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9. ABSTRACT

The activities of a three-day conference of 54 natural scientists, social scientists, and administrators are reported. One purpose of the conference was to examine recent evidence on the returns to investment in national and international agricultural research systems. A second purpose was to explore the relevance of social and economic factors for the organization and management of national and international research systems. Twenty-five papers were presented and discussed in six sessions. The first session was devoted to a series of studies of the organization and productivity of national research systems in both developed and less-developed countries. The second session discussed the impact of the new international research system on agricultural research capacity and productivity. The third discussed the growth of the new system of international agricultural research institutes. The fourth reviewed some issues bearing directly on the management of agricultural research systems. The fifth examined the role of economic and social factors in the choice of research priorities. The final session focused on research strategy and management issues that face both national research systems and the new international research systems. The conferees agreed on the need for developing a fuller understanding of the process of diffusing agricultural technology and knowledge. They also affirmed evidence that investment in national and international research has been highly productive.

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Resource Allocation and Productivity in National and International Agricultural Research

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This seminar report is based on a Conference on Resource Allocation and Productivity in National and International Agricultural Research held at Airlie House, Virginia on January 26-29, 1975.

The conference was sponsored by the Agricultural Development Council under its Research and Training Network Program. The ADC/RTN program is funded under a contract with the U.S. Agency for International Development. Support for international participation in the conference was also provided by the International Bank for Reconstruction and Development.

Single copies of this report are available at no charge from the Agricultural Development Council. A book based on the papers presented at the conference will be published by the University of Minnesota Press in 1976.

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Director

A/D/C Research and
Training Network

Introduction

Within the last decade significant steps have been taken to narrow the agricultural technology gap among countries. A new set of international agricultural research centers have been institutionalized. A number of developing countries have made substantial progress in strengthening national agricultural research capacity. On a global basis it is estimated that expenditures on agricultural research, in constant 1971 U.S. dollars, rose from approximately $1.3 billion in 1959 to $3.8 billion in 1974. In the less developed countries of Latin America, Africa and Asia, the increase was from an estimated $141.0 to $957.0 million.

The effect of this surge in research investment was dramatized by the Green Revolution. But the process of building research capacity in developing nations was less dramatic. Years of agricultural development effort, based on the extension of existing or imported technology, or on research directed primarily to increasing productivity in export crops, had revealed the need to develop more broadly based agricultural research capacity in tropical countries.

The organization of two international research institutes in the early 1960's, the International Rice Research Institute (IRRI) in the Philippines and the International Maize and Wheat Improvement Center (CIMMYT) in Mexico, was part of a wider contemporary re-focusing on research as a

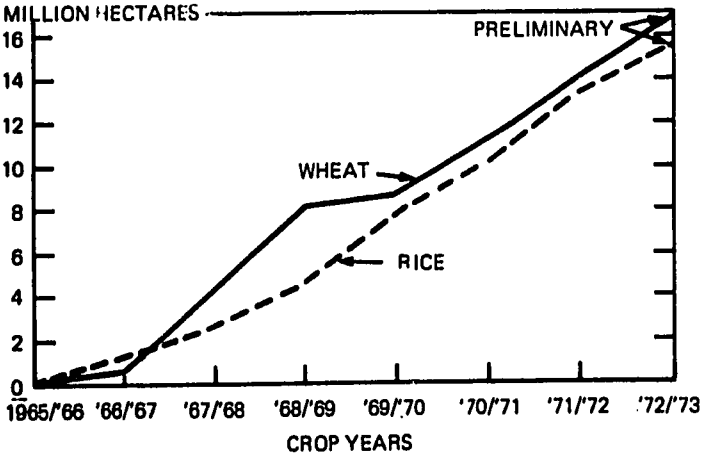
necessary instrument of agricultural productivity change in LDC's.

The ensuing story is well known. CIMMYT and IRRI developed short-strawed, fertilizer-responsive, high-yielding wheat and rice varieties which were rapidly adapted and adopted in parts of Asia and produced a take-off in grain production popularly known as the Green Revolution.

In 1972/1973, 30 million hectares of crop land in Asia were planted to high-yielding wheat and rice varieties adapted from IRRI'S AND CIMMYT'S research (Tables 1.1 and 1.2). The packages of technology derived from the work of these two institutes are estimated by one scholar to have added $1.0 billion to Asian grain production in 1972/1973.*

With the success in wheat and rice, support for internationally sponsored agricultural research mushroomed. Crawford identified 9 international research institutes and two other international programs, in operation or in the process of being established in the tropics (Table 2). Their budgets, which were $4 million in 1969, totalled $34 million in 1974 and are $46-48 million in 1975.

Figure 1.
ESTIMATED HIGH-YIELDING WHEAT AND RICE AREA, ASIA AND NORTH AFRICA, 1965/66 to 1972/73
(Excluding Communist Nations)



Source: Dana G. Dalrymple, "Development and Spread of High-Yielding Varieties of Wheat and Rice in the Less Developed Nations," Foreign Agricultural Economic Report No. 95 (Economic Research Service, U.S. Department of Agriculture, Washington, D.C., 1974).

The international institutes were originally conceived to back up the developing countries' own research programs. The attention accorded the institutes during the Green Revolution tended to obscure this relationship for awhile. It has increasingly re-asserted itself as the complexity of the research task facing developing countries has been recognized. The conception that began to grow in the 1960's—that effective research capacity in developing countries is a primary means to raise agricultural productivity—is now widely shared. Investment in agricultural research in developing countries has grown rapidly in recent years. The World Bank, USAID, and other donors have moved agricultural research up their lists of priorities for assistance.

As the agricultural research system has continued to expand, research productivity and research resource allocation have become important issues for development planners and for science managers. One purpose of the Airlie House Conference was to examine recent evidence on the returns to investment in national and international agricultural research systems. A second objective was to explore the relevance of social and economic factors for the organization and management of national and international research systems. Technical issues related to the measurement of research productivity, the planning of research programs, and the management of research systems were also discussed.

The conference drew together fifty-four natural scientists, social scientists and administrators from research and international agencies—some of them wearing more than one hat. This seminar report attempts to systhesize (but not summarize) the Conference's twenty-five papers and three days of discussion*.

The Conference Agenda

The first session was devoted to a series of studies of the organization and productivity of national research systems in both developed and less-developed countries. It includes a paper on cycles in research productivity and in international diffusion patterns for three commodities—sugarcane, wheat and rice (by Robert E. Evenson). There were two papers on returns to investment in agricultural research in developing countries—Columbia (by Reed Hertford) and India (By A.S. Kahlon, H.K. Bal, P.N. Saxena, and Dayanatha Jha). There were also two papers on the organization and productivity of research systems in developed countries—the federal-state system in the United States (by Willis L. Peterson and Joseph C. Fitzharris) and the national-prefectural system in Japan (by Yujiro Hayami and Masahatsu Akino). The papers in this session were summarized by Alain de Janvry. The model of the research process which de Janvry presented in his summary served as an organizing theme for discussion throughout the conference (see below).

The second session was devoted to a discussion of the impact of the new international research system on agricultural research capacity and research productivity. One way in which the new system of international agricultural research institutes exerts an impact on national systems is through the training of research scientists, technicians, and production specialists. The first paper in this session (by Burton E. Swanson) presented a comparison of the objectives and the performance of the CIMMYT and IRRI training

* *Based on 1972 world market prices and using a moderate factor to determine the portion of output growth resulting from new technology; from Dana C. Dalrymple, "Impact of the International Institutes in Crop Production," presented at Airlie House, Virginia Conference, January 26-29, 1975.*

* *The authors wish to express their appreciation to Lowell S. Hardin, (Ford Foundation), M.G.C. McDonald Dow (National Academy of Sciences), and Fred Wittenbert (Parker Pen Company) for access to their conference notes in preparing this summary. We have also made liberal use of comments by other conference participants on an earlier draft of this summary.*

Table 1.1 — Estimated area planted to high-yielding varieties of wheat in Asia and North Africa (in hectares)

