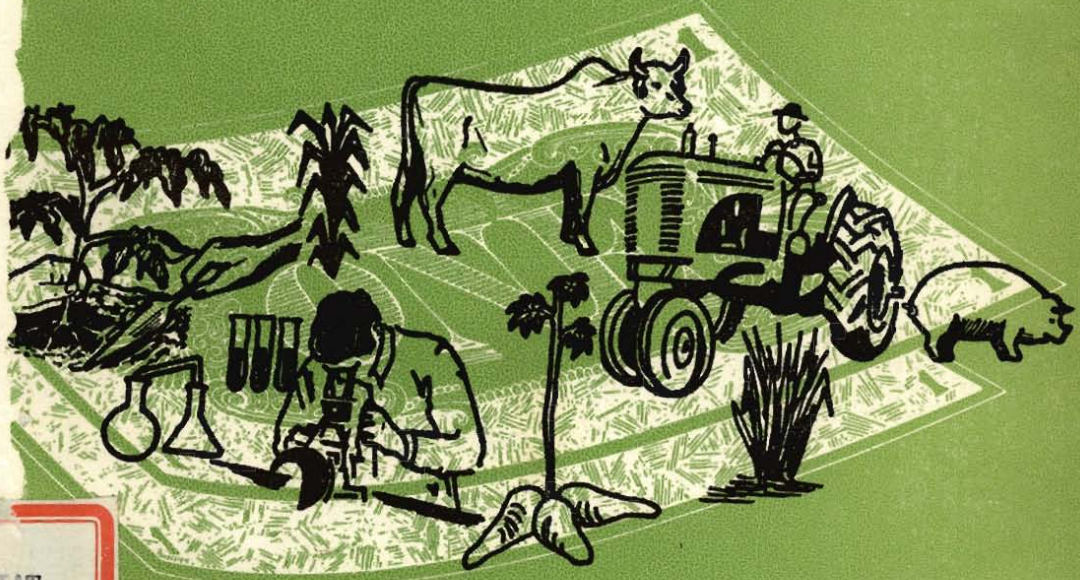
Series CE-11
November, 1975

Methods for allocating resources in applied agricultural research in Latin America



Editors: Per Pinstrup-Andersen
and Francis C. Byrnes

Series CE-11
November, 1975

Methods for allocating resources in applied agricultural research in Latin America

Editors: Per Pinstrup-Andersen and Francis C. Byrnes

CIAT/ADC Workshop, Cali, Colombia. November 26-29, 1974

CENTRO INTERNACIONAL DE AGRICULTURA TROPICAL (CIAT)
Apartado Aéreo 6713
Cali, Colombia S. A.

CONTENTS

ACKNOWLEDGMENTS

Acknowledgment for their contribution to workshop planning and execution is due to a number of persons from CIAT including Eduardo Alvarez-Luna, David Evans, Charles Francis, David Franklin, Barry Nestel, Grant Scobie and Alberto Valdés. Acknowledgment is due to the Research and Training Network of the Agricultural Development Council (ADC/RTN) and CIAT for co-sponsoring and financing the workshop. Special thanks are due to Dr. Abraham Weisblat and Dr. Vernon Ruttan for their continuous support of the workshop. The principal contribution to the success of the workshop was provided by the workshop participants through their preparation of papers and enthusiastic participation in workshop discussions. In particular, thanks are due to the discussion openers Lucio Reca, Hernán Chaverra and Helio Tollini and the moderators David Franklin and Barry Nestel for their effective discussion guidance.

INTRODUCTORY COMMENTS

The critical role of agricultural research in expanding food production and accelerating agricultural and economic development is now widely recognized. In view of the vast range of goals to which developing countries aspire and the magnitude of researchable problems in agriculture, how does the agricultural research manager establish priorities and allocate limited human, financial and physical resources among research programs and projects to assure the greatest benefits from research investments?

This real and persistent challenge to concerned individuals in both the developing and developed world led the Centro Internacional de Agricultura Tropical (CIAT) and the Research and Training Network of the Agricultural Development Council (RTN/ADC) in November, 1974 to co-sponsor a workshop aimed at the consideration of this issue.

Participating in the workshop were some 35 agricultural research managers, agricultural scientists, donor agency representatives, national planners, systems engineers and economists.

The workshop explored current decision-making processes for resource allocations in applied agricultural research in Latin America. Then, the workshop participants were asked to assess the needs for activities aimed at assisting in establishing priorities and allocating resources within agricultural research programs in Latin America. Finally, the workshop considered activities expected to be most effective and the possible role of national and international entities in carrying them out.

The purpose of this publication is to make available a summary of the workshop papers and discussions, to highlight workshop findings and conclusions, and to suggest possible follow-up activities. While this publication provides short summaries of the principal papers presented, texts of the papers in their original language may be obtained free as long as available from CIAT, Economics Unit, Apartado Aéreo 6713, Cali, Colombia, South America.

HIGHLIGHTS OF WORKSHOP CONCLUSIONS AND SUGGESTED INTERNATIONAL FOLLOW-UP

P. Pinstrup-Andersen
F. C. Byrnes

Conclusions from workshop discussions

A summary of the workshop discussions is presented in the following section; hence this discussion is limited to a brief presentation of some of the conclusions that were reached.

Workshop presentations on methods currently used to establish priorities and allocate research resources in four national and international research organizations clearly demonstrated a desire for more information on the relative expected pay-off from alternative research strategies. The scarcity of information on the relative importance of existing researchable problems at the farm level and the technology characteristics preferred by the farmer was obvious. Furthermore, information seemed almost completely lacking on the expected contribution of alternative lines of research to accomplish socioeconomic goals.

Workshop discussions indicated that in some cases, national research agencies had certain potentially useful data at their disposal, which could not be used because no effective framework existed for analysis. In other cases, attempts were made to develop such frameworks without the most essential data being available.

Participants clearly expressed not only the **need** for more and better information, but also the **desirability** and **expected high pay-off** of activities providing such information. They cautioned, however, that great care must be taken in selecting the activities, their content, and the method of putting them into effect in order to assure the development of **useful** information with **direct application** to the allocation of resources for agricultural research.

Discussions indicated that a few national research institutions are attempting to develop methods for improved research resource allocation. These attempts seem to suffer from either excessively bureaucratic procedures for project

selection or analytical framework too general to provide useful information. In both cases, it appeared that relevant basic data are extremely scarce. How to avoid these problems in order to assure that effective agricultural research is facilitated rather than hampered remains a vivid challenge.

The workshop considered a number of other methodological frames of reference for assisting the research manager in establishing priorities. Some are still either too preliminary to evaluate or too general for direct utility; others seem to offer great promise on specific issues, depending upon the availability of data.

Participants agreed that additional work is needed to adapt available methods to the specific needs of research managers and to develop certain methodological components that are still missing. Efforts are needed to integrate relevant socioeconomic and agrobiological issues into a viable methodology; and interaction among research managers, agricultural scientists and economists is essential. Such work would include the integration of the "macro" and "micro" approaches so that priorities among (1) commodities and (2) disciplinary inputs within commodities could be established simultaneously.

The work is expected to facilitate effective decision-making. Participants warned that no attempt should be made to develop a comprehensive model to replace the decision-maker but rather to develop one designer to improve his effectiveness through more and better information on the cost-benefit ratios of alternative research activities.

In addition to the foregoing, a number of other issues were introduced. A discussion group on research responsibilities between national and international institutions concluded that this issue should receive additional attention to ensure that international center objectives with respect to specific commodities correspond to national goals. On the question as to whether international centers compete with national programs for research funds, comments indicated that a relatively small amount of the "international community" funds currently allocated to the international centers would have gone to national institutions in the absence of the former. Discussants agreed, however, that more analysis is needed to estimate the optimum distribution of external funds between the two types of institutions, taking into account their interdependence. Such analysis would need to consider the influence that research investments made by international centers for specific commodities have on the amount of national funds a country decides to invest in research in these same commodities.

The question as to who actually sets research priorities in national institutions arose frequently; it appears that external donor agencies and international centers play a significant role through earmarked and/or

commodity-specific funds and technical assistance. This situation places great responsibility on these external agencies to ensure that their priorities do in fact correspond to national needs and that external and related national resources are invested correctly.

Where external funds are not obtained, it appears that few decisions are actually made on reallocation of research resources. Budget flexibility tends to be low and the total budget for one year and its allocation tend to be determined by multiplying the previous year's budget by some constant, with little consideration of changing technical and socioeconomic factors. One specific conclusion was that many national institutions do not appear to have effective mechanisms for deciding when to stop a certain program or project.

The lack of adequate delivery systems, supporting institutions and public policy are generally considered the principal limitations to the adoption of new technology. But discussions on means of accelerating the rate of adoption of new technology suggested the possibility that the inadequacy of technology to solve farm-level problems in a way acceptable to the farmer might well be the most important limitation to adoption. This limitation might be reduced or eliminated through efforts to provide the research manager with more and better information on actual farm-level problems and technology preferences.

The workshop suggested that agricultural and social scientists work together to help assure that (1) research is relevant to the farm-level problems and farmer preferences and (2) adequate technology is adopted rapidly and extensively. It was also recommended that measures be taken to ensure that research and public policies reinforce one another in an effort to maximize the contribution to the achievement of development goals.

International follow-up activities

While efforts to improve research resource allocation in national institutions are national responsibilities to be resolved within national contexts, the workshop clearly pointed out the need and desirability for certain international activities which might include

1. Facilitating effective interaction among individuals and institutions (within and outside Latin America) currently working on research resource allocation methodology or capable of and interested in doing so in order to (a) integrate the work, (b) reduce duplication, (c) promote additional work and (d) enhance the effectiveness of the total effort

2. Carrying out certain parts of the research aimed at developing methodological frames of reference

3. Assuring that the methodology is applicable and acceptable to the research manager by facilitating effective interaction among (a) institutions and individuals currently working on methodology or capable and interested in doing so (e.g., university economics and agronomy departments) and (b) agricultural research institutions and research managers

4. Preparing and testing pre- or postgraduate training material aimed at improving communication between agricultural scientists and economists, particularly with respect to research resource allocation

5. Training individuals from national research institutions in research management, effective data collection and analysis useful for establishing research priorities and allocating research resources

6. Providing assistance to national institutions in carrying out data collection and analysis

The role of international agencies

Because the success of international research and research support depend to a great extent on effective national research programs and since "strengthening national institutions" is an objective common to many international agencies, the above activities clearly fall within the mandate of such agencies and could greatly enhance the contribution of the overall international research and training efforts. Furthermore, the aforementioned activities and the resulting information would be useful to each individual international agency in allocating its own resources.

Frequently, international support to national research is commodity-oriented. Funding agencies, national government and international research centers arrive at decisions to carry out (through contractual projects) research, training, and other development activities associated with one or more commodities. Workshop participants stressed the need to consider carefully the possible problems that funding agencies and centers may create for national governments if appropriate attention is not first directed to how the national authorities determine their priorities and make their decisions to support a specific effort in the long run.

It is possible for external agencies to usurp the priority-setting function. This can happen when external agencies mount major international projects and draw scarce research talent away from problems of more current importance in these particular countries than those externally selected.

The national research director is faced with the problem of interacting with external funding agencies and enthusiastic commodity teams from various

international research centers, while dealing with the pressures generated by within-country production, consumption or marketing groups. He must pick and choose before finally designing a national agricultural research program. Up to now, external agencies may have contributed to the decision-making problems rather than helping to solve them.

