An Informal Report to the Executive Agent, Committee for Coordination of Investigations of the Lower Mekong Basin

SOCIO-ECONOMIC RESEARCH ON THE AGRICULTURAL DEVELOPMENT OF THE LOWER MEKONG BASIN : Selected Observations on the Experimental and Demonstration Farms

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Introduction

The Executive Agent of the Lower Mekong Basin Committee extended an invitation on November 4, 1965 to visit the Lower Mekong Basin Scheme and to advise "on relevant research problems for agricultural development." Three terms of reference were set out:

(1) look into the deficiencies of research other than those in a purely technological field, into problems that stand in the way of agricultural development in the Lower Mekong Basin, and to advise on such a program taking into account organized efforts in this line now being taken;

(2) study the possibilities of organizing a closely controlled research on the impact of a comprehensive approach to agricultural development, using a sizeable pilot study area in one of the irrigable projects of the Mekong Committee's Tributary Projects as a basis for establishing a set of experiences that may be applied to much more extensive irrigable areas of major mainstream projects;

(3) participate in the Mekong Committee's sponsored Seminar on Agricultural Experimentation and Demonstration and to make a statement, if possible, on the socio-cultural aspects of agricultural development.

The invitation from the Executive Agent was approved by the Agricultural Development Council as a worthwhile activity for a member of the staff of the Council and in keeping the Council's objectives of "supporting teaching and research related to the economic and human problems of agricultural development, primarily in Asia." Hence, the full costs of my visit were met by the Council.

/ The three terms

The three terms of reference were not accepted completely; only the first and third objectives were to be completed in January. The second objective was held in abeyance until the first two had been accomplished. Since the experimental and demonstration farms constitute primary activities for the diffusion of new crops, new practices, and improved water use in developing an irrigated agriculture associated with the proposed Lower Mekong dam developments, participation in the seminar offered an excellent opportunity to observe what has been accomplished to date and the problems which are being encountered. The Director of Economic and Social Studies, Mr. I.S. Macaspac, provided considerable material dealing with the scheme and the prior research on the socio-economic aspects of Lower Mekong developments. Several papers dealing with socio-economic aspects of the Lower Mekong Basin developments which had been submitted at previous seminars were also made available. Of course the most comprehensive previous study on the topic was the report prepared by Gilbert White and associates and submitted in January, 1962. All of these materials were consulted prior to arrival in Bangkok and Vientiane.

The background papers plus participation in the seminar provided only the briefest insight into a few of the issues and problems which seem to be emerging in connection with socio-economic research on the Lower Mekong River Basin development. Although the time was far too brief to allow for a full scale and proper evaluation, there were several issues which emerged quite rapidly, both from reading the materials and from participating in the seminars. The comments which follow should be viewed entirely as tentative and not as definitive by any means.

Since most previous socio-economic evaluations of the Lower Mekong have dealt with macro or aggregative issues, my remarks will deliberately concentrate most heavily on the micro, village or farm level. The primary focus of my remarks are therefore directed to the experimental and demonstration farms which are intended to serve as a key activity for the introduction of irrigation farming.

/ The keynote

The keynote address at the recent seminar given by the seminar Director set out the following objectives for the experimental farms and pilot demonstration farms:

"1) To establish through trials, types of crops, crop varieties, cultivation methods, rotations, soil management methods, irrigation requirements, methods and practices suited to local conditions, taking into consideration the prevailing socio-economic conditions and using for this purpose the experimental farms.

2) To define farming patterns and farm sizes, establish methods of management for irrigation schemes, including methods of water administration and participation of farmers in the operation of the schemes, using for this purpose the pilot demonstration projects.

3) To train technicians, farmers and extension workers in irrigated agriculture.

4) To provide the planner and the economist with the data needed for feasibility studies. Such information will be required mainly on crop yields, water requirements, operational methods and production costs, together with information on other technical and socio-economic questions"

Based upon my participation in the seminar and upon my observations both before and after, I would like to list several areas where I believe the approach is weak or incorrect and where new approaches seem required.

