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EDITED BY
FRED HOEFER
CARL PRAY
VERNON W. RUTTAN
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LOW-INPUT TECHNOLOGIES AND FARMING SYSTEMS: THE IMPLICATIONS FOR RESEARCH PRIORITIES

Charles A. Francis
Rodale Research Center
Box 323 RD#1
Kutztown, PA, 19530

Farmers in developed countries are becoming more concerned about reducing costs of inputs as the economics of farming become more marginal. This in some cases requires the development of low-input technologies which are consistent with the new yield and efficiency goals of the farmer. In developing countries, farmers are concerned with increasing yields and income or reducing risk without the additional costs of purchased inputs from off the farm. In many cases, this requires a new focus on the types of technology which should be developed for their specific needs and level of investment. Low-input technologies are an increasing concern to those involved in decision making in research, since development of these technologies may require a change in agenda within the research centers.

The farming systems approach to research and development is another dimension which is becoming more a part of the research/extension establishment in many developing countries. Although some claim that this logical extension of the farm management approach is not new, there is a growing awareness of how this useful concept concentrates on the interface between farmer and researcher and emphasizes the joint identification and resolution of problems on the farm. The growing importance of on-farm trials as one dimension of the farming systems research process requires careful examination by the administrator and researcher. There are important implications of resource allocation and time spent on the several possible levels of research activity which need to be evaluated.

These decisions all need to be consistent with the objectives of research, the biological and climatic limitations to production or stability of yields in a region, and the constraints which are perceived by farmers in the zone. The resources which are available for research through the international centers and through private industry must be considered in planning a national or university research agenda. There is little room for excessive duplication in research when total resources for this activity are limited, especially in most developing countries. A series of issues are addressed which are important to those making decisions in research, and many of these issues are applicable to systems other than those involving low-input technologies. The farming systems

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approach is used as an overall framework within which to fit the discussion. A key reference is the paper by Harwood (1980) which describes the complexity of agriculture and the needs which research must address.

What is unique about low-input technologies?

Design of agricultural technology in recent years has emphasized the dominance approach, with fossil fuels and derived products used in increasing amounts to more completely control the plant's growing environment. Adequate fertility is provided by application of soluble chemical fertilizers. Seedbeds are prepared with heavy machinery used several times before planting. Pests are controlled with chemicals designed to kill specific organisms -- insects, pathogens, weeds -- which interfere with crop growth and seed production. Crops are irrigated in some favored areas to produce maximum yields under an unpredictable rainfall pattern. And harvesting is accomplished with ever larger and more time-efficient combines. This describes "high-input agriculture", and defines in brief terms the green revolution approach to increasing food production.

In contrast, low-input technologies are used to help the farmer attain similar goals, although the expectations for crop yield in a given season may need to be altered in some situations. Without dominance of the production environment, we are faced with the need to prepare a seedbed, provide fertility, control pests, conserve water for the crop, and harvest with minimal use of the heavy inputs listed above. There are alternatives to the use of intensive fossil-fuel based technologies, and these are dependent on use of appropriate information and intensive management. These alternatives are called by various names, but we are calling them "regenerative technologies."

These agricultural practices depend as much as possible on resources which are available on the farm or in the immediate area. They make maximum use of rotations and green manure crops to supply nitrogen, mixtures of species and rotations of crops with different rooting patterns to explore different soil strata, and intensive use of crops and cover species to maintain living or dead vegetative material on the soil surface during as much of the year as possible. These practices drastically reduce erosion, thus increasing nutrients available to growing crops. There is minimal effect of these practices beyond the field or farm boundary. Specific examples are used to illustrate the other sections of the presentation.

