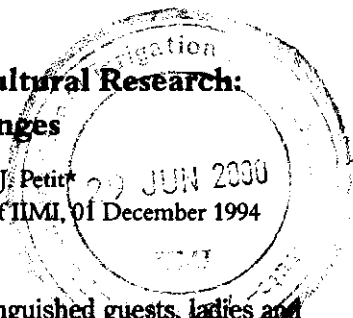


## **Trends in International Agricultural Research: Crises and Challenges**

Keynote Address by Michel J. Petit

Given at the Tenth Anniversary Celebrations of IIMI, 01 December 1994



The Honorable Minister, Mr. Chairman, distinguished guests, ladies and gentlemen, and dear colleagues: I am greatly honored to have been invited to address you on the 10th anniversary of IIMI. I would like to reflect today on the future of international agricultural research, a topic that is very sensitive and, of course, of great interest and concern to you. I will try to draw from these reflections some implications not only for agricultural research in general but for irrigation research in particular.

Given past achievements in agricultural research, it is somewhat surprising that agricultural research is facing a serious crisis worldwide, especially with regard to funding. The crisis probably results from a misplaced sense of complacency regarding the food situation. Yet this very crisis is the source of tremendous opportunities. It behooves all of us, whatever place we occupy in agricultural research, to seize those challenges and bring about the needed changes.

Some change has already occurred. This is particularly clear if one remembers the creation of CIMMYT and IRRI, the first two international agricultural research centers that are now a part of the CGIAR. New ways of funding and conducting international agricultural research have been found in the past, and that is something I would like to stress here. There is indeed a silver lining to this crisis; already we see signs of the new ways of doing business, of the new partnerships which must be forged. These are reasons for hope.

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## **Future of Agricultural Production and Research**

I would like to emphasize that the complacency in agricultural research is not justified. Since I am preaching to the converted here, I trust I need not overemphasize this point; however, there is enough of a debate that we need to collectively take stock of it. Personally, I have been convinced by the argument presented by Jock Anderson and Pierre Crosson, who looked at prospects for cereal production (cereals, of course, are the principal food grains). They present essentially a three-part argument.

First, we do not need a rate of growth of agricultural production in the next two or three decades that is as rapid as it has been in the past. And this first observation gives some scope for optimism. The second point, however, is that growth in the recent past has been exceptional. It relied on three major sources, namely the expansion of land under cultivation, the intensification of agriculture (due both to the expansion of irrigation and to the increase in the use of chemical inputs—particularly fertilizers), and biological progress. The third point of the Anderson-Crosson analysis is that we cannot rely on the same sources of growth in the future unless mankind would be ready to bear tremendous economic and environmental costs. There is not that much land available for expansion in the developing world, and the bulk of it is in the basins of the Amazon and Congo rivers. Both the economic costs (in terms of infrastructure needed) and the environmental costs of bringing this land under cultivation would be very high. We cannot rely on the expansion of irrigated area. Also, even though there is scope for increasing the use of fertilizer in many developing countries (particularly in Africa), there are limits also, again both economic and environmental.

Anderson and Crosson conclude that we need to have new sources of growth in agricultural output. The main sources of growth that spring to mind are both the continuation of biological progress and the more intelligent use of the natural resources that are available, particularly water for irrigation. The factors of production that have to be mobilized are knowledge-intensive and therefore there is a tremendous need for research and technology transfer, as well as for other factors that are knowledge-intensive, such as institutions.

Mr. Alexander McCalla's recent Sir John Crawford's lecture shows that even though there is tremendous variation among experts on the long-term prospects for food production in the world, all of the projections (even those optimistic projections that say there will not be a worldwide food crisis) rely on the continuation of growth and productivity. These projections therefore assume that agricultural research will continue and they therefore *must* assume that research funding will continue to be provided in sufficient quantity.

But we are now in the midst of a financial crisis. Many believe that the crisis is not only of a financial nature. Thus to overcome it, we must analyze the weaknesses of our current system of international agricultural research, so that we may derive from that analysis the actions that seem to be needed for the future. I would like also to address the consequences for irrigation research specifically.

