WORKING PAPER No. 17

PROGRAM FORMULATION IN NATIONAL AGRICULTURAL RESEARCH

Matthew DAGG
Fred HAWORTH

International Service for National Agricultural Research

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October 1988

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ISNAR WORKING PAPERS

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2. They are intended to be an effective vehicle for widening the discussion of continuing work, thereby increasing the quality of the final products. Critical comment is welcomed.

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The series is intended mainly for diffusion of materials produced by ISNAR staff, but it is also available for the publication of documents produced by other institutions, should they wish to take advantage of the opportunity.

PROGRAM FORMULATION IN NATIONAL AGRICULTURAL RESEARCH

Table of Contents

ACKNOWLEDGEMENT

This working paper has been developed from the combined experience of several ISNAR staff and from ISNAR reviews of agricultural research systems in a range of developing countries. The main issues have been discussed in ISNAR Working Groups on Program Formulation and Budgeting, and on Monitoring and Evaluation. The authors are grateful in particular for contributions from R. Devred, D. McLean and P. Marcotte. The diagramatic representation of the agricultural research process with axes of level of management against time arose from a discussion on appropriate indicators for monitoring and evaluating research performance, in which a consultant, R. Kinsey, took part. The substance of the paper was presented and discussed at the Agricultural Research Policy Setting Seminar for West Africa, Yamoussoukru, Côte d'Ivoire, January 1988, organized by the Economic Development Institute of the World Bank, and a subsequent version was reviewed in April 1988 at an Expert Consultation at ISNAR, which included eight senior agricultural research leaders from African countries.

PROGRAM FORMULATION IN NATIONAL AGRICULTURAL RESEARCH

I. INTRODUCTION

A national agricultural research system is concerned with determining a research program, implementing it, and communicating appropriately interpreted conclusions to users of the research outcome, i.e., information and material. People, facilities, and funds are needed to carry out all parts, with their attendant management requirements. Many complex factors enter into deciding what to do and to make these decisions, groups of people are needed at different management levels.

This paper is concerned with the first part of the process, the organization and planning required for determining the research program. This process comes down in the end to determining the annual research program that is to be implemented. Detailed procedures will depend to a considerable extent on the size of the research service and, partly in relation thereto, the time and effort that can be devoted to program planning. However, as the final usefulness and efficiency of all the effort that goes into research implementation is greatly dependent on the quality and relevance of the program chosen, it is worth management and research staff devoting a significant amount of time on the process and procedures which determine that program.

To set the scene in a broad context, a discussion of a generalized national agricultural research process is presented in the next section. This leads to a review of the scope of research program formulation, and it is followed by more detailed discussion of the component steps for assigning broad research priorities at the national level, long-term plans for research institutions (10-15 years); short- to medium-term programs (2-5 years); and the annual research programs (1 season) at the research station level.

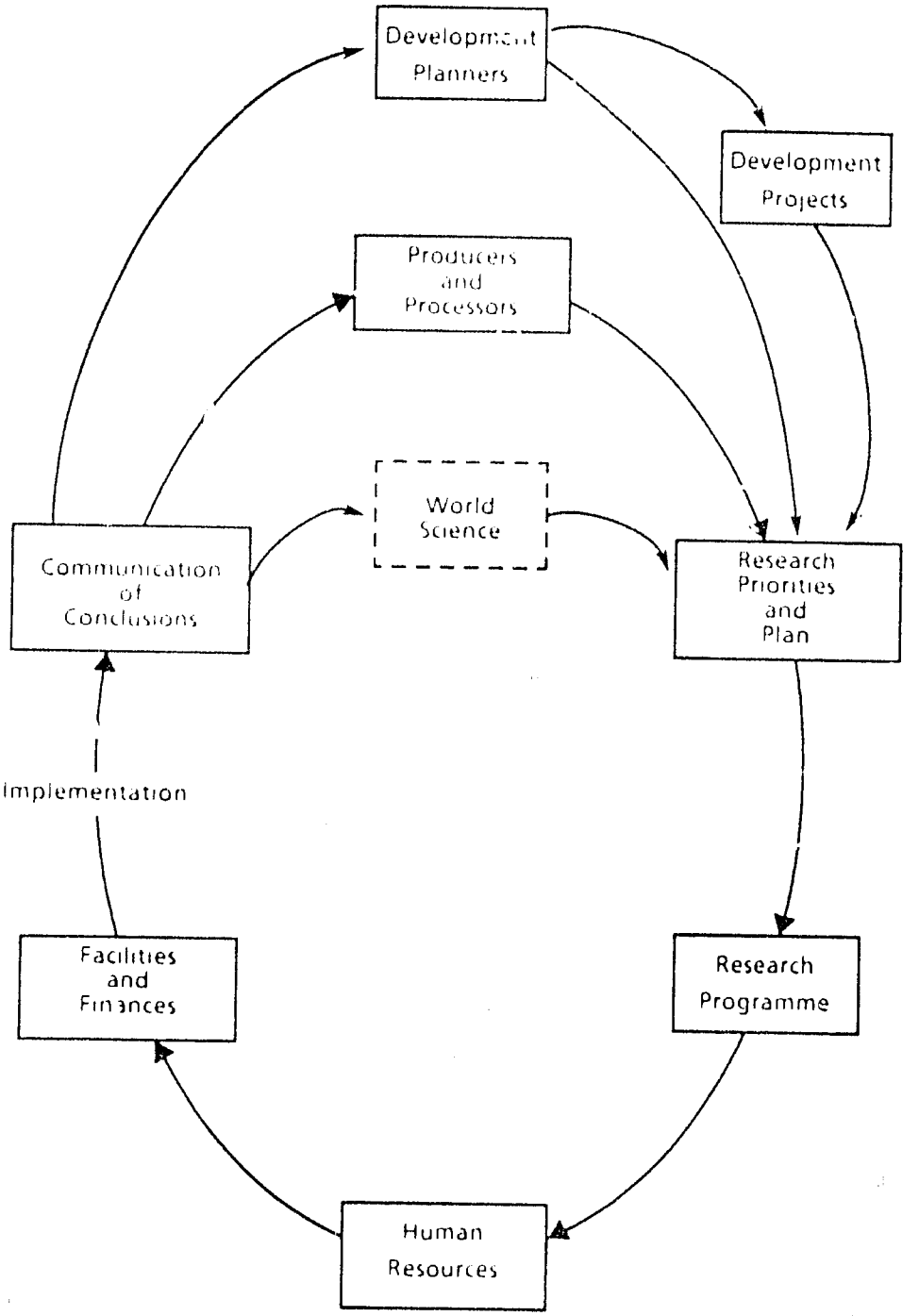
Definition of "program"

Because there are many general uses of the word "program", the particular use in this paper needs some definition. It is the collection or aggregation of the individual experiments, studies, and activities that researchers will carry out in order to obtain the information and materials that are required by clients. (It is not the allocation of funds to various research topics, or the pattern of training for research staff, or the sequence of research station construction, etc.) To clarify the issue, instead of "program", the acronym PRESA will be used in places: Program of Research Experiments, Studies, and Activities. Thus there will be a hierarchical sequence building up from individual researchers' PRESAs to sectional, departmental, station and institutional PRESAs, and finally to the national PRESA.

