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Evaluating Agricultural Research Institutions in Developing Countries

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I. Introduction

Historical Background

Agricultural research, as an organized, state-sponsored activity, had its beginnings with the development of botanical gardens in the seventeenth century (Brockway, 1979). These gardens were established by the colonial powers to select desirable plant materials and to transfer them to those colonies where they might be successfully introduced. For example, as a result of the work of Kew Gardens in Britain, rubber was introduced into southeast Asia, tea to Ceylon (now Sri Lanka), and Cinchona (quinine) to India.

As a result of Liebig's pioneering work on plant nutrition in the 1840s, agricultural research moved from the trial and error approach of the gardens to the experimental phase. Starting in the 1850s, the various German states began to develop experiment stations. France soon followed the German lead while the British preferred to expand the role of the already extant botanical gardens. During the ~~first~~ last quarter of the century, the United States, Russia, and Japan each established their own system of experiment stations. By 1900, there were over 800 agricultural experiment stations to be found around the world. By 1930, the number had increased to over 1400 (Busch and Sachs, 1981).

Given the needs of the European powers and Japan, it is not surprising, that the stations located in Europe and Japan tended to focus on food crops while those in the colonies emphasized export crops that would provide exotic foods (e.g., bananas, sugar, coconuts), exotic drinks (e.g., coffee, tea), or raw materials for european industries (e.g., cotton, palm nuts, groundnuts, rubber). Only after the end of World War II, and especially since independence of the former colonies, has food crop research become a significant part of the research agendas of most third world nations. Even

today the colonial legacy continues to distort research priorities in much of the Third World. Indeed, Janzen could observe as late as 1975 that "it is repeatedly stated that tropical staples are ignored in research programs, while export crops are studied extensively" (1975:107).

The so-called Green Revolution of the 1960s signaled the first successes in increasing yields of the newly emergent system of international agricultural research centers. Of particular importance is that its successes were accomplished by (1) drawing upon a substantial worldwide research literature for the crops involved (wheat, rice), (2) the heavy dependence upon both irrigation and chemical fertilizers, and (3) the development of significant national research competence in the countries affected that complemented the work of the international centers. Though expenditures for international research rose markedly through the late 1970s, and many new centers were established, no breakthroughs similar to those of the 1960s have occured. This has been due in part to (1) the lack of a research tradition of comparable size for the crops of concern to the new centers,* (2) the lack of or infeasibility of irrigation for these crops, and (3) the weakness of national research systems, particularly in Africa. Thus, the "easy" problems of research have been solved; those that lie ahead present even greater challenges.

The Land-grant model in the Third World

U.S. assistance to other nations wishing to create or improve their research institutions is not a recent phenomenon. While there was no U.S. presence in former European colonies until their independence, Land-grant

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*Busch and Sachs (1981) note that in 1977 over 3000 articles in scientific journals were published relating to wheat, while only 500 articles related to sorghum and 70 relating to millet were published.

universities were active in the independent nations of Asia and Latin America as early as the turn of the century. Dean Harry L. Russell, of the University of Wisconsin, was able to present an address to a Japanese audience of agricultural scientists in 1925. The address required no translation for those in the audience had all received part of their higher education in the United States. Moreover, Russell noted that the campus itself physically resembled those of American Land-grant universities, even to the elm trees and quadrangles (Russell, 1927)!

After the nations of Asia and Africa received their independence, American aid in establishing agricultural universities became an important, if not central feature of USAID and its forerunner agencies. It is worth noting briefly, and in somewhat stylized form, the stages that this aid took:

1. Early aid consisted in an extension of the Marshall Plan. Given the success of the Marshall Plan in reviving post-war Europe, it appeared reasonable to attempt to provide similar forms of technical assistance to the nations of the Third World. Thus, the emphasis was upon the transfer of already existing American technology. No one seemed to notice that the skilled labor and administrative personnel necessary to carry out this transfer task already existed in Europe but was largely lacking in the Third World. Equally unnoticed was the inappropriateness of direct transfer of agricultural products and practices from the U.S. to the Third World. The limits of this approach were rapidly reached, leading to the second stage.

2. It was argued that without effective extension services exhorting farmers to adopt new practices and innovations and teaching them how to use them, American technology never reached its intended audience. At this time, many studies of the diffusion of agricultural innovations were launched, some of them eventually suggesting that what was being diffused was ineffective under third world conditions. The emphasis on diffusion coincided with the

entry of social scientists into the international agricultural development arena, especially the entry of agricultural economists and rural sociologists. Three major issues dominated the rural social science agenda: (1)improving farm management, (2) better marketing information, and (3) improving the diffusion of agricultural innovations. Of particular note was that these social scientists largely accepted the products developed by technical scientists as undiluted goods. Indeed, they shared the view held by many of their colleagues in technical fields that scientific knowledge was always superior to tradition. Moreover, as a result of the peculiar politics of American agriculture, and especially the hegemony of the farm bloc, studies critical of the research process, or of agricultural structure, were avoided (Friedland, 1979; Kirkendall, 1966; Hardin, 1955).

3. Reasoning that climatic and soil differences might hinder the direct use of American technology, emphasis began to be placed upon "adaptive research." This research was designed on the assumption that relatively few changes were needed, and that they could be relatively easily achieved. Diffusion studies began to reveal that innovations were more often adopted by larger, higher-status, better-capitalized farmers (Rogers and Shoemaker, 1971). Some began to reflect on the nature of the innovations themselves. They reasoned that under certain conditions non-adoption might be the rational strategy for a farmer to take.

4. Eventually, it became apparent to both administrators and field staff that simple adaptation of American technology would not do the job. A full-scale effort was launched to develop research institutions and agricultural universities that had the full complement of teaching, research, and extension functions, usually under the auspices of one or more Land-grant schools. The approach was formalized under the rubric, "Institution Building." It was

avowedly top-down in its approach, reasoning that a well-trained cadre of scientists could turn around Third World agriculture:

> The IB model is an elitist theory with an explicit social engineering bias. Changes occur from the top down, not from the bottom up, and they are guided by persons enjoying a measure of official authority or sanction (Esman, 1972:66).

In short, peasants would be made to abandon outmoded traditions by a massive organizational campaign (Cochrane, 1972), conducted by "modernized" elites. This position assumed, of course that elites were genuinely interested in develpment and not in merely perpetuating their own elite status. It also assumed that the natural and social sciences would be able to provide all the right answers within a relatively short time if they had the proper institutions within which to work.

5. While the Green Revolution at first appeared to validate the idea of Institution Building, a chorus of critics emerged who began to point out that in some areas increased productivity went hand in hand with increased immiseration of at least some farmers and tenants (e.g., Pearse, 1980). While reaction to the critics was defensive at first-- it should be noted that the critics were mostly social scientists, while the defenders were mostly agricultural scientists-- the eventual result was the incorporation of some of these social scientists into the research programs themselves. While the Institution Building approach was not abandoned, the thrust began to move away from commodity research by disciplinary specialists, to the creation of multidisciplinary teams. This was especially true of the newer International Agricultural Research Centers.

6. With the expansion of international research in the 1970s and the incorporation of social scientists into the IARCs, the farming systems

approach emerged. This approach advocated greater attention to constraints on farmers, treatment of the farm household as a system, and direct contact of researchers with farmers including on-farm trials. Farming Systems Research (FSR) has yet to be fully developed as an approach. Two schools can be discerned presently: those who favor quick reconnaissance methods (the Sondeo approach), and those who favor more detailed analysis and dialogue with farmers. However, both schools share the view that appropriate technology is the key to improving smallholder production and income. Of particular concern is that neither school addresses the technology treadmill problem, of which I shall say more later.