Implications for research priorities come directly from the objectives of research and the conditions under which results are going to be applied. It is necessary for research specialists to be familiar with the farm applications of low-input technologies, and have an open mind about the potential range of solutions to a given constraint to production. When we consider the training of current agronomists and other specialists in high-technology agriculture, this change in orientation may not be easy. For this reason, our training programs and educational activities should be oriented toward a methodology for identification and solving of problems, rather than the learning of a series of solutions which can later be applied. The research administrator faces a challenge with researchers who are trained and rewarded by peers for results which compare favorably with others in the literature -- often under high-technology conditions. These are among the changes in attitude and conduct of research which must be anticipated when the focus is shifted to low-input technologies. Some suggestions for research priorities leading to a regenerative food system are listed by Harwood and Madden (1982).

How does the farming systems approach affect priorities?

The recent publicity given to farming systems research, and the farming systems approach to development, has awakened interest in a holistic consideration of farms and cropping systems. This has historic roots in the farm management programs which were developed by agricultural economists and agronomists earlier in this century -- in Missouri this was called "balanced farming" and the system made an impressive impact on farms there in the 1940's and 1950's. Most of the current interest has come as a result of a few key individuals working in the international centers and integrated development programs which are concerned with farm income and family well-being. This is contrasted to the more traditional objectives of plant breeders and agronomists to seek technology which will provide maximum yield or response to added inputs in a high-tech situation. This approach involves the farmer to a greater degree in the decision making process in research, and also in the field implementation of research. Key references on farming systems research are by Hildebrand (1976), Norman (1980), Gilbert, Norman and Winch (1980), Shaner, Philipp, and Schmehl (1981), the work of the CIMMYT economics program (Collinson, 1972; Perrin et al., 1976), and the series of workshops held at Kansas State University (for example, Flora, 1984).

The importance of this focus for research is becoming better understood in both the developing world and in more developed areas. With a greater involvement of the farmer in the design of priorities and the implementation of research, new methods of communication must be sought and followed. There is a balance of on-station and on-farm research to be worked out, and this involves a shift in the traditional concept of doing research. The administrator can play a crucial role in this process by working within institutions to find ways to assign resources to on-farm research, and to reward researchers who are involved in this type of activity. This may not be easy in some traditional organizations, where status has come from neat trials on stations and scholarly papers which are accepted in international journals in each discipline. There is a great need for courage and creativity on the part of administrators and scientists alike in overcoming these current problems.

What technology is needed for development?

There has been a fascination with "appropriate technology" for the small farmer, and more recently for any low-input farming situation. The term has become synonymous with small machines, those powered by pedals, small engines, or methane, and concentration on increasing labor inputs into the production process compared to "high technology" which is available in the marketplace in developed countries. There is much benefit to some of the research and development which has gone into appropriate or "intermediate" techology (see Congdon, 1977). It is also highly inappropriate to think that all "appropriate technology" must fit the criteria outlined in the many publications which have been written about it. A technology is appropriate to a given set of economic and resource circumstances if it meets the objectives of the farmer and is consistent with the best available information in a region. Thus, a huge Steiger tractor which can cover 200 ha in a day is appropriate to the wheat country of the US Great Plains if it is the best machine available, if it is used to implement a minimum tillage wheat system, and it is profitable to the farmer under a given set of labor and other input costs. A smaller 25 hp tractor can be equally appropriate under other circumstances, and so can a 10 hp tiller for a garden or a light weight hoe for limited areas. The important perspective for the researcher to consider is a range of potential solutions to each production problem, and that the recommended solutions are consistent with the farmer's objectives and resources, and can contribute to the sustainability of the farming system.

It is this last factor which is receiving more and more interest in the US and in other parts of the world. We are concerned with the long-term effects of many current tillage and cropping practices, and it is becoming increasingly evident that they are not sustainable with the present costs of fossil fuels and the degradation of the production resource which the systems are causing. There is a need for focus on regenerative practices which can build rather than destroy the soil resource, and for technologies which promote this improved production potential. Research on non-chemical systems or those with limited inputs of chemical fertilizers and pesticides is providing new information on the biological structuring of agricultural systems and how the elements can be fit together in more productive and less costly combinations (Harwood, 1984). There are strong benefits to crop rotations, to cover crops which prevent erosion and provide nitrogen, to overseeding cereals with legumes, and to mixtures of crops which can promote nutrient cycling in the root zone. There is a growing community of researchers who are addressing these questions in the public and private sector in the US.