Many of the issues most important **initially** for interaction between countries and international agencies **are not commodity specific** although they are usually commodity related and may involve commodities of no concern to the international research centers. This suggests that international agencies might seek ways to help national agencies establish new or revised machinery for research resource allocation, evaluate present criteria and develop new ones, collect and analyze data, and provide experience and guidance in these new approaches. The immediate goals would be the development and strengthening of research managers' decision-making processes.

Country representatives reminded the workshop that it is important that these actions be taken on an individual-country basis because the effective solutions of many development problems depend upon their congruence with the economic, social, political and cultural environment in which they are introduced.

SUMMARY OF WORKSHOP DISCUSSIONS

B. L. Nestel
D. L. Franklin

The summary of our discussions over the last three and a half days is presented within the framework of the workshop objectives as specified in the program document.

The first of these four objectives states that we met to explore how decisions are presently made on the allocation of resources in applied agricultural research in Latin America. Emphasis was to be placed on **who** made the decisions, **what criteria** were used, whether **pressure groups** influenced research demands, and finally what **quantity and quality** of information were available and/or utilized for decision-making. Secondly, we were asked to assess the efficiency of the present decision-making framework and the availability of relevant information to maximize the contribution of agricultural research to the achievement of development goals. The third objective of the workshop was to consider whether there was a need for improved decision-making tools and/or more and better information. The final goal of the workshop was to suggest ways to assist the decision-makers in applied agricultural research in improving research resource allocation. influenced research demands, and finally what **quantity and quality** of providing more and better information and/or analytical tools for the decision-maker. Finally we were asked to discuss potential benefits from collaborative interdisciplinary research, training and other possible action, and the role of CIAT in such action.

In his welcome address Nickel suggested that in order to have a more productive research approach, there was a need for a more effective use of resources in terms of orientation, organization and efficiency.

In terms of **orientation,** the conventional dichotomy between basic and applied research was unfortunate and could be regarded as a constraint. Nickel suggested that in place of the term "basic," either "opportunity" or "interest-oriented" research would be preferable; and "mission" or "problem-solving" would be preferable to the term "applied." In the latter

case, there was a need for more emphasis on action by interdisciplinary teams of biologists and social scientists rather than on descriptive research alone.

In the **organizational** field, Nickel decried the traditional division between research and extension with the low prestige traditionally afforded to the latter activity. He stressed the need for a stronger two-way flow between research and extension and the need for a dynamic approach to research with recurrent program reviews and mutual reports of outcomes.

In dealing with **efficiency,** Nickel stressed the static nature of many official and university research institutions that were often well endowed with both financial and human resources which were underutilized. Frequently, the best trained human resources in a developing country were found in its universities, but there was often no mechanism for channeling the research carried out by these people into national development programs. Indeed, in many cases university research did not relate to national problems, even in countries where universities constituted practically the sole center of agricultural research activities.

Fishel pointed out that the decision-making process involves three distinct levels: the national planning or policymaking level, the sectorial or "sectoral" level,* and the activity or operational level. In discussing the decision-making process, it seems that we have focused fairly heavily on decision-making at the "sectoral" level; that is to say, we have not talked very much about "policy" decision-making in terms of the intersectorial choices that confront top policymakers at the Cabinet or National Planning Office level. With the exception of the contribution of Andersen et al. and Brady we have not talked very much about decision-making at the level of the actual research leader or director—be he national or international—although this observation is perhaps less valid in the case of the private sector where Grobman's presentation did deal with the decision-making process at the operational level.

It is not surprising that we did not discuss decision-making at the highest levels since we are a group of sectorially oriented people. However, a number of speakers appeared to have been surprised and perhaps disappointed that there was some reluctance to enter into detailed discussions of decision-making at the operational level involving people who would actually have to implement the decisions. We shall return to this later, but essentially it seems that we may not yet have sufficient background to discuss this theme adequately.

Many of our discussions on the decision-making process appeared to be focused on Fishel's "sectoral" level, allocating priorities between commodities.

---

* The word "sectoral" was used by the author in preference to "sectorial" or "secular" because he felt it was more in accordance with economic terminology (Editor's note).

It did seem from the discussions that this was the level at which commodity priorities generally tended to be allocated, and a number of speakers recognized that at this level the decision-makers had a duty to develop priorities within the broad framework laid down by the National Planning Office.

Although most countries have planning agencies and prepare sectorial plans, it was pointed out that all too frequently there was little or no correlation between agricultural research activities and national development plans, and it appears that a great deal of past agricultural research has had a very limited socioeconomic impact. Guerra was particularly critical of university research and cited a study of postgraduate theses which indicated that a very high percentage bore no relationship to the realities of agricultural problems in the region.

It was pointed out forcibly by Brady that the scientific administrator had a role to play in attempting to feed information not only into the sectoral level but also into the highest political level, regarding both the developmental potential of research programs and the feasibility of their goals. In more basic language we might express this by saying that it seemed that the scientists in many agricultural research institutions play, or feel that they play, only a limited role in the orientation of that institution's research program although a strong suggestion has been made by both Brady and Steppler that scientists should adopt a more active role in this process. As far as decision-making at the operational level was concerned, we did not really discuss this fully.

The discussions on the ICA and INIAP presentations indicated that budget allocations for the previous year were a very strong determinant in the budget allocation for the next year. This may be a symptom that not enough consideration is being given to a review of research alternatives.

This leads to a discussion of the second point of the first main objective of this meeting; namely, the criteria used in decision-making. Here again we looked at two levels, the "sectoral" and the "operational," the discussion focusing mainly on the former. There was a general consensus that agricultural research could not be considered separately from national goals and that it was important that national leaders and decision-makers should provide the basis for determining the direction of research programs and for identifying priorities within these. Brady and Andersen et al. both stressed the importance of identifying constraints through the use of an interdisciplinary team including both biological and social scientists.

Brady highlighted four issues relating to the removal of constraints: (1) the relative significance of different constraints in order to identify those whose removal could be most meaningful, (2) the feasibility of constraint

removal (or the chances of success), (3) the cost of constraint removal and its comparative advantage in relation to other strategies, and (4) the probability that others might do the research and the need to avoid duplication or operating in a vacuum. This last point was taken up by a number of speakers, and full discussion was given to the relative roles of national and international institutes and the private sector.

The importance of the profit motive in private sector objectives was brought out by Grobman, and there seemed to be a consensus of opinion that international institutes were better equipped than national ones to undertake longer term research with a higher element of risk than many national institutes were prepared to take. It was also suggested that in high risk or speculative research the decision to go ahead was often based on the human resources available as much as on the potential value of the project or its chances of success.

Schuh and others indicated that there might be some element of competition for funding between national and international institutes because of the finite amount of resources available. It seemed to be generally felt that these activities could and should be complementary and that the whole justification for the existence of international institutes was to provide a strong backstopping service to national institutes. There seemed to be a consensus that through outreach programs there should be a strong feedback from national to international institutes which would ensure that the latter were responsive to national program needs.

It was also indicated that in discussing resource allocation between different types of institutes, the private sector could play an important role, particularly in those countries where the markets were large enough and sufficiently well developed for it to be able to market its products effectively. In Brazil the new national research institute EMBRAPA is structured as a private company and plans to operate very much along the lines of an international institute. In addition, it will subcontract work to universities and private institutes. In this way, it is endeavoring to combine the comparative advantages of all three types of institutes into one organization.

Both Valderrama and Dow presented preliminary work on research resource allocation at the operational level in their institutes and endeavored to include equity as well as productivity goals in their development of decision-making indexes. The state of the art in this field is obviously at a very early phase; and even in the United States, which probably has the world's largest public agricultural research service, the development of effective criteria for defining national research goals is still at a very early stage. Fishel presented the approach being adopted in the United States. This approach is a fairly simple one and involves a great deal of subjective judgment. Páez described a more complex model being developed in collaboration with his colleagues for the

allocation of resources. The model is based on the premise that research funds are first assigned to a preselected group of commodities and then assigned to problem areas within each commodity. This model is still in the testing phase.

Earlier reference has been made to the specificity of the objectives of the private sector's decision-making process. In elaborating on this, Grobman placed particular stress on the importance of market research and consumer acceptability in influencing the decision-making process. He pointed out that at various stages of the research and development activity these marketing influences could lead to the cancellation of a project. At many points in the discussion on public sector research, the floor comments focused on the problems of low adoption rate and the weakness of the "extension" process. To some degree it was possible to draw a close analogy between extension and adoption on the one hand and market research and consumer acceptability on the other; and it would seem that there may be an evident weakness in a great deal of public sector research which separates the carrying out of the research from its actual delivery to the farmer. Indeed this may highlight a fundamental weakness in the criteria for defining research priorities in many institutions where all the emphasis is given to carrying out research on objectives defined by people at a high level who may be oblivious of the consumer's needs, particularly when that consumer is a small farmer. This subject was covered in the specifically producer-oriented model by Andersen et al., which had a strong feedback to acquaint the researchers with farmers' preferences.

It would seem that there is a lacuna in the thinking of many public sector research institutions in that their selection criteria at the project level do not take adequate cognisance of what the consumer wants. This does not mean that the research consumer (i. e., the farmer) does not want a crop variety that will triple his yield; but if this involves a four- or fivefold rise in the cash cost of production, he may reject it in favor of a variety which gives him only a 50 percent rise in production but involves a cash outlay only 20 percent more than his traditional costs because cash is often his most limiting resource.

This observation appears to be particularly relevant in the light of Ruttan's comment on changing fertilizer/crop price relationships, since the fertilizer application has played a very significant role in the transfer of agricultural technology in developing countries during the last decade.

Let us move on now to some comments on the third factor in this list of initial objectives; namely, the influence of pressure groups in influencing the decision-making process.

We really did not have much in the way of hard data to discuss on this theme. It was suggested that donors may represent a pressure group; on the

other hand, nobody discussed donor motivation and whether or not donors were more efficient than research institutes in defining how resources might be allocated. An interesting observation was made that the disciplinary training of national planners and agricultural planners in particular might introduce a strong bias into the decision-making process. There may be some validity in this remark even at the experimental station level since in most developing countries the first agriculturalists to be sent overseas for doctoral studies are usually cereal breeders and it is much more common to find people with this background directing both national and international centers than it is to find, for example, an agricultural economist. A number of speakers stressed the need for a better dialogue between biological and social scientists in order to achieve more effective resource allocation.

In his presentation on CIAT, Alvarez-Luna looked at the question of pressure groups from a somewhat different angle. In the two programs that he specifically described—namely, beans and cassava—CIAT has endeavored to deliberately develop pressure groups at the operational level, with different disciplinary and geographical orientations, which feed in information and advice to its program committee and director general who are then able to utilize a wide range of different expertise to make their operational decisions.

In many institutions the most important pressure influencing the decision-making process may be the effect of an existing, frequently long-standing budgetary structure and the difficulty in making major short-term changes in the use of financial resources. This problem seemed to exist in all the organizations discussed although in the long run there did appear to be a great deal of flexibility in both national and international centers' budgets. The problem of pruning or even amputating long-standing programs that appeared to serve little objective purpose was one that seemed to confront most agencies.

