FOUNDERATION DAY LECTURE

FARMING SYSTEMS RESEARCH:
FALSE STARTS AND SOME THOUGHTS

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Dear Colleagues,

I deem it a great honour to be given this opportunity to participate in the Foundation Day celebrations of this premier institution. I feel grateful and even somewhat apprehensive delivering this Foundation Day Lecture. But my pleasant and productive past association gives me the courage to try. I am aware of my inadequacies compared to my illustrious predecessors. I seek your patience and indulgence. Before I begin, I must pay my tributes to this Institute for its valuable contributions. Few institutions can claim such distinction. The founders of this Institute, its leaders, and scientists have earned the respect of those who are familiar with the challenge it faces and its accomplishments.

I have chosen to speak today on a subject which means different things to different people. Farming system research. I hope to unify the different strands and, in this process, sort out some confusion. It is difficult to define farming system research operationally, though several definitions exist. However, at the conceptual level, there is an unusual clarity and a universal appeal. I make this statement as an
economist, of course, but I believe that my non-economist friends will have no
difficulty in following the basic argument. The operational implications are, however,
contentious. Some of my friends, who are engaged in such work, will surely like to
take me to task. I take this risk. Provocation often leads to positive debate and that
is my intention. No offense is intended and I apologize in advance if I do so
inadvertently.

I must also mention that the theme owes its origin mainly to social scientists.
Confronted with the question of poor adoption of improved technology, particularly
the failure of green revolution in Africa, agro-biological scientists usually blamed
post-research vehicles like extension, credit, marketing, etc. There is some
justification in these and a number of empirical studies substantiate this viewpoint.
However, authors like Mike Collinson, Simon Maxwell, John Farrington, Dunstan
Spencer and others focused on the research system itself and argued that research
planning and technology generation process was often flawed. As a consequence,
inappropriate technical recommendations were thrust on farmers. No wonder these
were rejected. They also buttressed their arguments with empirical evidence. Their
criticism challenged agricultural scientists to find a new approach—farming system
research.

In this lecture, I try to build a story around the subsequent experience, particularly
in India. Though the tenor may appear to be critical, it is not my intention to find
fault. It is so easy to be wise after the event, and, in any case, I too was a part of this
story. The intention is to learn some lessons. The issue is important and we must find a sensible paradigm.

**Conceptual framework**

All policies, interventions, rules and regulations seek to modify the behaviour of individual decision makers—producers and consumers, along intended directions. When a new technology is introduced with the intention of raising production and incomes of farmers, they react by adopting or rejecting it. The latter, which is quite common, baffles scientists because they make these recommendations only after rigorous experimentation and testing and are quite confident about their validity. As I mentioned earlier, they usually attribute this to ignorance of farmers, inadequacies in extension, credit, poor delivery of inputs etc. These are beyond their mandate as researchers. We say: 'we did our job, others failed.'

Economists argued that farmers’ decisions on adoption were influenced not only by the positive attributes of the technology, but by a host of other factors like his resource endowments, risk preferences, family requirements, resource flows, the price signals, product and factor market conditions, etc. A large number of studies empirically verified these relationships. Social scientists argued that improved technologies were often rejected because they failed to mesh with these real life conditions under which farmers operate. They pleaded for a research approach which is need-based and addresses these constraints.
For a long time, these two strands of explanations floated independently. There was little dialogue between the two groups. The farming system school tried to bring the two together and the idea of FSR was mooted. It stipulates that all farmers in an area operate in an environment which is a complex of several forces, such as natural resource endowments, infrastructural status including factor and product markets, policy environment, socio-cultural setting, etc. These define the space within which farmers make their decisions. Then the farm itself has several interacting components or sub-systems—the household which represents the human component and defines the goals, needs, resources and decision-making entity; the production system, which is a mix of enterprises and techniques and represents an integrated summary of farmer's decisions striking a balance between his capabilities, his options, and his goals. Each of these again has sub-components—we are all familiar, for example, that each enterprise in the production system is a sub-system. It has its own production function which is a surface depicting various combinations of different inputs which interact amongst themselves and also with the environment. A crop growth model illustrates such sub-system. Actually, scientists go down even further. Agronomists work on fertilizer response system, entomologists look at the pest complex system, and so on. A farming system is thus a hierarchy of systems starting with the feasible enterprises at the bottom. The hierarchical order goes up the ladder covering the household, infrastructural setting, price environment, eco-system, and so on. There are horizontal interactions with other sub-systems and vertical interactions with higher order hierarchy. We then merge different eco-systems and regions to arrive at the national agricultural system. We try to improve the productivity of the system and all these interactions must be factored in as we design a research programme.
This elegant concept becomes unmangeably complex when we try to empiricise. Even with rapid developments in system simulation and other computer-based tools, agro-biological scientists have had only limited success. Even in crop growth modelling which represents the lowest hierarchy, we are still struggling. At the risk of appearing to beat my own drum, I must say that economists have had a much better track record in system simulation—we have models which can describe, interpret and optimize systems at farm, regional, or even national level, in a deterministic or dynamic framework. Ours is a behavioral science and we have the theories and tools to impose behavioral patterns on technical parameters of great complexity. Why these have not been invoked is another story, for another occasion and audience. At this point I would only say that we have not been able to establish meaningful dialogue with agro-biological scientists who hold the other half of the treasure map.

