CONSULTATIVE GROUP ON INTERNATIONAL AGRICULTURAL RESEARCH

TECHNICAL ADVISORY COMMITTEE

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NOTES FOR DISCUSSION IN TAC ON PRIORITIES IN INTERNATIONAL AGRICULTURAL RESEARCH

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(Item 3)

TAC SECRETARIAT

FOOD AND AGRICULTURE ORGANIZATION OF THE UNITED NATIONS

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WS/5299
Terms of Reference:

1. Any discussion of priorities in international research must begin with TAC's terms of reference. These are:

"TAC will, acting either upon reference from the Consultative Group or on its own initiative:

(i) advise the Consultative Group on the main gaps and priorities in agricultural research related to the problems of the developing countries, both in the technical and socio-economic fields, based on a continuing review of existing national, regional and international research activities;

(ii) recommend to the Consultative Group feasibility studies designed to explore in depth how best to organise and conduct agricultural research on priority problems, particularly those calling for international or regional efforts;

(iii) examine the results of these or other feasibility studies and present its views and recommendations for action for the guidance of the Consultative Group;

(iv) advise the Consultative Group on the effectiveness of specific existing international research programmes; and

(v) in other ways encourage the creation of an international network of research institutions and the effective interchange of information among them.

These Terms of Reference may be amended from time to time by the Consultative Group."

* These notes are a revision of those written in April 1972 and circulated to members (16). I have taken into account some comments by members as well as the incidental discussions in our meetings during 1972.

Numbers in the text refer to the appended reference list.
2. There are in these terms several points which call for stress in relation to this paper:

(a) TAC is expected to act on its own initiative. This is particularly true in the matter of "main gaps and priorities";

(b) The work of TAC is explicitly related to the problems of developing countries both in technical (agricultural) and socio-economic fields. Research in developed countries is not irrelevant but is of concern to TAC only when it is or can be linked with the problems of developing countries as, for example, in soya bean research;

(c) TAC is not limited in its work of review to international research, but term of reference (ii) does stress "international or regional efforts". This has obvious implications for TAC. While its direct interest has been in "international" research TAC has frequently stressed the necessity of vital links between "international" and national research efforts - a point also implicit in term of reference (v). These links have been seen to have important two-way "feed-back" effects: as well as support national research efforts, the links ensure vitality in the research of the "international" centres. This vitality will be lost if the research results of the international centres are not tested under a wide variety of environmental conditions to be
found within national boundaries. International research complements rather than supplants national effort;

(d) A part of the linkage between national and international research is the training of national leaders in research and extension;

(e) TAC is advisory only: the Consultative Group will not act on important research proposals without prior advice from TAC but remains free to reject or modify the advice it receives;

(f) The terms of reference do not define "international or regional" research and certain problems have arisen in this respect which are likely to become more and more important as our work proceeds. TAC has recognised the basic inspiration in establishing the Consultative Group of the established international research centres represented by CIMMYT, IRRI, CITA and IITA. It has valued and stressed the importance of the independence in recruitment and methods of work enjoyed by these centres and would continue to stress these attributes in its recommendations. However, as we shall discuss later, it has found no easy way to separate international from regional. It has met cases where national research offers a clear base for international effort - especially in the transfer of concepts and basic research materials and procedures.
3. Before leaving the terms of reference it will be useful to emphasise a few other points particularly related to the end objectives of the work of TAC. Clearly on this it is essential if priorities are to be sorted out. Thus we are concerned with developing countries. In general terms this means that the basic premise of the Consultative Group (CG) system of which TAC is a vital part is that international research is crucial to the process of agricultural advance so necessary in many developing countries. The emphasis on international research (loosely defined as research carried out explicitly to serve the needs of a number of countries) follows from the wisdom of economising the use of scarce skilled personnel and expensive equipment - much beyond the economic ability of most developing countries adequately to organise.

4. It would be within the competence of TAC to see research simply in terms of the technical improvements (e.g., high yielding varieties) which flow from research. However, both the CG and TAC have realised that the practical achievements sought may be frustrated by economic and social constraints and by the inadequacies of general national agricultural and economic policies. This matter is discussed at some length later in these notes.

