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PRIORITY SETTING IN AGRICULTURAL RESEARCH

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International Service for National Agricultural Research

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PRIORITY SETTING IN AGRICULTURAL RESEARCH

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International Service for National Agricultural Research

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PREAMBLE

ISNAR, on the basis of a systematic review of past experience and accumulated knowledge, has identified a set of twelve critical factors which together determine the effectiveness and efficiency of a national agricultural research system. It has set up working groups to study each of these factors, priority being accorded to six of them. These six working groups are central to ISNAR's development of management tools and concepts, and of training materials. One of these working groups deals with research policy formulation and planning, and the present paper on priority setting in agricultural research has been produced under the auspices of this working group.

With several billion dollars a year spent on agricultural research in developing countries, the subject of priority setting has been identified as important, not only by developing-country research leaders and ISNAR, but also by donors in the context of their initiative of the Special Program for African Agricultural Research (SPAAR). In fact, following publication of the SPAAR/ISNAR paper entitled "Guidelines for Strengthening National Agricultural Research Systems in Sub-Saharan Africa", SPAAR set up a working group on priority setting in agricultural research, and it asked ISNAR to prepare a paper in this area. The present working paper is a response to these varied demands.

The content and form of this paper have gone through a process of intensive consultation within and outside ISNAR. First, the authors received much help from the members of the ISNAR working group and other close associates. Second, an early draft was discussed in Washington in October 1987 by the SPAAR working group. Third, an advanced draft was subjected to external scrutiny at the Agricultural Research Policy-Setting Seminar for West Africa, organized by the World Bank's Economic Development Institute (EDI) and the African Development Bank, at Yamoussoukro, Côte d'Ivoire, in January 1988. Fourth, in March 1988, the paper was sent to the SPAAR working group for comments. Most recently, detailed written and oral comments from eight leaders of African national research systems, invited as experts to a one-week consultation organized by ISNAR at The Hague, on April 11-15, 1988. Both at Yamoussoukro and at The Hague, the paper was well received. On both occasions it was strongly recommended that the proposed methods be thoroughly field-tested in several developing countries before being formally recommended. ISNAR is seeking funds to make this possible, in collaboration with selected agricultural research systems. This means, of course, that the methods described here are still in the process of development.

This working paper is intended for high officials in ministries of planning, agriculture, rural development, and science, who are involved in decisions concerning research priorities at the national level. The paper is also intended for directors of agricultural research institutions, who are responsible for determining priorities and allocating resources within their organizations among research programs, subprograms, and major themes.

The April 1988 expert consultation at ISNAR also reviewed a draft manual for those who actually have to carry out priority-setting exercises at different levels of sophistication: economic, technical, and planning staff in ministries of planning and agriculture, and senior scientists. It is intended that the manual, which will be published as soon as its editing has been completed, will be used, in combination with the present paper, as self-tutoring material and as a teaching aid in courses.

It is recognized that researchers and planners in some countries have already been exposed to priority-setting methods, but very few have had the experience of implementing these methods over sufficient time to assess their practicability in specific country settings. Such experience can only be gained by national research planners themselves trying out a choice of methods over several years. The authors have presented a range of methods from which countries at different levels of development can make a choice which is appropriate for their circumstances.

PRIORITY SETTING IN AGRICULTURAL RESEARCH *

Abstract

This paper discusses the issues involved in setting medium and long-term priorities for agricultural research and in choosing methods to improve the process. Reasons are given why more formal methods might be helpful, and some general difficulties in applying such methods are discussed. The national planning context in which research priorities are established is outlined. Some formal priority-setting methods are presented, together with the advantages and disadvantages of each.** The problems of priority setting for non-commodity problems, and a few other, broader, issues, are also discussed.

Resources are scarce everywhere. This is particularly so in developing countries. They need to be allocated in such a way that benefits are maximized for a given cost. This requires that choices be made between different patterns of resource allocation; between one research program and another. The rationale for doing this, and the different methods which may embody it, form the subject of this paper.

Why attempt to formalize priority setting?

Priority setting is important in improving the management of agricultural research. The adoption of more formal procedures, to complement informal ones based on experience, intuition, and cognizance of national agricultural development needs, is recommended in virtually every external review of public research in developing countries. Yet such procedures have been little used by these countries so far.

This is not to say that developed countries, notably those with market economies, have set an example in this respect. They too have largely ignored formal priority setting, for a variety of reasons.

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* This paper was prepared as a project of the ISNAR working group on agricultural research policy and planning. Its members are: R.B. Contant (chairman), J.A. Bottomley, N. Bosso, J. Casas, H.H. Hobbs, E.Q. Javier, P. Pardey, G. Rocheteau. Each member played a part in the preparation of this paper. J.K. Coulter and H. Elliott also made important contributions, and S. Chater edited the paper extensively. The authors also acknowledge detailed comments made by: S.A. Adetunji, G. Beye, R. Billaz, A.M. Macha, D.R.B. Manda, S.N. Muturi, J. Nya Ngatchou, B. Patel, A. Viereck, E. Whingwiri, and Yohannes Kebede. The authors alone remain solely responsible for any errors in fact or judgement which may remain.

** Examples of how these methods might be applied are available from ISNAR in the form of a manual aimed at those who must actually carry out a formal priority-setting exercise.

First, there has been a tradition of respect for, and confidence in, science and the scientific community: government and the public have felt that society's interests are best served by allowing scientists substantial freedom of enquiry. This freedom has been exercised through a network of interpersonal relationships and judgements, which seem to have been justified in the event. Failures have been largely overlooked in consequence. But the number of such failures might well have been lower if a more systematic approach to the allocation of resources within the research community had been applied.