Country	Unit	1965/66	1966/67	1967/68	1968/69	1969/70	1970/71	1971/72	1972/73
ASIA									(Prelim)
1. Afghanistan	Hectares		1,800	22,000	122,000	146,000	232,000	255,000	450,000
2. Bangladesh	Hectares	–	–	–	8,400	9,100	13,500	15,000	21,450
3. India	Hectares	3,000	540,900	2,942,000	4,792,700	4,917,600	6,480,000	7,861,400	10,236,800
4. Iran	Hectares	–	–	–	10,000	90,000	250,000	277,000	298,000
5. Iraq	Hectares	–	–	6,400	41,700	195,200	125,000	950,000	457,000
6. Jordan	Hectares	–	–	–	90	100	120	140	150
7. Lebanon	Hectares	–	–	50	400	2,500	7,000	12,000	20,000
8. Nepal	Hectares	1,400	6,600	24,800	53,800	75,500	98,200	115,900	170,300
9. Pakistan	Hectares	4,900	101,200	957,100	2,387,700	2,681,500	3,128,500	3,286,200	3,338,800
10. Syria	Hectares	–	–	–	–	–	38,000	75,000	180,000
11. Turkey	Hectares	–	600	170,000	579,000	623,000	640,000	650,000	(650,000)[1]
Subtotal	Hectares	9,300	651,100	4,122,400	7,995,800	8,740,500	11,012,100	13,497,600	15,822,500
AFRICA									
1. Algeria	Hectares	–	–	–	–	5,100	140,000	320,000	600,000
2. Morocco[2]	Hectares	–	–	200	4,900	46,500	90,000	206,000	294,000
3. Tunisia	Hectares	–	–	800	12,000	53,000	102,000	60,000	99,000
Subtotal	Hectares	–	–	1,000	16,900	104,600	332,000	586,000	993,000
Total Asia and Africa	Hectares	– 9,300	651,100	4,123,400	8,012,700	8,845,100	11,344,100	14,083,600	16,815,500

[1] 1971/62 area. [2] Unofficial estimate.

Table 1.2 — Estimated area planted to high-yielding varieties of rice in Asia (in hectares)

Country	Unit	1965/66	1966/67	1967/68	1968/69	1969/70	1970/71	1971/72	1972/73
ASIA									(Prelim)
1. Bangladesh	Hectares	–	200	67,200	152,200	263,900	460,100	623,600	1,069,600
2. Burma	Hectares	–	–	3,400	166,900	143,000	190,900	185,100	199,200
3. India	Hectares	7,100	888,400	1,785,000	2,681,000	4,343,500	5,589,200	7,411,400	8,639,100
4. Indonesia	Hectares	–	–	–	198,000	826,000	913,000	1,338,000	1,521,000
5. Korea (South)	Hectares	–	–	–	–	–	–	2,700	187,000
6. Laos	Hectares	–	360	1,200	2,000	2,000	53,600	30,000	50,000
7. Malaysia	Hectares	42,300	62,700	90,700	96,100	132,400	164,600	196,900	217,300
8. Nepal	Hectares	–	–	–	42,500	49,800	67,800	81,600	177,300
9. Pakistan	Hectares	–	80	4,000	308,000	501,400	550,400	728,500	643,500
10. Philippines	Hectares	–	82,600	653,000	1,012,800[1]	1,354,000	1,565,000	1,827,000	1,752,000[1]
11. Sri Lanka	Hectares	–	–	–	7,000	26,300	29,500	29,600	17,600
12. Thailand[1]	Hectares	–	–	–	–	5,000	115,000	315,000	350,000
13. Vietnam	Hectares	–	–	500	40,500	201,500	502,000	674,000	835,000
Total Asia (rounded)	Hectares	49,400	1,034,300	2,605,000	4,706,000	7,848,800	10,201,100	13,443,400	15,658,600

[1] Unofficial estimate.

Source: Dana G. Dalrymple, "Development and Spread of High-Yielding Varieties of Wheat and Rice in the Less Developed Nations," Foreign Agricultural Economic Report No. 95 (Economic Research Service, U.S. Department of Agriculture, Washington, D.C., 1974.

Table 2
Present structure of the international agricultural research network.

Center	Location	Research	Coverage	Date of initiation	Proposed budget for 1975 ($000) (7)
IRRI (International Rice Research Institute	Los Banos, Philippines	Rice under irrigation; multiple cropping systems; upland rice	Worldwide, special emphasis in Asia	1959	8,520
CIMMYT (International Center for for the Improvement of Maize and Wheat)	El Batan, Mexico	Wheat (also triticale, barley); maize	Worldwide	1964	6,834
CIAT (International Center for Tropical Agriculture)	Palmira, Colombia	Beef; cassava; field beans; farming systems; swine (minor); maize and rice (regional relay stations to CIMMYT and IRRI)	Worldwide in lowland tropics, special emphasis in Latin America	1968	5,828
IITA (International Institute of Tropical Agriculture)	Ibadan, Nigeria	Farming systems; cereals (rice and maize as regional relay stations for IRRI and CIMMYT); grain legume (cowpeas, soybeans, lima beans, pigeon peas); root and tuber crops (cassava, sweet potatoes, yams)	Worldwide in lowland tropics, special emphasis in Africa	1965	7,746
CIP (International Potato Center)	Lima, Peru	Potatoes (for both tropics and temperate regions)	Worldwide including linkages with developed countries	1972	2,403
ICRISAT (International Crops Research Institute for the Semi-Arid Tropics)	Hyderabad, India	Sorghum; pearl millet; pigeon peas; chick-peas; farming systems; groundnuts	Worldwide, special emphasis on dry semi-arid tropics, nonirrigated farming. Special relay stations in Africa under negotiation	1972	10,250
ILRAD (International Laboratory for Research on Animal Diseases)	Nairobi, Kenya	Trypanosomiasis; theileriasis (mainly east coast fever)	Africa	1974	2,170
ILCA (International Livestock Center for Africa)	Addis Ababa, Ethiopia	Livestock production systems	Major ecological regions in tropical zones of Africa	1974	1,885
IBPGR (International Board for Plant Genetic Resources)	FAO, Rome, Italy	Conservation of plant genetic material with special reference to cereals	Worldwide	1973	555
WARDA (West African Rice Development Association)	Monrovia, Liberia	Regional cooperative effort in adaptive rice research among 13 nations with IITA and IRRI support	West Africa	1971	575
ICARDA (International Center for Agricultural Research in Dry Areas)	Lebanon	Probably a center or centers for crop and mixed farming systems research, with a focus on sheep, barley, wheat, and lentils	Worldwide, emphasis on the semi-arid winter rainfall zone		

Source: Nicholas Wade, "International Agricultural Research," *Science* 188, p. 587, May 1975.

programs. A second paper (by Dana Dalrymple) documented the impact of the CIMMYT and IRRI varietal development programs on wheat and rice production in Asia, Africa and Latin America. A third paper (by Robert E. Evenson) presented measures of the rates of return to the resources invested in research at the international agricultural research institutes. A particularly noteworthy feature of the Evenson paper is .... the quantitative demonstration .... of (a) the complementarity between internal research capacity and capacity to borrow from other national systems and the international system and (b) the relatively high payoff to investment in supporting research (i.e. directed basic research) in both developing and developed countries.

The third session was devoted to a discussion of the development and growth of the new system of international agricultural research institutes. The first paper (by J.G. Crawford) traced the evolution of the institutional system for organizing, funding and managing the new institutes —first by the Ford and Rockefeller Foundations and later through the Consultative Group on International Agricultural Research. The problem of establishing effective working relationships between the international and national agricultural research systems was reviewed (by Sterling Wortman). The programs that have been developed to achieve closer articulation of the research programs of the international and national systems were outlined by the Director of IRRI (Nycle C. Brady) and by the Director of CIMMYT (Haldore Hanson). There was also a particularly interesting discussion (by S.M. Sehgal of Pioneer Hi-Brid

International) of the contribution of private sector agricultural research to the development and diffusion of genetic technology, in relation to public sector national systems and the international system. In his summarization of the material presented in this session Arthur Mosher raised an issue that received a good deal of discussion throughout the rest of the Conference—are the programs of the international agricultural institutes evolving from a primary research focus to a broader agricultural development focus? Is such an evolution desirable or undesirable?

The objective of the *fourth session* was to review some of the issues bearing directly on the management of agricultural research systems. The first paper (by Albert Moseman) outlined the evolution of coordinated commodity research programs as an efficient device for the organization of national research effort. Two papers focused on the problem of reorganizing or reforming national research systems in the United Kingdom (by T.L.V. Ulbricht) and in Brazil (by Jose Pastore and Eliseu R.A. Alves). Pastore and Alves argued that the national systems characterized by highly coordinated commodity-oriented programs are more relevant to today's LDC experience than the more decentralized system that had been employed in Japan and the United States. Two other papers focused on the methodology of research planning. The literature on the formal models and methods used to allocate resources in research was reviewed and evaluated (by C. Richard Shumway). A systems approach to research resource allocation was described and evaluated (by Per Pinstrup Anderson and David Franklin). Much of the discussion (led by Richard Nelson) focused around issues relating to centralization and decentralization in research resource allocation.

