REPORT OF AN EXPLORATORY WORKSHOP ON

The Role of Anthropologists and Other Social Scientists in Interdisciplinary Teams Developing Improved Food Production Technology

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Foreword

Whatever influence the international agricultural research centers (IARCs) have had on food production by small farmers in developing nations has been due largely to the improved crop varieties and technologies developed by their physical and biological scientists, and, in recent years, to the inputs of agricultural economists. But we increasingly recognize that factors relating directly to the farmer, his family, and his community must be considered if the full effects of agricultural research are to be realized. This recognition has come partly from the participation of anthropologists and other social scientists in interdisciplinary teams at several of the IARCs during the past few years.

The Exploratory Workshop on the Role of Anthropologists and Other Social Scientists in Interdisciplinary Teams Developing Improved Food Production Technology, held at IRRI 23-26 March 1981, was an outgrowth of these early projects. The six discussion papers reproduced here give preliminary results obtained. Using these discussion papers as background, workshop participants considered the implications for future interdisciplinary approaches and explored the strengths and limitations of anthropology and related social sciences for interdisciplinary work in developing improved food production technology. From the workshop discussions came policy recommendations in four areas:

- training social scientists for interdisciplinary work,
- identifying problems adapted to interdisciplinary teams of social scientists and natural scientists,
- defining roles for social scientists other than as members of interdisciplinary teams, and
- defining ways in which research institutions can make the most effective use of the skills of social scientists.

It would be inaccurate to say that consensus was reached on all points. Yet I believe that the workshop discussion summaries and the policy recommendations accurately reflect the concerns of the several disciplines represented. For an exploratory workshop, perhaps that is enough. I hope that anthropologists, social scientists, biologists, administrators, and policy makers will continue to forge interdisciplinary teams in our common effort to solve the agricultural problems of small farmers in the developing world.

The organizing committee for the workshop was itself interdisciplinary. It included, from IRRI, Grace E. Goodell, anthropologist (chairman); John C. Flinn, agricultural economist; and James A. Litsinger, entomologist; and from the University of the Philippines at Los Baños, Gelia T. Castillo, sociologist. William H. Smith, assisted by Gloria S. Argosino, edited this volume. I am grateful for their efforts.

Nyle C. Brady
Director General

Interdisciplinary development and transfer of postharvest technology at the International Potato Center

Robert Rhoades, Robert Booth, Roy Shaw, and Robert Werge

The issue before us in this exploratory workshop is: Can anthropology contribute to the generation and transfer of improved agricultural technology? If it can, what is the input of the anthropologist into these processes and can any cases showing the anthropologist's positive role be documented?

Anthropologists and other social scientists have often been labeled, and rightly so, as after-the-fact critics who study and report cases where change agents or technology designers have gone wrong in social, cultural, or economic terms. Many biological scientists are sensitive to the fact that new technology must be socially and economically relevant, but the 20-20 hindsight of social scientists has generally left them skeptical. Of course, one problem has been that anthropologists and sociologists, unlike economists, have rarely worked in agricultural organizations. In the international agricultural center network, we know of only a handful of anthropologists and most of them are employed on short-term contracts.

Since 1975, however, the International Potato Center (CIP) has made a strong push toward the use of anthropologists, including incorporating them into ongoing research teams. As members of an interdisciplinary team and joint authors of this paper, we present a case experience that demonstrates how and why anthropologists can have a positive impact.

BACKGROUND

Part of the mandate of CIP is the rapid development and expansion of the research and technological base to solve problems that limit potato production in developing countries. CIP's source research is organized around nine technical thrusts with objectives that range from collection and maintenance of a world germplasm bank, control of diseases and pests, agronomy, seed production and distribution, to

Anthropologist, postharvest technologist, postharvest thrust leader, and anthropologist, International Potato Center, Lima Peru.

tation on both sides (anthropologist and specialist), but painfully we knitted things together.

It turned out that real losses perceived by many farmers and scientists indeed existed. Because many farmers in the study area stored all potatoes together (whether for consumption, sale, or seed), they did not automatically offer information on different requirements and activities related to potatoes destined for different purposes. While there may not have been losses to farmers in consumption potatoes or those used for animal feed, losses in seed potatoes was another thing.

Through interaction with the biological scientists on technical aspects of storage, the anthropologist was able to sharpen his questions and ask them in a different way. Werge had learned from the technologists that potatoes stored in darkness produce long sprouts that generally are pulled off before planting. When asked about this activity, farmers complained of the costs of time associated with desprouting. Farmer losses were not merely pathological or physiological problems, but social and economic ones as well. Now the team was on common ground with the farmer. Drawing knowledge from the farmers and each other, the team members agreed on a problem needing action for which there were hypothetical solutions: seed potato storage.

THE INITIATION OF ACTION

The case of rustic seed stores

The biological scientists had set up on-station seed store experiments using the known scientific information that natural diffused light reduces sprout growth and generally improves seed quality. This principle was developed long ago by European farmers but has largely been abandoned as a result of the introduction of sophisticated storage systems. But the method is still used as a preplanting practice. Sprout growth was reduced in the on-station experiments. The design of the stores, however, was developed from the biological scientists' point of view alone. Again, the anthropologist ("The guy was always bugging us," said one specialist) was asking if the design related to the farmer. Was it potentially acceptable?

The anthropologist was anxious to find the answers from on-farm trials. He had been doing research on the architecture and uses of farmhouses and buildings and was concerned with how the seed stores might fit into them. A storage facility separate from the house did not seem realistic because of the lack of security and convenience. Nor did it seem possible to introduce diffused light into the dark rooms traditionally used as stores. Diffused light produces greening in potatoes and renders them unsatisfactory for food. The team inspected farmhouses and talked over the problem with farmer cooperators. Many Andean houses have a veranda with a roof that lets in indirect light. The team decided to set up experiments under the veranda using the conventional seed trays from the research station.

The on-farm experiments yielded the same scientific results as those on the experiment station. Farmers expressed interest, but were concerned about the cost of the seed trays. The technologists subsequently built simple collapsible shelves from local timber and used them in a second series of on-farm trials. The results in seed tuber quality and increased yields were similar, and the farmer was able to relate

families in the Andes produced dehydrated potatoes for home consumption, few were sold. Only 5 of 52 families Werge studied sold a part of their product. The demand for dehydrated potatoes among migrants from the mountains who were now living in coastal cities made us realize that a shift in scale was probably the direction to take. If it was realistic to produce dehydrated potatoes on a scale larger than the family (village level, cooperatives, or commercial enterprises), improved solar drying efficiency would be desirable as part of a complete process. A low cost processing plant was built using local expertise and equipment. This equipment was demonstrated to possible clients through field days. It is too early to determine the degree of acceptability of this innovation, but some plants are now being constructed in Peru and financed by Peruvians. In addition, peelers and cutters have been purchased by individuals who are not interested in a complete processing plant. This confirms the validity of Werge's early observations.

ANTHROPOLOGICAL IMPACT ON TRAINING

Paralleling the station and field research activity was a need to train national potato workers in storage principles. As a result of the Mantaro Valley experience, we developed a new orientation to training, which had been primarily a technical exercise. Robert Booth put it this way:

> Rob (Werge) was interested in training as a transfer mechanism, but at first he was irritated with our overly technical approach. Roy (Shaw) and I were initially regurgitating textbook storage principles and spouting static technological design. As a result of our years in the Mantaro Valley, however, we began to talk about technology and training in a social and cultural context and the need to design acceptable technology.

In training courses we began to push an integrated approach. To a great degree, the technologists had by this point become their own backpocket anthropologists and the anthropologist a storage expert. Today, Booth says, ". . . I would rather have Werge advising on the technical aspects of storage for certain developing country situations than many technologists I know." Perhaps more important for training was not the expertise biologists and anthropologists had gained from each other, but the development of a common philosophy that agricultural research must begin with the farmer and end with the farmer. Courses were subsequently conducted in many countries, mainly by the technologists, who had to articulate the anthropological perspective on their own, especially after Werge left the team in 1979. They related the Peruvian experience in great detail, arguing that unless the trainees also wanted to go through a long, drawn-out period of trial and error, they should pay heed to the Peruvian case. They encouraged trainees and national potato program workers to go to the farmer first and find out where he is, why he does what he does, and how he perceives his problems.

THE NEED FOR CONTINUATION

Werge's departure in 1979 left a break in the integration of the group. Anthropologist Robert Rhoades arrived about the same time, but he became involved in another

crucial when trainees want to know what group of farmers they should work with to set up demonstration stores. The anthropologist, finding the ambition explanation too simplistic and smacking of unfounded armchair psychology, prefers to leave this question open. He does not deny that there are good farmers and poor farmers in a management sense, but ambition alone as a single-factor explanation for a complex social and economic process is better left as a hypothesis.

Innovation diffusion through a social structure may indeed occur first among better educated farmers with more resources. But the risk factor may keep it from resource-poor farmers — the technology may not be relevant to their storage needs nor be acceptable. We raise these debates to point to a kind of dialogue that the team feels is necessary to keep the research approach dynamic and relevant to the farmers' needs.

CONCLUSION

In this case study, the primary role of the anthropologists has been that of a link between technologists and farmers. Does this role adequately justify maintaining or promoting the anthropological input into international centers and agricultural organizations dealing with the design and transfer of new technology? The biological scientists think that it is. As one biological science member of the team said:

> Getting us to really see the farmer's point of view is one hell of a contribution. We don't get hung up on the fact that anthropologists help link us with our clients. There is nothing degrading about this role and if anthropologists think it is then that is their problem. Communication and understanding between scientists and farmers is an art requiring an expertise which alone many biological scientists don't have.

What has been left out of the story is precisely what is involved in the anthropologist's expertise. Anthropology is characterized by a set of methods and theories that permit tracing, especially in rural communities, the connections between the mundane, bread-and-butter farming activities and the beliefs, religion, kinship, social institutions, material culture, and even ecology and economy. Anthropologists do not reject quantitative methods, but the crux of their methods centers on a total view of farming and social activities that can yield a special holistic understanding of farmer decision-making. Although not interdisciplinary from a team sense, anthropologists were studying farming systems in a holistic way long before farming systems research (FSR) became popular in international agricultural research. Also, at CIP, all anthropologists have been trained in the theoretical perspective of ecological anthropology. The training involved several years of living cross-culturally in rural settings. This orientation stresses the essential rationality of human adaptation to the immediate and wider social and physical environments. People (farmers in this case) do what they do for good reasons. For their survival they have, through long-term adjustment and adaptations, arrived at reasonable solutions.

Farmers in particular carefully weigh new technologies in the light of what they know works, however imperfect that may be, and in this way maintain a selective balance between the new and old. The anthropologist, in trying to see the world

through the farmers' eyes, will always ask: Can the proposed technology improve on those reasonable solutions? Is the proposed technology acceptable to farmers?

Could the biological scientists have arrived at the same point without the anthropology input in the storage case? "Perhaps we will never know, but it surely would have taken much more time at a great loss of energy and money," Shaw says.

Perhaps equally important to the anthropological perspective in the postharvest case was the team's working together from beginning to end — however conflictive that relationship may have been at times. Members of both disciplines had the same objective: the technology generated had to correspond to the farmer's objective to be successful. If the anthropologists had been attached to the team only to do an initial survey (which the technologists claim they would have never read), or to do an evaluation after the fact, the outcome would have been much less than desired. Constantly team members disagreed about how to accomplish their objectives and frequently old biases surfaced.

Technologist: "My problem is not getting the farmer to understand an improved potato technology, but getting the thick-headed anthropologist to understand it."

Anthropologist: "Telling them that the way they do it in Idaho has no bearing on the way to do it in the Peruvian highlands is like water rolling off a duck's back."

These are overstatements, but in the beginning there probably was a grain of truth in each reaction. The group has now developed beyond that point. Still, the CIP experience never would have been possible had not the technologists been receptive to viewing their technology through the eyes of the farmer and to the importance of sociocultural factors. Nor would it have been possible if the anthropologists had written off the technologists as pure researchers bogged down in the mud of a research station.

The organization of research activities was based on a circular flow of information. Anthropological and technological studies were not always carried out simultaneously because individual team members began working on the program thrust at different times. But once a technology had been initially developed, on-farm tests were carried out to measure the technology's efficiency (an economic evaluation) and its acceptability to farmers (a social evaluation). These tests invariably showed the need for modifying our testing methods and the technology itself. Modifications were made. We went back to farmers for more tests. The research cycle began again.

We are convinced that if biological scientists and anthropologists would agree to communicate (if only agree to disagree), the CIP postharvest experience could be repeated again and again. We conclude that the road to cooperation is a rocky one, but one well worth taking to reach the farmer.

Social organization and small watershed development

Victor S. Doherty, Senen M. Miranda, and Jacob Kampen

An overall strategy that has been proposed for the improvement of semiarid tropical (SAT) crop production is integrated land and water management for cropland development on a watershed basis (Kampen 1980, Krantz et al 1978). Research at the International Crops Research Institute for the Semi-Arid Tropics (ICRISAT) has focused on small watersheds, which would usually involve the land of more than one farmer in areas with operational holdings and field sizes similar to those of southern SAT India (Tables 1 and 2).

Modeling work and economic analysis at ICRISAT have strengthened this expectation, pointing for example to watersheds of 8-16 ha as a size likely to be economical for development on Alfisols and under rainfall and economic conditions similar to those near Hyderabad during the late 1970s (Ryan et a1 1980, Ryan and Pereira 1980). Small watersheds chosen as sites for experimental development in the villages Aurepalle, Shirapur, and Kanzara were close to this size range and comprised the land of 5, 12, and 14farmers (Table 2). These circumstances mean that an understanding of conditions for group action among farmers is needed for small

Table 1. Owned land of farmers[a] sampled in 3 villages of southern semiarid tropical India, 1975-76 crop season.

	Aurepalle	Shirapur	Kanzara
Mean land area (ha)	6.0	5.0	5.5
Median (ha)	2.6	5.0	3.1
Range (ha)	0.3-38.6	0.8-14.2	0.8-28.4
SD	8.3	3.3	6.2
CV	138	66	113

[a]Landowners in ICRISAT's village level studies (Jodha et al 1977). Sample size: Aurepalle = 29, Shirapur = 30, Kanzara = 30.

Principal social anthropologist and principal scientist, land and water management, International Crops Research Institute for the Semi-And Tropics, Hyderabad, India; and agriculturist, South Asia Projects Department, International Bank for Reconstruction and Development.

Table 2. Field size data from small watersheds selected for development in 3 villages of southern semiarid tropical India, 1978-79 crop season.

	Aurepalle	Shirapur	Kanzara
Farm owners per watershed (no.)	5	14[a]	14[a]
Av field size (ha)	3.5	1.2	1.4
Total watershed area (ha)	17.7	16.8	19.9
Fields (no.) by size (ha)			
0.1-1.0	1	7	8
1.1-2.0	1	3	2
2.1-5.0	2	2	2
5.1 and above	1	0	1

[a]In two cases in Shirapur and one in Kanzara, two members of a family hold title to portions of the same field.

watershed development.

Identification and understanding of anthropological conditions for group action have been a major research focus in ICRISAT's economics program since the latter part of 1976. Approaches have included study of the literature regarding cooperation for agricultural production, examination of particular cases from India, and a study of relevant anthropological work on group size and function (Doherty and Jodha 1979, Doherty 1980). We also analyzed on-farm experiments in watershed development, begun in 1978-79 and carried through the 1980-81 season, by staff of ICRISAT's farming systems research program and economics program in collaboration with Indian institutions. In this paper we analyze cases of cooperation involving well ownership in the same three southern Indian villages where the small watershed development projects were conducted. Some results of the on-farm experiments are also noted.

From the anthropological part of this work, we concluded that two distinct types of cooperative behavior can be discerned in human groups. Knowledge of these types of cooperative behavior can be applied along with knowledge of the relative sizes, longevity, and appropriateness of tasks for human groups under different conditions (Doherty 1980). A summary statement of the two types of cooperative behavior, as well as appropriate group size, follows:

> *Rule-based* behavior can be observed in individuals, small groups, or large groups. Predominantly passive and persisting over the long term, rule-based behavior is in principle predictable and invariant, although the rules themselves may change from time to time. Effective, long-term rules are most often generated and sanctioned by relatively large groups.
>
> *Decision-based* behavior requires management judgments and will call for different actions at different times. This behavior is situational and is effectively performed by individuals, or by small groups that cohere only for the short-term, decision-making task at hand. Such small groups may have a cross-culturally optimum size. They can cohere over the long term and make repeated, variable management decisions only if they have the strong and continually reinforced, rule-based sanctions of a large group or of an active, well-organized administration to support them. Decision-based activity may weigh the application of potentially conflicting rules or it may deal with areas where no rules apply.

We believe that, along with the results of land and water management experiments and of economic analysis, such conclusions can be important in the design and large-scale implementation of technology and programs to improve agricultural resource use in the SAT on a small watershed basis.

MULTIDISCIPLINARY RESEARCH AND THE ROLE OF ANTHROPOLOGY AT ICRISAT

Studies of how improvements to land and water management in SAT areas can be made on a watershed basis show how multidisciplinary research is conducted at ICRISAT, and how anthropological work contributes to such studies. The two studies on which details are given are 1) a special investigation focusing on unadministered, cooperative use of wells by farmers in southern SAT India, and 2) experiments in small watershed development in farmers' fields in the same area.

At the time work on small watershed development technology was initiated at ICRISAT, there was no anthropologist on staff. Scientists in the economics program and farming systems research program were agreed, however, that problems of social organization and group action could be expected in on-farm contexts because the proposed technology would be area-based rather than field-based and would involve the land, resources, and interests of more than a single farmer. It was felt that knowledge about possibilities for and limits of group action would be necessary, even in a research station context, for proper design, development, and evaluation of the technology. On-farm trials were planned for an early date, and it would be necessary to have an anthropologist as a member of the on-farm team to participate in analysis of farmers' assessments of the watershed-based technology.

An anthropologist was recruited in the economics program to work primarily on problems of group action connected with watershed development.

Initially, a joint anthropological and economic analysis was made of theoretical literature and of Indian case studies of cooperative action by farmers (Doherty and Jodha 1979). This 1976-77 study was followed by a more detailed examination of anthropological literature (Doherty 1980). Agronomic tests on farmers' fields were begun during the 1978-79 agricultural season to prepare the way for field testing a modified package for small watershed development the following season. These studies were the joint work of scientists from ICRISAT and from member institutions of the Indian Council of Agricultural Research. The studies of cooperation involving wells were carried out during 1979-80.

On-farm studies are a particularly important area in which ICRISAT researchers from different disciplines and programs combine their efforts, within a common framework, to focus on solutions to problems of SAT farmers. An important context for this cooperative, multidisciplinary research at the field level has been the village level studies program (VLS) (Jodha et al 1977, Binswanger and Ryan 1980), which were initiated in 1975 by members of the economics program in cooperation with agricultural universities of Andhra Pradesh and Maharashtra States. The studies have been expanded recently with the collaboration of the agricultural universities of Gujarat and Madhya Pradesh, as well. The studies are designed to enable analyses of farming practices and problems from a wide range of baseline

data from a stratified, random sample of farm and labor households in villages typical of Indian SAT sub-regions. The study villages have been envisioned from the first as areas where a wide range of on-farm experiments is possible, from evaluation of existing practices, through biological and physical observations, to the experimental testing of particular technologies such as watershed development.

The initial study of anthropological and economic material regarding group action was a cross-disciplinary effort similar to work on rainfall runoff modeling begun at ICRISAT in 1975. In each case, researchers from different disciplines pooled their insights and knowledge to suggest answers to a technological problem concerning ICRISAT as a whole. From preliminary results, different follow-up and investigations were suggested. In the group action study, it was clear that two sorts of studies would be needed. One would be on-farm experimentation, which would involve researchers from a number of disciplines and farmers in the field application and evaluation of technology designed according to hypotheses about what would be agronomically, economically, and organizationally successful.

It was clear to the anthropologist, the economists, and others concerned with the farming systems research and economics programs that additional disciplinary, focused studies would be necessary to resolve questions raised by the early group action studies. Such special purpose studies would also be needed if ambiguities likely to arise in the results of the on-farm work were to be resolved. These considerations led to the studies of indigenous cooperation around wells.

The data for the study on wells reported here were collected by an anthropologist and two assistants as part of a general data-gathering effort on a variety of problems with which ICRISAT is concerned. The data are evaluated here by a group of authors, including researchers in land and water management. The land and water management strategies proposed for SAT areas are given in Appendix 1. An anthropological evaluation of the strategies, based on the analysis in this paper, is given in Appendix 2.

SOME MEASURES FOR WATERSHED DEVELOPMENT IS THE SAT

In an agricultural sense watershed development means the conservation, improvement, and use of soil and water resources in a given drainage area for increased crop production. Development may or may not involve areas used for trees and grassland. This depends upon the demand for different products and upon the nature of the resource base. Natural resources include soils, subsurface geology, rainfall with its runoff and drainage patterns, and groundwater and surface water. Water resources may include aquifers and rivers, which may not depend wholly upon precipitation in the particular catchment. One can envision a variety of resource use and associated problems. Where rainfall intensity and surface and subsurface drainage limit crop production, improvement of land drainage should form a major portion of watershed development. Periodic drought stress often limits crop production in the SAT; in such areas, water conservation and water storage as groundwater or in surface reservoirs assume great importance.

Watershed development research at ICRISAT in Patancheru, near Hyderabad, India, is designed to identify principles that can be used to develop profitable,

intensive farming systems for areas with low and seasonally concentrated rainfall and with relatively infertile, tropical upland soils. Work has focused on improved land and water management suitable for small watersheds (3–15 ha). Broadbed-and-furrow cultivation on a slight grade has been used to improve rainfall infiltration and storage in the soil profile, while still providing surface drainage. Runoff is conveyed through grassed waterways and collected in small storage reservoirs or ponds arranged in series to recapture overflow.

This approach to watershed development and resource use has performed well in experiments at ICRISAT and elsewhere. The technique may be particularly useful in promoting intensification of cropping in some deep Vertisol areas of India where rainy season fallowing is common (Binswanger et al 1980). In some of these areas, drainage problems can prevent cropping in the rainy season. Broadbeds and furrows could alleviate the drainage problem and still allow significant amounts of soil profile moisture to be carried over into the dry season.

A 70-year simulation shows that on Alfisols under conditions such as those at ICRISAT, the optimum sizes of small watersheds are from 8 to 16 ha, if runoff is impounded and pumped to irrigate a second, postrainy season crop (Ryan et a1 1980). Water use is improved by more flexible decisions on cropping pattern, planting date, and irrigation pattern in response to seasonal and market variations. On Vertisols, with better moisture storage and less runoff, the economics of ponds seem less attractive. This situation becomes more pronounced the lower the rainfall and the deeper the Vertisol (Ryan et al 1980). A better understanding is needed of the potential for runoff collection and use on different soils under different rainfall regimes.

BACKGROUND TO FIELD INVESTIGATIONS IN GROUP ACTION

The suggestion that runoff collected in ponds be used for supplementary irrigation on small, upland crop watersheds raises many questions. What are the organizational, physical, and economic feasibilities of this upland crop system vs collection of runoff in tanks for gravity irrigation of paddy rice? (The common South Asian term tank refers to traditional reservoirs with earthen dams for collection of runoff. These can have catchments varying greatly in size and irrigate from 10 to 100 ha or more. What would be the returns to ponds for supplementary irrigation of upland crops, vs returns to wells? Could percolation tanks be built more profitably to recharge groundwater and improve the yield of wells? The hydrological, agronomic, and economic answers to these and other questions, as well as the formulation of the questions themselves, will be location specific.

In all cases, however, one can expect questions to arise about the social organization of ownership and use of such irrigation facilities. Therefore this paper concentrates not on any particular situation in any given area, but upon the derivation of social organizational principles that can be applied along with physical, biological, and economic principles. Ponds could be desirable from other viewpoints, but one must also be able to decide on the most efficient system of ownership and management, and be able to judge whether such a system can be instituted. The type of social organization required will vary not only according to the nature of the resource but

according to the desired results. It will be defined in part and limited by cross-cultural, social, and cultural elements.

Data in Table 1 are on farmer-owned land in three villages (Aurepalle, Shirapur, and Kanzara) of the southern Indian SAT. For situations in which median holdings are 2.6, 5.0, and 3.1 ha, optimum watershed sizes of 8 to 16 ha seem too large for most farmers to develop profitably on their own. Actual plots suitable for small watershed development are often much smaller than farmers' total owned areas (Table 2). This is due to diversification of holdings by soil and location as a risk avoidance mechanism, and to fragmentation of lands at inheritance. From the data in Tables 1 and 2, one could expect to encounter small watersheds owned by groups of 2-10 farmers if one were to begin a small watershed development program in areas of similar ownership pressure on agricultural land. If the farmers who own the fields on these small watersheds were to develop them in common and build collection ponds for supplementary irrigation, they would have to cooperate over the long term and make many seasonal decisions regarding water use and maintenance.