There are a number of important questions which the administrator must address in the quest for a series of appropriate technologies. First is the principle that no single technology or set of cropping practices is appropriate for all farmers. The research team must be encouraged to examine a range of alternatives, and to describe these to the client farmers so that the latter can make rational decisions about what to choose. Resources must be provided for research into alternative methods of land preparation, soil fertility, cultivation and pest control so that the best options for a low-input situation can be found. Often there will be little support and limited enthusiasm from researchers in conventional departments about this focus. There will be no support, and even antagonism from industry if this approach is seen as cutting into the traditional markets for farm inputs. A recent example in fertilizer recommendations has been well documented (Liebhart, 1983). The researcher must be encouraged to pursue these unpopular routes to developing a range of technological alternatives, and must be protected and supported by the administration to allow successful completion of the work. This again takes courage by administrators and researchers.

How do we anticipate changes in farmer technology?

Much of our current research is directed at solving immediate problems. Certainly the products which are on the market and the demonstrations or practical "research" carried out on farms by the commercial sector is focused on the immediate constraints to production and what can be purchased and applied to solve them. A more comprehensive research approach must include a time frame, however, which takes into account the time needed to solve different types of problems and the dynamic nature of the farming industry.

A new recommendation for a level of fertilizer or a mixture of herbicides, for example, may be derived from two years of research over several locations. The resource level of farmers in the region is unlikely to change dramatically in that time, unless a specific crop is abandoned for some macro-economic reason. On the other hand, development of a new maize hybrid or wheat variety may take 10 years of crossing, advancing generations and selection, and testing on farms before this is ready to be recommended to the farmer. Many things can change in those ten years. The lesson for the administrator and researcher is that level of technology must be anticipated, and not assumed to be static. If the farmer is unlikely to be growing maize in ten years, there is no reason to launch a new breeding program. If a new dam is being built to bring irrigation to a zone, then drought tolerance might not be a high priority for the breeder. If energy cost is increasing and erosion is taking a heavy toll in nitrogen and other nutrients, the time to start research and demonstration on alternative methods of providing fertility to crops is now. These are the types of questions which need to be asked, and the decisions which are needed in research for the results to be effective in solving real farm problems.

Can we quantify research priorities?

Most research priorities are set by specialists who have a reasonable knowledge of the crops and cropping systems into which they will fit. Often, the resource level of the farmer is taken into consideration, and the most limiting constraints to production evaluated in some way before initiating research. It is difficult, however, to gather all the information needed to make these decisions. One attempt to sort out the complexity of setting priorities was presented by Parkhurst and Francis (1984). The first step is evaluation of production constraints, through visits to farms, discussions with researchers, or conducting detailed surveys. Too much dependence on conventional wisdom of a previous researcher or an incumbent administrator is to be avoided. To set priorities from this information, the researcher should consider the relative importance of a series of constraints, by ranking them or giving each a value. A series of potential solutions for each constraint must be thought through, and a probability of a success through research attached to each potential solution. In addition to the probability of getting a research solution, the probability of adoption needs to be considered. These several numbers can be combined into a priority index, which quantifies the intuition and gives a numerical guide to priorities.