Finally, at this stage of the program, we came to the question of the quality and quantity of information which is available but which is often not utilized for decision-making. Here again, Ruttan pinpointed the issue when he related to the specification of the information base on which to identify the needs for shifts in research programs. In particular he raised the question of how the techniques used for conceptualization of the measurement of past benefits from research might be utilized for the analysis of benefits from future research. This latter issue was taken up by Hertford in his paper describing the utility of **ex post** methodology for **ex ante** analysis.

These presentations, together with the data presented by the national and international institutes represented at the meeting, did indicate that an absence of adequate analytical data was a major constraint on the decision-making process. From the presentations given, it appeared that we are still quite a long way from knowing what data we need, let alone knowing how to handle it;

and certainly from the standpoint of **ex ante** analysis, there is little hard information to go on at the present time although Schuh, Castro and Andersen et al. have developed or are attempting to develop methodologies to overcome this situation.

In trying to interpret the discussion on our first objective, we have really covered many of the points also discussed in the second and third objectives, which dealt with assessing the efficiency of the decision-making framework presently used and considering whether there was a need for improved decision-making tools. There are, however, a few supplementary points which might be added.

The first related to the economy of scale in resource allocation and decision-making, not only in relation to the location specificity of biological research but also to the type of institutions that should be carrying out that research. This comes back to the point about the relative contribution of the public and private sector, and there was an interesting discussion on the degree to which the public sector should support research on commodities, particularly export-oriented ones, which tended to be produced on large commercial farms. The history of the private sector's success in fields such as cereal and sugar cane breeding would seem to indicate that public policymakers might need to take a closer look at this problem in some countries.

Another issue, initially highlighted by Ruttan, related to the limitations in our knowledge as to how to bring resources to bear on institutional innovation and transfer. This related back again to the question of the transfer of technology, and it was of particular interest to hear Soto's presentation regarding the Interamerican Development Bank's new policy of attempting to identify their role in strengthening national agricultural research institutions from the institutional standpoint.

Returning to the question of the efficiency of the decision-making process, it was clear from discussion that research resources must be allocated according to the needs of a country and within the framework of the current state of development of that country; if there is no clearly defined development program, the decision-making process with regard to resource allocation cannot be properly carried out. It was suggested that the decision-making process at the farmer level might need to take into account the differences between traditional, transitional and commercial farmers and that more attention might need to be paid to research policies specifically oriented to do this.

With regard to the actual decision-making process, as mentioned earlier, various papers touched on the socioeconomic criteria used to identify commodity priorities, but the workshop participants appeared to be very reluctant to be drawn into discussion on the crucial issue of how decision-making took place in terms of the commodity budgets and the identification

of priorities within a commodity budget. The impression was given that the adequacy of the decision-making process for allocating research resources at the operational level in most of the institutions represented at the meeting was limited and offered considerable opportunities for improvement.

In view of this, the workshop passed on fairly logically to its fourth objective, which was to suggest how to assist the decision-makers in applied agricultural research in terms of improving research resource allocation. We were asked to explore the potential benefits and costs of providing (1) more and better information and (2) analytical tools for the decision-maker. The potential benefits from collaborative interdisciplinary research, training and other possible action and the possible role of CIAT in such collaborative action were also listed for discussion.

Again there is a certain amount of overlapping with the earlier objectives. The first point dealt with the provision of more and better information which we clearly need. This point was brought out forcibly by Soto, who indicated that the Interamerican Development Bank's lending activities were hampered through an inadequate information base although this situation had been worse in the past.

A series of models were presented to us for improving resource allocation in research. Tollini classified these into quantitative and nonquantitative, but there was in effect a sequential range of activities from complex policy decision-making models such as those described by Ramalho de Castro and Schuh and by Páez (through the Hertford and Fishel models) and the approaches of Dow and Valderrama, to the less complex type of analytical tools presented by Andersen et al. and Schuh, to the totally unquantitative approach of Brady.

We had an interesting schematic presentation from Guerra regarding the type of information one needed to be able to make better judgments. This subject was also referred to in the papers by Valderrama and Ardila, Dow and Ampuero, Páez, Hertford, Ramalho de Castro and Fishel. However, we did not really get into a discussion on the potential benefits and costs of providing a better information base, and there are some doubts as to whether we are equipped to do this.

The paper by Ramalho de Castro and Schuh was particularly interesting in the way that it highlighted the relationship between different goals of agricultural research and commodity and equity policy. Although it was suggested that the Ramalho/Schuh model was of particular relevance to Brazil, this point was disputed; and the approach would seem to warrant further exploration. It would also be interesting to relate some of the analysis in this model to the data from the **ex post- ex ante** comparison presented by Hertford

since it appears that at the present time we know how to be wise after the event but not before.

The final paper by Andersen et al. proposed a model for improving the information base for research resource allocation based on agro-economic surveys. Again we had an extremely interesting paper describing work in progress but as yet untested. Certainly the paper seemed a fitting conclusion to the earlier series and indicated some concrete proposals for answering the questions posed at this workshop. It also indicated that for this type of work, this approach could be used effectively by an international agricultural center in acting as the focal point for coordinating this type of activity on a regional basis, particularly through the physical and financial resources that an institution such as CIAT possessed, which gave it a considerable comparative advantage from the standpoint of the training of personnel.

In the final discussion session, the participants concluded that the workshop had presented a useful exchange of experiences. The dialogue suggested that this was a field that warranted more careful study and that periodic exchanges of this nature would be useful. It was recommended that future workshops should have narrower objectives and should focus on the decision-making process at a very specific level. CIAT's training and conference resources lent themselves well to hosting this type of meeting, and it was recommended that CIAT should take the initiative in follow-up activities.

STRENGTHENING NATIONAL AGRICULTURAL RESEARCH SYSTEMS: SOME CONCERNS OF THE INTERNATIONAL COMMUNITY

A. C. McClung

Present requirements for increased food production are such that the agricultural research services of many of the developing nations must step up performance. There are many reasons to believe that these agencies can improve their effectiveness and rather rapidly. The environment for improvement is better than in the past. Some of their needs are more clearly evident than they have been previously, and steps can be identified whereby others may help them.

These statements outline the conclusions reached at the Bellagio VI conference. At Bellagio V (May, 1972) attention was given to the need to protect the unique character of the international centers, their flexibility, and their freedom from political constraints. Linkages between the centers and national groups and gaps in the worldwide network were also subjects of concern.

When Bellagio VI was being planned, a summary of the developing network showed that the CG* system was becoming increasingly effective in dealing with research gaps and in marshalling support for agricultural research and development at the international level. The greatest need seemed to lie in the area of strengthening national agencies.

The common thread running through the Bellagio VI discussions was the need for vastly improved technologies. Three aspects of the current situation received attention: (a) world food supply, (b) interactions between international and national agricultural research organizations, and (c) the status of national agencies. The consensus at Bellagio VI was that the current food problem will cause new resources to be brought into use and that this time some more lasting gains can be expected in terms of continued support for the development and application of new technology.

Attitudes and reactions at the recent World Food Conference support the view that national leaders recognize increasingly that food production must

* Consultative Group for International Agricultural Research

receive top priority. It was obvious that the delegations recognized that only by increasing local production could the developing nations find a solution to their food needs.

While the international centers are a source of great satisfaction to their founders and sponsors and a basis of hope and reassurance for many others, several concerns have been expressed, chiefly concerning the interactions between the centers and the national agencies. There is some feeling that the centers' resources are being spread too thin. They are being asked to take on too many responsibilities or projects where their particular organizational advantages do not come into play.

The sharp focus of a multidisciplinary team on a clearly defined range of problems is recognized as one of the reasons for the centers' success. Institution-building projects, whether they are carried out as outreach or cooperative efforts, must be selected with care so that they will not lead the center away from this sharp focus.

Another problem may be the overlapping of several centers' outreach programs. For example, some of the small technical groups in Southeast Asian countries have received enthusiastic overtures from international center representatives interested in deep-water rice, corn, cassava, potatoes and vegetables. The proposals have been accepted with appreciation but also with some perplexity. These small countries, often with complex agricultural patterns, are precisely the ones that must rely most heavily on the centers for much of their new technology; but they may need help in putting together a mix of technical assistance which meets their needs and is within the reach of their resources. Also, they may urgently need help with cotton or jute or some other crop that has no advocate from an international center.

It seems clear that the centers will be funded generously so long as they produce results. The national research agencies are not merely going to be funded more adequately; they are going to be **required** by their countries to make unprecedented contributions in the years ahead.

Trained scientific manpower is still in short supply in most countries. Even those that have unemployed scientists are not overstaffed so much as they are underfinanced. As the organizations expand, the shallowness of the manpower supply becomes evident.

Crop and animal production specialists, who can do the integrated type of research needed to increase production, are in short supply in essentially all the developing countries. This is a type of training that most graduate programs do not emphasize and that many organizational charts fail even to identify.

Intermediate and top-level management personnel, who can analyze problems, plan and implement programs, are badly needed in all developing countries.

Funding of national programs is obviously well below the optimum. The real question is not how much should be spent by the developing countries under optimal conditions but rather what rate of increased expenditures can be efficiently utilized, given the manpower restrictions and other constraints of a specific situation.

Each country should examine its agricultural research services and make the indicated adjustments. We might add that the surplus producers among the developed nations are not immune to this need.

The developing country that wishes to upgrade its research system must be prepared at the top level to make a long-term, sustained commitment to the job. In making some of these determinations, national agencies may wish to seek assistance from outside. Technical and financial assistance from multinational and bilateral sources should be utilized when required.

Careful planning will help to make effective use of current budgets and of new funds which will likely become available. Such planning might include the following steps, among others:

a) The review of existing facilities and staff resources and an evaluation of their adequacy to meet national goals

b) The planning of national research systems of manageable proportions, designed to serve the different farming regions of the country

c) The development of a long-term schedule for facility and manpower development, with meaningful commitments of financial support

The individual nations must consider resource allocation for every crop or commodity that the nation produces. They must deal with the whole range of social, economic and political problems facing agriculture in their particular situation.

Food production, and not research for its own sake, must be the aim of the national agricultural agencies of the developing countries. Too often the researchers have tended to be out of touch with the farmer and even with researchers in related fields.

It is increasingly clear that the research structure must deal with more than the various disciplines and individual commodities. It must deal with integrated farming systems. The developing countries are faced not merely with achieving

production increases on a nationwide basis, but with achieving increased incomes for large numbers of small farmers.

Farmers cannot use new technology without the necessary inputs and credit. They must also have reasonable market facilities and prices that offer a reasonable return on their investment in new technology. All of these factors require decisions and actions by a number of agencies, both public and private.

THE CONTRIBUTIONS OF AGRICULTURAL RESEARCH TO THE ACHIEVEMENT OF DEVELOPMENT GOALS *

G. E. Schuh

In addition to a brief mention of the impact of agricultural research and public policy in the United States, the paper focuses on four issues: (1) the importance of dealing as explicitly and as operationally as possible with goals. We have to ask and answer the question of "research for what," (2) possible goals that might be considered, (3) the role social scientists and particularly economists can play in identifying these goals, (4) the need for economic policy and technological change to complement each other.

New technology is created as an input in the development process and not as an end in itself. Moreover, technology has an instrumental role in attaining a larger set of goals and objectives; its main goal is not to entertain the researchers.

Once the idea that knowledge has this instrumental role in goal attainment is recognized, the specification of these goals becomes important. In the case of agricultural research, this would require a specification of the goals society or the government would have with respect to the agricultural sector.