The purpose of the *fifth session* was to examine the role of economic and social factors in the choice of research priorities. The first paper (by Martin Abel and Delane Welsch) outlined a theoretical model for exploring the relative effects of environmental constraints, prices and the commodity composition of agricultural output on research resource allocation. An empirical investigation of the effects of efforts to relate research resource allocation to alternative goals such as labor absorption was presented (by John W. Mellor) using data from India. Use of an economic model to establish research priorities, based on Brazilian data, was illustrated (by G. Edward Schuh). The role of resource endowments and relative prices in inducing the choice of alternative paths of technical change—along labor-saving and land-saving paths—was documented (by Hans Binswanger). A paper which explored the theory of scientific discovery was presented (by Yoav Kislev). The discussion (led by Raj Krishna) focused on some of the conceptual limitations in efforts to define an optimum research strategy.

The *final session* focused on research strategy and management issues that face both national research systems and the new international research systems. In the final discussion paper (by J.G. Crawford) the policies and problems facing the Consultative Group on International Agricultural Research and its Technical Advisory Committee in its efforts to strengthen national and international agricultural research were reviewed. A second paper (by Theodore W. Schultz) focused on the problem of establishing an effective economic and institutional environment for the release of the creative power of agricultural science and the productive capacity of agricultural producers.

In the following sections we discuss in greater detail the issues which activated the conference dialogue. We have organized these issues under five headings: (a) agricultural research productivity, (b) the demand for research and technical change, (c) the generation and diffusion of agricultural technology, (d) the productivity and potential of the international agricultural research institutes, (e) the organization and management of agricultural research, and (f) improving research decision making.

Productivity of Agricultural Research

The fundamental significance of technical change is that it permits the substitution of knowledge for resources, or of less expensive and more abundant resources for more expensive resources, or it releases the constraints on growth imposed by inelastic resource supplies.

There has been a proliferation of studies in recent years which indicates that returns to investment in agricultural research have been high compared to other investment opportunities. Several papers presented at the conference added to this evidence (Tables 3.1 and 3.2).

The rate of return estimates for agricultural research were challenged by Webster and Ulbricht on both methodological and empirical grounds*. Webster pointed out that they tend to array gross benefits from research against only the direct costs of research, omitting implementation costs. Correcting this deficiency, some argue, would bring the returns more in line with conventional development projects (for which a 15-20% internal rate of return is considered good). Ulbricht argued that the estimated rates of return imply a spurious accuracy to the estimates of benefits that are at best highly subjective. Relatively few studies, other than the studies of tomato harvesting in California by Schmitz and Seckler and the Hayami and Akino paper at this conference (Table 3.1), have taken the distributional effects of technical change into account.

A counter argument is that the benefits are conservatively stated and that indirect effects, such as spillover benefits beyond the country or region originating the research, are not fully captured.

The majority of conference participants were of the opinion that while there are methodological problems with such studies, the over-all robustness of the return figures is not in doubt. This judgment was not accepted by Webster and Ulbricht. They supported the conclusion that investment in agricultural research in developing countries is warranted, not on the basis of the high rates of return that have been estimated, but because agricultural research has been an important input leading to increases in agricultural productivity.

* *The basic theoretical foundations for the returns to research studies are most thoroughly explored in John Martin Currie, John A. Murphy and Andrew Schmitz, "The Concept of Economic Surplus and its Use in Economic Analysis",* The Economic Journal, *Vol. 81, (December 1971), pp. 741-799.*

Table 3.1
Summary of Direct Cost-Benefit-Type Studies of Agricultural Research Productivity

Study	Country	Commodity	Time Period	Annual Internal Rate of Return (%)
Griliches (1958)	U.S.A.	hybrid corn	1940-1955	35-40
Griliches (1958)	U.S.A.	hybrid sorghum	1940-1957	(20)
Peterson (1966)	U.S.A.	poultry	1915-1960	21-25
Ardito-Barletta (1970)	Mexico	wheat	1943-1963	90
Ardito-Barletta (1970)	Mexico	maize	1943-1963	35
Evenson (1969)	S. Africa	sugarcane	1945-1962	40
Ayer (1970)	Brazil	cotton	1924-1967	77+
*Hertford, Ardila, Roches and Trujillo (1975)	Colombia	rice	1957-1972	60-82
	Colombia	soybeans	1960-1971	79-96
	Colombia	wheat	1953-1973	11-12
	Colombia	cotton	1953-1972	none
*Peterson and Fitzharris (1975)	U.S.A.	aggregate	1937-1942	50
			1947-1952	51
			1957-1962	49
			1967-1972	34

**From papers presented at ADC/RTN Conference on Resource Allocation and Productivity in National and International Agricultural Research, Airlie House, Virginia, January 26-29, 1975.*

Table 3.2
Summary of Selected Sources-of-Growth-Type Studies of Agricultural Research Productivity

Study	Country (Commodity) Time Period	Annual Internal Rate of Return (%)
Griliches (1964)	U.S.A. (Aggregate) 1949-1959	35-40
Latimore (1964)	U.S.A. (Aggregate) 1949-1959	not significant
Evenson (1969)	U.S.A. (Aggregate) 1949-1959	47
Tang (1963)	Japan (Aggregate) 1880-1938	35
Ardito-Barletta (1970)	Mexico (Crops) 1943-1963	45-93
Peterson (1966)	U.S.A. (Poultry)	21
Evenson (1969)	South Africa (Sugarcane) 1945-1958	40
Evenson (1969)	Australia (Sugarcane) 1945-1958	50
Evenson (1969)	India (Sugarcane) 1945-1958	60
Jha & Evenson (1973)	India (Aggregate)	40
*Kahlon, Bal, Saxena & Jha (1975)	India (Aggregate)	63

[a]Returns to maize research only.

[b]Returns to maize research plus cultivation "package."

**From papers presented at ADC/RTN Conference on Resource Allocation and Productivity in National and International Agricultural Research, Airlie House, Virginia, January 26-29, 1975.*

Sources: The estimates that were presented at the Conference on Resource Allocation and Productivity in National and International Research (Airlie House, Virginia, January 1975) are identified by an (*). The other estimates have been summarized by James K. Boyce and Robert E. Evenson, *National and international Agricultural Research and Extension Programs*, (New York: The Agricultural Development Council, August 1975).

The sources of the individual estimates are as follows:

Ardito-Barleta, N., *Costs and Social Returns of Agricultural Research in Mexico*, Ph.D. Dissertation, University of Chicago, 1970.

Ayer, H., *The Costs, Returns and Effects of Agricultural Research in a Developing Country: The Case of Cotton Seed Research in Sao Paulo, Brazil*, Ph.D. Dissertation, Purdue University, 1970.

Evenson, R., *International Transmission of Technology in Sugarcane Production*, Mimeo, Yale University, 1969.

Evenson, R., *The Contribution of Agricultural Research and Extension to Agricultural Production*, Ph.D. Dissertation, University of Chicago, 1968.

Griliches, Z., "Research Costs and Social Returns: Hybrid Corn and Related Innovations," *Journal of Political Economy*, 66 (419-431), 1958.

Griliches, Z., "Research Expenditures, Education and the Aggregate Agricultural Production Function," *American Economic Review*, December 1974.

Hertford, R., Ardila, J., Roches, A., Trujillo, C., "Productivity of Agricultural Research in Colombia," ADC/RTN Conference on Resource Allocation and Productivity in National and International Agricultural Research, Airlie House, Virginia, January 26-29, 1975.

Hines, J., *The Utilization of Research for Development: Two Case Studies in Rural Modernization and Agriculture in Peru*, Ph.D. Dissertation, Princeton University, 1972.

Latimer, R., *Some Economic Aspects of Agricultural Research and Extension in the U.S.*, Ph.D. Dissertation, Purdue University, 1964.

Kahlon, A.S., Bal, H.K., Saxena, P.N., Jha, D., "Productivity of Agricultural Research in India," ADC/RTN Conference on Allocation and Productivity in National and International Agricultural Research, Airlie House, Virginia, January 26-29, 1975.

Peterson, W.L., *Returns to Poultry Research in the US*, Ph.D. Dissertation, University of Chicago, 1966.

Peterson, W.L., and Fitzharris, J.C., "Productivity of Agricultural Research in the United States," ADC/RTN Conference on Resource Allocation in National and International Agricultural Research, Airlie House, Virginia, January 26-29, 1975.

Tang, A., "Research and Education in Japanese Agricultural Development," *Economic Studies Quarterly*, 1963.

The question was raised several times regarding the value of even the most precise historical estimates of research productivity for research planning. Kahlon emphasized, in discussion, that parliamentary leaders in India are continuously pressing for evidence of the productivity of public investments, including investment in agricultural research. The productivity estimates do represent a useful device for monitoring research program performance.

