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The product of science

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ABSTRACT
The relationships between agricultural science, invention, production and consumption are conventionally considered in a hierarchical model with consumption at the basis and science at the pinnacle of the hierarchy. Two important shortcomings of that model as a basis for organizing and managing agricultural research are identified. First, the model does not specify how the direction and intensity of research is guided in the absence of markets for the outputs from research. Second, the model does not identify the flows of specific information from the consumption and agricultural production sphere to the research sphere where such information is crucial for the successful conduct of applied agricultural research. An alternative conceptual framework for agricultural research is explored that considers research as a problem-solving activity which exploits general scientific knowledge as well as specific practical knowledge. Implications of a problem solving perspective for the organization and management of agricultural research are discussed.

KEYWORDS: agriculture, management, organization, problem solving, research.

INTRODUCTION

The Nobel price winner and agricultural economist Theodore Schultz once observed that "The only real friends that economists have are impersonal, adverse events: inflation, unemployment, and hard times" (Schultz 1981, p.101). I do not think that inflation motivated the organizers of the conference to include my topic in the program and I also expect few, if any, unemployed agricultural scientists to be in attendance. Not being able to count on inflation and unemployment among my friends today, I conjectured that hard times for agricultural research must have motivated the organizers to draw economists into this conference.

Is it reasonable to speak of hard times for the agricultural research industry? In some sense, I believe, it is. Agricultural research funding is nominally declining in some countries and in more funding increases do not keep up with inflation. Conventional agricultural research is being criticized for not being sufficiently sensitive to the technology needs of farmers and of society, as those needs are perceived and articulated by whatever special interest group occupies the public stage. Furthermore, funding agencies are calling for more planning and accountability and thus divert scarce research personnel from research to administration.

Finally, the ultimate nightmare of all incumbents in a protected industry, entry by new competitors, has become a reality with the emergence of a modern, private biotechnology research industry.

Despite the current gloom there is also considerable reason for hope. Most people, wherever they live, apparently consider life on earth still a sufficiently happy affair to provide a growing number of future adults a chance for building their own tomorrow. If agricultural science remains to be an effort towards contributing to the knowledge and technologies that present and future generations will need for a life worth living, there should be no reason for gloom.

That purpose has served agricultural science well since agriculture was first counted among the sciences by Roger Bacon in the 13th century or Ibn Khaldun in the 14th century (Machlup 1982), and there is no reason to believe that it will fail agricultural science in the future.

With the continued need for agricultural research assured, the crucial questions are whether the politicians in control of the public purse are prepared to translate the need for research into effective demand for researchers, and whether the agricultural research industry can effectively deliver what is needed. Many agricultural economists have contributed to the business of encouraging politicians to spend public monies on agricultural research. Conventionally, economists do that by first calculating the returns to past investments in agricultural research and then assuming that returns in the past, which generally have been found to be high (Harris and Lloyd 1991), are a good indicator for returns on investments made today or tomorrow. I do not intend to discuss the methodology employed in such calculations or to doubt the results obtained (for a critical discussion see Pasour and Johnson 1982).

It is the second step that is the critical one: Is it reasonable to assume that returns to investment in agricultural research made today or tomorrow will be as high as they used to be in the past? The answer to that question will depend on an assessment of the performance in the future of the agricultural research system as a whole. The performance of a system depends on the performances of its elements, in our case on the performance of researchers and research groups, and on how the system is organized and managed.

In this paper I explore the consequences for research organization and management of two perspectives of agricultural research. In particular, after a brief description of the extent of agricultural research world-wide, I first present a conventional perception of agricultural research and discuss its limitations. I then sketch a problem-solving perspective of research that encourages the use of methods developed in other disciplines for managing and organizing research. Examples of the applicability of these methods are presented and suggestions for further research on research management conclude the paper.

A SYNOPTIC VIEW OF THE AGRICULTURAL RESEARCH SYSTEM

After the demise of the British and French colonial empires in the early 1960s, agricultural research has become a flourishing international industry. A detailed, quantitative description of the industry has been provided by Pardey et al. (1991) and only a brief overview is required here.

Components and size of the system

The major elements of the international agricultural research system and their relationships are the national agricultural research systems (NARS), which comprise public as well as private research institutions, universities and non-agricultural research institutions that provide the trained personnel and the basic knowledge and research methods for agricultural research, and a cluster of international agricultural research centres (IARC), of which the institutes franchised by the Consultative Group of International Agricultural Research (CGIAR) are perhaps the better known ones (Fig. 1).

[Figure 1: The international agricultural research system]

Many of the national and international research institutions are linked to each other either through formal networks or cooperative agreements or, perhaps more effectively, through personal contacts of their research staff. Similarly, national agricultural research systems maintain linkages to the extension services, which may be institutionalized, as is the case, for example, in the United States, or be largely left to the initiative of researchers and extension personnel, as is the case in Germany.

Measured in terms of numbers of agricultural researchers, the size of the system has probably tripled from some 60,000 researchers in the early 1960s to an estimated 180,000 researchers in the early 1990s (Fig. 2). Much of that growth has occurred in low-income countries so that two out of three agricultural researchers are today working in a low-income country.

[Figure 2: Development of the number of agricultural researchers, 1961-65 to 1991-95]

Although Pardey et al. (1991) report a total of 152 countries with agricultural research systems, many of the systems, particularly those of low-income countries, are too small to be considered as viable independent systems. More than half of the NARS in low-income countries comprise 125 researchers or less and only some 20 NARS have grown to a considerable size with more than 700 research personnel (Fig. 3).

[Figure 3: Distribution of LDCs by their number of agricultural research personnel, 1981-85.]

A further indicator of the concentration of research is the share of the largest 15 agricultural research systems which account for 56% of all agricultural researchers in the world. And even among the 15 largest systems, the size distribution shows a pronounced L-shape (Fig. 4).

[Figure 4: The Top-15 agricultural research systems, 1981-1985.]

