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RESEARCH PROGRESS MEASUREMENT and MANAGEMENT DECISION MAKING

Proceedings of a Workshop

Organized by the
Board on Science and Technology for International Development
Office of International Affairs
National Research Council
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for

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NOTICE: The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the councils of the National Academy of Science, the National Academy of Engineering, and the Institute of Medicine. The members of the committee responsible for the report were chosen for their special competencies and with regard for appropriate balance.

This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Science, the National Academy of Engineering, and the Institute of Medicine

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RESEARCH PROGRESS MEASUREMENT AND

MANAGEMENT DECISION MAKING

Preface

This document is a summary of a workshop entitled ***Research Progress Measurement and Management Decision Making*** organized at the request of the Office of Research, Bureau for Research and Development (R&D), U.S. Agency for International Development (A.I.D.) by the Board on Science and Technology for International Development (BOSTID) of the National Research Council (NRC). As one step in a continuing effort to strengthen management oversight of its research programs, the R&D Bureau asked BOSTID to assemble a group of participants experienced in research management and evaluation from private firms, foundations, universities, research center, and government agencies to share experiences on research and development evaluation methodologies and strategic planning of research. The group was not asked to do a formal analysis of current A.I.D. research planning and management practices. Thus, no formal report with recommendations to A.I.D. was expected. The workshop goal was to raise issues in research management, capture ideas and experiences that may be applicable in a variety of situations and discuss problem solving methods in an open forum attended by a cross-section of A.I.D. persons responsible for research programs.

All remarks in these ***Proceedings*** were taken from notes, including a recorded transcript of the

sessions. The workshop was organized with a short lead time and invited participants were not required to prepare papers in advance. The panel discussion format was employed to permit informal exchange, questioning, and dialog. All sessions were held at NRC headquarters in Washington, D.C. on December 11th (all day) and 12th (morning) 1991.

Special thanks are due to the workshop chairperson, Dr. Arden Bement, Jr., for his time and valuable insights that provided a structure for the workshop itself. We also thank the workshop speakers, some of whom came from abroad, for their interest, time and enthusiastic participation. Their insights provide another step forward in improving the quality of research planning and its deliver to developing countries.

Finally, acknowledgement and thanks are given to the representatives of the Agency for International Development for their sponsorship of the workshop, their counsel during the preparatory period, and their enthusiastic participation. Among those whose efforts we note in particular are Dr. Richard Bissell, Ms. Katherine Blakeslee, and Dr. John Daly, all of the Bureau for Research and Development; and Dr. John Eriksson and Dr. Gerald Britan of the Agency's Center for Development Information and Evaluation.

Workshop on Research Progress Measurement and Management Decision Making in the United States Agency for International Development

National Research Council
Board on Science and Technology for International Development

NAS Main Building Room 180 & Room 250
December 11-12, 1991, Washington, DC 20418

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RESEARCH PROGRESS MEASUREMENT AND MANAGEMENT DECISION MAKING

Summary of a Workshop

A. Background

In 1990 the Agency for International Development (A.I.D.) embarked upon a new management initiative designed to support a strengthened and more strategically planned approach to foreign assistance activities having a greater development-oriented impact in the many countries where the Agency operates. Given current budget realities, the new initiative focuses on operations at all levels of the Agency's work in its overseas missions, and central operations in Washington headquarters and regional bureaus. Most of A.I.D.'s activities are country-oriented involving the ***delivery of development services*** under rather specific economic and social situations. To address issues within its highly decentralized management system, A.I.D. has designed, and is implementing a new review and evaluation initiative known by the acronym "PRISM," - Program Performance Information System for Strategic Management. The initiative is a mission and Washington based collaboration that provides many opportunities for participation from the bottom-up as well as from the top-down.

The Agency also has a significant ***research program*** with broad development implications for which an evaluation system designed for field operations may not be well suited. As one step in the design of an evaluation system for its research programs, A.I.D. asked the Board on Science and Technology for International Development (BOSTID) of the National Research Council (NRC) to organize a workshop on research evaluation methodologies and strategic planning. The workshop was given the title ***Research Progress Measurement and Management Decision Making*** and was held at NRC

headquarters in Washington, D.C., on December 11-12, 1991.

B. Workshop Terms of Reference

In its request for a workshop the A.I.D. asked that BOSTID invite participants thoroughly familiar with research planning, management, and evaluation systems from the private sector, universities, research centers, research donor groups, and from government agencies including some from overseas. Through panel presentations by the NRC group and through discussions with an A.I.D. management group invited by the Assistant Administrator for Research and Development (R&D), the workshop focused on alternative approaches to, and processes for performance measurement and strategic planning of research applicable to A.I.D. operations. The objective was to probe more deeply than a mere description of different evaluation methodologies by posing questions and raising issues such as the following:

1) What does a research manager need to know to determine if research is high quality, cost effective, and likely to lead to the end for which the research program or project was designed?

2) How are priorities developed for a research program, how is it determined if they are appropriate to the goals of the agency or firm and if the management structure is appropriate to implement the research?

3) How does the passage of time affect research both in the near and the long-term ? (Consider 3, 5, and 10 year time frames.)

4) What information is most important for the Agency's top decision makers to have in relation to investment in, and measurement of research results?

5) How should decisions be made about continuation of a research effort? What measures need to be taken to assure that information gained in a research effort is available both

currently and in the future?

Participants were also asked to discuss under what conditions, and in response to what type of objectives, qualitative and quantitative performance indicators are appropriate. Furthermore, suggestions of management principles and procedures that should govern the establishment of research objectives and indicators of progress toward those objectives meeting scientific standards were to be included.

Within the short time period available for the workshop, the A.I.D. did not expect consensus conclusions and recommendations on so broad an agenda. Rather, in sharing experiences, discussing issues, and exchanging information the workshop was seen as the beginning of a process that A.I.D. and its Research Advisory Committee would carry on.

C. An Overview of A.I.D. Research Activities

The Workshop on Research Progress Measurement and Management Decision Making opened with an overview of A.I.D. research thrusts and a description of the new strategic management performance information system (PRISM). Research in A.I.D. was categorized as:

1) Strategic, which is long term, applied and targeted toward a problem whose pay-off is over longer time spans than the normal 3-5 year programming cycle.

2) Adaptive, which draws heavily upon known research results and is targeted to specific site locations,

3) Operational, for the integration of country specific social and cultural issues, and

4) Data Base, for the generation or maintenance of information on global issues.

Two characteristics of A.I.D.'s organization that determine how its research programs function are: a) the decentralized nature of its operations with an extensive field-based technical staff, and, b) the integration of research into ongoing programs. The support and application of research within ongoing activities is designed to catalyze A.I.D.'s resource transfer program into a dynamic and creative technology transfer program.

PRISM is in its initial stages of strengthening mission and Washington operational-level performance monitoring and the introduction of key, agency-wide program performance indicators. When fully implemented it should help senior managers meet decision making needs and also provide essential information for managers at other levels within the entire A.I.D. organization. By itself such a performance monitoring system will not reveal why results have, or have not occurred or which alternatives are the most effective and efficient; however, in conjunction with other management analyses, evaluations, and reporting, PRISM will systematize and strengthen oversight both for evaluation and strategic planning purposes.

Because PRISM as currently designed focuses upon evaluation of field operations and central office operations that support the field, it does not seem to be as applicable to the research activities of the Agency, most of which are financed or coordinated through the Bureau for Research and Development. Although A.I.D. is not one of the major research-supporting agencies of the U.S. Government, its research activities are large in their aggregate, totaling between 30 and 40 percent of the R&D Bureau's $470 million (FY 1991) annual budget.

D. Panel Presentations and Discussions

Following an overview of the manner in which research operates and is managed within A.I.D., participants invited by BOSTID shared insights into, and experiences in evaluation methodology and strategic planning. For practical considerations, presentations were grouped into 3 "sectoral" panels and

2 more general or "functional" panels. The sectoral panels were those representing industry, international research donors, and U.S. government agencies; the functional panels included one on agricultural research and a second on multisectoral research in the social sciences within programs of the World Bank.

1. Industry Panel: The industry panel consisted of presentations on research evaluation and strategic planning from pharmaceutical, materials fabrication and manufacturing (aeronautical and automotive), and contract R&D (Battelle Laboratories). Common denominators from these divers industrial perspectives arise from the highly competitive environments in which the industries operate. Thus strong R&D involvement is essential to survive. Perhaps no more than 10% of that R&D results in a successfully marketed product. Most R&D projects are terminated before they reach the projected end product because of technical reasons that change or invalidate the original premises on which the research was based, or the realization that the new product or process offers only limited technical or economic advantage in the market, or the product has a such a long development time that the market potential is uncertain, or there are other reasons arising from market competition and constraints. Termination of an R&D effort, however, does not usually signify failure because findings are often applicable to other products or processes of a firm.

Panel members emphasized the need for strategic planning in industrial research that is inclusive in nature. A strategic plan should involve production workers and their immediate supervisors as well as higher management levels. The former help keep a planning process realistic and practical; the latter are goal setters and determiners of policy. To be successful a strategic plan must be a judicious mix of both bottom-up and top-down approaches to problem solving.

2. International Donors Panel: Three presentations were given: a) one from the Netherlands' technical assistance experience, b) one comparing French and European Community technical assistance, and c) a third from the Canadian experience of the International Development Research Centre (IDRC).

The program of the Netherlands Ministry of Foreign Affairs is currently integrating all research it supports in developing countries into one strategic plan for technical advice, coordination and capacity building. The goal is to overcome either a supply-driven or a donor-driven approach to one incorporating the best of both in a more coherent system.

A central principle of strategic planning for research assistance of IDRC has always been developing country participation. Nearly one-half the IDRC Board members are from developing countries. Increasingly the principle of joint project planning is also incorporated into the science and technology assistance programs of the Netherlands and France.

A recent review of French collaborative research with developing country partners revealed: a) to achieve jointly planned objectives in a research project, the generally unequal balance of power between donor country and host country scientists needs to be addressed, b) from the very inception of of a project, agreement on evaluation indicators must be agreed upon; c) recognizing that there are a variety of goals in any partnership, no single evaluation methodology is sufficient to provide both donor and host country with convincing results, d) economic or financial indicators are almost always inadequate for assessing scientific and technical projects with developing countries, and e) evaluations should always be conducted openly and the results made publicly available. Evaluations should be separately budgeted and be distinct from line items for the research itself.

Throughout its lifetime, IDRC has emphasized research which has a ***capacity building*** component for developing country scientists and institutions. That principle together with IDRC's increasing efforts to give greater authority to ***users of research*** is the key to long-term success in terms of quality, effectiveness, relevance, and the probability of adoption.

3. U.S. Government Agencies Panel: This panel gave examples of research programs and their evaluation methodologies from the National Science Foundation (NSF), the Office of Naval Research

(ONR), and Department of Energy (DOE).

The program of NSF that was discussed is known as the ***Industry-University Cooperative Research Centers Program*** and is designed to create centers that become self sustaining from non NSF funding within a five-year period, to link university research very closely with industrial/technological advancement while training students, and to develop industry/university interaction on industrially relevant research topics. The program has a direct correspondence to activities and program objectives that A.I.D. fosters in developing countries. "Success" in the establishment of the industry-university relationship is the long-term survival of the relationship. By definition, a "successful" center is one that is cooperatively funded from industrial sources, the universities own funds, state funds, and other non-NSF sources. There is also a well structured and on-going evaluation plan incorporated into the program that involves an independent evaluator who serves as an advisor to a center director and annual, formal evaluations open to university and industry participants as well as representatives of the sponsoring (funding) groups.

The relevance of the Industry-University Cooperative Research Center idea to A.I.D. lies not only in its potential application to sustainable capacity building in developing countries but also as a model of formal evaluation planning for research activities. (See Annex B)

Over the years ONR has evolved a system of funding its research at the program rather than the individual project level as is the case for NIH and NSF. The system works well for the Navy and serves its needs for ongoing research evaluation. The ONR system can be more narrowly focused than one for A.I.D. that has multiple clients, a wide variety of needs, and of development objectives. The evaluation process of the ONR, however, suggests several features that could be applicable to A.I.D. These include the use of a core group of highly competent specialists within the A.I.D. itself to manage the evaluation process, the need for independent, external groups for peer reviews, and the need for senior management involvement in the process.

The Office of Program Analysis of DOE is mandated by the Congress to do an independent check on the merit of the department's research. Results of its work are not available to the public because of the detailed and often highly technical aspects of research programs. In general, the system may not be so applicable to A.I.D. but several principles would seem to apply. They include assuring that evaluators are "world class" in their fields and are drawn from outside the Agency; i.e., from industry, universities, and other government laboratories. In DOE evaluations, reviewers use inputs furnished by principal investigators as one important ingredient, and those investigators know that the inputs will be critically examined. The review process is intended to probe technical aspects of the research first, and impact second. Research productivity and impact are important ingredients of cost-effectiveness and are included as one part of the overall evaluation process.

4. Agricultural Research Panel: The agricultural research panel described the program and evaluation methodologies for the national research initiative of the Department of Agriculture Cooperative State Research Service (CSRS) and the research system of the Cooperative Group for International Agricultural Research Centers (CGIAR).

When the National Research Initiative Competitive Grants Program was started within the U. S. Department of Agriculture in 1990, the decision was made that a wide and comprehensive process was required to identify research needs and priorities. Twenty-four areas of concentration were chosen for a research budget that began at $40 million and is expected to go to $500 million per year within a few years. Users of research such as commodity groups, agribusiness, and environmental groups were consulted as well as the scientific community. Based on meetings of a research planning group; and looking at research priorities of scientific organizations, agricultural research stations and others, the USDA advertized nationally for proposals. Proposals are then evaluated by a peer review system for scientific merit and relevance. Competition for the grants is keen; about 20% of the proposals received

were actually funded in FY 1990. Since the national initiative program is only a bit more than one year old, discussion of research evaluation and of impact is premature.

The CGIAR system is well known to the U.S. A.I.D. because core funding in the amount of about $40 million per year has been the level of A.I.D. funding. The International Centers are guided by the CGIAR Technical Advisory Committee (TAC) composed of 18 distinguished scientists, half from developing countries and half from the developed world. TAC's responsibilities include the setting of priorities for the Centers in cooperation with the group of donors, evaluation of research conducted at the Centers, and allocation of funds for the annual budgets. Each Center has its own Board that hires the Director General and with him set policies, operating procedures, and monitors and evaluates research results. In addition there are periodic external reviews of the scientific and management performance of each Center. These external reviews occur approximately every 3 to 5 years. In the early years of the Centers, there was little long-range strategic planning but competition for funding now dictates that Centers have well structured strategic plans, typically extending for 10 to 15 years into the future. Centers plan differently, depending on the nature of their boards and top management. Most employ a mix of top-down and bottom-up management methods.

That the CGIAR Centers have had an impact on commodity production increases worldwide is well recognized. Rice production in Asia has been catalyzed by the work of the International Rice Research Institute (IRRI) located in the Philippines. Corn (maize) and wheat production in India, Mexico, and many other countries has been greatly increased by the work of the International Corn and Wheat Center in Mexico. A specific example of impact evaluation is cassava, a root crop grown extensively in tropical regions. From a small program of about $300,000/year over approximately 15 years and conducted by a team of 11 professionals from the International Tropical Agricultural Center (CIAT) in Colombia, cassava production is now increasing about 3.5% per year in Asia and 2% in Africa. Since cassava is a staple food of the rural poor, the reliable increase in supply is having a

profound effects on diet and nutrition.

5. World Bank: Research in the World Bank is part of a continuum of analytic work that includes economic and sectoral studies, policy studies, and activities which the Bank more narrowly defines as "research." The three classifications are somewhat arbitrary; the percentage distribution runs about 60% economic and sectoral, 25% policy, and 15% research. The research budget is about $23 million per year and priorities are directly derived from the Bank's major concerns. Currently the priorities are environmental issues, private sector development, poverty, and the transition of socialist economies.

Bank studies and research have a heavy involvement of staff and generally may be said to have four objectives: a) support of Bank operations, b) broadening the understanding of "development," c) improving the capacity of the Bank to give sound advice to client countries, and d) assisting in capacity building of nationals in their own countries so they may carry on research activities independently.

Currently the bank is looking at research evaluation techniques based upon its own design of the performance indicator approach. Keeping track of what the Bank publishes, examining reading lists in development economics, tracking Bank publications to their place of use, as well as bibliographic analyses are being used. A key issue in evaluation is the application and impact of the research. Bank officials constantly ask themselves how policy decisions are being improved as a direct result of the research that the Bank supports.

Over the years the Bank has found that the absolute key to research success is in the personnel; i.e., selection of the researchers themselves. The Bank has a rather high turn-over rate in research staff; researchers drift toward operations. By having the in-house capability for research, the Bank is better able to pick and chose the programs and projects it can manage. From one perspective, however, A.I.D perhaps has great flexibility in sponsoring research because of its small in-house research staff. At the

same time this puts a greater burden upon A.I.D in working with contract researchers to assure that the product truly addresses the priority issues of the Agency.

As seen by the Bank its comparative advantages in research are: 1) in problem identification due to the large number of persons in the field with a good perspective on the issues that concern Bank operations, 2) in cross-country research, again because of the numbers and diversity of field staff and the ability to mobilize them on common issues of development concern, 3) in "length of perspective", i.e., the Bank is in business for the long-term, and 4) in implementing research results through the Bank's lending programs.

Looking at the Bank's directives for monitoring and evaluating its activities there are several ideas which seem as applicable to A.I.D. as they are to the World Bank itself:

1. The role of information: Information for development has value in its application and use in decision making; therefore, managers and users of information that is to be generated from a research project should be involved in the research design from the inception of planning that research.

2. The design of monitoring and evaluation in research: Monitoring and evaluation are also best designed at the start of the project planning phase. This helps to insure that the right data are collected and in a timely fashion.

3. Sources of information: No one source of information is entirely adequate; it is necessary that information be collected from a variety of sources and be interpreted in context. Unfortunately, there is often a lack of base-line and contextual information and this must then be developed before a project can proceed. (A.I.D. addresses this issue by including base-line information generation as one of the fundamental types of "research" that it supports.)

Finally, a goal of the Bank's work at all levels is to help build the capacity of developing country groups, private and public, to design, implement, monitor, and manage their own programs and to

understand that information is potentially a very constructive tool rather than a dangerous commodity.

E. Closing Session Summary

During the closing session the following points were reiterated as areas of emphasis and focus:

a). A clear statement of research objectives for a project or program planning phase is the starting point for evaluation and sound management of all research activities. Only when objectives are clearly and explicitly stated is it possible to conduct a balanced and useful evaluation.

b). It is essential that all qualified persons who are part of a research team be involved in the evaluation design. (Mixture of bottom-up and top-down inputs.)

c). Quantitative indicators are under development and are actually in use in research evaluations. There is also a methodology arising from risk analysis experience. One cautionary note is that performance indicators should be developed specifically for each program and not "borrowed" from another research project or program. Although one should pay attention to quantitative approaches, one should not be driven to conclusions solely by the numbers that result from their application. Quantitative techniques need to be utilized cautiously and with judgement. The user must understand the limits of the methodology and the special assumptions on which it is based.

d). A cautionary note also arises from the inherent nature of the research process. Research is a systematic probing of unknowns; there is a high chance that a project ***will not*** lead to the result hypothesized, or fulfill a market need, or result in a product or process that can survive among competing requirements. Research may fail in as many as nine of ten starts but this should not translate into a ***fear of failure***. Management systems need to be sufficiently flexible to allow for both ***initiative*** and ***failure of a project*** without necessarily translating into

failure of the individuals working on the project. Rare are the occasions when research has not enlightened the researchers and managers.

e). Any system used to evaluate research should be applied uniformly and consistently over time. The committment of top management to the system should be clearly evident to all who are involved. Specific performance indicators to be useful need to be designed, tested, and modified over time. The application of performance indicators and questions of continuity and accountability go hand in hand.

f). For its own internal planning purposes in the design of a research measurement methodology A.I.D. officials should consider preparing a draft strategic plan for research that includes evaluation components based on a total systems approach that would include the research itself, field testing, and a scenario for implementation.

GENERAL INTRODUCTION

I. Background

Bradshaw Langmaid

Deputy Assistant Administrator

A.I.D. Bureau for Research and Development

Mr. Langmaid welcomed all the participants to the *Workshop on Research Progress Measurement and Management Decision Making* and served as the chair for the session in which A.I.D. officials presented their rationale for a new and critical look at management practices. He explained that the program management task is a formidable one as A.I.D. works with more than 80 countries throughout the world, although among U.S. Government agencies, A.I.D. is not a major research-producing agency. Nonetheless, roughly 11 to 14 percent of the Agency's overall development assistance program (FY 1991 $1.3 billion) could be classified as research. By far the largest part of the Agency's program is concerned with the delivery of development services. In the Research & Development Bureau (R&D Bureau), however, the figure 30 to 40 percent of its approximately $470 million budget. In absolute terms, the research effort is large, and the Agency is constantly seeking ways to strengthen not only the relevance but also the management effectiveness of its research.

One component of the Agency's ongoing emphasis on strengthening management oversight is the Agency-wide Program Performance Information System for Strategic Management (PRISM) plan. Mr.

Langmaid asked Dr. John Eriksson, Director, Center for Development Information and Evaluation (CDIE), located organizationally within the Policy Directorate of the Office of the A.I.D. Administrator, to give an overview of the PRISM plan and its implementation.

II. An Agency-wide Program Performance Information System for Strategic Management (PRISM)

John Eriksson, Director
Center for Development Information and Evaluation A.I.D. Policy Directorate

A.I.D. is the primary vehicle of the U.S. Government for foreign assistance, administering several billion dollars of bilateral development programs annually through a Washington staff and resident Missions in some 80 countries throughout the world. Historically A.I.D's foreign assistance mandate has been broad, and the Agency has pursued a multiplicity of objectives under the much amended Foreign Assistance Act of 1961.

