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Farming Systems Research

A Review

Norman W. Simmonds

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ABSTRACT

Farming systems research (FSR) has become increasingly important as an element of the research programs of the international agricultural research centers, national research programs, and development projects with agricultural research components. In this context, as the volume's foreword explains, it is important to take stock of existing experiences with FSR, to assess ongoing trends and future perspectives.

The report on the State of the art of farming systems research reviews the history of this approach and attempts to define its scope, in both a broad and a narrow sense. The paper describes various on-farm research procedures, reports on experiences accumulated in this area in different international agriculture research centers, discusses the contributions of different disciplines to FSR, the relation of FSR to agricultural extension, and raises some wider questions related to the perspectives of, and needs for, carrying out FSR further in various institutional settings.

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FOREWORD

The growing interest in farming systems research (FSR) and the substantial expansion of FSR studies in many countries represents one of the most significant recent trends in the overall area of agricultural research. Turning toward studying the farm as a "system" is a substantial step forward -- from addressing only its technical or economic dimensions towards capturing the tight interplay between the agrotechnical, economic, sociological, managerial, and cultural variables intrinsic to the farm unit. At the same time, the orientation towards FSR expresses a recognition of the enormous diversity that exists among farm and farmers and an effort to translate the understanding of these differences into more precisely tailored agricultural research and advice fitting the needs, constraints, and potential of various categories of farm systems.

The World Bank, in supporting agricultural research through loans and credits to national programs and grants to the Consultative Group on International Agricultural Research, has a major interest in new ideas and new approaches to agricultural research in its borrower countries. Increasing emphasis on the need for new technology to support agricultural development and the need to spread that technology quickly, especially among small farmers, has made it more than ever necessary to improve the linkages between researchers and extension workers and between both these categories of workers and the farming population. Farming systems research, with its emphasis on determining the important problems of farmers and on testing potential solutions at the farm level, can strengthen these linkages and increase the efficiency of both research and extension systems, hence the Bank's interest in trying to synthesize and evaluate the many divergent views on the role and value of the farming systems research approach.

In order to assess the progress to date of farming systems research, its contribution to agricultural research and extension, and its likely perspectives of further development, the World Bank commissioned Dr. Norman W. Simmonds to prepare a state-of-the-art review of FSR. The resulting paper is published in this volume. It is based on an examination of the literature, discussions with many people involved in this research area, and visits to a number of national and international programs where farming systems research is being practiced. As the report makes clear there are many different perceptions within the scientific community of what constitutes "farming systems research." This state of affairs is obviously to be expected in a research field that is comparatively new, though to one or another extent it has been practiced under differing titles for some years prior to acquisition of its present label, shape, and content.

As a relatively new topic, we are convinced that farming systems research will continue to generate debate as to its role, its research methodology and its organization. Obviously there is no single "best way" to do farming systems research and such debate will certainly continue. The World Bank hopes that this paper, which represents the views of the author and not necessarily those of the Bank, will provide a useful contribution to that debate and to the continuing evolution of new ideas on research for small and resource poor farmers.

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The World Bank

PREFACE

The appearance over the last decade or so of widespread interest in, and a profusion of publications on, farming systems research (FSR) has been very evident, both in the International Agricultural Research Centres (IARC) and in national agricultural research and extension systems (NARES). Indeed, commitment to at least some elements of FSR in agricultural R & D programs in the tropics has become so general that one World Bank officer has referred to the phrase as having "incantatory value."

So it has, and many different meanings have been attached to it in what is now a large and heterogeneous (but erratically published) literature. No unified view of the whole field has appeared and it was in the hope of achieving such that the World Bank invited me, early in 1983, to prepare this paper. The Bank's interest in the field is, of course, two-fold: it is a major donor to the IARCs by way of the Consultative Group on International Agricultural Research (CGIAR); and it funds agricultural development projects throughout the tropics, often with at least a nominal FSR component. As current spending under these two heads is about US$19 million and US$3,000 million, quite small proportional FSR components must be of concern.

My terms of reference were given by the Agriculture and Rural Development Department of the World Bank, but it was accepted, so confused is the subject, that the structure of my report would have to be determined more by the logic of what I could discover than by prior instruction. In practice I have found it necessary to depart fairly widely from my terms of reference (as to order, though not as to content) and think of FSR, in its various guises, not as a more or less isolated set of related activities, but rather as a component of innovation in tropical agriculture at large.

Besides valuable briefing by Bank officers in Washington, I also had the benefit of discussions, in Britain, with a number of people having much experience of the matter. My travels in Latin America, East and West Africa, India, and South East Asia took in visits to several national agricultural research systems (NARS) committed to FSR procedures as well as to IARCs. The coverage, however, was far from complete but could hardly have been fuller in the time available (about four months). The treatment in this report is therefore broad and inevitably somewhat superficial. I doubt, however, whether a more extended survey would have revealed any new principles.

ACKNOWLEDGMENTS

The views expressed in this report are mine, and the Bank is not, of course, committed to them. Nevertheless Bank officers, both in Washington and elsewhere, had a substantial part in shaping them and I am most grateful for their guidance, particularly to Dr. J.K. Coulter and his immediate colleagues in Washington.

I add the name of Dr. M.H. Arnold and thank him also for several lively and informative talks. Grateful acknowledgment is also due to Dr. G.A. Watson and the Cambridge University Press for a quotation from Experimental Agriculture in Chapter 4 under "Agro-forestry and the Neglect of Perennials."

Before leaving the U.K. and while traveling, I had the benefit of advice and guidance from many people. I should especially like to acknowledge, with many thanks, the help of the following. In the U.K.: Dr. R.K. Cunningham, Mr. K.R.M. Anthony, Mr. R.W. Smith, Prof. C.R.W. Spedding, Dr. I.D. Carruthers, Dr. D. Gibbon, Dr. C.C. Webster, Dr. A.G. Smith, Dr. R. Matthewman. In Colombia: the CIAT FSR workers, Drs. J. Woolley, J. Davis, A. Bellotti, A. van Schoonhoven, C. Sere, R. Howeler, J.M. Toledo and, of ICA, Sr. Hiriam Tobon. In Costa Rica: the CATIE FSR workers, Drs. M. Avila, G. Budowski, E. Escobar, M.A. Esnaola, and L. Navarro. In Mexico: Dr. D. Winkelmann and his FSR colleagues at CIMMYT and, at INIA, Dr. Antonio Turrent. In Ethiopia: Mr. G. Gryseels and his FSR colleagues at ILCA. In Kenya: Drs. M.P. Collinson and P. Anandajayasekeram (CIMMYT), Dr. R.A. Kirkby (IDRC), Dr. J.B. W. Matata (NARL), Drs. B. Lundgren, and P. Huxley (ICRAF), Dr. P.K. Kusewa and colleagues at Katumani Research Station, Machakos. In India: Dr. S.M. Virmani and FSR colleagues at ICRISAT; Dr. R.P. Singh and colleagues at AICRPDA, Hyderabad; Dr. B.R. Hegde, Dr. K.V. Sampath and Mr. M. Jyoti and colleagues of the Karnataka State Agricultural Department. In the Philippine Islands: Drs. D.M. Wood and V.M. Carangal (IRRI); Dr. E.C. Quisumbing and FSR colleagues (P.I. Ministry of Agriculture); Drs. Ramon Valmayor and Deli Capasin (PCARRD); Dr. E.L. Rosario (UPLB); Mr. F. Quero (Ministry of Agriculture, Tacloban, Leyte); Drs. K. Takase, S.T. Senewiratne and A.K. Auckland (Asian Development Bank); Drs. D. Clark and T. Hobgood (USAID). In Indonesia: Dr. S. Sadikin and Mr. Inn G. Ismail (AARD); Mr. S. Draper and Mr. H.D. Matheson (World Bank). In Nigeria: Drs. C.H.H. ter Kuile, G. Wilson, H.J.W. Mutsaers, P. Ay, Malik Ashraf, and P. Juo (IITA); Dr. D. Sumberg (ILCA); Mr. D. Feldman (FACU); Mr. P. Reid (Ilorin ADP); Dr. C. Harkness (KNADP).

LIST OF ABBREVIATIONS AND ACRONYMS

AARD:	Agency for Agricultural Research and Development
ACSN:	Asian Cropping Systems Network
AFSN:	Asian Farming Systems Network
AICRPDA:	All-India Co-ordinated Research Project for Dryland Agriculture
CATIE:	Centro Agronomico Tropical de Investigacion y Ensenanza
CGIAR, CG System:	Consultative Group on International Agricultural Research
CIAT:	Centro Internacional de Agricultura Tropical
CIMMYT:	Centro Internacional de Mejoramiento de Maiz y Trigo
CIP:	International Potato Center
CSR:	cropping systems research
FACU:	Federal Agricultural Coordinating Unit (Ibadan)
FSAR:	farming systems approach to research
FSP:	farming systems perspective
FSR/E:	farming systems research and extension (Hildebrand in Sheppard, 1981)
FSR *sensu lato*:	farming systems research in the broad sense of more or less anything to do with farming systems, real or invented.
FSR *sensu stricto*:	farming systems research, here used in the narrow sense of study of existing farming systems
IADP:	Ilorin Agricultural Development Project
IARC:	International Agricultural Research Centre (of the CG system qv)
ICAR:	Indian Council for Agricultural Research

ICARDA:	International Centre for Agricultural Research in Dry Areas
ICRAF:	International Council for Research in Agro-Forestry
ICRISAT:	International Centre for Research in the Semi-Arid Tropics
IITA:	International Institute for Tropical Agriculture
ILCA:	International Livestock Centre for Africa
IRRI:	International Rice Research Institute
ISNAR:	International Service for National Agricultural Research
KNADP:	Kano Agricultural Development Project
NAES:	National Agricultural Extension System
NARES:	national agricultural research and extension system
NARS:	national agricultural research system
NFSD:	new farming systems development; phrase invented for the purpose of this report
OFAR:	on-farm adaptive research (=OFR)
OFE:	on-farm experimentation (IITA literature, the experimental phase of OFR)
OFR/FSP:	on-farm research with farming systems perspective (Byerlee 1982), the CIMMYT terminology, adopted here
R & D:	research and development
USAID:	US Agency for International Development

CHAPTER 1. INTRODUCTION

Context

Since the early 1950s, the population of the world has approximately doubled and food supplies have approximately kept in step. These processes have been very unevenly distributed, however, because the rich, temperate countries have grown slowly in population but rapidly in food production, whereas the poor tropical countries have, many of them, more than doubled in population and have maintained, at best, a modest food surplus. At worst, as in much of tropical Africa, population doubling times are of the order of 20 years (compound growth rates of 3 - 4 percent) and there is chronic and worsening food deficit. There is, therefore, a gigantic need for increased food production, both immediately and on into the indefinite future, until populations shall have stabilized within the resources available to sustain them. I reject, as being just silly, arguments to the effect that the world has a distribution rather than a food crisis; that the rich temperate countries need only give up feeding grain to animals and ensure equitable distribution for all to be well. Even temperate agriculture could not sustain an indefinite process of doublings, and imagination boggles at the socio-politico-economic obstacles to distribution. Food aid in various forms against local crises will no doubt be with us for decades but the fundamental need is for the enhancement of indigenous tropical food production. This can be achieved, in effect, only by intensification of production on existing arable land because, on the world scale, the potential for cultivating new lands is small and for irrigation of dry areas probably rather limited.

Food production in this context means essentially vegetable food with no particular emphasis on one kind at the expense of another. Cereals, pulses, oilseeds, tubers, vegetables, and fruits all have their places; so do animals, mostly as scavenging accessories to crop agriculture, but sometimes as a means of getting something out of environments too poor for crop production. The mythology of "the protein gap" (Payne, 1978) is now dead, and the related mythology as to the need for more of specific nutrients is nearly dead too. Nutritionally, reasonably diverse vegetable diets, supplemented with odd animal products, are a rational objective for agricultural research.

The crucial need - for enhanced yields in existing cultivations - was perceived by the Rockefeller Foundation in its Mexican program in the 1940s. The wheat production package followed and was exactly paralleled some years later by the rice package developed by the International Rice Research Institute established in 1960 jointly by the Rockefeller and Ford Foundations. The story of the ensuing Green Revolution has been often told but not always well understood. The essence of it was the large-scale exploitation of positive genotype-environment (GE) interactions (cf. Chapter 2): semi-dwarf, quick-maturing varieties of both crops plus enhanced environmental inputs (water, fertilizers, other chemicals) produced far greater yields than could be produced either by the new varieties alone or by the inputs alone. Indeed, the new varieties under low-input conditions and the old

varieties under high inputs were sometimes actually worse than the old-low combination. Furthermore, day-length insensitivity and quick maturity meant that, sometimes at least, the way was open to multiple cropping, with two or three crops a year instead of only one.

This exploitation of GE effects, by way of semi-dwarf varieties of small-grain cereals responsive to high-input environments, was not, of course, new. It was the same as what had been happening for decades in temperate agriculture, especially in Western Europe and Japan, where cereal plants had long been getting smaller and inputs higher. But the Green Revolution did quickly and in one dramatic step what had taken many years elsewhere.

The Green Revolution was, with qualifications, a triumphant success. Wheat and rice yields went up (Dalrymple, 1974), consumer prices came down and adopting farmers profited. The qualifications were that the technology was only possible where there was adequate water, where farmers were economically able and willing to provide the inputs, and where the economic infrastructure of supplies and marketing allowed. Criticisms (often apparently by politically motivated writers) to the effect that the process favored large farmers as against small ones and tended to cause unemployment have been shown to be without foundation; benefits are virtually scale-neutral (Ruttan, 1977). Other critics have urged that the use of chemicals in agriculture is environmentally hazardous, but often without also observing that starvation, to the victims, is also hazardous.

From the widespread (though not universal) euphoria generated by the Green Revolution there arose the notion that large-scale agricultural research on tropical food crops could initiate a whole series of new Green Revolutions, crop-by-crop, new responsive varieties matched to suitably enhanced inputs. This explains the emergence of the CGIAR system of IARCs in 1971 and its development to a powerful group of 13 constituent bodies with a current expenditure of about US$168 million. It was also behind the marked strengthening of NARSs in the past decade and the emergence of a body within the CG system (ISNAR) devoted to developing national systems.

From the start, the CGIAR and the IARCs were committed to the interests of the small farmer in respect of annual food crops (cereals, pulses, tubers), with some effort also on animals (especially in semi-arid Africa). Perennial crops were not included (a curious omission in view of their social importance) and cash crops (at least export cash crops) were deliberately eschewed. The IARCs have done much good work and, either directly or indirectly (through NARS), have had diverse local impacts on agricultural production. But there have been no more Green Revolutions; in retrospect none was to have been expected. The Green Revolution succeeded where wheat and rice were already grown under irrigation and were susceptible to application of the new technology. Elsewhere, the same crops in rainfed lands have hardly been touched and it is doubtful whether upland rice, on average, yields any more now than it did 20 years ago. Furthermore, irrigated wheat and rice farmers, though small, were evidently prosperous enough to take up the new technology quickly, even enthusiastically, and lived in places where it could readily be made

available. Most tropical small farmers, however, have no such advantages; for them, money is scarce, advice hard to come by, markets uncertain, supplies and communications poor. Can one really imagine a Green Revolution in cassava production, whatever the varieties and technology that might be available? I think not.

In short, I argue (following Wortman and Cummings, 1978) that the Green Revolution proper achieved the big, quick, spectacular success that was necessary to get tropical food crop research on the road (which it did) but that we are now faced with a much slower and more laborious process: of pushing up yields, step by step, a little at a time; of crops grown in socio-economic circumstances that simply are not susceptible to revolutionary change, or are susceptible to it only under really massive governmental intervention. I shall argue later in this report that the last will sometimes be necessary. Here I note that none of the very many deeply informed people that I have talked to in the past few months seemed to believe in revolutions. All thought and spoke in terms of evolutionary change, of research matched to the socio-economic circumstances of farmers and thoughtfully applied and diffused, but in time scales of decades rather than of years.

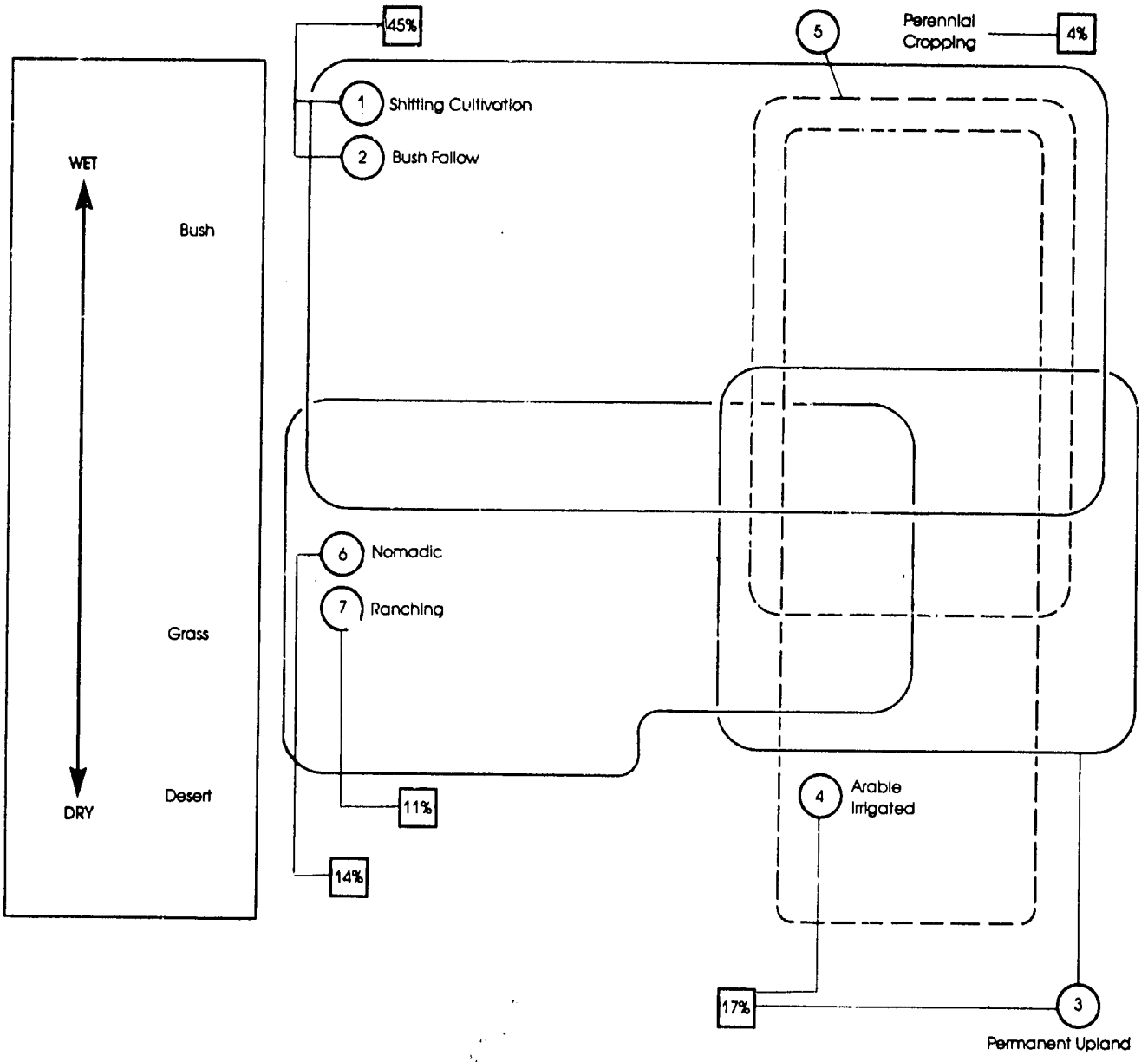
We now reach (somewhat circuitously, I admit) the context of Farming Systems Research (FSR). FSR developed, I think, from the (intuitive as much as explicit) realizations that: the Green Revolution was a "one-off job" that hit the socio-economic centers of interest of two important groups of farmers, the irrigated wheat and rice growers; that no more revolutions were in prospect; and that other farmers would adopt new technology only when they themselves (like the wheat and rice growers) perceived it to be in their own socio-economic interests and capacities to do so. The last phrase, of course, raises the question of how to identify those same socio-economic interests and capacities. This is what a substantial part of FSR is about.

Tropical Agricultural Systems

The standard work on the subject is that of Ruthenberg (1980) and I have used it much. Reference may also be made to Duckham and Masefield (1970) and Grigg (1974) (though not limited to the tropics); also to Webster and Wilson (1980), the standard general text on tropical agriculture. The third (1980) edition of Ruthenberg contains appendices by Collinson and by Zandstra summarizing FSR ideas. Johnston (1958) gives a useful systematic treatment of West African systems.

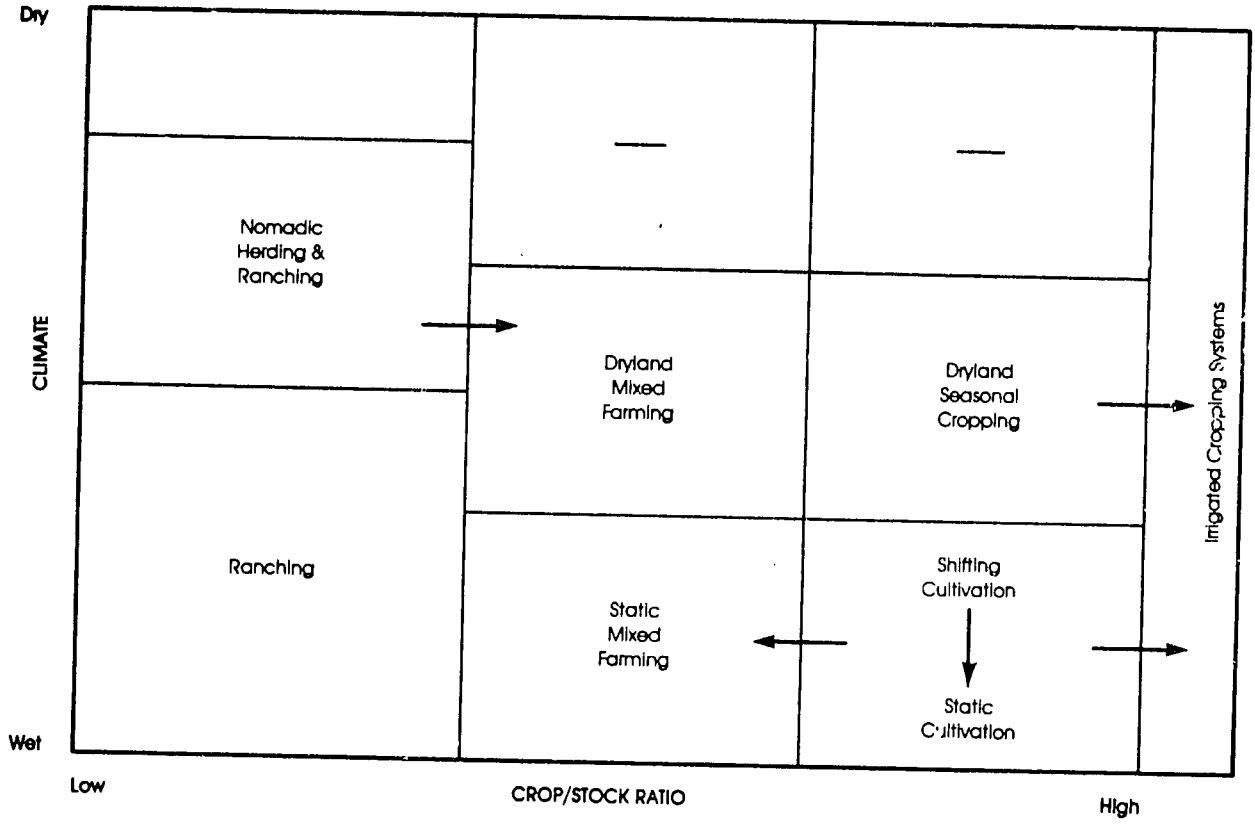
No simple but satisfying classification of tropical agricultural systems is possible. Any general classification would have to be multi-dimensional, taking in rainfall, altitude, crop/stock relations, annual/perennial crop components, and irrigation at the very least. Two dimensional classifications (Figures 1.1 and 1.2) are always deficient. The figures therefore merely serve to set the present study in context. They are based on Ruthenberg and they identify only macrosystems. On the ground, there is virtually infinite variation in detail and, at a practical level, any useful definition must be geographically restricted.

Figure 1.1: Tropical Agricultural Systems (1)



Examples (mostly referred to in this report): (1) (2) IITA program in wet West Africa, (3) ICRISAT work in India, CIMMYT programs in SAT Africa, ILCA's Highland Program, CIAT's bean program in the Andes; (4) IRRI's ACSN (now AFSN) in southeast Asia; (5) The Philippine coconut program; (6) ILCA (1980, 1983); (7) CIAT's pasture program in the llanos, EMBRAPA in the cerrados of Brazil.

Figure 1.2: Tropical Agricultural Systems (2)



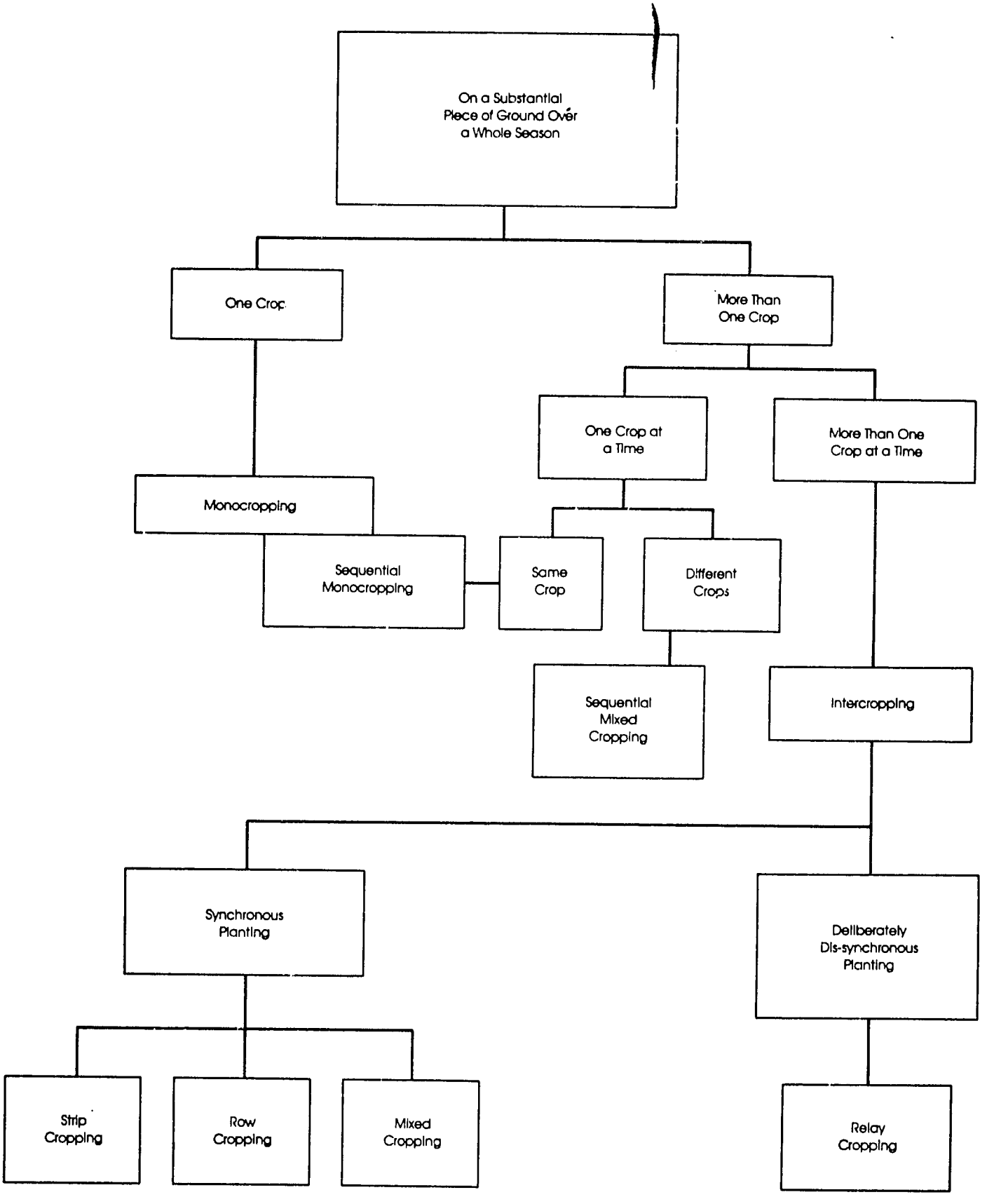
At the limit, no two farms are identical, so all FSR activities work at a micro-level of classification, a level at which farms may be judged sufficiently alike (for the purpose in hand) to be deemed members of a local farming system.