Private and public research

Agricultural research, particularly research towards the basic end of the research spectrum, was the reserve of the public sector, because research that has no immediate application would be conducted on a sub-optimal scale by the private sector. The argument puts considerable faith in the ability of governments to do something optimally, with the optimum defined in a narrow economic sense. The faith receives little support from theory, neither in general (Wolf 1988) nor in the specific case of agricultural research (Pasour and Johnson 1982).

Furthermore, if accepted as being correct, the high rates of return to research would suggest that governments also tend to under-invest in agricultural research. Nevertheless, whether supported optimally or not, much or even most research used to be funded out of taxes in the high-income countries (Ruttan 1982) and out of aid in the low-income countries.

The division of labor between the private and the public sector that prevailed in the past is unlikely to persist into the future. First, biotechnology research is likely to maintain a strong base in private industry. Second, international initiatives under the Uruquay round of GATT will strengthen private industry's ability to recoup its research investment and remove a hurdle to the international transfer of research results produced in the labs of private industry.

Furthermore, with increasing regulatory constraints on production agriculture, transnational suppliers of farm inputs are likely to see their long-term growth opportunities not in the high-income countries, but in low-income countries with a broad agricultural base and sufficiently stable governments and economies. Finally, in several low-income countries private agricultural research activities are emerging (Pray and Echeverria 1991) and private companies tend to transfer some technologies more effectively than the public sector (Pray and Echeverria 1989; Pray et al. 1991).

Research and extension

Some technologies require for their diffusion determined efforts by extension services and public extension services continue to exist even in countries where most technologies are propagated through the marketing and consultation services of the private sector. In comparison to the distribution of researchers, the world's public service extension workers are even more heavily concentrated in the low-income countries (Judd et al. 1991). Western Europe, North America and Oceania together accounted in 1980 for about one third of all public sector researchers in the world outside the former empire of the USSR and outside China, but only for 15% of the extension workers. Africa, in contrast, has a much higher share of extension workers than scientists, whereas the shares in extension workers and scientists were about equal in Latin America and Asia without China.

[Figure 5: Regional distribution of research and extension staff, 1980.]

The considerable growth in the agricultural research system since 1960 did not translate into a similar growth of public extension personnel. Starting from a high ratio between the number of extension personnel to number of scientists, which was about 8:1 in the low-income countries in 1960, this ratio dropped to 2.4:1 in 1980 for the world without Eastern Europe and USSR. As can be expected, the ratio is still higher in low-income countries, where it was about 4:1 in 1980, than it is in the high-income countries, where it is not unreasonable to expect today a ratio of 1:1 or less.

THE CONVENTIONAL KNOWLEDGE HIERARCHY: CONSUMPTION, PRODUCTION, INVENTION, AND SCIENCE

Agriculture has become a highly knowledge-intensive industry which draws on a considerable store of general or scientific knowledge as well as on an unfathomable store of widely dispersed specific or practical knowledge. The growth of agricultural knowledge requires a division of knowledge among specialists, the coordination of the specialists, and the organization of the flows of knowledge among the specialists. The result of differentiation and coordination is a complex knowledge system that has spontaneously evolved with the development of production agriculture.

A conventional description of the agricultural knowledge system can be developed by integrating science into a model suggested by von Weizsaecker (1981) who considered invention in relation to production and consumption.

Consumption, production, and invention

Some old fashioned textbooks of economics draw a picture of a world consisting of only two spheres, consumption and production, and knowledge is not considered explicitly for either sphere. If believed literally, such old-fashioned textbooks could not have been printed but would have had to be hand-copied by hordes of scribes. Surely, the existence of invention has not gone unnoticed in economics and invention is usually defined as "...the human activity directed toward the creation of new and improved practical products and processes" (Nelson 1959, p. 299).

Inventions may, but need not require systematic cognitive knowledge. Many inventions, in agriculture as in other industries, have been made not by people who would be considered as being particularly knowledgeable, but who either through some flash of serendipity or through patient and tenacious trial-and-error happened to stumble on a useful solution to a practical problem. Although some inventors may be lacking in cognitive knowledge, their knowledge of practical problems must nevertheless be substantial or else they could not have noticed that they had arrived at a solution.

Science

Where is the place for science in a world comprising consumption, production, and invention? Science is the human activity directed at the advancement of knowledge either by means of providing facts observed in reproducible experiments or collected in some other reproducible way, or by means of developing theories about the relationships among the observed facts (Nelson 1959).

Acknowledging the existence of scientific activity does not require the explicit introduction of a science sphere into our description. If science is conceived as an activity that satisfies human curiosity and desire for knowledge, then we can subsume science among the consumption activities because there is no reason here to distinguish between consumption by the stomach and consumption by the brain.

Such a descriptive model of a knowledge system would have been adequate more than a century ago when science was indeed an activity performed predominantly by highly educated men not for material purposes but for the intellectual betterment of themselves and of man-kind. Since the end of the last century, however, science has become a growing industry that produces knowledge employed in systematic invention and for the acceleration of the rate of inventive output (Rosenberg and Birdzell 1986). In agriculture, as in other industries, the growth and extent of the science industry alone is sufficient evidence that agricultural science is not performed for immediate intellectual consumption but for its potential and intermediate contribution to the extension of material consumption.

Frequently the subdivision of science into such categories as basic, strategic, applied, and adaptive science, or similar categories is suggested. The categories are usually distinguished on the grounds of the motives or objectives of the research, e.g. when research is intended to have practical applications, it is applied research, and when no such intentions exist it is basic research. It is not uncommon that the categories are then taken as the basis for structuring the division of labor in research, that is for organizing research (e.g. Bonte-Friedheim 1992). The distinctions are difficult or impossible to make at the outset of a research project and they are therefore of no use for the characterization of researchers and for organizing research. The distinctions are difficult to draw because the categories describe a continuum rather than a sequence of distinct classes and many classifications must be arbitrary. Furthermore, as Mowery and Rosenberg (1989) have pointed out, the unpredictability of research often plays havoc with the intentions that led to the conduct of the research and a research project that was classified as "basic" at its initiation may well end with an immediate application, or applied research may be the entry point for basic research. Finally, the intentions of the researcher may be quite different from the intentions of the organization by which the researcher is employed, and it is not clear whose intentions should count. For these reasons Mowery and Rosenberg (1989, pp.13-14) conclude: "The attempt to classify research into basic and applied categories is hard to take seriously in some areas and disciplines, for example, in the realms of health, medicine, and agriculture." Rather than worrying about such distinctions, agricultural research may be better served by following Pasteur's dictum that there is no pure science and applied science, only science and the application of science.