Second, the structure of research is often fragmented: publicly funded programs are complemented by research in the private sector. Within the public sector, research is departmentally segregated. Thus there are many parties with more or less independent interests and decision-making powers, and any attempt at central priority setting carries very little weight.

Third, in most Western countries, market forces rather than central planning determine which lines of development, and consequently of research, are pursued by the private sector. Those who judge priorities correctly in accordance with these market forces survive; those who do not go out of business. Thus, it is the "invisible hand" of market forces which strongly influences research priorities.

Fourth, the public sector increasingly sees its role as a provider of basic knowledge which the private sector can exploit. Because of the difficulty of predicting its impact, such basic research does not easily lend itself to formal priority setting.

Nevertheless, public demand for a more effective use of scarce resources, as well as for stricter control over the research agenda, has led in recent years to increased intervention by government in the research priority-setting process throughout the Western world.

In the <u>developing</u> countries, most agricultural research still takes place without formal priority setting; judgements are made on the basis of experience in the light of national development goals. In the past, however, priorities for research were often determined by political and commercial forces controlled by the then metropolitan powers. Also, the research community in developing countries acquired a similar image in the eyes of the public to that which it had in the West. Lastly, few people are familiar with priority-setting methods, while researchers themselves are often not convinced they would benefit from using them. Consequently, there is often no pressure for formal priority setting.

Although the circumstances of developing countries differ markedly from those in advanced countries, these differences actually reinforce the argument for adopting more formal approaches to priority setting in the Third World.

First, the structure of research is less fragmented. Private research is little developed, and governments feel obliged to cover everything. This provides a clear-cut argument for centralized priority setting.

Second, although development plans exist in many countries, they often contain goals which are not specific enough to guide research, or which are unrealistic, in part because the research community has not been adequately consulted. In such circumstances, separate formal priority-setting exercises may be needed for research.

There are several other reasons why there is now a growing need for a more formal approach to priority setting in agricultural research in developing countries:

1. As rural development lags, more and more is expected of national agricultural research systems. Often, expectations are unrealistic, particularly when manpower development is slow and funding unstable. There is pressure to obtain results from short-term, adaptive research without adequate attention being given to the longer-term strategic or applied research required to generate new technology. Formal priority-setting techniques would reveal where unrealistic expectations lie.

2. Planners confronted with conflicting national goals need methods to help them make their decisions, as well as arguments with which to defend them. The benefit and cost variables involved may be so numerous, and their interactions, particularly over time, so difficult to envisage that it is very difficult to synthesize them intuitively. Quantitative methods set values on these variables and clarify their interrelationships. A planner who has gone through such a systematic exercise is in a much better position to defend his or her decisions on priorities.

3. Staff and funds are often allocated on a historical basis, reflecting past priorities. There may be vested interests in maintaining the status quo. Radical changes, which may be urgently required to meet changing national needs, can be introduced more easily if formal methods of priority setting are used.

4. Investments by donors, too, are sometimes based on past priorities, and, in any case, often reflect their own perceptions, rather than those of the developing country concerned. Formal methods for comparing donors' project proposals with those of the national research system would strengthen the research manager's hand in directing donor resources to high-priority areas.

5. Research has both costs and benefits. On the cost side, planners must determine what programs are sustainable from national and external resources. This means that developing countries must look to the benefits which they, themselves, can capture in relation to the costs which they have to bear. National resource limitations restrict the size of the overall program and the number of themes which can be pursued, and they affect the share of technology that should be imported through scientific cooperation. In particular, the international community should be expected to support research which has potential impact across countries. Formal priority-setting approaches help to determine a rational division of research efforts between countries and institutions.

6. Trained manpower is scarce in most developing countries. As a result, many national research systems have difficulty in generating the critical mass needed to tackle important research topics. Techniques to help them decide where to concentrate their resources are therefore valuable.

7. Priority setting requires intensive consultation among and between politicians, administrators, planners, and researchers. Formal procedures facilitate this because they systematize the consideration of key variables and allow an iterative process to develop.

8. Much research is a long-term undertaking. A formal priority-setting exercise will help minimize the pressure to change the direction of research programs too frequently, particularly in those systems characterized by a rapid turnover of decision-makers.

9. A formal process can include economic policy considerations more systematically than can an informal one. It can address such issues as the potential for foreign exchange earnings, the effects of research on income distribution among producers, and the benefits to consumers. Use of a formal approach allows consideration of the interests of groups who might otherwise be unrepresented.

10. Formal priority-setting processes will identify the points at which personal research agendas and national interests appear to be in conflict.

A word of caution must be added. Any approach, intuitive or formal, to priority setting will not by itself guarantee the success of research. Other factors are likely to be just as important, if not more so. They are: the quality of the scientists involved, the stability in research structures and personnel, and the constancy and timeliness of release of the necessary funds. Thus, the maintenance of quality and stability become important priorities in themselves.

Difficulties in applying priority-setting methods

There are difficulties in applying formal methods for setting research priorities. The main ones are:

1. Comparing the different types of research program found in national systems. Programs may be organized around commodities (maize, cattle) or their products (maize flour and stalks, dairy products, meat, hides, manure), components of the natural resource base (soils), production inputs (farm machinery, labor), social and economic factors (policy issues and farm issues), and farming systems*. Formal priority-setting techniques have varying degrees of applicability to these different categories of research**.

2. Getting agreement on a joint approach to priority setting among the many different bodies allocating research funds in a decentralized decision-making system.