Under the current Administrator, Dr. Ronald W. Roskens, A.I.D. has embarked on a major reform of the Agency's planning and management systems. These strategic management and evaluation initiatives are aimed at focusing and concentrating its programs in a manner consistent with current budget realities, leading to more strategically managed foreign assistance with greater development results.

A.I.D.'s new Program Performance Information System for Strategic Management (PRISM) is an important part of this effort. PRISM will be a system of performance information systems, built collaboratively with the Missions from the bottom up, as well as the top down.

PRISM has three major components: (1) An Agency-wide program performance database; (2) technical assistance to strengthen operational-level (Mission and Office) performance monitoring systems; and (3) development of common reporting and analysis formats to link these operational systems and make relevant performance information available at other organizational levels.

PRISM has already made significant progress in strengthening program performance information systems in a number of field Missions and some Offices. At this time PRISM can provide management with a reasonably good picture of what and how the Agency is doing in terms of objectives and strategies, especially in the field. It can also outline what the Agency expects to achieve by providing detailed results that a number of Missions and Offices anticipate over the next 3 to 8 years. The detail provided

by PRISM will become sharper by Spring 1992 as programming reforms are implemented and Missions become more fully engaged in strategic planning. By Fall 1992, more detailed performance reports are expected from at least one-half of A.I.D.'s Missions. This should allow ready identification of programs that appear to be achieving results as well as those that are falling short of targets.

A review of PRISM by the Administrator and Senior Staff in early December, 1991, revealed that substantial headway has been made in just one year since its implementation; nonetheless, a great deal remains to be done, even at the Mission level.

While most of A.I.D.'s activities focus directly on achieving significant development results in specific host countries, a portion of the Agency's portfolio is devoted to research that has much broader development implications. During the planning phase of PRISM it quickly became apparent to CDIE staff and to the R&D Bureau management, that measuring the performance of research programs was different from the measurement of results from other programs (family planning; economic policy reform; building an irrigation system; etc.). Also, in the evaluation of the research programs it is not enough to say that research programs are not intended to support directly and specifically Mission program objectives; that they are riskier or longer term than typical field objectives; or that they are only valuable in terms of the quality of the research itself.

In short, research managers, like any other managers, need a better basis for making decisions over time; they need to judge which research programs to continue, which to expand, which to contract, and which to terminate. But what are the criteria for such decisions? What are appropriate indicators for measuring research performance? What is the role of more quantitative evaluation studies?

A.I.D. does not have any easy answers. This workshop -- of experienced research managers and scholars from industry, universities, and government -- was convened to bring insights and to give us some help. No one expects any silver bullets. Our work over the next day and a half is seen as the beginning of a process. We look forward to hearing your insights, receiving your counsel, and discussing

the issues with you.

III. Overview of Research in A.I.D. and Goals for the Workshop

Richard Bissell
Assistant Administrator
Bureau for Research and Development

Dr. Bissell stated that he would not summarize the A.I.D. research program; it is too rich, too diverse, and has too long a history. (See Proceedings Annex entitled "Research Directions at the Agency for International Development, dated July 29, 1991). He preferred to discuss what he felt are some of the contextual issues as that would be helpful in addressing both the problems faced by A.I.D. and the charge to the NRC panel.

To begin, one can start with several basic factors, or characteristics of research within the A.I.D. Perhaps the first of these is the wealth of choices available to A.I.D. due, essentially, to the opportunities available from America's expanding storehouse of science and technology. Over the last 30 years the choices growing out of America's technological strength have expanded constantly, and a comparison of the present A.I.D. program portfolio with past portfolios evidences a process of growth.

Although the basic research funded in the United States is turned into remarkably practical applications that are spread throughout the world by A.I.D., the Peace Corps, and perhaps especially by the 400,000 to 500,000 foreign students who study in the United States, A.I.D. does not fund basic research. Peace Corps volunteers and foreign students who return home provide windows that inspire American technological development and open opportunities for A.I.D. that may not be open in such variety and depth to other donors.

A second factor is the tremendous internal and external pressures for results in real-time. As we

watch the world through television, our awareness of the problems out there increases every day. The internationalization of American society sharpens American awareness about the state of the world. To give but one example, malnutrition afflicts 700 million people worldwide. There are 15 million children under the age of 5 dying needlessly every year. One could cite other examples, but it is sufficient to state that this awareness creates increased pressure on the Agency as it invests in research to make sure that the research expenditures will have prompt and visible impact.

A third characteristic is the need for quality work. AID faces one of the most difficult environments in the world. There are places where data bases are almost non-existent in almost any field -- agriculture, rain forests, epidemiological bases, etc... In effect, the Agency has to go out and create the ground upon which meaningful research can be conducted. Many institutions are weak in developing countries; they need our collaboration, but we also need theirs if meaningful research is to be done. In many respects both we and they face an objectively hostile environment in which to establish rapid and quality results amidst this wealth of choices from which to make investments.

A.I.D. has made certain basic choices about directions of research in this context. As was mentioned before, the Agency does not do basic research, per se. It does, however, engage in long-term and short-term applied research; and increasingly this may be defined as operations research. That doesn't mean it short-changes the long-term strategic research. A.I.D. has made powerful and significant investments over the years in agriculture, health, and environmental improvement, but those investments tend to be downstream where applications are more imminent rather than research to acquire new knowledge.

Long ago A.I.D. made a very basic choice, and that was to integrate the research and development aspects as completely a possible. The Agency has done this in a variety of ways, of which one is in setting the Agency's research agenda. If we can make research needs-driven, then we think we have fulfilled an important criteria. Integration also assists in moving research results into the field as

quickly as possible.

Another choice in A.I.D. research planning is that the Agency has decided it must identify and fulfill tangible, programmable outcomes. This may sound orderly, but it is not; A.I.D. lives in a disorderly environment. It is a government agency that must interact strongly with, and make use of research from other agencies of the Federal Government. Collaboration with the Department of Agriculture, the National Institutes of Health, and increasingly with the Department of Education and with the EPA, creates cross currents in the research area. The process is neither neat nor simple; all parties must work at "making the collaboration work."

In asking the NRC to convene this workshop, we specifically requested that participants bring to this table examples of research measurement systems and models that A.I.D. can look to for inspiration. I am glad to see the agenda and the diverse set of speakers from the United States and abroad who have separately wrestled with the kinds of research management issues that we face. As we move toward the end of one century and enter another, getting the models you bring bounced around and examined in the light of what A.I.D. management and its counsellors see as our reality, and where the Agency must go, will be most helpful.

With such a diversity in the choice of possible investments the Agency could make, there is a constantly increasing pressure to focus our choices. We cannot simply choose, therefore, to fund useful research, nor can we even use the standard of "quick" research results, but must, instead, ask the difficult question "What is the best research we can fund?" Where there are long-term investments, it becomes that much harder to do.

Allow me to give an example. Fifteen years ago, in the Office of Nutrition in the Bureau of Science and Technology, the Agency decided to fund research in the area of Vitamin A nutritional deficiencies in young children. At the time the scientific evidence on the importance of this issue was limited, at best. It was known that Vitamin A had a relation to blindness, but nobody really knew what

the effect was. The payoff for the research came 15 years later; today we know that vitamin A can reduce blindness and reduce childhood mortality by 20 to 35 percent. That knowledge is now going into field implementation, 15 years after investment and testing. I do not know whether we would have better criteria to make a decision on such long-term investment in research today as we did 15 years ago. I would like to think we do, but there is such a variety of tools that we would have to look long and hard at something like that in the context of other alternatives.

It is for such reasons that we in A.I.D. worry about focussing too much on short-term. There certainly may be short-term pay-offs, but as we go back to what the best investments were, there are many different factors. I'm not sure, for instance, that if someone had asked me for a very small amount of research money to eliminate striga from the sorghum plant in the Sudan, I would have recommended the investment. The environment in the Sudan to do such work was hostile. I probably would have declined. Yet we are told that the research can raise the productivity of sorghum by 30%, and this affects the lives of very poor people not only in the Sudan, but in arid zones in general. So the present process of making choices about investments in research has many shortcomings; and it is a little scary in hindsight. It is clear that all of us need help to do better with the limited resources and growing pressures on us today.

The last point to mention is that the outcome of these informal discussions should have utility within the research process; this is not an intellectual exercise. A.I.D. needs to assess research progress in particular projects. There is the internal issue about being able to identify interim impact and keep the project focussed on the best course of action. There is also an external issue, which is particularly important to senior management officials, and this focuses upon how one weighs potential utility and impact of one research course versus another. I hope that the kind of criteria suggested, and models discussed, will be helpful both internally and externally. If it turns out that logic drives us to two different kinds of measures to answer these kinds of questions, that is fine, but I would like to see

something that is unitary if possible.

My colleagues and I in the Agency appreciate the investment in time you are making in this workshop. A.I.D. has a tremendous history in what it has brought to development through its research; we would like to keep that up. These are tough times; it is more important than ever to do the right things. We need help to keep on the right track and to extend our excellent record.

IV. General Discussion

A. Introduction and Background

The general discussion period was opened by Dr. Arden Bement, Jr., chairman, for the workshop. He and 15 other participants invited by NRC
brought to the workshop their personal experiences in strategic planning for research and research evaluation. They were invited from backgrounds in industry, government laboratories, management of research grant programs, and international research on economic and social issues in development.

To help orient the invited participants, A.I.D. provided background information on its current R&D strategy, and on its new initiative for mission, office, bureau, and agency-wide strategic planning and program performance system. Included in the annex to these proceedings are two relevant background documents: a) "Research Directions at the Agency for International Development", and b) "An Agency-wide Program Performance Information System for Strategic Management (PRISM)."

B. Terms of Reference

Specifically, the terms of reference (A.I.D. charge) requested that:

1. Participants, through panel presentations and discussions, focus on alternative approaches to, and processes for measuring the performance of research and development programs including setting of goals and the selection of performance indicators.

Here the objective was not be to describe in detail particular methodologies but to probe beyond

method to substance, addressing questions and issues such as the following:

(a) What does a research manager need to know to determine if the research is: high quality, cost effective, and will probably lead to the end for which the research program or project was designed?

(b) How are priorities developed for the research program, how is it determined if they are appropriate to the goals of agency or firm, and if the management structure is appropriate to implement the research?

(c) How does the passage of time affect a research project or program? Consider near term and long term (3, 5, and 10 years down the road.)

(d) What information is most important for the Agency's top decision-makers to have in relation to investment in, and measurement of research results?

(e) How should decisions be made about continuation of a research effort? What measures need to be taken to assure that information gained in a research effort is available both currently and in the future?

2. Participants were also asked to discuss under what conditions and in response to what type of objectives certain performance indicators are appropriate. This was to include both qualitative and quantitative indicators.

3. Participants were to suggest principles and management procedures that should govern the establishment of research objectives and indicators of progress toward those objectives. Both the principles and the procedures should meet recognized scientific standards.

4. A product of the workshop would be a proceedings document summarizing salient discussion points and recommendations. Given the informal nature of the workshop no effort would be made to achieve group consensus on conclusions or recommendations.

The chair noted that the presentations by John Eriksson, Director of the Center for Information Development and Evaluation, and by Richard Bissell, Assistant Administrator for R&D, along with the background documents previously cited, gave the participants an introduction to the diversity and character of the research thrust in A.I.D. For an overview of research planning and evaluation methods and practices in public and private sectors that was sought by A.I.D., individual participants were asked to share their experiences. For practical considerations, presentations by NRC participants were grouped into 3 "sectoral" panels (industry, government agencies, and international donors) and 2 "functional" panels; one based on the role of the World Bank and another on agricultural research. The totality of the A.I.D. and participant presentations would then form the basis for questioning and discussion among all persons. These informal discussions and exchanges would begin a process by which A.I.D. representatives would, in their own manner, carry on a review of research strategy formation, and of program and project evaluation methodologies. The workshop, therefore, should be viewed as an introduction to a continuing process within the Agency.

C. Categories of Research that A.I.D. Manages

The Chair, Professor George Walter, Dr. Lowell Hardin, Dr. Bryant Rossiter and others engaged Dr. Richard Bissell in a discussion about the categories of research that A.I.D. manages.

First, the categories were grouped by Dr Bissell as follows:

- Strategic Research, which is long-term (10 years, perhaps longer) applied research, targeted toward a specific development problem whose pay-off must be quantified over longer time spans than the normal 3-5 years for Agency projects.
- Adaptive Research, called "development" in industry, targeted to specific sites (environments) that draws heavily upon known research results developed in the U.S. or elsewhere.

- o Operations Research, for the integration of country-specific social and cultural issues.
- o Data Base Research, for generation or maintenance of information on global development issues. (Strategic, adaptive, and operations research cannot proceed until data base information is available.)

Dr. Bissell stated that the A.I.D. goal whatever the category of research and wherever its use may be applied, is to enhance the capacity of a country to do its own research, and/or to facilitate adaption of research, where appropriate, from differing social and cultural traditions. A.I.D.'s role is not to conduct the research itself but rather to catalyze and enable targeted research to be done. Additionally, the Agency feels it has a responsibility to facilitate knowledge transfer, to link researchers to one another, and to train leaders for problem solving. This manifests itself in the formation of human capital, for which A.I.D. has a long history, and for participation with counterpart organizations in institution building.

D. Strategic Planning

Prof. Walters noted that A.I.D. emphasized the importance of the quantification of pay-off over long time periods as a requirement for entering into strategic research. He inquired about A.I.D. experience in, and success with quantification methods. Dr. Bissell stated that he did not want to imply that there are well defined and broadly accepted methodologies to quantify research pay-off, as in cost-benefit analyses, but there are perhaps other ways to measure results. These include improvement in the quality of life indicators and human survival statistics. It is difficult to attach dollar values to those measures. Similarly, it is difficult to quantify and give dollar values to such research advances as improvements in productivity, environmental conservation, protection of biodiversity, etc. Nevertheless,

efforts at quantification are being made and those techniques have utility when used with the proper caveats and cautionary notes.

Dr. Rossiter shared some of his perspectives on strategic planning explaining that he wished to comment from his industrial background and his experience as a member of the A.I.D. Research Advisory Committee (RAC).

o First, it was his observation that A.I.D. does a good job of strategic planning at the Mission level; i.e., in the developing countries. That is because it is always easier to do planning where goals and issues are quite well defined. And the Mission level is where the provider (A.I.D.) is directly involved with the customer (the developing country and its institutions.) It is more difficult in the research arena, and this is recognized in A.I.D.

o One of the problems for the RAC is that when a research idea is brought to it by A.I.D., questions are raised on the basis of a single project. Priorities and choices are always important in industry, but in A.I.D. it is very difficult to see relationships because options are not usually presented. Too often the question of how a proposed project relates to other choices in the A.I.D. research portfolio is not asked.

o Another observation by Dr. Rossiter was that there seems to be a lack of accountability for research. This isn't to imply that the people are not accountable, but that the span of continuity is short. A program conceived and started by one group in A.I.D. requires such a long gestation time that the originators "move on" to other assignments. Projects are too often left to someone unfamiliar with its origin and goals to complete it. Projects get caught up in goal changes and management changes and the research suffers.

o Closely associated with accountability are time frames in which things happen. In long-range research it is difficult at the outset to specify the critical path to complete a task. A.I.D. often stops its support at the stage of producing trained personnel or writing a report about a "pilot" experiment rather

than carry through to actual application in the country itself.

In summary, the personal feeling of Rossiter was that the Agency does a better job in identifying general goals than in identifying the critical gaps. In industry one asks, "Where are the bridges out?" That is where research serves. That process is what some in industry call "situational analysis" — where one identifies the competition, the real problems of the market, the actions needed to effect a change. Although Rossiter recognized that there are those in A.I.D. who are also very much concerned with that same pragmatic approach to change (or to "development"), it was his personal feeling that until strategic planning for research is internalized to a greater degree than in the past, it will be difficult for the Agency to achieve answers to many of the pertinent questions.

In responding to Dr. Rossiter, Mr. Lawrence Saiers, A.I.D. Policy Secretariat, remarked that the Agency is keenly aware of the global environmental setting in which it must operate. The world is becoming smaller and more internationalized, but that does not mean that it is less complex. Strategic planning cannot proceed in a vacuum and must be directed at a specific audience that is carefully identified. Although the Agency is not terribly far down the road in the global strategic planning process, it has consulted with its own staff both at headquarters and in the field, with congressional staff, with private voluntary organizations, with think-tank organizations, etc. The process to understand the new world environment has begun, and it is continuing so that pragmatic long-range plans and objectives will be formed as RAC and others have recommended.

Dr. Donald Plucknett, senior scientist for the Consultative Group for International Agricultural Research attached to the World Bank, stated that A.I.D. did not have to be too modest when discussing agricultural sector research and development. Prior to World War II there was no international research effort in agriculture. A.I.D. and a few private foundations led the way in commodities research in international agriculture and created the green revolution which increased food grain production worldwide very dramatically. Strategic planning in agriculture has paid off handsomely over the years.

A.I.D. is now taking a leadership role in the world effort to develop more sustainable agriculture practices in developing countries; i.e., sustainable in the sense of "partnership" among agriculture, environment, and human values.

E. Project Planning

Dr. Gregory Ingram from the World Bank policy and research advisory staff cautioned participants not to mix decisions about allocation of resources across different research areas with decisions about project selection. Techniques for decision making about resource allocations across sectors are very different from decision making about projects within a given sector. How does one go about justifying more money for the environment versus money for population, or health, or education programs? One needs to consciously think about differences. Opportunity-driven decisions versus requirements-driven decisions in research are a kind of project evaluation, not a program justification process. There is a literature about the rate of return to research and it is not built on specific projects. That literature is very aggregate in its scope.

Dr. Gerald Britan, A.I.D., added that there are other categories of literature related to decision making; e.g., about how to define objectives, how to select research areas, and how to measure progress over time. But even if one may know where he wants to go in research, if there are no signposts to check along the way, one will never know the right road has been chosen. A characteristic of research in A.I.D. versus that in industry is that the successful private firms tend to make sound research decisions over time as to which research areas warrant investment or abandonment, when to shift from more basic research to development research and finally to commercialization. Most experience in A.I.D. indicates that the Agency has been poor at development and commercialization. One of the things A.I.D. is asking this panel is how to get better at that process.

Mr. Bradshaw Langmaid and Dr. Eriksson, A.I.D., said that the Agency feels generally

comfortable with its assessment of its market - the developing world - but is less comfortable in judging alternative investment choices needed to change the nature of the environment versus delivering of services to improve the environment. Judging choices between major investments in pharmaceutical delivery services, training people, etc versus investing in development of a vaccine has very different risks. The eventual pay-off for a vaccine may be greater but intermediate risk factors such as safety, or delays in understanding how a vaccine will work in human subjects may be far more difficult, time consuming, and costly than A.I.D. can afford. Decisions of these kinds are very difficult for the Agency. Thus it does not seem too useful for this group to debate issues involved in changing the nature of a market vs delivery of services to that market. Dr. Eriksson emphasized that A.I.D. was anxious to benefit from the experience of the participants concerning management and monitoring of the research process. What are the kind of indicators to look for in the process of research monitoring - incremental decisions, mid-course corrections, decisions to expand or to cut back, decisions to terminate.

At this point Dr. Bement brought this introductory phase of the meeting to its conclusion so that the various panels of the participants could begin. He thanked Dr. Bissell, Mr. Langmaid, Dr. Eriksson, others from A.I.D., and all who participated in the explanation of the charge and the discussions to understand its implications.

PRESENTATIONS from INVITED PARTICIPANTS

I. Industrial Sector: Perspectives, Experience, and Discussion

Pharmaceutical Industries	Dr. D. L. Azarnoff
Battelle Columbus Laboratories	Dr. J. J. Duga
Manufacturing Industries	Dr. A. L. Bement

A. Dr. Azarnoff:

The pharmaceutical industry operates under the special situation that it is a highly regulated activity with many aspects of the introduction to the market mandated by law and federal regulations. Even after licensing of a product for marketing, quality control and product distribution are subject to designated standards and monitored practices. Thus to be profitable a company must pay attention to product control and accountability in ways that are unique to the pharmaceutical industry. These requirements add significantly to costs of research, management oversight, and general operations.

Nevertheless, a great deal is now known about the average cost of developing a new product and the probability that any one of those products will meet with success in the market. About one out of twelve new technical entries survives the rigors of testing and regulatory requirements prior to marketing, and only 25% of those that survive recoup their development costs.

In setting priorities, companies not only look at market opportunities but must also consider profitability using standard business practices for return on investment. The target may be a drug or treatment for an indication where there are no other responses available. This carries high risk and offers the potential for high profitability. Or one may look for a drug which is an improvement over others that are currently on the market. This carries a lower risk, and also a lower potential for profitability. Pharmaceutical companies also seek out products which are changes in delivery form; i.e., a capsule

rather than an injection, or a skin patch to deliver a medication. Strategic planning is very much at the heart of research in the pharmaceutical industry.

The A.I.D. is concerned with "development" of human resources, of activities for new environments, of management systems. In "development" work with which I am most familiar from the pharmaceutical industry, I would conclude that evaluation of the past is a difficult choice because one never really knows how the present evolved from the past. The present is a compilation of research work done internally, opportunities that presented themselves, research of other organizations, and a host of other factors. It is perhaps easier to evaluate the unknowns; i.e., the impact of R&D in the future, by taking a look at what the research program intends to change, what the probabilities are of success in the research and the probabilities of the adoption of the results of the research. The impact of successful research on both the company and on society must be considered.