To complete the description of the system, the linkages between the spheres or subsystems need now be considered.

Linkages between the spheres

Having identified the spheres of the agricultural knowledge system, the links between the spheres or subsystems must also be described. The linkages between the spheres are of two kinds. First, there are flows of outputs from one sphere that become inputs for an adjacent sphere. The flows of outputs are then mirrored by imputed values of the output flows that point into the opposite direction.

Input/output linkages: Considering the flows of outputs first, consumption is a useful starting point as it is the most basic of all human activities and produces no material outputs. Consumption, however, is limited by the available stocks of consumables. These stocks would be quickly exhausted if they were not replenished with the outputs from production. The supply of consumer goods from production is achieved by transforming various inputs using some production technology.

Spaceship-earth views of the world, however, keep reminding us that the supply of production inputs is also finite and that production processes have to be made more efficient, that substitutes may have to be found, or that by-products that were regarded as waste are turned into goods desired by consumers, if we want to prevent shrinking of the supply of consumer goods.

The output link between the science and invention spheres is not of the same nature as that between the other spheres. As the distinguishing target group for scientific discoveries is the society of peers and not the inventors, the maintenance of the flow of knowledge from science to invention is not an obligation of the scientist but of the inventor who may tap into the communications that define the science community. To do that effectively, researchers from the invention sphere must be familiar with the communication practices in the science sphere, i.e. the journals, conferences, specialized language and concepts, whereas the scientist has no obligation to deliver scientific knowledge in the inventor. As a result, the continued flow of knowledge from science to invention is not as assured as are the flows of inventions to the production sphere and of products to the consumption sphere.

The value chain: The flows of outputs from one sphere to the next are mirrored by imputed values that flow in the opposite direction. The value chain is anchored in the consumption sphere because consumption is the primary source of value from which the value of production is derived.

Invention, just like production, also is of no direct value as its value is only indirectly derived from consumption via the intermediate value of production. Science, finally, is thrice removed from the source of value, and its value is derived from the value of invention. The implications of the value chain for the value of the outputs from the spheres are easy to see: scientific knowledge that is not used for invention is of no value, and inventions that are not used in production are of no value, just as products that are not consumed must be considered as useless waste.

The value chain connecting the four spheres is imaginary in nature but has a manifest correspondence in reality where markets for the outputs from the spheres exist. Such markets nearly always exist for consumption goods and market prices of consumption goods are, under certain circumstances, excellent indicators of the value of consumption goods. The flow of receipts from market sales then provides the remuneration for production resources and the information required for avoiding waste and attuning production to consumption needs.

Market prices also guide invention where the invention is incorporated into a physical product, such as agricultural machinery, seeds, pesticides, or fertilizers. For inventions, however, that consist of new knowledge and information alone, such as advice about improved farming or management practices, markets do not always exist and invention cannot be fully remunerated and guided by the market. Science, which produces only knowledge and information that is not incorporated in physical products also cannot be remunerated and guided directly by markets for its outputs.

Where markets for outputs do not exist remuneration of the activity leading to the outputs is separated from the valuation of the outputs by their users and auxiliary means for guiding the direction and intensity of the activity must be found. Agricultural research is therefore usually funded by governments or large companies and guiding the direction of inventive and scientific activity is a central problem of research management, be it in the private or public sector.

Limitations of the description and an alternative

The conventional hierarchical perception of the place of science in agriculture has its main value in characterizing the governance problem of scientific research but it fails to suggest solutions to the problem. Furthermore, where this model, which has also been irreverently called the "sausage-machine mentality" of research (Grady and Fincham 1991), is used as the basis for organizing and managing research, the results of applied scientific research are disappointing. In sausage-machine research organizations the scientists tend to be removed from both inventors and farmers, research institutions are considered as cost centers and not as service centers delivering something of value, funding is determined on the basis of funding in the past, cost control is emphasized because output control is largely absent, and the mission of the research institution is only vaguely perceived. It is then no wonder when scientists often drift off into intellectually stimulating but practically useless research.

The main deficiency of the hierarchical model is its failure to recognize that all spheres are embedded in an agricultural knowledge system with dispersed specific, and often tacit, as well as general, and often explicit, knowledge (Bunting 1985). The importance of specific, practical knowledge is easily overlooked as it is often tacit and useful only in special circumstances. But a few examples may remind us of the importance of such knowledge. Early explorers of Australia would not have been helped by textbooks on human nutrition but they certainly appreciated being taught by aborigines how to survive on a diet of witchetty grubs washed down with a squashed desert frog. Furthermore, if specific, practical knowledge was not important, mothers would not pass on to their daughters recipe books inherited from the daughter's grandmother, and courses at mechanical institutes or colleges of vocational training would not be attended by apprentices and jilleroos. Consumption and production both require considerable knowledge, although that knowledge often is of a specific kind and may not always be amenable to symbolic representation and communication.

For lack of a better visual metaphor, the agricultural knowledge system may be better described as a knowledge soup with lumps rather than as a tidily layered pyramid. The knowledge soup metaphor removes the shackles imposed by the hierarchical view, which allows flows only between neighboring higher and lower levels, and it explicitly acknowledges the existence of specific knowledge and the concentration of that knowledge around the farming and consumption spheres. To serve as a guiding principle for organizing and managing research, the knowledge soup metaphor must, however, be amended by a common model of the activities in the four spheres. Such a model will now be developed exemplarily for the research sphere.

