EVALUATION OF RESEARCH PROJECTS
WITH SPECIAL REFERENCE TO THE
BIOMEDICAL, CLINICAL AND HEALTH FIELDS

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BIBLIOGRAPHY
I. THE CHANGING INSTITUTIONAL CONTEXT OF RESEARCH FUNDING

The institutional context of research funding is in a state of rapid change (Shattock, 1989) with the 1990s witnessing a major reorganization of research systems in most countries. A shared goal in this restructuring process is that the gains made possible by research should serve to enhance the country's competitiveness in economic terms or improve social conditions that will contribute indirectly to making the national economy more competitive (ECLAC, 1990). The attention that governments are now paying to research, particularly with respect to its utilitarian value, may eventually lead to a change in the very fabric of academia (Gibbons & Wittrock, 1985; Elton, L., 1986; Vessuri et al., 1993c).

As part of this restructuring, various governments are testing or implementing reforms in higher education (Neave, 1990 & 1991; Neave & Van Vught, 1991; Higher Education, 1992) and research councils (OECD, 1981; Vessuri and González, 1992), where the broad range of programs usually includes a research funding program as a priority objective. Greater emphasis is now being placed on selectivity and concentration in research activities. Selectivity aims to focus financing on those areas in which a country has—or can develop—specialized knowledge of international caliber and recognition. Concentration has two components: first, to establish and lend support to groups or teams of investigators in order to create a critical mass of effort capable of making a contribution at the international level; second, to provide full funding for the best investigators, e.g., the proposed Associated Laboratories Program in Brazil (Nussenzweig, 1992). Areas of research are thus identified that the governments consider to be of social and economic importance to the nation. More and more, efforts are being focused on assuring that these are the areas that receive priority in the allocation of available funding.

However, the impact of setting objectives for research funding goes far beyond special support for priority areas (however they may be defined). In the academic world, it can give rise to differences between individual investigators and institutions with respect to their willingness (and the resulting financial support) to undertake research activities that have a "market value," conceivably splitting departments, schools, or institutions up into "haves" and "have nots." Indeed, there seems to be an intentional effort to break the traditional link between teaching and research at
universities. In Mexico a proposal has been advanced for two clearly differentiated systems: the SNI would be directed toward and would review investigators, while a second system would be designed to reward excellence in teaching (Ibarrola, 1992). We are entering a stage in which governments do not appear to be willing to fund a broad range of research activities at all institutions. The transition toward technology-driven economies has, in many countries, led to the introduction of policies aimed at steering the demand for research toward fields that are perceived as important for future economic development. A set of indicators for measuring research performance is now taking shape in the Region, in which the ability to attract outside funding will be an important gauge of successful institutional performance, and this in turn will influence future funding levels. Some countries, however, such as Sweden, are rethinking the wisdom of concentrating research funding on a few select institutions, and at least one research university in the United States (Stanford) seems to have recognized that too much weight has been given to research as a basis for promotions and appointments (THES, 22 March 1991).

II. INTRINSIC AND EXTRINSIC VALUE

Ultimately, review is an exercise in setting standards. Review criteria not only measure achievements against specific research objectives, but they imply judgments on the relative importance of these objectives. The relative importance assigned to intrinsic or extrinsic objectives varies with the times, the subject area, and the social scenario and is sometimes a source of heated debate (Cf. Van Vught, 1993). Scientific research has both intrinsic and extrinsic value. Its intrinsic value lies in the ideal of the search for truth and the acquisition of knowledge. Its extrinsic value lies in the services that research institutions and investigators provide to society. This combination of intrinsic and extrinsic value has helped science to secure the important place that it occupies in history and in society, and it should be the foundation upon which any quality control system in the research area is built.

In underdeveloped countries, high priority is accorded the extrinsic objectives associated with fostering national development, as well as the concomitant need to build up domestic capacity. Intrinsic objectives in those countries, on the other hand, are generally associated with the idea of internationalism, implying the existence of innate universal characteristics of scientific problems that dictate the general shape of progress in specific fields. In terms of policy, this is expressed in the recognized need to make national contributions to the international (i.e., "intrinsic") growth of knowledge. "International" science is, to a great extent, the
"national" science of the center, which is largely the result of the interface of extrinsic and intrinsic factors. However, from the standpoint of the scientific communities in the underdeveloped countries, "internationalism" is usually taken as the standard for measuring the "intrinsic value" of science, which has important repercussions on the definition of objectives and criteria for evaluating scientific performance in the periphery.

Research funding trends have distinctive features in the Latin American academic milieu, where the scientific community is significantly smaller than in more developed countries, intellectual traditions are frequently more heterogeneous, and, accordingly, the research agenda is not as clearly defined. Moreover, international funding sources sometimes set strict parameters for lines of research to be pursued, leaving local groups little leeway for research relating to needs that are felt to be more local or at least diverging from the priorities set at the international level (CHRD, 1990). Given the scarcity of resources and the pressing need to channel scientific activity in such a way as to contribute to the attainment of national development objectives, the review of research is especially important. Nevertheless, it is a well-known paradox that review activities are less advanced in countries where the scarcity of resources--both material and human--makes planning and coordination even more essential than in the world's major scientific centers.