II. THE NATIONAL AGRICULTURAL RESEARCH PROCESS

The basic elements of a national agricultural research system are illustrated in Figure 1. Problems arising from the constraints and opportunities facing development planners, development agencies, producers, and processors in pursuit of national development objectives lead to the determination of national research goals and priorities.

Figure 1. Elements of National Agricultural Research Systems



Long-term research plans based on these priorities guide the selection and training of the necessary research manpower and provision of major facilities, but the immediate research program, PRESA, for the year ahead is based on what is currently available. Funding for staff and operations is needed for implementation of the program. After completion, results are interpreted in light of world knowledge and other information, and conclusions are communicated to the original clients. There are very many feedback linkages and interactions, and the whole process is iterative in practice.

To focus on the particular sector of planning the program it is useful to consider another representation of the national agricultural research process that is given in Figure 2. It does not pretend to be comprehensive, but it illustrates some important features, especially those concerning the formulation of the research program. Decisions leading to the PRESA have to be taken at various levels of management, and special attention is drawn to the existence of a logical sequence in the decision-making process. The axes are therefore levels of management against time.

The process is divided into three parts: program determination, program implementation, and the interpretation and communication of conclusions. Three stages and levels of decisionmaking are distinguished within program determination:

1. at the policy level: decisions on broad priorities and allocations of resources with respect to commodities, production factors, regions, etc.;

2. at the research institution level: decisions on long-term research plans within a commodity or factor (and at the program leader level: choices concerning medium- to short-term programs);

3. at the research station level: decisions on the choice of experiments and studies in the annual program of work for the coming year.

The output from each of the higher levels is an input to the decision-making process at the next level. Subsequently, when station-level choices have been made, they are the input to a review process back up the sequence to the policy level (see later on page 15 and figure 4). The overall efficiency of the PRESA depends on the effectiveness of decision making at each level. These levels will be discussed in detail later.*

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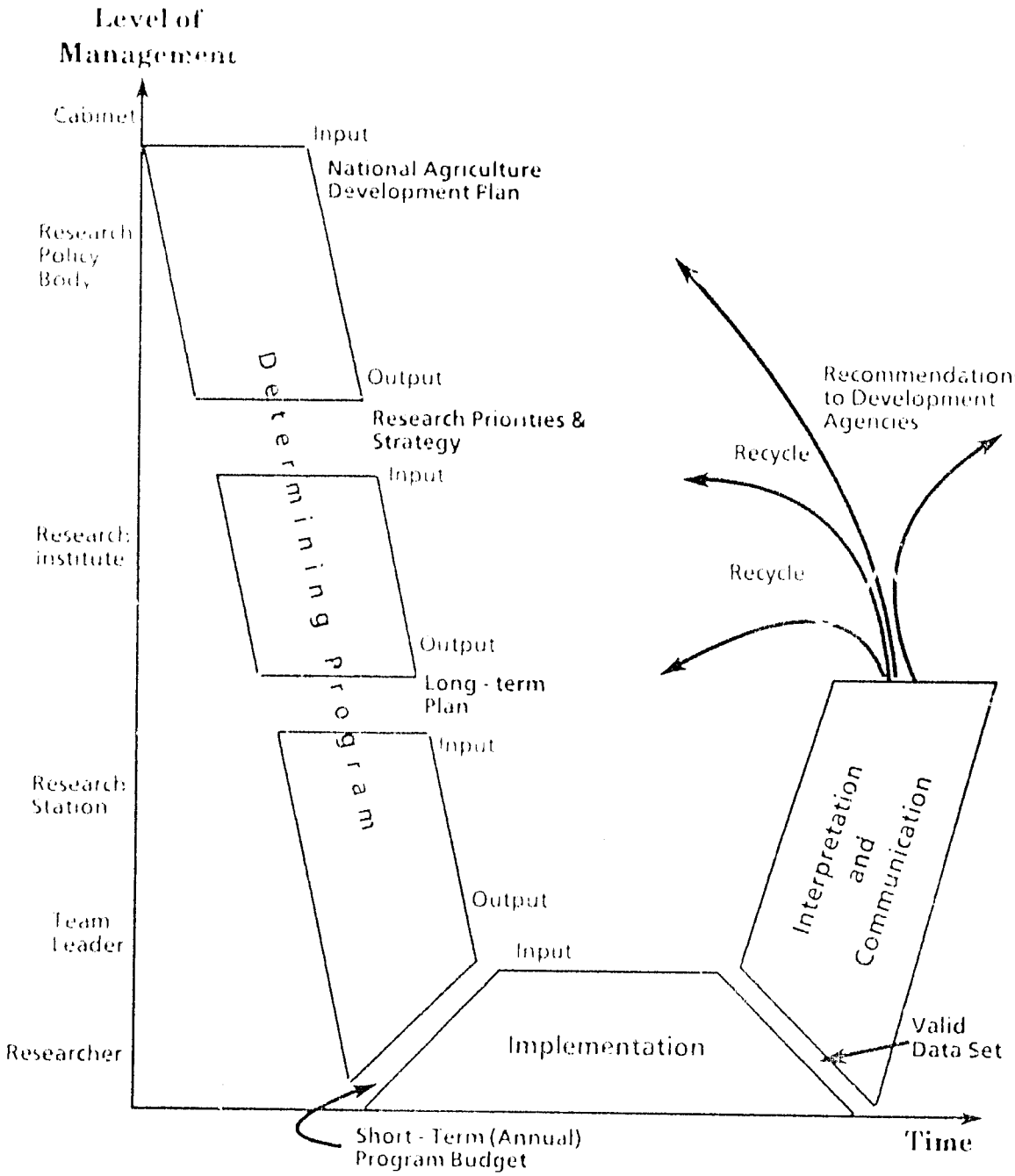
* The representation in Figure 2 continues through the implementation phase (mainly at the researcher level) to the desired product of a valid set of research data for that year, and in those circumstances. Interpretation of these raw data requires other information, notably experience in other years and circumstances, and finally comparison with world knowledge to assess what extra contribution to knowledge may have been made from the year's work. These steps in the process are usually taken at the senior scientist level, and the final decision on what to do with the new knowledge is taken at least at the level of head of a research department. That decision may be that the contribution is ready to be passed on for development application, or that it must be fed back into the planning and programming process, at the most appropriate of the three stages outlined above, to improve the next research program cycle.