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* Most attempts at developing methods for setting research priorities have concentrated on commodities. The greatest challenge in developing quantitative methods to improved priority setting lies in extending the range of research activities to which such methods can be applied.

Sometimes, non-commodity research can be analyzed in terms of its impact on several crop or animal species, and it may then be possible to compare the outcome of such research with that on individual commodities, even though the estimates of costs and benefits of the former may be somewhat less reliable. In other cases, for instance strategic research into the long-term effects of changes in soil structure, the outcome is far more difficult to express in commodity impact terms. In such instances, experience combined with common sense may be the best approach to making relevant decisions.

** Planners and authors sometimes add other subdivisions: agro-ecological zones, political/administrative regions, disciplines, or types of research (adaptive, applied, strategic, basic). Such divisions are not themselves research categories in the sense of programs among which priorities can be set.

3. Carrying out a process that is at once "top down" and "bottom up". Whereas major priorities are set at ministerial level, these may be modifed in the light of information filtered up through the national research system from individual scientists, projects, or programs. This information, concerning the costs of research, the chances of success, likely adoption rates, environmental impact, and so on, constitutes vital feedback. Obtaining it and using it implies much interaction between research institutions and planning authorities, and the mechanisms for this may not exist.

4. Dealing with uncertainty regarding the adoption rates for new technology, especially in the light of changing government price and marketing policies.

5. Dealing with uncertainty as to the length and cost of research, as well as its outcome. In general, the longer the time horizon, the greater the uncertainty regarding both costs and benefits. Similarly, strategic research has less predictable costs and impact than does adaptive research.

6. Getting detailed information on program costs. This information is often lacking, as in many institutions budgets are organized by administrative line items rather than by programs, and unit costs are seldom readily available.

7. Attributing the costs of essential services, such as libraries, common laboratories, biometrics units, and workshops, to individual programs.

8. Dealing with the complexity of mixed farming systems. The features common to such systems include complex interactions between different, and often multipurpose, commodities, as well as the integration of byproducts, and knock-on effects from one year to the next, into the analysis. This complicates both the assessment and the attribution of the costs and benefits of a new technology.

9. Dealing with social and political objectives, such as the development of a specific region or ethnic minority, which may have a value to society which is different from that indicated by monetary costs and benefits alone. These social gains can only be quantified by using weights agreed by all parties to the priority-setting process, but which nonetheless must remain somewhat arbitrary.

10. Accommodating unrealistic political demands. Unpromising projects may have to be given high priority at the insistence of politicians, even against scientists' advice.

11. Marshalling the necessary expertise for formal priority setting. Some approaches demand substantial amounts of reliable data, or a high level of forecasting ability, neither of which may be available.

12. Breaking with the past. In the short term, the implementation of new programs reflecting altered government priorities may be circumscribed by lack of the necessary scientific capacity. This deters research leaders from selecting such programs in the priority-setting exercise.

13. Convincing research directors, accustomed to using judgement based upon experience for priority setting, that more formal mechanisms, with their imprecise data, can improve the quality of decision-making. Research directors are often reluctant to allocate the necessary staff time and money to formal approaches to priority setting, in the belief that costs will exceed benefits.

Priority setting in the national context

Planning activities

Priority setting for agricultural research cannot be carried out in isolation, but must be related to other planning activities at the national or institutional level. These activities are:

- determination of national goals;
- preparation of a national development plan;
- preparation of a national agricultural research plan;
- allocation of research resources.

National goals are normally stated as broad objectives, such as increased employment, greater food security and additional foreign exchange earnings. Such objectives may be too general to be of much assistance in setting research priorities, but are not otherwise a constraint to the priority-setting exercise, although they may mask conflicts that will appear at a more detailed planning level.

National goals are expressed in national development plans, in which specific objectives and operational targets for each sector, including agriculture, are given, usually within a five-year time frame. The chosen sector objectives for agriculture are, again, usually sufficiently broad to accommodate all research activities. It is the operational targets for agriculture, which should quantify production levels for each commodity as well as specify any social, commercial, or environmental aims, that provide the key material for use by the research community in setting research priorities.

At present, with some notable exceptions, participation of national research staff in the preparation of national development plans is seldom institutionalized. However, in many developing countries, contributions are solicited from experienced research staff in a personal capacity. Such contributions can form an important bridge between research and development by:

- encouraging ministries of agriculture to be more specific in their development objectives;

- providing opportunities for ensuring that research and extension synchronize their activities;

- providing scientists with firmer targets to which their research should be directed.

It is while the agricultural plan is being formulated that conflicts are most likely to surface, as difficult choices have to be made, for example between different regions of the country, between benefiting rural producers or urban consumers, or between increasing food security and

protecting the environment. Provided research personnel are adequately in touch with producers' needs, involving them in the planning exercise may help to resolve the potential incongruence between government objectives and the interests of small-scale farmers and pastoralists. For this reason, the needs and views of small farmers in any decision-making process may require specific representation, intractable though this can prove to be.

Ideally, the priority-setting exercise should form part of a long-term national agricultural research plan. In such a context the exercise is broadened, taking into account the country's long-term socioeconomic and environmental needs. Because of the high cost, time commitment, and skills needed to prepare such plans, most developing countries do not have one at present. But it is possible to set priorities without at the same time developing a long-term research plan. Under such conditions, however, there is a risk of losing sight of long-term research challenges, and of not reconciling the objectives of researchers with those of the nation.

The agricultural research plan can play an important part in identifying new development opportunities, rather than merely reflecting existing policy. It should therefore address topics beyond the scope of the national development plan. Because of the large time-span (often a decade or more) required for generating a new technology, the long-term research plan should cover a longer period than the national development plan, say 10 to 15 years.

