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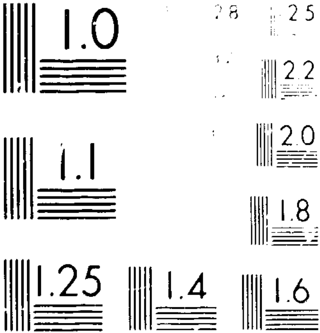
Vienna International Centre, P.O. Box 300, 1400 Vienna, Austria
Tel: (+43-1) 26026-0 · www.unido.org · unido@unido.org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ALLOCATION OF RESOURCES FOR SCIENCE AND TECHNOLOGY IN LESS DEVELOPED COUNTRIES:
Creation of capacity and use of installed capacity*

by

Alberto Aráoz*
Buenos Aires, Argentina

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* The views and opinions expressed in this paper are those of the author and do not necessarily reflect the views of the Secretariat of UNIDO.

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Introduction

The joint UNDP/UNIDO Staff Study on Industrial Research and Service Institutes (IRSIs), published as document ID/B/C.3/86, touched on the need to distinguish between resources applied to the creation of new capacity in science and technology from those that support current activities. The need for science and technology policy and planning was also stressed.

The author of this paper, a World Bank consultant and S+T authority in his own country, has developed a "utility-efficiency" approach to the choice of research and development projects - which will be of particular interest to IRSI sponsors, managers, and those who provide technical assistance to such institutions - under a typology of different situations.

Secretariat and field staff who are concerned with science and technology planning and, in particular, with the creation or strengthening of industrial institutes which are involved in research and development, will find this paper stimulating and useful.

Raymond E. Kitchell
Senior Evaluation Officer
UNIDO

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On allocating resources for science and technology purposes in less developed countries it is important to distinguish resources applied to the installation of new capacity in S+T from those that support current S+T activities.

A parallel may be made with economic activities in general, where it is normal to consider investment and production as separate categories for the allocation of resources. In discussions about decisions on S+T matters, and about S+T policy and planning, both categories are often confused, perhaps because it is felt that expenditure in R+D - and sometimes in other S+T activities - is truly an investment that will bear fruit sometime in the future.

The installation of capacity in S+T is related to the formation of a science and technology base or infrastructure: the creation and building up of institutions, the education, training and perfecting of human resources (scientists, technologists, technicians), the development of a network of information and communication and of links with other social systems. These are structural aspects, to which true investment resources should be allocated.

The installed capacity in S+T should be utilized and put to work, according to demands and priorities, through programs, projects, and scheduled activities. Current operating resources are employed, usually from budgetary sources which are sometimes complemented by special State funds.

S+T policy and planning should not confuse these two aspects; even though certain complex programs may combine both types of use of resources, each of them should be evaluated according to different guidelines. The problem in many less developed countries is still principally structural: how to get S+T capacity installed. This has often taken place in a haphazard way, guided by intuition, pressure groups and the imitation of S+T development patterns elsewhere. It is our belief that some rationality may be introduced into this process.

We shall not deal here with S+T investments of a general nature, such as those involved in university and technical training of human resources, or in the setting up of general facilities such as an information system. Our attention will be centered on institutions that have as their primary purpose the carrying out of R+D and other S+T activities.

The set of such institutions would make up the "S+T infrastructure" or "system". In many developing countries, S+T systems are still weak, inefficient, poorly connected with the productive system, and largely marginal to development needs. It may be suggested that among the principal objectives of S+T policy and planning are the expansion of the S+T system according to long-term national needs, the orientation of activities toward the needs of production and government, and the increase of efficiency through better organization of activities and other means.

A simple scheme to analyze our subject-matter would be the following. A country is interested in having installed capacity in science and technology in a number of major fields such as agriculture, manufacturing industry, housing, petroleum and petrochemicals, natural sciences, social sciences, etc. Within each major field, specific areas or <u>axes</u> may be identified as particularly relevant for the achievement of the country's