Figure 2. The National Agricultural Research Process



III. SCOPE AND UNITY OF PROGRAM FORMULATION

The general process should be similar in all national agricultural research systems. However, the scope of the actual program will vary greatly with the size of the country and the national research system. The minimum intensity of active research on any one commodity (or factor of production, or farming system) is an assignment to keep up with the literature and to spot opportunities or potential solutions to problems concerning that commodity, factor, or system. The next levels of intensity are testing and, if necessary, adapting available technology. Beyond those levels, applied research can be carried out to generate new technology. Finally, if new strategic or basic knowledge is needed to solve otherwise insuperable problems, and this cannot be obtained from elsewhere in the world, the national organization will have to consider arranging for the necessary research to be carried out, inside or outside the country. Each level of intensity implies an increase in investment in research time per commodity, and the possible planned research capability in each commodity (or factor) will depend on the overall scope, based on the total research investment, of the national system. The overall scope will be determined at high policy levels, hopefully guided by advice from technical staff.

Whatever happens at higher levels of planning, the annual research PRESA that is implemented always consists of the aggregation of experiments and studies proposed by individual researchers and their team leaders, and duly approved by senior reviewing groups. It is important to recognize that in the final analysis, this growth of the program is a bottom-up process, and there will always be enough proposals to match the research resources available. Whether or not these proposals are the most relevant to national objectives depends critically on the top-down guidance given to researchers and team leaders on priority areas for research and criteria for choosing alternatives. These issues are extremely important in focusing choices with respect to highly relevant and sensitive experiments and studies, and it is unfortunate that such clear guidance from higher levels of management is often lacking. In such circumstances it is difficult to ensure that the various research proposals, taken together, constitute a package that is even reasonably relevant to national objectives, even if the experiments are technically of a high quality. Primary responsibility for assuring that the national research program is relevant to national needs rests with higher levels of research management, not with the researcher. (However, the researcher must appreciate the relevance of his/her work to the solution of major problems of national development, and must be ready to make important technical advisory contributions to program planning and priority setting at higher levels).

The whole process of program formulation, from (i) national-level priority setting and resource allocation among broad categories via (ii) the setting of priorities and allocating resources for research thrusts within such broad commodity or factor problems in a long-term plan, and (iii) developing short- to medium-term programs (2-5 years) within the long-term commodity and factor plans, to (iv) choice of treatments in experiments, should be one continuous and consistent exercise. The unifying management thread could well be consistency of purpose and value systems that underlie the determination of national development objectives (see later).

These value systems are reflected in the criteria used in assigning priorities at points of decision about the research program.

IV. CRITERIA FOR PRIORITY SETTING

All stages in the program planning process involve setting priorities for choice among alternative activities, and a strong case can be made for adopting formal systematic procedures for setting priorities. The issues involved and the advantages and disadvantages of different methods of priority setting for agricultural research are discussed by R.B. Contant and A. Bottomley (Priority Setting in Agricultural Research. Working Paper No. 10, ISNAR 1988).

The criteria for assigning priorities, as well as the importance afforded to each criterion, change from stage to stage in the research process, and from country to country as their objectives and values change. No attempt is made here to list criteria in detail. However, broad groups of criteria can be discerned:

i. potential impact of research product on the national economy and society: area affected, current value of commodity; demand for commodity; urgency of need; distribution of benefits; scientific prestige to nation; etc.

ii. probability and cost of research success: preliminary information available; trained researchers available; kind of research (increasing probability of success from basic to adaptive research); duration of research before product is available; etc.

iii. feasibility of using research product to increase productivity/production: rate of adoption; efficiency of development services; risk to producers; etc.