In such a situation we need to know if there are rules. particularly cross-cultural ones, for cooperation in small groups. We then need to know, based on an understanding of such rules, the potential for cooperative ownership and management of ponds on small watersheds. In two earlier papers about cooperation among farmers, a concept of matching appropriate group size and function was developed (Doherty and Jodha 1979, Doherty 1980). On the basis of a comparative ethnographic view, one could hypothesize that small groups of unadministered, independent individuals are likely to be most effective only as short-term task groups, while much larger groups are likely needed to support social mechanisms for continued, variable decision making, and for drafting and enforcing impersonal rules. Both papers also hypothesized that individual and group interests would have to be served. This would be particularly important where individual farmers are independent decision-making agents.

Based on the general hypotheses regarding group action, a specific hypothesis was advanced regarding farmers' preference for ownership and operation of ponds and similar facilities. This hypothesis stated that farmers would prefer individual ownership of small sources for supplementary irrigation (Doherty 1980).

ORGANIZATION OF THE USE OF OPEN WELLS FOR IRRIGATION

Data on ownership and management of wells were collected to provide a partial test of the hypothesis that farmers would prefer individual ownership of small sources for supplementary irrigation.

The cases to be considered are the rules for ownership and management of open wells found in the same three villages where the experiments in small watershed development are being carried out. These villages are also located in the three districts where ICRISAT village-level studies (VLS) are being conducted (Jodha et al 1977). Background on the areas in which the villages are located is given in Table 3.

Only those wells in which a VLS respondent either shares or has full ownership rights are discussed (Table 4).

Table 3. Features of village-level studies districts, semiarid tropical India (adapted from Jodha et al 1977).

	Mahbubnagar District Andhra Pradesh State Aurepalle Village	Sholapur District Maharashtra State Shirapur Village	Akola District Maharashtra State Kanzara Village
Soil types	Shallow and medium Alfisols	Deep and medium-deep Vertisols	Shallow and medium-deep Vertisols
Av annual rainfall (mm)	713 (bimodal rainfall)	691 (rains undependable and received in two distinct phases during rainy season)	817 (rainfall relatively dependable)
Cropped area irrigated (%)	14.5 (tank and well irrigation)	10.7 (largely well irrigation)	1.5 (largely well irrigation)
Important crops	Sorghum, groundnut, castor, rainy season pulses, paddy on irrigated lands	Postrainy season sorghum, pearl millet, groundnut, pulses	Sorghum, cotton, groundnut, rainy season pulses
Regions represented	Alfisol tracts of the eastern Deccan Plateau	"Scarcity zone" of Maharashtra and Karnataka on the western, central, and southern Deccan Plateau	Vidarbha region of Maharashtra and neighboring parts of Madhya Pradesh State

Table 4. Well ownership among village-level studies (VLS) respondents in semi-arid tropical India, 1979-80 agricultural season.

	Aurepalle	Shirapur	Kanzara
VLS wells[a] (no.)	23	18	16
VLS wells used			
no.	17	12	11
%	74	67	69
VLS respondents with at least a share of well ownership			
no.	19	17	13
%	48	43	33
VLS respondents (no.) with an interest in more than one well	7	2	3
Av well share size among VLS owners[b](%)	0.67	0.46	1.00
Av cumulative well ownership among VLS well owners (%)	0.81	0.49	1.23

[a]All wells for which a VLS respondent was sole or part owner during the postrainy agricultural season of 1979-80. There are 40 sample families in each village, 30 depending mainly on farming for their income, and 10 depending mainly on agricultural labor. The 30 farming families are drawn 10 each from large, medium, and small landholding groups (Jodha et al 1977). [b]Includes active and inactive wells.

The most wells are in Aurepalle where the rainfall is low, there are many good aquifers, and an extensive system of tanks and bunds has the effect of recharging groundwater. Many of the Aurepalle wells are old, having been dug several generations ago, before diesel or electric pumps were available. In Shirapur, presumably because of low and undependable rainfall, there are many wells despite extensive deep Vertisols that are highly water retentive. Kanzara has the fewest wells; rainfall is higher and relatively dependable. The shallower soils in Kanzara are underlain by rocky substrata that do not provide high yielding, shallow aquifers. Many Kanzara wells have been built since the early 1960s when diesel or electric power for pumping began to be widely available and the government began subsidizing loans for well construction and the purchase of pumps. Differences in rainfall, cropping patterns, soils, and subsurface geology likely influenced the patterns of well ownership in the three villages.

The high incidence of overall well ownership in these three villages is striking. At the time of the study, wells were the primary source of irrigation in these villages.

The average number of owners per well and the average number of active owners per well (Table 5) suggest that small groups do form themselves around these organizationally independent sources of supplementary water. Many wells have been under shared ownership for several generations; most changes in ownership seem to occur through inheritance. Pumps are also owned in common. These results seem contrary to our hypothesis regarding group ownership of small sources of irrigation.

The natural agricultural environment appears to be a key determinant of common well ownership. The most owners per active well and the most irrigating farmers per active shared well are in Shirapur, where rainfall is the least dependable. Shirapur also has the most owners per pump. Although Shirapur's deep Vertisols retain

Table 5. Shared ownership and use of wells in village-level studies (VLS) sample in semiarid tropical India, 1979-80 agricultural season.[a]

	Aurepalle	Shirapur	Kanzara
Owners (no./active well)	2.4	4.8	1.0
Active owners (no./active shared well)	2.7	4.5	0
Active VLS wells with shared ownership			
no.	12	10	0
%	71	83	0
Owners (no./active pump) in VLS sample	1.4	3.5	0.7

[a] Actual use as opposed to ownership of pumps may vary periodically because of factors such as lack of production funds on the part of some farmers, and renting out of shares by others.

moisture well, farmers still want wells and own them in common. One hypothesis consistent with the data would be that although small groups of owners form and persist around these wells, shared ownership is organizationally difficult and it may be uncommon unless alternatives are not attractive.

Water control systems and the degrees and kinds of interaction among farmers were also investigated. Water control systems minimize interaction among the owners. Farmers do not meet to consider the season as a whole and to devise ways to increase the productivity of their shared water resources. On the contrary, the systems assure that the rights of each individual operate automatically by invariant principles.

Several principles govern the shared use of wells in Aurepalle. First, each owner's share is fixed at a known fraction of the total capacity of the well. Second, owners are individually responsible for raising the water. If a farmer cannot afford the electric bill or has no bullocks to raise water, no one else is obliged to help. Third, there seems to be a de facto upper limit on irrigated area in proportion to one's share in the well. If a well owner does not own enough land within reach of the well to make full use of his share, and if he cannot purchase land near the well, he may sell his rights in the well and perhaps the land. Fourth, all owners are obliged to share proportionately during drought; all pumps must be turned on and off at the same time. Fifth, the pump size can be limited by horsepower, being installed in at least some cases according to the size of a person's well share so that no one realizes an unfair advantage when all pumps must be operated together.

The greatest portion of irrigation in Aurepalle is for paddy rice, the locally grown crop with the highest water requirement. If all farmers use water at the maximum rate and if the other limitations are observed, proportional equality can be maintained.

In Shirapur the well sharing system is based on different rules. Presumably because of the drier climate and lower yielding aquifers compared to Aurepalle, Shirapur wells are not used for paddy. Farmers assume that any irrigated crop planted in the area needs water approximately every 8 days. Rights to water are therefore reckoned in terms of days, with 8 days' rotation a common figure. A

farmer will own 2, 3, or 8 days' rights in a given well. For one day's share, a farmer is entitled to as much water as the well will yield from sunset to sunset. No time extension is possible and a fixed rotation among the farmers is set. The practical irrigable area of a well is determined when it is built: this area is called the malha. In Shirapur as in Aurepalle, there seems to be a de facto irrigated land limitation on farmers in addition to prescribed rights to the well water itself. Well rights are inherited or sold along with malha land proportional to the number of day shares involved.

There is a greater incidence of joint ownership of pumps in Shirapur than in Aurepalle. The joint ownership system probably origmated in farmers' attempfs to cut their capital costs — the tendency is probably reinforced in Shirapur by the rotational pattern of well use.

VILLAGE-LEVEL EXPERIMENTS IN SMALL WATERSHED DEVELOPMENT

Beginning with the 1978-79 crop season, ICRISAT staff assisted in trials to develop small watersheds, which in 1980-81 involved cultivated areas of about 14 ha in Aurepalle, 13 ha in Shirapur, and 12 ha in Kanzara. Other data relating to these watersheds are given in Table 2. The work was done in collaboration with scientists of the All India Coordinated Research Project for Dryland Agriculture, the Andhra Pradesh Agricultural University near Hyderabad, and the Punjabrao and Mahatma Phule agricultural colleges in Maharashtra.

Recommendations implemented included the introduction of

- graded, broad bed and furrow cultivation, and sowing;
- improved crop varieties;
- fertilizer; and
- improved, bullockdrawn tool carriers for planting and for fertilizer placement.

Catchment drainage was improved by conveying runoff along existing field boundaries and by channeling it through waterways and concrete drop structures across fields along natural drainage patterns. In one village, two owners exchanged small portions of adjacent fields to simplify cultivation on the proper grade for the 2-year duration of the experiment. In another case, grade lines were laid out across field boundaries to simplify planting.

All these developments were directed and implemented by ICRISAT research staff. The farmers agreed to the various operations and cooperated actively in the work within their field boundaries. Where work was outside their fields or cut across boundaries, as in the construction and maintenance of the drainage system, the farmers were also cooperative, but their cooperation was mainly passive.

The experiments were begun with the understanding that the farmers in each village would be subsidized for the 2 years of the experiment. The choice of crops was theirs. No charges were levied for land drainage development, nor has the retention of these developments been enforced beyond the 2-year period. ICRISAT agreed to pay all extraordinary costs for labor and bullocks, and to advance the material inputs such as seed, fertilizer, and pesticide.

After the first year, it was agreed that in subsequent years cooperating farmers would repay ICRISAT for material inputs but only if their average net profits were

double those realized on similar nonexperimental fields in the same villages.

Because the techniques were untested on farmers' fields, the financial subsidy was necessary to minimize the cooperating farmers' financial risks. ICRISAT coordination was withdrawn when the financial supports were terminated but technical advice continued on request.

No ponds were built on the watersheds in the study. In Aurepalle an existing well in the watershed was used for supplementary irrigation to facilitate growing a second crop in any year there was sufficient groundwater. In Shirapur, where rainfall is low and unreliable and soils are deep Vertisols, a pond would be an unlikely investment. Possibilities for pond construction were also limited by the short duration of the experiment and the need to guarantee that farmers' freedom of action would be minimally affected during the experiment and would be completely restored when it ended.

We can make a broad social organizational assessment of farmers' reactions to the first 3 years' activities. Where the system could handle runoff without overload, farmers generally did not object to an improved drainage system that followed field boundaries and natural features within fields. Nevertheless they showed strong interest in maintaining boundaries, protecting individual rights, and adapting improved tillage and planting to individual field patterns. Some farmers have objected to concrete drop structures within fields, but not to those on boundaries. Farmers have expressed interest in renting or purchasing bullock-drawn tool carriers and attachments. They have shown a strong aversion to shared ownership of tool carriers.

The farmers' individualism expressed in these ways confirms some predictions of our earlier studies (Doherty and Jodha 1979, Doherty 1980). Nevertheless we have seen in the same villages that stable small groups form around water sources.

CONCLUSION

The behavior of the VLS sample farmers who share rights to wells in Aurepalle and in Shirapur contradicts our hypothesis that farmers would prefer individual ownership of small sources of water for supplementary irrigation. In the face of these data, we cannot simply assign short-term functions to small groups and long-term functions to large ones. The data can be accommodated, however, if we revise our hypothesis, taking into account decision-based vs rule-based behavior, as well as the functions of small groups as opposed to large groups.

The systems of cooperation followed by farmers who share rights to wells in Aurepalle and Shirapur are clearly rule-based. The systems governing ownership and management apply in the village as a whole. Farmers who obtain access to a well need not worry about what the rules will be. Decision-based interaction, in which one person's decisions on cropping pattern or irrigation timing might influence the well-being of his neighbor's crop, is carefully excluded by customs governing shared ownership and use of wells. We suggest that such rule-based activity is suitable for small or large groups, even though the larger group ultimately must sustain and sanction it. It is functionally and organizationally opposed to decision-based acti-

vity. Decision-based activity is efficiently carried out by individuals, by short-term small coalitions, or by small or large groups under a centralized management.

Thus we revise our hypothesis to state: farmers would prefer that small sources of irrigation water, such as collection ponds on small watersheds, be individually owned, unless simple rules for distributing water could be specified in such a way that interaction and common decision-making among owners would be reduced to low or negligible levels. To the degree our findings have cross-cultural validity, we expect that it might be possible to modify the severity of these requirements in certain cultural and social contexts, but not to evade them in any case. As a supplement to the present study and its precursors, additional cross-cultural comparison of cases and circumstances should be done. Social organizational insights also need further study. Still, the revised hypothesis seems well-founded. We submit that the distinctions drawn here between rule-based and decision-based behavior, and between the functions of large and small groups. will prove to be significant in the design and assessment of agricultural technology to meet the needs of the SAT and other areas.

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Appendix 1. Land, crop, and water management improvement strategies proposed for SAT agricultural areas.

Major problem area	Specific problem	Solutions proposed
Drainage	Stagnation and waterlogging	Land shaping; planting on grade; beds and furrows where appropriate; comprehensive drainage on an integrated, small watershed basis.
	Erosion	Land and water management measures same as for stagnation and waterlogging; grassed waterways; permanent drop structures in waterways; fast, efficient crop establishment in the rainy season because crop cover is the most important erosion alleviator.
Moisture deficits	Timeliness and precision of operations	Use of improved, bullock-drawn tool carrier for faster and more efficient land preparation and seeding; use of improved seed drills and fertilizer applicators to increase water use efficiency and nutrient uptake by seedlings.
	Increase moisture storage in the soil profile	Tillage and planting on grade; broad beds and furrows where appropriate.
	Use of runoff water for crop production	Collection and use of runoff water in ponds for lifesaving or supplementary irrigation or both. Construction of larger reservoirs (tanks) for gravity irrigation.
	Use of groundwater	Examine potential for wells of different types in the SAT, and for improving groundwater availability.

Appendix 2. Anthropological evaluation of proposals for improved agricultural land, crop, and water management in the SAT.

Improvement	Group action involved	Probabilities of success
Land shaping	Decision-based or rule-based agreement for an essentially one-time, short-term activity.	High likelihood of organizational success for individuals or small groups. Possible high cost relative to returns realized over a long term may require government subsidy or organization or both to initiate such activity.
Planting on grade with or without broad beds and furrows as appropriate	Decision-based activity. Adoption requires farmers to balance levels of labor available and other factors against costs in time and effort to set up and maintain a specific land management pattern which may require time and patience to realize full results.	Best confined to within-field patterns for individual farmers at first. Minor field boundary reorganization problems may emerge. If use of the new techniques is profitable enough, individuals or small groups can be expected to adjust these on their own.
Watershed-based drainage improvement with or without grassed waterways	Only rule-based cooperation would be required if government were to install the system and protect it legally thereafter.	High probability of success if imposed and then backed up by rules. Preference for systems using existing field boundaries as runoff removal areas to the greatest extent practicable.
Grassed waterways	Rule-based acquiescence at least is required because waterways serve the watershed as a whole.	Where the waterways could fully control runoff, little particular objection to the definition of waterways within fields was experienced in on-farm experiments. These experiments, however, revealed severe difficulties with establishment of the grass itself and this in turn made control of heavy runoff difficult in some cases. Solution of these technological problems is probably more important than group action if leaving waterways in grass is to prove more profitable to farmers than plowing and planting the waterway along with the rest of the field.

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Rice insect pest management technology and its transfer to small-scale farmers in the Philippines

G. E. Goodell, P. E. Kenmore, J. A. Litsinger,
J. P. Bandong, C. G. de la Cruz, and M. D. Lumaban

In 1978 the International Rice Research Institute (IRRI) formed an interdisciplinary team to test and improve IRRI's integrated insect pest management (IPM) technology for farmers tilling small irrigated plots in Southeast Asia. This paper decribes how the team developed the technology from an initial Western orientation to its present form.

IPM focuses on the fact that pests (insects and rats) move from field to field, a particularly grave problem in the tropics where the climate does not reduce the pest population annually by winter fallow. In Southeast Asia, where farms are small, entomologists advocating IPM assert that pests will be more effectively controlled if many small plots are managed as a single field.

IPM then calls for the consolidation of many small plots into a single management unit. The entomologists could not improve pest management technology and test it in the field unless farmers were willing to organize themselves by contiguous fields and practice IPM recommendations as a group. The interdisciplinary IPM team had two tasks. First, it had to persuade the farmers that the new pest management technology would offer them significant benefits. Second, the team had to devise ways to help the farmers organize themselves. The team recognized from the outset that while its technological recommendations were fairly well worked out, further development of the technology would be required.

THE PROJECT

Because IPM depends heavily on cooperation among farmers tilling adjacent fields, it was imperative to study ways in which farmers organized themselves for collective action.

Lowland Filipino culture generally is known for a marked absence of long-lasting

Visiting scientist, research fellow, entomologist, and entomology research assistants, International Rice Research Institute, Los Baños, Philippines.

and cohesive organizational forms other than the family. When the hacienda system was operating full force, group crop management was largely enforced by the photoperiod-sensitive varieties themselves, by landlord control of supplementary irrigation (where that existed), and by the top-down supenision by the landlord's foremen. But the hacienda system broke down during World War II. Even where landlords remain they rarely enforce group farming.

A strong peasant movement in Central Luzon in the late 1940s and 1950s left no sociopolitical structures still intact, and few active local leaders. Indeed, it made the rural population highly suspicious of outsiders, especially of young organizers.

Furthermore, within a single decade, the farmers here have received land reform, a vast new irrigation system, almost universal double-cropping enabled by the new rice varieties, and generous agricultural loans extended season after season despite low repayment — theirs is hardly a climate of communal self-help. Years before our project the government attempted to impose certain superficial groupings upon the farmers, such as irrigators' associations, but none functioned for long, deepening the farmers' cynicism of their own organizational abilities.

The anthropologist had generalized the sociological approaches used to organize farmers in the Philippines into two broad categories: top-down and bottom-up.

The topdown approach is more authoritarian. It is typically used by banks, other credit institutions, and the National Irrigation Administration (NIA). Farmers consent to follow centralized management directives in exchange for the services of such institutions.

The bottom-up approach is more participatory. It is typically used by Catholic and other religious groups to spark self-help projects among the poor and help them make collective demands for improved government senices. The NIA has made some moves toward the bottom-up approach in organiling irrigators' associations.

The interdisciplinary research team set out to test 1PX.I by organizing one top-down and one bottom-up project, each comprising fix villages, and introducing IPM into one control area in which no attempt would he made to organize the farmers. The Agency for Community Educational Services. Manila, was contracted to organize the farmers in each of the 5 bottom-up study villages. The agency provided one organizer for each village.

The villages, typical of Central Luzon, are in Nueva Ecija Province 120 km north of Manila, in an extensive fertile plain served by a large irrigation system. All of the arable land is planted to rice in the wet season (August-November) and 75% of the farmers grow rice in the dry season as well (January-May). All villages can be reached by unpaved roads within 5-10 km of a paved highway. None is more than an hour by public transportation from the provincial capital. Eighty percent of the villagers are leaseholders, with about 2 ha/family. At the time of the project, 90% of the farmers grew insect-resistant varieties whose average yield was 4.5 t/ha. Only a fraction of their annual income, which ranged from $900 to $1030 per family, was derived from nonagricultural sources.

The project anthropologist lived in three of the villages, each representing one of the three organizational approaches being studied, to closely observe the methods used to organize farmers into groups.

The project began in the wet season of 1978, and was to last for 2 1/2 years or 5

cropping seasons.

Throughout the study, the anthropologist collaborated closely with the entomologists in focusing the research on nearby villages of comparable agricultural, demographic, economic, and social characteristics so that a difference in these factors would not bias the research results. Our evaluation will deal only with the interdisciplinary research conducted in the bottom-up villages, where the project enjoyed considerably more success than in either the top-down or the control areas.

THE TECHNICAL CHALLENGE

IPM was developed in European and American agriculture and in plantation agriculture in the Third World. It tries to replace a pesticides-based pest control system with a cheaper and more holistic one to avoid many instabilities caused by pesticide dependence. Neither its underlying complexity nor the high motivation it assumes has been considered a constraint. It calls for monitoring pest populations, calculating damage thresholds to decide when insecticides are economical, and adopting crop management practices to reduce pest populations and increase the populations of their natural enemies.

The main components of IPM, or group pest management, are:

- synchronous planting — farmers over a large area plant varieties of the same maturation date at almost the same time;
- group pest monitoring — farmers examine their fields systematically and arrive at group decisions about pesticide use;
- group fallowing — preferably by plowing under stubble after harvest; and
- group pesticide purchase.

Group pest monitoring requires farmers to calculate damage thresholds to decide when pesticides are economical. It replaces spraying by the calendar or other control decisions that do not depend upon the presence of pests in numbers sufficient to justify the cost of the treatment.

IPM has been researched at IRRI since 1972, particularly when brown planthopper outbreaks seem to follow intensive insecticide use.

The original technological questions IPM research faced were:

- Did farmers planting wetland irrigated rice, even in the more progressive regions of the Philippines, have IPM components and were those implemented?
- Could small farmers implement group management?
- If IPM were implemented by group farming, could it actually reduce rice pest infestation and the costs of pest control?

Through the interaction with the anthropologist and the farmers in the research project, the entomologists have been able to answer some of these questions. But more important, they have begun to realize that their original questions were backwards. That is, if technology is to be used by farmers, its development must start with them and not on the research stations. Table 1 lists the technological starting points the entomologists considered important and the technology's *finished product* at the end of the study.

Initially, the entomologists needed the anthropologist to describe and interpret farmers' pest control practices, their receptivity to the new technology, constraints

Table 1. Modification of integrated pest management technical assumptions during the course of introducing the technology in 5 villages in Nueva Ecija Province, Central Luzon, Philippines, July 1978-December 1980.

Scientists' initial technological assumptions	Technological positions after 2-1/2 years collaborative work with anthropologist
Farmers' strong interests in pest problems will motivate them to adopt IRRI's IPM once it's demonstrated.	Farmers are not motivated to adopt complex IPM technology on their own, but work with technology presented in a personal context.
Postseason questionnaires provide reliable data on pest perception and control practices.	Ideally, farmers' pest control practices should be observed, not queried. Frequent queries are better than one postseason survey.
Pests significant to farmers throughout a village can be specified beforehand for all practical purposes.	Unexpected pests such as rice semi-looper make field trials less useful than feedback from farmers in making village level recommendations.
Rice whorl maggot is a serious seedling pest.	Rice whorl maggot is not a serious pest in isolation, but only in certain environmental contexts.
Government agencies can provide village level monitoring after training in IRRI surveillance methods.	Monitoring can be expected only from farmers, not agency technicians. Surveillance methods are being redesigned with farmers for farmers.
Frequent quantitative monitoring of trap catches and field populations is within the capacity of most farmers.	Light traps and most quantitative tools are tedious for farmers. The light traps are dropped and the tools redesigned to be qualitative.
Graphing of insect populations is within the capacity of better educated farmers.	Graphing insect populations is too abstract. Concrete infestations or damage levels at particular times are more usable.
Farmers' ineffective use of insecticides can be remedied by more careful measurement.	Excessive frequency of farmers' insecticide applications have been reduced, but dosages are still low. Scientists have not yet developed simple methods for farmers to calculate dosages accurately.
Quantitative economic thresholds for pest control decisions can be used by farmers and technicians.	Quantitative economic thresholds are not yet simple enough; qualitative decision rules are more usable.
The problems of insecticide use being tied to credit schemes can be circumvented.	Credit schemes still pressure farmers to spray too frequently (some of the best IPM practices occur in areas where credit is short).
Soil incorporation of carbofuran can be so profitable that farmers will try it despite initial expense.	Farmers don't incorporate carbofuran; it is dropped from recommendations.

Continued on opposite page

Table 1 continued

Scientists' initial technological assumptions	Technological positions after 2-1/2 years collaborative work with anthropologist
Cultural pest controls have great potential as low-cash technologies.	Large area-wide stubble plowdown is impractical and expensive.
Synchronous planting is an attractive IPM technology for organized farmers' groups, the larger the better.	The minimum effective size of a synchronous area is only now being determined.
Varietal resistance to insect pests is a foolproof foundation for IPM. As varietal resistance to a given pest becomes available, insecticides are no longer required for that pest.	Farmers' ignorance of the identity of a variety's resistance means they spray for pests already controlled.
Sustained baiting for rat control is a valuable addition to or replacement for farmers' practices.	Sustained baiting is less attractive than current farmers' methods of rat control.
Identifications of discrete pest entities and their causal relationships to yield losses are prerequisites for successful understanding and use of IPM.	Farmers perceive pests as part of a crop context; translating our cause-effect relations into contextual terms is a major challenge.
Clear classroom presentations, planned field demonstrations, and handouts in the local language are the key to transferrina IPM technology.	Frequent personal visits by technicians responding to farmers are the key to transferring IPM.

otherwise not anticipated, and the farmers' alternate views of pests that the scientists had not picked up in their surveys.