The Integration of Socio-Economic and Technical Research

A major weakness of the experimental farms is their failure to include basic socio-economic research as an integral part of the technical research and experimental facets of their work. A most puzzling feature connected with experiment stations on land development schemes, and not limited to the schemes involving new irrigation, is the repeated and persistent failure of such institutions to recognize that research on the socio-economic aspects of developmental change is equally as important as technological, physical

/ and biological

and biological research. For example, providing water on an irrigated basis to an area which previously has been rain-fed usually requires the introduction of new crops, new farm practices, new soil practices, new terracing, leveling, etc. Each one of these changes must be carried out by human beings -- the farmers themselves. Although everyone recognizes that these areas are important, only "lip-service" is given to its importance. The general rule is to view research on the economic, sociological, political, anthropological aspects as a trivial or bothersome appendage to be added after the technical problems have been solved. Rarely is it realized that the two must be attacked jointly.

Research on the farmers and what may be their desires, preferences, resistences, objectives and values as well as the economics of the changes which are being proposed are very rarely investigated. It is assumed that the provision of the water is in itself a sufficient answer and that the farmer will automatically see the benefits, will make use of it and make the necessary changes in production to adopt irrigation. If he does not then make the necessary changes which the experiment stations show on a technical basis to be feasible, it is "obviously" the farmer's own stupidity, traditionality, backwardness, illiteracy, etc., which prevents the change. It is very rarely recognized that in many instances the changes which are being proposed, while technically feasible, may very often have an economic or human facet which makes them unfeasible. Even in the United States, we do not persue technical perfection in each and every field or for each and every activity -- the human dimension inevitably comes into play. In all instances, man weighs and balances the technically optimum aspects of any change against the other facets of human life.

Under these circumstances, it is equally important to include in any technical research enterprise its social, human and economic dimension. For example, there is no question that an improved, higher yielding variety of rice can be developed. What is not frequently recognized is the fact that these changes may themselves involve a plethora of related changes which must take account of such elements as the taste of the new variety as it relates to the preferences of the consumers (the farmers themselves) and the height

/ of the rice

of the rice stalk as it relates to the physical height of the harvesting farmers. Such comments are well known and can be expanded at length, yet when it comes to the establishment of an experiment station or a demonstration pilot area the personnel to investigate these aspects are very rarely ever included.

Developing all the new varieties which might be grown under irrigated agriculture and all the new practices which might be required for the utilization of these new crops is well and good, but until there has been study of and experimentation upon the equally important, related economic and human facets which are correlative with the technical, the project is bound to flounder. This has been the finding throughout the world, and it is not limited to Asia. Farmers throughout the world are not necessarily backward or stupid because they resist changes. Until one is cognizant of what the causes of these resistances are and until one can establish the economic realiability of the proposed changes which are being promoted, then projects will not be adopted. I might point out parenthetically that it is no accident that in the pilot project area which was described so ably by the Israelis at the seminar, such a small percentage of the farmers were cooperating in the project even though the project had been operating in the area for several years. Note that when I asked the question whether or not any systematic investigation had been made: a) among the farmers who were not cooperating to find out why they were not cooperating; b) among those who were cooperating to learn why they were cooperating; and c) among any farmers who had previously cooperated and dropped out to see why they stopped -- the answer was that such studies had not been done. Even though the project had been underway for some time, no one was asking the questions as to why the farmers in the area were not taking full advantage of the water which was being provided free-of-charge. The simple answer of course is always to say that the farmers are illiterate, traditional and therefore do not wish to cooperate. However, if such a low percentage of the farmers in the pilot project area are not cooperating during this "experimental" or "pilot project" phase, then one must ask the question what will be the level of cooperation when the project is completed and water is available for all of the farmers in the area. Under these circumstances all the technical research and cost/benefit analysis in the world is not going to make the farmers shift to irrigated farming to secure the estimated level of benefits !

