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

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

October 20, 1969

Dr. Arnold Kluge
University of Michigan
Museum of Zoology
Ann Arbor, Michigan

Dear Doctor:

We have an efficient key punch operator ready to correct (clean-up) your BOOKs.

Please correct them and send us the corrected version soon.

Sincerely,

Gil Hersh

GH/ms

Taximetrics Lab.

October 10, 1969

Dr. George W. Nace
The Amphibian Facility
Department of Zoology
The University of Michigan
Ann Arbor, Michigan 48104

Dear George:

This is just a note to respond to your letter of September 29, an to let you know that we will be responding shortly to that letter. Unfortunately, we have, at this moment, a heavy burden of proposal writing, and will be finished with this job shortly.

I hope that by Monday, October 20, you will have a proposal in your hands.

Sincerely,

David J. Rogers
Professor of Biology

Taximetrics Lab., Armory 101

October 3, 1969

Dr. C. H. Hendershott
Division of Horticulture
College of Agriculture
University of Georgia
Athens, Georgia 30601

Dear Dr. Hendershott:

We have recently received the contract for signature between the University of Colorado and the University of Georgia, as well as two checks. The former is going through the channels and we hope to have it signed soon, and the latter is very much appreciated for your fine actions on it.

I enclose the necessary statement for the next quarter of Mr. Appan's pay. I suppose that it will facilitate matters for you to have the signed contract back before you take any action with the enclosed statement.

After our trip, I began to think of other ways than the survey in which we could serve the University of Georgia, and one idea came to my mind that you may want to consider. I would recommend that, if at all possible, you and several of the others involved, spend a couple of days here, so that we could intensively go into the agricultural and biological aspects of the crop. I could then give you the benefit of all the data I have gathered, as a sort of foundation for the continued work of your group. I would also like to have your economist come along, because we have informally but intensively been at work in that area, one which will be of considerable importance in the final analysis to be presented to AID. If this idea appeals to you, and can be managed, let me know and we can set it up at some time convenient to all.

Sincerely,

David J. Rogers
Prof. of Biology

Encl.

November 25, 1969

Dr. Douglas Caton
Agency for International Development
Department of State
Washington, D. C. 20523

Dear Dr. Caton:

We have been trying to arrange a time when members of the University of Georgia team on cassava studies (Under Dr. Hendershott) can get together here for a couple of days seminar on the crop. Tentatively, we have arranged for a two-day visit here in Boulder on December 15-16, and we would be happy if you cared to join us for those sessions.

I plan to give the Georgia team as complete a background as I can on the crop so that they will have more to go on when they make their travels. Information will cover not only the agricultural and biological problems, but also what we know of the economics of the crop.

If you can join us (provided the suggested time is amenable to the Georgia team) please feel free to do so. Also, if anyone else there wants to come along, they are welcome. Please let me know, and I can make arrangements to meet you, and make a reservation at a motel for you.

Sincerely,

David J. Rogers
Professor of Biology

DJR/mgslly also invited.)

DJR/mgs

November 25, 1969

Dr. Omer Kelly
Agency for International Development
Department of State
Washington, D. C. 20523

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David J. Rogers
Professor of Biology

(Dr. Caton also invited.)

DJR/mgs

November 25, 1969

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Agency for International Development
Department of State
Washington, D. C. 20523

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Sincerely,

David J. Rogers
Professor of Biology

(Dr. Caton also invited.)

DJR/mgs

November 24, 1969

Dr. John Popenoe, Director
The Fairchild Tropical Garden
10901 Old Cutler Rd.
Miami, Florida 33156

Dear John:

Enclosed is the first response to our appeal for germ plasm of Manihot - only some eleven months later. Maybe something will come of this fellow's letter, but I'll believe it when I see it.

We haven't given up on Manihot, but have been so busy just finding sufficient funds to keep alive that there has been no opportuhity to do any honest work. Howard Irwin (NYBG) is going back to Brazil in January, and we hope he will turn out some good seed material for us. On his last trip, he was in Manihot territory at the wrong time of year.