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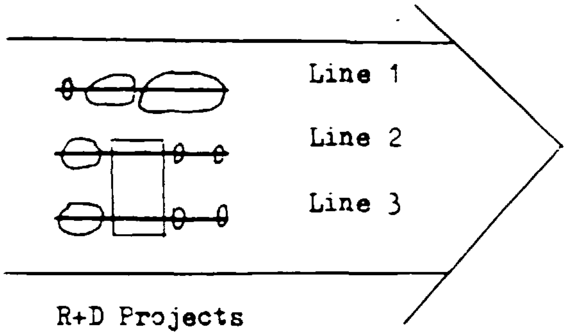
objectives. Within an axis, lines of work may be identified. Once there exists scientific and technological capacity (human and material resources installed in axes and their lines) this provides certain outputs:

a) New knowledge, by means of research and development <u>projects</u>, undertaken by people working along a line (several lines of the same or different axes may collaborate in a single, complex project);

b) Diverse scientific and technological <u>services</u>, such as testing, surveys, quality control, troubleshooting, feasibility studies, etc., which rearrange existing knowledge and transmit it to users.



R+D Projects

<u>Example</u>:

Axis: Food Technology

Line 1: Oils and fats
Line 2: Milk
Line 3: Meat

The installation of capacity in an axis is a long-term proposition; once resources are transformed into installed capacity, it is not at all easy to convert them over to a different axis. The discontinuation of existing lines, and creation of new lines within an existing axis, are medium-term tasks; periodic reviews are required, particularly when a development plan is laid out. Decisions about R+D <u>projects</u> are short-term. In the case of R+D projects financed from central funds this may be done annually by the agency in charge. Existing projects may be discontinued and new ones may be quickly started where the corresponding axis and lines already exist. Scientific and technological <u>services</u> are tied to demand, and their provision would normally be a decision of the S+T institution.

Major fields, axes and lines in which new capacity is to be installed, or existing capacity considerably reinforced, should be identified through a planning exercise that may be done concurrently with the preparation of an economic and social development plan. Long-term requirements may be compared with existing capacity to find out the axes and lines where existing capacity should be strongly reinforced or new capacity created. It should be remembered, though, that decisions about the creation of new scientific and technological capacity should take into account a horizon of time that goes beyond that of a five year planning period. When capacity is installed in an axis and its lines, resources are committed for investment, in persons and physical installations, which may require several years before being completed. To this should be added a period of time, variable according to the nature of the axis, during which the institution consolidates and grows in depth, acquiring

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new knowledge and experience, until it reaches a situation of "steady-state" in which it is able to produce efficiently the type of outputs it was originally designed to produce. The time of installation and maturation may not be a short one (for instance, the Metallurgical Laboratory of the Atomic Energy Commission of Argentina took about 10 years) and although in the meantime the developing institution may provide new knowledge and services, much of its energies are tied up with its growth and the improvement of its intellectual quality.

It would seem important to make these things explicit to policy makers so that they do not entertain expectations that cannot be satisfied in the short run. Investment in science is not quite like investment in industry or in economic infrastructure, where output depends principally on physical assets and equipment and where human resources required to run new installations are usually obtained without much difficulty from the labour market and are able to do a good job after a relatively short period of training. In the case of science, output depends principally on the number and quality of human resources, whilst buildings and equipment are more of a permissive factor. It takes much time to develop human resources from the usual B. Sc. level with only a general type of training to the type of researcher that will produce good results. Moreover, a collection of mediocre scientists will only produce mediocre results, so that an eye must be kept on scientific excellence. The fact that scientific institutions are basically made up of mean means that they are fragile and vulnerable, and the record shows how easily such an institution may be destroyed, though the loss of its top scientists, as compared to the long time it has taken to build it up.

Installing S+T capacity

A first task in S+T planning is the establishment of capacity in human and physical resources in axes that are considered to be of high priority. The planning of investment in S+T may be expressed in a set of investment projects to be chosen from a number of candidate projects to be gradually implemented along several years, according to the possibilities of allocating resources for such purpose. Investment projects may refer to new S+T units or institutions, or to structural expansion of existing ones.