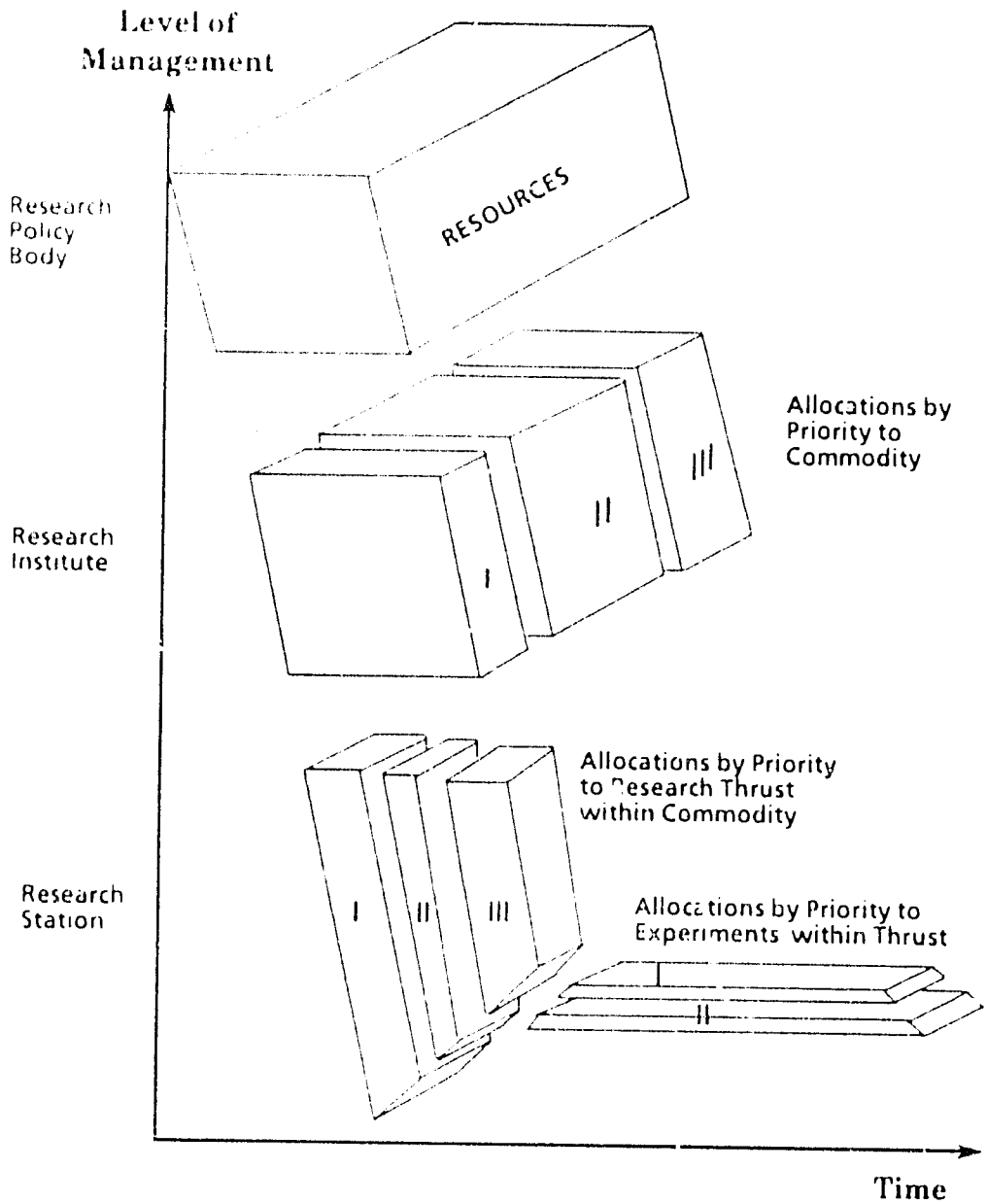
iv. personal satisfaction of researcher (mainly applied at the station level of decision): effect of achievement of product on reward; pursuit of favored discipline in good working conditions; probability of publication internationally; etc.

Quantification

All stages should involve allocating resources in accordance with quantified priorities. Figure 3 illustrates the allocation of resources to commodities or factors, to lines of research within each commodity or factor, and to experiments within each line. This does not mean an absolute equivalence of a share of resources with a numerical priority rating, but simply that the process of priority setting should go beyond terms of high, medium, and low, to an expression in terms of a share of resources.

There are limits on the shares of resources that can be sensibly allocated to any given commodity or area. Allocations should be "packaged" in effective blocks. There is a lower limit below which any allocation would not make an effective research contribution. And even a high-priority commodity in a large research system has an upper limit under a given set of national circumstances, beyond which output per unit

Fig. 3.Sequential Allocations by Priorities in National Agricultural Research Systems



investment sharply declines. Some important commodities have few researchable constraints to productivity and these do not merit large research allocations, but they should have first call on limited resources, if any problem should arise.

Quantification immediately conveys what expectations of results from research may be valid, e.g., if the research in question is to receive two researchers or twenty, or an experiment 1.0 or 0.1 person-years of attention. It is profoundly unhelpful to have a research program listed, as often happens, without an indication of strength.

In most developing countries, the most critical research resource is the research time of well-trained and experienced manpower. This is often the most appropriate parameter for program planning. Funds can also, of course, be a constraint. However, funds are readily transferable, and shortages in finance for priority programs can always be remedied in the relatively short term. But trained researchers cannot be so easily transferred between projects, and generation of a new national scientist or technician (or the importation and acclimatization of a suitable expatriate) takes time: this fact imposes severe limitations on planning and programming. However, it is of great importance to note that operating funds should be commensurate with the number of researchers. If detailed unit cost figures are lacking for precise budgeting, the easiest formula for realistic funding is to calculate and apply a modular rate of funding per researcher to cover emoluments and other personal charges for him and his support staff, research program operating funds, and a pro rata share of station maintenance and administration.

V. LEVEL 1: BROAD PRIORITY SETTING AND RESOURCE ALLOCATION AMONG COMMODITIES AND FACTORS AT THE NATIONAL LEVEL: THE RESEARCH POLICY BODY

In formulating the national agricultural research program, the process begins at the highest political level when development objectives are decided on. The setting of broad priorities for agricultural research in line with those development objectives comes at a somewhat lower level, but should still be at a high policy level. The purpose is to provide broad guidance to national research institutions as to where new information and research efforts are needed. This is ideally expressed as the allocation of research resources among major commodities, production factors, or regional development.

The criteria used for reaching decision on such broad priorities will be largely those reflecting the potential impact of research on the national economy and society (group i), involving macroeconomic analysis and political judgement. These will be modified by consideration of scientific possibilities (group ii) and the capability of development agencies (group iii).

Information from a range of sources should be assembled and digested to provide evidence relating to the political, economic, social, and scientific criteria which may be used in assessing priorities on a national scale. The extent of this activity will depend on the scope of the NARS and the manpower that can be allocated to this task. However, whatever the size, there is a need for a technical staff group or secretariat to gather, process, and present information to a high-level

policy body which has the authority to reach decisions or provide weighty advice to government on research priorities and allocation of resources. The methodologies that can be used by the technical secretariat are discussed by Contant and Bottomley (op. cit.). Policy insight is likely to be brought in through the membership of the body.

The research policy body should be a forum where all facets of agricultural development are appreciated, and where the role research can play in facilitating development is recognized. Its main role is to reach decisions on research priorities and subsequently to check that the resultant national research program and planned resource use are in line with the actual priorities assigned.

There are other roles for the research policy body. It should carry the responsibility for advising the government on a national agricultural research strategy. A strategy involves specifying how to move from the present to a desirable future situation by responding to likely changes in circumstances with planned changes in the research system. The information needed is similar to that required for setting broad research priorities, but it is used to address the future functions and contributions, and therefore the size and form of the research service. The detailed preparation of such a strategic plan is to a large extent the responsibility of the senior staff of the research institution, but the final evaluation and endorsement of the plan to government is a responsibility of the research policy body.

