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Some Further Considerations on the Origin of *Manihot esculenta* Crantz[1]

David J. Rogers[2]

Since my earlier papers (Rogers 1963, 1965) on the areas of origin of the cultigen *Manihot esculenta*, there seems to have been a renewed interest in the topic, and some more recent concepts concerning this interesting and difficult problem. The following comments are intended to bring this new thinking into focus.

First, I must make some comments about classification. This is necessary because we had to limit our own objectives on the type of classification we have produced for the cultivars of *Manihot esculenta*, (Rogers and Fleming, 1972). Obviously, there are many types of classification, such as those intended for classification by uses, as could be found for the commodities in a grocery or hardware store, or classification by chemical products of a plant, a type which has gained some ascendancy in biological circles in recent years, or classifications based on ethnological considerations. Which of these is "true?" All of them, or none of them, depending on one's requirements. Botanical classifications may be useful in describing the modern-day relations between various members of a taxon (say, a species or a genus), without any objective of determining the phylogenetic pathway by which the objects (cultivars) within the classification became so related.

Second, I include some comments on the content of cyanogenic glucosides in the *M. esculenta* cultigen, more to open up the discussions than to present any new "truth" about this important substance.

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[1]Paper presented at the annual meeting of the American Anthropological Association, Philadelphia, Penna., November, 1971.

[2]Department of EPO Biology, University of Colorado, Boulder, Colo., 80302

Now, about our classification: our work has the objective of describing the cultivars of M. esculenta using descriptors (characters) of external morphology. We attempt to define the species using an important biological concept, that of the "biological species," which says that any freely interbreeding plants belong to the same species. This we clearly have to modify to some extent because man has been influential in extending the boundary of "freely interbreeding plants" by artificially cross-breeding other species with the cultigen. Our immediate objective was to try to determine the "boundaries" of the variation, that is, what we can include under the binomial. Given this description, we should have a better take-off point to flesh out this bare-bones botanical classificatory structure. My associate, Henry Fleming, and I have described (or erected) 19 groups of morphological similarities within the cultigen, so that we can begin to work on the problems of how these groups have been evolved, or, in another way of looking at it, what inheritance patterns have been active to produce the groups.

Our input to the classification are 15 characters describing the cultivar populations from most of the areas of growth in Latin America. There seemed no reason to include population samples from other regions of the world, simply because the crop originated in the Americas, and any cultivars found outside this large area can only be combinations and recombinations of the genetic material found within the Americas, with perhaps a few minor mutants not necessarily represented in the New World tropics. The characters represent the best we could do under the circumstances, and there are good reasons for this. We had to select characters which were sufficiently genetically stable to reflect more than just environmental variability. Limitation on time and funds, two very pragmatic problems, prevented us from gathering more interesting data.

Our classification does not recognize differences between "bitter" and "sweet" manioc for the simple reason that there are no characters employed in our study which coincide (correlate) with the classification based on this one biochemical variation. We recognize that there are important reasons to know about the differences, but we also know enough about the biological aspects to know that there is a continuous variability of content of the cyanogenetic glucoside, and also, that the CN^- content varies within a single cultivar raised under different ecological circumstances. If a native grower can recognzie the differences, it is simply because he has acquaintance with only a very small segment of the total variability within the species, enough so that he can be safe when he selects a particular root to use as a boiled vegetable, or another that he has to process by various methods of grating, squeezing, sifting, and drying (sometimes with fermenting techniques thrown in at the beginning, sometimew without). However, if I ask a native to come with me to a large collection of cultivars (say, 200) which are growing together on an experiment station, and ask him to tell me how he distinguishes the bitter from the sweet, I discover that his recognition techniques soon fail him.

Botanical Characteristics of M. esculenta. Encountered within the species are a large variety of shrubs, from low, many-branched plants no more than 1 meter tall, to tall, unbranched plants 3 to 4 meters high. In habit they may be sprawling, inclined, or erect. All parts of the plant contain varying quantities of the cyanogenetic glucoside. The roots vary in shape and size, from globe-shaped to carrot-shaped to spindle-shaped. Though the roots are mostly round in cross-section, some are irregular. The roots are variously colored externally, largely dark brown, but some are only slightly pigmented, appearing light tan. The root surface is

either smooth (smaller percentage of the cultivars) or roughened (the larger number of cultivars), and there is little intergradation between these two. Internally, the roots are most frequently white, some cream, and a few have yellow flesh; there are occasional variants with pinkish-colored flesh. Interestingly, those plants grown from seed have only a single large tap root, and secondary roots are mostly fibrous, with few tuberous swellings. The edible roots are very largely adventitious, from plants propagated by stem cuttings. Stems usually have large swellings at the nodes, but these may be only moderately developed, or the stem may appear smooth, without swellings at the nodes. Colors of the stem are useful distinguishing characteristics, being either brown, yellow, red or light gray (silver). The colors of stems are correlated with colors of the roots - the darker pigments go together (and the roots of these groups are all roughened) and the lighter pigments of the root are nearly invariably associated with the light gray (silver) stems. No relationship has been detected between these correlated characters and the content of prussic acid. Leaves are usually deeply palmately lobed, though some unlobed, simple leaves are associated with the inflorescences of the plant. The lobes vary in number from 3 to 9, 10, or 11 lobed, the most common being 7 lobed vegetative leaves. Lobe shape may be linear to obovate or pandurate. Pigmentation of the petioles are striking, reds to yellow-green to dark green. Young foliage at the growing apex is typically green, but may be strikingly reddish-purple, through lighter shades of bluish-green to green. The inflorescences are typically monoecious panicles, with the female flowers basal and the staminate flowers on the upper portions of the panicle. Female flowers of any one inflorescence open first, before the staminate of the same inflorescence have fully developed. This feature is important in the production of the great variability of the species,

because it insures cross pollination, which is usually accomplished by insects. This insures a heterogenous gene complement in any one cultivar. The fruits are capsules, with three seeds per capsule. At maturity the fruits dehisce explosively, throwing the seeds some distance. Viability of seeds is variable because the pollen of some cultivars is sterile.

Distribution and relationship of botanical characters. To date, we have found no relationship between any of the above characters and any one geographical location. In our computer clustering procedures, we have found some faint relations between groups of cultivars and their geography, but so far, we have been unable to pin-point the botanical characters mentioned above with the geography. For example, the Central American cultivars seem to have a more glaucous "bloom" on the foliage, but this characteristic fades imperceptably into other areas. Plants of northeastern Brazil seem to tend towards lower, more frequently branched plants than in other regions, although there are some very tall, unbranched plants in this region. No other such correlations have been detected.

Relation of the cultigen to wild species of the genus. There is no doubt that M. esculenta is a distinct species, usually fairly easily distinguished from its wild congenors, but the easiest determinant is whether the plant is cultivated or not. Though a few other species are cultivated for their rubber-producing capacity, we know only that the cultivars never seem to grow as a part of the native vegetation. Old fields of cassava left to go back to bush are soon populated with a number of herbaceous and woody plants, and the cultivars seem to have no competitive capacity.

Many wild species of the genus Manihot have characters similar to the cultigen, in one respect or another. But by combinations of charac-

teristics, these can usually be distinguished from cassava. The closest relationship we have discovered is with Manihot aesculifolia, a variable species from Mexico and Central America. Its range is coincident with limestone derived soils down the west coast of Mexico, all the way to Panama, crossing the Isthmus of Tehuantepec to the Yucatan Peninsula, and southward into Guatemala. There are several ecotypic variations within this species, some of which bear a remarkable resemblance to M. esculenta, but the technical characters help to differentiate it. Also in Mexico, M. pringlei (northeastern), M. rubricaulis (western, mountainous), M. angustiloba and M. davisii (most northern and western) show greater or lesser relations to the cultigen.

