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Research on Rice

I) Introduction

In the general discussions of program which have occurred, particularly during the last two years at The Rockefeller Foundation meetings in December and April, it has been clear that the Trustees were prepared to consider some expansion of our agricultural activities and our interest in the basic problem of food. The officers of the Division of Natural Sciences and Agriculture accordingly studied a rather wide range of possibilities. In particular, they gave special attention to the opportunities in that part of the world where there are the most hungry people - the Orient. In this connection they have made two survey trips, one two years ago to the near East and India, and one year ago a round-the-world trip which involved visits at the Hawaiian Islands, Japan, the Philippines, Thailand, Burma, India, and the UNESCO Food and Agriculture headquarters in Rome.

Apart from any visit, and on the sole evidence of the over-all statistics, it is clear that need exists in this area. But need does not necessarily imply a practical opportunity that is appropriate to the resources and experience of The Rockefeller Foundation. In fact there were certain approaches which, before these trips, seemed attractive and promising; but which did not stand up under the evidence gathered in the various countries. These studies have, however, led us to a general plan which we believe sound and important; and the present memo will summarize the thinking that led to this plan. This plan was, in fact, presented to the Trustees in December, 1953. Although no formal approval was sought or given, the atmosphere of the discussion was clearly favorable, and the NSA Division was authorized to proceed to get personnel to start activating the program.

See memo filed under date 12/4/53.

II) Rice and the World's Food

Cereal grains constitute the predominantly important source of the food* for the human beings on this plante. "Some 85 per cent of the world's population obtain the majority of its calories in their daily diet from cereals ... . About two-thirds of the people in the world - in Asia, the USSR and Africa - derive from 80-90 per cent of their calories in their diet from carbohydrates, chiefly grain."**

The most important cereal grains, in decreasing order of production, are rice, wheat, corn, oats, barley, millet and sorghum, and rye. "Rice is the most important food in the world. Although surpassed by wheat in total acreage in the

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* The emphasis in this memo is wholly on plants, since the basic step in the conversion of solar energy to human food occurs solely in plants. If plant yields can be increased, then there would also be, of course, an increased supply of feed to produce more meat and other animal products.

** World Population and Production by W.S. and E.S. Woytinsky, Twentieth Century Fund, 1953.

world as a whole, the volume of food produced by the world rice crop is 10 to 20 per cent greater than that of wheat."* A great deal more rice than wheat crosses national boundaries (21.3 million tons as compared with 4.3 million tons); and this means that rice is predominantly the world's home-grown and home-consumed source of food energy. In fact, for about 60 per cent of the world's population, something like 80 per cent of the calory intake comes from rice.

Rice is, moreover, the major food for those parts of the world which are underprivileged, and where the race between food and population is so grim that starvation is a constant threat and a not infrequent reality. Any useful knowledge concerning rice thus bears upon the major food need of some one-and-one-half billion people, many of whom constitute the world's hungriest and most precariously fed group.

Although rice has fed millions of persons for thousands of years, it is nevertheless a striking fact that little is known about rice. Partly this is due to the fact that rice has been relatively important in those parts of the world where science has not progressed very rapidly. And partly, also, this is due to the interesting fact that the rice plant is, speaking roughly, too good for its own good. It is so vigorous, so able to cope with varied circumstances, and so resistant to many diseases, that it manages to produce a tolerable crop under almost any circumstances. And the crop being tolerable, there has been until now, relatively little incentive to study this marvelous plant in any deep and intensive way.

In various places, and notably in Japan, there have been very able and vigorous programs of research on the more obvious and applied aspects of rice cultivation - just what fertilizer to apply and when, the effect of depth of paddy water, selection of the most promising varieties, etc. In a very few places, and notably at Cuttack in India, there have been recent attempts to cross the high yielding Japonica types with the Indica types that prosper in the tropical latitudes;and at Cuttack there has also been made a small beginning on certain basic studies of the rice plant.

But generally speaking, the striking fact is that you can hardly ask a fundamental question about the rice plant that must not be answered, at present, by "No one knows."

Is it true that the phenomenon of hybrid vigor does not appear when pure lines of rice are crossed, or is it merely true that the hybrid vigor only affects unimportant aspects of the rice plant rather than yield and uniformity, as in the case of corn? If hybrid vigor does usefully occur in rice, or can be made to, is there any practical breeding program which could exploit it? No one knows. And this latter question is of all the broader significance because of the fact that any useful answer would apply not only to rice, but also to any other close-pollinated small grain.