The usual cost-benefit techniques are of difficult application here. The benefits of producing knowledge are not easy to estimate, and on the other hand, when an investment project in S+T is being considered, it is usually not possible to define with precision the types of knowledge that will be produced when the investment matures, since this would imply that it is known which research projects will be undertaken X years hence. This double uncertainty would seem to preclude the use of a quantitative benefit-cost approach. The present author has suggested that a primarily qualitative approach may be employed to guide decisions in this situation. The priority of an investment project will depend on its expected utility for the achievement of national objectives and on the expected efficiency in carrying out its activities. The approach - which has been termed "utility-efficiency" approach - may be applied

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through expert committees, though a sequential mechanism. 1/

The evaluation of utility, in the case of axes of applied science (installation of new capacity or structural expansion of already existing capacity), should consider how desirable future scientific and technological work in such axes promises to be. Thus, they should show relevance to national objectives, particularly those pertaining to social and economic development in the long run; they should be installed to deal with problems in which there is an assurance that useful results may be forthcoming in not too long a period, in order to discourage "technological adventures" that may be left for richer countries.

In the case of basic science, the country should attempt to cover a large part of the scientific spectrum in order that access is had to what goes on in the scientific world and that a good level of education is imparted to young people. Normally, it is to be expected that such a coverage of the science spectrum would be attained in institutions of higher education. If a survey of installed capacity in basic science showed large gaps in the coverage of that spectrum, there would be *prima facie* strong reasons for filling them up, probably through the creation of professorships or university institutes in the missing areas. 2/

Should there exist adequate coverage of the spectrum, there may be a need for reinforcing capacity in basic science in a certain field if this should be necessary to produce scientific inputs which are required by an applied axis. Such an investment would be justified through its indirect relevance to national objectives. A good example in Argentina is basic research in plant nutrition, important for agronomical research on the use of fertilizers and other related subjects.

As regards efficiency, investment projects in axes that show high utility should be carefully designed so as to assure that the conditions exist for a high level of efficiency in their future activities. Among the various items that should be checked we may briefly mention: the calibre of the person who will lead the scientific group; the adequate size and structure of this group; a good program for the training of the scientists that are to be incorporated; adequate buildings and equipment, taking into account that first priority is human resources; and last, but not least, an annual operating budget that will permit peace of mind and smooth operation at least during the maturation period. It is not worthwhile to set up a new group or institution in science and technology if conditions are not right for good scientific productivity.

1/ See A. Araoz, *Evaluation of Investment Projects in Science and Technology Infrastructure: The Utility-Efficiency Approach and its Application to Uruguay*, Science and Technology Report No. 28, The World Bank, Washington D.C., 1977.

2/ One of the advantages of a close cooperation between small countries (such as those in Central America or in the Caribbean) is that through a common S+T policy at the sub-regional level it may be possible to complement national efforts to attain a complete spectrum. A good example is the University of the West Indies, with four campuses in four English-speaking Caribbean countries, which between them cover the scientific fields of more relevance for those countries.

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The building of S+T capacity is a gradual process which should be carefully planned many years ahead. Two observations may be made. First, the bottleneck for such an expansion lies in human resources, and this underlines the importance of a good coordination between S+T policy and educational policy; it is desirable to produce able and creative scientists and engineers at home in strong academic institutions rather than to depend on training overseas. Second, insistance on "utility" should not play down the role of basic research. Basic research of a good level of excellence is crucial for producing competent researchers and professionals, it provides standards of quality for applied research, it supplies applied research with much-needed inputs in the form of required new knowledge or just plain competent advice, and it opens a window to the outer world of science without which local scientific activity may fall behind, may be unaware of new developments which are useful for its activities, or may be researching on topics already explored somewhere else.

Orienting the activities of the S+T system

We have remarked already that capacity in science and technology should be installed with the purpose of producing (a) new knowledge through research and development; (b) a flow of scientific and technical services; and (c) a contribution to the quality of higher education. The latter is a natural result of introducing basic research in universities and we shall not deal with this question any further. The provision of services, such as consulting, surveys, testing, analysis, quality control, trouble-shooting, computing, scientific and technical information, etc., would in principle respond to the demand of various sectors in government and industry. Such a demand shows stability in time and tends to guide within each S+T institution the allocation of resources to the different types of services. The S+T system may take an active role by searching for customers to employ these services, and this may be helped by central government policies through persuasion, publicity and a subsidy to the cost of such services.

With regard to research and development projects, it is useful to distinguish three cases. The first, academic research, generally of the basic type, is directed towards supporting educational activities and adding to basic knowledge. The choice of topics to be researched is usually in the hands of the researchers themselves, and thus we may speak of supply-oriented research. It may be possible to bring to the attension of those engaged in this type of research certain issues that are worthy of study, with the idea of transforming "non-oriented" into "oriented" basic research, the results of which may prove to be an important knowledge input for further applied research. This would require a good degree of communication between the scientists and persons in planning and other areas of government.