In order to obtain any degree of precision in research priorities, it is important for broad social goals to be translated into a more operational and objective set of goals in terms of which individual research projects can be evaluated. If this can be done, more focus will be provided for the research program, a more efficient research effort will result, and there will be a more objective means of evaluating the research in an **ex post** sense. The problem is to arrive at this more operational set of objectives.

One way to proceed is to take the three fundamental goals specified for the Iowa State program and see what can be made of them. In addition, I would lik to add a fourth goal—nutrition. Therefore, we have as possible goals (1) growth or development, (2) equity, (3) security and (4) nutrition.

* The views expressed herein represent those of the author and not those of the Council of Economic Advisors.

On the basis of a discussion of research goals, three propositions are identified. First, the nature of the objectives for the research program will be determined in part by the stage of economic development. Second, the objectives of the research program should be related to the particular development model the government is implementing and the specific economic policies it uses to implement this model. Third, with an adequate understanding of the development process and the set of policies being pursued by the government, the goals and objectives of the research program can be specified at a quite operational level.

Each of these three propositions provides an important analytical role for the economist in determining research priorities. His contribution is partly to identify goals and objectives in the light of the general policy matrix. If this is done at an operational level, there should be an increase in the efficiency with which research resources are used since they will be focused more directly on policy objectives. In addition, a sounder basis will be laid for evaluating the research program in an **ex post** context.

The equity issue is one that has long been neglected by both economists and production scientists. Economists have neglected it because of the difficulty in stating categorically whether one distribution of income is better than another. Production scientists have neglected it because of a failure to recognize that (a) much of their production technology was not uniformly adopted by different sized farms, (b) the benefits of production technology could accrue uniquely to one or another category of resource owners, and (c) that the ultimate beneficiary of technical change could be the consumer rather than the farmer.

Four aspects of the equity or income distribution issue are important. The first is the distribution of the benefits of technical change between the producer and the consumer. The second is the functional distribution of the benefits among the various resource owners. Third is the distribution of the benefits among the various sizes of farms. Fourth is the impact on regional income distribution within the country.

On the distribution of benefits between the consumer and the producer, two sets of considerations are important: (1) the relative conditions of supply and demand and (2) economic policy. If agricultural researchers want to benefit producers, the presumption is that they should concentrate on products that have a high price elasticity of demand. Examples of these are export products. If they want to benefit consumers, they should concentrate on products that have a low price elasticity of demand. Typically these will be food staples or necessities like rice, edible beans, wheat, etc.

On the distribution of benefits between the land owner and the laborers, it is assumed that in most cases it is the land owners who will benefit at the

expense of the laborers. This is not a straightforward case, however, and much depends on the relative elasticity of the supply and demand for the factors and the elasticity of substitutions among the production factors.

Regarding the distribution of benefits among different sizes of farms, the issues have to do with the extent to which the new production technology is adapted to the resource endowments and other conditions of the various size groups and the efficiency of the various economic institutions serving these groups.

The paper concludes by stressing three points. First, development needs are not the same for each country or even for different regions within the same country. Moreover, these needs will generally change over a period of time; therefore, the problem of analysis to determine what research priorities ought to be is almost never ending. For the same reasons it is difficult to generalize among countries. The analysis does have to be largely location specific. Because of this, there is an important need for strengthening national research capabilities.

Second, research priorities need to be defined in terms of the particular development model that a country is using as a basis for policy and in terms of the particular measures used to implement it. To fail to do this is to run the risk of having economic policy negate the results of the research effort and/or to forego a potential contribution that the research effort could have made.

Goals may well be in conflict; for example, the attempt to attain a higher rate of growth in the aggregate may well aggravate the equity problem. It is worth noting that had plant scientists and social scientists worked closely at the beginning to think about what the goals ought to have been, what weights to attach to them, and how the goals might have been attained, the counterproductive controversy over the Green Revolution might never have happened. Biological and social scientists do have a responsibility to attempt to understand each other and to work towards the common goal of improving the well-being of the large fraction of the world's population that is disadvantaged.

CRITERIA FOR ESTABLISHING RESEARCH PRIORITIES AND SELECTING RESEARCH PROJECTS

N. C. Brady

During the past decade we have witnessed the development of a two-pronged approach to agricultural research aimed at solving food production problems in the developing world. One approach is that of the network of international agricultural research centers; the second is that of the national research organizations in developing countries.

The total need for new knowledge to help farmers produce more food dwarfs the resources available to support and carry out the research needed. Priority setting becomes paramount. There is much to be desired in agricultural research priority setting, especially that of national research organizations. The imitation of researchers in the more developed countries is evident.

Research at universities and research institutes complements applied research trials carried out at small outlying stations. Characteristically, these stations are poorly staffed and equipped. Some regional research stations may have small plant breeding programs, but their efforts are not usually coordinated in a national crop improvement program.

There are some notable exceptions. In these cases, there is good national research coordination. Effective long-range plans have been developed and are being implemented. Trained researchers are effectively utilized and are training others to take their places.

If overall national policies do not give higher priority to agricultural research, the setting of research priorities may be meaningless. Agricultural research cannot be considered apart from the basic human needs of society and, perhaps more important, from the perception of those needs by national leaders. National social goals as **perceived by national leaders and decision-makers** must provide a basis for determining the direction of research programs and of the specific priorities within these programs.

Research administrators have a responsibility to help decision-makers identify national goals relating to agriculture and, more specifically, those relating to agricultural research. One of the most significant challenges is to force national leaders to think in terms of the future. Preoccupation with current problems forces them to think only of those research inputs which promise immediate results. They therefore bypass or eliminate long-range research planning and concomitantly the setting of meaningful research priorities.

An important task of research administrators and other decision-makers is to identify clearly the extent to which agricultural research can contribute to the attainment of a nation's or region's social goals.

The desire for national self-sufficiency, especially with respect to food crops, sometimes leads to the establishment of unrealistic and economically unsound production goals. Economists, as well as biological scientists, can help identify agricultural areas in which a given country has a comparative advantage.

To identify means of meeting social goals, agricultural research administrators must know the limitations society imposes on the agricultural industry.

The generalized procedure for determining appropriate resource allocation for agricultural research assumes that overall social goals will include goals that can be met only through the agricultural sector. In turn, agriculture's goals will require inputs from agricultural research, as well as other components of this industry.

Using this general criteria, it is possible to select a series of research alternatives and to assign them different levels of priority. Within each alternative, specific research projects can then be prepared and pertinent research methodologies developed. These become the research instruments to which funds and human resources are allocated.

Four major criteria are important in setting research priorities and in ascertaining projects to be initiated.

Relative significance of different constraints. The extent to which the removal of a given constraint would contribute to the achievement of important agricultural and, in turn, social goals is perhaps the most significant long-range criterion. The relative socioeconomic significance of the constraint is of paramount importance.

More specific factors that must be considered are the size of populations and of crop and land areas and the number of institutions potentially

affected by the proposed research. Also, effects on income distribution, effective land utilization and other socially worthwhile goals should receive attention.

Feasibility of constraint removal. The feasibility of removing, through research, different constraints on agricultural production, processing and marketing is an important criterion. Determination of this feasibility will depend upon a number of factors including the nature of the constraint to be removed, the availability of scientists sufficiently well trained to carry out the research, and progress already made in related research areas. Limitations in finances and their rate of delivery can adversely affect a scientist's ability to perform, as can an inefficient procurement system.

Cost of research to remove the constraint. The required inputs in terms of financial and human resources and of time needed to accomplish the research are important criteria. The financial cost-benefit ratio has been used commonly. Unfortunately, benefits from agricultural research cannot always be quantified in economic terms.

Aside from the cost-benefit analyses, research costs alone are important criteria. Even with high probable ultimate returns on research investment, poor countries may not be able to afford large inputs for agricultural research.

The time requirement for research accomplishment is significant. Research administrators must insist that some funds and manpower be allocated to projects that have high, long-term potential even though the immediate return probabilities may be low.

Probability that others will do the research. Each research organization tends not to take into consideration the research capabilities of others. Developing country organizations must consider the research which is being done and which can be done elsewhere. Regional cooperation permits interchange of crop and animal strains, as well as of published research results.

There are a number of other practical criteria; for example, the urgency of the research. Administrators must prevent "urgent" problem-solving projects from dominating research programs.

It is not difficult to identify the general procedures by which criteria and, in turn, priorities can be determined. The difficulty arises in implementing the procedures.

The setting of broad social goals is generally the function of society and is usually accomplished by political leaders and national planners. Scientists and science administrators should provide background information for these

decision-makers, not only to determine the social but agricultural goals as well.

Agricultural scientists and research administrators should be intimately involved in setting agricultural research goals with prime responsibility for identifying agricultural constraints, the role of research in removing these constraints, and the specific criteria to be used in developing priorities. Likewise, they should have major responsibilities for developing priorities and in deciding resource allocation to implement the priority research programs.

In most instances, research administrators use their own judgment in setting criteria and research priorities; in others, panels advise the administrator. In still others, panels of experts decide on the criteria to be used and identify the priorities.

Despite the weaknesses of criteria and priority-setting procedures involving scientists, the advantages outweigh the disadvantages. While it may be inappropriate to give the scientists the sole responsibility for criteria and priority setting, their knowledge of the potentials of science for problem solving must be fully exploited.

THE DECISION-MAKING PROCESS APPLIED TO RESEARCH RESOURCE ALLOCATION IN A NATIONAL INSTITUTION: THE CASE OF ICA IN COLOMBIA

J. Ardila V.
M. Valderrama Ch.

The system whereby resources are allocated for agricultural research in Colombia is part of a national planning process, whose line of authority includes the National Council of Socioeconomic Policies (CONPES), the Planning Office of the Agricultural Sector (OPSA), and the Colombian Institute of Agriculture (ICA) itself. The information related to this system is included in the annual budget proposals and in the four-year investment plans of the aforementioned institutions.

Competition for research resources begins at the intersectorial and sectorial levels (health, public works, agriculture, etc.) and at the level of entities (INCORA,* IDEMA,* ICA, etc.). Nevertheless, at the level of entities, research resources must also compete with resources for other programs that ICA carries out (such as the adoption of technology and education) in addition to operating expenses.

In the area of research alone, resources compete among different projects (rice, beef cattle, etc.); and within these projects, the same occurs at the activity level (breeding, crop practices, etc.).

At the intersectorial level (agriculture, public works, etc.) and at the sectorial level (IDEMA, ICA, etc.), the criteria for allocations are primarily associated with (1) the relative yield of investments in the socioeconomic sectors, (2) the availability of resources, (3) allocations made in previous years, and (4) Government objectives and goals as expressed in development plans.

At the program level (research, adoption of technology, etc.) and at the project level (rice, cotton, etc.), the criteria are prescribed by sectorial

* The Colombian Institute of Agrarian Reform (INCORA) and the Institute for Agricultural Marketing (IDEMA).

policies and Government goals, primarily in relation to an increase in productive employment and income, the equitable distribution of the same, improvement in productivity and an increase in the production of agricultural commodities, improvement of commodity marketing, increase and diversification of exports, training of small farmers and the promotion of their organization, and the adequate development and conservation of natural resources. The aforementioned criteria are complemented by specific commodity-oriented studies and aspects related to allocations made in previous years and results obtained in research programs.