Formally, the problem is common to nearly all biological classification, whether of plants, animals, soils, or farming systems. The question is: how to partition variation so that it is maximized between taxa and minimized within taxa? Even when useful discontinuities occur (and they are few in farming systems) the problem remains of coping with the residual continuous variation. The practical answer that always emerges is that a taxon (an entity of classification) is that which is defined by the competent taxonomist with a particular purpose in mind. Thus the plant taxonomist might be content with Solanum tuberosum for the cultivated potatoes but the potato botanist would need to recognize cultivar groups (based on cytogenetic criteria), clones, maybe even sub-clones. Similarly, the coarse classifications of Figures 1.1 and 1.2 may serve for some purposes, but are quite inadequate for any kind of FSR. Thus Permanent Upland Systems are very different things in India, Ethiopia, Kenya, the sub-Sahel of West Africa, and in the Andes. And, within these regions, numerous local systems can (and indeed must) be recognized.

I shall dispose here of another point which is sometimes a source of confusion. Mixed cropping is very common in tropical agriculture and one encounters the notion that the study of it is, in some sense, FSR. It is not. Mixed cropping is rare and unfamiliar in temperate agriculture but frequent and important in tropical agriculture because it contributes both to gross yield and to stability of overall performance. That mixed cropping has favorable features has long been recognized (in India back into the 19th century at least) and there is certainly room for more experimentation on and better understanding of the subject. I know of no comprehensive review but a partial one has been presented by Willey (1979) (see also ICRISAT, 1979). One would like to see a full treatment. Meanwhile, I regard it as simply a phase of agronomic-physiological research that must have something to contribute to tropical agricultural development. The nomenclature is sometimes confused. Figure 1.3 is based on Ruthenberg. There will be no more about mixed cropping per se in this report though researchers, of course, frequently encounter it in the field and the subject is present in the programs of several IARCs (notably, CIAT, CIMMYT, IITA, ICRISAT, IRRI) and NAR systems (as in India, the Philippines, and Indonesia, to name but a few) (Chapter 5).

Figure 1.3 relates to mixed cropping with annuals, the aspect that has had most study. Perennial crops are no less often grown mixed, with each other and with annuals. Watson (1980, 1983) gives valuable systematic treatments of such mixtures, of which there must be hundreds of variants. Again, perennial crop mixtures are not per se farming systems, though they may be very important components of them.

Figure 1.3: Annual Plant Cropping and Intercropping Systems



Innovation and Change

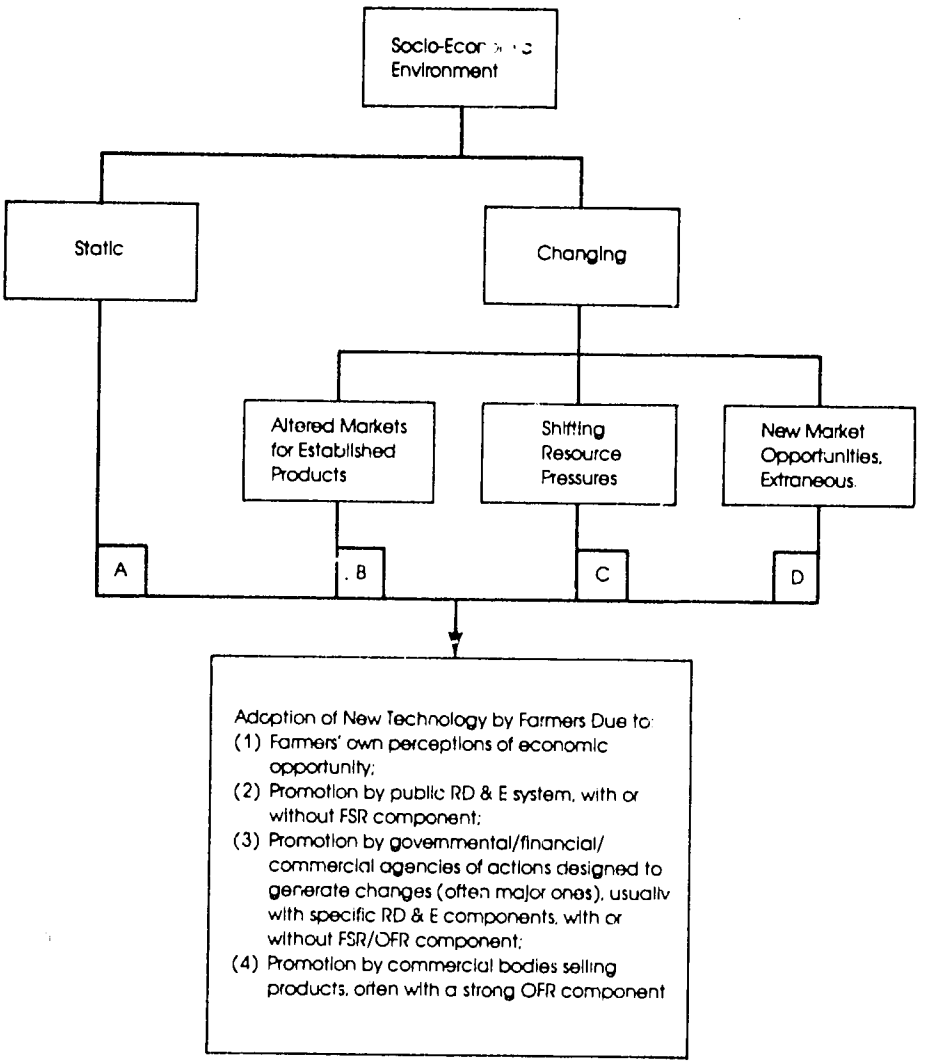
It is often said that farming systems tend to be stable unless perturbed. So they may be, but perturbations are rarely wanting for long. Thus, while bush fallows and shifting cultivations have no doubt existed in the wetter tropics for millenia, they must have changed greatly in structure from time to time, as new crops and stock became available and new opportunities appeared. Bananas entered Africa maybe 1,000-1,500 years ago and locally transformed the agriculture; the sweet potato did the same in Oceania, and the coconut was not without effect in its immense travels. Maize, cassava, and the aroid tubers have been in Africa only for about 500 years and bananas and rice in America for a like time. Cattle, sheep, goats, pigs, and horses are all Old World beasts introduced to the Americas about 500 years ago. All these (and many more) introductions wrought revolutions, invented and carried through by farmers, perhaps rather by rural societies as wholes.

So stability, if it ever existed, was always punctuated by change. On a short, backward view, however, a quasi-stability is often apparent: multitudes of small tropical farmers are probably now living and working much as their grandfathers did. Any apparent stability is now, however, plainly impermanent. Virtually all tropical agriculture has been or is being destabilized by population pressures that demand more food from limited (always finite, all too often diminishing) resources. Change is therefore, willy-nilly, rapid, even on personal time scales. It is the business of agricultural research to try to promote change in a socially favorable sense and that phrase lies at the heart of farming systems research, broadly understood. Futurology is no part of my remit, but I observe in passing that agricultural research can defer acute conflict between population and food supplies; it cannot, by any means technically conceivable, assure food for indefinitely increasing populations. In a serious sense, therefore, population control is a more fundamental matter than food production. The point has been made often enough but bears repetition.

There are several causes and agents of technological change in agriculture. I recognize four of each in Figure 1.4 which could, perhaps, for some purposes, be subdivided. Comments follow.

Case A, the more or less static socio-economic environment, must nowadays be pretty much an abstraction if only because there can be few places in which population and/or economic forces favoring "efficiency" do not press hard upon agriculture. Case D has, in the past, been of great importance; one thinks of the history of the great plantation crops and of small-farmer export crops such as cotton and tobacco, also of new local demands for such products as wheaten bread and beer; alas, such opportunities seem to be becoming scarcer as the rich countries promote sugarbeets and oilseeds or substitute synthetic materials for natural fibers and drugs. In our context, that of farming systems, cases B and C are surely the most important, the former reflecting the rising needs for food of burgeoning populations, the latter the ever increasing pressure on land or other limiting resources by those same populations. The preceding is a short list of the external causes of change (or [A], non-change). The lower part of Figure 1.4 recognizes four agents of change, remarks upon which follow:

Figure 1.4: Causes and Agents of Technological Change



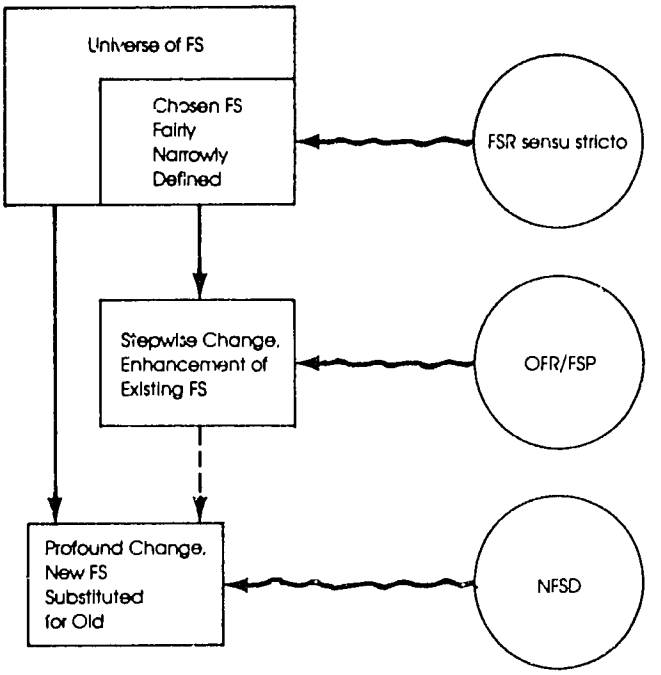
Note: The nomenclature of this diagram & the accompanying text anticipates that set out in the next section.

1. Examples are rarely documented but probably innumerable. Most farming systems have evolved thus and continue to do so. It is a substantial agent of change in temperate agriculture where enterprising farmers often do something first and leave the research, development, and extension system to tidy it up. But it is far from unknown among tropical small farmers, for example: the recent evolution in the Antioquia area of Colombia of potato-maize-bean cultures from more or less random mixture, through row cropping to an orderly, manured rotational-relay-cropping (with numerous local variants); in Costa Rica, evolution from coffee with full Erythrina shade, to coffee with pollarded shade, to to pollarded shade with a third-storey tree, Cordia; there are numerous examples of farmers exploiting a good new variety quickly, even to the extent of stealing it before release; and Dr. G. Gibbon has told me of Sudanese farmers who adopted camels for ploughing because the advent of lorries for load carrying had reduced the price of the beast.

2. The Green Revolution is a plain example (indeed the best one extant) of a research-push innovation, lacking any substantial and formal farming systems component, that went well because farmers rapidly appreciated the benefits. The farming systems perspective (FSP, see below) was, of course, there (though maybe intuitively) in the imaginations of the parent foundations and researchers. All current efforts by IARC/NARS groups, separately and jointly, are of this nature, with a strong and generally increasing farming systems component. This track is characteristic of all publicly funded agricultural research, development, and extension efforts and it is, of course, mostly aimed at meeting the pressures generated by causes B and C above.

3. The promotion of new crops (category D) is possible only when markets and adequate supply are assured. Historically, all the great plantation crops come here, typically estate cultures, but often with a strong small landholder sector (as in sugar, rubber, bananas, coconuts, tea, cacao, and coffee). Groundnuts in Senegal, cotton (very widely in Africa and Asia), and tobacco in all the continents are examples of small landholder developments that worked pretty well because the official/commercial agencies responsible were able to impose the necessary production disciplines. The current promotion of wheat in semi-arid West Africa is a contemporary example. All such developments require a strong technical/rural development/co-operative base provided by governments, banks, or industries or combinations of them. Broader rural development schemes (e.g., irrigation works, trans-migration or settlement schemes) make similar organizational demands but are usually aimed at enhanced production of existing crops (B and C) rather than promotion of new ones. The farming systems component was traditionally small and the technology-transfer bit of new crops programs was pretty much a "top-down" process (Chapter 2, "'Upstream' and 'Downstream'") as far as the small landholder was concerned. Maybe this is changing and, nowadays, such schemes, if they have a research component at all, recognize the need for a farming systems element (see Chapters 4 and 5).

Figure 1.5: The Three Main Categories of FSR Work (sensu lato)



4. Fertilizers, herbicides, fungicides, drugs, insecticides (indeed, agrichemicals generally), and machinery come to mind. Plant breeding companies are already strong (and highly beneficial) in temperate countries and must be expected to develop, too, in the tropics (especially, perhaps, for hybrid maize, in the knowledge that it has done very well in Kenya). Such businesses have a long history of on-farm work, at least in temperate countries, perhaps less so in the tropics. The objectives of on-farm work by commercial companies are not only to test innovations (chemicals, varieties, machinery) in farm practice but also, of course, to advertise the product.

5. I shall now try to place the foregoing discussion in the context of the present report. Of the four main agents of technological change identified above, it has been the growing interest of publicly supported research organizations (agent 2 in the list of Figure 1.4) that has been mainly responsible for the promotion of explicit farming systems ideas in the research process. The ideas have developed rapidly and not very coherently. Hence this report is mostly concerned with the work of IARC/NARES bodies, and my travels were virtually wholly concentrated upon a sample of them. The discussion above, however, will have made it clear that farming systems ideas are relevant to all four agents of change, in tropical and temperate agriculture. I shall return briefly to the theme later but the bulk of what follows is devoted to the CGIAR/NARES context.

Anticipation of Argument to Follow

On the excellent advice to a writer to say what he is going to say, say it, and then say he has said it, I interject here a short statement of what I understand by FSR in the broad sense. I have come to recognize three (fairly) distinct elements and use the nomenclature summarized in Figure 1.5. Justification of the terminology and the classification will appear later. Referring to the figure, I define the three elements as follows:

1. FSR sensu stricto is research on farming systems as they exist, their description, analysis, classification and understanding. It can go to any depth but typically goes deeply into the agriculture, economics and social context of the system studied. It is essentially an academic activity good for generating Ph.Ds, but not much use to agricultural research.

2. On-farm research with farming systems perspective (OFR/ FSP) starts from the FSP bit which is just enough FSR sensu stricto for the job in hand (but no more). It uses the FSP to help to define the on-farm research (OFR) necessary for practical progress. It is a "style" of doing agricultural research founded on the well-justified assumptions that changes need to be adapted to the circumstances of their users and that on-station experiments by no means always predict farm experience. The OFR/FSP "style" broadly assumes that progress will be step-wise rather than revolutionary and devotes itself to a cautious, empirical, evolutionary process. The terminology is that of Byerlee et al. (1982).

3. New farming systems development (NFSD) contrasts with the preceding in seeking to generate revolution rather than evolution, to build radically new systems *ab initio*. It is essentially practically orientated agricultural research and must (obviously) be founded on at least some FSP (but maybe not much). It differs from OFR/FSP in degree rather than in nature (both seek to generate beneficial change) but it necessarily has a lesser OFR component (indeed it may have almost none, as we shall see later). The terminology is mine, invented because, so far as I can ascertain, no one has invented a phrase that clearly distinguishes this sort of activity from other aspects of FSR *sensu lato*. From here on I shall use this terminology and, if I need to refer to FSR in the rag-bag sense unhappily prevalent in the literature, shall use the term FSR *sensu lato*.

Finally, to close this chapter and to anticipate a little further what is to follow, I shall argue that; (1) the practice of OFR/FSP is just becoming (and quite reasonably so) a standard component of the large body of agricultural research that seeks to generate step-wise changes in farming practice; it has, I think, proved its value and has come to stay; (2) NFSD, though more ambitious and much more uncertain of outcome than OFR/FSP, is no less necessary; in the real world not all changes should or can be step-wise; many farmers' circumstances cry out for radical alterations, however difficult they may be to achieve in practice; and there must surely sometimes be room in agricultural research for something wider and more imaginative than the step-wise process, even if the last, is, in real life, the norm.

CHAPTER 2. FARMING SYSTEMS RESEARCH IN GENERAL (FSR *sensu lato*)

History

Knowledge of, and interest in, tropical farming systems is not new. Many government agricultural officers in the colonial days had profound knowledge of the systems with which they lived, often for decades. That knowledge usually died with them or was lost in the files. Publications such as those of Tothill (1948), Jameson (1970), and Arnold (1976) summarize a little of the great knowledge that was available, and I have no doubt that similar works written in French and Dutch could also be identified. Bunting and Watts-Padwick (1983), in a recent perceptive article on tropical agricultural research, remind us that a quite explicit statement as to the importance of understanding what farmers do (i.e., FSP) was made as long ago as 1889 by Voelcker, writing of Indian agriculture.

The important ideas that have emerged to change the scene in the past 20 years or so have been, I think: (1) that the methods of farm management economics, well established in Europe and North America for decades, could be adapted to the circumstances of tropical small farmers; (2) that tropical small farmers were economically rational (though not necessarily profit-maximizing); (3) that risk, uncertainty, and therefore caution were dominant features of their existence; (4) that statistical and operational research methods, with or without computers, had provided means of thinking about uncertainty and the working of complex systems; and (5) that innovations proposed by agricultural researchers for extension were, rather often, simply not adopted or were adopted only in a partial or modified form.

The notion that small farmers in general present problems unfamiliar to conventional economics goes back to Chayanov in 1925 (Thorner *et al.*, 1966; Levi and Havinden, 1982), though Chayanov's view, that the peasant economy really demanded a different "system" of economics, would not now be generally accepted. The adaptation of farm-managementeconomic ideas to the circumstances of tropical small farmers was pioneered in India in the 1950s (Mellor, 1966) and developed in Africa by D.W. Norman and M.P. Collinson during the past decade (Norman, 1974-83, Norman *et al.*, 1981, 1982; Collinson, 1982, 1983); this latter initiative developed into the OFR/FSP now widely practiced. Other useful general references are: Andrew and Hildebrand (1982), Avila *et al.* (1982), Byerlee and Collinson (1980), Byerlee *et al.* (1982), CIMMYT (1981a, b) Dillon *et al.* (1978), Eicher and Baker (1982), Galt *et al.*, (1982), Gilbert *et al.* (1980), Harwood (1979), Ngambeki and Wilson (1983), Perrin *et al.* (1976), Shaner *et al.* (1981, 1982), Sheppard (1982), and Zandstra et al. (1981).

Formal "systems" ideas have proved, I think, far less pervasive. Full understanding of a system implies complete numerical specification, and this turns out, in the context of tropical agricultural research, to be rarely necessary and sometimes just a waste of time (as I shall argue further below). At all events, the important thing has turned out to be the systems perspective (i.e., FSP) rather than the formal models. To

say this is not to deny that models have their uses, even in an OFR/FSP context (see examples in Chapter 3) and they have certainly found a niche in rich temperate agricultures, as the pages of the journal Agricultural Systems and such useful animals as the "Edinburgh Model Pig" testify.

FSR sensu lato entered CGIAR thinking at an early stage and it figures in the mandates of most of the centers. This stemmed, I suppose, from the realizations that more Green Revolutions were not in prospect, that point 5 above was true, that agricultural economists had their uses, and that tropical food crop research had somehow to be adapted to farmers' circumstances. These were all, I think, accurate perceptions but, in the outcome, FSR was interpreted by the centers in very diverse ways, as the important TAC Stripe Review (Dillon et al., 1978) made clear. The diversity of views is hardly any less now, and they range from a little more or less pure FSR sensu stricto, through much OFR/FSP to some NFSD (or, at least, component research pointed towards NFSD).

Small-Farmer Characteristics

The socio-economic characteristics of small farmers have been stated many times (e.g., Collinson, 1984) and may be summarized as follows:

1. they are poor and have little ready cash;
2. loans to them are usually unavailable or expensive;
3. they are conscious of an uncertain environment, of cash shortage, and of family responsibilities and therefore;
4. they are risk-averse;
5. they often suffer cyclical labor shortage and under-employment;
6. they may have opportunities for competing off-farm employment;
7. they are economically rational but not necessarily profit-maximizing because;
8. they (like the rest of us) have their own scales of utility;
9. they live in countries in which the social infrastructure of markets, supplies, and communications is often weak and not to be relied upon;
10. they live in societies which normally have fairly clear codes as to what is socially acceptable and what is not.

This is a formidable list. It differs in nearly every point from any comparable list that could be written for rich farmers in temperate countries (or, indeed, locally in tropical ones). Given such constraints, and they do indeed seem to be universal, it is not surprising that small farmers generally approach innovation cautiously because it may be both costly and risky, however profitable.

Ideas on farmers' attitudes to innovation are summarized in Figure 2.1 and comments follows. The scheme of Figure 2.1 is quite general and must apply whatever the source of innovation and whatever the scale of operation of the farmer. Sources of innovation may be: endogenous to the farming community or exogenous (public R & D, official promotion, commercial) (see Figure 1.4). We concentrate here on the small farmer in the FSR context.

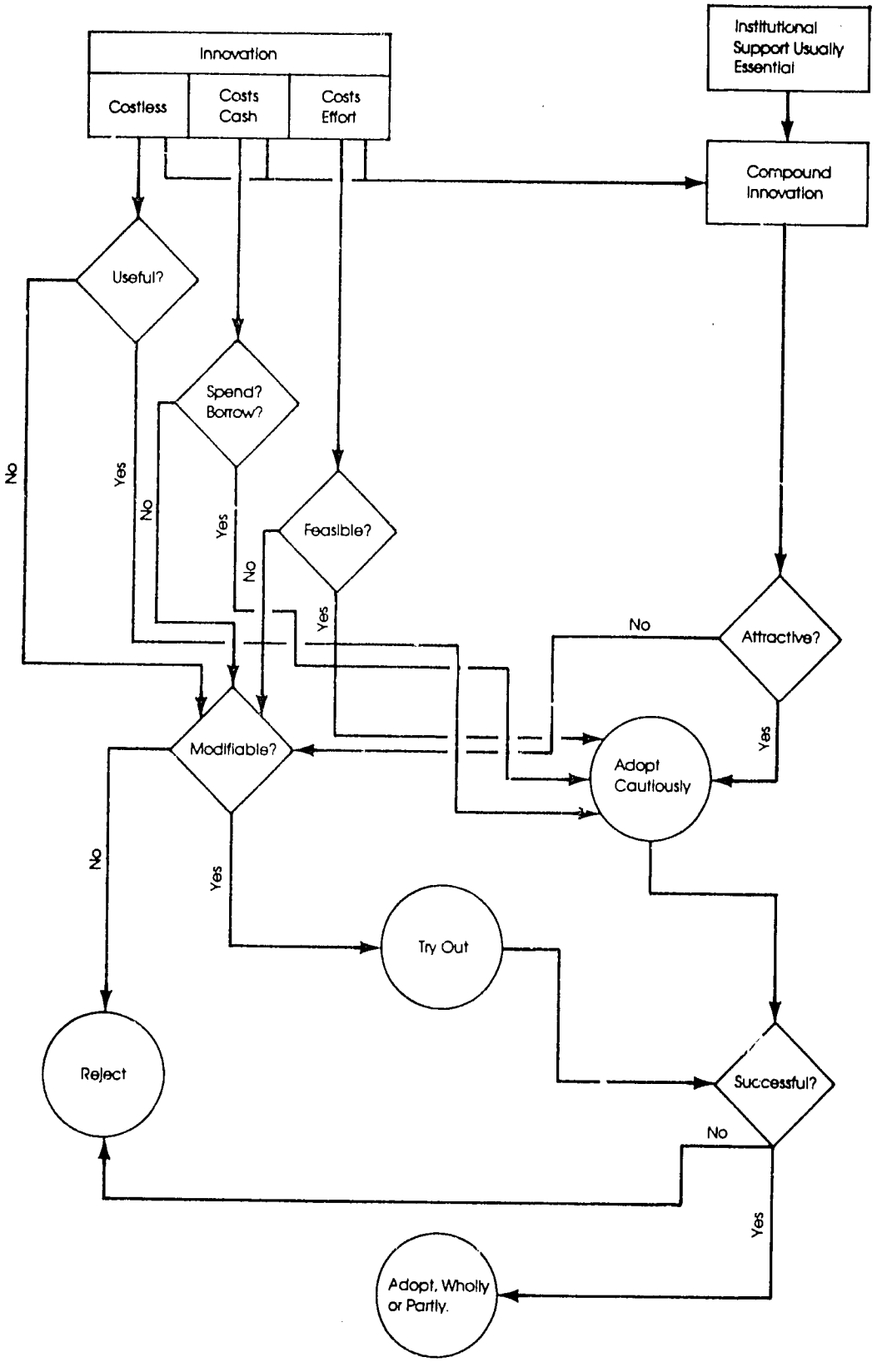
Costless innovations

The leading example is of new crop varieties, which will generally be accepted and quickly adopted if similar to the established one(s), but plainly better in some significant respect such as yield or maturity (usually earliness); more subtly, the more stably performing (reliable) variety will be preferred, though it may take several years of experience for a body of farmers to decide. New varieties will be rejected, as many (probably most) have been, if they do not meet farmers' needs in respect of yield on the farm (not the experiment station). They will also be rejected if they do not meet farmer requirements in some secondary field characteristics such as yield of straw for stockfeed, recovery from grazing in barley, or strength of stalk in maize destined to support climbing beans. Again, they will be rejected, or at least discounted, if they fail to meet local quality requirements such as seed size in pigeon peas and chickpeas, grain color in sorghum (white or red locall preferred), grain color and texture in maize (usually strong local preferences), seed color in beans (only a blotchy pink will do in the Antioquia area of Colombia). But there are trade-offs, and an exceptionally good variety may be taken up even if quality is less than ideal; thus I heard of a purple-seeded bean that was going well in Colombia because it was outstandingly drought-resistant, and one recalls that IR8 rice succeeded despite a "chalky" grain character. Market preferences are not immutable if the price is right; and tastes can change.

Cash inputs needed

The obvious examples concern chemical inputs such as fertilizers, herbicides, fungicides, and insecticides, all relatively small recurrent expenditures; and capital expenditures on such items as new stock, fencing, field equipment, spraying equipment, grain storage facilities. The former have shorter pay-back times (say, a crop season) but all are subject to the exigencies of the farmer's cash supply and discount rate. It will be noted that fertilizers are divisible, so that low-level partial adoption is feasible, whereas the other items listed are not; furthermore, capital expenditure normally incurs some prospect of future demands for maintenance costs.

Figure 2.1: Farmers' Decisions on Innovation



Innovations involving cash expenditure are notoriously likely to be rejected. First, the inputs may simply not be available, either generally or at the local village level due to failure of distribution mechanisms. Second, even if demonstrably attractive technically, the farmer may be unable to find the cash or unwilling to take the risk of borrowing. If the farmer does adopt, say, the use of fertilizer, then he is likely to do so at a lower level than the recommendation and later maybe adjust upwards to approach his preferred marginal rate of return (Figure 3.4).

Extra effort needed

The growing of two short-cycle crops instead of one longer-cycle crop might be virtually costless in cash terms, but make considerable labor demands of the farmer, of his family, and of his work animals. Examples are the undercropping of coconuts in the Philippines and the addition of ratoon rice and a quick pulse crop after maincrop rice in Indonesia (see Chapter 5). Numerous conflicts between such demands and those of other farm enterprises or off-farm earnings occur. Economists examining such problems by partial budgeting techniques will, of course, translate them into cash flows (see beginning of Chapter 3). And one should recall that an already hard-working farmer (who is not necessarily a profit-maximizer) also puts a value on hard-earned leisure.

Compound innovation

All the more radical innovations come here. Thus the ICRISAT proposals for the black cotton soils of India, and the Asian Cropping Systems Network proposals for irrigated rice, demand: new varieties (generally of quick maturity), cash inputs for chemicals and tools and more labor, all with complex biological and social interactions. ILCA proposals for upland Ethiopia involve a fairly modest change in cropping pattern but a profound change in kind of cattle, affecting working methods and equipment and milk production. There is no sharp discontinuity between such proposals (all already in some degree of adoption) and the still more radical proposals for virtually new systems (elsewhere in this report referred to as NFSD) that are now being explored; for example, the studies by IITA in Nigeria that seek to develop stable farming systems on poor, acid erodible soils in place of shifting cultivation (see Chapter 5 for all examples). All such compound innovations, from the simpler to the most adventurous, will need strong institutional (ultimately governmental) support to provide the essential communications, material supplies, marketing, research and extension services, credit, seeds, stock, and so forth.

To conclude this section, the heart of the matter is surely this: that, if innovations are to be effectively promoted, they must either fit the farmers' economic circumstances or those circumstances must be changed to make the innovation work. OFR/FSP seeks the former, in shape of relatively simple, even unit, innovations which fit the circumstances; complex changes, including NFSD, must generally seek to change the economic circumstances to fit the change.