The second type is applied research under contract, executed for a customer that determines the objectives to be pursued. In this case there is in principle an assurance that results are wanted and hence that the research project has a high expected utility. The choice of

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topics of research is determined by the market and we may then speak about demand-oriented research.

There is a third type of research carried out in government-funded institutions which is not strictly related to education and is not covered by a contract with specific purposes. This takes place in university and non-university institutes, with the support of normal budget funds or of special government grants distributed by the National Research Council or a similar organization. It accounts for the major share of all research and development in some countries. The topics are in many cases decided upon by the researcher himself or by his institution, and it is not unusual that results do not find application, or that money is spent on irrelevant topics or frittered away on what may be called "scientific hobby".

There is concern about such a situation; resources are scarce and they should be spent properly; value should be had for money. It may be possible to convert part of this research to contract research through an active sales programme, assisted by policies in favour of demand. But there are areas and topics in which this is not easy, and then the idea is to convert it into what may be called requirements-oriented research. To do so, research funds should be carefully allocated through methods of project selection which would assign relative priorities in accordance with socio-economic requirements. Ideally, there should exist compatibility between the methods followed for the selection of projects to be supported by central funds and those employed by each institution for deciding on its own projects. Though in some cases a benefit-cost approach may be used, this is not generally easy to employ on account of the difficulty of predicting R+D costs and particularly of estimating the benefits of R+D results. A variant of the "utility-efficiency" approach may perhaps be used to guide such decisions, based on the following considerations: the product of a research project must be desirable, i.e., it should promise a high social utility, and the project must be designed, organized and carried out in such a way that high efficiency is shown as well as a high probability of attaining the desired results within time and budgetary limits. The Annex expands further on this matter.

Implications for S+T planning in different national situations

We have suggested that S+T planning should contemplate on the one hand the creation of S+T capacity and on the other hand the use of installed S+T capacity. The emphasis to be put on either aspect will depend on the stage of S+T development in the country considered. Developed countries already count with an established S+T infrastructure, and it would seem that the principal role of S+T planning is to put it to work according to national needs and objectives. This type of action may be called "marginal", constrasting with "structural" action which would be the main business of S+T planning in developing countries where the S+T infrastructure is still to be built.

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A typology of different situations may be suggested:

Type of country	Type of S+T planning
1. Developing country with little S+T infrastructure (Ecuador, Nigeria)	Mainly structural - develop human resources, create S+T capacity in carefully selected axes and lines.
2. Developing country with S+T infrastructure not fully developed (Egypt, Brazil)	Predominance of structural over marginal.
3. Developing country with fairly developed S+T infrastructure (India, Argentina)	Predominance of marginal over structural.
4. Developed country (France)	Mainly marginal - what projects in which lines of existing axes.

The time horizon of planning, as we have suggested, is longer in the structural case than in the marginal case. In countries with a weak S+T infrastructure, an important part of the total resources assigned to S+T would be devoted to investment, according to long-range needs and requirements which usually do not clearly come out of a medium-range development plan. This plan, however, may be used to give many useful indications for orienting the activities of an established S+T infrastructure. 1/

One of the consequences that follow from this analysis is that the S+T policy and planning experience of developed countries is only partly relevant for developing countries. In the former countries the business of S+T policy is principally to orient the use of installed capacity where the environment is ready and willing to apply new results, productive units have attained good levels of technological development, S+T services exist in a wide spectrum and are efficiently produced and employed, there is a self-sustaining scientific community with solid traditions and good performance levels, and - very importantly - foreign technology inputs are not the overwhelming influence on the technical progress of the modern sectors. Under such conditions S+T planning becomes in practice the planning of research and development. Those conditions do not exist as yet in developing countries, or exist in an embrionary state, so that the role of S+T planning goes far beyond that of planning R+D. It is clearly seen that these countries have to look for their own solutions in this field and cannot rely much on the experience or the advice originating in the former countries.

