

**ExCo Member Comments on the Challenge Program Full Proposal on
“Unlocking Genetic Diversity in Crops for the Resource Poor”**

Kevin Cleaver

March 5, 2003

We endorse this program but raise a number of issues that need to be considered, some of which apply to all of the CPs.

1. While the CP now shows some flexibility with respect to the focus on drought, we think it is important to recognize that there are significant risks in putting the bulk of the resources in this difficult to tackle problem which is genetically highly complex. We would push for selection of some 'lower hanging cherries' that offer higher probabilities of success in medium term.
2. We are concerned that the deviation of core funding to this activity will further undermine the very fragile nature of core breeding activities in the CG that was noted by the 2000 System-wide review of plant breeding and biotechnology. Every effort should be made to protect core budgets for breeding and ensure that new activities launched under this activity are funded under truly new resources.
3. We feel that it is critical that the CG put in place immediately a system for tracking the overhead costs of the CPs. This CP and the others will **each** require a new steering structure with annual meetings, as well as a mechanism to implement a competitive grants program and monitor results. These overheads are in addition to existing governance structures and mechanisms for allocating funds. However, to our knowledge the CG has no way of knowing the total overhead costs.

Adel El-Beltagy

March 8, 2003

While we endorse the full proposal of CP on "Unlocking Genetic Diversity for the Resource Poor", which has reflected well the outcome of the deliberations of all the stakeholders in the Alexandria meeting, I would like to draw the attention of the ExCo that the response of the proponents to the first comment made by the ExCo and the CGIAR on the earlier proposal regarding the inclusion of CWANA region explicitly in the CP including the region's institutions with significant experience on drought is rather vague.

Klaas Tamminga

March 19, 2003

I can endorse this CP proposal with the following remark. The proposal indicates on page 43 under Intellectual Property that the CP will develop an IP management plan based on a set of Guiding Principles agreed among consortium members at the initiation of the programme. These principles should fully conform to the IT on Plant Genetic Resources for Food and Agriculture and the Convention on Biological Diversity.

Rodney Cooke

March 20, 2003

I have looked at this 6 February 2003 version in comparison with that of 3 Sept. 2002. On a procedural point it would be perhaps useful to ask CP authors to sideline the changes in text when responding to requests for change. Unsurprisingly most of the text remains unchanged.

We heard much in Manila about this proposal being in the heartland of the CG, and on this basis alone we should be looking to endorse this proposal. However, I admit to a slight disappointment regarding the rather limited nature of change between these two documents. I look forward to discussing this in the forthcoming ExCo Meeting in May. I comment here very briefly.

The first concern expressed last autumn was on the need for further focus. I see here that the proposal now focuses a little with regard to the 22 mandated species “not all species will be initially investigated.....”. The second issue was the challenge of the “proof of concept” i.e. drought stress. Rather than sharper focus, the comment following the Egypt meeting states that while drought should be a high priority the CP should not be limited to it. This is not quite what I was expecting in the nature of focusing.

On the issue of further involvement of partnerships: NARS and the private sector, this document shows little advance on the previous one. Rather than tangible undertakings, the challenge programme will seek ways in which the private sector can be included, and indeed “will seek to broaden the participation of NARS with relevant capacities”. The NARS issue appears to be deferred to a technical workshop later this year.

In the September 2002 version the in kind contribution was attributed to 10 founding members contributing USD 5.7 million, i.e. the equivalent of 25 full-time researchers. This appears to become vaguer in this version, i.e. the number 10 of founding members has been dropped while the other figures remain the same and I note in the Egypt response document reference to in kind contribution of at least USD 400,000 per annum, i.e. a slight reduction. This for the CG Centres is of course a small proportion of their average USD 25 million budget. This sits rather uneasily alongside the notion that for the CG this is a fundamental heartland project – a glance at the financial commitment would suggest that it is rather peripheral. This is another issue we can take up at the ExCo discussion in May.

On the scientific principles I have no difficulties with the proposal which I think is truly “challenging” and appropriate for this type of approach – but with the provisos above.

Jonathan Conly

March 21, 2003

We endorse the substance of this CP but are concerned about the weighty governance and ponderous management structures. Even more importantly, we are concerned about the budgetary implications. It should go forward only if substantial new funding is available to support it. Without this new funding, a "heartland" CP such as this one will only weaken the centers and system by undermining their ability to compete for funds if it redirects existing funds to a wider group of recipient institutions. In addition, there are substantial overhead and transaction costs. We are not as yet aware of new funding and seek the advice of the proponents and Secretariat on this subject.

