AGRICULTURAL RESEARCH PRIORITIES AND PROBLEMS:
Reflections from the World Bank's "Sector Policy Paper"

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Why Research? Why the Bank?

> Carefully implemented agricultural research can be an efficient source of economic growth and is an important contributor to the achievement of key development objectives ... (especially) Adequate food ... for much of the developing world ... investment must be significantly expanded to improve the capacity to conduct agricultural research ....

Agricultural Research's raison d'etre is thus squarely laid out on page 5 opening its "Summary and Recommendations". The reason for World Bank concern (related on page 11 to CGIAR and the ICRIS but elsewhere to agricultural research/extension at national level) flows from what it sees as:

> the Bank's dominant position as the supplier of external resources for agriculture and rural development.

In short research is critical to enhanced production (and to how it is distributed) and the Bank is heavily involved in furthering agricultural production. Therefore, it should both help finance research and form a view on how much and what type of research to finance and encourage.

Six Main Themes

The Report revolves around six main themes - or perhaps more accurately hypotheses supported (in most cases) by fairly convincing evidence and argument.

First, because land availability is increasingly limited, production per hectare increases of 2% a year will become increasingly important in

most Third World countries. Achieving this will require research. (A caveat might be entered that improving extension/use of what is already known and identifying/extending best known peasant techniques are critical complements, but they are not substitutes especially in the longer term.)

Second, Third World agricultural research is inadequate in quantity (using a 2% of agricultural GDP target which, while rough and ready, seems a reasonable rule of thumb) and especially so for food crops relevant to very poor farmers and consumers.

Third, national research often lacks overall design, specified objectives, field testing, links to extension and knowledge of actual farm conditions so that even the resources spend are not producing the results they might. (This might give pause - in the countries to which it applies most forcibly - before embarking on a 10% a year real research growth programme as recommended. Perhaps the base needs to be redesigned and reconstructed first?).

Fourth, the ICRIS/CGIAR network are 12½% of all TW agricultural research at 3% of all world agricultural research. They need selective expansion but also better links with national systems. (Amen seems the only answer to that but the implication that the gaps are all the result of the national systems, with none relating to "the big fat cigar" as a bitter national research official called it, seems much too sweeping.)

Fifth, the income distribution effects of agricultural research are not well understood. Absolute (and perhaps relative) inequalities among peasants is increased, tenant farmers often lose their land, low income consumers benefit, isolated and resource-poor geographic areas are further immiserized/marginalised. Part of this results from failure to specify income distribution goals and relate research to finding results serviceable in meeting them. (The starkness of the results seems understated while the commitment to research oriented to helping the poor peasant produce more is episodic rather than integral in the main text - and still more so in the Bank's actural rural production programme support.)

Sixth, research will not/cannot be adopted if the economic environment confronting farmers is unfavourable, eg. if prices are too low. (Concentrating on prices without equal emphasis on extension, land tenure, input availability, the relative cost shifts against mechanisation/ fertilisation resulting from the hydrocarbon price increases, the impact of lack of health - education - pure water on time use as well as ability to work and willingness to stay in farming seems somewhat surrealistic. Prices are important, like research, but also like research they are not everything.)

The Bank and Agricultural Research - Lessons of Experience

Agricultural Research reviews the Bank's long and not insignificant experience in the field with considerable incisiveness. This is notably not a Bank publication in which "The Bank can do no wrong", even if the reader may quibble at the degree of certainty implied for some of the new departures proposed.

Some of the "lessons of experience" are:

a. project research often is too small, too little related to overall national (or even crop) research and over too short a time frame to pay off;

b. support for research must involve concern with the sector's design and philosophy as well as bits and pieces within it if it is to pay off;

c. effective support for institutional and programme development requires a 10-15 year time frame in many cases (ie. "repeater" projects);

d. too many expatriates create too much discontinuity (and too much of a gap from actual farmers?);

e. too little interdisciplinary work (eg. re income distribution before the event), too little field testing in general and especially under conditions pertaining to actual peasant farmers has been included;

f. the value of ICRIS' work has been below potential because links with them have not been built into national programmes.