Having provided this synoptic view of the USAID experience with agricultural research, it is also worth noting the major role played by T.W. Schultz, and especially his seminal work, Transforming Traditional Agriculture (1964). In particular, Schultz was responsible for the realization that technically-trained personnel were essential to the development process. This idea was embodied in the now well-known concept of human capital. This suggested that Institution Building programs, modeled on the Land-grant universities were essential. A second issue influenced by Schultz was the decision to establish the international center system along commodity rather than regional lines. Third, whether through intent or oversight, Schultz suggested that the transformation of agriculture could be accomplished without reference to political (and especially distributive) issues. This made research programs appear particularly desirable to Third World elites, especially as compared to programs of land reform or income redistribution. It also made research appear to USAID to be an antidote to political unrest and social revolution. Finally, it suited the apolitical and technical orientation of the various American agricultural colleges.

Subsequent events have shown that this supplanting of politics was an unreachable goal in that (1) research has its own politics and (2) research cannot substitute for political reform. Only recently have the limitations of this approach become apparent.

One final point needs to be made by way of introduction; this concerns the way in which the Land-grant model was exported. It should be remembered that from the late 1930s until the early 1970s, the coalition in the U.S. Congress often referred to as the "farm bloc," insured unquestioned continual incremental growth in research appropriations in return for a strong emphasis upon increasing agricultural productivity. As a result, researchers and administrators lived within an institutional environment in which political relationships were taken for granted. Only with the critiques of the early 1970s (e.g., the Pound Report [National Research Council, 1972], Hightower, 1973) did conflict force reconsideration of those relationships. Therefore, the Land-grant model that was exported reflected not the actual workings of the system but an idealized image of it. This idealized image forms the starting point of my conceptual framework.

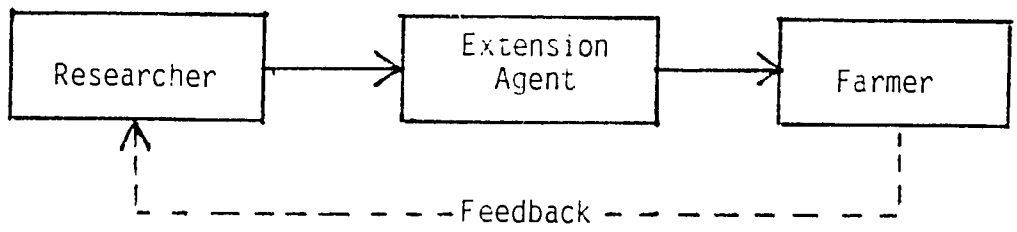
II. Conceptual Framework

A. The Conventional Model

According to the conventional model of agricultural research, borrowed from mechanical models of communications, scientists develop new technologies in their laboratories and fields, and hand them to extension agents who then disseminate them to farmers (Figure 1). In addition, scientists train undergraduates, who become farmers and extension agents, as well as graduate students who become the next generation of scientists. Thus, an apparently complete picture of the system is provided in the three functions of teaching, research, and extension.

This conventional model formed the basis for the diffusion research conducted through the land-grant schools from the 1930s onward. The practical goal of this research was to increase the rate and completeness of adoption of agricultural innovations. While feedback was later added to the model, it

Figure 1. The Conventional Model of the Research and Extension Process



consisted largely in the farmer informing the researcher whether or not he had adopted the innovation developed by the researcher. Thus, though the communication was two-way, it was strongly biased in favor of the researcher. It was assumed that the researcher had the right answer to the right problem and that failure to adopt could be best understood as stubbornness or ignorance on the part of the farmer.

What the conventional model failed to do was to distinguish between scientific or means-ends rationality and everyday rationality. Scientific rationality is the approach generally used in the process of doing science. Consider, for example, the situation depicted in Figure 2. The end of the research is to increase agricultural productivity. Various means, including those listed in the figure, are available for achieving that end. In general, the researcher will choose that (or those) means that are most suitable to his/her disciplinary background. While the choice of means is discussed at length, the end is taken as a given. This is the case even though the end is by no means a final one; it is itself a means to an end.

Figure 2. Scientific Rationality.

End	Means
To increase agricultural productivity	Breed more productive plants Control pests Increase soil nutrients Modify cultural practices Introduce irrigation

A central feature of scientific rationality is that it is instrumental (Idhe, 1979; Busch, 1984). This instrumental character of science is manifested in several ways. First, science involves the use of instruments (e.g., spectrometers, pH meters, microscopes, scales, etc.) to construct knowledge. We need only walk through a modern scientific laboratory to note the profound importance that instruments have for science. Second, science is instrumental in that it is concerned with the choice of means and not of ends. While individual scientists may employ symbolic, analogical, or even literary reasoning in their work (Knorr-Cetina, 1981), the end toward which they strive is not called into question. To return to the example presented in Figure 2, the decision to increase agricultural productivity is the proper subject of philosophy or politics, but not of science.

Now, consider what I shall call everyday rationality. In everyday situations both the ends and the means may be more varied and their interactions more complex. The case depicted in Figure 3 is illustrative. It is immediately apparent that the choice of ends is much broader than in the case of scientific rationality. Moreover, not all of them can be maximized at once. Thus, tradeoffs among them will be essential. The same will be true of the means: The degree to which a given means wil be employed will be directly related to the relative importance attached to attaining the various ends. While an economist might be able to calculate an optimal solution given a

Figure 3. Everyday Rationality.

Ends	Means
To Make a profit	Grow high productivity crops
To Demonstrate success to one's neighbors	Mechanize production
To Minimize heavy toil	Minimize cost of purchased inputs
To Spread one's work evenly over the year	Increase farm size
To minimize soil erosion	Find nearby markets
To maintain a certain degree of independence	Construct terraces
To insure a minimal harvest even in poor years	

particular set of weights for the ends, by virtue of the complexity of the decisions and the press of time on the farmer, no rational calculation of an optimal solution is possible for him. He will be forced to accept the solution that appears most appropriate to him. Finally, science and technology will only aid in attaining some of the desired ends.

In short, scientists not only control for those factors that might directly intervene in an experiment; they also 'control' on the larger political and socioeconomic environment, by limiting their research to the service of one or two relatively well-defined ends. Farmers, especially those in Third World Countries, rarely have this option. As I shall make clear below, this difference between scientific and means-ends rationality has profound effects on communications.

Other assumptions incorporated into traditional diffusion models are discussed at length in Busch (1978). They include:

1. Ontological Monism. Diffusion theory assumes that there is a single, objective social and physical world in which we all live. Ethnographic research in non-Western societies challenges this assumption by arguing that knowledge of aspects of the world is only attained through participating in

it; those who participate in the world differently, in a very real sense live in a different world.

2. Language as an object. Diffusion theory gives little or no concern to the problem of language. Translation of scientific to lay language or from one ordinary language to another is seen as a relatively straightforward task. In contrast continental philosophers and sociologists argue that language is a fundamental aspect of how we put the world together. For example, the categories of scientific botany and those of the ethnobotany of an African society reflect the different concerns of the two groups.

3. Communication as monologue. This aspect of the diffusion perspective is apparent in Figure 1. Feedback in no way directs the research or tells the researcher that a particular innovation is unsuitable or in need of modification. Hence, no dialogue is possible.