1/ The actual instances of S+T planning in Brazil and India would exemplify such situations. In Brazil, the current S+T plan puts much emphasis on building up the S+T infrastrcute. The S+T plan prepared in India in 1974 puts emphasis instead on the utilization of the existing S+T infrastructure, which is already of a considerable size.

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ANNEX

Choice of research projects: the utility-efficiency approach.

The appraisal of research and development projects under this approach takes into account two main parameters: utility and efficiency. Cost-benefit methods are taken to be largely inapplicable as a guide to decisions about the allocation of resources to R+D projects. Let us see why.

Various methods and formulae based on the economic cost-benefit approach have been proposed in the literature for the appraisal of research and development projects in industry and government. Results, however, have been disappointing. Other than the general problems that appear in cost-benefit, there are additional complications when the subject of the exercise is R+D. Cost estimations, apparently straightforward, have proven to be very unreliable when examined ex-post, large overruns are the norm. Time estimates in many cases have proven to be too optimistic. On the other hand, the estimation of benefits presents difficult problems, particularly in those cases in which results are not readily employable by prospective users. The connection between new knowledge, the "product" of a research project (itself subjectived to considerable uncertainty), and its economic value is not easy to determine and in the case of basic science it is well-nigh impossible. Prest and Turvey, in their survey of cost-benefit analysis, point out two major hurdles for the estimation of costs and benefits of R+D: "the impression of insecurity in cost estimation of research programmes and the extraordinarily complex nature of the benefits resulting from them".

In view of the dubious applicability of cost-benefit analysis, an alternative approach to the appraisal of R+D projects is suggested. The approach is of a qualitative nature, and takes into explicit consideration a number of non-economic factors, including those that refer to scientific aspects. On the other hand, it offers hopes that its application may be done quickly and cheaply, once the appraisal mechanism has been set up.

The idea behind the appraach is simple. The product of a requirements-oriented research project should be desirable, i.e. we should expect it to show a high utility for society; and there should be favourable conditions for its production, i.e. we should expect the research project to be designed, organized and carried out in such a way that a high efficiency is shown for maximum probability of obtaining the desired results within budgetary and time forecasts. The higher the utility promised, and the higher the efficiency expected, the higher the degree of priority to be assigned to the research project under scrutiny.

Such a viewpoint, of course, is not a novelty. It has been said by an Icelandic official that "to evaluate R+D projects we need scientific advice to tell us whether the project is scientifically or technically sound, and social and economic advice that will say whether the project is socially or economically worthwhile". At the other end of the spectrum the main criteria employed by U.S. Government funding agencies to select

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research proposals have been described as: a) relevance to broad or particular agency concern, and b) capability-cost considerations, that is, adequate assurance of proper execution and necessary resources.

The question is to make explicit the various criteria that should be used to appraise utility and efficiency, and to set up the procedure that should be followed in this appraisal exercise. Briefly, we say that:

a) R+D project proposals should be grouped by main applied and basic fields like Agriculture, Metallurgy, Electronics, Health, Chemistry, etc., so that there is a certain homogeneity in each of the sub-universes. Available funds should somehow be split up between these fields.

b) Criteria should be defined, in each of these fields, for utility and efficiency. Though different fields may call for slightly different sets of criteria, it is to be expected that no major differences will occur except in the case of R+D projects for basic research where the utility parameter cannot take into account practical socio-economic aspects but only aspects that have to do with scientific merit.

c) Various arguments for the appraisal of each criterion should be spelt out in order to guide such appraisal. Some of the arguments may be of a quantitative nature.

d) Guidelines or rules should be made explicit, as far as possible, for combining the appraisals of the various criteria into appraisals of the main parameters of utility and efficiency, and for combining the latter into a final value or index that expresses the priority assigned to the research project under consideration. This may be done through a scoring mechanism with weightings attached to each criterion, though this procedure gives a pseudo-assurance of precision and puts a straight-jacket into the deliberations of committees, so that it may be desirable - at least in a first stage, while the appraisal procedure is being tested and refined - to give general instructions and to employ few categories, such as A (optimal), B (good), C (acceptable) and D (no good) for each appraisal or combination of appraisals in the whole exercise. Another important point where a definition is needed is which criteria should be taken to be of the threshold type, so that if a good qualification is not obtained - say C or B on the above scale - the project is automatically assigned a low final priority index no matter how well it stands in regard to other criteria.