Developing the technology — or, what the entomologist considered adapting it to the Asian farmers needs — required various contributions from the anthropologist that laymen, even well-meaning biological scientists with some experience in social science, could not at first supply for themselves.

When the project began the staff assumed that there would be basic government extension services at the village and farm level to complement the scientists' and organizers'efforts to teach farmers the basic components of the new technology. The staff also expected sufficient government monitoring services to provide the farmers and technicians with regional and local pest information. Finally, the staff expected some government action to ensure that farm credit would be adequate, that pesticide sales would not be tied to farm credit, and that the integrity of pesticides would be monitored by a government agency. Virtually none of these government services operated at the village level.

Even among the farmers easily induced to try group crop management, the relatively low level of government or banking services meant additional burdens for the researchers, and additional hurdles for the acceptance of the technology, should it prove simple, cheap, and beneficial. The team then had to incorporate into IPM technology many components they originally expected the government to help provide. For instance, farmers would have to do their own monitoring and not rely on regional backup from the government.

On the other hand, these inadequacies made the community organizers' job easier. Even if most farmers were not immediately attracted to working together for group crop management, they were rather more easily inspired to group action demanding better services from the government. This gave farmers the chance to identify their leaders and to learn basic organizational skills. They could move on to the more complex challenges of group crop management once these lessons had been mastered, As farmers learned organization at this early stage, the organizers were able to evaluate the strengths and weaknesses of the budding groups. This slower start also allowed the anthropologist to appraise the farmers' group action so she could begin to predict the success of the new technology.

In the preliminary work, building farmers' groups that focused on government services, the scientists and the anthropologist realized that in developing new technology one must begin where the farmers are organizationally, just as one has to accept where they are technologically. The feasibility of group action must first be tested on genuinely felt needs before scientists can evaluate how well farmers might learn to organize themselves for goals that we think are important.

But even when active villagers formed groups to demand improved services, that did not mean that with a bit more practice, skill, and perhaps incentive, they would automatically succeed in group crop management. The anthropologist pointed out to the research team and the professional organizers that group crop management calls for collaboration among field neighbors who may have relatively little in common with one another economically or socially, who rarely are relatives or house neighbors, and who sometimes live in different villages. Particularly strong social pressure would be required of these field neighbors because even one individ-

ual's deviation from group action might destroy the effectiveness of the technology for everyone. (For instance, one green field among many fallow ones can be a pest nursery for those surrounding it.) Sociologically speaking, group crop management calls for a different principle of organization than kinship, neighborhood groupings, or even traditional village politics. One cannot assume that in every 50-ha area a farmer with outstanding leadership abilities will emerge.

THE ANTHROPOLOGISTS TECHNOLOGY

Group crop management in various forms has been a goal of many rural development projects in the Philippines over the past half century. Traditionally, outsiders have tried to organize farmers to manage their plots together for more efficient use of scarce irrigation water, for more efficient delivery of extension services, and for administrative savings in agricultural credit programs.

The anthropologist anticipated that farmers in some areas would share these organizational objectives, particularly after adopting IRRI's new high-yielding rice varieties. Because farmers already recognized that those who plant out of synchrony suffer severe rat infestations and high crop losses, adding the IPM objective seemed only to strengthen the case for group crop management. The anthropologist took entomologists beyond their own discipline by suggesting how the entomologists' technology might be closely connected to other agricultural problems of the farmers. This proved important when farmers suddenly discovered that pest management is indeed related to irrigation on the one hand and to credit policies on the other.

The technology changed drastically through the 2 1/2-year study, mainly because of intense interaction between the farmers and the scientists. The anthropologist's main contribution was in helping this direct interaction take place, although she contributed in other important ways as well.

The anthropologist analyzed ways to organize Asian (or at least lowland Filipino) farmers consistent with their culture. She evaluated community organizers that would be needed to organize farmers for testing IPM technology.

She continually monitored the organizational side of the research so that causes of the technology's success or failures could be accurately pinpointed. In this way, a shortcoming in the technology would not be attributed to sociological factors. Nor could the technology's feasibility and popularity be attributed solely to effective organizational approaches.

Finally, the anthropologist developed techniques to evaluate the structural effectiveness of whatever farmers' groups would undertake group crop management. If one organizational approach proved more effective than others, that approach's performance would be periodically measured to determine when it could be phased out, leaving farmers on their own.

The anthropologist was strongly convinced that the role of mediating between the farmers and the scientists should be temporary, and should not be used as a crutch for either party in the research process or as a rationale for employing anthropologists. Agricultural scientists are a part of the farmers' world and they must come to realize that farmers are a part of theirs. To the extent that feedback from farmers is necessary, and that some farmers are reluctant to deal forthrightly with the scientists

and their staff, then the anthropologist's task is to devise replicable ways whereby scientists and farmers can break through this barrier.

The scientist often overlooks the value of securing the farmers' active participation. But the anthropologists, trained in the holistic fabric of human society, must see the liabilities as well as the promise of long-term development. The anthropologist sees technology as a process in any society's growth. When scientists are working in a highly competitive sector of a society with a long history of colonialism, they must be aware of the farmers' timidity, obedience, and dependence. Scientists developing technology for farmers must bear in mind the infamous history of middlemen who buffer the elite from the farmers and vice versa. The anthropologist wanted to ensure that she did not simply recast these old relationships in a new form.

To the anthropologist, one of the main purposes of forming lively farmers' organization was to help farmers make scientists — and in turn bureaucrats and landowners — interact with them directly as partners in their own development. Because a few IRRI scientists already considered farmers' frank exchange as a requisite to technology development, the anthropologist's systematizing of farmers' feedback was a service to the scientists themselves.

The anthropologist proposed that farmers might be more lively partners if they interacted with the scientists in groups (consolidated by mutual interests and the routine practice of some form of collective action) rather than as individuals in the long, formal, one-to-one interviews standard at the Institute.

This concept of rural development as a process, not a series of technological achievements, was perhaps the anthropologist's most valuable contribution to IRRI.

MODIFICATIONS TO IPM TECHNOLOGY

In this section we review the changes which took place in the original IPM technology to explore the interaction between the anthropologist, the farmers, and the entomologists, and as a way of examining the anthropologist's impact on technology development. There are four areas of generalization within IPM technology: 1) problem identification, 2) the complexity of the technology, 3) group crop management, and 4) packaging the technology.

Problem identification

Plot trials in farmers' fields demonstrated that original technology worked, but surveys showed that the farmers were not using the technology. The entomologists concluded that the challenge was one for extension.

But the anthropologist had been constantly critical of the survey technique, particularly when it was used to evaluate something like pesticide use. She had warned that farmers would simply reply to the survey according to what they thought was the correct or desired answer, being strongly influenced by the popular image that a good farmer sprays frequently. To find out what farmers were actually doing, one had to see them do it or talk with them in the field when problems arose.

Initially the anthropologist and research technicians helped answer the entomologists' questions, but the farmers were most vigorous in providing the scientists with

accurate feedback and interpretation. In weekly discussions, attended regularly by the entomologists, it became clear that pests other than those originally expected were causes for concern throughout the season. One pest (rice whorl maggot) the entomologist had thought important by itself in fact affected yield only by a complex interaction with other pests. The farmers described this damage mechanism more accurately than the scientists themselves. Scientists learned that they had to recognize the serious limitations of a technology developed for a nationwide scale when it had not been adjusted for local variations. This in turn made them face the implications of local fine-tuning.

The common understanding is that government research agencies and regional universities or colleges will fine-tune broad technological recommendations to the regional and municipal levels, and the extension service will help fine-tune them to the village and farm levels. But when one realizes the overwhelming problems faced by understaffed and underfinanced government agencies, it strikes many researchers as irresponsible simply to hand over the broad technological recommendations to them in the hope that they will fine-tune the technology to regional and local needs. Knowing that the agencies are not equipped to fine-tune the technology systematically forces scientists, who are developing the technology, to consider how they can help farmers fine-tune it themselves, individually or in groups.

The scientists also began to appreciate how indispensable direct interaction with farmers' groups can be. They began to ask the anthropologist why, when they sat in on field interviews with individual farmers, the technology was never criticized. But those same farmers, responding to the same questions as members of a group called the Farmers' Club, sparked each other to push the questions or issues further. (The anthropologist ultimately articulated how extremely social farmers are in their learning.)

With the help of the anthropologist and the farmers, the scientists began to see the farmers' pest problems more in terms of damage symptoms as the farmers see them, instead of insisting on defining pest problems on the basis of numbers of pests caught in light traps at particular stages in their life cycles. Sometimes farmers combine the symptoms of several pests into one phenomenon, which guides them in their intuitive pest control measures. By studying these rather than discarding them as wrong, the entomologists began to understand aspects of pests that they had not been aware of when they studied the same pest from a different view on the experimental farm.

The technology's complexity

The staff rapidly began to appreciate how confused the farmers were about rice pests, their prevention, and the damage they caused. For example, the farmers were unable to identify some of the major pests at all, and lumped various pests together as worms. They considered the most threatening ones to be planthoppers and leafhoppers, although their rice varieties were resistant to these pests. Sometimes the farmers unnecessarily spent $50/ha per season to spray against these pests.

With dozens of pesticides under labels that change every few years (sometimes the same one under several different labels by the same company), and with many specialized pesticides requiring different dosage rates (some in the metric system,

others not) how could the farmers choose the right one? To make matters worse, some of the most popular chemicals were those that had been implicated in causing pest outbreaks.

The farmers were satisfied that they were applying enough insecticide in proper concentrations. In reality, when pesticides were needed. the farmers' dosage was but half the correct strength 80% of the time. They did not realize that spraying highly diluted pesticide kills only the natural enemies of the pests, causing more harm than good. Nor did they understand that an expensive pesticide requiring a low dosage is more economical than a cheap one requiring a high dosage. But figuring dosages correctly is extremely complex. Calculations often are in sprayloads per hectare, despite the fact that one rarely sprays an entire hectare at once.

Then there were the problems of when to spray. Farmers sprayed early in the season when, in fact, the plants were most vulnerable to insect pests later on. They often sprayed according to schedules recommended by the extension service, not waiting until they actually had a pest problem in the field. If they had first determined whether economic thresholds had been exceeded, they might have saved as much as their entire pesticide bill for the season. The anthropologist, herself dizzied by all these identifications, calculations, and choices, had a first-hand taste of the farmers' confusion. She appreciated the laboriousness of going to distant fields every day just to determine (by a percentage formula when one had no grasp of percentage) whether the pests were there in significant numbers. She experienced the difficulty of calculating different economic thresholds for different pests at different stages of their own and the plants' life cycles. She understood the extraordinary complexity of figuring dosages when one didn't even understand liters vs gallons, cost tradeoffs, or the English instructions on the labels. The farmers were simply too confused and too overawed to express their discouragement; they needed someone such as the anthropologist to articulate it for them.

It seemed to the entomologists that they had already exceeded their limit in compromising the scientific accuracy of the original technology, long ago having lost sight of their colleagues' standards for centralized computer-based pest management across whole counties in California's Imperial Valley. Whose standards? What standards? The anthropologist, joined in time by the research technicians and the farmers themselves, shed a different light on the challenge. Why should the farmers be interested in IPM, much less adopt it?

IPM entomologists may be certain that carbofuran should be incorporated into the soil before transplanting rice, but when one imagines all the things that can wipe out a crop in the monsoon tropics, isn't it a foolish risk to invest in it even before the seedlings are planted? The same objection held for sustained rat baiting, which should be started before the farmers see any rats and must be sustained even if they never see a single rat killed by the poison. Furthermore, the farmers worried about killing the snails, fish, and crabs, and about poisoning the transplanting crews, children, and buffalo that might walk around in the mud after the carbofuran had dissolved.

Why should one apply concentrated dosages and increase the costs if the intricacies of pesticides remain a mystery anyway? And how could the scientists insist that farmers spray, if need be, only once in a season when a misstep in that single spraying

might destroy an entire family's livelihood for half a year?

Entomologists argue the need to be ready when a region-wide pest outbreak occurs, as they are sure it inevitably will — an argument far more appealing to intellectuals than to farmers. But if the farmers are well protected by resistant varieties and if the nation has not suffered a serious pest infestation for more than 6 years, what is going to make them worry about IPM? Finally, if the pesticides are diluted before the farmer buys them (this had never occurred to the entomologists, but reconnaissance studies suggested the practice may be rather widespread), or if the purchase of pesticides (whether needed or not) from the bank's retail store is a condition of the farmer's agricultural loan, then don't the identifications, distinctions, and calculations required by IPM Seem futile? Farmers could save money if they knew IPM. But with the vagaries of the monsoon climate, the uncertainty of the scientists themselves about some issues, the economic realities of pesticide sales, and the overwhelming abstraction of the technology, was any of this worth it to the farmers?

"Because entomologists are excited by pest control research and its results, we tend to cling to our tools when attempting to explain IPM technology to farmers," one entomologist reflected. If paid villagers could be trained in the IPM technology, then could their neighbors and kinsmen master the technology? The entomologists had gone to great lengths to devise tools, such as the light trap, which were cheap and easily made, only to be told by farmers that they were too tedious to use.

The entomologists switched first from quantified measures to rough ball park averages (Table 2), then abandoned quantification altogether for qualitative measures such as *eyeballing* the field once a week. Soon farmers expressed a lively curiosity about aspects of the technology that stimulated their imagination and made intuitive sense, such as protecting the natural enemies of rice pests. Scientists realized how complexity intimidated farmers and suppressed their genuine enthu-

Table 2. Research technician and farmers' modification of rice insect pest economic thresholds from quantitative units to those more familiar to farmers.

Pest	Economic thresholds	
	original	Modified
Whorl maggot	15% damaged leaves 14 DT.[a]	6 damaged leaves in one hill 2 weeks after transplanting.
Caseworm	15% cut leaves.	50 cut leaves observed by crouching in one location in the field.
Stem borer	15% deadhearts before panicle initiation.	8 deadhearts in one hill before panicle initiation.
	5% deadhearts after panicle initiation.	3 deadhearts in one hill after panicle initiation.
Leaffolder	25% damaged leaves before booting.	8-10 damaged leaves in. one hill before booting.
	15% damaged leaves after booting.	5 damaged leaves in one hill after booting.

[a]DT = days after transplanting.

siasm. In two of the project study areas, the scientists' barrio research technicians were able to move farmers up to more sophisticated use of the technology once they had gained confidence through the simpler guidelines.

The entire team learned not to take farmers' motivation and interest in technology for granted without weighing what each component means from their viewpoint. The new rice technology has spread so rapidly in many wetland irrigated areas of Southeast Asia that scientists have become spoiled — and perhaps too self-assured. When something is not adopted they are loathe to blame themselves. That farmers might not want to exert themselves to carry out the experiments to the same degree that paid villagers would surprised the scientists. They asked the anthropologist rather sharply, "But what are farmers doing all day during the off-season that they don't have time to monitor their pests properly?" Her answer was, "Nothing, they aren't that busy. But IPM is a lot of bother." The incentive to save $50 a season was simply not strong enough, all things considered.

Crop management

Farmers were aware of a number of management practices for pest control:

- using resistant varieties,
- irrigating (to flood out mole crickets) in midseason,
- removing infested plants and weeds,
- using plant parts as repellants, and
- synchronous planting.

Except for a rather loose form of synchronous planting imposed on them by the irrigation system, farmers were not implementing any of these measures in groups over large fields. They knew that weeds along one farmer's field might harbor pests that could damage his neighbors' crops, and they were painfully aware that when one farmer delays land preparation and planting long after his neighbors, his field is apt to be entrapped by those around it at later stages of crop growth. Access to his field by buffalo is difficult or access to water is impossible when the adjacent fields are ready to drain before harvest.

Since resistant varieties have protected the farmers from serious pest outbreaks for more than 5 years, they are not inclined to master new forms of social organizations to protect themselves from a threat they do not feel. Even in the developed countries farmers have rarely accepted the far more intensive IPM techniques until they have suffered a disaster.

On the whole, the bottom-up community organizers have been only partially successful in prompting the farmers to attempt group crop management over some 150 ha. It is still difficult for the entomologists to appraise the technology on a large scale, but the 150 ha does provide them with fields large enough to test whether group crop management reduces infestation or lowers costs significantly. The anthropologist has recommended that the research project be continued until the permanence and replicability of this initial success can be determined.

Two factors suggest that the project may be short-lived:

1. The one area in which the farmers have taken an interest in group crop management was organized by the most seasoned community organizer.
2. Most of the area overlaps a critical drainage problem, which has given farmers

more dramatic and palpable incentive for departing from their traditional forms of social organization and collaborating as field neighbors.

Once again the anthropologist, living with the farmers at the village, sees how agriculture in the field crosses the scientists' disciplinary specialization. From the farmers' experience, solving the technology of drainage may facilitate experimenting with the technology of pest control.

The team's efforts to promote group crop management for IPM has led to certain changes apart from the success and possible enlargement of the present area. When the team began to realize the difficulty of organizing farmers, the entomologists started to ask whether much would be lost by not consolidating fields under single management, especially because the irrigation system imposes a loose form of synchronous planting. They have now resolved to address this issue as well as its corollary — how large an area should be farmed together for optimal results? The entomologists are finally responding to a question the anthropologist has been asking for 3 years and farmers began asking soon after they started interacting with the scientists.

Packaging the technology

The scientists began to realize the importance of packaging the technology. The farmers did not directly demand this of the scientists, but unwittingly did so by requesting the scientists to conduct a pest management course in exchange for their willingness to try group crop management. The entomologists drew up a 13-lesson farmers' class in the barrio and sponsored it 7 times during the study.

The entomologists and the principal research assistant came to recognize the highly beneficial discipline that packaging the technology for farmers' classes forced upon them. What began as a vehicle to persuade farmers to test the technology soon became a creative challenge in itself, requiring the scientists to understand their own technology so thoroughly that they could apply it in the field.

The anthropologist led the interdisciplinary team to explore how cultural differences influence the perceptions of individuals. The anthropologist was increasingly aware of the close link between technology and management. She realized how cultural differences between the farmers' management approaches and those that the scientists had in mind for the IPM technology must somehow be compromised if a useful technology were to be developed. Western trained scientists perceive the agroecosystem as an assemblage of linear processes or components, which can be observed and manipulated in isolation. Indeed, the explicit goal of the entomologists' research at the experiment station is the development of IPM *component* technology.

In contrast, Asian farmers and business managers do not break down phenomena into discrete, linear processes. Rather, their management perception is of the contextual whole of the system and balancing its parts. The developing crop is a series of multiple impressions, not a flow of serial processes. The Western notion of cause and effect, damage and yield loss may be less central to such contextual thinking, which sees the crop, water, weather, insects, weeds, frogs, and damage symptoms as a whole.

The distinction calls for the scientists to move from the general, abstract, and

future orientation to the immediate and concrete. From visits to other Asian IPM projects, the anthropologist learned that IRRI was the only one teaching IPM with such abstract tools as printed handouts, classroom instruction before field visits, and efforts to prepare the farmer for the unforeseen. Asians teaching IPM in Thailand, India, and elsewhere in the Philippines interpreted the technology to the farmers by repeated visits to farmers' fields. They introduced IPM at the pace at which nature and farmers raised problems.

While classes helped the IRRI scientists and farmers become acquainted and began a process of vocabulary sharing, it became clear that the technology should provide the farmers with much more concrete, systematic. and equilibrium-focused management practices and perceptions. The anthropologist's recognition of the effects of cultural differences on perception and management. and her efforts to describe these differences, metaphorically when necessary, were indispensable.

The entire research team now sees agricultural research in the Third World in a completely new light. Even if new technology could be handed over to efficient monitoring and extension services for implementation, the exchange between the farmers and the scientists would still be essential to the process of technology development itself. It is difficult now to imagine any technology as in-place before scientists have involved themselves in extension efforts. In retrospect we realize that scientists all too often drop out of the process of technology development long before it has been completed. The discipline of bringing technology fully to the level at which farmers can use it should be central to all research in technology development.

PROBLEMS OF INTERDISCIPLINARY RESEARCH

One major problem the scientists on the team had was taking the anthropologist seriously, a problem that persisted throughout the project. Conventions for establishing biological conclusions stress statistical analyses of controlled experiments. Conventions for establishing anthropological conclusions involve repeated observations, cross-cultural comparison, and verification. To a biological scientist analysis always requires quantification. Prose is condescendingly considered as description. To an anthropologist analysis requires only that the scientist satisfactorily address the questions of how and why. Statistics often seem merely to document what we already know, or circularly, what the scientist set out to prove rather than what he actually found. Charts and graphs may be used as gaudy shields to hide the lack of common sense. The anthropologist seems far more willing to give the biological scientists the benefit of the doubt than vice-versa.

When an entomological survey asked farmers to identify insects from pictures or pinned specimens, the scientists concluded that those who could correctly name the insects knew them and the damage they caused. But when the anthropologist insisted that aksip was a brown or whitish discoloration near the water surface having to do with storms, irrigation, and fertilizers, the entomologists dismissed her observation as farmers' ignorance of what scientists were sure from their surveys to be whorl maggot.

Indeed, it required the entire 3 years of the project for the scientists to begin to take seriously questions such as whether farmers know or do not know whorl maggot,

whether they perceive it as a part of a more complex phenomenon, and, if so, how the technology should be adjusted.

When the anthropologist predicted that light traps would not work because they were too tedious, the entomologists' response was, "show us statistics." The insights growing from her emergence in village life seemed flimsy and even mystical to the entomologists and prevented them from heeding ex ante warnings that often were correct. That the anthropologist was often proved correct instilled in the team entomologists confidence in her suggestions, but still they were poorly equipped to justify their acceptance to other biological scientists who demand proof in terms of biological conventions.

The most frustrating situation for the anthropologist was to receive the reply, "Sorry, Grace, that's just not possible," when she suggested that technology be developed along certain lines and when she asked earthy questions that the entomologist could not answer. As Table 1 shows, the pest control measures recommended by the entomologists were supported by the literature rather than by their experience. The scientists tended to pass off cracks in their cases as socioeconomic problems. "Get the farmers to plant together and then we can tell you how large a consolidated field is necessary."

The anthropologist at last resorted to putting one of her queries into a trip report after visiting IPM projects in India and Thailand. "What is the minimum size of a group crop management area for effective IPMT?" This written challenge to IRRI entomologists sparked a 7-member research project to try to answer the question. Because her question was uncomfortable and the entomologists did not share her perspective nor that of the farmers, it took a formal presentation to push them into action.

Shifting the focus of the team's problems now to the anthropologist's contributions, the staff found that while the entomologists made many miscalculations in devising their original technology, the anthropologist had miscalculated her technology as well. She too operated on some fairly tenuous assumptions. Although she was fully aware that Western forms of organization are rarely found in Southeast Asia and that social and political organization are not easily changed, she did not take advantage of her own knowledge. On the organizational side then, she assumed too readily, just as the entomologists had, that Western models could be made to fit the field challenge at hand. The team's experience suggests that biological scientists too can err in placing too much faith in social science.

Neither the anthropologist nor the entomologists were sufficiently familiar with the givens of the challenge to accurately evaluate *what* should be required, *where* problems would arise, and how *serious* the problems would be. Had they been required to submit a feasibility study, it would have proven to be far off the mark. What the entire team expected to be the *final stage* of technology development turned out to be the beginning of the process instead.

A second bias that the anthropologist brought to the project was that one cannot learn much about farmers' real attitudes, perceptions, and practices from surveys. One of her most vivid first impressions of IRRI had been shock at the extent and manner in which Institute economists frequently surveyed hundreds of peasant households over the large areas on detailed matters (*detailed, frequently,* and *large*

all measured by anthropology's norms) without being in the least disturbed by the liabilities of the method.

In addition, the anthropologist had never worked in Southeast Asia before and had not had the opportunity to read the anthropological literature about the region before her appointment. Feeling that she had no foundation on which to judge what to believe from Filipino farmers' reports of their behavior (especially when labor and money were involved), and still reacting strongly to IRRI's unquestioning confidence in the survey method, the anthropologist's bias for seeing rather than asking was strongly reinforced. This was particularly true when investigating central questions about farmers' organizations, with the knowledge that these had become virtually a national fad during the past several decades.

The anthropologist's professional bias to work intensively at a microlevel, at least at first, was greatly strengthened by the research conditions and challenge she faced. Far from being willing to take on survey research, she considered her professional skills stretched to the limit in having to study three villages at once. To a great extent the central research question that IPM development posed called for this intensity and was appropriate to the discipline. To determine whether Filipino farmers could learn to work together in a sustained way along lines of common interest rather than through personal bonds, one would have to know the members of seemingly successful groups quite well.