4. The sharp contrast of tradition and modernity. From within the diffusion perspective, it is always "us" moderns against "them" traditionals. This perspective, first developed in the self-conscious modernism of Descartes ([1637] 1956), denigrates tradition as inherently erroneous. Tragically, it is often manifested most extremely by members of Third World societies who have received scientific training in the West. Ironically, this position ignores the important, perhaps pivotal, role that tradition plays in science. It is not accidental that scientific journal articles begin with a review of the literature; like all traditions, those of science have withstood the test of time. They have been declared to contain the truth, verified by a (fallible) scientific community. This disregard for tradition has often led scientists to jettison the experience peoples of the Third World have in their particular agroclimatic zones.

In sum, the diffusion approach has been limited by a lack of

understanding of the difference between scientific and everyday rationality as well as a failure to recognize its own underlying assumptions. In retrospect, this is understandable for diffusion models could build on neither the early sociology of science literature nor that of neoclassical economics.

The sociology of science owes its origins largely to the pioneering work of Robert K. Merton (1938, 1973). Merton was among the first sociologists to call attention to the possibility of the scholarly study of science. However, Merton's work was limited in several ways: First, he and his followers have tended to confuse the norms of science (e.g., universalism, communism, disinterestedness, organized skepticism) with its actual practice (Mulkay, 19[illegible]). Second, they have confined their studies largely to physics on the essentially untested assumption that physics was in fact the model for all other sciences. Finally, studies were confined to the structure and not the content of science. Only within the last few years have the agricultural sciences been the subject of systematic study (Busch and Lacy, 1983; Hadwiger, 1982; Ruttan, 1982).

A similar situation has existed within economics. Mainstream economics has tended to treat science and technology as essentially residual categories. Indeed, one of the early studies of the contribution of research and development to increased productivity, attributed to R&D the unexplained variance remaining after the effects of land, labor, and capital were accounted for (Solow, 1957). Only with the recent development of the induced innovation thesis-- about which more below-- has R&D been seriously considered by economists.*

One explanation for the scant attention paid to science and technology

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*Schumpeter is an important exception. See Elster (1983) for a review of his position.

has been voiced by Mulkay (1979). He argues that both conventional sociology and economics rest upon a long unchallenged assumption that scientific knowledge is somehow unique in that it is independent of circumstance. Since this is assumed to be the case, the conditions under which scientific knowledge is generated can have no effect upon its contents. Recent studies, however, have shown that this special claim for scientific knowledge is at best too strong and perhaps unjustified (e.g., Latour and Woolgar, 1979; Schafer, 1983). In any case, it is clear that the diffusion approach was hampered by the virtual lack of models in either sociology or economics that would have revealed its limitations.

B. The Induced Innovation Model

The induced innovation hypothesis was initially mentioned in passing by Hicks (1935), but has only recently been developed into an articulated theoretical perspective (Binswanger and Ruttan, 1978; Ruttan, 1982). The argument put forth by its proponents is fairly straightforward: Agricultural scientific and technical innovations are said to be developed in response to relative factor scarcities. Thus, in nations where labor is scarce, innovations will tend to be labor-saving while in nations where land is scarce, innovations will tend to be land-saving. The United States and Japan, respectively, are frequently used as examples of this marked difference in research trajectories. (In contrast, Elster [1983] notes that some have argued that entrepreneurs, including farmers, will accept any cost reducing innovation, not merely those that reduce the cost of the scarcest factor.)

One key assumption of the model is that returns to research will accrue to the firm conducting the R&D. In most countries this occurs as a result of patent laws. Patents grant exclusive license to market a given innovation to a given firm for a given period of time. However, other government policies

may also change the rate and direction of technical change. Binswanger explains:

> If the rate of technical change in an industry is responsive to the price of that industry's output, then policies that alter prices of the output of one sector of the economy will affect the rate of technical change of that sector and of the sectors that produce substitutes (1978:18).

Such policies might include subsidies, high import or export taxes, regulation of use of certain technologies, etc. For example, Sanders and Ruttan note that "government resources used to subsidize tractors [in Brazil] and to subsidize domestic industrial capacity to produce tractors must be directed from other uses such as the creation of yield-increasing biological technology" (1978:277). It also served to shift sugar production from small farms in the Brazilian Northeast to large farms in the South.

Since much agricultural research does not yield patentable products, traditionally, there has been little incentive for the private sector to invest in it. Hence, early American agricultural research focused almost exclusively on machinery (Wik, 1966). Only with the development of USDA research capabilities, and the passage of the Hatch Act in 1887 establishing the state agricultural experiment stations, did biological, chemical, and later, social science research in agriculture become established. This same division of labor between the public and private sectors in agricultural research is apparent in virtually all the market economies of the world.

However, public sector research is outside the market:

> When ... research is publicly funded, the research resource allocation process becomes as imperfect as any public allocation mechanism. The latent demand for technical change must be filtered through political institutions, and the outcome depends

heavily or the political influence of various groups whose income position stands to be affected by the technical change.

Efficiency considerations are not the only criteria by which choices will be made (Binswanger, 1978:15).

Nevertheless, Ruttan (1980) argues that, despite the bureaucratic nature of public research, public sector research institutions are quite efficient. He suggests that this may be due to the highly competitive character of farming. In addition, a review of studies of returns to public sector research in a large number of countries using various estimation procedures, shows consistently high returns to research investments (Ruttan, 1982). Moreover, Trigo and Pineiro (1982) note that when the state fails to provide effective research institutions, the powerful interests in the private sector will attempt to provide those services themselves.

Ruttan (1982) has extended the induced innovations approach to induced institutional innovations. He argues that effective institutions innovate in response to changing environments. Such institutional changes include changes related to the conditions of land ownership, developing organizational structures that effectively produce research, and incorporating social science research that identifies bottlenecks and weak links in the agricultural production system and in research institutions themselves. A particularly interesting example of this latter point was a study of the economics of weed control conducted at ICRISAT. The study revealed that chemical control was much more expensive than hand and animal control. In addition it would displace landless laborers. As a result chemical control of weeds was deemphasized at ICRISAT (Binswanger and Ryan, 1979).

While the induced innovations perspective represents a significant step forward in our understanding of agricultural research and our ability to

improve its effectiveness, it has not been without its critics. For example, Pineiro, Trigo, and Fiorentino have argued that Latin American data suggest that inducement mechanisms described by Binswanger and Ruttan have not resulted in innovation. They note that "different social groups, and particularly those directly related to agricultural production, will have different attitudes towards technology depending on their expectations of the effects of the technology and their capacity to appropriate the potential economic benefits derived from its utilization" (1979:172). They go on to note that the state intervenes in creating both the supply of and demand for agricultural innovations. "A central point is that demand and supply are interdependent through the role played by the state in the determination of model components which affect both sides [of the equation]" (Pineiro, Trigo, Fiorentino, 1979:174). Since supply and demand for research can in no sense be considered independent, the usual assumptions of mainstream economics can no longer be considered valid.

DeJanvry and LeVeen pursue the consequences of this line of reasoning: "Technical change conditions the social control of the means of production, the organization of the labor process, the social division of labor, and the social appropriation of the surplus. As such it is a powerful instrument of social change or stasis" (1983:27). Therefore, research directions and appropriations are determined in large part through social conflict rather than by purely technical means (e.g., optimizing agricultural incomes or crop yields). They note that rising labor costs in California were the result of changing social relations rather than any scarcity of labor. In addition, the California experiment station undertook research on mechanization long before any problem was made manifest (Friedland, Barton, and Thomas, 1981).