### **Current Weaknesses of International Agricultural Research**

The first major weakness is that the NARS—the national agricultural research systems—are not as productive as they could be, and of course not as productive as they should be. There are several reasons for this, most obviously the internal allocation of resources. There has been (and that's part of the change that has occurred) a significant increase in the number of trained scientists in developing countries since CIMMYT and IRRI were created 30-35 years ago. In many countries, however, including this one, those scientists lack the means or resources to be as effective as they could be. Examining the total resources available gives the sense that there is too much manpower and not enough other resources. In a few words, the allocation of resources is not proper. Perhaps the overall resources are also insufficient, but until existing resources are utilized more effectively, it will be very difficult to convince decision makers that more resources are warranted.

There is another weakness associated with this: the set of bureaucratic constraints under which many national research institutions labor. Many of these agencies are not esteemed in government circles. They have low

productivity, low visibility, and therefore, a low level of support, both financial and political. In fact, there is a vicious circle, because as long as this low support remains, there is no reason why productivity will increase—and that's clearly the weakest part of the overall system. Unfortunately it is not the only weakness.

Before proceeding, I would like to emphasize that I am looking at the overall situation. One can and should differentiate among the NARS; there are tremendous variations both within and among regions, but I unfortunately do not have time to explore these.

A second major weakness is that the international agricultural research center programs are not integrated enough with the programs of the NARS. I am not going to argue that the international centers are so isolated that they do not have relationships with the NARS: that would be an insult to the international centers and to IIMI in particular. The point is not that there is no relationship between the international centers and the NARS, but if we accept the notional figure that the budget of the CGIAR (that is, the budget of the international centers) represents roughly 4 percent of the total funds devoted to agricultural research for developing countries, then clearly, for the international centers to be effective and to play a catalytic role, they must be completely integrated within the overall effort. Yet the degree of integration is presently insufficient.

The third major weakness is what I term an "identity crisis" in the specialized research institutions in the North. A significant number of countries in North America, Europe and Japan have specialized institutions devoted to agricultural research for tropical and subtropical regions. The identity crisis manifests itself first at the level of funding. For example, in the United States, the support by the U.S. Agency for International Development, or USAID, through its so-called collaborative research programs, has fallen precipitously. Similarly, other instruments used by USAID to mobilize the tremendous intellectual resources that reside in the U.S. "land-grant" universities have also fallen. My own country, France, which probably has the second largest stock of intellectual resources for tropical agriculture has two strong specialized institutions, namely CIRAD and ORSTOM, which are not doing too badly at the moment. My conviction is that they

are already beginning to face a crisis of governance and that they are not financially sustainable. I doubt very much whether the French government is willing to continue to support this high level of effort in tropical agriculture. The commitment to tropical agricultural research in other European countries is also declining.

The first manifestation of the identity crisis is the funding. The second one is a questioning of the need for specialized research institutions for tropical agriculture. With the spectacular revolution in the biological sciences, in particular, the distinction between what is research for temperate agriculture and what is research for tropical agriculture is less clear-cut. Therefore, the specialized institutions do not have a completely clear sense of mission. This also contributes to an identity crisis because, frankly, the other institutions are far less devoted to the progress of tropical agriculture than the specialized institutions such as CIRAD and ORSTOM.

These first three major weaknesses—underproductive national institutions, lack of integration of International Centers programs, and the funding and “identity crisis” of the specialized research institutions in the North—lead me to conclude that we are on the verge of a tremendous redistribution of roles and of labor in international agricultural research. We may be on the verge of a change as significant as the creation of the International Centers 30 years ago. This may be a strong statement, and I may be wrong. The probability that I am right, however, is high enough that the problem deserves attention.

