> (F ₄)	11 x 28	8n*	10

*artificial or spontaneous allopolyploid

† Representative specimens of all numbers are deposited in the herbarium of Indiana University and the labels give additional details on the geographical sources.

Corolla color	1-2
star	3-8
lobe shape	9-11
length	12-15
sinus/length	16-17
width	18-20
texture	21-23
style length	24-26
anther length	27-29
length style > anther	30-32
anther color	33-35
filament pubescent	36-39
calyx length	40-43
" depth of sinus	44-47
degree calyx lobes	48-50
fruit	48-50
style pubescent	51-53
pedicel	54-56
pollen diameter	57-61
number of flowers	62-65
angle of peduncle	66-67
" " pedicel	68-69
length of peduncle	70-3
" " pedicel	4-9
height	10-13
stem diameter	14-16
inflorescence type	17-19
calyx reflexed or not	20-21
branching type	22-24
"rosette" present or not	25-26
days to flowering	27-30
light area between	
calyx lobes or not	31-32
calyx regular to inv.	33-35
" - purple spot or no	36-37
stem ridged or not	38-39
fruit color	40-44
" shape	45-47
" dull or shiny	48-49

Card 1

card 2

C. B. Heiser - Solanum
characters

Corolla color	1-2
star	3-8
lobe shape	9-11
length	12-15
sinus/length	16-17
width	18-20
texture	21-23
style length	24-26
anther length	27-29
length style > anther	30-32
anther color	33-35
filament pubescence	36-39
calyx length	40-43
" depth of sinus	44-47
degree calyx embossed	
fruit	48-50
style pubescence	51-53
pedicel "	54-56
pollen diameter	57-61
number of flowers	62-65
angle of peduncle	66-67
" " pedicel	68-69
length of peduncle	70-3
" " pedicel	4-9
height	10-13
stem diameter	14-16
inflorescence type	17-19
calyx reflexed or not	20-21
branching type	22-24
"resette" present or not	25-26
days to flowering	27-30
light area between	
calyx lobes or not	31-32
calyx regular to inv.	33-35
" purple spot or no	36-37
stem ridged or not	38-39
fruit color	40-44
" shape	45-47
" dull or shiny	48-49

Card 1

card 2

number of seeds	56-60
" of scarlet granules	61-65
diameter of seeds	66-68
color " "	69-70
leaves - shape	1-2
" " - length	3-6
" " - index	7-9
" teeth - depth	10-14
" teeth - number	15-18
leaf length / petiole length	19-20
basal angle	21-23
number of hairs	24-27
length of hairs	28-30
presence or absence of	
long glandular hairs	31-32
short " "	33-34
simple hairs	35-36
pedicels hooked or not	37-38
inflorescence simple or	
divaricate	39-40
branching from base	41-42
berries hard or soft	43-44

card 3

number of seeds	56-60
" of scarotie granules	61-65
diameter of seeds	66-68
color " "	69-70
leaves - shape	1-2
" - length	3-6
" - index	7-9
" teeth - depth	10-14
" teeth - number	15-18
leaf length / petiole length	19-20
basal angle	21-23
number of hairs	24-27
length of hairs	28-30
presence or absence of	
long glandular hairs	31-32
short " "	33-34
simple hairs	35-36
petioles hooked or not	37-38
inflorescence simple or	
diplicate	39-40
branching from base	41-42
hairs hard or soft	43-44

card 3

Table 2. Pollen stainability of diploid *Solanum* hybrids

	OTU	1	15	16	17	5	7	8	21	3	31	24	14	38	19	32	28
1	<i>S. anthystrum</i>	1				--	--	--		--					--	--	--
2	<i>S. boulaeti</i>	15	77			0	--	86		--							
3	<i>S. gracile</i>	16	49	--		99	32	33	--	16					--	--	--
4	<i>S. gracile</i>	17	48		95	35	+			--					--	--	--
5	<i>S. nodiflorum</i>	5	43	--	22	59		57		45	36	--			68	--	--
6	<i>S. nodiflorum</i>	7	51	--	27	+	42		55	39	17	28			ng	--	--
7	<i>S. nodiflorum</i>	8	41	--	32		46		20	16						ng	--
8	<i>S. nodiflorum</i>	21				--	42	23		8	9						
9	<i>S. americanum</i>	3	+	--	--	62	36	0	--	0		93	60		24	35	
10	<i>S. americanum</i>	31				40	8		2	88		72					
11	<i>S. americanum</i>	24							4	98			--	22			
12	<i>S. interius</i>	14								ng		ng		95			
13	<i>S. interius</i>	38		--								ng	--				
14	<i>S. sp.</i>	19	ng		ng	25	76									--	--
15	<i>S. sp.</i>	32	ng		ng		ng	ng		17			14		69		--
16	<i>S. saracoides</i>	28	--		--	--	ng	--							--	--	

Explanation: Female parent is given in vertical column. A blank indicates that the hybrid was not attempted. A dash (--) indicates that the hybrid was attempted but no fruit was obtained. A cross (+) indicates that the hybrids died before flowering. The abbreviation ng indicates that seed was obtained from the cross but failed to germinate. The figure indicates the percentage of stainable pollen grains.

Heiser -- Solanum

184 attributes
75 objects

<u>Character- istic</u>	<u>Last punch attribute</u>	<u>Character- istic</u>	<u>Last punch attribute</u>
1	102	30	230
2	108	31	232
3	111	32	235
4	115	33	237
5	117	34	239
6	120	35	244
7	123	36	247
8	126	37	249
9	129	38	255
10	132	39	260
11	135	40	265
12	139	41	268
13	143	42	270
14	147	43	302
15	150	44	306
16	153	45	309
17	156	46	314
18	161	47	318
19	165	48	320
20	167	49	323
21	169	50	327
22	203	51	330
23	209	52	332
24	213	53	334
25	216	54	336
26	219	55	338
27	221	56	340
28	224	57	342
29	226	58	344

C. B. Heiser

Solanum184 alt
75 obj.char. last
punch
alt
all

1	① 2	28 - 24	55 - 38
2	8	29 - 26	56 - 40
3	11	30 - 30	57 - 42
4	15	31 - 32	58 - 44
5	17	32 - 35	
6	20	33 - 37	
7	23	34 - 39	
8	26	35 - 44	
9	29	36 - 47	
10	32	37 - 49	
11	35	38 - 55	
12	39	39 - 60	
13	43	40 - 65	
14	47	41 - 68	
15	50	42 - 70	
16	53	43 - ③ 2	
17	56	44 - 6	
18	61	45 - 9	
19	65	46 - 14	
20	67	47 - 18	
21	69 ①	48 - 20	
22	② 3	49 - 23	
23	9	50 - 27	
24	13	51 - 30	
25	16	52 - 32	
26	19	53 - 34	
27	21	54 - 36	

(1)

a mod of .01724 = 1 Character

(123-28, 29, 30)

Area 1

3 species - S. sarachoides all come out together. 29 (Cochine-tetra failed + 30 at Sig 88 + then other spec. 28. at 84.5. The failed tetra 29 came out with tetra 30 together & both derived from diploid which came out at 84.5. 28 came out with 29. .793 Sig min for 28 & 30.

28 .845 29 .88 30
 .793

Area 2

(4-34)

1 spp eximius cluster next to look & comes in on (67+68) [67 = burbankii X villosum] [68 = villosum X burbankii] The plot indicates a difference of only 1 character. It clusters at .637. It is then at least 1 more character dissimilar to anything present above the horizontal line than anything above the horizontal line is to anything else above the horizontal line.

Area 3a

1 spp S. melanocerasum (5-11) It clusters at level 18, C-.706+ with the hybrids S. nigrum var X melanocerasum, [1-75] + S. nigrum var X melanocerasum [10-71] It differs (mod) by at least 4 characters from anything discussed above (areas 1 & 2) & by at least 1 character from (75-22) s3

(2)

other members in the analysis than these same members differ among themselves. The 53 members differ by no more than 17 char. S. melanocerasum differs from the hybrids 74, 75, 73, 72 + 71 by at least 5 characters. There is a nice moat between the 5 members + 2 complex hybrids.

Area 3B This area contains 74, 75, 73, 72, 71 (6-10) + consists of the only simple hybrids of S. melanocerasum. They are hybrids of the species with S. nigrum + S. sarachoides. S. melanocerasum came in simultaneously with both species at the .706 level. The hybrids came together at the -.79 S_{ij} level as opposed to the hybrid species cluster at .706 - a considerable moat.

Area 3C The complex hybrids 58 + 59 come at 89.5. S. melanocerasum is a parent of both with two new species S. nodiflorum (58) + S. villosum (59). The complex hybrids have good moats. Note that they are near the species S. melanocerasum + that they are less related to the species than to the simple hybrids. Note too that in order to have (on the basis of the plotter) the complex hybrids + simple hybrids in the same cluster with

(3)

the species that other hybrids that do not contain S. melanocerasum flood must be ~~included~~. On the basis of the plotter it is better to consider the 3 area as 3 different clusters? ' ' ' ' ,

Note that the cluster that would contain the species if we followed out the horizontal line would contain all but 4 specimens in the whole analysis (69 spp), the single hybrids all but 22 (53 spp) + the complex 19 spp. (exclude 55 spp).

Note also that the next two single hybrids in order contain the two additional + remaining species of the complex hybrids.

Note that thus far we have accounted for the species melanocerasum + sarothoides + their hybrids whether simple or complex. The species S. excisor-hornbeum from Ecuador has also been segregated - there are no hybrids in the analysis containing this species

(4)
Area 4 The shall discuss the plot from
point 13 to 31 - 18 members

Two pairs are formed at Sig at
96+. One pair is 64+65 *S. villosum*,
burbankii * *villosum*, the other pair is
12+13, *S. villosum*. These are
the only two specimens of the species
S. villosum. 66+67 come together at
Sig 93+ (66 = *villosum* x *burbankii*) (67 =
burbankii x *villosum*). The 4 hybrid spec
come together at Sig 86+. They then
join 55 (*villosum* x *nodiflorum*). With a gap
of 1 more character - 83 Sig join 54
(*villosum* x *nodiflorum*) which also brings
in the two specimens already clustered
of *S. villosum*. 56+68 then come in
56 is *nodiflorum* x *villosum* + 68 is
villosum x *burbankii*.

In summary for the above - on
the left side of the plot we have
the 3 specimens of the hybrid of
villosum + *nodiflorum* grouped together
with one character difference between the
specimens. These 3 specimens are also
juxtaposed to the complex hybrids
containing the above two species with
one character difference.

after (containing 8 the right) the
villosum x nodiflorum 3 hybrids of
barbentzii x villosum follow; then the
 two villosum species specimens. This
 Sig⁸¹⁷ level closes with another specimen of
villosum x barbentzii. Diagrammatically we
 get

Complex ² / hyb.	vill. ³ x nod.	barb ³ x vill.	vill. ²	barb ¹ x vill.	vill. ¹ x nod. ⁶⁰
at .814 Sig					

It is displeasing that at -.795 Sig
 60 x 26 come together. This is the hybrid 60
 (villosum x nodiflorum) and 26 B. nigrum
schantzianum f. lucidum. The plot base of
 this pair is #45+ which is a 2 character change or
 drop from the above mentioned group with all
 the remaining hybrids of villosum x nodiflorum.

- Taximetrics Laboratory

January 24, 1967

Dr. Llewellyn H. Colinvaux
Botany and Plant Pathology
College of Biological Sciences
The Ohio State University
Columbus, Ohio 43210

Dear Llewellyn:

It was good to hear from you and know that you want to go along with the analysis of Halimeda, especially the H. opuntia complex. I do believe you can put the data together very easily, particularly so since we have sent the paper on setting up the characters (Bio-Science 16(11)).

Data coding can be most easily done on sheets similar to the one enclosed. If there are any questions about the techniques, please do not hesitate to say something about it.

After you have finished the data preparation, send it to us and we will make a run on it. After the thing has been computed, we will send you back the printout with instructions for its interpretation and use. I look forward to more work on Halimeda.

Sincerely,

David J. Rogers
Professor of Botany

DJR:ch

Enc.

THE OHIO STATE UNIVERSITY
COLUMBUS, OHIO 43210

~~XXXXXXXXXXXXXXXXXXXXXXXXXXXX~~
~~XXXXXXXXXXXXXXXXXXXXXXXXXXXX~~
College of Biological Sciences

BOTANY AND PLANT PATHOLOGY
BOTANY AND ZOOLOGY BUILDING
1735 NEIL AVENUE

January 17, 1967

Dr. D. J. Rogers
Taxonomy Laboratory
Department of Botany and Plant Pathology
Colorado State University
Fort Collins, Colorado 80521

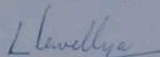
Dear Dave:

I am still tussling with the Halimeda opuntia complex--and will be for at least another year. But on the way to answering the questions I have posed myself, I have looked at all pertinent specimens (12) of one or two entities that have been located thus far. I have comparable new data on some material of typical H. opuntia (for I am treating at least two characters differently), and would be willing to share this data--or put it in your desired form if I can do so relatively easily--if you have an interest in working with it.

Very probably the problem--whether I have a cluster of 1, 2, 3, (or more!), Entities is too small and it would be better to wait until I have worked out much of the complex. But at present I have not clearly resolved it in my own mind--and it seems a particularly opportune time to think of a computer program for it.

My herbarium "expedition" to Europe in late autumn was most rewarding.

Yours sincerely,



Llewellyn H. Colinaux

LHC:mo

TABLE I

LIST OF HALIMEDA CHARACTERISTICS AND ATTRIBUTES

1. Color of Plants (category)
 - 1a. Contains or sometimes contains brown or brownish shades.
 - 101 brown
 - 102 no brown
 - 1b. Yellow or yellowish shades
 - 103 yellow
 - 104 no yellow
 - 1c. White or off-white
 - 105 white
 - 106 no white
 - 1d. Green or greenish shades
 - 107 green
 - 108 no green
 - 1e. Blue or bluish shades
 - 109 blue
 - 110 no blue
2. Height or length (if holdfast 1 cm. or less, then included in measurement)
 - 111 1-9 cm.
 - 112 10-19 cm.
 - 113 20-29 cm.
 - 114 30-up cm.
3. Nature of holdfast
 - 115 holdfast small--rarely over 1 cm.
 - 116 holdfast large--rarely less than 1 cm.
 - 117 holdfast small but sometimes long and fibrous
 - 118 holdfast small but plant layers
4. Degree of calcification
 - 119 calcification light
 - 120 calcification light to moderate
 - 121 calcification moderate
 - 122 calcification moderate to heavy
 - 123 calcification heavy

5. Friability of segments

- 124 segment friable
- 125 segment brittle
- 126 segment durable

6. Length and width of segments in mm.

- 127 0.25-4 x 2.5-5
- 128 5-10 x 1-4
- 129 5-10 x 5-10
- 130 5-10 x 11-15
- 131 5-10 x 16-20
- 132 11-15 x 11-15
- 133 11-15 x 16-20
- 134 11-15 x 21-25
- 135 16-20 x 16-20
- 136 16-20 x 21-25
- 137 21-25 x 26-
- 138 26-30 x 26-

7. Thickness of segments in mm.

- 139 0.3-0.5
- 140 0.5
- 141 0.5-0.75
- 142 0.7-1
- 143 0.75-1 25
- 144 1
- 145 1.-1.5
- 146 1.5
- 147 2
- 148 5.

8. Nature of branch terminals

- 149 rope like
- 150 otherwise

9 Nature of surface of plant

- 151 surface dull or somewhat glossy
- 152 dull
- 153 somewhat glossy or often glossy
- 154 smooth and somewhat (usually?) glossy
- 155 smooth
- 156 dull and rugose but not pitted
- 157 dull, rugose and pitted
- 158 dull and somewhat rough
- 159 generally glossy and smooth, but sometimes rugose
- 160 slightly glossy and rugose, tending to crack on drying

10. Nature of branching habit

- 161 complanate
- 162 radiate

11. Number of branches (from segment)

- 163 generally ditrichotomous
- 164 mainly trichotomous
- 165 mainly di to tetrachotomous
- 166 may be more than tetrachotomous
- 167 commonly dichotomous, but with up to four branches

12. Shape of margins of segments

- 168 upper margin entire
- 169 upper margin entire to (or) slightly lobed
- 170 upper margin entire undulate or lobed
- 201 upper margin lobed
- 202 upper margin entire to trilobed
- 203 upper margin two to five lobed
- 204 upper margin entire or shallow lobed occ. deeply indented
- 205 upper margin trilobed with terete arms
- 206 outer margin entire or slightly undulating
- 207 outer margin entire, undulating or somewhat deeply indented
- 208 outer margin entire, undulating or shallowly lobed
- 209 outer margin entire, undulating or deeply lobed

13. Ornamentation of surface of segments

- 210 plane
- 211 plane, often becoming concave on drying
- 212 plane, occasionally or sometimes slightly ribbed
- 213 ribbed or frequently ribbed
- 214 plane, ribbed or keeled
- 215 frequently ribbed--sometimes keeled
- 216 occasionally or slightly ribbed
- 217 convex

14. Presence or absence of cushion segment

- 218 present-stipeate
- 219 absent--sessile

15. Shape of basal segments

- 220 segments often subcuneate
- 221 cylindrical to subcylindrical
- 222 cylindrical to cuneate or subcylindrical to subcuneate
- 223 compressed cylindrical or subcuneate
- 224 terete or subterete
- 225 cylindrical, often moniloid
- 226 subcylindrical, subcuneate to subreniform
- 227 compressed cylindrical to trapezoidal
- 228 cask-shaped
- 229 flabellate (cuneate)

16. Shape of other segments

1a. cuneate or subcuneate

- 230 mainly or commonly
- 231 sometimes or at some loci
- 232 otherwise

2a. discoid or subdiscoid

- 233 mainly or commonly
- 234 sometimes
- 235 otherwise

3a. reniform or subreniform

- 236 mainly or commonly
- 237 sometimes
- 238 otherwise

4a. cylindrical or subcylindrical or compressed cylindrical

- 239 mainly or commonly
- 240 sometimes or at certain loci
- 241 otherwise

5a. obovate or subobovate

- 242 mainly or commonly
- 243 otherwise

6a. trapezoidal

- 244 mainly or commonly
- 245 otherwise

7a. spherical

- 246 commonly
- 247 sometimes or at certain loci
- 248 otherwise

8a. auriculate

- 249 commonly
- 250 otherwise

9a. oblong

- 251 commonly
- 252 otherwise

17. Fusion of basal segments
 - 253 frequently consolidated laterally
 - 254 never consolidated laterally
18. Development of secondary utricles
 - 255 constrictions at the site of branching
 - 256 constrictions slight
 - 257 constrictions absent (linear)
19. Number of utricle layers in mature internodes
 - 258 two or three layers
 - 259 three layers
 - 260 two to four layers
 - 261 three to four layers
 - 262 two layers
 - 263 two to five layers
 - 264 three to five layers
20. Number of occasional layers of utricles in mature internodes
 - 265 two layers
 - 266 three layers
 - 267 four layers
 - 268 five layers
21. Shape of surface view of peripheral cells
 - 269 hexagonal or polyhedral
 - 270 round
 - 301 somewhat rounded or hexagonal
 - 302 usually round
22. Thickening of primary cell wall
 - 303 lateral and peripheral wall occasionally thickened
 - 304 lateral and peripheral walls usually thickened
 - 305 occasionally thickened toward periphery
 - 306 not thickened
23. Adhesion of mature segments of primary utricles after decalcification
 - 307 generally attached
 - 308 firmly attached
 - 309 usually slightly attached, occasionally separating
 - 310 attached or separating slightly
 - 311 slightly attached or separating
 - 312 adhering slightly
 - 313 separating