The anthropologist acquired a working command of Tagalog and of the ethnographic background of each of the villages. She formulated a theoretical understanding of rural social organization for the region and appraised the top-down and the bottom-up approaches within this context. She played a unique role in the development of the new technology by helping the entomologists' understand farmers' agronomic practices and their constraints.

One must, however, explicitly recognize the importance of the microlevel and highly concentrated methodology in anthropology, at least for an anthropologist unfamiliar with a given culture and working on technology development problems that require fine-tuned monitoring. This methodological requirement is fundamental not only to the discipline, but to professional legitimation among one's colleagues and vis-a-vis others (above all, as a pioneering anthropologist in an international agricultural research center).

Yet this methodology poses a serious initial constraint to interdisciplinary work. It means that anthropologists cannot easily leap from one culture to another without a rather intense transition period. And until this period has passed, anthropologists are in no position to produce the vast quantities of information, much less the statistics that institute scientists require as affidavits.

Working in an intellectual environment hostile to qualitative analysis, especially when the hostility comes from social scientists, the anthropologist is seriously handicapped in basic communication skills. Actually this problem never arose with her biological science partner, presumably because he could verify her conclusions for himself in the field. But the problem continually arose at Institute headquarters.

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Even her economist colleagues frequently inquired, "How long does an anthropologist need before he or she can start making a survey?"

The point is that regardless of its intellectual merits and its contributions to the interdisciplinary team developing new technology, the anthropologist's traditional methodology may be viewed as inappropriate within the larger sociopolitical context of many research institutions.

A new reality: Western technology faces pastoralism in the Maasai Project

Jon R. Moris and Colby R. Hatfield

The Maasai Livestock and Range Development Project arose from a marriage of national and international concerns. The Food and Agriculture Organization had warned of an impending worldwide protein shortage in the 1970s, which timely livestock improvement in the drier areas of the world could help avert. A resurgence of ecological concerns re-awakened a dormant history of fears that the tropical grasslands of the world — in this case, East Africa — were being misused and destroyed. The Tanzanian Government, in its first development decade, was seeking ways to improve its foreign exchange situation. International marketing of beef, canned in Tanzania's Dar es Salaam plant, would be another step toward self-sufficiency. Finally, attempting to transform traditional Tanzanian cultures, the government was particularly anxious to find better means of accelerating the entry of its pastoral peoples (for us, the Maasai) into a modern economy.

All these issues appeared solvable through the adoption of modern western ranching practices. In Tanzania, Maasailand was one of four designated areas where such inputs were deemed feasible.

The Tanzanian Range Development and Management Act of 1964 (hereafter called the Range Act) established a range management division in the Ministry of Agriculture range commissions at district levels — mechanisms for incorporating stock owners into ranching associations with status similar to cooperatives — and gave commissions and associations legally binding powers over areas targeted for range development (Fallon 1963). By 1967, the range program in Maasailand had shown little progress and the Tanzanian Government requested USAID to review the situation. The USAID report emphasized sociological factors as base problems in implementation. Recommendations for expanded USAID involvement included an extension-sociologist among the four positions composing what was to become the Maasai Project (Deans et al 1969).

The project was to develop and integrate a package of structures and skills aimed

Associate professor of anthropology, Utah State University, Logan, Utah, USA; and visiting associate professor, University of Denver, Denver, Colorado, USA.

at improving livestock production and range preservation within the framework of the Range Act. The initial team included an animal production specialist, a range ecologist, a livestock marketing specialist, a water development engineer, and an extension-rural sociologist. By the end of 1978 the team consisted of 10 expatriate specialists, none of whom had been involved in the early stages of project implementation, nor in its planning.

Specific inputs from the team were:

- providing stable water supplies,
- upgrading livestock,
- establishing veterinary structures and an outreach animal health program,
- re-establishing a marketing system,
- developing modern range management practices.
- organizing new managerial structures, and
- building a center for training association members in modern ranching.

Ten years after the establishment of the Maasai Project, its goals had not been realized. A full understanding of the factors working against successful implementation of all project goals would require a lengthier history than is warranted here. Nevertheless, a discussion of the role of the social scientist in the development and implementation of the technologies within the project can give some understanding of the constraints.

We will begin by discussing the structures of two interrelated technologies: scientific and managerial. Then we will explore the various roles the social scientist played in these technologies over the project's lifetime. Finally we will discuss some of the conflicts that arose from these tasks. We hope this exploration into an only partially successful development project will provide a matrix for understanding more universal problems of developing and implementing appropriate technologies for the Third World. Further, we hope our experience will provide guidance for social scientists on other development teams.

TECHNOLOGY DEFINITION

We wish to consider technology in a broader sense than what was perhaps originally planned for the workshop. A technology is more than a material object (a seed, a shovel, or tractor) or, for that matter, a collection of objects. Technology includes procedures for using these objects. And the procedures themselves are constituents of a broader set of implementation structures.

From this viewpoint a research organization, a technical department of a government ministry, or a development project can be considered technologies — collections of techniques (including personnel) used to achieve certain goals and focusing upon the development of specific tools to exploit an environment.

These structures also carry assumptions about what they can and cannot do. Some assumptions can be stated explicitly as limitations of the scope of the instruments. Others are implicit in their use, characteristics which those who have developed them take for granted. At no point in the development of a technology can its three constituents be ignored. The tool, technique, and its implementing structure must be calibrated to produce maximum efficiency.

Simultaneously the technology has to fit into an even broader set of structures: a farmer's practices, a village network of cooperation and land use, a national system of economic and social linkages, or a set of national policies. In the context of the Maasai Project, action had to conform to a wider set of constraints forming the premises and policy boundaries of its funding agency and to international development in general.

In taking this view of technology, we intend to obscure a common distinction between the physical tool and the social structure developed around it. Both have explicit and implicit characteristics that ultimately determine their usefulness in the field. Many of the problems in the Maasai Project derived from the inappropriateness of its various tools, which, for the most part, were exempted from interference by the social scientist. His role was viewed as dealing more with the alien social structure into which the technology was being integrated. In the development of new technologies, we must be as concerned with the structures that have fostered their growth as with the structures that will have to carry them into alien situations. The social scientist should be involved as much in the creation of a new technology as in its implementation.

We also distinguish between (for lack of a better term) a scientific technology and a managerial technology. Scientific technology isolates a set of tools and techniques related to a given material means of exploiting an environment. Our primary example will be the technique of range management. Managerial technology is a set of organizational tools and techniques that are developed to support a scientific technology or a particular organization.

In the early phases, technical specialists in Maasailand viewed technology transfer mainly as adjusting tried-and-true solutions to the local scene. This was also partly the case when introducing various social technologies. For example, range specialists did not attempt to introduce new species of grasses into Maasailand, but they did attempt to upgrade existing forage through management planning. Livestock specialists could not develop new breeds more tolerant of arid conditions than the local Zebu cattle, but they did introduce genetic improvements by offering producers a variety of exotic livestock, most of which were genetically rather close to the local breeds. Water technicians made few attempts to develop water storage other than dams, charcos, and deep wells.

Some of these improvements were not new to the Maasai. In some areas deep wells had been in existence since late colonial times. Pumping had graduated from hand or wind power to the diesel engine. Some dams that had been constructed during the colonial period were still in use. There was, however, little local input to animal health until the project began. Some dips existed, but the value of dipping livestock against east coast fever and other tick-borne diseases was not widely recognized. The Maasai's main approach to disease control was to try to avoid infested areas. Livestock dipping produced dramatic results. Calf mortality from east coast fever decreased by almost 90%, enlarging a farmer's herd substantially in practically one season.

Range management, as defined by American specialists, was unknown and in some instances directly violated the Maasai traditions of open range and flexible adjustment to the weather of the Maasai steppe.

Although the ranching association structure converged with the Tanzanian policy of communal development, most of its organizational requirements were unknown to the Maasai. Certainly the possibility of having legal usufruct to land was new. Intensive cooperation on a larger scale than the engang (kraal camp), neighborhood, and water using group was not known, especially for the purposes mandated by the Range Act.

SCIENTIFIC TECHNOLOGY: RANGE MANAGEMENT

We shall explore the explicit and implicit premises of range management as an example of a technology crucial to the successful implementation of project goals, but which initially was inappropriate to the Maasai environmental, economic, and political circumstances. It is more or less possible to do this for all of the technical skills, including those of the social scientist, and find similar cultural biases that are constraints to successful technology transfer.

Range management is the science and art of planning and directing range use to obtain the maximum livestock production consistent with conservation of range resources (Abercrombie 1974). To the technician the explicit premises embodied in the discipline appear self-evident:

- A certain number of animals depend for their livelihood upon the vegetation of a territory.
- Forage consists of a community of plant species, which in tropical environments are often considerably more complex than in temperate zones.
- When too many animals are kept in one place too long, they overgraze more palatable and nutritious species.
- Under extreme misuse the plant community will degenerate into bush thicket and hardpan soils.
- Carrying capacity will diminish steadily under chronic overgrazing.

But there are a number of implicit premises upon which successful range management practices rest. Some technologies are relatively self-sufficient in that most of the ingredients for successful implementation are part of the specific package. Unfortunately, western range management is not one of these. Its technology depends heavily on natural, administrative, and commercial environments, which are not explicit requirements of the intervention program. In practice the anticipated benefits will occur only if these environments approximate the implicit conditions built into the technology. The difficulties that the Maasai Project experienced were only partly derived from the cultural features of the Maasai people. To a much greater extent they represented intrinsic weaknesses of range management approach, which was based upon six premises appropriate only to the western U.S. cultural setting:

1. There is a substantial identity of interest between the livestock producer and the person who actually manages a range.
2. The approach yields optimum long-term rather than short-term returns.
3. Drastic meteorological fluctuations are absent or there are sufficient techniques for coping with them.
4. Livestock producers are oriented toward production for profit rather than for subsistence.

5. There is baseline information about livestock numbers, plant communities, herd structures, rainfall patterns, etc.
6. External supports exist to control land use.

These implicit premises proved stumbling blocks to implementing range management in the project to some extent even on lands managed by a range technician as opposed to association lands governed by the Range Commission and resident members. Some of these obstacles were eventually overcome, but until they were understood, progress in range development was effectively stifled.

MANAGERIAL TECHNOLOGY

Structures and professionalism

Managerial technologies also have components that can be inappropriate in settings different from that in which they were developed or settings in which the realities differ from the ideal for which they were designed. Managerial procedures are not universally applicable, nor equally effective in all settings (a viewpoint called the contingency approach in American management theory). The social scientist views these management structures and processes as forms of social implementation that are derived from specific cultural traditions to meet specific needs. This attitude is not universally shared, particularly by technical specialists who are normally trained to believe that their operational systems are universally applicable.

For example, U.S. management techniques tend to assume open access to information, the desirability of high standards of information quality (irrespective of cost), and the feasibility of intricate time scheduling. These are the organizational correlates of high technology developed to serve western nations since the 1950s. A similar problem concerns technical procedures. The technical specialist in the U.S. is surrounded by complex supportive procedural and material mechanisms. He may find it extremely difficult, even personally threatening, to have to modify customary work habits and do without customary equipment to suit field conditions. The technical specialist considers these paraphernalia absolutely necessary. Much of the tension in interdisciplinary teams revolves around the issues of how much compromise in standards is necessary.

Most individuals, including social scientists, learn that the job can get done with minimum damage to their sense of professional integrity. But for some — paradoxically for those who appear to be most professionally oriented — compromise is so great an adjustment that their very ability to work under the new conditions is impaired. Anthropologists call this culture shock. Culture shock applies with peculiar force to those very parts of the professional and managerial realm that their proponents would consider core and beyond the influence of culture.

Maasai Project managerial technology

The Range Act was replete with structures and techniques that formed the basis of project activities. But some of those structures actually worked against efficient implementation. Some were in conflict with other structures and were virtually ignored by Tanzanian regional administration. The technical and legal requirements for registration of the ranching associations were so difficult to meet that most technical specialists had to find proxy means of accomplishing their tasks. (Lack of

adequate baseline data was a major constraint here.)

The ranching association concept also presupposed a number of on-site organizational characteristics that simply did not exist in most of the areas designated for range development. Although it was based on a cooperative ideal, there really were no links between it and another Parliamentary act dealing with the formation of cooperatives. Finally, the whole notion of a ranching association was passe because at the time the project initiated field work, the Tanzanian Government was moving toward socialist production through the establishment of Ujamaa villages that concentrated on smaller land units and population concentrations around technical and social services such as dispensaries, schools, dips. shops, and permanent water.

The Range Commission, which was created to administer range development, had neither a precedent, nor a place in the regional administrative apparatus. It eventually faded into obscurity.

Mandated originally as a national effort, the Maasai Project was in theory directed from the Livestock Division of the Ministry of Agriculture in Dares Salaam for technical matters, and by the Range Commission for policy and legal matters. Unlike other national projects, it had a major field component and had to develop considerable credibility with local government even though there were few real links with the system at that level.

In mid-year the project's national status was eliminated as a result of a reorganization of Tanzanian field administration. Planning and finance functions were put under regional and district authorities. The project was placed in a new Livestock Division under the direct supervision of the District Livestock Development Officer at Monduli. Later, Monduli was divided into three separate districts and the project was moved up to the region. Project staff then ansuered to three sets of district administrators, regional officers, The Livestock Division, and USAID and its contracting agency.

In theory USAIDs in-house annual review and 3-year project evaluations were the mechanisms through which project work would adjust to changing political, social, and ecological realities. But the main problems of the original design of the Range Act and the project's mandate were never completely resolved. Five years elapsed before the project was given direction on integrating the formation of ranching associations with the Tanzanian government's villagization program. This may seem trivial on the surface, but it had tremendous implications for the scope of work, both technical and organizational, that project staff was expected to do. Of course, no field staff would work for long under these constraints, so the staff had already partially conformed to the realities of local administrative life. They had been cooperating in the resettlement programs despite official requirements that they concern themselves solely with ranching associations long before this was reflected in USAID documents.

A beef production bias also remained with the project throughout its lifetime. The entire edifice of pastoral development and foreign aid was based on it and it could not be altered. At one point any mention of dairying development was literally taboo. No project staff member was able to successfully express his concerns about the appropriateness of this production emphasis to the Maasai situation either verbally or in writing.

Tanzanian managerial technology

Maasai project problems with Tanzanian managerial technology stemmed primarily from project development and implementation structures that did not fit into the inflexible Tanzanian administrative system, and in at least one instance were inappropriate to local economic conditions. But if project structures could not adjust to Tanzanian conditions, neither could the Tanzanian administrative structure itself.

The decentralization of planning and integration of local development had built-in weaknesses. Here, again, it was a case of attempting to superimpose Western managerial technology on a developing country. The Tanzanian local development strategies had been created by a U.S. management consulting firm whose expatriate staff had designed procedures for regional Tanzanian planning officers.

The U.S. managerial technology was oriented more toward new projects than toward developing the functional capabilities of local administrators to intervene effectively at the grass roots level. Funding focused on initial capital investment in local projects, not on the more necessary examination of and continuing support for recurring costs. There was no provision made for establishing a data base that was required in the use of complex planning forms. Nor was any provision made for cross linkages of local investments to ensure they were beneficial. None of the managerial procedures contained the necessary diagnostic components to delineate the strategic role of any proposed development activity.

All these structural flaws were masked by an apparent comprehensiveness of the managerial technology. The planning structure gave the appearance of having actually tapped local problems and having provided adequate implementation procedures and bonding linkages. But like the superficially comprehensive Range Act and the overtly complete Maasai Project design, the planning structure was unrelated to the actual field situation. Like their expatriate counterparts, Tanzanian administrators and planners engaged in visions and revisions of work plans and schedules without confronting the questions: How will it work? What are its effects? How will it be maintained? How will it fit into other programs? The managerial devices were geared to solve problems without asking what the problems were. This tendency sifted down through the system to the field officer, who created elaborate but unrealistic work plans.

ROLES OF THE SOCIAL SCIENTIST

The Maasai Project enjoyed a substantially greater social science input than most other technical projects of its generation. USAID's preliminary survey report explicitly recognized earlier anthropological research and from the onset included an extension-rural sociology position within the project field team, but from what appears to have been the wrong reasons.

Technical assistance projects in livestock development began to add social scientists to field teams in the late 1960s in the expectation that they might counteract a blatant polarization once new production technologies were introduced to the people. Livestock producers considered technicians grossly uninformed about details vital to herd management. The range technician, on the other hand, observed what he could define only as irresponsible site use requiring immediate remedial

action. But his proposals were ignored, evaded, or refused. These reactions confirmed the technician's view that pastoralists shared an underlying conservatism about livestock management that placed them beyond the reach of rational (modern) practices. It did not occur to the technician that there might have been local constraints or that his theory was defective when applied to non-Western field situations.

Technical aid projects expected anthropologists and rural sociologists to probe for factors underlying pastoral conservatism, identify indigenous corporate groups that could be used as bases for action programs, and persuade the pastoralists of the value of the measures recommended by technicians.

Formulating Maasai Project technologies

Although the survey report preceding the project anticipated several features of Maasai lifeways and technology that later emerged as difficulties within the field program, these were not reflected in the project design. For example, the report pinpointed the importance of sheep and goats to Maasai economy and the Maasai's orientation toward dairying with regard to cattle. It also warned of the egalitarian decision-making structures that make it difficult to maintaina sustained sequence of innovations in other than small face-to-face groups. But these and other insights were apparently ignored at the project formulation stage, with the consequence that the Maasai Project was defined officially as a more conventional beef ranching venture.

The original mandate under which the project operated did not include any social science inputs although it was to be a vehicle through which the Maasai would be modernized. The Tanzanian Government emphasired to its original agronomist that it wanted advice ". . . on development requirements for the Maasai people," a phrasing that reflected how officials viewed future program goals. But this social intent was translated into a specific technical goal, "a sustained high level of livestock offtake," measurable by increases in average liveweight of slaughter steers, increased calf drop, reduction of calf mortality, etc. Redefining a broad goal with proxy goals is what sociologists call goal displacement, a process by which the specifically defined means of achieving a goal eventually preempt its redization over time. Unfortunately, the inversion in this case occurred at the very onset, in the framing of project targets. Displacement takes place when technicians simplify means to goals to fit them to their professional conventions. Substitutions are then made routinely. Rarely is there feedback to determine whether the larger goals are still attainable in the altered setting.

The developers saw no problem in replacing the broad Tanzanian goal of development of the Maasai with a more easily measured proxy objective: achievement of sustained high offtake. Ideally, high offtake of livestock through sales implies increased participation of the Maasai in a market economy and a transition from subsistence production to modern, profit-oriented livestock ranching. But the intermediate steps and interfaces with other developmental segments in Maasailand were not specified. It was assumed that they would fall into place, but they did not.

Scientists often view the sociopolitical component of a project as the final variable to consider. Having formed a trial package, the technical expert expects that either it

will automatically weather the tribulations of on-site reality or that the social scientist can 'fine tune" it to a particular environment. The social scientist and the recipients of the new technology, then, are given the responsibility of integrating it into the existing system. Inadequate implementation or ingrained local conservatism is most often blamed when projects fail, not the inappropriateness of the package for the setting in which it was introduced.

Sociopolitical constraints constitute boundary conditions that vitally affect program design at the onset. In one's own society they are masked by management's familiarity with the setting; they tend to be implicit in project design. Characteristically they are ignored when the design is applied overseas.

In spite of the neat appearance of duties and responsibilities dictated by the Maasai Project design, we found that we were actually operating along the juncture of four conceptually distinct systems: the natural production cycle of livestock keeping, the Maasai sociocultural system, the Tanzanian administrative system, and USAID. Each had built-in constraints, some of which were not modifiable in the short run — the start of the financial year, for example. A combination of all four systems left project members little room to maneuver. Even apparently neutral technical decisions associated with the natural production cycle carried pronounced, political ramifications for all other systems. Technical specialists often find it extremely difficult to accept such constraints as being real in the same way that kilos of forage per hectare is real, or else they view them as insignificant or someone else's responsibility (Ilchman and Benveniste 1969).

Once a project is designed with only ideal or implicit judgments as to how the various systems will interface, its extension staff will suffer the consequences of trying to operate with inappropriate mandates. The social scientist, trained to view the entire process as one of the system in interaction, may be put in the awkward position of defining what should have been done.

The first anthropologist on the Maasai team found himself precisely in that position. The essential rules for operation had been formulated when he arrived, and a locally-organized ranching association was experiencing severe difficulty. He contributed what constituted unwanted and tardy advice about the fit between project technological inputs and Maasai social dynamics. He also warned of the team's inability to integrate its managerial technologies into the Tanzanian administrative and policy structure. That he was correct on both counts did not endear him to his hard-pressed colleagues and superiors who were hoping that the problem resided solely in Maasai opposition to change. The first Maasai project anthropologist had no choice but to point out problems where they existed, but there was no easy way to alter scientific or managerial technologies already in motion. For example, one could not just disband the troubled ranching association and start over, nor could one ignore that some project inputs were aggravating the problem.

Project implementation: data collection and evaluation

Compared to the duties of other team members, the anthropologist's role was relatively unstructured. In theory, this flexibility is an asset. It allows social science input into team activities to evolve and permits in-depth work with particular problem communities. But in practice the wide latitude left to the social scientist can

be a disability, especially as work tends to be more dependent upon individual energy, imagination and willingness to cooperate with other team members than on any standardized set of professional tasks. It also makes the social scientist vulnerable to being captured by extraneous project demands that do not fall into any one staff member's domain.

We have already pointed out that the implicit reason for having a social scientist on the project was to cope with anticipated problems of Maasai conservatism. Although the job included options for assisting technical staff, the initial focus was indeed on collecting information to isolate areas of potential blockage, identifying existing social structures that could facilitate project inputs, and persuading the Maasai to adopt project technology. Aside from these people-oriented tasks, there was no specific sociological component.

The first anthropologist explored the social organization of peoples resident in the deeply troubled Komolonik Ranching Association. He also investigated their attitudes toward the burgeoning national policy of Ujamaa, which required collectivization of scattered populations. His conclusions of necessity touched upon project-government-producer relations and were received unfavorably. Some of the staff thought he was a loner who was not contributing useful information for team activities. Although operating well within his job description and professional prerogatives, he inadvertently established negative relationships with his teammates. Subsequent social scientists had to deal with this climate of opinion. The geographical scope of work and the composition of the team also had expanded to the point where different approaches to data collection and cooperation had to be explored. By the time Jon Moris;the second anthropologist, joined the project, he realized that in-depth techniques of participant observation, which his predecessor had used successfully at Komolonik, could not provide the baseline social information required for work in the many association areas in which the project had become engaged. Simultaneously, the Range Act required detailed population censuses for any group forming an association. They were considered the task of the social scientist. Aside from range surveys, which were performed to form management plans, little attention was being paid to the ethnotechnical aspects of Maasai range use and livestock management. Technical experts on the team generally were reluctant to directly question Maasai about their use of the environment and even considered such questions unnecessary.

In 1973 Moris initiated a field survey entitledrhe sociological census as a complement to range surveys. It in fact overlapped with most of the technical specialties represented on the team: patterns and variations in grazing and water use, use of dipping facilities and attitudes toward animal health innovations, felt needs for livestock improvement, etc. When Colby Hatfield took over the position (Moris became Chief of Party in 1973), he continued the census but modified it to gain more rapid information feedback. Many of the data collected in the original census were not being used by technicians (Hatfield 1975, 1976).

Hatfield's field data reinforced the necessity of adjusting each technical input not only to a particular physical environment but to specific social and ethnotechnological patterns. Although the census revealed that the nomadism of the Maasai in general is circumscribed, there is considerable local variation depending upon

previous establishment of permanent water and upon meteorological fluctuations and disease. The general pattern of wet and dry season grazing also has many local variations. In general, the sociological census showed how the Maasai exploit their environment and perceive problems. The information could be used to develop appropriate technological responses to the Maasai needs and to provide the technician with a clearer picture of the system he confronts.

There is no intrinsic reason why the project anthropologists should have explored the practices revealed by the census. Had some of the early technical staff been more sensitive to the importance of tapping the local level more fully, they might have undertaken this research themselves. Certainly the inherent belief that there was nothing to be learned from an understanding of local livestock management dynamics was a powerful disincentive. But there were other reasons why the technical specialist seldom engaged in Maasai research. Not only was his job description more restrictive than that of the social scientist, but he was caught up in an unrealistic production time frame, given the perceived demands on his skills and the nature of field work. And the Tanzanian Government and USAID were explicitly fearful that team members would be doing *research* and not a *job.* In his capacity as a resource person, the anthropologist was something of an exception. He was expected to "do his thing in the bush" and in some manner produce recommendations, but his teammates were under more specific scrutiny. The range management specialist, in particular, was expected to be grinding out locally meaningful management plans. Finding out what the locals did would not have been considered a good use of the techcian's energies.

