

INTER-OFFICE CORRESPONDENCE

915  
P+P

Agriculture

FROM: DR

DATE: January 8, 1954

TO:

LCD		LCD
JHW		JHW

COMMENTS:

MAY 10 1955

*- extra copy filed separately with ECSS covering  
letter of 12/22/53*

SUBJECT:

Attached is some material prepared at my request by Professor E. C. Stakman along the lines of an inter-disciplinary and inter-divisional approach to our agricultural program. I should greatly appreciate it if you could find an early opportunity to study and to think about this material in preparation for an inter-divisional discussion in the near future.

DR

Copy of material without memo went to JDR, 3rd.  
Copies, each with memo, sent to: LFK, WW, AJW, CBF and LCD (in absence of JHW).

UNIVERSITY OF MINNESOTA  
Institute of Agriculture  
University Farm, St. Paul 1

Department of Plant Pathology and  
Botany

December 22, 1953

MAY 10 1955

My dear Mr. Rusk:

On December 15 you invited me to write my reactions to a series of fundamental questions that you asked, which I hope are correctly summarized as here stated.

1. How best could studies be made to assure the soundest possible conceptual basis, the formulation of the wisest conceivable plans of operation, and the adoption of the most effective feasible procedures in agricultural improvement programs?
2. Assuming that acceptable answers could be found to question one, to what extent and by what means could concepts, plans, and procedures be universalized?

In the attached memorandum is a suggested plan of study, with the underlying considerations and reasons on which it is based.

If there are obscurities or ambiguities in the memorandum, it would be a pleasure for me to attempt to clarify them.

Sincerely yours,

(signed) E. C. Stakman  
E. C. Stakman

Dean Rusk, President  
The Rockefeller Foundation  
49 West 49th Street  
New York 20, N. Y.

Memorandum to President Rusk  
Regarding questions in attached letter

MAY 10 1955

The Problem

There is need for a politically and intellectually independent organization to make an objective and fundamental study of the degree to which man's primary physical wants remain unsatisfied, of the desirability and feasibility of helping to satisfy them, and of the principles and procedures that have been and are likely to be most effective in contributing to the solution of a problem of such great magnitude and complexity.

Some of the operating programs of the Rockefeller Foundation have been so conspicuously successful that they are widely considered as models of their kind. Can the accumulated and accumulating experience of these programs, supplemented by further studies and experiments, be combined to build an even broader and deeper basis for concepts, principles, and procedures that will amplify the benefits emanating from the programs themselves and also serve as a generally reliable guide for thought and action?

There now is wide diversity, considerable bias, and much confusion in the ideas and activities of individuals and organizations concerned with human improvement programs. Can a scholarly, scientific, and experimental study of the problem result in enlightening clarification and useful synthesis of thought and effort?

It would be a noble experiment to try.

Varying Aims and Criteria in Agricultural Improvement Programs

The aims and criteria of success in programs of agricultural improvement vary with time, place, and appraisers.

*Memorandum to President Rusk*  
*Is food enough?*  
*Unbalanced diet*  
*Effort on diet*  
*Who gets food?*  
*Women - pop - food - resources*  
*Country's recent work - primary food production*  
*Problems*  
*subject of our int*  
*on people*  
*Carded - 115 - A.P. Agric.*  
*Best mtg of all*  
*All def.*  
*Best - stand*  
*Worst confers*  
*Not a Bunch*  
*minute*  
*only*

*One would*  
*clear answer*  
*Be.*  
*Handwritten*  
*critic*  
*say*

*yes*

At times and places of food shortages the emphasis is on increasing total production, with efficiency of production and quality of product a secondary consideration. When there is adequate or surplus production, on the other hand, the emphasis is likely to be on efficiency and quality of production and on markets.

Aims and evaluation of accomplishments vary also with individuals and groups. Members of different scientific and other intellectual guilds are likely to place different emphasis on objectives and results. Certain groups tend to be satisfied with improvement in agricultural production itself; others consider it essential to determine the ultimate sociologic effects of this improvement. The difference is in the relative emphasis that is placed on the material accomplishments and on their significance in terms of human values.

In the narrow sense "agricultural improvement" connotes improvement in agricultural materials and methods that make it possible to obtain bigger and better yields of plant and animal products as easily and as cheaply as possible. This requires the application of scientific knowledge, technical skills, and certain economic principles to the business of farming. But curiosity and social motivation insistently demand an answer to the question as to how and how widely the benefits of better farming are diffused. This requires application of the criteria and methods of the social sciences. The combined knowledge of the natural sciences and of the social sciences should become the basis for a sound philosophy of agricultural improvement in the broader sense. It becomes, then, a question of the role that improvement in agriculture can play in human betterment and social progress.