e) Guidelines should be laid out for the formation of appraisal committees, and for the set of criteria that should be assigned to them for appraisal. In principle, only one committee per main field is not appropriate, and there should be at least two, one made up of scientists to provide "peer judgement" on criteria that have to do with scientific matters, and one where planners, economists and other people should take part to appraise criteria that have to do with economic and social matters. Several fields

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may share one committee of the latter type, but each field should have its specific committee of the first type. The big problem is of course how to preserve impartiality and do away with biases in the operation of "scientific" committees in particular, since we are dealing with small scientific communities where everyone knows everyone and where it may happen that one of the members is himself submitting a project. This is a problem that would seem to need some research.

The preceding points have to do with setting the rules of the game of the appraisal exercise, the purpose of which is to provide within each main field a ranking of research projects that are candidates for funding. Obviously, such a task is a matter for policymakers, and should not just be delegated to technical persons only, since there are important aspects of policy involved in the choice of criteria and the weight to be assigned to each. The drawing up of the appraisal procedure should be undertaken by the science policy authority as an important part of its mission.

The appraisal exercise may be schematized as shown in the graph on the following page. Let us now look at some of the principal criteria that may be proposed for use in a country which has attained a certain dimension in its economy and its science.

Utility

1) Relevance of the research project to social and economic development requirements.

In the case of applied research, the project should deal with a topic that is relevant to development objectives, production problems, exploration and exploitation of natural resources, etc. Questions to be asked are how important is the problem to which the project is relevant, and how high is the relevance. Guidance should be sought from planners and economists, and it would be helpful if some preliminary work has been done on the R+D requirements of economic and social development - this may on the other hand provide a guide to researchers looking for topics.

In the case of basic oriented research, the relevance will be of an indirect nature: knowledge produced would serve as an input for further applied research, and the basic oriented research activity may be seen as back-stopping (to a higher or lower degree) applied research in desirable topics. This type of relevance needs the judgement of scientists in addition to that of planners and economists.

2) Relevance to other national goals.

Examples are: self-reliance, prestige, defence, preservation of the environment.

3) Scientific and technical maturity.

Under this criterion we should examine the intrinsic likelihood of obtaining desired research results as far as this can be judged from