A final point is worth making. In the new-found belief that small farmers are economically rational, we ought, perhaps, to be wary of the assumption that they are always right, have always optimized within their own constraints. Even rich, well-educated farmers can be collectively wrong under persuasive advocacy from articulate neighbors or from fertilizer salesmen or simply from conservative retention of a judgment that was correct in the past. I can see no reason to suppose that small farmers are immune to error from similar causes. Sometimes, perhaps, the outstanding farmer (even the scientist) may point the way against the general trend of opinion?

Systems

The ideas of systems grew out of wartime operational research applied later to industrial problems. Numerical application became widely possible when computers became available. Thus complex, interacting flows over time and space, far too complex for algebraic or analytical treatment and far too bulky for pencil-and-paper numerical methods, are now generally accessible to fairly exact specification (provided the data are good enough). Thus, no self-respecting treasury is now without macro-models of the economy, weather forecasts are at least somewhat better than they used to be, road traffic, tide and river models are much used by civil engineers, and any substantial manufacturing company runs optimizing models of plant throughput, cash flow, and distribution system. Even world systems have been attempted.

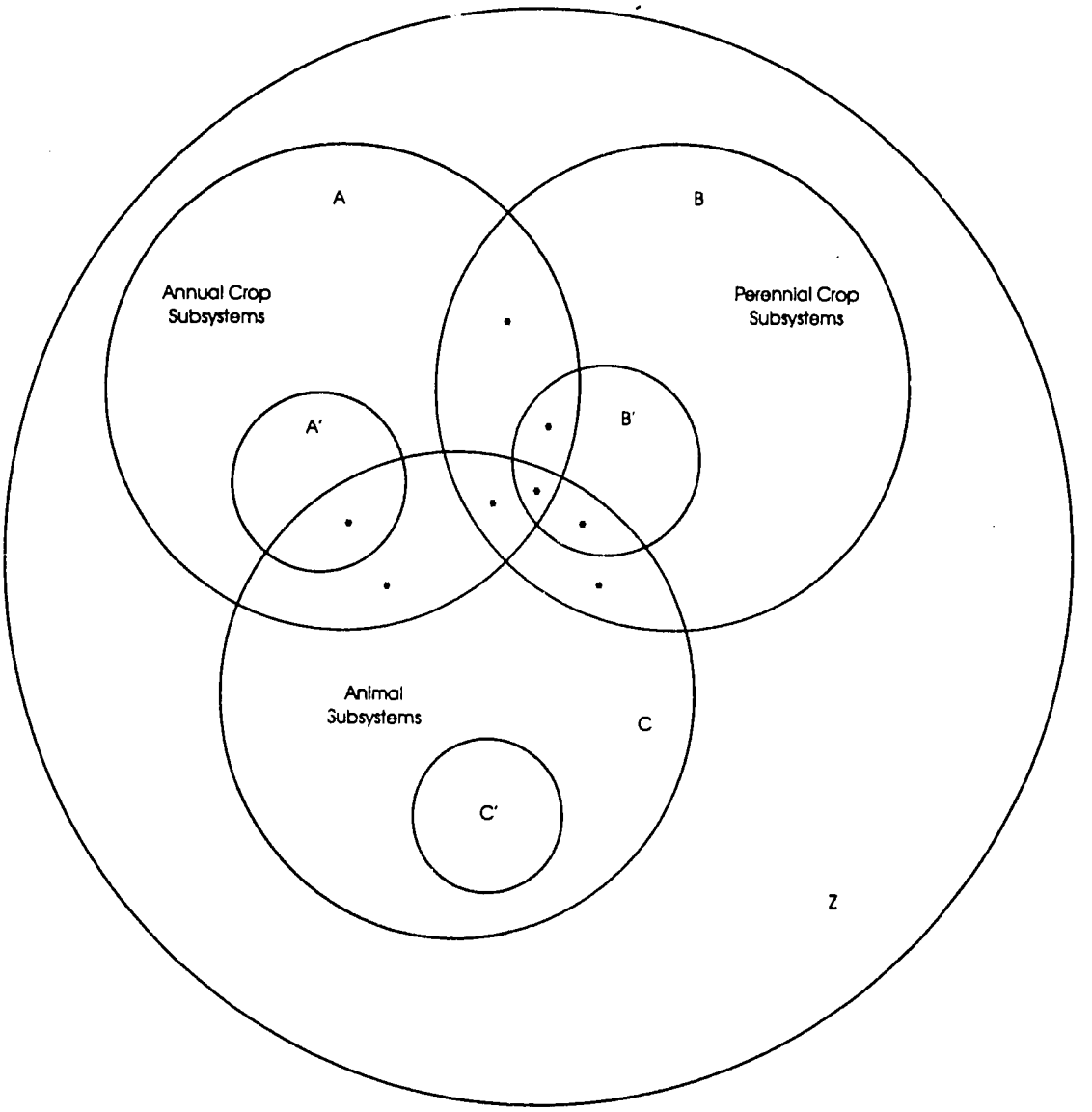
Systems ideas have also entered technology-based agriculture in the rich countries where enterprise, whole-farm, and production-unit models, usually with a maximizing/optimizing object in view, are ever more widely used; when the physical, biological and economic data are good (as they commonly are), such models are generally agreed to be potent aids to efficiency. In agricultural research, too, systems models have their place, for example, of crop and animal growth, of epidemics, and of production systems.

There is a vast literature of systems in general and a substantial one on agricultural and biological applications on which the following are standard general texts: Dalton (1975); Dent and Anderson (1971); Spedding (1979).

Any systems understanding starts from qualitative enumeration of components and their interactions, goes on to quantitative description of states and flows, and, only when the latter are tolerably well defined, attempts modeling or synthesis. In our context, that of FSR, understanding rarely gets far beyond the initial, qualitative stage and is usually confined to a bit of the whole, or a subsystem (Figure 2.2).

In Chapter 1 I made the point that the macro-systems such as permanent upland systems, irrigation systems, and so on are far too wide and heterogeneous to be much use for the FSR worker; at the other extreme the single farm is far too narrow. The choice of what to call a system for practical purposes is always arbitrarily determined by the inquirer's interests. I return to the point later in discussing what constitutes a

Figure 2.2: An Agricultural System



"recommendation domain" (Chapter 3) and note here that, for Figure 2.2, we imagine that a set of farms sufficiently homogeneous for the purpose has been identified.

The grandly "holistic" approach of FSR sensu stricto would be satisfied by no less than a description of the entire system, Z. More realistically, and certainly for OFR/FSP purposes, a subsystem is selected (e.g., A or A', depending on the interests of the investigator) in awareness of the interactions with other subsystems implied by the intersections marked with dots in the diagram. Sub-subsystem C' is a crop-non-interactive one (should such exist). Though diagrams such as this are helpful in illustrating ideas, they would have to be multi-dimensional to be accurate and can give no quantitative information about components and interactions. At a low, far from truly holistic level, flow charts can convey a good deal of quantitative information (examples in Hart, 1975; Gibbon, 1980; Dillon and Hardaker, 1980) but even they quickly become too complex to be easily read.

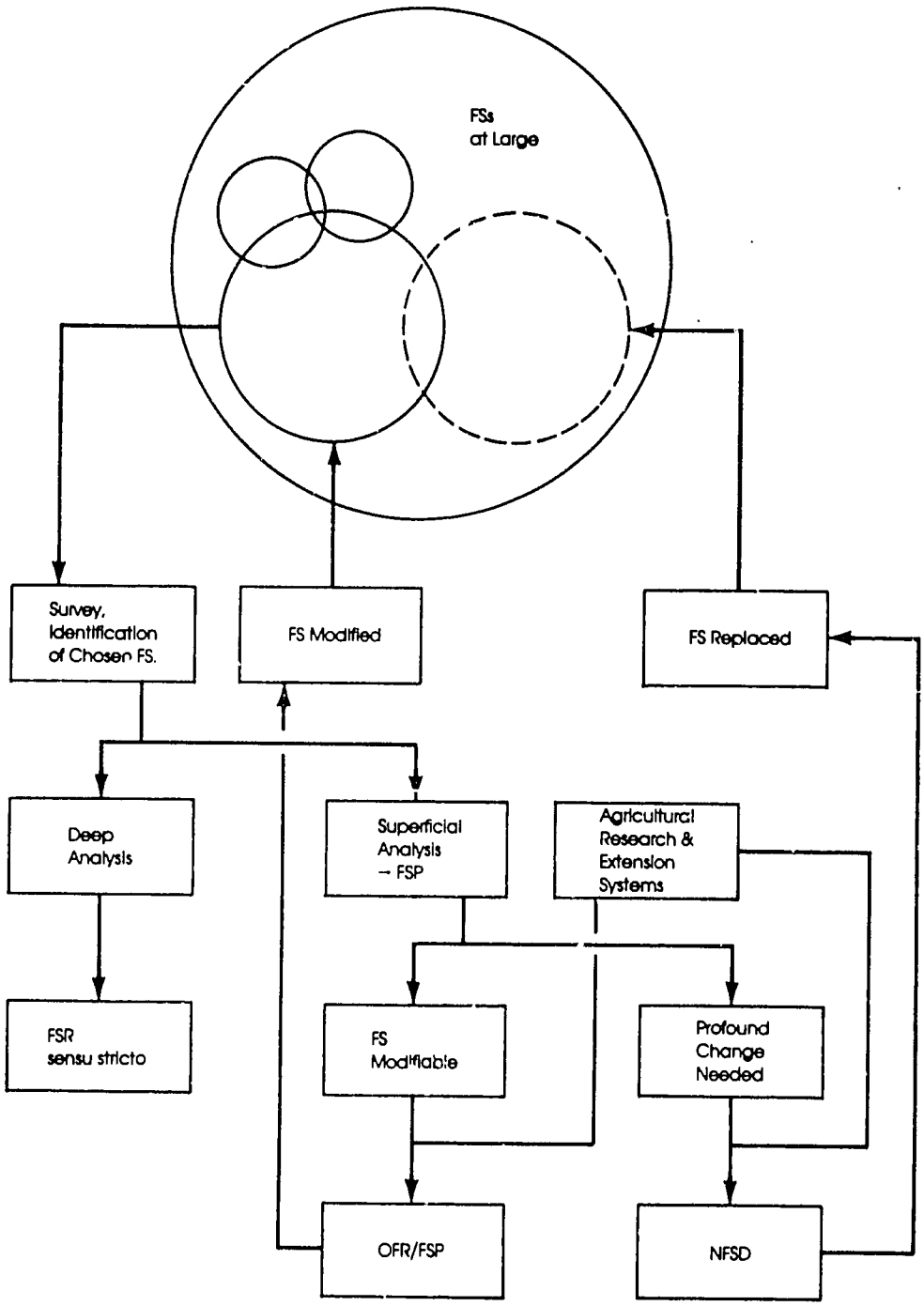
It would not be difficult, but might not be very profitable, to invent diagrams of the type of Figure 2.2 for the examples discussed in Chapter 5. Thus for the CIAT bean program subsystems B and C would be, for all practical purposes, absent and A would be represented by a set of strongly interactive annual crops; ICRISAT might be thought of as concentrating on A', cropping of the black cotton soils, interactive with the draft animal component; ILCA might be regarded as concentrating on C, the cattle, interactive with A, but in the absence of B; the Philippines coconut program would contain all three subsystems but with concentration on the intersection of A and B, undercropping with annuals.

In real life, therefore, systems isolated for study are always subsystems arbitrarily defined for the purpose in view. They are never holistic in any serious sense of that rather over-used word. In practice, what is wanted is sufficient understanding to attain the necessary level of FSP and no more. I wish the words holism and holistic were avoided in FSR contexts except when a really deep analysis of a whole-farm system is being attempted, that is, FSR sensu stricto (approached, e.g., by Norman et al., 1982) for the Nigerian Savanna area). For OFR/FSP, a partial, non-holistic, subsystem knowledge will suffice or, anyway, has to suffice in practice.

FSR sensu stricto and OFR/FSP

The essential distinctions between FSR sensu stricto, OFR/FSP, and NFSD have already been drawn in Chapter 1 and in the earlier parts of this chapter. I elaborate them in this section, but defer consideration of operational details of OFR/FSP until Chapter 3. Figure 2.3 extends the very brief statement of Figure 1.5. It makes clear that, a specific FS having been chosen for attention, deep analysis of it leads to FSR sensu stricto, but superficial analysis (FSP) suffices for OFR/FSP. Some readers might object to the implication that superficial analysis would suffice to decide that profound change and hence NFSD were necessary, but I think the implication is correct. It requires, for example, no deep socio-economic analysis of numerous shifting systems in the humid African

Figure 2.3: FSR sensu stricto, OFR/FSP and NFSD



tropics to conclude that they simply cannot sustain ever increasing populations, effective though they may be to sustain small, stable populations. No amount of FSR can negate the brute technical facts of old, erodible, infertile soils under heavy rainfall. The diagram does, however, make it clear that understanding at the FSP level will (or at least should) contribute to any NFSD that might be invented.

FSR sensu stricto and OFR/FSP, though quite different in intention and outcome, have, at least, initial stages in common as Figure 2.3 shows. I shall now examine the overlap in a little more detail. The listing below distinguishes between the broad survey, or background, work needed to identify the chosen FS and the analysis of it, once chosen. The listing follows.

A. BACKGROUND

1. Objective:

To understand the agro-ecosystem of an area well enough to identify specific FS worthy of more detailed analysis.

2. Activities (in descending order of scale):

a. Understanding of climate, soils, topography, vegetation, biotic factors (such as erosion and fire) of a substantial area of land; by collation of existing knowledge and some survey if necessary.

b. Understanding and mapping of land use, including forestry, of the area.

c. Understanding of agricultural components in broad terms, including cropping/stocking patterns, perennial crop usage, forest/timber relations, land tenures.

d. Identification of "target" FS for detailed study on basis of special interest or social importance; possible designation of characteristic ("benchmark") sites/areas.

3. Observations: broad patterns defined above will usually be at least moderately well known but some filling in may be necessary, especially as to item d.

B. ANALYSIS

1. Objective

Analysis of chosen FS with aim simply of understanding the system (FSR sensu stricto) or of identifying potentially favorable innovations (OFR/FSP).

2. Activities (in ascending order of detail):

a. Analysis at verbal/descriptive level; identification of crops, annual and perennial, and of animal components (including fish and fowl); cropping patterns in space and time, rotations; crop/stock integration; farming-woodland relations, sources of fuel and timber; influence of land tenure; technology used; labor; within and between family relations; farmers' objectives; cash and energy flow patterns over time; utilization of farm products, marketing; more or less intuitive identification of limiting factors.

b. Analysis at semi-quantitative level; elements listed under (a) quantified by appropriate surveys in terms of cash/energy/labor flows; development of descriptive diagrams; some explicit economic analysis (e.g., partial budgets); identification of limiting factors.

c. Analysis at system-synthesis level; extension of (b) to numerical computer models with object of specifying the system fully, identifying critical interactions, predicting the effects of specified changes or perturbations, investigating robustness of data and assumptions.

3. Observations

FSR sensu stricto will tackle 1 and 2 in depth and may proceed to 3. OFR/FSP will do just as much 1 and 2 as is deemed necessary and will either eschew 3 as being unnecessary or use computer models rarely, cautiously, and for specific purposes.

The listing is obviously highly generalized and would rarely, perhaps never, be fully followed in practice. Thus ICRISAT, with rather a wide mandate for crops in the semi-arid tropics, has done extensive survey and agro-climatological background work under A(2) in India and Africa but has, naturally, concentrated on cropping pattern improvements in Peninsular India; while ILCA has naturally (and by mandate) concentrated upon animal production systems in Africa, especially in upland Ethiopia. The crop-oriented institutes such as CIAT, CIMMYT, and IRRI, under their mandates, can perfectly well by-pass most of A(2) and look hard at their chosen crops (while keeping an eye on others and on stock). Thus the notion that a sequence of ever finer analyses of agro-ecosystems will finally reveal those farming systems worthy of detailed study is a bit of a fiction. The FS chosen are, in practice, chosen pretty much on the basis of the mandates of the researchers, of their skills and understanding, and of sheer practical convenience.

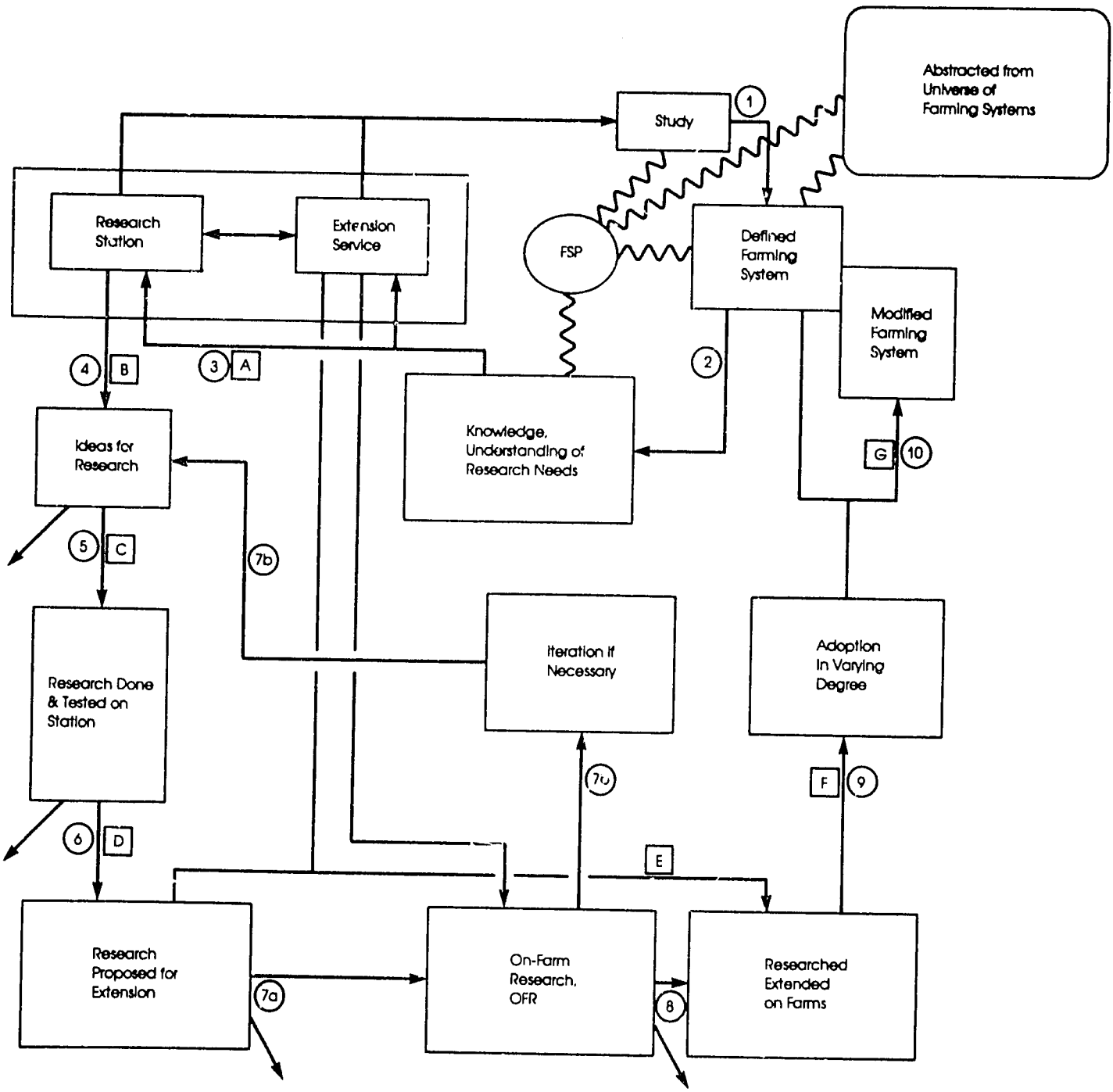
Under B. ANALYSIS, we need only observe that, as remarked above, FSR sensu stricto will go deep (certainly in activities 1 and 2, maybe also in 3, take a long time about it, and will choose systems to study on the basis of its own criteria of importance, social, political, historical, scholarly, or whatever. OFR/FSP will analyze the chosen system superficially (in the literal, not denigratory, sense), as quickly and cheaply as possible, with the object of finding out where and how research may have a useful economic impact. The work will lie largely in activities (a) and (b) but will be highly selective in those areas and will only very rarely touch activity (c). We explore this further in Chapter 3.

"Upstream" and "downstream"

The words "upstream" and "downstream" recur repeatedly in the literature of FSR. I do not propose to use them often but they must be explained and interpreted. I remarked above that one of the several reasons for the emergence of FSR as a component of the research process was that, too often, ideas proposed for extension failed: farmers did not adopt them or adopted only partially or in modified form. Such failures have certainly been numerous: of varieties that looked good in the breeders' plots but were rejected on the farm; of tillage, fertilizer, or plant protection recommendations that were technically sound on the station but unadopted on the farm; of grazing management or stock-feeding proposals which came to naught; and so on. It is generally held that such failures have been due to the research process having been of an upstream, top-down, or research-push nature, innovations technically sound in themselves being ill adapted to farmers' circumstances. The upstream process is to be contrasted with the downstream, bottom-up, or "farmer-pull" process characteristic of research guided by OFR/FSP (Figure 2.4). The difference lies in two features: first, that the traditional upstream approach started from such intuitive FSP as the researcher already had rather than from explicit analysis; and, second, that results were translated directly from experiment station to extension, without an intervening OFR stage. The two routes are indicated in Figure 2.4, respectively by the numbers 1 to 10 for downstream (with provision for an iterative cycle at 7) and the letters A to G for upstream.

There is now, I believe, little doubt that the downstream approach, with its OFR/FSP emphasis, is the proper and effective one in the small-farmer food-cropping context, and this belief (now nearly dogma, perhaps) is widely shared by IARCs and NARSs alike (but not by Arnon, 1981, who advocates a very top-down approach). It is not true, however, in the context of technology-based agriculteres, where what is at least a nominally upstream approach works very effectively (though no one would claim perfectly). The differences are that: first, growers are generally well educated, technically and economically aware, interested in and often eager for innovation, and rich enough to be adventurous; second, the researchers are often in close touch with growers, so the FSP is there all along; third, though there is rarely a formal OFR phase (sometimes a deficiency, I think), there is no lack of informal OFR activity because there is nearly always a core of growers who will try any idea even before the researcher thinks it is ready; and, fourth,

Figure 2.4: Upstream and Downstream



communications, by press, radio, meetings, and, not least, by personal contact, tend to be good, even excellent. The above is as true for privately run tropical estates as it is for prosperous farms in temperate countries. In historical retrospect, therefore, it was the unquestioning transfer of an upstream model, from social circumstances in which it worked (and continues to work) very well, to a socio-economic milieu in which it did not, that caused the difficulty. It took a long time to see the point but it is now well taken.

New Farming Systems Development (NFSD)

I now try to draw together points from the foregoing discussion and list what I take to be the main features of NFSD, as follows:

1. any NFSD consists of many and complex changes which must, of their nature, be made more or less simultaneously;

2. it is inaccessible to the stepwise change characteristic of OFR/FSP, at least on any reasonable time-scale;

3. of its nature, NFSD is a top-down or upstream process which must owe something to a FSP of what is already there on the ground but cannot be other than an invention by imaginative researchers with some conception of what is technically and economically possible;

4. while OFR/FSP seeks to adapt new technology to the socio-economic circumstances of small farmers, NFSD has to do the opposite, namely, adapt the economics to the technology; government intervention in one form or another is implied;

5. there are no rules for how to invent a NFS because every case must be sui generis;

6. nor are there, for want of experience, rules for how to develop a NFS into practice; I can only suggest (tentatively, because there is probably much literature that I have not seen) that an approach through spreading clusters of model/unit farms might be appropriate (cf Chapter 5); a somewhat authoritarian element here seems to be inescapable;

7. no NFSD would be at all likely to be "correct" from the start; so there would have to be an adaptive phase, which might well take the form (if point 6 is right) of an OFR/FSP approach to the problems of the hypothetical model/unit farms.

Historically, there have been many examples of land-settlement, irrigation, rural development schemes (by whatever name) which were, in effect, attempts at NFSD, or were built around NFSD ideas. One's impression is that a great many of them failed, though some (the Gezira scheme in the Sudan comes to mind - Tothill, 1948) worked well for decades. A critical survey would be worthwhile because analysis should yield clues as to how and how not to try to implement NFSD schemes in the future.

As a historical aside, one wonders what might have emerged from the Namulonge farm (Arnold, 1976), as the center of a network of unit/model farms, had history been otherwise. Namulonge, I suggest, bears thinking about as an example of a possible starting point for a NFSD.

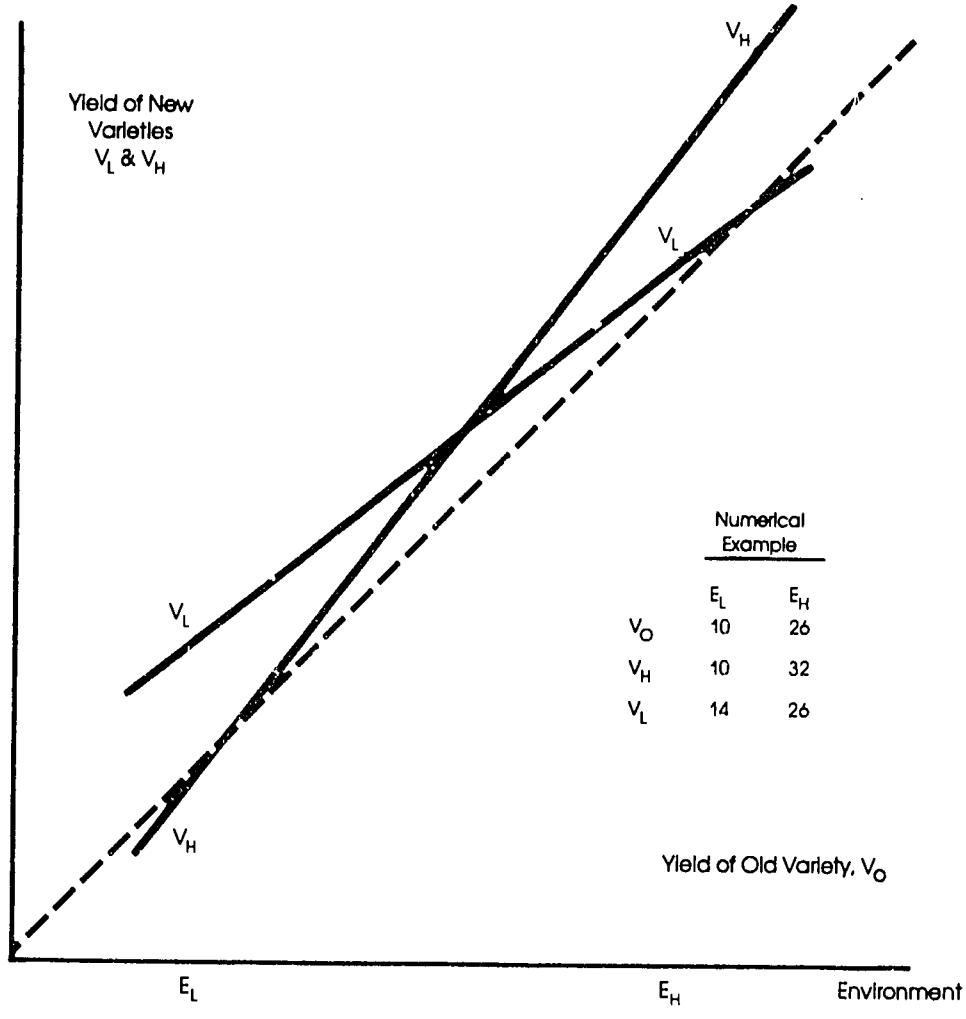
If points 4 and 6 above be accepted, it is clear that the development phase of a NFSD would demand a high level of management and would be expensive. There is a possibility that the needs for expertise and money would just be too high to be acceptable and Collinson (1983, and personal communication) inclines to this view. This is discouraging but I do not believe that the possibility absolves us from the responsibility of thinking and trying.

All the above is written in the context of tropical small-farmer food crop agriculture. The market pull of an important cash-export crop, of course, changes the situation fundamentally. The great banana, sugarcane, coconut, rubber, oil palm, tea and other cultures were all outstandingly successful NFSDs that carried along with them important small landholder developments (Chapter 1; Figure 1.4; Chapter 4). Perhaps one can discern here a clue that future, deliberately contrived, NFSD need a cash-pull element and a degree of autocracy in their management?