At the level of activities and regions, the criteria refer to (1) the degree of urgency of the research, (2) the time needed to carry out this research, (3) the cost of the same, (4) the possibilities and costs of its adoption, (5) the technical feasibility of carrying it out (personnel, equipment, methods, etc.), (6) the number of farmers, operations and areas benefited, (7) the implications of production results (possible benefits).

Assuming that there is a high correlation between research priorities and resource allocation, the present list of priorities at ICA is given here:

Top priority: tubers, maize and sorghum, grain legumes and annual oil-bearing crops, vegetables and fruits, cacao, perennial oil-bearing crops, beef and dairy cattle.

Secondary priority: cotton, rice, wheat, pasture grasses and forages, swine, sheep and poultry.

Low priority: oats, sugar cane, barley, plantains and bananas, and tobacco.

An attempt has been made to establish a pattern of systematic measurement that will show whether or not budget allocations are made in accordance with budget measures.

The criteria used for resource allocation are based upon priorities established by the national Government: (1) nutrition, (2) employment, (3) income distribution, (4) the balance of payments, (5) the comparative advantage of the crop, (6) the importance of the crop in the economy, (7) the demand for the commodity, and (8) the needs for doing research. These criteria are applied to each of the crops selected to determine the pattern (maize, wheat, beans, soybeans, potatoes, cotton, rice, cacao, sugar cane, barley and tobacco). The variable is quantified and then used to establish an Ordinal Index of Priorities (I_p). As these characteristics have a different relative importance, a table is constructed of the relative weights of these characteristics. The index is as follows:

$$1_{pi} = \sum_{j=1}^{n} P_i/V_i \cdot v_{ij}$$

P_i is the relative weight of characteristic i so $P = \sum_{1=i}^{n} P_i = 100$, and V_i is the total quantified value for each characteristic, and v_{ij} is the value of characteristic i of crop j.

Indices 1_{pi} are in descending order so as to compare them with the Ordinal Indices of Budget Allocations (1_a). Index (1_a) was also obtained by arranging in descending order the budget allocations for research during 1973 and the average for the years 1970 to 1974. The coefficient of the linear regression of the two indices is 0.974 for 1973 and 0.9688 for the period 1970-1974.

In accordance with the pattern of measurement constructed, the aforementioned index shows that the budget allocations for the crops studied were made in accordance with established priorities.

The authors would like to call attention to the vulnerability of the method at the level of relative weights. The present attempt should be taken simply as an illustration of what can be done but requires further analysis as far as the validity of the method employed is concerned.

THE DECISION-MAKING PROCESS APPLIED TO RESEARCH RESOURCE ALLOCATION IN A NATIONAL INSTITUTION: THE CASE OF INIAP IN ECUADOR

K. Dow
E. Ampuero

The National Institute for Agricultural Research (INIAP), formed in the year 1962, is responsible for all agricultural research. Before its creation, research work was divided among several public entities, each focusing its work on areas of its own interest; and research was a subordinate activity. In its initial stage, INIAP had three main objectives: (1) the formation of technical personnel, (2) the development of an infrastructure for research, and (3) the creation of an atmosphere of institutional stability.

In allocating the resources necessary for accomplishing its objectives, INIAP has maintained a flexible policy, evaluating each project according to the quality of its technical personnel, its results and those needs arising during its duration.

Yearly meetings for reviewing programs, as well as five-year meetings for evaluating objectives, serve as criteria for the institute's directors to estimate a program's needs and to adjust the assignment of human and physical resources. Agricultural leaders, managers of agricultural institutions and international advisors take part in these meetings.

Based on the budget allocations made by the central government, INIAP's technical committee—formed by station directors, subdirectors and the administrative director—meets at the beginning of the year to make the necessary adjustments in accordance with established priorities. At least once a year, the budget is modified, transferring resources from programs that have surplus funds to those that have none. The reforms made by the Technical Committee must be approved by the Administrative Counsel and the National Budget Office. At the station level, each director has the autonomy to distribute the resources in accordance with current needs.

There are diverse groups whose demand for research is reflected in the priorities allocated by INIAP. For instance, the National Planning Board asks

the institution to carry out certain programs the country needs within the context of the National Development Plan.

Through the chambers of agriculture, agricultural centers and national conventions, the farmers request INIAP to undertake research programs they consider to be of priority or to establish additional experiment stations.

In addition, the institutions of regional development—such as the Commission for Studying the Development of the Guayas River Basin (CEDEGE), the Center for Reconverting the Manabí (CRM), the Center for Reconverting the Austro (CREA)—and regional projects present INIAP with requests for research on the behavior of new varieties, crop practices and the development of production systems designed for their zones in particular.

INIAP continually receives a great deal of pressure from farmers and government institutions to install new experiment stations and create new research programs; nevertheless, INIAP has been careful not to multiply the number of its activities before finishing the development of its present stations and arriving at acceptable levels of productivity in existing programs. Once this has been accomplished, the institution can expand on the basis of socioeconomic criteria with development priorities.

INIAP is aware that the essentially subjective criteria used so far are not entirely adequate to control resource allocation in the long run, not only because allocations of previous years influence in one year's results, but also because many times that allocation obeys external factors, such as the availability of external funding and the scarcity of qualified personnel at different levels.

In order to give the decision-making process a certain degree of objectivity, an attempt was made to include a series of socioeconomic criteria.

The first effort to include these criteria was a paper that used the following model:

$$PTi = \sum_{j=1}^{n} \alpha j \; Wij \qquad i = 1,\ldots,m; \quad j = 1,\ldots,n$$

PTi = Total number of points corresponding to activity i
αj = Relative weight given to criterion j
Wij = Relative weight given to activity i within criterion j
n = number of criteria studied

The criteria used were the following: (1) the number of operations, (2) the implications in the balance of payments, (3) future growth of demand, (4) production costs, (5) labor, (6) social impact.

For the assignment of relative weights to the alternatives within each criterion, these were grouped into three categories: top, secondary and low priority. For applying weights to the different criteria, their relative objectivity, the possibility of quantification, and their importance within the national development plans were taken into account.

On the basis of INIAP's experience, it is suggested that the following points be discussed at this and other workshops:

1. Develop a refined model for comparing alternatives and determining priorities in research resource allocation

2. Make this general model adaptable for different cases according to the availability of information; that is, it should not be a rigid model.

3. Explore new criteria to be used in determining priorities

4. Explore alternate criteria, which although oriented towards fulfilling the same goals, may have better characteristics of objectivity or measurability

5. Develop criteria permitting better assignment of relative weights for the different alternatives within each criterion, as well as for each criterion

6. Discuss possible ways of quantifying (within the framework of the criteria) the importance of the different "support programs" that play such an important role in agricultural research. For obvious reasons, it is more difficult to include these programs as measurable alternatives to be compared with production programs. Their priority is, in many instances, conditioned to priorities of other programs, where they receive different emphasis. For this reason, priorities for support programs are determined after priorities for production programs have been defined.

7. Highlight the need for finding better ways of measuring the return to research in such a way that cost-benefit criteria can be used more frequently to evaluate different alternatives. It would be useful to develop the tools for determining the function of the benefits of research in different cases.

MECHANISMS FOR ALLOCATING RESOURCES IN APPLIED AGRICULTURAL RESEARCH AT EMBRAPA IN BRAZIL

A. S. Lopes Neto

The Brazilian Agricultural Research Company (EMBRAPA) is the Ministry of Agriculture's tool for promoting the coordination of all applied research in Brazil.

At present, this is a historic moment in which both institutional and operative transformations are radical and profound. These transformations are being carried out throughout the existing structure at the national level, at the same time as a new approach was being adopted in agricultural research. Nevertheless, EMBRAPA's managers are trying to minimize risks and disseminate, as far as possible, new ideas that are being introduced.

In addition to not hindering the continuity of ongoing research, it was considered essential that the company implant two points: the effectiveness of the operative system and the definition of a policy in regard to human resources.

The operative system is supported by two others: the institutional system and the planning system, which were already introduced at the national level and together form the new approach of applied agricultural research systems in Brazil. These two systems are explained in detail in the paper presented during the workshop.

In addition to this, the original paper presents a historic synthesis of the evolution of Brazilian agricultural research and the bases for the creation of EMBRAPA.

It should be mentioned that EMBRAPA was instituted on March 28, 1973, and is therefore a very young enterprise. As far as decision-making is concerned, the following basic policy tools orienting national agriculture stand out: (1) The National Plan for Socioeconomic Development and (2) The Basic Plan for Scientific and Technological Development.

On the basis of these two plans, EMBRAPA defines its course of action and priorities at the national, regional, state and local or institutional level. EMBRAPA's plan of work and its budget are analyzed and approved by the National Agricultural Research, Technical Assistance and Rural Extension Commission (COMPATER). As regards mechanisms for resource allocation at the level of EMBRAPA, the document attempts to define the Indicative Plan of Agricultural Research and the National Program of Agricultural Research.

During the workshop, it was put on record that EMBRAPA has limited experience as regards mechanisms for resource allocation. In the interim, they are in the process of defining parameters and methods on a scientific basis in order to carry out this allocation.

As far as defining research priorities is concerned, three basic and closely related factors are taken into consideration: growth, equity and reduction of risk. In addition to these, fourteen additional criteria were considered important in the definition of priorities and resource allocation for research. These were the importance of the commodity, its role in nutrition, price elasticity of demand, its role in the balance of payments, the possibility of an immediate response (margin of return), the industrial demand, price movement, availability and use of resources, possible beneficiaries, regional equity, risks and uncertainties, the technology employed (known and potential), the competitive capacity in the production of technology, and the possibility of importing and adapting technology.

At present, EMBRAPA is attempting to obtain the data necessary to be able to utilize all the aforementioned criteria, as well as to define adequate methodology for the evaluation and control of research activities carried out in Brazil.

THE DECISION-MAKING PROCESS FOR RESOURCE ALLOCATION IN PRIVATE AGRICULTURAL RESEARCH

A. Grobman

The private firm in the agribusiness sector undertakes research for the express purpose of developing marketable products, which contribute to its growth through added revenues.

Competition creates the need for innovation and research, but the search for new products transcends this simple explanation. This continued quest in research in the modern firm is part of its very reason for existence nowadays, as research has become an integral part of the operational structure of the firm and one of the foundations of its profit expectations, which should be considered as society's payment for the firm's service in carrying it out.

New product development is not a simple proposition; it entails risks. Management is aware of it and tries to evaluate the nature and magnitude of the risk. After quantifying it and comparing it to profit opportunities, management makes the decision on an acceptable level of commitment of the firm's resources in research and development leading to the production of new products and processes.

Products or processes are objectives of private research; but as opposed to results of public or institutional research, the requirement of salability of the product or process is essential.

The product has a life cycle characterized by various phases or stages—such as (a) introduction, (b) growth, (c) maturity, (d) saturation, and (e) decline—during which volume of sales and profit margins evolve gradually, reaching independent peaks and declining later. Provisions for product improvement or substitution are figured out so as to prevent total profit evolution from dropping off.

A premise that has been established in R & D operations in a company is a **"new product policy"** which, as stated by Gregg in 1958, "should define the limits within which the business will operate in the new product activity."

These policies should be so clearly defined that "they can be understood and carried out by each company faction without constant referral to higher authority."

Policy, however, is not a fixed body of thought and action, rather it experiences a constant reevaluation and adjustment to circumstances and opportunities. It takes the form of determining (a) the acceptance, continuation and duration of research commitments; (b) the allocation of research funds on a selective target basis; (c) the levels of specialization or expansion in product lines; (d) return factors; (e) investment factors; (f) the maintenance of research capability in the company; and (g) the company's research image.