24. Adhesion of inner utricles

- 314 inner utricles firmly adpressed
- 315 inner utricles not adpressed

25. Nature of production from lateral branches of the medullary
filaments

- 316 dichotomous
- 317 dichotomous and tetrachotomous

26. Shape of secondary cortical utricles

- 318 greatly inflated
- 319 capitate
- 320 clavate
- 321 obpyramidal
- 322 generally globular or subglobular
- 323 tubular
- 324 otherwise

27. Shape of tertiary utricles

- 325 greatly swollen or expanded
- 326 globular or subglobular
- 327 tubular
- 328 otherwise

28. External appearance of periphery of cells

- 329 indurated spine
- 330 convex
- 331 flat
- 332 round
- 333 hexagonal

29. Usual number of primary utricles borne on secondary utricles

- 334 two
- 335 four
- 336 one to seven
- 337 one to eight
- 338 one to fourteen
- 339 six to eighteen
- 340 two or four
- 341 one, two, or four

30. Less frequent number of utricles borne by secondary utricles

- 342 occasionally two
- 343 occasionally five
- 344 occasionally up to six
- 345 occasionally up to eight
- 346 occasionally one or four utricles

- 31 Average length of attachment of utricles (peripheral) after decalcification
347 0-10 u
348 11-20 u
349 20-45 u
- 32 Nature of utricle fusion after decalcification
350 separating in some degree but not combined as below
351 sometimes in twos
352 sometimes in twos or threes
353 often in twos and threes, occasionally in fours
33. Attributes based on average of Hillis' usual diameter of primary utricles
354 0-24 u
355 25-50 u
356 50-75 u
357 75-175 u
34. Same as characteristic 33 but based on length dimensions
358 25-50 u
359 51-125 u
360 126-225 u
- 35 As in 33 but diameter of secondary utricles proximally bordering medulla
361 30-40 u
362 40-50 u
363 70-80 u
364 90-130 u
36. As in 35 but diameters of secondary utricles within cortex
365 20-29 u
366 30-49 u
367 50-79 u
368 80- u
- 37 As in 35 but lengths of secondary utricles when not in cortex
369 50-200 u
370 200- u
38. As in 35 but lengths of secondary utricles within the cortex
401 40-48 u
402 49-59 u
403 60-69 u
404 100- u

39 As in 35 but diameters of tertiary utricles

- 405 46-55 u
- 406 56-65 u
- 407 76-86 u
- 408 86- u

40. Behavior of nodal medullary filaments

- 409 separate (may adhere slightly)
- 410 all together
- 411 occasionally separate, but more commonly in 2's and 3's
- 412 uniting in pairs
- 413 commonly in twos but occasionally in threes, rarely fours
- 414 uniting in twos, threes and fours
- 415 uniting in twos and threes and larger units

41. Length of fusion of nodal medullary filaments

- 416 one and a half or several times filament length
- 417 several times filament diameter
- 418 39-49 u
- 419 50-59 u
- 420 60-69 u
- 421 no fusion

42. Association of filaments at node

- 422 entangled
- 423 not entangled or but little

43. Nature of attachment of nodal medullary filaments

- 424 free or laterally adhering
- 425 adhering for at least 40 u
- 426 short tubular attachments
- 427 pore connection

44. Nature of filament cell wall in nodal area

- 428 thickened and pigmented
- 429 not thickened and pigmented

45. Geographical distribution

- 430 Pan tropical
- 431 Indo-Pacific
- 432 Western Atlantic
- 433 Western Atlantic and Pacific
- 434 Pacific

46 Climatic zonation

- 435 tropical (exclusively)
- 436 subtropical (mainly)
- 437 both tropical and subtropical

47. Nature of substratum

- 438 among rocks
- 439 among rocks and sand
- 440 among stones and sand
- 441 among rocks, gravel and sand
- 442 among sand
- 443 among mud and sand

48. Depth distributional range

- 444 low tide mark and shallow water
- 445 low tide mark to 20 meters
- 446 low tide mark to 40 meters
- 447 low tide mark and over 40 meters
- 448 shallow water
- 449 shallow water or near low tide mark to 20 meters
- 450 shallow water or near low tide mark to 21 to 30 meters
- 451 shallow water or near low tide mark to 31 to 40 meters
- 452 shallow water or near low tide mark to 41 to 60 meters
- 453 only 46 meters
- 454 moderately deep or deep water to at least 55 meters
- 455 four or six primary utricles (refer to characteristic 29)

TAXIMETRICS LABORATORY
DEPARTMENT OF BIOLOGY
ARMORY 101
UNIVERSITY OF COLORADO
BOULDER, COLORADO 80302

16 April 1968

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

Dear Mr. Rhyne:

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

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

Sincerely,

David J. Rogers
Professor of Biology

DJR:gm
CC: Dr. Llewellyn H. Collinvaux

Dear Llewellyn,

What's new? We have not heard from you for a long time.
Please note our present address.

List of characters found + copy mailed 22 April

July 21, 1966

Dr. Illewellya H. Colinvaux
Botany and Plant Pathology
College of Biological Sciences
The Ohio State University
Columbus, Ohio 43210

Dear Illewellya:

It was good to hear from you in your letter of the first. I should have answered sooner but I am just back from Mexico.

I would like to suggest the following as a procedure for us with regard to Halimeda. We have developed a completely new computer program for clustering of organisms which we find to be much more satisfactory than the one we had used earlier on your data. I am enclosing a reprint of an article which describes this new methodology, at least as far as the biological aspects of the program are concerned. This method gives much clearer pictures of the structure and hierarchical organization than we got with the other program, and you can now get a much better idea of the inner relations between your species and subspecific categories than we ever had before. With this in mind, would it not be a good idea to put all of the species data onto cards, run it through this program, and then you would have an overall interrelationship between each of the entities which you wish to recognize?

We have also become a bit more sophisticated on the method of use of characters and how to set up the characters. We can now come a little closer to reflecting the taxonomist's desires and thoughts about the characters that he uses, and I think that it would be interesting to go back and do the whole thing again. This is not to suggest that the work that has already been done in coding up the species that you had already wrapped up in the monograph need to be completely recoded. The characters as they were set up can still be used, but the way they are set up can be modified to fit the new conditions. It is probably the case that you have had an opportunity to discover that the characters as we use them need some sort of modification, however. We no longer use the Port-A-Punch coding device or recording device because it is too much difficulty. We just use a standard card format input that I will show you if you care to go ahead with this. The advantage is to this process is that you can go to the new species or new material that you have and incorporate

whatever data is available on them in the same manner and run it right through. I would suggest that the whole process should not take very long because you can probably just go to the new material rather quickly.

Unfortunately, we can no longer code the data for the specimens for you. This is something that we are asking all of our collaborators to do for themselves. What we will do is to provide you with aid in setting up the characteristics and punching the cards, and running the program. After the program is run, we can suggest a method of interpretation that should be put upon it. If you are interested in this process, we will be glad to go ahead on that basis. I certainly hope you are because we have a warm spot in our heart for Halimeda. We are looking forward to having your affirmative reply.

Sincerely,

David J. Rogers
Professor of Botany

DJR:ch

Enc.

THE OHIO STATE UNIVERSITY
COLUMBUS, OHIO 43210

~~XXXXXXXXXXXXXXXXXXXXXXXXXXXX~~
~~XXXXXXXXXXXXXXXXXXXXXXXXXXXX~~

College of Biological Sciences

BOTANY AND PLANT PATHOLOGY
BOTANY AND ZOOLOGY BUILDING
1735 NEIL AVENUE

July 1, 1966

JUL 8 1966

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

Dear Dave:

There is one aspect of my current research on Indian Ocean *Halimeda* that I think might particularly interest you: the *H. opuntia* complex. I do not know yet--and this is probably good from your point of view--exactly how I am going to handle it--but splitting seems imminent, possibly into several species.

If you are interested I should be happy to send you my records from time to time as they are completed. Accumulation of them is slow, however, partly because I need to sift facts in my own mind so I can make a critical not just routine examination, and also because material of some entities is restricted. I have only a fragment, or nothing at all so far, of some groups I hope to include. Probably various herbaria will help out here; I am currently expecting some likely specimens from the New York Botanical Gardens, and will look for them when I visit various European herbaria later this summer.

With best wishes,

Yours sincerely,

Llewellyn

Llewellyn H. Colinaux
Assistant Professor

LHC:mw

April 1, 1964

Dr. Llewellyn H. Colinaux
Department of Biology
Yale University
New Haven, Connecticut

Dear Llewellyn:

Here is the paper we've been referring to off and on for many months. This is a very rough draft--full of grammatical errors--"data is"--for example. We make no excuses for these, but we do know they're there.

Will you kindly go through the paper for any corrections you can make? First off, I think we may not have handled your name correctly. If you can suggest how we should do it, please do not fail to indicate the correct procedure. Anything else that you care to say about the wording, the concepts, etc., will be welcomed. So use your red pencil with complete freedom.

We're asking all kinds of people to read the paper because it is in such an interdisciplinary field, we cannot ourselves be certain that we have seen ~~all~~ fundamental errors. There are already a few feedbacks that have made us change some of the things, but nothing so far has modified our basic model. And yet, it is in the conceptualization of this basic model that we need to have the most thought and criticism. As a taxonomist, you will probably be able to follow the basic argument, even though some math has appeared here and there. We make no bones about its mathematical elegance, for here it is probably gauche, but we are not writing this paper for mathematicians--its going to biologists, and for this purpose, we want to submit it to BIOSCIENCE, the publication of AIBS.

(in its present state)

Since this is not for publication, I would not want it to get too far from your hands, though if you can talk your husband into reading it, that will be fine.

Hope you find it interesting, and we shall look forward to your comments.

Sincerely,

David J. Rogers
Curator of Quantitative Taxonomy

Yale University *New Haven, Connecticut*

Department of Biology

Osborn Memorial Laboratories

11-11-64

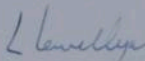
Dear Dave

The Port-a-Punch cards and gadget have arrived. Thanks for the suggestions on habit. I will indeed do what I can, but first on my own Indian Ocean collections since the Papenfuss and Scagel ones have yet to be sorted out and may well not arrive until summer.

I am most interested in progress on the manuscript and hope the "last throes" of first drafting were not too agonizing. I will be glad to help where I can.

With best wishes

Sincerely



Llewellyn H. Colineux

February 28, 1964

Dr. Llewellyn Colinvaux
Department of Biology
Osborn Memorial Laboratories
Yale University
New Haven, Conn.

Dear Llewellyn:

Henry sent you a batch of Port-a-Punch cards and the gadget to do the punching. You have, I think, already received the list of characters and attributes as we prepared them.

You will note, of course, that we left off the habit character, and we were sorry to have to do that, but at the time we were just not secure in how to handle it. We hope that one of the things that you can do will be to wire this character down a little better. Just as a suggestion, you might think of making not one character out of it, but of making several characters to describe the habit. One might be the internode distances; another might be the angles of the branching; another might be the "density" of the plants. These suggestions are made merely with the hope that you can find some way to get the valuable information into a form similar to what we have made of your data already. I hope you will keep in touch with us on your progress in this respect. We are very concerned, as you already know, about Halimeda and would be interested to see what comes from your new data from the specimens that you have received from Papenfuss.

We are in the last throws of the first draft of the big manuscript. When we have gotten it into a sufficiently satisfactory form, we will probably have it mimeographed and sent to knowledgeable and interested people for criticism before publishing.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

Yale University *New Haven, Connecticut*

Department of Biology

Osborn Memorial Laboratories

21-11-64

Dear Dave

I hope your trip to Washington planned for the day after my visit to the Botanic Gardens was successful -- though it could not have been more pleasant than my visit to the Gardens.

I am becoming increasingly anxious to get on with my proposed study of Indian Ocean *Halimedes* but have heard nothing from O.M.R. about support. I had hoped it would start Jan. 1. Do you think the grant processing might be speeded up in any way? I remember you mentioned Mr. Flynn, but he was out when we phoned that day.

I have just been offered the Papenfuss-Scagel collection of *Halimedes* made in the Indian Ocean a year ago, mainly along African shores -- and am delighted. When I begin recording characteristics I should like to use the Port- a -Punch system. My spirit rebels at absolute reliance on note-writing techniques.

Sincerely yours

Llewellyn

February 5, 1964

Dr. Llewellyn H. Colinvaux
Biology Department O.M.L.
Yale University
New Haven, Connecticut

Dear Dr. Colinvaux:

Enclosed herewith is check in the amount of \$50 to cover consultation fee for your recent visit to New York. We very much appreciated your help.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

December 10, 1963

Dr. Llewellyn H. Colinvaux
Zoology Department
Yale University
New Haven, Connecticut

Dear Dr. Colinvaux:

We have no idea of exactly your present locality,
but I suspect it is "somewhere in the Indian Ocean."

We have run through the Halimeda. We have a tremendous print-out, and we think that we have come fairly close to reflecting your taxonomy. There are some very interesting facets of the print-out, and it would certainly be helpful to us to have you go over them with us if you happen to be returning to this area in the near future.

Sincerely yours

David J. Rogers
Curator of Quantitative Taxonomy

DJR:MDF

26. August 1963

Dear Dr. ROGERS

I have had accepted for publication a paper on a lovely new species of Halimeda. It should appear late this year, but if seeing the manuscript in advance would be of interest in the computer project, I will have a copy made gladly. I might add - I have incorporated some suggestions made by you and Mr. Fleming last year - into the description.

This summer has been a profitable and full one for both of us. Paul has returned from a successful Expedition in the Proterops. I have made some advances with my new study of calcification in Halimeda. We will leave here - probably about mid-September. I will shortly thereafter begin my trip to the Indian Ocean. Most reachable address for next year is:

Zoology Dept.
Yale Univ.
New Haven, Conn.

With best wishes

Cordially
Llewellyn H. Calverton

ans in
9/4

Montana State University

College of Agriculture

Agricultural Experiment Station
Bozeman, Montana 59715 Tel. 406-587-3121

Plant and Soil Science Department

8 March 1971

Dr. D. J. Rogers
Taximetrics Laboratory
Dept. of Biology
University of Colorado
Boulder, CO 80302

Dear Dave:

The clean skyline and your letter arrived here safely together today, and many thanks.

Progress is being made on the thesis, but not necessarily at high speed, and I'm not sure whether I'll be ready to take it down to Claremont before the end of our winter quarter, which happens the end of next week. The subgraph for the std. dev. has been done and the alternate one for the arbitrary scoring method is nearly done. I have a grad student drafting the plates from the std. dev. graph printout, so things are moving a bit. In the meantime, I'm working on income tax return which, for me, is moderately complex.

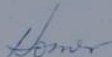
Claremont requires a visiting examiner on doctoral finals, at the expense of he who is being examined. This would get you a free trip to Claremont, but I don't know exactly when. It would likely be some Friday this spring, as the forepart of the week will be taken up with classes for me, and I would be able to get away before Thursday noon to fly south. I believe the final is essentially a defense of the thesis.

I'm sorry that the Nigerian trip was less fruitful than you hoped, but keep trying. Something has to break!

Would you, by any chance, be interested in the headship of our Dept. at Bozeman? It's open, the incumbent requesting to be relieved by 1 July, but not retiring. There's about a 40-man staff here + 6 branch stations scattered about the state. Areas of endeavor include agronomy, horticulture, soils and recreational area management. There's a lot of emphasis on breeding of small cereal grains and forages at present, but, I feel, a need for attention to development of alternative crops. Can get your name in the pot if you'd like. I think some new points of view, such as could be provided by an economic botanist, would be beneficial. Dollar supply is not munificent, but there are some.

Best wishes,

Sincerely,



Taximetrics Lab.

Mar. 4, 1971

Mr. Homer Metcalf
Plant and Soil Science Dept.
Montana State University
Bozeman, Montana 59715

Dear Homer:

Herewith the charges for your runs and some consulting for Henry. The final, clean skyline is in the mail today. The only suitable container to keep it in good shape turned out to be the rather large box.

The trip to Nigeria was hard, and for us, not too productive. I had hoped to get the Ford Foundation (and the Rockefeller) to cooperate with us on some research, but didn't fare too well.

Things are beginning to get back to "normal", whatever that means. I still have much work to do.

Yes, I will be glad to serve on your committee as an outside individual. Let me know what that entails, if other than reading your paper.

Sincerely,

March 4, 1971

Charges for Taximetrics runs to Homer Metcalf.

Computer costs:

8 runs	\$146.79
mailing	2.00
	<u>\$148.79</u>

Consulting for Henry Fleming	<u>\$200.00</u>
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Total charges	\$348.79
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David J. Rogers

Montana State University

College of Agriculture

Agricultural Experiment Station

Bozeman, Montana 59715 Tel. 406-587-3121

Plant and Soil Science Department

27 February 1971

Dear Henry:

I got back to Bozeman OK on Tuesday evening, and found the desk piled high with mail which I've spent these last few days clearing away. It's nearly all taken care of, except some requests for scions from the cherry orchard in the Flathead, the shipment of which I'll have to arrange thru the caretaker there.

From Cheyenne to a bit south of Wheatland, the road was very icy and there was a ground blizzard blowing. Hence, I was glad to get to Casper on Tuesday evening. On Wednesday, I stopped to see a county agent at Hardin and at the branch exp. sta. at Huntley, east of Billings, so it was early evening before I made it back to Bozeman. I was really appalled at how much mail and stuff had accumulated in 10 days absence.

I think it would be wise if you'd get me a clean skyline from the std. dev. cards you have on file. I wouldn't need the graph part right now, but perhaps you should hang onto it in event I do. Please send the skyline airmail, and I'll reimburse the costs.

After you get the skyline, please get together with Dave on a bill for computer charges and services rendered. I'll remit promptly.

Ever so many thanks for your help and hospitality. You'll be better compensated one of these fine days.

I hope Dave's trip to Nigeria was a success.

Please tell Dave that the paper about archeological discoveries relating to agriculture in the Nile Valley appeared in an issue of Science that came out last September.

If arrangements can be worked out, I'd like Dave to be the "outside examiner" on my final at Claremont. The date is yet indefinite.

Sincerely,

Montana State University

College of Agriculture

Agricultural Experiment Station

Bozeman, Montana 59715 Tel. 406-587-3121

Plant and Soil Science Department

17 October 1970

ams- 10/24

Dear Dave:

Don't believe we've communicated since I was in Boulder last February, and am wondering how things are going with you and the Taximetries Lab.

I've been silent because there's been no opportunity to do anything with the printout from the Charanal program since I returned. Now, however, the growing season is revving down, and I'm not teaching this autumn or winter, so have an opportunity to get back to iris thesis. It's more than an opportunity; in fact, it's a necessity, as the allowable time for finishing the thesis runs out next July, and Lenz is prodding me from Claremont to get it done. It's really necessary that I wind up activities on it no later than mid-March next, as I have 1½ courses to teach in the spring quarter. I'm eligible for an 8th quarter leave, so time should not now be a problem. This means that I can get winter quarter off with full pay, and would be able to run to California with the thesis and spend sufficient time to do a good job on the final exam on the dissertation, etc.

Now, I have two questions to ask you. Are you set up so that you'd be willing to continue working with me on this long-drawn-out venture that was embarked upon originally 14 years ago this fall? Needless to say, I've been most appreciative of the time & effort that you and your associates have devoted to me. You may well have grown tired of my seeming procrastination. Second, assuming that you're still in operation, is there any time in November that would be amenable to your schedule when I could come down to Boulder for further work? I would hope to have worked through the Charanal material before coming to Boulder. I would be taking annual leave time to do this.