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* The Production and Marketing of Rice by J.N. Efferson, p 2.

When two types of rice are crossed, lethality occurs in the progeny, a high percentage of the offspring sometimes failing to live. Why? No one knows. Here again any gain in knowledge would have wide application, for although lethality is specially marked in certain rice crosses, it also occurs to a lesser extent with other important crop plants, such as corn.

The Indica type of rice raised in India, Burma, Thailand, and other tropical countries, is generally thought to show little response to fertilizers, whereas the type raised in Japan shows a large response. Is this in fact the case? If so, why? No one knows.

Rice grows with its roots soaked, as in paddies, or with its roots dry or damp, as in upland rice. Yet it seems to suffer little if any from root rot. Why? Is there some secret here which would be of very great importance to the other cereal grains of the world, which often suffer greatly from root rot? No one knows.

These are illustrations of the fact that the rice plant, although outstandingly important to the feeding of the world, is largely an unknown object. Contributions to a basic understanding of this plant would be of direct benefit to millions of persons - in South America, Africa, and other parts of the world in addition to Asia.

III) The International Value of a Rice Research Program

Although a rice research program would be primarily directed toward the food problem of the great crescent-shaped region which has one tip in Japan and one in Pakistan, this program would undoubtedly have some activities located in other regions (the United States, South America, or possibly Europe). And the results of this program would in two important ways have wide international significance.

First, it should be realized that a great deal of rice is raised and eaten outside the Oriental crescent referred to above. Africa and South America each have a total annual production of rice which is upwards of two hundred million bushels. The annual production of North America is well over a million bushels, and that of Europe half as much. Of the rice imported by all countries, something like one-third is imported by countries outside the crescent area. And rice consumption is rising in both North and South America.

Second, a really basic study of rice - which would involve a broad and basic study of its real potentialities for yield - is a study which is urgently needed for reasons which are important to the whole world. The point involved here is of such general significance that we devote the next section of this memo to an explanation of it.

IV) The Potential Yielding Ability of Crop Plants

In considering the over-all possibility of increasing the world's food supply, it is usually argued that the application of improved scientific methods and the use of all arable land might increase agricultural food supplies by as

much as a factor of two. This 100 per cent increase, or any major part of it, would constitute an important holding action in the contest between population and food. But this assumed upper limit for agricultural food has been an essential part of the argument of the neo-Malthusians who foresee inescapable doom unless population increases are rather promptly curbed.

But the plain fact is that no one really knows what is the upper limit of the capacity of crop plants to produce usable yield. Actual crop yields vary greatly from place to place. The average yields of wheat in countries throughout the world vary from 7 to 50 bushels per acre, and yields up to 131 bushels per acre have been produced. Comparable figures for rice are from 20 bushels per acre to 100 bushels for "normal" yields, with records up to 260. Small-plot record yields are still higher.

But these rather striking ranges in "normal" yields should not be thought of as necessarily indicating the upper limit of possibilities.

The absolute upper limit is, of course, set by the amount of solar energy which falls on the land being cultivated. The total calories of energy in the corn plants raised on a plot of ground is something like one six-hundredth of the total solar energy falling on this same plot during the growing season. A great many factors operate to keep this over-all efficiency low, and no sensible person supposes that it could be made to approach 100 per cent. One major limiting factor, from which we see no present escape, is the fact that the efficiency of the photosynthetic process is about 20 per cent. There are, of course, many other limiting factors, having to do with water, soil nutrients, etc. But it is nevertheless interesting to note that the present very low efficiency could be multiplied by a factor of 120 before reaching the limiting efficiency of photosynthesis.

Thus when one thinks of the ultimate gains that might be made in crop yields there is no reason, if one considers only the matter of available energy, why yields could not be increased not 50 per cent, or 100 per cent, but by a factor of 100 or more.

The problem of obtaining greater crop yields, has, of course, received a great deal of attention. Broadly speaking, however, the studies have sought, within the framework of exacting agricultural practices, to obtain yield improvement by adjusting one or at most a few factors. According to our agricultural advisors, there has never been a broad and imaginative attack, with a group of high competence financed for a sustained study, on the combined and interrelated influence of all the factors that influence yield, using all the knowledge now available at both applied and basic levels. <u>It is considered high time that such a study be made for one (or more) of the great food plants of the world; and it is considered well within possibility that such a study would reveal yield potentialities not now viewed as possible</u>. Basic as such a study would be, and large as might be its practical consequences, there is no indication that any other agency is thinking of setting up, or is prepared to finance, such a study. Thus there seems to be here a unique and most significant opportunity for The Rockefeller Foundation.