A related criticism concerns the international dimensions of agricultural research. Much research in the Third World is as capital-intensive as that

conducted in the United States. In addition it has often centered on products of interest to the developed nations and has employed modes of production more appropriate to developed nations than those of the Third World (Trigo and Pineiro, 1983). In short, not only national groups but international groups as well participate in the research resource allocation process. Certain kinds of indigenous research, even in the private sector, may be suppressed or discouraged by more powerful international interests.

A final point concerns the implicit assumption of a democratic society embedded in the induced innovation perspective. Public support for research is seen as the result of an open policy-making process that, though it may favor some interests over others, nevertheless allows all to be heard. In many Third World nations such an open process simply does not exist. Steinberg et al. (1984) and, especially, Burmeister (1985) suggest this to be particularly true of Korea. Burmeister suggests that directed innovation might be a more apt term to describe that nation.

In sum, the induced innovations approach offers a substantial number of insights over and above those of either diffusion theory or mainstream economics. Nevertheless, it tends to adopt a tacit consensus perspective, perhaps appropriate for developed democracies, but of less value for Third World nations. Recent developments in the sociology of science, however, complement the induced innovations perspective well. Two "schools" may be defined, focusing on (1) the role of clients in the broadest sense in creating a demand for science, and (2) the role of scientists and administrators in creating a supply. It is to these schools that I now turn.

C. Client-driven Science

Traditionally sociologists have conceived of science as a largely autonomous activity conducted by scientists with little concern for the larger

social world. Knorr-Cetina (1981), in her recent work, The Manufacture of Knowledge, has challenged that view. After a year observing a food scientist at work in a university laboratory, she coined the term 'transcientific fields.' By this she means to include not only those who work within a given research group but also non-scientists who have more or less interest in the outcome of the research. She begins by noting that scientific journal articles are written to serve a special purpose: "Scientific papers are not designed to promote an understanding of alternatives, but to foster the impression that what has been done is all that could be done" (1981:42; emphasis in original). This, however, conceals the complex negotiations over just exactly what will be done (Busch, 1980). Moreover, it is misleading to consider non-scientists' influence on research problem choice as an external influence:

> To refer to research problems as an 'external' input ignores the fact that the process of defining a problem penetrates to the core of research production through the negotiations of its implications and operationalizations (Knorr-Cetina, 1981:88).

In short, Knorr-Cetina argues that clients cannot be seen as outsiders but form an integral part of each 'transcientific field.'

Wolf Schafer (1983) and his colleagues take a different path but arrive at a similar conclusion. They argue that science has become a social resource which can be aimed at the solution of various social problems. Bohme et al. (1983) argue that scientific fields may pass through three phases: The first is an exploratory or pre-paradigmatic phase where discovery rather than theory is the rule. The second is the paradigmatic phase in which the problems of research are determined by theory (e.g., Kuhn, 1970). The last is the post-

paradigmatic phase. During this phase, theoretical issues have been 'finalized.' That is to say, the methods and exemplars of the research group are well-defined and subject to little debate. Moreover, there is no compelling theoretical reason for pursuing one research trajectory as opposed to another. At this point the practical concerns of the larger social world begin to take a central role in guiding research.

Not surprisingly one of the first examples they use to illustrate their perspective is that of Agricultural Chemistry. Krohn and Schafer (1983) note that agricultural chemistry was developed as a separate field by Justus Liebig in response to Malthus. Liebig reasoned that the only way out of the Malthusian dilemma was to increase agricultural productivity. This would be done through the development of the scientific specialty that came to be known as agricultural chemistry. Liebig's agricultural "chemistry emerged as a science not only to explain the processes of agriculture, but also to shape them in accordance with human purposes" (Krohn and Schafer, 1983:29). In short, for Liebig agricultural chemistry was at once a science of natural cycles and a technology that could be used to alter those cycles for human purpose.

It takes little extrapolation from this approach to realize that agricultural research itself consists largely of 'finalized sciences.' Moreover, the commitment to application, the mission orientation of agricultural research, insures that clients are central in directing it. Hence, while the basic principles of plant and animal physiology and pathology were worked out many years ago, research is conducted to develop special theories that explain bovine physiology or the physiology of corn. This is the case because it is these organisms (commodities) that are of concern to

client groups.*

A third perspective on the role of clients if offered by Busch and Lacy (1983). Their study attempted to answer the question: How are research problems formulated in agricultural research? To answer this question they (1) reviewed the official literature on the State Agricultural Experiment Stations and USDA from their inception to the present, (2) conducted in-depth interviews with scientists at several locations, (3) administered a mail survey to which 1431 scientists responded (a response rate of 76%), and (4) reviewed the recent technical literature for state-of-the-discipline statements.

When scientists were presented with a list of 21 criteria for problem choice they ranked "feedback from extension" as twentieth. In contrast, "demands raised by clientele" ranked 13th and "client needs as assessed by you" ranked 7th. In addition, 36% of the scientists rated "client or potential user" as an influential person in their choice of research problems. In contrast, their colleagues were seen as influential by only 20% of the respondents. In short, extension had little to do with problem choice. Clients did, but not in that client demands were simply responded to in a passive way. To the contrary, scientists were more likely to take their own assessments of choice. The words of one respondent sum it up:

> Researchers in agricultural economics (as in most disciplines, I suppose) have difficulty in determining what research would be most useful. They prefer to research those areas in which a lot of people would appreciate getting the results. The

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*This has often been carried to extremes. Researchers in agriculture have often felt it necessary to confine their activities to crop plants while those in the basic sciences have studiously avoided them (Levins, 1973).

public and their other clients, however, do not communicate their needs well so the researcher has to decide on his own what is important (quoted in Busch and Lacy, 1983:47).

In short, Knorr-Cetina, Schafer, and Busch and Lacy emphasize that science is conducted in response to clientele demands -- demands expressed not through the market mechanism but through negotiation, persuasion, and coercion. Successful science, it would appear, must respond to those demands rather than go off on interesting theoretical paths for which there is no demand. But if there is a demand side to science, then presumably there must be a supply as well. It is to that supply that I now turn.

D. Science-driven Research

Bruno Latour (1984) proceeds by asking whether science can be separated from politics. He answers this rhetorical question with a resounding 'no.' Louis Pasteur is often viewed as one of the great scientific geniuses of the nineteenth century. He is often described as a lone genius who made his contributions to science and medicine through dedication and hard work. While not denying Pasteur's genius, Latour shows that he was also a great organizer: "Pasteur, from the start of his career, was an expert at fostering interest groups and persuading their members that their interests were inseparable from his own" (Latour, 1983:150). Latour shows at great length how the Pasteurians positioned themselves between the social world and the world of microbes. Only the Pasteurians had access to this microworld, and only they seemed to be able to reproduce that microworld in the laboratory. The hygieneists could produce statistical relations between diseases and certain physical phenomena (e.g., raw sewage, standing water, polluted air), but only the Pasteurians could recreate that relationship in the laboratory. Moreover, every time that Pasteur encountered an 'applied' problem, he turned it into a fundamental one

to be resolved by disciplinary methods.

