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Farming Systems and the International Agricultural Research Centers: an Interpretative Summary

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Introduction

Let me commence this summary by recalling the purpose for which this workshop has been held. The subject of farming systems research has become quite popular in recent years, particularly within the international agricultural research centers and in many of the developing countries with which the Centers work. The popularity of the subject matter has led to some confusion about the meaning of the term "Farming systems research," the scope of research undertaken in its name, and to a lesser extent, the effectiveness and relevance to agricultural development of what is being done.

The Technical Advisory Committee (TAC) to the CGIAR in its 1985 priorities paper indicates a need for increased attention by the Centers to the wise use and management of natural resources and the development of sustainable agricultural production. TAC also believes that on-farm research is an area of work done by national agricultural research systems, presumably because of its location specificity, and hence believes that input by the IARCs should decline in importance.

In 1978, TAC, finding some confusion about the nature of farming systems research and its implementation in the IARCs, requested a Stripe Review Team of John Dillon, Don Plucknett, and Guy Vallaeys to make a review and indicate what were the essential features of the subject, at least in the Centers. They came up with an excellent report, but some confusion about farming systems technology still or again exists. The Stripe Team have presented in the keynote address at this workshop an update of their earlier report which deals in part with this problem.

Farming Systems Related Research

I believe this workshop has been successful in illustrating what the IARCs and some national programs are doing in farming systems research. Virtually all Centers are involved, including two that do no biological research. The research is both commodity-based and area-based, as indicated in the titles of two sessions of the workshop, and almost all of it can be conveniently and quite simply classified into three subject-matter areas as developed at this conference and similar to those defined by Simmonds in his 1984 review. These are:

1. Farming systems analysis, i.e., the study of farming systems as they exist;

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2. Farming systems adaptive research, i.e., on-farm research with a farming systems perspective; and
3. New farming systems development.

All have to do with the efficient development of innovations and their testing--innovations as simple as a new crop variety or as complex as new farming systems for recently settled nomads.

Most Centers are undertaking studies of existing farming systems as an adjunct to and prior to their research. For this purpose they may be able to rely upon data from other sources, supplemented by additional rapid surveys. But in some cases it has proved necessary to undertake intensive and lengthy base-line studies.

The commodity-based Centers are involved in on-farm research with a farming systems perspective, i.e., on-farm research that tries to study and understand the broader implications, including policy implications, of the innovation being tested. We have had at this workshop examples of this approach from CIMMYT, CIP, IRRI and the Bean Program at CIAT. All use rather similar methods. A similar approach is used by commodity programs at Centers that are also involved with area-based farming systems research. Examples given at our workshop are the root and tuber program of IITA and the sorghum program in West Africa of ICRISAT.

Farming systems related research at the area-based Centers is primarily concerned with the development of new or improved farming systems. This applies to IITA, ICARDA, ILCA, and ICRISAT. CIAT, although predominantly a commodity-based Center, has adopted a similar approach in its tropical pastures program. Much of the farming systems development research is done at the research station, but on-farm testing of these more complex innovations is also considered necessary. CIAT has referred to the value of undertaking even the technology development in on-farm situations as well as the testing and verification.

The national agricultural research programs in Ecuador, Indonesia and Zimbabwe undertake base-line studies and on-farm, commodity-based research and have working relations with relevant Centers, CIMMYT, CIP, and IRRI, in particular. These programs are aimed at bringing production improvements into the hands of the smaller farmers. Linkages with extension services, where they existed, become something of a problem, and one reviewer considered that the programs have a rather narrow production focus.

India has a large national research program involved in area-based and commodity-based systems. It takes administrative and legislative policies into account as well as production-oriented problems.

All the activities seem to fit satisfactorily into the classification developed at this workshop. A few activities discussed here that are part of the work of Farming Systems Programs at a couple of Centers remain outside our classification. Plucknett, Dillon, and Vallaeys (this workshop) have questioned the wisdom of retaining separate farming systems units or programs. By definition, any research carried out by a farming systems program is presumably farming systems research, whether it fits the classification or not. An example might be the bacterial survey of soils being carried out by ICARDA. I have no doubt that ICARDA has good reason for carrying out this research. The problem is not the research but the name of the program in which the bacteriologists work.

But we should not strive to classify all our work in FSR into a few categories. Simmonds reminds us that too much standardization and coordination stifles creativity. The problems we are dealing with are not easy; they will require innovative solutions.