Hatfield encouraged a broader approach to technical problems that would make the technical staffs products more locally acceptable. For example, the 1973 evaluation strongly recommended that livestock statistics be collected, mainly to measure the degree to which specific practices were leading to the proxy goals established in the project design. From the anthropologist's view, expression of this kind of interest by technicians would help establish greater rapport and trust with the Maasai. The procedure should also be a teaching device to help the literate producer monitor his herd's composition, its growth, and decline. The program was attempted several times but was never established. The technical specialist's work situation precluded his detailed participation.

The anthropologist also evaluated technological inputs and monitored the development of the Maasai for the Tanzanian Government, USAID, and the project. The work was tantamount to a retrospective field test of project technology and normally set in motion a series of buck-passing. Failure of any project goal ultimately was attributed to local Tanzanian junior staff (who could plead no transport) or the ingrained conservatism of the people.

A program of introducing improved bulls to selected Maasai producers is a prime example. The exotic breeds were Sahiwal-Boran crosses raised on parastatal ranches and research stations in the country. (Hatfield followed up the animals after the initial distribution effort.) The project staff and the government believed that the distribution had failed because stock owners did not know how to care for the exotic bulls or had not followed the rules for their upkeep. The study of this first distribution highlighted the difficulty of mixing a single component from one technological

system into another, even when the systems appear similar (Hatfield et al 1975).

Some of the recipients had not understood requirements for maintaining the new breeds and had simply treated them as they did their Zebu stock. But others went to great lengths to keep their new bulls alive.

Most failed, not because they weren't interested, but because the modern technical facilities they used were defective. One crucial dip was consistently understrength, for example. Nonrttheless, a few bulls had been integrated into the herds, but only because the producers had sought means other than those recommended by the project to ensure their animals' survival.

These data raised questions about easy assumptions concerning local conservatism, but more important, they indicated the pitfalls which any new bull distribution program would have to avoid. Death loss in the second distribution was considerably less and formerly reluctant producers were now eager to have new bulls. In the long term, project recommendations provided the basis for a radical experiment at Komolonik, the introduction of European dairy cattle to selected stockmen. When Hatfield left Tanzania in 1980, the program was proving fairly successful, although it was heavily subsidized through resident technical staff, and owners had invested in a variety of back-up equipment.

In-field technology development and implementation

Social scientist inputs into any project technology were determined by a number of factors. Some activities, because of the way in which their procedures were defined, allowed no room for sociological considerations in the planning and implementation stages. Also, the sociopolitical constraints surrounding a particular input directly affected the social scientist's input.

Water development provides a salient example. Not until late in the project was any sociological component recognized as useful in the establishment of water facilities. Water engineers were too involved in technical and fiscal aspects to find the assistance of the social scientist anything but a nuisance and a delay. Conversely, project anthropologists viewed water development as an exercise in fostering participation, community spirit, new forms of cooperation, and leadership as much as a means of satisfying a physical need. But to bring water development into this broader sphere meant essentially recycling the current planning process, which was never effected. Consequently, social inputs into water development remained minimal.

On the other hand, those specialties that stressed day-to-day management on the local level without major capital development tended to integrate more easily with social science concerns. The human ingredient influencing the outcome of these programs was perhaps more quickly apparent; the disasters resulting from water development were not. Neither was there any chance of water being initially rejected by the producer.

As an example, we can return to the second bull distribution discussed previously. But even here sociopolitical considerations ultimately determined the nature of the innovation. For the second bull distribution phase, the animal production officer had determined bull maintenance requirements that were aimed at ensuring their survival as well as their maximum use in a producer's herd. He had also followed the

explicit government policy that the bulls had to be owned collectively.

The animal production officer viewed the distribution as primarily a technical exercise that did not require interference from the social scientist. He did, however, welcome the anthropologist's company in the field as an observer, especially because his program was not meeting with the producer enthusiasm he had anticipated.

A quick reading of the maintenance requirements, which a Tanzanian field officer was to deliver to groups of elders, revealed a number of problems. The requirements for maintenance of the bulls were relatively simple, but they were couched in an inordinately complex form in English. When we asked the officer the meaning of some of the terms and how he would translate them into Maasai, we discovered that he did not understand the procedures himself. We revised the field officer presentation that night. The field officer also expressed his concern that the Maasai would reject the requirements, especially the one requiring collective ownership of a bull. He was, of course, correct. But at the last moment there appeared no alternative.

The social scientist viewed the local challenge of collective ownership as positive, one that could possibly bear much developmental fruit. The size and make-up of the bull-owning collective were left up to the producers who wanted bulls. The social scientist urged that this flexibility be stressed and that the field officer allow producers time to reflect on the idea while he suggested ways in which the collective could be formed. The new distribution procedure was a golden opportunity to use a technological device to its fullest in promoting the social goals that were part of the project's purpose.

The elders reached the consensus that drought precluded their accepting any new responsibilities. "How can you talk of our accepting these strange animals when ours are dying in the kraal?" one man said. After the meeting, however, at least five of the participants came by the camp privately to explore the possibility of dropping the collective requirement indicating where the real reluctance lay.

We later drew up a more refined document stressing the need for time to make creative adjustments to thecollective rule, but we were disappointed by its reception. To the Tanzanian administration, time was scarce — accomplishment of goals was already taking too long. The animal production specialist was concerned about housing the bulls until locals came up with their compromises, even though the project had a small demonstration ranch. Ultimately the head of the Livestock Division dropped the collective ownership requirement and bull distribution proceeded. We won at least half of the battle. The outreach component was designed to meet the requirements of stock keepers and ensure better management of the bulls. But long-term lessons to be gained in managerial technology were sacrificed for short-term efficiency.

Grazing management plan formulation

Until the midterm of the project, the social scientists'only inputs to grazing management plans were to provide human demographic data and suggest boundaries of grazing units based on existing land use and cooperation. Grazing pattern data from the sociological census were noted, but not extensively utilized. Most plans relied too heavily on the ideal, either that water and health facilities would be established or, that government resettlement of populations would follow certain courses and that

existing or new conflicts over water would be resolved.

We want to reemphasize that a great deal of pressure from above was placed on range specialists to produce and implement management plans. They, too, often found themselves in the social scientists' position of defining what should have been done. That water development was hitting full stride without range inputs and Tanzanian field staff were, for the most part, more interested in animal production than in agronomy, aggravated the situation.

Of necessity, management plans became extremely simple, so much so that some members of the final evaluation team discounted them as irrelevant. On the contrary, this simplification process was the outcome of the growing response of range specialists to the exigencies of the local scene and to their recognition that they had to abandon some of the premises of their scientific technology to do range management.

The sensitization of range scientists to Maasailand culminated with the last technician to occupy this post on the project. After getting acquainted with the project, he proposed informal on-site discussions with village livestock producers to acquaint them with the basic components of modern range management. Each discussion was geared to obtain local knowledge about a specific range use problem and how it was being dealt with. Planners and residents would tour the management unit and learn about the area from range management and local points of view. The technician maintained that the Maasai knew all about range management, but they didn't know how to put their knowledge into an explicit system, nor did they know that there were solutions to problems they thought unsolvable.

The water engineer and animal production officer agreed to make their inputs coincide with the outcomes of the discussions. The anthropologist Hatfield participated in the preparation of discussion topics from the start. (As Acting Chief of Party when the proposal was made, Hatfield was able to pave the way for more intensive teamwork.) The sociological census provided the baseline data. Each discussion was prepared jointly by the range specialist and the anthropologist. The discussion package was field tested and refined.

At the end of 5 months, we decided that the program was ready for broader applications. It had beengeared to obtain fullest particlpation at the local level, and its procedure had been simplified so any trained range technician could use it.

When the village discussions were first proposed, most of the U.S. trained Tanzanian technical staff were openly hostile to the program. They claimed the approach forced them to be extension workers and not range planners. The Tanzanian project co-manager, himself a Maasai, rejected the proposal because he felt it would be too timeconsuming. But at basis was a fundamental prejudice that Maasai producers had nothing to contribute to range management planning — that to include them in the process was wasteful and demeaning of the profession.

Reception of the program on the district and local levels was quite different. District officials had carefully followed the new approach through frequent reports from the range technician. They were pleased with the local responses despite some problems. District officials finally requested that some of their own field staff and representatives from villages be trained in the discussion techniques at a newly established outreach program center at Monduli.

Hatfield observed the extent to which the system was working over the short term in three constituent management units of the Talami Ranching Association at Kijungu in southern Maasailand. Some major problems had been left unresolved because the participants were not yet willing to deal with them. The outcome of the 12 discussions was pictorially represented by simple, colorful maps that were displayed at all the cattle dips and water points. They amply demonstrated to the villagers the results of ignoring their problems and the implications of inaction. For example, in one area the residents insisted that they could easily use pasture outside their boundaries. The management plan, which showed the three management units in continuity, showed that they could not. When the team left, the villagers were discussing what to do next. In the second area, residents took the initiative to deepen waterholes to make better use of a grazing block. In the third area, the team found a general meeting taking place in which a herder was being reprimanded for violating a new rule for resting a certain pasture.

ROLE CONFLICTS AND CONFUSION

All members of the Maasai project experienced conflicts between their official job descriptions and field realities. The social scientists' conflicts were associated with the emphasis upon the collection of basic cultural information, the promotion of specific project activities on the local level, and assisting other team members with their work.

To conform to the Range Act requirements for hard data to register associations and to the informal demand for statistics, not opinions, we carried out field surveys. At least four problems are inherent in the task. In the first place, most anthropologists are trained to work intensively face-to-face in small communities. Quantitative data collection usually forms only one portion of a broader set of techniques used to elicit information about local life. We utilized the questionnaire approach to gather hard data at a high opportunity cost, although with the same effort we could have learned much more about local dynamics by utilizing more qualitative techniques. After 1976 we abandoned the sociological census for a simpler survey that was quick, covered large geographical areas, and conformed to the Range Act requirements.

A second constraint is the generation of unwanted side effects among those who are being surveyed. A questionnaire survey is not a neutral technique when it is introduced into an isolated community, especially where a population may be quite fearful of outsiders' motives. Local Maasai secondary school students who interviewed villagers directly or indirectly promised future benefits to gain cooperation, benefits which neither the project nor the Tanzanian Government could deliver.

Third, because of the ill fit between survey technology and field circumstances in a developing country, the social scientist can be fully occupied with the many routine tasks of surveys, which, in his own country, can be taken care of by others. Survey research in a third world setting is basically a full-time task — a technology difficult to apply in an alien setting. Other team members who were expected to obtain field statistics encountered similar problems. The livestock specialist finally abandoned his attempt to obtain accurate livestock figures for Maasailand through head counts. They changed daily and the effort would have required his full-time presence and

possibly all the transport available to the project.

The fourth constraint is perhaps the most problematic. By using multiple research strategies, and by virtue of his prolonged residence in one area, the anthropologist is able to obtain a fair notion of how validly his surveys reflect local conditions. He is also better able to offer more educated answers to questions raised by the surveys. When he spends most of his time in a supportive capacity rather than being on the spot, the anthropologist severely limits his interpretative skills. However, the statistic, whether or not it is based on faulty or uncertain evidence, takes on the cosmetic of reality for planners who need figures but are less concerned about facts.

Obviously, field surveys reduced the time we could spend in the two other major roles we were asked to play — promoting team activities and project implementation. While a technician is heavily engaged in promoting team activities, the possibilities of his learning how these efforts are being received locally diminish. Similarly, the social scientist, heavily engaged in gathering baseline data or in establishing field rapport, cannot serve as the project's material production liaison officer at the home base as well. As an economy move, USAID had prescribed dual positions, but in no case could the team member accomplish both jobs. The pressure of field duties in each area was simply too demanding.

We want to emphasize that the extension side of project work cannot be accomplished by an extension functionary in the third world alone. Every technical exercise is an extension problem. Thus the technician also has a stake in the extension role. All too often, however, implementation on the project was considered the task of the social scientist because of his contact with the people. The technology to be extended was the province of the technician. Tanzanian range officers consistently said, "We are not extension workers." The social scientist's mandate to do extension work was unrealistic. There were no mechanisms for joint participation in the production of the technology, nor for collaboration in the implementation itself.

Although the social scientist was directed to assist the technical specialist, there was no corresponding mandate to the technician to accept the social scientist's help. Often our only input into technical affairs was to provide baseline data, which a teammate could use or ignore. A mechanism enforcing cooperation was lacking. In this connection we should point out that cooperation from the social scientist can also be minimal. If he defines his role as that of an impartial data collector, he will remain isolated from his colleagues to maintain an ephemeral and unrealistic neutrality. He will collect unusable data and generate unwanted advice. Teamwork requires that compromise be built on a firmer foundation than an implicit assumption of goodwill and cooperativeness on the part of team members.

A number of duties were derived either from implicit assumptions about the nature of social science skills or from our involvement in the project itself. The first concerns the role of public relations officer. Although almost never a stated objective in project design, every project has to cope with what Chambers (1980) called *development tourism.* This ranges from meeting visitors and taking them on tour to preparing reports on any subject any one of the administrations involved in the project happens to be interested in at the moment. As the human specialist on the team, such responsibilities fell almost by design to the social scientist.

A function derived more from working conditions arose from our perceiving difficulties in leadership and coordination in the project. We feel that social scientists, by virtue of their appreciation of the nature of human interaction and institutions have valuable skills to bring to the dynamics of donor-client interactions. They do not necessarily have the requisite skills in managing teammates any more than anyone else. Nonetheless, both of us, at different times, assumed coordinating and administrative responsibilities and were given opportunities to deal with what we understood to be managerial problems. We attempted to:

- strengthen the interface between the project and the Tanzanian Government, USAID, Maasai, etc.;
- provide more realistic job definitions for the Tanzanian field officers; and
- encourage greater in-team cooperation by emphasizing collective effort and collaboration in work plans.

When acting as administrators, we were better able to impose the recommendations that we made in our capacities as social scientists. But we did not reform the managerial structure even though we tried to influence those who could have changed it.

TOWARD A NEW REALITY

We have been discussing the anthropologist's role in a context in which effective development of technology is hampered by explicit or implicit premises inherent in the skills of the Western expert. All too often these skills are culturally specific. When technology derived from one cultural setting is superimposed on a different cultural setting, failure often results.

Remedies are not simple. Certainly *the* remedy is not a miracle worker view of a resident social scientist who can shatter an imagined local conservatism. Sensitization of all those involved in the development process is a basic requirement. Attention must be paid not only to one's hidden biases, but to the skills and needs of the client population. But does this mean that we can avoid the problems simply by ascertaining what the client population does and what it wants and then tailoring our technology to theirs?

Maasai stock owners often rejected technical specialists' reasoning about their livestock and grazing practices. Their experience with their animals and their physical environment invalidated some of the technical arguments. The issue of overgrazing is an example.

Maasai producers knew the results of using one area more heavily than another. Indeed they attempted to balance their use of grazing areas as much as possible. But balanced land use did not mean the same to the Maasai as it meant to the range technician. For example, in the dry Rift Valley, where overgrazing appeared to be greatest, the volcanic soils are unusually rich in nutrients and livestock hold their condition under minimal forage far longer than an American technician would have predicted. Thus, when a technician would recommend that a herd be moved from a range area, the producer would see no reason to hurry. Additionally, Maasai animals feed extensively on certain species of browse that produce new growth just before the rains.

The technician knows from his experience that repeated heavy grazing leads to range deterioration. But many African grasses translocate protein to the root zone during the dry season and, because of root systems up to 6 m deep, can be intensively grazed without permanent damage. Annual grasses recover rapidly with the rains, again seemingly contradicting the technician's prediction of impending doom. And sheep and goats flourish in dry environments where browse has replaced grassland. Obviously the Maasai producer knows a lot more about his environment than he is given credit for knowing. If permitted the luxury of on-site research, the technician can learn to apply his own skills better once they are purged of their Western bias.

But what of the producers? Do they have all the answers? In our experience, the answer was often no, although it is currently more stylish to say yes. The Maasai continuously emphasized that the solution to their production problems — other than disease control — was water. A producer always knew of some additional grass his stock could use *if it were not so far from a water source.* From the air — a vantage unavailable to the average Maasai herdsman — a sacrifice zone of from 10 to 15 miles diameter around each waterhole is obvious. Water input alone will not alleviate problems arising from herd increases. In a sense the producer is viewing his problem as if all other things are equal, which they are not. Even the most acculturated Maasai did not accept that they were worsening their plight by increasing herd sizes without a corresponding increase in offtake. Decreased rainfall and forage appeared so obvious to the Maasai that average decline in range condition tended to be masked. Many Maasai also blamed continuing appropriation of rangelands for national and private uses as a contributing cause of their predicament. These alternative explanations predisposed Maasai stock owners to reject their own responsibility for improving conditions. With water and range becoming scarce their traditional maximization of herd size, which had insured them against great losses in prolonged stress or sudden epidemic, was now working against them.

This situation poses a quandary for the social scientist. What the producers see as the technical solution to their needs is seen by the specialist as a contributing factor to the problem. Moreover, the kinds of solutions that come from developmental policies — managerial technologies — tend to aggravate the situation, first, through their incomplete character and second, through the inadequacy of their linkages with other systems. In our example, water development and the adoption of livestock dipping resulted in massive herd increases, yet the requisite components of the entire package — range management and marketing incentives — were not integrated into the system. Thus the facile assumption that a field program must be tailored to address the needs perceived by the people themselves is challengeable. Who knows best? No one does! We do know that all participants in technology transfer have talents to contribute to common solutions.

In its day the Maaeai Project was thought to be a pioneer design for modernizing pastoral peoples. But it began with what now seem naive assumptions about the capabilities of Western technologies to solve Third World problems. As the project matured, the weakness of these assumptions became more evident. Many problems such as range management were ultimately resolved. But other technologies, because their problems were not revealed early enough or were ignored, because they were poorly linked to other systems, and because they were introduced with inap-

propriate procedures, formed a matrix of inadequacy that ultimately blocked achievement of the project's full potential.

We have mixed hindsight with our own skills in social science to examine the course of the project through the interplay of anthropologist and technician to create and use scientific and managerial technology. The social scientist can contribute to the development process, beyond the application of his skills to specific project endeavors, by assisting in the creation of a new reality for technology transfer. He can offer challenges to the social and technical realities of donors and clients; provide inputs into preliminary project design that would ensure more realistically defined goals, lead to managerial components structured to operate within, rather than in opposition to or in isolation from, extant systems; reveal the necessity for providing firm linkages with other systems; instill the need for flexible design to enable the project to adapt to changing local and national realities; and then ensure continual feedback between donors and clients so that the outcome is truly collaborative. In this view, the social scientist has a role to play in a broad developmental adventure purged of the false phasal isolation of technological design, transfer, and integration, along with the division of labor these imply. The different reality, which we hope will result from this collaboration, will not simply be new, but better and more efficient in meeting the challenges of development.

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Socioeconomic use of aerial survey and intervention testing for interdisciplinary research

C. Okali and K. Milligan

The Subhumid Programme of the International Livestock Center for Africa (ILCA) is concerned with two aspects of livestock production: designing improvements for livestock production systems and classifying areas — and the pastoralists within — that are suitable for a particular improvement. This paper looks at the role played by a socioeconomist in interpreting aerial survey data to assess the value of interventions being tested in the field.

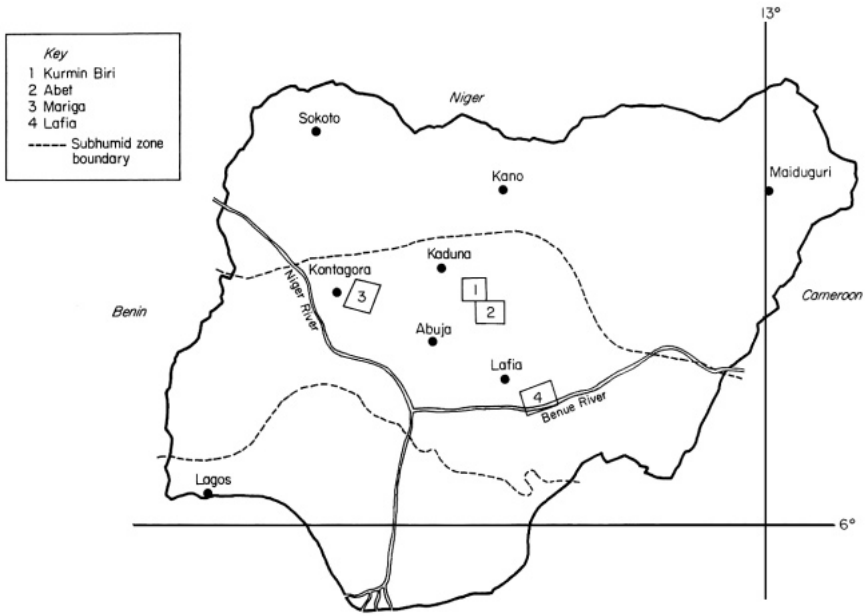
The gap between experimental research and actual farm situations is a major problem of development research. ILCA's Subhumid Programme began its research in 1979, working directly with pastoralists and their herds. The program had no research station and all team members were in daily contact with the pastoralists. The team included a livestock economist, ecologist, pasture manager, agronomist, animal nutritionist, veterinarian, and socioeconomist. The initial liaison work with individual pastoralists was carried out by an anthropologist, the first team member to be employed. The anthropologist was replaced by the present socioeconomist in December 1979.

We decided to work directly with the pastoralists because avenues to increase the production of cattle, milk, and milk products had already been identified. The main thrust of the work has been on improving animal nutrition during the dry season.

Although possible improvements had been identified, they remained to be linked with the circumstances of pastoralists in different locations. This work, identified as classification, was initiated by the ecologist in 1979. Four regions — Abet, Kurmin Bin, Mariga, and Lafia, each 2,500 km^2 — were chosen for case studies (Fig. 1). Each region represented different degrees of population pressure, tsetse challenge, and government assistance. Inventories were carried out by low-level aerial survey (Milligan et a1 1979).

The economics and uptake of identified improvements were tested in Abet and Kurmin Bin. ILCA's role then was adaptive research, quantifying and monitoring

Socioeconomist and ecologist, Subhumid Programme, International Livestock Centre for Africa, Addis Ababa, Ethiopia.



1. ILCA survey areas in Nigeria.

traditional production levels and using this base to gauge the value of observed improvements. Testing of improvements to date involves 30 families with more than 1,000 cattle. This reference frame was expanded in 1980 when the ecologist was joined by the socioeconomist who complemented the aerial monitoring with follow-up ground surveys. In this paper, we use Mariga to demonstrate the use of ground-air linkage to assess the potential value of the interventions at Abet and Kurmin Biri. The work at Abet and Kurmin Biri determined the data collection at Mariga.

TECHNOLOGY IMPLEMENTATION

Most pastoralists at Abet and Kurmin Biri practice a mixed economy of livestock production and arable crop farming. Abet is the most intensively settled and farmed of the four regions (Table 1). The pastoralists' reported traditional dislike of farming is not exhibited in practice. Even women of the pastoral households contribute to farm work, at least by harvesting crops. In some households they assist in planting.

The adaptive research at Abet and Kurmin Biri has focused on the settled pastoralists, those who remain in one place throughout the year although the cattle may be moved within the immediate vicinity during the dry season. At Kurmin Biri, the pastoralists live in a zone the government reserved solely for cattle keeping, but they are not prevented from growing food crops. Pastoralists moved into this zone as early as the 19th century. Many with whom ILCA works were born there (Philippson 1979).

Two common principles of pastoralism exist in Abet and Kurmin Biri. There are no individual or group rights to grazing land or water, and no restrictions on where

Table 1. Summary of land use and vegetation in four regions of the subhumid zone in Nigeria (Milligan et al 1979).

Land use	Region			
	Kurmin Biri	Abet	Mariga	Lafia
	Extent of agriculture (%)			
Cultivation	15	24	13	12
Fallow	15	33	16	13
	Human habitation (km^2)			
Fulani huts	0.4	2.5	1.4	1.1
Arable farmer Compounds	0.5	2.5	0.9	0.3
Tin: grass roofs	1:100	1:8	1:100	1:56
	Extent of vegetation (%)			
Woodland	14	8	18	5
Tree savanna	35	13	36	34
Scrubland	21	22	17	36
Canopy cover	14	11	12	10
	Water availability			
	1.09	0.71	0.43	0.69

animals are herded or moved. Therefore the head of the stock-owning unit is free to determine the disposition of the herd in the pastures. Despite these principles, seasonal concentrations of livestock producers and cattle occur. Because the settled pastoralists are involved in arable crop production, they graze their cattle on the crop residues during the dry season. In the relatively intensively settled region of Abet, the grazing of crop residues extends to the farms of the arable crop farmers who often pay the pastoralists for the manure.

Other links between the pastoralists and arable crop farmers include the exchange of food; the products of both economies are eaten by herders and cultivators alike. The pastoralists depend heavily on the arable crop farmers for income derived from daily milk and butter sales. This is true even for the pastoralists living within the Kurmin Biri reserve. Arable crop farmers purchase animals for slaughter from the pastoralists.