The Pasteurian laboratory, however, has certain very important characteristics: "The laboratory positions itself precisely so as to reproduce inside its walls an event that seems to be happening only outside... and then to extend outside to all farms what seems to be happening only inside laboratories" (Latour, 1983:154). Thus, Pasteur first brought animals into the lab and created an epidemic; he then literally brought his laboratory into the field and prevented anthrax. He accomplished this latter task by convincing French farmers to vaccinate their sheep and to keep their barns clean -- in other words, to make their barns as much as possible resemble his laboratory.

In short, Latour argues that Pasteur's success was due not only to his genius as a scientist but to his ability to organize various interests so as to transform the world. It takes only a few moments reflection to realize that the same may be said for both the successes and failures of the Green Revolution of the 1960s. Scientists and administrators, first in the foundations and later in the International Agricultural Research Centers, identified the need for high-yielding varieties -- not Third World smallholders. They then set about developing those HYVs on experiment station fields and in their laboratories. Yield response under optimal levels of fertilizer, water, light, temperature, etc. was the goal of much Green Revolution research. The HYVs were diffused by convincing farmers to reorganize their fields so they more closely resembled the experimental fields of the researchers. This meant introducing irrigation, fertilizers, pesticides, and various new cultural practices. Only those farmers who had the wherewithal (e.g., capital, access to credit and inputs, etc.) to replicate the research plots on their farms were able to benefit from the Green Revolution bonanza.

The examples of Pasteur and the Green Revolution clearly show that scientists can and do package and sell their products and processes to the larger public. And, they often do this aggressively. Thus, not only clientele but also scientists themselves contribute to both the scientific agenda-setting process and the determination of the products that emerge from scientific research. What remains is to integrate the sociology of science perspectives with the induced innovation approach. That is the subject of the following section.

E. Synthesis: Supply and Demand in Science

An attempt to synthesize the various perspectives described above is depicted (in simplified form) in Figure 4.* Here, the information flow in the research and extension system is expanded to include in addition to farmers, agribusinesses, administrators, government agencies, scientific disciplines, other funding sources, legislators, and others. These various clients/users all impinge upon the choice of research problems that the researcher addresses (Busch and Lacy, 1983). They may do this through funding mechanisms, by lobbying funding organizations, by direct pressure upon the research organization, or by administrative decree. Moreover, each of the client groups will want different things from the researchers. For example, farmers may desire lower production costs, agribusinesses may desire greater use of fertilizers, administrators may desire annual progress reports or large numbers of articles in scientific journals. In contrast, government agencies may desire new seeds that can be multiplied, scientific disciplines may desire major "breakthroughs," and legislators may desire a reduction in urban food

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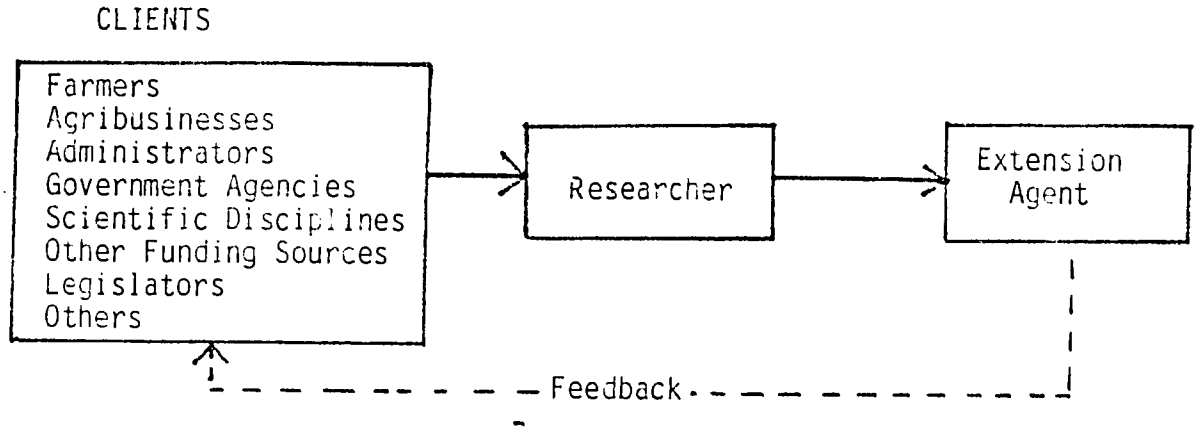
*A much more detailed analysis of the structure of contemporary U.S. agricultural research can be found in Busch (1980).

Figure 4. An Alternative View of the Research and Extension Process



prices. Thus, the variety of groups that create a demand for agricultural research is substantial. However, unlike the demand for a commodity such as beef, which is cumulative and quantitative, the demand for research is, in reality, a set of partially competing and conflicting demands. Thus, the problems finally selected for research arise out of negotiations, persuasion, and coercion involving the full range of clients and the researcher(s) (Busch, 1980).

In addition, the researcher controls the supply of research. The researcher has substantial control in this regard as he or she is one of a very small group -- in small organizations perhaps the only one -- who fully comprehends the ways in which research can be brought to bear upon client demands. Moreover, since research cannot be produced on an assembly line, like automobiles, but instead requires arcane skills and instrumentation, the researcher retains substantial latitude in defining the research even in the most hierarchical research organizations. The practice of doing one thing and calling it something else is so commonplace as to have acquired a clear designation: bootlegging (Greenberg, 1966). Moreover, a competent researcher is likely to have, as a result of training and background, many ideas of his/her own. These ideas will doubtless enter in the

negotiation/persuasion/coercion preceding the research and into the research itself. Finally, if the researcher has been a member of the research organization for very long, he/she will understand the reward system. The research program selected will reflect those rewards, whether they consist of filing annual reports on time, publishing many journal articles, or working in farmers' fields. The outcome of this complex process will, necessarily, set limits on the range of products that are provided to extension agents for diffusion.*

In sum, innovations are induced, in part by the relative scarcity of the factors of production. However, the relative scarcity of the factors of production is only one of many considerations that enter into the public research decision making process. Of necessity many other considerations, some of which are only remotely related to commodity production, impinge upon the research process. This is true even of large private organizations with R&D laboratories. There, too, conflicting pressures from sales and production staff, as well as researchers' own interests, must be taken into account.

If the researcher does his/her homework, then the products created are those that were demanded by (some subset of) clients in the first place. The diffusion process largely involves making those products and how to use them known to the clients that requested them.

Of course, all client groups are not alike. Farmers may be wealthy or poor, may grow different crops and livestock, may or may not hire labor, and may have very different interests. Similarly, as noted above, legislators may be more interested in keeping urban food prices low than in augmenting farmers' incomes. The diffusion literature has often noted the tendency for

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*This is necessarily the case for any research system as a result of limits on financial and other resources.

better capitalized, better educated, higher status farmers to adopt innovations more rapidly than their neighbors (Rogers and Shoemaker, 1971). This is not due to any innate propensity to adopt on the part of these individuals, but because they are more articulate and have greater access to the research system itself. Ruttan explains why:

> Under competitive market conditions the early adopters of the new technology in the agricultural sector tend to gain while the late adopters are forced by the product market "treadmill" to adopt the new technology in order to avoid even greater losses than if they retained the old technology. One effect of the treadmill phenomenon is ... to limit the economic motivation for [farmer] support of agricultural research to a relatively small population of early adopters of new technology. The early adopters also tend to be the most influential and politically articulate farmers (1980:540).