Although this summary so far has emphasized the classification of farming systems research activities, few of the Centers showed much interest in these taxonomic details. They have concentrated on explaining what they are doing and the types of results obtained. This has been much to our advantage and has enabled us to learn from each other's experience. All of us will do better farming systems related research in the future as a result of the papers that have been read and discussed here.

The Nature of Farming Systems Research

The 1978 Stripe Review report pointed out that a farming system is not simply a collection of crops and animals to which one can apply this input or that and expect immediate results. Rather, it was "a complicated, interwoven mesh of soils, plants, animals, implements, workers, other inputs and environmental influences, with the strands held and manipulated by a person called the farmer who, given his preferences and aspirations, attempts to produce output from the inputs in technology available to him. It is the farmer's unique understanding of his immediate environment, both natural and socioeconomic, that results in his farming system."

If crops research is research about crops then farming systems research, similarly, is research about farming systems. The keynote paper describes it as an approach to research which has eight interrelated objectives:

1. To understand the physical and socioeconomic environment within which agricultural production takes place.
2. To gain an understanding of the farmer in terms of his or her skills, constraints, preferences, and aspirations.
3. To comprehend and evaluate existing important farming systems, in particular the practice and performance of these systems.
4. To improve the identification of problems and opportunities for change in existing farming systems and thereby focus research on specific key aspects that limit production or farm income and their sustainability.
5. To enhance the capacity of research organizations to conduct research on priority problems of farming systems.
6. To conduct research on new or improved practices, principles, system components or subsystems within an FSR context, and to evaluate these for possible testing on farms.
7. To evaluate new or improved systems, or system components, on farms in major production areas under normal farm conditions.
8. To assist the extension, monitor the adoption, and assess the benefits of improved farming systems.

This list seems adequate to our purpose and relates well to what we have said we are doing. We freely acknowledge that in many cases we are focusing our efforts on farming subsystems, including cropping systems, rather than the system as a whole.

Farming systems research clearly requires the measurement of numerous variables, not all of which can be controlled. The statistical requirements have been touched upon only in a few papers. It is an important subject, not only to help us reduce to an essential minimum the magnitude, and hence the cost, of farming systems research, but, even more importantly, to help us determine whether we are really doing research or not.

It is easy enough to find in the workshop papers the use of the common scientific method of setting up and testing hypotheses. The general hypothesis is that the innovation being tested will succeed in solving the identified constraints. The examples used by the participants to illustrate their approach to farming systems research all give positive answers. This is not surprising with the illustrative and explanatory papers that have been developed for this workshop. But I think that there is indeed some cause for concern, both because the complex mix of parameters and variables in which we are interested are not always all easy to measure, and because of the value-driven nature of so much of farming systems research. The ICRISAT paper mentions that there is some scope for improvement in our on-farm methodology. I hope you would all agree. It is something that we should work on both individually and together—and it has been suggested that we have additional seminars and workshops.

The Necessity of Farming Systems Research

Is farming systems research necessary? I believe it is true that present-day agriculture has been developed without it. Why use it now? Our simple answer is that too much research is being done that does not benefit the target group. I do not know whether this statement is more true today than it was in the past or that it is more true in agriculture than in industry, medicine, or space. Research is a risky business. The chances of producing useful results are fairly small. It is justified because successes, though small as a percentage of the total effort, are still numerous, and many give very high rates of return on the investment made. So should we do farming systems research? Our workshop answer is a resounding "yes."

Virtually all our Centers have farming systems research activities, as do a growing number of national agricultural research programs. The list of objectives given by Plucknett, Dillon, and Vallaeys for farming systems research is also its justification, but we have several additional reasons. First, and prominently, we believe that the use of a farming systems approach will benefit the more disadvantaged farmers, farmers on marginal lands, poorly endowed farmers, small farmers and women farmers, more effectively than conventional research; i.e., there is a large equity issue involved in using the farming systems approach. CIMMYT also points out that a farming systems approach helps a farmer make short-term improvements that are preferable to longer-term improvements because of the high discount rates on investments in agricultural research. Many farmers, particularly in the rainfed areas and marginal areas where resource constraints tend to be more severe, are reluctant to adopt complex packages of practices; a farming systems approach can help overcome these barriers to adoption—although we must emphasize that the time scale will still be long.

Technologies that increase the productivity and sustainability of low-input agriculture tend to increase management input by the farmer himself and labor requirements. Such technologies are better promoted through the farming systems approach. Standard procedures of extension are largely limited to extending innovations to more progressive farmers in sole cropping using manufactured inputs. The promotion of innovations in intercropping, double cropping, residue management, and some forms of land management need the farming systems approach.