This is a commendable exercise in self-criticism even if the authors - perhaps understandably - do not put the record quite as pungently as

this. (However, all these points are made in the text - especially on pages 7-8.) In all candour, the Bank's record is no worse than average and other institutions are often less willing to face up to the fact of failures.

Toward a National Paradigm

From its review of the present situation, the Bank's experience and what needs to be done, the authors draw up six basic criteria/guidelines for national research programmes. These are related to (and modified by) the diversity of existing situations (regionally and nationally) which are summarised rather lucidly, if perhaps too succinctly.

First, agricultural research should receive resources equal to about 2% of GDP (in many cases requiring sustained medium-term 10% annual increases) backed by more professional and more technical staff.

Second, greater emphasis should be placed on nutrition, food crops directly relevant to poor consumers and to poor peasant growers, and on relating proposed improvements to what actual small peasants can understand/do/afford. (In this context risk reduction is mentioned but probably not stressed enough as inability to weather - often literally - setbacks is perhaps the most critical single factor holding back and/or immiserizing poor peasants in Africa and Latin America as well as Asia.)

Third, national research needs coherent sets of goals, programmes articulated toward (and plausibly adequate for) meeting them and a clear, two way interaction with extension.

Fourth, links to ICRIS to gain information and to do field testing and adaptation should be a central component in most national programmes. (Advocating ICRIS' setting up of sub-centres in the main sub-regions to which their work should be relevant might be a useful complement to this.)

Fifth, research philosophy should be multidisciplinary, based on field studies, related to use "circumstances" (unfortunately not described as including peasant perceptions, needs, targets and knowledge!).

Sixth, creating an environment and conditions conducive to holding on to staff and to encouraging them to be productive. (Unfortunately as with the 10% growth and the emphasis on prices as "the" evironmental factor this seems largely to mean "higher salaries" which has the demerits of trying to overcome complex problems with the simple brute force of money and that finance is in very scarce supply in most Third World countries).

Commendations, Caveats and Queries

Few studies on agriculatural research set out the situation, the weaknesses, the requirements and guidelines for development so succinctly, lucidly and accessibly to the non-specialist reader. Few in-house institutional studies demonstrate an equal rigour in looking at their own record and learning from it.

However, the Bank is not a homogenous - let alone a monolithic - body. Its research and consultancy reports (even when approved at Board level as this one has been) and its operational programmes often seem to have rather separate lines. How much Agricultural Research - especially its calls for research on income distribution and using income distribution goals in selecting areas of technical research as well as of relating to the conditions under which poor peasants actually live and cultivate - will become an integral part of Bank programmes and projects remains to be seen.

The Report raises a number of issues which seem to require further exploration (and to be more complex) than it provides. Given its broad scope and brevity this is as much a commendation of power to provide a stimulus to further thought as a criticism for simplification. Several of these topics are sketched in the balance of this review.

What Do We Know?

The data base on Third World agriculture is almost unbelievally weak. This is as true for research as for other topics. The Report uses the best (least bad) data available but some questions arise.

First, are research expenditures and research personnel really accurately known? For the countries the author knows best some of the expenditure and personnel data seem surprising (both high and low) compared with other sources.

Second, if expenditure in some cases is much above what is estimated (eg. near the 2% target) and results are still meagre, what does this imply about immediate expansion?

Third, if the FAO production per hectare data are even approximately correct, the cross country variations within regions are quite startling eg. -1% a year in 1970s for Nigeria and +2% a year for Tanzania in cereals; +2% a year for China and -½% a year for Thailand in legumes; +3% a year for Colombia and -½% a year for Jamaica in sugar cane. Static? Random fluctuations? Differences in research? Or what?

The point is not that action should be held up until the numbers are perfect. But they are so bad both a certain reserve about what they mean and a research priority to improving and interpreting them seem warranted.