III. DEFINITION OF PRIORITIES, BASIC RESEARCH, AND HEALTH RESEARCH

Basic research requires not only highly skilled human resources but also a critical mass that will allow for ongoing discussion of new scientific findings; it requires as well a certain degree of interrelationship among the various institutions, together with the availability of technology and financial resources. In Latin America, these conditions are rarely met for research in the health area. H. Brooks (1982) has drawn attention to the fact that, in an underdeveloped environment, one can only attempt to be at the international vanguard in science in a few, carefully selected areas. Important questions then are: What fields of research should be supported in order to remain marginally ahead in the quest for knowledge? And at what cost? To what end? Such questions are particularly delicate in the debate over whether to give preferential support to the biomedical sciences--i.e., those that are closely related biology, biochemistry, and, in general, the scientific disciplines--and clinical research, or to epidemiological research. The epidemiology of diseases, the relationship between sociocultural factors and disease, and health services delivery have all been studied much less than biomedical and clinical issues, and it is often argued that the quality of the studies is lower. Indeed, many of the health problems that afflict us are not being studied. In only a few isolated cases have institutions
in the Region undertaken comprehensive studies of the sociocultural and epidemiological, as well as the biomedical, aspects of health problems (Bonilla et al., 1990).

In its analysis of research priorities, CHRD (1990) looked into who establishes priorities and for what population groups. The answer it found was that the needs expressed by a community may not necessarily coincide with the priorities identified by policy-makers. Consequently, the report asserts that any process aimed at setting priorities should bear in mind the following fundamental question: what voices will be listened to, what viewpoints will prevail and, as a result, what interests in the health field are to be served? This points up the eminently political nature of setting priorities for health research. CHRD also felt that research activity in the Region is still marginal and that quality control is deficient, which undermines confidence in the results of research conducted in underdeveloped countries (Bernardes Marques, 1991).

Health research in Latin America is geared mainly toward applying knowledge that was generated elsewhere in the world. Basically, then, it is concerned with the implementation of new technological advances in the diagnosis and treatment of the diseases most common in the Region and the identification of risk factors that can be acted upon in order to prevent disease. This requires that there be the ability to adapt and apply new technologies, a function that has been assigned to the public universities and research institutes. Generally speaking, this type of applied research receives funding only from government agencies, although the need for new tools and strategies to deal with health problems has also attracted the involvement of international sources of funding for research on epidemiology, the social sciences, and health.

In these fields, additional intrinsic criteria need to be designed for assessing the value of research projects, so as to include such factors as their contribution toward the production of new ideas, the integration and adaptation of new knowledge to research problems of special national interest, and the promotion of the cross-disciplinary skills necessary for ensuring the potential for innovation (Stolte-Heiskanen, 1986). Although much thought has been devoted to the problems of basic research conducted in the underdeveloped countries to advance knowledge in the health field and to benefit all humankind, much still remains to be done in concrete terms with regard to the review of the "applied sciences," which focus on specific, tangible health problems, such as simple and effective measures for managing acute respiratory infections; diagnosis, prevention, and ambulatory treatment of tuberculosis; identification and modification of risk factors for diabetes, coronary diseases, and hypertension; studies on fertility and human reproduction.
with emphasis on contraception; studies to evaluate and design national programs for the prevention of micronutrient deficiencies; and research on key issues to guide policy-making and management in national health systems; as well as studies of health service performance and financing, and the development of health information systems.

Competition is keener in some areas than in others. In some, a scientist has to work harder in order to maintain a certain level of research. In this connection, it has been argued that different review mechanisms are better suited to different types of scientific research. H. Brooks maintains that peer review can be useful in assessing research aimed at discovering "truths" of nature, but that it is not suited for judgments on the usefulness of research. However, the basic problem here is that abandoning the conventional protective shield of "excellence" or "intrinsic" scientific "value" would open the door to all types of opportunism or just plain incompetence. As a result, it has become common practice to use the conventional criteria of the system of peer or expert review, and these are the criteria most used by research funding agencies in Latin America.

A further problem in underdeveloped peripheral areas is the lack of adequate monitoring systems and performance indicators that meet the conditions of major world centers for "measuring" the quality of local research projects (Velho, 1989). Although a quick tally of the number of publications produced and the use of citation listings may give a useful initial idea of a country's contribution to a specific discipline and how it has changed over time, it is only of limited usefulness, since international comparisons are not necessarily the most important ones for defining policy in the national sphere. More important is a strategic definition of goals in terms of actions and knowledge for specific countries and the Region as a whole. For this, it is necessary to gather quantitative and qualitative information locally at the different levels, so as to make it possible to construct better indicators of research and development efforts and results than those that are available in the Science Citation Index or other sources used at the international level. The only way to reduce the biases and prejudices in publication and citation indexes and obtain a more valid understanding of research activity, methods, and reasoning in Latin America is to build national databases that systematically compile information on each country's scientific production. The same holds true for indicators of inventiveness and innovation. Larger amounts of useful information need to be produced and made available to respond to the need for information on crucial aspects of R&D in Latin America, using analysis to exclude those information sources that are so partial or fragmentary that they only serve to distort the facts or mislead decision-making. The realistic identification of social priorities lays a solid base for outlining the strategic goals to be pursued by science and for initiating
the study of existing capacity and of what is needed in order to successfully meet those goals. Combining "national" indicators (i.e., those related to specific objectives of social and economic development) with "global" indicators (which would provide rough comparative parameters of a given country's R&D levels) would seem to be a good solution. However, priority needs to be placed on defining "national" objectives and indicators in context as part of the review process itself which is essential to any true policy for science and technology (Vessuri, 1993).

IV. THE CONCEPT OF PEER REVIEW

The general idea behind the concept of peer review is to provide investigators with a way to evaluate scientific work in order to confirm the correctness of procedures, establish the plausibility of results, and allocate limited resources (Chubin & Hackett, 1990). It has been recognized as a basic tool of the scientific community and defended as a symbol and guarantee of the autonomy of science.