In spite of the fact that the research plan deals with the same agricultural products as the development plan, it need not reflect the same order of priorities. This is because products with a high priority for national development may have few researchable problems, or because research done elsewhere reduces the need for domestic outlays.

If formal priority-setting methods are used in formulating the research plan, relative weights will have to be attached to national development goals. It should be emphasized that this is a political act beyond the competence of the research community and should not be carried out by researchers. Nevertheless, researchers remain under an obligation to advise politicians as to the cost, in terms of opportunities foregone, of slanting research choices in any particular direction. This is one more reason, and a very compelling one, for making the national planning exercise a joint one involving both higher and lower levels in the decision-making hierarchy, as well as a wide range of expertise from both the social and the biological sciences.

In practice, research planners will be confronted with an existing complement of professional staff and limited funds. They will know that a research program is unlikely to be successful unless it is assigned, at a minimum, one experienced scientist, one or two junior colleagues and technicians, and appropriate operating funds. These requirements clearly indicate from the outset the maximum number of research programs which can be pursued. Donor funding may be available to alleviate these domestic resource constraints. A judicious adaptation of the findings of international research through a well-planned information and task-sharing network can reduce resource constraints still further. An identifiable limit on the overall number of research programs which can be financed and staffed will thus emerge. The priorities to be set within this limit may then be determined by means of the techniques outlined in this paper.

In this respect it is important to distinguish the priority-setting exercise from the process of allocating resources to specific research activities. The former is simply the ranking of activities in their claim on resources; it does not determine the resource allocation itself. A high priority ranking does not always imply a high resource allocation.

Priority-setting processes

Priority-setting processes are of three broad types, or some combination thereof:

A. Priorities and resource allocation are essentially determined historically, with little or no regard for the need to formalize the process. There is little consultation, either within or between the different levels of decision-making in the national research system, and few searching questions are ever asked. Research systems following this procedure are likely to have a stereotyped research program, out of touch with both producer needs and national development goals. Such programs often lack the confidence of senior planning authorities, and run the risk of reduced budgetary support from government.

B. There is limited, informal, participation, but within the senior echelons only: planning staff and a few key research administrators and senior scientists typically determine research priorities at the interministerial or ministerial level. This tends to be a "top-down" approach, which at best leads to a research program firmly tied to national development aims, but at worst may be open to unrealistic political demands or expectations.

C. The approach to priority setting is open and participatory. It typically involves a multi-agency task force or commission, convened by the minister of agriculture or other high authority, and supported by specialist subcommittees. The effort is comprehensive, highly visible, and designed to rally the support of the whole scientific community, as well as that of producers and national planning bodies.

Of the three types, only the third provides a really suitable basis for the introduction of more formal priority-setting methods. The approach used is iterative, involving repeated consultation within and between three different hierarchical levels of decision-making.

Level 1 involves the setting of macro-priorities for research by national planners. These usually indicate the broad balance of effort among commodities, major agricultural problems, and national or regional development goals (e.g., maize, erosion, underprivileged regions). These priorities may be adjusted in the light of information received from level 2.

Level 2 encompasses the setting of priorities among research problem areas within the macro-priorities identified at level 1. Relative priorities must be established among specific research lines within a commodity or resource base factor, and among different disciplinary approaches (e.g., maize agronomy or breeding, reafforestation or terracing, search for a cash earner or improved subsistence). Decisions of this kind take place at the level of the agricultural research system as a whole and in the individual research institutes. They influence level 1, and are influenced in turn by considerations raised by level 3.

Level 3 addresses the setting of priorities among research topics, and the definition of projects and experiments (e.g., fertilizer application or crop rotation in maize agronomy, timber or fuelwood production in countering erosion, goats for local sale or groundnuts for export). Such decisions are made at institute, program, or station level, with individual scientists, and preferably clients as well, playing an important part.

The main factors influencing the choice of priorities at level 1 are economic, political, and social; those operating at level 2 are mostly social and technical; and those at level 3 are predominantly technical and operational.

While each of these levels retains ultimate responsibility for the decisions appropriate to it, the crucial characteristic of the participatory type of priority-setting procedure (C) is that the communication between hierarchical levels should be maximized in committee and subcommittee sessions, with members from each level free to express opinions on matters falling within the competence of other levels. It is this interchange between, as well as within, levels that constitutes the participatory nature of the type-C procedure. The specialist committees within the priority-setting process should thus be made up in such a way as to facilitate communication across ministries, institutes, and clients.

The formal priority-setting exercise itself will require a limited time, normally anything from six to eighteen months. After this period, the intensive consultations between different levels, characteristic of the exercise, will cease. However, planning and implementation of the new research program will need to be monitored and priorities periodically adjusted in the light of new circumstances. In particular, the short-term planning and evaluation of research will continue at level 3, typically in the form of annual in-house reviews. At these meetings, research proposals are vetted, taking into account the results of previous years' experiments, the views of senior scientists, and the availability of resources and facilities.

Priority-setting methods

In introducing the proposed quantitative methodologies for priority setting, it should be stressed that these have many imperfections, and that it is the systematic thinking which they require, as much as the precise outcome, which matters. Their purpose is to inject more objectivity and clarity into what is usually an intuitive exercise.

Any methodology for research priority setting and resource allocation ideally should be easy to understand, have costs in line with the size of the research program, demand as little data as is reasonable, be compatible with existing widely used priority-setting techniques in other sectors of the planning process, and be capable of achieving as "correct" a ranking for research resource allocation as the imprecisions of the situation allow. The techniques involved must be largely based upon plausible hypotheses, supplemented by whatever relevant data may be available.