On the issue of focus, we believe the major but not sole emphasis on drought is reasonable, since much information will be developed with relevance beyond drought. In this respect drought is a

model system, not a straightjacket for the program. We look forward to important findings that relate to other problems as well. The opportunity approach with respect to commodity focus makes sense for a system as large and diverse as ours.

Sam Dryden

March 31, 2003

Highlights of PSC Comments:

As we have said several times, this Challenge Program has the potential to significantly advance the field of genetic improvement. As such, it is important to take the time to correctly design and construct the Program. Our comments are made with the intent of assisting the Program designers and sponsors in achieving the optimum results.

We continue to feel that the Project proposal is overly ambitious, has high transaction costs, and is under-resourced. - We also continue to feel that the project should limit its focus at the outset on no more than 5 key staple crops. We suggest that these should be Sorghum, Millet, Maize, Wheat and Rice.

Drought tolerance is a laudable goal that could benefit from this approach and that we strongly endorse. It is a highly complex trait that will require a very tightly focused program with strong and experienced management.

Database management and data quality control will require considerably more attention than is given in the proposal. Project strengths lie in the "consortium" that fosters intercenter collaboration, but diversity of information sources and platforms will make it very difficult to share molecular and phenotypic data across crops.

Phenotyping, required to validate transgenes and key genomic regions, is extensively under-resourced. Specifically we have problems with subprogram 2 and 3.

The project lacks unequivocal and verifiable milestones to measure progress and success. Clearly defining key accomplishments to be achieved by horizon 1 (1-3 years), horizon 2 (3-5 years) and horizon 3 (5-7 years) would allow for donor groups to be able to better understand the progress being made.

We remain concerned with the lack of a new model for the private and public sector interaction and the lack of private sector representation on any project committee.

The private sector has considerable, experience and resources to bring to such an effort but remains an afterthought and is held at a distance. There are clear areas where the private sector can contribute and obtain benefit as well (e.g., sharing transformation and associative genetics technology; mutants and transposon knockouts; modeling of marker systems; information management; candidate gene information; support for training and conferences).

Additionally, new mechanisms for delivery of improved varieties and hybrids in the developing world remains a major bottleneck. There are very few examples of the sort of seed distribution systems proposed in the CP that have actually worked. The system needs a new version of sound but simple seed production technology-which is what the private sector does well.

The Private Sector Committee has consulted widely within our respective organizations for these thoughts and is available for further discussion to clarify and expand on our comments.

Detailed Comments

1. Strengths :

- a. This project pulls together the resources 3 IAR centers, 2 leading NARS and five Advanced Research Institutions (ARIs) in a consortium with a common purpose. It is very encouraging to see these centers working together.
- b. New genomic techniques (genetic analysis, supported by high throughput laboratory analysis and new approaches to data management) are very rapidly changing and reducing in cost. We can assume that unit costs of genetic analysis and information management will continue to fall throughout the project's lifetime. At the same time, new facts are emerging that may make the sampling of germplasm more demanding. Nonetheless, it is encouraging that CG centers are attempting to associate a molecular component with their genetic resources.
- c. This project has the opportunity to stimulate the injection of new money and resources into the CG system and generate valuable new products.

2. Goals and resourcing: Goals are lofty and ambitious with similarities to those of many private seed companies. The ambitious nature, however, is evident when we compare annual research budgets in the private sector for the 5-8 crops largely targeted at temperate environments, and compare it with the proposed budget of the CP of around \$14 million per year.

- a. The inadequacy of the research budget is of particular concern assuming 22 crops are going to be considered, given that there is no common database that can handle information from more than a few of these at this point in time.
- b. We believe the focus should be on the 5 main cereals—maize, wheat, rice, sorghum and millet. “Model” systems may be appropriate in the first phase of this project. Examples include resistance to Black Sigatoka in bananas, resistance to storage insects in cereal and grain legumes, or resistance to *Striga* in maize, sorghum and cowpea might also be suitable as shorter term less complex candidates. Other CG-mandated crops should be encouraged to continue to assemble basic genetic and molecular information, and to conduct quality conventional breeding programs.

3. Private Sector involvement: Oblique reference is made to private sector involvement (e.g., p38-39), but it is unclear as to mechanisms for interaction. The Private Sector is not included in the Consortium, or in the committees managing or advising the project.

- a. The private sector can contribute greatly to the success of this Program. This could involve: access to gene chips for expression arrays, TUSC assays, assistance with modeling MAS breeding systems, information on candidate genes that are not of commercial importance but may be of interest to allow the crop to survive under severe stress, software that could be shared for linkage disequilibrium analyses and haplotype identification, and high throughput SNP and SSR analysis. Limited access to proprietary database software may help the consortium resolve issues of database alignment and quality control.
- b. Product development and delivery is an area of specialization for the Private Sector. The material transfer agreement will have to be flexible enough that

private companies find it worthwhile investing resources for the development and delivery of this germplasm.