However, crucial differences exist in the specific ways of implementing the general idea. The concept has been used for such divergent purposes that it has been said that ultimately it serves none of them well. Furthermore, it is interpreted in so many different ways that it loses its effectiveness from that standpoint. Some "peer groups" are made up of scientific experts, others include leaders from outside the scientific community. In some cases, the peers make decisions; in others, they merely advise. Perhaps the root of the problem lies in the fact that the impact of peer review has turned out to be greater than was initially expected. Another possibility is that perhaps undue importance is being attached to peer review, considering the competitive atmosphere for research funding and the exponential increase in the number of applications for funding over the past decades.

Peer review is an intensely private process that begins in the mind of the scientist/reviewer. It is then converted into a bureaucratic procedure, and finishes behind the closed doors of a funding agency. The process is inaccessible at almost all points, it is opaque, and it is heavily imbued with the values and interests of the participants. Few, if any, details about the process are disclosed, and many of those who participate in the system insist on minimizing public access to the information. For example, the names of scientists whose grant applications are approved are almost always accessible, but the names of the others are usually not available to independent investigators. Thus, in order to preserve confidentiality, anonymity, and power, the process remains shrouded in secrecy. In addition, since the grant system occupies such an important place in modern science, it elicits unconditional
support among those who benefit from it and, concomitantly, often unreasonable criticism from those who feel they have been unfairly treated by it.

Peer review is viewed as the ideal tool of a "scientific community" whose values respond to the Mertonian ethos of universalism, communalism, organized skepticism, and critical neutrality. Today it is recognized that Merton represented one end of the continuum of scientific practice. It is evident, nevertheless, that Mertonian criteria are unable to explain the behavior of scientists in circumstances not only outside the scientific community but even within it (Nelkin, 1975). Contemporary practice in the scientific community exhibits much less solidarity than conventional expectations might imply. Internal competitiveness for funding and power surfaces more frequently than would be desirable, and in such conditions the shortcomings of peer review become evident. Despite this, few empirical studies have been undertaken to test hypotheses on "appropriate" scientific practice and to reconcile traditional conceptions with contemporary roles.

Most social analysts of science defend the axiom that the behavior of scientists is embedded in various social and cultural contexts, which are subject to a wide range of influences and are often directed toward purposes other than the advancement of knowledge. Once science is portrayed as a craft (Ravetz, 1971), appreciated not only by the scientific community but by the broader culture that sanctions it, the stereotypes surrounding the scientific method and the certification of knowledge give way to other dimensions of scientific practice, in particular, strategies that endow this practice with social responsibility (Chubin, 1992).

The review of research becomes a social-technical exercise wherein it is very difficult to separate the social and normative objectives from the strictly intellectual purposes. To start with, the functions of peer review are basically social in nature but appear to be at cross-purposes. On the one hand, peer review seeks to ensure that scientists are held accountable for the public funds they receive; on the other, it seeks to preserve the professional autonomy of the scientific community and, ultimately, to certify the reliability of new scientific and technological developments. Part of the value--and, at the same time, the vulnerability--of peer review is precisely its complex and, to a certain extent, contradictory role in the "new social contract" between science and society.

Peer review seems to produce more reliable results in relatively closed scientific fields and research specialties, where there is high degree of consensus and the intrinsic objectives of research alone are deemed important. In areas where there is diversity of thought, or where interdisciplinary problems exist, traditional review mechanisms leave much to be desired--not so much because they are
inherently deficient but because in the final analysis judgments about the merit of complex phenomena require experience and competence. The application of judgment criteria in any area of complexity should be placed in the hands of experts who understand the phenomena in question. The shared views of those with the necessary knowledge should be a key component in any system for evaluating quality, but in areas where there is diversity of thought reviewers may differ considerably in their criteria for interpreting quality.

The problems connected with peer review have been exacerbated in recent years by budgetary restrictions, the increased bureaucratization of science, growing evidence of scientific fraud, the lack of social incentives to pursue scientific vocations, lobbying by universities for government funding to be invested in local projects in order to garner more votes, and the trivialization of research, together with unwillingness to undertake long-term studies, caused by the spreading commercialization of academic activity. In view of the limited information available on review practices not just in our countries but at the international level as well—because, among other reasons, this practice is based on strict confidentiality and is not open to scrutiny—how can the criticisms be examined? How can it be argued that "old boys' clubs," intellectual particularism, the limited ability of specialists to understand subjects outside their immediate area of expertise, continued support for individuals and/or groups at well-known institutions, block funding for laboratories and centers, the allocation of funds on the basis of track records, or the extension of standard grant periods will give us better, less bureaucratized science, or vice versa? How are we to throw off the oppressive yoke of tradition—the arrogance, inertia, and fear—and begin to face the most fundamental issues involved in adapting science to society and thereby discover new abilities and enhance current quality levels?

V. THE PROCESS OF FUNDING RESEARCH PROJECTS

The process of funding research activities in Latin America poses sociological problems that range from the way knowledge is monitored, structured, and valued, up to such issues as scientific fraud—motivated by the high value of the prizes that can be won in the game of research funding—as well as other problems related to improper practices, poor project quality, and errors in judgments of merit by review boards. This reflects the need for an empirical analysis of the philosophy and funding mechanisms of research funding agencies as well as of the nature, expectations, and behavior of their clients.
Most agencies follow a very similar format. Applications are usually screened according to criteria such as those listed below and are reviewed by independent experts (in this case, we took those of CONICIT in Venezuela and FONDECYT in Chile, but they are very similar to those used by other institutions both within and outside the Region, e.g., PAHO):

— For individual investigators: their qualifications and scientific competence to carry out the project, their management abilities, and the relationship of their specialty to the project's objectives. For teams of investigators: the multidisciplinary makeup of the team with regard to the project, and the formation and/or consolidation of the research team.