5. Again, judgement about priorities is directly affected by the varying situations of developing countries. Thus, TAC has so far been much influenced by the needs of developing countries in which the following circumstances are marked:
(a) the agricultural sector includes the largest proportion of the total population, almost always larger than its share of the national income available for consumption and saving;

(b) the agricultural sector of necessity (trade or migration offering no alternative solution) provides close to 100% of the food needs of the national population; and

(c) new technologies are necessary to raise output, incomes and employment in the agricultural sector and to provide a stronger complement to industrial and tertiary sector expansion.*

Priorities:

(a) Food

6. Against the background of the foregoing observations it has not been difficult for TAC to give food production first

* The growing interest in socio-economic "agricultural" research is in part the recognition that urban development apart from its own social costs cannot fully relieve the population pressure reflected in unemployment and under employment in the agricultural sector. It also accounts for the ever present concern to find labor intensive, but advanced, technologies for agriculture.
priority in its work thus far. Although forestry and fisheries have been declared within its terms of reference as are agricultural raw materials for industry, the priority has been given to food crops. The very tentative approach to agriculture has been justified under this head. I doubt if members would wish to depart from this primacy of priority* but as we shall now see this may not get us very far.

7. To begin with, it has become increasingly necessary to describe more clearly what the "gaps" are. Moreover description must then lead to prescription as to the best way of developing needed research. This paper does not attempt this task of declaring gaps but it is desirable that TAC grapple with it. Thus we must resolve our ideas on legumes; water management; food grains such as barley not yet covered by international research; several crops now well studied under irrigation conditions which need to be considered under rainfed conditions; considerable areas of livestock research; processing (?) and so on and so forth.

8. Even if there were no financial constraints (13) on investment in research through the CC/TAC system and all the major gaps defined, not all research gaps in the "food" area can be filled by a single decision at one point of time. Even if we were confident that in the longer run all gaps would be filled we would be faced with important questions within the apparently simple priority for agricultural research related

* This is the clear burden of letters received from several members commenting on my notes (16) of April last (9, 10, 11, 12).
to food supply. These derive from the question: "Which gaps should be filled first?" Thus if we base our argument on the assumption that, say $2-3M a year could be added to CG/TAC sponsored investment, will we give priority to:

(i) items like rice production in non-fed areas which clearly affect so many millions of poor people;

(ii) livestock in areas more sparsely settled but in which the pressures of near starvation and malnutrition (as well as under economic need) are no less;

(iii) making better use of existing research results before embarking on specific gaps in technical research?

Such a priority might even take us outside our apparent terms of reference and amount to a plea for direct support of national effort. Some of the efforts to improve water management may fall under this head. But other steps might also fall under the umbrella of socio-economic research.

9. Again, we will of necessity to decide whether research to be approved by TAC is limited to crops in the field or covers food processing as well. While the protein and vitamin quality of food can be improved by appropriate crop research it may well be that nutritional research of a technological kind beyond this point should have a high priority.
10. Yet again, being rather sceptical of attempts to define priorities too precisely and completely unwilling to accept cost-benefits analyses as the sufficient means towards precision*, I suggest that it will be impracticable to deny the benefit of international research investment to any major region of developing countries meeting the definition given in paragraph 5 above. We are likely to define and support a top priority for a given region even though not sure that it ranks above the third or fourth need of another region in which population pressure on food supply is more desperately apparent.

(b) Non-food (Agricultural) Research

11. Even if we manage, in pragmatic fashion, to do a reasonable job in the food supply field over the next decade, we have not thereby disposed of the case for research in the non-food crops (or their processing). This question is now officially before TAC (18). In the absence of financial constraints a strong case can be made for research (or more research) in commercial crops such as jute, cotton, copra and

*My scepticism is based on the high degree of arbitrariness in assessing intangible factors which of necessity must come into the analyses. Thus, how can one weight the social and economic constraints which may be greater for science research programmes than for others? Even the broad judgement that "food" should be given top priority could be opposed by those who see merit in widespread population destruction through mass starvation. After all our research effort may only buy time for nations to get their population policies in effective order.
oilseeds (for industrial use), all of which are important to developing countries. For reasons of employment and foreign exchange they may be (and are) vital to economic growth in many small and some large developing countries.

12. Yet we do operate (so far) within a financial constraint. While I have sketched these in fairly generous terms in my informal remarks to the Consultative Group in August last (13), they rule out any large scale substitution of non-food research on agricultural research programmes related to food supply. As now requested we will have to advise the Consultative Group on these matters (especially jute): when we do so we should pay strong attention to the financial implications and indicate especially whether we are prepared to give jute a priority over other "needs" which already claim our sympathetic attention. While I do not attempt to do so in these notes, I believe we may have to ask questions about alternative forms of financial support for a concentrated regional effort in relation to jute.

