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Assessing the Impact of Farming Systems Research: Framework and Problems

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SUMMARY

Farming systems research (FSR) is a feature of several International Agricultural Research Centre mandates and programmes and is an accelerating activity among national research programmes. Few attempts have been made to assess its impact, perhaps because of the several inherent difficulties that are outlined. The difficulties include the multiple attributes by which agricultural households judge their achievements and the multiple constraints and technological relationships under which they operate, as well as the several challenging tasks of aggregation, over research projects, target farms and time, and of accounting, over individuals and markets. There are, however, clearly demonstrated advantages in FSR's role of providing feedback and guidance to research workers.

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INTRODUCTION

Increasing proportions of the budgets of both the International Agricultural Research Centres (IARCs, about 15 per cent) and of many National Agricultural Research Programmes (NARPs, unknown, but surely a lesser proportion), especially those assisted by some donors such as USAID and IDRC, are being dedicated to Farming Systems (FS) Research (FSR) The time seems opportune to review the special difficulties that may be involved in assessing the impact of FSR.

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An immediate difficulty is the lack of a precise and agreed definition of FSR. Simmonds[20,21] has recently proposed a tripartite categorisation of

(a) FSR in the strict sense (FSRSS);
(b) on-farm research with FS perspective (OFR/FSP); and
(c) (radically) new FS development (NFSD).

Whether these survive for long in the crowded proliferation of acronyms remains to be seen, but the apparent greyness of the boundaries between them does not augur well for longevity.

The present discussion probably applies to impact assessment of all three categories although, definitionally, there will be little impact ever evident from FSRSS (except for the occasional new PhD) and, given its rarity in contemporary practice, NFSD cannot yet have much impact. Most impacts of empirical interest are thus likely to be concerned with something approximating OFR/FSP, but here the vaguer general term of FSR will be sustained in use (and possible abuse).

A FRAMEWORK FOR ASSESSMENT

FSR workers, if they indeed practise what they preach, are never far from assessing their impact. Whether it is in the early diagnostic phase of identifying problems, later stages of testing changes or endloop stages of measuring the exploitation of modified farming techniques, the close association with the human elements of FS provides, in principle, a continuous harvest of impact information.[9]

Hardaker *et al.*[10] argue, perhaps too glibly, that FSR is 'assessable by the extent to which it leads to the development of socially desirable farming techniques that are readily adopted by its (FSR's) specified groups of client farmers'. This argument may pose more questions than answers, particularly regarding 'socially desirable' (from whose vantage point?), 'readily adopted' (neither word is unambiguous in the wastelands of FSR) and 'client farmers' (just how are they specified and grouped?).

The position taken here is to dodge the issue of social desirability and to focus initially on what the FSR evangelists are generally agreed on, namely that the relevant goals and objectives to be considered are those of farm families. Even this focus does not resolve the problems of accounting for the goals of non-farmers, especially landless labourers, if they are defined as being beyond the scope of the implicit target.

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The complexity of family arrangements in diverse cultural settings intimidates this FSR observer into a reluctance to open this particular Pandora's Box—in spite of earlier speculations on testable hypotheses about its contents.[1] In principle, however, a household utility or preference function $U(X)$ must exist and must embrace all the important attributes (vector X) that contribute to the household's happiness, and survival.

Elicitation of what will inevitably be a multi-attributed preference function is not easy, although apposite methods are available[2,13] and a few attempts at applications of relevance to the present context have been made.[11] A sceptical analyst might, with understandable disbelief, adopt either an existentialist position and question the very knowability of such preference structures or a Simonian position and challenge the notion that householders, especially on resource-poor farms of the Third World, attempt to maximise or behave as though they were maximising (the expectation of) such a function.[8,15]

Analysts who do not resort to such neoclassical assumptions and models face considerable challenge to progress and, in the judgment of this observer, are unlikely to be able to close a model sufficiently to be able to conceptualise, let alone measure, impact.

The next most important element in conceptualising FSR impacts follows naturally in the tradition of FSR, namely determining the resource constraints, R, to which the households targeted in FSR are subject. This, again, is no easy task and requires insightful imagination. In some cases, there may even be doubts as to what are objectives versus constraints—although with Day and Robinson's[7] argument as to their conceptual equivalence, perhaps this is not a critical issue. Concern for identifying and measuring constraints is mirrored in the importance attached to diagnostic surveys and descriptions of farmers' circumstances. Pursuit of some constraints such as those involving credit supplies and off-farm employment may well take the practitioner of FSR a long way from the farm.

The description of farmers' circumstances is completed by describing the existing technical relationships, T, in the FS. This task, also challenging, involves the whole gamut of technological understanding, the production economics of factor and product interrelationships and, if done in a manner that explicates the risk inherent in the farm environment, possibly very demanding stochastic specifications.[3]

With these several tasks successfully completed, the analyst is

presumably in a position to assemble a comprehensive model of the FS as it exists, or subsists. In general terms, this might be written as

$$U^* = \max U(X) \text{ s.t. } T, R \tag{1}$$

as shorthand for indicating the operation of a system optimally (denoted by the asterisk), by maximising (max) satisfaction ($U(X)$), subject to (s.t.), the technology (T) and available resources (R).