It seems desirable to me that I seek to include as complete an analysis of available data in the thesis as may be possible. In other words, I'd like to essentially wrap up work on this group of iris in the thesis, without having much in the way of leftovers to worry about post-thesis.

It would be another week or 10 days before I could get "shook loose" from the fieldwork and meetings schedule here. There are 3 in-state conferences in the next 10 days in which I'm involved to greater or lesser degree.

This old typewriter is about to fall apart, but am hoping it will last thru the thesis.

Sincerely,

Samuel Thetcal

Montana State University

College of Agriculture

Agricultural Experiment Station
Bozeman, Montana 59715 Tel. 406-587-3121

Plant and Soil Science Department

21 February 1970

Dr. D. J. Rogers
Taximetries Laboratory
Armory 101
University of Colorado
Boulder, CO 80302

Dear Dave:

No sooner had I written you than the rest of the computer print-out arrived (on Thursday), so my apologies for the inquiry.

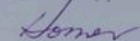
Enclosed are checks for \$51.65 made out to the Taximetries Lab, and for \$50 made out to Bill Carson. The postage costs have been included in the Lab's check, since you didn't indicate otherwise. Henry's \$\$ have to come from an out-of-town savings account, so will take a few days to retrieve, but will be sent just as soon as possible, and could reach him by the end of next week. I'm pleased to provide a little solid support for the Lab and staff.

It's taken me all week to uncover the desk from the accumulation of a week's absence, but that is now fairly well done. Next thing up is putting together the annual report of progress for the Dept's annual planning sessions the first week in March. Still have some work to do on that, but hope to clear it this weekend, as the secretaries need some lead time to type, duplicate and assemble the reports. Some years they've weighed 5 lb/copy, but not this time. As soon as this is put to bed, I'll be on the Caramel.

We got back to Bozeman OK on Friday night, following the Laramie-Rawlins-Lander-Riverton-Cody route, with a diversion to Billings to shop a bit, making it nearly 1700 mi. round trip in all. Al Fulton was a great help in driving & I think he found some motivation in seeing advanced math being applied to biological problems.

Enclosed is announcement of another meeting that might be of interest to you.

Sincerely,



H. N. Metcalf
Professor of Horticulture

hnm

Taximetrics Lab.

December 19, 1970

Mr. Homer Metcalf
Montana State University
Bozeman, Mont.

Subject: Charges for computer and taximetric services:

5 Computer runs	115.00
Consulting, Henry Fleming, 2 days,	200.00
Consulting, S. G. Appan, $\frac{1}{2}$ day	50.00
	<u>\$ 365.00</u>

Make check payable to the Taximetrics Lab.

David J. Rogers, Director
Taximetrics Lab.

Taximetrics Lab.

December 19, 1970

Mr. Homer Metcalf
Montana State University
Bozeman, Mont.

Subject: Charges for computer and taximetric services:

5 Computer runs	115.00
Consulting, Henry Fleming, 2 days,	200.00
Consulting, S. G. Appan, $\frac{1}{2}$ day	50.00
	\$ 365.00

Make check payable to the Taximetrics Lab.

David J. Rogers, Director
Taximetrics Lab.

February 18, 1970

Mr. Homer Metcalf
Plant and Soil Science Dept.
Montana State University
Bozeman, Montana 59715

Dear Homer:

Below is a summary of the various charges for consulting and computing for your last visit.

Henry Fleming, 3 days consulting, @ \$100.00/day	\$300.00
(Make check payable to him).	
William Carson, 4 hours program consulting,	
@ \$12.50/hour (also check direct to him)	50.00
Computing costs, 3 runs	\$50.00
(Pay to Taximetries Lab.)	
Mailing of print-out, parcel post,	1.65

I trust that you learned what you needed to from this work. I would rather not charge a colleague for such, but this is a fact of life.

Sincerely,

David J. Rogers
Prof., Project Director

Henny

We worked thru Level 10 on Sat AM before heading north. For the most part, things appear quite reasonable.

We are leaving one set of iris information to facilitate communication with the team here regarding results of the analysis. There is one set based on arbitrary scores + another on std. dev.

Many thanks to everyone for everything; + best wishes for the future of the Taxometrics Lab + Camp Inc. Let me know where the money is to.

Henny - Charles

Key to Scores, Based on Arbitrary Intervals, Recorded on
Computer Data Sheets for Longipetalan Irises

Column

1 Leaf-base color (qualitative score)

- 0 -no data
- 1 -white
- 2 -white-pink or "pale"
- 3 -pink
- 4 -reddish-pink, or red-pink
- 5 -red
- 6 -mixed

2 Scape branching

- 0 -no data
- 1 -no branching
- 2 -none to vestigial
- 3 -vestigial branched
- 4 -branching prominent

Mean: 4.96

Range: 0.00-68.00

3 Signal (qualitative score)

- 0 -no data
- 1 -signal intensity much below average
- 2 -signal intensity below average
- 3 -signal intensity average
- 4 -signal intensity above average
- 5 -signal intensity much above average

Mean: 1.86

Range: 1.00 - 3.64

4 Number of flowers per scape

- 0 -no data
- 1 -much below average
- 2 -below average
- 3 -average
- 4 -above average
- 5 -much above average

Mean: 2.68

Range: 1.96 - 5.24

5 Scariousness of spathe valves

- 0 -no data
- 1 -much below average
- 2 -below average
- 3 -average
- 4 -above average
- 5 -much above average

Mean: 2.00

Range: 1.00 - 5.72

Column

- 6 Spathe valve width
- 0 -no data
 - 1 -much below average
 - 2 -below average
 - 3 -average
 - 4 -above average
 - 5 -much above average
- Mean: 6.13 Range: 3.44 - 11.14
- 7 Spathe-valve length
- 0 -no data
 - 1 -much below average
 - 2 -below average
 - 3 -average
 - 4 -above average
 - 5 -much above average
- Mean: 58.91 Range: 42.00 - 118.68
- 8 Ratio: Spathe-valve length/Spathe-valve width (SVL/SVW)
- 0 -no data
 - 1 -ratio very low
 - 2 -ratio low
 - 3 -ratio average
 - 4 -ratio above average
 - 5 -ratio much above average
- Mean: 9.94 Range: 6.06 - 21.33
- 9 Spathe-valve separation ("S. Opp." in data sheets)
- 0 -no data
 - 1 -much below average
 - 2 -below average
 - 3 -average
 - 4 -above average
 - 5 -much above average
- Mean: 4.67 Range: 0.64 - 32.64
- 10 Sepal Length
- 0 -no data
 - 1 -much below average
 - 2 -below average
 - 3 -average
 - 4 -above average
 - 5 -much above average
- Mean: 60.91 Range: 46.34 - 81.60

Column

11 Sepal width

- 0 -no data
- 1 -much below average
- 2 -below average
- 3 -average
- 4 -above average
- 5 -much above average

Mean: 21.66

Range: 14.96 - 29.20

12 Ratio: Sepal Length/Sepal width

- 0 -no data
- 1 -much below average
- 2 -below average
- 3 -average
- 4 -above average
- 5 -much above average

Mean: 2.82

Range: 2.33 - 3.38

13 Percentage sepals reflexed

- 0 -no data
- 1 -none reflexed
- 2 -proportion much below average
- 3 -proportion below average
- 4 -proportion average
- 5 -proportion above average
- 6 -proportion much above average
- 7 -100 % Sepals reflexed (no entry)

Mean: 54.3

Range: 0.00 - 92.00

14 Percentage sepals emarginate

- 0 -no data
- 1 -none emarginate
- 2 -proportion much below average
- 3 - " below average
- 4 - " average
- 5 - " above average
- 6 - " much above average
- 7 -100 % emarginate

Mean: 50.1

Range: 0.00 - 100.00

Column

- 15 Petal length
- 0 -no data
 - 1 -much below average
 - 2 -below average
 - 3 -average
 - 4 -above average
 - 5 -much above average
- Mean: 54.39 Range: 43.56 - 71.48
- 16 Petal width
- 0 -no data
 - 1 -much below average
 - 2 -below average
 - 3 -average
 - 4 -above average
 - 5 -much above average
- Mean: 10.81 Range: 7.28 - 14.84
- 17 Ratio: Petal length/Petal width
- 0 -no data
 - 1 -much below average
 - 2 -below average
 - 3 -average
 - 4 -above average
 - 5 -much above average
- Mean: 5.08 Range: 3.74 -7.11
- 18 Percentage petals emarginate
- 0 -no data
 - 1 -proportion much below average
 - 2 - " below average
 - 3 - " average
 - 4 -100 % emarginate
- Mean: 88.00 Range: 24.00 - 100.00
- 19 Style branch length
- 0 -no data
 - 1 -much below average
 - 2 -below average
 - 3 -average
 - 4 -above average
 - 5 -much above average
- Mean: 28.74 Range: 21.57 - 34.16

Column

- 20 Style crest length
- 0 -no data
 - 1 -much below average
 - 2 -below average
 - 3 -average
 - 4 -above average
 - 5 -much above average
- Mean: 11.43 Range: 8.60 - 17.44
- 21 Ratio: Style branch/ style crest
- 0 -no data
 - 1 -ratio much below average
 - 2 - " below average
 - 3 - " average
 - 4 - " above average
 - 5 - " much above average
- Mean: 2.54 Range: 1.82 - 3.35
- 22 Percentage stamens exerted
- 0 -no data
 - 1 -none exerted
 - 2 -proportion much below average
 - 3 - " below average
 - 4 - " average
 - 5 - " above average
 - 6 - " much above average
 - 7 - 100 % exerted
- Mean: 40.3 Range: 0.00 - 96.00
- 23 Percentage stigmas bilobed
- 0 -no data
 - 1 -proportion much below average
 - 2 - " below average
 - 3 - " average
 - 4 -100.00% bilobed
- Mean: 91.30 Range: 40.90 - 100
- 24 Ovary length
- 0 -no data
 - 1 -much below average
 - 2 -below average
 - 3 -average
 - 4 -above average
 - 5 -much above average
- Mean: 18.90 Range: 12.28 - 33.28

Column

- 25 Tube length
0 -no data
1 -much below average
2 -below average
3 -average
4 -above average
5 -much above average
Mean: 9.49 Range: 6.04 - 17.60
- 26 Ratio: Ovary/Tube
0 -no data
1 -much below average
2 -below average
3 -average
4 -above average
5 -much above average
Mean: 2.04 Range: 1.34 - 4.40
- 27 Pedicel length
0 -no data
1 -much below average
2 -below average
3 -average
4 -above average
5 -much above average
Mean: 36.97 Range: 15.36 - 62.00
- 28 Ratio: Spathe-valve length/Pedicel
0 -no data
1 -ratio much below average
2 - " below average
3 - " average
4 - " above average
5 - " much above average
Mean: 1.68 Range: 0.76 - 3.14
- 29 Ratio: Spathe-valve width/Pedicel
0 -no data
1 -ratio much below average
2 - " below average
3 - " average
4 - " above average
5 - " much above average
Mean: 0.181 Range: 0.019 - 0.338

Table _____. Frequency distribution, by population means, for scored signal intensities, taximetric analysis of variability in Iris, series Longipetalae.

Mean signal intensity scores						Total
1.00- 1.49	1.50- 1.99	2.00- 2.49	2.50- 2.99	3.00- 3.49	3.50- 3.99	
011	003	001	026	027	029	
012	005	002	032	070		
015	006	004	053	071		
016	009	007	059	096		
017	013	010	094			
018	014	021				
019	037	024				
020	039	025				
022	040	028				
023	042	030				
033	045	031				
034	047	035				
036	049	044				
038	050	048				
041	056	051				
043	060	052				
046	067	054				
066	073	055				
068	074	057				
069	076	058				
072	077	061				
085	078	063				
086	079	064				
087	080	065				
088	082	075				
089	097	081				
090		083				
091		084				
092		095				
093		098				
		099				
		100				

Mean: 1.86
Range: 1.00-3.64

n						
30	26	33	5	4	1	99
Scores 1	2	3		4		

Table _____. Frequency distribution, by population means, for number of flower per scape, for taximetric analysis of variability in Iris, series Longipetalae.

Mean number of flowers per scape									Total
~~1.00-~~ ~~1.49~~	1.50-	2.00-	2.50-	3.00-	3.50-	4.00-	4.50-	5.00-	
~~1.49~~	1.99	2.49	2.99	3.49	3.99	4.49	4.99	5.49	
	002	005	001	007	017	015	none	022	
	021	006	003	008	019				
	079	011	004	018	025				
	100	014	009	020					
		023	010	024					
		026	012	035					
		027	013	042					
		029	016	043					
		030	028	044					
		031	033	047					
		032	034	055					
		037	036	057					
		038	045	059					
		039	048	060					
		040	049	064					
		041	050	081					
		066	051	084	087				
		067	052	089					
		071	053	092					
		073	054	093					
		074	056						
		075	058						
		076	061						
		077	063						
		078	065						
		082	068						
		085	069						
		086	070						
		090	072						
		091	080						
		094	083						
		095	088						
		096							
		097							
		098							
		099							

Mean: 2.68
Range: 1.96 - 5.24

n	4	36	34	21	3	1	0	1	100
Scores	1	2	3	4	5				

Table _____. Frequency distribution, by population means, of scores for scariousness of spathes, for taximetric analysis of variability in Iris, series Longipetaleae.

Mean scores for scariousness of spathes						Total
1.00- 1.49	1.50- 1.99	2.00- 2.49	2.50- 2.99	3.00- 3.49	3.50- 3.99	
015	004	006	022	001	002	
016	013	010	024	003		
017	026	012	067	005		
018	031	014	068	007		
019	032	025	059	009		
020	033	027	092	011		
021	035	028	098	023		
036	041	030	099	029		
037	042	034	100	058		
038	045	043		066		
039	047	044		095		
040	057	049				
046	059	050				
048	060	053				
051	065	055				
052	070	056				
054	072	071				
061	073	082				
062	074	083				
063	076	088				
064	079	089				
075	080	093				
077	081	094				
078	084	097				
085	086					
087	091					
090	096					
						Mean: 2.00
						Range: 1.00-3.72
n						
27	27	24	9	11	1	99
scores1	2	3	4	5		

Table _____. Frequency distribution, by population means, for width of spathe-valves in mm., for taximetric analysis of variability in Iris, series Longipetalae.

Mean spathe-valve width, mm.									
3.00-	4.00-	5.00-	6.00-	7.00-	8.00-	9.00-	10.00-	11.00-	
3.99	4.99	5.99	6.99	7.99	8.99	9.99	10.99	11.99	Total
039	023	002	001	076	004	017	084	none	015
090	034	005	003	077	008				
091	036	011	006	078	024				
	040	012	007	079	025				
	085	014	009	095	027				
	086	018	010	096	028				
	088	032	013		033				
	092	037	016		046				
	093	038	019		048				
		043	020		052				
		047	021		054				
		051	022		063				
		058	026		070				
		059	029		094				
		061	030		097				
		062	031						
		066	035						
		067	041						
		068	042						
		069	044						
		072	045						
		073	049						
		080	050						
		081	053						
		082	055						
		083	056						
		087	057						
		089	060						
		098	064						
		099	065						
		100	071						
			074						
			075						

Mean: 6.13

Range: 3.44 - 11.14

Table _____. Frequency distribution, by populations, for mean spathe-valve length in mm., for taximetric analysis of variability in Iris, series Longipetalae.

Mean spathe-valve length, mm.								
40.00-	50.00-	60.00-	70.00-	80.00-	90.00-	100.00-	110.00-	Total
49.99	59.99	69.99	79.99	89.99	99.99	109.99	119.99	
001	005	067	004	008	038	019	015	017
002	006	068	010	037	039	022	016	
003	007	069	012	040	072		018	
011	009	070	021	085			020	
024	013	071	023					
025	014	073	036					
026	027	074	065					
028	029	075	077					
032	030	076	078					
033	031	080	079					
034	035	094	084					
041	043	096	086					
042	046		087					
044	048		088					
045	051		089					
047	052		090					
049	054		091					
050	055		092					
053	056		093					
059	057							
066	058							
081	060							
082	061							
083	062							
095	063							
097	064							
098								
099								
100								

Mean: 58.91
Range: 42.00 - 118.68



Table _____. Frequency distribution, by population means, for the ratio: spathe-valve length/
spathe-valve width, for taximetric analysis of variability in Iris, series Longipetalae.

Mean ratios: SVL/SVM											
6.00-	7.00-	8.00-	9.00-	10.00-	11.00-	12.00-	13.00-	14.00-	15.00-	16.00-	17.00-
6.99	7.99	8.99	9.99	10.99	11.99	12.99	13.99	14.99	15.99	16.99	17.99
025	001	006	005	010	087	089	092	017	016	085	020
028	002	009	008	012		093		019	023		040
033	003	011	015	014				022	036		090
042	004	013	021	061				037	072		091
053	007	029	034	073				038			
063	024	030	043					086			
097	026	032	051					088			
	027	035	058								
	031	055	062								
	041	056	065								
	044	064	067								
	045	066	068								
	046	070	069								
	047	071	076								
	048	074	077								
	049	075	078								
	050	081	079								
	052	083	080								
	054	096									
	057	098									
	059	099									
	060	100									
	082										
	084										
	094										
	095										

n

7

26

22

18

5

1

2

1

7

4

1

4

Scores:

1

2

3

4

5

4

Table _____. (cont.)

Mean ratios: SVL/SVM			
18.00-	19.00-	20.00-	21.00-
18.99	19.99	20.99	21.99
Total			
none	018	none	039

mean: 9.94
Range: 6.06 - 21.38

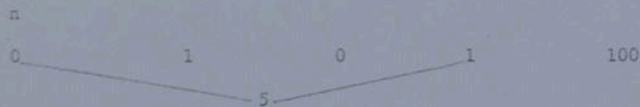


Table _____. Frequency distribution, by population means, for separation of spathe-valves in mm., for taximetric analysis of variability in *Iris*, series Longipetalae. (Original data listed under heading "S. Opp." in appendix data tables).

Mean separation of spathe-valves, mm.										
0.00-	2.00-		6.00-	10.00-	14.00-	18.00-	22.00-	26.00-	30.00-	Total
1.99	5.99		9.99	13.99	17.99	21.99	25.99	29.99	39.99	
001	004	050	085	010	015	018	none	022	020	019
002	007	052	087	024	017	038				
003	008	053	089	072	036	084				
005	011	054	090-091	073	051					
006	012	055	092	088						
009	014	056	093							
013	016	057	094							
021	023	058	095							
029	025	059	096							
032	026	060	097							
034	027	061	098							
067	028	062	099							
078	030	063								
080	031	064								
086	033	065								
100	035	066								
	037	068								
	039	069								
	040	070								
	041	071								
	042	074								
	043	075								
	044	076								
	045	077								
	046	079								
	047	081								
	048	082								
	049	083								

Mean: 4.67
Range: 0.64 - 32.64

Mean: 4.67
Range: 0.64 - 32.64

n

16

69

5

4

3

0

1

1

1

100

Table _____. Frequency distribution, by population means, for sepal length in mm., for taximetric analysis of variability in Iris, series Longipetalae.