Another of the questions that A.I.D. has referred to the participants deals with management structure. "Management", in my opinion, depends on the personalities and qualities of managers. You can go about management in a variety of ways but my experience suggests that one should delegate authority (and responsibility) as much as possible to lower levels. Top management must still review overall progress, resource allocation, and distribution. But employees, in research, in production, or wherever else they may be within the structure of an organization, respond more responsibly when they are taken seriously and given incentives to perform.

B. Dr. Duga:

As Dr. Bissell spoke he stressed that A.I.D. must make its program and research choices from a large number of alternatives available to it; that A.I.D. is always under pressure for results, and to achieve the results as quickly as possible; and that quality is the essential factor in whatever the Agency does.

As I listened, I was struck by the similarities to my work in industrial research and research management. Battelle Columbus Laboratories is a large contract research and development institution that provides services for industry and for government, including the A.I.D. From our experience with all kinds of industry and over the years with the A.I.D., I see few differences in the project selection and evaluation processes be they performed for an industry or for the Agency. For A.I.D. it is essential to determine the environment for research programs and the social and economic benefits that can accrue if the research program is indeed carried out. Increasingly, the same may be said for an industry sponsored project. To do so industry uses benchmarks to compare the performance of one product (or outcome) versus another. At the macro-level industries themselves are ranked on the basis of "benchmarks" such as technology employed, operational efficiency of management, organizational structure, or overall operations (return on investment, productivity, etc).

Similarly, A.I.D. has an opportunity to measure itself against the kinds of environment it faces and the kinds of activities supported by other agencies - its impact in country "A" versus impact in country "B". It is also important to look at research using time as a frame of reference - how does today's program of research or that which is proposed for the future compare with the past.

And finally, we must also observe that A.I.D. is constantly faced with special situations because their operations are in different countries. Policies, politics, priorities, institutional structures change in ways that A.I.D. cannot anticipate. These situations make it difficult for research and demonstration programs to succeed.

C. Dr. Bement:

In my remarks I will focus principally on: a) performance indicators and conditions that make those indicators relevant, and (b) principles and management procedures that should govern in the establishment

of research objectives.

First, I will remark on differences and similarities between industry and A.I.D. In my judgement the reference document by Dr. Britan "Measuring Performance for Federal Agencies" written as a discussion paper for the General Accounting Office, is as applicable to most industries as it is to government. But that may be a bit of an abstraction. To make a comparison between a large corporation and the A.I.D. is a bit strained. In the case of some industrial organizations decentralization is as much an organizational reality as it is in A.I.D. Often decentralized industrial organizations, like A.I.D. shy away from basic research. "Targeted" research, development, may be the more common element. In my experience performance indicators in decentralized organizations are not yet the norm, although industry has been working on them for a number of years. There are some guidelines, however.

One is the importance of having a common language within your management system. This involves a rather inclusive series of guidelines and documentation formats for strategic planning, resource allocation, operational and action planning, compensation, performance appraisals for organizations and individuals, monthly management reports, etc. Systematic training and education within the general management structure is often used to assure that common nomenclatures are used, understood, etc.

A second point is that goal setting and performance evaluation in any hierarchical system must be consistently applied and should be understood from the "top-down" and the "bottom-up." It is essential that goals are written in the language of the performance indicators so that one knows what the targets are.

Organizations are not accountable, individuals are. It is essential that the goals are written in the language of the performance indicators so at least you know what the targets are. This creates a direct coupling and alignment of performance indicators and the language of the goals at each level in the organization.

The question of what does the boss need to know, when does he or she need it and in what form,

really comes from understanding "success factors" and these come from the individual goals. Success factors have to be defined in terms of accountability, responsibilities, decision making authorities and also the idiosyncracies of the individual - leadership style, etc

Now, performance indicators in PRISM have also been seen as necessary for "early warning" systems. Relying primarily on performance indicators, however, can get one into a "trap". Performance indicators have to be recognized and interpreted correctly. They have to be reported in a timely manner up the chain of command. If that is not done then information becomes highly filtered and red lights have a tendency to turn amber, and amber becomes green. So early warning systems need to be robust, need selective probing, and need validation.

The Chief Executive Officer (CEO) of a firm or the Administrator of the A.I.D. clearly are high-level risk takers. There are some programs that capture the attention of these individuals either because of political sensitivity, customer interest, etc. It is a mistake however, to assume that these are the sole interest of the CEO or the Administrator. One has to be reasonably selective what is reported and the quality of the information relayed to the top has to be high. One has to recognize, also, that there are two chains through which information gets passed up the chain-of-command. One is the management chain which is for the more immediate and focused information. The other is the staff chain which pays more attention to the technological and programmatic details and health of the organization.

As the person who has to be "eyes and ears" for the research function of an industrial organization, I pay attention to R&D investment as a percentage of sales and correlate this with growth of a division. I have to ask if the technologies will address future needs and requirements. This is essential to protect our investments for future markets, etc. I worry about turnover, the number of new R&D starts as a fraction of the total budget, and about projects completed as a fraction of the total budget. Programs have to be dynamic. There is leverage in the percentage that R&D is relative to total revenue. All these are indicators.

I believe it is well to close at this point and ask for discussion on the industry panel presentations.

D. Industry Panel Discussion

1. R&D and market success

One question which is rather specific, and rather narrow is this - - - pharmaceutical companies in general have large and costly research departments and markedly different results in terms of market success. Is there any analysis of why this is so? Merck, for example, does very, very well.

Merck has been studied and various observations have been given for its phenomenal success. Senior management in Merck came from R&D and not from the business management or marketing areas. The CEO came out of research but it is said that the first thing he did when he took over the top management position was to make a major commitment to strategic management and planning. Secondly, it is clear that Merck has succeeded in rationalizing the R&D activity. Expected results are targeted, they do not just happen randomly. Merck has a mission statement that specifically quantifies the targeted results. Senior project leaders stay with a project from beginning to end. They build their own teams and work with them. There are periodic reviews or "exams" so that a project can be cancelled if targets are not going to be met. New projects are re-assigned to a team to keep it together.

Merck makes a very heavy investment in research, spending 16% to 17% of gross sales on its R&D; it probably has the largest R&D budget of any pharmaceutical company. It hires highly competent scientists; it has tremendous depth. As soon as a piece of information (from research from any source) becomes available, Merck has the resources to follow-up to see if that new information has relevance to its R&D program.

Merck also has a very good understanding of the market; it seems to be customer oriented. In summary, Merck seems to have a clear concept of what constitutes strategic planning and management

and it continuously sharpens its tools to improve its performance, in R&D, in general management, in customer relations, in understanding its market.

2. Models of strategic planning and measures of success

Dr. Rossiter's first comment dealt with models, or a "process" for strategic planning. At first strategic planning in industry was considered to be the prerogative and responsibility of the manager of a firm or a department. This, however was not so successful as it ought to have been. Then organizations formed special groups or even permanent units in charge of strategic planning but the plans that resulted often were removed, even unrealistic in terms of the market and the production process. A newer method of strategic planning is to involve the entire organization. Production workers and their immediate supervisors have a role to play in making the planning realistic and practical, higher management in setting overall goals and policy. To be most successful, strategic planning needs to be a combination of "bottom-up" and "top-down" approaches to problem solving and management.

A second comment, also by Dr. Rossiter, was the query of how various research departments deal with the termination of a project. He observed that <u>most</u> R&D does not reach the market but that does not spell "failure". The nature of research in recent years is that a 10% success rate is considered good. A.I.D., however, might have great difficulty in convincing the Congress that only 1 research project out of 10 starts would lead to an adoption in one or more developing countries.

Dr. Bement in addressing the question of "success" and "failure" stated that R&D programs that are terminated often produce information, products or processes that apply more generally to the operations of the firm or industry. Only when you terminate a program <u>without</u> learning from that program, can the R&D be considered a "failure."

In looking at methodology for strategic planning, Dr. Bement stated that a strategic plan first ought to be built on a strong functional plan. This assures that when engineering or R&D departments develop a functional plan those in-depth analyses identify barriers and where opportunities exist. That information is essential for strategic planning as one moves up the ladder of responsibility within an organization. Overall, strategic planning is a process that needs the inputs from responsible individuals at all levels of an organization. Then the manager works with the basic material to shape it for the overall goals.

In remarks to apply the industrial experience to the case of A.I.D., Dr. Duga stated that the Agency has a variety of clients (field missions) but that it is still possible to plan a research program that addresses a wide ranging clientele. A.I.D.'s strategic plan should take into account the variety of opportunities, challenges, and clients it serves. At the top level of management this requires a definition of the environments and the goals; at other levels it requires the freedom, and the resources, to adapt research to particular situations.

3. End point of research in A.I.D.

Ms. Johnson a project manager in A.I.D. stated that one of the encouraging aspects of the PRISM (Program Performance Information System for Strategic Management) activity is that it takes line managers seriously and allows them to participate in the process of strategic planning. In applying the principles of PRISM in the field missions to A.I.D. research, Ms Johnson observed that there is very little expertise in the "D" of the "R&D" process in developing countries. Research seems to be done "to publish" rather than "to apply". Can R&D then be considered "successful?"

Dr. Bement recognized that research to develop the knowledge or technology base of a nation is a legitimate end in itself, but this is not what was being discussed in this workshop.
In his view, R&D for developing countries is a continuum and the process is not completed until the

technology is used, until a product is being sold. Besides the acquisition stage (the "R"), there is also the adaptation stage and the integration stage. Thus one is dealing with socio-economic factors inherent with receiving a technology and putting it to use.

II. International Donors: Perspectives, Experience, and Discussion

The Netherlands	Dr. S. Volbeda
France and the Europoean Community	Dr. J. Gaillard
Canada	Dr. T. Dottridge

A. Sjoukje Volbeda

The program of technical assistance of the Directorate-General for International Cooperation, Netherlands Ministry of Foreign Affairs makes an extensive investment in research, currently over 170 million gilders/year, or about $50 million expressed as U.S. dollars. Over 50% is for capacity building in developing countries. There is a separate account called research, done in cooperation with Dutch universities, that includes a wide variety of subjects, but always related to a specific project. In addition there is about $10 million in U.S. dollar equivalents of direct support to institutions in developing countries. There is also a separate fellowship program. Another classification is policy identification (preparation) research having a time span of about one year; policy sustaining of 2-3 years; models and methods research, about 4 years; and technological research (adaptive) which has a variable lifetime depending upon the subject. Scientific research is always in a specific sector such as rainforests, health, agriculture, etc. Agriculture constitutes about half of the scientific research.

There is a new effort to integrate all research into one strategic plan, and we call this the "Spearhead" program. Our goal is to include under the one umbrella technical advice, coordination, capacity building and some 10 to 15 regional programs which, in turn, seek to have a totally integrative approach to projects. We want research to be an integral part of the development process. Instead of being supply driven, or donor driven, as it used to be, we want research to be more integrated. In the longer term, we see research as product rather than process. We recognize the dilemma of the differing

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priorities of the government agencies involved, issues of the criteria of managing and financing, setting the agenda for research, and getting participation of everyone involved, particularly at the local level in the developing countries. In the end our goal, and that of our partners is one of getting the best use of research results.

B. Jacques Gaillard[1]

This presentation deals with the evaluation practices in France and in the European Community, focusing on specialized tropical research institutions and funding programs aimed at structuring and strengthening the European scientific communities involved in tropical science.

While some 10 years ago most scientists were opposed, or at least reluctant, to evaluation other than "peer reviews" of scientists, evaluations of scientists, research programs and institutions are becoming common practices today. Thus, evaluations are receiving a wider acceptance among the scientific community not only to assess scientific achievements, but also as a tool to design and implement better science and technology (S&T) policies. At the same time the need for 'quantifiable criteria' has been emphasized leading to the concept of output and performance indicators. This short presentation deals with evaluation practices in France and the European Community (EC) focusing on specialized tropical research institutions and funding programs aiming at structuring and strengthening the European scientific communities specializing in tropical scientific research. It also proposes general recommendations derived from a number of evaluations with which I have been associated. But first, I think it is necessary to briefly present eh specificity of the French system and the EC's programs as

[1] Text of presentation revised by Dr. Gaillard.

compared to the U.S. system.

France

<u>French Cooperation in S&T for Development</u>

As you may know, in France there are no research donor institutions such as A.I.D., the Canadian International Development Research Centre (IDRC,) etc., but research institutions specializing in tropical research. The French system so far has been (and is still) more researcher than program oriented. The main course of the system is set by three Ministries, which have traditionally relied on a number of specialized institutions, of which the Institut Français de Recherche Scientifique pour le Développement en Coopération (ORSTOM) is one. There are two others: the Centre de coopération internationale en recherche agronomique pour le développement (CIRAD), which is entirely devoted to agricultural research; and the Institut Pasteur Outre-Mer (IPOM), which is the overseas Pasteur Institute working on tropical diseases.

Despite a clear reorientation towards Latin American countries (mainly Mexico, bolivia, Ecuador, and Brazil) and to a much lesser extent Asian countries (mainly Thailand, Indonesia, and soon most probably soon Vietnam) during the 1980's, priority is still given to French-speaking African countries. As for research areas, agricultural research is receiving the bulk of the support, followed by medical research. Reforms implemented between 1982 and 1985 have slightly modified the structure and the direction of the system, allowing among other things, a greater mobilization of the entire French scientific community, better coordination as well as a broadening of the research areas to be supported. These changes have been accompanied by an increased participation in the international research system and a

renewal of methods of cooperations, thus, partnership research is gradually replacing scientific and technological assistance.

France is also working at strengthening European alliances. The idea of a European Association is envisaged to establish between member countries joint channels of scientific cooperation with developing countries. The establishment of a European Foundation for supporting research activities in Africa has also been proposed by the French government to the European community. Collaboration between European institutions have also been enhanced by the Science Technology and Development (STD) program of the EC.

Evaluation Activities and Research Management in France

Given the specificity of the above described system evaluation activities have tended to focus more on the evaluation of the researchers themselves, through mainly in-house peer reviews, than the evaluation of the research programs and institutions. Evaluation of departments and research units have however become more and more a common practice within CIRAD and ORSTOM during the 1980's.

At the national level, a committee, le Comité national d'évaluation de la recherche (CNER), was established in 1989 to evaluate the implementation and the results of the national S&T policy. CNER is responsible for the evaluation of research programs, institutions and procedures. It reports directly to the President of the Republic through an annual public report. Based on the outcome of the evaluations, CNER submits suggestions and recommendations to the pertinent Ministries. Seven evaluations have been carried out so far, and ORSTOM is among the next institutions to be evaluated.

The Specificity of Research Activities in Cooperation for Development

Even if research in partnership is more and more acknowledged as a key variable, one can clearly distinguish three different objectives which are at the same time conflicting and complementary: research; cooperation (partnership); and development.

Experience has shown that the evaluation of a researcher's[2] (or research program's) achievements depending on which one of the three objectives is concerned cannot be evaluated the same way. The evaluation should differentiate between the achievements of the research results, the extent to which the work has been carried out in partnership with scientists from developing countries, and the contribution to development. This requires a different set of indicators and a different kind of expertise. In the case of research one needs to assess, for example, the validity, the recognition, and the degree of innovation of the research results. This can only be done by individuals who are themselves competent researchers in the fields concerned. The degree of cooperation and partnership can be, for example, evaluated by measuring the degree of involvement of the various partners in the project design, the balanced sharing of activities among partners, the exchange and training of scientists among the partners, and in general the variety of mechanisms set up to exchange experiences. The contribution to development is even more complex. It should involve not only indicators of adoption of research results, but also the overall impact of these results on production and society as a whole. This also requires another type of expertise which most often is not available in a research institution.

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[2]The criteria for evaluating researchers at ORSTOM are being reconsidered. In addition to evaluating the scientific achievements, the peer review committees are given new evaluation criteria, including implementation of research results, diffusion of knowledge in general, research in partnership with scientists from developing countries, research supervision and management, and research training.

The outcome of the evaluation will finally depend on the relative weight one decides to give to each of these objectives and to each of the corresponding criteria, as well as on the negotiation taking place among the different actors involved in the evaluation process. For strategic planning exercises of institutions such as ORSTOM and CIRAD, one often needs to combine these three types of evaluations.

The European Community

In Europe the funding at the EC level represents approximately 3 percent of the total European public R&D funding. Although this level of funding could be considered relatively low, it has important marginal effects and it influences both the definition of priorities and the scientific collaborations in Europe. Since the early 1980's, the EC has conducted more than 40 evaluations. In addition to scientific and technical achievements, the objective of these evaluations has been to assess the "added value" due to the implementation of these activities at the European level.

This requires an appropriate methodology to measure intra-European cooperations and networking effects. This methodology is different from those normally used in the evaluation of scientific outputs. Specific indicators have been developed and become essential tools of science management of the EC's programs. This includes indicators measuring the improvement of the relationships between "pairs of laboratories" or between a whole set of laboratories, the transnationality of papers and their scientific impact as compared with other papers published in the same journals and the constitution of a "European club of contractors" as shown in the Evaluation Report of the Non Nuclear Energy Programme (Callon, M., et al., 1989). Another important outcome of these evaluations is that objectives and goals of the EC programs are linked to evaluation criteria. Thus, the importance of planning evaluations already exists during project design. Hence, evaluation plans are becoming an integral part of effective planning.

I discuss below in greater details the only research funding program within the EC aiming, among other objectives, at networking laboratories in Europe and in developing countries.

The Program on Science, Technology, and Development of the European Community

The Program on Science, Technology, and Development (STD) of the European Community is specific as it intends to promote collaborative research between the laboratories of member countries of the European Communities and those of developing countries in the field of tropical medicine and agriculture. The program is entering its third phase (1991-1994), the second phase being now evaluated. A typical project involves at least two research teams in Europe and at least one developing country team with an average budget of US$300,000 per project.

The objectives and expected results are, again, manyfold. They include development of knowledge and contribution to development, but also other results as well, especially:

the identification of competent laboratories in Europe and in developing countries having an interest to collaborate (even if they were not at first dedicated to tropical studies);

- the strengthening of collaboration among them;

- the contribution to a gradual structuring of scientific communities in the developing countries;

- the structuring of a European scientific capacity in the field of tropical agriculture, environment and health.

The multidimensionality of objectives is reflected in the individual assessment form being used by the experts evaluating the proposals submitted to the program. Thus, criteria have been devised to evaluate the degree of innovation, the feasibility, the appropriateness of cooperation, the strengthening of research capacities in the developing country, the training and exchange of scientists, the links with work supported by EC programs or member-states, the relevance to development as well as some environmental implications. A relative rate is given to each of these criteria which leads to a final ranking in three categories: excellent; good; or rejected.

The evaluation of the first phase has, among other things, demonstrated a clear bias towards scientific quality warranted by the competent laboratories selected in Europe as opposed to capacity strengthening. Very few approved projects had been designed by scientists in developing countries, and less than 1/3 of the projects have been administered by research teams from the developing countries. The research training component also proved to be very weak. Representatives from the developing countries also requested that more for their experts be associated with the selection process. These criticisms and recommendations have been taken into consideration in the implementation of the second phase of the program.

One of the objectives of the evaluation of the second phase of the program is to propose a system for strategic management which would better take into account the variety of objectives of the program. It is, however, too early to report any result.

As a way of conclusion, I would like to raise a number of general issues and recommendations derived from the above examples as well as a number of evaluations with which I have been associated.

General Issues and Recommendations

It is a necessary, but not a sufficient, condition that evaluation begin at project design by providing a clear definition of objectives and expected results, as well as adapted indicators. One should also recognize that different objectives require different types of evaluations and different types of expertise. One should also balance the power of different actors invoived in the selection and evaluation process in order to better reach these objectives.

Given the multidimensionality of the objectives, which is most often the case of donor organizations, there is not a singie evaluation methodology which can provide convincing results. Several, sometimes overlapping, methodologies are necessary. They may include peer reviews, enquiries addressed to grant recipients, institutional studies, contextual studies, just to give a few examples. This is the convergence or the divergence of the results obtained with the different methodologies that can bring about more convincing conclusions.

Most often economic or financial indicators are inadequate for assessing scientific or technical projects particularly in developing countries. Strict adherence to a financial calendar can endanger the logic and success of the scientific calendar. Contextual factors and in particular socioeconomic issues are often most important to explain a project's failure or success.

To evaluate the work of grant recipients it is most often advisable to adopt a pro-active approach. This will diminish the risk of having them taking a negative view and perceiving the evaluation solely as a control of the use of funds and ultimately as a sanction.

The results of the evaluation should be public and transparent. They should, as much as possible, be made available to the people who contributed and/or participated in the evaluation (e.g., through mail questionnaires or interviews). Scientists, in particular, often feel that the money spent in evaluations is lost and stolen from research. Thus, the importance of proving the usefulness of evaluation and indicators as a tool for improved science management.

Evaluations are expensive. The cost of conducting evaluations and even more so the cost of implementing the results of the evaluations are most of the time badly underestimated. Who has not participated in evaluations whose results have not been implemented just because no provision had been made for it or because it was not made clear from the beginning who should be using and implementing the evaluation results? I have.

C. Timothy Dottridge

The International Development Research Centre (IDRC) is a Canadian public corporation funded by the development assistance budget, and mandated to contribute to development through applied research. Based on a study done for us by John Lewis in the 1980's, it is estimated that the total budget going from donors to development centers to be about 1.5 billion dollars. Up-dated, that would amount to approximately 2 billion dollars.

1. IDRC's Comparative Advantage

In looking at strategic matters, the question of an IDRC comparative advantage comes up time

and time again. The question of comparative advantage for a relatively small donor is very important to us. This has led to decisions such as the reduction in assistance for agricultural research, the move away from some of the bio-medical fields, and movement toward the social and systems areas.

Although we are involved in some experimental development projects and push for greater dissemination of useful research results, we do not have any choice as to how much goes into (basic) research; but we do have a choice in setting program priorities. This has been accomplished up to now by taking a broad reading from our regional offices, and consultations with developing country scientists and policy makers.