While the difficulty of doing this has been noted above, it pales into insignificance compared with the really and necessarily imaginative next phase of identifying what research activities might lead to desirable changes in the technological environment and, indeed, at least in probabilistic terms, what the performance of the new technologies being sought may be like. Two sources of uncertainty are involved that surrounding the research effort itself and that which is related to the performance of any unknown technology in a risky environment.

Symbolically, if the research activity vector is indexed as r and the uncertain resultant new technology is T^r, with performance captured in attributes X^r, the utility maximising farm household would act to achieve

$$U^{**} = \max U(X^r) \text{ s.t. } T^r, R \tag{2}$$

where, analogously to (1), the double asterisk denotes the new (post-research) optimal operating situation.

The FSR workers in their (not always recognised) role of consultants to target farm households should, in the spirit of maximising (social) welfare, select r^* in order to maximise the social advantage of research:

$$DU = \max^r U^{**} - U^* \tag{3}$$

where advantage is defined as the difference between the two optimal levels of satisfaction and the change in utility following the maximising of advantage is denoted by the prefix D.

The simplicity of this expression arises from the implicit assumption that the target of an FSR programme is but one farm. When there are, instead, very many farms, the optimiser of the research portfolio must also deal with the inherent aggregation problems. Non-additive utilities, and distributive weights of uncertain veracity, will make this analytical task more or less impossible.

So much for the intermediate task of selecting a research portfolio. Needless to say, as it is implemented, the FSR approach properly will monitor field performance with a view to corrective control actions and to

impact assessments. A criterion function in this new sense would be most helpful. As uncertainty is resolved, some things become easier to measure. Objective evidence of impact is confined essentially to the past tense. Contemporary data on the effects of technologies, especially so-called pipeline technologies, are inherently noisy.

The new aggregation problem to be faced is the one over time indexed by the subscript t. The present, t', provides a convenient time for partition. Impacts of FSR to date, for the representative farm, are

$$I_{t'} = \sum_{t=t^r}^{t'} DU_t \tag{4}$$

where t^r dates the time of first adoption of the innovations/techniques stemming from the FSR and the summation over time ($\sum_t$) continues up to the present. Once an innovation/technique is widely adopted, a significant estimational problem is what the performance would have been in the absence of the novelty, analogous to the assessment of performance of traditional crop varieties that are no longer widely grown. Aggregating utilities over time is never straightforward and netting out the research costs attributable to a particular programme is also intrinsically difficult.

Continuing the accounting only to the present is clearly very conservative since, in most cases, the new technique will continue to be used to advantage in the future until it is supplanted by something new and better, perhaps the product of further FSR. The future (f) benefits should be included, even if the best that can be assumed is that the innovation will last forever at the present level of adoption, namely

$$I^{fr} = \sum_{t=t'}^{\infty} DU_t \tag{5}$$

But future accounting does not end here in any on-going programme of FSR. There should be a continuing stream of innovations, superior techniques, improved varieties, new crops, etc.—most immediately those associated with research in the pipeline. These may range from techniques presently under test in farmers' fields through to mere twinkles in the eyes of farming systems research workers. For each new innovation, the impact analyst could compute new estimates of U^{**} and DU and

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aggregate forward to assess the expected impact of the FSR programme. Naturally, the more futuristic the assessment, usually the more uncertain are the benefits and, if household decision makers are strongly averse to risk, the greater the risk discounting of benefits. This will result in a series of I^f that, again in principle, can be aggregated for a more holistic assessment of impact.

LESS ABSTRACT CONSIDERATIONS

The framework outlined above might most descriptively be designated as an *F*-impact framework to emphasise its orientation to farmers *per se* and its assessment in terms of what they themselves see as being important to their welfare, irrespective of the degree of connection to the rest of society through commercial and barter trade. Changes in technology that result in no increase in marketed surplus and no adjustment in purchased inputs are readily accommodated. For instance, almost costless innovations that result in more stable crop yields (like more drought-resistant cultivars), less disease-prone livestock (e.g. through vaccines), less arduous weeding and crop processing, can be measured for impact more or less appropriately. The difficulty is, however, to measure all the components adequately. While such measurements are established parts of the creed of FSR, there do not appear to be any reports of such comprehensive measurement.

Most impact assessments of research have more of a social accounting perspective where the assessment is concentrated on: (1) netting out changes in flows of inputs to and outputs from the farm households; (2) estimating adoption rates and thus, eventually, aggregative effects; (3) taking proper account of consequential changes in market prices; and (4) assessing the final distribution of gains and losses associated with the research. Applications of such assessments in the context of FSR in particular, however, seemingly have been very scarce to date.[19]

A good example of the first stage of FSR impact is provided by Paudyal's[18] evaluation of cropping pattern innovations in a hill district of Nepal. He formulated a linear programming model for representative hill farms and used a simplification of eqns (1) and (2) by assuming that farmers wish to maximise farm cash income, after family food and other subsistence requirements of households are met. Different combinations of new technologies could then be considered, and their merits compared

(analogously to eqn (3)) by examining differentials in optimal net incomes. The very modest size of these differentials (of the order of 10 per cent for new maize and rice technologies, and negative for new wheat) matches the very cautious adoption of such technologies by local resource-poor farmers. Paudyal was also able to highlight the sparsity of the information on interactions among livestock, fodder (including from trees) and manure, and the rest of the system that are crucial in determining possibilities for technological changes in hill-farming systems.