— The quality of the study proposed, including an analysis of its objectives, hypotheses, methodology, and viability. With regard to the expected outcome: the likelihood of the project's leading to significant conceptual progress, or an important discovery or innovation, or the solution of an important practical problem; social or economic benefits for the country; and its contribution to the training of scientific or technical personnel.

— The relevance of the proposal to defined high-priority areas (as applicable).

Specifically, independent reviewers are usually asked to consider the following questions when forming their opinion:

— Is the proposed study original and innovative?

— Does the project have a solid conceptual base and structure?

— Is the project viable, bearing in mind the candidate’s qualifications and research history and the institutional infrastructure?

— Is the budget justified and is it appropriate?

— What progress has been made to date?

— Are there any issues that the review board should discuss with the applicant?

Occasionally, research funding agencies define priority areas. Often special panels are appointed to consider applications, although sometimes the evaluation of
the conventional review board—which is supposed to judge scientific merit—is complemented by a special board empaneled to consider other questions in addition to those listed above, in relation to the policies that underlie the identification of the priority areas. The questions raised in such cases can include the following:

- What is the probable impact if the research project is successful?
- Will the project have any impact on the country's economic development in terms of exports, foreign exchange savings, economic growth, and social development?
- Does the proposal have commercial potential?
- Does the project contemplate collaboration between institutions or groups?

As can be observed, these priority areas refer mainly to what is termed "applied research," with its potential for practical or commercial applications based on a favorable view of collaboration between academia and industry. In regard to these priority areas, there is no empirical information available in the Region on the implications of their definition. It would be revealing to analyze whether the number of grant applications in non-priority areas has dropped. A study on Australia has shown that applications in non-priority areas fell 15% between 1990 and 1991. The agency in charge attributed this to the highly competitive nature of the previous year's grant process, in which only one out of every four applications was accepted (Wood, Meek & Harman, 1992).

Reviewers normally submit a written report, which usually includes a qualitative appraisal of the application based on scales that are quite similar around the Region. Continuing with the examples used above, in Venezuela projects are rated as being of no interest or of limited, moderate, considerable, or high interest; in Chile, reviewers rate a project as poor, adequate, acceptable, good, or very good. FONDECYT specifies that explicit reasons must be given for exceptional ratings, especially the "very good" category, indicating that this rating should only be used for elements that are truly outstanding and in projects that distinguish themselves by virtue of their excellence. However, in keeping with this type of approach, I would argue that it is also very important for well-founded reasons and opinions to be given in the event that projects are rejected.

With the ever-keener competition for limited funds, the situation in Latin America seems, at first glance, not to be very different from other regions. A 1990
report from the United States National Science Foundation (NSF), for instance, observed that from the earliest days of that foundation's activities there have never been sufficient funds to support all the highly competitive proposals. The agency did approve more projects during the last decade, but success rates dropped because approvals were not able to keep pace with the number of applications submitted. Similarly, the prestige that external financing brings to an institution or an individual investigator is not unique to our countries. Here again, the NSF (1990) has noted that institutional and peer pressure on individuals has increased, since professional "success" is being measured more and more in terms of the number of grants received from the NSF. It could be argued that the competition for external research funding has been kindled as much--if not more--by institutional policies and national policy on higher education as by scientific curiosity. However, the Latin American region is very heterogeneous and there are more than a few cases in which it seems there are more funds available than suitable projects and investigators to invest in. This is another problematic area that needs empirical study.

VI. SOME VIEWS ON SELECTING REVIEWERS

Generally speaking, the more basic the research, the more a scientist tends to defend the idea of laissez rechercher; in other words, in the pact between science and society, society should not attempt to dictate the direction of science. The goal of basic science is to conduct fundamental research and produce new knowledge, and this requires a high degree of freedom. The most common criticism heard in this field is that funding processes tend to be based on bureaucratic review processes that stifle creativity under the weight of agency-imposed regulations and conservatism. This notwithstanding, the elitism inherent in the basic sciences and their arcane linguistic and conceptual barriers have made it possible for basic scientists to secure a situation in which science is judged on the basis of special rules. This is especially true in Latin America, where it has often been the basic scientists who have set, sometimes in a very rigid way, the general review standards used by research funding agencies in the Region for all areas of research, not just basic research.

The problem of whether science can be merely facilitated--without being controlled--is far from being resolved. Indeed, one of the goals of peer review is to reconcile the contrasting roles of scientists and decision-makers, bringing scientists closer to the administration of science. Although it could be argued, paraphrasing Clemenceau, that science is too important to leave matters of scientific policy to scientists, what seems to be occurring in recent years is that scientists are gradually being excluded from key decisions while program managers are receiving more freedom to manipulate the system, with the result that the manager's preferred
projects end up getting the grants through astute selection of project reviewers (Roy, 1985).