(c) Financial Constraints

13. If I reflect the views of other members on the wide range of food crop and animal research to be undertaken and on this question of non-food crops I believe one inference to be drawn is that we must ask the Consultative Group to give us some financial guidelines. While I have pitched my sights (13) considerably in advance of those of Dr Hill (8), I readily accept one suggestion he makes. I believe we should ask for authority to put forward proposals which will possibly add an
annual increment, say on average some $3-4M a year (at constant prices). This would cover completion of establishment of centres already approved and allow for some major new ones together with some critical outreach activities. It would within 10 years from 1970 cover the kind of programme roughly envisaged by me in my remarks earlier referred to (13). It would not enable a great deal to be done in commercial crops although possibly jute could be incorporated.

14. At the time of this meeting I lack data I have been seeking about costs of international research, but hope to be able to give a less imprecise view by the time of our meeting. In the crudest terms, however, we can build on the known 1973 recurrent costs of CIAT, CIMMYT, CIP, ICRISAT, IITA and IRRI of $US18.7M. Allowing for ICRISAT to reach an annual level of $3.5M (excluding African relay stations), and for a minimum core programme of $1.0M for upland rice, we quickly come to $23M annual recurrent. Now add for African livestock, gene conservation, ICRISAT relay, WARDA, Legumes, Socio-Economic, major efforts in Latin America and the Near East, and some other activities of a co-ordinating kind (see later paragraphs). I

* Whether jute research can "catch up" with its problems is a moot question. Wool research in Australia has cost millions and has been reasonably successful in containing the substitutes; but the serious enlargement of wool research began in the mid-forties. If it had not it is doubtful whether wool would have withstood the challenge of substitutes.
have no trouble in adding another $25M or more - say $50M all told.* To this add some $25M for critical outreach programmes and the CG/TAC system could easily reach $75M during the next decade. These figures do not allow for inflation which will normally be not met in full by donors as the squeeze on and funds increases with the rising total cost. Nor is capital included: this item could well average $2-3M over the decade (largely because of say five or six significantly large ventures in Africa, Asia, Near East and Latin America.) Altogether in constant price terms, $80M per annum for core and outreach budgets and capital construction by 1980 is not a wild dream. Yet it will ask much of the CG/TAC system and necessarily assumes a good deal of bilateral and multi-lateral grant and loan support for national and lesser regional research efforts. Once again it will be obvious that any attempt to include non-food research will greatly squeeze food oriented research ventures - a matter which cannot be side-stepped either by CG or TAC. In view of its responsibility for final decisions it is strongly to be hoped that CT can give TAC some advice on the financial aspects of matters raised in this document.

(d) Types of Organisation

15. Earlier I have indicated that the CG/TAC system was inspired by the IRRI, CIMMYT models and many TAC members (including me) remain convinced that this model is still of

* It must not be assumed from this purely illustrative listing of new activities that I have adopted firm positions on their final inclusion or the cost in our sponsored programme.
prime importance. It best enables the economy of scarce
skills which can concentrate on basic problems in complement
to the more directly applied work of national systems. I
have already stressed the importance of the two-way research
links and of the support such centres can give to training
of research and extension workers. I will later comment on
the socio-economic aspects of research work so largely
concentrated on finding higher yielding and more adaptable
varieties having regard to the wide variation of climatic,
soil and water conditions prevailing. Nevertheless, while
the case for concentrated "centres of excellence" in research
is very strong, our terms of reference do not explicitly limit
our activities to these.

16. There may be other forms of activity which TAC
could properly sponsor. Thus in the legume field we are
not yet sure that we need one centre to deal with fundamental
properties of the legume as a whole or (or perhaps and/or)
whether we should develop more effective results by seminars
and workshops in the spirit of term of reference (v) in
paragraph 1 above. Certainly small central groups formed
to develop such workshops are likely in my view (I think
Mosher's also) to be an effective device in socio-economic
research. This may well be true also of other activities
like aquaculture.

17. Regional activities are not excluded from our
terms of reference and yet I believe these cannot easily be
supported without some limiting framework or criteria. Thus
we need to be sure that these are international in the sense
of being independent in the full sense of the type of control and recruitment policies which mark IRRI, ICRISAT and other established centres. Skilled personnel are scarce and they must be given proper facilities; they must also have the full benefit of relations with other relevant centres (vide term of reference (v) again). WARDA for me is clearly a borderline case. It clearly is rationalising national research and may not be in full control in the sense of a scientist directed centre. On the other hand, this could perhaps be met by establishing proper links with IITA, IRRI and IRAD which give the Consultative Group confidence in what may be a superior form of relay or outreach programme.