RESEARCH AS PROBLEM SOLVING

The task of a scientist comprises two processes: the discovery of solutions to problems and the testing of the solutions. Whereas the sciences have developed elaborate methods for testing solutions to scientific problems, the philosophy of science is mute about how scientists arrive at the solutions that are to be tested. Langley et al. (1987) have argued that the same conceptual models used for decision making and problem solving that have been found useful in domains outside research can also serve as a model of scientific discovery. The practical implication of that assertion is considerable. It invites the application to scientific discovery of proven techniques for decision making and problem solving. Such techniques, which have been called "intellectual technologies" by Bell (1973), have been and continue to be developed by several branches of science, but particularly by operations research, management science, and recently also by the computer sciences.

Problem solving as heuristic search

From the various definitions of the term "problem" suggested in the literature, Smith (1988) has extracted three important characteristics. For a problem to arise, there must exist a gap between a state (actual or anticipated) and a desired state, the gap must be difficult to close, and the gap must be sufficiently important to attract the attention and inspire the activity of the problem solver. Being a "disharmony between reality and one's preferences" (Smith 1988, p. 1491) a problem cannot exist independently of the desire or goal of somebody and problems are not objective entities in their own right (Dery 1983).

The widely accepted heuristic search paradigm of problem solving then describes the discovery of problem solutions as goal seeking in a maze or problem representation created by some information-processing system (e.g. the brain or a computer program) and where the search is guided by heuristics (Langley et al. 1987). The type of heuristic that can guide the search depends on the nature of the maze or problem representation. Powerful task-specific heuristics, such as definite solution procedures, can be used for well-structured problems that come with a definite criterion for testing the solution and that provide sufficient informational clues for the application of knowledge and search heuristics. Weaker general purpose heuristics have, however, to be applied for the search through ill-structured mazes with few or weak informational clues and with vague goals and criteria for testing solutions.

Agricultural research as vicarious problem solving

The discovery of problems and solution in agricultural research is distinguished from problem solving in general by its processes for generating problems. Whereas the general theory of problem solving does not distinguish between somebody who has a problem and somebody else who contributes to its solution, agricultural research is often vicarious problem solving where the researcher contributes towards solving the problem of somebody else. The vicarious problem solving characteristic is clear in the case of agricultural invention because an invention cannot be regarded as a solution unless the intended adopters of the invention have a problem. In agricultural science, however, not all problems are of the vicarious kind. Agricultural science also produces its own problems, such as the accidental discovery of phenomena that require explanation, the exploration of the implications for agricultural science of discoveries made in other sciences, or the recursive problem generation in endeavors of exploring the cause and effect chains of natural processes. Although there is no reason to doubt the legitimacy of scientific research on problems generated by agricultural science itself, such problems are not further considered here.

Furthermore, vicarious problem solving in agricultural research is mostly concerned not with the problems of individuals but with discovering solutions for types of problems held by a number of people from some identified clientele. For such problems to exist, the clients must have similar, but not necessarily identical goals and similar environments determining the gap between the current and the desired state. The researcher as vicarious problem solver must then include in the problem representation the often ill-defined range of clients' goals and the diversity of their environments. Whereas a richer representation renders search more cumbersome but does not necessarily affect the structure of the maze, loss of goal definiteness weakens the structure of the representation and renders search guided by strong task-specific heuristics less successful.

As an example for the effect of complex goals on the structure of the maze and the heuristics for search, consider breeding of a new variety for use in monocultures of commercial farms in different natural environments. Breeders could well apply their strong task-specific heuristics to solving the yield problem of farmers in a single environment because the structure of the problem does not change across environments. The main difference would be that the size of the task is increased as more screening and evaluation experiments have to be conducted in the different environments. The structure of the breeding problem would, however, be considerably changed, if the new variety was to solve a yield problem of commercial farms as well as a yield-stability problem of subsistence farms. In that situation breeders would have to apply different, and often less powerful, search heuristics and evaluation criteria. A further example is the difficulty of representing problems that are not held by people individually but in common by a group of people, such as problems of overgrazing of pastures or depletion of groundwater tables. Representations of such problems are always complex and, as the experience with many proposed solutions shows, most of our conventional strong heuristics fail to discover acceptable solutions and weaker, general purpose heuristics must be applied.

Problems and the research agenda

In vicarious problem solving the problem solver is separate from the agent having the problem. How does the agricultural researcher as problem solver become aware the problems of clients, and which of the problems are put on the agenda of research problems?

Some research problems are generic problems of a particular discipline that are routinely addressed by research. Examples for generic problems are the estimation of the costs of production of major farm enterprises by economists, maintenance research in crop breeding, or monitoring of insect hot spots by entomologists. Demand for the results of such research tends to be assured because of continuous changes in the farming environment. No specific information is required to initiate generic research, but information may be periodically required to determine the scale of such research. If such information is not obtained, generic research tends to be continued although the problem that lead to its initiation exists no more. An example of generic research that was perpetuated beyond the existence of the clients' problem is research on stem borer control in sorghum. Once entomologists had adopted stem borers as a research problem they continued to hone stem borer control measures although farmers neither considered stem borers to be a major production constraint nor were the controls suggested by the researchers adopted by farmers (Nwanze and Mueller, 1989).

In contrast to generic problems, specific research problems are only put on the research agenda if some researcher becomes aware of the problem, chooses to address the problem, and translates the clients' problem into a feasible research problem. The problem is not put on the agenda if any of these functions fails.

A major cause of failure of the awareness function is the necessity to concentrate researchers in research institutions. The informational separation of researchers from their clients which concentration engenders can, however, be overcome. For example, research institutions may be geographically decentralized beyond what would be cost effective or administratively convenient, continued contact with farmers may be established by periodic monitoring of a panel of farm households (Binswanger and Ryan 1980), researchers may be mobilized and encouraged to undertake farm reconnaissance tours, or farmers may be granted some representation on research-steering boards.