Coming back to the theme, the point to note is that any external intervention in the system, be it in the form of a new irrigation project in the area, or a rise in price of a commodity grown there, a new road, or a new technology, interacts with the various dimensions of the system. The consequence may or may not be as expected. Those who look for impacts often attribute the consequences to the intervention. Sometimes a technological option fails to click and people take it out on researchers. This partial interpretation is not always fair. A correct diagnosis emerges only when we take a system view. Sometimes the intervention is inappropriate, sometimes other elements of the system block adoption. The point I am laboring to bring out is that
researchers who are aware of the system characteristics, can design research programmes which are more relevant and therefore less likely to be rejected. I should quickly clarify that I do not imply that scientists are not aware of the system characteristics. They are fully familiar of the physical and biological parameters of the system – soils, climate, growing conditions, etc. What they generally lack is an understanding to the socio-economic factors which also condition behavior, both at micro and micro levels.

It should be obvious by now that a farming system view is a much better way of looking at agricultural development, particularly at the micro level. Understanding the farming system of the target area should, therefore, be the starting point for any programme of development. Neglecting this has been the singular flaw of our development experience—and I am not talking only of the research system. Most of the programmes including those of agricultural research are centrally designed and ignore system differences.

**Farming system in agricultural research**

Agricultural research, regardless of discipline, has always focussed on subsystems—an enterprise, an input. Scientists are ‘specialists’, trained to focus only on the eye of the bird, as Dronacharya told Arjuna. This reductionist tradition has created disciplines and then further specialization within disciplines to add depth. Our methodologies are also designed to focus on critical variables and a few interactions.
This makes it very difficult to design a farming system research programme along conventional lines. Neither our training nor our tools can bear this burden.

The most common response of the research system to address this has been to assemble multi-disciplinary teams to tackle specific problems, with the hope that this inter-disciplinary interaction among specialists will lead to holistic analysis. This is another interesting story about which we shall have more to say later. Coming back to FSR, a large number of FSR programmes were hoisted by donors on many African research systems in the 70s and 80s with enthusiastic support from the World Bank. All of them have disappeared today or have been drastically recast. The only significant output from these was an occasional good insight which arose from careful description and analysis of existing systems. It proved to be a good tool for diagnosis, no one had a clue about designing a research programme on this basis.

Fortunately, we were spared these painful and infructuous experiments and one must be thankful to the leaders of the research system in those times who could resist these pressures. We did have a number of seminars on the theme, but not much more. We responded to the challenge of low adoption in a different mode. Our research leaders articulated that this was a problem of inefficient transfer of technology and it could only be improved by better researcher—farmer linkage. I presume the success of the scientists’-managed national demonstration programme in late 60s and early 70s prompted this view. Anyway, this idea has persisted and starting with operational research programmes of the early days, we had lab-to-
land, KVK and, now, IVLP. In other words, our research system has remained locked to the transfer of technology framework, pinning its hopes on an improved feedback process to guide the research system. Since these programmes are managed by scientists themselves, one expected an objective and effective feedback. Unfortunately, like our story on multi-disciplinary research, we have not much to write about this experience either. I have not seen one credible scientific study evaluating these programmes, since IIMA did one for the first generation ORPs. Incidentally, this study showed that all except one failed. There has been no evidence, except anecdotal, on how the feedback process has influenced research agenda. Interestingly, in ICAR, and also SAUs I presume, such programmes are under the extension division which continues to be biased towards forward linkages only. It is my personal view that such programmes have not provided much system-based guidance to the research system so far as determination of research agenda is concerned. These continue to be driven by supply-rather than demand-based considerations.