The IDRC's comparative advantage also lies in the fact that nearly half of our board of governors are from developing countries. IDRC, then, is looking to fund research for research output, and to some extent for capacity building. We do not make a clear distinction. The objectives at this broad level in deciding how much we are going to put into agriculture, health and so on, have tended to be set more in terms of delivery, rather than in terms of measurable outcomes. Given the size of our agency and the size of our projects, we have looked at the impacts of IDRC funded research realistically. And reexamining the impacts of past projects (what changes occurred in the way people live and function) has required all actors at all levels to reconsider their activities, and resulted in the IDRC revising its strategy, much in the same manner as is A.I.D.

At the project level, things are a lot clearer and objective setting is easier. It is imperative at this level to link potential research results to implementation in the field on a planned basis rather than waiting for the final product. In addition to interim project measures, we are concerned with whether or not the research results are being used at all, and finally, we are concerned with the impact -- the effect -- that

research results are actually having. There is usually somebody benefitting, but there are also some costs associated with the use of the research results.

2. Research Environment

The environment of the developing countries is of major importance to us because the research that we fund is undertaken by developing countries scientists in their country.

It is very important for us to build the kinds of capacities in developing countries that we are talking about here -- the planning and evaluation of research, the strategy setting for research. As we look at the next 5-10 years, our strategy should take into account the possibilities of accessing new knowledge that will be generated and technology that will be produced. This can be a key for deciding which countries can harness their potentials and which cannot. In addition to having some competent research scientists, you also require some kind of strategy for fixing which areas to support. I feel that current understandings of how research in developing countries functions is inadequate.

In looking at the questions of efficiency of research, or cost-effectiveness of research, we recently held a meeting of specialists in livestock to look at what the minimum resource requirements would be necessary to carry out a specific research project of productivity with small ruminants, and compared this with some station level data. There was a very poor fit in all respects. This is not to say the expert group was correct. Our look at what the minimum critical mass in certain stations and research centers should be is very important in deciding the kinds of efforts the IDRC should support.

We think it would be helpful to give greater authority to the users of research, whether they be

policy makers, farmers, etc. The key is not to only get quality research, but to get effective research that is relevant and used in the field. That is the key thought of those agencies such as IDRC that are funding research for developing countries. Yet another problem in our relations with developing countries, has been in getting them to set targets for achievement.

My last point is to say is that IDRC has just prepared a strategy paper; it is currently available (*Empowerment Through Knowledge*, IDRC, November 1991). We expect the IDRC project size will increase and pressure on our staff will increase, too, because these projects are fairly labor intensive. One area that will be opening up is our research systems development program. This is an attempt to understand what are the conditions which promote improved research in developing countries. With the cooperation of researchers from developing countries, we will be looking across the board at methodologies, resource allocation, models, etc.

D. International Donors Panel Discussion

1. Discussion was dominated by an examination of differences arising from supply-driven vs demand-driven research.

a. One observation coming from A.I.D. was that the "push" from supply-driven (usually from the physical and biological sciences) tends to be far more dynamic than the "pull" from demand-driven (usually social and behavioral sciences) research. Usually if one asks the A.I.D. Missions what kind of research is needed, one tends to get a six-month time horizon - things needed in the short term to solve immediate problems. It is the supply-driven people who tend to see break troughs farther down the road. Nevertheless, given the nature of A.I.D., the pressure is to have priorities derived so that the

bottom-up (demand-driven) development will occur.

b. Mr. Dottridge observed that there is a lot of experience of supply-led solutions either looking for a problem that is not perceived at the user level, or coming up with solutions that are inappropriate. One has to begin by asking what are peoples needs and then what may be feasible.

c. Other participants reminded the group that social scientists can be on the supply-driven side of the equation, coming up with studies of great interest to libraries, but little relevance to real needs or useful products.

d. Dr. Plucknett suggested that the manner in which questions are asked of end users is a determining factor. If someone asked a farmer in the 1930s whether he were interested in hybrid corn, the answer could predictably be "no." If the question were, "Do you want to improve your corn crop," the answer would undoubtedly be "yes".

e. Dr. Ericsson speaking as a social scientist felt that the social sciences can play a role in better articulating both supply and demand, and in integrating social science research into a policy inquiry environment. Social sciences also play a role in helping the physical and biological science supply side to understand more fully what the problems and the constraints are. Mr. Chetwynd illustrated this with an example from the forestry-fuelwood project of southeast Asia where the initial thrust of the biological scientists (the foresters) was to select varieties of fast growing trees for fuel purposes. However in the agroforestry environment of the small landholder, the desire was to have fruit-bearing trees that were also useable for fuel purposes. When that information was conveyed to the foresters and the desires of the farmer-landholders were known, greater progress and acceptance of the research

resulted.

f. Dr. Daly observed that A.I.D. in agriculture was in a position that it has to work with supply-driven and demand-driven research requirements simultaneously. On the one hand, the Agency builds national research capabilities in developing countries which are designed to respond to the bottom-up approach. Simultaneously A.I.D. supports international research centers where the latest technologies are utilized, where investigations are undertaken on a scale that cannot be done in the smaller, local research stations. A.I.D. is also involved in building the capacities of U.S. institutions to address problems of agriculture in developing countries, and often in environments that don't exist in the U.S. So the Agency has some peculiar management problems that involve priority setting, measurement indicators of research from the different research settings, etc. Even the Congress which is the ultimate sponsor of the research cannot give an entirely coherent set of guidelines for these multiple situations and multiple objectives.

B. In closing the discussion period, the chair suggested that there were three additional points that arose from the presentations:

- o Over time in any given project, there is the need to use different evaluation methodologies. When the various methodologies converge in giving similar results, one has greater confidence in the evaluation process.

- o There is a geographic leverage in having proximity to centers of research excellence.

- There is the need for more systemic oriented research to integrate better the opportunities which science brings more adequately into the total social-economic environment.

III. The World Bank: Perspectives, Experience, and Discussion

Characteristics of Research	Dr. G. K. Ingram
Monitoring and Evaluation of Research	Dr. J. Murphy

A. Dr. G. K. Ingram

This presentation deals with different characteristics of research at the World Bank. Some of the characteristics and problems faced by the Bank are similar to those faced by the U.S. Agency for International Development, but there are also those aspects not shared by the two organizations.

1. Characteristics of Research and Studies at the World Bank

In the Bank we see research and studies as part of a continuum of analytic work very arbitrarily divided into economic and sectoral work; policy work; and "research" itself. Economic and sectoral work is not considered research per se. Although policy work is more generic and cuts across countries, it remains highly focussed with a product that has a prescriptive or normative content. A third element of the continuum is the research program itself, which has a budget of about $23 million per year. This tends to be cross-country, less prescriptive, highly applied and somewhat longer term. The percentage distribution runs about 60% economic/sector, 25% policy and 15% research.

Most organizations allocate resources in a command and control format. The Bank has a central research budget of about $5 million that is competitively allocated in response to internally submitted research proposals which are peer reviewed by internal and external reviewers. Having a committee of people who are serious players on the research side is very important. I have had projects that received great reviews and have been turned down by the committee. The committee is not a rubber stamp.

About three-quarters of the proposals are funded. In terms of cost-effectiveness, we take a good long look at the proposed budgets and fund, on average, about two-thirds of the money they ask for. Sometimes we may even give more. The net result is that we are currently funding about 40% to 50% of the gross requests that come to us.

Our research budget is 3.5% of the Banks administrative budget. It has averaged that for 10-15 years. Recently, we have been trying to develop information to evaluate the overall program, because we do not feel that just because it has always been 3.5% of the administrative budget that it should continue at that level.

A second characteristic of Bank studies and research it the heavy involvement of Bank staff. In-house staff is leveraged with outside consultants. We have found that Bank staff involvement is key to keeping the research applied in orientation, and to disseminating the product widely within the Bank. The term "embodied dissemination" is used, which means the researcher works in the field and gets first hand knowledge of what is going on in his/her topic. We have had pretty bad experience with turn-key products that come into the Bank in the mail; they tend to be filed away and neither disseminated nor assimilated.

Project proposals that come to the review committee are heavily blue-printed out. There are a set of standarized outputs that makes project management fairly straight forward, and enables the Bank to be aware of what it is buying. Although the outputs are not strictly tracked during the research period, the project budget is held reasonably firmly. Greater concern lies in completion on schedule and the comparison with the set of expectations developed at the beginning.

The Bank is itself an active research client; alwasy interested in applied research and studies we perform ourselves with our other clients. Our research has four objectives: Bank research is supposed to support Bank operations; to broaden our understanding of development; to improve our capacity to give sound advice to client countries; and to assist in capacity building for independent research in developing countries.

Currently, the Bank is looking at research evaluation techniques very much like the performance indicator approach. We have been trying to keep track of what we publish, which is not easy to do. We have made citation analyses of our output which, while having many problems, are one kind of indicator to use. We have looked at reading lists in development economics to find out how many of our publications are showing up on the lists. (The answer right now appears to be about 16% with a range of 0 to 45%). We have looked at where and how our publications have been distributed and to whom. At the program level these kinds of tracking can be a worthwhile exercise. We did a great deal in about three months.

A key issue that we ask is the "so what question." If this project is completed, and we all agree that it was a success, what kind of decisions would we be able to make better on the policy side? We are very interested in how this research output is going to interface with the policy side of the Bank.

Our research priorities are directly derived from the institution's priorities. Currently, for example, those include work on environmental issues, private sector development, poverty, and the transition of socialist economies. We try to anticipate issues taht will be of importance to teh Bank in the future and encourage some work on those issues.

The Bank did anticipate the environment issue, set up a new department staffed it with specialized personnel, and gave it resources to develop a long-term program. Two and one half years ago we also set up a unit to coordinate work on the transition of socialist economies.

Funding is another issue for these new initiatives. There is sort of a two track system -- the central part which is competitive and which modifies the institutional structure as appropriate. It is rare for the Bank to fund a project from the central budgt for over two years. This raises the question of terminating a project. That is done. Terminating a department once created is quite another matter. The only case I know of was the Tourism Department.

2. Comparative Advantage

Another set of characteristics involved in the evaluation of research at the Bank deals with our perception of four areas of comparative advantage. They are: problem identification; cross-country work; length of perspective; and implementation of research results.

The Bank has a comparative advantage in problem identification due to the large number of persons in the field with a good perspective on issues as they arise, and the ability to feed that information into the problem identification phase. Along a similar line, the Bank's comparative advantage is also in cross-country work. For example, here are eight countries that tried a particular policy - how did it work and why? We can do those cross-country studies because we have a presence in a number of countries, as does the A.I.D.

Our comparative advantage also arises from the fact that the Bank is in business for the long-term. We probably have a somewhat longer perspective than A.I.D. because our President is replaced every five years, while the A.I.D. top management may be in a shorter cycle. We once had a President for twelve years, which can make a real difference.

We think we also have a comparative advantage -- and this is both good news and bad news -- in implementing research results. Our lending program allows us to tie our policy advice to our loans. This means we had better be right.

3. Conclusion

This gives you a sense of the size of the program, what its mix looks like, what we are trying to do on evaluation, and how we face different levels of issues. Someone has said that research management is an oxymoron, and I have said the same many times to myself. It turns out that the key -- and this is where supply and demand come together -- the absolute key to research success lies in personnel selection. We have rather frequent turn-over in our research staff; researchers drift toward staff operations in the Bank. If you do not have a large in-house research staff, your main concerns are program and project selection. In a sense, the benefit of not having your own staff is that it gives you great flexibility. The downside is that you have less internal capacity to select and manage.

B. Dr. J. Murphy

One problem which I think every agency has is that top management has expectations that are not

always valid from a technical point of view, especially when it comes to the information system for monitoring and evaluating progress indicators. There is often a desire on the part of development managers to be able to say that for several years we have been funding plans for operations using a strategy based on certain objectives. Management likes to see a monitoring system set up within those projects. Then management cites cost/benefit of that strategy for the past two years. The point is that the monitoring system and the information flow on the implementation of the development program is not by itself the proper basis for a research type of analysis -- what are the detailed results for that program and what is the comparative advantage of that strategy and that approach? Of what can we do a cost benefit analysis? The point is to be able to monitor and evaluate research results. This has to be planned right from the beginning of the design of the research activity. Too often it is not, and it is after the fact that the right questions are asked.

There are a few key concepts used at the Bank to increase the capacity of the implementing agencies to monitor and evaluate their programs. Looking at the basic principles in the Bank's directives on monitoring and evaluation of its activities, there may be ideas useful to the Agency.

1. The Role of Information

The first key principle is that information by itself has no value; its value is derived from utilization. Further, what information is needed depends on what one wants to do with it; therefore, from the beginning there must be the involvement in a research design of managers and potential users of that information. We also must ask what kinds of analysis and decisions are to be accomplished, what is needed, and by what point in time. This has become a basic principle in the Bank's efforts to improve the capacity of the implementing agencies - by having them set up information systems for their own

managers. To prepare a report on the progress of implementation for the Bank every six months is not the key objective. Rather, the key objective is what decisions do the managers have to make and what do they need to know in order to make better decisions?

2. When to Design Monitoring and Evaluation Systems

A second principle is that monitoring and evaluation systems be designed at the start of the planning phase. We have found that this helps to insure the data needed for decision making when an evaluation is to be made are readily available, and most importantly, one has a better project right from the start because the objectives will have been specified in much more detail and with more clarity. The target populations must be defined specifically in order that appropriate indicators and measurements can be used and relevant information be developed to support the calendar of decisions through the project.

It is amazing, sometimes, how you can talk with managers and technical leaders of a program which is underway, ask them what the objectives are, and they all come out with some sort of standard response. But when you get more specific and ask them what kind of output indicator they expect, then they get into a great discussion and they realize that what they have in their minds is not the same as the objectives they had in the first place. This gives them some immediate feedback into the quality of the research program in identifying the objectives and the target populations which are too often misidentified.

3. Sources of Information

A third key principle is that any one source of information is never quite enough. It is necessary

that information be from a variety of sources and be interpreted in context. This is a difficult problem for all of us as the context is changing. There can be acts of God, earthquakes, etc., which can disrupt the research and even change its objectives. There are often changes in policy, political upheaval and so on which require re-interpretation of information consistent with the context. Changes in the environment and results of research elsewhere also change the context and may require adjustment of a program that is underway.

4. Available Contextual Information

Another problem an organization sometimes faces in developing countries is the lack of sufficient information available on the context – the total national economy and those elements that may be relevant to a specific project, but are not to be collected as part of the ongoing project. This issue is broader than any one research project. It does raise the question of all donors and development agencies in developing the data base capacity and infrastructure of countries for the long term. There are many approaches and initiatives to developing standardized data bases that are underway.

5. Conclusion

Institutional staffs of developing countries need to have the skills necessary to identify the information they need, in what form, and to make it available when required, not six months or a year late. Research has its own complexities and time frames for information needs and for dissemination of results. Utilization of information data bases requires capacity and institutional building across the board in many developing countries.

The last point I would like to make is that all of the Bank's work at all levels should help build the capacity of national agencies to design, implement, monitor, and manage their own programs for the future, to understand that information is potentially a very constructive tool, and is not a dangerous commodity.

C. The World Bank Panel Discussion

1. The Role of the Program Manager

To begin the discussion, we will first consider the role of the program manager as the architect of the program in terms of trying to get an alignment of the objectives of a research project with the objectives of the Agency. The capability of doing that depends entirely on the effectiveness with which the program is sponsored.

For example, if it's an in-house program, there is a direct hands-on shaping of the project right from the very beginning. This includes determination of evaluation criteria, setting of performance indicators, designing mid-term and final evaluations, etc. Sometimes it means structuring the performers of the program. If, on the other hand the agency works under contract, the same opportunity may exist depending on the agency's contracting policies. In many cases the contractor's proposal can be recycled until it is right. This gives direct input in the choice of principal investigator, how the performance criteria are established, etc. However, if the agency operates mostly under a grant system, the opportunity to provide direct head-end influence on the architecture and criteria of the program will be limited.

At the Office of Naval Research, one also finds that a key indicator for a quality program is the caliber of the program manager. In extramural programs, while it is true that ONR sets overall strategic objectives, tactical objectives and goals, a lot of the program development is what I would call bottom-up. Within the framework of the goals which ONR establishes, the program manager has the task of developing the various programs under his or her purview. A lot of a program manager's time is spent going through the community trying to ascertain what the interesting and timely topics are, keeping in mind the mission and objectives of the organization.

ONR has a competition as part of its program. It is an internal competition at the program level, not at the work unit level. Program managers are the primary competitors. They are expected to spend a lot of time developing these competitive programs while in contact with the research community and performers. A major factor in the succes of a program, more than goal setting from the top of the organization, is the caliber of the program manager.

2. Shaping Performance Indicators

Another important consideration is the shaping of performance indicators in relation to information required by decision makers. This appears to be fairly self-evident, but the question is how best to go about doing it right at the outset of a program. I'm reminded of the way that Japanese industry, which is so effective in compressing lead times, does so by building in consensus with top management right from the start of a program. Once they have determined their performance indicators, criteria, expectations, and so on, they get a person that they all trust to run the program and keep their hands off. Many U.S. industries seem to have a great amount of management intervention (micro managers) along the way. This does nothing more than stretch out time. There are probably some

lessons to be learned, but the question again is how does one get closure with decision makers to be sure that a consensus in terms of information, design, and performance indicators is reached at the very beginning.

3. Shaping Policy

The question was asked if the World Bank has been able to trace the influence of research on policy as a feed-back into decision making of agencies in developing countries.

Dr. Ingram stated that it was done on a case by case basis, where a few projects are chosen, evaluated, and then followed up to see what happened on the policy side. The Bank does an evaluation of each project after it is done, but that is like the ex-ante review.

IV. Agricultural Sector: Perspectives, Experience, and Discussion

U.S. Department of Agriculture (USDA)	
(Cooperative State Research Service)	R.Grady
International Agricultural Research Centers	D. Plucknett
Other International Research	L. Hardin

A. R. Grady (USDA)

1. Cooperative State Research Service

I shall describe the research grant evaluation system of the Cooperative State Research Service (CSRS) of the USDA. In science and education, CSRS functions as the extra-mural funding arm. I will not speak about the intra-mural funding arm which is the Agricultural Research Service (ARS). Within the CSRS there is a new program started in 1990 called the National Research Initiative Competitive Grants Program.

CSRS has a number of mechanisms through which it supports agricultural research. One is formula funding to land grant institutions and state agricultural experiment stations. Formula funds support long-term research and, to a lesser extent, infrastructure expenses. The experiment stations have an elaborate mechanism by which research priorities are set every four years, with a biannual update. CSRS also supports research through special grants which are either competitive in nature, and usually address regional issues, or are mandated by Congress as a line item in the budget.

Because the National Research Initiatives Competitive Grants Program deals with some of the same issues raised in this workshop; i.e., how priorities are set and how research progress is evaluated, I thought it might be useful to share with you the mechanisms that are used.

2. National Research Initiatives

From 1978 until 1990 there was a relatively small program within CSRS called the Competitive Research Grants Office which in its last year funded about $40 million in research grants. The program was open to all investigators regardless of institutional affiliation, included basic research, and was

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primarily focused upon the biological sciences. That program has now been expanded into what is called the "National Research Initiative." Although the emphasis remains on fundamental research (about 70% of the funds) "mission linked", or applied research, is the new emphasis.

a. Setting Priorities

Setting the priorities depends on the level with which one is dealing. Congress, in large part, sets priorities through such mechanisms as the Agriculture Appropriations Committee and the 1990 Farm Bill. This legislation is quite explicit as to the types of research to be supported. For example, CSRS is mandated to spend 20% of its budget for mission linked research in FY92. Twenty percent of its funds is also mandated for multidisciplinary research, and another 10% must go to strengthening awards.

When the National Research Initiative was implemented in 1990, the Assistant Secretary for Research decided that the widest possible input was needed to identify research needs and priorities. Two different planning processes were used to do this, one involved the users of this research (the commodity groups, agribusiness, environmental groups, etc.), and the other the scientific community. In order to have an interactive dialogue to ascertain their present and future research needs, user workshops were held involving the first group. This also gave the USDA an opportunity to explain to users where the Initiative fits into the overall agricultural research program. The other planning process asked the scientific community for research areas that would address user needs. It is important to note that a research "need" and a researchable "objective" are two different things.

b. Evaluation of Research Proposals

Based on meetings of the research planning committee, looking at research priorities of scientific organizations, the experiment stations and other documentation, Requests for Proposals are then advertized nationally. Proposals are evaluated by a peer review system on the basis of scientific merit and relevance to the program area and mission of the USDA. Only about 20% or less of the proposals that are received are actually funded. This makes the proposal evaluation and selection process very

important. Each program area, of which there are 24, is managed by a director who is a full-time USDA employee and a panel manager who is a visiting scientist who works with the for USDA for a year on a part-time basis. Neither the program director nor the panel manager judges proposals. Judging for merit and ranking of proposals is the responsibility of the peer panel.

Another input on the scientific merit of research proposals comes from ad hoc reviewers in the world-wide scientific community whose mailed-in comments are discussed by the review panels. On average, we fund about 70% of what the proposals ask for. During the peer review process we are very cognizant of issues of conflict of interest.

c. Evaluation of Research Progress

Since the National Initiative Program is only one year old, it is premature to discuss research impact. With 70% of the funding going to fundamental research, it is unlikely that impact assessments can be done soon. Nevertheless, it is important to report information as it becomes available to the Congress and to the American people.