*More to it than Bell's*  
*not food alone*  
*Do it in balance; Balanced moderation*

The basic objective of any improvement program should be to find the best ways of doing the most good for the most people. When more than half the people in the world suffer hunger, one of the most urgent human problems is to help them satisfy the primary need for food. But what are the best ways of helping the largest number? When the life expectancy of upwards of a half billion people is less than 30 years, another of the most urgent human problems is to improve public health. But what are the best ways? And what are the best ways of translating physical improvement in these fields into general social progress? Are people better when they are healthier and better fed; are they more sensible and more ethical? Does satisfaction of physical wants contribute to intellectual, moral, and spiritual development? Can natural scientists, social scientists, humanists, and social philosophers jointly develop sounder concepts, devise better procedures, and obtain more far-reaching results in cooperative improvement programs?

There is a plethora of diverse opinions regarding these questions, but the most valid answers require thorough study and adequate experimentation by a qualified group representing several scientific and intellectual fields.

#### A Proposed Plan of Study

*Why only agr.*

To assure broad and objective consideration of the multiple phases of agricultural programs, it is proposed that a group comprising representatives from appropriate fields devote about a year to a comprehensive and thorough study of past progress, present activities, and future possibilities.

It is suggested that the group comprise the following:



*Type of person?*

*Four fields*  
*Consultants*

1. A natural scientist, with wide interests and experience in agriculture
2. An agricultural economist, with interests in human geography *ag econ*
3. A rural sociologist, with interest in general sociology
4. A specialist in public health, preferably with some rural experience
5. A trained nurse, of the county-nurse type, preferably with some knowledge of home economics
6. An anthropologist, preferably with especial interest in demography.

The first four should be considered the minimum and the first three the irreducible minimum for effective study. If necessary to eliminate any because of financial or other considerations, the elimination should be in reverse numerical order. In any case, provision should be made to procure part-time service from each of the eliminated categories, either for special studies or for consultations.

*Thinking that should rather than do it own*  
*shortened*

In addition, it would be desirable to obtain information and judgments occasionally from several additional fields, such as agricultural engineering, agroclimatology, and history of agriculture. Knowledge and wisdom regarding agriculture and its implications does not reside solely in the scientific groups mentioned. Much can be learned directly from the experience of small- and large- scale agricultural producers; from those engaged in transportation, storage and marketing; from processors; and from consumer groups. Moreover, the ideas and concepts of a study group should be tested by sound thinkers who are free from the unconscious bias that sometimes insinuates itself into the thinking of specialists. Although this is implicit in "a comprehensive and thorough study," there are cogent reasons, derived from some non-Foundation activities, why it is made explicit.

It would be desirable to study the following:

1. The history of agricultural improvement and its implications in certain countries of Western Europe
2. The Rockefeller Foundation Mexican program
3. The Rockefeller Foundation Colombian program
4. One or more densely populated, food-deficient Asiatic countries.

*How  
about 1940*

Reasons are given seriatim.

1. The efficiency of agricultural production is high in such countries as Sweden, Denmark, Germany, and Belgium. There are reliable data for a considerable time on agricultural productivity, industrial development, education, public health, and population trends. Can meanings be extracted from these data; can principles be derived regarding causes, effects, and interrelationships? Could the facts and principles be used as guides in thinking and in action programs?

2. The Mexican program obviously should be studied because it is relatively adult. It was begun on a small scale in 1943 with clear recognition that it was an experiment that should continue for a decade or longer. The following reasons for selecting Mexico were stated rather explicitly: a, Mexico is our nearest Latin-American neighbor; b, the need for agricultural improvement is obvious and urgent; c, the obstacles are so numerous and formidable that it is "the hardest nut to crack" and "if we can operate a successful program there, we can do it anywhere."

There were indeed many obstacles to overcome and exceptional understanding, skill, and persistence were required to overcome them. Some still remain and new ones periodically obtrude themselves. To those who knew agriculture in Mexico for a quarter century prior to 1943, however, the Foundation's

success in producing materials, improving methods, and educating Mexican agronomists seems almost miraculous.

The values of the Mexican program expanded naturally to other countries, and, with some help and encouragement, the rate and extent of expansion have continually accelerated. But every program, however successful, should be studied and evaluated continually with a view to even greater success and wider usefulness, both in itself and for whatever transfer values it may have for other actual or potential programs.

Some of the concrete accomplishments of the Mexican program, such as increase in crop yields, can be measured with a fair degree of accuracy. Other accomplishments, such as improvement in attitudes, knowledge, and skills, are not precisely mensurable, but they deserve evaluation by systematic and adequate study rather than by incidental and occasional observation. The very success of the program in attaining so many of its primary and secondary objectives is incentive to study it more fully.