In South America, in various areas, there are species of somewhat similar morphology to various cultivars. In northern areas, along the very dry coast, just adjacent to the coast of Colombia and Venezuela, M. carthagenesis is found. This species differs from the cultigen in that it has a well-defined dormant period during the dry weather when all leaves drop. This species is extremely poisonous, and would require very well developed techniques to remove the cyanogen. In the Guianas, M. tristis, ssp. saxicola has proved to be closely related because of hybridization work in Africa and Java, where crosses to M. esculenta were made to improve resistance to virus diseases. In Pernambuco, M. epruinosa and M. caerulescens ssp. caerulescens have very similar morphologies to the cultigen. M. leptopoda (formerly M. palmata), M. janiphoides, M. inflata, and M. pilosa, all from the moister areas in Rio de Janeiro, Minas Gerais, Bahia, and São Paulo have habits and morphologies reminiscent of the cultigen.

In addition to the above-named species, there are several others which could share genetic material with M. esculenta. The only reason for citing each of these is to indicate that I believe there to be no barriers to

hybridization with cassava, and further, we have either direct or indirect evidence that some part of the total genetic constituency of M. esculenta has been contributed to by these species. An intriguing hypothesis has been developed by Jack Harlan that many wild species are nothing more than genetic recombinations developed out of the cultivated complex. If this is true, then we have to be extremely cautious about the statement that this or that wild species is a potential progenitor to the cultigen.

The above information and hypotheses do not give much hope that the botanical evidence, at its present stage, can shed much light on the precise origins of this cultivated species. My own earlier suggestions that the first cultivation of M. esculenta was in Meso America is no better or worse than others - Northeastern South America (Sauer), Brazil (Vavilov and earlier, DeCandolle). Having searched (rather superficially, I fear) the literature on archeological references to cassava, I have no reason to think that there is any real evidence for any of the above areas as first choice. The only real evidence that cassava was a part of an earlier culture is that from the Peruvian coast (Sauer, 1951), where beautifully formed pottery reflect what could only be cassava, and no one has taken that area seriously as a candidate for the origin of M. esculenta. All the other evidences are very tenuous. For example, the seeds found in a Tamaulipas cave by McNeish (1958) could have been any one of several species of the genus Manihot, and not unrealistically, could have been from any number of other related genera in the Euphoribiaceae, whose seeds have very similar morphologies. The griddles of Colombia (Reichel-Dolmatoff) do not convince me that they are necessary indicatiors of cassava cultivation because any number of starchy sources from a variety of roots could be treated in a similar manner.

This leaves me only with my own logic to go on, and we know how faulty

that logic can be in dealing with the complexities of agricultural origin. But for what it's worth, here is what I thought: the people of all the New World had their origin in migrations out of Asia, via the Bering Straits, then overland southward down through continental North America, then to South America. Coming down through California, the first possible Manihot species they would have encountered (given climates and distributions essentially the same as now) would have been M. angustiloba and M. Davisii, then M. aesculifolia, and possibly M. rubricaulis. Had the people taken a more eastern route southward through present New Mexico, and West Texas, they could have encountered M. pringlui along the slopes of the Sierra Madres Oriental, a fit candidate for early use because the toxic substances seem to be minimal in this species. From these, and perhaps, even M. esculenta itself somewhere in North America, would have begun the development of the cultigen.

My logic further says that North America had to be the place where cassava was first taken into cultivation simply because the people had to cross these areas before getting to South America.

But further speculation leads me to reject this, in that long before the arduous tasks of agriculture began, the migrations had occured and that people in any of the hypothesized areas could be responsible.

Where does this leave us? Still with many doubts.

Before ending this discussion, I would like to say something about the poisonous properties of the cultigen, and some more speculation about how the Indians learned to deal with it.

As with other properties of this cultigen, the biochemical property of cyanogenic glycosides varies. Therefore, to treat the species as either "sweet" or "bitter," covers up in our thinking some important problems. It seems to me that the poisonous property is not necessarily the most sig-

nificant one which drove primitive peoples to invent the rather arduous process of peeling, grating, fermenting, sieving and heating (some of these stages may be omitted, or they may not occur in this order, but the ones listed are the general ones). To my way of thinking it was far more important that needs for some way of storage of the crop drove the development. The natives used the processed casabe or farinha to tide them over lean periods, or to provide sustenance on long journeys, or as subsistence for troops. It may be (as frequently reported in the literature) that the so-called bitter cultivars produced more carbohydrate, and this reason could help to account for the maintenance of the cultivars with the higher content of cyanogen.

Another line of reasoning seems to be worthy of investigation. The raw root has very little but starch in it. By various fermentation processes, (which are generally distributed wherever the crop is raised) . conversion of some of the starch to other products would be attractive. It is hard to think that the natives consciously developed such processes because they knew that the converted product contained better nutrition (such as sugars, organic acid flavoring, and some protein), but fermentative processes are ubiquitous in food processing in many parts of the world. A line of research that seems to me to be very attractive would be to determine whether two effects are produced by the fermenting organisms: one, whether the micro-organisms themselves do not use the nitrogen from the cyanogen to produce microbial protein which adds nutritive benefits to the finished product, and two, the nitrogen so used helps to eliminate the poisonous properties. Research to date has examined either one or the other of these two activities, but they have not been put together, nor have there been attempts to follow (or emulate) the processes used in any native culture. We need to work with specialists in microbiology and others in

biochemical nutrition studies to get at the answers to these questions.

Along with the above considerations, I would caution those who have chosen to say that the presence of griddles in archeological digs is a positive indicator that the makers of the griddles were the only ones to process or cultivate bitter cassava. Dr. Dole's finds on the various types of implements used in processing cassava point to the fact that the non-griddle making people also processed cassava, but with implements which would not lend themselves to preservation in archeological digs.

Summary

The botanical evidence is still too tenuous to provide exact data on the progenitors of, or points of origin of the cultigen, Manihot esculenta. In each area of cultivation, numerous wild species are found which can, and apparently do, hybridize with the cultivars growing in that area. Such newly hybridized forms are "new" and constitute a point or origin. The most closely related wild species to M. esculenta is M. aesculifolia, which is widely distributed in Meso America, but other wild species are also closely related, and these are found in many regions of South America.

Too much emphasis has been placed on the differentiation between the sweet and bitter cultigens, to the point where some other types of arguments have been submerged in our thinking. The poisonous principle, a cyanogenetic glycoside, is found in many different plants which have become important food species, and this has not deterred the development of the crop. Fermentation, which is probably better designated as "microbial conversion," may play roles other than the ones which have been ascribed to it.

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(Sobre)

UNIVERSITY OF COLORADO

BOULDER, COLORADO 80302

Department of Environmental, Population and Organismic Biology

Oct. 27, 1972

Dear Frank:

In regard to contributions to the Newsletter, are you willing to accept a manuscript of 10 pages length, double spaced? I gave a paper last fall "On some further considerations on the origin of Manihot esculenta" which might be interesting to the readers of the Newsletter. Let me know, and I'll send it in, given your approval.

yes

What about short notices? I am now in the process of reading galley on a Monograph of the gems Manihot, to be published by Flora Neotropica (by Haffner Publishing Co.) and hopefully, the monograph will be in print by the time you are ready to send out the Newsletter. Perhaps it could be mentioned that the work is forthcoming. I have no idea what the price will be, but it isn't the sort of thing that I can send out free reprints off. It will be book-length. As soon as I can find out these pieces of information, I will notify you.

ok

I sent off some comments on the 5th section of the cassava work recently. You may have received it by now.