B. D. Plucknett (Technical Advisory Committee, CGIAR)

1. Background

The Consultative Group for International Agricultural Research (CGIAR) system originated in 1943 as a Rockefeller Foundation project in Mexico, in collaboration with the government of Mexico, to improve food production for Mexico. Three American scientists were part of the original resident research team. and one of the "fathers" of the CGIAR system is participating in this workshop. He is Dr. Lowell Hardin, former director of agricultural programs for the Ford Foundation, and now emeritus professor of agricultural economics. Following early success in Mexico with maize and wheat, attention was shifted to rice and the International Rice Research Institute (IRRI) was established in the Philippines in 1961. This was the first of the international centers which embodied a completely different approach

to agricultural research. It had an international staff under dynamic, independent leadership. It was designed to be free of bureaucracy and politics to the extent that was possible. The Rockefeller and Ford Foundations completely funded initial operations. A.I.D. became a partner later in time. IRRI was successful in releasing new, high yield rice varieties within about 6 years. The Mexican activity became CIMMYT -- the center for the improvement of (maize) corn and wheat -- in 1966. These were the first two of many international centers for agricultural research.

The next two were the International Institute for Tropical Agriculture (IITA) in Nigeria and the International Center for Tropical Agriculture (CIAT), in Colombia. IITA and CIAT had more regional responsibilities for farming systems work and natural resource management as well as commodity responsibilities. By this time there was enough interest and so many research requests were coming in that further funding and a larger research resource base became major issues, which led to the founding of CGIAR in 1971 with leadership from Robert MacNamara of the World Bank.

2. Organizational Structure

The CGIAR organizational structure is a very informal: it has no voting, and is based on the consensus of the donors, which currently number 24. The Centers are essentially autonomous and are run by independent boards of trustees. They hire the Center Director General, who in turn hires a staff to carry out the mandate (the responsibility given to the Center by the CGIAR to do research in a particular area). The Board, the Director General, and the staff decide how a Center's research will be done, based on a review process which is relevant to our workshop today.

An external Technical Advisory Committee (TAC), currently composed of 18 distinguished scientists, half of its members from developing countries and half from the developed world, was formed by the CGIAR in the early days. TAC's responsibilities include the setting of priorities in consultation with donors, evaluation of research and of research concepts, and allocation of funds to the Centers. During an annual meeting of CGIAR each donor makes its pledge to the various Centers as it sees fit.

Given the informal and decentralized nature of CGIAR, an operational style based on proved performance and trust in the competence of management has evolved. Donors have confidence that the right problems are being addressed, that the right programs are being executed, that quality work is being done, and that the proper management of the Centers is maintained. There is no other way that the International Centers could command core funds to the extent that they do. Achievement and impact are all important.

3. Strategic Planning

In the early years of operation of the Centers, there was no formal strategic planning process. Beginning about 1990 when the level of funding for CGIAR seemed to be leveling off, the TAC was given the responsible for the budget, and began to look at priorities among the Centers. Competing demands for funds have now led each Center to become very explicit in what it is attempting to do, and this has led to strategic planning that may go out 10-15 years, although each Center also prepares a five year program budget.

Centers plan differently -- some are top-down and others are bottom-up and there are variations in between. The planning methodologies that seem to work best are those that seem to combine a judicious mix of both top-down and bottom-up participation by scientists and managers. In order for the Centers to have an idea of the full range of potential research opportunities, a sense of the possible, both physically and biologically, must be developed. There is also the need to be quite specific about what the Center plans to do over the next several years.

4. Evaluation

TAC reviews the Centers in depth every five years. At first this was only a program review with a cursory look at management. After 1980 TAC began external management reviews. I shall let Dr. Hardin speak of these management reviews as he has conducted a number at different Centers.

Program reviews are conducted by an external group named by the TAC with potential members suggested by the Center. Review teams vary in size from 4 to 12 members and may take two to eight weeks to complete their work, depending on how many relevant actors are contacted or interviewed. In the past few years program reviews have become much more strategically oriented, looking more to the future than to the past.

In the early 1980's there was great concern about the impact of the Centers and this concern led to a number of studies and publications. Each of the Center Boards is responsible for the scientific quality and the policies of its Center. Each Center has its own internal review process independent of the CGIAR external reviews, so there should be no need of individual donors running reviews. Each donor can and does comment on terms of reference for an external review and donors suggest questions they would like to see addressed in the review process. Every attempt is made for full participation at all levels.

And finally I might add that the TAC is looking at citation analyses for publications arising from some Centers, and also looking at adoption studies of new technologies as further indicators of progress.

B. L. Hardin (Agricultural Economics, Purdue University)

1. Management of Research

The management of research to increase the involvement of scientists in the evaluation process, and use of what comes of that process, has long been one of my concerns. My approach has been to ask, "As a scientist you are constantly testing your hypotheses in the laboratory and in the field, what hypotheses can be used to test management of your research?" Possible evaluation guides are governance; personnel management; financial management; and principles of information management. A participatory process based on these guides but specifically suggested by the evaluation group in

consultation with the researchers to be evaluated is prepared. Through this process changes are discussed with a Center's staff. Then any recommendations arising from the review are likely to be accepted and used.

When I was a Ford Foundation officer, our frame of reference with respect to our mission and our criteria involved the investment of scarce resources, usually measured in terms of the monetary costs. Ford sought to be catalytic and not create independent organizations that if successful, would be sustainable with local resources.

2. Inter-disciplinarity

As a University research manager, I have noticed an increase in the inter-disciplinarity of development related research. I interpret this as a move towards greater team-oriented research and systems analysis that have real implications for monitoring, review, and management as solutions are sought in a mission-oriented sense rather than one solely to advance understanding and knowledge. This requires that the representatives of the various disciplines be present during the planning stage of a program, and not just during the implementation stage. Cost-effectiveness of strategic planning monitoring, and evaluation must also be carefully considered with all the relevant actors present. There is much to be gained if it is done carefully with all the professionals involved in the entire process.

3. Self-Evaluation

I have a lot of confidence in self-evaluation if the structure of the evaluation and the reporting methods are appropriate, and the exercise is done routinely. It is the sporadic external review that takes up six weeks of the researchers time that should be minimized. Evaluation is a regular part of the management function and not an "occasional event" conducted to satisfy donors.

C. General Discussion

1. Peer Review

Dr. Grady stated that in the National Research Initiative Cooperative Grants Program, peer review is the mechanism of choice in evaluating and ranking the research proposals. But the peer review system does not constitute the final word. The programmatic staff is responsible for taking a panel's recommendations into account as one component in making awards. For each proposal there are at least six individuals providing recommendations as a part of the review process, and the recommendations are then discussed and ranked by consensus of the panel.

Dr. Bement added that within the NSF system, peer reviews are not followed blindly. The NSF program manager has final authority in the selection. Excellence in the peer reviews are a necessary but not all-determining factor in research grant awards.

Mr. Langmaid raised an question about the adequacy of peer review as the determining criteria for program decisions. For example, A.I.D. has adopted the N.I.H. peer review process for determining the adequacy of the science of individual proposals in the malaria vaccine program. But does the sum of the science in the proposals lead to the optimum solution to the vaccine issue? If the overall strategic plan is the sum of five different investments, will peer review add up to the right way to get the quickest and most cost-effective result? There is reason to question a blind adherence to peer review in such situations.

2. Strategic Planning

Dr. Britan reminded the participants that there are different levels for monitoring research projects and programs. At the individual project level peer review seems to work rather well as a principal, but not the sole, evaluation methodology. A second level of evaluation has to do with research institutions and their management. The third level has to do with monitoring or evaluating the achievement of research missions.

Dr. Bement referred to the various levels of evaluation that are used by the National

Institute for Standards and Technology (NIST). NIST has asked the National Research Council to conduct reviews at the program level; the institutional level in terms of management management capability, sustainability and competency; and finally by looking at achievement of mission in each of the major laboratories as well as the NIST as a whole. The results of these levels of evaluation are given to a statutory visiting committee of the NIST that does the final review the results of which goes to the Secretary of Commerce.

Dr. Plucknett added that in addition to self evaluation conducted by individual centers within the CGIAR system and the 5 year external reviews, there have been two reviews of the entire system - in 1970 and again in 1981. In 1987 the Technical Advisory Committee (TAC) of the CGIAR released an advisory paper on priorities and strategies. This has been discussed by the TAC and the international center directors at the annual meetings. There is now a proposal to have a strategy meeting with all donors to the CGIAR system within the next few months. The process is complicated because there are some 40 donors who represent governments, foundations, and other private organizations.

Experience with strategic planning in the World Bank was described by Dr. Ingram. The Bank set up a strategic planning unit in July 1987 which was abolished in January 1990. It did not work; management found it could not use the results to optimum advantage. The Bank currently uses a 3-year planning cycle with is highly work-program and budget oriented. There is also a 1 year management contract that serves as a blueprint for the year. But this entire process itself is under review by the new Bank president.

Dr. Plucknett finished the discussion by giving an example of impact evaluation. The tropical crop cassava has been the focus of research of a small group in the International Center for Tropical Agriculture (CIAT), located in Cali, Colombia, since 1973, when the project was initially funded at the level of $250,000-$300,000 per year under funding from the International Development Research

Centre of Canada. Cassava ranked fifth in the world for food production. All in the tropics, raised by the poorest farmers economically in the world, and consumed principally by the rural poor. The research program has never had over 11 staff members, but the payoff from that research has been phenomenal. Production of cassava is going up by about 3.5% per year in Asia and about 2% in Africa.

V. U.S. Government Research Programs: Perspectives, Experience, and Discussion

A.	National Science Foundation	S. G. Walters
B.	Office of Naval Research	R. Kostoff
C.	Department of Energy	R. Rader

A. S. G. Walters[3]

1. Industry-University Cooperative Research Centers: Background Goals, and General Description

One of the National Science Foundation programs may have special relevance to a portion of the research mission of this organization. That program is the NSF's Industry/University Cooperative Research Centers Program.

The Industry/University Cooperative Research Centers (I/UCRC) Program encourages highly leveraged industry/university cooperation by focusing fundamental research defined by groups of firms supporting the centers. The NSF role is catalytic. NSF funding usually starts with a planning grant and operational centers continue with a combination of NSF, industrial, and state support for five years, after which they are expected to be self-supporting. At the end of Fiscal Year 1991, there were 50 operational centers, of which 20 were self-supporting. For the total program, the industrial, state and university support is approximately $45 million, with NSF providing an additional $3 million annual support.

the current centers focus on topics such as: polymer, ceramics, or steel processing; welding, food processing and automated manufacturing; chemical process control, hazardous waste control and management; computer graphics and software engineering; optical circuitry; and topics in biotechnology.

[3] Revised text of Dr. Walters' presentation

Goals

The Industry/University Cooperative Research Centers Program is administered by the Engineering Centers Division in the Directorate for Engineering of the National Science Foundation.

The goals of the Industry/University Cooperative Research Centers Program are to:

- develop industry, state, and other support for industry/university interaction on industrially relevant fundamental research topics;

- promote university research to provide a knowledge base for industrial and technological advancement while training students; and

- promote research centers that become self-sustaining with industry, state, and other funding within a five-year period.

Background

The Industry/University Cooperative Research Centers (I/UCRC) Program was initiated by the National Science Foundation in 1973 to stimulate the interaction of the university and industrial communities on fundamental scientific and engineering research development. The vehicle for this interaction is a research center developed to create long-term interaction between the two communities on research topics pertinent to industry.

The National Science Foundation supports the initiation of these centers through a phased approach. The I/UCRC Program provides some support to study the feasibility of establishing an

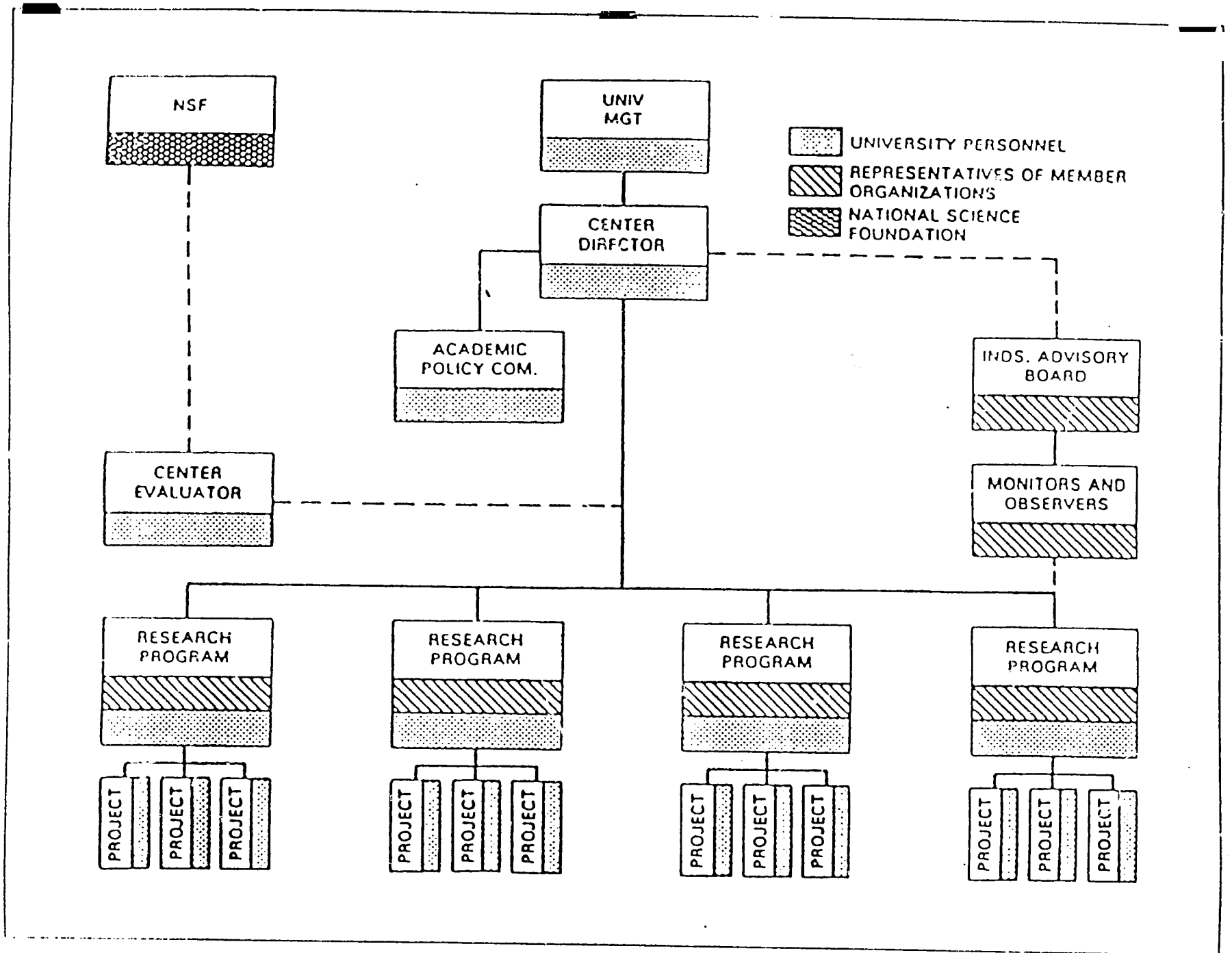

Fig. 1—Organizational Structure *of a typical Industry-University Cooperative Research Center*

operating center. Once a center reaches the operational phase, the program provides seed funds; but, a significant proportion of a center's support comes from industrial, state, and other funds. As the center progresses, it becomes less dependent upon I/UCRC funds, until it reaches the point of a self-sufficiency, and the program discontinues operational support. Most fully operational centers require funding of $300,000 annually from at least six firms to have a sufficient research base. All are expected to expand their base of support to include the university, state agencies, federal laboratories, or other organizations. Established centers have the opportunity for supplemental NSF funding to accomplish special activities such as cross-center collaborative projects, minority out reach efforts, etc. Centers are based in academic research institutions and some centers combine the research talents of more than one university or college.

The structure of a typical I/UCRC is shown in Figure 1. the center director has total authority for all activities and output of the center. The Industrial Advisory Board, composed of paying industrial members, provides advice and counsel regarding the activities of the center, but the center director has final veto power. An academic Advisory Board consists of university persons who can assure compliance with university research objectives and protocols and who can make available in a timely way university resources critical to the progress of the center. Faculty project managers, students, and postdoctoral students report to the center director. Centers may have a dozen or more projects, clustered around several principal research thrusts.

A center evaluator attends all center meetings and administers research instruments to measure the center's intervening success. The evaluator also reports in an advisory capacity to the center director and to NSF.

Center Research and Objectives

The scientific and/or engineering research agenda is established by the researchers and firms participating in the center and is focused on topics important tot he contributing firms. I/UCRC Centers may combine a range of disciplines and skills necessary to address the research issues posed by industry. Proposed centers should not significantly duplicate the research focus of other centers in the program.

The objectives of the centers are:

- to pursue fundamental engineering and scientific research having industrial relevance;
- to produce graduates who have a broad, industrially oriented perspective in their research and practice; and
- to achieve self-sufficiency from NSF support within five years of operation.

Center Proposal Evaluation Criteria

The primary criteria for evaluating a center proposal are technical and managerial quality of the proposal, qualifications of the proposers, industrial involvement and the likelihood of achieving the goal of the I/UCRC program. As such, the NSF reviewers consider the extent to which there is evidence the center will:

1. Have aggregate industrial support of approximately $300,000/year (on a membership fee basis) and growth potential;

2. Focus on high quality, industrially relevant research that will involve students with industry and the research;

3. Have a strong leader;

4. Become self-sustaining by the end of the time period of the request (within a five-year period);

5. Have university, state, and other sources of support;

6. Have an effective structure and management plan with an industrial advisory board;

7. Proprietary rights, patent, and publication policies clearly defined in a university/industry membership agreement; and

8. Have an independent evaluation component to assess the progress of the center.

2. Evaluation

Each NSF Industry/University Cooperative Research Center has an evaluator and an evaluation protocol to be used to assess the progress of the center in achieving its goals. The evaluator is generally someone with industry/university experience, drawn from outside the Center faculty.

The role is advisory to the Center Director, the Industrial Advisory Board and the NSF. Funding

for the evaluator is provided by the NSF. The Evaluator attends all meetings and administers several research instruments which are used at all fifty centers.

The basic assessment involves several types of studies for each Center as follows:

Historical Profile

The purpose of this study is to develop a comprehensive picture of the development of each Center. The data base consists primarily of program documents, interviews with the staff and the public record. The result is a narrative history of each center highlighting the qualitative similarities and differences across the various institutions. This study is updated yearly.

Industrial Sponsor Questionnaire

The primary "success" variable for the Centers is their long-term survival. However, it is useful to ascertain an intermediate interviewing success or effective measures for Center operation. One obvious intermediate outcome measure is the nature of the research projects developed in a Center and the amount of monies industrial members have invested in transferring and applying that knowledge. This study, administered yearly to industrial sponsors, employs structured questionnaires. A similar questionnaire designated for faculty researchers is also administered each year. It is useful in determining the responsiveness of the various projects to faculty and university needs. Data from both studies are also aggregated for all Centers and periodically analyzed by all constituencies of the program.

Level of Interest and Feedback Evaluation (LIFE) Form

This form is administered twice a year during semi-annual Industrial Advisory Board meetings. As seen in Figure 2, it allows industrial members to indicate their level of interest in Center research projects (proposed and ongoing) and calls for sponsor comments regarding the merit and relevance of the work. The scientific and technical interaction triggered by this form, makes it one of the most powerful activities for maintaining and enhancing a Center's vitality. Figures 3 and 3A outline the full protocol. These four survey protocols are also being used by non NSF funded Centers of the I/UCRC type in the U.S. (i.e., Rutgers Center for Advanced Food Technology) and overseas (Questor in Northern Ireland, and the Silicate Research Institute in Sweden).

Summary

Each of these studies provide input for an NSF Program Officer. In addition responses from LIFE forms and several questions from the other surveys also provide additional information in a timely way to the Center Director and project researchers. Together with other information these inputs facilitate management analysis, diagnosis and the taking of corrective action.

The NSF Industry/University Cooperative Research Program may well be the most significant piece of social technology to come out of the National Science Foundation in the last twenty-five years.

The great enemy of innovative social technology such as the I/UCRCs... "is not the lie-deliberate, contrived and dishonest, but the myth -- persistent, persuasive and unrealistic. We enjoy the comfort of opinion without the discomfort of involvement and/or thought. Mythology distracts us everywhere -- in government, business, politics, and economics." I hope that this introduction to the NSF I/UCRC model and management protocols has disposed of some of the myths and that your organization will consider what role these experiences may play in reaching your agency mission.

FIGURE 3. LIFE, INPUTS PROJECT MANAGEMENT.

LEVEL OF INTEREST AND FEEDBACK EVALUATION
(LIFE, SEE FIGURE 2)

An important object of an NSF/IUCRC semi-annual Industrial Advisory Board meting is to facilitate scientific discussion about Center research projects among faculty, students and industrial members.

LIFE forms hold the potential for stimulating project relevant discussion.

- One of these forms is filled out by each IAB voting member during the five to ten minute period set aside for this purpose following each project progress report or new project proposal.

- The form, which includes instructions, can be produced by any commercial printer. It is a carbonized cluster of three sheets lightly glued at the to- white sheet for the Evaluator and Director, yellow for the researcher, and pink for the IAB member.

- The Evaluator, NSF Program Officer, or others designated, collect the forms and make the yellow copy available immediately to the faculty in time for them to discuss comments with members during coffee breaks, lunch, and poster sessions.

- Following poster sessions and any discussions with researchers, IAB members may modify their forms and give the revised copy to the Evaluator.

- Based on the final comments, a single flip chart size page or viewgraph with a brief, bulletized statements of the comments they wish to respond to and the responses is prepared.