Improved animal nutrition is sought through supplementing the usual forage with forage legumes and agroindustrial products such as molasses and urea, groundnut, and cottonseed cake. Forage legumes were planted on the crop farms of the pastoralists, preferably on land that was to lie fallow the following year.

Cattle were fed supplements only during the dry season when the reduced quantity and quality of rangeland grasses decreased milk supplies. Only animals that could provide the maximum immediate demonstration effect received supplements, in this case, lactating cows.

The feed was weighed daily and delivered to each cooperating pastoralist. Because the whole herd was usually present during the dry season, separate corrals were built for the animals selected.

Team members were concerned about different aspects of the technology includ-

ing the delivery system. Interdisciplinary exchanges led to changes in the way the feeding program was implemented. In the long run, improved nutrition will result in healthier animals and more calves.

AGROINDUSTRIAL BY-PRODUCTS

The animal nutritionist was primarily responsible for use of the agroindustrial by-products as feed. His concern was for the benefits of additional feed, including milk output, animal health, and fertility, to be demonstrated. That proved difficult given the strict rationing program adopted by ILCA.

The production economist had played a major role in selecting animals for the study. His responsibility was to ensure that changes were cost effective. In the short run, benefits could be calculated only from increased milk production expressed directly in milk offtake or indirectly in additional weight gains of calves.

The socioeconomist observed that animals are not valued solely in terms of end uses as sources of milk, butter, and meat. A particular concern in the selective feeding program was the presence of the whole herd during the dry season. Much discussion also revolved around program definition of a herd. A herd was in fact a grazing unit, whereas animals within a grazing unit were owned by different persons. Although the anthropological literature usually identifies the male head of a grazing unit as the final decision maker (Stenning 1959, Hopen 1970, Van Raay 1974), it looked as though his control was more restricted. He had to be seen as impartial in his treatment of animals from different owners. The animal nutritionist pointed out, however, that the dams had a greater need of supplements because they suffered most during the dry season. A further problem arose from the assumed direct link between increased milk offtake and cash returns. This link was broken by the sex-linked roles of household members — men milked the cows, but women controlled the use of the milk.

The pastoralists did try to feed animals other than those selected by the program. In the initial exchanges about the problem between the pastoralists and the team members, the pastoralists argued that it was difficult to control the animals because all were accustomed to being corralled together. They argued constantly that all the animals should be fed because all suffered from lack of feed in the dry season. Intensive interviews with one pastoralist by the socioeconomist and the pasture manager revealed that ownership patterns made it difficult for him to comply with ILCA's wishes. The animals in his herd were owned mainly by him, an uncle, and a cousin. He had been accused of partiality because his animals were the main ones that qualified for supplementary feed.

The team's first reaction was to increase the supervision of feed delivery; individuals were instructed ta watch the actual feeding in the evening. Currently a broader feeding program that includes all dams is being considered, although it is recognized that the program must be cost effective. In 1981 the pastoralists were asked to state under what circumstances they would wish to give supplementary feed, bearing in mind current costs. Particular attention was given to the animals' sex, age, and health. The work required close cooperation between the socioeconomist, the range manager, and the animal nutritionist.

When asked about the cash value of increased milk offtake and hence the availability of cash to buy feed, the men pointed out that they might have to sell an animal to buy feed because they did not have cash. The possibility of the women buying the feed has yet to be explored.

FORAGE LEGUMES

The forage legumes were largely the responsibility of the forage agronomist and the range manager. The initial problems were those of land availability, and how to improve forage in a situation where there is no private ownership of the grazing area. Both realized that the farms of pastoralists were too small to provide sufficient forage. Food-crop farms were identified as the most appropriate areas for planting by individual pastoralists, preferably those that were to lie fallow the following year. This proposal generated considerable discussion and conflict with the research director who argued that such a strategy would make the feeding program too vulnerable. The socioeconomist played a major role in encouraging a program that enhanced existing links between the arable crop farmers and the pastoralists by emphasizing known links and providing additional information.

Although the reported traditional conflict between pastoralists and arable crop farmers should have excluded widespread land improvement by the planting of forage legumes, the evidence accumulated by the team suggested that verbal statements of conflict, particularly those by the pastoralists, did not correspond with behavior. Some of the existing links have already been mentioned. Another important link is the practice of the indigenous arable crop farmers to lend arable crop land to pastoralists and other arable crop farmers. Court records supported the decision to encourage land improvement (Van der Valk 1980). Few of the court cases investigated involved actual land conflicts between pastoralists and arable crop farmers. Most were concerned with animals straying onto farms where crops had not been harvested.

The shift in strategy, planting of farms of both pastoral and arable crop farmers, led to a greater concern with intercropping — identifying a forage legume that would not compete with existing grain crops. The most appropriate intercrop was sorghum, one of the staples of the people in the area. The problem was enlarged when arable crop farmers were asked to identify fields that could be planted. The team had already identified fields that were to lie fallow as the most appropriate. The need to plant on potential fallow land arose from problems of establishing good pasture; a single year's growth was considered inadequate.

In individual interviews with the agronomist and the socioeconomist early in 1981, the arable crop farmers reported that they were no longer interested in a strategy that included leaving land fallow. They noted that many of their young men had migrated to the towns and they lacked the labor to bring fallow land under cultivation. Many denied that they left land fallow. Most claimed that they cropped fields continuously by using fertilizers or animal manure or both. As a result, the agronomist had to concern himself more with the additional advantages — soil regeneration — the arable crop farmers would gain by growing forage legumes.

The range specialist raised the final problem of improved forage. The fields

needed to be fenced to secure the forage for cattle rather than for all the livestock in the area — sheep, goats, and pigs. At Kurmin Bin. the grazing reserve site, fields were fenced. Fencing is not new in the Abet area; fields have been protected by hedges of spiny Euphorbia for decades. But wire fencing is new and pastoralists have never practiced fencing. Fencing implies more permanent use of land than is understood in agreements between individual pastoralists and arable crop farmers. Even though pastoralists may have been in residence for more than a generation, the indigenous farmers expect them to move at some undefined time in the future.

The socioeconomist held discussions with arable crop farmers and started a closer investigation of land tenure relationships in the area. He agreed to the wire fencing because it was clear that the forage had to be secure to be beneficial.

CLASSIFICATION

The relevant questions for the follow-up ground survey in the four areas were based on the experience gained at Abet and in the earlier aerial survey work. The major concerns were grazing patterns, interest in arable crop farming, and land control. The interviews were limited to a single visit using only one set of interviewers.

The aerial survey involved flying a systematic grid pattern at 1,000 m above ground level (Milligan et al 1979) and covered a 20% sample of each region during the wet and dry seasons. Pastoral settlements mapped from the air were subsequently located on the ground. The ground follow-up was interpreted against the overall spatial relations between settlement types, agricultural practices, livestock numbers, herd sizes, and environmental conditions.

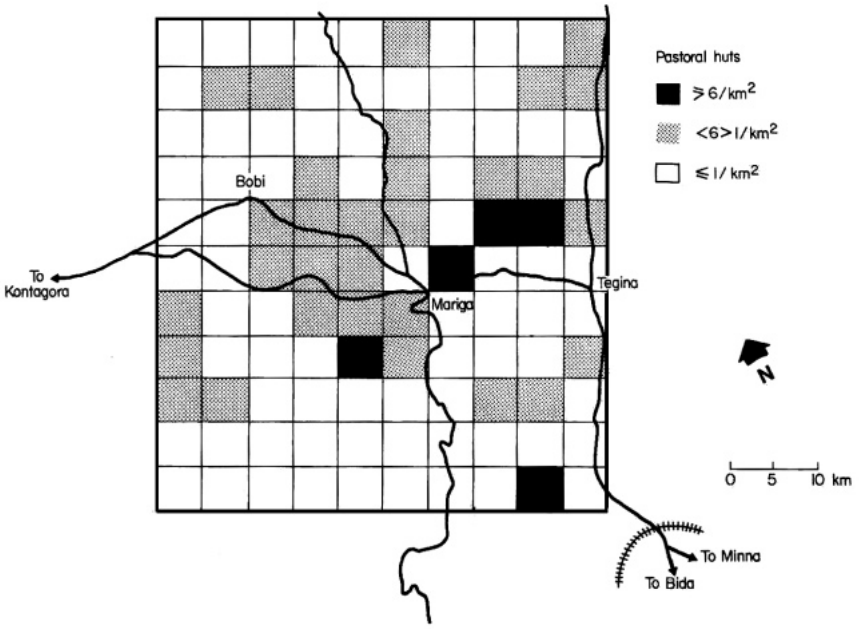
Mariga was initially identified as an area of low population density with a high tsetse challenge. The 1979 aerial survey characterized it as a wet season grazing area. There were more and larger herds and significantly more cattle during the wet season. This situation is unusual for the Subhumid Zone, which is described as a dry season grazing area. Two hypotheses were postulated to explain the situation. First, the region may reflect the supposed increasing tendency of pastoralists to settle permanently in the Subhumid Zone. Second, there may be more nomads or transhumants in the area during the wet season.

Three hypotheses were advanced to explain the small herd sizes during the dry season:

1. Herds may be divided into small management units to graze the patchy and limited fodder resources.
2. Cattle rearing may be a secondary occupation of arable crop farmers or pastoralists.
3. The pastoralists may represent a semisettled community where most of the cattle are taken sway during the dry season, a small milk herd being left behind for the family elders who may not follow the migration (Milligan et al 1979).

Surprisingly, cattle were more randomly distributed through the region during the dry season. Trend surface analysis suggested that the Mariga River, which runs through the center of the region from north to south, had an overriding influence on the wet season cattle distribution (Fig. 2).

Significant correlations were observed between seasonal cattle densities and



2. Distribution and density of pastoral huts in Mariga region, Nigeria (Milligan et al 1979).

environmental conditions. During the dry season, cattle tended to be concentrated near settled cultivated areas with high water availability. Small herds were more closely correlated with settlement areas than larger herds. During the wet season, cattle (particularly smaller herds) were concentrated mainly in areas of fallow and cultivation. Again, this suggests that settled pastoralists are more closely associated with the arable crop farming community than visiting nomads and transhumants (Milligan et al 1979).

The aerial survey indicated that less than 1% of the pastoral camps seen in the 1980 dry season and 25% seen in the wet season were not readily accessible from the ground — they were situated more than 10 km from a major access road. Six accessible concentration areas were identified and visited. All were all-season sites. One area was visited each day and occupants of as many households as possible were interviewed. Interviewing was discontinued once the pattern of land use, grazing, and settlement was established for a group of pastoral huts including people from the same lineage or from the same area.

The main finding of the follow-up ground survey was that although Mariga might be defined as a wet season grazing area (four times as many cattle were seen during the wet season), it is a settlement area for pastoralists. The seasonal difference in the number of settlements sighted in nonaccessible areas suggests the presence of nomads. Most (60%) of the pastoralists originated from the northern states, particularly Sokoto, and from those farther west. Through a process of migratory drift, the pastoralists first arrived to settle this area about 20 years ago. The largest numbers arrived in the last 7 years and the settlement process is continuing. In addition,

although more cattle are in the area during the wet season, cattle are grazed elsewhere during both the wet and dry seasons. They are taken to the former area of settlement about 180 km to the northwest around Zuru town in the wet season, and 180 km to the south around Bida and 80 km to the southeast around Minas in the dry season. Animals are moved to the wet season grazing areas in mid-July and to the dry season grazing areas in November. Mariga, situated in the center of the zone at the crossing of the Mariga river and the east-west road, is an important cattle market for the wet and dry season offtake.

Usually only the young people move with the herds, but it is not a community only of old people that is left behind. The only indicator of demographic status available from the ground follow-up was the number of married men resident in a homestead at the time of the interviews.

On the average there were five married men per homestead in both seasons. That means that in the area of greatest cattle concentration and settlements in Mariga region, there are permanent homesteads not disrupted by the movement of stock camps. Groups of brothers or their sons, who may or may not live together, tend to merge their animals for transhumance. This partly explains why settled pastoralists appear to have smaller herds. Owners of small herds invariably add theirs to larger herds for movement between grazing areas. They may not even accompany the animals themselves. Herds identified from the air, therefore, reflect grazing units and not ownership. We met no nomads during the wet season ground survey, but more remote herds may be owned by them.

The general pattern of transhumance that emerged from the ground follow-up might vary annually. Discussions with pastoralists indicated that individual herd owners, even those within the same homestead, might make different decisions. In some years a whole herd might be moved, in others. only a few animals. One man out of five may decide to graze his animals elsewhere, and the remaining four may add just a few of their animals to his.

The Mariga region was formerly a route for cattle passing south to dry season grazing areas. The availability of land for farming was of major importance in selecting settlements. At only one site were some pastoralists not farming. This was to the northeast in a hilly area not entirely suitable, but even here some pastoralists had farms. A number of the pastoral settlements visited on the ground were not identifiable as pastoral settlements from the air because the structures resembled those of the arable crop farmers. The concentration of pastoral settlements would probably be greater than indicated from the air, but only an intensive demographic survey could confirm this.

The ground follow-up suggests that the close correlation between cattle and farmed areas found in the aerial survey data reflects more the involvement of pastoralists in farming than a close relationship in land use between arable crop farmers and pastoralists. Almost 70% of the pastoral households visited in both seasons had settled in the area independently. The remainder either asked the District Head or another pastoralist for permission. No system of tribute for land rights exists. A number of arable crop farmers also keep small herds, but they owned only 3 of the 55 herds identified on the ground in the wet season. Just over 1% of the settlements identified from the air as cattle owning units were arable crop farmer

settlements. To the owners of the three herds located by the ground survey, the region represents an all-season grazing area. Their herds were small (less than 20 animals). Their livestock economy was not a dairying economy, but one of fattening steers for sale.

Settlement, defined as people remaining in one place throughout the year, is common. Settlement is concentrated to the north and south of the main east-west road and is focused on Mariga. This underscores the importance of infrastructure for regional planning purposes even among pastoral groups. Stock-owning units exercise independence in the disposition of their stock in pastures.

A small number of all-season settlements of pastoral and arable crop farmers are isolated. They are linked to major access roads, often laterite, by small narrow tracks. Even Bobi, one of the few larger settlements away from the Kontagora-Tegina road, was accessible by only one laterite road during the wet season. These isolated, possibly self-contained, communities would not seem to lend themselves to the interventions of the ILCA Subhumid Programme.

The strategy of planting forage legumes on arable crop farmers' plots for the use of pastoralists is not relevant in the Mariga area. There are no exchanges of manure or crop residues. Arable crop farmers could, however, plant forage legumes for their own use, although their goals almost exclude milk production. Therefore the strategy of restricting supplementary feed to lactating cows would not appear suitable.

Crop production appears to be even more important for pastoralists in Mariga than in Abet and Kurmin Biri. An attempt was made during the aerial survey to estimate the significance of arable crop production by counting the granaries attached to pastoral settlements. We have already discussed the problem of distinguishing pastoral settlements in the area of high settlement concentration. Granaries were difficult to identify because in the Mariga area they are frequently located in fields. Some pastoralists even store their grain with arable crop farmers. If indeed arable crop farming is important, forage legumes must not be perceived as competing with arable crop production. This has been recognized at Abet and Kurmin Biri, particularly for the arable crop farmers who plant on behalf of pastoralists.

The most significant grazing pattern in the Mariga area is one based on a system of transhumance with a small number of animals, most of them lactating cows, being left at the all-season permanent settlements. This strategy has implications for both the feeding and veterinary interventions. First, problems arising from restricting feed to lactating cows could be avoided. This would facilitate the work of the pasture manager. But routine veterinary servicing of the whole herd throughout the year would not be possible.

INTERDISCIPLINARY RESEARCH

Although individuals representing different disciplines identified particular problems in the program, it was recognized that interdisciplinary research in the field demands that scientists work together. Therefore interviews with cooperators rarely included only one scientist. In this situation, each team member has to learn something about the other disciplines. No individual can define his role too strictly.

It was not always possible to state clearly which discipline contributed most to a change in research strategy. The animal nutritionist probably made the most accurate observation when he noted that the disciplines ask different questions or ask the same question in different ways.

Particularly important for the success of the program was the emphasis placed on the team approach by the team leader. Each team member was required to work closely with another team member before starting or outlining his contribution. The range manager played a major role in incorporating new team members.

Major problems always arise when a new technology is first applied in the field. No technological innovation is complete until it has been applied in situ. The major advantage of adaptive research, as discussed in this paper, is that the relevance of research is questioned quickly and the important socioeconomic factors are readily identified. Identifying socioeconomic factors is an essential aspect of technology development with a regional focus.

SUBHUMID PROGRAMME TEAM MEMBERS

R. von Kaufmann, livestock economist, team leader;
B. Sule, range manager, deputy team leader;
J. Maina, veterinarian;
K. Milligan, aerial survey coordinate.
C. Okali, socioeconomist;
E. Otchere, animal nutritionist: and
M. Saleem, agronomist.

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The role of a cognitive anthropologist in a farming systems program that has everything

Christina H. Gladwin

What can an anthropologist-agricultural economist add to a center or institute that has everything: a farming systems program aimed at increasing the yields of food grains on small farms; a methodology to design, test, and evaluate site-specific technology appropriate for small farmers; technicians trained to listen to farmers and respect their beliefs and culture; and a socioeconomic team actively involved in the design of new technology? Unbelievable as it may seem, that was the case at the Guatemalan Institute of Agricultural Science and Technology (ICTA) in June 1978.

ICTA in 1978 was different from the other centers or projects discussed in this volume. It was a national center run by nationals (aided by several Rockefeller Foundation field staff) who were committed to increasing yields of the basic food commodities: maize, wheat, rice, sorghum, sesame, beans, fruits, and swine. It had a farming systems program that had been operating in five regions of Guatemala since 1973. Actively involved in the program was a socioeconomic team led by Peter Hildebrand, an agricultural economist. The team included two anthropologists, one agricultural economist, one sociologist, and one statistician. The social scientists on the team were all Guatemalans. Although they were stationed in Guatemala City each was responsible for a specific region and frequently spent time interviewing farmers in the region. There were also one or two technicians at the regional experiment stations who were trained by and worked with the socioeconomic team under the supervision of the technical director.

Besides having an active social science unit, ICTA had many dedicated technicians, some with university training as well as training at an agricultural secondary school. By 1978, almost all agronomic experiments and farmers' tests were conducted in farmers' fields in the villages, often an hour and a half away from the regional experiment station. By their having been pushed out to the farmers' fields and having been subjected to some consciousness-raising by the socioeconomic team, many of the technicians I met in 1978-79 had been trained to listen to farmers,

Anthropologist-agricultural economist, University of Florida, Gainesville, Florida 32611, USA.

not preach to them or order them about. Most, although not certainly all, ICTA technicians realized that the success of the farming systems programs required good communication with the farmers who loaned their land to ICTA for experiments.

More important to ICTA's success than the presence of either a socioeconomic team or sensitive technicians, however, was its development of an innovative farming systems program. In general, a farming systems program treats each farm as a unique system of interrelated activities: crops, livestock, forest, pasture, off-farm labor, etc. Within that system each farmer uses a set of resources (land, capital, time, energy) and faces a physical and social-cultural-economic environment that imposes certain constraints on his farming operation. (The pronoun "he" includes both male farmers and female farmers.) Given the physical and socioeconomic environmental constraints, the farmer makes management decisions, integrating his set of available resources and the environment (Hansen et al 1981). The farming systems approach starts with the farmers' constraints as givens and develops, through experiments on his fields, recommendations to improve his family's standard of living. Most farming systems programs accomplish this aim via a multidisciplinary team that

- diagnoses farmers' problems, goals, and constraints;
- identifies new technologies or strategies to deal with or alleviate farmers' constraints;
- tests the promising technologies or strategies by experiment-station and on-farm trials; and
- diffuses or extends the new tested technologies or strategies to the local farmers (Gilbert et al 1980).

ICTA'S FARMING SYSTEMS PROGRAM

ICTA's farming systems program is a five-stage, multidisciplinary effort, summarized as: 1) sondeo (survey), 2) generation of technology, 3) testing, 4) evaluation, and 5) extension. Because they are described in detail elsewhere (Fumagalli and Waugh 1977; Hildebrand 1977, 1979, 1981; ICTA 1977; Ortiz 1979), only brief descriptions of each stage are given here.

Sondeo

A multidisciplinary team of plant breeders, pathologists, agronomists, agricultural economists, and sociologists informally gather information about farmers' cropping systems, socioeconomic conditions, and constraints in the area where technology generation is proposed. The information is summarized and conclusions and recommendations are written up all within 6 days. In most cases the technical team that will conduct the farm trials also participate in the initial sondeo. This gives the biological scientists cn the technical team a chance to see the area and traditional technology, and talk to the farmers themselves before the farm trials are planned. It also gives the social scientists an input into the planning of the farm trials through the written recommendations and the three-way direct communication between biological scientist, social scientist, and farmer. The importance of the three-way communication cannot be overemphasized. The social scientist learns firsthand about the physical constraints imposed on the farmer and the biological scientist hears the

farmer talking about socioeconomic constraints in response to the social scientist's questions. While the farmer is educating biological and social scientists, they are educating each other about their respective expertise and their solutions to the farmer's problems (Hildebrand 1979).

Generation of technology

The technical team tries a new technology. Some experiments of the commodity programs (maize, bean, sorghum, etc.) are highly controlled trials on regional experiment stations. The majority of experiments, however, are conducted on land the farmer has loaned to ICTA. Typically, technicians perform the work, holding constant the farmers' levels of inputs and traditional cultural practices, manipulating only the experimental variable such as crop variety or plant population (ICTA 1977). Farm budget data are also collected at this stage to add to ICTA's knowledge about traditional technologies.

The test stage

The farmers themselves test the technology. Volunteer farmers perform the work on their own fields, pay all input costs, and typically plant half of one field to ICTA's technology and the other half to their own in a contest to see which does better (Ortiz 1979). Data concerning the time and capital requirements of ICTA's technology vs the traditional technology are gathered. Because farmer cooperatives or informal groups of farmers often attend the planting, fertilizing, and harvesting of the contest field, the farmers' tests often start the process of technology diffusion and transfer.

The evaluation stage

The socioeconomic team returns to the farmer one year after he has tested ICTA's technology to see if he is still using (has adopted) the new technology. An *index of acceptability* is calculated for each ICTA recommendation (Chincilla and Hildebrand 1979). If acceptability is low, the technical team will reconsider the benefits of the improved technology and may even drop it.

The extension stage

ICTA promotes the use of the acceptable new technology in collaboration with the extension service Direccion General de Servicios Agricolas (DIGESA). ICTA and DIGESA technicians work with farmer groups, cooperatives, and paraprofessionals working with farmer groups such as promoters of Escuela Extra-Escolar (Adult Education) or World Neighbors (ICTA 1977, Fumagalli and Waugh 1977).

The success of ICTA's program in the Altiplano of Guatemala, an area of about 22,000 km^2 including the States of Chimaltenango, Sololá, Totonicapán, Quezaltenango, San Marcos, Huehuetenango, and El Quiché, can be seen by the diffusion of several technologies:

- San Marceño, an improved maize variety, adopted in many parts of Quezaltenango (especially the subregion of Llanos de Pinal);
- Chivito, an improved wheat variety, adopted in parts of Quezaltenango and Totonicapán;
- urea as a second application of fertilizer for both maize and wheat; and

- the introduction of vegetable production in irrigated, terraced parts of San Marcos.

Although agricultural production in the Altiplano still has a long way to go to keep up with population increases, the methodology to generate production increases is clearly there.