/The need is not

The need is not only for the inclusion of socio-economic aspects in the operation of experimental farms and pilot project demonstration areas, but also for its inclusion as an integral part of the research endeavor. One step toward this end would be the assignment of a farm management expert or agricultural economist to each of the experimental stations. Also, the central secretariat of the Mekong Committee should have in the Economic and Social Studies Division at least one full-time rural sociologist with specialization in innovation diffusion. Such an expert could suggest sociological research for the experimental stations and supervise research projects in the various pilot areas.

Research with "Operational Significance"

One of the stated objectives for the experimental farms and pilot demonstration areas is that they should "take into consideration the prevailing socio-economic conditions." Conducting new research will be required, but not in all cases. The prevailing socio-economic conditions in certain areas of the basin have already been studied by many social scientists. The library of the Mekong Committee should make every effort to assemble in its archives all published and unpublished research which has been done within the Basin areas.

This task requires more than the collection of descriptive summaries of the socio-economic conditions of the areas which will be serviced by the irrigation schemes. Collecting the findings of previous research in the form of purely academic treatises is also not the full answer. There is an even greater need to capture the "action relevant" aspects of previous research findings. Researchers are not always cognizant of or interested in those facets of their work which have operational usefulness. Moreover, determining those findings of social science research which have "operational significance" or "programmatic usefulness" is difficult.

As I have pointed out in a previous paper, there are three ways in which research findings may have relevance for action programs: diagnostic, prescriptive and evaluative:

"First....

"First, determining the major and priority problems in a given area, country, or region. This task is "diagnostic" because it requires careful examination to distinguish symptoms from causes in the determination of the real problems.

"Second, assisting in designing programs and projects which will have maximum effectiveness. The task here is basically "prescriptive" or "constructive" in that research findings may help in the formulation of activities which will effectuate the desired changes successfully. Research helps to make the appropriate selection of organization, institution, technique, approach, etc., from among the wide range of available choices.

"Third, providing for on-going or ex post evaluation of the program or project. The task here is to improve either upon the execution of the activity in question or upon similar activities in the future. Research in this instance adds to our available "operational" or "programmatic" knowledge."

My comments are particularly devoted to the second area. Socio-economic research, when related to capital infrastructure projects, is far too frequently thought of purely in "feasibility" terms -- the collection of data to determine the feasibility of the construction or else to prepare the cost/benefit estimates for evaluating the project.1/ What tends to be almost completely forgotten or ignored is the feasibility of the implementation of the project. Even more important its implemental feasibility must be approached from the farmers' standpoint. I would argue that the more relevant feasibility should be that of the farmer who is going to be served by the project. In other words, to what extent are the suggested changes, practices, operational methods and so forth, viewed as feasible by the farmers rather than by the researcher. Most researchers engaged in making feasibility studies tend to do so from the standpoint of their own values and their own set of criteria for determining feasibility. They rarely pay attention to the notions of feasibility which are held by the farmers themselves. The only way in which one can discover what are the notions of feasibility held by the farmers is to study the farmers. Unless this is determined in advance, one is apt to find that once the dams have been built, and that the water is flowing, the projects, practices, and schemes which were considered to be feasible by the researcher are not considered to be feasible by the farmer, and the project founders.

/ Implemental

1/ See my chapter "The Infrastructure for Agricultural Growth" in Agriculture and Economic Development, Herman Southworth and Bruce F. Johnston, Eds. (Ithaca: Cornell University Press, 1966)

Implemental feasibility studies are only one facet of action-oriented research. Many other studies are required. Other studies have already been made but require "translation" before they will be useful for action projects.

Every effort should be made to collect previous studies and to "translate" their findings into "operational" terms which are relevant and useful to the Mekong projects. This activity should be an on-going effort by the Mekong secretariat staff responsible for economic and social investigations.