Genotype-environment Interactions

I stated in Chapter 1 that a genotype-environment (GE) interaction was an important element of the Green Revolution: the matching of the responsive semi-dwarf varieties to environments enhanced by irrigation and agri-chemicals. Plant breeding is a substantial part (about 50 percent - Coulter, 1979) of the crops research effort of the IARCs and is probably a comparable proportion of most NARS programs. Temperate crop agricultures have achieved very large yield advances in the past four or five decades, generally of the order of a doubling; such calculations as have been done (surprisingly few, in fact) tend to show that plant breeding and improved husbandry have been roughly equal in effect but with a large GE component as well. Improving crop environments have, therefore, generally (not always) been matched by responsive varieties (Simmonds, 1981). Such varieties, when compared with their predecessors, would typically show regressions of the V_H type of Figure 2.5, superior in a "high environment" (E_H), little better, even slightly inferior, in E_L. Though selection for this kind of "responsiveness" seems to have been the general pattern as crop environments improved, there are varieties (e.g. of potatoes and sugarcane) that show, at the extreme, a contrasted pattern of response (V_L in Figure 2.5): excellent performance in E_L, unremarkable performance in E_H and low regression slopes.

Figure 2.5: Genotype-Environment Interaction



GE interactions are omnipresent and pose endless problems for plant breeders (review in Simmonds, 1979); animals show them, too (cattle example in Chapter 5), but temperate animals are usually kept in such uniform environments that breeders there hardly have to reckon with GE effects. Their importance for improving tropical food crop yields is great but not yet, I think, sufficiently recognized.

Consider the following points:

1. Selection in E_H (e.g., on the experiment station) automatically tends to favor the responsive V_H type of variety with a high regression slope and tendency to do poorly when grown at E_L, in rough farm conditions.

2. Plant breeders are brought up to take pride in their plots and to be pleased by high-yielding trials. Very few breedersindeed (though the number is increasing) do the obvious thing and select for V_L in E_L. Strict on-farm selection would rarely be feasible but deliberate simulation of E_L on-station would be open to trial; CIMMYT already practices some low-input selection in maize, and CIAT simulates the climbing bean's small-farm maize environment (Chapter 5). There is room for much development along these lines, I think.

3. Given an array of potentially good, new varieties widely spread for local trial, selection, and use, how are the best ones for local small farmers (generally operating in E_L under low or moderate inputs) to be identified? More trials at E_H (international nurseries or whatever) must simply tend to pick the responsive V_H types again, not necessarily the best for small farmers. A partial answer must surely lie in variety testing on-farm in OFR/FSP programs and this has of course been, and is being, widely practiced; but the samples so tested have already been biased by at least one and often two cycles of trials selection in E_H, with unavoidable contra-selection against the survival of such of the V_L type of variety as might have survived the original selection process.

4. Identifiable environmental stresses such as peculiar soils, drought, pests and diseases are coped with by the plant breeder by selecting for resistance in the presence of the stress. To select resistance is deliberately to try to exploit a GE effect (though not normally thought of in these terms). Conversely, not to select for resistance to a particular stress (because it is absent from the breeder's environment, for example) is to court disaster when the varieties that emerge are grown more widely. There are plenty of examples of unforeseen disease susceptibility; neither disease resistance nor tolerance of highly adverse soils is likely to arise by accident.

From these points, I conclude that GE effects, both macro and micro, are omnipresent, that plant breeding is therefore essentially a local activity, that over-centralization tends to lead to local disadaptation, and that, insofar as small farmers working at low yield levels are the ultimate customers of a program, FSP at the most (and maybe more OFR as well) ought to have a substantial place in framing breeding plans.

Some readers might think that, in arguing thus, I am merely over-emphasizing professional interests at the expense of the proper subject of the report. I do not believe that I am. If plant breeding is roughly half of crops research and represents roughly a half of the potential for progress, and GE effects are as important as I contend, then the matter is economically weighty and there must be a substantial area of intersection of interests between plant breeding and OFR/FSP yet to be well exploited. It cannot be good enough, these days, simply to hope that widely adapted "universal" varieties will turn up so long as one tries hard enough.

I note, finally, that researchers in India have come to very similar conclusions. The reliability of conventional, high-input trials as predictors of farm performance has been questioned for some years, and IARI is therefore moving towards a policy of on-farm testing and the release of numerous varieties rather than few (Jain and Banerjee, 1982).

CHAPTER 3. THE NATURE OF ON-FARM RESEARCH WITH A FARMING SYSTEMS PERSPECTIVE (OFR/FSP)

OFR Procedures

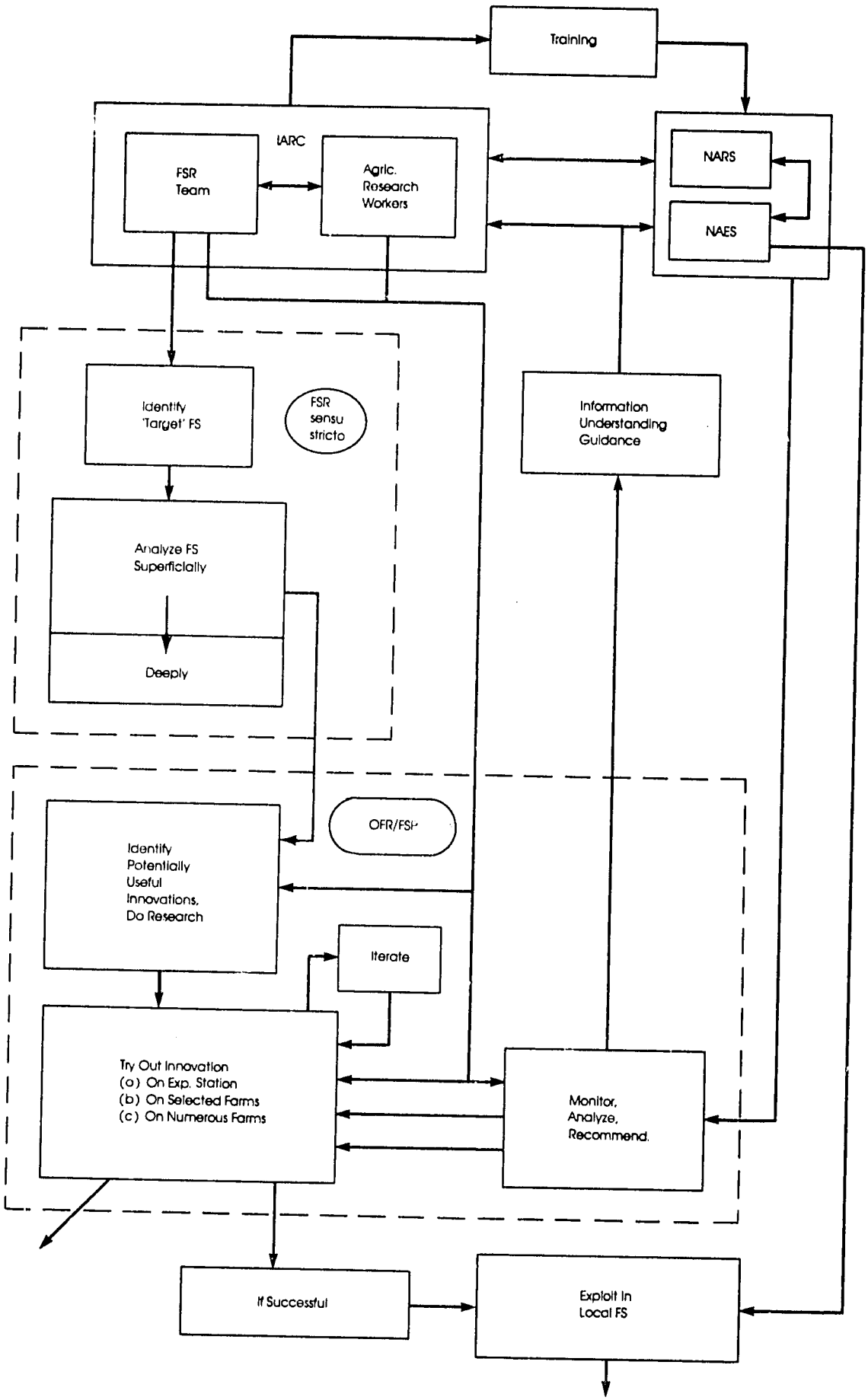
Introduction

A distinction has been drawn in preceding chapters between FSR sensu stricto and OFR/FSP. In this chapter I concentrate upon the latter with the object of outlining the phases recognized and the procedures recommended by its practitioners. The leading references are: Byerlee and Collinson (1980), Perrin et al. (1976), Zandstra et al. (1981) and Shaner et al. (1982). Wide variation in terminology tends to conceal the essential similarity of all the approaches and the fact that the "methodology" (an over-used word) is, in essence, straightforward. This report is not a handbook of OFR and my only concern is to try to reveal the leading principles. Those desiring specific practical guidance could best refer to the references above, in the knowledge that they are by the leading and most experienced practitioners in the field and represent the best opinions we have.

Figure 3.1 makes the main points and, of necessity, reiterates the FSR sensu stricto - OFR/FSP distinction. The diagram is set in the context in which, historically, systematic OFR/FSP has developed, namely, that of an IARC (specifically here CIMMYT and IRRI) working in collaboration with NARESs. The main features are as follows;

1. There is a team (multi-disciplinary) in the IARC which
2. identifies the "target" FS or "recommendation domain" and
3. analyzes its technical/economic structure just deeply enough for the purpose in hand;
4. the team identifies economically sensible innovations and
5. does research on them, on the experiment station, on few selected farms, on more numerous farms, according to circumstances;
6. under points 2-5 the team solicits the interest and collaboration of the NARES and
7. the experimentation is iterated as necessary;
8. the experiments are monitored/analyzed economically and, if successful, lead to recommendations for the national agricultural extension system (NAES) to exploit;

Figure 3.1: Relation Between FSR and OFR/FSP



9. there is a continual feed-back of information to the IARC/NARES consortium as experience and understanding widen;

10. given that there is agreement that the OFR/FSP "style" of research is valuable, the IARC must assume a responsibility for training and network development (see "Training and Networks" section below).

The above list is stated in the existing context of IARC-NARES collaboration in the enterprise. As time goes on and experience accumulates, the responsibility for OFR/FSP must move towards the NARES (Chapter 4). With this qualification, the list is, I think, perfectly general and is likely to remain a satisfactory logical structure, whatever the vagaries of nomenclature.

The "recommendation domain"

The "recommendation domain" (or "target" farming system) is the farming system narrowly enough defined that any recommendations that arise from OFR work may reasonably be expected to apply, in large part at least, to all the constituent farms of the domain. I remarked above that any classification of FS is arbitrary and will go just as deeply as is required for the purpose in view and no further. The depth required here will be a matter of judgment for the OFR team asking the question: "Is this assemblage of farms sufficiently alike for the purpose?" In practice (Chapter 2) the question will always be asked of a subsystem (a crop, a group of crops, an animal) in a limited geographical area but in awareness (FSP) of possible interactions with other subsystems. The factors of which the researchers will have to take account are usually classified thus:

1.	Technical:	a.	physical	
		b.	biological	
2.	Human:	a.	Endogenous:	farmer resources, goals, attitudes, and constraints
		b.	Exogenous :	community relationships, institutional arrangements (communications, markets, government intervention, etc.)

In real life, obviously, no great depth of understanding will be possible when numerous possible recommendation domains present themselves. Generally, that one will be chosen which represents the greatest number of farmers, the greatest area of land or some such criterion, tempered, no doubt, by some a priori sense of what is technically feasible. The choice, however arrived at, will not be independent of other possible choices, because there will generally be

overlaps/intersections (Figure 3.2) with them such that decisions/recommendations for one domain will be transferable, in part, to others. Within reasonable limits, the wider the domain the better.

Survey or diagnostic phase

A recommendation domain having been defined, there usually follows a farm survey to define OFR activities. If local knowledge is already good, of course, a formal survey may be unnecessary. Experienced practitioners are agreed that short, simple surveys suffice (Table 3.1). A few man-weeks will normally be enough, encompassing an exploratory survey to decide what specific questions to ask and a formal (but still simple) numerical survey. As to the former, experience is all-important. As to the latter, there is an abundant statistical theory of surveys which will, no doubt, be kept in mind but rarely exactly observed as to precepts: if defects of public data are such that farmers can not be accurately enumerated, let alone described as to economic characteristics, strictly random or stratified random procedures hardly apply. Accessibility, co-operativeness, the opinions of experienced extension agents, and so forth will probably be more weighty factors in choice than the statistical niceties. Even if statistical precision must, perforce, often be foregone, results should, in experienced hands, be reasonably representative and should yield useful measures of uncertainty.

In connection with the design of the survey questionnaire, a good working principle is: as short and simple as possible for the purpose in hand. Long questionnaires are self-defeating, and I have seen a sad example of one of 50-odd pages, in two languages, rarely properly completed and ill adapted to the local computer; the local OFR workers had already waited months for non-existent analyses.

The data taken in a well-designed survey will take the form of time-tabled flows of labor, material inputs, other costs, yields, prices, and other returns. Collectively they will allow the economist on the OFR team to construct cash flows (imputing costs and prices where necessary), to get at least some idea of the farmers' situation with regard to risk and uncertainty, to detect potential conflicts of demand for labor and cash, and at least start to identify critical or limiting processes. The methods are essentially those of conventional farm economics, but with some necessary translation of non-cash items into cash equivalents for the purpose of calculation. Adherents of Chayanov might be unhappy with these procedures because they treat the small farmers as a more or less conventional "firm" that employs family labor at imaginary wages and sells goods to itself at imaginary prices. Chayanov argued that the conventional economic theory of wages-interest-rent-profits was simply inapplicable to peasants who paid themselves no wages and sold few goods. They were, he thought, simply outside the market economy. The test of the OFR economists' approximations must be empirical: so far they seem to work pretty well. Perhaps a re-reading of Chayanov would suggest useful refinements?

Figure 3.2: The "Target" Farming System or "Recommendation Domain"

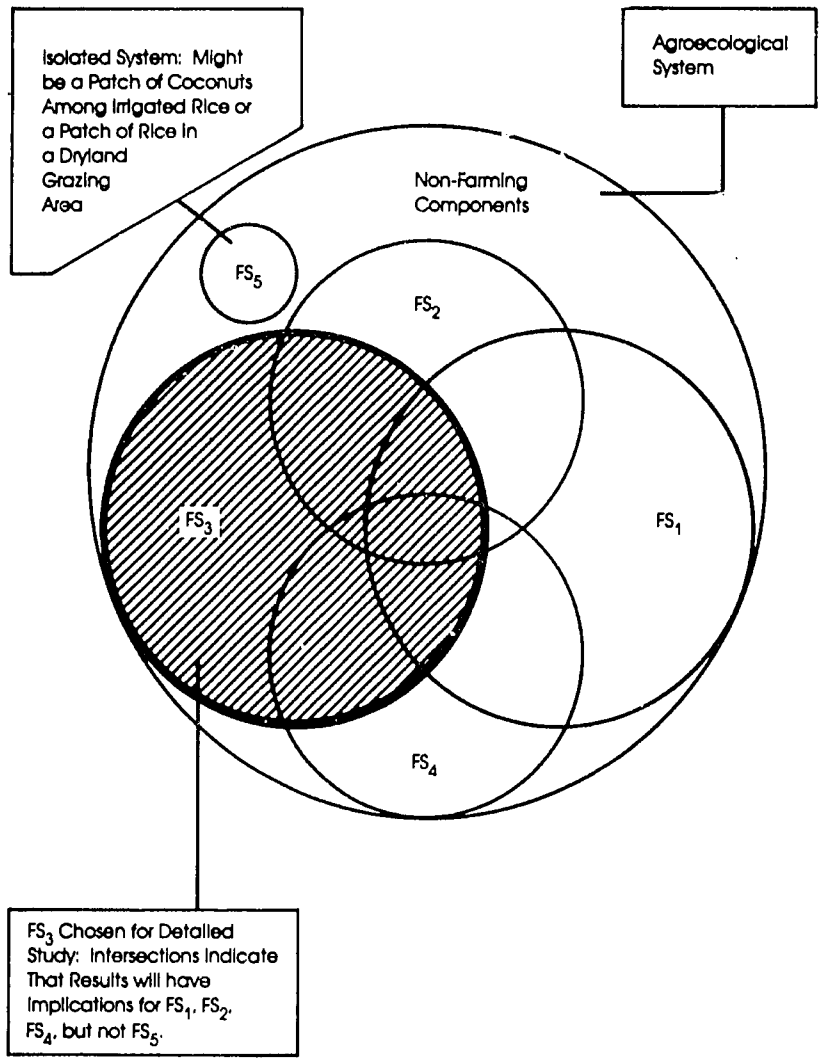


Table 3.1. SEQUENCE OF OFR/FSP ACTIVITIES

Season (year)	Activity		Time (weeks)
First	1. Exploratory Survey	In crop	
	2. Formal survey		
	Prepare		2
	Execute		3
	Analyze		4
	3. OFR work		
	Plan		4
	Prepare		8
Second	Execute (crop season)		20-25
	Evaluate		6
	Report and Plan		8

Source: Byerlee and Collinson (1980)

Survey questionnaires are notoriously difficult to design really well to give accurate and unbiased answers without excessive length. The difficulties in the OFR context are often compounded by language problems, by socio-economically sensitive issues, even by an amiable desire to tell the interviewer what he wants to hear. These difficulties are usefully listed by Matlon (1982). The only remedy is experience, cross-checking when possible, and cultivation of a cautious skepticism.

OFR work proper

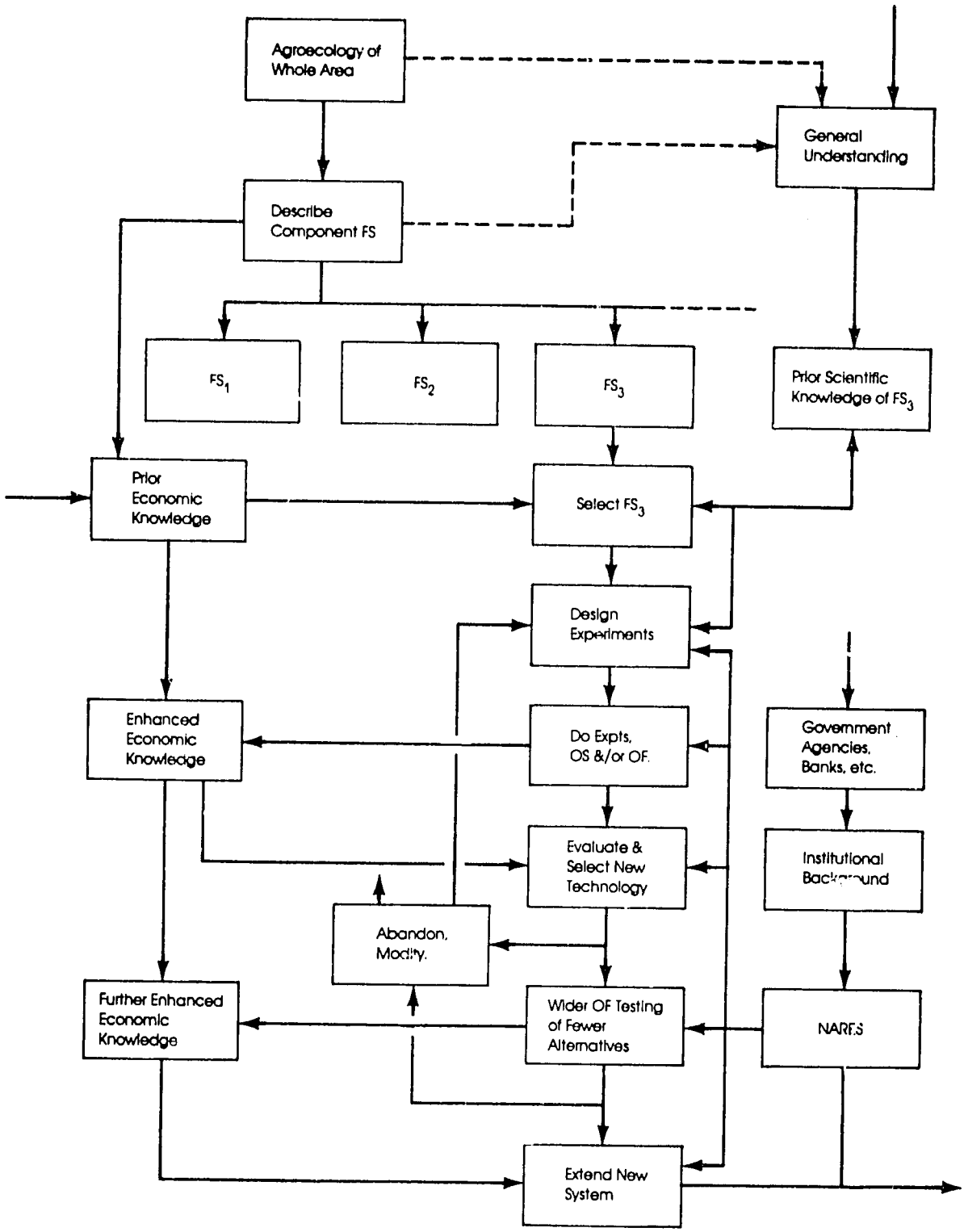
OFR operations in the field start with planning and preparing by the team on the basis of the findings of the preceding survey (Table 3.1) and of prior scientific knowledge of the system (or of related systems). Beyond stating the obvious, that simple unit changes or small packages of changes will be preferred, no simple rules can be given: all depends upon the circumstances and the knowledge, experience, and imagination of the members of the team. Nor can simple rules be given as to what follows: Table 3.1 supposes that it has been possible to take appropriate technology off the shelf, so to speak, and go straight to the farm in the next season. But, often, preliminary on-station experimentation will be needed (Figure 3.3) and entry on-farm deferred. Similarly, the number of cycles of OFR needed is generally unpredictable; sometimes a single cycle of a couple of years will suffice; more often, perhaps, two or more cycles, narrowing down the alternatives, will be necessary. If the last is true, then the first cycle will probably consist of researcher-managed trials (statistically structured, often factorial) and the second of farmer-managed trials of, at most, two or three treatments against traditional practice as the control. In the latter, replication will be of substantial plots across farms so that, formally, farms are treated as blocks.

All on-farm trials are economically monitored, and there will be regular feed-back of scientific and economic understanding to the OFR team to guide subsequent experimentation or to reach a decision either to abandon or recommend adoption (Figure 3.3).

All experienced OFR workers are agreed that the NARES must be involved from an early stage and throughout (Figure 3.3). The extension worker's FSP is agreed to be a potent complement both to the formal economic survey and to the actual conduct of on-farm experiments; furthermore, there can be few things more likely to conduce to successful extension than official NAES involvement and interest *ab initio*.

The composition of an OFR team (whatever the institutional arrangements) has to be, in practice, decided *ad hoc*. There will generally be an agronomist and economist, at least, with technical assistance, and contributions from specialists such as plant breeders, pathologists, entomologists, and soil scientists according to need. Practice is beginning to suggest that some four to six workers will be fairly deeply involved in any one operation. Institutional structure is referred to later in this chapter.

Figure 3.3: Formal Structure of OFR/FSP Work



Practical points for OFR workers have been usefully discussed by Tripp (1982), and a condensed summary of his list follows:

1. Site-collaborator selection

   a. Domain: See above.

   b. Sites: Chosen within farms, suitable as to rotation and access.

   c. Logistics: Sites and farmers easily visitable.

   d. Diversity: Rotate sites between farmers over years; do not stick to a few chosen collaborators.

   e. Extension: Use extension officers' experience.

   f. Biases: Adjust choices of sites over years.

2. Communication

   a. Details: Agree formally with farmer as to sites, treatments, cultural practices.

   b. Visits: Make them in presence of farmer whenever possible.

   c. Address: Adopt courteous forms of address.

   d. Local knowledge: Learn about farmer's circumstances.

   e. Opinions: Encourage farmer to express his own opinions.

3. Data

   a. Fieldbook: Should be pre-designed in field format.

   b. Records: Made immediately but preferably not in farmer's presence.

   c. Opinion: Record farmer's opinions

   d. Visits: Record each one.

   e. Other fields: Make notes on.

   f. Posterity: Aim for complete records, usable by successors.

   g. Approach: Keep it seemingly casual.

4. Miscellanea

a. Other farmers: Visit and learn from them.

b. Markets: Learn about from local merchants and traders.

c. Research: Inquire whether there is other relevant work in the same scientific/ geographical area.

d. Experience: Use that of former collaborators.

e. Contacts: Maintain close touch with extension personnel.

f. Questions: Develop lists of questions arising.

The question of compensation to farmer-collaborators is a tricky one. I think a fair summary of rather diverse views would be as follows. In researcher-managed trials, the inputs are provided and the farmer is guaranteed that he will not be worse off at the end of the season than he would have been had he used the land himself; he might, for example, receive the produce (rather than any cash payment). In farmer-managed trials, the less free inputs the better, though some may be unavoidable; the aim, at the end of the cycle of experimentation, anyway, should be full economic responsibility lying with the farmer, because only thus can the researchers be fully confident of the economic viability of the innovation.

Economics

As I said above, this is not a how-to-do-it handbook, so detailed economics would be out of place as well as beyond my competence. The references at the start of this section, together with Collinson (1983), Dillon and Hardaker (1980) Valdez et al. (1979) will provide details for those who need them. Levi and Havinden (1982) give a useful general introduction to African agricultural economics. Here I shall only refer to a few general principles and give some examples.

There are four main points, as follows. First, the methods are essentially those of conventional temperate-country farm management economics, but using imputed costs and prices where necessary (see above in this section). Second, small farmers are poor and risk-averse and this factor is taken into account in two ways: by doing specific uncertainty analyses (could the farmer stand a 10 percent chance of failure?) and by setting the discount rate at some arbitrarily high level (maybe 40%) so that marginal returns must be high. Third, it is assumed that the farmer is nearly enough profit-maximizing for practical purposes, though the balance to be struck between costs, profits, and rates of return must be a matter of economic judgment. Fourth, some innovations are divisible (e.g., fertilizers - Figure 3.4), some indivisible (e.g., packages of agricultural practices - Tables 3.2 and 3.3); the former can be adopted

Figure 3.4: Economic Responses of Farmers to a Divisible Input

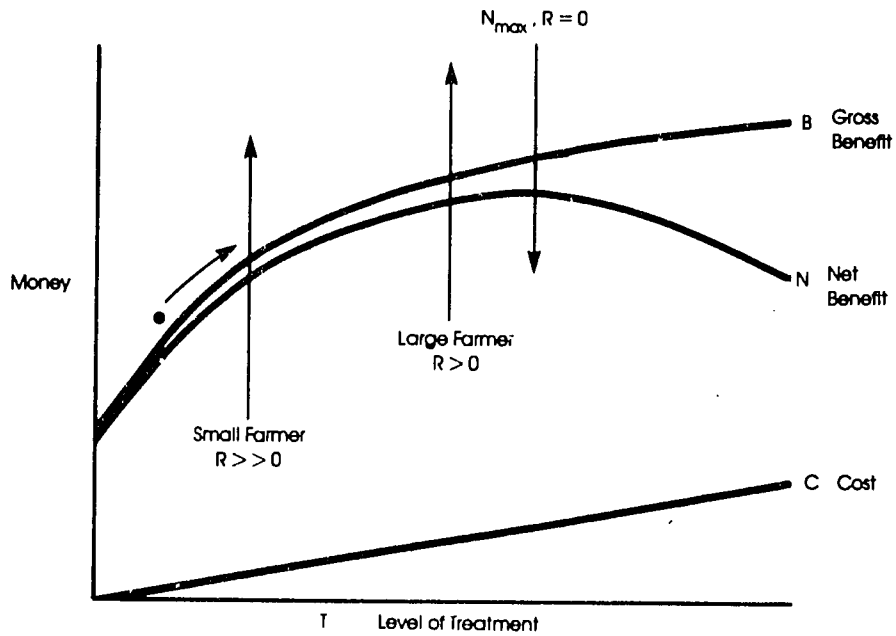


Table 3.2. ON-FARM ECONOMIC ASSESSMENT OF RICE-CROPPING PATTERNS IN THE PHILIPPINES

(arbitrary money units)

Patterns	V'ble costs		Returns		Net Returns		Marginal Rates	
	(a)*	(b)	(a)	(b)	(a)	(b)		
Traditional Pattern	C	C'	B	B'	N	N'	B/C'	N/C'
Rice-fallow	8	--	20	--	12	--	--	--
Alternatives								
Rice-mungbean**	11	3	25	5	14	2	1.7	0.7
Rice-ratoon	10	2	27	7	17	5	3.5	2.5
Rice-rice	22	14	41	21	19	7	1.5	0.5

* Of the columns headed (a) (b), the former are the actual figures, the latter the marginal differences over the traditional base figures.

** Rice-mungbean was already partly adopted technology known the farmers.

Source: After Zandstra *et al.* (1981)

Table 3.3. RETURNS FROM CROPPING SYSTEMS INTRODUCED IN INDONESIA

(US$/ha.)

System	C Costs	B Benefits	N = B-C Net Benefits	(B-C)/C Rate of Return
Farmer practice	170	360	190	1.12
Introduced system	464	1,070	606	1.31
Differences (Marginal C, B, [B-C])	294	710	416	1.42

Notes: (1) See Figure 5.7, Section 2, for general patterns of systems tested (rainfed multiple cropping).