The second stage is illustrated by the analysis by Martinez and Sain[17] of a case of OFR/FSP in Panama. They emphasise estimation of the differentials in rates of adoption between OFR/FSP and traditional station research (TSR) and estimate returns to investment in OFR/FSP of the order of 200 per cent. They argue that prices in product and factor markets are undisturbed in this case.

A search of the literature revealed no reports in the context of FSR that deal with the third, and thus also potentially the fourth, stage of impact assessment. The possibilities seem to be

(a) either no one has come around to doing it or the search has been deficient;
(b) FSR has had such negligible primary impacts that induced market effects have been inconsequential; and
(c) the task is just too difficult, so analysts have shirked it.

The latter possibility warrants contemplation on several grounds. First, there is the multimarket nature of the supply shifts that typically must be considered. FSR, especially in developing agricultural settings, is inevitably addressed to complex farming systems in which many products are grown, often literally together. Application of a partial equilibrium framework, even with the nuances advances by Lynam and Jones,[16] will need sensitive accounting of the productive interdependencies among competing farm enterprises. Secondly, if the first problem is circumnagivated by aggregating diverse output changes into aggregate supply shifts, the fact that target farms are usually small, with a low proportion of production as marketed surplus, means that the weights used in aggregation should perhaps depart substantially from average market prices, in order to reflect preferences among home-consumption goods. Taken together, such grounds are suggestive of a need for general

equilibrium approaches to impact assessment of FSR, but the conjunction of the several difficulties noted may explain analysts' apparent reluctance to take the plunge.

IMPLICATIONS IN RESEARCH MANAGEMENT DECISIONS

The foregoing discussion is deliberately abstract and gives emphasis to conceptual issues that must be faced by those committed to measuring the impact of FSR work. The conclusion that presently seems imperative is that the challenge will overwhelm both the analytic costs and the philosophic enthusiasm of would-be comprehensive farming systems research workers. Perhaps this may change with further experience and endeavour. Meantime, what, if any, are the lessons for practitioners?

At least three readers* of the foregoing sections complained that insufficient attention was addressed to the potential impacts within a research system itself—what might be designated *R*-impact. The point is especially relevant when the organisational structure features component research units that work in association with an FSR unit.[12] To backtrack to the preamble to eqn (3), in this conceptual model research workers are credited with being able to choose an optimal research portfolio, r^*. At this (probably fictional) level of abstraction, this would mean that the wise and all-knowing research administrators (and their research personnel) would (if they enjoyed the fair fortune to have the services of such an FSR unit) choose a programme of research activities that would maximise the incremental welfare of the target clients.

Reality, naturally, differs somewhat from this abstract ideal. First, in spite of accelerating donor project support, FSR teams working within wider research organisations are still the exception rather than the norm. Secondly, for the good reasons already noted, the pursuit of optimality in the several steps in full-blown FSR has been very circumscribed and, accordingly, the information for making the presumed ideal decisions has seldom been to hand. Where does this leave the more pragmatic FSR aspirant?

The emerging empirical experience of unavoidable sub-optimisation is

* Without implicating them in any of the idiosyncrasies herein, I wish to acknowledge the appreciated interventions of John L. Dillon, Stephen D. Biggs and David F. Nygaard. For a related discussion, see Biggs and Gibbon.[6]

that many significant benefits can still be realised from merely partial implementation of the approach. In particular, the early stages of FSR (variously dubbed diagnosis, diagnostic survey, domain identification, etc.) can be, and indeed have been, an effective feedback vehicle to carry information that leads to the modification/reorientation/redirection/cancellation of research thrusts (see Biggs for examples concerning research on triticale and maize in North India[4,5]). Plant breeders, for instance, typically have multiple breeding objectives and FSR will almost inevitably add to the multiplicity of these with, presumably, added relevance for the work. Needless to say, institutions (both national and international) vary greatly in their commitment to, investment in and exploitation of FSR, even where there is an official or obligatory requirement to conform with the declared ideology (see Trigo *et al.*[22] for their contrasts of product-line and production systems research). Also, although FSR has been around in one form (name) or another for at least half a century, the newness of most of the major investments under this rubric makes any attempt to reach a definitive conclusion premature. Most modern FSR enterprises are still best regarded as essentially experimental[14] and, accordingly, much will be learned about the validity and utility of the models sketched herein over the next few years. Developing agricultural economics have a profound interest in the results. Even a little FSR practice (perhaps even especially if it is not identified as such) may be very cost-effective through the guidance afforded to research to be oriented better toward the most pressing agricultural problems in the Third World.

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