Associated with decisions on strategic plans is the concept of the "planned research capability" for a commodity or factor (as discussed earlier in p.2). Research institutions should propose, and the policy body rule on whether, within the resources available, the planned research capability for a commodity should be at the level of testing, or adaptive research, or adaptive plus applied research, or adaptive plus applied plus strategic research.

Finally, the policy research body should also provide a respected voice with policymakers on behalf of researchers themselves, and a guide to the value system for the national research service which is consistent with national development objectives. Many more tasks may be laid on the policy body, but these are minimum roles that apply to both large and small research systems.

The form and title of a such decision-making body can vary widely (council, board, committee) provided it performs the above essential functions. (Conversely, no matter what additional duties are added to the policy body, they should not obscure these essential functions.) The membership should have a majority of senior and responsible users of research with mature political insight (including suitable representation of farmers' views), supplemented by senior technical and scientific research staff. These last have the important role of interpreting opinions in both directions between the policy body and the research service, but the weight of decision should rest with the users of research. The decision-making body should be relatively small in order to discuss issues efficiently: probably not more than 20 members. Broader representation can be secured on committees advisory to the main body.

In general, the research policy body, in view of its roles, should not be constrained by the bounds of responsibility and interest of any one ministry, and should act as a national advisory body to the finance and

planning ministries, or even the cabinet. However, its precise nature depends very much on the structure of government in a country. It may be within a ministry of agriculture, or a ministry of science and technology, or it may be established as an interministerial autonomous body.

Given the needed seniority and stature of policy body members to have the ear of a high level of government authorities, the policy body itself cannot be expected to do much technical work or to meet frequently. (It is probably counterproductive to have membership of lower seniority that can perhaps afford more time to meet frequently and perform more tasks, but may have less ultimate weight with government.) The hard preparatory work of fact-finding, compiling, analyzing, digesting, consulting and synthesizing will have to be done by the technical staff group servicing the policy body, but which has no authority for decisions in itself. This group would typically have one or several full-time staff supplemented by part-time contributions from senior staff of research institutions.

In the absence of a broad-based research policy body, it can be difficult to reach priority decisions that carry weight up to highest policy and financing levels, and the research system must make the best of other senior groups, perhaps within a ministry, to reach major research policy and priority decisions. The servicing role of the technical staff group remains, regardless of the precise nature of the decision-making body to which it reports.

VI. LEVEL 2: A LONG-TERM RESEARCH PLAN AT THE INSTITUTION LEVEL

Ideally, a research policy body at Level 1 would recommend the allocation of, or itself allocate shares of resources to national research institutions, in accordance with declared national priorities. But practice usually falls short of this ideal, especially when there are several ministries with research institutions which have overlapping mandates and/or gaps between them. However, a research policy body can have a considerable influence on resources allocated.

In any case, each research institution, within its likely allocation of staff and funds per broad priority area (i.e., major commodity, group of commodities, or factor) should prepare a long-term plan of research as a basis for development and operation. Assuming broad priorities and allocations have been set by the research policy body on the basis of the three general groups of criteria outlined earlier, the task of the institution will be how best to use the resources to meet the constraints on, and exploit opportunities for, improvement of productivity with respect to the commodities and factors within its mandate.

The dominant criteria used to assess priorities at this level are those groups concerned with the probability of research success (group ii), and the feasibility of implementing research advances (group iii). The national economic and political aspects should have been incorporated into priority decisions at a higher level, but the urgency of need for technological improvement will still be a very pertinent issue at Level 2. Resource allocations should again be related to trained personnel or expressed in person years.

The most sensitive criteria require information flows on the current capabilities of the research and development agencies; on the constraints to productivity or production; and on technical possibilities of improvement in productive technology. Some of the information will come from documentary surveys, but some will be brought to meetings in the personal experience of members of the reviewing group. Linkages with external and international research organizations are most important to help assess what technology is available, what opportunities for developing new material and techniques there are, and what the probabilities of success might be.

The authoritative group at this level should be a multidisciplinary technical group of senior research staff, supplemented by representatives of extension and other development agencies. Such a group would usually act as advisors to the research institution's director, who would carry final responsibility for decisions. The conclusions and activities of these senior technical advisory groups (one per institution), would provide feedback to the national-level policy body that would influence broad priority setting. Senior decision-making or advisory groups, in some form or another, exist in most research institutions, although the disciplinary breadth is often somewhat narrow, and external membership is limited or absent altogether. The senior group should be broad enough to give good disciplinary spread and representation, but small enough to conduct business efficiently, probably not more than twelve members. The technical assessment of constraints in a commodity may require supporting technical subcommittees chaired by a member of the senior group in a large institution. The station-level committees will be suitable in smaller institutions.

The planning horizon should be fairly long, for several reasons. First, it is at the level of the institution that decisions about future research staff size and disciplinary mix must be made. Such decisions, implying the establishment of a recruitment and specialist training plan, need a 10-15-year perspective. Second, it is at this stage that forward plans for major station facilities, or new stations, or the phasing out of stations must be considered, and these too are decisions requiring a long time span for implementation. Third, decisions on new research lines are also usually long-term, though they may be as short as five years. Difficulties arise in trying to adopt a 15-year perspective when national development planning is on a 5-year basis. Nevertheless, every effort should be made to defend the larger time scale (as this is more realistic for research) while accomodating components of the plan to the shorter development time frame.

These are by no means the only duties of a senior advisory group in a research institution. Routine management of the means of carrying out research is always a pressing concern, and the senior technical and administrative group is an appropriate advisory body for such matters. The long-range planning duties need to be emphasized to avoid the group being overwhelmed by immediate administrative affairs. In particular, the group, and its individual members, should be responsible for ensuring that all researchers are well aware of the main lines of research and that they program their research within these lines. They are also responsible for reviewing the ensuing short-term programs and annual PRESAs, and they must check that these are in line with the long-term plans. But, as a senior technical group, it is not their task to review the detailed design of experiments; this should be done through other review mechanisms in the institution, usually at the research station level.

VII. LEVEL 3: SHORT-TERM AND ANNUAL PROGRAMS AT THE TEAM LEADER AND/OR RESEARCH STATION LEVEL

The long-term plan is heavily concerned with arranging for the means to carry out research programs in the future. Short-term programs are mainly concerned with what specific research work will be carried out with current research staff and facilities over a limited period of two to five years. The short-term program is usually made up of a number of sub-programs and perhaps other subdivisions, which may be called projects, each consisting of several experiments, studies, and activities that may need to be carried through for several years before they are reviewed. Very short-term programs verge on annual PRESAs, and the procedures used to reach decisions are similar in both cases.