(2) The marginal rate is 142 percent.

Sources: Means of three bodies of data from various sites in Sumatra and Java, 1976-82 (AARD, 1981; Inu, 1983).

at a low (sub-optimal) level, thus leaving open the option of subsequent upward adjustment if/when the farmer feels inclined to greater expenditure; the latter incur a fixed cost per unit but need only be adopted over part of the farm, so they, too, are functionally divisible.

I now give three examples, with brief discussions. Figure 3.4 is based on the maize fertilizer data of Perrin et al. (1976). Given a fixed dose of phosphate, the nitrogen response curve is of the form shown and can be described by a simple quadratic. The net benefit (N = B-C) curve has a maximum when R, the marginal rate of return, is zero, a point which few farmers would choose. The rich, non-risk-averse farmer, with a secure low discount rate, would operate near the maximum; the small, poor farmer could reasonably be advised to use nitrogen at a rate corresponding to R = 0.4, say (40 percent), but it might be prudent to suggest a somewhat lower rate, in the knowledge that adopting farmers are perfectly capable of using collective experience to adjust rates upwards over time. (I should add that this is a simplification of the example, adopted for heuristic reasons; Perrin et al. treat the data as discontinuous, an approach that has computational advantages but is less easy to expound). The number R=0.4 just cited represents simply an order of magnitude for the discount rate suggested by the empirical experience of the CIMMYT workers. It is not a universal constant.

Examples of indivisible "packages" are given in Tables 3.2 and 3.3. In the former, three alternatives are compared with the traditional rice-fallow system. A second full crop of rice looks less attractive than the other two: it is, in one sense, the most profitable but it incurs high costs and offers a low marginal rate of return. No single, simple criterion for decision is apparent. The Indonesian example of substituting relayed row cropping of maize, rice, peanut, cowpea and cassava for the traditional maize-rice mixture relayed with cassava looks very attractive in terms of marginal return but demands a heavy extra expenditure (costs are more than doubled). Could the farmer find that amount of extra money? Would he want to do so? Is there a market for extra produce? These are the sorts of questions the OFR economist would have to try to answer.

Modeling

I talked to many people about the use of numerical system modeling and found many and diverse views about the subject. Three uses of models need not detain us here: (1) whole farm models as outcomes of FSR studies sensu stricto (which are academic in nature rather than practical); (2) plant or animal growth models as aspects of physiological research; (3) macro-economic models as components of economic studies wider in scope than OFR/FSP (ICRISAT analysis of the "common property" problem would come in this category, I think).

For the present purpose the question is: what place do models have in practical OFR/FSP work? Most practitioners say "none," that the questions asked and prospective changes in farm practice are so restricted in scope that the experienced economist, using conventional farm-management procedures, can discern what is workable and what is not.

Further, time usually presses (Table 3.1), and the need is for quick, sensible decisions in a feasible economic area rather than precise, optimal decision. The proponents of this view would add that both data and models are rarely good enough to allow optimal prescription anyway, even if it were wanted; and they sometimes add, as well, that the delights of modeling per se are liable to divert the modeler from his proper function, namely, practical economic advice. A variant of these views (not widely encountered) is that models have their uses in defining uncertainties and feasible areas of decision (even if optimization is rejected as the object).

The arguments against modeling in OFR/FSP just outlined come from practitioners experienced in work with annual crops and, in that context, I find them persuasive. Our views of OFR/FSP procedures have, so far, been dominated by students of small changes in annual cropping systems and for these, as I have just argued, modeling seems to have little or no useful place. One notes that successive years can be regarded as essentially independent of each other and that the changes sought are small enough not to perturb the whole system. When there are effects spread over time (year-to-year correlations) or when larger changes are sought, the situation may well be different and modeling may have a more important place. Thus the CIAT pasture-beef workers argue (convincingly, I think) that both pastures and herd structures change over time, results in each year being largely determined by what preceded them, and that data simply can not be handled by pencil-and-paper methods. Numerical models, they contend, are the only available way of thinking about possible consequences, several years ahead, of relatively small changes made now. Without such models, indeed, it would be hard, perhaps impossible, even to predict the character and timing of equilibria let alone their responses to changed inputs.

As I say, I find this argument convincing and believe that crops OFR work, if and when it moves to more complex changes, may find increasing need for models. Likewise, NFSD, involving very complex systems with interactions beyond what the unaided imagination or mere diagrams could cope with, is, I think, likely to need models to help to identify the impossible, if not to define the best, solutions.

Finally, a few words of caution are necessary. First, I have no personal experience of numerical modeling and the comments above may be regarded by the experts as naive. Second, models are as good or as bad as the skill and the data that go into the making of them; they can be very wrong without the fact being apparent or testable. Third, the bigger they are, the more complex the systems that they describe, the more assumptions and guesstimates that they embody, the more likely they are to be inaccurate, but the more useful, in principle, they ought to be. And, fourth, modeling is evidently a compulsively interesting activity for those so inclined (as well as being a fashionable one); the dangers of doing too much and of believing the results too readily are evident.

Costs and benefits of OFR

To get a rough idea of costs of OFR I examined recent annual reports of several centers and counted economists (and a few anthropologists) as a proportion of total senior scientific staff (administration, works, libraries, etc., excluded). For six centers (CIAT, CIMMYT, IITA, ILCA, ICRISAT, IRRI) I found a grand mean of 8.4 percent economists, rather a wide range (3.1 percent IITA, 6.9 percent ICRISAT, to 19.6 percent ILCA) and a tendency for those places most committed to crops OFR to lie around 9 percent (CIAT 8.4, IRRI 9.4, CIMMYT 10.8). Assuming that some of the economics can not be assigned to OFR (which is certainly true, e.g., at ICRISAT), it looks as though about 5 percent of the centers' budgets might be attributable. OFR work, of course, incurs other costs over and above on-station research (Chapter 5) so it looks as though total costs are, at most, under 10 percent of budgets. If anything, commitment is tending to rise but I can think of no way of calculating an objective "best" or "optimal" number.

The uncertainty attaching to the above crude estimate of "under 10 percent" draws attention, I think, to a general problem. Accounts of the IARCs are not reported on a very informative basis so it is next to impossible to derive good estimates of expenditure on anything; to do this, a several-dimensional matrix of expenditure would be needed. In the FSR context, some institutes identify expenditure under this head but include much that should really be attributed to component research (soils, agronomy, machinery, etc.) while others do FSR sensu lato but do not call it that.

As to benefits of OFR, we have the one available C/B analysis referred to later (Chapter 5) to show that, in that one case, B fairly certainly much exceeded C on a DCF basis. In general, if OFR work can indeed, as is claimed for it, point research effectively at realistic practical objectives, deter the adoption of unrealistic objectives, and expedite uptake on the farm, then it must be generally beneficial. I think these claims are reasonable, which is not to predict universal success, for there must be some failures; nor does it help in calculating an optimal level of OFR activity which must, for the present at least, remain a matter of informed judgment.

I have no cost data for national programs and can only note that economists are already quite widely employed. As to benefits, a widespread commitment to OFR methods in the tropics of all three continents (cf. Chapter 5) implies local acceptance by NARESs of the likelihood of substantial gains.

Training and Networks

Training has been an important element in the work of virtually all the IARCs since the early days of the CGIAR, it having been recognized that one of the their prime functions was to help the NARESs to develop their own local skills and organizations. While much of the training has been technological, there is already a strong OFR training element at several centers (CIMMYT, IRRI, ICRISAT) and prospective developments elsewhere (CIAT).

Training and networking naturally go together and, jointly, they surely represent the most economical way available of diffusing the skills and knowledge of the Centers where they will do most good, that is, on the ground. To be only a little frivolous, the "old-boy network" (politely called an "invisible college") is probably the most potent agent for influence and information ever invented (libraries and computers not excepted). Its value in drawing together a dispersed body of people with common interests is incalculable.

The structure of IRRI's ACSN is summarized in Figure 3.5. It grew naturally out of a training program in the technology of rice agriculture, the outposting of IRRI staff to Asian national centers, and a sharply focused program on intensive wetland rice production. Now there are about 120 sites in 11 countries, each site with a local NARES-OFR team. As the results for wetland rice become assimilated into local practice (which has widely though not universally happened), so the program is diversifying, by a natural progression, into the wider, but still rice-related, contexts indicated at the bottom of the figure. Hence the emergence of the AFSN (Chapter 5).

The operation of the network, beyond the fact that it is a centralized but two-way system, is not detailed in Figure 3.5. In Figure 3.6, I outline the structure of the CIMMYT East African program which has, since 1975, evoked local interest in member countries, trained numerous local staff, done short survey/diagnostic jobs, and got the network going. The main activities of the network are: a newsletter, to sustain interest and attention among workers on the ground; occasional meetings of NARES administrators (e.g., CIMMYT, 1983); technical workshops; and visits and travels for specific jobs. All this is designed to provoke both the necessary administrative support for the OFR approach (sometimes not readily obtained, since the ideas are fairly new) and, no less important, the diffusion process (the old-boy network under a different name). As the network develops, so there is a natural tendency to shift training from in-center to in-country because: it is cheaper to do so; national interest is sustained; and local needs can be better met.

Economists, Anthropologists and Institutions

I remarked in Chapter 2 that economists entered the IARCs at an early stage and that their use seems to have been an article of CG policy. It is to the economists, I think, at the fairly mundane level of farm-management economics, that we owe the development of OFR/FSP and the decline of the traditional upstream approach in tropical food-crop research. That economists, in this role, have come to stay, and that they perform an extremely useful function is not, I think, now in doubt.

Some problems remain, though. Besides institutional questions (see this section, below), the relation scientist-economist is not always an easy one if neither fully understands what the other is doing and if each tends to defend his professional interests. On the one hand, I have heard it argued that the essential economics is fairly simple (which it is) and that all that is required is a bit of economic education for the scientist; on the other, it is argued that the necessary economic

Figure 3.5: The Asian Cropping (Farming) Systems Network

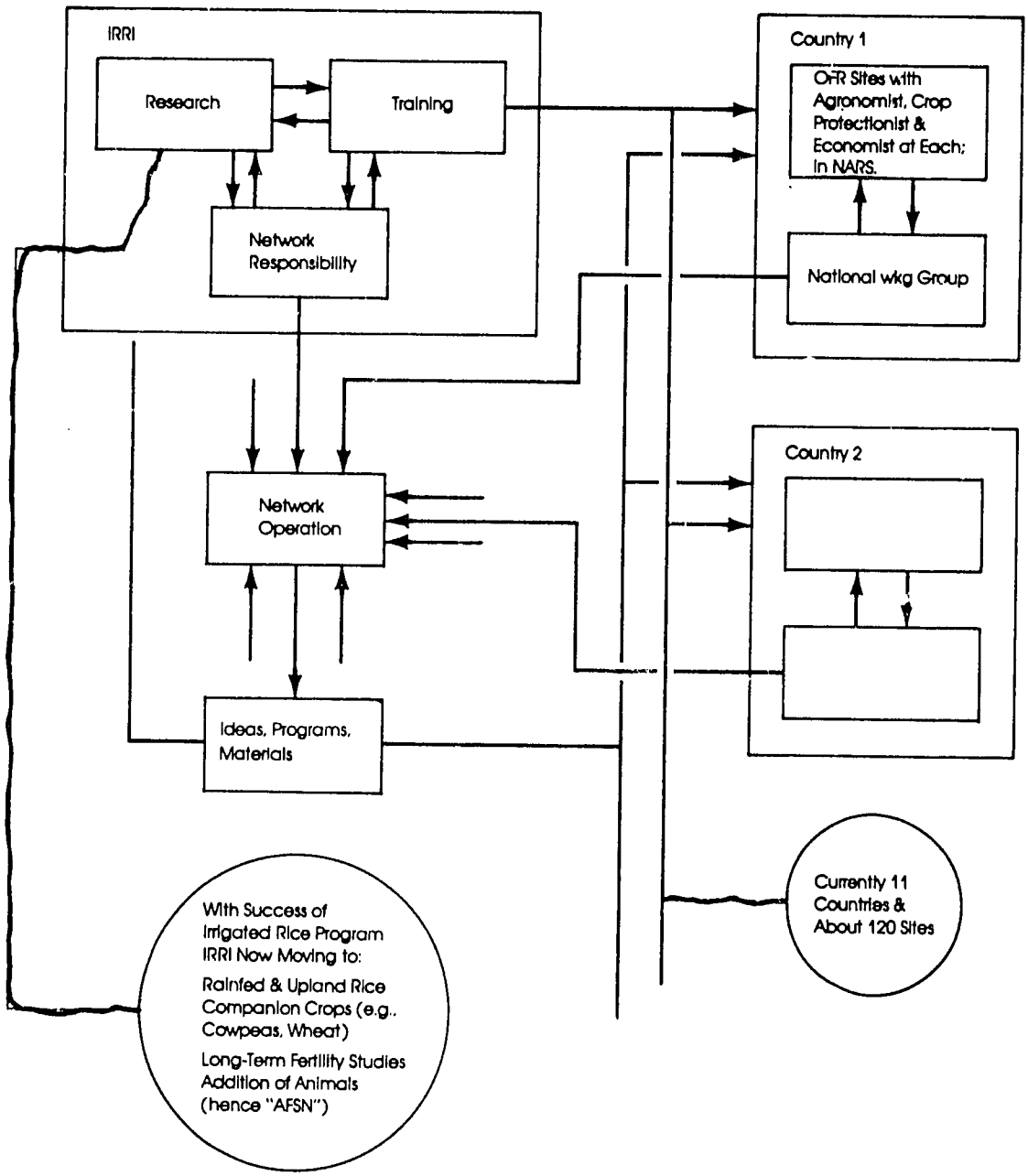
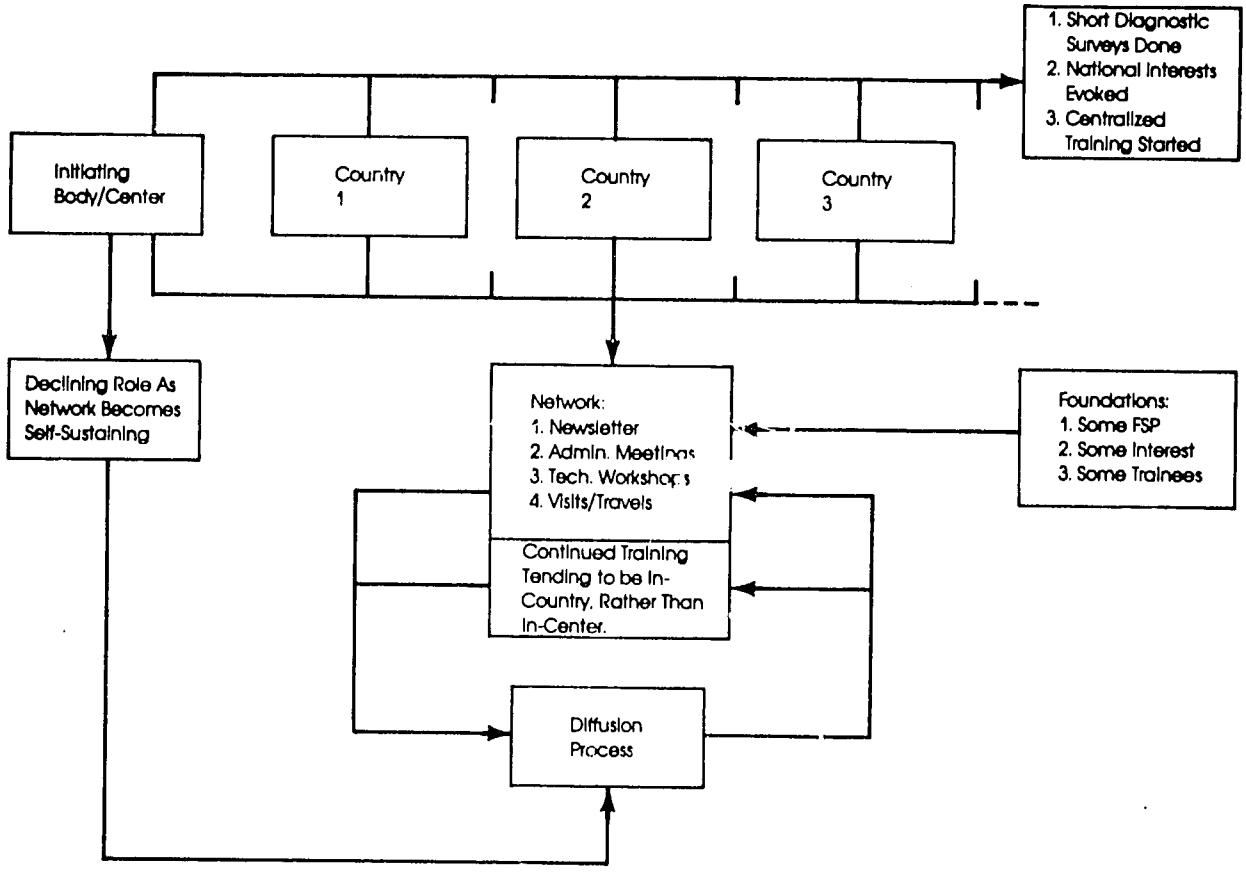
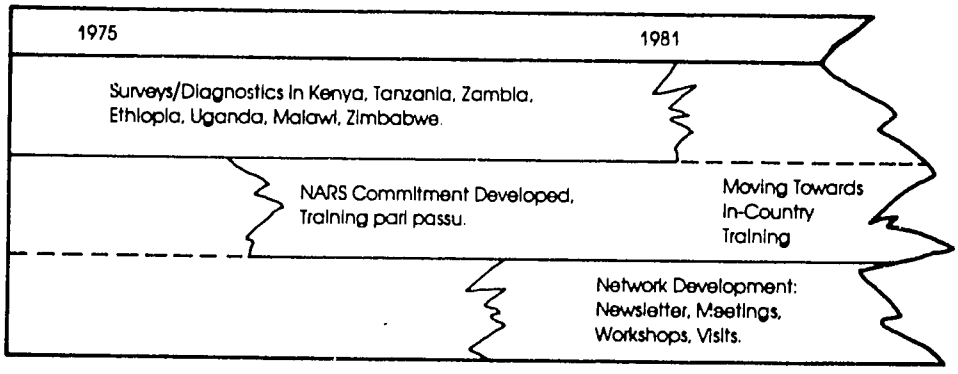


Figure 3.6: Training and Networking in OFR/FSP, Based Upon the CIMMYT East African Program





judgment, the ability to handle farm management data critically, comes only from experience and that the scientist with a smattering of economics is unlikely to arrive at that judgment. I incline to the latter view but add that some process of mutual education is needed; both scientist and economist must know enough of each other's fields to be sensible. Sometimes, a sort of osmosis by personal contact will suffice; sometimes, an explicit educational effort might be desirable (see below). In general, scientists, brought up in the conventional B.Sc-M.Sc.-Ph.D mill, will be more in need of economic understanding than agricultural economists (who can hardly avoid knowing something about agriculture) will be in need of scientific education.

So economics has come to stay. The position of anthropology is less clear. Anthropologists are inclined to argue (e.g. IRRI, 1982) that their skills in social understanding transcend those of economists and that they could therefore get under the crude questions of cash flow and marginal rates of return to the deeper social realities. Maybe, but one recalls the not altogether unfair stereotype of an anthropologist living in a village for years and emerging at the end with the view that the villagers are all splendid chaps who ought to be allowed to get on with agriculture in their own way regardless of the fact that the world around them will not allow them to do so. Thus, if there is a place for anthropology at all, I incline to the view that is for short, highly specific inquiries closely co-ordinated with agricultural research programs and directed at answering important questions beyond the reach of economics. The economic anthropologist rather than the strictly social kind would probably be the most useful in this context: there might be little to distinguish him from the economist with well-developed social perceptions. But any generalized adoption of social anthropology would be, I believe, merely an expensive way of avoiding a few, not very costly, mistakes by OFR/FSP teams.

To conclude this section, I refer to institutional arrangements for OFR/FSP work. As preliminary points, I note that: (1) OFR/FSP is hardly a decade old and the term itself was only coined a year ago; (2) it developed more or less independently in several different institutions and had to be fitted into structures and personalities that were already in place; (3) the predominant discipline and/or commodity orientations of most institutions (IARC and NARES alike) meant that OFR, essentially a non-traditional and multi-disciplinary activity, did not always fit easily into existing structures; (4) ad hoc arrangements have therefore dominated the scene.

So no consensus as to the "best" institutional arrangement has emerged and it is perhaps doubtful whether one exists. In practice, structures range widely from those (e.g., CIMMYT) in which an economics program runs the OFR work, drawing on specialist colleagues' knowledge as necessary, through those (e.g., ICRISAT) in which a multi-disciplinary team is drawn from several departments (including economics), to those (e.g.,CIAT, ILCA) in which economists are individually committed to membership of OFR commodity teams. I confess to a personal fancy for the last model, the commodity-team-cum-economist, because I believe that it should best promote the scientist-economist interchange that is so

necessary. But it may not always be applicable if the work is not focused on a commodity (as ICRISAT's is not). Again, what is one to say of a NFSD program such as that of IITA? It can not be commodity-orientated but must clearly be multi-disciplinary and include economics alongside the science (which it does).

I conclude that existing structures are diverse and likely to remain so. The only guiding principle evident is the need for integration of diverse disciplines - not a very surprising conclusion!

The need for integration and cross-discipline understanding is widely recognized but perhaps less widely achieved. I have certainly met specialist scientists who wanted to stay that way and had rarely been on a farm; also economists more interested in linear programming than in FSP. The problem is a real one and will not go away; blame, if any, attaches to the excessively specialized educational schemes adopted by universities in rich countries and copied elsewhere. The Ph.D. system, with its tendency to generate a series of near-replicas of the teacher in terms of skills and interests, is perhaps the most damaging part. There is certainly a case for debunking the Ph.D. as a general license to practice science though this, alas, is unlikely to happen. For the time being, anyway, the example of senior colleagues at working level is likely to be more potent than exhortation. Something could surely be done, by IARCs and NARESs alike, to ensure that young scientists see enough of farming systems on the ground to generate at least a modicum of FSP? If my earlier points about GE interactions (Chapter 2) be accepted, it will be evident that plant breeders are no less in need of FSP education than other scientists (which is not to deny that many are already well informed).

Relation to Extension

The importance of close links between OFR/FSP work and the local extension system has been emphasized by many writers and is referred to above. Given that there is an extension system this must be correct, but there are, I think, grounds for believing that the whole structure may be open to change.

First, OFR/FSP has itself a rather potent extension effect, as we are now just beginning to understand. Small farmers are far readier to take up and diffuse innovations than was realized as little as a decade or so ago. If OFR/FSP is practiced on a wide scale with numerous farmers, then useful changes go far and fast without any formal intervention by extension agencies; or so accumulating experience seems to say.

Second, extension agents on the ground tend to be young, inexperienced, and less knowledgeable than the farmers they are supposed to guide. This was, of course, the basis of the "training" part of the training-and-visit system of extension which is now more or less conventional wisdom. So farmers often do not take extension agents very seriously, but they do listen to researchers and senior assistants who clearly know what they are talking about; hence the evident efficacy of an OFR/FSP operation that really has something to offer.

Third, if the two foregoing points be accepted, there is clearly room for speculation as to the future place of extension. A somewhat cynical view is that, if there is nothing to extend, extension is helpless and that, if OFR/FSP is effective, then farmers will quickly know about it and adopt anyway. If this is even nearly true, then the future may lie with smaller but better-trained extension services working in the OFR/FSP area in close collaboration with researchers, leaving diffusion rather than exhortation to do the rest. Only time and experience can tell, I think, but this trend appears to be at least a possibility to be taken seriously. It amounts to saying that OFR/FSP shows signs of bridging the gap between researcher and farmer that it was the traditional function of the extension service to cross.

CHAPTER 4. WIDER QUESTIONS

Technological Change and the CGIAR/NARES Context

In Chapter 1 I enumerated the main causes and agents of technological change (Figure 1.4). Of the four agents recognized, farmer-generated change and the activities of commercial bodies selling products do not concern us here. There remain the rather wide body of changes deliberately generated by governmental-industrial agencies (Figure 4.1). Consider, first, the export-cash crops at the left-hand side of the diagram to which the following points are, I think, relevant.

1. They have a long history of industrial/governmental support by way of highly efficient research, development, and extension.

2. Some such programs have moved entirely into the hands of the industries themselves (sugar in the West Indies, rubber and oil palm in Malaysia) while others retain joint governmental/industrial interests (diverse crops in Brazil, India, the Philippines, and Indonesia, for example): thus, while NARESs, world-wide, have considerable commitment to industrial crops, the CG system has ignored them.

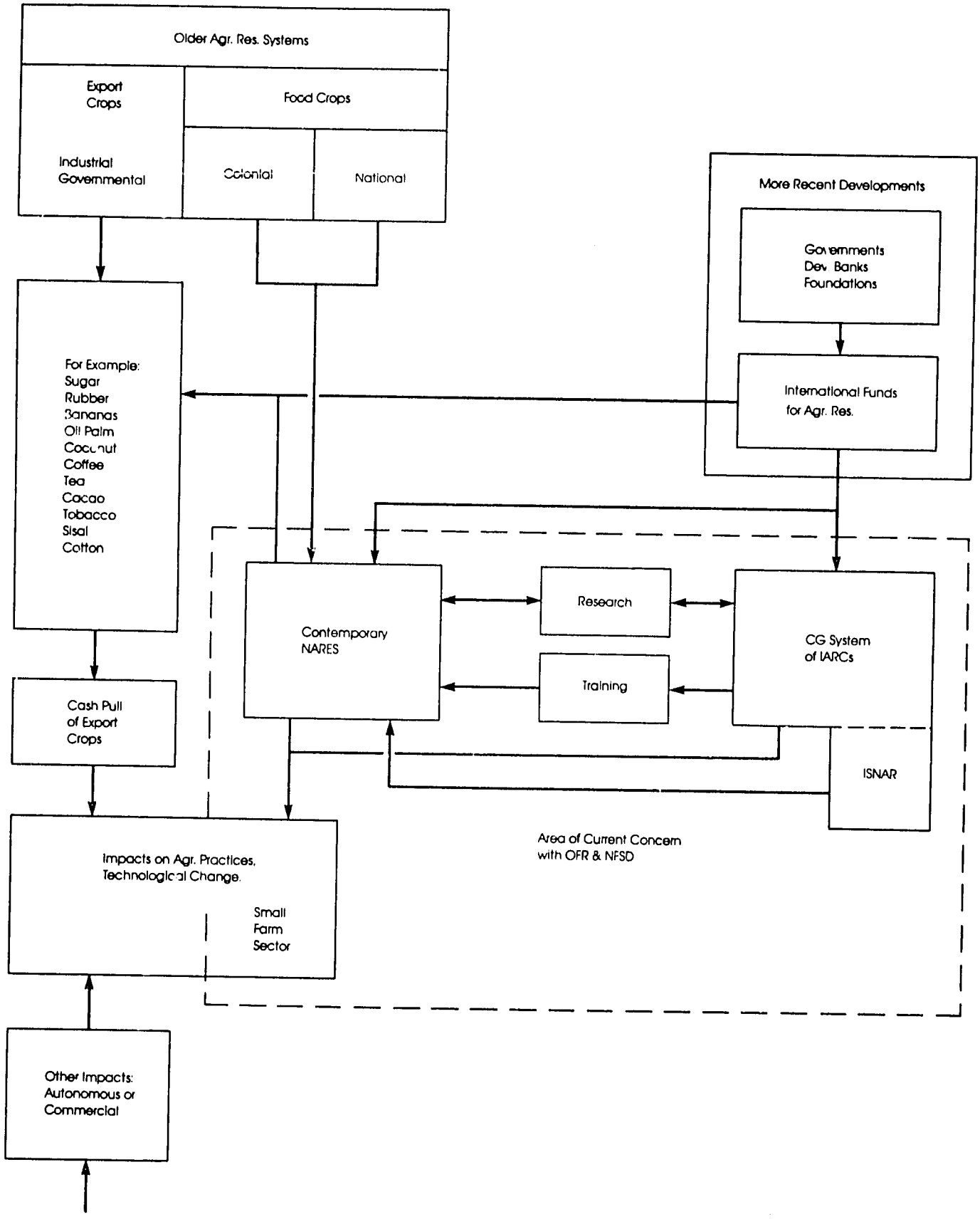
3. I know of no general historical assessment of the socio-economic importance of these crops but believe it must have been immense, in several ways: in drawing in external development capital; in generating local cash; in creating communications infrastructures of shipping, roads,and railways;and in demonstrating new agricultural technology transferable to other crops.

4. Plantation agriculture led the way and has been widely criticized (I believe over-criticized) for its exploitative features; but all the crops listed in Figure 4.1 have small landholder sectors, sometimes strong and long-established ones, of substantial local economic importance.

