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Fund Council Comments on CRP 3.6 Proposal (Revised 11/15/11)

(Working Document - For Discussion Only)

*Document presented for Agenda Item 9:
CRP 3.6 Proposal - Dryland Cereals*

Submitted by:
Fund Council

FC Members' Comments on CRP3.6 Proposal: Dryland Cereals

FC Member	Comments
<p>Australia</p>	<p>Overview</p> <p>The proposal provides a convincing case for investment in the improvement of four staple dryland cereal (DC) crops (barley, finger millet, pearl millet and sorghum) of special significance to the poor in the marginal environments of Africa and Asia. The focus will be on: (i) producing high yielding varieties with resistances to the major biotic and abiotic stresses; (ii) improved crop management to reduce yield gaps; and (iii) improved systems to get new technologies, both production and post-harvest, into the hands of smallholders. The aim is to increase yields by 15% on up to 45 m. ha in the two continents by 2020, which will benefit 33 million smallholders to the extent of increased net incomes of \$1.5 b. plus added stover valued at \$0.7 b. These outcomes and impacts will be achieved by research and related activities under six strategic objectives (SOs), namely: better targeting; enhancing use of genetic diversity, genomics and informatics; developing improved varieties and hybrids; developing improved pest and disease management options; more effective seed and information systems; and adding post-harvest value and improved market access.</p> <p>Scientific and other synergies provide the rationale for inclusion of the four crops within one CRP. These include: (i) the use of similar breeding approaches; (ii) similar researchable issues (e.g. drought and low soil fertility tolerance) and research methods required; (iii) all four crops are important for both food, feed and fodder in the crop/livestock systems of marginal environments. There is expected to be improved cost-effectiveness as a result, with attendant breakthroughs that have been missing in the past.</p> <p>The proposal is well written, and has been developed with considerable inputs and commitment from the designated partners. This is evidenced by the inclusion of measurable output and outcome performance targets and milestones, where precise contributions from partners are specified. Further clarification and justification for the priorities and proposed budget allocations among SOs and the DCs are required. It appears for 2011-13 these may have been based largely on historical allocations rather than a systematic reassessment of needs and opportunities. The highest priorities for the CGIAR would appear to be SOs 1, 2 and 3, which best express its comparative advantages and will result in IPGs.</p> <p>As with other CRPs, it is not clear to what extent an emphasis on value chains and post-harvest value addition will benefit the poor, especially poor consumers or what the comparative advantage of the CGIAR is in value-addition research or market oriented development. Value-adding means</p>

higher prices almost by definition, and *a priori* this cannot be to the benefit of the urban poor, and maybe even not to poor rural net buyers of DCs. Even here it may well be that value-adding innovations such as sweet sorghum for ethanol may be of significant benefit to middle-people rather than to smallholders or women. However these are researchable issues that deserve to be prominent components of the agenda of this and other CRPs. The value chain approach suggests that the “structure, conduct and performance” paradigm, popular with market economists in decades past, will be at the forefront of the value chain analyses in this and other CRPs. It is doubtful if the CG could or should be leading this drive.

Compared with other CRPs, this proposal does have a number of strong points (targeting, breeding, crop management, gender) and offers good prospects of impact in the moderate-severe zones of the drylands. However the proposal suffers from somewhat loose framing of the justification, proposed research and management (no definition of “drylands”, claim of 1 million people and 100 million ha of these crops, questionable IPGs in aspects of the research design, plethora of non-specific partnerships).

With appropriate attention to the above concerns and the more specific issues raised below in a revised version, the proposal deserves support.