The essential task at this stage of the process of program formulation is to reach decisions on the detailed experiments and studies that will finally constitute the national research program when they are aggregated with those from other stations and research institutions in the country. The decisions at Levels 1 and 2 are about planning the means to determine and carry out a program and providing guidance for Level 3 decisions. The actual program (PRESA) begins at Level 3 with decisions on the component elements.

With respect to management, the process of formulating short-term programs falls between the research station and the institution level, depending on the size of the research organization, but it can best be envisaged and planned under leaders of research teams operating in one or several research stations. When a team leader puts forward a project proposal to carry out part of the long-term plan, he or she, in effect, requests permission to do this work as well as requesting the allocation of time and funds with which to do it. But there are many projects that might be included within a long-term plan, and the director will need some assurance about the relevance and quality of the particular proposal--and relative priority ranking against competing uses of resources.

Reviewing Group for proposals at station or researcher level

Only in the smallest research institutions can the director and the most senior staff group give detailed consideration to all research proposals. In most institutions, therefore, it is vital to have grassroots, decentralized groups to carry out the careful reviews required and to report to the central group of senior scientists. Moreover, it is in the nature of research that junior specialist scientists may be more deeply informed on technical details than directors and senior staff, and better suited to discuss the relative quality of different experiments and studies. However, assessing the relevance of sub-programs and experiments is often more difficult than checking on quality.*

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* The quality of an experiment or study refers to the technical incisiveness of the conception and design of the experiment: how readily, reliably, and efficiently it will yield the data targeted. Relevance reflects the urgency and usefulness of the information that the experimental data might lead to. Inevitably, the urgency and usefulness will depend greatly on the need and judgement of particular clients (national planners? farmers? consumers? extension services? fellow scientists?).

Decisions taken within groups composed of a single discipline tend to stress disciplinary values. This is satisfactory when the objective of the national public research system is to make breakthroughs in basic or applied research within these disciplines. This is often the case in industrialized countries with an extensive private-sector research capability to follow up on such progress with adaptive research. In many developing countries, however, private-sector research is minimal, and the public sector must handle the whole spectrum of research. It is therefore highly desirable for each Level 3 reviewing group to have a multidisciplinary membership that reflects the many facets of practical problems of productivity in a commodity, including the socioeconomic limitations within different farming systems.

The reviewing group in this sense should be bottom-level committees made up mainly of the researchers involved in the research on a commodity, group of commodities, or factors of production. The degree of specificity will depend on the size of the research service. Each committee should have a program leader (or coordinator) and about 7-20 members: less than 7 members restricts the spread of disciplines possible, and more than 20 members is getting unwieldy for detailed interactive discussion. Each committee should have at least one or two members of senior staff to give experienced guidance in research design. (But they should be collegial members of the group, not exercising direction from a higher position in the hierarchy.) There should also be members acting on behalf of farmers and extension services. It is usually very difficult to find articulate and representative farmers for this particular task, and it may not be easy to arrange for attendance of suitable extension staff, but it is important that their viewpoint is brought into the decision-making process.

Proposals for experiments and studies should come mainly from the committee members themselves, and be presented for review with respect to quality and relevance. Again, depending on the size of the research organization, proposals might come from individuals, or from small teams, or from small disciplinary groups.

Reviewing groups or committees should be formally constituted by the institution's director, and only proposals that have been recommended by a committee on the basis of both relevance and quality should be passed on to the senior PRESA review group. There they will be considered for inclusion in the institution's overall program, consistent with the long-term plan and within the resources likely to be available.

Documentation

If it is to be reviewed adequately, a research proposal must be well documented by the team leader. This documentation must include, for instance:

* a brief outline of the situation, particularly with respect to client conditions;
* a statement of why the research is needed, supported by whatever evidence there is to indicate the significance of the new work;
* the precise objectives of the research, the expected outputs, quantified where possible, and the relevance of potential results to clients, the research institute, and the country as a whole;

* the plan of work, defining responsibilities of each member of the team, if appropriate;
* cost estimates, including research and support staff time, facilities and recurrent funding required, and the program's estimated duration;
* a reporting schedule, with identification of indicators that might reflect progress.

The program documentation is not only valuable in reviewing program proposals, it is also the basis for any subsequent planning of staff time as well as for monitoring and evaluation of progress.

Criteria for assessing priorities

At the short-term program stage, program formulation should get close to the final specification of those component experiments and studies which are to be implemented. Even after the choices among main commodities and factors, and the choices of major lines of research within each commodity and factor have been made, there is still a wide range of themes, operations, and experiments from which the most relevant and effective must be chosen and priorities assigned. These choices are made at the researcher level, backed by the reviewing group. The methodologies for setting priorities at this level are essentially those discussed in the ISNAR paper on that subject, with a choice being made between checklists, scoring techniques, and benefit/cost analysis, depending on available staff time and analytical capabilities.

The general criteria groups (ii) and (iii) previously listed reflect the technical chances of a successful outcome of the research program, and the chances of applying its conclusions. These remain the dominant groups of criteria at this most disaggregated level of choice and priority setting, and there should be similar kinds of information flow to the group to facilitate decisions. The long-term plan is concerned with ensuring that there are suitably trained staff and facilities to seek solutions to problems. But there may be many kinds of technical solutions to general problems. At the detailed programming stage, there must be more concern that the types of solutions envisaged can be implemented by both the farmers and the development agencies supporting them. These conditions place restrictions on the treatments that are worth considering in experiments, and the parameters worth recording in surveys. Representatives of extension services and proxies for producers on the reviewing group can be effective in encouraging this perspective and in providing information on the farming micro situation. Matching potential new technology to existing farming systems and circumstances can be very important in resource-poor areas, but the reviewing groups should always consider, too, realistic opportunities of changing circumstances to accomodate potentially highly profitable technologies.

Regrettably, researchers sometimes do not show enough concern for the likely applicability of their experimental results. The fault is often in the choice of treatments or even the experimental objective, despite high quality of design.

