CORNELL AGRICULTURAL ECONOMICS STAFF PAPER

A FRAMEWORK FOR ESTABLISHING AGRICULTURAL RESEARCH PRIORITIES

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January 1972 No. 72-8

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The subject proposed for our discussion is the process of establishing research priorities and allocating scarce resources among competing alternatives. The question clearly was posed in the language of economics. But the process requires more than economic analysis - social, political, and human issues are all involved. By inference there is a suggestion that allocation may be different in LDC's from that in richer ones. Yet one cannot help but wonder if it is really so. Perhaps the cost of committing errors is relatively greater or the gains recognizably larger. But, economists are likely to approach the question in a traditional manner, borrowing on theory and experience, often mixing pragmatism with a generous dash of expediency.

A short paper[1] by my friend and former colleague, Randy Barker, arrived about the same time as the invitation to participate in this workshop. He asked a set of questions about production economics as taught in our graduate schools and its applicability in detail to problems in quite different societies. He concluded that "there are problems in the direct transfer of theory and techniques from the developed to the developing economies. It is not that the machine doesn't work (that the principles don't apply), but that often the machine is not well suited to the new environment and resource situation."[2]

Barker's perceptive observations on the problems of American trained Ph.D.'s trying to use their recently learned quantitative techniques to solve real world problems in LDC situations should give us all some humility in providing stock

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* Prepared for Research and Training Network of A/D/C Workshop, New York, N.Y., January 17 and 18, 1972.

1/ "A Note on Production Economics," paper prepared by Randolph Barker for ADC Conference on Production Economics, Korea, December 2-4, 1971.

2/ Ibid, page 4.

answers to a tough administrative question. It is interesting to think about the actions of agricultural economists who have some opportunity to make these same kinds of allocative decisions in their own environment as department chairman or research administrators. We lecture on marginal analysis and comparative advantage much more eloquently than we apply it to administrative decisions on our own campuses or research establishments. It is always easier to give advice than to act on it.

But, all this prologue does not speak to the original problem posed. Despite the folly of responding to the question directly, we will make some comments. The resulting advice did not cost much and may be of equivalent value.

Some basic generalizations

1. Accept the local institutions of the LDC as given. The foreign expert has no business trying to force his ideas about "efficient or workable institutions" on the administrators or government he seeks to advise. Many already know something about western successes and failures. Discovery of new or different institutions should be the province of resident nationals. If it is their idea, or they feel it is, then it has more chance of success.

2. Spend most of the time in your advisory role as a listener. It is important to learn as much as you can about the people, institutions, social and political structure. Objective information is necessary if sound decisions on allocation are to be made. Much of this must come from local sources. Initial statements from resident nationals should be pursued. It is important to listen to the technicians within the system as well as the political and administrative leaders. Take time to learn the pattern of leadership that exists and how that pattern came to be. Too many comments too early may well limit any chance of hearing from those who have something to say.

3. Expand research effort where accomplishment has already been demonstrated. The old adage of backing a winner is perhaps over used, but still valid. Investing more resources where someone has already demonstrated the capacity to produce objective results and bring them to some degree of use is wise in all societies. As an adviser listens, he should try to learn who has the capacity to do research on applied problems and who has also demonstrated the ability to carry through on a project. Both the capacity to do research and the drive to get it done must be demonstrated. Here resident nationals and foreign technicians deserve equal scrutiny.

4. Propose new areas of research or work with care. New ideas and innovation must be encouraged. Yet, research programs must develop information which deserves and obtains internal support. Research which will influence public policy over a period of years must be strong, objective and understandable. A long-run objective of any research enterprise is public recognition of the importance of that research. Few research agencies have the administrative capacity or personnel to embark on a whole new set of activities simultaneously. New directions using local support must come in sequence.

5. Encourage work on research projects for which proven methodologies and techniques of analysis are available. It is easy to find or identify problems that need solution. It is much more difficult to establish or propose methodologies or procedures that have a reasonable chance of providing answers. This generalization has been demonstrated repeatedly by scientists in all kinds of settings. The scarcer the resources, the more important this point.

6. Decision making on research and its administration is the province of the institutions or the governments for whom you are working. This statement is so obvious it may seem out of place. Eager advisers too often forget their function particularly if they serve over a period of years in such a capacity. Good

advisers will encourage local administrators to learn by doing, including making "errors" and taking responsibility for them.

7. Try to use the linear programming format as an organizing device in making allocation decisions. This is not to suggest that allocation decisions should be formalized into a simplex and run on a computer. Quite to the contrary. But, the organizing principles have great appeal in getting ready to make decisions.

(A) There is a need to determine a specific "objective function" for the research enterprise. This should be formulated in the language and terms of the resident nationals. But, the need for a specific statement is very real. If you can't define what you seek to accomplish and who is to benefit, fuzzy analysis is likely.

(B) As every LP student knows we must determine the fixed or limiting resources and their quantity. The same need exists in studying a research organization. Human or scientific resources in various categories, research overhead including land, buildings and equipment, financial resources for annual expenditures, potential sources of help from international agencies or other countries and the like should all be considered. Limiting resources need the same careful evaluation and categorization as required by LP in any familiar business management problem. The capacity to classify these resources into mutually exclusive groups and to identify quantities available is no less difficult or important. For example, what really is limiting: able plant breeders, controlled experimental plots, field managers, analytical equipment or what?

(C) Recognition of the set of conditions within which research is to be conducted is fundamental. Again the analogy with linear programming seems appropriate. Any solution has meaning only within the constraints stated for the original problem. If these are not established, then the allocation decisions provided by the analysis may easily be misinterpreted. Such issues as the audience to whom research results are directed, the existing level of knowledge of these audiences, and the other sources of information already available to these groups are cases in point.

(D) Feasible activities or processes and the technical coefficients which go with them must be specified. For example, it is easy to argue that research on improved varieties and associated production practices are important subjects for research in most LDC's. Feasible activities in the tableau are determined by understandable technical coefficients that come in lumpy or sometimes non-divisible packages. Work involving a plant breeder who seeks to develop new varieties of a crop requires a substantially different package or supporting resources than a project seeking to test a range of varieties already developed at other locations. Developing "activities" like these for analysis is an important part of the analytical process both for the foreign adviser and the group with whom he works.

In summary, an economic problem phrased in the language of economics should be solved by an economist using tested economic methodology. This is the reason for drawing on the analogy from traditional linear programming. But, a successful adviser or administrator should use the logic of the tool rather than simply drawing on its mechanics. Once research allocation decisions are specified in these terms, some economic solution is at least possible. Resident nationals can then draw on the complex on "non-economic" criteria as well in making "better" final decisions. This process may not provide the "best" or "optimal" decisions in the language of economics, but it should lead to better judgments which is a good objective in any society.