Sincerely,

Dave

David J. Rogers
Professor of Biology

Thanks for your comments on the 5th section of the study group report. I'm not sure when I'll get around to the next steps of the writing, but will take your suggestions into account. Pardon my informality this time.

Frank

1 January 1972

Carl Spath
109 Davenport Hall
University of Illinois
Urbana, Ill. 61801

Dr. David J. Rogers
Food Research Institute
Stanford University
Stanford, Calif. 94305

Dr. Rogers,

Thank you very much for the answers you were able to give to my somewhat lengthy letter of 1 December. I hadn't really expected that there were ready answers to many of the problems that were on my mind. I've devoted a rather excessive amount of time and study to the problems of manioc considering that within the foreseeable future my work will be in the Andean highlands where the crop cannot be grown. My agronomy paper is in a rather late stage of writing at the moment, in fact it is dismembered and scattered about the desk in front of me right now, so that it is rather impervious to any more active input for now.

The paper which I presented at the AAA meetings will, on the other hand, be rewritten and hopefully substantially expanded. I have been asked to prepare the paper for publication and therefore would like to incorporate a certain amount of additional information gained at the symposium. In particular I would like to be able to say something about the possible implications of microbial conversion, but I have been rather frustrated in my attempts to obtain the relevant articles, most notably the one by yourself and S. G. Appan in Tropical Root and Tuber Crops Newsletter. Could you offer some help in obtaining a copy of this particular paper? In terms of the abundant literature on microbial biochemistry I am at a loss where to begin or how much of it is really applicable to my interests.

I will be in Santa Cruz at the end of January. If time and circumstances allow I would be interested in taking the opportunity to come up to Stanford to meet you and perhaps have a short discussion.

Sincerely,

Carl Spath

Carl Spath

12/20/71

Dr. Carl Spath
109 Davenport Hall
Urbana, Illinois 61801

Dear Dr. Spath:

I am sorry not to have replied to your letter of Dec. 1 sooner. I, too, was disappointed not to be able to attend the manioc symposium, but I fear that my botanical knowledge about the species is not nearly as great as you might hope for. I'll try to answer some of your questions and beg off on the others.

The first of your questions dealing with variation, adventitious roots, and vegetative apomixis, has many things wrapped together. What is true is that the primary root from the seed develops into a single large tap root, whereas adventitious roots are those that develop from cuttings. The stems developed from plants grown from seed will produce the "normal" enlarged roots we are familiar with in manioc. The adventitious part has nothing to do with modified genetic behavior. Furthermore, I don't think that mutation plays much of a role in producing the variability within the species, simply because there are sufficient heterogenieties in the genes to get the many phenotypic expressions that are found.

The list of characters used in our classification study was inadvertently omitted from the paper you saw at the meetings. A copy is enclosed.

We are in the final stages of manuscript preparation of a monograph of the whole genus Manihot. An integral part of the monograph is a precise mapping of each species of the genus. This mapping is made from herbarium specimen data, and represents the most accurate information available for such mapping. I'm sorry I can't give you a date when the monograph will be published, but we hope that it will be no more than 12 months from now.

I do not know how prevalent sterile pollen occurs in the cultivars. I would guess not more than 30%, but that is a figure I don't hold much value in, simply because we haven't got the data. And further, whether this sterile cultivars are different in percentage from wild species is unknown. If we are dealing with feral species, these may represent very successful (and therefore highly fertile) populations derived from the cultigen. "Success" of feral species is again unknown. This type of species, if it does indeed exist, as I have hypothesized does occur, may have only a fleeting stability in disturbed habitats. The competitive advantage of such a species against "wild" species of other genera is unknown. When we get into this type of discussion, we have trouble even deciding what a wild species of the genus is. You mention "yuca brava" a term used in so many ways that I have no fixed opinion about whether "natives" are referring to a well established wild species, or those chance variants of the cultivars that are hanging on without apparent care from anyone. Both conditions may exist.

M. aesculifolia is found widely distributed in Mexico and Central America. It overlaps with the cultigen only from Yucatan and southward to Panama. It does not occur in South America.

You ask about a dormancy period, and in the same paragraph mention maturation in 3 - 7 months. To my knowledge, the species never goes into total dormancy, but does shed all but a top-knot of leaves in the drier periods (at least some cultivars, but perhaps not all). Whatever loss of leaves occurs is coincident with dry periods, but this is really quite relative, and quite variable. I have seen some cultivars with vigorous leaves in the dryest part of the distribution, in northeastern Brazil, when most other plants in the vicinity were without leaves. Incidentally, have you really found varieties maturing in 3 months? I had though the shortest possible term was 6 months, but I may have missed something you have seen.

In our monograph of the genus, M. saxicola Lanj. has been relegated to supspecific status under the species M. tristis, and M. melanobasis is a synonym of M. esculenta. What plants were actually used in the African breeding program are unknown, since the descriptions given of them do not fit M. melanobasis at all. Someone misidentified the plants used in the breeding experiments, and for this reason, the work done is not only suspect, but not reproduceable. Likewise, workers in Madagascar have claimed to use M. pringlei in their experimental crossing, but they are delightfully vague when describing the plants they call M. pringlei. Since I know that species to be a very narrow endemic in northeastern Mexico, with very spotty and limited distribution, I seriously doubt that they actually had that species available to them in Madagascar.

You ask for references on cytology and general botany. A good place to start would be with an unpublished master's thesis by Seymour H. Sohmer (a former student of mine) Taxonomical and Cytological Studies of some cultivars of Manihot esculenta Crantz. This thesis is available through the Graduate School, University of Tennessee, Knoxville, and was submitted in March, 1966. He has a pretty good discussion, and a fair bibliography. On male sterility, see Jos, Magoon, Sadasivaiah and Appan, 1966. Indian Jour. Horticult. 23(3&4): 177. Studies on Sterility in cassava.

Unfortunately, my own latest work on manihot is still in press. The one on the classification of the cultivars should be in galley proof very soon, in the journal Economic Botany. The generic monograph will be in Flora Neotropica, published by the New York Botanical Garden.

Now, I have not answered satisfactorily any of your questions, and I am sorry that I haven't, but at the moment, there is very little grant money available for continuation. Hopefully, this situation will improve, but that depends on my luck with the funding agencies, which hasn't been too good recently.

Sincerely,

1 Dec 71

Carl Spath
109 Davenport Hall
Urbana, Illinois
61801

Dr. David J. Rogers
Dept. of Biology
Univ. of Colorado
Boulder, Colorado

Prof. Rogers,

I have undergone some difficulty in the time since the AAA meetings trying to get this letter off. Janet Siskind could not, in the rush and bustle of the meetings, recall your address, so that I had to wait until asking Prof. DeVet to obtain your present address. --As an aside, since I failed to get a copy for him while at the meetings, I would appreciate it if you could either send me another copy of your paper for Prof. DeVet or send one directly to him. He was quite interested in having a copy.

I was particularly disappointed in your inability to attend the meetings, as there were a number of problems of the botany of manioc on my mind that noone else there was able to answer to. Though the symposium was in fact very good, the combined absence of yourself and Prof. Lathrap took a great deal of potential away from the discussions. Noone, for instance, was able to clarify the question of microbial conversion, which quite a number of people were interested in. The questions of botany seemed, by and large, to be restricted by my limited exposure to the field and theoretical biases, which I found highly disappointing. Though I have profited much by several of your publications and the guidance of Prof. DeVet, someone much better versed in the botanical aspects of manioc and its close relatives was needed.