In short, various clients will have differential access to researchers depending upon their wealth, power, status, class, and even their ability to articulate their demands to researchers. And, obviously, in societies in which income, wealth, class, and status differences are already pronounced, the problem of differential access will be proportionately magnified.

The recent move to farming systems research (FSR) represents an attempt to overcome the problem of differential access.* By collecting information about problems directly from smallholders, the probability that research will directly serve their needs is enhanced. However, we should not look to FSR to resolve the technology treadmill problem. That problem is endemic to

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*Key approaches include Hildebrand (1980) and Norman (1978). For a review and critique see Oasa and Swanson (1985).

competitive markets. Simply put, the early adopters capture most of the gains from adoption. If they use those gains to increase the size of their farm operation, as is often the case, then they are in an even better position to be early adopters of the next innovation that is produced. If this process continues for a long period of time, then the distribution of farms by size becomes bimodal irrespective of the scale bias of the innovations. A few large farms grow most of the marketed agricultural produce while a larger number of small farms produce mostly for home consumption. The medium sized farms disappear, their owners migrating to the city in search of more lucrative employment. A recent study examined this problem for the United States for the years 1915-73. It concluded that public agricultural research significantly increased farm size during that period independently of other contributing factors (e.g., debt, taxes, unemployment) (Busch et al., 1984).

In the United States most farmers and farm laborers who have left the farm have been able to find employment in the industrial or service sectors of the economy, though displacement was not without its costs. However, in Third World nations, cities are already overflowing with unemployed and underemployed workers. Clearly, there are good reasons for maintaining a larger percentage of the population on farms until non-farm employment is available. However, with few exceptions, the research system is incapable of accomplishing this task. Only an informed national agricultural policy can mitigate the undesireable effects of a steady stream of innovations into a competitive market. Such policies might include production quotas, elimination of subsidies on machinery, or perhaps taxes on machines.* In any

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*To my knowledge no nation has adequately addressed this problem. Perhaps other policy options need to be invented. However, few social scientists are addressing the issue.

case, agricultural research policy must be integrated into a national agricultural policy if they are not to work at cross-purposes.

In summary, this synthesis adds several dimensions often neglected in evaluating agricultural research systems. First, the importance of client demand and researcher supply is clearly stated. Second, it is noted that the supply of and demand for research are not market functions but must, of necessity, enter into the larger political sphere. Third, it is recognized that some clients have more access to the research system than others. Fourth, diffusion is seen largely as providing certain clients with the innovations that they initially requested. Fifth, the necessary linkage between agricultural research policy and agricultural research is clarified. In short, "while research can provide the required technology improvements, a research program will be more effective if it is not planned in isolation, but as part of the political, social, and economic system that it must serve" (Murphy, 1983:19). Let us now turn to the methodological issues raised in using this conceptual model to evaluate agricultural research.

III. METHODOLOGICAL ISSUES

A. The Need for Comparative Data

One of the difficulties in evaluating a research institution, is that there is often nothing with which to compare it. As such, it is difficult to know if resources are well-allocated, if funding is adequate, if productivity is sufficient, etc. There are several ways of overcoming this problem:

1. Examine evaluations of other research institutions of about the same size, historical background, region, etc. that have already been conducted (White, 1985). In particular during the last decade, large numbers of such studies have been produced, but they are often what librarians call "fugitive literature," hard to find and only occasionally

indexed. (AID may wish to consider sponsoring a project to support collection and indexing of such documents.) Examples of such studies include Lacy, Busch, and Marcotte (1983), Pray et al (1982), Evenson, Waggoner, and Bloom (1981), ISNAR (1983), and Murphy (1983).

2. Examine statistical information collected comparing research systems in developing countries. In particular, Oram and Bindlish (1981) have compiled an enormous range of statistical information on research systems and provide both an analysis and the raw data. Evenson (1974) has produced a similar study of Extension activities; however, data on extension are more spotty. Other studies include Evenson and Kislev (1975), and Arnt, Dalrymple, and Ruttan (1977).

These data can also be useful for historical comparisons of the same research systems. For example, consider the data for Tanzania contained in Table 1. It is apparent that expenditures per scientist declined by half during the decade in question. Moreover, when comparative data from other low-income nations of sub-Saharan Africa are examined, we find that Tanzania had the second highest rate of growth in scientists (11.34% per annum), surpassed only by Togo; at the same time, it had the lowest rate of growth in expenditures (0.66% per annuma). No complex statistical analysis is necessary to conclude that Tanzania has poorly utilized its research resources.

B. The Perils of Quantification

Ever since Descartes and Galileo, scientists have appreciated the importance of mathematical information to the progress of both science and technology. Unfortunately, this mathematical emphasis is often translated into a naive belief that quantified information is more valid, and even more real, than qualitative information. This has had and can have absurd and even tragic consequences. Let us consider some of the perils of quantification:

1. Accuracy. One major reason for quantifying things is to increase accuracy. However, the quantified data analysed are only as good as the procedures used in collecting them. If those procedures are poorly defined or in dispute, then quantification can be misleading. For example, Lele and Candler (1981) note that statistics on maize production collected for Tanzania by the United States Department of Agriculture, the Food and Agriculture Organization of the United Nations, and the Tanzanian Ministry of Agriculture are vastly different. They correlate poorly and do not even show the same trend lines. Especially when statistics refer to Third World countries, they may reflect the inadequacies of the reporting system more than the phenomenon they were intended to measure.

2. Ambiguity. Another reason for quantifying is to remove ambiguity. However, poor collection methods can increase rather than reduce ambiguity. For example, some years ago when Rhodesia unilaterally declared independence, a well-known survey research organization asked respondents whether the U.S. should intervene. Unfortunately they neglected to state on which side the hypothetical intervention was to occur (Lang, 1981). Thus, numbers do not guarantee that ambiguity is eliminated.

3. Relevance. A curious argument often given by researchers fascinated by numbers runs as follows: Certain variables were excluded from consideration due to a lack of (quantified) data. Yet, often those things most relevant to evaluating a project are precisely the things not quantified. For example, changes in cultural practices are often easily identified permitting collection of data on the number of hectares on which improved practices are employed. In contrast, illness caused by chronic exposure to low levels of pesticides is extremely difficult to quantify yet very relevant in certain Third World situations.

Table 1. Expenditures (in thousands of 1975 constant US dollars) and Scientists in Tanzanian Agriculture Research, 1970-1980.

Year	Expenditures	Scientists	Expenditures per Scientist
1970	3,329	90	37.0
1971	4,388	100	43.9
1972	6.564	112	58.6
1973	5,820	130	44.8
1974	6,492	145	44.8
1975	7,074	158	44.8
1976	5,506	184	29.9
1977	4,860	194	25.1
1978	4,847	200	24.2
1979	4,878	256	19.1
1980	4,715	256	18.4

Source: Oram and Bindlish, 1981.

Similarly, surveys can be designed that will obtain answers to irrelevant questions (e.g., Do you, as an American, approve of the current government of the Maldive Islands) while answers to relevant questions may be extremely difficult to obtain (e.g., What is your current income and worth). In short, relevance must take precedence over quantification.

4. Time and cost. Evaluations must be done expeditiously or they cannot be justified. Hence, the collection of quantified information must be weighed against the time and cost involved in the collection. In many cases, responses from a few qualified key informants will be sufficient to answer the question. Random samples are not absolutely essential (White, 1985:37). Moreover, research administrators rarely desire highly detailed reports, but, instead, prefer clear pictures of the current situation and potential payoffs for development (Murphy, 1985).