Hopefully, I will get some travel money next spring, and can get some specimens myself. How are the plantings there? Did the M. pringlei survive?

Best regards.

David J. Rogers
Professor of Biology

Taximetrics Laboratory

11/5/69

Professor Morris E. Garnsey

Dear Morris:

With respect to the Man-Environment seminar (program) I suggest that four committees be formed to address the four principal tasks which are involved in planning an expansion of the Program.

These committees or working groups should be samll in the number of members and consist of those persons who can devote some time to the work of the comittee.

The first committee should address the problem of the collection, collation, coordination and dissemination of information concerning on campus (and peripherial) interest, suggestions and on-going activities which might relate to the Program.

This committee actualy exists at this time and is headed by Dr. C. Johnson/ It should be under the overall structure of the current M-E Program, however.

The Second Committe should address the problem of Program Policy. What are the alternatives open to this type of expanded program. What should be the goals and directions of such a program. This committee should study this problmem with respect to the interestes of themembers of this University Community and then report alternatives to the overall Program Group.

The Third Committee should be concerned with the functional and administrative Organization of the overall program once the policies have been made more or less clear. Organization should concern itself with the best manner to set up information flows and lines of communication among all interested parties.

The Fourth groups should address the problem of funding and financing/ First to research the funding possibilities for an expanded program. Second to consider how these funds might be obtained and third, once the organization of the expanded Program is agreed upon to make proposals and presentations to receive funds.

Most of the this work should begin at once. Information and an examination of the feasible alternative should be studied as soon as possible.. No reasonable Program development can be expected without sufficient information to which committees one and four must address themselves.

All of this work should be carried out under the present organization of the Program of Man-Environment under your direction.

11/3/69

To: Lary Blick
From: Gil Hersh, Taximetrics

RE: Report to Manning

1. Dr. Manning wants a copy of the HUD proposal. I think it is important that he receive one immediately and be informed that more are on their way for the other aministrators- (The Office of Research Servies, Dean Crowe, Dean Briggs).

2, I think that a letter fro Mr. Tedesco to Dr. Manning reporting0 - -so to speak- on our involvment in the writing and preperation of the proposal.

such a report might include the following points:

1. The Taximetrics Laboraorty threw from 4 to 8 people into the project over a period of 8 weekss.

2. That the people basicallŷ involved spent somewhere in the area of ~~160 man-hours and meeting and facilities~~ 160 man-days and other facilities.

3. That the Taximetrics Laboraotry has been crutial in the writing of the grant with an eye to success and that the lab did a good overall ǫob as well as a ~~gr~~ good job in its specific fields of responsibility vis a vis the Project.

4. That we understand from our contacts that few other Universitŷes have submitted s an extensive or as a meaningful section with respect to the Proposal. This should enhance our chances of success.

5. From previous work with the Lab we appreciate both what they have developed in the way of interesting and new systems and the manner in which they attempt to have them employed and installed in all ǩinds sof situations.

3. We will need about 5 or 6 other proposals for our use and distribution.

would you autograph mine??

gil

11/3/69

To: Lary Blick
From: Gil Hersh, Taximetrics

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   would you autograph mine??

   gil

December 23, 1969

Dr. Edward Weiss
Office of Science Information Service
National Science Foundation
Washington, D.C. 20550

Dear Dr. Weiss:

With respect to our phone call this morning to you and Mr. VerDerber we wish to ask for a 6 week extension of our grant GN 656. This request concerns the use of funds for computing services only and affects no other aspects of funds which will be frozen as of 31 December.

We have a series of tests to run especially for cost/effectiveness analysis of the TAXIR system which are essential the successful completion of this project. We are running a bit late in this work for several reasons 1) down time on the University Computer recently, 2) the Holiday vacation schedules both of our staff and the University staff and recent necessary travel made by some our staff.

We have to work on hand delivered batch process at the University and have no way of working through a remote terminal. Turn about time has been longer these days than is usually the case which has added to our delay. This coupled with a CDC systems change of a month ago has further added to our schedule back up.

The information from these tests is necessary for inculsion in our final report which would be delayed by a period of three to four weeks as well.