The studies by themselves, however, provide only partial evidence with respect to the policy choices involved in research resource allocation. They do not explain the many unsuccessful research investments nor the variability in returns to research (for instance, the disparate results in the Columbian studies reported by Reed Hertford). They indicate little about the distribution of benefits of research among various groups in society. They do not explain the relationship of research investments at home to research done elsewhere. They do, however, serve as a point of departure to explore some of these issues. And the productivity measures do represent useful measures for monitoring the returns to the resources devoted to agricultural research.

The Demand for Research and Technical Change

The theory of induced innovation is the launching point for much of our understanding about the nature of the demand for technical change in agriculture. The study by Hayami and Ruttan* of technical change in agriculture in Japan and the United States indicated that both countries had similar agricultural growth rates despite radically different factor endowments. In Japan land was expensive and labor was cheap. In the United States labor was expensive and land was cheap.

The capacity of each country to introduce a series of technical innovations which utilized cheap factors while conserving expensive factors was a key source of productivity growth in their agriculture.

In both Japan and the United States factor endowments have provided the compass and much of the motive power for technical change in agriculture. Relative factor scarcities have been reflected in relative factor prices which in turn have induced a search for technical innovations to conserve scarce factors. In Japan this led largely to land saving biological innovations; in the U.S., this led toward labor saving mechanical technology.

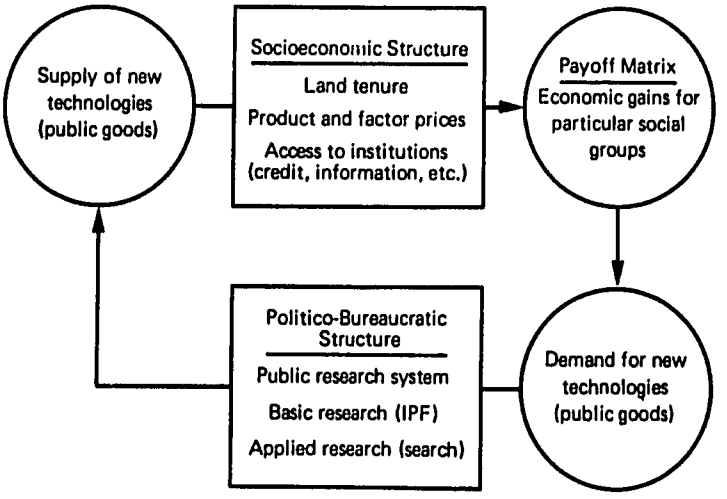
The effectiveness of the process by which technical progress is generated along a path induced by relative factor scarcities or by changes in factor-product price ratios is conditioned by many circumstances. These include the state of scientific knowledge, the capacity of industry to supply inputs and materials, the levels of technical and scientific skill embodied in people, market distortions, and the tugs and pulls of social/political circumstances.

In its simple form, the theory of induced innovation assumes that all technical innovations are equally possible. Binswanger, in a paper presented for the Conference, asked if technical change in some directions (e.g., labor saving technology) is not more easily produced than in others. If so, there may be "fundamental biases" in technical change which offset or neutralize the inducement mechanisms which pull toward conserving relatively scarce factors.

Binswanger's analysis indicates that the paths of land-labor intensity of the five countries which he has studied (United States, Japan, United Kingdom, France and Germany) are generally consistent with the induced-innovation theory. The countries with limited land endowments have followed much more labor intensive paths of agricultural development. However, fundamental biases do exist and have conditioned the direction of technical change. In the United States technical change has contributed to a continued increase in capital intensity well after increases in relative machinery prices should have induced a reduction in the rate of substitution of capital for labor. He concludes from this that it may take massive changes in relative factor prices to alter the direction of technical change in the absence of public policies designed to reinforce the inducements provided by relative price changes.

In an attempt to articulate the variety of forces which condition technical change, de Janvry presented the Conference with a conceptual model of the process involved in the inducement and diffusion of technical innovations. De Janvry views technical change as a circular, cumulative process rather than the essentially linear view implied by Hayami and Ruttan.

Figure 2
The inducement and Diffusion of Technological Innovation (based on Alain de Janvry, "The Organization and Productivity of National Research Systems" ADC/RTN Conference on Resource Allocation in National and International Agricultural Research, Airlie House, Virginia, January 26-29, 1975.



The key to the model is the pay-off matrix in the upper right hand corner. This consists of particular interest groups in society—commercial farmers, landed elites, subsistence farmers, consumers—who derive income gains or losses from alternative public goods such as research. The supply and demand for research is centered in the pay-off matrix and is conditioned by the socioeconomic structure on the one hand and the political-administrative structure on the other. Each social group pressures the political-administrative

* *Yujiro Hayami and Vernon W. Ruttan,* Agricultural Development: An International Perspective *(Baltimore: The Johns Hopkins Press, 1971)*

structure for research goods to be (or not be) generated depending on the particular pay-off that it expects.

The relative social power of different groups determines whether and how their demands get translated into the allocation of men and money for particular lines of research. The extent of basic scientific knowledge determines the area within which technical innovation is possible.

The resulting supply of research is filtered through the socio-economic structure and produces specific pay-offs for different social groups. In agricultural research, the pay-offs are determined by: (1) the physical characteristics of the innovation in terms of its ability to raise yield or reduce cost; (2) the extent of the diffusion of the innovation which is conditioned by its suitability to local ecologies and social and institutional arrangements such as land tenure, access to credit etc.; and (3) prices which determine its relative profitability. These pay-offs induce further demands for new research.

De Janvry's model is a useful intellectual articulation; it helps in interpreting some of the studies of agricultural research presented at the conference.

Hayami and Akino's study on Japan indicates how agricultural research can prosper where social and economic forces flow together to present a clear demand for technical change.

During the Meiji era when Japan was modernizing there was a unified pull for agricultural research from several groups: farmers were seeking land-augmenting technology, consumers were seeking lower food prices, industrial employers desired low priced wage goods to keep costs down and save foreign exchange, and government sought higher land tax revenues. The social and political structures in Meiji Japan, particularly the breakdown of feudalism and the high degree of social organization in the countryside, were uniquely conducive to the generation and diffusion of agricultural technology.

Similarly, Peterson and Fitzharris' study of U.S. agricultural research on how "better, wealthier farmers", abetted by a social structure which encouraged the organization of farm groups and a political structure which enabled them to press their demands on the body politic, created a highly productive federal-state research system.

In most countries, particularly the developing countries, effective clientele groups capable of serving as an "agricultural research constituency" have not emerged. The demand for technical change in agriculture remains latent.

Hertford's analysis of research in Colombia notes that concentration of rice, soybean and cotton growers in limited areas or in organized groups had a major impact in inducing research, and in affecting adoption of research results. The land tenure arrangements among wheat growers in Colombia, on the other hand, had a negative effect on the spread of new wheat technology and the subsequent demand for technical change.

Hertford's study also reveals the importance of prices in inducing or dampening research. Rice prices rose 82% over three years following imposition of import controls. This stimulated rice research. In wheat, the availability of PL 480 wheat dampened incentives for research for a number of years. In this vein, Schultz pointed to the persistent under-pricing of food grains by political authorities in developing countries as a force-majeure constraining demand for research and diffusion of its products.

The Conference's exploration of the demand side of technical change ended with a trail of question marks leading to unexplored territory. There has been limited empirical work in LDC's which indicates how the demand for new technology derives from particular groups and how the pay-offs are distributed. The studies on returns to research noted above are based on aggregate estimates of benefits. Research to identify how the new income streams generated by productivity growth are partitioned among laborers, tenants and landowners, and between producers and consumers would be an important step in understanding how research is induced, why it takes the direction it does, and whether it will serve broad development goals.

There has also been little work on how political/bureaucratic institutions create and condition demand for technical change and agricultural research. How is the political and economic position of those who gain (or lose) as a result of agricultural research reflected in research investment decisions? And there remains the persistent question: why, despite the evidence of high returns, does investment in agricultural research remain so low in many developing countries? Does the answer lie primarily on the demand side? Or does it lie primarily on the supply side, as some at the conference argued? If so, what is the nature, origin and direction of this demand?*

The Generation and Diffusion of Agricultural Technology

Recent econometric investigations have made some major inroads in understanding how advances in agricultural technology are made and diffused. Applied agricultural research may be understood as a search for new technology within the boundaries of existing scientific knowledge. Basic knowledge establishes the frontier within which innovation is possible. If basic knowledge is static, applied research is subject to diminishing returns and eventual exhaustion as the cost of successive technical innovations within the existing knowledge boundary rises. Without advances in basic knowledge, technical change will eventually stagnate as the marginal cost of innovations rises to meet marginal returns.