My particular interest at present is in the mechanisms of variation in a plant such as M. esculenta under domestication--and the implications of this for domestication. I find the available literature on vegetative apomixis tenuous and disappointing. If, as you note, most of the edible tubers of the cultivar are in fact adventitious,

does this involve random fixation under long sustained clonal propagation or did it involve a more conscious selection of clones showing desired somatic mutations of this type? At this point I am somewhat confused by your statement that "plants grown from seed" have only the large tap root, when many of the hybrids and such described by Jennings, Nichols, and others in their East African breeding studies seem to possess the characteristic starchy tubers of M. esculenta. I had the impression that you were saying that the enlarged tubers were not transmissible through the sexual genetic material, but only through the somatic chromosomes of the cuttings.

I have a number of other relatively minor points about which I am unclear in your paper for the symposium. First I am curious whether the list of characters for classification which was noted as being at the end of the paper (but seems to be missing) is in fact available. I would be highly grateful for such data. Along related lines, I would like to know if there is available a more recent and complete map of the distribution of "wild" Manihot species than that included in your 1963 article.

- How prevalent, in fact, is the tendency for the pollen of cultivars to be sterile? Is this trait significantly more prevalent among the cultivars than among wild species, and does it in any way correlate with the more "bitter" strains as has often been suggested?

- The low competitive capacity of a weedy plant like manioc is clearly to be expected in tropical forest areas. Are M. esculenta and its feral seedlings equally unsuccessful in areas where wild Manihot species are endemic? Are you suggesting that manioc has in fact been reduced to the nature of a true weed under domestication and is no longer successful even as a colonizing species? What then of references of primitive tribes collecting from untended stands of "yuca brava" (which I have always thought translated more or less as feral manioc)?

- How closely does M. aesculifolia and its range of variation resemble the cultivated species?

- I had been given to understand that most strains of manioc--except those maturing in 3-7 months--had a fairly long, well-defined dormancy period during which the plant sheds its leaves. You indicate that this is not true of the cultivars. Could you clarify?

- Is your *M. tristis* ssp. *saxicola* the same as *M. saxicola* Lanj. and does *M. tristis* also encompass *M. melanobasis* Lanj.?

I would very much appreciate it if you could indicate a number of good references to clarify problems of the cytogenetics and general botany of *M. esculenta* and the mechanisms of variation in vegetative clones.

I am sending separately an earlier paper of mine discussing the utility of ceramic griddles as evidence of food processing. I don't completely disagree with the statements you make about this, but thought you might be interested in this treatment of the question. You will have to pardon my somewhat more cursory treatment of the Mesoamerican materials.

Sincerely

Carl Spath

Carl Spath

P.S.: Prof. DeWet noted as a possible reference: *For the Life of Man* (Natural History Press, 1969). According to this library it was never printed. Could you clarify this confusion?

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December 8, 1971

Professor David Rogers
Food Research Institute
Stanford University
Stanford, California

Dear Professor Rogers:

Over the last several years, I have been engaged in studying the folk botany of a group of Mayan Indians in Chiapas, Mexico. We corresponded briefly about this work in the mid-60s and I think you are acquainted with my botanical collaborators on the projects, Drs. Peter H. Raven and Dennis E. Breedlove. Recently, I have moved my ethnographic field area to Amazonian Peru (or hope to do so if funds are forthcoming) where I intend to initiate long-term study of the folk botany of a group of primitive horticulturalists in the Upper Maranõn River Valley. I enclose a brief sketch of the proposed research for your information.

Given the central importance of manioc to this group, I hope to spend a good deal of time in the description of its cultivation. I am anxious that I collect the kinds of data that will also be of use to specialists such as yourself, hence the present letter. I am aware of some of the problems surrounding the collection of determinable specimens of Manihot esculenta from our experiences in Chiapas. Since manioc is of supreme cultural importance, I am more than willing to take as much time as needed in procuring the kinds of information that you would feel necessary for adequate treatment.

By way of information, the group I hope to work with, the Aguaruna Jívaro, have as many as twenty-five distinct named varieties, but I am fairly certain that the number is higher. Each of these is recognizable on the basis of gross morphological characteristics, the discovery of which is, of course, a major goal.

Since we are relatively close geographically, I would hope to have the opportunity to visit with you personally, should you find the project of interest. I am free most afternoons and could easily drop down to Palo Alto at your convenience.

Cordially yours,

Brent Berlin
Associate Professor of
Anthropology

Enclosure: Cognitive Aspects of Aguaruna Jívaro Botanical Ethnography

Cognitive Aspects of Aguaruna Jivaro Botanical Ethnography

A. Description of research: Abstract

Building on the results of more than six years of research in the area of botanical ethnography, the applicant proposes to initiate basic ethnobotanical research among the Aguaruna Jivaro, a primitive horticultural group of American Indians residing along the Upper Maranon River and its many tributaries in Northern Amazonian Peru. The major focus of the study will aim at (a) compiling initial detailed data relevant to the testing of hypotheses concerning the nature of primitive ethnobiological classification and nomenclature which have been suggested to be applicable to all pre-scientific peoples (Berlin, Breedlove and Raven, in press; Berlin, in press), (b) providing more information on pre-scientific man's ethnobotanical conceptualization of the tropical forest leading ultimately to a study of Aguaruna ethnobiology in general and (c) providing data on the use of tropical forest resources which may be of value to modern man at a time when such research is of crucial importance. The methods to be employed will be those developed in the area of ethnoscientific research over the last decade, accompanied by systematic botanical collecting of the flora recognized linguistically by the Aguaruna. The immediate research goal will be to discover the underlying cognitive principles of classification and identification utilized by these people in interacting with their floral environment and to describe in what ways this knowledge is used by them for their own benefit. The long term goal is directed toward illuminating the nature of primitive classification and cognition which is supported by detailed empirical field research on actual folk systems.

B. Specific aims

The applicant proposes to spend 15 months of research time in Amazonian Peru living among the Aguaruna. There are two specific aims of the field research during this period. The first is to determine the full inventory of Aguaruna plant taxa and their botanical referents by extensive ethnoscientific elicitation and botanical collecting. Compilation of these basic data will form the foundation for the research leading to the discovery of those cognitive principles utilized by the Aguaruna in classifying, naming, and identifying these plant taxa. The final description of the native conceptualization of plants will contribute to the testing of hypotheses relating to the principles of ethnobiological classification which have been claimed to be highly general.

The second specific aim is to provide a detailed ethnobiological description of the ways in which the knowledge of plants is utilized by the Aguaruna in ways of cultural significance. Two broad and fundamental areas of human activity will be examined in detail: economic folk botany and the cultural use of plants in medicine and ritual. In this regard, an intensive description of horticultural knowledge is envisioned, along with studies of the significance of non-cultivated plants of major economic importance. The relationship of plants to social structure will be examined, especially as it relates to myth, social groupings, and religion. A preliminary study of Aguaruna theories of disease and disease classification will be undertaken with special reference to the use of plants which are employed because of their presumed medicinal value.

C. Methods of procedure

An adequate ethnobotanical description carried out from a cognitive point of view should specify explicitly the cultural knowledge held by members of a particular society which enables them to behave appropriately in any cultural contexts where botanical knowledge is utilized by them, consciously or unconsciously. The completeness of such a description must be evaluated in terms of the extent to which one can provide concise answers to at least the following questions (modified, in part, from Conklin 1969):

1) Given the array of potentially recognizable organisms in the plant world with which a society in the tropics must interact, which groupings are habitually segregated into cultually recognized conceptual classes?

2) Given a knowledge of the inventory of recognized plant classes, what are the taxonomic structures by which these classes are organized? Furthermore, what are the ethnobotanically relevant facts recognized by native speakers which allow them to classify, name, and identify plant taxa correctly?

3) How are the conceptually designated natural resources of the plant world exploited in ways of cultural significance as determined by their status in economic, social, and ritual activities?