This is illustrated with two examples taken from Parkhurst and Francis (1985). The first is monoculture sorghum grown in Nebraska, and shown in Table 1. Eight limiting factors to production of sorghum are listed, along with a rating of the importance of each. The data come from a survey of researchers working in the state on this crop. Then two potential solutions are listed for each constraint, such as solving drought by either tolerant hybrids or by irrigation. Next the probability of adoption is given for each solution: there is a high probability of adoption of drought-tolerant hybrids (0.9), of greenbug-resistant hybrids (0.9) or of hybrids which are more efficient in use of fertilizer (0.9). The proability of adoption of irrigation is very low (0.1) as is the appliction of chemical fungicides for stalk rot control (0.1). The three numerical factors are multiplied (importance x probability of solution x probability of adoption) to give an index of priority. These priorities are then ranked to show which items should logically be included in a breeding program. In the example, the program should include breeding for greenbug tolerance, drought tolerance, systems including legumes in a rotation, and increased fertilizer use efficiency. The priorities derived in this way are only as good as the information which is used to generate them. The procedure is an advance over the highly subjective and non-quantitative methods which are usually employed to set priorities.

A second example is for the maize/bean cropping pattern in the medium elevation of the Andean zone. Six constraints are listed, along with importance of each. Two potential solutions are suggested for each constraint, as well as the probabilities of getting those solutions through research and that farmers will adopt the new practice or varieties. Using the same method as above, we find that greatest priority in an intercrop research program should be breeding for resistance to anthracnose, testing the potential of compost or manure, and seeking lodging resistance in maize. Although most researchers will consider the probabilities of solving problems in some general way, they rarely try to quantify this process or objectively compare one activity with another. Although adoption by clients is considered in traditional research, it is not given the emphasis that a farming systems approach would require. The involved administrator can implement this type of review of priorities for research, and whether this precise method or another is followed, the right questions during project reviews can lead to the same types of inquiry into setting priorities.

How specific are research results?

At one end of a spectrum, the farming systems and components of those systems are specific to each farm and the unique combination of resources, education, and management skills of the farmer. At the other extreme, certain widely adapted hybrids or varieties of crops are expected to have utility across a wide range of altitudes, rainfall patterns, and levels of resource use. There are also biological principles derived from research which have wide applicability, and which may be applied under specific farm circumstances. The researcher must make a decision based on experience and results from trials about how widely a given variety or practice could be expected to extend.

The farmer must sort out the multiplicity of information available and decide which practices in fact apply to a specific farm. Commercial enterprises would like to extend recommendations as far as they find feasible, to spread the cost of research over as many units as possible and bring the price of product down to where it will be acceptable to the farmer. And the administrator often is concerned with some geographical boundaries -- state, region, country -- within which the particular organization has jurisdiction to study agriculture and provide recommendations. Within this complex social environment, we need to study how specific or how widely applicable research results can be.

The first obvious approach to testing range of adaptation of a variety or practice is through regional trials, on branch stations or on farms. This commonly is done in breeding programs, public and private, across the range of geographical sites where the varieties are expected to be sold. In many areas, variable rainfall patterns require that a number of sites be used to approximate the conditions which would be expected over a number of years in a zone. Commercial breeding programs often use 20 or more locations each year to quickly make decisions on what materials to release. Public institutions may be limited in travel budget and thus opt for a series of years at a single or small number of central locations. The trade-offs of years, locations, and replications have been quantified for sorghum by Saeed et al. (1984), who showed that precision is about the same in 2 years and 2 locations as in 1 year and 5 locations, and is about the same in 3 years and 2 locations as in 1 year and 9 locations. The apparent research solution to getting results quickly is to multiply locations in a single year when possible. This could require a reorganization of resources assigned to projects which have this responsibility, or better a collaboration among researchers working on the same crop or in the same locations to collaborate to get more trials of a given type.

Another approach to extrapolation of results is intensive use of historical climatological data. With results of experiments in hand and a good idea about the temperature and rainfall data for a broader area, it often is possible to extrapolate the data to a wider range of sites with similar conditions. This is not a substitute for on the ground testing, but could lead to fewer tests in the most indicated sites. More information is needed on the range of application of research results, and some way to quantify the experience of older researchers and key farmers would be one approach to providing this information.

What is the role of on-farm research?