There are seven methodologies, of varying degrees of sophistication, which can be used to establish research priorities:

1. Congruence
2. Checklists

3. Scoring
4. Domestic resource cost ratios
5. Benefit/cost analysis
6. Mathematical programming
7. Systems analysis and simulation analysis.

Methods 1, 2, and 3 are the simplest, and are already in experimental use. However, method 1, congruence, is really no more than a starting point in the priority-setting exercise. Methods 2 and 3, checklists and scoring, are more comprehensive. They have much to recommend them, especially for institutes currently using no formal methods whatsoever.

Methods 4, 5, 6, and 7 represent the more quantitative formal methods seldom used in research at present. Method 4, domestic resource cost ratios, is just as complex as method 5, without being as informative. The most informative, but at the same time the most difficult to apply among the more formal approaches, is likely to be method 5, benefit/cost analysis. Because of their high levels of sophistication, data needs, and skill requirements, methods 6 and 7 are unlikely to be widely used by national agricultural research systems in developing countries in the foreseeable future.

Each of the methods 1 through 5 attributes the entire net return of an innovation to the research which brought it about. Other inputs, such as fertilizer and water, are assumed to be available at cost. The justification of this point of view as far as priority setting is concerned is that research findings alone activate these inputs in a new and more productive way.

Given their higher costs and greater demands on staff time, three important points should be borne in mind when applying any of the more formal methods:

- Time and money can be saved by using a common sense approach. Some programs so obviously have low priority that they can be eliminated in an initial screening process. But there may be other, highly desirable programs for which the requisite staff are simply not available. While these programs should be retained as priorities, their implementation must wait until external collaboration has been secured and the necessary recruitment and training completed.*

- Formal methods require more precise data over a wider range of subjects than do informal ones, but there is a trade-off between the accuracy of data and the cost of obtaining it. In the absence of reliable data, it is essential to use hypotheses generated by experienced scientific staff whose objectivity and judgement can be trusted.

- Even when the conditions for applying a formal method are far from ideal, it may still be worthwhile applying it. This is because formal methods impose systematic thinking by a group of people, in a process which can give rise to new insights for all involved. Although the final results of the priority-setting exercise may remain

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* At decision-making level 1, referred to above, research priorities should first be identified without reference to manpower availability. At level 2, however, manpower considerations must come into play, with any severe problems referred back to level 1, where the necessary resources should either be made available or the priorities altered.

imperfect, a better understanding is gained through the exchange of ideas. This will be equally true whether the choice is being made at level 1 or at level 2 discussed above. It is even possible that these methods may be applied to certain research topics at level 3.

The rest of this section outlines methods 1 through 5, briefly discussing the advantages and disadvantages of each.

Method 1: Congruence

Congruence simply means that, assuming other things are equal, total available research funds should be allocated to commodities in the same proportions as their existing contribution to the agricultural domestic product. If, for example, maize represents 10% of the total value added in agriculture, it should get 10% of the resources allocated to research. Congruence can be applied directly only to programs based on commodities.

The congruence concept is expressed by the formula:

$$C = 1 - \sum_{i=1}^{n} (A_i - S_i)^2$$

where A_i is the share of a particular commodity in the research budget, and S_i is the share of that commodity in agricultural value added. Perfect congruence arises when C = 1 for n commodities.

This approach has serious shortcomings. Unless applied to planned rather than current production levels, congruence favors commodities that are already well established, discriminating against new ones and those with low current value. Financial outlays might justifiably be lower than congruence dictates for commodities heavily researched elsewhere, and for those with limited research needs. For these reasons, fund allocation by congruence is not a good general approach, although it is a useful starting point in an allocation exercise, and can be used for checking whether current research is broadly in line with planned development.

As a matter of insurance, few countries would be willing to do without research on their most vital commodities, even if today's research needs were minor and use could be made of results obtained elsewhere. The notion of congruence may therefore be used as a basis for high-level decisions on the retention of a minimum research capacity for the main staple foods and export products.

Method 2: Checklists

Checklists, though the least sophisticated of all the priority-setting techniques, can greatly improve the quality of priority setting at little extra cost. For national research systems which currently rely entirely on historical allocations and personal judgement, use of a checklist is probably the most appropriate initial approach to formal priority setting. The planner uses a list of the criteria and associated questions which he or she must consider and answer in deciding on priorities. The technique is simple to apply, but it does require much understanding of both agricultural research and development.

The usefulness of this approach is closely related to how well the questions are developed and how relevant they are to the matters under review. Most of the questions on checklists revolve around three areas: the impact of research, its cost, and its feasibility.

One question every checklist should raise is whether the scientific manpower for a given program is available. But if appropriate scientific expertise is not immediately available for a particular program, this should not prevent it from being placed on the initial priority list (see footnote on page 10). The program must then be postponed until technical assistance has been secured and recruitment and/or training undertaken. In the meantime, a lower-priority program may be put in its place.

The checklist method does not include any quantification of what is important and what is not, but the experience and perspicacity of the checklist-user may safeguard against major errors.

The checklist concept can be expanded into a "scoring model" by attaching weights and scores to the criteria it lists. In this case special care is needed in choosing criteria. When a more sophisticated technique is used, such as the benefit/cost analysis outlined further on, a checklist can and should still be used to insure against the omission of important considerations. In fact, whatever formal method they use, planners should never wholly abandon the concept of a checklist.