3. The Colombian program should be studied because of its precocity and future possibilities. Although young in years, it already is rich in results and promise. Why? An easy answer would be that it is because of the direct transfer of key men, materials, and methods from the Mexican program. This certainly is part of the answer, but is it the complete answer? Because the program is young, it is possible to set certain bench marks against which to measure progress. It would be possible also to incorporate new features into the program early enough to synchronize them with those now in progress. The relatively easily accessible climatic and ecologic zones, with gradual transitions from steaming tropics to the cold sierra, furnish exceptional

*Defects  
of program*



opportunity for agroclimatological and other studies of all of the most important kinds of crop plants. This diversity, strategic geographical location, and ease of communication with other Latin-American countries, make Colombia a potential center for basic and regional studies that could benefit all of Latin America.

Moreover, the ethnologic diversity of the population furnishes excellent opportunity for studying the human implications of improvement programs.

4. The aim of studies of the kind under consideration here should be not only to explain and evaluate the past and present but also to predict future possibilities and contribute to wise planning and efficient action. Like any complex, long-time biological experiment, agricultural improvement programs should be planned with as much foresight as possible in order to obtain the maximum of information and enable analysis and interpretation of results. How much of the experience in Latin America can be projected into the future; how much of it could be applied in Asia? What factors should be taken into consideration in trying to make a perceptible impact on hundreds of millions of Asiatic people? What are the prospects that the situation can be alleviated? The problem deserves careful study.

The Mexican and Colombian programs can be considered as controlled experiments on the effects of agricultural technology, research, and education. The fullest meanings should be extracted from them and they should be improved if possible. Moreover, the Western Hemisphere is actually or potentially the strongest bulwark of western civilization. Can a demonstration be made of the value of international cooperation, rather than contention, in the solution of common problems?

*Subject  
Procedure*

*Approval  
of Mexico*

*My  
suggestion  
to CIB*

Latin America in general is rich in natural resources but relatively poor in scientific resources. Asia is poor both in natural resources in relation to population and in scientific resources in relation to the magnitude of the problems. Both should therefore be studied. There are human needs and scientific opportunities in both areas.

#### Probable Value of Proposed Studies

The proposal is essentially for a study of important aspects of past and current agricultural history, for purposes of orientation and future effort. The history of the development of farming techniques is important, but a comprehensive history of agriculture must include its economic, social, and political implications.

Agricultural history is being made in the Rockefeller Foundation Mexican and Colombian programs. Not only have they increased agricultural production but they are blazing new trails, because the experimental attitude has been maintained throughout. It was a foregone conclusion that acre yields of important food crops could be increased; but is it completely known how much and how economically they can be increased? What is the maximum potential of crop yields and what is the maximum possible under natural and economic limitations? Fundamental researches are needed to answer these questions, and they should be made in close conjunction with operating programs. This would require supplemental effort, because the demand for immediate improvement often is so insistent as to preclude the practicability of doing enough fundamental research. And yet many ideas and materials for needed basic researches are important by-products of programs of experimentation. One of the great values of the proposed study would be to make sure that these by-products be fully utilized.

Effort  
of  
increase

If the present operating programs are "controlled experiments," everything possible should be learned about their educational value. After all, the ultimate objective is to help the Mexicans and Colombians to help themselves, an educational objective. In formal education every class is or should be an experiment. Most classes are complex, but certainly a nation is far more complex. After decades of experience in teaching, some professional educators still maintain that almost nothing is known about it; others sometimes profess to know so much that their dogmatism kills the experimental attitude. Can it be that disagreement regarding teaching is because too little is known about learning? The Mexican program is old enough so that some of its educational values can be determined, and it is young enough so that they can be augmented. The system of "internship" assuredly has had great educational value, but how great? What has been the educational impact on rural schools, on adult farmers, on administrators, and on politicians? A systematic study would be valuable.

To what extent could experience from the Latin-American programs be utilized elsewhere? There already is a demonstration of considerable transfer value for Latin America. But how much would be applicable to a politically, racially, and linguistically heterogeneous region like the overpopulated areas of Asia? If present population trends continue in Asiatic countries, more land must be brought into cultivation, or lands now under cultivation must be made more productive, or more food must be obtained from water, or food must be imported, or there will be even more hunger and famine. If the objective is to do the greatest good to the largest number as quickly and inexpensively as possible, all available and applicable experience should be used as a basis for plans and procedures if it were decided to try an experiment in Asia.

*Principles  
of  
teaching*

The alternative possibilities are clear. Either more land and water must be utilized or what there is must be utilized better.