We are not entirely agreed on whether FSR is a science or an approach to research but we know, at least, that it is not a new paradigm; i.e., it does not represent the way in which all agricultural research will be done in the future. It is an approach used to provide greater benefits to certain target groups and for propagating certain types of innovations. It does not substitute for but supplements the conventional approach.

And it is not without its problems. It requires a committment to multidisciplinary activities by scientists from different disciplines, which is not always easy to obtain. ICRA is unique among our participants in concentrating its work on base-line and diagnostic studies. It has provided a number of insights into the difficulties and weaknesses involved in the farming systems approach. Poor communication and the lack of mutual respect among scientists from different disciplines are two that ICRA highlights. It has been suggested that ICRA can help us to improve our skill in using the farming systems approach.

ICRA points out, as Michael Lipton did in an earlier paper, that a farming systems approach is conservative and tends to be constrained by what the farmer already knows or can perceive. We have been reminded of this several times. The microcomputer probably would never have been developed had scientists been limited to public perceptions of what was needed in communications 40 years ago. Farming systems research also tends to make the assumption that the farmer knows best. That is not always true. The world concern for the problem of desertification reminds us also that the farmer, particularly in the poorest developing countries, but not only there, can be in conflict with the larger needs of society. ICRA and Anderson remind us that the farmer can be in conflict with his labor. Farming systems research concentrating narrowly on farmers' values or on production alone has a tendency to overlook some exogenous and endogenous constraints. On the other hand, it can get too broad to be useful.

Farming systems is probably more costly than the conventional means of disseminating research innovations in agriculture. ISNAR highlights the difficulties of managing farming systems activities and mentions the disappointing impact of some farming systems programs.

These difficulties notwithstanding, our workshop is clearly in favor of continuing research with a farming systems perspective. The positive results achieved by each Center seem to speak for themselves. The Centers are engaged in these activities because they clearly see their value and have every expectation that they will continue to do so. If CIMMYT is correct in believing that the adoption of new technology is mostly a question of assuring that recommendations fit farmers' conditions, farming systems research, and particularly on-farm research, is the way to ensure that this will happen. We believe that agricultural research for development should have a farming

systems perspective. We must recognize, nevertheless, a real need to analyze the cost and effectiveness of the farming systems approach.

The Framework for Farming Systems Research

The keynote paper by Plucknett, Dillon, and Vallaeys advocates a conceptual framework for farming systems related research comprising three major elements: base-data analysis, research station studies, and on-farm studies. All are part of the system. All must be conducted, although not necessarily by the same institute, and certainly not necessarily all at the same time. Indeed, there is an implication of a sequence, of moving from one step to the next and from the last back to the first.

This is the same framework these authors advocated in their 1978 Stripe Review report. ICARDA, ICRISAT, IITA, and ILCA, the Centers that have spoken about area-based farming systems research, all use it in their work. It can apply equally well to commodity-based farming systems research. Crop improvement research fits into research station and on-farm studies but clearly not all the crop research of our Centers can be or needs to be considered as part of farming systems research. Crop improvement research is part of the farming systems cycle if, and perhaps only if, it fits into the framework; that is, if it is undertaken as a consequence of base-line studies or of previous on-farm experiments and if the improved cultivars are tested in on-farm situations. CIAT, IRRI, and the IITA paper on root and tuber crops appear to be in consonance with this idea.

The framework might be made more useful if one additional element dealing with technology design were added. As ICRA has pointed out, and as we in ICRISAT know full well, the effective utilization of base-line studies in determining the nature of research station studies is the greatest real weakness in farming systems research. There is need for a special place in this framework for the use of mathematical models and other forms of ex ante analysis, as has been pointed out by both ILCA and ICRAF.

Particularly in rainfed agriculture, ICARDA and ICRISAT find that operational research at the research station is necessary to learn the probabilities of success in relation to climatic variability. It is difficult to obtain this information on-farm, because of the many uncontrolled variables, and because it is generally difficult to maintain a program of on-farm research on the same farm or even in the same village for more than 2 or 3 years. Also, as ICRA and ICARDA point out, it may be very costly to develop technology for a single recommendation domain, if the latter is defined with any degree of rigor. Ecuador has produced a description of a recommendation domain that mentions a large "homogeneous" group of farmers, but the impression remains that we are talking about relatively small numbers in each domain. A research institute, and particularly an international research institute, must work at a higher level of generalization, which requires the development of some form of agroclimatic or agroecological stratification and, perhaps, the use of benchmark sites.