- These sheets or a viewgraph showing the Level of Interest and Comments for each project are then used as a basis for discussion during the Executive Session of the Industrial Advisory Board.

- They also provide a basis for further IAB faculty discussions during the IAB/Faculty debriefing session.

- This procedure can help foster scientific discussion and exchange of expertise among Center participants.

With some analysis by the Evaluator, LIFE forms also hold the potential of contributing to the Director's project management system and of alerting the Director to those members whose rankings of Level of Interest, and Comments signal possible dissatisfaction with Center activities.

(continued)

FIGURE 3A - LIFE Protocol

- Collate the revised final LIFE forms including any changes in the Level of Interest rankings made during the IAB Executive session, and develop two matrices.

  1. In Matrix I list project titles and researcher names along the X axis and VHI, I, IWC, NI and ABS Levels of Interest across the top or Y axis. Post the LIFE Level of Interest data from each LIFE form. Projects and researchers with a third or more votes in the combined IWC, NIU, ABS score should be looked at carefully. They could be entering the "endangered species" area. Director and researchers need to be informed so that discussions can take place and, if appropriate, corrective action taken.

  2. In Matrix II list company member names along the X axis and Levels of Interest along the Y axis. Once again the rule of thumb, to be adjusted by your Center experience, is that a company with a third or more of its votes as IWC, NI, and ABS could become a candidate for withdrawal from the Center and thus should receive special attention.

This LIFE process has a log of power and it is an integral required component of I/UCRC operations. Use it carefully.

Level of Interest Feedback Evaluation Form
(LIFE Form)

Project Title:_____

Research Leader:_____

____ I = Interested

____ IWC = Interested with Change

____ NI = Not Interested

____ ABS = Abstain

To facilitate scientific and technical interaction between Center Faculty and Industrial Member Representatives, each company represented is requested to rank each presentation as follows: Interested, Interested with Change, No Interest, and Abstain (i.e., not qualified to comment). The level of interest checked should reflect the opinion of the company (not one's personal interest).

Comments should include: Precompetitive suggestions/applications/industry benefits as well as comments regarding suggested changes, quality of research, scientific merit, innovativeness of research, industrial relevance, level of effort, progress since last report, offers of help, and other suggestions that might relate in a timely way.

Your completed LIFE forms will be picked up immediately after each research presentation. Please keep the pink sheets for your records.

Comments:_____

Name:_____ **Company:**_____

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Date:

FIGURE 2 - Administered twice a year during semiannual Industrial Advisory Board Meetings for ongoing and proposed projects.

B. R. Kostoff

I will attempt to summarize very briefly a study I have just completed on the assessment of research impact and then summarize the Office of Naval Research program development evaluation process.

1. Assessment of Research Impact

The study on Federal practice in research impact assessment will be a chapter in a forthcoming book on impact evaluation. In the study, impact evaluation is examined under the following three conditions:

a. During the research selection process we try to get a prediction of the research impact;

b. During the review of ongoing research we try to get a better idea of the potential impact, especially possible near-term impacts; and

c. In ex-post research assessment we look back to see what impact actually occurred.

I also looked at three types of approaches used to assess the impact of research:

a. Qualitative methods which were primarily variations of peer review;

b. Semi-quantitative methods; for example, historical tracings of accomplishments and critical scientific events; and

c. Quantitative methods such as cost/benefit analysis and bibliometric techniques.

2. Peer Review

Peer review is the method of choice for selection, review, and ex post assessment, although peer review is not a perfect technique of evaluation. Other methods, such as bibliometrics, are used to

supplement peer review. There is no case I know where an agency will use bibliometric methods by themselves or the semi-quantitative methods by themselves as a complete research impact assessment. That's where we get to the method of partially convergent indicators. They may bolster the results of peer review or may raise questions as to why the results are different.

The Office of Naval Research (ONR) compared its proposal evaluation methods with those used by the National Science Foundation (NSF) and the National Institutes of Health (NIH). The NSF and NIH peer review proposals on an individual-proposal basis. The ONR uses external reviewers on a program basis -- an aggregation of proposals, and allows the Scientific Officer to review proposals on an individual basis. I also examined how the Department of Energy (DOE), ONR, National Academy of Sciences (NAS) review of NIST, and the annual review of the DOE National Laboratories use peer review for ongoing programs.

The quality of a peer review does not come through in the literature. It has been my experience that there are three principal components which determine the quality of a peer review:

a. Motivation, the desire of the team leader to get quality results above and beyond the perfunctory approach. (Motivation of the team leader is <u>all</u> important to the process)

b. Competence of a reviewer inherently limits the quality of the review -- one competent reviewer is worth more than a half-dozen partially qualified reviewers; also, the review team should have an integrated competence to evaluate the many facets of research impact.

c. Independence -- peer review is a very subjective process and the quality of the results will be limited by the conflicts and biases of the reviewers.

3. Semi-quantitative Methods

On semi-quantitative methods, historical accomplishment accounts provide insight into what an organization feels is important, and what the impact has been. Project Hindsight, performed for the Department of Defense some twenty-five years ago, looked at twenty military systems and traced back through time the critical R&D events that led up to these systems. They found that most of the critical events were 'directed' (applied) research done primarily in military labs. They only went back some twenty years. If they had gone further back they might have seen the impact of basic research on the development of these systems.

Another study done a few years later, perhaps in response to Hindsight, was Project Traces, which was funded by the NSF. They saw that fundamental research findings had major impacts on the five technologies studied. The a time span extended up to 80 years, longer even than the introduction of some of the technologies. I think this was a more valid approach than used in Hindsight.

4. Evaluation Process at the ONR

I would like to conclude with a summary of ONR's program development and evaluation process. Our investment strategy is implemented through our guidance to what we call claimants. These are internal naval organizations to whom funds are allocated. Claimants in turn go to the community and give grants, contracts, etc. We send three types of guidance to the claimants:

a. Naval-needs guidance, which deals with our view of the operational requirements, scenarios, and research required to address identified problems;

b. Fiscal guidance with 5 years of fiscal controls for program planning; and

c. Technical guidance to identify promising opportunities and research gaps.

The Office of Naval Research, for administrative purposes, divides the program into fifteen technical disciplines. An internal expert or monitor is assigned to each discipline. Once a year each expert writes a report. One component of each report is about the promising opportunities in that discipline. This component is thus bottom-up driven. For each of the disciplines, we also have the National Academy of Sciences (mainly its Naval Studies Board) generate a Promising Opportunities Panel on a triennial basis. On the average we have five panels a year. The outputs of the three sources of opportunities -- research requirements, the monitors' reports, and the NAS Panels' reports -- are reviewed with some culling and integration to create the technical guidance. This is sent to our claimants who then propose new programs and modify their current programs.

Our program is broken down into two parts: two-thirds is core funding (long term evolutionary programs); one-third of the program is called Accelerated Research Initiatives, which are nominally five year thrusts concentrating on promising areas of science of interest to the Navy. The Accelerated Research Initiatives awards are selected through a competitive process at the program (million dollar per year) level using external panels for evaluation and recommended prioritization.

A senior group from the ONR then reviews the proposals from the following perspectives: the balance among the scientific disciplines; balance among the eighteen warfare areas; how are proposals distributed among our three areas of core competency -- ocean science, advanced materials and information science; how are the proposals to be allocated between categories of claimants and performers; basic versus applied research; revolutionary versus evolutionary technologies; and high risk, moderate risk and low risk. The Department of Defense puts out a list of some twenty critical technologies every year: how does our list compare with it? Dual use has come to be another significant criterion -- military and civilian potentials.

At the program level we use an evaluation process with external reviewers. Research has many

different facets and in the evaluation process we want to look at as many as we can using appropriate expertise. These are the key factors that we have identified based on our own work and that of other agencies as applied primarily to basic research: scientific merit; research approach; near and long-term impact on mission productivity, such as number of papers, patents, honors, awards, etc.; and finally, quality of the research team.

Our largest claimant for funds are mainly the universities. Criteria used by this claimant include: scientific quality and uniqueness of research; scientific and technological opportunities in areas of likely importance to the Navy; need to establish a balance between revolutionary and evolutionary research; position of research relative to forefront of other scientific efforts; responsiveness to present and future Navy needs; possibilities for follow-on programs in higher R&D categories; and appropriateness of the research to ONR as opposed to the Army, Air Force, NSF, etc. Then there is a list of questions: can specific advantage to the Navy be identified if the program is successful? Would the research be supported if it were not already underway? What is the technological context of the program? How does it fit with ongoing research elsewhere? Is the research coordinated with other Navy research programs? What are the decision milestones of the program?

C. R. Rader

I will try to share with you today is my experience in terms of questions the Chairman posed for us this morning.

1. Background

The peer review done at the Department of Energy (DOE) is of ongoing research. Our office performs this function because Congress decided an independent check on the merit of DOE research was

required. Our office is funded independently. We do not review by fiat, but are invited by the program managers to do our reviews. The results, which are extremely candid, are labelled "internal use only" and do not go to the Office of Management and Budget or to Congress. Dissemination of the results could threaten the career of the principal investigators we review. Most of the program managers that use our process end up with about 18 percent of the results requiring management attention; it is then up to the project manager to make corrections. Only technical information is measured, we do not do a management review.

Our process began about ten years ago. Dr. Bromley, of OSTP, and seventeen of his colleagues came up with nine questions that they thought were important to answer for any R&D project. We have discovered that we cannot directly measure technical matters at a program level. We have to measure it at a lower level, at the level of the principal investigator and his team, who are responsible for one set of objectives. And, one of the first thing we do is to find out what the objectives really are. This is part of the preliminary screening necessary to sort out the reviewable projects.

2. What Does a Research Manager Need to Know?

The first question posed in this workshop, "What does a research manager need to know?" means that you must have people as reviewers who really know what they are talking about. We try to get world class researchers and a balance between industry, academia and laboratories, and we also try to balance out bias where we are aware of it.

We have not found a general rule to check cost-effectiveness for a program manager. He has to do that for himself and be self-correcting. We do measure productivity and impact which are important ingredients in cost-effectiveness. We ask the principal investigator how the project will lead to the designed objectives; this requires a statement of the major problem the principle investigator is faced with

and how does he translate the statement into a technical approach? This provides focus on one of the weakest aspects of most programming, there is often no clear technical statement of the project's purpose.

Our findings are turned over to the program managers to decide how to deal with the technical issues. If he gets nine out of forty projects judged to be not very good, most of the time his upper management will say "What have you done about it?" The first thing that program manager will have to do is come up with an action plan and brief his superiors on it. We participate in these briefings giving any common insights we may have gained during the review.

3. How Does the Passage of Time Affect the Research Program

How does the passage of time affect the research program or project: For the most part projects change their objectives over time and, typically, for the better. We see this because most programs come back to us every four to five years for re-examination.

In our review process, we try to check technical aspects first and impact second. We ask program managers and principal investigators to recommend reviewers. We also check with the professional associations and societies to get an initial candidate list. We have the program office check for conflicts of interest and select reviewers based on the factors mentioned above. This is is not a simple approach, but it has worked well for us for the past ten years.

In the view of many of our program managers, DOE research still has a lot of problems, in part because the DOE has 29 appropriations, many masters with many agendas in addition to our own concerns about technical merit.

C. General Discussion

For the remainder of the afternoon, Dr. Bement asked that the A.I.D. participants summarize what they would like to see coming from the workshop before it closed midday on December 12th and what areas should be the emphasis for discussions on the final day. He then stated that a workshop of this type, although undoubtedly useful for getting ideas and experiences on the table, is troublesome to the National Research Council. To produce a thoughtful, careful analysis of the state of the art in research progress measurement and strategic planning methodologies takes far more time than is available in this instance. The discussions may well represent 80% of current thinking and examples of current practice but to achieve the remaining 20% and crystallize the process into a coherent, thoughtfully researched document could well take an additional year or year and one half. The participants would have to meet on two or more occasions to fully understand the A.I.D. programs and review/evaluation processes sufficiently well to recognize what advice may be most appropriate for the Agency. That is why in NRC evaluations there is not only a "research phase" but also a "consensus building phase." Only if both phases are carefully structured and implemented can there be truly thoughtful findings and recommendations A workshop provides fairly strong "top of the head" impressions from experienced people. Within NRC, the conduct of a panel consensus building process is subjected to a systematic peer review. In this workshop format, there is no opportunity to do that. Now, recognizing the limitations, what is is that A.I.D. would ask.

Dr. Eriksson stated that on the part of A.I.D. the Agency recognized that it was not going to reach definitive conclusions by tomorrow. In the Center for Development Information and Evaluation (CDIE) staff is particularly interested in being able to incorporate the Agency's support for R&D into the Program Performance Information System for Strategic Management (PRISM). Thus, in terms of the conceptual building blocks of the system, first and foremost are the objectives or goals. That process necessarily rests with program managers taking into account the already enunciated policy priorities for A.I.D. as a whole. CDIE contributes on the basis of the work done with field missions. You here may

contribute from your experiences in management evaluation and strategic planning in settings very different from those of A.I.D. But I think it is largely the job of the R&D Bureau in A.I.D. to carry on with the process. You can help sharpen the definition of objectives. That needs to be done ideally at the planning stage and it needs to involve the key participants -- not only research managers but also the direct research actors. At an intermediate stage, or some more advanced stage, it needs to involve users. What then, are the next steps we need to work on?

Dr. Langmaid followed, stating that it was his sense that as one gets closer to actual applications, the use of research indicators becomes easier.

Another problem the Agency has is deciding on research investments in 2 or 3 totally disparate areas -- animal vaccines, human vaccines, social science research, behavioral research, etc. A.I.D. has to solve that problem by developing its strategic plan and within that plan make the judgments.

It would also be helpful to focus on the methodologies for assessing some alternative values of the high risk areas. We are frequently faced with making a decision in the area of knowledge during the early stage. The Agency is not afraid to take risks, but will do so haphazardly. I think it would be helpful to spend a bit more time on the approaches to high risks, those areas where there are unknowns. My sense from what I heard from the discussions is to keep the research on a short leash until there are fewer unknowns. Maybe that is the best one can do for high-risk projects. But it would be helpful to hear how those kinds of issues are dealt with in industry, in other U.S. government- funded research, by foreign governments, etc.

Dr. Rossiter followed: We have talked about strategic planning at 3 levels: the broad institutional level; the program level; and the project level. I would like to by-pass the broad institutional level and also say that the Agency has within its own organizational make-up the capacity to do a good job once the projects have been defined.

Looking at the program level, one has peer review to answer the question, "Are we doing things

right?" The more important question as it pertains to us now is, "Are we doing the right things?" That has to do with strategic planning at the program level. How do you institutionalize the process within the Agency so that good and sufficient strategic plans are produced, and incorporate into those plans not only the research indicators which are an intrinsic part of the plans, but also incorporate into the process accountability? Furthermore, if there is a change in personnel, as there inevitably will be, how is continuity maintained through the process?

Dr. Eriksson added a further observation. For the CDIE the workshop offered the potential to learn more about performance indicators that are used by other research management evaluation groups. We certainly recognize that one cannot talk about performance indicators in a vacuum. One has to speak about broader kinds of questions which are of equal, indeed greater interest to the R&D Bureau, the Office of Policy, etc. There is an Office of Strategic Planning and next week it will be brought together with an inter-Bureau long-range planning group. The issues discussed in this workshop will be of relevance to that new inter-Bureau group.

DISCUSSION and SUMMING-UP

I. General Outline

Dr. Arden Bement, having previously consulted with workshop participants, suggested that discussions on the second day of the workshop focus upon the following issues:

A. Characteristics and Methodologies of Research Evaluation

Question #1. What are methods for improving the objectives, or criteria in research evaluation? Who should be involved in the process?

Question #2. Are there methodologies for assessing the relative value of projects in high risk areas? What approaches are there for evaluating unknowns?

Question #3. Where can quantitative (measurable) performance indicators be applied? Are those quantitative performance indicators applicable to A.I.D. and can they be incorporated into PRISM? (Program Performance Information System for Strategic Management).

Question #4. How does one build accountability and continuity into performance evaluation systems?

B. Summing-up

Question How might A.I.D. proceed in its quest to strengthen its research evaluation and management decision making systems?

II. Question I: Improving Objectives

Dr. Rossiter emphasized that the first requirement for a successful research evaluation is a clear statement of objectives that are incorporated into the research design. Setting objectives is a matter of leadership and experience but there is a subtle dichotomy in the process. If a management evaluation process is to be respected and followed, those to be evaluated must share responsibility with their managers for the design of the system and want the process to succeed. Although top management must initiate evaluation and give it priority, inputs from researchers themselves and their immediate supervisors must be sought as the plan is designed. It is essential to bring to the process all qualified persons who are part of a research team and to take their suggestions seriously.

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Dr. Duga cautioned that A.I.D., the World Bank, and other international donors have a special problem; i.e., the objectives desired by field missions arising from the host countries where those organizations work, often are quite different and much more specific than objectives arising from central management and headquarters staff. The latter are very greatly influenced by political processes and the necessity to show short-term results. Dr. Bement, Dr. Murphy and others recognized those realities but added that the challenge for management is to construct indicators that build in a balance of specificity as well as more general goals.

Dr. Daly observed that spin-offs from research justified for one purpose often prove applicable to problems in other situations. A.I.D. is constantly searching for spin-offs from domestic oriented research in the United States. How then, does one handle objectives in these circumstances.

Dr. Hardin cited an example of a program that seeks to apply spin-offs from U.S. domestic agricultural research and adapt advances to situations in developing countries. It is the collaborative research support (CRSP) program of the U.S.A.I.D. and land grant colleges and universities in the United States. He pointed to the joint planning process for programs. Evaluation of progress is always built into the original, overall plan. Mr. Langmaid remarked that the illustration was valid but that the program required legislative authorization to allow it to happen.

Dr. Bement stated that in any strategic planning process one should look for several options (spin-offs) for research applications but in industry one cannot justify a project on the basis of those potential options. Dr. Kostoff from the Office of Naval Research added that in the more fundamentally focused research that his Agency supports, potential spin-offs can be a supporting factor in making a decision on research support but are never a justification to approve otherwise marginal projects.

Finally, Dr. Britan alluded to the literature of management methods stating that a telling criticism of "management-by-objective" is that the practice when institutionalized too rigidly can become an excuse for not making decisions. Dr. Bement commented that any system of research progress measurement and

any system of evaluation could be subject to abuse. But systems when understood and used by competent supervisors and managers guide the processes of strategic planning (i.e., "doing the right things") and also guide more day-to-day decision making (i.e., "doing things right.")

III. Question II: Assessing Risk

In addressing this issue Dr. Bement and others drew upon an established, and growing literature of risk assessment and risk management. There is a relationship between "opportunities" "risks." that may be expressed as the "figure of merit." It is the ratio of the sum of the opportunities to the sum of risks within time:

$$\text{Figure of Merit (F.O.M.)} = \frac{\Sigma \text{ Opportunities}}{\Sigma \text{ Risks}}$$

This can be modified as follows (for time factors)

$$\text{F.O.M.} = \frac{\Sigma \text{ Opportunities}}{\Sigma \text{ Risks}} \times \frac{\Delta t_{wo} + \Delta t_{LC}}{\Delta t_{RD}}$$

where Δt_{wo} = Window of Opportunity = Time from technology availability to Time of need
Δt_{LC} = Product life cycle period
Δt_{RD} = Time interval from proven technical feasibility to proven commercial feasibility

Opportunity factors might differ depending upon whether one is doing more fundamental (basic) research or applied research (technology). There are a number of opportunities that one can assess and it is helpful to look at Figure 1 for examples such as: a) the potential for new, fundamental understanding, b) the potential for unlocking revolutionary concepts or principles, c) the value of the market potential, etc.

Risk factors may have several weighted values and should also be described quantitatively. Figure 2 gives examples of the kinds of elements that are commonly used in assessing risk.