18. One important purpose of our terms of reference is to enable TAC to review the state of research in all significant regions. The probable constraints are such that it cannot lightly select national programmes for support nor perhaps many regional efforts even though these represent desirable co-operation between national organisations. We might well advise on these in a situation in which finance may have to be sought outside the CG/TAC system because other priorities prove to have a stronger claim on limited funds available directly through the Consultative Group.

19. I recognise that I have posed no easy answer to our dilemma of how far can we move from highly concentrated international effort towards more localised effort albeit regional in form. There is room for some movement but I believe we would be wise to contain that movement within
the framework of outreach programmes or special efforts of co-ordination and stimulus through workshops, seminars and free movement of personnel from national centres to work in sabbatical style.

Socio-Economic Research

20. Members of TAC will be aware of the wide concern felt about the social and economic implications of applying in developing countries the results of agricultural research. This concern is naturally greatest when the research results in a marked change in the level of technology required. Almost invariably the degree of change from traditional systems is substantial calling for new levels or different types of skills; greater usage of fertilisers, better water and soil management, different systems of cropping (over a year), pesticides, and improved infra-structure facilities such as roads, markets (and pricing policies) and not least, new or expanded credit for production and from development. Many of these factors are economic and institutional in character and have to be costed in terms of the new level of input called for as against the higher gross returns to be expected. For many the most important question of all is whether the new technologies can be used on small scale farms so dominant in many countries. A related question is whether the status of tenants and employment levels for labor will be downgraded as the result of the incentives provided by new technologies which are enhanced by mechanisation on largish holdings.
21. It is important to know as much as possible about these things. Much can be determined at the outset in the centres of research. Serious technological constraints on the adoption of new varieties (such as susceptibility to disease) must be taken into account by research workers. On the one hand, to allow research to be frustrated by difficulties of application is unthinkable, for traditional systems, by and large, cannot cope with inexorable population pressures let alone the now strong and almost universal urge for economic betterment. On the other, it is increasingly obvious that agricultural research cannot be divorced from a concurrent study of the implications of any new technology for land use and employment structure, or from the development of a policy package designed to make the new technology effective (16).

22. What, under the aegis of the CG/TAC system can be done about these problems? As already remarked a good deal can be done at the centres of research and as part of outreach programmes to define the new technologies and alert users (or their governments) to the economic and general policy implications.* It is essential that the work at the central institutes lead to a clear general

* I am not writing at length on this subject for members will have the papers given by the economists of established institutes to the Consultative Group in November, Mosher's paper (7) and, hopefully, the results of a further meeting of the economists and our own talks with them. Some FAO material may also have been made available before the meeting.
statement of the package requirements of promising new varieties especially in respect of the physical inputs and labor and management skills required. Under outreach and independent national programmes these requirements and multiple cropping possibilities can be made more specific for differing circumstances of climate, soils and water supply. All this points to the rising importance of economic analysis and farm systems research as part of total central institute and outreach programmes.

23. But the issues of national economic and social policies cannot be completely defined nor can effective advice to national governments be offered in this way. Something more is required and much of Mosher's paper (7) is devoted to this subject. He in effect suggests some regional bodies which can help clarify the economic and social factors which affect the rate of exploitation by farmers of new technology. Yet for the very reasons he gives - wide variations in economic and social circumstances between nations and between areas within nations - we must, I believe, be cautious in assuming that a model of research which hold good for the technical aspects of agricultural research is equally good for the economic and social investigation required for the making of policies needed to increase the rate of exploitation of "desirable" technology. I prefer to break the problem down into two sub-areas:

(a) Further help established centres can give in terms of paragraph 22 above.

* Including in this word national policy decisions about land tenure and employment structure,
(b) What specially established economic and social research centres can do over and above what is done under (a).

24. As to (a), I remember paragraph 22 again only because it would be surprising and disappointing if a good deal of stimulus to economic and social enquiry and policy thinking within national units did not emerge through the links now likely to be even more widely established between international research bodies and national governments, universities and other research authorities in national areas. This can be encouraged by associating social and economic researchers with Activities I-IV suggested in Mosher's paper (pp 14 - 21). Indeed some of this is happening.