The discovery of major conceptual groupings

One of the early, if not the first tasks, in any ethnobotanical description is to obtain an initial inventory of the standard and habitual linguistic designations of the locally recognized flora. Standard techniques developed for the elicitation of native taxonomies will be employed in this aspect of the research. Many of these techniques have been successfully used in similar studies in ethnobiology (Anderson 1968; Bulmer 1967, Bulmer and Tyler 1968; Conklin 1954, 1962; Fowler and Leland 1967; Frake 1962, 1964; Perchernock and Werner 1969; Wyman and Bailey 1965; Metzger and Williams 1966) and were quite productive in the Tzeltal ethnobotanical research described in Berlin, Breedlove and Raven 1966, 1968, in press.

Initial botanical collections

While no effort will be made to obtain complete coverage of the area botanically, collecting procedures of named classes will be such as to maximize the acquisition of information relating to the stability of named classes and informant variability. Named plants will be collected numerous times with different informants in different areas throughout the year. Such repeated collecting of recognized groupings should allow one to make fairly reliable statements as to the accurate botanical ranges of each Aguaruna plant taxon. A lack of broad-based botanical collections for recognized plant classes is a weakness of many early ethnobotanical studies where the botanical determinations were rarely based on more than one collection, taken at one time during the year, and identified by a single informant.

The applicant has worked closely with field botanists over the last six years and is competent to take botanical specimens which can be accurately determined by competent specialists when returned to the States for identification. Special attention will be made to the excellent collecting procedures suggested by Barrau (1967), Bailey (1946), Fosberg and Sachet (1965) for collecting usable botanical information on special groups (e.g, bananas, palms, aroids, etc.).

Discovering native principles of classification and identification

At the time of collection of all plants, information concerning native principles of classification are most readily elicited. Questions relating to the major life form of the plant, properties or characters used in recognizing it, and other plant taxa seen closely related to it will be asked. Attention will be given to eliciting those perceptual characteristics of plants which are recognized by Aguaruna ethnobotanist in identification, namely, stem habit, edibility, smell of leaf or bark, texture of stem, leaf shape, etc.,

In addition to actual observations of Aguaruna naming and classifying plants in the field, information on native taxonomy will be elicited from informants in the form of detailed textual and graphic materials. Texts, written by informants or taped from interviews and later transcribed, will be collected on a variety of topics, especially as regards plant morphology and growth stages. These textual materials will provide an unlimited source of raw data for further eliciting. In addition, these linguistic materials will represent a permanent native language record of botanical knowledge available for only a small number of non-Western cultures.

The cultural significance of plants

In addition to describing the cultural knowledge possessed by the Aguaruna as regards their conceptualization of the plant world, an adequate description must also account for the ways this knowledge is utilized in ways of cultural significance. While it is expected that knowledge of plants permeates many aspects of Aguaruna life, at least two broad areas of social activity will be examined in detail in this initial study: economic folk botany and the ritual and medicinal importance of plants.

Economic folk botany

Survival in the forest depends directly on those aspects of plant knowledge which relate immediately to basic subsistence. A primary area of human activity that will be examined in this regard is Aguaruna horticultural knowledge. No complete description of this important domain can be completed during this first preliminary study but the major outlines can be ascertained. A central concern will be to determine Aguaruna views of horticulture as a process. The goal will be to discover what the Aguaruna must know in order to prepare, maintain, and harvest a successful swidden. Detailed behavioral observational records will be kept which will complement the description of those aspects of the swidden process which can be verbalized directly.

The economic significance of non-cultivated plants and plant products from the forest will also be studied in detail. In earlier research in Mayan ethnobotany, the applicant and his colleagues have attempted to quantify, in a rough fashion, the cultural significance of plants as can be ascertained by the amount of time spent in readying their useful parts for human consumption or use (see Berlin, Breedlove and Raven, in press; Berlin, Breedlove, Laughlin and Raven, in press). Several hypotheses relating linguistic facts to cultural significance have been posed and will be further tested in the field.

Standard data on economic botany will be collected for all recognized plant classes as outlined in several reference works on this topic (Barrau 1967, Castetter 194[illegible], Conklin 1954,1957). Photographic records of individual sequences of economic activity (such as house construction, boat building, etc.) will be carried out as a matter of course.

Ritual and medicinal importance of plants

Next to the importance of plants as fulfilling basic needs, any study which aims at completeness must examine the significance of plants in a broader sociological context. From the outset, systematic elicitation will be carried out to determine the potential mythological relevance of the known flora. Texts will be collected relating to the proposed magical qualities of plants and plant products. Efforts will be made to determine to what extent social groupings are considered to be reflections of the Aguaruna's classification of the flora, and so on.

Concomitant with the research will be a concern with providing a preliminary description of folk medicine. This study necessarily requires a knowledge of the relevant ethnotheories of disease held by the Aguaruna. The applicant will be aided in his study of disease classification by his wife who is a trained registered nurse. As a consequence, Aguaruna descriptions of symptoms of various named native diseases may be more readily interpreted in terms of Western categories, an essential first step if a meaningful description is to be accomplished.

Facilities available

Principal research facilities available to the applicant at his home base are the Language-Behavior Research Laboratory. Arrangments for identification of plant materials have been made with the Missouri Botanical Garden through the kind services of Dr. Peter H. Raven, Director, Dr. Thomas Croat, and Dr. John Dwyer. The University of California Botanical Garden will provide space and assistance in the propagation of seed collections and live plants of particular cultural importance. Collaboration with scholars in Peru is indicated in the section of the proposal dealing with affilation with a university in Peru.

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State University of New York
at Stony Brook
Stony Brook, New York 11790

Department of Anthropology
Undergraduate Office
telephone: (516) 246-6745

StonyBrook

December 1, 1971

Dear Dr. Rogers.

Here are three of the four prepared papers. Schwerin's was used up before I could get an extra copy for you, however he said he would send on to me some extras and I will relay one to you when he does.

Broster and Lathrap didn't make it to the meetings. It was a good session and we all wished you could have been in on it. Janet Siskind and I are thinking about pulling these papers together for publication. It will probably take until the Spring, however, to get anything like that organized. Meanwhile, if you have more material you might want to include let one of us know. We'll be in touch again.

Sincerely,
Dolores Newton

RUTGERS UNIVERSITY *The State University of New Jersey*

NEWARK COLLEGE OF ARTS AND SCIENCES
Department of Sociology
Newark, New Jersey 07102

November 9, 1971

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

Dear Dr. Rogers:

I am really sorry that none of our effort were able to turn up funds. Wenner-Greu told Miss Newton that they were giving almost no money out this year and, I believe, none for domestic travel.

We would very much like to present any paper you cared to send, since your research is of interest to everyone in the symposium. We would also be glad to send you copies of the papers that others are presenting. If you would like to receive them just send me a note.

Again, our real regret that you cannot join us.

Sincerely,

Janet Siskind

Janet Siskind
Assistant Professor

JS:pl

Nov. 4, 1971

Dr. Dolores Newton
Dept. of Anthropology
SUNY, Stony Brook.

Dear Dr. Newton:

I enclose the masters for ditto for my contribution to the manioc symposium. Since I don't have time, facilities, nor addresses of the participants, I hope you can manage from this point.

I would be pleased to receive similar material from each of the participants.

I am very sorry not to be able to participate, and wish you the best of luck with it.