After risks are rank ordered, it is helpful to map them on a matrix such as that of Figure 3. This

Figure 1

Opportunities and Risks

Opportunity Factors	Scientific Research	Technology Research
Potential for New, Fundamental Understanding	X	
Potential for Unlocking Revolutionary Concepts/Principles	X	
Potential for Productive Innovative Advances	X	X
Value of Potential Market		X
Market Dynamic (Potential for New Technology Insertion)		X
Economic Impact on Market		X
Nature of Impact	X	X
Duration of Impact	X	X
Leverage (High Returns/Investments)	X	X
Pervasiveness (Genericness)	X	X
Absence of Alternatives (Uniqueness)	X	X
Simplicity	X	X
Competitive Position	X	X

Figure 2

Opportunities and Risks

Risk Factors	Scientific Research	Technology Research
Newness of Concept	X	X
Lack of Knowledge Base	X	X
Peer Biases	X	X
Societal Taboos/Lack of Acceptance	X	X
Limits in Working Environment		
- Intellectual Resources	X	X
- Financial Resources	X	X
- Capital Facilities	X	X
- Information Networks	X	X
Market Risks		X
Competitive Risks		X
Government Restraints		
- Trade Barriers		X
- Export Controls		
- Liability Exposure	X	X
- Regulatory Constraints	X	X
Costs	X	X
Time to Success	X	X

Figure 3

Risk-Opportunity Matrix and R&D Strategies

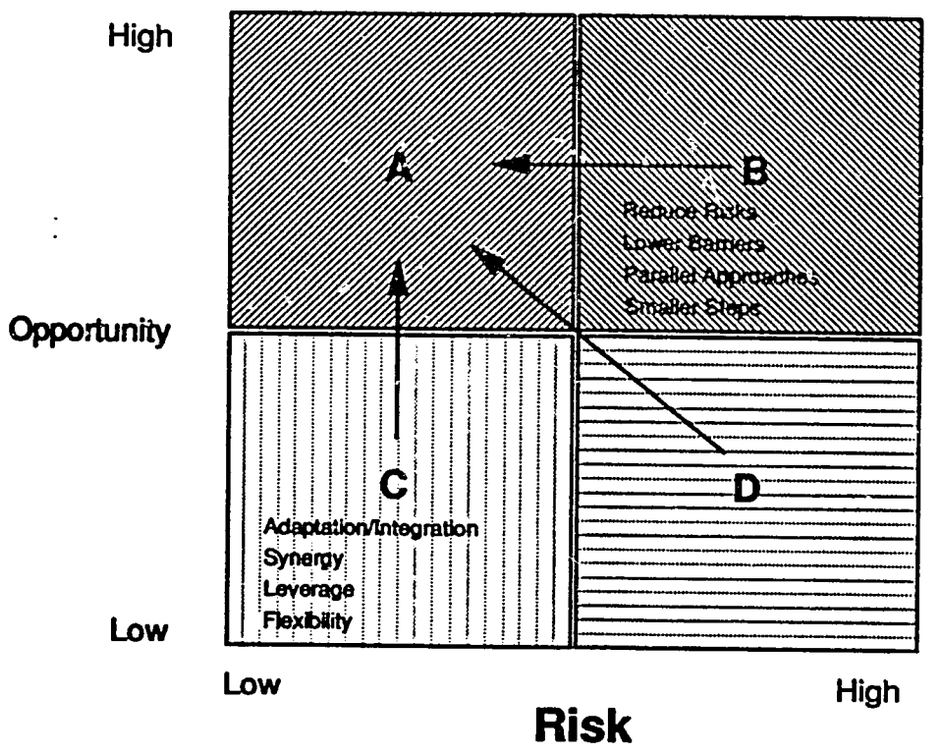

illustration uses opportunity as the ordinate rising from low to high and risk as the abscissa, also from low to high. The quadrant that one seeks is that which has high opportunity and very low risk. But the matrix is useful in analyzing other situations. If risk is low and opportunity also low, perhaps one can enhance opportunity and still maintain low risk. Should one have an estimate that falls in the quadrant of high risk, but also high opportunity, then the strategy is to find ways to reduce the barriers; i.e., risk over time.

To give an illustration of how risks vs. opportunities are measured one can turn to the Department of Defense and the so called "defense critical technologies." When the Defense Science Board was given the challenge to rank the critical technologies, the programs that were perceived by the Board before any ranking to have the highest priority, at the end fell near the bottom. Although variation of mean scores among 10 experts was plus or minus 20%, the order of ranking was fairly consistent. It turns out that the technologies that have glamour such as sensor pulsed power and super conductivity in the end dropped in their rank order because of high risk factors.

One has to use methodologies well adapted to the situation. The system described above is a way of sorting; judgement on the part of the decision maker is essential. One pays attention to the numbers but one must not be driven by the numbers.

Kostoff suggested that the system might be better adapted to the situation where 20 experts in a given technical area, perhaps even a fairly broad one, were evaluating projects in that area. What about the situation of evaluation across areas and disciplines?

Bement and others stated that at the higher levels of management and evaluation, multidisciplinary panels apply the same principles across disciplinary boundaries. A.I.D. applies that principle with its Research Advisory Committee as does the National Science Foundation with the National Science Board.

In summing-up Dr. Britan of the A.I.D. policy directorate staff stated his conclusion from the discussions was that industry has begun to develop quantitative methodologies for project assessment in

high risk areas and that these methodologies seem to be a starting place for A.I.D. to discuss quantitative measures for its projects. Dr Bement agreed and added a cautionary note. Management must understand the methodologies are imperfect and still evolving. Quantitative techniques must be applied within limits that are clearly understood, and must be applied with judgement.

IV. Question III: Application of Performance Indicators

A number of participants felt performance indicators have the greatest validity as a management tool when applied to specific program and project evaluation situations. Dr. Bement described evaluation as a process of analysis. One compares accomplishments to carefully stated objectives that were prepared prior to research initiation. This requires one to estimate at the time of project or program design and authorization results that he or she believes can be accomplished. It is a learning process over time. That may seem a bit abstract, but one has to be very, very focused in terms of inputs, and eventually of outputs. For A.I.D., as in industry, you have R&D costs, start up costs, and the costs of bringing a technology into the field. You certainly have a potential project life - 5 years, 10 years, etc. You can estimate that at the beginning and then go back to it to see if the numbers still correlate reasonably. From industry an indicator can be net sales for the first year, and for the second year; for A.I.D. these might translate to net benefits for the first and second year. There is annualized internal rate of return from the date of introduction; it can be calculated, and it has validity. I am not saying that these are the indicators for A.I.D. but they are the kinds of indicators that need to be designed before the fact and then examined after the evaluation to see how closely they compare.

Dr. Cummings for A.I.D. stated that he felt the illustration was certainly valid for an industry, but not so valid for A.I.D. The Agency is highly decentralized which may set one type of objective, and

simultaneously A.I.D. is required to fulfill objectives mandated from the outside.

Dr. Bement replied that there are translation problems, but the processes are similar. To become useful performance indicators have to be designed, tested, and modified over time. Inherently it is a dynamic process and management has to be committed to evaluation as an essential, long-term function. The application of performance indicators and questions of continuity-accountability go hand in hand.

V. Question IV: Accountability and Continuity

Dr. Rossiter reiterated that evaluation has to be founded on the participation of the researchers. It then must be implemented with consistency and clear commitment of management. And, as time passes and there are management personnel changes, the commitment by management to an "established" evaluation process must not waiver. If that does not occur the evaluation process will soon fade and be forgotten.

Dr. Murphy seconded the remarks of Rossiter stating that leadership and commitment from the top is crucial. PRISM is being built from the bottom up and if it is to be taken seriously by those in the field, the system must be perceived as being necessary for decision makers at the top.

Several A.I.D. participants stated that the evaluation processes described by the participants from the World Bank, other government research agencies, and industry appear to be costly. They asked for estimates of those costs.

It was stated that the Consultative Group for International Agricultural Research spends between $2 and $4 million per year for evaluation out of its total budget of about $240 million per year. In industry the tendency is to make research evaluation a function the line research manager responsibility and thereby charge costs as an element of the research itself. Catalyzing a company wide approach to evaluation and interpreting results for higher-level management decision making then tends to be the task

of a very small number of persons, often one or two individuals in a research program that may total as much as $600 to $800 million per year.

VI. Summing-up

In summing-up, Dr. Bement suggested that there seem to be three points that the participants wished to convey to A.I.D. for its own internal planning on research progress measurement and strategic planning methodologies before continuing a dialog with other advisory groups. The three points are:

1. Clarification of Objectives: A.I.D. research objectives should be clearly stated by function, or by sector and should recognize geographic differences in their application. Whenever possible objectives should be stated quantitatively with clearly enunciated intermediate milestones or benchmarks to facilitate in the evaluation process.

2. Need for a Draft Strategic Plan: To improve internal and external discussions on research planning and evaluation, A.I.D. should first prepare its draft strategic plan for research that includes evaluation components. The draft strategic plan should follow a total systems approach that clearly describes (or defines) the research to be undertaken, any field testing of results, and implementation. The plan should also include an appropriate information linkage component.

3. A.I.D. Research and PRISM: Workshop participants were asked to discuss how research (R&D) might be incorporated into PRISM. They felt, however, that they as individuals did not have sufficient knowledge about PRISM (A.I.D. Program Performance Information System for Strategic Management) to comment on a potential relationship.

VII. Next Steps

Dr. Rossiter as the representative of the A.I.D. Research Advisory Committee (RAC) stated that the next stage would probably best be one in which the Agency would draw upon the input from the workshop, and from other sources to construct within its own environment the strategic planning and research evaluation methodologies that will best work for them. The RAC stands ready to assist in that process and already has suggested that the topic, and the results of the workshop be discussed at its April 9-10, 1992 meeting.

In closing Dr. Bissell stated that the dialog opened new insights on the process of strategic planning and on the players within A.I.D. who need to be involved. He agreed with Dr. Rossiter that there is a role for the RAC and he expressed once again his appreciation for the time and efforts of all participants.

Workshop on Research Progress Measurement and Management Decision Making in the Programs of the U.S. Agency for International Development

Sponsored by:
National Research Council (NRC)
Board on Science and Technology for International Development (BOSTID)

In Cooperations with:
U.S. Agency for International Development (A.I.D.),
Bureau for Research and Development
and Center for Development Information and Evaluation,
Bureau for Program and Policy Coordination

Wednesday, December 11, 1991 and Thursday, December 12, 1991
Main Building
National Research Council
2101 Constitution Avenue, N.W.
Washington, D.C. 20418, U.S.A.

Wednesday, December 11, 1991 — Conference Room NAS 180

Time	Activity	Speaker
8:30 am	Coffee/continental breakfast	
9:00 am	Welcome	Mitchel Wallerstein Deputy Ex. Officer, NRC
9:05 am	Introduction	Bradshaw Langmaid Deputy Asst. Admin, R&D
9:10 am	A.I.D. Program Performance System for Strategic Management (PRISM)	John Eriksson, Director, CDIE
9:25 am	Overview of Research in A.I.D. and Goals for the Workshop	Richard Bissell Asst. Administrator, R&D
9:40 am	Questions and discussion	Arden Bement, Workshop Chair presiding
10:10 am	Break	
10:20 am	Panel I: An Industry Perspective on Research Performance Evaluation	A. Bement, panel member D. Azarnoff, panel member J. Duga, panel member
10:50 am	Questions and General Discussion Note: All participants expected to enter into discussions	

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11:20 am	Panel II: International Research Donors Perspective and Experience	A. Bement, Chair S. Volbeda, Netherlands J. Gaillard, France T. Dottridge, Canada
11:40 am	Questions and General Discussion	A. Bement, Chair
12:15 pm	Break for Lunch (Salads/sandwiches served in NAS 180)	
1:15 pm	Panel III: World Bank Perspective and Experience	A.Bement, Chair G. Ingram, Policy Advisor J. Murphy, Evaluation/Africa
1:35 pm	Questions and General Discussion	
2:00 pm	Panel IV: Evaluation in Agricultural Research	A. Bement, Chair R. Grady, U.S. Dept. Agr. D. Plucknet, CGIAR L. Hardin, Purdue Univ.
2:30 pm	Questions and General Discussion	
3:00 pm	Break	
3:15 pm	Panel V: U.S. Government Agencies and Research Performance Evaluation	A. Bement, Chair S.G. Walters, NSF R.Kostoff, ONR R. Rader, DOE
3:45 pm	Questions and General Discussion	
4:15 pm	Executive Session for NRC Participants	A. Bement, Chair
5:00 pm	Adjournment for Day 1	

Thursday, December 12, 1991		Conference Room NAS 250
8:30 am	Coffee/Continental Breakfast	
9:00 am	Special Session arranged as determined by discussion on Day 1	A. Bement, Chair
10:00 am	Break	
10:20 am	Summing Up Workshop Discussion Questions from A.I.D. Questions/Comments from participants and observers	A. Bement, Chair R. Bissell, A.I.D.
12:00 am	Adjourn	

An Agency-wide Program Performance Information System For Strategic Management: A Plan For Design and Implementation

I. Introduction

This plan describes the key components of program performance information systems evolving in A.I.D. missions and bureaus, discusses how these systems can improve our ability to "manage for results," and lays out next steps for creating a collaborative agencywide program performance information system for strategic management (PRISM), intended to provide one important guide for senior decision-makers.

No single set of program performance indicators can satisfy every manager's information needs equally and simultaneously. Even with a sharper overall strategic focus, different A.I.D. programs, different managers, and different organizational levels will continue to require different kinds of performance information. CDIE therefore envisions a network of partially overlapping program performance information systems that can meet management needs at different organizational levels while providing agencywide performance information for top executives. Existing systems will be drawn upon to the greatest extent possible and new information requirements will be kept to a minimum.

II. Background

Management excellence -- "doing fewer things, but doing them very well" -- has become A.I.D.'s central managemtn theme. But to manage strategically, for better development results, managers need a sound basis for assessing how programs are performing. On October 31, 1990, Administrator Roskens announced a new initiative to "strengthen the role of evaluation in A.I.D." as one basis for better program and policy decision-making and more convincing performance reporting for Congressional accountability. One important aspect of this initiative, building on ongoing regional bureau and CDIE efforts, focuses on improving A.I.D.'s program performance monitoring by strengthening mission and other operational-level performance information systems and developing a core of agencywide program performance indicators.

III. Proposal

A. Objective

To develop an agencywide program performance information system for strategic management (PRISM) and strengthen operational-level performance information systems to provide better information on program results for more informed management decision-making.

B. PRISM Components

CDIE envisions a network of partially overlapping program performance information systems, designed to meet the decision-making needs of senior executives while also providing essential information for managers at other organizational levels. The key requirements include:

Establishing An Agencywide Program Performance Information System by focusing initially on about 10 to 15 performance indicators for approximately four to six key strategic objectives. These objectives and indicators would be selected through an iterative process, facilitated by PPC/CDIE, that would integrate common objectives identified by managemtn at the field level with top managemtn goals.

Over time, the system would be expanded to include performance indicators for other significant agencywide programs and for programs whose strategic objectives still need to be more clearly delineated before performance indicators can be formulated (beginning in FY 92).

Depending on senior management needs, the agencywide system could be further automated and broadened to selectively skim other relevant program performance, activity status, and activity completion information from operational-level performance information systems (decision by mid-FY 92).

A more detailed implementation timeline is provided in Annex 1. Conceptual development of the agencywide system would take place during the Spring 1991; operational development during the Summer of 1991; preliminary data would be available by the Fall of 1991; and PRISM would be an operating system by the Fall of 1992.

Strengthening Operational-Level Program Performance Information Systems (for missions, bureaus, and offices) by continuing and expanding CDIE's support to help missions, bureaus, and functional offices clarify their strategic objectives, formulate more rigorous and credible program performance indicators, utilize similar indicators wherever possible, and make better use of performance information in management decision-making. This includes the development of standards for operational-level program performance information systems (and substantial technical assistance in implementing those standards), standards for documenting activity completion and status reports, procedures for performance information system quality control and review, and procedures for upward reporting as part of the agencywide PRISM.

Making this program performance information useful to senior managers will, however, require other supporting CDIE capabilities envisioned in the Administrator's evaluation initiative. This includes (1) establishing an In-house analytical capability for interpreting the data, (2) relating it to other CDIE and operational-level evaluation findings, and (3) establishing reporting mechanisms to inform the Administrator and other senior managers of program results and their implications.

Ongoing technical assistance to missions and offices is already being expanded; additional standards and guidance will be developed by early FY 92; decisions on developing automated reporting systems will be made later in FY 92.

C. Uses and Limitations

The program performance information system that we have proposed is, essentially, a system for agencywide program performance monitoring. As such, it can tell us whether desired results are occuring and whether program outcomes are basically on track. It can also provde, at a fairly aggregate level, at least a rough comparison of the kinds of results that different programs are seeking and achieving (or that similar programs are achieving in different locations). Perhaps most importantly, it can provide a warning when something is wrong and when intended results are bot being achieved.

By itself, such a performance monitoring system cannot tell us why results have or have not occurred or which, among a range of program alternatives, is the most efficient and effective. Some program performance indicators may also be relatively "slo-moving," and could lag behind more immediate results of program interventions. However, in conjunction with the other evaluation, analysis, and reporting activities embodied in the Administrator's evaluation initiative, program performance monitoring becomes a much more powerful tool for assessing program alternatives.

CDIE's program evaluation studies, for example, will be specifically directed at answering questions about how programs are working, why results vary, and which program alternatives have the most impact and are the most cost-effective and sustainable. Similarly, as operational (mission and office) performance information systems are strengthened, management will be able to better assess intermediate results (the achievement of program outcomes and purposes) and compare the efficacy of program variants across countries and regions. These information systems will also enable us to summarize performance in relation to Congressional earmarks (such as women-in-development) and perhaps, equally important, to demonstrate which earmarks are central to A.I.D.'s strategy and which are more peripheral. All of this information and analysis will then be synthesized in the planned annual report on program performance to the Administrator.

In combination with these analyses, program performance monitoring should provide one useful source of information for budget decisions. This information should also be useful in assessing managemtn performance. It should be noted, however, that the relationship between program performance information and budget decisions or managemtn assessments is neither simple nor straightforward. Poor results may, for example, reflect the extent of the problem being addressed rather than inadequacies in the program, indicating a need for more rather than less resources. Alternatively, performance data may be more relevant to choosing among program alternatives than to establishing overall funding levels for a program area. Nor is past program performance necessarily a good indicator of a country's future receptivity to development investments. Placing too much emphasis on narrowly-defined performance indicators in budget decisions could also distort the program objectives being sought and the data being reported. More generally, performance-based budgeting raises difficult questions -- not fully addressable here -- about how various program objectives, performance levels, other criteria (e.g., bilateral relationships), funding categories, and budget time frames should be inter-related. To begin answering these questions, CDIE plans to initiate an in-depth study of performance-based budgeting as part of our current evaluation agenda.

D. Attributes of an Agencywide Program Performance Information System

For the Agencywide performance information system to be an effective strategic management tool:

1. Program performance indicators should reflect clearly defined strategic objectives.

As the old adge goes: "if you don't know where you are going, any road will take you there." The definition of a performance indicator presumes a clear understanding of what it is we are trying to achieve. In the absence of a clear objective, the arbitrary choice of an "indicator" may implicitly define an objective that is inappropriate or unattainable. Program objectives without performance indicators provide no basis for accountability, but performance indicators without ocrresponding strategic objectives remain empty promises.

Based on ongoing work in developing mission program performance information systems, it is apparent that in some functional areas (e.g., population) strategic objectives are already clearly formulated and indicators are quite similar across a wide range of countries. In other functional areas (e.g., private enterprise) programs encompass a relatively small range of strategic objectives in different countries and may be amenable to more precise formulation. However, in some functional areas (e.g., as agriculture), strategic objectives appear to vary substantially in different country settings, and common objectives and indicators cannot yet be easily fomulated. The process of developing an agencywide program performance information system should help clarify where well-defined objectives exist and where such objectives (and useful indicators) are currently lacking.

Annex 2 suggests some "illustrative" agencywide objectives and indicators. These are based on a review of strategic program objectives and measurable performance indicators developed by more than 30 field missions as well as on recent strategy documents, including the new A.I.D. mission statement and the Administrators' four initiatives. Additional steps to further clarify these objectives and indicators are described in the final section on the "Need for Top Management Action and Support" and in the schedule proposed in Annex 1.

2. Agencywide strategic objective should reflect the highest level of results for which A.I.D. expects to have a significant impact in at least some substantial number of missions within a five to ten year period.

Strategic objective should be within A.I.D.'s "manageable interest," that is they should be largely achievable through the outcomes (targets, benchmarks, or purposes) of A.I.D. project, non-project, and policy dialogue activities. Strategic objectives in turn should contribute to broader, country-level goals that A.I.D. plays a role in achieveing, but could not normally acieve on its own. No single field mission would be expected to encompass all Agencywide strategic objectives. Most missions would pursue a few strategic objectives; some missions might pursue several; while others (presumably very small ones) might pursue only one or even none at all.

3. Program performance for core strategic objectives should be measured consistently across countries.

Performance should be comparable and, to the extent possible, additive for reporting across countries, groups of countries, regions, or worldwide. This does not necessarily require a single, quantitative scale, but does require common definitions of objectives and indicators across countries to that, for example, percentage changes can be compared. In accordance with the Congressional WID mandate, agencywide indicators referring to people should also be reportable by gender. If performance cannot be measured reasonably consistently across countries, then the objective should probably not be part of the PRISM system.

4. Agencywide strategic objectives hould not be specfic to a particular region.

Agencywide strategic objectives should not reflect programs that are unique to a single country or region. They should also be capable of encompassing program performance related to non-regional functional offices (e.g., in S&T, FVA, APRE, etc.) and earmarks. Regional bureaus may, however, have important objectives that are specific to their regions for which they would develop their own distinct indicators for assessing and reporting performance.

5. Agencywide strategic objectives should also reflect a convergence of both top management goals (for example, as expressed in the Agency's mission statement and the Administrator's new initiatives) and major operational emphases in the field (such as chile survival, population, trade and investment, etc.).

However, strategic objectives for different programs do not necessarily have to embody results of the same magnitude or level. In some program areas A.I.D. can reasonably be expected to accomplish more than in others. Strategic objectives should reflect the highest level of results in a program area that is within A.I.D.'s manageable interest and for which A.I.D. is willing and able to be held accountable.

E. Organizational Roles

1. On-going experience in developing field mission performance information systems, facilitating a dialogue between management goals and operational objectives, has proven quite effetive and can be roughly duplicated in developing an agencywide PRISM system.

The development of an agencywide program performance information system should, in other words, be both "bottom up" and a "top down" process. This process has already begun. The initial work of regional bureaus and missions, supplemented by CDIE and consultants, to strengthen performance information systems in nearly 30 missions (including more than a dozen intensive "Program Performance Management and Evaluation Pilots"), has provided extensive information on strategic objectives and indicators at an operational level. The new A.I.D. Mission Statement and the Administrator's new initiatives provide a broad framework of Agency goals and objectives. In mid-February, CDIE convened a two day workshop, involving wide regional bureau and functional office participation, that took the first important steps (see Annex 3) towards articultating the relationship between development programs in the field and agencywide objectives.

This plan has itself been intensively reviewed and substantially revised based on discussions both with managers and technical staff in the operating bureaus. It will be further revised based on feedback from a scheduled April 22nd briefing for the Administrator and the Senior Staff. As outlined in the implementation timeline (Annex 1), the development process will continue intensively and collaboratively through more detailed analysis of operational-level objectives and indicators; further discussions with technical and management staff; and continued feed-back from reorganization transition teams, sector councile, and initiative working groups.