- c. We are confident that the private sector is willing to negotiate access to the whole dataset generated, as well as to candidate genes identified, from the project. Negotiations on access to technology would likely take the form of intellectual property protection for the private sector in established and certain potential markets, but free access to all in countries showing little or no market potential in the next 10-20 years.
- d. An intern/sabbatical program for project scientists with the private sector would be an excellent way to help this program. Training scholarships are always popular at MS and PhD level, or paying travel costs to conferences, funding of a conference, etc, are easy areas to consider

4. Weaknesses:

- a. The document is written at a high level and approaches are sufficiently vague that it is difficult to comment on them. We would like to see more articulate plans for the molecular characterization work.
- b. We feel the involvement of the CG center field breeders is essential to this effort but are unable to ascertain their role in the current draft. We are concerned that the project may be driven more from a molecular genetics perspective than from a molecular breeding impact perspective.
- c. There is no discussion of the costs of quality control of molecular, pedigree and phenotypic data. This is a big job and should not be underestimated. The proposal works on the assumption that all steps will run smoothly without the need to put a substantial quality control system in place, yet typically Post docs and grad students will be working on sections at different times in different crops and locations. Much of the phenotypic data may have been collected historically and be of suspect value. Thus project managers will need to be proactive in this area from the start. The real cost of IT infrastructure and statistical analysis support are going to be higher than the combination of the in-kind and additional funding.
- d. While transgenic approaches are mentioned as having value from the discovery viewpoint, there is no mention of these in Subprogram 3. We acknowledge that transgenics currently face problems in the developing world because of IP issues, regulatory costs, public acceptance, and the lack of in-place biosafety regulations. However, markers and associative approaches only find alleles that are already extant in the germplasm being used, and quantitative genes are generally interdependent upon other genes in that background. Indications are becoming clearer that major impacts can be made by modulating a few genes even for complex traits, that transgenes are affected less by genetic backgrounds, and that the same strategies that are successful in one crop are likely to succeed in others. Transformation technologies are no longer a hurdle in the major crops. Transgenics could result in real progress in stress tolerance in a ten-year timeframe, and, as Golden Rice has demonstrated, FTO constraints can be negotiated.
- e. We cannot overlook the transaction costs when so many widely separated groups are collaborating. We presume that it would make sense to name one institution (e.g., Cornell, or CIMMYT) as the headquarters for the project, where up to 50% of the research would get done and a critical mass of researchers working on

these crops could be housed. If marker analysis is centralized for high throughput genotyping, the issue of getting tissue samples to that central point in good condition for analysis will have to be addressed.

- f. Subprogram 3, Gene Transfer and Crop Improvement, as it is currently written, is weak. We think it is naïve to assume that superior alleles from germplasm bank collections “not identified by traditional breeding methods” will be identified by SubPrograms 1 and 2, unless a substantial new investment is made in phenotyping such collections. As a backcrossing scheme, some system for developing superior donors adapted to key ecologies should be proposed, given that transformation and regeneration often will often not take place in elite germplasm. The use of markers to develop elite donors seems fully justified, and when backcrossing enters a more extensive stage markers may be less essential.
- g. Phenotyping of the germplasm collections that would allow scientists to attach an accurate phenotype to the genetic information receives little emphasis. It is one of the most challenging questions associated with landrace collections because of their often very narrow adaptation. Nowhere is emphasis placed on the development of managed stress environments for drought tolerance, though it may be implied—the dry season in the tropics provides many opportunities. To do this well will not come cheap. It is implied that NARS will play an important part in phenotyping germplasm, but our experience suggests that data quality will remain a major challenge from this source, given the general downturn in NARS funding.
- h. Crop information systems are critical. We would like to a single platform through which the project data can be shared. The support for the whole IT infrastructure required to develop and maintain high quality data sets appear to underestimated. Costs of new software development are formidable, and could consume most of the budget. Reference is made to possible sharing of data from private sector databases. It may be worth exploring to see if some of the software development costs could also be shared through limited access to the database software developed by Pioneer, Bayer, Syngenta and Monsanto
- i. The project lacks specific and verifiable milestones and timelines. Without these it is difficult to measure success of failure, or to identify areas of clear under achievement. As it stands it will always be possible to place a successful “spin” on the outcome so that the programs seems to have succeeded.