Sincerely yours,

David J. Rogers
Visiting Scholar

State University of New York
at Stony Brook
Stony Brook, New York 11790

Department of Anthropology
Graduate Office
telephone: (516) 246-3629

StonyBrook

October 15, 1971

Dear Dr. Rogers:

We are still working on travel funds and hope to have definite word within two weeks. The symposium has been scheduled for 9:00 A.M., November 18th the first day of the meetings at the Statler-Hilton Hotel. If it turns out that funds are not forthcoming, we would very much like to have you send us your paper and would appreciate your allowing one of us to present it. This will, of course, not make up for your absence since several of the symposium members have questions we are hoping to ask you.

If it is possible for you to have fifteen ditto copies made please send these to Dolores Newton at Stony Brook, by November 5th. If you prefer to send the typed dittos for us to run off, try to have them reach Stony Brook by November 1st. This will allow us time to mail copies to all the symposium members.

We hope that all the members of the symposium will be able to join us for an informal drink on Wednesday, November 17th, between 9:00 and 11:00 P.M. in Dolores Newton's room at the hotel (ask at the desk for the room of Mrs. Jefferson Fish).

Sincerely,

Janet Siskind

Janet Siskind
Rutgers University

Dolores Newton

Dolores Newton
Stony Brook University

WENNER-GREN FOUNDATION FOR ANTHROPOLOGICAL RESEARCH
INCORPORATED

14 EAST 71st STREET
NEW YORK, N. Y. 10021

CABLE ADDRESS: WEGEFOUND
TELEPHONE: 737-2900

October 7, 1971

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

Dear Dr. Rogers:

This is to acknowledge with thanks our receipt of your request for funds from the Wenner-Gren Foundation for Anthropological Research.

Please note that this acknowledgment does not insure that funds will be forthcoming. All decisions concerning grants are made by our Board of Directors, and you will be notified of the Board's action as soon as possible.

We regret to have to reply to you by form, but because of the volume of requests sent to us, we must do so in order to answer promptly.

Sincerely yours,

CHRISTOPHER D. DODDS
Assistant Director of Research

P.S. Thank you for letting us know about the problem you had with the copies of the travel application form. We are sorry if it caused you any inconvenience. We are checking our supply of forms on hand to see if this might be a mistake made by the printer. We appreciate your informing us.

71/2

September 30, 1971

Dr. Christopher D. Dodds
Wenner-Gren Foundation
14 East 71st Street
New Yor, N.Y. 10021

Dear Dr. Dodds:

Thank you for sending applications for travel assistance. I return my application herewith.

I may have confused the forms a bit, because there were seven copies of page one, three of which were behind page two; there were no copies of page two included, and only 2 copies of page three. I "averaged" by returning the original and 3 copies of page one, the original and 3 xeroxes of page 2, and 2 copies of page 3. There are 4 copies of my curriculum vitae. I trust this is satisfactory.

I appreciate your consideration of my application.

Sincerely yours,

David J. Rogers
Visiting Scholar

Encl.: Application for travel assistance.

WENNER-GREN FOUNDATION FOR ANTHROPOLOGICAL RESEARCH
INCORPORATED

14 EAST 71st STREET
NEW YORK, N. Y. 10021

CABLE ADDRESS: WEGEFOUND
TELEPHONE: 737-2900

September 28, 1971

Professor David J. Rogers
Food Research Institute
Stanford University
Stanford, California 94305

Dear Professor Rogers:

This is to acknowledge with thanks receipt of your letter requesting funds to attend the American Anthropological Association Meetings in New York.

Enclosed you will find the necessary materials with which to make a formal presentation to our Board of Directors, and you are welcome to do so. Please be aware, however, that the financial situation this year is extremely tight and the Foundation is unsure as to whether we will be able to assist travel to the AAA meetings this year, and I suggest that you look into other possible sources of aid as well as the Wenner-Gren Foundation.

Thanking you for your interest in the Foundation, I am

Sincerely yours,

CHRISTOPHER D. DODDS
Assistant Director of Research

Enc:

WENNER-GREN FOUNDATION FOR ANTHROPOLOGICAL RESEARCH
INCORPORATED

14 EAST 71ST STREET
NEW YORK, N. Y. 10021
TELEPHONE: 737-2900

IMPORTANT REMINDER

Please be sure to include sufficient copies of Curricula Vitae for all personnel directly involved with the project so that we need not delay the processing of your application.

70/cv

GENERAL INFORMATION
ON ASSISTANCE OFFERED BY
WENNER-GREN FOUNDATION FOR
ANTHROPOLOGICAL RESEARCH, INCORPORATED

The Wenner-Gren Foundation for Anthropological Research, Incorporated, is a non-profit foundation created and endowed in 1941 as The Viking Fund, Inc. by Axel Leonard Wenner-Gren. Its sphere of interest is the support of research in all branches of anthropology.

Grants-in-aid: Grants are awarded by the Board of Directors of the Wenner-Gren Foundation in support of projects initiated by individual scholars, affiliated with accredited institutions and organizations. Applicants from all over the world are eligible. Petitions may be submitted by or on behalf of the individual or organization. It is not necessary to present an application by means of a personal interview. Because of limited funds, the Board of Directors gives preference to the consideration of applications for small amounts and does not accept petitions for projects marginal to this Foundation's sphere of interest. Potential applicants undertaking long-range or large-scale research programs should explore other possible sources of aid. As the Foundation's policy is to rely upon the integrity and scientific ability of its grantees, awards are made without limitations or conditions attached to the conduct of research, but it requests only the following: 1) acknowledgement of Wenner-Gren Foundation aid in publications reporting results of research, 2) presentation of three copies of such publications to the Foundation's Research Libraries, and 3) final report on accomplishments of project.

A limited number of grants are awarded by the Board of Directors to students for support of research at the advanced pre-doctoral level. Application must be made, however, by the senior scholar who will undertake the responsibility of supervising the project and who will, upon its completion, submit a final report supplementary to that of the student's. (The Pre-Doctoral Fellowships Program was discontinued as of January 31, 1969.)

Post-Doctoral Fellowships: A limited number of nonrenewable Richard Carley Hunt Memorial Fellowships, having a maximum stipend of $2,500, are awarded by the Board of Directors, usually to aid completion of specific studies or preparation of field materials by younger scholars.

Application forms may be obtained by writing to the Foundation.

WENNER-GREN FOUNDATION FOR ANTHROPOLOGICAL RESEARCH
INCORPORATED

14 EAST 71ST STREET
NEW YORK, N. Y. 10021

CABLE ADDRESS: WEGEFOUND
TELEPHONE: 737-2900

DEADLINE SCHEDULE FOR APPLICATIONS

Applications for grants-in-aid must be submitted to the Wenner-Gren Foundation for Anthropological Research, Inc., New York office, between July 1 and January 31.*

No applications for grants-in-aid will be accepted for processing between February 1 and June 30.

Applications may be obtained by writing to the Foundation's New York office, 14 East 71st Street, New York, New York 10021.

*This schedule does not apply to Museum Research Fellowship applications which are accepted throughout the year.

RUTGERS UNIVERSITY *The State University of New Jersey*

NEWARK COLLEGE OF ARTS AND SCIENCES
Department of Sociology
Newark, New Jersey 07102

September 20, 1971

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

Dear Dr. Rogers:

I'm sorry not to have answered your letter sooner, but I got back from Peru a couple of weeks ago and started teaching last week, so things have been a bit disorganized.

We are going to try and see what can be done about obtaining funds for your travel. I will try the Agricultural Development Council this week, since they often support research and might be helpful. My co-chairman, Dolores Newton is speaking to someone at the Smithsonian and will also try Wenner-Grenn. I am also planning to ask Robert Carneiro at the Museum to see if there is any money available there.

I will write to you as soon as I have any information. Please hold the date for us (and, perhaps, cross your fingers); we will do our best, since we are very anxious to have you take part in the symposium.