Advances in basic knowledge widen the horizon for applied research and make it more productive by providing new opportunities for technical innovations. Kislev states that the faster the advance of basic knowledge, the higher the productivity of applied research will be. The rate of technical progress thus reflects both (a) the rate of growth in the supply of new knowledge, resulting from investment in basic or supporting research, and (b) the rate of growth in the effective demand for technical change as reflected by investment in agricultural experiment station capacity.

Since basic knowledge does not expand continuously or

* *These questions are not confined to agricultural research. There has been low investment in research by LDC's in all fields. For one interpretation of this phenomen see the* Journal of Development Studies *(Special Issue on Science and Technology in Development), Vol. 9, No. 1, October 1972.*

smoothly, technical progress can be expected to move in cycles or spurts. A break-through typically leads to an initial rapid harvest of new innovations, followed by a slowing down of innovative activity.

Evenson documents the existence of such spurts in the successive break-throughs and expansion of improved sugarcane varieties throughout the world beginning in the late 1800's. In a similar vein, Hayami and Akino show that agricultural research in Meiji Japan, which was based on development of technology from existing knowledge, was slowing down until revitalized by a turn to more basic research in the 1920's.

Given this characteristic of technical progress, the introduction of "miracle" rice and wheat by IRRI and CIMMYT was not miraculous at all. Based on established scientific knowledge, IRRI and CIMMYT did some sophisticated genetic engineering and produced technology which was markedly superior to that previously in use in many areas of Latin America, Asia and Africa. This led to very large spurts in production.

The institutes filled a gap created by the delay on the part of the developing countries in taking advantage of technical opportunities which were available to them through previous advances in scientific knowledge. The primary reason for this failure, Evenson asserts, was the low level of research investment in LDC's in the 1940's and 1950's and hence their consequent incapacity to capitalize on this knowledge. This delay was particularly apparent in many former colonial countries where agricultural research capacity had been developed primarily to facilitate production of export commodities rather than domestically consumed food commodities.

If technical innovation is defined as filling the gap between the technology in practice and the technology which is possible given existing knowledge, then innovations are achieved by well trained scientists who know what is possible and who can design new technology to take advantage of it. This skill—Swanson terms it the skill of the "biological architect"—is what made CIMMYT and IRRI so successful.

Productive applied research in LDC's—Evenson argues—is strongly dependent on the availability of this type of high-order technical skill in these countries. The highly trained scientist has an understanding of science and the basic knowledge embodied in existing technology which he manipulates to create superior technology for the production conditions in his country. Evenson demonstrates that the availability of high order research skills represents an important source of agricultural productivity growth in LDC's.

Without these high order skills, Evenson asserts, LDC's tend to engage in relatively unproductive low level research, which often replicates work done elsewhere. Typically there are a number of small research stations each performing limited (and often duplicative) experiments based on accepted textbook principles. As skill levels increase, there is a tendency for LDC research systems to pull together around a lead research institute in order to take advantage of the higher-order and more productive skills developing there. The higher the skill levels, the more capable the research station is of generating new technology. At the highest level, it is capable of basic research to produce scientific knowledge.

Availability of research skills in developing countries also has important effects on the diffusion of technology discovered elsewhere. The diffusion of agricultural technology is circumscribed by geo-climatic conditions. A breakthrough in one area may directly diffuse to other areas characterized by similar environment and economic conditions. This is direct diffusion of technology.

Skilled adaptive research magnifies the area in which the discovery is applicable. Adaptive research, as noted above, is based not only on diffusion of the technology but on diffusion of the knowledge embodied in the technology. It is reformulated by local scientists to produce superior technology for local conditions. Ultimately, however, the extent of technology diffusion through adaptive research is also constrained by geo-climatic conditions.

Evenson's studies of the international diffusion of sugarcane varieties and other commodities shows both direct and adaptive diffusion processes at work. He demonstrates that the rate of both processes depends on the availability and quality of indigenous research capabilities.

The availability of sugarcane research capacity in various countries had, in the first instance, a simple extension benefit. It speeded up the importation, testing, and release of sugarcane varieties generated elsewhere. Evenson showed that in Australia, South Africa, and the Caribbean area this speed-up effect alone justified the countries' investment in research, notwithstanding any adaptive research produced by the countries subsequently.

Hertford's studies of cotton research in Colombia showed a similar result. Initiation of cotton research there facilitated the importing and testing of U.S. cotton varieties which yielded high returns even though the Colombian research itself did not produce varieties which were superior to U.S. varieties. This is also the role that is often very efficiently performed by private sector research and development, as described by Sehgal.

Subsequent adaptive research enabled countries to take further advantage of the initially imported varieties. Evenson shows internal rates of return to adaptive research on sugarcane of around 40% in South Africa, 50% in Australia, and 60% in India. Kislev observes that the improved wheat varieties in Israel, drawing on Mexican materials, went through three stages: (1) direct transfer of the improved Mexican varieties to farmers; (2) selection from among the Mexican materials (Mexican Norin 10 dwarf types) and (3) development of improved varieties which incorporated both local and Mexican materials. Only the varieties that emerged in stage three were significantly superior to the local materials previously used.

Evenson concludes that countries without the capacity to do internationally significant agricultural research also lack the capacity to benefit from the research of others in similar geo-climatic zones. Evenson's estimates of the magnitude of these spillover effects are shown in Table 4. His conclusions buttress the argument for investment in agricultural research in developing countries. They imply (but do

not yet prove) that developing countries will need to emphasize not only development of the capacity for adaptive or applied research but also the development of high-order conceptual-scientific skills if they are to take full advantage of the potential contribution of agricultural science to national development. In the future it will primarily be people with high level conceptual skills to break new ground and lead effective national research programs.

The Productivity and Potential of the International Agricultural Research Institutes

During the early 1970s the international agricultural research institutes constituted about a tenth of a percent of world expenditure for agricultural research. Even in the developing world they account for only about five per cent of agricultural research expenditure. Yet, their impact has been very large. As indicated earlier, Dalrymple estimates that the technology packages derived from the work of the institutes added $1.0 billion in wheat and rice production in Asia alone in 1972-1973. These technology packages were the joint products of research at the institutes and of original and adaptive research within the LDC's.

In addition, the institutes have had substantial indirect consequences, which are not measurable. Wortman points out that they have demonstrated the potential of science-based agriculture and stimulated investment in agricultural research in LDC's. They have instituted a trend toward problem-oriented, commodity-focused, multi-disciplinary research in a number of countries.

However, it is important to place the success of the institutes in perspective. The lag in investment in applied agricultural research in the LDC's during the 50's and early 60's provided a ready opportunity for the international centers to convert existing scientific knowledge into technologies which were superior to those in use in the tropics. Furthermore, both CIMMYT and IRRI adopted a research strategy designed to develop grain varieties which were usable under relatively wide environmental conditions.

Progress has slowed as the new varieties spread onto less-favored lands. The productivity of the international institutes remains high. But it will be difficult to maintain the rate of return that was achieved from the initial investment. Evenson estimates that the "second generation" returns to wheat and rice research, though exceedingly high by conventional standards, has fallen below the "first generation" levels (Table 5).

Progress also has not been uniform. The return on investment in corn research at the international centers (and predecessor institutions) has been realized more slowly than wheat and rice, although private research on corn has been relatively profitable. Except for IRRI and CIMMYT, the other nine institutions are in their early stages and have yet to make major contributions. This does not mean that the international centers will become just another type of research unit in the near future. They are at present uniquely structured for effective action. They have independent boards of trustees, organizational discipline, well established pipelines to the aid resources of donor countries and an ability to recruit skilled staff from all over the world.

Table 4.0
Estimated Marginal Benefit Streams Associated with National Research Investment. (Estimated levels (in 1973 $US) to which benefit streams associated with a one-thousand dollar research investment will rise eight or ten years after the initial investment.)

	Research Investment in Developed Countries		Research Investment in Developing Countries	
	Technology Oriented	Science Oriented	Technology Oriented	Science Oriented
Part 1: Appropriated by investing country				
a) direct contribution	630	12,300	3,710	35,600
b) through complementarity with research in other countries	1,620	1,620	7,200	7,200
c) total appropriable	2,250	13,920	10,910	42,800
Part 2: Contributed to countries other than the investing country	5,150	17,000	49,000	37,300
Total benefit stream realized from an international perspective (Part 1 + Part 2)	7,400	30,920	59,910	80,100
Part 3: Benefits realized by a typical country from research investment by other countries in similar climate zones (or regions)				
(a) with average indigenous research capability	8,580	520	55,000	1,700
(b) with no indigenous research capability	4,560	520	1,700	1,700

Source: Robert E. Evenson, "Comparative Evidence on Returns to Investment in National and International Agricultural Research" ADC/RTN Conference on Resource Allocation and Productivity in National and International Agricultural Research, Airlie House, Virginia, January 26-29, 1975.