Mean sepal length, mm.									Total
46.00- 49.99	50.00- 53.99	54.00- 57.99	58.00- 61.99	62.00- 65.99	66.00- 69.99	70.00- 73.99	74.00- 77.99	78.00- 81.99	
045	002	004	001	005	067	017	015	039	
047	003	007	011	006	071	020	016		
049	009	013	012	008	072	038	019		
061	021	024	014	010	073	040	036		
062	022	029	027	018	075	065	037		
064	026	058	068	023	084	086	085		
	034	063	069	025	087	097			
	035	066	076	028	094				
	042	082	080	030	096				
	044	083	081	031	098				
	046		088	032					
	048		090	033					
	050		091	041					
	051			043					
	052			070					
	053			074					
	054			077					
	055			078					
	056			079					
	057			089					
	059			092					
	060			093					
				095					
				099					
				100					

Mean: 60.91
Range: 46.14 - 81.60



Table _____. Frequency distribution, by population means, for sepal width in mm., for taximetric analysis of variability in Iris, series Longipetalae.

Mean sepal width, mm.							
14.00- 15.99	16.00- 17.99	18.00- 19.99	20.00- 21.99	22.00- 23.99	24.00- 25.99	26.00- 27.99	28.00- 29.99
							Total
035	021	003	001	002	030	037	016
045	022	007	004	005	036	038	019
	049	009	008	006	039	098	
	061	010	011	015	041		
	083	012	018	017	070		
	090	013	023	020	071		
		014	025	028	074		
		024	026	031	075		
		029	027	033	079		
		034	032	040	084		
		044	042	043	085		
		046	048	054	087		
		047	052	057	094		
		050	053	058	097		
		051	059	063			
		055	060	065			
		056	066	067			
		062	072	069			
		064	080	073			
		068	081	076			
		082	089	077			
		088	091	078			
			092	086			
			093	095			
				096			
				099			
				100			

Mean: 21.66
Range: 14.96 - 29.20

n
 2 6 22 24 27 14 3 2 100
 Scores: 1 2 3 4 5

Table _____. Frequency distribution, by population means, for the ratio: sepal length/sepal width, for taximetric analysis of variability in Iris, series Longipetalae.

Ratio: sepal length/sepal width						Total
2.25- 2.49	2.50- 2.74	2.75- 2.99	3.00- 3.24	3.25- 3.49	3.50- 3.74	
002	003	001	012	010	090	
042	004	006	013	015		
052	005	008	014	035		
053	007	009	017	045		
054	016	011	018	068		
057	019	021	020			
058	026	022	023			
060	030	024	025			
062	034	027	032			
063	038	028	039			
074	041	029	040			
	046	031	065			
	047	033	072			
	048	036	073			
	049	037	083			
	051	043	085			
	055	044	086			
	056	050	088			
	059	061	091			
	064	067	092			
	066	075	093			
	059	077				
	070	078				
	071	080				
	076	081				
	079	082				
	087	084				
	094	089				
	098	095				
		096				
		097				
		099				
		100				

Mean: 2.82
Range: 2.33 -3.38

n						
11	29	33	21	5	1	100
Scores:	1	2	3	4	5	

Table _____. Frequency distribution, by population means, for the ratio: petal length/petal width, for taximetric analysis of variability in Iris, series Longipetalae.

Ratio: petal length/petal width								Total
3.50- 3.99	4.00- 4.49	4.50- 4.99	5.00- 5.49	5.50- 5.99	6.00- 6.49	6.50- 6.99	7.00- 7.49	
054	002	001	003	004	035	025	090	
	021	008	005	007	083			
	030	011	006	009				
	042	017	012	010				
	047	023	013	014				
	048	028	016	015				
	052	029	019	018				
	053	036	020	024				
	057	037	022	026				
	063	038	027	032				
	069	041	031	049				
	074	046	033	068				
	087	051	034	072				
		055	039	080				
		056	040	084				
		058	043	088				
		060	044	095				
		062	045	096				
		064	050					
		066	059					
		067	061					
		070	065					
		071	075					
		073	076					
		079	077					
		085	078					
		092	081					
		093	082					
		094	086					
		098	089					
			091					
			097					
			099					
			100					

Mean: 5.08
Range: 3.74 - 7.11

	n								
	1	13	30	34	18	2	1	1	100
Scores:	1	2	3	4	5				

Table ____ . Frequency distribution, by population means, for style branch length in mm., for taximetric analysis of variability in Iris, series Longipetalae.

Mean style branch length, mm.								
19.00-	21.00-	23.00-	25.00-	27.00-	29.00-	31.00-	33.00-	
20.99	22.99	24.99	26.99	28.99	30.99	32.99	34.99	Total
044	045	002	001	005	021	016	015	
	047	003	004	010	024	017	020	
	049	009	006	011	025	018	065	
	050	034	007	022	027	019	067	
		035	008	023	029	028	071	
		046	012	026	030	037	072	
		048	013	086	031	038	073	
		051	014	090	032	039	084	
		052	033	091	036	041	092	
		053	042		040	066	096	
		057	054		043	070	097	
		059	055		063	074	099	
		061	056		068	075	100	
		062	058		069	076		
			060		079	077		
			064		081	078		
					082	080		
					085	094		
					087	095		
					088	098		
					089			
					093			

Mean: 28.74

Range: 21.57 - 34.16

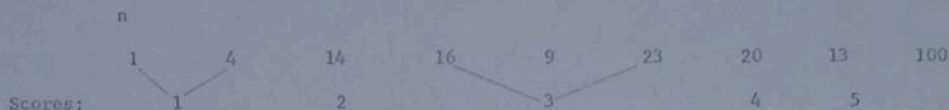


Table _____. Frequency distribution, by population means, for style crest length in mm., for taximetric analysis of variability in Iris, series Longipetalae.

Mean style crest length, mm.									Total
8.50- 9.49	9.50- 10.49	10.50- 11.49	11.50- 12.49	12.50- 13.49	13.50- 14.49	14.50- 15.49	15.50- 16.49	16.50- 17.49	
002	001	011	015	010	019	020	036	016	
003	004	014	017	018	038	040	037	039	
013	005	027	021	041	065	085			
024	006	028	023	071	073				
045	007	030	031	072	089				
047, 049	008	033	032	081					
061	009	042	043	086					
062	012	044	057	087					
076	022	048	070	092					
083	025	055	075	093					
	026	056	080	094					
	029	058	088	098					
	034	060	091						
	035	063	097						
	046	066							
	050	067							
	051	068							
	052	069							
	053	074							
	054	077							
	059	078							
	064	079							
	095	082							
	096	084							
		090							
		099							
		100							

Mean: 11.43
Range: 8.60 - 17.44

	n									
	11	24	27	14	12	5	3	2	2	100
Scores:	1	2	3		4		5			

Table _____. Frequency distribution, by population means, for the ratio: style branch length/style crest length, for taximetric analysis of variability in Iris, series Longipetalae.

Ratio: style branch length/style crest length							Total
1.75- 1.99	2.00- 2.24	2.25- 2.49	2.50- 2.74	2.75- 2.99	3.00- 3.24	3.25- 3.49	
016	010	002	001	013	029	024	
036	020	009	003	015	067	076	
039	037	014	004	025	074		
044	040	018	005	028	077		
	048	019	006	062	083		
	050	021	007	066	084		
	057	023	008	069	095		
	085	032	011	078	096		
	086	033	012	079	099		
	089	034	017	082			
		035	022	097			
		038	026	100			
		041	027				
		043	030				
		045	031				
		046	042				
		047	051				
		049	061				
		052	063				
		053	064				
		054	068				
		055	070				
		056	071				
		058	072				
		059	075				
		060	080				
		065	090				
		073	092				
		081					
		087					
		088					
		091					
		093					
		094					
		098					

Mean: 2.54
Range: 1.82 - 3.35

n							
	4	10	35	28	12	9	2
Scores:	1	2	3	4	5	6	100

Table _____. Frequency distribution, by population means, for perianth tube length in mm., for taxinetric analysis of variability in Iris, series Longipetalae.

Mean perianth tube length, mm.								
6.00- 6.99	7.00- 7.99	8.00- 8.99	9.00- 9.99	10.00- 10.99	11.00- 11.99	12.00- 12.99	13.00- 13.99	14.00- 14.99
090	008	002	001	006	074	none	none	none
091	016	004	003	007	075			
	017	012	005	010	078			
	018	013	009	024	079			
	022	015	011	028	083			
	023	019	014	031	094			
	085	020	025	033	099			
	086	026	027	042	100			
		032	029	048				
		034	030	053				
		036	035	056				
		037	039	057				
		038	041	058				
		040	043	059				
		044	045	067				
		047	046	070				
		061	049	076				
		063	050	077				
		065	051	081				
		073	052	095				
		092	054	096				
		093	055	097				
			060	098				
			062					
			064					
			066					
			068					
			069					
			071					
			072					
			080					
			082					
			084					
			087					
			088					
			089					

Mean: 9.49
Range: 6.04

n	2	8	22	36	23	8	0	0	h
Score	1		2	3	4	5			0

table ____ (cont)

Mean perianth tube length, mm.		
15.00-	16.00-	17.00-
15.99	16.99	17.99
Total		

none none 021 (= I. douglasiana)

0 0 1 100
6

Table _____. Frequency distribution, by population means, for the ratio: ovary length/perianth tube length, for taximetric analysis of variability in Iris, series longipetalae.

Ratio: ovary length/tube length							Total
1.25- 1.49	1.50- 1.99	2.00- 2.49	2.50- 2.99	3.00- 3.49	3.50- 3.99	4.00- 4.49	
007	001	010	008	016	019	018	
009	002	011	012	017	090		
021	003	014	013	037			
034	004	022	015	085			
049	005	023	020	086			
050	006	025	036				
056	024	026	038				
058	028	027	039				
059	029	030	040				
066	033	031	065				
	035	032	087				
	041	052	091				
	042	071					
	043	072					
	045	073					
	046	084					
	047	088					
	048	089					
	051	092					
	053	093					
	054						
	055						
	057						
	060						
	061						
	062						
	063						
	064						
	067						
	068						
	069						
	070						
	074						
	075						
	076						
	077						
	078						
	079						
	080						
	081						
	082						
	083						
	094						
	095						
	096						
	097						
	098						
	099						
	100						

Mean: 2.04

Range: 1.34 - 4.40

n
10 50 19 13 5 2 1 100

Table _____. Frequency distribution, by population means, for pedicel length in mm., for taxometric analysis of variability in *Iris*, series *Longipetalaa*.

Mean pedicel length, mm.						Total
15.00- 23.99	24.00- 31.99	32.00- 39.99	40.00- 47.99	48.00- 55.99	56.00- 63.99	
009	001	003	038	007	008	
030	002	004	039	016	015	
047	011	005	040	019	017	
067	012	006	043	022	018	
094	013	010	050	049	020	
096	021	014	055	058	023	
097	024	027	056	064	035	
099	025	033	059	093	037	
100	026	034	069		044	
	028	036	086		080	
	029	045	089			
	031	046	092			
	032	048				
	041	051				
	042	052				
	053	054				
	066	057				
	070	060	051			
	074	062				
	081	063				
	084	065				
	087	068				
	095	071				
	098	072				
		073				
		075				
		076				
		077				
		078				
		079				
		082				
		083				
		085				
		088				
		090				
		091				
n						
9	24	37	12	8	10	100
Scores	1	2	3	4	5	

Mean: 36.97
Range: 15.36-62.00

Table _____. Frequency distribution, by population means, for the ratio: spathe-valve length/pedicel length, for taximetric analysis of variability in *Iris*, series longipetalae.

Ratio: spathe-valve length/pedicel length						
0.50-	1.00-	1.50-	2.00-	2.50-	3.00-	Total
0.99	1.49	1.99	2.49	2.99	3.49	
007	003	001 079	012	009	099	
035	008	002 081	013	096		
044	023	004 088	016	097		
049	027	005 089	017	100		
080	033	006 090	021			
	034	010 091	024			
	037	011 095	030			
	043	014 098	031			
	045	015	067			
	050	018	072			
	055	019	084			
	056	020	085			
	058	022	087			
	059	025	094			
	060	026				
	063	028				
	064	029				
	068	032				
	069	036				
	082	038				
	083	039				
	086	040				
	092	041				
	093	042				
		046				
		047				
		048				
		051				
		052				
		053				
		054				
		057				
		061				
		062				
		065				
		066				
		070				
		071				
		073				
		074				
		075				
		076				
		077				
		078				

Mean: 1.68
Range: 0.76 - 3.14

n						
5	24	52	14	4	1	100
Scores	1	2	3	4	5	

Table : Frequency distribution, by population means, for the ratio: spathe-valve width/pedicel length, for taximetric analysis of variability in Iris, series Longipetalae.

Ratio: Spathe-valve width/pedical length.

0.000- 0.049	0.050- 0.099	0.100- 0.149	0.150- 0.199	0.200- 0.249	0.250- 0.299	0.300- 0.349	0.350- 0.399	Total
015	018 023 037 039 040 044 030 093	007 008 016 017 019 020 022 034 035 036 038 043 049 050 055 056 058 059 064 068 069 072 085 086 088 089 090 091 092	003 005 006 010 014 027 032 033 045 051 054 060 061 062 065 073 075 076 078 079 081 082 083 098	001 002 004 011 012 013 026 029 041 042 045 048 052 053 057 063 066 067 070 071 074 077 087 095	021 024 025 028 030 031 047 084	094 100	009 096 097 099	
n	8	29	24	24	8	2	4	100
Scores 1	2		3		4		5	

Mean: 0.181

Range: 0.019 - 0.338

Key to Scores. Based on Standard Deviations, Recorded on
Computer Data Sheets for Longipetalas Irises

Column

1 Leaf base color - scored as follows:

- 0 -no data
- 1 -white
- 2 -white-pink, or "pale"
- 3 -pink
- 4 -Reddish-pink or red-pink
- 5 -red
- 6 -mixed

2 Scape branching; scored by deciles

- 0 -no data
- 1 -no branching
- 2 -none to vestigial
- 3 - vestigial-branched
- 4 -branching prominent

Mean: 4.96

Range: 0.00-60.00

3 Signal

- 0 -no data
- 1 -mean - 2 s
- 2 - " - 1 s
- 3 - " + 1 s
- 4 - " + 2 s
- 5 - " + 3 s
- 6 - " + 4 s

Mean: 1.86

Range: 1.00 - 3.64 s = 0.54

4 Number of flowers per scape

- 0 -no data
- 1 -mean - 2 s
- 2 - " - 1 s
- 3 - " + 1 s
- 4 - " + 2 s
- 5 - " + 3 s
- 6 - " + 4 s (no entry)
- 7 - " + 5 s

Mean: 2.68

Range: 1.96 - 5.24 s = 0.55

5 Scariousness of spathes

- 0 -no data
- 1 -mean - 2 s
- 2 - " - 1 s
- 3 - " + 1 s
- 4 - " + 2 s
- 5 - " + 3 s

Mean: 2.00

Range: 1.00 - 3.72 s = 0.57

Column

6 Spatha valve width

0 -no data
1 -mean - 3 s
2 - " - 2 s
3 - " - 1 s
4 - " + 1 s
5 - " + 2 s
6 - " + 3 s
7 - " + 4 s
8 - " + 5 s

Mean: 6.13

Range: 3.44 - 11.14

s = 1.01

7 Spatha valve length

0 -no data
1 -mean - 2 s
2 -mean - 1 s
3 -mean + 1 s
4 -mean + 2 s
5 -mean + 3 s
6 -mean + 4 s

Mean: 58.91

Range: 42.00 - 118.68

s = 15.49

8 Ratio: Spatha valve length/Spatha valve width

0 -no data
1 -mean - 2 s
2 -mean - 1 s
3 -mean + 1 s
4 -mean + 2 s
5 -mean + 3 s
6 -mean + 4 s

Mean: 9.94

Range: 6.06 - 21.38

s = 3.29

9 Spatha valve Separation (S . OPP. on data sheet)

0 -no data
1 -mean - 1 s
2 -mean + 1 s
3 -mean + 2 s
4 -mean + 3 s
5 -mean + 4 s
6 -mean + 5 s
7 -mean + 6 s

Mean: 4.67

Range: 0.64 - 32.64

s = 5.17

Column

10

Sepal length

- 0 -no data
- 1 -mean - 2 s
- 2 -mean - 1 s
- 3 -mean + 1 s
- 4 -mean + 2 s
- 5 -mean + 3 s

Mean: 60.91

Range: 46.14 - 81.60

s = 8.16

11

Sepal width

- 0 -no data
- 1 -mean - 3 s
- 2 -mean - 2 s
- 3 -mean - 1 s
- 4 -mean + 1 s
- 5 -mean + 2 s
- 6 -mean + 3 s

Mean: 21.66

Range: 14.96 - 29.20

s = 2.81

12

Ratio: Sepal length/ Sepal width

- 0 -no data
- 1 -mean - 2 s
- 2 -mean - 1 s
- 3 -mean + 1 s
- 4 -mean + 2 s
- 5 -mean + 3 s

Mean: 2.82

Range: 2.33 - 3.38

s = 0.26

13

Percentage sepals reflexed

- 0 -no data
- 1 -none reflexed
- 2 -proportion much below average
- 3 - " below average
- 4 - " average
- 5 - " above average
- 6 - " much above average
- 7 -100 % sepals reflexed

Mean: 54.3

Range: 0.00 - 92.00

Column

14

Percentage sepals emarginate

- 0 -no data
- 1 -none emarginate
- 2 -proportion much below average
- 3 - " below average
- 4 - " average
- 5 - " above average
- 6 - " much above average
- 7 -100 % emarginate

Mean: 50.1

Range: 0.00 - 100.00

15

Petal length

- 0 -no data
- 1 -mean - .2 s
- 2 -mean - 1 s
- 3 -mean + 1 s
- 4 -mean + 2 s
- 5 -mean + 3 s

Mean: 54.39

Range: 43.56 - 71.48 s = 6.80

16

Petal width

- 0 -no data
- 1 -mean - 3 s
- 2 -mean - 2 s
- 3 -mean - 1 s
- 4 -mean + 1 s
- 5 -mean + 2 s
- 6 -mean + 3 s

Mean: 10.81

Range: 7.28 - 14.88 s = 1.67

17

Ratio: Petal length/ Petal width

- 0 -no data
- 1 -mean - 3 s
- 2 -mean - 2 s
- 3 -mean - 1 s
- 4 -mean + 1 s
- 5 -mean + 2 s
- 6 -mean + 3 s
- 7 -mean + 4 s

Mean: 5.08

Range: 3.74 - 7.11 s = 0.56

18

Percentage petals emarginate

- 0 -no data
- 1 -proportion much below average
- 2 - " below average
- 3 - " average
- 4 - " 100 % emarginate

Mean: 88.00

Range: 24.00 - 100.00

19

Style branch length

- 0 -no data
- 1 -mean - 3 s
- 2 -mean - 2 s
- 3 -mean - 1 s
- 4 -mean + 1 s
- 5 -mean + 2 s

Mean: 28.74

Range: 21.57 - 34.16 s = 3.59

20

Style Crest Length

- 0 -no data
- 1 -mean - 2 s
- 2 -mean - 1 s
- 3 -mean + 1 s
- 4 -mean + 2 s
- 5 -mean + 3 s
- 6 -mean + 4 s

Mean: 11.43

Range: 8.60 - 17.44 s = 1.85

21

Ratio: Style branch/Style crest

- 0 -no data
- 1 -mean - 3 s
- 2 -mean - 2 s
- 3 -mean - 1 s
- 4 -mean + 1 s
- 5 -mean + 2 s
- 6 -mean + 3 s

Mean: 2.54

Range: 1.82 - 3.35 s = 0.32

22

Percentage stamens exerted

- 0 -no data
- 1 -none exerted
- 2 -proportion much below average
- 3 - " below average
- 4 - " average
- 5 - " above average
- 6 - " much above average
- 7 - " proportion 100 % exerted (no entry)