The Role of the International Agricultural Research Centers

The IARCs are only a small part of the total agricultural research effort, even in developing countries. Few of them would spend more than 20% of their total funds on farming systems related research. Thus, they can play only a small role in this field and it should be carefully chosen. The keynote paper lists 11 areas where IARCs could be involved if or when the national agricultural systems need our help.

New farming systems development is one area in which the Centers have a comparative advantage that is shared by only a few national agricultural research centers. The new systems can include cash crops and other commodities important to the farmer, and should not be thought of as the exclusive province of the Centers with geographic or climatic mandates. Comparative advantage also applies in the agroclimatic studies needed to relate new systems to their most probable recommendation domains. On-farm research is also needed to test the technologies as they are being developed and when available for utilization. The Centers should also be involved with national agricultural research systems in developing methodologies for farming systems research because these are transferable and will lead to improvements in performance, scientific validity, and cost-effectiveness. Present methods are not satisfactory; we should encourage methodological research.

The Centers can play a major role in training for farming systems research, recognizing that there are others such as ICRA and the Farming Systems Support Program at the University of Florida that are also involved. There would be value in exchanging training materials and in understanding each other's training goals and objectives. A seminar on Training in Farming Systems Research would be worth considering.

On-farm research with a farming systems perspective even to test simple commodity innovations can be conducted only in a few places by each Center, as is pointed out by IRRI. Regional networks of countries and locations are utilized by several Centers to spread their contributions in the widest possible manner. Inter-Center, multiple-country networks merit some consideration. It is accepted that the national agricultural research systems must do most of the on-farm research both because of its location specificity and because it is often as much demonstration as it is research. But in some parts of the developing world, particularly in Africa, the current capacity of national systems to do on-farm research and participate in networks is limited, and Centers and other agencies need to assist. Some Centers feel it necessary to assume this national role until such time as farming systems can be institutionalized in the national agricultural research systems. As ISNAR points out, however, even the national systems should put only a portion of their research effort into this form of adaptive research.

In some parts of the world several Centers are involved in on-farm research in the same countries and there is an obvious need for coordination and the formation of inter-Center teams and networks. Most Centers participated in a coordination workshop on on-farm research in eastern Africa in 1984. The proposals for coordination among the Centers and with the national agricultural research systems given in the report of that workshop merit wide circulation. Ecuador reminds us, however, that coordination of agricultural research efforts within a country is the responsibility of the country itself. We agree, and ask them to exercise their right with vigor and good judgment.

I have referred in the early part of this summary to the classification, following Simmonds, developed by this workshop, which satisfactorily includes virtually all the farming systems related research we have heard about and discussed. I have also referred to the conceptual framework given by Plucknett, Dillon and Vallaeys. To me these two serve different purposes and we can gratefully accept both. The first, to tell TAC and our donors what we are doing under the heading "farming systems research"; and the second, to remind us of the steps that we must follow for the research to be done well. As Dr Gomez has put it, our strategies may differ, but our conceptual framework is the same.

I would like to take this opportunity to express my gratitude to the Chairman of TAC, who first suggested the possibility of this workshop. I believe it has given him, his colleagues in TAC, our donors, and our partners in the national agricultural research systems the information that they need to know about our farming systems research. I thank the participants for their valuable contributions and my colleagues in ICRISAT, led by Drs Kanwar, von Oppen, and Virmani, for the considerable efforts they have made to organize and conduct this workshop for us all.

Review

E.T. York, Jr[1]

Let me congratulate Dr Kanwar, his planning committee, and all workshop participants for what I think has been an excellent meeting. The quality of the papers, their commentaries, and the overall discussion have truly been outstanding.

This workshop has been particularly helpful to me in providing a better appreciation of what FSR is all about. My association with this subject is probably quite different from that of any other workshop participant. And that difference may provide a perspective that might be relevant to this discussion. Let me explain.

During the decade of the 1970s, I was involved, almost full-time, in wrestling with the bureaucracy of higher education administration—totally removed from the agricultural research arena. In the early 1980s, I decided to get out of academic administration and devote essentially full time to my primary interests—international agricultural development, with particular emphasis on agricultural research and education.

Upon making this change, I immediately began to sense some of the feelings Rip Van Winkle must have experienced upon emerging from his long sleep. I found that in the preceding 10 years something which *appeared* to be new and different had emerged

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