Research with collaborating farmers is one key to the farming systems research approach. Since farmers are involved in setting priorities and in the entire research process, they are active participants in the series of events which leads from problem identification to trials to recommendations. They help the researcher decide what types of trials could best be done on the farm as compared to those which need to be done on the experiment station. Some situations are obvious -- crossing parents to produce new hybrid combinations of maize should be carried out under the controlled conditions of the station. When seed has been increased and a series of hybrids are ready for final testing, this can most effectively be done on the farm. This is done routinely by commercial hybrid companies and by some public institutions. Other questions are not this easy. Determining nitrogen recommendations, for example, could be done either on station or on farm -- the researcher needs to decide how much precision is needed and how much data is to be collected, and whether this can only be done on the station. At times, there are trials which can be conducted in a replicated fashion on station and at the same time as demonstrations on farms. This was done with new bean varieties in the coffee zone in Colombia by CIAT. There is no single answer to what type of trial should be done in each place. It is important that the station be representative of the type of soil and climate where the results are to be applied. Some guidance is available for on-farm research, from CIMMYT (Perrin et al., 1979), and from the midwest of the U.S. (Havlin and Elmore, 1984).

In farming systems methodology, there are trials which are researcher designed and implemented as well as some which are researcher designed and farmer implemented. These are the most precise of replicated trials, and provide data for defining recommendations. The most practical set of trials are those designed and implemented by farmers, reserved for the most promising of new varieties or technologies which are ready for observation and adoption on the farm. There are again a series of decisions which need to be made at the administrative level, on what proportion of resources to devote to this on-farm part of the research - extension process. The Regenerative Agriculture Association and Rodale Research Center are involved in exploring other routes to on-farm research and testing. A network of collaborating farmers is being organized through the New Farm magazine to solicit coooperators from among interested farmers (Barker, T. C., personal communication). The Center for Rural Affairs in Hartington, Nebraska is working with farmers in a practical series of tests which will help farmers design tests and arrive at their own recommendations (Goodman, S., personal communication).

How de we implement information exchange?

Although the cooperative extension service has provided an invaluable service in the development of U.S.

agriculture, there is some question about its effectiveness today. Studies have shown that farmers obtain from 5 to 15% of their information from the extension service. Some major changes from the time when this was a major source of recommendations are apparent. There is much more rapid communication today, and more people are more closely in touch with an array of sources from newspaper and TV to instant weather and market advice by computer network. The commercial sector bombards the farmer with information about an astonishing range of products which will protect crops or make them grow better. How does the farmer sort out these various resources, and how does low-input technology reach the farmer?

When there is a recommendation for elimination or reduction of application rates of fertilizers or pesticides, the commercial sector will not actively pursue that recommendation. There need to be other channels. Field days and workshops have been presented in the past year by the state experiment stations in Iowa and Nebraska, as well as by the Regenerative Agriculture Association. One field day at the Dick Thompson farm near Boone, Iowa attracted more than 500 people from nine states -- to see the Thompson's non-chemical approach to tillage, fertility and pest control. The Regenerative Agriculture Association has recently set up the Farmer's Own Network for Extension (FONE) through which farmers can locate and talk to other farmers about mutual problems and recommendations. These are two of the innovative ideas which have come along in the past several years. There are bound to be more ideas as farmers organize themselves and interact more actively with the land grant university system.

Challenges of extension of information outside the U.S. are great. In many countries and cultures, the concept of an active and well funded extension service has not met with success. In part, this is due to well-meaning and energetic advisors attempting to organize systems which have worked in the U.S. but are not appropriate to other languages and cultures. The extension service often is administered under a different ministry from agriculture, and in most countries is independent of the university as well. Creative approaches within other cultures need to be sought out to find the most appropriate and efficient way to communicate information to farmers. The farming systems approach is a logical one, although implementation will have to adhere to cultural realities. When technology is available which is consistent with farmer's objectives and resource base, there is a greater chance of acceptance of the new variety or practice.

How do public and private organizations work together?