Column

23

Stigma bilobed

- 0 -no data
- 1 -proportion much below average
- 2 - " below average
- 3 - " average
- 4 - " 100 % bilobed

Mean: 91.30

Range: 40.9 - 100.00

24

Ovary Length

- 0 -no data
- 1 -mean - 2 s
- 2 -mean - 1 s
- 3 -mean + 1 s
- 4 -mean + 2 s
- 5 -mean + 3 s
- 6 -mean + 4 s

Mean: 18.90

Range: 12.28 - 33.28

s = 3.74

25

Tube Length

- 0 -no data
- 1 -mean -3 s
- 2 -mean -2 s
- 3 -mean -1 s
- 4 -mean +1 s
- 5 -mean +2 s
- 6 -mean +3 s (no entry)
- 7 -mean +4 s (no entry)
- 8 -mean +5 s (no entry)
- 9 -mean +6 s

Mean: 9.49

Range: 6.04 - 17.60

s = 1.40

26

Ratio: Ovary/tube

- 0 -no data
- 1 -mean -2 s
- 2 -mean -1 s
- 3 -mean +1 s
- 4 -mean +2 s
- 5 -mean +3 s
- 6 -mean +4 s empty
- 7 -mean +5 s

Mean: 2.04

Range: 1.34 - 4.40

s = 0.57

Column

27 Pedicel length

0 -no data
1 -mean -2 s
2 -mean -1 s
3 -mean +1 s
4 -mean +2 s
5 -mean +3 s

Mean: 36.97

Range: 15.36 - 62.00 s = 11.15

28 Ratio: Spathe valve length/ Pedicel

0 -no data
1 -mean -3 s
2 -mean -2 s
3 -mean -1 s
4 -mean +1 s
5 -mean +2 s
6 -mean +3 s
7 -mean +4 s

Mean: 1.68

Range: 0.76 - 3.16 s = 0.46

29 Ratio: Spathe valve width/ Pedicel

0 -no data
1 -mean -3 s
2 -mean -2 s
3 -mean -1 s
4 -mean +1 s
5 -mean +2 s
6 -mean +3 s

Mean: 0.181

Range: 0.019 - 0.338 s = 0.069

Table : Leaf-base color scores, by population samples, used in taximetric analysis of variability in iris, series Longipetalae

Population number	Score	State of origin
001	1	Arizona
002	1	Arizona
003	1	Arizona
004	1	Arizona
005	1	Arizona
006	1	Arizona
007	1	Arizona
008	5	California (Cuyamaca)
009	5	California (Baldwin Lake)
010	5	California (Big Bear)
011	5	California (Mono Lake)
012	5	California (Bridgeport)
013	5	California (Mammoth Lakes)
014	5	California (Hilton Creek)
015	6	California (Lenz #9-001)
016	3	California (Carmel)
017	3	California (Rancho Blucher)
018	2	California (Mtn. House)
019	1	California (Chileno Valley)
020	3	California (Dillon Beach)
021	4	California (I. douglasiana)
022	1	California (Cordelia)
023	5	California (Gazelle)
024	1	Colorado
025	1	Colorado
026	1	Colorado
027	1	Colorado
028	1	Colorado
029	1	Colorado
030	1	Colorado
031	1	Colorado
032	1	Colorado
033	1	Idaho
034	1	Idaho
035	1	Idaho
036	1	Idaho
037	1	Idaho
038	1	Idaho
039	1	Idaho
040	1	Idaho
041	1	Idaho
042	1	Idaho
043	1	Idaho

<u>Population number</u>	<u>Seeds</u>	<u>State of origin</u>
044	1	Montana
045	1	Montana
046	1	Montana
047	1	Montana
048	1	Montana
049	1	Montana
050	1	Montana
051	1	Montana
052	1	Montana
053	1	Montana
054	1	Montana
055	1	Montana
056	1	Montana
057	1	Montana
058	1	Montana
059	1	Montana
060	1	Montana
061	1	Montana
062	1	Montana
063	1	Montana
064	1	Montana
065	5	Nevada (Ophir)
066	2	Nevada (Pack Creek)
067	4	Nevada (Scott Summit)
068	2	Nevada (Elko)
069	2	Nevada (Contract)
070	1	New Mexico
071	1	New Mexico
072	5	Oregon (Albion Rd.)
073	2	Oregon (Little Bear Creek)
074	1	South Dakota
075	1	South Dakota
076	1	South Dakota
077	1	South Dakota
078	1	South Dakota
079	1	South Dakota
080	1	Utah (Vernal)
081	1	Utah (Tooele)
082	2	Utah (Kaysville)
083	2	Utah (Alton)
084	1	Utah (La Sal Mtn.)

<u>Population number</u>	<u>Seeds</u>	<u>State of origin</u>
085	1	Washington (Pullman)
086	1	Washington (Union Flat Creek)
087	5	Washington (Ellensburg)
088	4	Washington (Douglas Co.)
089	4	Washington (Leahy)
090	2	Washington (Elbur)
091	1	Washington (Dearden)
092	5	Washington (Coulee City)
093	5	Washington (Selah)
094	1	Wyoming
095	1	Wyoming
096	1	Wyoming
097	1	Wyoming
098	1	Wyoming
099	1	Wyoming
100	1	Wyoming

Table _____. Frequency distribution, by population⁴ means, for percentage branched scapes, for taximetric analysis of variability in Iris, series Longipetalae.

Mean percentage branched scapes									
None	1.00- 9.99	10.00- 19.99	20.00- 29.99	30.00- 39.99	40.00- 49.99	50.00- 59.99	60.00- 69.99	Total	
016	048	077	017	005	004	002	025	001	007
018	049	078	029		024	006		003	
019	050	079	070		095			015	
020	051	080	084						
021	052	081	094						
022	053	082	096						
023	054	083	098						
026	055	085							
027	056	086							
028	057	087							
030	058	088							
031	059	089							
032	060	090							
033	061	091							
034	062	092							
035	063	093							
036	064	097							
037	065	099							
038	066	100							
039	067								
040	068								
041	069								
042	071								
043	072								
044	073								
045	074								
046	075								
047	076								

Mean: 4.96
Range: 0.00 - 68.00

n = 75 7 1 3 2 1 3 1 93
Scores 1 2 3 4

Table _____. Frequency distribution, by population means, for scored signal intensities, taximetric analysis of variability in *lris*, series Longipetalae. (Based on standard deviation)

Mean signal intensity scores						Total
.79	1.33	1.87	2.41	2.95	3.49	
1.32	1.86	2.40	2.94	3.48	4.02	
012	003	001	026	027	029	
015	005	002	028	070		
016	011	004	032	071		
017	013	006	053	096		
018	022	007	059			
019	037	009	075			
020	038	010	084			
023	039	014	094			
033	040	021				
034	041	024				
036	042	025				
043	046	030				
068	047	031				
069	049	035				
072	050	044				
085	066	045				
086	067	048				
087	073	051				
088	074	052				
092	077	054				
093	079	055				
	080	056				
	082	057				
	089	058				
	090	060				
	091	061				
		062				
		063				
		064				
		065				
		076				
		078				
		081				
		083				
		095				
		097				
		098				
		099	Mean: 1.86	s: 0.54		
		100	Range: 1.00 - 3.64			
n						
21	26	39	8	4	1	99
scores						
1	2	3	4	5	6	

Table _____. Frequency distribution, by population means, for number of flowers per scape for taximetric analysis of variability in Iris, series Longipetalae. (Based on standard deviation).

Mean number of flowers per scape								Total
~~1.04-~~ ~~1.58~~	1.59-	2.14-	2.69-	3.24-	3.79-	4.34-	4.89-	
	2.13	2.68	3.23	3.78	4.33	4.88	5.43	
	-3.2	-1.2	1.0	1.7	2.3	2.9	3.5	
	002	006	001	007	015		022	
	005	010	003	008	017			
	021	011	004	018				
	026	012	009	019				
	030	013	020	024				
	031	014	034	025				
	032	016	042	035				
	066	023	043	044				
	074	027	045	081				
	075	028	046					
	076	029	047					
	077	033	048					
	078	036	049					
	079	037	050					
	090	038	051					
	091	039	052					
	095	040	053					
	096	041	054					
	097	062	055					
	098	065	056					
	099	067	057					
	100	068	058					
		069	059					
		070	060					
		071	061					
		073	063					
		082	064					
		085	072					
		086	080					
		094	083					
			084					
			087					
			088					
			089					
			092	Mean: 2.68	s: 0.55			
			093	Range: 1.96	~ 5.24			
n								
0	22	30	36	9	2	0	1	100
scores								
	1	2	3	4	5	6	7	

Table _____. Frequency distribution, by population means, of scores for scariousness of spathe, for taximetric analysis of variability in *Iris*, series Longipetalae. (based on standard deviation)

Mean scores for scariousness of spathe						
~~0.00-~~ ~~0.66~~	0.67-	1.34-	2.01-	2.68-	3.35-	Total
	1.33	2.00	2.67	3.34	4.01	
	~~-2.0~~	~~-1.0~~	~~+1.0~~	~~+2.0~~	~~+3.0~~	
	015	004	006	003	001	
	016	013	010	005	002	
	017	021	012	007	011	
	018	026	014	009	058	
	019	031	022	023		
	020	032	024	029		
	036	033	025	066		
	037	035	027	067		
	038	040	028	069		
	039	041	030	095		
	046	042	034	098		
	048	045	043	099		
	051	047	044			
	052	054	049			
	062	057	050			
	063	059	053			
	064	060	055			
	077	061	056			
	090	065	068			
		070	082			
		071	083			
		072	088			
		073	089			
		074	092			
		075	093			
		076	094			
		078	097			
		079	100			
		080				
		081				
		084				
		085				
		086				
		087				
		091				
		096				
				Mean: 2.00	s: 0.67	
				Range: 1.00	- 3.72	
n						
0	19	36	28	12	4	99
scores	1	2	3	4	5	

Table ____ Frequency distribution, by population means, for width of spathe-valves in mm., for taximetric analysis of variability in *Iris*, series Longipetalae. (Based on standard deviation)

Mean spathe-valve width, mm.							
3.10~	4.11~	5.12~	6.13~	7.14~	8.15~	9.16~	10.17~
4.10	5.11	6.12	7.13	8.14	9.15	10.16	11.17
039	023	002	001	004	017		015
040	034	005	003	008	084		
090	036	011	006	025			
091	085	012	007	028			
	086	014	009	048			
	088	018	010	052			
	092	020	013	054			
	093	022	016	063			
		029	019	070			
		032	021	097			
		035	024				
		037	026				
		038	027				
		041	030				
		043	031				
		045	033				
		047	042				
		049	044				
		050	046				
		051	053				
		056	055				
		058	057				
		059	060				
		061	064				
		062	065				
		066	071				
		067	074				
		068	075				
		069	076				
		072	077				
		073	078				
		080	079				
		081	094				
		082	095				
		083	096				
		087					
		089					
		098					
		099					
		100					
				Mean: 6.13 s: 1.01			
				Range: 3.44 - 11.14			
n							
4	8	40	35	10	2	0	1
scores							
1	2	3	4	5	6	7	8
							100

Table _____. Frequency distribution, by populations, for mean spathe-valve length in mm., for taximetric analysis of variability in *Iris*, series *Longipetalae*. (Based on standard deviation).

Mean spathe-valve length, mm.						
27.94-	43.43-	58.92-	74.41-	89.90-	105.38-	Total
43.42	58.91	74.40	89.89	105.38	120.87	
034	001 053	004	037	018	015	
041	002 054	008	038	019	016	
047	003 055	010	039	020	017	
066	005 056	012	072	022		
082	006 057	014				
	007 058	021				
	009 059	023				
	011 060	036				
	013 061	040				
	024 062	065				
	025 063	073				
	026 064	077				
	027 067	078				
	028 068	079				
	029 069	084				
	030 070	085				
	031 071	086				
	032 074	087				
	033 075	088				
	035 076	089				
	042 080	090				
	043 081	091				
	044 083	092				
	045 094	093				
	046 095	-				
	048 096					
	049 097					
	050 098					
	051 099					
	052 100					
			Mean: 58.91 s: 15.49			
			Range: 42.00 - 118.63			
n						
5	60	24	4	4	3	100
1	2	3	4	5	6	

Table _____. Frequency distribution, by population means, for the ratio: spathe-valve length/spathe-valve width, for taximetric analysis of variability in *Iris*, series Longipetalae. (Based on standard deviation)

Mean ratios: SVL/SVW						
3.37-	6.66-	9.95-	13.24-	16.53-	19.81-	Total
6.64	9.94	13.23	16.52	19.81	23.10	
025	001 053	010	016	018	039	
	002 054	012	017	020		
	003 055	014	019	040		
	004 056	061	022	090		
	005 057	065	023	091		
	006 058	073	036			
	007 059	087	037			
	008 060	089	038			
	009 062	093	072			
	011 063		085			
	013 064		086			
	015 066		088			
	021 067		092			
	024 068					
	026 069					
	027 070					
	028 071					
	029 074					
	030 075					
	031 076					
	032 077					
	033 078					
	034 079					
	035 080					
	041 081					
	042 082					
	043 083					
	044 084					
	045 094					
	046 095					
	047 096					
	048 097					
	049 098					
	050 099					
	051 100					
	052					
				Mean: 9.94 s: 3.29		
				Range: .6.06 - 21.38		
n						
1	71	9	13	5	1	100
1	2	3	4	5	6	

Table _____. Frequency distribution, by population means, for separation of spathe-valves in mm., for taximetric analysis of variability in *Iris* series longipetalae. (Based on standard deviation) (Original data listed under heading "S. Opp." in appendix data tables).

		Mean separation of spathe-valves, mm.						Total
		4.68~	9.85~	15.02~	20.19~	25.36~	30.53~	
		4.67	9.84	15.01	20.18	25.35	30.52	35.69
		- 1/2	+ 1/2	+ 1 1/2	+ 2 1/2	+ 3 1/2	+ 4 1/2	+ 5 1/2
001	053	008	015	018	022	020	019	
002	054	010	017	038				
003	055	024	036	084				
004	056	026	051					
005	057	042						
006	059	048						
007	060	058						
009	062	061						
011	063	072						
012	064	073						
013	065	081						
014	066	085						
016	067	088						
021	068	090						
023	069	092						
025	070	093						
027	071							
028	074							
029	075							
030	076							
031	077							
032	078							
033	079							
034	080							
035	082							
037	083							
039	086							
040	087							
041	089							
043	091							
044	094							
045	095							
046	096							
047	097							
049	098							
050	099							
052	100							
		Mean: 4.67 s: 5.17						
		Range: 0.64 - 32.64						
n	74	16	4	3	1	1	1	100
Scores								
	1	2	3	4	5	6	7	

Table _____. Frequency distribution, by population means, for sepal length in mm., for taximetric analysis of variability in *Iris*, series Longipetalae. (based on standard deviation).

Mean sepal length, mm.						
36.64	44.60	52.76	60.92	69.03	77.24	Total
44.89	52.75	60.91	69.07	77.23	85.39	
	009	001	005	015	019	
	021	002	006	016	037	
	022	003	008	017	039	
	034	004	010	020		
	035	007	018	036		
	044	011	023	038		
	045	012	025	040		
	046	013	028	065		
	047	014	030	072		
	049	024	031	073		
	050	026	032	075		
	051	027	033	084		
	052	029	041	085		
	053	042	043	086		
	054	048	067	097		
	055	057	070			
	056	058	071			
	059	063	074			
	060	066	077			
	061	068	078			
	062	069	079			
	064	076	087			
		080	088			
		081	089			
		082	090			
		083	092			
		091	093			
			094			
			095			
			096			
			098	Mean: 60.91	s: 8.16	
			099	Range: 46.14	- 81.60	
			100			
n						
0	22	27	33	15	3	100
scores	1	2	3	4	5	

Table _____. Frequency distribution, by population means, for sepal width in mm., for taximetric analysis of variability in Iris, series Longipetalae. (based on standard deviation)

Mean sepal width, mm.						
13.24-	16.05-	18.86-	21.67-	24.48-	27.29-	Total
16.04	18.85	21.66	24.47	27.28	30.09	
035	009	001	002	030	016	
045	013	003	005	036	019	
	021	004	006	037	038	
	022	007	011	039		
	044	008	015	070		
	047	010	017	071		
	049	012	020	074		
	050	014	028	075		
	061	018	031	079		
	064	023	033	085		
	068	024	040	087		
	083	025	041	094		
	090	026	042	097		
		027	043	098		
		029	054			
		032	057			
		034	058			
		046	063			
		048	065			
		051	067			
		052	069			
		053	073			
		055	076			
		056	077			
		059	078			
		060	084			
		062	086			
		066	089			
		072	095			
		080	096			
		081	099			
		082	100			
		088				
		091				
		092				
		093				
				Mean: 21.66	s: 2.81	
				Range: 14.96 - 29.20		
n						
2	13	36	32	14	3	100
scores						
1	2	3	4	5	6	

Table ____ . Frequency distribution, by population means, for the ratio: sepal length/sepal width, for taximetric analysis of variability in Iris, series Longipetalae. (based on standard deviation)

Ratio: sepal length/sepal width				
2.31-	2.57-	2.83-	3.09	3.35-
2.56	2.82	3.08	3.34	3.60
-2s	-1s	+1s	+2s	+3s
002	001	006	013	010
026	003	008	014	015
030	004	012	017	035
042	005	022	018	090
047	007	023	020	
052	009	024	025	
053	011	027	032	
054	016	029	039	
057	019	036	045	
058	021	037	065	
059	028	040	068	
060	031	043	072	
062	033	044	073	
063	034	050	083	
070	038	067	088	
074	041	080	092	
079	046	081		
098	048	082		
	049	084		
	051	085		
	055	086		
	056	089		
	061	091		
	064	093		
	066	096		
	069	099		
	071			
	075			
	076			
	077			
	078			
	087			
	094			
	095			
	097			
	100			

Range: 2.33 - 3.38
Mean: 2.82 s: 0.26

n				
18	36	26	16	4
scores				
1	2	3	4	5

100

Table _____. Frequency distribution, by population means, for percentage reflexed sepals, for taximetric analysis of variability in Iris, series Longipetalae.