2. Operational units (missions, bureaus, and functional offices) will continue to play the primary role in collecting program performance data.

Every regional bureau and most missions and functional office are already developing performance information systems to meet their own program management, strategic planning, and reporting needs. Most of this mass of information, which goes well beyond what CDIE could collect on its own, will be collected and analyzed through activity-funded mechanisms. Much of this information is also direcly relevant to agencywide performance information needs. Although more consistent and comparable performance measures may be needed for some core objectives, every effort will be made to keep new information requirements to a minimum.

3. CDIE will provide coordination and technical assistance to support the comparability,

credibility, and rigor of operational-level program performance information systems, including reporting on common agencywide core program objectives and indicators.

Rather than creating entirely separate data collection and reporting requirements, CDIE will draw upon existing operational-level program performance information systems, incorporating only those additional elements needed for meaningful agencywide program performance reporting. At the same time, CDIE will play a continuing role in providing technical assistance, delineating standards and guidelines, and reviewing the quality of operational-level information systems. This will result in better and more comparable operational-level performance data, more suitable for summary and synthesis or for eventual incorporation in a more comprehensive automated database.

4. Operational units will continue analyzing program performance information to meet their own decision-making needs and CDIE will take the lead in analyzing program performance information as a basis for Agencywide decision-making and Congressional reporting.

Unanalyzed program performance information has limited utility. Missions, bureaus, and functional offices will continue to play a major -- and, likely, an expanded -- role in analyzing such information as a basis for operational decisions about program design, implementation, and alternatives. In conjunction with its broader evaluation, analysis, and reporting functions, CDIE will play the lead role in analyzing performance information as a basis for top-management program and policy decisions and Congressional accountability.

IV. The Need for Top Management Action and Support

If the Administrator concurs with this plan, we recommend that he:

1. Announce his approval in principle for the approach outlined in this plan and his commitment to pimproving program performance information as a ley element in strategic management;

2. Ask Assistant Administrators, Mission Directors, and other operational managers to participate in clarifying performance objectives and indicators and in developing program performance information systems;

3. Indicate his commitment to a collaborative approach in working with operational program managers to:

- focus the program, "stay the course", and do fewer things, but do them very well;
- delineate agencywide performance indicators that to the greatest extent possible build on and strengthen operational-level objectives and information systems;
- develop core agencywide strategic objectives that reflect both major continuing operational program themes and top management priorities (e.g., new initiatives);
- support the development of "management contracts" through which operational units would be held accountable for measuring program performance and using performance information in program performance and using performance information in program

decisions;

4. Clarify how program performance information will and will not be used, recognizing that it will only be one factor informing management decisions about programs, budgets, and personnel; and;

5. Ensure that sufficient budgetary resources are available to implement program performance information systems, including substantial technical assistance to operational units and support for related data collection and analysis activities.

Research Directions at the Agency for International Development[4]

President Bush stated in his FY 1992 budget sent to the Congress, "Investment in research and development is a top priority for an Administration that believes in investing in the future. Investments in research and development form the foundation for the exploration of all of the new frontiers of today and tomorrow."

The Agency for International Development is certainly no less committed to the need for investing in the future. Throughout the history of A.I.D., the roles of science and technology have been acknowledged as key variables in long-term social and economic development. That emphasis has taken many forms: investments in new technologies, support of science relevant to developing countries, training of LDC nationals to manage research and introduction of new technologies, and the creation and support of institutions in developing countries to manage their own research.

Two features of A.I.D.'s organization and mandate set it apart from every other bilateral assistance agency. The first is the extensive field-based technical staff. The second is the integration of research into the ongoing program. It is the support and application of research that transforms creatively assistance from a resource transfer program into a technology transfer program.

The benefits of such an approach are both anecdotal and systemic. Early investments in the Cholera Research Laboratory in Bangladesh provided at small cost the oral rehydration salts technologies that now save millions of lives each year. The minimal support provided to striga research in Africa could easily result in a 40% increase in sorghum production for years to come. Research investments in general provide both direct and indirect productivity benefits to society. Many studies suggest that research and development spending has a high social rate of return, indeed much higher than the rate of return in a specific project or to a specific enterprise funding the research.

The President's Budget statement for FY 1992 acknowledges that it "is not possible to determine analytically the optimal level" for investments in research or even the best mix. At the same time, however, it asserts that the evidence is clear that increased research investments add to productivity and justifies growing federal support for research and development.

The Agency's approach to research is unique among the programs of the federal government in being intimately related to the needs and opportunities in developing countries, particularly those where A.I.D. has field missions. Indeed, A.I.D.'s research effort is an essential element in the minuscule resources invested in LDC problems; less than 1% of the world's R&D is spent on issues of direct use by developing countries. By building on the research investments of the western public and private sectors, and by relating its support of research to long-term development needs, as well as formulating a research program sensitive to eventual implementation opportunities and hurdles, A.I.D is able to amplify the likelihood that research investments will result in an above-average impact on social and economic indicators.

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[4] Background statement prepared by Dr. Richard Bissell, Assistant Administrator, Bureau for Research and Development, Agency for International Development, Washington, D.C. 20523, July 1991.

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The Agency supports science and technology, in part, because the developing countries ask us to do so. The U. S. has a large comparative advantage in its existing technical capacity, and LDCs look to us for such support. Institutions already exist in the U. S., both public and private sector, working on LDC issues, of a high quality that generates respect among LDC nationals. Finally, it needs to be pointed out that the U. S. benefits from such LDC-oriented research, too, in areas such as agriculture, health, and the environment. The growing internationalization of technical issues means that science has little respect for national boundaries, and U. S. institutions play a crucial role in generating mutual benefits from scientific cooperation. A.I.D.'s support is thus generating a new, collaborative relationships that benefits the scientific infrastructure on both sides.

The Agency supports four kinds of research through its programs, particularly in the Research and Development Bureau: (1) strategic research; (2) adaptive research (called "development" in industry); (3) operations research, for integration of country-specific social and cultural issues; and (4) data base construction and maintenance on global development issues. Each of these areas will be described below.

Strategic Research

The United States government spends about $13 billion each year in support of basic research. As the FY 1992 request states, "Basic research provides the new knowledge that leads to new products and processes. Basic research, especially at universities, is an essential investment in the Nation's scientific and technological future, including its future scientists and engineers." Much of the work done by other agencies of the USG is not immediately relevant to the developing countries, but where it is, as in some aspects of health research, close collaborative ties have been constructed to allow this agency to draw on the work of the National Institutes of Health and other agencies. Similarly, some of the research areas funded by the Department of Agriculture are of direct, long-term value to A.I.D. programs in the field. The work done by the National Science Foundation on biodiversity links extremely well to A.I.D. development work.

A.I.D. does not support basic research (as usually defined), but it does support longer-term applied research, i.e., strategic research, which is targeted on a specific development problem and whose high payoff must be quantified over longer time-spans than covered in the normal Agency planning period. In that sense, its rationale is very close to the USG policy on basic research. A.I.D. proposes very focussed areas for such work in FY 93. The Agency chooses to support strategic research where problems exist uniquely in developing countries, where other agencies and the private sector will not undertake investments, where these efforts can also provide mutual advantage to the U. S., and where unique implementation issues require input of a development perspective.

As examples, the Agency proposes spending:

- $8.5 million in FY 93 to investigate potential malaria vaccines to counter a killer of over one million children each year in Africa alone;
- $4 million to support development of new contraceptives, through the CONRAD project;
- $20 million to support the excellent collaborative research support programs (CRSP) with U. S. universities and USDA in agriculture;

- $2.5 million to the multi-donor Tropical Disease Research program at WHO next year;
- $45 million to the widely-respected Consultative Group on International Agricultural Research.

In each of these enterprises, not only is there tremendous leverage of resources from other donors and institutional partners, but the payoff in long-term increases in health and agricultural productivity demonstrates their value per dollar invested.

A.I.D. also offers limited competitive grant opportunities in the areas of strategic development research. The Program in Science and Technology Cooperation has offered for some years a chance for researchers to explore scientific possibilities with relatively small grants. A.I.D. also offers a competitive grant program for the historically-black colleges and universities. In each of these cases, the purpose is to encourage small-scale risk-taking, and to entice capable U.S. researchers to become engaged in the problems of developing countries. It is small seed corn, from which some very important long-term capabilities are developed into sustainable and substantial scientific efforts.

A.I.D.'s strategic research is inevitably limited in scale and scope. While greater risks are taken in such research in terms of probable success, they need to be fully justifiable in terms of developmental needs, priorities, scientific excellence, and the history of returns in the particular disciplines. Most of the Agency's access to the results of basic research is second-hand, through research funded by the private sector or other agencies, rather than through direct support from A.I.D. The Agency's own investments must be targeted, competitively selected, and subject to vigorous peer review.

Adaptive Research

The Administration has proposed a broad-scale increase in support for adaptive, or short-term applied, research in FY 92, and expects to increase it each year. The President's budget calls for increases "across a wide range of technology areas," with an increase of 10% proposed in FY 92. A number of the areas of federal emphasis apply equally to research at A.I.D., including new energy technologies, protection of public health through biomedical and behavioral research, the U.S. global climate change program, new materials, telecommunications, expanding the human frontier through biotechnology, and strengthening science/math education.

A.I.D. brings a special perspective to Administration priorities as well as a set of priorities generated by the nature of mission-based programs. In other words, funds are best invested in adaptive research where A.I.D. has the capability to be involved in their ultimate application in the developing countries themselves. In most cases, such application is through A.I.D. country programs; in other cases, adaptive research is given effect through cooperation with international agencies, PVOs, universities, and for-profit private sector entities. Given the importance of technology transfer in nearly all A.I.D. programs, the focus of adaptive research investments tends to follow the scale of resources made available through Administration proposals and Congressional priorities.

The gap between technological capabilities in the United States and the developing countries is on that A.I.D. has an interest in helping to bridge. If that gap counties to grow, we can be assured that the standard of living in developing countries with fall further behind the industrialized countries. Much of the gap will be bridged by the private sector, although it is not likely to contribute much, for example,

to production of indigenous foods such as cassava or millet, or where natural resource protection is needed. The special niches where A.I.D. can be of assistance are more generally those where potential profits are small or where the public sector monopolizes access. For instance, small or where the public sector monopolizes access. For instance, the provision of new primary health care technologies (such as nonreusable syringes and heat exposure markers for vaccine vials) is of universal value and certainly has a high social rate of return. Nevertheless, because these are not items purchased by individual consumers and because the profit margin is likely to be marginal at best, investments in development of such technologies are not attractive to private commercial interests. Hence, the role of the PATH project and similar endeavors is to make the early, risky investments in product development, after which commercial groups may be more assuredly involved. Similarly, reliable Vitamin A dosing systems will have to be created to develop fully the benefits of the Agency's past investment in that nutritional element. Education is another such area: A.I.D.'s Interactive Radio Instructions raises student performance on core curriculum subjects by nearly an entire standard deviation -- an improvement in learning outcomes unmatched by any other input. Finally, policy is a crucial area bridging all technical sectors. Adaptation of policies is as important as the technologies themselves, since people victimized by bad policies will never be able to take advantage of technological opportunities.

Given the large proportion of the population devoted to agriculture in developing countries, extensive potential exists for supporting the translation of science into useful products. The large investments already made in crop science have fed a generation. The next generation will require significantly greater leaps in agricultural productivity, both for food and to be able to set aside inappropriate agricultural lands for environmental reasons. Indeed, we already know that productivity increases needed will be far more difficult to achieve than those accomplished by the green revolution technologies. The site-specific nature of agriculture makes clear the necessary connection between research and farm-level realities. Neither can prosper without the other. New approaches will have to involve end users more directly while also incorporating a broader range of disciplines to address the issues. Research will be necessary on an increasing scale for the income needs of rural populations, particularly in agribusiness, where the possibilities of absorbing rural labor forces are substantial. More centrally, A.I.D. will need to support additional work on the interaction of agriculture with the natural resource base. The creation of the Sustainable Agriculture CRSP in FY 92 will provide both the systematic research base for improving understanding of issues, and an access point for missions to obtain the help they need for application to local conditions.

The adaptive research needs in natural resource, energy, and the environment will continue to grow. As the global problems in the natural resources base are focussed down on national solutions, A.I.D. is being called upon to assist developing countries in the search for appropriate answers. While much of environmental science has been developed for the advanced countries, relatively little has been accessible to developing countries. The policy environments need tremendous adjustment to take natural resource issues into account. Several major efforts are already launched by A.I.D.: e.g., the work on forestry and fuelwood in Asia, the work on biomass energy in decentralized systems, and the research on biodiversity conservation. Each of these areas illustrates what has already been accomplished as well as the potential for what can be done as we understand better the underlying science and policy issues of developing country natural resources bases. Renewable energy technologies, in particular, appear likely to receive growing emphasis in upcoming research programs. Energy efficiency has the potential for serving environmental interests as well as U.S. export interests; research in India has already resulted in extraordinary efficiency gains in design of its future baseload plans, predicated on export of cogeneration and clean coal technology from the U.S.

The social science bring major issues of adaptive research to the A.I.D. agenda. Whether in education, or in women in development, or the construction of new macroeconomic models, the basic research conducted in the U.S. on these issues requires translation for use in developing countries. In each of these areas, the problems will not be met by simply increasing resources; the one hundred million children not in school will not be enrolled by any foreseeable increase in the A.I.D. education account. On the other hand, the application of more efficient and effective educational systems, along with sensible human resource policies, could achieve the enrollment goals within reasonable resources. A.I.D.'s education research projects began the decade by introducing sector assessments to gather necessary data, then moved to educational management information systems and ultimately to study data-based educational planning models. Confronting future challenges in the environment will require investments in social science efforts such as DESFIL, which investigates the potential for protection of fragile lands in the midst of economic and social change, and the Environment and Natural Resources Policy and Training Project, which will both perform policy research, and strengthen capacity in developing country to develop new economic analysis methods. Understanding and then attempting to modify high-risk behavior that puts entire families at risk of HIV infection also demands concerted attention. The R&D Bureau believes that major new investments must continue to be made in the social sciences, to create the kind of social nd micro-economic analysis to complement the progress being made by the macro-economic planners in developing countries.

The behavioral sciences have brought new opportunities to the world of development in the last decade. U.S.-designed research in communications, marketing, and information processes are now bearing fruit throughout the developing countries in all technical sectors. The U.S. and A.I.D. are the recognized leaders in this area. The sophisticated marketing of contraceptives, health supplies and services, AIDS education, integrated pest management, and educational aspirations has not only had a significant effect already; it holds out the promise of reaching those pockets of developing country populations that many had relegated to immeiseration. Behavioral research continues to go forward in all sectors of Agency work. Special emphasis will have to be given in coming years to the environment, HIV infection and drug demand reduction. Research needs to be nurtured and sustained over time, with close integration into field application and replication across regions. The R&D request for such behavioral research is embedded in a range of projects, generally combined with operational research in direct support of USAID missions efforts. The population program's support of behavioral research, particularly in the popular music area, has won awards from many quarters. The respect accorded A.I.D.'s work in behavioral research is such that the National Institutes of Health is committing over $2 million of its own resources to supplement A.I.D.'s behavioral research grants in the area of AIDS prevention.

Operations Research

Research in support of current field operations is conducted throughout the Agency, and enters into the design, implementation and evaluation of successful projects everywhere. Such research makes up the bulk of R&D core funding in mission support projects. It consists in large pare of drawing the relevant experience from the Agency's global portfolio and making that experience available in usable form to missions and host governments on a variety of analytical, design, and implementation issues. Such research draws upon and reinforces capacity in U.S. institutions to respond to country needs. It provides the insights to adapt proven interventions to specific country settings, as well as to improve the efficiency and impact of service delivery programs. The operations research is executed and managed in the technical offices of the R&D Bureau, as well as in technical offices of the regional bureaus and

especially in their missions.

As an example, the outstanding work of the family planning effort in A.I.D. is based upon well-established operations research. It involves careful analysis of field worker needs, given the importance of face-to-face contacts between consumers and workers. It identifies the best process for introducing new technologies and services, many of which may be culturally alien to most populations in developing countries. It involves communications skills that have been best developed in societies with the market orientation of the United States, and are equally transferable to social services and innovative economic goods. Operations research allows missions and back-stopping projects to improve service delivery, test innovative approaches, demonstrate the demand for new social services, influence and change policy, and to promote a problem-solving mentality in societies terribly skeptical about the value of change. In each of these ares, and many others, operations research has given A.I.D. a unique form of value added to the development process that no other aid donor possesses.

Research Infrastructure

The issue of research infrastructure, or more specifically research data management, is an emerging area for A.I.D. support. In prior decades of A.I.D. support for building research capacity in LDCs, much investment went into "bricks and mortar" projects. In that way, the U.S. was a party to establishing the kinds of national research systems that most developing countries aspire to create. In the process of transferring technologies to developing countries, such centers of research play an important role.

It has also been recognized that such a strategy has limitations, and for that reason, A.I.D. also has a long history of investing in "human infrastructure" and institution-building. The long-term training programs supported by A.I.D. missions, with some 10,000 students in the U.S. each year in such educational work, have been necessary components of capacity-building in developing countries. The buildings would have been meaningless without the knowledge and pursuit for excellence that comes from the training of motivated scientists and technicians.

As we enter the 1990s, we increasingly recognize that viable research and development efforts most include adequate information systems. The weakness of data (and the systems for collecting, analyzing, and applying those data) in developing countries is evident to anyone who has worked there. One of the overwhelming impressions of the Administrator in his trip to Africa in early 1991 was the paucity of reliable data. How can countries design meaningful health programs, for example, when they have no valid estimate of the number of measles deaths each year -- and whether the number is rising or falling? How can developing countries and USAID missions work together on development issues without being able to define problems adequately and accurately? Drug use prevalence surveys are necessary, but generally not available, in the design of narcotics education projects.

The R&D Bureau has been working on the issue of data access and management for some time, and identifies it as a major infrastructural issue for the coming decade. In the area of life sciences, the work of the Demographic and Health Surveys has not only provided data on family planning and health issues; they have also established a standard for other fields to emulate in focussed, affordable, and useful data sets for data-poor environments. The results of the surveys are accessible not only to USAID missions and U.S. scientists, but also to developing country researchers and international institutions in a way that has significant multiplier effects. The surveys also provide a reliable data base for developing

countries and for donors such as A.I.D. to help measure the impact of their efforts. They provide the baseline for policy dialogue and the analytical underpinnings for non-project assistance. The approach developed in the RAPID project, which established a reputation for integrating research data into programming on family planning, is now being applied to similar models for AIDS, an early version of which is credited with persuading the Ugandan leadership to make major policy changes on AIDS. Similar work needs to be launched in other sectors, such as the environment, global warming, and urban health, to establish data bases and to provide the electronic sinews for collaborative research and development work. The R&D Bureau is moving towards broadening the strategy on data and information, through discussions with other donors, with USAID missions, with universities, and the private sector. While some investments in this area consist of discrete projects (such as the DHS in Population and Health, and GREENCOM in the Education Office), others are emerging as integral to program efforts. One can anticipate that, over time, such investment in data systems will be seen as important as the buildings, institutions and scientists that characterized earlier A.I.D. investments.

Setting Priorities in Research Directions

The world of technological change is vast. A.I.D. must pick its investments carefully, responding to needs, opportunities, and capabilities. Research must respond to issues both globally and locally, from global ecological changes, such as the diminishing ozone layer; to pervasive, local erosion of topsoil; to U.S. commitments to eradicate particular diseases, such as measles or polio.

Opportunities are important id determining investments. The availability of new biotechnologies to meet food and medical needs creates an opportunity that A.I.D. and the developing world cannot afford to pass up. The change to meet generational food needs, much as the green revolution accomplished in the 1970s, is one to be seized avidly, and the state of biotechnological sciences promises that investments will be rewarded handsomely.

Finally, capabilities need to assessed from an objective point of view. The Agency needs to know capabilities both in the U.S. and in developing countries -- on the one hand, to adapt science to the scale of developing country realities, and on the other hand, to give technologies a sustainable home in institutions within developing countries. The formation of the Center for University Cooperation and the expanding cooperation underway with other USG agencies such as NSF and NIH, and the for-profit private sector will provide A.I.D. with better knowledge of capabilities. USAID missions are increasingly unable to track changes in various sectors outside their main focus, and yet critical opportunities may be missed simply because missions have narrowed their sectoral interests to the point where important information is available at little or no cost, but remains unseen. But A.I.D. also has to be careful not to assume that all we need to know for meeting LDC issues is on the shelf. Whether for old or new problems, the development of new knowledge and technologies is as important as it was several decades ago.

The process of bringing together needs, opportunities, and capabilities requires many partners. The Research Advisory Committee (RAC) has the central mandate from the Administrator to oversee the research function in A.I.D. Where the RAC requires additional expertise, it turns frequently to the National Academy of Sciences and its Board on Science and Technology for International Development. The subcommittees of the BIFADEC provide substantial expertise in particular areas. And in each of the research areas support by R&D, technical advisory committees consisting of recognized experts from the private sector have been created. To ensure exchange of views with those defining field needs, R&D

has proposed in the reorganization the creation of a Research Council on which each of the Assistant Administrators would be represented. Finally, research must relate to the strategic planning of the Agency, and the unit in Policy responsible for such planning has committed itself to incorporating research into their long-range exercises.

Research at A.I.D. is not a portfolio; research is a process that continually provides the value added in the development assistance business. The commitment of the Agency to research and its development into field-usable products focusses management and officers on innovation. Economic and social development cannot afford to remain static patterns. The fruits of research, gathered from whatever source by A.I.D., are essential to providing developing countries with the prospect of long-term progress.

Workshop on Research Progress Measurement and Management Decision Making in the United States Agency for International Development

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