I would like to turn to a fourth major weakness that has little to do directly with institutions, but a great deal to do with them indirectly. This is the absence of a proven research paradigm for natural resources management research. The Honorable Minister already alluded to this fact. There is a fairly well-proven paradigm for increasing the productivity of crops. For commodity improvement, plant breeding is the central discipline since it is capable of mobilizing other disciplines, even agronomy and economics. With commodity programs, we have a paradigm, or an approach, that has proven its effectiveness. It can even incorporate progress in biology through biotechnology, providing the plant breeder with new tools to do his or her work.

I would like to contrast this with the situation for natural resources management. Whether for soils, forestry, or aquatic resources the research paradigm is much less well-established; we are not absolutely sure what needs to be done. We know one common comment is that those problems are very site-specific, and therefore there is a question as to whether there is any sense in conducting strategic research on those issues. My own conviction is that there are concepts and hypotheses that are generic in nature and therefore the corresponding theoretical and methodological issues are appropriate strategic research topics. Yet we still do not have a proven research method or strategy to address those issues.

Given these four main weaknesses, what can we do about them? What are the directions to suggest for the improvement of international agricultural research in the future?

### **Challenges and Directions for International Agricultural Research**

The first challenge is to develop an effective paradigm for natural resources management research. This is really a challenge to the scientific community. I am not a researcher anymore, but since there are quite a few researchers here, this point is addressed especially to you. The systems approach is probably going to be useful in this endeavor. Ecology, as a science, probably has something to teach us because of its use of the systems approach. Clearly, a systems approach would need to incorporate the social dimension. We need to have some merger or some synthesis of ecology and social sciences. That is a very difficult intellectual challenge, but it is precisely the one that needs to be faced. Unless we find some satisfactory solution to that problem, we have hardly a chance to solve the other problems with which we are confronted.

The second challenge is to develop partnerships. Given the expansion in the number of scientists in the national systems, it is clear that International Centers, in particular, have to work in closer partnership with the national systems than before.

A dimension of this challenge is revealed by the use of the word *system* in the common expression "research system". We often speak of "NARS," or

national agricultural research systems. By system, we usually mean the set of various partners within the nation or society that can contribute to the progress of knowledge. One of these groups of partners comprises the universities and other teaching institutions. There is no doubt that those institutions have a lot to contribute to the research effort. Also, their greater involvement in research will make them much better teaching institutions. If we want to renew the human resources devoted to agricultural research, those institutions have a critical role to play; it is fair to say that the level of integration of those teaching institutions in the research effort is far less than it should be.

Another group of partners who are important in the system are the users of agricultural research. These include farmers, community-based organizations, and nongovernment organizations. It is clear that individual farmers, for example, have something to contribute to commodity-oriented research, both in terms of specifying the nature of the problems to be addressed by the research and also of contributing to the evaluation of the relevance of the research. And as you know, several methods of participatory research have been invented for the purpose. It is even more necessary to involve the users in research on the management of natural resources. As people involved in irrigation research know, many of the users of research are collective actors, including groups of farmers as well as all kinds of organizations. At IIMI, there is a specific program devoted to those issues, and I am convinced that the involvement of research users should be a high priority for all research organizations.

Advanced research teams are another set of partners. While many of them are in the North, more and more teams can also be found in the South. Improving collaboration with universities, for example, could improve the possibilities for collaborating with advanced research teams in the South as well as in the North. This is particularly obvious for agricultural research dealing with biological sciences, because of the tremendous progress in biology. While many countries and organizations already have partnerships with advanced research teams, I submit that these will have to be increased significantly in the future, because such collaboration is full of potential breakthroughs.

The need for a greater degree of partnerships all over the world creates a tremendous opportunity for the International Agricultural Research Centers. It is of course nice to say that partnerships are necessary, but we also know that they are costly to organize; the transaction costs are indeed very high. Here, the International Centers have a tremendous comparative advantage as conveners, facilitators, and builders of partnerships. The main role of the Centers may not be to do research but to ensure that the various competencies are mobilized on problems. Let me call this a 'convener' function. I believe this is a very significant role for International Centers in the future.