Specific Comments (In no particular order)

- The case for research on the four DCs is well made on the basis of their importance to the food security of arguably the most vulnerable of the poor in the developing world and they will become more important as climate change unfolds. Demand projections show that it will grow for DCs, although at a slower pace than for rice, wheat and maize. It is notable however that Table 1, p. 12 shows the demand for sorghum will in fact decline in the target countries by 2020. This table (and also Table 3.1 on p. 147) requires checking, as the years are in error and the current and projected production figures for barley seem extraordinary. A production of 109 m. tons of barley in 2000 compares with only 23 m. tons of sorghum. Also this barley production is from 10 m. ha of land sown (Table 2.1), which implies a yield of more than 10 tons per ha! Maybe the production figures include stover?
- Impact from research in dry areas has been limited and the aggregate investment in these ‘dryland’ crops is far less than in the core cereals underpinning global food security. It would have been better to define the boundaries of ‘drylands’ at the start of the document. The central argument rests on local improvement of household food security (in contrast to the rice, maize and wheat CRPs). This has implications for the targeting which are brought out in this document.
- The SOs are well framed within the System level outcomes, *viz*, reduced rural poverty, improved food security, improved nutrition and health, and

sustainably managed natural resources. The explicit up-front emphasis on targeting (in SO 1), and the use of the farming systems framework, is to be commended (and aligns well with some of the conclusions of the recent Science Forum 2011).

- The narratives for the six SOs provide the rationale for each and well-articulated cases for the proposed research agendas to respond to the identified research issues. It is clear there has been a lot of synthesis and review of past research drawing lessons, together with assessments of constraints in defining priorities. There are verifiable output and outcome performance milestones included, with the relevant partners responsible for them listed. This illustrates there has been considerable interactive planning among the partners in crafting CRP 3.6 that augers well for success.
- Unfortunately the impact pathways for each SO are rather generic. Continuous adoption studies will form the basis for M & E with a learning focus. This will be accompanied by *ex ante* and *ex post* impact assessment aimed at dynamic refinement of priorities by looking at trade-offs.
- SO 1 includes food and livestock value chain analysis and SO 6 aims to add post-harvest value and improve market access. As in other CRPs with a value chain emphasis, the same comments apply. Namely, it is not clear to what extent such an emphasis will benefit the poor, especially poor consumers or what the comparative advantage of the CGIAR is in value-addition research or market oriented development. Value-adding means higher prices almost by definition, and this cannot be to the benefit of the urban poor, and maybe even not to poor rural net buyers of DCs. At one point (p. 22) the proposal states the benefits of CRP 3.6 will especially accrue to poor consumers, but in the value proposition impact assessment (pp. 6-9) the emphasis is entirely on the potential benefits to poor smallholder households. Even here it may well be that value-adding innovations such as sweet sorghum for ethanol may be of significant benefit to middle-people rather than to smallholders or women. However these are researchable issues that deserve to be prominent components of the agenda of this and other CRPs.
- Value chain analysis may be so location specific that IPGs will be difficult to generate. The proponents need to consider this more explicitly in deciding on which value chains to select for intensive research. The approach suggests that the “structure, conduct and performance” paradigm, popular with market economists in decades past will be at the forefront of the value chain analyses in this and other CRPs. It is doubtful if the CG could or should be leading this drive.
- SO 3 focused on management is a fully relevant area in the dryland zones. However, given the risky complex systems in the drylands, one