In sum, while quantified information is useful, its value in evalutions, as in research, should not be overdrawn. A good evaluation employs the best data available within the time and cost limitations imposed upon it, whether that data is quantitative or qualitative.

C. Exogenous factors effecting research effectiveness.

As the conceptual model presented above makes clear, research institutions are ultimately linked to other actors in the agricultural sector. Therefore, there is no way to evaluate them without taking the broader agricultural economy into account. A hypothetical example should make this point clear. Consider an adequately funded research institution that successfully produces agricultural innovations of relevance to smallholders. However, the institution operates in an environment in which producer prices are restricted by government decree. As a result, no one adopts the innovations. Clearly, such an institution would have little impact on agriculture; yet, it would be a mistake to argue that it was an institutional

failure. The problem lies in government policy, not the information. Therefore, it is essential that background information about the agricultural sector be collected as part of the evaluation. It is also helpful that at least one member of the team be familiar with local conditions.

IV. Components of an Evaluation

In this section I discuss some of the kinds of information that an ideal evaluation would include and analyze. No evaluation would be able to document fully each of the many issues described below. However, this should not be necessary as some of the issues raised here will be moot in a given study. In addition, it should be emphasized that the list provided below should not be seen as so many compartments into which all evaluations can be sorted. Effective evaluation of research will require that the evaluators remain flexible and willing to incorporate issues raised by the actors in the research system in the evaluation. Overly rigorous conformity to an evaluation model is likely to produce formal results without an adequate understanding of the underlying dynamics.

A. Background Information

1. Agriculture

a. Changing crop and livestock mix. Even the most traditional agricultural sector changes. Collecting information on the major crops and livestock over the last decade or two will reveal trends in productivity, crop mix, and regional distribution. This will allow matching research projects with the relative importance of various agricultural commodities. For example, Hargrove (1978) found that rice research in several Asian nations gave too much emphasis to irrigated varieties and not enough to upland cultivars.

b. Food and agricultural policy. Has food and agricultural policy

encouraged production? Has it been consistent over the years? How effective are the current polices? Are they enforced or widely ignored? What has been the effect of agricultural policy on farmer prices for key commodities? Understanding these and other country-specific issues is essential for a satisfactory evaluation. In some countries this information will be well-codified and easily accessible, while in others personal interviews with policymakers will be essential.

2. Agricultural Research

a. Budgetary History. As Table 1, above, makes clear, a great deal can be learned from reviewing budgetary histories. This is especially true given the relatively long-term between the inception of a project and its impacts. Evenson (1978) suggests that productivity impacts may average 12 years, while Busch et al. (1984) found that distributive impacts could take as long as 10-12 years to be completed. Of particular import are declining resources per scientist, unstable research budgets, salaries so low as to encourage researchers to take second jobs (e.g., Cardwell, Moomaw, and Ruttan, 1981), inadequate staff or equipment, or lateness in delivering appropriations. These and other fiscal problems may impede the effectiveness of a research organization.

b. Institutional history. While some Third World research systems are of recent origin, many go back to colonial days. Often, research is still biased toward colonial objectives years after independence. Given the need for stability in research institutions, this is not surprising. A case in point is Sudan. The Agricultural Research Corporation of Sudan inherited a strong emphasis on cotton research from the needs of the now-defunct Empire Cotton Growing Corporation. It still reflects that bias (Lacy, Busch, and Marcotte, 1983).

In addition institutional histories offer an opportunity to assess long-term progress as well as policy shifts. Such histories need not be very detailed and can often be gleaned from older records.

c. Initial goals. Related to history are the initial goals of the institution. Have they been regularly examined? Are they formalized? How have they changed over the years? Looking at goals offers an opportunity to determine how self-critical the organization has been.

d. Issues raised at the inception. In some cases research institutions have started as a result of particular issues of local importance. What were these issues? Have they been resolved? Is their lack of resolution problematic for the institution? In the case of institutions established with foreign aid, original project proposal documents may shed some light on these issues. In the case of other institutions, interviews with older staff or public officials may be necessary to clarify these points.

e. Changes in structure. No institution stands unchanged for years at a time. Government reorganizations and administrative styles may alter institutional structure over the years. In some cases a research institution may be shifted from one Ministry to another. In other cases, it may be physically relocated. Functions performed may be combined or separated. Such changes may improve or impede institutional performance. Understanding them gives insight into institutional rivalries and instances of cooperation.

B. Internal Dynamics

1. Formal Structure. Understanding the formal structure of an organization is the first step toward comprehending its internal dynamics. Organizational charts, ubiquitous in most nations, are particularly useful in this regard. Having an administrator use the chart to explain how the institution works can be particularly enlightening.

2. Quality/Quantity of Staff. It goes without saying that no research

institution can function without sufficient numbers of qualified staff. This is the foundation upon which all else is built. Happily, information on quantity and quality of staff is almost always available as it forms a part of the administrative record. Of particular value, if available, are staff records over time illustrating growth or decline of the system. Specific information to be collected includes: (1) percent with PhD or equivalent, (2) schools and nations where PhD received, (3) number of scientists of various ranks, and (5) number of administrators of various ranks.

3. Scientists' Family Background. Even in the United States today, fully 38 percent of Agricultural scientists come from a farm background. As a result many scientists have practical farming experience that helps them in their work. While it cannot be expected that every scientist would come from a farm background, the lack of such persons in Third World countries would suggest a rigid class structure with minimal upward mobility and perhaps a weak link between research and its application.

4. Adequacy of support and infrastructure. Even the most well-qualified staff will be ineffective if the physical facilities, instrumentation, and supporting units (e.g., library, greenhouses, motorpools, experimental fields) are not adequate. Overall adequacy can, of course, be assessed quantitatively by looking at the capital and expense budgets of each scientist. However, a more careful assessment will required asking questions about the quality and relevance of experimental equipment, fields, greenhouses, library, etc. It is not necessary that such equipment be the most up-to-date available but that it be relevant and appropriate to fulfull the mission of the institution. Making such an assessment will usually require someone with substantial technical expertise.

5. Reward Structure. All organizations, whether explicitly or implicitly,

reward their members in a variety of ways. While some rewards are pecuniary, others include promotions, perquisites, access to special facilities, travel, and vacation. In research organizations, of particular significance is the consonance between rewards and organizational goals. For example, in the United States, Busch and Lacy (1983) found that the quantity of journal articles published was perceived by scientists to be most important criterion in promotion and tenure decisions. In contrast, Lacy, Busch, and Marcotte (1984) found that submission of annual reports was perceived by Sudanese scientists to be the most important criterion for obtaining rewards in that system. In both cases questions could be, and were, raised regarding the appropriateness of the reward structure to the voiced goals of the research enterprise.

In evaluating reward structures, it is important to examine both formal documents, and statements by administrators, and scientists' views as related in interviews. This is necessary as scientists do not respond to formal statements or administrators' perceptions but rather to their own perceptions of the reward structure. Also, given the enormous variation in reward structures across organizations, starting with an open-ended question such as "How does one get ahead within the _____?" will be more revealing than a series of closed-ended questions about particular rewards.