Your assistance in this matter is essential.

Thakk you for your time and cooperation.

Sincerely,

David J. Rogers,
Professor of Biology and
Project Director

DJR:gh
cc: ORS

Taximetrics Lab., Armory 101

December 8, 1969

Dr. John Reeder
Botany Dept.,
Univ. of Wyoming
Laramie, Wyo.

Dear John:

Finally, the review you asked for some time ago. I've spent quite a bit of time on this one, and the review is unfavorable.

Some how or other we have to stop all the "data-diddling" that's going on in numerical taxonomy, where guys unwittingly dump unassorted garbage into somebody's computing programs, then try to make "sense" out of it and then compare the results with a former, existing classification. Usually, such papers parade under some such title as "an objective comparison of computer methods with classical taxonomy," and they miss the mark both in terms of objectivity and in comparison.

So, I spent some time here trying to point out where the authors failed to accomplish their objectives, and why such papers are absolutely worthless to the taxonomic community. They do not decide on what is a good numerical method, and they do not solve any taxonomic problems. I, therefore, see no reason for publication.

This isn't to say that we don't need good mathematical models and computer programs--we desparately do for such complexes as the Solanum one. I believe we've just about got such models coming up here. But I also believe that we should keep our mouth shut until we prove our case. Proof, to a taxonomist, is a well-worked monograph, or good floristic paper. What else makes sense? We're coming out with a computer-based classification of Manihot. It is book-length, and we're negotiating with the Colorado Assoc. Univ. Press right now. Hopefully, we can get it published in extenso. That isn't easy, for taxonomic work, as you know.

Merry Christmas!

David J. Rogers

Critique of "A Numerical Taxonomic Study of
The Mexican Species of Solanum, section Tuberarium"

1. Has the material been published previously in the same or similar form?

Studies on Solanum, using some sort of statistical technique have been published before, (Heiser, et. al., 1965) and, there have been many "objective" comparisons on other groups made between the statistical methods and some "classical" classification, and most are invalid, for the following reasons. (1) The statistical methods are based upon some assumptions which may or may not be valid, and these assumptions are quite different from the assumptions expressed or inherent in the classical methods. (2) The authors do not state the bases for accepting one or the other statistical package, and leave the reader to guess why they chose the ones they did. (3) There is no effort to explain (if they indeed can) whether the mathematical bases underlying the statistical assumptions are satisfactory for the purposes of taxonomy.

2. Has the research been carried far enough to warrant publication?

Clearly, it has not. There are no clear-cut decisions as to the arrangement, or re-arrangement of the taxa included in the section, and this is the only basis for publication of some work in BRITTONIA. We are left at the end with no decision which method of numerical taxonomy is valid, if any, nor why there are differences in the output. If the information of the species taken from cultivated specimens differs from the information on the wild-grown plants, we clearly deserve to have these differences reconciled. Why, one asks, were not both sets of data run together?

The authors do not seem aware that, having used nearly 50% of the information on leaf characters, they have heavily weighted the classifications toward the vegetative information about the plants. This, in itself, could help to explain why their various results do not agree with the earlier classifications where I am sure the authors considered reproductive or chromosomal differences uppermost. Furthermore, in nearly every case of the measurement characters, they have duplicated the character in such a way that they have introduced false biological weighting. It is not conceivable that there are that many independent genetic systems at work in determining leaf dimensions, and if our classifications, numerical or otherwise, do not make some effort to reflect genetic variation in the plants under study, we have no reason for calling ourselves taxonomists. However, if the authors had clearly stated at the outset that their objectives were to build a classification predominantly on information from the leaves, then they may proceed, and all will understand them.

3. Is BRITTONIA the most suitable journal for the publication of this material?

No, nor any other, considering the present state of the work.

4. Illustrations--

The "phenograms" by themselves lose much of the information about the interrelations of the objects (OTUs), where there seem to be no connections of any of the species to any other than the primary ones at the ultimate ends of the arms. In such groups as Solanum, a good taxonomist will suspect some reticulate relationships to exist, and the phenogram does not give one any picture of this type of relationship. The Pehnogram is a gross over-simplification of the relations. There are techniques for indication of such relationships, though not in any of the techniques here employed.