	<p>could have expected the CRP to explore G x M analysis more thoroughly, i.e., the interaction between SO 2 and SO 3 - this appears to be a missed opportunity.</p> <ul style="list-style-type: none"> • The key priorities ought to be on SO 2 (enhancing use of genetic diversity, genomics and informatics) and SO 3 (developing improved varieties and hybrids) as these offer the best prospects of breakthroughs in the productivity and stability of the DCs. Both involve use of strategic research and new methodologies to exploit more fully the genetic diversity available in the germplasm. They represent a comparative advantage of the CGIAR and will lead to IPGs. The narratives in the text and appendix have excellent systematic discussions from the literature of the rationale for choice of priorities within the SOs, with a summary provided by crop, constraint and system (Tables 6 and 7, pp. 41-44). Unfortunately unlike CRP 3.5, these constraints were not translated into quantitative assessments of their individual contributions to yield gaps and the likelihood that research could close these gaps and what each would be worth. This would have strengthened the overall impact assessment and importantly provided a more informed framework for establishing and refining priorities and resource allocations. SO1 on targeting will provide a framework to allow such dynamic priority assessment, as well as a better characterization of recommendation domains so that the products of SO 2 and 3 will find their way into farmers' fields, in collaboration with NARES. In this sense SO1 has equal priority to SOs 2 and 3. • Lower priority ought to be accorded to SOs 4 (developing improved pest and disease management options), 5 (more effective seed and information systems) and 6 (adding post-harvest value and improved market access), as these seem to be location-specific adaptive research cum development, which are more appropriate for NARES and the private sector. While in theory the development of integrated technology packages seems desirable to exploit multiplicative effects from various components, in practice it has been found wanting. Experience with the green revolution in India was that farmers, especially smallholders, adopt components sequentially and rarely end up with the whole package. Developing a wide range of technology options hence would seem a more desirable approach in SO 4 than a package approach, especially with the proposed emphasis on participatory research with farmers. To expect to develop integrated packages for many different regions and systems that will be gender sensitive along with new communication tools, is a tall IPG order for this CRP. • The emphasis on training the next generation of breeders in modern tools via graduate degrees and courses seems highly appropriate in order to ensure the products of CRP 3.6 will be utilized at the level of recommendation domains (pp. 49-50). However it is not clear the CG has
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	<p>a comparative advantage in reviewing extension systems or training farmers' organizations in developing radio and TV messages (p. 72).</p> <ul style="list-style-type: none"> • In SO 6 envisages research to add value by developing novel products and marketing approaches, including business development and promotion, hardly a comparative advantage for the CGIAR. Admittedly collaboration is planned with academia and the private sector but it is not clear what the precise roles and responsibilities of the partners will be vis-à-vis the CG. Presumably the CG should primarily facilitate and catalyze rather than be an active participant, which would require new staff competencies to be developed? This should be clarified. • While the analysis of production and livelihoods risk is introduced, further resources could be allocated to risk analysis as it is core to the management and technology adoption choices of the drylands. • An elaborate gender program is emphasized throughout CRP 3.6, which is commendable and one of the strengths of the document. It goes well beyond gender differentiated data assembly and analysis to include proactive development of gender sensitive technologies and participatory involvement in priority setting and decision making. There needs to be more consideration given to what performance indicators will be used to establish that gender objectives have been met. • Among the rationales for combining the four SCs in this CRP are: <ul style="list-style-type: none"> ○ Ability to approach smallholders with coherent and common messages and a unified voice to policymakers and investors; ○ Increased research effectiveness by capitalizing on new tools like next-generation whole-genome sequencing and selection thus reducing time per unit of genetic gain; ○ Using large platforms for phenotyping of traits; ○ Building on Generation Challenge Program's integrated breeding platform for storing and retrieving information; ○ Exploiting heterosis for hybrids. • A business plan will be developed within six months and build on the process and performance indicators in Table 11, which are measurable but require targets to be set. • The budget for 2011-13 is \$77 m, with 32% from Windows 1 and 2, 37% from existing bilateral contracts and a 32% (\$24.7 m) funding gap. What we consider to be priority SOs, namely 2 and 3 will in fact have a reduction in their share of the budget from 45% in 2011 to 41% in 2013. Other SOs either remain the same or increase slightly. Is this appropriate in view of the renewed emphasis after the CGIAR reforms on breaking yield barriers if we are to enhance overall food security? The allocations among SOs should be more fully justified before the above gap of \$24.7 m is filled by the FC, as it would appear that the allocations may have been largely based on historical shares rather than a critical assessment
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of priorities, opportunities and comparative advantages.