Mean percentage reflexed sepals						
None	1.00- 19.99	20.00- 39.99	40.00- 69.99	70.00- 89.99	90.00- 99.99	Total
				026		
020	015	017	016	027	054	
023	022	018	024	028	055	
	050	019	029	030	059	
	079	021	032	031		
	086	025	033	040		
		034	035	041		
		035	039	042		
		037	043	051		
		038	047	052		
		049	045	053		
		066	057	056		
		067	058	062		
		068	060	064		
		077	061	065		
		078	063	082		
			069	083		
			070	084		
			071	087		
			072	088		
			073	089		
			074	090		
			075	091		
			076	093		
			080			
			081			
			085			
			092			
			094			
			095			
			096			
			097			
			098			
			099			
			100			
n						
2	5	15	34	24	3	83
Scores:	1	2	3	4	5	6

Mean: 54.3
Range: 0.00 - 92.00

Table _____. Frequency distribution, by population means, for petal length in mm., for taximetric analysis of variability in Iris, series Longipetalae. (based on standard deviation)

Mean petal length, mm.						
34.00	40.80	47.60	54.40	61.20	68.00	Total
40.79	47.59	54.39	61.19	67.99	74.79	
	-2s	-1s	-1s	+1s	+2s	
	003	001	005	015	016	
	007	002	006	017	019	
	009	004	008	036	037	
	013	011	010	038	039	
	022	012	018	040		
	034	014	020	065		
	035	021	025	075		
	044	023	028	084		
	045	024	030	085		
	047	026	031	086		
	049	027	032	097		
	050	029	041			
	051	033	043			
	053	042	067			
	054	046	068			
	055	048	069			
	061	052	070			
	062	056	071			
	064	057	072			
		058	073			
		059	074			
		060	076			
		063	077			
		066	078			
		080	079			
		081	087			
		082	088			
		083	089			
		090	091			
			092			
			093			
			094			
			095			
			096			
			098			
			099	Mean: 54.39	s: 6.80	
			100	Range: 43.56 - 71.48		
n						
0	19	29	37	11	4	100
scores	1	2	3	4	5	

Table ____ . Frequency distribution, by population means, for petal width in mm., for taximetric analysis of variability in Iris, series Longipetalae. (based on standard deviation)

Mean petal width, mm.						
5.81- 7.47	7.48- 9.14	9.15- 10.81	10.82- 12.48	12.49- 14.15	14.16 15.82	Total
-34	-72	-62	+14	+52	+32	
035	004	001	002	016	037	
090	007	003	008	017		
	009	005	011	019		
	013	006	015	030		
	014	010	020	036		
	022	012	021	038		
	024	018	023	039		
	025	027	028	041		
	026	029	040	067		
	034	031	042	069		
	044	032	048	074		
	045	033	052	085		
	049	043	054	087		
	050	046	057	098		
	061	047	058			
	083	051	063			
		053	065			
		055	066			
		056	070			
		059	071			
		060	072			
		062	073			
		064	075			
		068	077			
		076	078			
		080	079			
		081	084			
		082	086			
		088	092			
		089	093			
		091	094			
		095	097			
		096	099			
			100			
				Mean: 10.81	s: 1.67	
				Range: 7.28	- 14.88	
n						
2	16	33	34	14	1	100
scores						
1	2	3	4	5	6	

Table _____. Frequency distribution, by population means, for the ratio: petal length/petal width, for taximetric analysis of variability in *Iris*, series Longipetalae. (based on standard deviation)

Ratio: petal length/petal width							
3.41~	3.97~	4.53~	5.09~	5.65~	6.21~	6.77~	Total
3.96	4.52	5.08	5.64	6.20	6.76	7.32	
054	002	001	003	014	025	090	
	011	006	004	015	035		
	021	017	005	024			
	030	023	006	026			
	042	028	007	032			
	047	029	009	068			
	048	034	010	080			
	052	036	012	083			
	053	037	013	088			
	057	038	016	096			
	058	041	018				
	063	046	019				
	069	051	020				
	074	055	022				
	087	056	027				
		059	031				
		060	033				
		062	039				
		064	040				
		066	043				
		067	044				
		070	045				
		071	049				
		073	050				
		079	061				
		085	065				
		092	072				
		093	075				
		094	076				
		098	077				
		099	078				
			081				
			082				
			084				
			086				
			089				
			091				
			095				
			097	Mean: 5.08	s: 0.56		
			100	Range: 3.74 - 7.11			
n							
1	15	31	40	10	2	1	100
Scores							
1	2	3	4	5	6	7	

Table _____. Frequency distribution, by population means, for percentage emarginate petals, for taximetric analysis of variability in Iris, series Longipetalae.

Mean percentage emarginate petals.									
20.00- 29.99	30.00- 39.99	40.00- 49.99	50.00- 59.99	60.00- 69.99	70.00- 79.99	80.00- 89.99	90.00- 99.99	100	Total
002	003	028	098	007	004	001	010	008	073
	026	084	099	025	006	005	016	011	080
	100			029	009	014	061	012	081
				030	032	018	065	013	083
				094	074	021	069	015	085
				095	078	024	071	017	087
				096	089	027	072	019	092
				097		031	075	020	093
						043	076	022	
						044	082	023	
						070	086	033	
						077	088	034	
						079	090	035	035
						091		037	
								038	
								039	
								040	
								041	
								042	
								045	
								046	
								047	
								048	
								049	
								050	
								051	
								052	
								053	
								054	
								055	
								056	
								057	
								058	
								059	
								060	
								062	
								063	
								064	
								066	
								067	
								068	

Mean: 88.00

Range: 24.00 - 100.00



Table _____. Frequency distribution, by population means, for style branch length in mm., for taximetric analysis of variability in *Iris*, series Longipetalae. (based on standard deviation).

Mean style branch length, mm.					
17.98	21.57	25.06	28.75	32.34	Total
21.56	25.05	28.74	32.33	35.92	
	-3s	-2s	-1s	+1s	
044	002	001	011	015	
	003	004	016	019	
	009	005	017	020	
	034	006	018	037	
	035	007	021	038	
	045	008	024	065	
	046	010	025	067	
	047	012	027	071	
	048	013	028	072	
	049	014	029	073	
	050	022	030	075	
	051	023	031	077	
	052	026	032	084	
	053	033	036	092	
	057	042	039	096	
	059	054	040	097	
	060	055	041	099	
	061	056	043	100	
	062	058	063		
		064	066		
		086	068		
		090	069		
		091	070		
			074		
			076		
			078		
			079		
			080		
			081		
			082		
			083		
			085		
			087		
			088		
			089		
			093		
			094		
			095		
			098		
				Mean: 28.74	s: 3.59
				Range: 21.57 - 34.16	
n					
1	19	23	39	18	100
scores					
1	2	3	4	5	

Table — Frequency distribution, by population means, for style crest length in mm., for taximetric analysis of variability in *Iris*, series Longipetalae. (Based on standard deviation)

Mean style crest length, mm.						
6.89-	7.74-	9.59-	11.44-	13.29-	15.14-	16.99
7.73	9.58	11.43	13.28	15.13	16.98	18.83
	-2.2	-1.4	+1.2	+2.2	+3.2	+4.2
	001	004 078	015	010	020	016
	002	005 079	017	019	036	039
	003	006 082	018	038	037	
	013	007 084	021	040	085	
	024	008 090	023	055		
	045	009 095	030	073		
	047	011 096	031	081		
	049	012 099	032	086		
	061	014 100	041	087		
	062	022	043	039		
	076	025	057			
	083	026	070			
		027	071			
		028	072			
		029	075			
		033	080			
		034	088			
		035	091			
		042	092			
		044	093			
		046	094			
		048	097			
		050	098			
		051				
		052				
		053				
		054				
		055				
		056				
		058				
		059				
		060				
		063				
		064				
		066				
		067				
		068				
		069				
		074				
		077				
				Mean: 11.43	s: 1.85	
				Range: 8.60 - 17.44		
n						
0	12	49	23	10	4	2
scores						
	1	2	3	4	5	6

Table _____. Frequency distribution, by population means, for the ratio: style branch length/style crest length, for taximetric analysis of variability in *Iris*, series Longipetalae (Based on standard deviation)

Ratio: style branch length/style crest length						
.159-	1.91-	2.23-	2.55-	2.87-	3.19-	Total
.190	2.22	2.54	2.86	3.18	3.50	
016	010	002	001	025	024	
039	020	006	003	029	076	
044	036	007	004	066	084	
	037	008	005	067	096	
	040	009	011	074		
	048	014	012	077		
	050	018	013	083		
	057	019	015	095		
	085	021	017	099		
	086	023	022	100		
	089	032	026			
		033	027			
		034	028			
		035	030			
		038	031			
		041	061			
		042	062			
		043	063			
		045	064			
		046	068			
		047	069			
		049	070			
		051	071			
		052	072			
		053	075			
		054	078			
		055	079			
		056	080			
		058	082			
		059	090			
		060	092			
		065	097			
		073				
		081				
		087				
		088				
		091				
		093				
		094				
		098				
				Mean: 2.54	s: 0.32	
				Range: 1.82 - 3.35		
n						
3	11	40	32	10	4	100
scores						
1	2	3	4	5	6	

Table _____. Frequency distribution, by population means, for percentage exerted stamens, for taximetric analysis of variability in Iris, series Longipetalae.

Mean percentage exerted stamens						
	1.00- 19.99	20.00- 39.99	40.00- 59.99	60.00- 79.99	80.00- 99.99	Total
none						
020	001	005	010	014	034	
076	002	006	011	026	042	
077	003	008	012	035	045	
100	004	009	013	044	047	
	007	015	023	046	051	
	016	017	028	048	052	
	018	019	033	049	053	
	022	021	041	055	054	
	025	024	050	056	061	
	027	030	080	057		
	029	031	081	058		
	032 033	036	082	059		
	039	037	086	060		
	043	040	088	062		
	071	063	091	064		
	073	065	093	066		
	074	070		067		
	075	072		068		
	078	084		069		
	079	085		083		
	094	089		087		
	095	092		090		
	096					
	097					
	098					
	099					
			Mean: 40.3			
			Range: 0.00 - 96.00			
n						
4	27	22	16	22	9	100
Scores	1	2	3	4	5	6

Table _____. Frequency distribution, by population means, of percentage bilobed stigmas, for taximetric analysis of variability in *Iris*, series Longipetalae.

Mean percentage bilobed stigmas							Total
40.00- 49.99	50.00- 59.99	60.00- 69.99	70.00- 79.99	80.00- 89.99	90.00- 99.99	100	
003	009	006	001	010	008	016	073
	092	007	002	013	015	017	080
		075	004	024	025	018	081
		078	005	031	028	019	082
		097	011	076	030	020	083
			012	077	032	021	084
			014	090	035	022	088
			074	093	037	023	089
			094	096	040	026	
			095	100	045	027	
			099		050	029	
					052	033	
					058	034	
					059	036	
					069	038	
					070	039	
					071	041	
					072	042	
					079	043	
					085	044	
					086	046	
					087	047	
					091	048	
					098	049	
						051	
						053	
						054	
						055	
						056	
						057	
						060	
						061	
						062	
						063	
						064	
						065	
						066	
						067	
						068	

Mean: 91.30

Range: 40.9 - 100

1 2
 \ /
 1

5 11
 \ /
 2

10 24
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 3

47 100
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 4

Table _____. Frequency distribution, by population means, for ovary length in mm., for taximetric analysis of variability in Iris, series Longipetalae. (based on standard deviation).

		Mean ovary length, mm.						Total
		7.69- 11.42	11.43- 15.16	15.17- 18.90	18.91- 22.64	22.65- 26.38	26.39- 30.12	
n	0	12	49	25	10	2	2	100
	scores	1	2	3	4	5	6	
		009	001 082	008	010	037	018	
		022	002 083	011	015	087	019	
		029	003 091	012	016			
		034	004 094	013	017			
		044	005 095	014	021			
		049	006 096	020	039			
		050	007 098	027	040			
		058	023 099	030	065			
		059	024 100	031	085			
		061	025	036	086			
		062	026	038				
		066	028	052				
			032	054				
			033	070				
			035	071				
			041	072				
			042	073				
			043	079				
			045	084				
			046	088				
			047	089				
			048	090				
			051	092				
			053	093				
			055	097				
			056					
			057					
			060					
			063					
			064					
			067					
			068					
			069					
			074					
			075					
			076					
			077					
			078					
			080					
			081					
					Mean: 18.90	s: 3.74		
					Range: 12.28 - 33.28			

Table _____. Frequency distribution, by population means, for perianth tube length in mm., for taximetric analysis of variability in Iris, series Longipetalae. (Based on standard deviation).

Mean perianth tube length, mm.									Total
5.30	6.70	8.10	9.50	10.90	12.30	13.70	15.10	16.50	
6.69	8.09	9.49	10.89	12.29	13.69	15.09	16.49	17.89	
090	008	001	003	028				021	
	016	002	005	074					
	017	004	006	075					
	018	009	007	078					
	022	012	010	079					
	023	013	011	083					
	036	014	024	094					
	085	015	027	099					
	086	019	030	100					
	091	020	031						
		025	033						
		026	035						
		029	039						
		032	041						
		034	042						
		037	046						
		038	048						
		040	052						
		043	053						
		044	054						
		045	056						
		047	057						
		049	058						
		050	059						
		051	060						
		055	062						
		061	064						
		063	066						
		065	067						
		071	068						
		072	069						
		073	070						
		080	076						
		082	077						
		092	081						
		093	084						
			087						
			088						
			089						
			095						
			096						
			097	Mean: 9.49	s: 1.40				
			098	Range: 6.04 - 17.60					
n									
1	10	36	43	9	0	0	0	1	100
scores									
1	2	3	4	5	6	7	8	9	

Table _____. Frequency distribution, by population means, for the ratio: ovary length/perianth tube length, for taximetric analysis of variability in *Iris*, series *longipetalae*. (based on standard deviation)

		Ratio: ovary length/tube length					Total
0.91-	1.48-	2.05-	2.62-	3.19-	3.76-	4.33-	
1.47	2.04	2.61	3.18	3.75	4.32	4.89	
-7 s	-7 s	+1 s	+2 s	+3 s	+4 s	+5 s	
007	001 057	010	008	019		018	
009	002 059	011	012	090			
021	003 060	013	015				
034	004 061	014	016				
049	005 062	023	017				
050	006 063	026	020				
058	022 064	030	036				
056	024 067	032	037				
	025 068	071	038				
	027 069	072	039				
	028 070	073	040				
	029 074	084	065				
	031 075	088	085				
	033 076	089	086				
	035 077	092	087				
	041 078	093	091				
	042 079						
	043 080						
	044 081						
	045 082						
	046 083						
	047 094						
	048 095						
	051 096						
	052 097						
	053 098						
	054 099						
	055 100						
	056						

Mean: 2.04 s: 0.57
Range: 1.34 - 4.40

n							
8	57	16	16	2	0	1	100
scores							
1	2	3	4	5	6	7	

Table _____. Frequency distribution, by population means, for pedicel length in mm., for taximetric analysis of variability in *Iris*, series Longipetalae. (Based on standard deviation)

		Mean pedicel length, mm.			Total
14.68	25.83	36.98	48.13	59.28	
25.82	36.97	48.12	59.27	70.42	
-3.2	-1.2	1.2	-3.2	-5.2	
009	001 023	003	007	008	
021	002 034	034	015	044	
024	004 085	036	016		
028	005 087	038	017		
030	006 088	039	018		
031	010 090	040	019		
047	011 091	043	020		
065	012 098	045	022		
067	013	050	023		
094	014	054	035		
095	025	055	037		
096	026	056	049		
097	027	059	058		
099	029	064	080		
100	032	068	093		
	033	069			
	041	078			
	042	086			
	046	089			
	048	092			
	051				
	052				
	053				
	057				
	060				
	061				
	062				
	063				
	065				
	070				
	071				
	072				
	073				
	074				
	075				
	076				
	077				
	079				
	081				
	082				
			Mean: 36.97	s: 11.15	
			Range: 15.36 - 62.00		
n					
15	48	20	15	2	100
scores					
1	2	3	4	5	

Table _____. Frequency distribution, by population means, for the ratio: spathe-valve width/pedicle length, for taximetric analysis of variability in Iris, series Longipetalae. (based on standard deviation)

Ratio: spathe-valve width / pedicle length						
0.000-	0.044-	0.113-	0.182-	0.251-	0.320-	Total
0.043	0.112	0.181	0.250	0.319	0.388	
-3.2	-2.2	-1.2	+1.2	+2.2	+3.2	
015	018	003	001	021	009	
	020	005	002	024	096	
	023	007	004	025	097	
	034	008	006	028	099	
	035	014	010	030	100	
	037	016	011	031		
	039	017	012	084		
	040	019	013	094		
	044	022	026			
	049	036	027			
	058	038	029			
	080	043	032			
	086	045	033			
	091	050	041			
	092	051	042			
	093	055	046			
		056	047			
		059	048			
		061	052			
		062	053			
		064	054			
		068	057			
		069	060			
		073	063			
		076	065			
		078	066			
		082	067			
		083	070			
		085	071			
		088	072			
		089	074			
		090	075			
			077			
			079			
			081			
			087			
			095	Mean: 1.68	s: 0.46	
			098	Range: 0.019 - 0.338		
n						
1	16	32	38	8	5	100
scores						
1	2	3	4	5	6	

Montana State University

College of Agriculture

Agricultural Experiment Station

Bozeman, Montana 59715 Tel. 406-587-3121

Plant and Soil Science Department

4 July 1968

Dr. D. J. Rogers
Taximetrics Laboratory
Armory 101
University of Colorado
Boulder, Colorado 80302

Dear Dave:

Your inquiry about Iris is timely!

I can report that the wheels are turning, or maybe churning, but the thesis is not yet completed. Perhaps I wrote you that I took the spring quarter off with the intention of finishing things up. I got out 2 or 3 other papers, mostly dealing with various aspects of garden annuals. Some of these were in the works before the quarter started and needed to be finished up, others came about de novo due to the interest of a grad student from India in Zinnia and Coreopsis. I had also written, last summer, a 50-pg. history of pomology in Montana, but found that the editor of a proposed book would prefer a much shorter version, so chopped it down to 8 pp., which took time.

My grad student is proposing to study the nature of self-incompatibility in several species of Coreopsis for a doctorate in genetics, and we had to dredge up some material, since only one species is known to occur in Montana. California botanists say they're running out of projects for grad students, but think they've overlooked this genus, which may prove to have an interesting batch of ring chromosomes and other features. By the end of the year we'll have several new counts of Coreopsis species to contribute to Løve's periodic lists.

We are also in the throes of planning a hoped-for new building that will provide better facilities than the 1908 opus in which we're now housed.

The upshot of all this, and other things, was that I didn't get near the iris data until about 1 June. Two years ago I'd paid some gals \$3/hr to type up the basic data tables, but didn't get around to proofing them until last month. Imagine my chagrin to discover they'd done a bum job - one I wouldn't accept from any student of mine, and certainly couldn't turn in to Lenz at Claremont. Hence they must all be done over, and since there are 100 large pages it will take some time. I spent about 10 days proofing the old ones, and discovered some simple computational errors that led to a decision to recheck everything. This is now in progress.

I've also calculated the standard deviations and coefficients of variation for every pertinent character for all populations, but the grand values await the outcome of the fine-tooth comb processing the information is getting.

I believe the values mentioned at the bottom of pg. 1 will help in description and in deciding which characters are stable and which the more variable. You may take issue with this.

At this time I'm uncertain whether enough errors will be turned up to affect the results of the analyses run on the Colorado machines. If it appears that such is the case, I'll be seeking your advice.

Lenz knows only of the graph theory analysis, not about the topology one, and I'm uncertain whether it would be advisable to complete the spaghetti work for both for the thesis. So far, no working out of the topology analysis has been undertaken, altho I've had it sitting on the desk all the time.

Over a year ago, Mary did an editorial job for me on a section telling when & where the data were collected. This has now been reworked mostly in accord with her suggestions and is in the hands of a typist getting ready to send down to Claremont. Mary wouldn't accept any compensation for her services, but, this year, I got around to ordering a fancy corned beef thing sent to them at Loveland, but fear they may never have received it as I had no Rt. no. for their address.

Although I'm supposed to be back on duty, it's my hope that things will now work out so that the thesis can be completed before the fall semester begins at Claremont. This would please me no end, and save me \$100 tuition at the same time. I have to keep registered in absentia every semester until I finish.

The general range map for the series Longipetalae is essentially finished, but have recently borrowed material from a number of western herbaria that I'd not had an opportunity to visit to assure as complete a map as possible. For the U.S. portion of the range it's built on a county-unit basis. I've not seen the material in the U.S. National, in New York, or at the Gray, and doubt it would be worth the trouble. At one time or another, I've catalogued the material in most of the western herbaria, and have here now the material from the herbaria at Pullman, Seattle, Boulder, and Albuquerque. The big surprise in this group was the volume of sheets held by Albuquerque. Considering the widespread occurrence of *I. missouriensis* in Colorado, the Boulder holdings are rather slight. I have a student's wife copying off label information; when she's done I'll be checking on her & adding pertinent annotations to her cards. Then we'll be able to put the general range map to bed. In the original, it's a big 2 x 3 foot map, which will have to be reduced photographically. I have another blank one of the same size to use for indication of the relationships that come out of the analyses.