Method 3: Scoring

Scoring methods represent a more sophisticated version of the checklist technique. They have been tried more often than any other formal method for ranking research priorities. The scoring matrix is really no more than a checklist with the answers to questions assigned numerical values and weights. Criteria weights are multiplied by the values which a particular research program merits under each criterion to produce a final score. Programs can then be ranked in order of priority according to their scores.

There are practical problems connected with attempting to rank programs dealing with aspects of the natural resource base along with those on commodities. Whereas the benefits from a commodity research program can be expressed directly, those from research on a factor such as soils can only be derived from assumptions regarding increased productivity over a range of commodities. In some cases even this approach is impractical. Because of these problems, some analysts have chosen to treat commodity research programs differently from factor research programs. But the results do not then inform the decision-maker as to where priorities between, as opposed to within, the two categories of research should lie. In this case, informed discussion between all concerned is the only way to reach a consensus; direct comparison of quantitative results will be extremely difficult.

There is always the possibility that the personal judgements which lie behind the determination of weights and scores may result in misleading conclusions, as is the case, in varying degrees, with the other methodologies dealt with here. For example, decision-makers tend to weight program costs high because of the constraints of the national budget within which they must work. In so doing they may overlook the likelihood that the returns on any reasonably successful research program often dwarf its costs.

When looking at final scores, decision-makers may conclude that they do not accord with common sense. If this happens, they may wish to adjust the values, and sometimes the weights, until a result emerges which is more reasonable to them. Scores may be adjusted either by an individual decision-maker or by group interaction, but preferably by the latter. In either case, the process of adjustment should not be carried too far: results should not be manipulated until they merely reflect existing prejudices.

Scoring has significant advantages over the use of checklists. Not only does it force the research planner to consider all the significant factors which may bear upon prioritization, but it also compels him or her to try to assess their relative importance. The approach is straightforward and can be applied without any special training. As with checklists, data requirements are very modest, but experience and knowledge, both broad and deep, are essential.

Method 4: Domestic resource cost (DRC) ratios

DRC ratios indicate where a country's comparative advantage over other producers in the world market lies. The concept is embodied in a simple formula:

$$\text{DRC ratio} = \frac{A}{B-C}$$

where: A = domestic value added per unit of output of a given commodity,
B = foreign exchange earnings or savings per unit of output,
C = foreign exchange cost of imported inputs per unit of output.

All these values should be expressed in international or border price terms, otherwise the ratio will not reflect the genuine opportunity cost of the resources involved.

If the country in question can produce and deliver to the consumer a unit of a given commodity at lower cost than can other countries, this means that the domestic labor, land, water and capital which go into its production, i.e., its value added (A), will be lower than the unit cost of the equivalent item produced abroad (B), minus the unit cost of the imported inputs which have gone into domestic production (C). A DRC ratio of less than 1 shows a comparative advantage in the production of a commodity, whereas a ratio of more than 1 shows a disadvantage. In using the method it is implied that the lower the DRC ratio, the more resources the country should allocate to research on the commodity in question.

The trouble with this method is that it is usually applied using only single points in time to compare the costs and benefits of different programs. While the method can be adapted to trace these over time, it then requires just as much hypothesizing, data provision and analytical ability as does benefit/cost analysis (see below), without being equally informative.

There are also non-technical reasons why this method cannot be applied without reservation. Any comparative advantage which it reveals may be confined to a certain group of producers only, such as plantation owners rather than poor farmers. Further, where a country attaches much weight to self-sufficiency, DRC ratios cannot be used to set this objective aside, only to question its rationality.

Method 5: Benefit/cost analysis

Benefit/cost analysis is widely used by governments and funding agencies for deciding on investments in development projects, and has the distinct advantage with respect to research that its language is already familiar, and persuasive, where national planners are concerned. It is based on the concept of discounted cash flow: the premise that a dollar earned a year from now is worth less than a dollar earned today, because of the interest-earning potential lost during the intervening period. Clearly, taking into account the effect of time is of major importance when comparing the benefits of one research project with those of another, especially in view of the uncertainty of both the time required to complete the research and the subsequent rate of uptake by farmers.

Reasonably accurate estimates of these parameters are crucial to determining the return on resources invested in research. For example, a delay of three years in completing experimentation, or a reduction of 10% in the rate of adoption by farmers, may reduce cumulative net returns (benefits minus costs) in present-day values by considerably more than would a substantial increase in the cost of the research itself.

Benefit/cost analysis compares the time-valued estimate of the net returns from the results of a research program as farmers adopt them, with the time-valued costs of the research itself. It thus reflects the fact that events occur sequentially, even if their magnitude cannot always be accurately predicted. The changing value of variables over time, and their changing relations to one another, are taken into account.

Different national goals can be weighted, albeit with varying degrees of conviction. Foreign exchange earnings can be valued above the official rate of exchange if foreign currencies are rationed by various exchange and import controls; wages can be priced below their real level in order to reflect the advantages of a technology which uses underemployed labor rather than capital; and so on. But the truth is that there is little difference between the supposedly subjective checklist and scoring methods and benefit/cost analysis when it comes to weighting such variables as the income distribution and nutritional impact of a research program.

The degree of sophistication of benefit/cost analysis may vary widely. It provides a framework for thinking clearly about the impact of a new technology on society at large. This framework can be kept simple, and it can make use of assumptions in place of hard facts, yet still be worthwhile. It comprises, at a minimum, the sequential estimation of eight distinct characteristics of a research program and its impact. They are: the annual research cost, its duration, its probability of success, on-farm implementation costs, resulting benefits, rate of adoption, adoption ceiling, and life of the innovation. These factors must also provide the basic items for checklists and scoring matrices.