- A simple congruence analysis comparing proposed budget shares among commodities in Table 14, p.115 (the % share calculations in this table appear to be in error so we recalculated them), with the share of the commodities in total gross value of production (GVP) in target countries (Table 2, p.13) shows some imbalances that require clarification and justification. Barley will receive 16.4% of the 2011-13 budgets but only represents 10.8% of the total GVP of the four DCs. Its apparent overinvestment comes largely at the expense of the millets (pearl and finger), which will receive 44% of the budget, yet represent 49% of the GVP.
- It appears that no provision has been made for system overheads in the budgets and CRP management has been charged at nearly 3%, compared to the norms of 2%. Institutional management overheads are set at 20%, which is well above the 16% charged in CRP 3.5. These charges all deserve review by the CB to ensure consistency and transparency.
- Appendix 3 has a good description of the assumptions that were used in the overall impact assessment. However it requires some clarification, especially in regards to the grain: stover production ratios in Table 3.6, p. 151. It seems they should read stover: grain ratios.
- Appendix 4 describes SO outputs, methodologies, milestones and partner roles. It provides clear evidence of extensive discussions, planning and commitments among partners, which is commendable.
- In SO1 the value chain analysis will include exploration of new non-food uses of target crops. Besides not being an obvious comparative advantage of the CGIAR, and the earlier doubts raised about whether such added values benefit either the producing or consuming poor, is the fact pointed out in Appendix 4 that in India 30-40% of sorghum and 50-60% of pearl millet are already used for non-food purposes. This reinforces the need to critically evaluate the desirability of devoting scarce resources to such post-harvest value adding.
- New coordinated genetic resources collection missions will be mounted under SO 2, which will focus on wild species. This is to be applauded as it may help to achieve the breakthroughs in DC yields and their stability that are required. Core, mini-core and reference collections will be employed in pre-breeding to better exploit germplasm and identify novel diversity. Phenotyping throughput will be increased and modelling used to identify valuable traits for targeted improvement. New sources of resistance to hardy perennial pests (e.g. stem borer, shootfly) and diseases (blast, downy mildew) will be pursued. There is a very clear delineation of the respective contributions of centers, ARIs, private sector and NARES to SO 2 outputs. One notable omission in this list is any role for Bioversity International, which is surprising.

	<ul style="list-style-type: none"> • So 4 provides further information about the scientific synergies possible across the DCs. Examples are the sharing of methods and facilities for screening for tolerance to salinity and terminal water stress, sought-after traits for all four DCs.
<p>EIARD</p>	<p>EIARD considers that dry land cereals are very important components of cropping systems, in particular for poor farmers, both for human consumption and for fodder. Developing research on dry land cereals is fully consistent with the SRF and the CGIAR vision.</p> <p>While some members considered too “harsh” some of the comments of the ISPC, EIARD supports the overall assessment of the ISPC that the proposal should be approved subject to major revision and resubmission (Category 2).</p> <p>Among the “must haves” identified by the ISPC, EIARD insist in particular on the following points:</p> <ul style="list-style-type: none"> - The proponents need to significantly strengthen the case for this CRP. It must be done at two levels: <ul style="list-style-type: none"> ○ Explaining why CGIAR and its partners must invest in dry land cereals; ○ and why they should invest through <i>a specific CRP</i> dedicated to these dry land cereals as opposed to integrating these activities in other CRPs (based on commodities or systems). - Researchable questions and hypothesis must be clarified. - Local innovations are only mentioned once in the proposal. EIARD considers that more consideration should be given to the potential of local innovation to inspire novel research. - Impact pathways have to be significantly improved. Impact Pathways are discussed (in a short separate chapter and each of the 6 sub chapters for the 6 strategic objectives) but not in very comprehensive or innovative ways. Many statements are made (on the role of the development partners, the enabling "environment", the non linear pathways), but they fail to convince that the proposal will “make a difference”. (maybe with the exception of the section on adding post harvest value). - The Monitoring and Evaluation system need further attention. - The synergies and working interactions with other CRPs, in particular CRP 1.1 but also CRP 4 and CRP 7, must be explained. - Regarding the governance of this CRP, EIARD shares the concerns raised by the ISPC. EIARD notes that the Consortium has commissioned a consultancy on CRP governance and would like to know when the results of this work will be available and how the related recommendations may impact CRPs under revision. (<i>and already accepted CRPs ?</i>)