Introspection is a reasonably good guide for illuminating the reasons why a problem of which one has become aware is not addressed. For example, the problem may be from a problem domain outside one's own area of specialization, attempts to do research on similar problems may have failed in the past, or the problem may not promise sufficient intellectual challenge. Some of the reasons for not addressing a problem are rooted in the nature of the problem, others in the self-interest of the researcher. The question is whether the selfish reasons are illegitimate or not. The issue is related to questions of freedom in research and the question whether research should be driven by needs or by science. A clear case against selfish problem selection and for needs-driven research could be made if the success of research problem solving was independent of the motivation of the researcher. Experience suggests, however, that success and motivation are intimately related through the researcher's creativity and serendipity, both inputs to discovery that are only imperfectly controlled by the researcher and that can therefore not be controlled by the researcher's principal. Where researchers' interests are not aligned with research needs, little will be achieved by imposing research problems onto researchers unless the interests of the researchers have been aligned with the research needs.

Because a problem is not an objective entity but a disharmony between somebody's preferences and the external environment, a client's problem must be translated into a research problem, i.e. a feasible research problem must be defined. The translation may be difficult or impossible if the researcher lacks specific knowledge about the client's preferences, or the environment, or both. In general, lack of knowledge about the environment is the smaller problem in problem translation because the external environment of the client can be observed, whereas client's preferences cannot be observed directly but must elicited from the client or inferred from observations of the client's behavior. Problem translation is particularly difficult if the client's problem is based on a perceived disharmony between abstract social preferences and the environment, as is the case, for example, in some sustainability and ecological problems. For such problems, considerable effort may have to be spent on goal clarification before the client's problem can be successfully translated into a feasible research problem.

Problem definition or representation

Just as the problem is not given to the researcher but must be translated from the client's problem, the problem representation, or "...the scheme for holding information in memory together with the processes for operating on that information to access it, alter it and draw inferences from it" (Langley et al. 1987, p.315), also requires elaboration. Smith (1989) suggests several important components for inclusion in problem representations, such as the relevant stakeholders of the research and their goals, externalities where they are important, crucial variables and relationships, as well as hypotheses for further study of the problem.

Problem representation requires considerable information about clients, their goals and environments, as well as considerable knowledge and information from memory about how to organize the information so that it can be operated upon by the researcher. The creative task of the researcher is to develop representations that suggest possible solutions. The temptation for the researcher is to match the representation to his or her problem solving skills at the risk of searching effectively within the wrong maze. Examples of adjusting the problem representation to the skills of the researcher abound. Farm management economists have been prone to ignore externalities that would challenge their problem solving skills, breeders have tended to represent crop production problems as yield level problems and disregarded crop characteristics of value to housewives, such as taste or ease of processing, and relevant stakeholders of the research, such as farm laborers, nearly always are omitted from researchers' representations of farm production problems because their goals, if in conflict with the goals of the research clients, would lead to a more complex problem representation.

Drawing on multiple sources of knowledge and skills

Many agricultural problems are too complex to be solved by specialists from any one discipline. When the maze defeats the heuristics of disciplinary specialists, the problem may be considered as ill-structured and requiring the application of general purpose heuristics. Examples of general purpose heuristics are the "progress principle", which suggests the use of a measure of progress towards the goal, "problem splitting", which involves finding a way of splitting the problem into simpler ones and solving the components separately, or "means-ends analysis", where the problem solver's memory is searched for actions that are likely to lead toward the goal (Minski 1988; Simon et al. 1987).

Problem splitting is the general heuristic that gives rise to multidisciplinary research and allows the specialists to apply to problem components representations suitable for specialized powerful search heuristics. The difficulty with multidisciplinary research is to find a split of the problem in which the nature of the disharmony between the clients' preferences and environment is not lost but which allows the specialists to make best possible use of their problem solving skills. Too often, however, the problem is split such that the component problems can be readily solved, but where the sum of the component solutions does not yield an adequate solution for the original problem.

Means-ends analysis may be a grossly underutilized heuristic in many agricultural research organizations. Too often, only the memories of the researchers belonging to a particular organization are searched for potential solutions and other potentially accessible memories are left untapped. With the rapidly falling costs of digital storage devices and the extension of computer networks, several agricultural organization are developing digital memories together with the means for accessing them (e.g. the CABI, AGRIS, AGRICOLA, and FIS-ELF databases of CAB, USDA, FAO, and ZADI, respectively). It is therefore reasonable to expect that means-ends analysis will become a much more powerful heuristic in the future.

Test criteria in science and invention

Although there is no conceptual difference between the discovery of solutions in agricultural science and invention, science and invention are clearly distinguished by their criteria for testing problem solutions. The purpose of scientific research is to discover generalized knowledge about some aspect of reality and the veridicality of discoveries is tested according to standards set by the community of scientists working in that particular area of learning.

The primary purpose of invention, in contrast, is not to arrive at valid new knowledge, but to discover solutions to problems of agricultural producers. Such solutions may but need not be tested for their veridicality, but they ought to be tested against criteria that reflect the usefulness of the new solution to solve some specific problem of agricultural producers. Hence, it is the criteria for testing that distinguish science and invention.

Apparently, the purposes of testing research discoveries are often confused. An example is the continued neglect of risk considerations in the design and testing of research. Experiments that have no other purpose than the invention of a new agricultural technology or product are routinely designed and tested according to standards and criteria accepted in science, such as the conventional statistical significance tests. But rarely are the experiments designed to produce results that can also be tested against criteria that reflect the risk behavior of farmers, although suitable methods have been suggested in the research literature and are occasionally applied (Dillon and Officer 1971; Mazid and Bailey 1992).

INTELLECTUAL TECHNOLOGIES FOR ORGANIZING AND MANAGING AGRICULTURAL RESEARCH

The motivation for describing research as problem solving is to encourage the application of intellectual technologies to problems of research management and organization, and to provide some heuristics for selecting suitable technologies. Such technologies would hardly be required if markets for research outputs could be relied upon to guide and coordinate research. In the absence of such markets, however, research managers must forge their own devices for guiding and coordinating research. Space limitations do not allow me to survey the intellectual technologies that have already been applied in agricultural research management and examples, one for each, of a misapplication, a success, and a promising new development must suffice.