There are two other initiatives which are relevant in this regard—trials on farmers’ fields and cropping systems research. We need to say a little on these as well. Trials on farmers’ fields were initiated in the fifties, mainly with a view to verify fertilizer response under farmer conditions and derive meaningful coefficients which could be used for planning purposes. This was a national effort which provided valuable information for planning fertilizer production and distribution policies and programmes as well as fertilizer recommendations. In late seventies, the approach
was extended to cover other elements of improved technology and even improved
cropping systems. This has been more of a demonstration kind of effort and there is
little evidence of feedback. The NARP and, recently, NATP, placed considerable
emphasis on adaptive on-farm trials but we have completely failed to document
how these influence research.

I think there is a basic flaw in the concept in our minds. We take those ideas for on-
farm testing on which we have put in considerable efforts at the experiment station.
It is difficult for us to accept that on-farm results could be different. That would
question the credibility of our on-station work. So, even though we call these as
OFT, we make sure that we get results which are similar to those obtained on
experiment station. Moreover, such trials are very casually analysed and interpreted.
Even the normal courtesy of statistical analysis are not observed. This shows quite
clearly that the concept has not fitted as part of the research process. Even our
statisticians have not been able to provide adequate guidance on experimental design
and analysis protocols for on-farm trials. This reduces on-farm and adaptive trials to
a non-rigorous, demonstration-centred programme which does not interest researchers.
In any case, lack of rigour precludes publication of these results in 'scientific' journals
and scientists donot like to waste their time analysing or writing these up. We find
them only in annual reports which are rarely read.

I now make a provocative statement. In almost all cases, results of on-farm trials
show that the suggested improvements outperform local practices. This has been
the conclusion of our field demonstrations from the earliest times. From these we continue to draw the inference that reasons for non-adoption lie elsewhere. Since this is rarely investigated by those who conduct these trials, we don't really gain anything.

On-farm research is meaningless unless it is interpreted in context of the system. There are examples of such insights—farmers rejecting a variety because of its unsuitability in terms of taste or local recipes, or poor fodder quality, or clash with sowing of succeeding crop, and so on, but these emerged after a lot of time was wasted in trying to push the variety. On-farm research does not mean mere verification; its value lies in guiding future research and that is not possible without a system view. It is heartening to note that the PDCSR has initiated a systematic study of farming systems all over the country. This was overdue, but we need a lot more work linking technologies to system attributes. Most of us erroneously feel that only economists, extensionists and agronomists have to do this.

We now come to cropping systems research. What I say now should not be viewed as a critique of the PDCSR which has made excellent contributions to our understanding of crop management. But do its programmes qualify the 'system' test? Substituting cropping system for crop rotations does not accomplish this. What has actually happened is that conventional crop management research themes have been transposed on rotations. This has taken it a step further in the systems hierarchy and that is a distinct improvement. But the domain of such work is almost entirely
confined to physical-biological dimensions. The obsession with maximising production continues and yield level are still the primary discriminating criterion. In some cases, this is ridiculous because the main impact of the innovation is on cost and not yield. Experiments on tillage, direct seeding, or weedicides are cases in point where analysis focuses on yields and not costs. Profit maximization, efficiency of production, risk minimisation are important objectives for farmers but these are not used as evaluating criteria.

All socio-economic factors are sometimes subsumed under an omnibus "farmers’ practice" or "conventional practice" treatment. These are rarely explained or interpreted. In fact, I have a suspicion that treatment comparisons are of doubtful validity because the test treatments are not really transposed over farmers’ practice. To illustrate, I suspect that evaluation of improved practices is not really done in context of farmers’ practices in terms of non-treatment variables. A trial on integrated nutrient management on farmers’ fields compares treatment yields over the fertilizer practices of farmers, but uses optimum levels of other inputs like irrigation. This is not proper because farmers’ use of fertilizer levels is based on availability of other inputs as well. All these need to be imposed over other treatments as well for valid comparison.

In this context, I find the idea of imposing generic themes across locations quite contradictory. As mentioned earlier, location-specificity of systems is the crux of FSR. Yet we follow an AICRP format to report the results of such work. In this
process the essence is lost. My basic question is: can there be a national FSR programme? If a prescriptive role is expected, the answer would be negative. However, a lot of upstream work needs to be done on concepts, methodologies, coordination and synthesis. Should we then take another look at the mandate of PDCSR?

As I make these observations, I must confess that I am not a statistician nor do I have authoritative knowledge of the programmes of PDCSR or other on-farm experiments. I have taken this opportunity to air my doubts and I shall be the happiest person if these are shown to be incorrect. In that case, these comments may be attributed to ignorance and perhaps senility, and ignored. I shall gladly apologise.