Another activity which would promote the "translation" of previous research findings into useful program guides would be to convene an international conference. At such a conference presentations could be made by Southeast Asian and Foreign social scientists specifically designed to "translate" their previous work and to summarize current knowledge about the basin. The participants in such a conference should be limited to persons who have made empirical studies in the area. The conference should also include officers from the national governments charged with various action and developmental efforts related to the Mekong scheme, plus selected experts from the United Nations.

Cooperative Research

The available human resources for the conduct of research on the social, economic, and human aspects of agricultural development in the Lower Mekong Basin are naturally quite limited. It is also understandable that the budgetary allocation by the Lower Mekong Committee to such efforts has been very small. However, these facts need not result in a deficiency in the amount of research undertaken since there is a sizeable reservoir of research talent available outside the region.

The Executive Agent should take every opportunity to foster and encourage joint, cooperative research between the nationals of the riparian countries concerned and foreign social scientists. I believe that every piece of social science research conducted on the Lower Mekong Basin by an "outsider" ought to include a social scientist from the area. The projects should be joint in the sense of one of the participants or project leader

/ should be a

national of the Lower Mekong group and the second cooperative person should be somebody who is a foreign expert from abroad. Such a linkage would guarantee or at least insure that the research findings of projects would have a far greater degree of validity for the national scene and would also give the recommendations a greater degree of acceptability on the part of the national governments concerned. I am convinced that only on a joint or cooperative basis will it be possible for the Lower Mekong Committee to take advantage of the available manpower which is outside of the Mekong Committee area and at the same time improve the level of competence in technical skills of the social-scientist who are nationals of the countries concerned.

There are several individuals in the United States who have had experience in or have conducted research in the Mekong country areas and who would be available for the purposes of engaging in a joint research effort of the kind which I have in mind. For example, the newly developed Thai Committee of the Southeast Asia Development Advisory Group (AID/Asia Society) is a perfect example of a resource which might be employed for "beefing up" the research which could be undertaken by the Economic and Social Studies Division of the Lower Mekong Committee. Even within our own special program of the American Universities Research Program, it is possible for various professors at the American universities to come out to engage in research projects dealing with any one or several facets of the Lower Mekong River Basin Development.

Careful attention must be given to the various techniques which might be employed in order to encourage this kind of joint or cooperative research effort. The Mekong Secretariat itself could be useful as a coordinator for such joint research ventures. In addition, other arrangements may be required which would assure that the research is joint and that the findings become available to the countries concerned. Another approach might be the establishment of "Mekong Research Centers", either governmental or private, in each of the countries which would serve as the hosting institution for the foreign researcher when he comes into the country.

/ Research Outside....

Research Outside of Pilot Areas

The projects of the Lower Mekong basin will undoubtedly cover more than just the pilot areas. The amount of variation and variability which exists between regions to be served by the projects is considerable. Under these circumstances, every effort should be made to not only to assemble the information which has already been collected on previous surveys for the pilot project areas but also to secure additional information for areas outside of these areas which would be useful for the eventual conduct of the projects.

As I have pointed out elsewhere, the previous research which has been undertaken has dealt almost exclusively with the collection of data which is important for feasibility studies or for the purposes of cost/benefit analyses, but not with regard to the question of the implementation of the projects themselves. Much more information is required about various facets of the on-going agricultural productive process in these regions as a whole. Very often such surveys are referred to as "base-line" surveys for the purposes of making the necessary comparisons of the "before" and "after" variety. In the present instance, I believe that the "before" and "after" variety of investigation or study is a very minor consequence. Far more important is the information which is needed in order to be able to step up the pace of water utilization, crop adoption, and new practices, innovation, which will be required if the project is to achieve its maximum contribution to agricultural production in the countries concerned. The farmers who will ultimately be served by the projects do not necessarily have the characteristics of those who are currently found in the pilot areas. Under these circumstances, it is especially important that the research which is conducted cover as wide a range in variance based upon the predominent characteristics of the farmers of the region, as is possible.