6. Disciplinary mix. Traditionally, agricultural research organizations have centered around the disciplines of agronomy and animal science. As time went on, it became apparent that entomology and plant pathology had to be represented as well. More recently, the social sciences as well as nutrition and food science have also become well-established.* Having the full complement of disciplines is particularly important if (a) interdisciplinary teams are to be effective, and (b) the organization is charged with the development of a final product. This is of even greater concern for Third

World countries as a private sector that takes partially developed products from experiment stations and markets them is often lacking. (This was also the case in the early days of the U.S. system. Only as the input industries developed did experiment stations abandon product development. In some areas, e.g., varietal seeds, the stations still develop marketable products.)

Of concern here is the appropriateness of the disciplinary mix to the agricultural problems faced. For example, a research organization may have the agronomic scientists to develop an improved sorghum but lack the food scientists to test its palatability or the social scientists to test its social and economic viability.

7. Appropriateness of projects. By this I mean more than the disciplinary mix described above. Specifically, the question that must be raised is whether the scientific projects underway are well conceived and whether they are likely to lead to positive changes in the agricultural economy. These are two interrelated but distinct issues. For example, the probably apocryphal story of the Indian soil scientist who made a career out of studying New York soils in India, is an illustration of high quality research unrelated to development objectives. On the other hand, research may be related to development objectives but poor conceived and likely to fail (cf., Murphy, 1985).

Clearly no evaluation can examine all the research projects in even a medium-sized organization. Hence, some sort of sampling procedure is essential. While a random sampe might make for a more elegant design, there are likely to be better reasons for sampling. These include (1) the cost of

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*Of course, some agricultural research organizations have been organized along commodity lines and so included only those disciplines concerned with a particular commodity.

reaching the research site, (2) an informed observer's account of areas that are particularly strong or weak, and (3) the centrality of the project to the organization's mission.

In practice the decision as to which projects to evaluate will have to be made with the particular local circumstances in mind.

C. Relations with the Agricultural Sector

As noted in the conceptual framework section above, effective research institutions are in continuous close contact with the clients for and users of their products. Hence, in addition to asking scientists and administrators about their contacts with clients, client perceptions of the institution should help in the evaluation. Among the clients/users to be considered are:

1. Farmers
2. Farmworkers
3. Students
4. Input industries
5. Processing industries
6. Extension agents
7. Other research institutions (including the IARCs)
8. Legislators/Policymakers
9. Ministry officials

Of course, no evaluation of reasonable length can sample persons in each of these categories of clients/users. Hence, some compromises will have to be made as to whom should be contacted. Decisions as to who to contact might be based on mutual discussions with administrators of the research institutions. For example, if certain types of clients (e.g., processors) are said to have no contact with the research institution, interviewing them about their contacts will yield no useful data. On the other hand, lack of contact with

ministry officials might well be worth an inquiry. Finally, it should be noted that some relations may be the subject of printed documents; whenever possible such documentary evidence should be used.

D. Costs, Benefits, and Distributive Issues

A strict, formal cost-benefit analysis of a research institution is likely to yield little of value. Unlike large capital construction projects. research can rarely be evaluated in this manner. However, there are costs and benefits associated with research programs. Two key questions need to be addressed: (1) Do the benefits potentially to be gained from the research outweigh the costs? And, (2) are the benefits and costs distributed fairly and equitably?

The first question is easy to answer in a general way. Though not without methodological problems, most studies show very high positive social returns to investments in agricultural research (Ruttan, 1982). Moreover, returns to effective research are likely to be higher in Third World countries where little research has been done, and major gains are possible, than in countries where there is a long research tradition. However, such returns are always realized over a substantial period of time; research rarely, if ever, pays off within a one to two year period.

Of more importance are the complex distributive issues related to research. These are particularly important in countries in which income, wealth, and power distributions are already highly skewed. It is there that research is likely to have the most serious distributional consequences.

Eide et al. (1985) have suggested that many distributive impacts can be best analyzed and understood by starting from the normative assumption that each person has a right to a nutritionally adequate diet, in short, that everyone has a right to food. This suggests that improved food security-- for

individuals, households, communities, and nations-- must be a central goal for agricultural research (Busch and Lacy, 1984). If this is taken as a starting point, then many distributive issues can be defined as follows: Under no conditions should agricultural research reduce or eliminate access to a nutritionally adequate food supply. Moreover, ideally the products of agricultural research should improve the nutritional status of the most marginal segments of the population. Let us apply this to several of the distributive issues that typically face agricultural research.

1. Economic issues. In areas with landless labor, the introduction of labor-saving technologies may have little effect on productivity but throw thousands out of work. As unemployment in most Third World countries means a complete loss of income, it it equivalent to being deprived of the means of subsistence. Not surprisingly, when such events have occured, riots and bloodshed have occured as well (e.g., Brass, 1982).

2. Labor issues. Third World farmers often plan their farm activities so as to spread labor out evenly over the year. The introduction of new crops or varieties that radically alter labor needs may interfere with other essential family activities. Such effects can reduce the demand for casual labor, thereby eliminating a traditional method for redistributing wealth (i.e., the smaller producers work occasionally on the farmers of the larger producers), as well as change the labor periods for the various members of the household. Both can contribute to reduced food security. In the former case, casual laborers may need that extra income to provide for their families. In the latter case income may go up while nutritional status (particularly of women and children) declines:

> For example a study from Ghana in a place where migrant labor was common, showed that the women, as a consequence of men's

> participation in cash crop production, would have had their work burden increased should they have continued the cultivation of yams which was the traditional staple food. Because of this they changed to cultivating cassava, for which less labor is needed. However as cassava is less nutritious than yams, this move to counteract an increase in women's work load had the effect of lowering the nutritional value of the staple food available to the household (Eide et al., 1985:7-27).

A similar study conducted in Tanzania found that infant mortality was highest in the area of greatest agricultural productivity. It appears that changes in cropping patterns had reduced women's breast feeding from five to three times daily with a consequent rise in mortality. Finally, Murphy (1983) reports a Tunisian project with detrimental impact on the status of women and overall nutrition.

3. Environmental issues. Certain new varieties or crops may encourage farmers to cultivate marginal lands better left in pasture or forest. Such environment degradation undermines the food security of future generations. At the same time it may shift labor and meal patterns by reducing the availability of firewood and/or potable water (e.g., DeWalt, 1983).

4. Role of Women. It is now commonplace to note the important role played by women in Third World agricultural production. Also of import is that women play a major role in each of the four aspects of household food use: procurement, handling, distribution, and consumption (Eide et al., 1985). Of particular note is that, in most Third World families, these four tasks are fully integrated; all are part of the daily work activities of women. As a result, as noted above, when labor patterns in one area are modified, other areas must be modified as well. Effective agricultural research in the Third World cannot ignore this intrinsic linkage as it is

fundamental to food security.

5. Agricultural research policy issues. Broad issues of agricultural research policy may also effect food security at both the household and national levels. For example, DeWalt (1985) notes that Mexican agricultural productivity has continued to rise since the 1940s, but in the 1960s grain sorghum was introduced from the United States. As the varieties introduced have been suitable only for feed, per capita production of grain has continued to increase while consumption has leveled off. Since only the most affluent segment of the Mexican population can afford meat products, the overall effect of sorghum introduction has been to reduce Mexico's food security.

E. Summary

In short, evaluations of agricultural research programs must include background information on the agricultural situation, an analysis of the internal dynamics of the system, a description of relations with various client groups, and an examination of costs, benefits, and distributive issues. Only a comprehensive analysis of this sort can (1) sort out with some degree of certainty those events traceable to agricultural research and those associated with other factors, and (2) provide information that is relevant to the improvement of the effectiveness of the research enterprise.

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