5. The technologies of growing such crops efficiently are well established, and transmission to growers must, of its nature, be rather a top-down process.

6. In the plantation sector, OFR/FSP is hardly relevant because the researchers live with the crop, FSP is not lacking,and competent estate managers practice OFR anyway (without calling it that); for small holders, industries commonly maintain specialized extension/advisory services but it is common experience that yields and quality are below what might reasonably be expected; maybe there is a place here for more OFR/FSP input?

Figure 4.1: The Wider Context of Tropical Agricultural Research



7. An efficient industrial-advisory base for the technology of the chosen crop can, in principle (and sometimes does in practice), have three beneficial side effects: it maintains a high level of farmer/adviser contact; it obviates what may be an inefficient bureaucracy in the supply of cash and physical inputs; and it makes possible some support of crops other than the leading one (as the Colombian Coffee Federation, I was told, is very effectively promoting cassava).

8. All-in-all, a good export-crop industry is an excellent focus for agricultural development.

9. The difficulties of finding reliable markets for export products are well known and are more political than technical in nature; but point 8 remains good in principle.

Turning now to the rest of the diagram (Figure 4.1), the food-crop sector, contemporary NARESs are often direct historical descendants of older colonial or national systems, though generally greatly expanded in the past 30 years or so. The CG system is both recent in origin and quite small in size, with expenditure of the order of 10 percent the amount spent on NARSs (Coulter, 1983). In my opinion, history will praise the CG system for having done four things, namely:

1. it recognized the need for centers of excellence in various areas of tropical agricultural research at the "strategic" level (not "basic" - no agricultural research is basic in any useful sense of that often misused word);

2. it recognized from the start that it would have to work through and help to build up NARSs (hence the invention of ISNAR);

3. it recognized, following point 2, that training and the development of networks were activities of central importance; and

4. it recognized that the traditional top-down approach to small farmer extension work was not good enough, so instituted the use of economists and the study of FSR ideas which eventuated in the subject matter of this report.

These are all excellent achievements though the praise should perhaps be a little qualified: thus IARC/NARES relations, though sometimes excellent, are not always so and FSP, though widespread and growing, is yet far from universal.

What then of the future of the CG system especially in relation to FSR <u>sensu lato</u>? The following points are, I think, relevant:

1. the NARSs are growing, and it must remain a major function of the IARCs to help them to do so effectively by scientific exchange, by training, and by networking;

2. as the NARSs grow, they must, for obvious logistical reasons, take over most of the large body of OFR/FSP work that will be necessary;

3. given point 2 and assuming that the principles of OFR/FSP are well enough established (as I believe they are), I can foresee no need for any substantial expansion of OFR/FSP work in the IARCs, indeed perhaps the reverse; it will be for the NARSs of adopt the OFR "style" (as many already do) and for the IARCs to back up the effort by their networking activities;

4. if my earlier analysis is correct (Chapter 2), FSR sensu stricto will have little or no place in the scheme of things and OFR/FSP will be mostly for the NARSs (point 3), which leaves NFSD for consideration; I believe this is an area that cannot be ignored and that it must lie in (but will not, of course, be confined to) the domain of the IARCs.

I think the last point is important, though I can offer no recipes as to how to do it. Certainly, much tropical agriculture, willy-nilly, faces profound change in the next few decades, and it were better, surely, that it were purposeful rather than haphazard. Workers in the area will have to forsake the comforting certainties of component research on annual crops and think of unlikely combinations of annuals, perennials, and animals; they may have to resurrect the unit/model farm idea and, in the end, try to persuade foundations, banks, and governments that they really have something worth trying. OFR/FSP, I think, excellent in itself as an aid to sensible step-wise change, does not lead in the right direction. Imaginative jumps are needed and, so far, no one has been willing to make them. They will be made only by workers able to transcend the customary discipline/commodity boundaries; in this I suspect that multi-disciplinary thinking may be more important than multi-disciplinary teams.

Agro-forestry and the Neglect of Perennials

Perennial plants, mostly but not all, woody, are of profound socio-economic importance in tropical agriculture and, broadly, the wetter the place, the more important they are. Under population pressure, trees have almost disappeared from huge areas that once grew them and remaining woodlands are fast vanishing. Locally, firewood gathering makes labor demands comparable in magnitude with those of agriculture. Elsewhere, as in the backyards of Southeast Asia and Central America and the banana gardens of Central Africa, perennial plants make major direct contributions to human nutrition.

With the partial exception of IITA (which has some work on bananas) the CG institutes do not work on perennials. I understand that work on tree crops has been considered but, so far, rejected. Perhaps the time has come for a review of the question? Trees, however, have not been universally neglected. Thus CATIE has a long history of taking perennial plants seriously and of integrating them into Central American farming systems (Combe et al., 1981; Sales, 1979); and the NARSs of India, the Philippines, and Indonesia are hardly less aware of their importance; all

devote substantial shares of their programs to them. Further, the IDRC inspired a prescient report (Bene et al., 1976) which eventuated in the foundation (1977) of the International Council for Research in Agroforestry (ICRAF). ICRAF has only very recently (say 1981) really got going, and the intention seems to be that it shall operate world-wide in collaboration with local institutions, both IARCs and NARSs (ICRAF, 1983; MacDonald, 1982; Lundgren and Raintree in Nestel, 1983; Raintree, 1983). There will be institutional, even psychological, difficulties, of course. Many agriculturists think of trees as things which should be confined to forests or, perhaps, hedgerows, while many foresters regard agriculture as a competitor, a despoiler of woodland or, at best, as a transient phase in the re-establishment of forest (taungya). These views are, happily, by no means universal and there are welcome signs of convergence of interests in the general area of FSR and forestry, evidenced, for example, by the extremely interesting review by Burley and Spears (1983).

From the FSR point of view, there are three points to make, as follows:

1. OFR/FSP, concentrating upon improving the yield of maize or cowpea or whatever, could quite well note that trees were there (or not there) as a matter of FSP, but thereafter ignore them, unless they were in some sense critical for the performance of the chosen crop (maybe yam poles are required or sheep have to be cheaply confined);

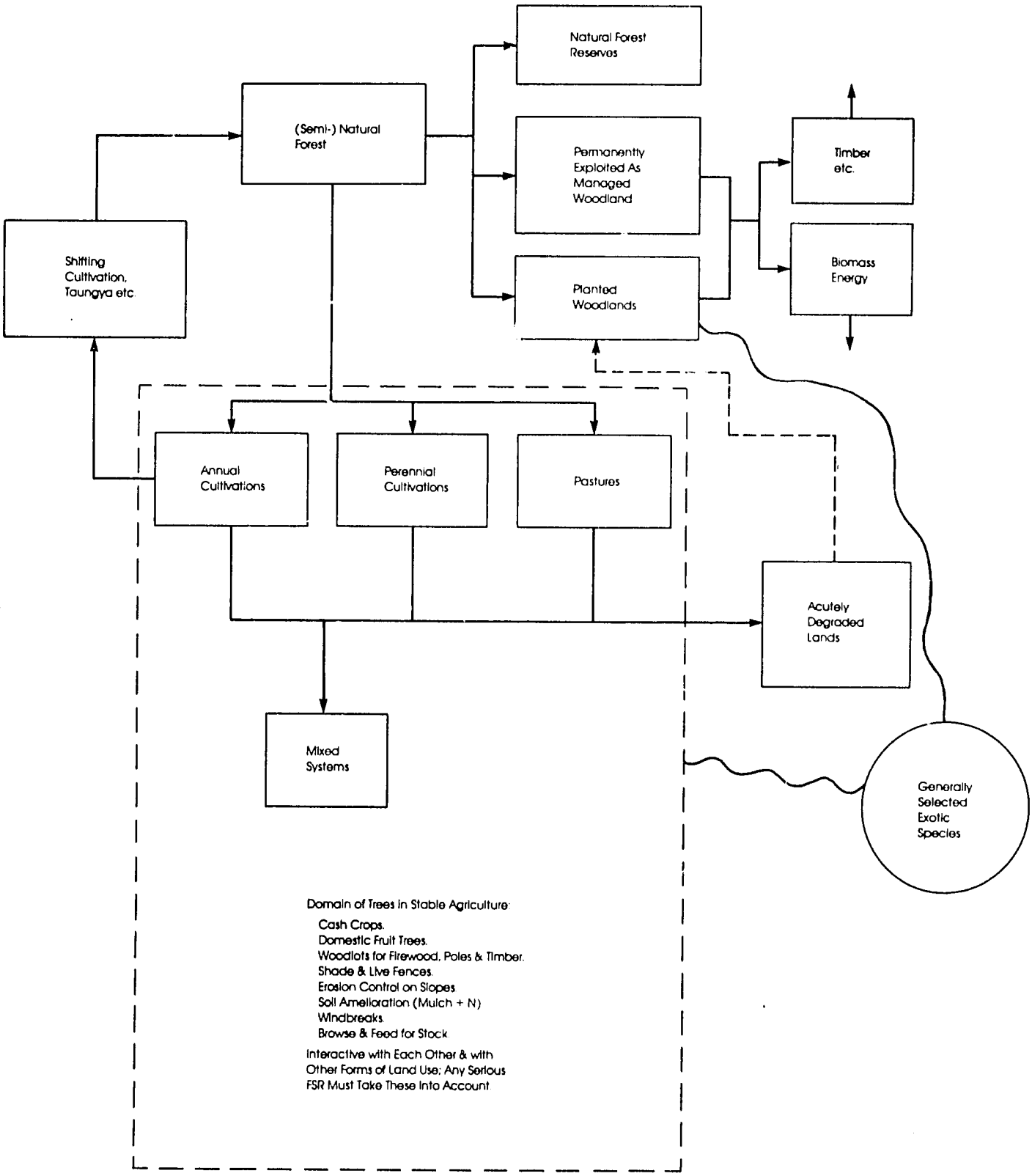
2. at the other end of the scale, FSR sensu stricto would include the trees (or lack of them) in its description but would provide no practical prescription as to what to do next;

3. the agro-forester would be inclined, I think, to perceive critical local needs for trees for various purposes (fruit here, firewood there, fence posts and browse in the next place); but he would be constrained by mandate to "trees in agriculture" - he is not in the NFSD business.

I conclude that agro-forestry is likely to make valuable contributions to tropical agriculture, provided that its activities can be operationally integrated with those of agricultural research; but that it is no more likely than conventional agricultural research itself to arrive at what is surely needed, namely, practical and testable visions of NFSDs for the wet tropics on poor soils.

To pursue this vision, I note that most of the items in the box of Figure 4.2 might well be involved: a patch of rubber, cacao, or citrus for sale; a mixed patch of breadfruit, avocado, citrus, mango, pechibaye, coconut, other fruits for home consumption; bananas (which are "trees" for this purpose) everywhere; woven legume fences to contain the animals; hedged contour beds; mixed leguminous alleys pruned for mulch, browse, and

Figure 4.2: The Place of Trees



feed; annual crops in the alleys; poles and firewood from the alleys and hedges, and maybe also from a special Eucalyptus or Gmelina woodlot; the soil as nearly as possible invisible, almost untouched but probably a little fertilized. There may be a few small farms like this here and there (in the Philippines or Indonesia perhaps?) but they cannot be many.

The potential diversity of patterns of this kind is virtually indefinite and only extensive on-farm experience could tell which, if any, were workable. Some of the elements of the technology are available, of course. IITA and ILCA (see Chapter 5) have the beginnings of understanding of leguminous alleys; the soil-conservative effects of tree cover and mulch in the wet tropics is established beyond doubt, as well as the nitrogen-supplying capacity of leguminous trees or bushes. But we are essentially ignorant, I think, of the productive capacity and economics of mixed stands of perennial food-fruit plants. As Watson (1980) points out in a valuable compilation, perennial crops are there all the year round and must have high biomass potential, but we know very little of partition ratios; we do know, however, that breadfruit and bananas are virtually non-seasonal and (I should suppose) as productive as tuber crops but less labor-demanding. There is a long way to go, not only in understanding the food potential of the perennials but even in introducing the species around the world and getting them known; many (maybe 100?) species, each represented by diverse genotypes, should be very widely spread round the wetter tropics as a preliminary to local selection, followed by purposeful breeding as local needs emerge to view. There is here an immense and almost wholly neglected task.

I believe that any serious development of the NFSD, so sorely needed in the wet tropics, must be built around a far better understanding of the perennial crops (and other trees) than we yet have; they have been terribly neglected. This view is growing (e.g., Harwood, 1979) but is hardly yet widespread. I conclude this section with an apposite quotation from a masterly recent review by Watson (1983), which should be required reading for anyone concerned with tropical agricultural research:

> Under these circumstances, prospects for the rural poor are grim and there is a great deal to be said for the suggestion that the development of tree crop farming systems is one area where aid programs should be directed, rather than continuing to channel capital and expertise (together with subsidised food imports) into the urban-industrial complex....Tree crops are needed: for the fruit, nuts, oil, rubber, cocoa, coffee, pulpwood and timber they provide, for the rural industries they support and for the protection of water catchment areas and regeneration of abandoned land. Not least, they offer shade and protection against the tropical climate, and can help to bridge the gap between rural subsistence and urban affluence. They protect the environment, offer a diverse and steady income to the subsistence farmer and a source of amenity and recreation for the city dweller. If the social value of mixed tree crop systems were added to their more easily-quantified direct returns, they would undoubtedly be seen as deserving a greater allocation of the development resources now available.

The Interests of the World Bank

The Bank spends money on tropical agricultural research in two ways: in its support of the CG system to which it is a major donor of funds (12 percent in 1983); and in its agricultural/rural development projects in various countries. In this section I make some observations on how the Bank's spending might be influenced by FSR ideas. First, I state three premises which have emerged from preceding sections of this report:

1. FSR sensu stricto is irrelevant (except to the extent that a little of it provides the necessary FSP);

2. OFR/FSP is now a well-established and effective adjunct to research that aims to make modest step-wise changes in established farming systems; methodology is secure enough and the "style" has come to stay;

3. the longer term need for NFSD is implied in many systems (Chapter 5) and there is acute need of it in the lowland wet tropics, where any sensible development is likely to be based upon the perennials, of which our understanding is yet very deficient (see above).

Following these premises, I think the Bank should, in relation to its CG responsibilities, help to encourage the CG institutes:

1. systematically to develop FSP among its researchers, use OFR/FSP methods themselves when they are relevant, and enhance training and networking for their NARS colleagues;

2. to take perennial food crops and other trees, especially in the wet tropics, really seriously, in a deliberate attempt to develop the NFSD which are so sorely needed.

In relation to its own agricultural development projects, the Bank's interests can not, I think, be quite so concisely defined. I have the impression that agricultural development projects at large (not only the Bank's) have not, in general, been spectacularly successful. Broadly, such projects have usually had three components: infra-structural (roads, land clearing), supplies (fertilizers, planting materials) and extension (with recent emphasis on training-and-visit methods). The approach has usually been of a package of practices delivered top-down and time scales usually short (say five years). I am in no position to analyze the causes of such failures as have occurred (cf. Chapte) but have encountered a growing belief among experienced people that top-down "packages" ill adapted to local circumstances have been a significant element. This seems to me to be at least likely. I note that the three current agricultural development projects in the savanna zone of Nigeria have all introduced research elements with explicit OFR/FSP components into their programs and are convinced of the good sense of doing so (cf. Chapter 5). I understand (non-authoritatively) that Bank thinking is tending to replace the traditional five-year project, founded on a pre-determined

package, by longer, more cautious, flexible, phased schemes that embody a research component with OFR/FSP in it. I think we now know enough of OFR/FSP to assert that this seems entirely sensible in areas where step-wise enhancement of the agriculture is feasible. Collinson (1984), it seems, would take the idea still further and would make the formulation of an agricultural development project conditional upon a successful preliminary OFR/FSP study, and he points out that the OFR economist would then be singularly well placed to conduct any subsequent monitoring-and-evaluation operation.

It is hard, however, to see such ideas working very well in shifting/bush fallow areas in the lowland wet tropics where the opportunities for step-wise improvement are severely constrained and massive land clearing leads merely to disaster. Here, the Bank can hardly evade confrontation with the problems of NFSD. But, due to our lamentable lack of understanding of perennial food crops and the use of trees (see above), working NFSDs can not yet be defined and it may be decades before the knowledge is available to do so really sensibly. In these circumstances I believe the Bank should be adventurous, try to short-circuit the missing research, put some groups of unit farms (on a best-guess basis) on the ground, and expect to lose money. Even if they more or less failed, the FSP feedback to the researchers could be of great value. It could be argued that deliberate financial imprudence should have no place in a respectable Bank's activities, but the cash at risk would be small in relation to total spending and the gains potentially great if the technical and economic issues were clarified.

I conclude that, in relation to its agriculture development projects, the Bank should:

1. continue to introduce a research element, with OFR/FSP, into schemes in which step-wise change is intended and phase those schemes in such a way that objectives can be adapted to improving knowledge;

2. make some bold, if financially imprudent, efforts to generate NFSD in the lowland wet tropics, on a best-guess basis, in the hope of at least partial practical success, of short-circuiting the research process, and of generating FSP of the problems involved.

CHAPTER 5. EXAMPLES

Beans in Highland Colombia

Phaseolus beans, a major element in the CIAT research program, are a very important food crop in upland tropical America. Some 50-70 percent of them are climbing beans grown on maize stalks, so intercropping is the rule. They are an important (often the leading) cash crop for small farmers. Though several main cropping systems can be recognized (e.g., four in Colombia: Table 5.1), there are many local variations (e.g., climbing beans on poles and wires and bush beans for green vegetables locally in the eastern Antioquia area).

I saw something of the OFR work by CIAT/ICA in the Antioquia province, near Medellin. OFR/FSP methods are essentially identical with those of CIMMYT. Antioquia is high and only one crop a year is possible. The beans are rotated with potatoes (or vegetables locally) and are relay-planted into maize. The maize is broken over at maturity to form a fence upon which the beans climb and the cobs usually dry off in the field, to be picked as needed. The area presents an interesting example of farmer innovation, from fairly crude potato-maize-bean mixtures 50 years ago to an orderly rotation-relay cropping about 1970, though at low yield; bean yields are estimated to have nearly trebled in the past decade as a result largely of fungicide use and some fertilizer application. The beans are essentially a cash crop and only a mottled pink bean, like the current land-race variety Cargamanto, is acceptable to the market (though trade-offs are conceivable: cf. observations on Figure 2.1).

The ICA/CIAT OFR team concentrates on step wise improvements of agronomy (which tend to be interactive with variety). The breeders have had the oft-repeated experience of breeding varieties that were excellent in station trials but unimpressive on-farm. Stability of yield is clearly (and explicitly) very important to the farmer, and there is an interesting suggestion that the genetic heterogeneity of the local land race (Cargamanto) somehow contributes to it; at least, pure lines isolated from Cargamanto seem less reliable over sites and seasons. So the CIAT breeders are considering the possibility of deliberately retaining heterogeneity (which seems to me to be an excellent idea: purity is often over-valued). In their station plots, the breeders grow and select their beans on maize under diverse cropping regimes. To the farmers, the maize is relatively unimportant so long as it yields tolerably and holds the beans up. A tall local variety at wide-spacing is used and stalk strength is crucial. Shorter, modern maizes at high density, with thin stalks, are unsuitable (especially if susceptible to Helminthosporium). Perhaps maize breeders somewhere should do the complementary selection and pick varieties tolerant of beans hanging on them?

Local uptake of new methods seems to be good, and there is no doubt that the farmers will be receptive to good new varieties when they emerge (CIAT beans are being adopted elsewhere). There is an incipient training-network scheme in hand (Woolley and Pachico, 1983). Sanders and Lynam (1982) comment upon the CIAT bean and cassava OFR programs.

Table 5.1. BEAN-CROPPING SYSTEMS IN COLOMBIA

Element	(1) Northern Narino	(2) Central Narino	(3) Eastern Antioquia	(4) Southern Narino
Alt. (km)	0.9-1.5	1.8-2.3	2.0-2.3	2.4-2.9
Period (months)	3-4	4-5	5-6	8-9
Crops per year	2	2	1	1
Rotation	none	peas	potatoes or vegetables	potatoes or barley
Plant habit	bush or semi-climbing	bush	climbing	climbing
Association	row intercrop with maize	monoculture	relay intercrop with maize	associated with Vicia, and urbits
Technical Index*	23	64	226	145

* A round-figure measure of the technical inputs already used by farmers.

Source: Woolley and Pachico, 1983.

Maize Production Methods in Panama

Several examples of CIMMYT's OFR/FSP work in diverse countries in Latin America and Africa are outlined in CIMMYT (1981). In this example I briefly describe the IDIAP (Panama) program in the Caisan area, 1978-82 (Martínez and Arauz, 1983; Martínez and Saín, 1983 [economics]). Survey revealed two recommendation domains of which one was chosen. Spacing, weed control and fertilizing were identified as probable limiting factors to maize production.

The first season of OFR work concentrated on factorial experiments on these factors and results suggested that higher planting density, better weed control (by herbicide), and less fertilizer would be favorable. The second season therefore restricted the treatments sharply but added the idea of zero tillage (reasonable in the context of an erodible soil, labor shortage, and herbicide usage). The third season was devoted to verification trials of the package: no fertilizer, zero tillage, higher density, herbicide. Farmers proved keen and made highly practical suggestions for enhancement. Uptake was excellent and, by the end of the study, 60-80 percent of some 300 farmers had adopted (in rather varying degree). Fortified by the experience, the NARS (IDIAP) has greatly expanded its OFR/FSP activities.

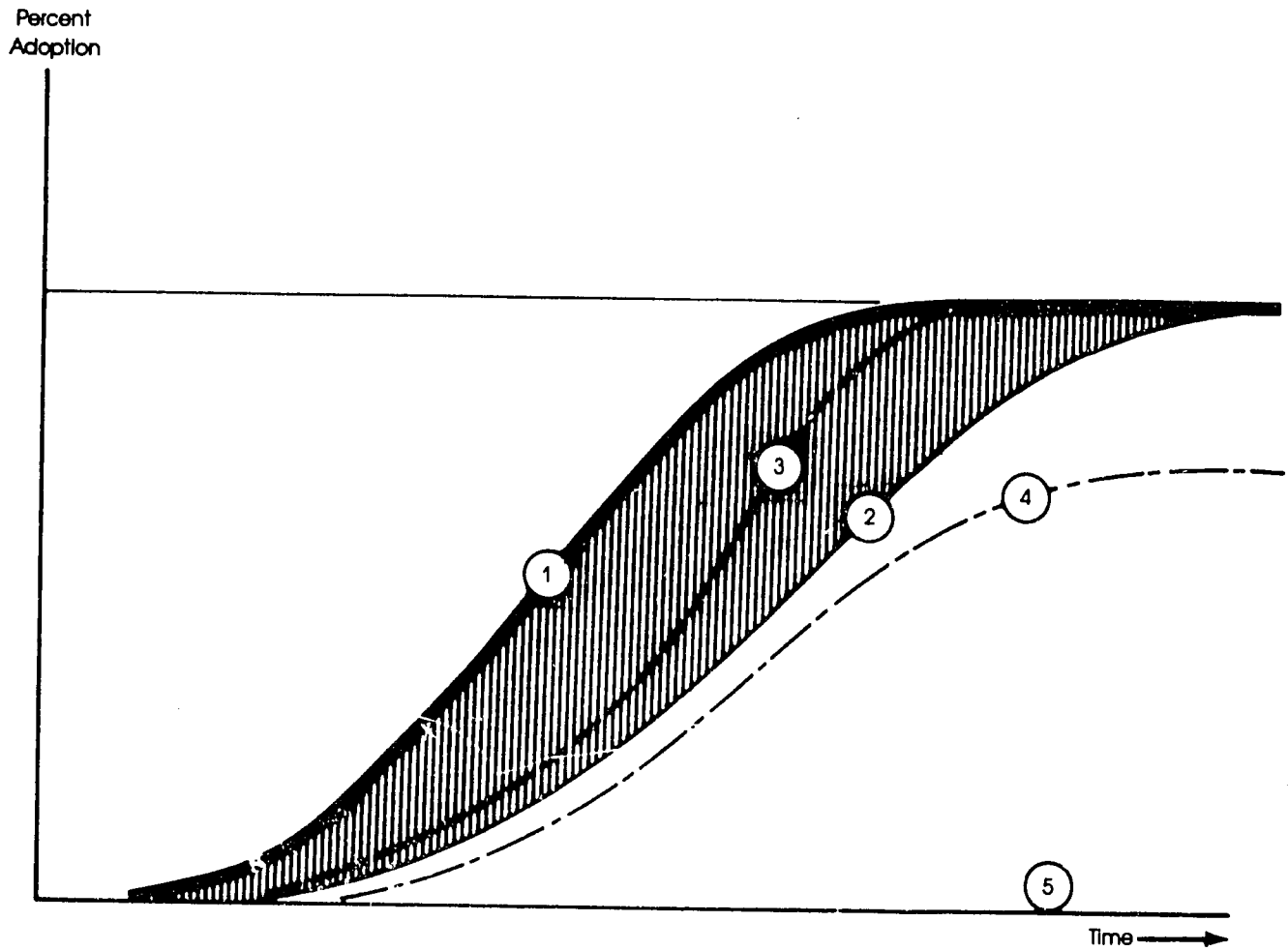
Martínez and Saín (1983) provide the only example yet available of an attempt to examine the economic consequences of OFR work by cost benefit analysis. Their analysis shows pretty convincingly, I think, that B/C>>1. Costs for 1978-82 attributable to the OFR work amounted to about 78 k$. They used Griliches's methods of estimating consumer surplus (though methodological doubts attach, as to all cost-benefit analysis) and compared the cost flows with the flows of returns estimated in the presence of OFR with hypothetical returns at lower rates of farmer uptake had OFR not been available to speed the process. The last is the most doubtful bit (Figure 5.1) but a range of reasonable assumptions all showed substantial net benefits, with the most likely B/C ratios> 10 and (the Griliches) rates of return around 200 percent. The authors used a discount rate of 15 percent and assumed (reasonably, I think) that CIMMYT experience was an external "free good." I hope that no one will now say that OFR must be a good thing because it yields a social rate of return of 200 percent upon investment.

NFSD and OFR/FSP in Nigeria

NFSD work at IITA

The problems of replacing shifting cultivations or bush fallows by something more stable and more productive have been recognized for decades, even though they are workable (even more or less stable) systems at low population density. Very similar agro-climatic problems are, of course, faced in the wetter parts of lowland tropical America and southeast Asia (see below).

Figure 5.1: Cost-Benefit Study of Cimmyt/IDIAP OFR/FSP Work on Maize in Panama



es: Social rerums (as economic surplus) are estimable for each curve. Curve (1) relates to OFR as it was applied. Curves (2)—(5) represent ideas as to what might have happened in the absence of OFR. Something like (2) seems the most plausible. Benefits attributable to OFR are estimated from areas between curves. Curve (5), the abscissa, represents the extreme case of no adoption (not considered by Martinez & Sain, 1983).

FSR sensu lato has been an element of the IITA mandate from the start. It has been interpreted as, essentially, a NFSD problem, which it surely is. Accordingly, the Institute has devoted itself primarily to component research, recently authoritatively summarized by ter Kuile (1983) as: (1) land-development methods and forest-clearing techniques; (2) minimum or zero tillage, mulching, and fertilizers; (3) varietal improvement, especially in disease and insect resistance; (4) mixed and relay cropping methods; (5) agro-forestry systems, especially alley cropping; (6) integrated pest and weed management; (7) appropriate mechanization and farm energy development; (8) integration of livestock into farming systems.

Present practices are generally agreed to be under acute pressure and of declining productivity as cycles shorten and soils are "mined" (cf. Ruthenberg, 1980, Figure 3.5; Greenland and Lal, 1977; Lal and Greenland, 1979). They are summarized here in Figure 5.2; Figure 5.3 presents a fairly imaginative interpretation of what an emergent NFSD might look like.

IITA is certainly thinking of NFSD. Apart from some survey work, the Institute has hardly been on-farm in the CIMMYT/IRRI sense but more OFR is planned for the near future (Ngambeki and Wilson, 1983) and training-networking activities are in preparation. IITA workers are inclined to think (and I agree with them) that, of the available component technology, alley cropping is probably the most promising starting point (Kang et al., 1981). The ILCA group at IITA already have substantial (and seemingly fairly encouraging) experience of alley cropping on-farm in the area, primarily with a view to feeding small animals (Sumberg and Okali, 1983). No doubt the two groups will have much to learn from each other.