Another initiative recently being experimented is that of 'farmer participatory breeding'. This directly involves farmers in deciding breeding objectives, testing, and verification processes. Other themes are also being pursued in this participatory mode. This is an extreme example of demand-driven research. I have doubts whether this is an efficient process in terms of our research resource endowments. Moreover, this basic idea was there when we created the zonal research stations all over the country. Why that did not take roots is another story. We do not have time to go into that, suffice it to mention that farmer participatory approaches of any kind are relevant and efficient only in location-specific contexts. Unless we truly decentralise and strengthen research capacity at this level, the thrust on on-farm research will remain unrealised. The state agricultural universities which control the ZRS need to think seriously about it.
It is clear to me that despite hundreds of seminars we are still groping to develop a model for FSR. As I said earlier, systems are complex and this complexity defies conventional research approaches. Yet somehow the concept must be integrated for improving the efficiency of the research system.

As an economist in the research system, I share the blame for the inadequacies in this regard. Our fraternity has the training as well as tools for capturing these elements but we have been busy with our disciplinary agenda and have not contributed beyond simple profit-loss calculations and even that is often flawed. We may not be aware of it but the system now expects us to make such contributions. Ignoring these issues will undermine our credibility. The onus is on us and this challenge cannot be addressed unless we aggressively seek multi-disciplinarity.

**What do we do?**

Can we organise FSR as we do wheat or buffaloe or soils research? I am convinced that this is infeasible. It will be impossible to assemble all relevant disciplines in one structured organisation. Even PDCSR has glaring omissions in this regard. It is inconceivable that such research units can be established everywhere.

One of the early exponents of the FSR concept, Michael Collinson proposed the idea of 'research with a system perspective'. He argued that all applied agricultural research should be planned with this perspective in mind. This ensures relevance
and clearly identifies system complementarities and conflicts. A varietal improvement research programme, for example, would then focus not only on yield and disease resistance, but also build in rotational factors, preferences of farmers, by-product attributes, etc. rather than finding out ex post that these system constraints hindered adoption of the variety developed. I would strongly recommend adoption of such framework. It is simple, conceptually elegant, easy to organise, and least disruptive. We only need to enlarge our vision as scientists. We will continue to work as specialists but we shall tailor our work to the realities of the system.

Implementation of this framework, however, requires fresh thinking and a reorientation of our outlook. Firstly, it presupposes that all researchers have an understanding of farming systems in their target domains—what farmers do and why? The former can be documented relatively easily. That is what the PDCSR has embarked upon on a countrywide basis. I think that as time passes, more local research resources will be devoted to such descriptions as scientists from different disciplines seek specific information. Understanding why farmers do what they do requires more analysis and a conscious effort on the part of scientists to look for interactions beyond their disciplinary domains. To understand varietal choice of farmers, for example, breeders will need to factor in consumer taste, price differentials, storability, by-product use, etc.—matters which they generally consider outside their domain. Other disciplines and the extension system is expected to provide this feedback. This is what we have been trained to expect though all of us agree that such feedback linkage has been a historical weakness of the system. Exceptions apart, few scientists undertake field visits, or even talk to extension workers.
Is this very difficult? I would like to submit that such orientation does not require difficult skills. All of us are taking decisions everyday on things for which we are not trained, including share market transactions. We exercise commonsense. Most of us with our omnibus Bachelor’s degree in agriculture, have a reasonable idea of the basics anyway. The above proposal implies only one burden—interaction with farmers in the client domain and a willingness to talk to other specialists. This will provide the necessary perspective which can then shape our research plans.

Secondly, this would require structural changes, some of which are simple others are not so simple. For example, a larger budget provision for field visits for scientists will have to be provided. In today’s climate of tight budgets, implementing this will pose problems. In any case, as we plan for incremental resources in NATP or Tenth Plan, this need can be covered. The more difficult part would be to reconcile this ideology with the centralist mode of research organisation which has become so dominant today. This requires major policy debate and I donot wish to argue this case today. I would only reiterate that all applied research which is targeted to given zone or eco-region must be decentralized and organised in this mode.

Thirdly, there are some basic problems with our incentive and reward system. Researchers spending their time on-farm work are rarely recognised. When that time comes, we go back to scientific publications. Few professional journals would publish such work. Young people find this frustrating and shy away. The seniors find this pedestrian and non-scientific.