Research Base and Project Content

How serious the lack of an adequate, field tested production oriented research base can be is well illustrated by the World Bank's Lake Nyanza area rural development project in Tanzania. The whole production improvement package consisted of three varieties of millet seed (not field tested), five cultivars of cassava (limited field testing) and seed dusting with insecticides (fully tested). The main three crops - in terms of cash income - of the region cotton, maize and paddy had nothing.

The projected gains from utilising the new "knowledge" were necessarily pure guesses (except for the seed dusting) as no evidential base existed. The acceptance rates and gross output growth (unit gains times numbers accepting) had no basis beyond pure guesses (or the rates of acceptance "needed" to get a 24% social rate of return?).

The results are hardly surprising. Output grew very little - except for cassava; one cultivator was successful leading to a surplus (leading to a loss by the public sector "last resort" purchaser) and to shifts out of the badly needed cotton and maize. Acceptance rates were lower than estimated. This could not be attributed to peasant inertia - acceptance of and village input into water supply, health and education facility and tree planting aspects of the project (all with more prior research and field testing than production) exceeded targets.

What Is A National Strategy?

The absence of coherent national research strategies is plain enough. So are some elements toward constructing them, eg. selecting production, consumption, distribution of gains goals on a crop by crop, region by region, target group by target group basis, and feeding these into technical research design in a way comprehensible for picking objectives such as risk of crop failure reduction, increased yield with low water and fertilizer inputs, reducing peak (but not necessarily overall) labour input demand (a serious constraint in many peasant systems). Similarly an institutional structure furthering both initiatives on new paths and response to former needs and potentials is critical. To achieve these requires closer political-professional, interdisciplinary research, professional and research-extension interaction than now exists in more than a handful of countries. To accomplish all this requires more resources - both personnel and financial - on a sustained basis.

To get that far is progress. But the sub-segment articulation in the Report seems to loose focus and cohesion. Finance, personnel, institutional structures, priority to research, ultimate (or political or real world) goals, technical goals (or means) are discussed separately with no clear indication of what systematic interactions are needed. One is still several steps away from having a set of guidelines for articulating a national research programme of the type Agricultural Research posits as necessary.

Extension, Extrusion and Excrescence

Extension services make repeated appearances in Agricultural Research, largely in calls for improving them, relating them better to research and to getting field testing and feedback from them. They are not examined systematically - perhaps because they are not usually considered to be research proper (albeit the Bank in projects and programmes tends to treat research and extension as a single, integrated component).

A taxonomic analysis would seem to be needed in many countries. In a number - notably in Africa - the extension services appear to have the poorest demonstrable overall benefit/cost ratios of all major heads of government expenditure and to survive - even if starved of funds - more because they are one of the things "every government has" than for any evident good they do beyond a few specialised crops and a few atypical officers.

What are some of the reasons?

a. nothing to extend (research inadequacy, lack of field testing, failure to learn best peasant techniques to generalise);

b. inability to comprehend what they are to extend (inadequate education and inadequate effort by researchers to reach their immediate "audience");

c. lack of locally relevant (as opposed to generalised, slightly out of focus to wierdly inappropriate) knowledge to extend (lack of articulated research and field testing, distrust of local extension workers ability to adapt and interpret from general guidelines, too frequent transfers of officers);

d. procedures for extension which are not time effective - eg. always using one on one as opposed to group contact and demonstration - or educationally unsound - eg. advising a farmer briefly twice a year with no demonstration of most of techniques and no comprehensible written or graphic aids to leave as reminders. (Failure to perceive that in a bsic sense agricultural extension is a branch of adult education and to organise both its substance and training for its personnel accordingly.);

e. recurrent shuffling of system personnel structures (eg. generalist to crop specialist and back) and institutional responsibilities (eg. ministry to crop authorities to regions to a random clutter of all) in a way maximising confusion and minimising stability (mistaking institutional reform as a cure for basic content deficiencies and/or pulling up the seedling to look at it every few days);

f. deep peasant suspicion and low acceptance (often from a history of inappropriate advice and/or advice which ignored the rational risk and pre-results cash outlay aversion of peasants);

g. staff who are objectively incompetent (underpaid, undertrained, of low status and - in many African field level cadres - probably, as a result, a fair sample of below average capability and diligence peasant farmers).