An additional group of criteria (iv) for the determination of research priorities always enters at the stage of choice of experiments, but it is not considered at earlier stages. This group of criteria involves the personal satisfaction of the researcher. Frequently it is found that the promotional system especially rewards progress in the researcher's

scientific discipline, as judged by publications in international journals. Promotion criteria and their underlying values can profoundly influence the final selection of experiments, treatments, and style of operation, even if they do not relate closely to, or even are in conflict with, the values that have gone into broad, national-level priority setting and into long-term research plans at the institution level.

While recognizing the desire of individual researchers to progress within their specialized fields, management must ensure that this wish is balanced with an appropriate research contribution to the development process. The reward system should reflect such a due balance and emphasis. The research policy body discussed earlier is probably the most appropriate body to rule on the balance of values to be followed consistently throughout the system. It can be very detrimental to the formulation of an effective research program if there is, de facto, a drastic change in the values guiding priority decisions at this last stage, that of choosing experiments.

While the policy body may be best to give policy guidelines on the balance of values, it is the senior management of the research institutions that must interpret them into criteria for assessing desirable research productivity that merits accelerated promotion or other reward in recognition of better-than-average performance.

Annual work program and plan

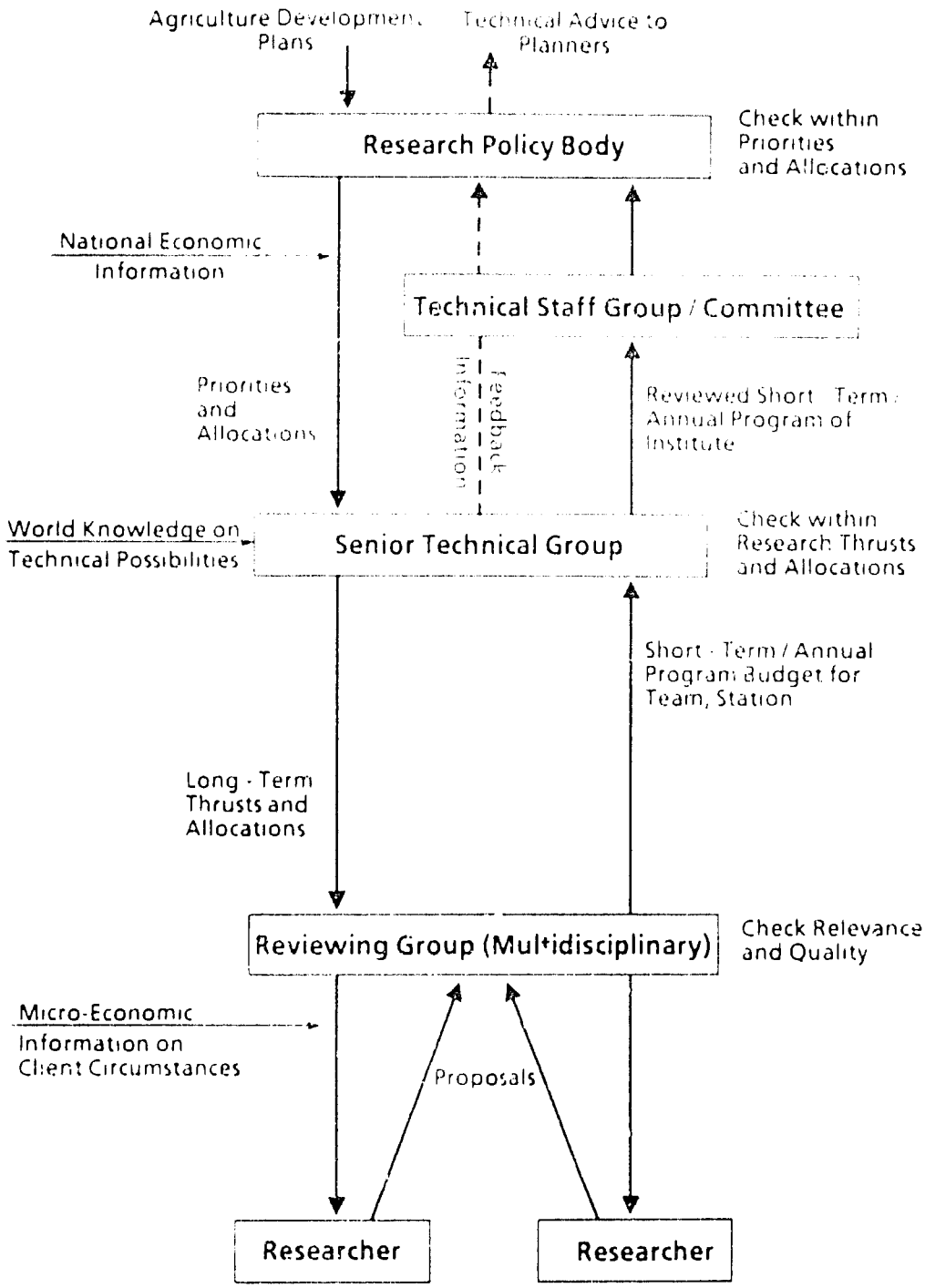
A short-term research PRESA cannot be precise: the unpredictable results of current experiments will often dictate the need for changes in subsequent experiments. There must always be some flexibility. Furthermore, even when there is agreement at the policy level to committing resources to a three- or five-year program, the reality in most countries is that budgets are formulated on an annual basis, and that funds allocated fluctuate. The annual work program is therefore much the firmest statement possible of the aggregate of experiments and studies to be carried out during the year with the capital resources and budget available. Even then, a contingency element is desirable to allow for sudden and unexpected demands for very short-term studies or new starts.

Review of program

The overall work plan for the country as a whole derives from proposals from individual researchers, aggregated at section, department, station, and institution levels. Individual proposals for the annual component of short-term programs, properly documented, should again be carefully scrutinized at the level 3 review group for relevance and quality, using the same criteria as for short-term programming. They should then be assessed for long-term program consistency and trimmed to budget availability by reviewing groups at increasingly higher levels of management up to the policy body. If no guiding plans have been made previously, then any review will be either perfunctory and arbitrary, or belated attempts will be made to try to apply macro-priority criteria in discussions on very micro-experimental proposals.

A smooth, desirable sequence of planning and programming is illustrated in Figure 4, with each decision body, group, or committee restricting itself to its particular area of jurisdiction.