The rice plant, primarily for the reasons stated above under II) would furnish a pre-eminently suitable plant on which to conduct such studies. It would, however, almost surely be desirable to carry out some collateral research, aimed at the same great central problem, which can be more easily or appropriately done on corn, wheat, or perhaps even potatoes. One has to take into account not only the importance of the plant in question, but also the scientific resources which can be brought to bear. The latter consideration, specially, would doubtless lead to some supplementary work on corn and wheat. In fact the Division has already made two grants, one to Cornell and one to Minnesota, which are a part of this general attack.

V) An International Rice Research Institute in Asia

Before DNSA officers went on the trip primarily devoted to rice, through Asia and Southeast Asia, there had been suggestions from several sources that perhaps The Rockefeller Foundation should aid in setting up and financing a single large international institute, somewhere in Asia, devoted to rice research. Having gathered information on this subject from many individuals from the rice producing countries from Japan, around Southeast Asia, and as far West as India and Pakistan, we would now sum up this particular possibility as follows:

A. Advantages of a single definitive center for rice research in Asia

a) International, or at least multiple-country, cooperation in any field of science is, broadly speaking, a good thing. It furnishes a basis for international friendships and understanding, and contributes toward a pattern of global living which is undoubtedly a desirable and necessary part of the future.

b) The basic problems concerning rice are universal problems, which can be properly attacked in one central laboratory which would then make the results available to all. Many of the really fundamental physiological, biochemical, and genetic problems are essentially independent of geography and are certainly independent of political boundaries; so that these problems could effectively and efficiently be attacked in one central institute.

c) A central institute should, at least in theory, lead to financial savings and related gains in efficiency, since it should eliminate unnecessary duplications of facilities and effort.

d) At such a central institute it should be possible to concentrate expensive types of instrumentation (phytotron, electronmicroscope, mass spectroscope, etc.). More important than this, it should also be possible to concentrate a high-powered and efficient international team of experts, supplementing each other and forming in total a more effective group than any one country could hope to produce. This should, moreover, result in the best utilization of this top group of experts, working on common problems under optimum conditions.

e) Such an international center should furnish otherwise unobtainable facilities for training of personnel for use in the cooperating countries.

f) Such a center could serve as a depository for research publications on rice, and should thus develop into the definitive library location for this field of research.

g) Such a center should work out some reasonable agreement, among the cooperating countries, with respect to the language or languages for rice research papers, thus making more readily useful the work done in various countries.

B. Disadvantages or Difficulties Relative to an International Center

a) The rice-raising countries of the Orient are, at least at this particular moment in history, exceedingly nationalistic. The peoples are proud and sensitive. They are heavily influenced by bitter experiences of the recent war, as well as prejudices much older in origin.

All of this produces a situation within which certain relatively mild types of inter-country cooperation can cautiously go forward (the working parties of the International Rice Commission, for example); but every informed and thoughtful person with whom we talked on our recent trip was of the opinion that these factors make extremely difficult, if not impossible, any scheme of international cooperation which would require a large-scale and active participation by the various countries, and which would involve firm and sustained commitments on their part.

b) In particular, no one with whom we talked believed that it would be possible to get the relevant countries to pledge funds for the sustained support of such a venture, even though its initial capital costs were all met by some external agency.

c) Certain local inquiries elicited interest in such a possibility, but always on the assumption that the institute would be located in the country where the inquiry was made. Any other solution of the problem of location seemed, as viewed locally, just absurd. If the country in question was strong in rice research, then they deserved the institute for that obvious reason. If weak, then they obviously needed it. If average, then their situation was characteristic, etc.

d) Although certain basic questions are universal in character, the more applied, and hence more pressing and more easily appreciated, problems tend to be pretty local in character. How should one raise rice here, with our soil, our climate, our water conditions, our cultural preferences? These conditions, even though admittedly of limited validity, tend to make each country interested in a specific local program, but rather cool toward a general program located elsewhere.

e) There is also a great need to develop local personnel, and because of the extreme scarcity of good men, to utilize good personnel locally.

f) It is surely true that, generally speaking, indigenous developments, rooted in the local soil, the local culture, the local problems, have a natural stability and a natural promise of growth which can never be matched by an activity which is to a large extent foreign in character.

g) Communication presents a real problem for an international institution located in the Orient. English is probably the only possible common language for Japanese, Philippine, Siamese, Burmese, Indo-Chinese, Indonesian, Indian ... scientists. But many of the scientists cannot use English effectively, if at all; and from some points of view it would be unfortunate to force an occidental language on an eastern institution. This problem is not solved, but is at least avoided, if one works within each country separately.