Misapplication: Formal methods for determining research priorities

Considerable effort has been spent in the recent past on the design of formal methods for determining research priorities and selecting research projects, and even more effort went into debating the pros and cons of various candidate techniques (e.g. Anderson and Parton 1983; Contant and Bottomley 1988; Mueller 1990). The intensity of the application of such techniques is not commensurate with the intensity of the debate. A recent survey among some 374 agricultural researchers in Germany found that formal methods, such as congruence analysis, scoring, mathematical programming or systems simulation are not among the procedures routinely used by researchers to prioritize their research (Irmen et al. 1992). Similarly limited use of formal methods for prioritizing and selecting research projects was also found by Cardoso (1992) among 152 British organizations involved in R&D. This research indicated that few research managers base their project selection on formal methods, but that a considerable number of managers nevertheless used the techniques to support and defend their choices.

The empirical evidence of the actual use of formal methods for prioritizing research is not surprising when prioritizing research is considered as a problem solving activity. The choice of a research project typically is an ill-structured problem because of the uncertainty about alternative courses of research and because of the risk associated with the outcomes of research. Furthermore, the researcher charged with the choice may not be fully informed about the goals that the organization pursues. Most formal techniques, however, have been designed for well-structured problems with definite objectives and for which precise quantitative data are available. Given the considerable discrepancy between the nature of the problem and the requirements of the formal methods, decision makers are understandably loath to substitute formal rigor for their intuition and judgment.

Success: Managing multidisciplinary research groups

When complex problems have to be split into subproblems to which disciplinary specialists apply knowledge and heuristics from different research paradigms, some undesirable consequences of paradigmatic variation in multidisciplinary teams require management attention. Paradigmatic variation may diffuse the focus of research because of "classificational thinking", which leads people to perceive their own specialized problem focus as always valid and applicable (Maruyama 1974), and different research paradigms may impede the communication between the specialists (Petrie 1976). Various procedures have been proposed to overcome the disadvantages of paradigmatic differences whilst preserving gains from specialization. Swanson (1979) summarized four: (i) common group learning, (ii) negotiation between experts, (iii) modeling, and (iv) integration by a leader. Maruyama (1974) suggested interdisciplinary research with a holistic systems view, initiation of cross-paradigmatic processes that aim at mutual understanding of paradigms, and the initiation of transparadigmatic processes for the creation of new paradigms.

The choice of a means for overcoming paradigmatic differences in a team also depends on how long the team can be expected to exist. In problem-oriented agricultural research, teams may have to be assembled at short notice and for periods too short to warrant the use of more demanding procedures for integrating team members. An intellectual technology that can be readily applied to structuring and focusing communication in multidisciplinary research teams is the analytic hierarchy process (AHP). This general-purpose decision making technique has found a number of applications in multidisciplinary research settings, including the choice of cropping experiments (Anders and Mueller 1990), the selection of a research portfolio at ICI (Lockett et al. 1986), and planning the research activities of a fisheries research center (Dyer and Forman 1992).

Promise: Hierarchical organization

Institutionalized coordination, or organization, is necessary where gains from specialization are to be reaped. Yet nearly all organizations develop into hierarchical sledge dog teams where only the lead dog has a change of scenery. It is then no wonder that many agricultural researchers are either suspicious of hierarchical organization in research or resent it openly.

Hierarchies have several characteristics and not all are detrimental to problem solving in research. The detrimental ones have their cause not in the hierarchy itself, but in the distribution of power within the hierarchy. When power is concentrated at the pinnacle of the hierarchy, organizational flexibility tends to be lost and conformity is preferred to creativity. Power hierarchies in research deserve their poor reputation as their success in research is generally disappointing (Rosenberg and Birdzell 1987).

Hierarchies, however, provide stability (Simon 1981) and they are effective arrangements for processing information. Radner (1992) has shown that for given amounts of information to be processed and for a given number of information processors, hierarchies minimize the delay in information processing. However, in contrast to the inflexible and balanced hierarchies with approximately equal numbers of agents reporting to a principal at the next higher level, efficient information processing hierarchies are neither balanced nor must they be rigid if the amount of information varies. Radner's (1992) insights provide support for flexible research organizations that organize specialists in teams or task forces to solve specific problems, rather than organizing them in disciplinary research groups, where the tendency is to attend to generic rather than specific research problems.

CONCLUSIVE SUGGESTIONS

I have begun my paper with the question whether it is reasonable to assume that investments in agricultural research will continue to yield high returns. I follow the good practice by investment bankers not to offer definitive answers to such questions. I believe, however, that the vast island empire of agricultural research (Mayer and Mayer 1974) can ill-afford to remain isolated from mainstream research on research management and organization. The danger is real, as many publications on agricultural research management that lack any theoretical basis indicate.

I hope that more cross-fertilization between different strands of research on research management will occur. What is needed are research management methods that assure better representation of farmers' perspectives of their problems in research, and that warrant effective use of scarce research resources, including the effective use of researchers' time and creativity. And in spite of the current concerns about sustainability and the threats to the environment, agricultural research and its management should be reminded that "the decisive factors of production in improving the welfare of people are not space, energy, and cropland; the decisive factors are the improvement in population quality and advances in knowledge" (Schultz 1981, p. 4).