I can be reached at the address above.

Sincerely,

Janet Siskind

Janet Siskind
Assistant Professor

Sept. 17, 1971

The Wenner-Gren Foundation
14 E. 71st St.
New York, New York 10021

Gentlemen:

I have been invited to participate in a symposium on manioc, by Dr. Janet Siskind, Anthropology Dept., American Museum of Natural History, at the forthcoming annual meeting of the American Anthropological Society, in New York, and need subvention for travel.

Will the Wenner-Gren Foundation support travel for this purpose? I need approximately $600.00 for transportation and per diem costs. Please send appropriate forms, if you can support such requests.

Sincerely yours,

David J. Rogers
Visiting Scholar, and
Professor of Biology,
University of Colorado.

Sept. 17, 1971

The Wenner-Gren Foundation
14 E. 71st St.
New York, New York 10021

Gentlemen:

I have been invited to participate in a symposium on manioc, by Dr. Janet Siskind, Anthropology Dept., American Museum of Natural History, at the forthcoming annual meeting of the American Anthropological Society, in New York, and need subvention for travel.

Will the Wenner-Gren Foundation support travel for this purpose? I need approximately $600.00 for transportation and per diem costs. Please send appropriate forms, if you can support such requests.

Sincerely yours,

David J. Rogers
Visiting Scholar, and
Professor of Biology,
University of Colorado.

, Ext. 2342

Sept 3, 1971

Dr. Janet Siskind
The American Museum of Natural History
New York, New York

Dear Dr. Siskind:

At this late date, I find that I will not have sufficient funds to participate in the symposium on manioc. Would it be possible to get the Wenner-Grenn Foundation to support my travel? I have tried them once before without success, but it might be more likely if you (or someone in Anthropology)made an application for me.

Will you let me know your reaction to an application to Wenner-Gren? What other procedures do you suggest? Thanks for your time.

Sincerely,

David J. Rogers
Visiting Scholar

The American Museum of Natural History Central Park West at 79th Street New York, New York 10024 (212) 873-1300

DEPARTMENT OF ANTHROPOLOGY

June 3, 1971

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

Dear Dr. Rogers:

Miss Newton and I plan to wait until after the summer to set a deadline on papers. The deadline will probably be about October 15, with the expectation that we would be happy to have them at the beginning of November.

I will also be out of the country until the beginning of September, and I am hoping that my work in Peru this summer will contribute to my own paper for the symposium.

Best wishes for a good summer.

Sincerely yours,

Janet Siskind

Taximetrics Lab.

April 2, 1971

Dr. Janet Siskind
The American Museum of Natural History
New York, New York

Dear Dr. Siskind:

I trust your deadline of April 1 for abstracts for the manioc symposium is just a little flexible. Otherwise, I am out of luck. Sorry.

Hope the enclosed blurb is satisfactory. If not, let me know.

Sincerely,

David J. Rogers
Professor of Biology

P.S. After August 15th, I will be at the Food Research Institute, Stanford University, spending a sabbatical leave there. Any correspondence should be sent there, c/o the Director, Dr. W. O. Jones.

Did you have any luck getting Jones to talk?

The American Museum of Natural History Central Park West at 79th Street New York, New York 10024 (212) 873-1300

DEPARTMENT OF ANTHROPOLOGY

March 10, 1971

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

Dear Dr. Rogers:

I am very glad that you are planning to contribute to the symposium on manioc and I am following your suggestion to write Dr. Jones. In terms of topics, I have a few ideas, though frankly I think everyone would be interested in whatever current lines of research you are pursuing. My first thought is the origin problem, which might be aimed at suggesting to archaeologists and culture historians the kind of data that would seem to you significant in attempts to trace the use of manioc throughout the lowland area.

My second thought is really a question related to my own field work among the Sharanahua Indians. These people plant sweet manioc on well-drained land and prefer to begin harvesting after a year to a year and a half. This preference precludes the use of any land that is inundated during the rainy season since the manioc they grow has no tolerance for swampy soil.

I have wondered whether the varzea-raised manioc is more tolerant of wetness or if harvesting is simply completed before the rivers rise, and I plan to check ethnographies to see if the latter is invariably the case. I would be very interested in your opinion concerning the relative tolerance of manioc cultivation to damp soil as well as whether there appear any significant difference besides size in manioc harvested at six months rather than eighteen months.

Please feel free to ignore my suggestions. I became involved in organizing the symposium in order to learn more about manioc, and, again, whatever contribution you decide to make will be valuable.

Sincerely yours,

Janet Siskind

Janet Siskind

JS:kp

State University of New York
at Stony Brook
Stony Brook, New York 11790

Department of Anthropology
Graduate Office
Telephone: (516) 246-3629

StonyBrook

May 6, 1971

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

Dear Dr. Rogers:

Thank you very much for your abstract. We are enclosing a copy of each of the abstracts that we have received for the manioc symposium. One or two others may still be forthcoming.

We would like to suggest that most of the actual time of the symposium by spent in discussion rather than in reading papers. Therefore, if you are planning to write a long paper, we would like to ask you to distribute it in advance and present either a summary or a short piece of it. If it is possible for you to send 15 copies to Miss Newton at Stony Brook, it would be very helpful. If not, we can arrange to have it zeroxed. Our preference is for five-minute presentations, but this is not an absolute limit.

We are enclosing our plan to date of the symposium and would like your assistance in recruiting firends and acquaintances with field experience to participate in Part III.

Sincerely yours,

Dolores Newton

Dolores Newton
Instructor-Curator

Janet Siskind

Janet Siskind
Department of Anthropology
The American Museum of
Natural History

DN:sc

It is now known that a single species, Manihot esculenta, encompasses both "sweet" and "bitter" varieties of manioc. Many questions still remain to be explored in assessing the botanical characteristics of this plant and their relationship to particular cultural patterns of cultivation, processing, and consumption. Data bearing on these questions have been recorded by archeologists and ethnographers working in the tropics. The aim of this symposium it so pool these data and to work out the kinds of information to which we and other anthropologists of this region should direct our efforts.

MANIOC IN LOWLAND SOUTH AMERICA

Chairmen: Newton, Siskind
Discussant: Carneiro

Part I Manioc: a consideration of botanical origins, classification and relationship to processing techniques; ecology of cultivation, storage, and diet.

Papers: 1. Rogers
2. Schwerin

Discussion

3. Siskind
4. Bolian
5. Spath

Discussion

Part II Manioc: a consideration of the tools and techniques of processing in ethnographic and historical perspective.

Papers: 1. Dole
2. Broster

Discussion

3. Newton
4. Kensinger
5. Lathrap

Discussion

Part III Ethnographic Smorgasbord: Informal contributions from ethnographers in the audience; additional and comparative materials from South America and the world.

TECHNIQUES OF MANIOC PREPARATION AND THE IDENTIFICATION OF MANIOC TYPES

Karl H. Schwerin
Univ. of New Mexico

A survey is made of South American manioc-using cultures in order to determine the variety and distribution of techniques for processing the tuber. This data is then used to determine whether or not and to what extent techniques of processing are related to classification of cultigen types (particularly the distinction between "bitter" and "sweet") as erected both by native cultivators and European explorers and ethnographers.

MANIOC, MAIZE, OR PLANTAINS

Land with good drainage is a crucial factor in manioc cultivation, and its availability influences decisions on apportioning land and labor between manioc and other subsistence crops. The decisions made vary from one ecological niche to another along a range from relatively high land to flood plain. A comparison is made between two groups of Panoan-speaking Indian societies of the tropical forest, one of whom has only recently left the high, interfluvial environment, while the other has spent generations producing crops along the flood plain.