As with the other methods, much common sense will be required when applying the benefit/cost method, since there will always be uncertainty surrounding the assumptions underlying the calculations. Some research programs typically have a high risk of failure or may take a long time to reach a successful outcome, but are certain to have a large impact if they do succeed. This would be the case with the discovery of a vaccine against an animal disease, for example. In other cases, for instance in certain plant breeding research, the outcome and time scale can be predicted with some confidence, but the rate of adoption by farmers is very uncertain. Then again, some research will have both an unpredictable outcome and an unpredictable impact.

To deal with uncertainty, the benefit/cost method uses sensitivity tests. A sensitivity test involves varying the value of each component of the benefit/cost analysis separately in order to determine what will happen to the benefit/cost relationship* of the program. It is a way of finding out which factors have important effects and which do not. For instance, a slow rate of adoption by farmers might outweigh virtually all other variations in costs or benefits.

Where the benefits and/or costs of a particular research program are highly speculative, a useful test is to estimate not only the most likely benefit/cost relationship for this program, but also those which are based on optimistic and pessimistic assumptions. If under pessimistic assumptions the results still indicate a high priority for this program, then it would be reasonable to accord it that, in spite of all the uncertainties.

As stated earlier, techniques exist for weighting cost and benefit flows over time in order to take into account certain national policy goals. In several developing countries standard weights have been estimated by central planners for goals such as providing employment or increasing foreign exchange earnings. Applying such weights, once they have been produced, is relatively easy, although it does not always give convincing results. However, as has already been said, for some social goals, such as increased nutrition and the need to compensate ethnic minorities, there are no such quasi-objective weights, and judgements must remain completely subjective. A more useful approach in such cases would be to use comparative benefit/cost analysis to show the price of conducting this research in terms of the present value of opportunities foregone elsewhere.

The concepts of producer and consumer surpluses** are important in any application of cost/benefit analysis for programs developing new technology, for without them it is impossible to predict the distribution of benefits between early adopters, late adopters, and consumers. Estimates of the way in which price will vary with output of the commodities in question are essential for this part of the analysis. Then, too, research outcomes can have international and in-country spillover effects. Benefits overall will therefore exceed purely local gains from outlays on research, and the priority-setting process should recognize this. Where spillover effects can be quantified, a case clearly exists for international or federal funding. If, therefore, the necessary calculations with respect to the distribution of benefits, both within and between nations, can be carried out and some confidence placed in the results, governments and donors will have a picture of what countries, regions, and income groups are likely to benefit from a particular line of research.

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* Expressed as internal rate of return (IRR), benefit/cost ratio (B/C ratio), or net present value (NPV).

** Many consumers would pay more than the market price for the maize they buy, which gives them a consumer surplus, and many producers produce it at a lower cost than the price they receive, which gives them a producer surplus.

Unless these concepts are properly applied, a bias will exist in favor of production for the local market and against research which leads to additional exports. Domestic consumers derive no surplus from exports. Such surpluses arise, however, when the foreign exchange earned from exports is spent on either imported consumer goods or productive inputs. In the first case there will be a consumer surplus; in the second a producer surplus. But tracing the chain of events involved is complicated, and often omitted.

Producer and consumer surplus concepts can also be used to assess the effects of government policy interventions on the impact of agricultural research. For instance, if the government imposes a price ceiling on maize to benefit urban consumers, producer gains from research will be reduced, and the incentive to adopt its results will also be diminished. Again, if maize is exported but foreign exchange is undervalued, local producers will not get as much as they should. On the other hand, if the domestic maize market is protected from foreign competition and prices are above world prices, then producers will benefit at the expense of consumers. Research which increases local maize production as a substitute for imports will then clearly benefit one section of society rather than another. Weights may be used to remove such biases, revealing what would happen if policies were to change.

Priority setting in non-commodity programs

As indicated earlier, there are several major research program categories other than those geared to commodities/products. These are: components of the natural resource base (including zone-specific problems), production inputs, social and economic factors, and farming systems.

The natural resource base

Research into the components of the natural resource base -- soils, water, and climate -- is directed towards increasing or maintaining the productivity of commodities. Research on improving the supply of soil nutrients leads directly to increased commodity production, so that this aspect of soil research can be included in the calculations dealing with commodity research. On the other hand, it may be difficult to attribute the benefits of research on soil classification, soil mapping, or soil erosion directly to commodities.

Formal methods of priority setting are much more difficult to apply to such problems. In the case of soil erosion, for instance, the losses caused by ignoring it are hard to quantify and, in any case, may only reach serious proportions after many years. In these instances, where the emphasis of research is on protecting the environment, the assessment of benefits becomes highly speculative. Here, formal methods can do little more than provide a framework for reasoning, and decisions on priorities have to be taken separately from those for commodities.

Research on water and climate, as well as on soils, is closely connected with a country's regional problems. Low rainfall and poor soils are the main physical characteristics of low-productivity areas. The solution to these problems will involve, among other things, research on water conservation and soil nutrient status. The returns to this regionally oriented research will be expressed in terms of increased commodity yields, but these increases are likely to be much smaller on drylands than in areas with good rainfall or irrigation. On the other hand, the total number of hectares and people in the poorer areas may be much greater, so that the total impact on production of an innovation designed to serve them could be larger. These considerations can all be accommodated in the priority-setting methods described earlier; and provided such factors are taken into account, along with social considerations, regional or zonal research priorities can be dealt with using formal techniques.

Production inputs

Research on the use of certain production inputs, for example fertilizers and pesticides, may be dealt with mainly in conjunction with research on commodities, and to that extent they are amenable to the priority-setting methods dealt with above. Others, like labor, affect whole farm output and can be looked at from the commodity or the systems point of view.