Table 5.0
Income Stream and Cost Calculation for International Center Research

	First generation varieties		Second generation varieties	
	1965-6 to 1969-70		1970-1 to 1973-73	
Item	Wheat	Rice	Wheat	Rice
Annual Increment to income stream (1973 million$)	135	270	56	360
Associated cost on an annual basis* (1973 million$)	.6	1.0	1.2	2.8
Income stream per $1,000 investment	$225,000	$270,000	$46,666	$128,500

**Computed from Dalrymple (1975), Table 1. The second-generation costs are based on annual budgets for 1966-68. First-generation costs are all prior costs at IRRI (capital expenditures are amortized) and a capital adjustment is made for CIMMYT costs to make them roughly comparable with IRRI.*

Source: Robert E. Evenson, "Comparative Evidence on Returns to Investment in National and International Agricultural Research, ADC/RTN Conference Resource Allocation and Productivity in National and International Agricultural Research, Airlie House, Virginia, January 26-29, 1975.

More importantly, the institutes have carved out positions as centerpieces in the constellation of research institutions working on improved agricultural technology world-wide. They have access to scholarly capital and technical raw material from around the world. They have established communication links with the national research centers and production programs with responsibility for work on the committees on which the centers are conducting research.

The extent of the centers' relationships with developing countries is indicated by the following array of IRRI's activities:

(1) Germ plasm collection: 33,000 samples collected at IRRI, 16,000 samples sent to LDC scientists in 1973;

(2) Publication Workshops: Maintains bibliography of world rice literature; eight major symposiums held since 1966;

(3) Training: 90 man-years of training for LDC rice scientists and educators provided in 1973;

(4) Collaborative Research: Research with LDC's on location-specific problems such as deep water rice; tolerance to toxic soils.

(5) Research Networks: Formal research network with LDC's on evaluation and testing of genetic material cropping systems with rice, agro-economics, etc.

(6) Direct Assistance to LDC Research Systems: 12 country projects since 1966.

(7) Assistance to Country Rice Production Programs: Planning expansion of pilot rice production program developed in Philippines to other Asian countries.

CIMMYT conducted collaborative wheat trials at 1,140 sites in 66 countries in 1973. Through this procedure, CIMMYT and its collaborators are able to test wheat varieties under widely different conditions of day length, temperature, moisture, diseases, and insects: They are able to obtain yield data which can guide future experiments both at CIMMYT and at cooperating research stations. They obtain germ plasm from around the world for future trials and breeding. It is estimated that it would take any one national experiment station fifty years of repeated trials to obtain data as comprehensive as that for one year of trials organized through this international network.

The institutes have used the tools of organization to achieve significant multiplier effects from limited resources. There are only 13 wheat scientists at CIMMYT itself.

It is this institutional innovation which may in the long run be judged the most significant real achievement of the institutes and ensures their continuing relevance for the future.

The established institutes are now vigorous adolescents. What adulthood is expected? One rare path to adulthood is to plan an adult role based on a careful assessment of one's strengths and limitations. The other is to just grow, responding to present circumstances as seems most appropriate and trusting in one's innate strength to bring one out all right in the end. Mosher sees the institutes as tending to follow the latter path. He asked the conference to consider the correctness of this.

The trend is for the institutes to evolve from research and training centers into research-based institutes of agricultural development. To their core research and training programs, they have increasingly added commitments to strengthen national research capabilities of LDC's. They are tentatively involving themselves in production programs in LDC's.

The reasons for these developments are first, that the institutes are conditioned to judge the success of their programs by actual increases in commodity production in LDC's. This has given them a practical orientation and a sharp sense of purpose. But as the spread of high yielding varieties has slowed and as evidence mounts that farmers are not adopting the whole package of practices, the tendency for IRRI and CIMMYT to be concerned about LDC production programs has increased.

In addition, the quality of resources available at the institutes has led to increasing demands from LDC's and aid agencies for the institutes to take on variegated responsibilities. In some cases they are attributed credibility in areas where they have not yet achieved substantial research capacity.

Mosher asked if these developments will dilute the centers' ability to apply high-level conceptual-scientific skills to research problems. Should not the institutes sharply limit activities other than research and training?

On the other hand, the involvement of the institutes in strengthening LDC research and production programs is consistent with the evidence on international diffusion of technology. If lack of capacity for indigenous research implies lack of capacity to benefit from international research, then the institutes, the developing countries, and AID donors can expect high returns in both the short and long term from the centers' outreach efforts.

The important question, as Mosher points out, is how the centers will define their role in relation to other actors who are now on stage or in the wings. These include not only LDC governments and traditional aid agencies but also representatives of the private sector, whose role in production programs and adaptive research may well expand. It includes the potential for regional institutes, such as the West African Rice Development Association (WARDA) and the South East Asia Regional Center for Graduate Study and Research in Agriculture (SEARCA) which are funded by the developing countries themselves and by aid consortia.

While these questions were posed, they were not closely examined by the conference. The representatives from the institutes did not see their involvement with outreach programs as diluting their effectiveness. On the contrary, the institutes have yet to come up against budget constraints which would require hard choices. The consensus was that the institutes probably would and should continue to evolve as research-based agricultural development institutes. However, some participants did question whether acceptance of a broader charge might not weaken the capacity of the institutes to contribute in the area where their advantage is greatest relative to national institutions—the design of efficient technologies capable of releasing many of the technical and institutional constraints in production.

The Organization and Management of Agricultural Research

The United States and Japanese agricultural research systems have been more thoroughly studied than others. The Hayami/Akino and Peterson/Fitzharris papers indicate that each system successfully responded to the needs of farmers. Each shared certain attributes. Both Japan and the U.S. evolved decentralized Federal/state systems. The state units (prefectures, in Japan) were able to respond flexibly to changing local circumstances and to develop locally appropriate technologies for even micro-environments.

In each country, the state units were backed up by national research systems. In 1926, Japan and the U.S. independently reinforced their federal/state systems by introducing centrally orchestrated, nation-wide research programs on specific crops and problems. These "coordinated" research programs coincided with a trend toward more basic research in both countries. They were effective in mobilizing scientific talent around specific programs without sacrificing the responsiveness to local problems which characterized the state or prefectural units.

The United States formally integrated research, extension, and education in the land grant colleges. Japan did not. But relatively close liaison between the three levels was maintained by less formalized arrangements. In both countries, there were well organized groups of farmers, relatively equitable land distribution, high levels of education, growing industries, progressive governments, and a social/political structure which favored communication with farm groups*.

Whether the Japan and U.S. experiences provide models for the LDC's is an open question.

The Pastore/Alves paper on reform of Brazilian agricultural research argues that the model is not applicable to the extent it implies relatively antonomous, multi-purpose, locally responsible institutes such as U.S. land grant colleges. The social and economic circumstances, such as farmer organization, which ensured that these institutes responded to needs of farmers are not present in most developing countries.

Pastore and Alves assert, in effect, that autonomous institutes work best in cohesive social structures. They imply that the structures which characterize rural areas in developing countries favor a centrally directed, aggressive research system. "Directed" means that the system has a central planning unit which coordinates the activities of various units. "Aggressive" means that it systematically seeks information about the farm sector through social research or other means, that it orients itself around explicit development and production goals, and that it gears its research program to these ends.

Brazil is moving toward greater centralized direction and coordination of its national and state research systems. Wortman argues that research should be organized on a multidisciplinary, commodity basis to achieve production targets established by governments. Moseman urges governments to take hold of their typically scattered research units and institute centrally coordinated national programs around specific crops or problems.

Moseman's appeal—in a sense—is the reverse side of Evenson's paper on productivity of national and international research. Evenson, from an analytic standpoint, notes that decentralized small-scale research stations are characteristic of countries with low skill levels. As skill levels rise, concentration occurs in order to take advantage of the economies of scale and higher productivity which consolidating high order skills can achieve. Moseman, from a practitioner's standpoint, urges governments to begin the process of consolidation which will be needed to fructify the high level skills which the country presumably will be training.