Janet Siskind

Rutgers University

Manioc Cultivation in Periodically Flooded Areas

Historical records indicate that the Omagua island dwellers of the upper Amazon Basin used underground storage facilities for manioc during floods. Modern Tikuna, who are currently occupying this area, use the same underground storage system when there are floods. However, this involves not only manioc storage but a rearrangement of crop priorities with maize temporarily replacing the role of manioc in the diet. The normal system of gardens being predominantly located in the floodplains also changes with many new gardens being placed in the less fertile but dry areas of old alluvium.

Charles E. Bolian
Dept. of Anthropology
University of Illinois

Carl Spath
University of Illinois
26 March 1971

Manioc as a Determinant of Settlement Patterns

Though the presence of cyanogenic toxins in manioc is often noted in the literature, this factor is rarely dealt with in any systematic fashion. It is normally assumed that these toxins are disposed of by the elaborate porcessing of the pulp of "bitter" manioc. These toxins are only partially and secondarily removed by the processing in question and the cyanogens in the diet have a very real effect on other dietary needs and ultimately on settlement or economic patterns. The free hydrogen cyanide radicals react with sulphhydryl groups in dietary proteins and place an additional demand on the human body for consumption of sulphur-bearing proteins. This is of particular significance in a diet of manioc which provides virtually none of its own protein.

A FINAL STAGE IN THE DEVELOPMENT OF THE TIPITÍ

Gertrude E. Dole

New evidence bearing on the development of the tipití (sleeve press for manioc) will be presented. A plaited mat press used by the Witoto will be described and illustrated; and the relation of this type of mat press to the tipití will be discussed.

ABSTRACT

"Identification of Manioc from Archaeological Ceramics"

John B. Broster

The purpose of this study is to determine the presence of vegital residue on archaeological ceramics. Through chemical and microscopic analysis of sherd collections from Venezuela, Mexico, and Central America, it is proposed that manioc, maize, and other vegetal materials can be identified as residue on these ceramics.

This approach may provide a test for determining functions of archaeological ceramics and aid in establishing temporal placement of cultigens, such as manioc, in a certain regional sequence.

MANIOC AND THE RISE OF CIVILIZATION IN THE NEW WORLD

It has been a rarely questioned assumption that maize agriculture provided the economic base for New World civilizations. Since Spinden's 1917 article it has been widely accepted that the first appearance of the various cultural elements indicative of civilization or initiating the trend toward civilization was synchronous with the appearance of efficient patterns of maize agriculture. A growing body of evidence is accumulating that root crop agriculture, and specifically manioc cultivation, provided the context in which the first civilizing influences appeared in both Mesoamerica and the Central Andes. It seems worthwhile to review this evidence and its implications concerning nature and time depth of the Tropical Forest cultural pattern.

2"x2" slides

Donald W. Lathrap
Department of Anthropology
University of Illinois, Urbana

A TUBER IN THE POT...

Recipes are a significant aspect of cultural variation and are subject to the same forces for conservatism and change as other aspects of culture. For example, Jones notes a single pattern in the African use of manioc in the structure of the meal: it is the starch over which some kind of sauce is poured. The manner in which manioc, and other foods, are structured in the meal form should be considered worth recording and analyzing for South American cultures too. Examples of manioc recipes from a Timbira tribe (the Krikati) are offered here for initial comparative discussion.

Dolores Newton
Instructor--Curator
SUNY at Stonybrook

Abstract for Symposium "Manioc in Lowland South America"

American Anthropological Association November 1971 Meetings.

Manioc and the Cashinahua (Peru)

Kenneth M. Kensinger
Bennington College

Sweet manioc is one of three major vegetable staples in the Cashinahua diet, and its production represents a significant investment in both time and energy. This paper will present data on the technology and ideology of manioc cultivation and usage, in relation to the semantic/cultural domains of which it is a part.

Abstract for Manioc Symposium

Botanical Considerations on the
Origin of Manihot esculenta.

David G. Rogers

The question of origins of a cultigen are complex, and is not settled within a single discipline. While of a cultigen Vavilov used the hypothesis that the center of origin/coincided with the geographical area in which most variations of a cultigen are found today, later studies indicate that present-day distributions are not necessarily coincident with the starting point of cultivation. Because we are faced with a very recent and explosive evolution when speaking of plants most closely associated with man, we cannot invoke arguments of plant evolution in general to discover useful information about the origins of most of our cultivated species.

It seems futile to to speak of a single geographic area as "The origin" of Manihot esculenta because in reality there are many. In a genus of 95 species, distributed over much of tropical America, North and South, we can expect that wherever manioc is found, it will have a number of genes contributed by the wild species in each a given area, and thus, that area is another point of origin. Our recent computerized classification of the genus Manihot, and a separate one on M. esculenta bear out the above observations.

The American Museum of Natural History Central Park West at 79th Street New York, New York 10024 (212) 873-1300

DEPARTMENT OF ANTHROPOLOGY

February 16, 1971

Dr. David Rogers
Colorado State University
Fort Collins, Colorado

Dear Dr. Rogers:

I am organizing a symposium on manioc for the November meetings of the American Anthropological Association in New York City. My aim is to get people together to share knowledge from a variety of areas and thus collect comparative data on the range of uses and problems of manioc.

I would be delighted if you would like to contribute to the symposium. So far, I've only been in touch with people who are in the city, but I am also writing to invite Donald Lathrap and Carl Schwerin as well and would welcome your suggestions. At this point the following people have agreed to take part: Robert Carneiro, Gertrude Dole, Kenneth Kensinger, and Dolores Newton.

I am enclosing a copy of the symposium abstract. If you are interested in taking part, would you send me an abstract by April 1.

Sincerely yours,

Janet Siskind

JS:mw
Enclosure

Symposium on manioc in lowland South America

Manioc is one of the most important staple crops grown throughout the Amazon Basin. As such, the choice of land and drainage necessary for its cultivation may influence the population movements and settlement patterns of many Indian cultures of the tropical forests and savannas of South America. The labor requirements of raising and processing manioc vary between bitter and sweet, flood plain and high ground, and the organization of these requirements has implications for the social organization of societies dependent upon manioc. The development of methods for utilizing bitter manioc brings up questions of innovation and technological change in primitive cultures, and the non-consumption of manioc leaves, a rich source of vegetable protein, brings up problems of non-adaptive behavior. The invention of bitter manioc itself is important for further investigation of tropical forest culture history.

Every ethnographer who has worked in the Amazon area has collected data on the utilization of manioc by "his people." The aim of this symposium is to pool these data and to work out the kinds of information to which we and other ethnographers of this region should direct our efforts.

Dr. Janet Siskind
Department of Anthropology
American Museum of Natural History

Taximetrics Lab.

March 3, 1971

Dr. Janet Siskind
Department of Anthropology
The American Museum of Natural History
New York

Dear Dr. Siskind:

Thank you for your invitation to participate in a symposium on manioc at the Anthropology meeting next November. I will be pleased to participate, and will send in an abstract before the deadline.

I would suggest that you also invite Dr. William O. Jones, Director, the Food Research Institute, Stanford University, to participate in the meeting. He, as you know, wrote the book Manioc in Africa (1959), and has gathered a tremendous amount of information on manioc in the American tropics. Perhaps he would be willing to put together some of this information for the symposium. I believe his insights might well be useful in this context.

I expect to be spending a sabbatical next year, and will be working in Dr. Jones' institute. I will, therefore, have Dr. Jones' same address from August onward. In the meantime, please note that I have moved from Ft. Collines, and my correct address, through July will be the one on the letterhead.

Thanks again for your invitation. Any further ideas on the topic will be appreciated.

Sincerely,

David J. Rogers,
Prof. of Biology.