Most noncultivated Asiatic lands are either arid or tropical. Arid lands can be made productive only by getting more water for irrigation, by using what there is more efficiently, by trying to increase rainfall, or by introducing or producing kinds of crop plants that need less water than those now grown. Sorghums in general need much less water than corn; many forage grasses thrive in semi-arid areas. A long-time objective, and in some areas the most hopeful one, would be the improvement of xerophytes, plants with low water requirement. There already is some experience in Mexico with plants of this type, notably sorghums and some grasses.

Study of the results and intensification of experiments in arid areas of Mexico and adjacent United States should yield information of general and far-reaching importance in the possible agricultural conquest of arid zones.

Both Mexico and Colombia have considerable areas of land in the humid tropics, where the problems are too much water; alternation of extreme wetness and dryness; the persistent encroachment of jungle or other undesirable vegetation on cultivated land; the continual menace of crop diseases and pests; and the enervating effects on man of weather, insect pests, and diseases. Lessons of wide potential utility can be learned in many areas of Mexico and Colombia.

The programs in Mexico and Colombia have combined education with experimentation. Direct participation in experimental programs has given many young Mexican agronomists a new and valuable kind of education; the use of experiments as demonstrations has helped in the education of many farmers.

*Dr. R. H. Whittier  
was up  
to  
Zacatecas*

Some of the methods learned in Mexico would probably succeed in other areas also, but much more could be learned about procedures and results with relatively little additional effort.

Plant materials, especially varieties of corn and wheat produced in the Mexican program, already are being used either directly or in varietal improvement programs in many countries of the western hemisphere, including Latin America, the United States, and Canada. And some varieties of wheat are being used in breeding programs in many of the wheat-growing countries of the world.

Certain universally applicable scientific facts and principles also have been exported directly from the two operating programs. This is notably true of two of the most devastating of all plant diseases, stem rust of wheat and late blight of potatoes.

*Other values* | The question always arises as to the kind and scope of effort that will do the most good at least expense. Education, experimentation, and research, each has its partisan supporters; but Mexican experience is eloquent in teaching that they can and should be combined into a synergistic system. Prior to 1943 many beautiful blueprints were made for agricultural extension in Mexico and elaborate organizations were put into operation. But they failed because there was not enough pertinent knowledge and there was no effective experimental organization to acquire it. It was relatively useless to advocate growing improved varieties when none were available; to preach soil improvement when fertilizer requirements were not known; and to prescribe for the control of the sigatoka disease of bananas when the little that the better growers had learned empirically greatly exceeded what the extension men had learned by rote. Intellectually honest teachers and extension men



felt frustrated, as did those whom they tried to teach. The Rockefeller Foundation program of experimentation, together with some research, has yielded many materials and a sound body of facts for teaching and extension. But agriculture is a biological enterprise and therefore dynamic. It has become clear that more basic research is needed in order to maintain some of the gains from experimentation and to overcome new obstacles. The changes in the wheat rust situation is only one example.

*which  
other  
side*

Agricultural education in Mexico was largely sterile until fertilized by experimentation; and it soon became apparent that experimentation alone was inadequate for the solution of some problems unless fortified by research. This is one of the clearest lessons of the Mexican experiment. How to combine research, experimentation, and education into an optimum synergistic system in Mexico and elsewhere, however, is still a problem for study.

Scope of effort can be measured by the number or by the magnitude of objectives. The scope of the cooperative program in the State of Mexico, as an example, could be expanded to include all activities and agencies that could contribute to social improvement: research and education in agriculture, health and sanitation, home economics, and community organization. This could be a wonderful experiment of broad scope in an area small enough to make intensive study possible.

Objectives in limited fields could be national, regional, or worldwide in scope. An original objective in Mexico was to improve corn, wheat, and beans for Mexico. The objective could be made worldwide instead of nationwide by modifying it somewhat. It would be possible, and possibly desirable, to concentrate on incorporating into lines of the principal food crops the

best possible combination of genes for yielding ability, disease resistance, or any other universally useful character, without considering adaptability to particular areas. These lines could then be given to breeders in all interested countries for use in developing varieties adapted to their conditions. Obviously some countries have the personnel to do this; others would need help. There are numerous problems, such as control of certain epidemic diseases and insect pests, that require regional experimentation and research. And others, such as improvement of crop varieties, could be put on a regional and international basis where ecologic zones extend over several countries. Still other biologic problems are national or even local in scope; and many economic and sociologic problems are national in scope because they are peculiar to certain peoples and nations.

Because needs and objectives differ so greatly, it is unlikely that a single master plan would be universally effective. Certainly the benefits of the Foundation's national programs already have expanded internationally. Can they be modified and supplemented in such ways as to make them more useful to more people in more countries? Can generally valid guiding principles for improvement programs be developed? The prospects are good, the problem deserves study, and there is consensus among many interested people that the Rockefeller Foundation is the best agency to study it.

E. C. Stakman