If this taxonomy even begins to hit the mark - and IRRI work showing how badly its advice was misextended (extended?) to peasants (especially small peasants in low income areas) suggests it may be quite general even among apparantly above average systems - there is a clear implication. Parallel to any national agricultural research development programme must go a comparable extension rehabilitation and redesign programme. More of the same or marginal adjustments are unlikely to be adequate in more than a few countries.

From CIGIAR to the Field

The Report's emphasis on national system linkages with and orientation to identifying, field testing, adopting and extending ICRIS work nationally is clearly justified. Its apparent view that all is well on the ICRI side vis a vis their openess and outrench to national programmes seems rather more controversial.

Especially (but is it uniquely?) in Africa most ICRIS seem to have little contact with national agencies and to do little to initiate such contact. When the nine Southern African states made semi-arid research (most of their agriculture is semi-arid) a joint priority and asked ICRISAT to establish a regional sub-station as a catalyst for increased and co-ordinated national work, ICRISAT clearly indicated lack of interest on its part.

Surely, more regional sub-centres and more ICRI initiative in explaining what they have to offer and how it can be used to national institutions is needed. The ICRIS have more money, more staff, more experience, more knowledge of what they have to offer and how it can be (and indeed has been) successfully used nationally than do a majority of the national research institutions.

Staffing - Issues of Quantity and Composition

Agricultural Research makes a good case that more fully qualified professionals are needed in most national research programmes and a plausible one that an 8% a year increase in their numbers (where technically feasible) would be desireable in a majority of countries. But is this the greatest personnel bottleneck?

The Report's own comment that - especially in Africa - technical, paraprofessional and other supporting staff ratios to fully qualified professionals are as low as under one to one versus four to one in many developed economy programmes deserves more followup than it is given. Since professionals are in very short supply and take a long time to train, the optimal ratio should surely be higher - say five to one - not lower than in countries with ample supplies of professionals. If that be the case, then ten to thirteen times as many additional technical/paraprofessional/ supporting staff are needed as fully qualified professionals.

In fact, agricultural research seems to be a fairly extreme example of a general problem - middle level personpower is often scarcer and its training less well provided for than high level personpower. As a result high level personpower are forced to do the work their supporting staff should do and/or fail to get much valid work done at all because of lack of backup. Middle level personpower requirement estimation and training development is often (especially in Africa) the central problem in personnel strategy, not a secondary or peripheral one.

"A Man Outstanding In His Field"

A peasant farmer and an agricultural research expert can each be described as "a man outstanding in his field"(more accurately, a person as many peasants

and some researchers are women but women make no appearance in this Report). But often they appear to have little else in common - their fields apparently do not adjoin let alone overlap.

The interdisciplinary approach - from setting research objectives and collecting baseline data through selecting technical research targets and field testing results to extension and evaluation - should at least make the fields adjoin. Its aim is to discover peasant crops and potentials (to see what crops and techniques deserve research priority), how what innovations could or could not actually be employed (ie. what typical "farm systems' are and to extend knowledge (and hopefully access to inputs, buying points, storage) in such a way as to raise "acceptance ratios".

So far so good but the approach remains top down and Platonic guardianship oriented (the standard weakness of the World Bank). Two way communication is indeed commended - among research and extension professionals of various disciplines, not between them and peasants. There is no clue that asking peasants what they needed or what their priorities were should or could be a key element in target setting and research design. (One baseline study for the Kigoma Regional project in Tanzania done by a local institution for the Bank, did adopt that approach. But its very interesting findings as to peasant priorities do not seem to have influenced project design at all.)

Still less is there any hint that "best known" peasant technology/systems is often much better than "average in use" and more easily extendable/ adaptable to other peasants (including by demonstrations by the peasants who use it). Unprofessional this concept may be and "dangerously" participatory, but especially until more field tested modern research results are to hand it has substantial potential (at least partly realised on the too rare occasions it has been practiced) for raising yields. It is also the way to make the peasants and the research experts' fields overlap securing a genuine common understanding and effort.