The stress on coordinated or directed national research programs in a sense alters the traditional debate over what type of research institute works best in an LDC (single crop vs. multi-crop; land grant colleges or government research institutes). It shifts the focus to developing a national system. It implies that many types of institutions can be productive if integrated (coordinated) effectively around specified national/regional research goals. But can a research system continue to be productive if its direction continues to come primarily from the center? It was pointed out in discussion, by Kahlon, that one effect of a productive agricultural research program (as in the Indian Punjab) is to create a research constituency.

The discussion of central coordination and planning for research systems touched off some lively debate. Nelson raised the question of whether formal criteria should be used in research planning and project selections. He asked what these criteria should be and how the weights can be derived. Nelson reported that the Rand Corporation studies of the economics of research and development had shown that cost projections on research and development projects were typically highly inaccurate. Studies by Mansfield of industrial research indicated substantial error in predictions of time to completion and project output.

Ulbricht reported that at a recent OECD meeting on the relationship between agricultural research and socio-economic policy there was a general consensus that attempts to develop weighting criteria resulted in spurious precision. He argued that subjective judgement was inevitable but could be refined by systematic analysis combining technical and economic information. Schultz argued strongly that research is an entrepreneurial activity, whose success depends on relatively rare personal qualities of creativity and insight. The organizational task for research is to create structures where talented individuals (and teams of individuals) can exercise their creativity. He pointed out that the market was an effective transmitter of information about technological needs and that researchers usually read these signals pretty well. Nelson asked, do you want to bet on proposals or on people?

The counter argument was that coordination and planning may be potentially stultifying but need not be. They can enhance the effectiveness of individuals and organizations by clarifying goals, increasing the flow of

* *It should be noted that in regions where these conditions did not prevail, as in the United States "Old South", agricultural research, agricultural productivity, and rural development lagged.*

information, and promoting team work. In any case, the potential problems of over-centralization hardly apply to developing countries where the problem is that research units are few and typically diffuse. Since many LDC's have fewer qualified research scientists than a single major U.S. experiment station, the question of competition among multiple centers does not arise except in larger countries. It was also argued that the entrepreneurial concept of research reflects Western, intellectual traditions which do not operate well in transitional societies where economic signals may be distorted and where particularism and communalism are still strongly held social values. There may be a tendency for scientists in developing countries—left to respond to the market—to direct their entrepreneurial talents toward the international scientific market from which rewards and emoluments flow. Or they may respond to highly limited demands such as from large land owners.

The discussion of this issue was characterized by Fishel as excessively ideological. He insisted that it is possible to quantify and communicate events that lie in the undiscovered future, based on the experience of the past. Ideally the resulting measures should not be neat point estimates, but probability functions which incorporate all the information about the future including the uncertainty involved. Schultz insisted that the analytical capacity is not yet available to respond to Fishel's challenge.

Bernstein stressed that the discussion was hampered by a failure to differentiate between the control and rationalizing dimensions of research management. He pointed out, in discussion, that systems for rationalizing the use of scarce and potentially high-yielding research resources can operate with very small amounts of control built into the system.

The discussion did underline the need for better understanding of the origin, nature, and direction of demand for agricultural research in developing countries. Such understanding would permit consideration of whether to stimulate research productivity by altering the market rather than by striving for more comprehensive systems of planning.

Better understanding of demand would also help resolve one of the traditional questions about research institutes in LDC's. That is, what sort of organization might be established to provide the feed-back to central research units from regional research stations in order to achieve the interplay between national and local problem solving that was so evidently productive in the U.S. and Japanese systems?

The question marks arise around the local units—the sensors at the end of the system. The Pastore/Alves view implies that such units will not relate to local farm conditions well in the absence of the coherent socio-economic circumstances which nourished local research organizations in Meiji Japan and the U.S. Making them effective would require the thorough reform of the rural sector. Many rural development specialists argue that almost no rural development institution, including agricultural research and extension programs, can be effective at the local level in the absence of a reasonable degree of equity in the distribution of resources in rural communities.

The pertinent concern, for our purpose, is with the productivity of local research units. As noted above, Evenson argues that location-specific research is related to the level of skill of a country's research establishment. Low skill levels imply numerous small-scale research stations doing location-specific adaptive research based on cookbook principles. As the country develops, concentrations of highly-trained scientists with access to international networks and wide scientific knowledge are able to produce markedly superior technology, which is suitable for more diverse ecological conditions. Branch stations attenuate and serve as locations for testing and evaluation.

The validity of Evenson's point has been demonstrated in part by the international centers. To an extent, they are reversing traditional plant breeding procedures which are devoted to development of highly location-specific varieties. They have applied their scientific skills to developing varieties which are tolerant to wider variations in day length, disease and other conditions.

Schuh notes that the extent to which central, high quality research can supersede branch stations is tempered by the state of scientific knowledge as well as the supply of skills available. For instance, scientists have to conduct local tests for such things as the response of plant varieties to fertilizer because biological science does not yet have a unifying theory of how plants respond to fertilizer under different soil and climate conditions.

The attempt to derive general principles for the organization of LDC research can not be pushed too far because of the incompleteness of theory and empirical knowledge. In the judgment of many research managers, increasing the effectiveness of local or regional research stations by incorporating them into national research programs directed toward specified national/regional research goals should be placed toward the top of the agricultural development policy agenda.

Improving Research Decision-Making

Optimizing research decisions is difficult and important.

First of all, research resources in developing countries are scarce. Well trained scientists are few, and the organizational competence necessary to facilitate research is in even shorter supply. Because research is potentially very productive, the opportunity costs of bad decisions are high.

Secondly, different kinds of technical change affect a nation's economic and social goals unequally. Monotheism in development planning—with a single focus on growth—has been superseded by pantheism in recent years as other social goals have moved into the sanctuary. There are economic growth goals such as increasing the net income of the agricultural sector, or maximizing the contribution of agriculture to the economy as a whole; there are welfare goals such as increasing employment and the income of labor employed in agriculture, reducing the real price of food for consumers, and improving health and nutrition; and there are equity goals such as mitigating income inequalities and opening the benefits of growth to particular groups, such as small farmers. The choice of crops, regions or disciplines for research affects these goals unequally.

Choice is further complicated because the production process and outcomes of research are uncertain. This is true in two senses: a) The time and resources required to attain stated technical objectives can only be estimated. The risks of misestimation are high. b) The appropriation of benefits, once technical objectives have been attained, is quite uncertain. In agriculture, the decision to adopt new technology flowing from research is in the hands of many farmers, each of whom maximizes his private welfare and acts in accordance with his own assessment of the risks involved in new technology.

Research may shift production functions but many farmers will tend to operate at less than what appears to be the optimum level of production. How much below and for how long is imponderable. It depends on extension, input supplies, access to credit, economic policies and other circumstances. It also depends on the accuracy with which the research planners have judged the private welfare optima of the farmer. Agricultural commodities are produced by farmers—not by planning commissions, research scientists, or extension workers.

Finally, the market place is an imperfect decision-making guide for research. Society places a value on research, but the market place may not reflect this value accurately. The benefits of the research cannot ordinarily be fully captured by the individual or firm which bears the cost of producing it. This is also true of much agricultural technology produced by research (e.g., new seeds, or cultural practices). Hence, private profit is often an inadequate incentive for research particularly in industries, such as agriculture, which are characterized by small producing units. The market undercompensates private innovation in agricultural research. Research decision-making receives indirect guides from the market place through factor and product prices, but little direct guidance.

These uncertainties evoked two types of responses from the conference—neither of them mutually exclusive.

The first was a consensus that there are severe limits on the ability to make quantitative objective assessments of the value of particular kinds of research. This implies that the choice among alternative lines of research must continue to incorporate large areas of subjective judgment. The objective criteria available to guide research decision-makers through the uncertainty surrounding research decisions are limited. Use of conventional tools such as cost benefit analysis is limited by the precision that the research scientist can bring to bear in estimating both the resource requirements and the output of a research project or program.

At the research project and program levels, the judgment involved is essentially that of the scientist. At higher levels, the judgments are partly scientific and partly political. There was agreement that the high rates of return from past research implies that the subjective judgment of knowledgeable scientists and science administrators should receive high marks. Given the right institutional and social setting, including efficient markets for inputs and commodities, scientists' judgments of technical constraints and opportunities for increasing production have led to effective research resource allocation decisions. Nonetheless, the second response was that the tools of social science can and should be developed as guides to decision-making. These tools become more significant in countries characterized by the absence of efficient input and commodity markets.

On the micro-level, Pinstrup-Anderson described a methodology being developed at CIAT for determining research priorities within a single commodity. The method proceeds through logical stages: 1) attempt to identify reasons for low productivity; 2) identify researchable problems which are expected to improve productivity; 3) estimate the impact of solving each of the problems on production; 4) estimate the probability of research success, likelihood of adoption of results, and the time required; and 5) estimate the impact of alternative research results on product supply, input demand, farm income, and farm size.