h) The over-all distance from Pakistan to Japan is large, and air is the only sensible mode of travel. Thus the travel budget for a regional institution would have to be large. Local developments have the advantage of offering some relief from such costs, but they by no means offer entire relief. For country exchange of personnel would remain important under any pattern of development.

i) A great many of the rice problems are not really scientific or even technological, but are rather concerned with economic, social, and political factors. These factors vary widely from country to country. And thus the over-all question, "What is possible and sensible to do about rice production" is one which has to be answered country by country. This important fact supports arguments both for and against a country-by-country development. On the one hand it makes a country-by-country development the easier, and doubtless the more realistic, alternative. On the other hand, one can sensibly argue that a basic over-all program of improving rice yields and quality would, in the long run but inevitably, have a sound and desirable influence on the non-scientific factors.

C. Conclusions

These pros and cons are too varied, too important, and too interrelated to lead to a single conclusion, so clear that one need only state it to be convinced. The situation just is not that easy.

In general, however, the arguments for a single great regional center are somewhat theoretical and idealistic; while the arguments against are extremely hard to meet or disregard. As a purely practical point, moreover, a very substantial amount of money would be required to set up and support a really definitive "Asian Rice Research Institute." The initial capital costs would be at least $2,000,000, plus perhaps $100,000 for the costs of a year or two of preliminary planning conferences. The annual cost of running such an

institute would probably be something like $250,000. Thus, a ten-year run (which might conceivably be justified, even though the subsequent future support was entirely problematical) would involve approximately $5,000,000.

This is by no means too large a sum to devote to a program of rice improvement. But it is too much money to wager unless the terms of the bet are good.

It is our own present judgment that it would be unwise to wager $5,000,000 on such an institute, located either in Japan or India (the locations of strength), or in Siam, Indo-China, or Indonesia (the more centrally and neutrally located positions of weakness).

VI.) The Recommended Program

We think it would be wiser to follow the less dramatic but, we believe, sounder procedure of simultaneously sponsoring rice research activities in several different locations in several countries, suiting each of these individual developments to the local needs but also trying to plan the parts so that they add up to a significant whole.

More specifically, it is our present and necessarily tentative notion that we ought to make larger moves in Japan and India, and smaller moves in the Philippines, Thailand, (perhaps and/or Burma), and probably eventually in Indonesia and Pakistan.

In Japan we could undoubtedly work out with the Japanese scientists and governmental authorities a substantial augmentation, improvement, and concentration of their rice research, furnishing suitable instrumentation and increased emphasis on basic and long-range problems. It would be well worthwhile to put $1,000,000 into such a Japanese development if we can actually work out with them - as we are confident we can - a sound scheme. It would be, in its primary emphasis, a Japanese institute intended to serve Japan; but if the institute developed as well as we would expect, it would naturally and inevitably earn a large amount of international recognition and use.

Within the last weeks a high official of the Ministry of Agriculture of Japan consulted with NSA officers, and indicated their strong desire to work with us on a rice program.

In India, and assuming here that the Indian Government would wish to enter into such a development*, we could profitably invest up to $500,000 in improvement and augmentation of the facilities and personnel at the Central Rice Research Institute at Cuttack. This is now, in our opinion, the best rice research program, group, and facility in the world.

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* Which may be considerable of an assumption. The Australian government offered substantial support for more facilities at Cuttack, but the offer was refused. Perhaps this was because the offer came from a Government.

The first step in inaugurating such an activity is to obtain officer personnel of the requisite high qualifications. It is, of course, impossible to overemphasize the importance of this step, which so largely determines the future success of the program.

For the strategic planning of such a program, and in the negotiation of the first moves which will so largely determine the future course, we are exceedingly fortunate in having obtained the services of a plant scientist of the highest technical qualifications, and a man who has established an international reputation for competence and good judgment. Professor Richard Bradfield, head of the Department of Agronomy at Cornell, will go out to the Orient very shortly after January 1, 1955; and for an initial period of twelve to eighteen months will be based there (initially in Tokyo, but perhaps later in Manila, Bangkok, and/or India). Subsequent to this full-time period, Professor Bradfield will devote three months of each year to this program in a supervisory and consultative capacity. It will be remembered that Dr. Bradfield was one of the original group of three who explored and planned an initial agricultural operating activity in Mexico. A connection of over ten years with us has made him completely familiar with Rockefeller Foundation ideals and procedures. He is intimately informed concerning our present agricultural activities. He has had extensive experience in Latin America, Europe, and as a trusted advisor of our government. He is, as these words are written, in Africa.