<p>FAO</p>	<p>The CRP 3.6 aims to improve the productivity and profitability of four major cereal crops – barley, finger millet, pearl millet and sorghum, for drylands of Asia and Africa to respond to an expected increase for demand for these crops in the near future.</p> <p>FAO finds the proposal sound and well written. However, it is suggested to address the following comments when revising the proposal:</p> <ol style="list-style-type: none"> 1. Regarding the structure of CRP management and relationship/responsibility between the Steering Committee, Research Management Team and the Advisory Panel we find the commentary provided by the ISPC is in large valid. 2. Seed delivery and acceptance by the farmers are considered very important and critical. Despite the amount of background information and justification provided, the outputs presented in SO4, 5 and 6 are vague. The methodologies and milestones seem to have been thought separately for each component and not considered with an overall view. This may cause redundancy in the program components or failure to deliver proper messages to the end users. For example, output 4.1 deals with Integrated Striga, pest and weed Management and plans to implement Training/FFS in 2012 and 2013. But output 5.1 also deals with Integrated technology packages including Striga management, having the package developed by 2014. The timeframe of events to be implemented between different outputs need to be carefully examined and planned.
<p>IDRC</p>	<ul style="list-style-type: none"> • The program will undertake up-stream research (breeding and technology development) on millets so it could supplement several Canadian International Food Security Research Fund (CIFSRF) millet projects. • There is room for improvement in showing how the program builds on previous work. ICIRSAT and ICARDA have been researching cereals and agricultural practices in dry ecosystems in Africa and Asia for quite a long time. However, the proposed program is not explicit about how previous or ongoing research will be used by the proposed program. • The program has done a good analysis of supply and demand issues of barley, millets and sorghum, in the face of changing economic and environmental contexts in Africa and Asia. • The objectives are well defined but challenging program’s lifespan: In 8 years the program intends to deliver improved technologies to 33 million smallholder farmers; achieve 15% increase in yields on about 45 million hectares in Africa and Asia. • It must be noted that: i) Breeding new varieties and hybrids from gene collections will take at least 4 years and ii) 3 years will be needed to produce or support the production of seed in enough quantities to be

	<p>distributed to the large number of farmers targeted. Therefore, effective adoption of these varieties and hybrids will have to be achieved over a one or, at the most, a 2 year period in order to meet the stated targets.</p> <ul style="list-style-type: none"> • During the 2012-2014 very few concrete deliverables will be produced. • The program follows an orthodox breeding & technology development pathway. • Some breeding innovations are proposed (i.e the use of genomics to speed up the breeding processes) • There is an opportunity to increase the effectiveness of seed systems by using innovative public-private arrangements; the program could benefit by directly engaging private seed enterprises by, for example, co-investing in seed production in India. • The methods outlined are effective, but effectiveness will be constrained by the program’s lifespan. The breeding and technology developing methods are well designed, but the above mentioned time constrains will likely affect their overall effectiveness. • An explicit gender strategy has been proposed. Gender issues have been analyzed throughout the proposal. The analysis tends to oversimplify the complexity of gender relations among poor rural families; for example, the analysis does not take into account the huge cultural differences between Asia and Africa farmer families in terms of gender relations. • The monitoring and evaluation plan is effective. Detailed milestones for each program objective are presented with ex-ante assessments proposed. • It would be nice to have an explicit mention to de development of an integrated baseline (not only ex ante assessments). • All orthodox breeding and technology developing programs face the risk of non-adoption. Participatory breeding and technology dissemination using farmer field schools is usually not enough to assure adoption, this is particularly true with seed adoption. • A more aggressive non-orthodox approach might work better. For example: why not include private organizations as full-fledged partners, from the beginning? Why not include local seed enterprise development and massive seed production as an explicit strategic objective? • It would be nice to have more resources devoted to strengthen the seed and input delivery objective (SO 5.2); only 6% of the budget is allocated to arguably the most critical task in the program
GFAR	<p>General</p> <p>GFAR welcomes the Program stated objectives in the Foreword of the Proposal</p>

as follows:

“Our overriding goal is to achieve farm-level impacts, primarily through higher and more stable dryland crop productivity on smallholder farms in Africa and Asia that will increase incomes and reduce rural poverty, increase food security, improve nutrition, and help reduce adverse environmental impacts (especially in dryland crop-livestock systems).”