Data are collected by a four-man team of agronomists and economists who visit a sample of farms throughout a complete crop cycle.

CIAT is also developing a method of systems analysis for small farms. The method attempts to describe the motivating forces and constraints on small farmers in light of the biological, ecological, and institutional environments facing them. Once these relationships are understood, it is possible to use the model to simulate the likely results of alternative technical packages which might be developed through research.

A test of the system in Guatemala indicated high probable returns to the use of herbicides. This finding has led the Guatemala Research Agency to initiate research on herbicides.

The type of method developed by CIAT and other methods reviewed by Shumway provide research managers with vastly more information than do traditional informal methods. The methods do entail costs both in terms of time and trained people. Whether their marginal advantage over informal methods justifies their cost is not yet certain. CIAT reports interest in their systems among several research agencies in Latin America. This is perhaps one indication of their potential value.

Attempts to introduce social goals, in addition to explicitly economic considerations, into agricultural research planning is relatively new. DeCastro and Schuh used data from Brazil to demonstrate a preliminary model for assessing national research priorities in light of a country's factor endowments and socio-economic goals.

The model adopts the Hayami and Ruttan thesis that the task of research in promoting economically efficient growth is to introduce technical change which conserves relatively scarce factors of production. Using trends in relative factor prices in Brazil, DeCastro and Schuh show that even in land-rich Brazil, research efficiency implied greater emphasis on land-augmenting technology. This includes, for instance, soil research to open problem lands to production or biological research to improve yields. The authors note that there is great regional variation within Brazil, however. In some parts of the country, a labor constraint is emerging which calls for a different technical choice.

Knowledge of what research is consistent with relative factor scarcities, still leaves the question of how to allocate

research resources among commodities. In considering this issue, DeCastro and Schuh pose the question in terms of whether a nation wishes to favor the welfare of consumers or producers. Whether the benefit of technical change in particular commodities redounds to consumers or producers hinges primarily on the relationship between the demand and supply elasticities for the commodity. Crops with low relative demand elasticities (e.g., food grains, beans, manioc) distribute their benefits primarily to consumers in the form of lower food prices. Lowering the price of food grain also releases wages—good constraints and permits expansion of employment programs. However, as prices fall acreage may shift to other crops. Even labor intensive technical change in crops with low demand elasticities can be labor-displacing by releasing labor from that crop to other crops or to the non-farm sector. Crops with high relative demand elasticities (e.g., cotton, sugarcane, export crops or other crops where the producing region is a price taker) return most of the benefits of technical change to producers and thus can stimulate demand for labor and increase rural incomes.

DeCastro and Schuh state that research planners are faced with the task of striking a balance between the following trade-off. If their goal is to increase income and employment of agricultural labor, emphasis in research should be given to crops with high relative price elasticities of demand. If the goal is to increase consumer welfare, the research emphasis would be given to food grains and other crops with relatively low demand elasticities.

Mellor elaborated on these trade-offs in an analysis of research allocation and social goals based on Indian data. He argues for a sequence of agricultural policies to which research should be tied. First, emphasize increasing yields of food grains in productive areas of the country. This increases the supply of calories which conditions the health of the poor and increases grain supplies thus relaxing the wages good constraint to increased employment. Relaxation of the wages good constraint should be followed by employment programs to keep demand up. Otherwise, the incentive for increased production may diminish with falling prices.

As the wages good constraint is released, the second-stage strategy should be to promote food grain production in less productive regions and expand production of labor intensive crops. These are usually those with high relative demand elasticities. Promotion of labor intensive crops needs to be complemented with policies to encourage demand for them either through expansion of exports, raising domestic incomes, or subsidies.

The Mellor, DeCastro-Schuh papers and related discussion at the Conference illustrate several points about relating research resource allocation to social goals.

First, the models clearly indicate that the contribution of research and technical change to society's goals is dependent on other policies. Economic policies, such as the systematic reduction of product prices, can weaken the ability of research to contribute to growth or welfare goals. Distortion of input and product prices not only affects farmers decisions regarding use of inputs and choice of commodities to produce. It also affects decisions regarding research priorities and hence the new technologies that will become available in the future.

By the same token, many social goals may be achieved more effectively through policies other than research. In some areas, for example, land reform may be necessary to achieve widespread participation in the gains from productivity growth. There was general agreement with Mellor's point that biological research represents an inefficient instrument for the solution of problems of rural income distribution. Over-reliance on it may be self-defeating both in generating improved technology and in achieving the desired goals. Yet, as Ulbricht insisted, the design of agricultural research strategies should not ignore the potential income distribution effects of technical change. Crawford noted that it should be the policy of international institutes to make a range of technical options available and not bias the technical innovation in a capital intensive direction. It was also pointed out that it may also be important to make available technologies which are less "management intensive."

The economic models of the type presented at the conference have a great deal of difficulty in accommodating multiple social goals. The definitions of welfare used in the models discussed above are relatively simple. A country's actual welfare is more complex. The fact is that development planning has only recently moved "beyond growth." It is still in an early stage of specifying what the various social goals of developing countries might be, yet alone understanding the relationships among them. However, clear articulation of goals is critical for making research choices. Consideration of research priorities must proceed from an understanding of what a country or region's goals are.

A second problem with such models is that, because of uncertainties about the production process for research, they are forced to make heroic assumptions about returns to research. Marginal returns to certain lines of research may be increasing while others may be declining. This would significantly affect the flow of benefits and condition research choices but is difficult to specify in advance. In addition, there are the difficulties in knowing research cost functions and the distribution of benefits flowing from research mentioned earlier.

These uncertainties are stubborn barriers to developing better tools for predicting the consequences of research choices on social welfare. Economic analysis at present only yields gross indications of the multiple consequences that are implicit in alternative research resource allocation decisions. More data on the appropriation of research benefits and on the research cost function as well as further theoretical development and empirical testing of models are needed to improve decision-making tools.

Concluding Notes: Areas for Research

Viewing the Conference in retrospect raises the proverbial question of whether to measure how far one has come or how far one has to go. From the latter perspective, it is

evident that we remain some distance from a full understanding of agricultural research and a fully convincing theory of technical change. The capacity of scientists to generate new technology outstrips our understanding of social and economic implications of technical change and our ability to provide guidance for policy makers.

Some areas in which further research is warranted are:

(1) *There is a general need for a more precise understanding of the sources of demand for technical change in agriculture.* This includes further specification of the natural and institutional biases which condition processes by which technical change is induced. The need for more careful analysis of the incidence of benefits from technical change is particularly important for understanding more about the origin and nature of demand for agricultural technology and for the further development of allocative tools. Study of research "failures", such as the relative lack of pay-off to date from wheat research in Colombia, might also be instructive. There is also a need to study how the political/bureaucratic process impinges on research allocation and conditions the demand for technology.

(2) *More analysis of research cost functions and the production process for research is needed.* One area for further inquiry is Evenson's hypothesis about the relative productivity of various levels of research skill in conjunction with different levels of research organization. The effort thus far has had to rely on a relatively weak data base. Although the results are consistent with economic principles regarding the productivity of scarce resources, they do counter some popular views about the relevance of simple adaptive research and low-level skills to LDC's.

(3) *There is need for further understanding of national and international diffusion of agricultural technology and scientific knowledge.* The relationship between different types of technology-based and/or science-based research investments within a country or region to investments outside it needs to be examined more carefully. How does a country develop the capacity to take maximum advantage of research investments made elsewhere?

(4) *There is also need for research on elements of the technical change process which were not well covered at the conference.* This includes the relationship between technology policy and economic policy, particularly price policies. It also includes the relationship between formal schooling (or literacy) and rates of technical change. Only a beginning has been made in quantifying the relationships between investment in research and investment in extension.

Despite these uncertainties, when one looks back at the distance travelled, it is clear that some big steps have been taken.

There have been strides forward in general understanding of how technical change is induced, in modelling the discovery process, and in mapping international diffusion processes for technology and scientific knowledge.

There is solid evidence that investment in national and international research has been highly productive. The social returns to agricultural research have been high relative to the alternative investments available to most poor countries. It is clear that investment in agricultural research in developing countries by both national and international agencies should expand.

The Airlie House conference was something of a milestone in effective collaboration between natural and social scientists in analyzing the sources and consequences of technical change in agriculture. This collaboration has been evolving hesitantly in recent years. There is reason for confidence that it will continue and yield in the future better understanding of the central role of research and technical change in agricultural development.

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