When total resources are scarce, there is even greater incentive for public and private sector specialists to work together. An impressive example of this collaboration took place in 1974-1976 when a graduate student from the University of Florida was supported by CIAT to test new bean varieties and cultural practices with farmers in the coffee zone at about 1500 m elevation in Colombia. Groups were identified and organized by the Federation of Coffee Growers, and their agronomist helped to supervise trials and demonstrations. The private corporation ABOCOL provided fertilizer and technical assistance as well. This scheme worked well for two years while all the people were in place. As with so many projects, when key individuals were transferred to other duties the project was discontinued.

There is room for cooperation between public institutions and industry. Grants from industry can help university researchers to undertake specific projects which would otherwise not be possible -- yet it is important that these funds not become overly important to the extent that they set research priorities for the public sector. A number of cooperative breeding projects have been conducted in Nebraska between companies and university, to the mutual benefit of both. Yet the stimulus in low-input agricultural technology will no doubt have to come from public sector researchers and organizations, since there is less commercial incentive to recommend that farmers cut back on inputs.

What are the implications for research priorities?

Harwood (1980) describes four broad areas of expertise which are needed to stimulate production development for low-input agriculture:
-- ability to develop components of technology which are appropriate to low-input agriculture:
-- ability to put these components together in combinations and the integration of cropping activities with livestock.
-- understanding of the microstructure of agriculture, including nutrient cycling, energy flow, and role of locally available resources for production.
-- understanding of the macrostructure of agriculture, and concentration of scarce energy on food production rather than on handling, processing, and transporting food.

These are global types of issues which need to be considered in the orientation of a program toward greater sustainability and eventual regeneration, but these need to be divided into more detailed operational priorities on which to base decisions on specific projects. A series of items are listed by Harwood and Madden (1982) which could form a research agenda for regenerative agriculture. The following outline is abstracted from their essay, with a simplification of some of the more detailed points.

1. Evaluation Methods: an assessment of existing systems is needed to determine energy flow, extent of soil loss/formation, nutrient and water balance in the system, and the biocide load which the current system places on the total environment. This evaluation can provide a baseline against which to measure alternative systems.

2. Crop Production Research: a wide range of species for overseeding needs to be found or developed; agronomic practices for overseeding need to be extended; seed production for these species must be studied; living mulch systems and species for their practical application need to be explored; research on perennial grains can yield long-term results for the future; weed control through tillage and crop competition needs to be studied, including the effects of allelopathy; insect and disease management through rotations and IPM holds great promise; more energy efficient food systems need to be found, including the cultural phase and the processing, packaging and marketing phase; research on the holistic nature of crop-animal integration needs to be advanced.

3. Soil fertility research: efficiency of nutrient cycling through use of cover crops needs more work; role of soil-borne insects in nutrient flow needs research; other nutrient sources which have minimal disruptive effects on biological systems need to be found; municipal wastes have a great potential for nutrients for crops; more efficient recycling of nutrients in organic wastes can provide better crop nutrition; availability of phosphorus from rock phosphate and potassium from low solubility sources need to be improved for non-chemical systems; better soil testing procedures and recommendations are essential; greater emphasis on biological nitrogen fixation is important.

4. Transition from chemical intensive to regenerative systems: factors active in the transition phase and reasons for decreased yields need to be studied and overcome; factors which promote a conservation ethic by farm families need to be determined and promoted; research on the macrostructure of agriculture which will promote regenerative production systems can provide new directions for encouraging this change.

Harwood and Madden (1982) also discuss imperatives for forest production, animal production, and health which are outside this review -- but cannot be ignored in the overall strategy to develop a new research agenda. Their recommendations for education and research are useful to the current discussion, and will be adapted to summarize the paper.