Even if alley cropping goes well on-farm, it can hardly suffice in itself. Any effective NFSD for the low, wet tropics must surely be more complicated. If such is to be developed into practice, it is hard to see how it could come about except with suitable government intervention by way of model (unit) farms, credit, and infra-structural development. Unit farms have indeed been tried by IITA (in the context of wet bottomlands: Menz, 1980). A key element may well turn out to be land tenure: communal land tenure is not conducive to improvement. Herein probably lies the difference between much of Africa and Asia. In Southeast Asia, in similar physical environments but with private tenure, there is already much static (as against shifting) agriculture and some prospect of step-wise advance (see below). Nor are examples of static agriculture in humid Africa wanting: the great banana-based cultivations of East Africa (Jameson, 1970; Arnold, 1976) may owe something to soils and altitude and are probably not a general model for development on low, moist areas of degraded land; but they suggest bananas as a basis for development and serve to remind us, yet again, of the extraordinary neglect of perennials.

The key questions before IITA seem to me to be: (1) the extent to which step-wise advances are possible and the use of OFR methods in generating them; and (2) whether one or more socio-economically feasible NFSDs can be invented and proved in practice. The bigger question remains: whether African governments will have the power and will to use appropriate NFSDs to counter what is generally agreed to be already a nearly critical food-supply situation.

Figure 5.2: Generalized Shifting System of Humid and Subhumid Africa

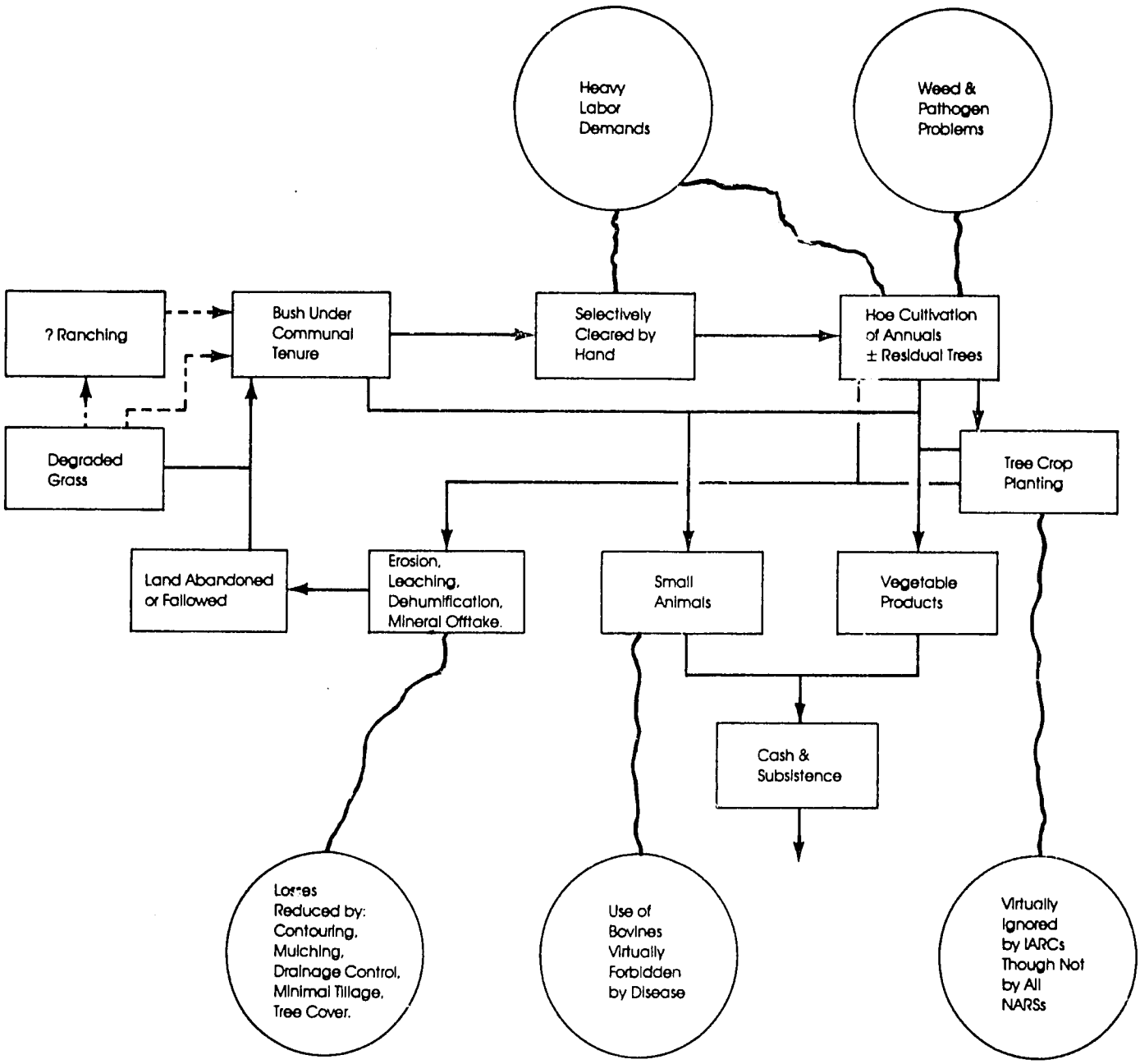
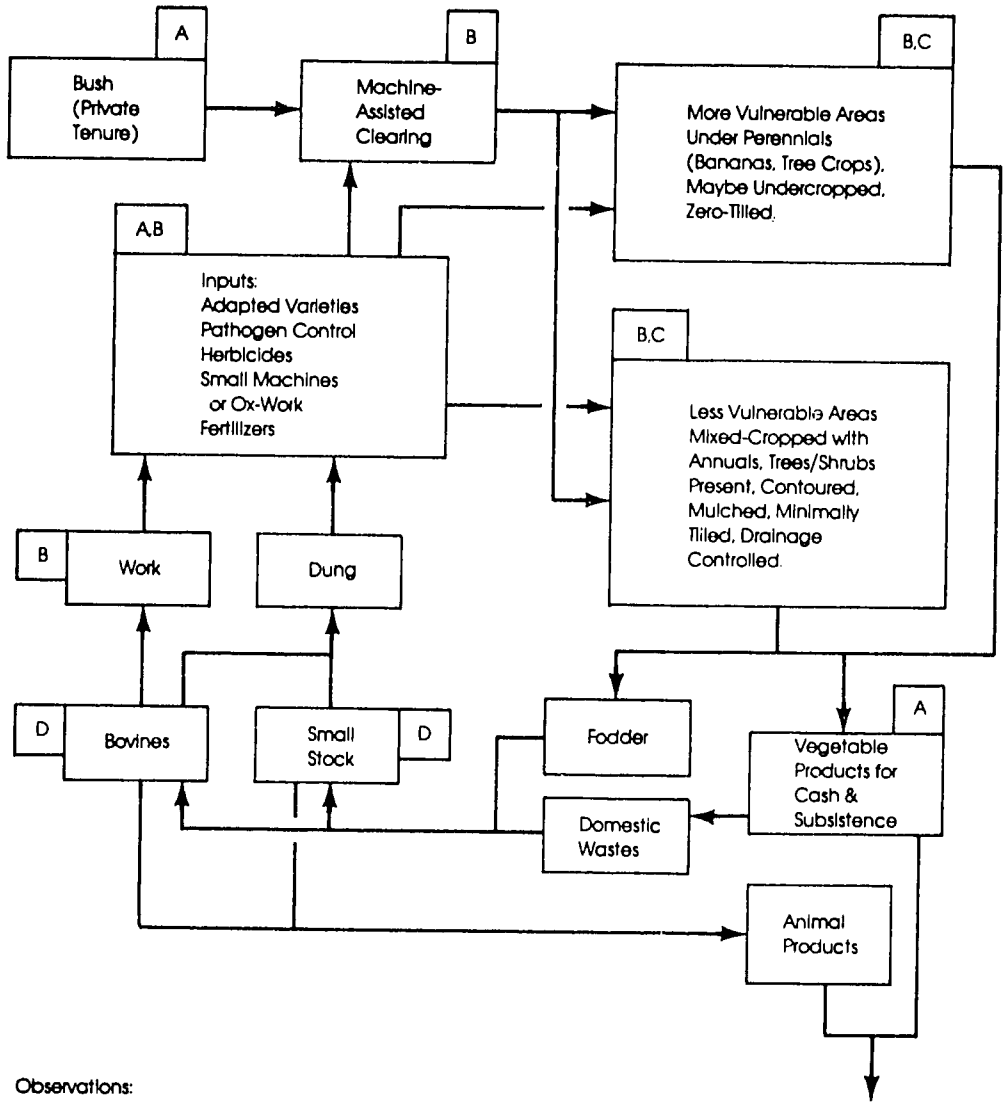


Figure 5.3: A View of Possible Stable Farming Systems in Humid/Subhumid Africa



Observations:

A	Social Change in Land Tenure, Cash Available, Markets Secure.
B	IITA Work Relevant
C	ICRAF Work Relevant
D	ILCA Work Relevant

OFR/FSP in the Nigerian Savanna

If the problems before IITA in coping with NFSD for the lowland wet tropics appear to be peculiarly intractable, those of the drier savanna zone of Nigeria appear, to many observers, to be somewhat less so. At least, there seems to be some prospect of immediate step-wise improvement by OFR/FSP methods. The context is, not of IARC/NARS research, but rather of the pressing practical problems of agricultural development projects already fairly far advanced. Consortia consisting of the Nigerian Government, state governments, and the World Bank have three major agricultural development projects in the center and north of Nigeria, in Kwara, Niger, and Onde States. Activities are co-ordinated by the Federal Agricultural Co-ordinating Unit (FACU) in Ibadan. All three have experiment stations and all run OFR/FSP activities, using their own agricultural research staff working in collaboration with the NARS and with IITA economists. The work is on a very considerable scale (the Kano project has 800 collaborating farmers) and is primarily, though not wholly, directed towards small farmers by way of infra-structure, supplies, and extension.

A substantial part of the work is directed towards the management of valley bottoms, or fadamas. These are, in total, very extensive in the savanna zone and offer patches of land which, given some modest water control, are potentially rather productive; at least they are better watered and more fertile than the dry sandy areas around them. Large irrigation schemes are not in prospect but much can be done, it seems, with quite modest resources. There are problems, of course, for example: locally high concentrations of ferrous iron, indicating the need for accurate drainage or plant (rice) breeding for tolerance of iron toxicity, or maybe both. I did not see anything of the OF work in the field in the fadamas or in the dry part of the Kano project area but understood that the philosophy adopted was to minimize the formal researcher-run experimentation and to go as quickly as possible to numerous, simple, widely dispersed farmer-managed trials.

I saw something of the OFR work in the Ilorin area of the Ilorin agricultural development project. The project is aimed primarily at enhancing the agriculture of about 60,000 small farmers in about one-third of Kwara State. The OFR work is backed up by a research station used for trials and seed production. The local farming system is a bush fallow one in savanna woodland and is based on a two-year cycle of cropping repeated two or three times, followed by about eight years of fallow. A common cropping pattern is early maize, relayed with sorghum, relayed with yams grown on a trellis of sorghum stalks. The terminal yam crop is often replaced by cassava. Cowpeas used to be important in the area and are much favored for food and cash but have declined due, it is thought, to a combination of recurrent drought and insect pests (the farmers agree). The OFR work concentrates on new varieties of maize, sorghum, sweet potato, cassava, cowpea, soybean, and okra and on spraying cowpeas with insecticide. Ultra-low volume spraying looks promising (water is scarce) and is likely to be just within the economic reach of many farmers, either on their own or by contract. They took very well the point that, sprayed early, cowpeas promised to restore a crop that they valued highly. They

liked the IITA cassavas with mosaic and bacterial blight resistance (their own varieties looked terrible and the IITA material was being enthusiastically passed on to neighbors), insisted that any new sorghum had to be tall in order to hold the yam vines and liked the idea of streak-resistant maize (it had been a very bad year for the disease locally). In general, the farmers we saw were critical and receptive, and I do not think there can be any doubt that the Ilorin OFR work is contributing materially to the progress of the project. As I understood the situation, the Ilorin project is something of a pioneer in this area of work and was the first deliberately to seek (from IITA in 1981) collaborating economists to help to design the program and monitor progress (cf. Chapter 4).

Rainfed Farming in the Medlterranean

The ICARDA mandate contains specific reference to the adoption of an FSR approach. Studies in the early years of the Institute (1977-80) of Mediterranean farming systems have been summarized by Gibbon (1980, 1981) and by Martin (1981). These authors present fairly detailed studies of samples of rainfed arable farming in the area, concentrating mostly on specific villages. The general approach might be described as FSR sensu stricto but with the explicit objective that others should identify research needs and develop OFR work from them.

Rather broadly, four main kinds of agriculture are recognized: (1) nomadic or semi-nomadic herding of sheep and goats on steppe grassland; (2) arable rainfed farming on deeper soils in moister areas (fallow-wheat or fallow-wheat-legume rotations with orchards - (olives, vines, figs) - as the main cash source); (3) arable rainfed farming on shallow soils in drier areas (fallow-barley, with sheep, often grazed on the immature barley, as a main cash source); (4) irrigated farming, in which, of course, very diverse cropping is possible. There are important interactions between (1) and (3) whereby sheep move seasonally between steppe and arable, with increasing reliance on stock the drier the local climate. Rainfall unreliability is a major source of economic risk to farmers. There has been considerable mechanization of arable crops and consequent rural unemployment.

OFR work is in hand at ICARDA (some, at least, has been going since 1977 and is being developed - ICARDA, 1982) but I find it hard to judge from the literature the degree of commitment of the Institute and its associated NARSs to the general OFR approach (which may not have much to offer the rich irrigated farmer but surely must be relevant to the small man on the dry land).

Mixed Upland Farming in Ethiopia

Ethiopia is a large country with diverse agricultures (systematically described by Amare Getahun, 1978, 1980). ILCA, with a farming-systems commitment in its mandate, has done a good deal of survey work on Africa, pastoral systems in general (ILCA 1980, 1983) and, in its highland program in Ethiopia, has concentrated on the dominant mixed upland systems of the plateau. Like the Deccan of India, the soils are a

complex mosaic of less fertile sandy soils on the (often) gentle slopes, with more fertile but ill-drained black clays in the bottoms. In Ethiopia, the sandy soils are arable and the clays provide seasonal pasture for milk cows, work oxen, and miscellaneous small animals. (Diseases are little problem in upland Ethiopia, unlike most of lowland Africa.) One form of the system (there are several, of course) is outlined in Figure 5.4 which applies at Debre Zeit, an ILCA station and on-farm site near Addis Ababa. The farmers are small (about 2 ha each) and operate a complex land tenure system, with common pasture in the bottom and cropland rotating among members of an "association." Tef (Eragrostis) is the preferred cereal here (wheat elsewhere), despite its low yield and difficult harvest: its retention is determined jointly by a strong market preference, by its tolerance of waterlogging, and by the facts that its seeds store well and that its straw is good for fodder. The pulses are peas, horse-beans, and chick peas, and it is noteworthy that it is these that the farmers chose to sacrifice to grow fodder under the ILCA scheme. The ILCA program started with surveys and proceeded to OFR work using the ideas sketched in Figure 5.4. The key notion is to replace numerous small work oxen and cows by fewer, larger, hybrid beasts and feed them better. An extension of the idea (yet to be developed far in practice) is to abolish oxen and use (well-fed) milk cows for draught. There is certainly a local market for extra milk and the local farmer-adopters around Debre Zeit seem pleased.

The farmers are good stockmen and take well to the Boran x Friesian crosses that have been tried. Dr. G. Gryseels tells me that average milk yields (liters per year) are about:

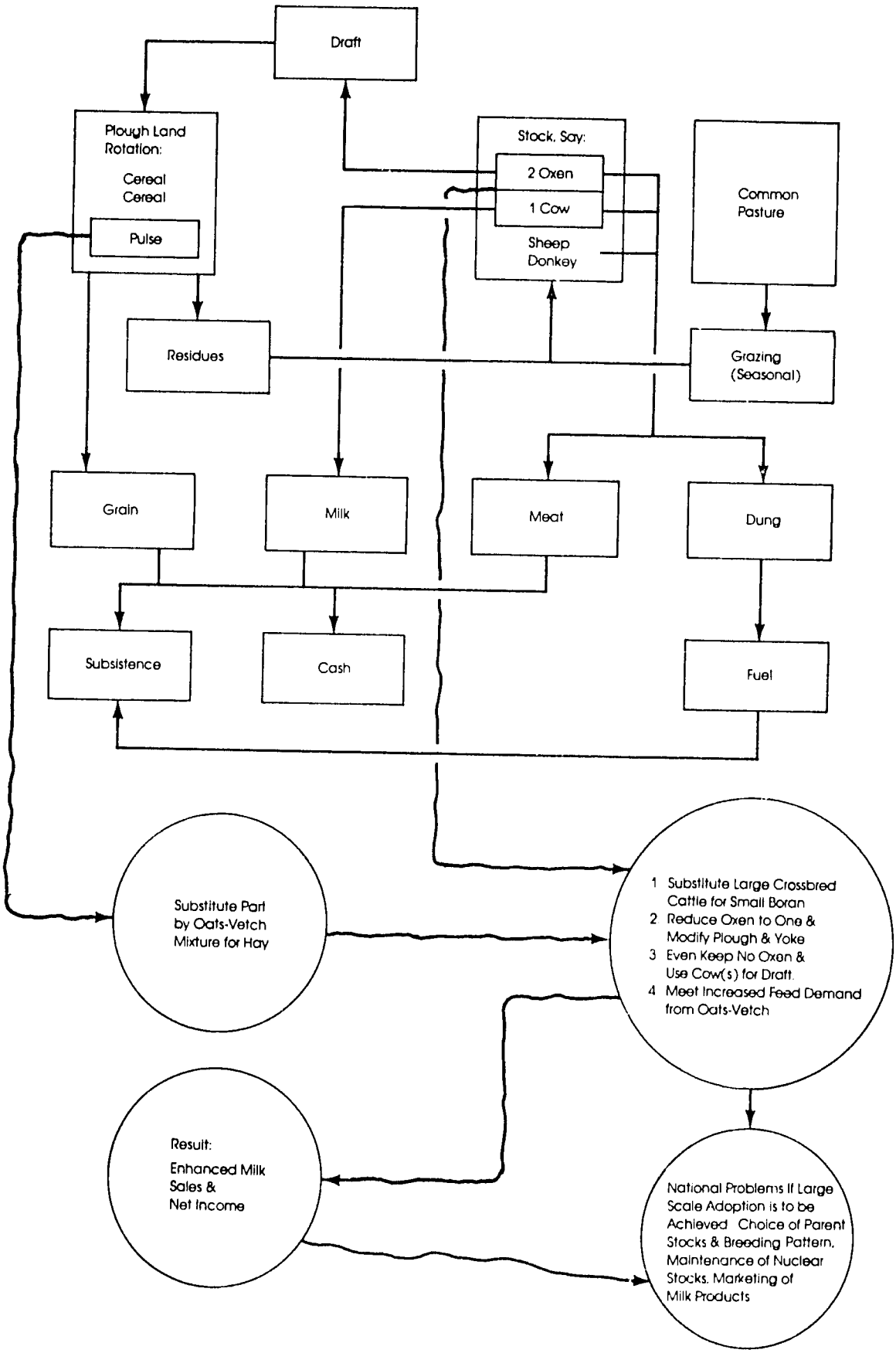
	Local (Boran)	Cross-bred (B. x Friesian)
On-farm	300	1,500
On-station	600	2,800

so a spectacular increase in milk yield and a clear GE effect are evident.

The ILCA scheme looks attractive but implications for the NARS are daunting. First, there are 30 million cattle in Ethiopia so, even if the numbers declined, there would be a gigantic breeding demand. Second, the best breeding plan has yet to be determined (50:50 Boran/European or perhaps a rotational Sahiwal/European crossing program?); whatever is chosen could be costly and would have long-term implications. Third, though there is now a market for more milk, greatly increased supply would probably imply a need for uptake in other than liquid form and, again, government intervention. Though established by OFR methods, this is not, I think, an innovation that the farmers, unaided, can themselves exploit.

Timber is generally scarce in the plateau but trees grow well. This is an obvious area for agro-forestry, with the attractions of supplying local timber, fuel, and fodder and of saving dung for fertilizer.

Figure 5.4: ILCA Proposals for Mixed Upland Farming in Ethiopia



It is interesting to reflect that it was ILCA's mandated preoccupation with animals that led them in the direction they have taken. I suspect that if ICRISAT had looked at the plateau of Ethiopia they would have seized upon the relatively fertile black soils in the bottoms, drained them for cropping, and banished the stock to the sandy slopes. This thought is in ILCA's mind, and it has been found that drained beds on the black clays do indeed seem to be (at least locally) workable and productive. But ILCA is presumably debarred by its mandate from proceeding far with the idea. To invert the system thus, so to speak, would certainly be to propose a NFSD.

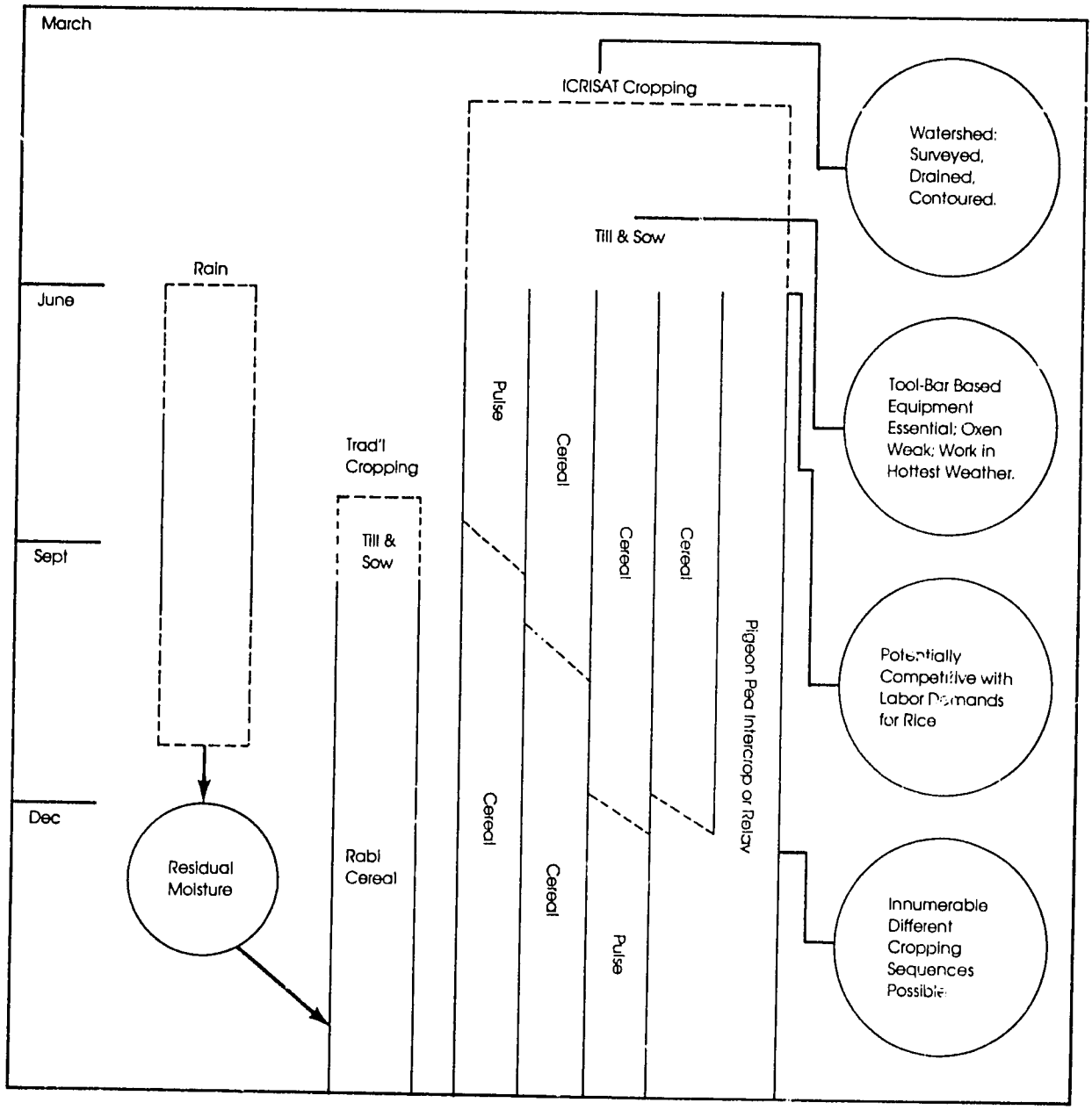
Cropping Systems in the Deccan of India

ICRISAT, from the start, was committed to FSR *sensu lato* and it conceived this in terms of NFSD; in a recent authoritative summary, Dillon and Virmani (1983) use the phrases "system replacement in India" and "virtually a new farming system" (see also ICRISAT, 1983). The outcome to date has been rather less radical; it is, in effect, a set of proposals for intensified cropping of the black cotton soils.

In Andhra Pradesh (and characteristic also of a good deal of the Deccan) the agricultural scene is dominated by a gently undulating relief with a mosaic of black, cracking clays in the hollows and red sandy soils on the slopes, interspersed with knobbly granitic outcrops. The black clays (vertisols, black cotton soils) are sticky when wet but retentive, hard to work when dry, excellently structured, and moderately fertile; the red sandy soils (alfisols) are infertile and easily worked but very sharply drained. Ploughing (with a pointed, not a mold-board, plough) is done by oxen; their power is generally insufficient to till the black cotton soils in the dry season so they are ploughed in the rains and, typically, one crop per year is taken on residual soil moisture (Figure 5.5). Farmers, however, know very well that *some* patches can be tilled dry so that end-of-dry-season sowing is possible and hence, locally, double-cropping (*kharif* and *rabi*). The ICRISAT scheme simply generalizes this idea by exchanging the traditional plough for a wheeled tool-bar which the oxen *can* work in the dry season and which can be variously adapted to forming graded beds-with-furrows. Fairly reliable double cropping becomes possible (Figure 5.5); cultivation is faster and more timely, farmers *like* to sit on a tool bar rather than walk behind a plough, and ICRISAT calculations suggest that the cost of the equipment will be within reach of many farmers.

ICRISAT has had farm survey work going back a decade or more but the black-soil-toolbar technology (Thierstein, 1983) outlined above was developed on-station and only taken to the OFR phase fairly late (1981 on). Now, a collaborative OFR program with the All-India program (AICRPDA; ICAR, 1982), state governments, and universities is being developed. No doubt, generalized OFR economic assessments will soon be available (they look very attractive on-station). The farmers I talked to in Andhra Pradesh were enthusiastic but the overall economics is not yet clear.

Figure 5.5: ICRISAT Proposals for the Cotton Soils of the Deccan



The essential outcome of the ICRISAT program so far has thus been a very promising bit of technology but only one component of a much wider scheme. The Institute conceived of "watershed management" (very small watersheds) with contoured cultivation, controlled drainage, grassed drains, integrated cropping of black and red soil patches, perhaps water storage in small tanks fed by drains (or improved watering of the rice paddies). In practice, integrated watershed management would be a huge task and would imply extensive consolidation of dispersed landholdings (something that the local farmers evidently do not fancy). As to the red soils, ICRISAT grows excellent crops on them on-station with high inputs; what farmers might do is likely to be revealed only by OFR/FSP methods, I think; leguminous mulching/incorporation is one interesting possibility.

There are wider questions. I remarked earlier in the chapter that, if ICRISAT looked at upland Ethiopia, it would probably have gone to the black soils rather than, as ILCA did, to the cattle. Indeed, both institutes were effectively constrained by their mandates to do what they did. ILCA arguments for fewer and more productive, better-fed cattle apply also in India, where cows mother oxen and buffalo produce milk. A social revolution would, of course, be implied but one can at least imagine a NFSD for the Deccan, even wider in scope than that conceived by ICRISAT. Managed catchments, small machinery, double-cropped black cotton soils, mulched red soils, agro-forestry for timber, fuel, fodder, and mulch, fewer but better-fed cattle for both work and milk, and so on. In short, ICRISAT started with a NFSD concept but has arrived at OFR/FSP in collaboration with local agencies. The Institute has a strong training program with FSR ideas as a component of it.

The NARS (that is, AICRPDA in the context of dryland farming - see ICAR, 1982) and their State counterparts whom I met in Andhra Pradesh and Karnataka seem collectively to have, broadly, a commodity/discipline orientation but with explicit commitment to OFR/FSP methods (usually under the title "operational research" - see, e.g., Sanghi, 1982).