25. As to (b), I have so far seen no need to create new centres for economic and social research apart from the basic agricultural institutes themselves. It remains true, however, that many of the issues calling for policy decision and action programmes will not be adequately dealt with by workshops and seminars and fellowships for national personnel which will naturally concentrate on systems analysis and the package requirements of new technologies. What to do about land reform (if called for), marketing (including price) policies, the development of industries or import capacity to supply input like fertilisers and the development of needed credit institutions calls for something further. Here, at the time of writing I am not sure that I know where I stand in relation to Mosher's proposals. The real trouble is that a wide
variety of institutions, private and public, national and international, are already involved in these matters.

26. I can see value in regional centres staffed by people able to do the following:

(a) work closely with the agricultural research institutes and participate in their workshops and seminars;

(b) develop work on price, marketing, land reform and other infra-structure policies going beyond the capacity of the economic divisions of agricultural research institutes to undertake; and, perhaps,

(c) undertake, as a kind of task force, work at the request of national governments designed to strengthen national social and economic research capacity, to clarify policy issues and advise on them.

27. My hesitancy about the further development of these further research centres arises from two factors:

(a) a desire to minimise duplication of work best done under the programmes (core and outreach) of established agricultural research centres; and
(b) a bother lest such centres cut across the advisory and investment activities of FAO, UNDP, IBRD, regional banks and various aid agencies.

Certainly the good will of nations to be served would be essential and I would think it also vital that these bodies join forces in establishing the new regional groups to work in ways that would call on the already experienced staff of the sponsors. Not least, it would be essential that the wishes and priorities of national governments in economic development be recognised and understood lest the very centres Mosher advocates find themselves guilty of the crime he rightly deplores:— "usurpation of the priority-setting function in research by external agencies".

28. I rest content, for the moment, with a desire to hear the matter more fully debated, believing there is on the one hand a great deal more we can do than is now being done, through existing research centres, multilateral agencies and national aid programmes, and, yet, on the other, a need for some co-ordination of these efforts to ensure more effective application of the basic technological advances made possible by agricultural research.

Conclusion

29. Without formulating an agenda, my paper suggests we explore assystematically as possible:
(a) the adequacy of our terms of reference in relation to the determination of priorities;

(b) how far we can specify (make more particular) our judgement on research "gaps" and their relative priorities especially over time with respect to agricultural and pastoral products (food and non-food), processing problems, and broad regional needs;

(c) what variety of forms of advancement of research are open to TAC to develop;

(d) the nature and bearing of financial constraints on the judgements made under (b) and (c); and

(e) what particular way we think it best to handle socio-economic research.

J.G. Crawford
2.1.1973
References

(1) See early IBRD releases and advices to TAC from Consultative Group.

(2) Draft paper by Lowell S. Hardin and Norman R. Collins "International Agricultural Research: Organising Themes and Issues".

(3) Various internal FAO office memoranda of late 1972 dealing with Socio-Economic Research.


(5) J.G. Crawford: "Agricultural Research Priorities". Speaking notes prepared in March 1972. (Revised for April TAC meeting in April 1972 - see (16) below.)

(6) P. Oram: "Brief for Centers Week Discussions" (July 1972).


(10) D. Bommer: "Suggestions to 'Topics for Discussion in TAC on Priorities in International Agricultural Research". (Letter and note to Chairman TAC, 14 November 1972).

(11) M. Elguela: "Suggestions Concerning Policy Definition for TAC" (Paper and letter to Chairman, late October 1972).
(12) L. Marcano: Memorandum to Chairman TAC on priorities in agricultural research (October - November 1972).


(15) H.A. El-Tobgy: Letter to Chairman TAC on priorities in agricultural research (27 October 1972).

(16) J.G. Crawford: "Notes for Discussion in TAC on Priorities in International Agricultural Research" 2 April 1972. (See also reference (5) above.)

(17) Documentation for agenda Item 7 of Consultative Group Meeting, 1-3 November 1972. Papers all relate to socio-economic research and include letter of 3 October from Mr Joel Bernstein, a report of May 1972 from CIAT, an excerpt from IITA's draft annual report for 1971 and a brief statement from CIMMYT. (Barker's paper - (4) above - also relates.)


(19) "Ch. 4: Accelerating Agricultural Research in Developing Countries". (Origin of text not clear but appears to be FAO and is not unakin to item (14) above.)