After policy delineation and establishment of corporate goals has been accomplished, **research management** is called upon to establish what Villers (1964) has called the alternatives of **Programmatic and Nonprogrammatic Research.** Certain corporations define the former as those that are scheduled, and a beginning and end are established within a flexible plan of action. Nonprogrammatic research is not scheduled.

Research management must be on the alert, collecting, collating, storing and distributing facts and information within the organization, maintaining an awareness of research opportunities within the research community of the corporation. The opposite flow of information occurs also.

As ideas for the development of new products or the improvement of old ones appear (originating either in the R & D Division, in the Marketing or Production Divisions, or at the level of top management) and as projects are presented for evaluation and approval, an overall decision-making setup is established; and several phases of an evolutionary process leading to the development of a product take place.

The problem of resource allocation to a R & D project involves considerations of resources needed, as well as resources available, and their partial and proportional distribution throughout the spectrum of products the project aims to develop.

Final product performance created through R & D is subjected to experimental tests, market tests and ultimately to consumer acceptance, which is the final and only valid test of final performance.

It is not often realized how dependent a firm is on this final measure of performance and acceptance of its products, which transcends intermediate certification by public institutions. This is why the firm doing R & D work in the agribusiness sector needs a final confirmation of acceptance by the

consumer; market tests, laboratory and plot tests can approach and predict reactions but cannot ever really substitute this. The consumer usually finds value in a product where others do not; conversely, he may find faults that could not have been predicted accurately by the R & D group, were it not for the response and interaction of the consumer and the product during its use.

Thus, decision-making in resource allocation at the firm level is a highly critical process, unique among the different types of research institutions because it deals not only with the production of outputs as a result of allocation of inputs, but is also highly sensitive to consumer response, the feedback of consumer-product interaction to its own organization and its future actions, to the performance of life expectation of the product, and to the marketing and economical outlook of the firm as a whole.

THE DECISION-MAKING PROCESS APPLIED TO RESOURCE ALLOCATION IN AN INTERNATIONAL AGRICULTURAL RESEARCH INSTITUTION: THE CASE OF CIAT

E. Alvarez-Luna

Faced with the worldwide threat of famine, the need was seen in 1966 for creating two international centers in the lowland tropics of the world—CIAT in Colombia and IITA in Nigeria. These centers would develop research designed to accelerate the utilization of new areas in Latin America, Africa and Asia to produce food more efficiently and with better nutritional quality.

At the end of 1966, Roberts and Hardin drew up a document proposing the creation of CIAT; they suggested the simultaneous initiation of two areas of research activities: one in crops and one in beef cattle. They recommended also that the center's activities should concentrate on the improvement of a few well-selected crops, rather than working on too broad a range of species which would lessen the effectiveness of research. The premise was established that crops that were finally selected should have great potential for widespread utilization in the humid lowland tropics throughout the world and should be important for human nutrition. These criteria served as a basis for suggesting initial areas of research.

During CIAT's formative phase, the decision-making mechanism was helpful in developing the content of the program that should be the basis of the center's operations; and to complement the first document presented by Roberts and Hardin, several outstanding scientists were employed to carry out feasibility studies on some of the areas of activity suggested in the reference document. These studies and the original philosophy proposed for CIAT by Roberts and Hardin served as a base for CIAT's Board of Trustees to define the initial structure of the institution's programs.

Within the development of different programs, a series of adjustments has been made among teams and within each multidisciplinary team in response to the needs expressed by the external review teams, program leaders, individual scientists, CIAT's director and the Board of Trustees itself.

Each of the programs has a similar makeup and a research component oriented towards improving specific aspects according to well-defined characteristics or priorities, such as yield capacities, resistance to insects and diseases, economic aspects of production, ecological adaptation, planting and fertilization systems, methods of insect and disease control, economic aspects of production, and training to develop production specialists, to give graduate students the opportunity to do field work for their theses, and to prepare researchers for national centers. Theoretically, these components concur to develop systems that will increase agricultural production on farms at all levels and scientists trained for national programs.

This is just a broad outline of the organization's development and the structure of CIAT's research programs. The rest of the work reviews the development process for two of the program's priorities—cassava and beans—with the purpose of describing the mechanisms used in both cases to establish the general objectives of each program, the content and work objectives for each of these programs, resource allocation for each research project and the mechanisms used to make any necessary adjustments within each of these components to accomplish their goals.

The principal objective for both programs has been to increase productivity and production as a means of increasing the availability of food for the inhabitants of the humid lowland tropics.

International conferences and advisory committees are two of the mechanisms utilized for establishing priorities in the cassava and bean programs. Basically, the objectives of the international conferences are (1) to study the status of the research up to that time, (2) to identify priorities for future research, and (3) to study possible programs of international cooperation at the institutional level for research development and for the interchange of ideas and improved materials. The participants at these conferences are scientists highly qualified in the areas dealt with.

The main purpose of the Advisory Committee is to identify constraints, to make a critical evaluation of the progress made in research and the value of scientific contributions, to review the specific objectives of the research projects within each discipline, and to suggest the adjustments that should be made in the emphasis of each discipline and therefore in the assignment of personnel and financing.

The Advisory Committees for the cassava and bean programs are mechanisms of continuous evaluation that have ample bases for decision-making and suggesting changes of emphasis in priorities of ongoing research. The final decision to accept these suggestions always remains in the hands of CIAT's director and the institution's research teams.

From the aforementioned descriptions, one fact stands out in common: The decisions regarding the allocation of available resources for research at CIAT have been made by diverse components of the system and have been based on diverse criteria. The Board of Trustees, directors, leaders of scientific teams, the scientific teams themselves, as well as the individual scientists, have in one way or another participated in this complex, but interesting and valuable process.

In addition, it is evident that diverse entities outside this institution have also been influential in the establishment of priorities and in resource allocation. Donor institutions, the Consultive Group for International Agricultural Research (CGIAR) and its Technical Advisory Committee (TAC), the specific Advisory Committees, the Special Study Committees, and the national institutions that also have ideas and needs to be considered, are just a few examples of the entities that have in one way or another influenced and contributed to the present formation of CIAT's programs.

RESOURCE ALLOCATION IN APPLIED AGRICULTURAL RESEARCH IN LATIN AMERICA: THE CASE OF IDB

J. Soto Angli

Throughout the history of the Interamerican Development Bank (IDB), the institution has cooperated intensively in the financing of development in Latin America, placing special emphasis on the agricultural sector of the member countries; therefore, it has become the principal source of external funds for the agricultural development of the region.

Acting as an international financing organism in its activities supporting agricultural research programs, IDB attempts to (1) aid in the identification of research priorities at national and regional levels within the agricultural sector, (2) provide top-level experience in specific areas where some countries are lacking, (3) provide incentives for developing new long-range programs with multinational effects, (4) coordinate the simultaneous activities of several national institutions that are operating independently on the analysis of similar problems, and (5) provide sufficient funds for carrying out these programs and placing the results of the same within the reach of the countries and within their respective areas of action.

The process of resource allocation for this activity has not always warranted careful analysis and has on occasion resulted in scarce resources being directed to areas or subsectors of relative priority.

The limited resources assigned to this type of research can be observed and measured, but it is very difficult to measure the value they generate. In general, it can be said that the value of this type of research depends upon (1) the satisfaction it can give individuals and society and (2) the information it generates and that is of demand in other economic sectors.

As agricultural processes become more modern in Latin America, the demand for agricultural research becomes greater and more effective. The validity of this statement can be shown by the fact that (a) more commercial agriculture causes production for on-farm consumption to decline, in absolute as well

as relative terms; (b) those farmers who are more oriented towards marketing their commodity obtain information on new inputs more rapidly and at a lower cost than those who are self-sufficient; (c) the commercialization of agriculture also implies that farmers become more dependent on purchased inputs; (d) there is a better supply of complementary agricultural inputs for new varieties in those areas where agriculture is being carried out on a commercial scale, as opposed to those where it is still self-sufficient.

In the month of May this year, the Executive Director of IDB approved a contribution equivalent to 2 million dollars in national currencies in order to contribute to the basic budget of the international agricultural research centers located in Latin America. Nevertheless, in view of the fact that research per se is not sufficient, the Executive Director authorized in August of this year additional sums also equivalent to 2 million dollars in national currencies to finance in part these training programs and the transfer of technology in countries where the centers themselves could develop them in benefit of the Latin American and Caribbean countries.

The bank recognizes that improvements in food crops and cattle production depend a great deal upon the quality and quantity of services that the national institutions of the member countries offer farmers. At the same time, the capacity of these national agricultural research institutions for obtaining the most productive varieties of food crops and cattle production depends to a great extent upon the interchange of information which takes place between national institutions within and among countries, related to research techniques and their results.

Through many of its loans, the bank has helped to finance the training of professionals from national agricultural research institutions, has funded research and equipment, and has collaborated in the organization and operation of national research and extension programs.

For the aforementioned reasons, the bank has begun its activities in this direction. A technical aid mission visited the southern cone of Latin America in an attempt to establish an outreach research and transfer of technology program, mainly in regard to cereals. On the other hand, a mission formed by international experts, as well as bank officials, visited the Central American countries and Panama in an attempt to identify priority programs of the national research institutions of these countries so that with nonreimbursable technical aid, they can establish true food production packages.

In a speech given before the World Food Conference sponsored by the United Nations in Rome, Italy, Antonio Ortiz Mena, President of IDB, stated that "in answer to the requirements of the member countries and in view of the world's

present economic situation, the bank will orient its activities in the agricultural field towards the following objectives in the immediate future:

1. To increase food production for international consumption and better income levels in rural sectors by means of production activities for them

2. To encourage export-oriented food production. Taking into account these objectives, the bank will allocate its resources for technical and financial cooperation, giving preference to the following areas:

   a. Integral rural development. The bank feels it can cooperate more closely with member countries to improve living standards of the rural population.

   b. Water and fertilizers. Considered essential components for the success of the Green Revolution, the bank will continue stimulating the greater utilization of multipurpose hydraulic resources.

   c. Improvement of productivity and increases in agricultural production. This should be supported by installations for storage and marketing, installations for processing foods as well as producing essential inputs including fertilizers, pesticides and agricultural machinery."

Finally, at the same conference, the President of IDB indicated that "the bank would, in the coming years, give firm support to those activities related to applied agricultural research through the international centers in Latin America, as well as regional and national institutes."

RESOURCE ALLOCATION IN THE AGRICULTURAL RESEARCH SERVICE AND AND THE DEVELOPMENT OF NATIONAL PROGRAMS

W. L. Fishel

(The Agricultural Research Service of the U.S. Department of Agriculture is currently initiating action to establish effective national agricultural research programs. This paper describes the general organization and program structures of ARS, discusses the basic factors which must be reflected in any resulting national program, presents a strategy for implementing the development of national programs, and proposes procedures for their development. Condensed here are those sections of the paper dealing most directly with considerations, principles and procedures.)

In developing national programs, there are six general areas to consider: (1) Background considerations including diagnostics about organizational and power relationships and the basic reasons for concern about national programs in the first place, (2) the clarification of general concepts involved in specifying national programs and plans, (3) the factors that distinguish one national program from another or that specify the dimensions of any particular national program, (4) the practical limitations or prior conditions that may have significant effect on both national program characteristics and development procedures, taking care not to mistake the act of reflecting these factors for simple expediency, (5) the probable characteristics of such a national program or plan, and (6) the probable procedures for implementing the development of the national program and the best strategy to carry out the procedures.