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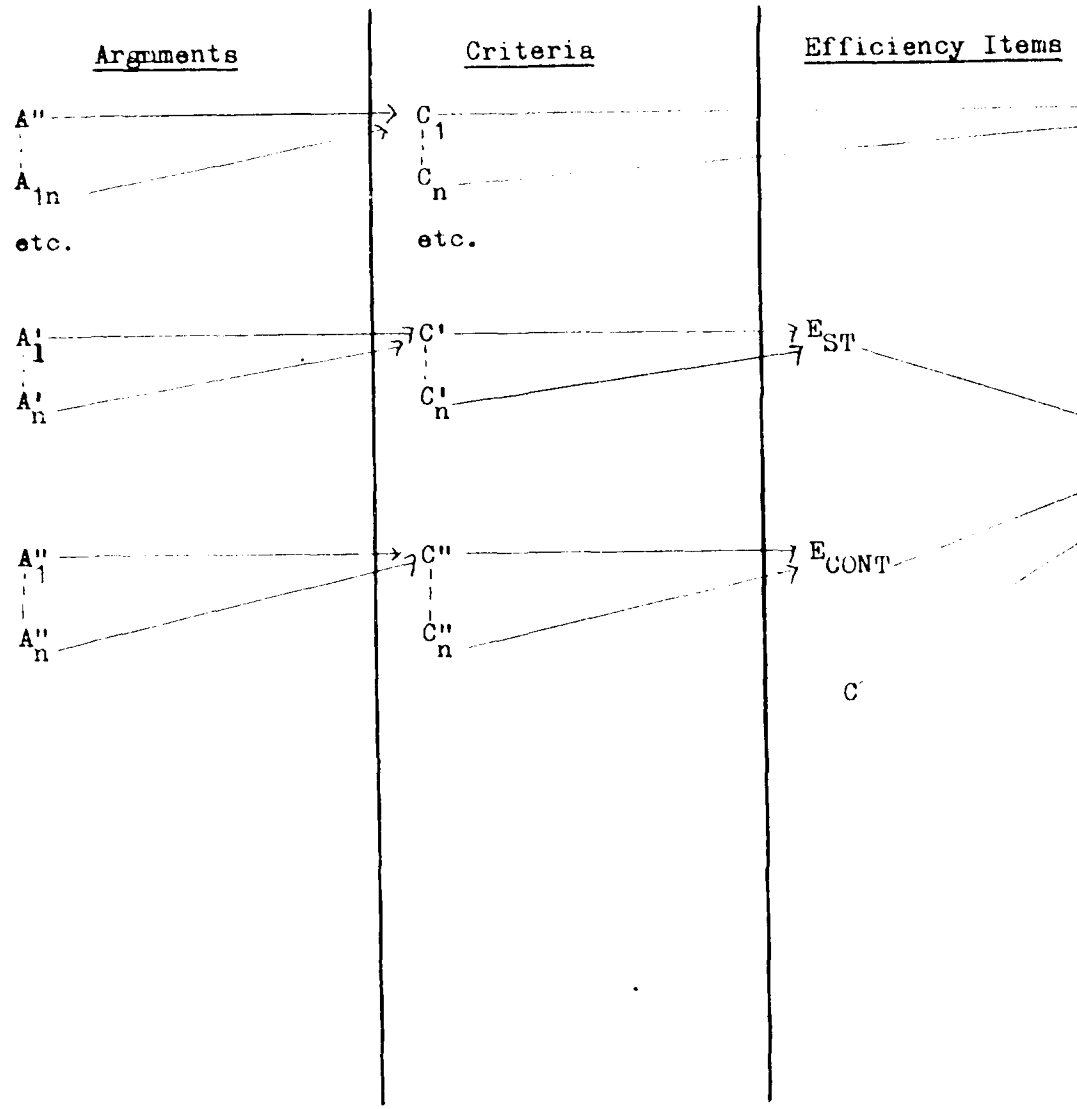
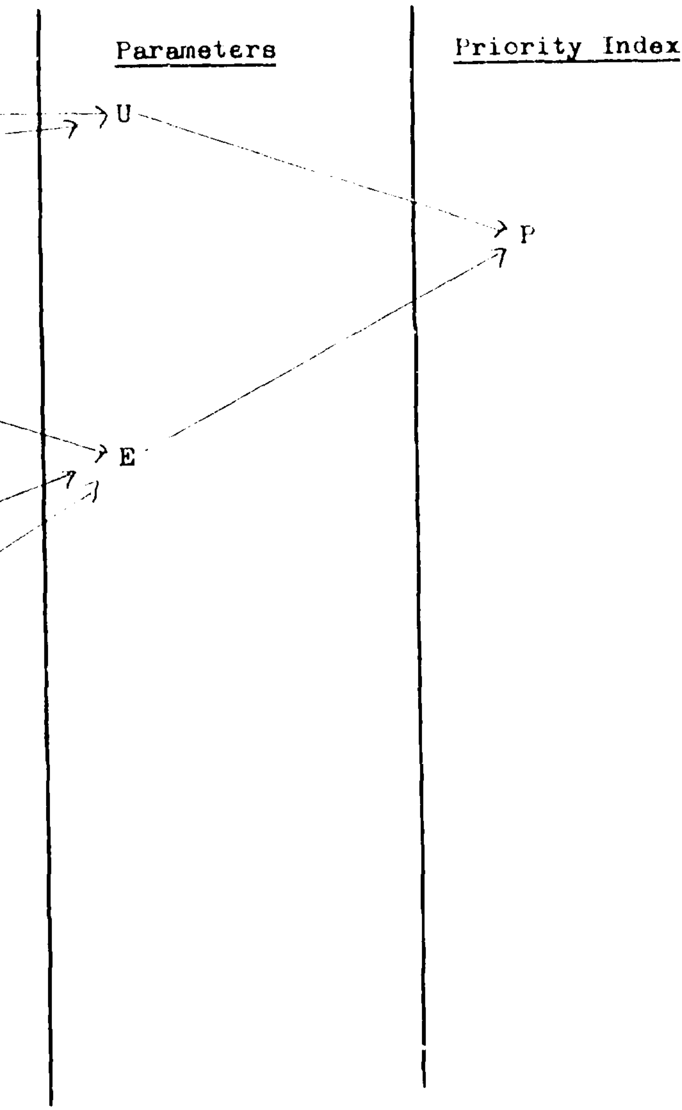
SCHEME FOR APPRAISAL OF R+D PROJECTS

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scientists' appraisals of the difficulty of the topic, the state of research on it in the world and the country, etc. In principle, scarce research resources in a developing country should not be applied to "adventurous" or unlikely projects - the place for exploring new areas of scientific research is the university, where such a work may be justified on grounds of scientific interest and contribution to teaching, and only once scientific maturity is established should funds be assigned for a project.