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Figure 1: The international agricultural research system

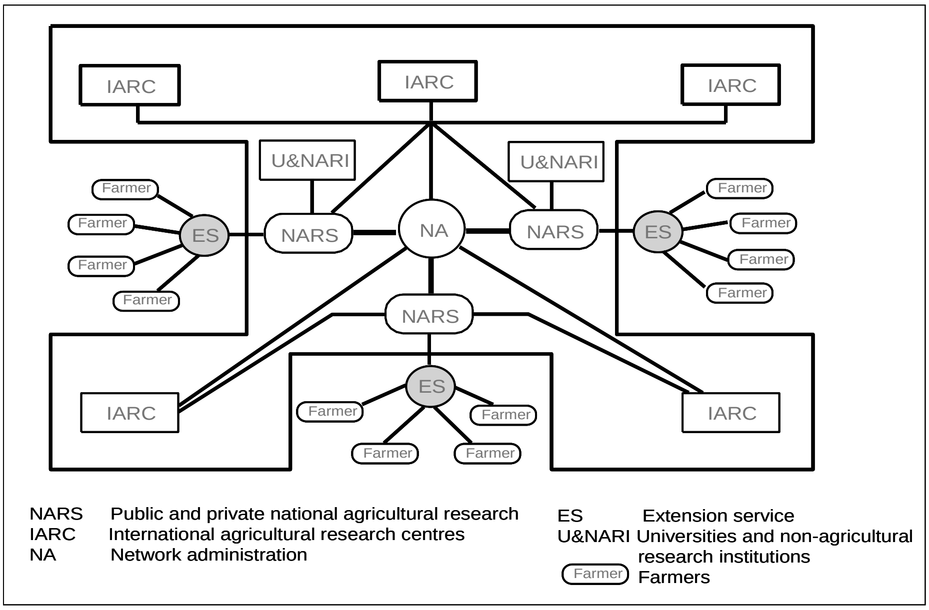

Figure 2: Development of the number of agricultural researchers, 1961-65 to 1991-95

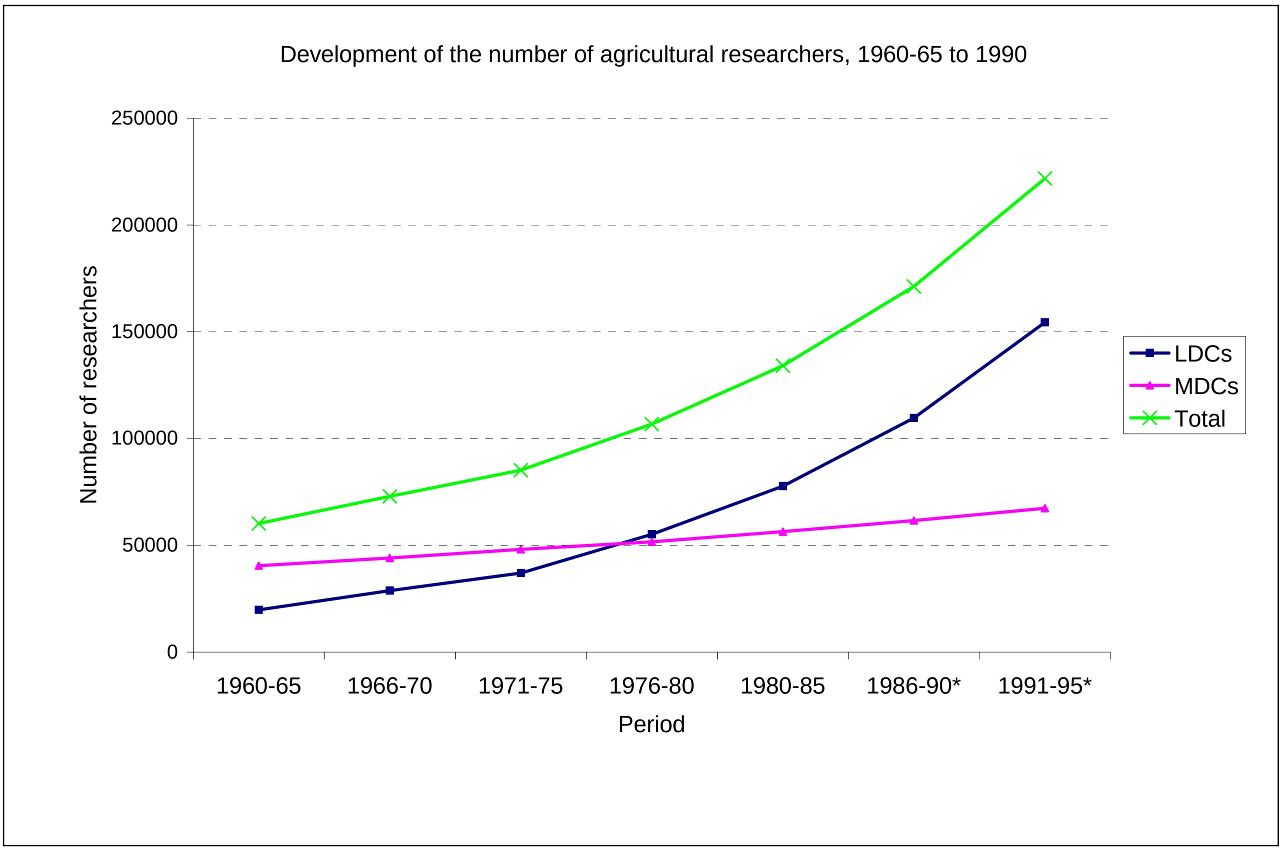

Figure 3: Distribution of LDCs by their number of agricultural research personnel, 1981-85.