Finally, it should be recognized that many farmers will not be within the irrigated areas when the projects are completed. Prior studies of these areas are important both to learn the possible complementarity in production

/between them

between them and the irrigated areas as well as to be prepared for divergent growth rates between the two. The latter point has obvious political significar since more rapid rates of agricultural growth within the irrigated areas without corresponding improvement in the unirrigated areas adjacent to the project will inevitably lead to political disaffection.

Demonstration, Extension, and All That

Although my terms of reference were directed at research, I would be remiss if I did not conclude with a few remarks on the attempted use of experiment farms and the pilot project areas for demonstration purposes.

There is a basic fallacy in the way in which the agricultural experimentation and demonstration farms are being used. This fallacy bluntly stated is the belief that demonstration farms will be capable of providing the necessary basis for communicating the needed changes in agricultural practices among the farms to be serbed by the dams. There is no question whatsoever in my mind that the experimentation aspect of these farms is vitally important. But the demonstration aspects of these farms is basically fallacious as a means to achieve the necessary changes by the farmers served. Demonstration farms admittedly have had a great deal of promotion in a number of quarters and they do make some contribution to innovation and diffusion. But I believe that it is a very proper conclusion that demonstration farms have rarely "demonstrated anything" or been the major force for conversion of agricultural practices, the introduction of new technology, or the introduction of new crops.

There are a wide range of techniques which might be employed for disseminating information and leading or causing farm practices to change. I would rank demonstration farms at the bottom of the list. This does not mean that one should alter the experimentation aspects of these farms. These should be continued without any question. But to make use of the farms as a demonstration center for the purposes of disseminating information with regard to new water use practices and agricultural practices, or for causing the farmers served by these schemes to change their agricultural practices will prove to be a most inefficient method.

/ Further,

Further, I would add, that my view does not mean that a proper method for disseminating new information or leading to necessary innovation and technological change could not take place using the demonstration farms. There are several possibilities of developing viable means for extension education using the experimental farm. The best way to do so would be under the following conditions.

(1) That each experimental farm have attached to it a minimum of one extension education specialist;

(2) That these indiviauals coordinate their activities with the technical people on the problems of agronomy, soils, water use and water practices, etc.;

(3) That the team engage in what I would call "experimental extension education." (This is a point which I stressed a great deal during the conduct of the seminar.)

Any careful study of agricultural development programs to date will reveal that there has been repeated failure to recognize that agricultural development must be analyzed in its complex totality focusing upon the key interacting facets, each of which must be studied in a particular context and in a continuing on-going fashion. Solutions which concentrate upon a single factor to the exclusion of all others rarely are successful. Moreover, the critical problems in any region need not be the same as in another. Even where a program attacks successfully a combination of critical factors, we must recognize that there is very limited transferability to another problem situation. Failure to appreciate this fact has been the cause of "pilot project" failures when attempts are made to "extend" the project on a wider scale. A fascinating chapter in the history of agricultural development could be written on the rise and fall of "success stories" in programs of agricultural development -- SCIPA in Peru, ACAR in Brazil, the Rockefeller Foundation in Mexico, Etawah in India, Gezira in the Sudan. Today's magic programs are: JCRR in Taiwan, Comilla in East Pakistan and the Red Book Rural Development Scheme in Malaysia.

/ Maintaining an

Maintaining an innovative and experimental spirit is vital to the success of village level developmental approaches. Many successful "pilot projects" have failed when attempts have been made to duplicate them on a national basis because of a failure to recognize the importance of maintaining an experimental spirit. It was this spirit in the pilot project which frequently enabled it to develop a program with activities which met the localized needs of the pilot area. In many cases it was not the particular projects or approaches which were the basis for success but the fact that innovation and experimentation led to the adeptation of projects and techniques as well as the development of uniquely suited approaches. When attempts are subsequently made to duplicate or to extend the pilot project unchanged as though the original was the "master plan" or "magic key", there are predictable disappointing results.