It is important to begin to educate farmers about the value of cutting costs of production and understanding the potentials of low-input technology. This may require changes in production patterns, especially the introduction of new crop sequences or rotations to improve soil fertility and increase non-chemical control of weeds, insects, and pathogens. It is important that the land grant institutions and federal establishment become involved in this major agenda. This is true in developing countries as well as in the U.S. Farmers need to become more self-reliant in production inputs, substituting locally available resources wherever possible for expensive imported chemicals. This will help cut production costs, reduce or eliminate pesticide and nitrate contamination in the groundwater, and if cover crops are used should drastically reduce nutrient and soil loss from erosion. Administrators play a key role in the articulation of this new philosophy, as they are in direct contact with special interest groups and industry on a regular basis. They are also in charge of a reward system which evaluates and recognizes the contributions of individual scientists in their institutions. Their attitudes and reactions can be an invaluable stimulus to these changes, or can stifle them completely.

Another important role which the administrators share with researchers is the explaining of agriculture to the general, non-agricultural public. It is essential that the urban sector understand the importance and nature of agricultural production and how it must be preserved as a part of the national economy. In all countries, there is need for a high degree of self-reliance in basic food production, and a control over much of the resources needed to produce that food. Concentration on regenerative technologies which maximize use of resources found on the farm or in the region is a positive step in the right direction. This is a large challenge which will take all the imagination and creativity of specialists in research as well as administrators. The time to begin is now, to be able to reach a sustainable food supply which will depend in the future on an ever-improving production base. That is the ultimate goal of low-input agriculture.

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TABLE 1

Step-wise method of calculating research priorities for sorghum in Nebraska, U.S.A. (numbers based on survey of active research workers in state)

Limiting Factor	Importance of Factor a/	Potential Solution	Probability of Solution b/	Probability of Adoption c/	Index of Priority d/	Order of Priority
	(X)		(Y)	(Z)	(X*Y*Z)	
Drought	9	Tolerant Hybrids	0.3	0.9	2.43	2
		Irrigation	1.0	0.1	0.90	10
High Temp.	3	Tolerant Hybrids	0.4	0.9	1.08	9
		Planting Date	0.2	0.3	0.18	14
Low Temp.	2	Tolerant Hybrids	0.8	0.7	1.12	8
		Planting Date	0.5	0.6	0.60	12
Green bugs	6	Chemical Treatments	1.0	0.2	1.20	6
		Resistant Hybrids	0.8	1.0	4.80	1
Stalk Rots	4	Rotations/Mgt.	0.7	0.6	1.68	5
		Chemical Treatment	0.1	0.1	0.04	16
Fertilizer Cost	7	More Efficient Hybrids	0.3	0.9	1.89	4
		Rotate Legumes	0.8	0.4	2.24	3
Chinch bugs	3	Resistant Hybrids	0.2	0.8	0.48	13
		Chemical Treatment	0.8	0.5	1.20	6
Stalk borer	2	Resistant Hybrids	0.5	0.6	0.60	11
		Chemical Treatment	0.4	0.2	0.16	15

a/ 10 = most important, 1 = least important.
b/ Probability based on prior research on this crop and experience with other crops.
c/ Chance of wide adoption of new technology in state.
d/ Product of these three items, to give a weighted priority for each factor.

TABLE 2

Step-wise calculation of research priorities for maize/bean cropping pattern in the Andean Zone, medium elevation (from Parkhurst and Francis, 1984b)

Limiting Factor	Importance of Factor	Potential Solution	Probability of Solution	Probability of Adoption	Index of Priority
	(X)		(Y)	(Z)	(X*Y*Z*)
Anthracnose/beans	9	Resist. variety	0.7	0.8	5.04
		Chemical	1.0	0.1	0.90
Lodging in maize	7	New variety	0.9	0.6	3.78
		Less aggressive bean	0.5	0.2	0.70
Competition in maize	5	Taller variety	0.7	0.5	1.75
		Change mgt.	0.4	0.3	0.60
Rust/maize	3	Resist. variety	0.9	0.3	0.81
		Chemical	0.8	0.1	0.24
Water stress	6	Irrigation	1.0	0	0
		Reduced densities	0.7	0.4	1.68
Fertility for maize	8	Chemical fert.	1.0	0.2	1.60
		Compost/manure	0.7	0.8	4.48