Where assumptions become highly conjectural, comparisons with other lines of research will no longer be reasonable, although the thinking process involved may still serve as a guide. There is no alternative to allowing common sense and experience to determine whether, and to what extent, quantitative priority-setting techniques may be used in such cases.

Social and economic factors

These studies are of a micro- and a macro-nature. Micro-studies pertain to agricultural production units, and concern problems of revenue, savings, investment as a function of farm size, tenure, division of work and of income, and so on. Macro-studies deal with aspects of the farm unit's socioeconomic environment: trade, credit, price, village organizations, technical assistance, and the like. To the extent that costs and benefits of this type of research cannot be easily attributed to specific commodities, priority determination will rely mostly on personal judgement.

Farming systems

In many cases the benefits from systems research can be attributed to commodities, singly or in combination, in which case a benefit/cost relationship (B/C ratio or IRR) can be derived. Where they cannot, the determination of priorities for this type of research will not be amenable to the use of formal methods, and balanced judgement must again be relied upon. In any case, the current view is that if systems research is to have an impact it must be backed up by strong commodity research. Where this is the case, benefit/cost analysis can usually be applied.

Other concerns in priority setting

Strategic research

Strategic research is aimed at understanding specific but difficult problems or mechanisms which stand in the way of achieving a practically oriented goal (e.g. the difficulty of reliably determining available phosphorus as an obstacle to fertilizer work). It is little undertaken at present in developing countries. Such research often has a high potential pay-off, but also a high risk of failure. In the absence of stable long-term funding and highly trained and experienced staff, strategic research should only be contemplated when regional or international collaboration is possible, allowing economies of scale. In such countries, the priority-setting exercise must involve organizations lying beyond the national frontiers.

In the few developing countries where staff and funds are available for strategic research, the question as to whether such research is best handled nationally or internationally should still be subject to benefit/cost analysis. While the benefits to an individual country may be the same in both cases, the cost of gaining them will sometimes be much lower if the research is conducted centrally rather than in each country separately. This argument is also valid for much applied research.

However, the strategic research of today is the applied research of tomorrow. The larger national systems in developing countries might do well to stimulate some research on the borderline between strategic and applied, in order to modernize their methods (e.g., propagation through tissue culture), retain their best national staff, and train future researchers in techniques they will have to use in the future.

Imported technology

Developing countries often seem unaware that research systems in the developed countries, as a matter of policy, often import over 50% of their new technology. Because they have fewer resources, research systems in the developing countries have all the more reason to emulate this approach.

Their reluctance to do so is understandable, given the frequency with which imported technology has proved neither economically nor environmentally appropriate. However, there are many instances of successful importation of technology from abroad. Such technology is not always tailor-made, but the necessary adaptive research has often borne fruit.

Access to information on new technology from other countries can help in the priority-setting exercise. For instance, it might help predict rates of adoption of a new plant variety imported from a neighboring country. Or it might open up altogether new avenues of research ready for application. The issue of imported technology illustrates the need for efficient systems of information exchange as a likely high-priority investment in national research.

Support services

Any research system needs a range of support services, which in many cases are not directly identified with particular lines of research. These can include libraries, laboratories, experiment stations, workshops, and financial and administrative units. Proper operation and maintenance of these take up a considerable part of the institutional budget. It is difficult to set priorities among the various support services, and fund allocation may be better based on informed judgement rather than on the application of a formal method.

Periodic review of the usefulness and cost-effectiveness of support services is essential. It may reveal the need for substantial changes. For instance, technological developments and/or a revision of research priorities may require documentation systems to be upgraded or experiment stations to be closed or created and laboratory space to be converted to different use. The expense or savings of some such changes can be included in the cost of new or modified programs, and this then becomes a factor in their benefit/cost calculation.

Industry-financed research

Research by the agricultural industry itself, primarily for export commodities, is an important part of the system in some developing countries. Such research should be encouraged, because it tends to optimize resource use and to mobilize new sources of funding, even though it also tends to favor some commodities over others (often cash over subsistence crops). The priority-setting exercise at the national level should not normally seek to intervene in setting the agenda of industry-financed research.

The dramatic developments in biotechnology, particularly in genetic engineering, currently taking place within private companies in industrialized countries, will be immune from governmental priority-setting. Nevertheless, research planners in developing countries must give some priority to developing an awareness of these advances and how they may affect the allocation of domestic resources.

Conclusions

Adopting more formal priority-setting methods can help national agricultural research systems in developing countries improve the relevance and pay-off of their research. For those countries which currently have no procedures in place at all, it is perhaps advisable to start by using the checklist method, graduating to scoring when expertise becomes available to help determine weights and values.

Among the more sophisticated quantitative methods, benefit/cost analysis is the most suitable, as it compels the planner to take into account the anticipated sequence of events, and their magnitude. Even where national data bases are not yet adequate, this method can still be used, provided adequate expertise is available to make sensible hypotheses. Use of benefit/cost analysis should lead to greater efforts to collect relevant data, which will then be added to the information base.

Nevertheless, it seems likely that for all practical purposes checklist and scoring methods will continue to be favored by priority-setters in agricultural research. This preference for simplicity is acceptable, so long as the questions asked and the manner of their answering, or the criteria designated and scores afforded, are continually informed by a growing knowledge of the relative importance of the different factors underlying benefits and costs. In this way, the potentially diverse nature of individual judgements can converge, on the basis of an increasingly analytical and factual understanding of what actually happens with respect to different research programs over time.