The objectivity of peer review can obviously be enhanced by carefully selecting experts so as to circumvent any personal and “paradigmatic” biases, by using more reviewers and standardized rating forms, and by eliminating the Matthew effect by ensuring anonymity in the process. However, such improvements are very difficult to implement in the oftentimes small scientific communities in the periphery. A solution commonly adopted when scientific groups at the national level are too small has been to submit projects to the international scientific community. For practical and conceptual reasons, however, this approach can be applied only to a limited number of specialized fields concerned with scientific problems that are of recognized interest to the international scientific community. Also, a key problem in using foreign experts to evaluate even non-controversial areas of research is that the reviewers usually only receive information on the initial project or results and lack information concerning the context of the research activity.

The development of two-stage approaches or joint reviews by national and international reviewers, use of the Delphi technique, or meetings between foreign reviewers and the members of the local scientific community concerned are some of the ways that peer review could be fine-tuned to meet the specific needs of the periphery and thereby serve as an indicator of scientific performance. But this is just one side of the coin. The confusion, disillusion, and frustration of candidates who have received what they consider to be unfair, inappropriate, or inconsistent treatment in their reviewers’ observations should not be overlooked. The concerns they voice represent serious criticisms and objections and should be used to enhance the guidelines that reviewers for the agencies are asked to follow in providing constructive criticism of grant applications that are rejected. In cases where research needs to be expanded and enhanced, this component of project review—in order to be truly meaningful—has to be undertaken with utmost care, in keeping with the very objectives of the review process. This is important to mitigate the discouragement and personal frustration that come with rejection and also to provide the applicant with the information he needs to know should he wish to resubmit a project.

Existing information at the international level on the experience of applicants who have sought support from external research funding agencies—in particular with regard to the appropriateness of the reply provided by such agencies to unsuccessful applicants—is limited. The results of a study conducted in the United States in 1979 revealed that 70% of the applicants whose projects had been rejected by six federal agencies felt that the feedback received from those agencies was inadequate.
A survey done in Australia on the research grants process from the viewpoint of applicants whose projects had been rejected (Wood, Meek and Harman, 1992) showed that, depending on the institution, between 38% and 50% of those applicants felt that the observations they received from their reviewers regarding the qualifications of the investigator or research team were inappropriate or inconsistent. Overall, 41% of the applicants expressed reservations about the quality and/or usefulness of the feedback received. Inconsistency in reviewers' observations and ratings was frequently cited. The agencies' policies on the number of independent evaluations requested vary. Normally, between two and four reviewers are consulted, but there is also the belief that a single bad opinion is enough to scuttle a project. Typical comments were: "I received very different responses from the two reviewers. One gave me an excellent rating while the other one was indifferent. The indifferent reviewer thought the subject was a difficult one"; "All the reviews were good except for one. The board seems to have opted for the exception."

Another frequent criticism is the element of chance, which some call the lottery of peer review. This includes a series of problems attributable to certain reviewers. Applicants are sometimes criticized for what they have or have not done: an applicant might complain that when his project was rejected the previous year, a reviewer recommended that two projects be joined in one, only to have the reviewers the following year observe that the project was trying to cover too much in a single attempt. Other times, an unsuccessful applicant may feel that the criticisms are not well-founded or that the arguments presented are too brief or general for ruling out the project. This criticism is frequently associated with another one: the apparent lack of familiarity of some reviewers with the subject area or with the applicant's research history.

Furthermore, when the area of study proposed by a applicant lies in a new field as yet unexplored by research, the "victims" consider it unjustifiable that reviewers criticize the applicants' lack of experience in the field. Here, there has been very loud criticism of the way peer review has been used by some funding agencies to support "safe science" and kill creativity. Even the NSF has acknowledged this problem with research funding in the United States, observing that the growth in the number of people doing research, coupled with limited government funds, is leading to a system that supports only "safe" proposals. Another report arrives at a similar conclusion, noting that it is no exaggeration to say that the agencies expect an application to outline the anticipated discoveries. The requirement that solutions to all problems be obvious before the study is begun is a strong disincentive to innovative work.
In connection with this last point, attention has been drawn to the difficulty of defining the limits of what counts as science at any given time, and to the fact that "expert knowledge" is subject to the ever-changing nature of science and cannot be locked into a permanent definition for all time. While it is necessary to recognize the need for some degree of specialization and expert knowledge in order to make judgments about merit, consideration must also be given to the intricate nature of decision-making with regard to what constitutes fair play in areas where thinking diverges. The risk in these cases is that decisions might be made by a reviewer who subscribes to a specific school of scientific thought. This would not be just a case of "old boys' networks" among institutions—a quite significant phenomenon pointed out in recent literature on peer review (McCullough, 1989)—but also, and of more consequence for science, it would constitute support for similarity of thought, or intellectual particularism, which can be observed in the decision-making processes of well-defined communities of thought, among which divisions are more pronounced (Travis & Collins, 1991).

VII. REVIEW AS AN INSTRUMENT OF SCIENTIFIC POLICY

The review of research projects in the area of public health necessarily needs to take each country's social programs and policies into account. Most of the reviews done in Latin America have taken the form of studies conducted by technical teams from the same public agency that carried out the program, and their final reports limit themselves to highlighting the achievements of the policy or program and the difficulties that had to be overcome during execution. Specifically, they compare the results obtained against the expected outcomes, followed by an "explanation" that is supposed to account for any discrepancies but is rarely nothing more than after-the-fact justification. Generally speaking, such reviews serve to give legitimacy to the program and show its rationality, but a role they have never performed is that of contributing to the improvement of project management (UNESCO, 1986; Coimbra & Lamounier, 1986).