### **World Bank Role**

I would like now to address the challenges that the current international agricultural research situation present to my organization, the World Bank, and therefore to me as an individual. The first challenge is to design projects that support international agricultural research in such a way as to provide proper incentives, especially incentives that promote collaboration. As cosponsor of the CGIAR and as Chairman of its Finance Committee, my influence can be exerted through the incentives. We want these incentives to be such that you do the things that we believe need to be done.

A second challenge for me is to provide the most effective support possible to the renewal of the CGIAR. My Vice President, Mr. Ismail Serageldin, who is the Chairman of the CGIAR, has exerted vigorous leadership to renew the CGIAR system and sustain financial support to the Group. We have been successful in 1994 and 1995. This process will culminate in a high-level meeting in February. The heads of the cosponsoring agencies are inviting high-level representatives of the donors and of developing countries (hopefully at the ministerial level) to rededicate themselves to the CGIAR. I am convinced that this is going to be possible only if the agenda of the meeting is not inward-looking-oriented only toward the Consultative Group, but is instead devoted to the future of the global system of international agricultural research and to the themes that I am discussing here. We would like to interest and convince the high-level officials that through agricultural research they have an outstanding instru-



ment to tackle fundamental problems confronting mankind—the rising populations and the threat to natural resources. This is probably the agenda that could provoke the enthusiasm that will hopefully ensure the future of the CGIAR.

As you may know, the proposal has been made to create and encourage global and regional fora to place the CGIAR in an overall context, and to have a place where the various stakeholders or partners have a voice in defining what the overall context will be and in defining the needs for CGIAR contributions within that context. In the past, we have essentially assumed what the needs should be and we have defined what the comparative advantage of International Agricultural Research Centers would be without giving enough of a voice to the other partners, particularly to the NARS. In spite of some attention to this issue, we have not catered at all to the advanced research organizations in the North or in the South and not at all to the final users of the research. Participation is easier said than done. I must admit to share some apprehension about our ability to create effective fora for that purpose. It is always easy to have a big meeting, but making it effective is not that simple.

One role that the World Bank can play is to contribute to the political support for agricultural research, mainly in the developing countries. We are probably the only organization dealing with agriculture that is convinced that agriculture and agricultural research are very important for the main development agenda, which is alleviating poverty. The World Bank is the only organization dealing with agriculture that has access to all levels of government. The challenge for me within the Bank is to convince my colleagues, who have access to the Ministers of Finance and the Prime Ministers, that indeed they have to put agriculture and agricultural research high on their agenda.

## Conclusion

I would like to conclude my remarks by briefly addressing irrigation research. I submit that the paradigm issue—that is, creating a natural resources research paradigm—is the key challenge to irrigation research.

It is incumbent on an institute such as IIMI, which is devoted to irrigation management (and therefore to perhaps the most important natural resource, water) to create that paradigm. I personally feel quite close to the intellectual enterprise here; I was at the Ford Foundation in the 1970s, and my successors in the Delhi office crystallized the idea of IIMI. I was also at the World Bank when the IPTRID program was created. We spent quite a bit of time thinking about the relationship between technology and irrigation management research; many of these debates were clouded by the fact that we do not have an adequate research paradigm. IIMI has devoted a lot of time to thinking about the relationship between field programs and research at headquarters. If you had a fully developed and clearly understood research paradigm, the complementary nature of these two programs would appear and this would probably facilitate your work.

IIMI has collaborative partnerships. Many of them born of necessity, because IIMI doesn't have any experiment station (as the biologists do) or mass of data (as the economists do) to work with. IIMI must work closely with irrigation managers in order to say anything sensible about irrigation. IIMI has learned lessons that the whole CGIAR has yet to learn from. As I suggested, the role of "convener" may be important to think about when reflecting on the future of IIMI.

Finally, I am certain the World Bank will continue to be a partner in international agricultural research. My own group in the Bank, dealing with agricultural research and trying to spur the support of the Bank to the global effort in this field, is devoted to helping other organizations as well. I therefore hope you will consider the Bank as a privileged partner. You can count on my personal commitment.

Thank you very much.