Rice-based Systems in Asia

The outstanding success of the Asian Cropping Systems Network in raising irrigated rice yields in Asia is well known. The ACSN arose directly out of IRRI's perception that the quick maturity and day-length insensitivity of some of the newly available dwarf rices would permit sequential multiple cropping to two or even three rice crops per year. So IRRI concentrated first on areas in which good water control was available (or could be developed) and, recognizing that collaboration with local NARSs was of the essence, developed the ACSN, with strong emphasis on training, information flow, and OFR/FSP operations (Figure 3.5). The last was not so entitled, but OFR/FSP was in fact how the ACSN operated (Zandstra, 1979; Zandstra *et al.*, 1981; Quisumbing, 1982.) The outcome was not just sequential rice-cropping systems but, sometimes, such sequences as rice-rice-cowpeas (Figure 5.7). Now that the potential for developing fully irrigated rice systems is nearly exhausted, and the rainfed crop has been intensively developed too, attention is shifting to rice under less than perfect irrigation, to upland rice, to associated crops (e.g., cowpea), even to animals; hence the change in title to AFSN noted in

Figure 3.5. The Network's attention to fully irrigated rice will not, presumably, be allowed to lapse completely because yields can yet go higher and second-generation problems are emerging, for example: possible minor nutrient problems (a consequence of past success) and effects of agri-chemicals on fish and other wildlife.

The outstanding success of the ACSN was due, I think, to the fact that the objective was never in doubt (raise wetland rice yields per ha/yr), that it was shared by all participants, and that the potency of the OFR/FSP approach in the context was fully appreciated. And it may be that, in retrospect, it will appear that not the least achievement of the ACSN is to have stimulated, by example and training, the participant NARSs to develop their own research systems and local OFR/FSP. Certainly, the Philippines and Indonesia seem strongly committed to developing local agricultural research with OFR components (cf. next two sections).

Small-farmer Coconuts in the Philippines

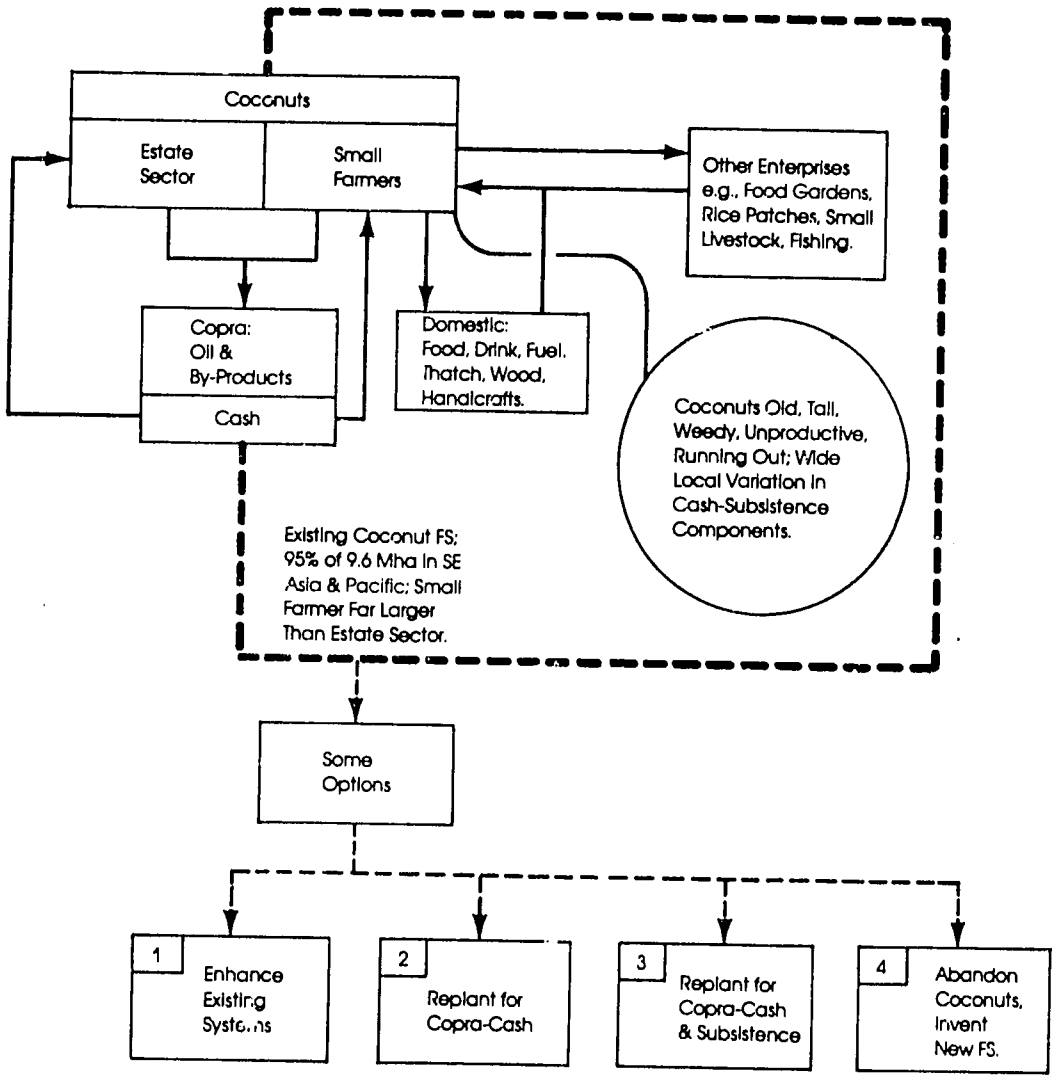
Some 95 percent of the 9.6 million hectares of coconuts are in Southeast Asia and the Pacific and the small farmer is far larger than the estate sector. The Philippine islands are the leading copra exporter and the small farmers there mostly grow for cash (elsewhere, coconuts are more of a subsistence crop). The system is broadly summarized in Figure 5.6.

The Philippine Government, through the agencies of PCARRD, the Ministry of Agriculture, the University of the Philippines at Los Banos, and local Colleges of Agriculture (e.g., VISCA, the Visaya College in Leyte) are committed to an OFR/FSP approach to enhancing diverse agricultural systems, including the coconut one (PCARRD, 1982). I saw something of the work in the field near Jaro, in the Tacloban City area of Leyte. The Eastern Visayas are Region 8 of the 12 Regions of the Philippine islands; six areas have been identified in the Region, the islands of Leyte and Samar; in each area, several on-farm sites representative of "recommendations domains" have been identified and preliminary surveys carried out. OFR teams, including economists, live at each site.

Farms are small and, as noted in Figure 5.6, the coconuts are tall, old, gappy, and running out, since most are residual from a surge of planting in the early years of this century. Since nothing dramatic can be done about the coconuts in the short term, the OFR work at Jaro concentrates on seeking to undercrop the coconuts much more widely than they now are. Diverse food crops such as dryland rice, bananas, and cassava and cash crops such as pineapples and cocoa are being tried; small animals are another possibility (though the goats I saw were not a spectacular success: they looked unhappy, needed shelter and ate the neighbor's vegetables). The farmers were clearly responsive but no economic analyses of the outcomes were available.

The OFR work along these lines may well prove beneficial to the farmers because the soils are quite good, rainfall adequate, and the potential for enhanced production is clearly there. A wider question remains, however, as indicated at the bottom of Figure 5.6. The coconuts

Figure 5.6: Small-Farmer Coconut Systems in the Philippines



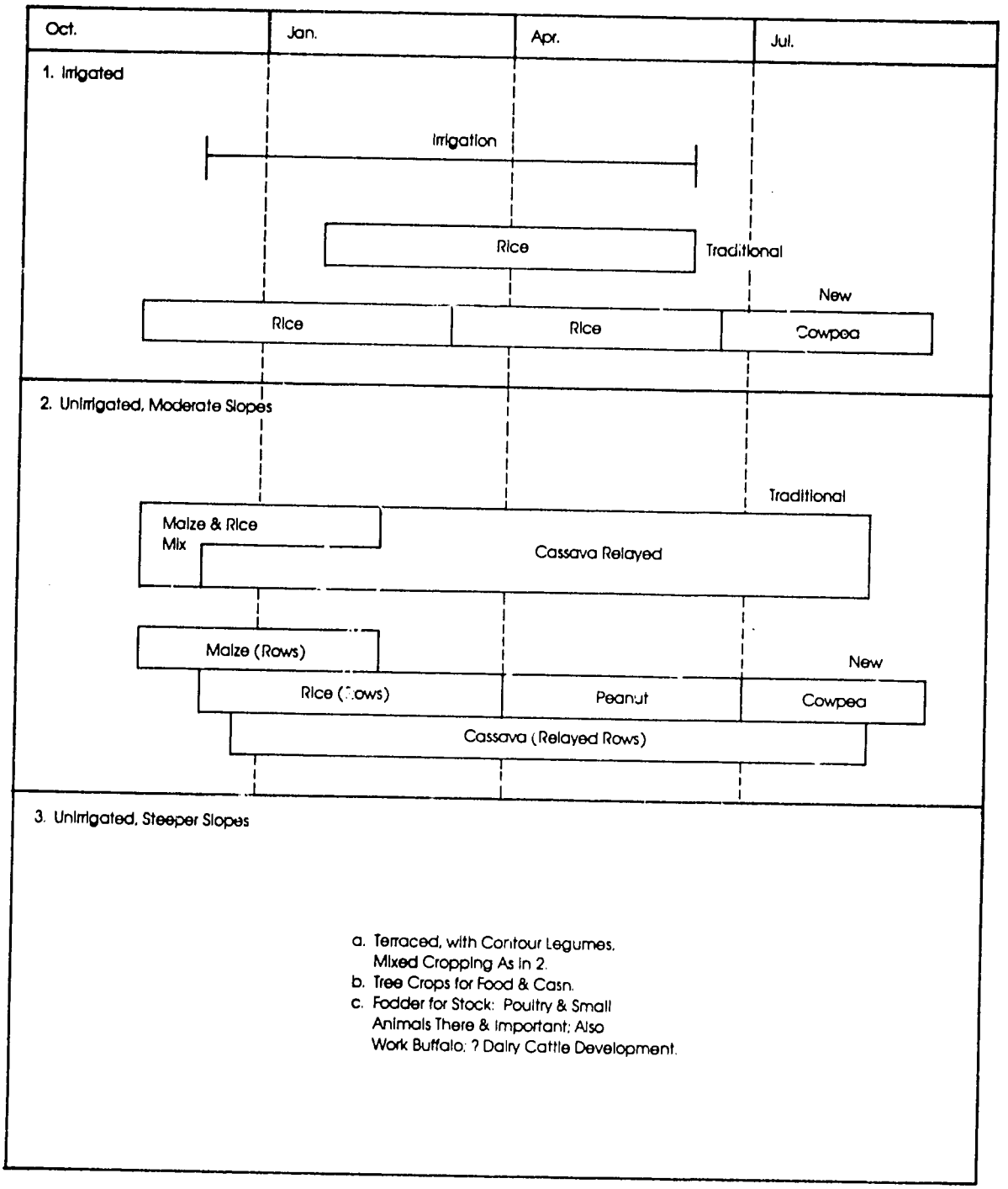
everywhere are old and the Philippine Coconut Industry Board has been embarked, for some years, on a policy of replanting with hybrid semi-dwarfs. Though some doubt apparently attaches to the choice of the best hybrid combination, it can not be doubted that semi-dwarfs are the correct combination for the estate sector, if coconuts are to be replanted at all. However, competition from other oils such as that of oil palm makes the future for coconut oil less certain. So what is the future for small-farmer coconuts? Should they be replanted with whatever hybrids are chosen for copra-cash (the best choices are far from clear anywhere), at high density, and with little opportunity for under-cropping? Should the same hybrids be used but at low density, deliberately to leave space for intercropping? Or should coconuts be abandoned as the old talls die out and some NFSD be invented to take their place? These are hard questions to which the Philippine authorities are no doubt applying themselves. Meanwhile all that the OFR/FSP workers can do, I think, is to try to devise ways by which the farmers can get more out of the old coconut system while it is still there. Thus, what is now plainly an OFR/FSP problem may become transformed in time into an NFSD one.

Small-farmer Food Cropping in Indonesia

Indonesia was very active in IRRI's ACSN (Figure 3.5) from the start and, fortified by the success of that program (Bernstein et al., 1982) for irrigated rice, adopted the OFR/FSP approach generally for subsequent small-farm developments. The basic commodity orientation of the NARS (AARD, 1981) is not thought to be an obstacle to the OFR approach, because specialists are drawn into OFR teams as necessary. The government (AARD) officers work with collaborative support from outposted IRRI staff and international agencies and, in total, the effort is a very substantial one, with several sites in all the main islands. The practical problems have not been eased by the fact that much of the work has had to be done in "transmigration areas" of Sumatra and Kalimantan where migrants from the more fertile but over-populated lands of Java have been settled. Even with migration, however, Java remains over-populated and there is severe pressure on the erodible hilly lands (unhappily, often referred to as "watersheds"). The general problem therefore is to enhance and stabilize food production in rather diverse soils, altitudes, and rainfalls (but exclusive of irrigated rice on lower ground).

The main points of the approach are summarized in Figure 5.7, and further details of OFR methods (essentially those of the ACSN) will be found in AARD (1981), Inu (1983), McIntosh and Suryatna Effendi (1981). Some economic results are cited in Table 3.3. It will be seen from the figure that three levels of difficulty can be identified. With moderately good irrigation, a dry-seeded rice/irrigated rice/cowpea sequence is possible and being adopted; it lies in the area now being developed by IRRI under its AFSN (see above). This part seems fairly secure and, to the extent that irrigation can be further improved, could be transitional to really intensive rice production on the IRRI model. Rainfed agriculture on moderate slopes is a major concern in the trans-migration areas where multiple cropping in space and time with moderate fertilizer inputs are evidently attractive (Figure 5.7 [2], Table 3.3). The steeper slopes present the hardest problem and, though the general form of the technology needed is clear enough (Figure 5.7 [3]), economic feasibility of possible solutions has not yet, I think, been established.

Figure 5.7: OFR and NFSD in Indonesia



Technically, the problems of rainfed agriculture here look much like those of humid Africa (section on Nigeria, above). However, the soils of Indonesia are probably rather better than most of those of Africa, people are accustomed to settled agriculture, land is privately owned, there is a long tradition of more or less integrated rice/dryland, food/mixed tree cropping, terracing has been understood and practiced for millenia, and stock are not particularly threatened by diseases. So the ingredients are there and it seems to me likely that a systematic OFR effort, coupled with good extension, could lead, step-wise, to stable and productive systems (however difficult it is to conceive of such a change in the wetter parts of Africa now dominated by shifting systems). So the problem before Indonesia, even on the steepest ground, has not necessarily the character of a NFSD; step-wise progress seems conceivable.

Another area of activity in Indonesia, currently assisted by the World Bank, lies in the tidal swamps. These are very extensive areas of difficult, acid soils and peats which have, nevertheless, considerable potential when drained. I do not know enough to try to review the problems but simply note that: (1) local farmers have already shown considerable ingenuity in growing annuals, perennials, fish and fowl and that there must be much to be learned from them; hence (2) multiple land use will almost certainly be appropriate; (3) given wetness and very acid soils, fairly massive government intervention in respect of drainage and fertilizers will be necessary; (4) whatever ingenuity the farmers show in adapting details, a degree of centralized control and something approximating to NFSD will be necessary.

CHAPTER 6. SUMMARY

Introduction

Diverse activities collectively described as Farming Systems Research (FSR) have become a prominent feature of the work of the International Agricultural Research Centres (IARCs) and of many National Agricultural Research Systems (NARSs), too, in the past 10-15 years. The Bank's desire to sort out a confused terminology and assess the usefulness of FSR studies in agricultural research prompted it to commission the study reported here.

The broad context of the study is that populations in most tropical countries are growing rapidly and that there is an ever more acute need to produce more food from finite (sometimes already limiting) patches of land. The Green Revolution very effectively increased yields of wheat and rice over large areas by exploitation of genotype-environment interactions (i.e., dwarf, responsive varieties plus enhanced husbandry). Green Revolutions in other crops did not follow and it became apparent, a decade or so ago, that many innovations proposed by agricultural research, whether in unit or "package" form, were simply not being adopted by farmers.

It became apparent that the reason for non-adoption was generally that the innovations were unsuitable for the socio-economic circumstances of the farmers. The adaptation of the ideas of temperate farm-management economics to tropical small farmers revealed that they, the small farmers, were economically rational but risk-averse and sharply constrained by uncertain environments and shortage of cash; they were ready enough, however, to adopt innovations that they themselves perceived to be economically attractive. Accordingly, the doctrine grew that research should be determined by explicit farmers' needs rather than by the preconceptions of researchers.

The Consultative Group on International Agricultural Research (CGIAR) therefore introduced economists into all the institutes at an early stage and explicitly espoused FSR ideas with the object of linking research with perceived farmers' needs.

Tropical agricultural systems are almost infinitely various, and the relatively coarse classifications necessarily adopted in the textbooks are little use to the FSR worker in the field. Various kinds of mixed cropping are characteristic of many tropical agricultures and are important in contributing to yield and stability of yield; FSR workers very often meet them, not as farming systems per se, but as components of cropping systems. Mixed cropping is interesting and important but not central to FSR ideas.

Despite a frequent, seemingly stable situation in the short term, most tropical farming systems are in a state of change. Four agents of change may be recognized: the farmers' own perceptions and actions, the stimulus of publicly supported research, the economic pull of

a cash crop, the commercial push of a product sold to farmers by a firm. The second of these is the main context of FSR and the question is: how to do the right research, the kind that farmers really want, and transmit it to them?

Farming Systems Research in General

The diverse activities subsumed by FSR in the broad sense (FSR *sensu lato*) fall into three categories:

1. FSR *sensu stricto*, the study of farming systems *per se*, as they exist; typically, the analysis goes deep(technically and socio-economically) and the object is academic or scholarly rather than practical; the view taken is nominally "holistic" and numerical system modeling is a fairly natural outcome if a holistic approach is claimed.

2. On-farm research with farming systems perspective (OFR/FSP) is a practical adjunct to agricultural research which starts from the precept that only "farmer experience" can reveal to the researcher what farmers really need; typically, the OFR/FSP process isolates a subsystem of the whole farm, studies it in just sufficient depth (no more) to gain the necessary [illegible]P and proceeds as quickly as possible to experiments on-farm, with farmers' collaboration; there is an implicit assumption that step-wise change in an economically favorable direction is possible and worth seeking.

3. New farming systems development (NFSD) takes as its starting point the view that many tropical farming systems are already so stressed that radical restructuring rather than step-wise change is necessary; the invention, testing and exploitation of new systems is therefore the object; while OFR/FSP seeks to adopt the technology to the farmers' economics, NFSD must usually imply government intervention and the adaptation of economics to technology.

After a decade of work in the field, the economic characteristics of small farmers are well enough understood for practical purposes, though they present endless subtleties of detail. Small farmers are poor, economically rational (though not necessarily profit-maximizing), risk-averse, and subject to high interest rates. The methods of farm-management economics can be well enough adapted to their circumstances for the practical purposes of OFR/FSP but experience and caution on the part of the economist are of the essence.

Several kinds of innnovation can be recognized: the costless one (a new variety), the one that costs cash (chemical inputs), the one that costs effort (an extra crop in the sequence), and the complex innovation ("package") that demands several inputs. Generally, the simpler and cheaper, the readier the adoption; OFR/FSP is mostly concerned with unit changes or simple packages while NFSD has to face the problems of multiple changes in complex packages.

"Systems" have been made much of and may appear formally in FSR sensu stricto. For OFR/FSP, the preoccupation is always with a sub-system, often a small one, but in awareness of the interactions that are perforce ignored; FSP rightly takes a commonsensical rather than a formal view of systems and only exceptionally needs to make numerical models. At whatever level of analysis is relevant, the systems considered by FSR sensu stricto and OFR/FSP are far narrower than the systems recognized at textbook level (see above); in practice the only definition of a system useful for these purposes refers to an assemblage of farms (in a limited area) which are like enough to each other, technically and economically, for the purpose in view. No useful objective typology/classification is possible at this level. A system is what an experienced worker says it is.

If FSR sensu stricto, however interesting per se, is largely irrelevant to agricultural research (above) and OFR/FSP is becoming well established as an adjunct "style" of research (also above), the position of NFSD is far less clear. That there is local need of it is not in doubt and there is much component technology in several IARCs which has yet to be synthesized. The need for NFSDs in the lowland wet tropics on fragile soils is agreed to be acute yet there is little or no generalized vision as to how they might be accomplished. Somewhere, somehow, experienced researchers must step outside the component technology and make imaginative guesses; and development agencies must be persuaded to try those guesses in practice, even at risk of making some expensive mistakes. I do not believe that OFR/FSP, useful as it is in its own context, can make the kind of imaginative jumps which are needed. I return to the subject later in relation to the general neglect of perennial crops and to the activities of the Bank.

Genotype-environment (GE) interactions occur when different varieties respond differently to different environments. They are a significant feature of all plant breeding; the more diverse the environments, the more likely they are to be economically important. Their existence implies that experiment-station selection may not produce farm-adapted genotypes and that breeding tends to be rather site-specific. Decentralization of breeding responsibility and selection are indicated. The matter is economically important because a very substantial part (probably approaching one half) of tropical agricultural research is devoted to plant breeding and a like proportion applies to our expectation of plant breeding as a component of progress in crop production. From the OFR/FSP point of view, variety adaptation on-farm, as distinct from on-station, is the crucial point.

On-farm Research with Farming Systems Perspective

OFR/FSP (the term was invented by CIMMYT workers) has largely been developed into practice by CIMMYT and IRRI working in collaboration with local NARS in Central America and eastern Africa and in Southeast Asia, respectively. Their methodologies have been fully described in the literature and, despite differences in terminology, are effectively identical in structure. Essentially (cf. Figure 3.3):

1. There is a team (multi-disciplinary) in the IARC which

2. identifies the "target" FS or "recommendation domain" and

3. analyzes its technical/economic structure just deeply enough for the purpose in hand;

4. the team identifies economically sensible innovations and

5. does research on them, on the experiment station, on few selected farms, on more numerous farms, according to circumstances;

6. under 2-5 the team solicits the interest and collaboration of the NARES and

7. the experimentation is iterated as necessary;

8. the experiments are monitored/analyzed economically and, if successful, lead to recommendations for the extension system (NAES) to exploit;

9. there is a continual feed-back of information to the IARC/NARES consortium as experience and understanding widen;

10. given that there is agreement that the OFR/FSP "style" of research is valuable, the IARC must assume a responsibility for training and network development.

The "recommendation domain" in the preceding is the farming (sub-) system chosen on the basis that it is broad enough to be operationally sensible, but narrow enough that there is reason to think that any recommendations that emerge would be applicable throughout. Choice is based on survey (as short and simple as possible) and economic analysis. There can be no fixed schedule for the OFR part of the work but close collaboration with and feedback from farmers is agreed to be essential; generally, researcher-run, factorial-type experimentation (if necessary) precedes farmer-run, few-factor comparisons of selected treatments or packages compared with traditional husbandry. But practical empiricism must rule; sometimes, the process may be shortened and, sometimes, iteration will be necessary.

The involvement of the NARS and extension service throughout the OFR/FSP process is agreed to be necessary so that both the methodology and technology may be transferred to the users. In pursuit of NARES involvement, the leading IARCs in the area, notably CIMMYT and IRRI, have long recognized the importance of training and network development (Figures 3.5, 3.6). In the longer run, most OFR/FSP work must pass to the NARSs but prolonged IARC involvement in training and networks can be foreseen. The network is a centralized but two-way information system which depends a good deal on personal contact (not the least of the benefits of a sustained training program).

As a result of historical accidents, the institutional provisions for OFR/FSP work in the IARCs are quite various and likely to remain so. No "ideal" structure is apparent but there is an over-riding need to generate the essential FSP among researchers. Multi-disciplinary team work is usually thought to be essential but the idea does not always work well, and multi-disciplinary thinking by individuals could well be more productive - if it were achievable. Economists are already well established as elements in OFR teams and will remain so. I can discern no clear need for social anthropologists.

Wider Questions

Export cash crops have been, historically, an important agent in tropical agricultural development. They have been avoided by the CG system but maintain fair to excellent R & D systems of their own and remain a substantial element of many NARS programs. Many such crops are perennials; they constitute an excellent economic base for small landholders in the wet tropics and a focus for agricultural development. The technology of managing them is well known and its transfer tends, of its nature, to be of a conventional "top-down" nature; maybe there is room here for more OFR/FSP in the small landholder context?

For annual food crops, the CG system has done a highly creditable job, over the past decade or so, of introducing economic ideas, of developing OFR/FSP into a working technique, of training, and of the building of networks of collaborating NARSs. The groundwork having been done, one can begin to perceive training and the sustaining of networks as key activities in the future, as constituent NARSs grow.

The need for NFSD, especially in the lowland wet tropics, is widely recognized to be important, yet the synthetic thinking that goes beyond component research is hardly anywhere to be found (see above).

Agro-forestry ideas (Figure 4.2) have been abroad for several years but an effective institutional structure (ICRAF) has only recently emerged. Widespread recognition, that trees (more generally, perennial plants) even exist in tropical small-farmer agriculture has been long delayed, extraordinarily so in the light of their socio-economic importance, especially in wetter areas. Interest, however, is now beginning to stir. We may be certain, I believe, that trees will play a central role in any serious NFSD in the wet tropics, though our scientific understanding of their potential for food production and even simple introduction-trial work are still grossly deficient. The neglect of perennials is, I suggest, a lamentable deficiency in contemporary tropical agricultural research.

With regard to the interests of the World Bank, I suggest that it has a dual role. As a major donor to the CG system, I think that it should encourage the CG institutes: to develop FSP ideas among researchers, to use OFR methods where relevant, to enhance training and network operation, to think hard about NFSD, and to take perennial plants seriously. As a funding agency for agricultural development projects, I think that the Bank should: retain an R & D element (with OFR/FSP) in

such phased development projects as are aimed at step-wise advancement but should also be prepared to be bold and try to construct and exploit one or more types of NFSD in the wet tropics. The latter might lose money but could provide, nevertheless, valuable guidance on how to "do" NFSD and short-circuit what may otherwise provide to be an intolerably prolonged research process. There is a real need for bold thinking coupled with resources and who better to provide them than the World Bank?

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ADDENDUM TO THE BIBLIOGRAPHY

A number of older publications to which I should have referred in writing this report have come to my attention only since the report was submitted, and a very relevant book has been published in the same interval. After each reference I refer briefly to the contents in the context of my report.

Zandstra, H., Swanberg, K., Zulberti, C. and Nestel, B. (1979). Caqueza: Living Rural Development. IDRC, Ottawa, Canada.

The Caqueza book bears on most of the main theme of my report. The project (in Colombia, 1971-75) descended historically from the Puebla project in Mexico. The socio-economic features of small farmers are well explained, with an enlightening discussion of risk and uncertainty, even a brave (if not very successful) attempt to quantify the risk factor. OFR/FSP, though not under that title, was an integral part of the operation, which must have been one of the earlier rural development projects deliberately to adopt a "bottom-up" approach, in recognition of the general failure of the obverse. There is an effort to quantify the social cost-benefit aspects of the project which, like the work of Martínez and Saín, leaves it fairly clear that there were positive net benefits of uncertain magnitude. The book contains a good deal of rather dull administrative detail which, however, does give a feel for the sorts of practical reasons for which RDPs work or do not work, as the case may be. The Caqueza project was surely one of the pioneering works in the area, and I regret that I did not assimilate it into my report.

Okigbo, B.N., and Greenland, D.J. (1977). Intercropping systems in tropical Africa. In Multiple Cropping, ed. R.I. Papendick et al., American Society of Agronomy, 63-101.

Okigbo and Greenland give a valuable compendium of intercropping systems in tropical Africa, with special reference to annual crops in shifting/bush fallow systems. Some 50-80 percent of all crops are grown mixed.

Greenland, D.J. (1977). Increasing food production from the lowland humid tropics of Africa and Latin America. Ann. New York Acad. Sci., 300, 112-120.

Greenland discusses the problems of maintenance of soil fertility in the humid tropics under production regimes more intensive than those of traditional systems. Though he is clearly inclined to think of intensification of what is already there, his arguments have profound bearing upon thinking about NFSD. He recognizes the importance of trees, less for their products and soil surface protection than for their capacity to extract basic actions from the subsoil.

Huxley P.A. (ed.) (1983). Plant research and agro-forestry. ICRAF, Nairobi, Kenya.

Huxley's book is a multi-author one with some useful reviews of perennial-based systems in, for example, Indonesia, West Africa, Costa Rica, and Brazil. There is much here that is highly relevant to my advocacy of trees as the bases of many farming systems, especially when any kind of NFSD in the wet tropics is contemplated. Once again, perennials must be at the heart of any imaginative thinking or experimentation upon NFSD. This is but one among several recent publications which, collectively, show that the traditional boundaries between agriculture and forestry are beginning to dissolve: the paradigms are shifting, to adopt the fashionable phrase. The sooner we come to think of perennials and annuals as complementary and interlocking elements in tropical land use systems the better. It is surely time that tropical agricultural research at large got out of the annual crop rut in which much (but not all) of it resides and really looked at what tropical farmers already do with perennial plants. This book presents many useful ideas on where and how to look.