Another type of review is the practice of commissioning external boards or teams of consultants from outside the organization where the program has been implemented; in these cases, outside funding has usually been received from an international technical- or financial-assistance agency. Here, the methodologies have often been drawn up along technical lines and the basic scheme followed is that of measuring to what extent targets were met. However, since the review often begins in the last stages of program execution, difficulties can arise in ascertaining the pre-project situation, thus making it hard to carry out a correct "before" and "after" comparison (Sulbrandt, 1989).
Special mention should be made of reviews of social development programs and projects carried out by nongovernmental organizations that work in this area with international financial assistance. In this connection, studies have been conducted that clearly show the type of knowledge that can be obtained from a review and its usefulness to program managers. The assessment of programs carried out by nongovernmental organizations is one example of the type of study that would be useful for all the agents involved in the respective programs.

Finally, mention should be made of a last type of review of social programs in the area of health: the academic-type assessments carried out by investigators at university centers. These reviews focus on the relationship between the public sector and the community, centering their attention on the program's stated purposes and the results obtained with regard to the target social groups. Such studies typically take a comprehensive approach to the subject and their critical component assesses not only the program's results and impact but also the initial proposal, formulation, implementation, and social impact, constituting thus an invaluable source of critical ideas on public policy in the Region (Coimbra & Lamounier, 1986).

However, especially when shifting from the area of public health to clinical and biomedical research, the tension between politicians' demands for greater regulation, accountability, and response capacity of science and scientists' demands concerning their freedom, autonomy, and the intrinsic benefits of research becomes more acute. This tension is rooted in the social roles assigned to politicians and scientists and it is unlikely that it will change very soon unless there is a social emergency, such as an epidemic, a war, or some other similar catastrophe. In principle, as was mentioned earlier, this is a healthy tension that does not need to be defused, since it maintains a balance between freedom and responsibility in science.

Some of the more common criticisms launched by the government sector against peer review include the following (Chubin & Hackett, 1990): Peer review removes decision-making power from the hands of elected officials and their subordinates and puts it in the hands of people who are not answerable to the public, thereby enabling the scientific community to use public funds for its own purposes (i.e., "pure" research), ignoring the pressing needs of society, which could benefit from applied research; it discriminates against scientists who work in small science departments at universities and other institutions with little prestige; it does not give due weight to the opinions of non-academic scientists regarding project merits; and it obstructs careful examination of projects of questionable merit. The general observation to be made here is that officials fear losing control and are
suspicious that the vested interests of the representatives of the scientific community will channel any support provided toward scientific rather than social purposes, besides the community's elitism and suspected inability to resolve practical questions (which may be a way of referring to differences in the standards used to assess project effectiveness).

The pertinent questions to ask with regard to the funding agencies are whether the agency supports high-quality research and whether the people involved in the decision-making process have appropriately assigned functions and whether they carry them out well. How open and impartial is the grant-awarding system? Does the decision-making process produce a desirable distribution of funds? How efficiently are resources distributed? How effective is the agency in supporting innovative research?

For the funding agency, peer review should be an effective tool for allocating resources and communicating priorities to scientists. However, the different facets of the effectiveness of peer review have not yet been articulated, its role in effectively promoting science has not been assessed, and no comparison has been undertaken of the scientific or practical effectiveness resulting from the peer review process vis-à-vis other mechanisms for allocating funds for research. Funding agencies usually expect research projects to respond favorably and rapidly to the demands of official scientific policy.

Nevertheleess, it is necessary to acknowledge scientists' complaints that the bureaucracy does not always provide clear information on new initiatives undertaken by different government sectors. Another complaint has to do with the presence of ideas that are deeply rooted in some funding agencies and block the way for any new ideas, or, conversely, the emergence of passing fads that interfere with and distract scientists from their work in the mainstream of their disciplines (this accusation has been leveled against the use of molecular biology in the life sciences, for example).

Alternative methods have been proposed. Perhaps the most notable are the allocation of research funding to individual institutions in the form of block, lump-sum grants, ongoing support for individuals with good research histories, and the use of a lottery to award grants. In this connection, senior U.S. scientists have proposed that peer review be eliminated as a system for research funding allocation, suggesting in its place a system whereby applications are evaluated only to ensure that they are technically sound. Each application passing that test would be placed in a lottery and would receive funding if it were pulled from the hat before the available funds ran out (Ince, 1991, p. 1). According to its proponents, such a system would reduce the bureaucratic burden associated with research and the cost of
involving some 100,000 people in a project-screening process based on peer review, especially when the lack of available funds minimizes the likelihood of the projects even receiving any funding at all. The chief reviewer at the NSF has pointed out the need for new methods of funding, particularly since funds are currently available for only 30 percent of the projects that are submitted.

VIII. THE INSTITUTIONAL MANAGEMENT OF RESEARCH

Research is only possible when there is an institutional site where it can be conducted. In Latin America there are several important centers linked to medical research. Some, including the Instituto Oswaldo Cruz, of Brazil; CIDEIM, of Colombia; the National Cardiology Institute and the Hospital for Nutrition-Related Diseases, of Mexico; and IVIC, of Venezuela, have focused their research on biomedicine. Others, such as UNAM, of Mexico, have focused on clinical or health research, although their degree of professionalization is more limited (Malo, 1988). A research institution's administrative capacity is crucial to success. Good management promotes the scientific output of investigators, can help to consolidate the organization, and can influence the environment in which it operates. Flexible and decentralized administrative mechanisms also contribute to the research process. Highly bureaucratized and centralized institutions hamper research activities, discourage scientists, and undermine productivity.