“We believe that the success of DRYLAND CEREALS will dramatically improve the livelihoods, food security, nutrition, and health status of millions of our fellow citizens”

While these objectives are well aligned to the CGIAR SRF and GFAR Roadmap for transforming agricultural innovation services to deliver impact on development, one can express doubts about the level of impact expected on improving the well being of the “world’s most vulnerable poor”. How many of them the DRYLAND CEREALS Program can truly lift out of poverty? According to data from the proposal, expected net income will be 2,2 billion to be divided among 33 million households. This means a US\$68 additional income per HH per year, which for the most vulnerable of the poor (average of 5-people per household) corresponds to an increase in daily income of 0.03 dollars/day. This is 2% of the daily poverty line income (at 1.5 dollars). From the 33 million households expected to benefit from this program an optimistic figure would be to lift about 700 000 persons out of poverty. And that would concern only those who are at 1.47 US \$ /day. Since the most vulnerable of the poor have much less than that, we are skeptical about the CRP 3.6 indicative number. The same remark can be applied to hunger reduction. The numbers are not totally adding up.

We would like to suggest that instead of starting from the usual entry point of crops, agro-ecological region and farming system categories, the proposal should have focused more on the specific conditions of the targeted population (where and who live the people for which an appropriate research program on dryland cereals can really make a difference). This would be more consistent with the acknowledgement of a wide diversity of situations and would be sound with the claims of Farmers organizations for the CRPs to be designed with and for the beneficiaries, not just by consultation but by co-decision.

We completely endorse the comments made by ISPC on the proposal impact pathways. The proposal would certainly gain in being more tightly connected with other initiatives and build on partners’ comparative advantage, starting from those of a number of NARS.

1. Partnerships in the proposal and significant involvement of national partners and national development priority setting processes

There are a couple of statements such as “*DRYLAND CEREALS partners believe that strong and sustained efforts are urgently needed to deal with these changing realities*” or “*smallholder farmers heavily involved in identifying preferred traits and driving participatory breeding programs*” (Executive Summary) that refer to consultations that have been done for the design of the proposal, however there is very little evidence of this and that a real collaborative approach have been adopted widely in the formulation of the proposal. This is not substantiated anywhere in the proposal with precise reference(s) to involving regional fora and/or other GFAR constituencies in the project design (with the exception of so-called initial DRYLAND CEREAL partners: two CGIAR sister centers, the Generation Challenge Program -which includes two ARIs from France- one fast growing country NARS(India) and Iran.

No indications are given regarding how priorities/concerns from users have been incorporated and no reference to Farmer Organizations being involved in the formulation of the proposal. Criteria for selecting focused crops (because “*All four crops, for example, can be improved using similar breeding and development approaches, e.g. participatory breeding, the use of genomic-based methods, and the exploitation of heterosis*”), areas (more than 1000 000 ha), farming systems, or setting targets are only defined by the initial partners, without apparently including expectations from other constituencies.

We were not able to find concrete evidence of linkages with the Regional Fora and other GFAR constituencies and/or community of practitioners in the development of the proposal, nor particular indication of relative allocation of resources between the CGIAR centers involved and partners. No budget is planned for other than the limited number of initial partners (see table 5...)

2. Likelihood of impact on the ground and support to other stakeholders capacities to take up necessary functions in delivering impacts

So far, beyond general impact based on the IMPACT forecasting model, there is no really detailed evidence of the real impact at farm level, especially without a more refined typology of beneficiary household (who?, where?) that would have helped better define the local partners and responsibilities in delivering impacts. A better definition of who are the really “world’s most vulnerable poor” needs would have been helpful because this reference is simply transformed into “smallholder farmers” in the text of the proposal.

3. Practical commitments and investments required of other partners

	<p>engaged</p> <p>There is no formal commitment of other partners in the budget, beyond statements of expected contributions in kind and/or activities to be conducted by the partners as indicated in Annex 4. There are a lot of expectations and numerous partners are cited but there is no commitment of resources so far.</p> <p>4. Role of international public goods in adding specific value to national processes and capabilities</p> <p>As a global program its intrinsic value would be in engaging in activities that national research programs cannot address because they are transversal, multisectorial and/or international. This is envisaged under Strategic Objective 1, but it would have been better to have it done before so that the proposal could have clearly stated to what extent it brings and added-value and what kind of added-value (beyond the prime meaning of added-value, that is increase in net income of farmers).</p> <p>5. Responsibilities and accountabilities to other partners/beneficiaries</p> <p>Not found in the document except capacity building being considered as mean to transfer knowledge about technology....</p>
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