4) Regional considerations.

In a large country it may be considered important to promote research in various places away from the main concentration of the capital city, and hence good marks may be assigned to projects submitted by research centers inland to tip the balance slightly in their favour.

5) Other criteria that may enhance utility.

We may briefly mention the following:

- scientific merit, meaning that the project will contribute to learning in its discipline, or that it may help in the development of other disciplines through a multiplier effect (this may be said for some branches of mathematics, like probability theory and computer sciences). In fact, the utility of basic research (non-oriented) is principally of this type;
- the project provides employment for scientists which the country badly wants to retain;
- the project has as one of its byproducts the formation of needed human resources and the gathering of expertise that may provide useful services to Government and production.

Efficiency

We may distinguish three items under this heading:

a) scientific and technical efficiency, which has to do with the design and organization of the project, the calibre of the people that will carry it out, the resources at their disposal, and other items that would assure success;

b) contextual efficiency, related to the environment within which the project is executed; the best scientific team, well equipped and organized, may not be able to carry out its research project to fruition within time and budget limitations if the institution in which the work is done does not provide adequate support or a congenial atmosphere;

c) cost, which may be taken to be an indicator of social efficiency in the sense that given a certain objective, more cost means less social efficiency and vice versa. Alternatively, cost may be kept apart as an item and be brought to bear against the final priority index or ranking order, when the projects presented in one field are looked at together after the appraisal exercise, to decide which is to be supported.

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(a) Scientific and technical efficiency

1. Quality of the project leader 1/ and his staff.
2. Composition of the research staff; weekly hours devoted to project work; support it has from other groups.
3. Work programme and time schedule for the different phases; adequacy of budget.
4. Available equipment; type of equipment to be purchased.
5. Duplication with other research projects elsewhere; interaction expected with other research projects in similar or related topics.

(b) Contextual efficiency (This refers to the institution housing the project).

1. Stability of research personnel; institutional engagements that may affect the time devoted to research by project staff; levels of remuneration.
2. Central facilities of the institution, such as library, documentation, computing, workshop and other support services.
3. Appraisal of whether the institution offers an environment favourable to serious research work.
4. Administrative management of the institution: efficiency of procurement of supplies, efficiency of accounting, rapidity in recruitment of staff, and red tape in general.
5. Institutional contacts with possible users of research results. This would appear to be an important point for applied research; if there are no established interactions with users the practical value of research may not be forthcoming. The personal contacts of the research staff may replace institutional contacts, but this should be clear from the start.

A few remarks may be made regarding this approach and its possible application. In the first place, the adoption of the seemingly logical and comprehensive methodology does not assure that objectives will be successfully attained: there are always biases, ambiguities and errors in application, loopholes exist, people learn to "beat the system" by presenting their case very favourably according to the ruling criteria, and it is very difficult to attain what may be called "replicability" or basically the same results if the judges are changed, in a complex, qualitative exercise of this sort. Nevertheless, the use of this approach may introduce more rationality, lead to better decisions, and slowly change behaviour patterns of research people formerly used to working on topics just because they happened to be interested in them. Seconldy, the approach may be used both for disbursing central research funds in the nation and for the allocation of research funds within an institution. A suggestion may be made that the principal features of the nationwide appraisal system should be taken as guidelines for the choice of projects at the institutional level, so that compatibility would be reached between national and institutional criteria. Finally, a system applying the

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1/ Much scientific work in the world is supported on account of this factor alone.

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approach we have suggested would be rough to start with and would have to be reviewed and perfected as time goes on. In addition, the arguments and criteria related to utility are subject to change as the country identifies new important opportunities and goals (partly on account of research activity) so that periodic reviews of the system should take place.

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