National programs are unified strategies encompassing all elements in the nation contributing to the achievement of certain goals. Many of the problems in specifying national programs arise because they may be defined in several ways. However it may be defined, the national program is in a very real sense a contract between society and a research agency.

This role of national programs can be a difficult thing to get across to practicing scientists and even to research managers. The best we can expect is to understand better the process of which we are a part.

Both policymakers and scientists, stripped of all political subterfuge on the one hand and professional subterfuge on the other, are fundamentally concerned with the problems of society. The difficulty in allocating research resources arises from the difference in perspectives the policymakers and the scientists have. The policymakers' interest in problems is in terms of their impact on society; scientists view the same problems in terms of cause-and-effect relationships. Someone must help bridge this communication gap.

How do we get from one side of this gap—say, a statement by the President that we as a nation must be assured a plentiful supply of food—to the other side of the gap—say, a scientist's proposal to conduct photosynthesis research on corn? The framework for the process is the program structure; the process itself is one of communication. Program structures are predominantly research management devices; national programs are predominantly administrative and policymaker devices. They are not devices of the scientists.

There are two extreme points of view about how national programs and program structures should evolve. One I call the "information demand" principle and the other the "research management" principle.

The information demand principle of information **disaggregation** assumes that there are clearly distinguishable categories of information which are required by the greater environment within which any level of research activity operates. The program management principle of information **aggregation** may recognize the ideal status of the above principle conceptually, but stresses the practical aspects of dealing with people in implementing research programs. The former reflects social needs, while the latter reflects considerations about the effectiveness of implementing research plans. Hence, both viewpoints should be reflected in defining national programs.

Four areas of communication are important for national programs: (1) within the agency to communicate with each other in setting priorities and the direction of major thrusts in research, (2) among agencies in the Federal Government so the agency can more effectively relate to action agencies and vice versa, (3) with legislators to better communicate our needs and they to better evaluate where we are, where we are going, and what we need to get there, (4) with various "clientele" groups in industry to better communicate our mutual interests.

One essential feature of the national program, however it is defined, is that it should reflect a "unified strategy" for achieving goals. It is not merely enough to provide some kind of taxonomy of objectives and subobjectives; there must be some plan for logical progression toward some goal or goals.

These new national programs will be implemented by agency personnel while they continue to conduct an effective research program. Also, as they are developed, the national programs must be **sold** to the national community of research as a whole; therefore, some strategy for implementation is required. At the base of the one this agency has adopted is a simple proposition: **start demonstrating real leadership and see if anyone follows!**

We should begin to develop national programs only in those areas in which ARS has a legitimate right to be leaders because of the existing base of highly competent scientists and the existence of persons having leadership qualities. For other areas, leadership can be exerted in a catalytic role, encouraging other organizations and persons to follow our lead in developing national programs and leadership roles.

With respect to an internal strategy, we might start by running a pilot study in one area only, selected on the basis that it would not tend to create much conflict or discord either within or outside the agency.

A structure that has been proposed for developing the national programs would consist of three components: (1) The Secretariat, (2) the Advisory Board, and (3) the National Program document itself. The Secretariat would be an assemblage of expert staff and clericals located at headquarters. This group would direct the study, oversee data collection, make the final analysis, assemble reports and communicate with the Advisory Board.

A ten-man Advisory Board would be composed of representatives of the agency, industry and other USDA agencies, all having expertise in the subject area of national programs. It is important that this be an advisory group and **not** a task force.

The structure of the National Program document would include (1) goals of research, (2) National Program logic, (3) delineation of research areas, (4) background analysis, (5) research program analysis, and (6) summary and analysis.

While an approach to creating national programs is suggested, the principal guide must be one of flexibility. A strong central control of the **process** is recommended, but identification of research needs and the **data** for analysis must be supplied entirely by the experts.

RETURNS TO AGRICULTURAL RESEARCH IN COLOMBIA

J. Ardila, R. Hertford,
A. Rocha and C. Trujillo

This paper presents results of our studies of the economic returns to varietal improvement of rice, cotton, wheat and soybeans in Colombia. Following recent analyses which indicated that social rates of return to public investment in agricultural research have been exceedingly high in the United States, as well as in Brazil and Mexico, the main hypothesis tested was that returns to the four Colombian programs had been equal to at least 50 percent. Rates of this magnitude, of course, would point to significant underinvestment in agricultural research since the opportunity cost of public funds in Colombia—really the rate of return the Government could anticipate earning on additional public investment in the average, already active project—has been estimated to equal 10 percent.

The methodology used to test the hypothesis of high returns was developed by economists a long time ago and was used in all previous studies of returns to agricultural research. It associated the benefits of a research program with a shift in product supply or a decrease in the costs of producing a given output as a result of farmers adopting higher yielding, improved seeds generated through a program of varietal improvement. Total benefits include gains to consumers resulting from the commodity's lower price, as well as gains to producers associated with lower production costs. For purposes of exposition, these total benefits for any year can be approximated by the term kV, where k is the supply shift parameter or the percentage change in average, on-farm production costs due to research and V is constant price measure of the total value of production of the commodity under examination. This measure of benefits is reduced each year by the price-adjusted costs of the research program (C), and a rate of interest is then found which makes discounted net benefits (kV - C) zero valued over the relevant time period. That rate is then taken to be the net internal rate of return to the research program.

The supply shift parameter, k, was estimated as the product of two separate variables: a difference in yields, termed the yield advantage, between two

farm plots (one being planted entirely with the improved seeds and the other with the unimproved varieties) and the percentage of cropland planted with improved varieties. The yield advantage was actually estimated from regressions of yields (from on-farm trials administered by the research program staff) on a series of key independent variables, including the variety of seed planted. In this way the yield effects of different seed types were not mistaken for effects of other production factors. The second variable, the percentage of cropland planted with improved varieties, was calculated as a function of available data on annual sales of certified seeds.

Given this estimation procedure, larger yield advantages and/or higher percentages of cropland planted with improved varieties are obviously associated with larger values of k, as well as larger net benefits and higher rates of return. Thus, differences in rates of return among programs can be attributed directly to differences in yield advantages of improved varieties and observed levels of use or adoption of the new seeds. The yield advantage, of course, is technically and biologically determined, while socioeconomic factors and the structure and organization of production are usually primary determinants of the amount of cropland planted with improved seeds.

Within this framework, two other variables also assist in explaining differences in calculated rates of return to individual research programs. These are revealed by rewriting the simple definition of net benefits as $V(k - C/V)$. It is seen that an agricultural research program which is costly in relation to the value of the final output of the commodity worked on will be associated with lower net benefits and a lower rate of return, other conditions being equal. Similarly, the less important the commodity in terms of its domestic value of production, the lower is its rate of return.

The main results of the paper are summarized in Table 1. Although rates of return calculated for the rice and soybean research programs were found to have exceeded the 50 percent level by a wide margin, it is seen that returns to wheat improvement turned out to be rather modest and that those for cotton research were negligible.

The high returns to soybean research were attributed principally to the rapid and high levels of adoption of the improved varieties. This striking adoption pattern was, in turn, attributed to a strong demand for the product, the geographic concentration of producers which facilitated rapid diffusion of information about the new seeds, and the fact that soybean farmers are among Colombia's most progressive.

Although the high returns to varietal improvement in rice were partly explained by the yield advantage of the new varieties, their levels of adoption and the overall importance of rice production, they were mostly credited to

Table 1. Colombia: Selected comparative data on the rice, cotton, wreat and soybean varietal improvement programs

Concept	Unit	Rice	Cotton	Wheat	Soybeans
Estimated net internal rates of return	Percent	60–82	0	11–12	79–96
Estimated value of the supply shift parameter, 1971	Percent	10–16		16	17–35
Estimated yield advatnage, 1971	Percent	25–39		46	17–36
Land area planted with improved varieties, 1971	Percent	41	100	35	98
Total research costs/ value production, 1968-1971	Percent	0.5	0.1	3.0	0.1
Average yields, 1971 Colombia/United States	Ratio	0.68	1.03	0.53	1.01

the fact that the rice program tapped an accumulated stock of plant-breeding capital through collaborative agreements with two international centers (CIAT and IRRI) and the World Collection of Rice maintained by the U. S. Department of Agriculture. These sources of information, know-how and plant materials hastened the discovery of new varieties at minimal cost to the national program.

Lower returns to wheat research did not reflect obvious technical failures in plant breeding. On the contrary, the estimated yield advantage of the improved wheat varieties was the highest among the programs analyzed. However, adoption of the new varieties was laggard; from the time they were first sold commercially in 1953 until they were planted on 25 percent of all wheatland, fully 12 years elapsed. Furthermore, rates of adoption peaked at 50 percent in 1968 and then began a downward trend. This slow uptake of the new seeds, their currently low levels of use, and the distressing downward trend in recent adoption patterns were attributed to certain socioeconomic constraints on wheat production, not the least of which were large and sustained imports of wheat under P. L. 480, which depressed the domestic market. The high relative costs of the program in its later life, the low value of wheat production in Colombia, and a long "dry period" of public investments in research before new varieties were released also forced down the estimated rate of return.

Cotton was a special case. On-farm yields increased sharply, partly as a result of the rapid adoption of improved U. S. varieties. Yet, it was concluded that the national research program should not be credited with these gains, essentially because of the nature of its activities and objectives. It was designed only to import, test locally and distribute to Colombian farmers the highest yielding U. S. varieties. The premise was that yields of U. S. cotton grown in Colombia would vary by type or variety; thus, a payoff was anticipated from an effort which identified those varieties yielding best under local conditions. However, careful examination of over 500 commercial field trials performed in Colombia did not uncover significant differences in yields of the improved U. S. varieties. Therefore, it was concluded that the main activity of the research program was unnecessary. U. S. varieties could just as well have been selected at random for distribution to local farmers.

AN ECONOMIC MODEL FOR ESTABLISHING PRIORITIES FOR AGRICULTURAL RESEARCH AND A TEST FOR THE BRAZILIAN ECONOMY

J. P. Ramalho de Castro
G. E. Schuh

The potential contribution of technical change to agricultural development has been recognized for some time now. Only recently, however, has it been fully appreciated that technical change can take alternative routes in its resource-saving effects and that the particular route that it takes is conditioned by relative factor scarcities. This immediately implies the concept of an efficient path for technical change and suggests the importance of allocating scarce research resources in such a way as to direct technical change along this economically efficient path.

Although the notion of an efficient path for technical change (in the resource dimension) can serve as an important basis for allocating research resources, this in itself is not sufficient. Technical change has important income distribution consequences. In the first place, the extent to which its benefits accrue to the consumer or to the producer depends to a great extent on the conditions of supply and demand for the product. In addition, the extent to which the benefits that do accrue to the producer are distributed among particular factors of production will depend on both the "direction" which the technical change is taking (in the resource dimension) and the conditions of supply and demand in the individual factor markets.

The study is directed to the problem of developing and testing a model which would provide a basis for establishing priorities for agricultural research.

The paper is developed in four sections. The first section contains the conceptual model. The empirical results are reported in the second section, and the economic and policy implications of these results are discussed in the third section. Finally a review of principal conclusions is presented.