5. Are there errors of facts, interpretations, and/or calculations? Is the material presented logically?

I believe the authors have committed errors of interpreation of the statistical methods which they employed. The methods were intended to be used on data with dimensions--length, width, weight, etc., and the authors have chosen to force some dimension on their qualitative characters where indeed no dimension exists. But more fundamentally, I do not think that the authors have a sufficient idea of the taxonomic rules they accept in making classifications nor whether the mathematics of classical statistics is indeed the valid mathematical base for classificatory purposes. One can understand how a taxonomist may not have investigated the literature of mathematics sufficiently to know the bases of this or that ' mathematical approach, because most taxonomists are not sufficiently well-trained in the various branches of mathematics. This does not, however, excuse the taxonomist who employs some mathematical manipulation technique because he is calling into question decisions made by taxonomists not using such techniques.

6. Were the methods employed adequate to yield the results given?

Yes, but the results are not useful because we do not know why the results exist. Several examples are needed here, taken from the paper. Ex., page 16, 3rd para. "Curiously, S. polyadenium (OTU 15 series Polyadenia) shows its relationship to S. polytrichon...." Why is it curious, if one can trace the calculations all the way through the mathematical process, one should be able to say precisely, and there should be no "curiosity." Ex., page 17, 3rd para., 3rd sentence: "This suggests that the species of series Pinnatisecta are relatively unaffected by cultivation in northern latitude on the plants. It is inconceivable that a whole section of plants, with at least 6 species, would be unaffected by the latitude when much of the rest of inconsistencies in the relationships are claimed to be based on such variations in the plants. Ex., page 23, 2nd para., 3rd sent.: "Inexplicably, the relationship of the species in series Pinnatisecta...." The authors forget that they are dealing with another clustering method from earlier ones, whose basic objectives are different. It is the methodology that they have not explained, and this forms the basis for the difference. Here again, I emphasize that the authors are obligated to know what the different methods are doing, before they try to make some explanation of the differences in the biological factors.

7. Is the literary style clear and concise? Are there errors in grammar? Could the material be condensed?

The style is all right, but the necessary contents of the paper leave something to be desired. We have not received, in a precise statement, what each of the

various methods are intended to do. I think the authors are obligated to show how the various methods are related, and by some flow-chart, show the processing of the information from beginning to end. If one looks to the references given by the authors, one finds that there are no precise set of flow-charts given there either. At the present state of the art, I feel that numerical taxonomists are obligated to give the reader more of a straight-forward knowledge of what the statistical methods are based on, how they operate, and how the results are to be interpreted. Certainly the readers of BRITTONIA are not all aware of what various activities are going on in num. tax., and the authors are obligated to provide a clear description.

There seem no glaring errors in grammar. Clearly, the paper does not need condensation--it needs to be completely recast.

8. Is there evidence of an adequate familiarity with the pertinent literature? Is the bibliography carefully prepared to conform to the style as set forth in current issues of BRITTONIA?

Had the authors been aware of the content of two well-known papers, I do not believe they would have chosen the methods they did. They should read a paper by D. C. Eades (1965) in Systematic Zoology 14: 98-100, entitled: "The inappropriateness of the correlation coefficient as a measure of taxonomic resemblance." This paper clearly points out the invalidity of the correlation coefficient as a means of similarity measure of taxonomic purposes. The second paper, by Jardin, et. al. (1967) in Mathematical Biociences 1(2): 173-179, entitled The Structure and Construction of Taxonomic Hierarchy, mathematically proves that the only clustering method to be employed for taxonomy is one based on single-linkage methods, as first suggested by Sneath, P. H. A. (1957) Applications of computers to taxonomy. J. Gen. Microbiol. 17: 201-226, and employed by Wirth, Estabrook and Rogers, 1966, A Graph Theory Model for Systematic Biology, etc., Systematic Zoology 15: 59-69, and Irwin and Rogers, 1967. Monographic Studies in Cassia, etc. Mem. N. Y. Bot. Gard. 16: 71-120.