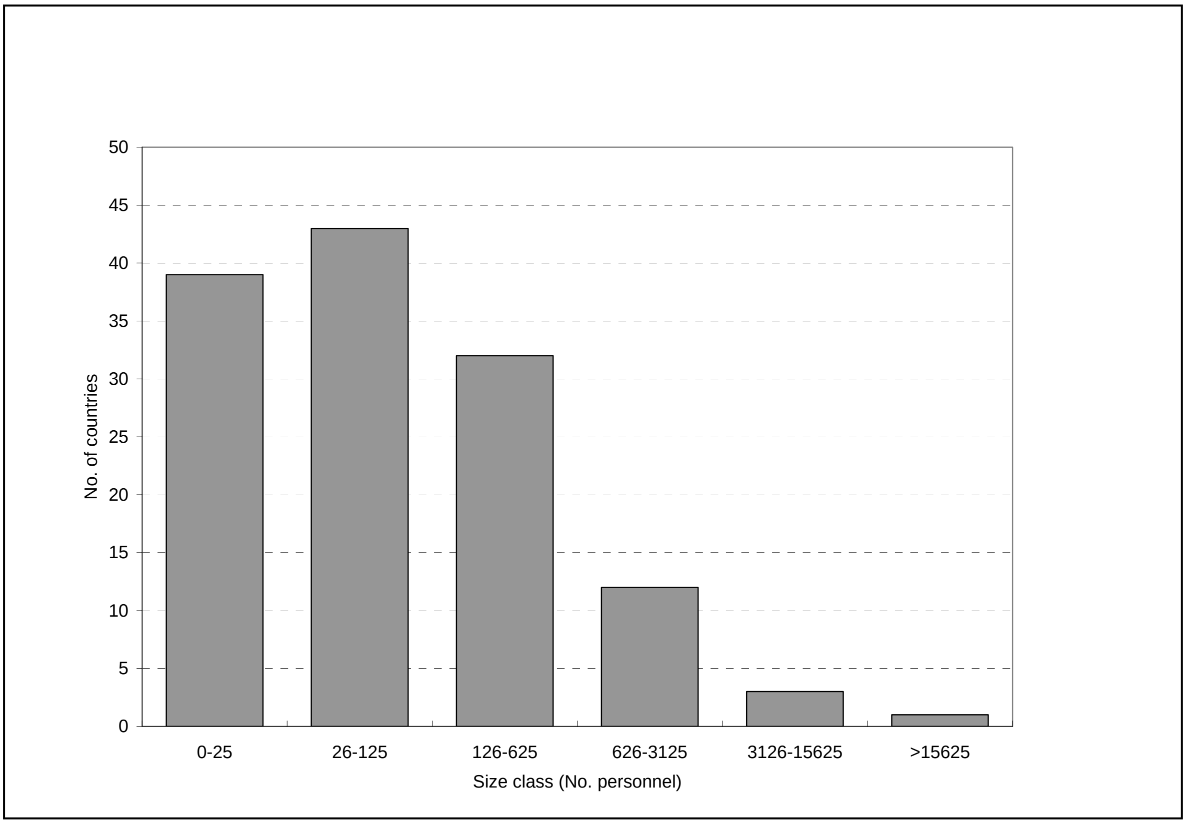

Figure 4: The Top-15 agricultural research systems, 1981-1985.

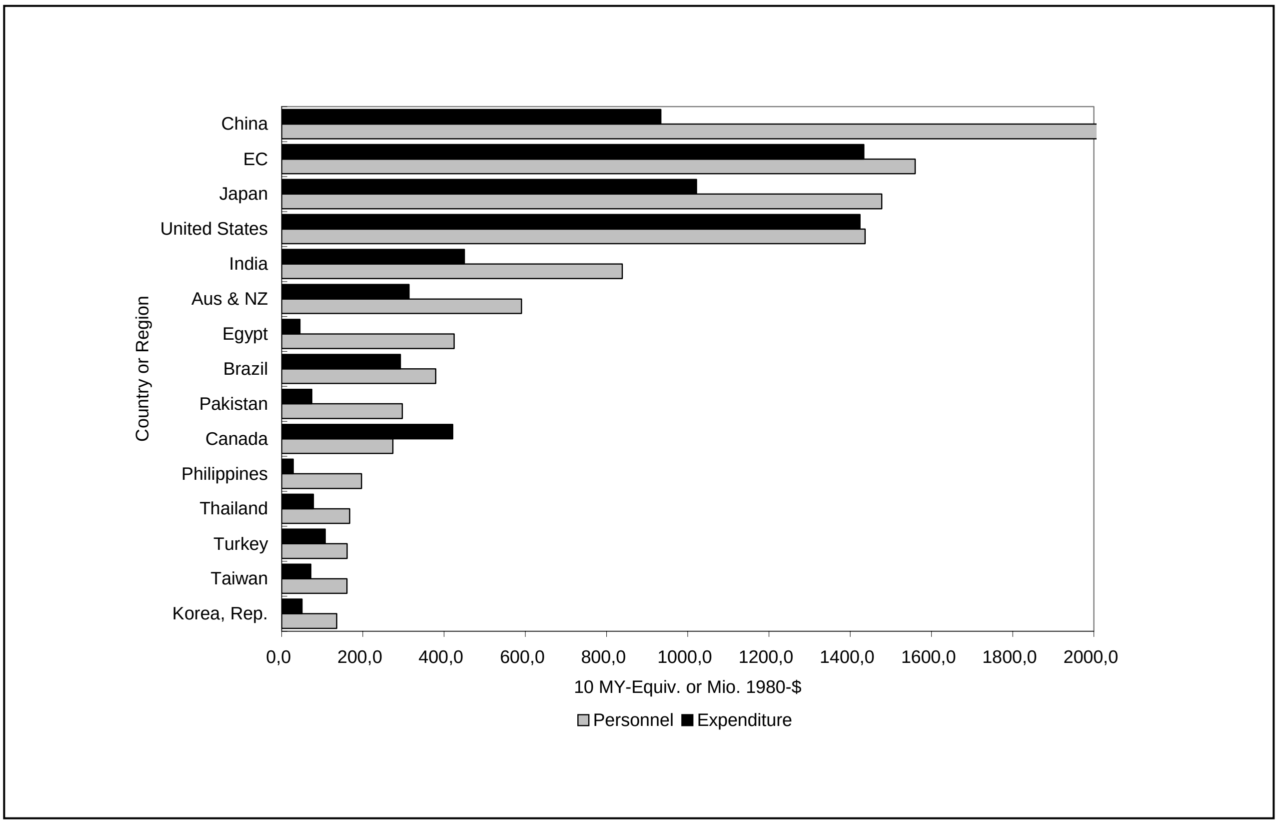

Figure 5: Regional distribution of research and extension staff, 1980.]