Research institutions should follow an administrative process of planning, organization, management, and control. This will strengthen them so they may define their own lines of research, acquire more independence for their basic operation, and influence their environment. Public universities and official institutions frequently face the same problems as other government agencies in the Region, i.e., administrative inefficiency, improvisation, bureaucratic difficulties, and financial restrictions that hinder the proper functioning of the research process (Kumate, 1987). Independent institutes, however, tend to have better management skills, administrative flexibility, decentralized decision-making, and a simple, streamlined organization that contributes to efficient management in the research process. The factors that should be taken into account as components of this independent variable include: the existence of a development plan for the organization with medium- and long-term objectives; the type of decision-making process (delegation versus concentration); definition, by the institution, of priorities and lines of research versus merely responding to offers from funding agencies; administration of the research process (complex bureaucracies versus administrative flexibility); administration of the institution; management oversight (Carrasquilla Gutiérrez, 1993).
With respect to universities, there is discussion in the Region—for instance, in Mexico (Luna-Barradas, 1990) and Venezuela (Bianco, 1992)—as to whether, given their function of training professionals and investigators, they should not perhaps step up the training of investigators through graduate-level and doctoral work in medicine, which is their primary function, leaving clinical training as a responsibility of the health sector. A recent study at the School of Medicine of the Central University of Venezuela, for example, estimated that over 77% of graduate students were pursuing studies in areas of specialization, which attach special importance to training in care-related activities rather than the production of knowledge, their objective being not to promote research but instead to evaluate students' clinical abilities (Díaz Polanco et al., 1991). Given the traditional linkage between graduate studies and research activity, this figure is cause for considerable concern.

The variables to be monitored at the institution where the research group is located include those that pertain to the qualifications of the human resources: percentage of investigators with a doctorate or master's degree and where they were trained; the amount of time dedicated to research; available technological resources; the institution's participation (i.e., its investigators' participation) in international research networks; scientific publications printed; coordination with other institutions for the exchange of resources, information, and experience; opportunities for investigators to participate in other forums for scientific information, such as congresses, meetings, or exchanges.

IX. CONCLUSIONS AND RECOMMENDATIONS

The concept of quality seems to be as elusive as it is persuasive. Despite the fact that it is impossible to establish a universal definition of quality, we should be aware that it is a political concept. It forces investigators and agencies to ask themselves not only what its virtues are but also what its limitations are. Focusing on the quality of work means running the risk of exposing both its positive and negative sides. The need to assess the quality of scientific research is much greater in the underdeveloped countries than elsewhere, even though the prerequisites for extending and enhancing the review process and applying it to benefit scientific research may be less favorable than in more developed countries. The following paragraphs summarize some of the ideas that should be considered in the search for what constitutes appropriate review:

1. Since the late 1980s, quality has become a basic concept in many research systems in Latin America as a result of the increase in the number of investigators and institutional growth, but also as a result of the lack of
international visibility of research that is conducted in the Region. This concern is even more pressing given that many countries have already reached the limit of their public spending. Budget cuts and scaled-back operations have spurred questions on the relative quality of the processes and products of scientific research in the Region.

2. Quality has both an intrinsic and an extrinsic dimension. Currently, but with greater urgency in the underdeveloped countries, the extrinsic dimension has been taking on more importance in review processes. Research output is no longer assessed exclusively in terms of scientific value. Decision-making on research funding now assigns equal importance to the benefits that society perceives as well as the related costs. However, the complexity of decision-making with regard to the immediate social or economic potential of scientific activity is much greater than in a review based exclusively on an intrinsic analysis of quality. In attempting to achieve a supposed social impact, technical quality may end up being sacrificed.

3. Accordingly, account must be taken of the environment in which the review is performed, without, obviously, replacing it: any effort to improve the appraisal of research activity has to bear in mind local economic conditions, differences in the quality of higher education available, the existence or lack of quality assurance mechanisms built into the system, regional differences, the training and remuneration of research staff, as well as many other factors that can play an important role.

4. Any new review process should be designed in such a way as to improve the prerequisites and the environment for review in the future. Before designing the content and methods for the process, however, it will first be necessary to identify what conditions are to be evaluated and to study the experiences that were unsuccessful or that enhanced review and the atmosphere of trust or mistrust between agency, users, external actors, local institutions, etc. The goal is to develop a "culture of review."

5. The establishment of priorities in scientific research is eminently political in nature. Review mechanisms should be adapted to the different priorities, although the format of peer review is so deeply rooted that it still tends to be the most common one for assessing the potential social or economic "utility" of specific research projects.

6. The excessive and sometimes exclusive use of the conventional performance indicators applied by developed countries has a distorting and harmful effect
on local research activity in an underdeveloped environment. "Local" performance indicators need to be designed for the specific objectives and priorities of social and economic development; they can then be complemented by "international" indicators in order to achieve comparative parameters for a country's R&D levels.

7. Peer review has become the central mechanism for assessing the quality of scientific research, although recently criticism has been growing, as the increase in the number of investigators and projects makes it more and more evident that this is an activity in which it is virtually impossible to dissociate strictly intellectual purposes from social purposes.

8. The role of peer review in funding decisions seems to follow a similar format in the various agencies. In any case, the limitations do not seem to lie as much in the formats used, where elements can be added or removed, as in the simple rating procedures or the excessive importance assigned to measurement indicators, starting with the forms that are filled out by reviewers. The elements considered in the review forms are useful instruments within a broader approach to quality, but they lose their validity when taken as the only legitimate format for measuring the concept of quality. Knowing how many members of the team have a Ph.D. or how many papers the principal investigator has published is not enough in and of itself.