The basic analytical model is built up within a framework that considers the distribution of the benefits from technical change between producers and consumers and their distribution among the factors of production, given the producers' share of benefits. The direction of research is postulated as a function of relative factor prices. A two-sector general equilibrium model is used to analyze the adjustment problem among sectors as technical change proceeds.

Allocation decisions with respect to agricultural research are generally made on a crop basis, taking into account whether and in what proportions resources should be allocated to specific crops. The analysis of the present study is designed in part to provide information which will help in decision-making based on the assumption that the total flow of benefits expected from a given technological change is important and that policymakers or research managers have some notion of the extent they desire to benefit producers and consumers.

The principal conclusions of the analysis are

1. The choice of products which should have priority in the research effort will depend upon government goals:

   a. If the goal is to increase income in the agricultural sector, the products to be selected are those with a high price elasticity of demand. An important group of such products are those with a comparative advantage in world markets, such as cotton and sugar cane.

   b. If the goal is to increase the income and employment of farm labor, the choice would be the same products.

   c. If the goal is to increase consumer welfare, the products to be considered must be those with a low price elasticity of demand, such as corn, rice, edible beans and cassava.

   d. If the goal is to enlarge agriculture's contribution to general economic development, the choice will depend upon the prevailing constraint at the particular time. If, for example, the constraint is capital, the products to be selected are those which give the greater flow of gross benefits; namely, corn and rice. On the other hand, if the constraint is foreign exhange earnings, cotton and sugar cane would be higher on the priority list.

2. The results suggest that the bulk of research should go to increase land productivity. However, there is room for research on the

subfunction of labor if the research is directed to activities which are not strongly labor displacing (for example, research with tractors to improve land preparation).

3. The results obtained in estimating the parameters of the production function with time series data suggest a basic change took place in production technology in the early 1960's. Modern inputs such as fertilizer and machinery have substituted the primary inputs which they previously complemented.

4. Finally, the problem of adjustment in the labor market between the agricultural and nonagricultural sectors is expected to be sizable if research is directed to the crops with a low price elasticity of demand, such as corn, rice, edible beans and cassava, even if the research is basically designed to improve land productivity. On the other hand, if research is directed to export-oriented crops, such as cotton and sugar cane, the demand for labor will be expected to increase even if the research is focused on land subfunction. It could also increase the demand for labor if focused on the labor subfunction as well, as long as the technical change that results is not strongly labor displacing.

A PROPOSED MODEL FOR IMPROVING THE INFORMATION BASE FOR RESEARCH RESOURCE ALLOCATION

P. Pinstrup-Andersen, R. O. Díaz, M. Infante and N. R. de Londoño

In order to establish sound research priorities, information is needed on expected benefits, costs and time requirements for each of the lines of research considered.

Priorities in applied agricultural research are frequently established on the basis of very limited information on existing problems and their relative economic importance in the production process. Because of this situation, some research may be irrelevant to actual farm problems and research results may not be adopted.

A continuous flow of information to the research manager on the potential gains in production, productivity and risk involved in alternative research activities, as well as the farmers' preferences with respect to new technology, is likely to be useful to assure that new technology corresponds to the farmers' needs and preferences, thereby accelerating adoption and increasing research pay-off.

Such an information flow may consist of a continuous feedback of information from the farmer through the extension service to the research institutions. Direct contact between researchers and farmers through meetings, farm visits, etc. would be another effective vehicle for such information. To complement these, we are suggesting a third method. This method consists of a combination of agro-economic surveys and agrobiological experiments.

The agro-economic survey attempts to transmit to the research manager the farm-level demand for applied agricultural research through the establishment of a direct link between the farm and the research institute.

Attempts are made to describe certain key aspects of the structure, performance and results of the production process, the farmer's objectives, and the interaction among these factors. Emphasis is placed upon identifying the principal factors limiting production and productivity and estimating the implications of changing these factors.

A small specialized team of agronomists and economists obtain primary data from a panel of farms expected to be representative of the farms for which agrobiological research is intended. The field team makes periodic visits (normally 3-4) to each farm throughout a complete crop cycle. About half of the time on the farm is spent in the field, collecting data on agrobiological issues (by direct observation), while the other half is used to interview the farmer.

Direct participation of a highly qualified multidisciplinary research team in the training and field execution phases is essential to the success of the survey. The field teams working on the ongoing CIAT agro-economic surveys have received three to four months of presurvey training in direct contact with the scientists from the relevant disciplines.

The agro-economic survey provides an estimate of the area affected by each of the problems identified. Furthermore, it gives an indication of the yield depressing effect. However, it is frequently difficult to estimate the yield impact from survey data with a great deal of accuracy; hence controlled experiments are carried out to help quantify the yield impact of the problems.

In addition to aggregating the data for the purpose of presenting a description of the process, emphasis is placed on estimating the economic loss caused by each of the agrobiological and ecological factors, such as disease, insects, weeds, soil deficiencies and adverse rainfall conditions, and the implications of changing these factors. Furthermore, estimation is made of (1) production costs and labor absorption by production activity, (2) net returns to the process for each of the principal cropping systems, (3) the contribution of each of the principal resources to net returns and (4) the factors influencing the farmer's decision-making in reference to the adoption of new technology and the choice of cropping system.

Projects are currently under way in Colombia to field test the above methodology for maize, cassava and beans. Although the information obtained from these empirical studies is expected to be useful to Colombian national institutions and CIAT, the primary purpose of the work is to develop and test a simple methodology for use by national research agencies in Latin America and elsewhere. The paper presents a few preliminary results of this work to illustrate the kind of information provided by the agro-economic survey.

In addition to the expected utility of the information made available by the agro-economic analyses, the work provides a valuable training opportunity for young agronomists and economists interested in production.

No claims are made that the agro-economic survey is a new invention. However, certain aspects of the work discussed above tend to distinguish it from traditional farm surveys and hopefully make it more useful for establishing priorities in applied agricultural research. These aspects are (1) A considerable proportion of the data are obtained from direct field observations made by agronomists previously trained for this job; (2) each farm is visited periodically during a complete growing season; (3) the work is multidisciplinary in nature and involves direct participation by professionals from all the relevant disciplines; (4) the work is specifically focused on providing information needed to establish research priorities. Although the information may be useful for other purposes, such utility is considered secondary.

LIST OF WORKSHOP PARTICIPANTS

ALVAREZ-LUNA, EDUARDO, Associate Director General, CIAT, Cali, Colombia.

AMPUERO, ENRIQUE, Director General, INIAP, Apartado 2600, Quito, Ecuador.

ARDILA V., JORGE, Economist, Office of Planning, ICA, Apartado Aéreo 7984, Bogotá, D.E., Colombia.

BRADY, NYLE C., Director, International Rice Research Institute, IRRI, P. O. Box 933, Manila, Philippines.

BYRNES, FRANCIS C., Leader, Training and Communication, CIAT, Cali, Colombia.

CHAVERRA G., HERNAN, Director of Planning, ICA, Apartado Aéreo 7984, Bogotá, D.E., Colombia.

DIAZ, RAFAEL ORLANDO, Research Associate, Economics, CIAT, Cali, Colombia.

DOW, KAMAL J., Economist and Head, Technical Assistance Mission of the Universiy of Florida, INIAP, Apartado 2600, Quito, Ecuador.

FISHEL, WALTER LEE, Systems and Computer Scientist, Program Analysis and Coordination, Agricultural Research Service, U.S. Department of Agriculture, Room 209, North Building, BARC-W Beltsville, Maryland 20705, U.S.A.

FRANCIS, CHARLES A., Leader, Small Farm Systems Program, CIAT, Cali, Colombia.

FRANKLIN, DAVID L., Systems Engineer, Leader, Biometrics Unit, CIAT, Cali, Colombia.

FRANSEN, JAMES M., Agricultural Research Adviser, International Bank for Reconstruction and Development, 1818 H. Street, N.W., Washington, D.C. 20433, U.S.A.

GIRON, ALFREDO GUILLERMO, Economist, International Potato Center (CIP), Apartado Postal 5969, la Molina, Lima, Perú.

GROBMAN, ALEXANDER, Director of Research and Development for Latin America, Northrup, King & Co., Avenida Arequipa 340, Of. 601, Lima 1, Perú.

GUERRA, GUILLERMO, Economist, Interamerican Institute for Agricultural Sciences, Apartado Aéreo 11185, Lima 14, Perú.

HERTFORD, REED, Program Advisor, Ford Foundation, 320 East 43rd Street, New York, NY 10017, U.S.A.

INFANTE, MARIO A., Research Associate, Economics, CIAT, Cali, Colombia.

KOZUB, JACQUES J., Chief, Agricultural Economics Section, Interamerican Development Bank, 808 17th Street, N.W., Washington, D.C. 20577, U.S.A.

LOPEZ NETO, AUGUSTO SIMÕES, Technical Department of Planning Methods, Empresa Brasileira de Pesquisa Agropecuaria, EMBRAPA, SBN Palacio do Desenvolvimento, 9º Andar, Sala 908, 70.000 Brasilia, D.F., Brasil.

MARIÑO NAVAS, RAFAEL, Director General, ICA, Apartado Aéreo 7984, Bogotá, D.E., Colombia.

MERRILL, WILLIAM C., Agricultural Economist, AID, Room 2246, N.S. Department of State, Washington, D.C. 20253, U.S.A.

MONGE, FERNANDO, Leader, Library and Information Services, CIAT, Cali, Colombia.

NESTEL, BARRY L., Associate Director, IDRC, Apartado Aéreo 53016, Bogotá, D.E., Colombia.

NICKEL, JOHN L., Director General, CIAT, Cali, Colombia.

PAEZ, GILBERTO, Head, Department of Quantitative Methods, EMBRAPA, Palacio do Desenvolvimento, 9º Andar, Sala 908, Brasilia, D.F., Brasil.

PINSTRUP-ANDERSEN, PER, Leader, Economics Unit, CIAT, Cali, Colombia.

RECA, LUCIO G., Head, Economic Research Division, Banco Ganadero Argentino, Juncal 735, Buenos Aires, Argentina.

RAMALHO de CASTRO, JOSE P., Economist, Ministry of Agriculture, A.T.E., 9º Andar, 70.000 Brasilia, D.F., Brasil.

RUTTAN, VERNON W., President, Agricultural Development Council, 630 Fifth Avenue, New York, NY 10020, U.S.A.

SCHUH, G. EDWARD, Senior Staff Economist, Council of Economic Advisors, Old Executive Office Building, Washington, D.C., U.S.A.

SCOBIE, GRANT M., Economist, CIAT, Cali, Colombia.

SOTO ANGLI, JOSE, Training Officer, Interamerican Development Bank, 808 17th Street, N.W. Of. A 384, Washington, D.C. 20577, U.S.A.

STEPPLER HOWARD A., Professor of Agronomy, McDonald College, McGill University, St. Anne de Bellevue, Quebec, Canada.

TOLLINI, HELIO, Economist, Ministry of Agriculture, A.T.E. Sala 908, 70.000 Brasilia, D.F., Brazil.

TWOMEY, MICHAEL, Economist, International Potato Center (CIP) Apartado Postal 5969, Lima, Perú.

VALDERRAMA CH., MARIO, Director, Division of Agricultural Economics, ICA, Tibaitatá, Apartado Aéreo 151123 (El Dorado), Bogotá, D.E., Colombia.

VALDES, ALBERTO, Economist, CIAT, Cali, Colombia.

FECHA DE DEVOLUCION