Who Is To Get What? Why? How?

Distribution is a central political economic issue and targeting who gets what and how a central policy and research problem. That point of view is fully accepted by Agricultural Research - unlike some more recent Bank reports which seem to view distribution as a dispensable optional extra which should be stripped off the production vehicle when hard times come.

This approach has several implications for research design, operation, extension and review.

First, more knowledge about present income distribution (by area, occupational group, size of holding etc.), consumption patterns/sources of income in relation to income distribution, impact of post agricultural research and extension is needed. Precision (especially neat decile scales) is not essential nor for many purposes much to the point - a reasonably accurate, even if rough sketch map of reality and the directions in which it is changing will serve.

Second, a rough set of goals as to distribution (who to benefit? How much? Where? In what ways?) is needed from political decision takers. (This is not an area in which technicians can decide and make their decisions stick - nor should it be. Nor is an agricultural research institute usually a very promising institutional base for overturning an oligarchy or junta). These then need to be articulated in a way allowing identification of a technical research programme which, if successful, would move income distribution in the desired direction - a task for inter (multi) disciplinary professional cooperation and dialogue.

Third, identifying research approaches which can benefit poor farmers (if they are a priority group as in the Report). These do exist but are not likely to be chosen - eg. by seed breeders - unless the technical parameter sought - eg. low risk, suitable for poor soil, adapted to low and erratic rainfall - is specified because these characteristics do not usually go together with maximum output per hectare of good land with high, assured water and input availability.

Fourth, reorienting extension services to save low income peasant households (especially those headed by women) who normally receive less and less competent attention and creating an institutional structure which actually gives such households at least equal access to seeds, tools, fertiliser and other necessary inputs and non-exploitative buyers (admitedly much easier to specify than to design and to design than to achieve!).

Fifth, to design ongoing evaluation of results in terms of distribution (of cash income, of household food supply, of rural jobs and wage-rates) so that negative deviations from planned/desired results can be identified in time to take corrective action before they become entrenched patterns.

The Invisible Women

In one respect Agricultural Research does not break with the main tradition of Bank reports and studies. Women are invisible (in this case not even being "given" a token page or two under a "women's issues" rubric). This is a trifle surprising since many peasant farmers (a majority of household heads in some areas and a majority of workers in most) are women and since problems of intra-household labour, decision taking, production and income division have been repeatedly demonstrated to be critical to understanding, working with, introducing changes in peasant agricultural systems.

This invisibility of women presumably partly explains lack of attention to two key aspects in peasant household "provisioning" and capacity to work - water and fuel - since both are predominantly "women's (and girls) work". In some areas half women's time goes to collecting wood and fetching water with fairly evident implications for peak crop season labour availability (and/or availability of water and cooked food to sustain labour in the fields during such periods). Further, lack of pure water is a (in some cases the) major cause of disease which tends to lower ability to work and therefore lower food availability and greater susceptibility to disease. While by no means attacking only women, such diseases seem to have a particularly severe impact on infant, young child and mother mortality. Thus research on improving access to pure water and to low cost

(preferably labour cost only fuel) is not a "social" or "amenity" concern - it is part and parcel of comprehensive agricultural research oriented to raising peasant production potentialities and attainments.

In some cases the same is true of food preparation. For example, in much of Africa, millet is a low risk crop compared to maize because it is more drought resistant. However, it requires much longer to prepare (decorticate and grind) and must be prepared more frequently (because the meal spoils quickly). This high preparation time cost is one reason improved millet seed, even when available, does not have high acceptance ratios - especially when the women who do the preparation are also the main household grain growers. In Botswana research on improved (time saving) preparation of millet seems to have increased acceptance of growing millet. The payoff on selected similar agricultural research in associated time cost reduction (and not just in millet) would probably be high. The invisibility of women - quite apart from its normative implications - is a barrier to professionally competent and comprehensive agricultural research designing and to raising agricultural production.