The type of *I. missouriensis* was collected by Nathaniel Wyeth and exists in the herbarium of the Phila. Acad. I have it here now, and it consists of merely two leaves and 1 cut scape. The label information is very scanty, comprised merely of the information "sources of the Missouri" and the date "July 9". I've been studying Wyeth's diary trying to figure out where he was on that date in either 1832 or 1833. As nearly as I can determine he was in the Snake drainage just west of the Tetons on 9 July in both years. Maybe he was forgetful of dates, or did some tipi-crawling in Wyo. or Montana that he chose not to record in his diary.

Most of the literature relating to the Longipetalae was abstracted while I was in Claremont 2 years ago. There's not much point in trying to write up this aspect until I know for sure what taxa should be recognized within the group on the basis of the numerical studies.

The thing that bothers me a bit about the numerical analysis is that the phenetic approach doesn't weight the leaf-base color character, which is nicely distributed geographically, and is a good, practical field character the worthless with herbarium specimens. There's a group with red leaf bases that follows along the e. side of the Cascades and Sierras, running out across the Great Basin in Nevada and fading out at the west front of the Wasatch in central Utah. Other areas have white leaf bases, by & large. I think that overall we've got a good set of characters, but it may prove difficult to apply the results to isolated specimens without knowing the geographic origin. We shall see.

I have very little botanical scuttlebutt to pass along. You've perhaps heard that Verne Grant found himself unhappy at Texas A&M, and is leaving for the Boyce Thompson Desert Lab in Arizona. There's an isolation chamber if you ever saw one.

Lenz writes that Grant's replacement at Claremont is to be a fellow recently at Cornell, who prefers to be known as John Simon, but who was christened Jean-Pierre Jacques Simon LeClerq. He's a biochemical systematist, working most recently on the annual species of Medicago.

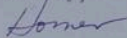
Money continues to be scarcer than hens' teeth, both for salary increases and work funds. I won't be able to do anything in fiscal 1969 that requires capital investment from exp. sta. funds.

I'm trying to get our president interested in acquiring a site for an arboretum-botanic garden complex (not alone, with help of others), but land acquisition by the University is a politically sensitive issue - a hue & cry goes up above removal from property tax rolls. We got a gift of about 60 acres at the edge of the forest south of Bozeman, and there's a neighboring ranch that would make a fine addition if someone will give, or bequeath it to the U.

Hopenthat things are going well for you and your crew at Boulder. Do you plan to be in town most of the summer?

I've chattered along at far too much length, but remember that Homer won't let you down on this iris thing, altho he can hardly be called a speedball about it. The thesis will likely run to over 200 pages when done.

Sincerely,



H. N. Metcalf
Professor of Horticulture

— *Montana State University* —

Bozeman, Montana 59715

Tel. 406-587-3121

Department of Plant & Soil Science
College of Agriculture

19 May 1967

Dr. D. J. Rogers
Taximetrics Laboratory
Colorado State University
Ft. Collins, Colorado 80521

Dear Dave:

Have been up in the Flathead cherry orchard for a week, trying to catch up on both late winter & spring work there- like spreading $1\frac{1}{2}$ tons of fertilizer by hand last Tuesday. I should have gone back this weekend to play Geo. Washington - removing excess trees, but couldn't round up a crew to help, so must postpone a week. Bloom is late this year, probably 2 weeks later than 1966, and a week later than normal, so their annual Cherry Blossom festival had to be postponed. However, it got no colder than 7 above in our experimental orchard this winter, so the prospect is for an excellent crop, barring frost in the next few days.

Many thanks for the kind words to Foskett - I gave him your name as one reference & should have asked your permission previously. Love's administrative asst. acknowledged my letter to him, saying he was abroad for a spell.

Being a mountain-country man, I'm sure I'd appreciate having air-conditioning at College Station, and will send in my request for a reservation right away. If you have room in the car, I'd certainly appreciate riding along with you - it would be a jolly arrangement, and I'd have a chance to go over things with you before going front & center.

Classes are over here by 9 June, and I'm intending to get busy with the thesis as soon as the classroom cum tables is cleared of students, etc. We should have most things planted and under control by then, altho the gardens are a mess right now.

Sincerely,

Don
hnm

P.S. It occurs to me that if we travel together, it might be wise for you to complete the housing application form for me, since you probably know how many days you plan to stay at Texas A&M (the full series of sessions?), 27 Aug - 1 Sept.?, & would be better able to specify arrival-departure dates, etc. Hence, I'm enclosing my form, partially completed -. I think I'll be able to get travel support from the Dept here of not less than \$16/diem.

h

Dave:

I suppose you've received notice of this affair already, but if not, you might be interested.

I've not been able to touch the thesis since the autumn round of meetings was completed in November, but hope to take the spring quarter off and GET IT DONE!

Lots of snow & cold weather here this winter.

15 Febr 68

Hom

- Taximetrics Laboratory

May 10, 1967

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

Dear Homer:

I got your letter of the 5th. I just talked to Foskett and found that while you were not one of the front runners, neither were you yet excluded from the running. I said a few words to him which may or may not be useful, but can't tell.

You asked about the conference at Ann Arbor. This seems to be a gathering of "big guns" taxonomically from around the world, both botanical and zoological but with an emphasis on the latter. I am one of the discussants on a paper to be given by an unknown in the computer taxonomy field, a young man from Harvard (natch). This conference is sponsored by the National Research Council, but why the thing is coming off now, I am not sufficiently in the higher councils to know. All I know is that it is a free trip to Michigan in June which should be pleasant.

I think things are all set for our symposium at AIBS. Have you sent in your reservations for a room yet? Not all dormitories are air conditioned and I suspect that College Station would be miserable without air conditioning. I think we will try to drive down in a car from Boulder. Would you care to join us? Let me know so that we can make arrangements.

Sincerely,

David J. Rogers
Professor of Botany

DJR/ch

— *Montana State University* —

Bozeman, Montana 59715

Tel. 406-587-3121

Department of Plant & Soil Science
College of Agriculture

5 May 1967

Dear Dave:

So you're moving to Boulder - about 1 July I suppose? I think you were working on this when I was down in February, and in fact, think you may have been fishing in Smorgasbord Pond for the past year or so.

I hope that, in addition to an improved salary schedule and better scientific atmosphere, that you succeeded in wangling that big grant in information retrieval and that you will have a big CDC computer available at Boulder. I would infer that perhaps Boulder beat out Ft. Collins in the Legislature budgetwise again this year.

Teaching has taken up so much time this winter, along with curriculum rewriting, that I can't report much of any progress on the thesis since I last saw you, to my chagrin. I won't make the deadline for the degree at 1967 commencement at Claremont, but may manage to get the thesis turned in.

Lee Lenz was in Bozeman a week ago for his mother's funeral (she had been living in Sacramento, but was originally from around here), so had a chance to chat with him, and show him the general outline of things, and he seemed to be pleased. I learned from him that V. Grant will be moving to Texas about 1 August, and that they are seeking a younger man, probably not more than 5 years past his degree, as a replacement. No public announcement is being made of the vacancy. Also, Sherwin Carlquist, who is on my committee, is off on a 10-month's trip around the World, being last reported in the Canary Islands, so that leaves Lee & Bob Thorne as en scene members.

The letter to Löve has been written, and I worked up quite a batch of lather in the process. I do indeed hope he will be duly impressed.

I have heard nothing from Foskett beyond an acknowledgment of my application, so suppose they have found a more promising candidate for the job in the Director's office.

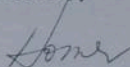
What's the scoop on this conference at Ann Arbor next month?

I haven't taught the course I'm giving this spring since 1961, so there's a lot of updating to be done for it, and it's proving to be highly time-consuming.

Next week I must go to the Flathead to work on the cherry orchard which has languished sans my TLC since last August. The other man who works on cherries has suffered a heart attack, so expect I may have to do his work too.

We have had nothing but snow during April, the pack in the mountains being so heavy that flooding is likely should it warm up in a hurry. Today was the first pleasant day in some time.

Sincerely,



— *Montana State University* —

Bozeman, Montana 59715

Tel. 406-587-3121

Department of Plant & Soil Science
College of Agriculture

7 March 1967

Dr. D. J. Rogers
Taximetries Laboratory
Colorado State University
Ft. Collins, Colorado 80521

Dear Dave:

I'm pleased to be able to send you a check for \$100 promptly, and hope it will do a bit toward tiding you over until the grants come through. I feel it's a very modest charge for all the services your team has rendered me.

I should have acknowledged the safe return of the drafted plates well before now, as well as Henry's wrapping paper efforts to advise me on the red circle convention. I've not found time to study this advice in any detail at all, as yet, but will soon be with it.

Gather you decided not to run the nodal distance array on these data, since no print-out has shown up here as yet.

Hope that the Colorado Legislature decided to come through with the funds for the CDC machine. The Montana Legislature seems to have spent about 2 weeks arguing over the price of fishing licenses, and about 2 days considering the budget for the University system. I still have no idea what kind of budget will be available here for the next biennium, but am very dubious of any significant increase in work funds since the policy seems to be to put available \$\$ into salary increases - which is fine if one does nothing but teach. I suppose this also indicates that one should study up on grantsmanship.

Was sorry to hear that nothing could be arranged for the Texas meetings, but will plan on joining with you at the New York affair. Enclosed is a notice of a regional meeting of the Biometrics Society at UCLA, which might be of some interest. We also received a notice of a symposium in population biology at Syracuse in early June. It's 14 years since I've been east of the Black Hills, and this meeting may afford a good reason to go. My relatives mostly live in Connecticut.

In looking further at the Biometrics Soc. announcement I note that the 1967 prexy is Franklin Graybill of the CSU Stat Lab.

It has been a busy two weeks since I was in Ft. Collins. I had to make up a batch of student curricular study programs, have been to the Flathead, and just returned Sun. night from a meeting in Salt Lake to which I took two students. Last week was also the dept.'s annual research planning conference and although someone else was supposed to do it, it became my lot to assemble and write the general summary for one of the committees with much of the material not becoming available to me until a day and a half before the pertinent session.

I much appreciate your having mentioned my name to Dr. Foskett for the position in the Director's Office at Ft. Collins. I hadn't gotten around to doing anything about it until last Thursday, just before leaving for Salt Lake. I merely wrote saying that I'd like to be amongst those considered for the job, and would be pleased to supply any information necessary for an application, playing it dumb, as it were, because I had no idea whether you'd care to be involved, nor of CSU campus politics. Anyway, the hat is in the ring, and we shall see what kind of case I can build against the competition.

Incidentally, I trust that you are already an AAAS Fellow. I've been serving on the Fellowship Committee for the Southwestern & Rocky Mtn. Section, and I anticipate a call for 1967 nominees ere long.

If I can manage to get the work done, I hope to take a rough copy of the thesis to Claremont between quarters.

Phil Munz writes that Rancho Santa Ana Bot Gard is in the market for a replacement for Verne Grant, with likely preference going to a somewhat younger man than Verne, since virtually all the present staff is even-aged & will retire virtually simultaneously. No tears are being shed over VG's departure.

Sincerely,

Homer

H. N. Metcalf
Professor, Horticulture

P.S. Learned that our dear Admin. VP proposed last week that all staff sans the union card be given terminal contracts, but think the idea has been quashed, temporarily at least, since it would mean finding replacements for over 150 staff members, & several deans and dept heads jumped all over him about it.

Montana State University

College of Agriculture

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Plant and Soil Science Department

January 27, 1967

Dr. David Rogers
Taximetrics Laboratory
Dept. of Botany & Plant Pathology
Colorado State University
Fort Collins, Colorado 80521

Dear Dave:

It's all too true that you haven't heard anything from me in regard to progress on the Iris project, and that's because not too much has been done with it since college started in late September. Most of the endeavors have been peripheral in nature, and I haven't really attacked the heart of the writing job.

Since my sole associate in horticulture, George Evans, has been away at Michigan State, working on his degree, I've had responsibility for doing, or overseeing, all the teaching - 17 credits in the fall term, and have had a big slug of reports to write that relate not at all to the thesis project with Iris. The winter quarter, so far, is about the same, except that I've not had to do any traveling to regional committee meetings, etc. I feel I'm about a month behind, and have only this week finished a batch of busy work that should have been completed by Christmas. Another factor in the picture is that the administration here is unhappy because our statistical picture in regard to student registration in hort. courses is unfavorable, and we are trying to promote a curriculum in the husbandry of outdoor recreation as a life-saver, and this is due for decision by the middle of February. Consequently, my morale has not been at outer-space levels.

I have been trying to complete all the potential appendicular material before starting on the main body of the thesis, in order to facilitate referencing, and quite a bit has gotten completed for this aspect. The original data has all been typed up in large format (but proofing is incomplete), the sub-graphs have all been drawn (in 32 plates) and photographed on Kodolith negatives, and much progress has been made on development of a range map (on a county-unit basis for the 12 western states). I have also borrowed for examination, or acquired photos of, all the types in the series *Longipetalae*, and have only a couple more years to cover in preparing the annotated itinerary of my travels on the iris trail. I also have selected and had reproduced 140 color photos of habitats and flower close-ups. I've also had all the computer output photographed on Kodolith. All this has cost somewhere in the vicinity of \$1200 since summer, and since I'm trying to do this on a pay-as-you-go basis, it's another reason for slow progress. I'm estimating a cost of \$500 per copy for the completed thesis, and that may prove to be conservative.

Post Card
to Dave
1/2

Dr. David Rogers
27 January, 1967
Page 2

Because I've not progressed any further than I have, it would still be perfectly feasible to incorporate your nodal distance array concept into the treatment of the iris material. I have been between Scylla and Charybdis trying to decide whether to attack first the thesis or a big batch of data, covering four season's observations, from our experimental cherry orchard in the Flathead. It's altogether possible that your letter has tipped the balance in favor of the thesis.

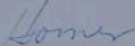
The earliest break I can see in the schedule that might permit me to come to Fort Collins is about the middle of February, when we get a long weekend, and I could be away from the 11th-14th or 15th, perhaps. Would this work out with you?

I would urge you to go ahead with plans for the AIBS symposium at College Station, and will give you all the support possible from the standpoint of the results of treatment of the iris data. I'll plan on Texas in August.

If I can get the thesis done, there's a bare possibility that we might even be able to get a paper on it in print before the Symposium is held, if that is desirable. I'm hopeful that Lee Lenz will be interested in seeing it published in Aliso, which makes its annual appearance in late spring. Would you favor that site of publication, or have suggestions for another? I think Lee is getting converted to computer use, at least for inventorying RSABG's living plant collections, and possibly even the herbarium which now combines both the RSABG & Pomona College collections and is probably the third largest on the west coast. Anyway, it's a start.

Had you heard that Verne Grant is moving (or has already moved) from Claremont to College Station?

Sincerely yours,



Homer N. Metcalf
Professor, Horticulture

HNM:ae

Homar Metcalf, Mont. St. U.

Taximetrics Lab.

Jan. 24, 1967

Dear Homer:

We've been wondering what happened to you. Do you folks get snowed in there in Montana? Maybe the mails have quit running. Seriously, we would like to know of your progress with the Iris work. Since we last talked, a new way of using the computer program has been worked out, particularly with the type of information we have for Manihot esculenta, whose problems we believe to be somewhat similar to yours with Iris. We should get this information to you, for it does not involve ~~xx~~ any changes in your data format, the program runs the same, but there's one additional output we call the nodal distance array which is very helpful. If you can get down here, let me know.

I have a request to make of you. I think it's time that we started beating the drums for taximetrics, and one way to do it is to put on a symposium at the AIBS meetings next August in College Station, Texas. What I propose is that you, and several others who have used our methods, get together to discuss our results. I've asked Howard Irwin, Ean Brance, ~~xxx~~ Pierre Dansereau, and Frank Hawksworth to participate, and I'm now asking you to join in. Your work is not duplicated in any of the others works, nor theirs in yours. Altogether, we have a rather impressive array of success stories to tell, and we need to do it.

Part of the reason is that the granting agencies need to be impressed, particularly NSF, who have consistently dragged their feet in support of our work. One way to impress these non-believers is to lay out some real honest, practical classifications which have been derived by the methods, and which could not have been otherwise derived. Another reason is that ~~xxxxx~~ Sokal and his crowd are making all the noises, and if we don't soon say something, he is going to kill the use of computers in taxonomy for a long time. We can prevent it only if we're willing to tell people. I hope you understand my urgency and need, and that you will be willing to participate next August. Let me know as soon as you can, so that I can go ahead with arranging the symposium with the program chairman of the Am. Soc. of Pl. Taxonomists, where I think the meeting ~~xxx/xx~~ to be held.

/ought

Looking forward to your reply.

Sincerely,

- Taximetrics Laboratory

March 2, 1967

Mr. Homer N. Metcalf
Plant and Soil Science Department
Montana State University
Bozeman, Montana 59715

Dear Homer:

We were unable to get a spot on the meeting of the AIBS next August. The program had already been filled. We will try instead to get a spot on the AAAS meetings next December in New York. I hope this is not an impossible thing for you. I think the other people--Dansereau, Irwin, and Hawksworth--will be able to participate in the meeting at Christmas time. Let me know whether you can participate in the symposium or not.

I have proposed your name to Dick Foskett for the job as Associate Dean of Agriculture. If you do not hear from him within the next week, I suggest strongly that if you really are interested in the position, that you send in your application saying that I have recommended you.

I am enclosing herewith a statement of computer charges. I wish we could provide free services, but can't any longer.

Sincerely,

David J. Rogers
Professor of Botany

DJR:ch

Enc.

- Taximetries Laboratory

March 2, 1967

To Professor Homer N. Metcalf for computer services on Iris
data \$100.00. Please make check payable to Colorado State
University and mail to Dr. David J. Rogers.

—Montana State University—

Bozeman, Montana 59715

Tel. 406-587-3121

Plant and Soil Science Department

September 23, 1966

Dr. David R. Rogers
Dept. of Botany & Plant Pathology (Taxonomy Laboratory)
Colorado State University
Fort Collins, Colorado 80521

Dear Dave:

Your card of the 8th reminded me that your group hadn't had so much as a peep out of me since Henry was here some weeks ago.

The Astragalus was from the collection of one of our students, Jim Murphy, who had been busily collecting Montana flora to convince his doubting-thomas relatives that genera of native plants have correlates among cultivated plants. He tried finding material to collect for your needs in August, but without much luck, so gave me one of his spares to send you. I'm not sure whether it will be feasible to get any further collections this year, with the season so advanced, unless it be from material that Bob Eslick has under cultivation here. He's been working with cicer milkvetch as a forage, and has gathered quite a few other species, as well.

I made three trips to the Flathead cherry orchards during July and early August. This pretty well killed off those weeks. However, I've been at the thesis fairly steadily since the 2nd week of August, and have gotten quite a bit done, but I won't get it finished before the start of the autumn semester at Claremont, so have gotten an extension of time. I hope to get it done by mid-October at the latest, as I'd like to go to Hawaii with it off my mind.

During July and August I arranged to get all of the original data put on porta-punch cards with a view to getting standard deviations computed, by populations, and for the whole universe of data, at our local computing center. The result was a \$275 mess, and we have been working for 2 weeks doing 1600+ standard deviations on desk calculators. I haven't decided whether it's worth the attempt of getting the overall s.d.'s, where $n = 2571$, with 16 of these to do. I hoped these s.d.'s would assist pointing out to the Claremont crew which characters were stable and which quite variable.