We have also added a second man to our staff, who may very probably fit into the Far Eastern program. This is Dr. Robert F. Chandler. Dr. Chandler was a member of our group in Mexico in 1946-1947. Subsequently he has been Dean of The College of Agriculture and President of The University of New Hampshire. Convinced that the international work of The Rockefeller Foundation in agriculture offers a unique opportunity for scientific activity and human service, Dr. Chandler has rejoined our staff. He and Dr. Bradfield are old colleagues and trusted friends; and we think that Dr. Chandler's unusual background of experience in scientific agriculture, in educational administrative positions of responsibility (which incidentally, he has occupied with distinction), and in international agriculture, fit him in a very special way to serve our program.

It seems to us premature to attempt to give at this time any detailed or specific statement of just what we could do, or what we ought to do, in the various countries named. This proposed program has the double purpose of serving the Orient in connection with rice, and of serving plant science in the whole world by a really broad and intensive study of the yield potentialities of a predominantly important cereal grain.

In the initial stages of developing a program we would propose to concentrate upon two aspects:

1) Basic and for the most part long-range scientific studies of rice (genetics, plant physiology, plant biochemistry, etc).

2) Training of personnel for work on rice.

The first of these objectives will doubtless involve:

a) Improving the existing centers for basic research on rice, chiefly in Japan and India.

b) Possible aid toward the creation of new facilities in the Orient for basic research on rice. Such studies will be primarily aimed at rice culture in the Orient, but will undoubtedly be of value to rice culture in Latin America, southern Europe, Africa, and elsewhere; and also of value relative to important problems of the other cereal grains of the world.

c) Possible aid toward improving the facilities for research on rice in places other than the Orient.

The second of these two objectives will require scholarship, fellowship, and professor training aid, both local and foreign, of highly varied and flexible sorts. Since lack of trained personnel is the basic limitation on scientific advance in the Orient, we would need to have a sizable, sustained, and very carefully managed activity for training men. We might also find it useful to give some limited assistance to places in the United States (chiefly Louisiana, Texas, and Arkansas) and to one or two places in Latin America, where men can be trained for international service in connection with rice.

We need to know a great deal more than we now do before we can forecast, with any realism, further steps in developing this program. All subsequent ideas about the general strategy of this situation will be presented to the Trustees well in advance of commitments of any sort - including commitments in our own thinking.

But it nevertheless seems reasonable to point out, even at this stage, that our experience to date in agriculture, and our present knowledge of agriculture in the Orient, both strongly hint that it will not prove wise or effective to follow any tight formula of procedure.

Thus while we do not at the moment contemplate any operating activities to be carried out by our own personnel, nor any large-scale demonstration activity to be carried out by others but partly supported by us, such activities may later emerge as highly desirable. But no commitments of any sort will be made in such directions without early discussion with, and approval by, the Trustees.

Similarly it seems likely that at some place or places in the Orient it may later prove desirable to work on crops other than rice, the degree of monoculture being almost surely excessive in some areas. But again, that is a problem which we can face later.

One feature of this program deserves special mention. All our work in agriculture overlaps medical and public health interests at various points (nutritive value of new agricultural products, health and the work output of a farmer, diversification of agriculture and diet, etc., etc.), and the social sciences at other points (marketing of farm products, farm management, political control of agricultural prices, land tenure, cultural acceptance of new foods, etc., etc.). In some instances it seems feasible to start, at least, scientific improvement of agriculture before beginning to take account of the "nonagricultural" factors.

But in the case of rice, the "natural science" and the "social science" and the "medical science" factors are so importantly interdependent that we are convinced that we must have available to this rice program, from the outset, expert counsel and help on the social, economic, and medical aspects.

In particular, we think that this program should, from the outset, if feasible, have the benefit of advice and guidance from an able agricultural economist - and, if possible, one who already has some familiarity with Asia and her problems. We may be able to work this out in terms of the part-time services of some outstanding person; or it may prove desirable to take on a full-time officer in this area. In any event, we think that these aspects of any agricultural program in Asia are so important that we should not neglect them, even temporarily.

Finally, in terms of money, we roughly estimate that something like five million dollars would be necessary for the now foreseeable part of this program. This would presumably suffice for about five years. The future from that point on would, we believe, be clear and compelling.

Warren Weaver
J. George Harrar

October 21, 1954