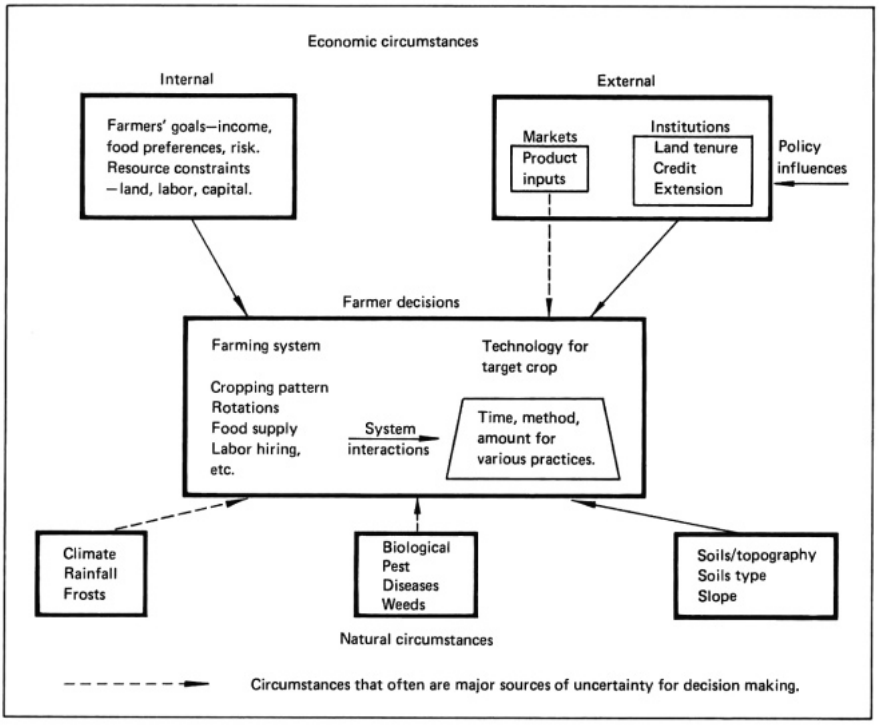
Given the integrated methodology of ICTA and an outstanding socioeconomic team actively involved in the design of new technology, I questioned whether or not my contribution would be marginal. ICTA technicians in 1978 did not often go to the farmer with a good answer to the wrong question. They knew one had to talk and listen to the farmers before coming up with a new variety of food grain or a new storage facility or a new way to teach integrated pest management to small farmers. In regions where they didn't know this, Hildebrand would quickly inform them. As head of the socioeconomic unit, he waged the primary battles. For example, in the case of one sondeo in Zacapa, Eastern Guatemala, most of the regional technicians refused to participate in the interviewing. When two of them finally did, after a half day of arguing on Hildebrand's part, they brought back incredible stories about what "the communists" on the socioeconomic team said to the farmers. Fortunately, Hildebrand was not deterred much by controversy or constructive conflict and, under the direction of the technical director, continued to involve the socioeconomic unit in the design, testing, and evaluation of ICTA's methodology. However, the reader should not get the impression that biological and social scieatists in ICTA were always in agreement. Both during and after my stay in Guatemala, serious disagreements arose regularly, and the status of the socioeconomic unit was always in question with the biological scientists questioning its role, influence, and budget.

FOCUSING ON THE FARMER AS DECISION MAKER

I knew that I, or any cognitive anthropologist-agricultural economist, had something to offer a farming systems program that has everything. I knew that a farming systems program cannot do anything without first identifying the problems and constraints the farmer is operating under. The focus of a farming systems program is on the farmer, rather than on the plant, a new technology, or on the environment (Fig. 1). Given that farm trials and farmers' tests are on farmers' fields, and the farmer is consulted during both the diagnostic and evaluation stages, the farmer is clearly at the center of the program. With this focus on the farmer whose adoption or rejection of the new technology can make or break a farming systems project, the program staff should know:

- what decisions the farmer is making,
- what alternative he is considering in each decision context, and
- why he chooses a particular outcome.

Why farmers do what they traditionally do must be understood by the multidisciplinary team before cultural practices can be improved. Fortunately, I had a methodology which did just that in a systematic, replicable, and scientific way.



1. Various circumstances that affect farmers' choice of a crop technology (CIMMYT Economic Program 1980, reprinted with permission).

DECISION-TREE METHODOLOGY

During the past decade anthropologists in several cultures have used natural process or hierarchical decision-tree models to predict the actual choices of individuals. Decision trees have predicted with a high degree of accuracy selling decisions made by Ghanaian fish sellers (H. Gladwin 1971, C. Gladwin 1975, Quinn 1978), farmers' adoption decisions in Puebla, Mexico (C. Gladwin 1976, 1979a), farmers' land use patterns in Costa Rica (Barlett 1977, and farm families' choice of treatment for illness in Pichatero, Mexico (Young 1980). The predictability has been as high as 85 to 95% of the actual choice data used to test the model. These success rates are remarkable, however, only because most studies of economic decision making do not test the model against actual choice data (Anderson 1974, Benito 1976, Moscardi and de Janvry 1977).

More recently the decision-tree method has been shown to be generalizable to a wider geographic region than a village or town, because some agricultural production decision rules are shared by farmers who live in different agroclimatic, socioeconomic zones (C. Gladwin 1979c). Moreover, a consumer decision process model based on in-depth interviews with decision makers in one region (car buyers in Orange County, California, USA) was tested, with 70% reliability against choices

made by individuals selected at random in a national survey (H. Gladwin 1980, Murtaugh and H. Gladwin 1980).

The form of a hierarchical decision-tree model is simple, with decision criteria at the nodes or diamonds of the tree, and decision outcomes or choices at the ends of the branches. The decision criteria can be orderings of alternatives on some aspect of the alternatives (Is profitability of potatoes > profitability of maize?), or they can be constraints that must be passed or satisfied (Do you know how to plant potatoes?). In either case, the criteria or constraints are discrete: the alternative "potatoes"either passes the criteria or constraints or it does not. A decision tree then is a sequence of discrete decision criteria, all of which have to be passed along a path to a particular outcome or choice. Figure 2 is a hypothetical model of a farmer's decision of whether or not to plant potatoes. "Potatoes" must pass profitability, knowledge, and capital constraints for the farmer to choose the outcome "plant potatoes." If potatoes fail any one of these criteria, the model predicts that the farmer will not plant potatoes.

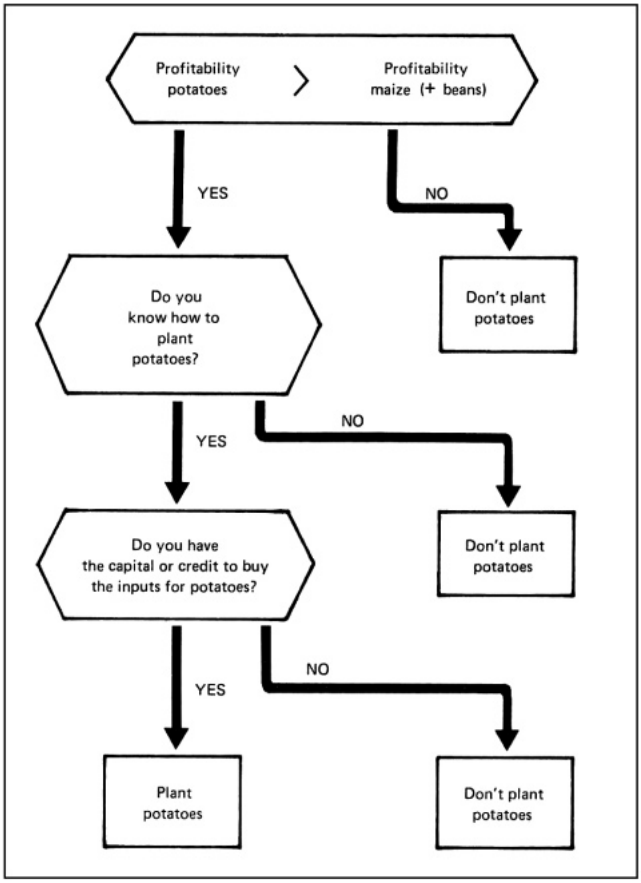
HOW TO BUILD A DECISION MODEL

Given a form of decision model, the researcher must select the decision criteria or constraints to use. He must decide which information is actually considered by farmers when they make a particular decision, and discard information that might be interesting, but which farmers don't seem to use. In-depth interviews with decision makers are necessary to build the model. Only the decision makers are the experts on how they make their decisions; only they process the information that the researcher wants to represent in his model.

Eliciting techniques and ethnoscience ethnographies are uniquely anthropological inventions (Spradley 1979, Werner and Schoepfle 1979). Although some anthropologists question the reliability of decision criteria that are elicited from the decision maker (Barlett 1977; Chibnik 1980 Cancian 1972, 1980; De Walt 1979), most accept the need for ethnographically valid, inductively built, testable decision models. To test an inductively based decision-tree model, one must collect actual choice data from a second, independent sample of decision makers whose data were not used to build the model.

Decision-tree studies, although relatively easy to apply, have not yet been used regularly by national or international agricultural research centers. The reason is that the decision-tree tool presupposes a farming systems research and extension (FSR/ E) program in which the farmer as decision maker is directing the program. Decision-tree research fits naturally in that kind of program, although it may be a luxury to an agricultural program that talks about, but has little interaction with the farmer.

The methodology is most appropriately used at the diagnostic stage and the evaluation stage. (For applications of adoption of decision trees, see Gladwin 1976, 1977, 1979a, 1980.) Finally, while adoption decision models are most common in the evaluation stage, in an international research center it is useful to evaluate experimental technology before its transfer to a national research-extension center. Ashby and de Jong (1980) give an excellent example of how to use decision models in this way.



2. Hypothetical example of a farmer's decision whether or not to plant potatoes.

FARMERS' CROPPING DECISIONS IN THE ALTIPLANO

The farmer's cropping decision is a two-stage choice process. In stage 1 he first narrows the range of possible crops to a feasible subset that satisfies minimal conditions. For example, given 8 to 10 possible crops, a farmer may rapidly, often unconsciously, eliminate vegetables because of lack of irrigation. He might not consider planting potatoes because he doesn't know how to plant them or apply pesticides. Alternatively, he might not even think of growing coffee because the land is at too high an altitude.

With the smaller subset of feasible crops that emerges from this *elimination-by-aspects* stage (Tversky 1972), the farmer proceeds to stage 2, the hard-core part of the decision process (Gladwin 1980). Stage 2 allocates the farmer's available land to the crops that pass stage 1 constraints. If the farmer has a lot of land, stage 2 is a simple decision process; he will plant all the crops that pass stage 1 constraints. If, however, the farmer does not own or operate much land, the crops that pass stage 1

constraints compete for the little land there is, and the decision process and model become more complicated.

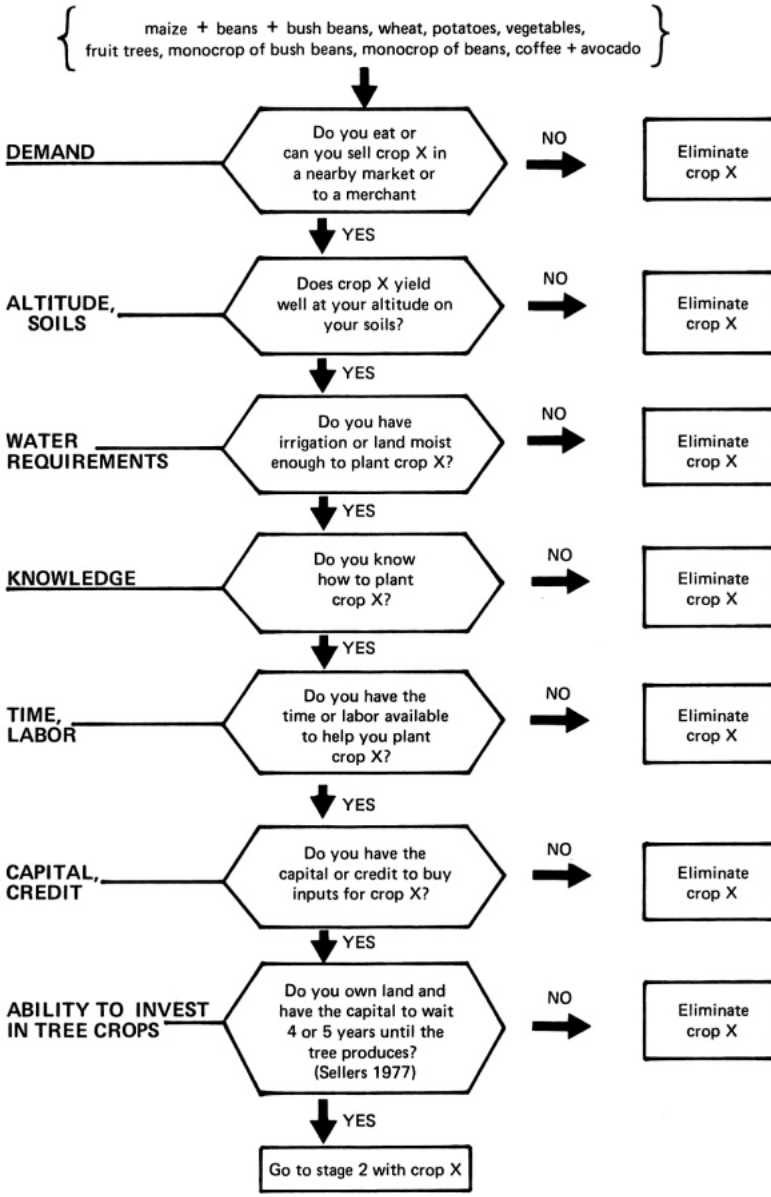
In the most general terms, stage 2 of the model proposes that farmers in the Altiplano give first priority to crops or systems of crops that are at least twice as profitable as maize, the main consumption crop. Usually maize is intercropped with beans (*fiiijol* and *haba*), so is written maize (+ beans) in the model. For brevity, "maize (+ beans)" will be referred to hereafter simply as maize. A system of crops is defined as a set of crops that is harvested on the same field in one year (a first harvest of wheat and a second harvest of peas, 2 harvests of potatoes, or 3 harvests of vegetables). Second in the farmers' priorities is the planting of as much maize as is necessary to meet the family's consumption requirements between harvests. Third, if farmers still have more land, they plant a crop or system of crops that is not twice as profitable as maize. It may be as profitable as, a little more profitable, or less profitable than maize.

Figure 3 represents the choice process of stage 1, in which a farmer unconsciously eliminates (H. Gladwin and Murtaugh 1980) some of the possible crops in the set at the top of the tree. To shorten interviewing time, only crops that have some possibility of passing the stage I constraints are included in the system of crops. Each possible crop of the farmer is then put down the decision tree — the farmer is asked a series of six questions about each crop possible in the set. For a stand of tree crops such as fruit, coffee, or avocado, there is a seventh investment constraint. If a crop passes all six or seven constraints, then the model in stage 1 sends the farmer to stage 2 with that crop.

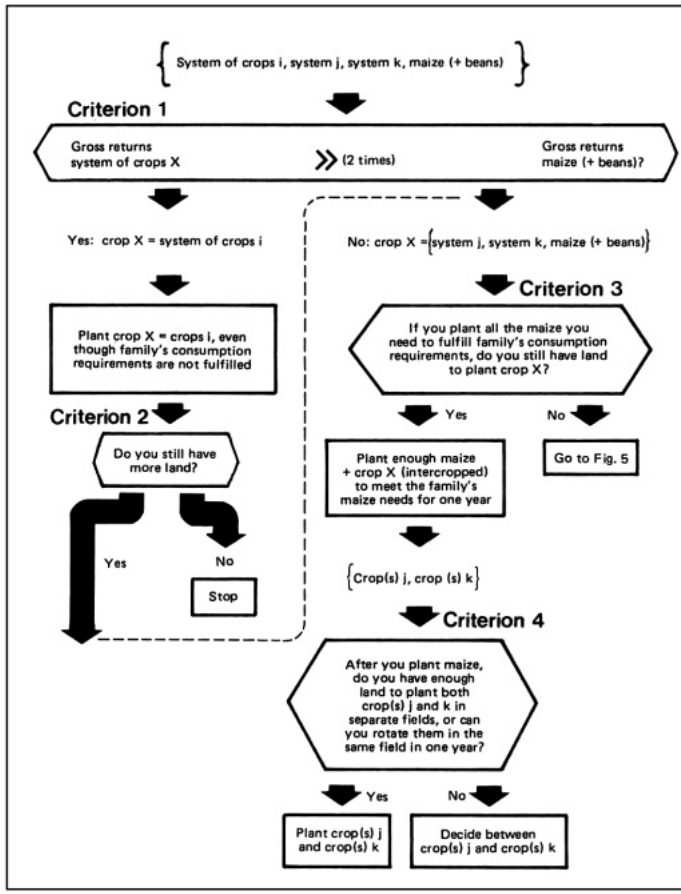
For generality, it is assumed here that three systems of crops and maize have made it to the feasible subset at the top of Figure 4. The first criterion in the flow chart then considers each crop system independently of the others and looks for a very profitable crop, i.e. one that is at least twice as profitable as the consumption crop maize. Each alternative cropping system is compared with maize because — as the farmers testify — "maize is first." Because all the feasible crops are not rank-ordered on profitability, the order in criterion 1 is a partial not a full order. The profitability of wheat is compared to that of maize and the profitability of potatoes is compared to that of maize, but wheat and potatoes are not compared or ordered on profitability.

The very profitable crops, which may be up to five times as profitable as maize, are then sent down the left-hand branch of the tree. In this flow chart (Fig. 4) the farmer considers only crop *i* to be twice as profitable and it is sent down the left-hand path. Crops *j* and *k*, and of course maize, are not considered veryprofitable with respect to maize. They are sent down the right-hand path to criterion 3. On the left-hand path, however, the model predicts that the farmer will plant the very profitable crop first, even though he has to take some land out of maize production. The result is that the farm may not be able to produce the family's yearly consumption requirements for maize.

If the farmer still operates more land after planting the very profitable crop i, criterion 2 in the model sends him to the consumption criterion 3 on the right-hand branch of the tree. Here the farmer is asked if he has enough land to plant the not-so-profitable cash crop(s) after he has planted enough maize to fulfill the family's



3. Choice process in stage 1 of decision making – elimination by aspects.

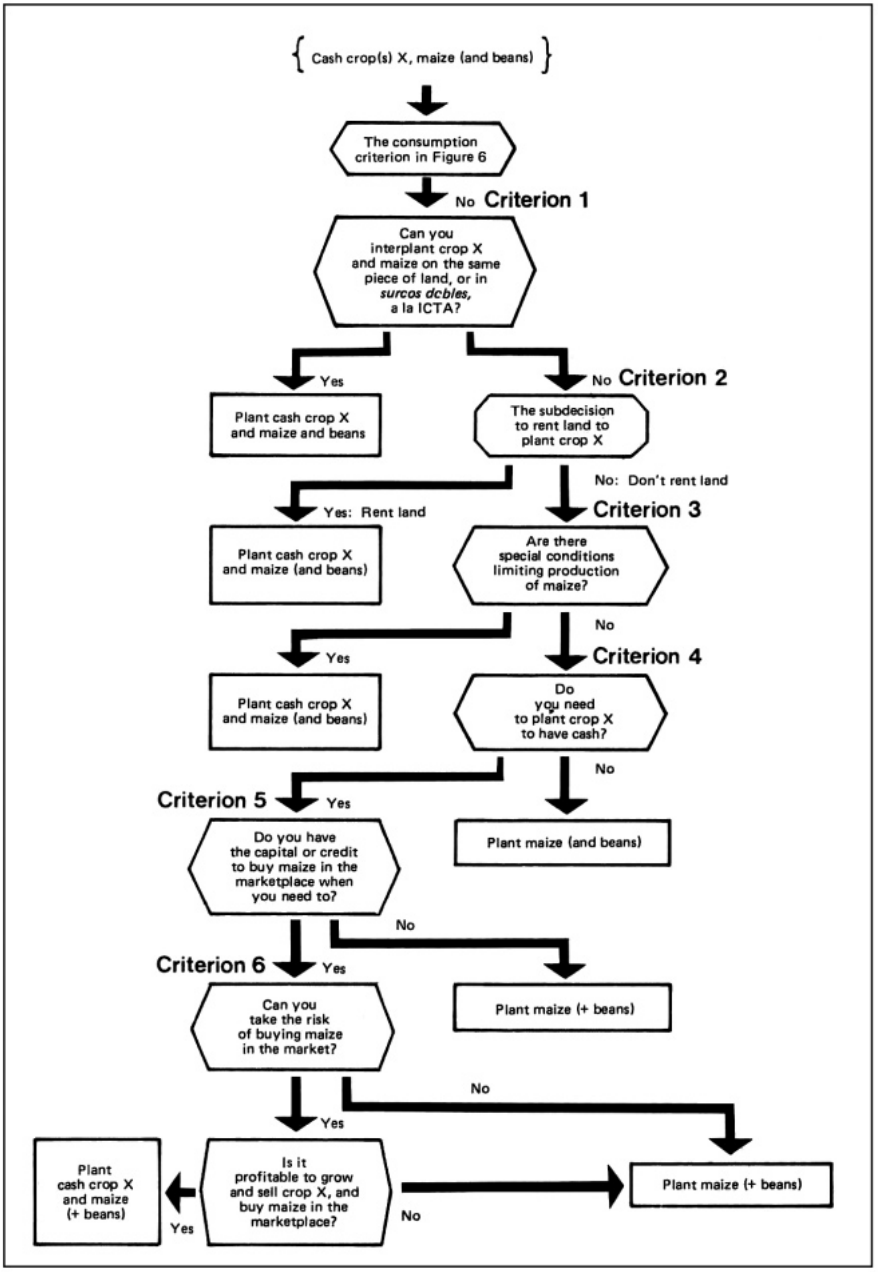


4. Stage 2 of the cropping decision in the Altiplano.

consumption requirements. If there is enough land, the subset below criterion 3 predicts that maize will be planted first.

The decision between two or more cash crops is simple if the farmer has enough land to plant both crops. If there is not enough land and the farmer cannot rotate the crops within the year, then he must decide between them by trading off the profitability and risk of the cash crops. Because results show that most farmers manage to squeeze in both cash crops on their land, the model of this subdecision is presented elsewhere (Gladwin 1980).

If farmers do not have enough land to be self-sufficient in maize and plant a cash crop as well, they are asked the questions in the decision model in Figure 5. These questions identify extenuating circumstances that would lead the farmers to take some land out of maize to put into a cash crop, even though they would then have to buy some maize for home consumption. The decision for farmers now is between a crop mix of cash crop(s) and maize vs a crop mix of just maize.



5. The decision to plant a cash crop and maize or just maize.

Four criteria lead farmers to grow a cash crop and maize.

- First, they can multicrop or interplant cash crop *x* and maize so that their production of maize does not diminish substantially.
- Second, they can rent land for the cash crop and devote their own land to maize. This is really a subdecision: farmers will rent land if rental land is available, they find the owner before planting time, they have the capital to pay the rent, and they think that renting land is profitable.
- Third, they may think there are special conditions that limit the production of maize to only a portion of their land. For example, farmers may plant maize only on the fields around the house to discourage the theft ofgreen maize in the field by people and birds.
- Fourth, farmers need cash and don't have another source of cash such as full-time, off-farm employment.

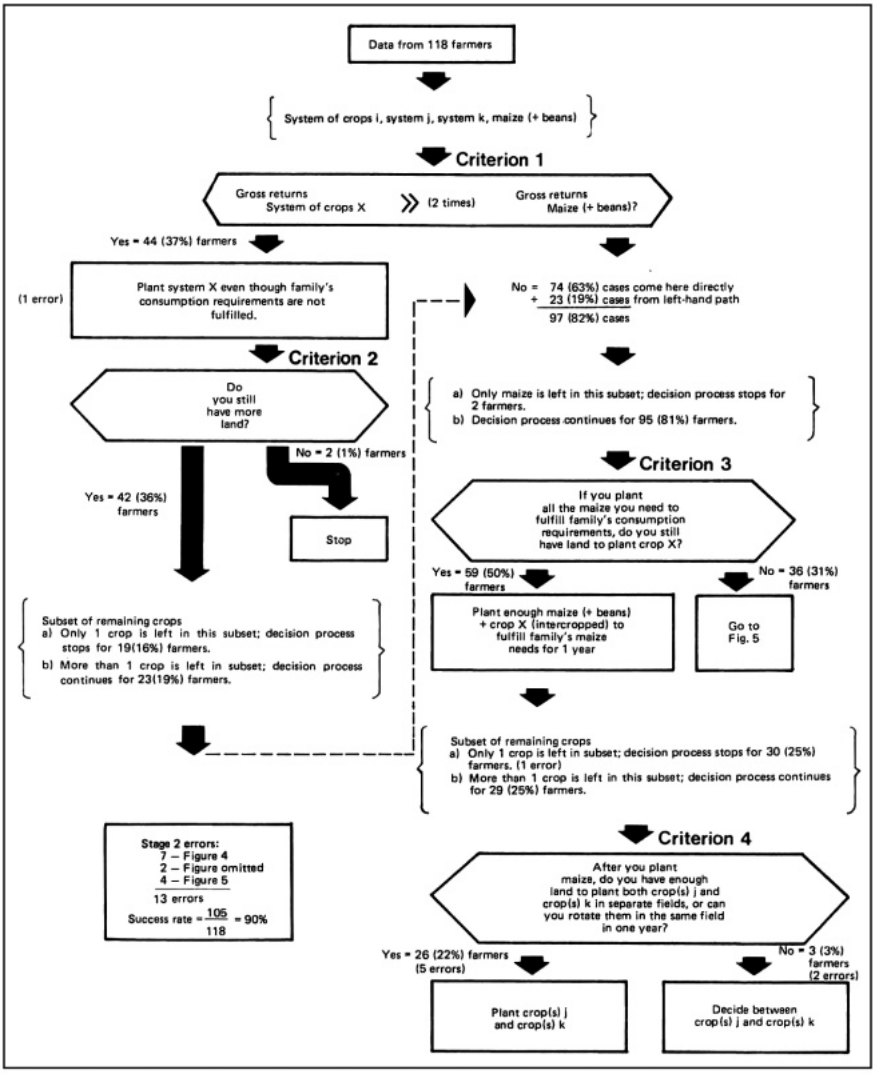
Besides passing these criteria, which act to encourage them to grow a cash crop, Altiplano farmers who need cash must also pass three constraints that discourage cash crop production.