One possible limitation in the review of scientific research in the Region is the use of overly formal approaches. In mature societies that have a long administrative tradition, the formalization of procedures that arose alongside the development of the bureaucracy often served to set standards that were the result of years of experience. Our countries, on the other hand, often begin by drawing up regulations, standards, and rules of procedure before implementing and promoting the targeted actions, and there is too much reliance on administrative personnel who lack a sufficient grasp of the problems involved. In Latin America, scientific research needs to be promoted more and, for this, ways need to be found of fostering diversity, adaptability, and cross-fertilization between the different fields so that new ideas can lead to totally unforeseen developments.

9. Given the heterogeneity of national scientific communities, there are varying degrees of resistance and suspicion of the actors involved in the process, depending on the areas they come from and their relationship to the groups that have "scientific" control over the funding agencies. When potential applicants opt out or eliminate themselves from the system of their own
accord because they doubt it is worth investing time and energy in preparing a research proposal, then agencies need to look inward and review their procedures, especially when the alternative sources of funding for research in the investigator's field are limited or non-existent. The presence of dominant groups of scientists connected with the agencies--sometimes referred to as "old boy's clubs"--usually means that numerous other applicants are unfamiliar with the rules of the game set by the agency. Applicants need to assume responsibility for making sure that they are fully familiar with the procedures and guidelines of the agency and the professional background and leanings of the members of the respective review board when they prepare their requests. Very few applicants take this aspect into consideration when preparing their proposals.

10. On a deeper level, review in the Region often encounters difficulties owing to the limited availability of skilled human resources qualified for the task. From the criticisms launched against the system, it is clear that some reviewers are perceived as taking a flawed or inappropriate approach to peer review. It is important, then, to overcome this situation in both the organization and the direction of review work, while respecting the individual nature of the different types of research and targets established. Agencies that fund research should make sure their reviewers receive clear guidelines on the type of judgment and observations expected of them. They should also make sure that the reviewers they appoint to review a specific research proposal are indeed familiar with the state of knowledge in the subject area. Here, the way these agencies compile and update their lists of reviewers also needs to be more transparent so as to enable them to respond effectively to the criticisms launched against them. In sum, before revising the guidelines for evaluating research proposals, it is crucial to first examine carefully, at the agency level, the procedures for selecting and evaluating the members of the review boards for specific disciplines or priority areas and reviewers in general.

11. Considerably more attention should be given to the notification that funding agencies send to investigators whose projects are rejected. More attention should be paid to the guidelines that these agencies' reviewers are to follow when providing constructive criticism on rejected requests. With research increasing in quantity and quality, this component of project review should perform an instructive role and, when applicable, provide encouragement, in keeping with the very objectives of the review. In most cases, it is easier for us to have an awareness of our own virtues and weaknesses than of how best we might improve on them. Besides the discouragement and personal
frustration that come with rejection, there is also the consideration that, if applicants wish to resubmit a proposal that was not successful in one format, they need to receive appropriate information from the agency on how best to go about it. The substantial effort involved in project review is difficult to justify if it is not combined with an effort to suggest possible ways of improvement.

12. The disciplines, groups, and institutions that have a well-established culture for securing research funding usually receive higher rankings than others where that culture is less developed. Here, there may very well be an aspect of chance, or lottery, in the funding of research. But the larger institutions and the more active research groups, just like big players in the lottery, do not win the big prizes by accident, and so it has been observed that the greater experience and organization of more prestigious groups and institutions results in them having more projects approved. The problem here is how to make sure that investigators and institutions who lack experience in garnering external funding have the chance to learn and become more adept at handling funding possibilities.

13. Review methods need to be adapted in order to make room for projects in new fields of research or in fields that have not yet been explored locally and therefore imply a higher degree of "risk." The periphery tends necessarily to be more conservative than the center because it has had to follow rigorous guidelines in environments where no legitimate scientific traditions exist; this has often relegated science to a secondary role as a replicator, repeating experiments first done elsewhere. It is also true, however, that the underdeveloped countries need more imaginative and heterodox approaches in order to overcome these obstacles. It is necessary, then, to strike new balances—a difficult process at best—between security and creativity.

14. In addition to broad differences from one country to the next, there are also differences in purposes, frames of reference, procedures, and, in particular, the importance assigned to development vis-à-vis the emphasis placed on the funding criteria set by public policies. This is also linked to the importance attached to meeting targets or to the normative data that are frequently related to the cost.

15. The institutional site of the research activity is one of the prerequisites that, by virtue of its importance, requires a particularly significant level of analysis in the study of factors that affect quality. Institutional transformation has proven to be one of the most complex components of development strategies,
although success has been limited in improving the performance of most public-sector agencies and companies. High-quality scientific research will depend on the emergence of the right institutional arrangements.

16. The scientific community should invest some of its own time and talent in regulating itself. At the same time, however, it should also accept the incorporation of other social standards into its own values. The quality of science is based on a solid, three-point foundation made up of individual scientists, organized scientific activity, and national and international scientific policy. Each of these components should be examined because each is a dependent variable in the quality equation. We can only aspire to enhance the quality of research at the regional level if international research organizations invest some of their own time and energy, lending support and entering into dialogue with national agencies and local scientific communities. Would this not be a task for PAHO?

Even the best of efforts will prove ineffective if society's general knowledge is behind the technological times, as current trends in Latin America seem to indicate. In its own interest, as well as in that of the society with which it should communicate, the scientific community should attach high priority to efforts aimed at raising the sophistication of the entire society. In the long run, only when society as a whole is able to participate in an informed way can we expect wise decisions with regard to the quality and direction of science.
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