Figure 4. Determining Agricultural Research Program: Planning and Review



It should be noted that each group has a dual role, that of proposing and planning research activities, and of reviewing and evaluating the program (PRESA) that has arisen from the proposals. But for the higher Levels 1 and 2, these roles are separated in time. It is important that this distinction is recognized and made explicit (e.g., operate as different subcommittees of the same body). Essentially, the bases for comparison in the review process are the guidelines laid down in the planning process: without the first step it is difficult to find grounds for judgement in the subsequent stage.

Budget

The preparation of a budget is an integral part of programming, and this process will be described in detail in a companion working paper.

Budgeting by researchers for annual activities can be more precise than at any other time. Operations can be costed and operational budgets and staff time requirements may be allocated to each component proposal. Individual returns aggregated to full program budgets for section, station, and institution can constitute powerful management tools at each level. If the documentation has been completed properly (and this is little planning burden for an individual researcher, who will spend most of the rest of the year implementing the program proposed), this should enable the director, program leader, or department head to know what experiments each member of staff is involved in, and to what extent and what the total resources needed are likely to be.

It is possible, with some analytical effort, for managers to use the records in many ways: for instance, in assessing allocation of funds and research staff time to different research stations, different regions, different commodities, and the like. Although this does not particularly help further in financial control within existing budget heads, it does give a greater flexibility in reviewing accounts by alternative headings.

Program budgeting also:

* helps maintain a realistic ratio of operating costs to salary;
* permits the making of a case for realistic funding from the program elements;
* makes the program more specific with regard to people and cost by module;
* helps guide the effective use of funds by specific objectives;
* provides a base for monitoring physical and financial progress.

This system is valuable when carried out by normal clerical methods, but it is a laborious exercise to recast budgets under a series of different headings. Program budgeting is a much more powerful tool if the information recorded is coded and the process computerized. Management can then call up information by groups and sub-groups at a speed that makes program management more flexible and efficient.

VIII. ADJUSTMENTS TO COUNTRY SITUATIONS

The paper has discussed an ideal situation in general terms concerning principles and logic to be applied in the process of program formulation. In any country, the specific circumstances of size, scope, history, and form of governance will have a great influence on what

precise forms of management mechanisms are possible. The names of decision-making groups may vary widely from country to country. The smallest countries may need to draw scientists from different departments or ministries to form "grass roots" reviewing groups of sufficient multidisciplinary composition. Larger countries may have several effectively independent research institutions within each of several ministries, and each institution may have several reviewing groups. In some countries it may be possible to introduce effective management mechanisms within existing institutional structures. In others, some basic structural changes may be essential to accomodate key principles of program formulation.

However, at the smallest unit (beyond the individual researcher) of the multidisciplinary reviewing group at the grass roots level of researchers in a research station, there is a wide degree of commonality across countries, even though some larger stations may have several reviewing groups, and others only one, or participate in a joint inter-station committee. The most frequent shortcomings encountered at this level are:

a) a lack of guidance on value systems and priority research thrusts to be addressed, and therefore researchers have to start from scratch; and
b) the basic group is uni-disciplinary.

The "senior technical group" in a research institution can have many forms, but most research institutions have such groups. Some larger institutes or organizations may have several effective "senior technical groups" reporting to a more senior coordinating group (which has a somewhat different mix of functions). There are two major practical points to note in their role in program formulation:

a) It is important to distinguish their roles as planners and evaluators. In the planning role, they must interpret broad priority guidance from above into priority research thrusts for the guidance of their reviewing group downstream; and they must provide technical insight and information to the senior research policy body, to help in their decisions on policy issues. In the evaluator role, at a different time, they must examine research program proposals coming from reviewing committees in relation to the research thrust guidelines and availability of resources.
b) Senior technical groups are usually concerned with administrative management aspects of resources (personnel, finance, physical facilities) as well as the research program. They must ensure that frequent short-term hierarchical administrative issues do not completely overwhelm the longer-term collegial programming issues, and that due time is allocated to the latter.

The variety of forms of research policy bodies at the highest level is widest of all, because the pattern of political governance of countries is a major influence at this stage. In some countries there is no recognizable authority for taking decisions on research policy and priorities; in others the appropriate body is poorly serviced by the technical group and does not have suitable information for decision making. Frequently, there are several authoritative bodies (e.g., planning, science and technology) that have jurisdiction over policy issues and resource allocation that impinge an agricultural research, but none are wholly responsible for agricultural research.

The research managers should strive to see that a high-level policy body (or a joint sub-group of other bodies) is established that will serve as a disinterested agency on their behalf at the highest government levels, in addition to reaching conclusions on broad research priorities on the basis of information supplied to it. A "technical secretariat" of one form or another is an essential adjunct to a research policy body.

One important variant that is relatively common is where there are several semiautonomous research institutes with their own policy "boards", concerning a limited number of commodities or factors (e.g., tea, soils, savannah). They play an intermediate role between a national policy body and the senior technical group. They already have a restricted mandate within national priorities, but they usually have a strong "research-user" membership that plays an important part in identifying major constraints to productivity, and in evaluating the institute performance from a users' point of view. They do not substitute for a research policy body with a fully national perspective.

IX. CONCLUSION

The objective of this paper was to highlight the importance of annual program formulation at the research station level; i.e., the need to get the program right before implementing it.

However, annual program formulation is part of an iterative process which involves policymakers who must establish the broad priorities from time to time, senior scientists who must say what is technically feasible and economically reasonable for long-term research plans, and researchers who must each year make proposals of which detailed experiments and studies shall be done first within the long-term plan. It has, therefore, both top-down and bottom-up aspects.

The paper outlined the various types of decisions that have to be made at the national, institute, and station levels, and stressed that appropriate authoritative bodies must be organized at each stage to reach decisions on pertinent information supplied, and on suitable criteria for each stage. While the incisive quality of component experiments and studies of the national program depends on the creativity, training, and experience of individual researchers, the relevance of the experiments to national objectives and to producers is the primary responsibility of senior management, who must ensure appropriate guidance to researchers before the choices of experiments are made. As in any planning process, the essence is coherence among objectives, structures, and resources. In the case of research, which depends critically on the quality and motivation of the scientists themselves, there must be a coherence between the national objectives of research and the criteria for rewards the scientists receive for pursuing those objectives.

The most critical resource for research is trained manpower. Allocation of resources based on priority assessment should be in terms of person-months of trained manpower, which therefore constitutes the key parameter for planning.