Also I paid a gal \$450 to type up the original data tables and some other material, and it appears that her proofing was considerably less than perfect, so we are busy reading proof, too - something I'd not anticipated doing. She'll get the stuff back with a request to please correct errors on her own time.

I have redrafted the graph theory analysis about three times, trying out different ways of presenting it. In the process, I found that we made a wrong connection the first night we worked on the sub-graphing at the lab at CSU, and this has necessitated some minor changes in the finished configurations. Nonetheless, I have gotten it all drafted up for the thesis in 32 plates, which are now ready to be photographed on Kodalith negatives to reduce them to the size required by the rules of the Medes & Persians at Claremont. I believe I can recognize two, and perhaps three, subspecies in the group. Whether the third group should be given specific status, I haven't decided. The Californian coastal material that has been called I. longipetala seems most closely associated with colonies in the margins of the Palouse in north Idaho and eastern Washington. I don't have enough samples from Oregon to be dead sure that there's a cline running through there, but suspect so. In any case, I think I. longipetala may find itself fraternizing with some new bedfellows, which I would not have suspected without benefit of graph theory. There is another association which runs from Arizona and New Mexico up along the east side of the Rockies to the Black Hills, and a third which occurs in the northern Rockies and Great Basin. The major articulator proves to be a nonedescript colony from the vicinity of Dillon in Beaverhead County, Montana - at least it didn't attract special notice when I collected the sample.

In drafting the plates of subgraphing for the thesis, I adopted a few expedients, especially regarding the "red circle" thing, which appeared to simplify the presentation. Holding always to the concept that three connections are needed to bring a population into the circle, we found it helpful to show the multitude of internals only at the level at which they first appear, unless they have exterior connections. Otherwise we'd have a blackout of lines. Eventually we got nearly everything inside the "red circle", the sole exception being a garden hybrid of Lenz' that never did develop more than one connection. The plates are too large to go through the Xerox machine, or I'd send you a set, now.

The analysis did several things that may interest you:

1. Duplicate samplings of the same population were properly associated, although not at high C-values.
2. There appear to be definite geographical associations of characters.
3. A sample of I. douglasiana (a member of the series Californicae), did not associate until C-values had declined very materially.
4. A garden hybrid of different races of Longipetalae did not associate closely until quite low C-values.

Where there was very little action, or no moat, between levels, I combined them into one plate. Proliferation of plates means proliferation of error potential.

When completed, I expect to supply you with a copy of the thesis, which is likely to run over 200 pages, especially if I photograph the computer printout for inclusion in order to explain to the Claremonters what happens in the process. I have about 140 color prints for inclusion, too, to show the varying ecology of collection sites, and variation in flower form. I also expect to include the annotated itineraries of my travels on the iris trail, as appendicular material.

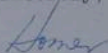
As part of the computer run here, I'd hoped to get additional sets of ratios to use as additional characters should it be deemed wise to make another run before committing this opus to public print, but they're fouled up too, so will have to do them on the desk machine if they are to be used. However, I'll have to defer action on this until the thesis is completed.

The freshmen arrived here on Sunday, and teaching operations start up next week. I have two courses, one in general horticulture, another in pomology, to teach, but don't expect large registrations. George Evans, my associate here, left to spend the year at Michigan State, working on his degree, so I have his work to look after, too.

The garden mums are doing fine, there not having been any frost so far, and I should spend a couple of days with them, as there are 2 or 3 that should be released. Our potato breeding stock is also ready for digging, and we'll be at that later this week. From now until mid-November, there are more meetings, conventions, etc. (mostly intra-state) to be attended than I care to think of, some of which involve considerable preparation time on my part, and I shall be very happy to see Thanksgiving get here. I'm spread too thin!

Best of luck with the NSF proposal.

Sincerely,



H. N. Metcalf
Professor, Horticulture

HNM/gtw

- Taxonomy Laboratory

August 11, 1966

Mr. Homer N. Metcalf
Plant and Soil Science Department
Montana State University
Bozeman, Montana 59715

Dear Homer:

We're glad your cherry crop came through with flying colors. You had better be careful or everyone will be going to Polson instead of Washington, D.C. to see the cherry blossoms.

We're returning the motel check. It is carrying graciousness too far--having gourmet meals and lodging too--it is not only immoral but indecent.

We did not return by Glacier but to the south. We figured we could do Glacier from North Dakota. Went down by 95 and 15 to Boise and then on to Logan where we cut west through Logan Canyon to Bear Lake and stayed the night at Little America. In transit we saw the Craters of the Moon. I don't recommend 95 for making time on, but we were surprised that such country was there. Knowles comes up on 95. Turned south shortly after Little America through Flaming Gorge to Vernal--saw the Dinosaur diggings and then on home. Figured we could conveniently come from Fort Collins back to see the Yampa and Green River Canyons when we had more time.

Sam Dietz sends you his best and the business with Knowles went off well. We all had dinner together the last night. I did not notice the heat if it was warm. Maybe Sam was too entertaining showing us the country, etc. Curious--all that wheat on top of the lava. The trip was worth it all just to see the red cedars on that Missoula to Lewiston run. The road from Lewiston up to the top to get to Pullman reminded me of the old road from La Guiva to Caracas, Venezuela.

We expect Knowles the last weekend of this month, and the following Monday and Tuesday a chap from NSF (Washington) is coming out to see us; otherwise, Dave and I are free. So come at your pleasure if you wish. The boys are off but expected back momentarily which will take some of the computer push off me. I can't promise gourmet meals but the house is yours.

Sincerely,

Henry S. Fleming

— *Montana State University* —

Bozeman, Montana 59715

Tel. 406-587-3121

Plant and Soil Science Department

6 August 1966

Dear Henry & Mary:

Returned just the other night from spending the better part of two weeks at our experimental cherry orchard at Polson. The crop was good this year in spite of frost in May & cold weather in June, and I think they harvested at least 2000 tons. One of the packing houses was to close on Friday - I suppose so they wouldn't run into the problem of competition with the major local event of the season - an annual power boat regatta on the Lake. Otherwise, they'd probably have too high a percentage of worker absenteeism.

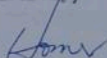
In checking thru my bills, I found that I didn't have one from the motel in which you stayed while here at Bozeman. This was contrary to my instructions to them when I made the reservation, and inquiry revealed that you had paid your own way. So with sincere apologies for any inconvenience it may have caused you, I'm enclosing my check for \$20.

I hope you had a pleasant journey on to Pullman, and back to the Fort. I know it must have been pretty warm down at Wawawai on Snake River, but hope you found the safflower interesting.

There has been little progress on the thesis material, beyond locating a typist with experience in such matters. However, my attention for the next few weeks will be pretty well riveted on the subject, as time is flitting by rapidly. If I encounter any problems, you'll be likely to hear from me, or it might prove wise for me to make a quick trip to Colorado - we shall see. In any case, I don't have any further out of town engagements until it's time to defend the thesis in September at Claremont, and there is much to be done.

It's been pretty warm here much of the time since you were in Bozeman, and we now appreciate the cooler weather that has followed a thunderstorm on Thursday.

Sincerely,



- Taxonomy Laboratory

June 14, 1966

Mr. H. N. Metcalf
c/o Rancho Santa Ana Botanic Garden
1500 N. College Ave.
Claremont, California 91711

Dear Homer:

Congratulations! Will set a bottle aside for you in case you have sufficiently recovered to imbibe when you reach here.

Everybody will probably be on deck here when you arrive. The trip has been delayed to coincide with Manihot flowering time.

Saw I. missouriensis just north of Laos, N. M. about 3 weeks ago and think I saw some last Saturday at North Park (Walden) Colorado. (Last identification made at 60 miles per hour!)

Sincerely,

Henry S. Fleming
Professor of Botany

HSF:ch

— *Montana State University* —

Bozeman, Montana 59715

Tel. 406-587-3121

Plant and Soil Science Department

c/o Rancho Santa Ana Botanic Garden
1500 N. College Ave.
Claremont, California 91711

11 June 1966

Dr. Henry Fleming
Dept. of Botany & Plant Pathology
Taxonomy Laboratory
Colorado State University
Fort Collins, Colorado 80521

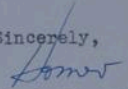
Dear Henry:

Just a note to let you know that I successfully passed the prelims here yesterday.

In the time that remains for me here in s. California I shall try to complete working out the graphing of the iris data so that we can discuss it when I come to Ft. Collins. I probably won't leave Claremont until the 20th or 21st.

Best wishes and bon voyage to Dave and the boys for their Mexican jaunt.

Sincerely,


H. N. Metcalf
Professor, Horticulture

hnm

RANCHO SANTA ANA BOTANIC GARDEN
OF THE NATIVE PLANTS OF CALIFORNIA
(Founded in 1927 by Susanna Bixby Bryant in memory of John W. Bixby)
1500 North College Avenue, Claremont, California

LEE W. LENZ
DIRECTOR

PHILIP A. MUNZ
DIRECTOR EMERITUS

1 June 1966

Dr. Henry S. Fleming
Taxonomy Laboratory
Dept. of Botany & Plant Pathology
Colorado State University
Ft. Collins, Colorado 80521

Dear Henry:

Have run out of Montana letterheads, so borrowed one from the Garden here.

The print-out we studied in Ft. Collins arrived here safely yesterday, and I think that Lee Lenz was satisfied with my explanation of what was going on. Many thanks for sending it.

I think it's OK to run another print-out with the corrected programming. I've not had time to do anything about altering the character input, altho it might be interesting to do so later.

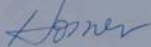
I'll be happy to work with you folks on a joint paper in due course. First, I must get a thesis written covering the series Longipetalae, even tho my knowledge of it is imperfect, lacking adequate cytological information, etc. All aspects of the degree must be completed before the autumn semester starts here, or so many credits will be heavily discounted as to make the thing unfeasible.

It's indeed most generous of Dave to assume the computer costs, and for all of you to have devoted so much of your valuable time to the iris project.

I'm scheduled for prelims on Monday & Tuesday, and facing them, I feel appalled at my ignorance.

When they are thru, I shall perhaps take a day or two in the desert sun, then work for a spell on the data, and other aspects of thesis preparation. I have an apartment here until the 21st, so will likely plan to stay here until approx. then. Would rather like to go thru Zion Natl. Park on the way north, as I've not visited it previously, but would plan to come to Ft. Collins for a day or two to check with you on data interpretation. Actually, my schedule is rather adaptable after the prelims are over, so that if you have any urges to go places, let me know & I can rather readily adjust to your schedule. Your offer of accomodation is most generous, but you shouldn't put yourself out in any way in this regard. In the event I might have someone traveling with me, I would plan to stay at a motel. In any case, I would give you advance notice of arrival intentions.

Sincerely,



- Taxonomy Laboratory

May 25, 1966

Mr. Homer N. Metcalf
c/o Rancho Santa Ana Botanic Garden
1500 N. College Ave.
Claremont, California 91711

Dear Homer:

I am answering your letter in lieu of Dave. We have the bug out of the internal connections. It was minor, but one never knows until the program actually runs correctly.

I have been waivering back and forth as to whether or not to have the data run again and obtain the correct internals or to wait and see if you want to alter some of the characters. My present feeling is to run it now and get a new printout. If you are inclined to the contrary, let me know by return mail. Your old print-out is being mailed to you.

Also let me know if you expect to return to Montana by way of Denver on or about the 22 of June. Dave and the boys will be away to Mexico, but I will be here. If you come, arrange to stay with us at Loveland for as long as you wish.

C. Jorgenson (Hort. Dept., at dinner with us the first time you visited) stopped by the house last night and said your Iris was in bloom in front of the Student Center.

Dave says that you don't owe anything (we'll let him starve a bit searching for Manihot or eat the specimens). However, we earnestly hope that we can get a joint paper out with you on the Iris study.

Best of luck on the "prelims" and waiting to hear from you.

Sincerely,

Henry S. Fleming
Professor of Botany

HSP:ch

*We are making up new deck, so you will be reciving
a new print out with correct internals.*

— *Montana State University* —

Bozeman, Montana 59715

Tel. 406-587-3121

Department of Plant & Soil Science
College of Agriculture

21 May 1966 MAY 24 1966

c/o Rancho Santa Ana Botanic Garden
1500 N. College Ave.
Claremont, California 91711

Dr. D. J. Rogers
Taxonomy Laboratory
Dept. of Botany & Plant Pathology
Colorado State University
Ft. Collins, Colorado 80521

Dear Dave:

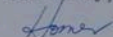
Just a note to let you know that I got back to Claremont OK yesterday afternoon. Henry's recommendation that I hustle down the Valley Hiway at 70 proved to be just about right, as I got there in time to turn in the car & board the plane without any protracted waiting around in the Stapleton terminal. After that it was a case of hurry up and wait, as there was quite a long layover between planes at LAX, so I was able to take something of a walking tour of the place, getting better acquainted with it than previously.

I want to express my deep and sincere appreciation of all the attention, courtesies, and services you, Henry, George, and Bob have shown me in working on the iris data. My appreciation also goes to Connie and Mary for their hospitality. My most pleasant sojourn under the blue skies of Colorado was a welcome change from several weeks under the smog of the L.A. Basin. Believe me, I wouldn't want to live down here regularly unless I could find an area outside the smog belt to live and work.

I'll be expecting an appropriate bill from you in due course. It might help you hunt for more Menihot.

Hasta la revista.

Sincerely,



H. N. Metcalf
Professor, Horticulture

hnm

— *Montana State University* —

Bozeman, Montana 59715

Tel. 406-587-3121

Department of Plant & Soil Science
College of Agriculture

c/o Rancho Santa Ana Botanic Garden
1500 N. College Ave.
Claremont, California 91711

10 May 1966

Dr. David J. Rogers
Taxonomy Laboratory
Dept. of Botany & Plant Pathology
Colorado State University
Fort Collins, Colorado 80521

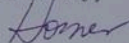
Dear Dave:

The way the schedule for the trip to Colorado is working out, it now appears that I should show up in Fort Collins on Tuesday, the 17th, and be able to spend Tuesday & Wednesday, if desirable, going over data with you. Present plans call for me to fly back to L.A. from Denver on Thursday, just before 1 PM.

I'd much appreciate your making motel reservations for Tuesday and Wednesday nights for me. The same place as before would be fine.

Thought you'd appreciate a bit more definite word on my schedule.

Sincerely,



H. N. Metcalf
Professor, Horticulture

hnm

— *Montana State University* —

Bozeman, Montana 59715

Tel. 406-587-3121

Department of Plant & Soil Science
College of Agriculture

15 April 1966

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

Dear Dr. Rogers:

After protracted delays, I am enclosing what I hope will be a start on material towards a cluster analysis of my iris data. I've had to be involved in working up our 1965 potato breeding information, in planning a potential new building, income tax preparation, etc. etc., so that I'm already a month late in getting away for Claremont. Also, we've fired our secretary, so am currently my own typist.

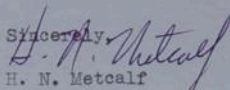
Enclosed you will find a set of 14 scores, written on computer data sheets, and described on accompanying pages. To back these up, I'm sending the data for totals, dividing factors for means, means etc., which I hope may make it easier for us to correspond about them, or converse over the phone.

In order to arrive at a reasonable set of criteria for scoring the morphological measurements, I felt it necessary to compute the means and base the scores on them. This meant many hours with a desk calculator & adding machines.

Some of the data have been converted to ratios, to reduce the number of items of input. In some cases this did interesting things to the frequency distributions (also supplied). Not being sure whether we are more apt to find things of interest in normal type frequency distributions than in multi-modal ones, I have included data derived from both types in the scores.

There may be too many factors for construction of efficient matrices. In this case, would it be wise to construct a matrix involving say, only spathe valve characters, or alternatively (or additionally) ones involving principally sepal, petal characters, etc. I would be guided by your experience in this regard. Additional ratios beyond those I computed could be prepared, but thought these enough for a start. I'm having all the raw data put on Porta-punch cards to facilitate checking the means, ratios, etc. for descriptive purposes. This will result in a file of 5090 cards.

As soon as I get this in the mail, I'm packing my mental whisk-broom and leaving for Claremont, where I expect to be until sometime in June. You can get in touch with me at Rancho Santa Ana Botanic Garden, 1500 N. College Ave., Claremont, California 91711. Potentially, I should be able to make a trip to Ft. Collins sometime in May, if desirable.

Sincerely,

H. N. Metcalf
Professor, Horticulture

hnm

- Taxonomy Laboratory

May 4, 1966

Professor H. N. Metcalf
c/o Rancho Santa Ana Botanic Garden
1500 N. College Ave.
Claremont, California 91711

Dear Homer:

We are remiss in not even responding to the receipt of your data. You have surmised correctly that we let pressing matters push out even post card acknowledgement.

We have looked at your data, it is in good order. We had questions about the possibility of ordering some of the states, but we feel that we can make some educated guesses about how to do it. I think that a good way to proceed is for us to make the appropriate statements about characters, run the data, and have available for you when you come (sometime at your convenience near May 16) a print out for you to examine. Then if we have goofed in our assessment of the characters and how they may be ordered, you will have the opportunity to straighten us out and get another run very quickly. Thanks for your patience. We hope this procedure will be useful, and we look forward to seeing you on whatever date near May 16 is best for you.

Sincerely,

David J. Rogers
Professor of Botany

DJR:ch

— *Montana State University* —

Bozeman, Montana 59715

Tel. 406-587-3121

Department of Plant & Soil Science
College of Agriculture

c/o Rancho Santa Ana Botanic Garden
1500 N. College Ave.
Claremont, California 91711
2 May 1966

MAY 4 1966

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

Dear Dr. Rogers:

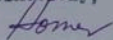
I've been in Claremont about two weeks, already, and time seems to be slipping by rapidly. Probably too rapidly, as I'm scheduled for the qualifying exams here during the last week of May. Seems to be a great deal of material to review that I haven't given much thought to for some time.

I noted that both you and Henry were scheduled to give papers at the AAAS meeting at Las Cruces, and between getting these ready, and giving your new course, expect you've been well occupied.

However, I'm wondering if you've been able to do anything with the data I sent you before leaving Bozeman. Do you find any need for additional preparation of the data on my part? I have to come to Denver for another purpose on 16 May. Would it be convenient for you to discuss results of the data analysis either just before or just after that date?

It's hot & smoggy in the L.A. Basin today.

Sincerely,



H. N. Metcalf
Professor, Horticulture

hnm

Montana State University

Bozeman, Montana 59717

Tel. 406.587.3111

Department of Plant & Soil Science
College of Agriculture

3 January 1966

Dr. David Rogers
Dept. of Botany
Colorado State University
Ft. Collins, Colorado 80521

Dear Dr. Rogers:

Over the past year or so I have accumulated a considerable volume of morphological measurements and means derived from flowering organs of *Lotus*, *Medicago lupulina*, a highly polymorphic group having an extensive range in western North America.

I am interested in finding sites and sufficient means of analyzing these measurements and, we believe that computer applications will be about the best way to achieve a reasonable analysis. With I am not knowledgeable of computer techniques, I believe I have no appreciation of their potential.

Would it be possible to have an appointment to discuss this matter with you in Feb. I have questions in your February or early early March, another meeting perhaps would

Respectfully,

 D. H. Howell
 Professor, Botany, MSU

LH

Mar 4-11

- Taxonomy Laboratory

February 11, 1966

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

Dear Professor Metcalf:

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

Looking forward to seeing you.

Sincerely,

David J. Rogers
Professor of Botany

DJR/pam