- First, they must anticipate that they will have enough cash to buy maize in the market. For farmers with severe capital constraints, planting and then storing a year's supply of maize is insurance against later shortage of capital.
- Second, they must be willing to take the risk that there will be maize available when they go to buy it.
- Finally, they must think it is profitable to grow and sell a cash crop before they will plant some maize land to a cash crop. In summary, the first four criteria in Figure 5 encourage farmers to switch some needed maize land to a cash crop while the last three act as a brake on this switch.

RESULTS

The model in Figures 3 to 5 was tested against actual cropping-choice data gathered from farmers in six subregions of the Altiplano. The regions differ in altitude, the predominant crop mix, the extent and type of off-farm labor opportunities, language, and the percent of the population that is rural, indigenous, and in agriculture (Gladwin 1979c). There is considerable individual variation in crops grown by farmers within the same subregion, so the model tests or processes data from each farmer independently. Indeed, there can be a separate decision tree for each farmer with different subsets of crops proceeding from stage 1 to stage 2, and to different branches in stage 2.

The results of testing stage 2 of the cropping decision model (Fig. 4) are summarized for 118 farmers in Figure 6, again assuming that a subset of crops *i, j, k,* and maize has passed stage 1 constraints. Of the 118 farmers, only 44 have a crop or system of crops that passes stage 1 and is twice as profitable as maize. Data from these farmers pass to the left-hand branch of the tree to the outcome "Plant system *x* even though the family's consumption requirements for maize are not fulfilled." Farmers consider only a handful of cash crops profitable enough to plant before maize. Those cash crops require irrigation or sandy soils and an afternoon cloud cover. Results show that one crop per year of rainfed vegetables, potatoes, or wheat



6. Stage 2 results in six zones of the Altiplano.

is not profitable enough to plant before maize.

After planting the twiceas-profitable crop, the farmers on the left-hand path then pass on to criterion 2, to see if they have more land left to plant another crop. Only 2 of the 44 farmers do not have land left to plant another crop.

Farmers without very profitable cropping systems

Ninety-seven farmers proceed to the decision process on the right-hand path of the tree (Fig. 6). Seventy-four of them go directly to the right-hand path of the tree because they do not have a crop that passes stage 1 and is twice as profitable as

maize. Therefore they consider their family's consumption requirements for maize before their need to plant a cash crop. Twenty-three come from the left-hand path because they have more land left after planting the twice-as-profitable cash crop, and have two or more crops left in their feasible subset. At this point, the decision process stops for two farmers, because maize is the only crop left in the feasible subset.

Of the 95 remaining farmers, 59 pass the consumption constraint — they have the land to plant enough maize to fill their family's consumption requirement and one or more cash crops. They proceed to the outcome "Plant maize plus a cash crop that can be interplanted with maize in the same field." After planting enough maize to satisfy their consumption needs between harvests, these farmers allocate their remaining fields to the cash crops that remain in their feasible subsets. For 30 of the 59 farmers, only one cash crop is left in the feasible subset. The remaining farmers have two or more cash crops still in the feasible subset so their decision process continues on to the diversification criterion 4. Here, 26 of them manage to squeeze out the land required to grow both crops or the climate is such that the farmers rotate the two crops on the same field within the year.

The cash crop and maize compete for land

Thirty-six of the 95 farmers on the right-hand branch of the tree fail the consumption criterion. Data from these farmers are sent through the decision process in Figure 5. There were 48 cash crop options for the 36 farmers. This model predicts a cash crop will be planted, even though consumption requirements of maize are not met in these cases:

- 7 in which crops are interplanted or multicropped with maize,
- 5 in which land can be rented for the cash crop,
- 15 in which special conditions limit the production of maize, and
- 9 in which the farmer needs cash and passes the profitability, capital, and risk constraints associated with buying maize in the marketplace.

The model predicts that the farmer will plant only maize in these cases:

- 8 in which he doesn't need a cash crop,
- 2 in which he cannot risk buying maize in the marketplace, and
- 2 in which he considers it unprofitable to grow the cash crop to buy maize.

Twenty-four of the 36 farmers will plant cash crops, even though they failed the consumption criteria for maize; 12 will plant just maize.

IMPLICATIONS OF THE RESULTS FOR ICTA

The results of testing the model have two main policy implications for ICTA. The first concerns institutional allocation of resources to specific commodity programs. The second regards policy recommendations to the technical teams designing farm trials.

Support for ICTA's maize program in the Altiplano

Constant debates in Guatemala over the value of maize production in the Altiplano are expressed in statements such as:

> "Maize is not the right crop for the Altiplano."

"The growing season in the Altiplano is too long for maize."

"There is too little rain for maize."

"Maize is not a profitable crop that will help conditions change."

If one were to suggest, "But the people eat maize three times a day," the typical reply would be, "But farmers should grow and sell higher-valued cash crops and buy maize."

Because one of ICTA's original aims was to increase food grain production and maize is ICTA's main commodity program, debates over the value of maize production in the Altiplano hit home.

The counterargument is, of course, that farmers do not always do what they should. Sixty percent of the farmers sampled plant a cash crop only if they can first meet their consumption needs for maize. Fifty percent plant the family's consumption requirements for maize first, and a cash crop second. Another 10% plant only maize because they do not have enough land to be self-sufficient in maize and plant a cash crop.

These results suggest that any attempt to diversify farmers' cropping patterns in the Altiplano must try to improve maize yields. When maize yields are improved, farmers can then divert some land from maize production to a cash crop. Improving maize yields seems to be the diversification strategy most capable of reaching the majority of farmers in the Altiplano, whether or not they should plant maize.

Other diversification strategies are also implied. Because a sizable minority of the farmers sampled (37%) now plant a very profitable cash crop first, and maize second, ICTA might try to introduce a profitable cash crop into more subregions of the Altiplano. The results show, however, that only a handful of cropping systems are twice as profitable as maize. They include: two crops of potato, two or three crops of vegetables plus one crop of potato, a rotation of wheat and vegetables (or potato), coffee, and a monocrop of fruit trees. Few farmers perceive one crop of rainfed vegetables, potato, or wheat to be twice as profitable as maize and capable of replacing maize as the number one crop. Furthermore, only a few subregions of the Altiplano have the climate or irrigation or both necessary for these multiplecrop systems. Finally, the lack of a strong market for some of the crops (vegetables and potato) may limit their profitability (Smith, pers. comm.). Therefore introducing a very profitable cash crop into an area will not be an effective diversification strategy capable of reaching the majority of farmers in the Altiplano.

Data summarized in Figure 5 show other ways to diversify a farmer's crop mix. Multiple cropping or intercropping with maize, without signifcantly decreasing maize production, should prove the most effective diversification strategy for small farmers with one-quarter of a hectare or less who have family labor available (Hildebrand 1976). Unfortunately, knowledge of ICTA's relay crops or double rows has not yet diffused widely in the subregions of the Altiplano sampled. Another problem with this diversification strategy is the shortage of family labor in some parts of the Altiplano (Totonicapán) due to competition from the indigenous weaving industry (Smith 1978). Farmers do plant a cash crop when they can rent land, but the scarcity of rentable land limits this diversification strategy.

The special conditions criterion accounts for more cases of cash crops planted than any other criterion in Figure 5. It is clear that many farmers feel maize is not

suitable on some of their fields and plant a cash crop on them. This criterion, however, is not easily amenable to policy intervention. (The exceptional policy recommendation would be to encourage extension agents to suggest that farmers rotate their crops.) Because farmers plant cash crops to have cash, one way to push them into diversifying their crop mix is to increase their need for cash by intensifying their involvement in a cash economy and decreasing their self-sufficiency. The Conquistadors did that in Totonicaphn by introducing wheat. They levied on the indigenous population taxes that could only be paid in cash or wheat. This diversification strategy has adverse secondary effects such as an overall decrease in real rural family income; its social costs are greater than the benefits.

Design of farm trials

Because the decision model was developed after sampling 10 subregions of the Altiplano, and was tested in another 6 subregions, the results could be generalized to predict farmers' cropping patterns in a new subregion with different cash crops. To test this, I participated in two other sondeos initiating technology generation in the Altiplano — the potato zone of Quezaltenango and an apple producing area in El Quiché.

Using the decision tree model, I contributed to those parts of the sondeo report that discuss:

- the constraints that limit potato production in the peripheral subregion of the potato zone and lead farmers to plant maize or rotate wheat with potato, rather than plant two potato crops as do farmers in the center of the zone (Hildebrand et al 1979); and
- the factors that lead some Chichicastenango farmers to plant a fruit tree stand or orchard while the majority interplant one or two rows of fruit trees in their maize field, and the constraints that prevent the majority from switching to a monocrop of fruit trees (Socioeconomia Rural/ ICTA y DIGESA 1979).

The results of the sondeo led to recommendations that:

- ICTA should place most of its potato trials in the center of the potato zone, but include in the peripheral areas trials in which an early-maturing wheat variety followed a first crop of potato; and
- DIGESA technicians in Chichicastenango should give technical assistance to farmers growing fruit trees interplanted with maize as well as to those with large orchards or credit with DIGESA.

Sources of conflict

In accordance with sondeo rules that all interviews are informal and no notes are taken in the farmer's presence (Hildebrand 1979), the test of the decision model was also informal. I had to remember all stage 1 and stage 2 questions and work them in between questions posed by the other members of the group. I had to internalize the model to mentally put each farmer interviewed down the tree. This testing method caused some professional conflict for me. Although 1 benefited greatly from participating in these sondeos, I became increasingly wary and critical of the use of the sondeo method.

The length of time sounding out farmers in the field is too short; the sondeo is

unavoidably superficial. Even though the team may interview a total of 60 farmers in different areas of the region of interest, each team member interviews only 10 to 15 farmers in 3 or 4 days. Each team member then is generalizing from too small a sample by any statistical standard. Usually, observations of the more articulate or assertive team member are written up as the findings of the whole team. Disagreements between team members are not treated fully because there is not enough time to investigate the problem further.

The sondeo method ignores individual variation in farmers' decision rules, plans, and farming practices. The heterogeneous nature of farmer behavior is swept aside in the effort to generalize from the too-small sample of the population. Team members forget that there are differences in farmer behavior and decision rules even in a region with homogeneous agroclimatic conditions.

Although Hildebrand claims that the sondeo is used only to point the technical team in the right direction and that detailed socioeconomic data are gathered later, there is no procedure to test the importance or universality of the constraints identified in the sondeo. Unlike the test of the decision model in which the constraints identified by a first set of farmers are compared with choice data from a second sample, the hypotheses and generalizations in the sondeo report remain untested.

The anthropologists in the socioeconomic team waste their uniquely anthropological training. While they contribute to ICTA's program by surveying farmers, analyzing regional budget data, and evaluating last year's farmers tests, they do nothing that cannot be done by a sensitive agronomist with two or three courses in social science. They could be collecting ethnolinguistic or ethnoecological data on the ways farmers process information about their environment and categorize their traditional varieties of seed (Brush 1980, Brush et a1 1981); their lands, soils, and crops (Johnson 1980), and the pests and diseases that attack their crops and livestock (Araujo 1977). Ethnoecological analyses of native terms and expressions and ethnographic decision modeling of the kind presented here are tools that would make an anthropologist's contribution to a farming systems program unique and invaluable.

This paper would end neatly if I could say that all anthropologists on ICTA's socioeconomic team learned to use decision trees, or that the sondeo's procedure of identifying farmer constraints is now accompanied by a more rigorous testing procedure. But the conflict was not resolved before the Rockefeller field staff (myself among them) terminated their work in August 1979 at ICTA's request. Consequently, most of the original socioeconomic team left ICTA to be replaced by agronomists with some social science training. Although many of the technicians I worked with in Quezaltenango became proficient in the use of decision-tree methodology, the model was never fully nor formally integrated in ICTAs farming systems program. Its potential in a farming systems program, as a tool to be used along with the sondeo in the design stage, or complemented by an adoption measure such as the acceptability index in the evaluation stage, still remains to be explored.

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Workshop summaries

Half-day workshops were devoted to the strengths and to the limitations of anthropology and related social sciences for interdisciplinary work in developing food production technology. In each workshop a social scientist, a biologist, an economist, an administrator, and a policy maker gave a 10-minute presentation to introduce various viewpoints for discussion. The workshop discussion summaries follow.

STRENGTHS OF ANTHROPOLOGY AND RELATED SOCIAL SCIENCES

Summarized by Jacqueline Ashby

The strengths of the anthropological and related social sciences in interdisciplinary research can be grouped into major areas related to the:

- contribution of their methodologies to interdisciplinary team research,
- the stages of technology development research in which their contribution is likely to be effective, and
- substantive research issues and orientation.

Contribution of methodologies

The value to an interdisciplinary team of a social scientist who provides a depth of understanding of farm-level circumstances, based on participant observation research that requires immersion at the field level, was emphasized. First, qualitative field research complements survey work and provides a balance to teamwork conducted in an experiment station or farm trial setting, provided the issue of extrapolating results from the in-depth study to a more general population is addressed. Second, anthropological field research requires a suspension of the normative approach to farmers' beliefs, practices, and circumstances that characterize natural sciences. Field research sensitizes other team members to the nature of farmers' practices and to the implications of those practices for other research disciplines. In each of these respects the strengths of social science field work in a feedback role among farmers, scientists, extension workers, and policy makers were emphasized.

Technology development stage

The importance of clarifying the social implications of alternative technologies before they are transferred to farmers was the central theme of the discussion of the stage at which anthropological social sciences can most effectively contribute to team research in technology development.

The anthropologist's or sociologist's entry point into interdisciplinary teamwork often will be late in the adaptive research phase where the payoff to farm level research is likely to be most immediate. Social science research, however, will necessarily have important feedback links to basic technology design research. If such research is integrated into the early stages of a team's basic research, it can contribute to the identification of design problems.

Substantive orientation

Turning to the substantive orientation of anthropological and sociological research, the necessity for interdisciplinary team members to come to grips collaboratively with the constraints of interrelated physicobiological, economic, and social systems was stressed. One of the strengths of social science specialists lies in their orientation to the organizational setting and management of resources in rural areas. This includes looking at the capabilities of research and transfer organizations. The organization of research and transfer systems involves fundamental implications for what is feasible in technology design. Research on the organizational setting into which food production technology will be introduced can help to develop new models of how to do research and extension.

A second substantive research area is the interpretation of what the physical environment and ecological factors in agriculture mean to human use of technology. This is especially true of technologies that require farmers to organize as a group or imply changes in existing group management of an environment.

A third area involves identifying the social consequences of introducing technology for food production and consumption systems. This can include defining potential as well as actual beneficiarygroups, and interpreting the consequences of technology with respect to the policy environment. A focus on institutional factors provides interdisciplinary team members with a context for evaluating how farmers and other social actors may respond to different technological alternatives and their outcomes.

LIMITATIONS OF ANTHROPOLOGY AND RELATED SOCIAL SCIENCES

Summarized by Robert Rhoades

The limitations of anthropological and related social sciences in interdisciplinary research can be grouped into three broad areas:

- limitations in methodology and disciplinary assumptions,
- limitations in the ability to define and communicate roles and contributions in agricultural research, and
- limitations in training for applied agricultural research.

Limitations in methodology and disciplinary assumptions

Anthropological and related social science research often is difficult to replicate. The scientific method is not clearly followed — frequently there is no set of working hypotheses. Frequently research is descriptive, site specific, and, because sampling is not random, cannot be generalized. The lack of quantification, statistical methods, and clearly articulated models tends to reduce the credibility of anthropological or related social science research among agricultural scientists.

Some social scientists, especially anthropologists, tend to view society as static and to concentrate their research on small groups (villages, tribes, etc.) that are not fully integrated into monetarized economies. Anthropologists are largely concerned with reporting what exists or has existed, apd have not developed models as predictors of agricultural change.

Limitations in ability to defme and communicate roles and contributions

Although anthropologists and other social scientists have long been concerned with rural peoples, they have had little professional contact with biological and technological scientists. This makes it difficult for them to articulate their potential contribution in terms understandable to scientists from other disciplines. Social scientists have not explained the relation between social organization or ideology and agricultural technology in a form understandable

to biological scientists. Their tendency to write unjustifiably lengthy papers quickly causes biological scientists to lose interest.

Anthropologists are stereotyped as social scientists who do research alone among exotic peoples who are not representative of the larger body of world farmers. They tend to identify more with target populations (farm families in this case) than with their colleagues from other disciplines in agricultural research. Frequently they function as defenders of traditional ways rather than as team members concerned with identifying and generating new technology.

In contrast to disciplines such as agricultural economics, agricultural education, or agricultural engineering, anthropology has no specialization in agricultural anthropology. The field lacks a professional grouping and perspective concerned strictly with explaining agricultural problems.

Limitations in training for applied agricultural research

Many anthropologists and other social scientists lack basic training in technical agriculture and few anthropology schools encourage training in agriculture. Anthropologists are generally associated with liberal arts colleges or nonagricultural universities. This historical separation of anthropology from agricultural sciences in universities has prevented an interchange of ideas between anthropology and agricultural sciences.

Conclusion

The limitations summarized here were not agreed upon by all participants, but all were raised. Scientists in other disciplines did not feel that anthropologists and other social scientists were entirely to blame for these limitations. But they did feel that the burden was on anthropologists to better articulate their positions to correct the misconceptions held by those unfamiliar with anthropology or sociology. This better articulation is necessary if social scientists want a greater and future role in agricultural institutions.

Workshop recommendations

Workshop participants formed policy recommendation groups to consider four questions germane to the role of anthropologists and other social scientists in interdisciplinary teams developing improved food production technology.

1. How do we train anthropologists and social scientists to assume a vital role in interdisciplinary teams at various levels?
2. What types of research problems lend themselves to anthropologists and social scientists playing a productive role in interdisciplinary teams, and what methodologies contribute most effectively?
3. In what roles, other than as members of interdisciplinary teams, can anthropologists and social scientists contribute to the work of developing improved food production technology?
4. What institutional arrangements can best facilitate the fruitful role of anthropologists and social scientists in interdisciplinary teams?

TRAINING

Training social scientists for work in interdisciplinary teams is a function of the roles they are expected to play in agricultural research institutions or agencies. Because training for scientists is a continuing concern of research institutions, the recommendations deal with steps institutions can take to strengthen the training of social scientists in an interdisciplinary setting.

1. Research institutions should encourage formal training in agriculture, ecology, and geography as well as practical experience in agricultural development.
2. Research institutes should expand internship programs for social science students at local universities to supplement their academic training with experience in interdisciplinary settings.
3. Agricultural research institutions should develop or continue internship and postdoctoral programs for social scientists to provide them with training in interdisciplinary work on technology development.
4. Social scientists at research institutes should establish strong ties with local universities, especially with social science departments where they can act as links between the two systems. These linkages should be used to train national social scientists for work with research institutes.
5. As part of an overall strategy for strengthening national programs, national universities should be encouraged to strengthen programs that emphasize applied social research.

Policy Recommendation Group I

Michel Benoit-Cattin
Alan C. Early
Grace E. Goodell
Enrique Mayer
Donald L. McCune
Roger Montgomery
Robert Werge

RESEARCH PROBLEMS AND METHODOLOGIES

Research problems

Specific research topics in agricultural development, which directly or indirectly involve human beings in their physical, social, or cultural nature, are as numerous as the concerns of the research institutions as a whole. Anthropologists and other social scientists must contribute to the definition and solution of these problems at all stages. They must work jointly with the institution's scientists of other disciplines. Specifically, they should be concerned with:

1. Classification research to describe social, cultural, and human ecological aspects of the farming regions of concern to the research institution.
2. Diagnostic research focused on the isolation and description of production problems within regions and farming systems of concern to the research institution.
3. On-farm evaluation of developing technology. Research in a context of on-farm evaluation might be one of the easiest points of entry and first articulation of an anthropological social scientist with other members of a technology development team. Work in a team context on actual technology design could follow.
4. Evaluation of technology transfer should be done by the anthropolgists or social scientists in cooperation with other team members to ensure that farmer feedback is considered in further technology design and development.

Methodologies

A number of methodologies are available to the anthropological or social scientist.

1. Surveys are perhaps of greatest use in classification work. They can be broad and definitional, or intensive and diagnostic.
2. Participant observation is important for documenting processes in describing production systems, and evaluating on farm-testing and transfer of technology.
3. Analysis of farmers' knowledge systems is an important area in which anthropological social scientists can contribute. Farmers' knowledge systems are likely to be complex, culturally specific, and only partially articulated by the farmers themselves. Farmers' knowledge tends to combine information on farming techniques in subject-matter areas often systematically separated by scientists.
4. Literature review of ethnographic and other primary and secondary material is useful to provide a guide to existing information on social, cultural, and human ecological aspects of farming.

In each of the methodologies there will be the need to apply particular disciplinary techniques in various combinations for different purposes.

Overall, the contribution of the anthropological social scientist most often will lie in describing systems, their bases, and their dynamics, and in relating this information to the identification of interventions that can increase production.

Policy Recommendation Group II

Nyle C. Brady
Gelia T. Castillo
Victor Doherty
Colby Hatfield
James A. Litsinger
Christine Okali
Jean-Francois Richard
Jim G. Ryan

OTHER ROLES

The anthropologist or social scientist has a role to play in an organization developing improved food production technology by increasing efficiency through process-oriented rather than product-oriented activities. These process-oriented activities include institutional goal formulation and educational, professional, and research methodology development.

These activities partly coincide with the functions of an anthropologist or social scientist as a team member, but extend beyond that role. If a social scientist is not available, some of these activities may be carried out by other disciplines although without the specialized focus on social organization.

Institutional

1. Define the cognitive, cultural, and organizational limitations to prospective technology. That includes farmers' perceptions of the technology as well as the organizational structures within which the technology will have to be used.
2. Contribute to assessment of research priorities.
3. Contribute to the examination and development of the relationships between international and national programs.
4. Provide anthropological insights to clarify the institute's concept of the target clients, and to refine the clients' concepts of particular research programs.
5. Help research administrators to improve the flow of communication among and within research groups.
6. Help to ensure that technology is in practice accessible to and usable by farmers.
7. Help to ensure that farmers' feedback on technology is used by the institute.
8. Raise new research challenges on the basis of observed village level needs and institute's capacities.

Educational development

Help trainees in institute and national programs recognize and work with sociocultural considerations in designing on-farm research and extension methods.

Professional development

Involve national and regional anthropologists, and social scientists from national programs and universities, in agricultural research through training courses and regular exchange of ideas and information.

Research methodology development

1. Make anthropological data produced elsewhere accessible for institute purposes.
2. Improve the conduct and content of surveys.

Policy Reconmendation Group III

Susan Almy
Benjamin Bagadion
Syarifuddin Baharsjah
Robert Brinkman
Sir John Crawford
Robert W. Herdt
Romeo Raros
Robert Rhoades
Iwao Watanabe

INSTITUTIONAL ARRANGEMENTS

There is no unique set of institutional arrangements that can best facilitate the fruitful participation of anthropologists and other social scientists in interdisciplinary teams developing improved food production technology. But there are general principles and strategies institutions can follow to make the most effective use of anthropologists and social scientists.

Objectives

1. Facilitate teamwork and the attainment of team objectives. The most effective contribution of social science disciplines — anthropology, economics, sociology — will be made in institutions where an interdisciplinary team approach to research problems is part of the structure.
2. Stimulate flexibility. Recognizing that individual specialists in the social science disciplines will represent different emphases within a range of skills and orientations, institutional arrangements should encourage and reward flexibility of all team member specialists in social and natural science disciplines.

Strategies

I. Recognize that research teams tend to form spontaneously out of group definition of research problem areas. Disciplinary specialists will work in various teams for different problem areas. To facilitate the inclusion of the social scientists in the formation and work of research teams, a permanent staff anthropologist or sociologist will be required to participate on the same institutional basis as other principal staff scientists. The reward structure for team research should be determined by the policy group designed to stimulate team research in the institution concerned.

2. The entry point for social scientists should not be one in which the social scientist plays the role of an outside scientist coming in to evaluate the results of research in other disciplines. Rather, institutional arrangements must facilitate and reward collaboration and constructive exchange of views among peers. Although the entry of anthropologists or sociologists into an institution may coincide with the adaptive stage of many research projects, it should be recognized that their role requires them to be integrated into the team's basic research, technology transfer, and training.
3. Institutional arrangements should recognize that technology design is a continuous process involving feedback among research teams, transfer systems, and farmers. The maximum contribution of anthropologists and sociologists to the interdisciplinary team will be achieved by involving them, at the earliest feasible point, in identification of a problem area in technology design.
4. The policy-making group for the institution should have some representation from the social sciences.

Policy Recommendation Group IV

Jacqueline Ashby
Robert Booth
Ralph K. Davidson
Christina Gladwin
Christine Okali
Dennis J. Greenland
Senen Miranda
Richard Sawyer
Pierre Weiss