(13 October 2011)

ISPC Commentary on the Proposal for CRP 3.5 Grain Legumes

Summary

CRP 3.5 is a comprehensive proposal built around combined research on eight grain legumes (GLs) – groundnut, soybean, chickpea, cowpea, common bean, pigeon pea, lentil, and faba bean – proposed by four CGIAR Centres (ICARDA, ICRISAT, IITA and CIAT) and by several NARES (India, Brazil, Turkey and Ethiopia). These crops are important dietary staples and sources of income for farmers in many, low income, food-deficit countries. Moreover, GLs can make important contributions to the nutritional intake of the poor.

The proposal is centred on six strategic objectives (SOs). The SOs are clear and activities well laid out. Thoughtful attention has been given to ways of targeting women through varietal improvement efforts, and there are strong components on management of databases, capacity building, communication, knowledge sharing and partnership (although a detailed strategy on partnership management is lacking). CRP 3.5 integrates crop- and region-specific programs into a global program. It aims to streamline the partnerships with NARES, offering a single interface to reduce the burden that these institutions currently bear in interacting with multiple Centers and stakeholders, and aims to strengthen research in areas critical to improving the livelihoods of people who depend on GLs for food and income. These are laudable goals, and a set of promising initiatives and cross-learning opportunities are identified although actual progress towards realizing these synergies will need rigorous monitoring.

The overarching framework for research prioritization is promising but incomplete, resulting in selection of eight GL species and five regions of focus. The rationale for resource allocation across the 6 SOs, GL species and regions is lacking and specific milestones given for each SO output are generally weak. Expected outcomes in terms of yield gains are aggregated and uniform across all eight GL species and five focus regions. This general approach seems unlikely and hinders prioritization in relation to potential impact. Taken together, the rationale for amount and allocation of resources does not give confidence to investors that the proponents have rigorously prioritized the research agenda across objectives, species and regions. Missing is an analysis of past successes and failures of GL research over the past 30 years. Such an analysis would provide the basis for a stronger case for the research that is proposed. It is well known there is much less documented success from GL improvement research compared with such work on the major cereals. Despite examples of prior successes given in the proposal, evidence of lessons learned from previous GL research is lacking.

The case made for the comparative advantage of the proposed CRP, over individual Center programs as they currently exist, could be strengthened by mapping the transition from the present state to an integrated global program that captures the proposed synergies, including showing what current activities have been dropped. While clear advantages exist for genetic improvement and agronomic systems research that seek to better utilize the unique physiological attributes of GL crops (e.g. biological N fixation, and abiotic stress tolerances), research components such as climate change, seed systems improvement and some components of the value chains are more difficult to justify.

The budget is disaggregated by region and crop, but significant challenges exist with the proposed management and governance arrangements. Virtually all of these challenges are common to the proposal for CRP 3.6 (Dryland Cereals), and many to CRP 1.2 (Humid tropics) as well. The ISPC highlights the limited authority and resources provided to the CRP Director, obstacles placed in the path of management accountability and unrealistic time allocations to coordination of the SOs. The arrangements understate the value and cost of effective management.

In general, the proponents could and should make a stronger case for inclusion of CRP 3.5 in the CGIAR portfolio. The CGIAR (as reflected in the SRF) has always found a strong rationale to work on rice, wheat and maize as the three major staples for developing country agriculture, and has well documented success and impacts from research on these three crops. For GLs the niche is often smaller or they play a complementary rather than dominant role in other crop-based systems. Further, the documented impacts from previous CGIAR efforts on GLs have not been impressive. The CRP proposal appears to oversell the relative importance of GLs, in terms of growth in demand over time and their contribution to human protein. (In Food Deficit Low Income countries, on average, pulses currently contribute less than 4% of the total protein consumed, and in most cases this is decreasing over time). This CRP needs to make the arguments for research investment *de novo* and in relation to the developmental and research constraints – not accepting current crop choices and research topics as the status quo particularly when an increased budget is requested. The proposal could be strengthened by objectively demonstrating the relative importance of these crops in the CGIAR portfolio. The use of a 20% yield increase on 20% of the total planted legume area by 2020 as a basis for production, income and incremental protein benefits fails to discriminate across crop species, regions and the most important constraints, and reduces credibility of the analysis. A disaggregated analysis could provide confidence in justifying the proposed allocation of resources.

Thus, while the overall thrust of this CRP is good and has potential, the work plan provided suggests a 'business as usual' approach. Success will depend on rigorous prioritization in the first instance and subsequent monitoring and evaluation to ensure progress, maintain focus, and support a continuing process of prioritization and refinement. A significant obstacle is the lack of robust data on legume production area, systems, yields and yield trends, and supporting biophysical data on climates and soils in key production areas of GLs (precisely because they are typically in marginal areas of SA and SSA, where infrastructure is poor). CRP 3.5 should work with partners towards rectifying this information vacuum, as a key component of the revision of the proposal and the subsequent work plan.

Recommendation

The ISPC recommends that CRP 3.5 be approved subject to substantial revisions and resubmission, taking into account the commentary that follows, with emphasis on:

- A much stronger description of the potential of research on GLs to decrease poverty and hunger as a basis for prioritizing crop-region-constraint combinations—perhaps different from that done in the past. It should:
  - Objectively demonstrate the relative importance of these crops in the CGIAR portfolio, drawing on information related to GL-specific producers and consumers in the different target regions.
  - Undertake a comprehensive assessment of past research efforts and current barriers to adoption of technology, as a basis for identifying key constraints and opportunities that could be influenced by CRP3.5 research products.
  - Establish targets for outcomes in a crop by region matrix to account for actual situations and current status from a regional and crop species perspective, and

strengthen capacity to prioritize allocation of resources for GL research within this CRP and within the CGIAR.

- A work plan with more focus and fewer product lines: that this CRP concluded with such a large number of product lines (61 crop/traits for genetic improvement) indicates the difficulty of moving from individual programs to a global program within a CRP.
- Given limited success to date in the adoption of 'improved GL technologies', demonstrate feasible impact pathways, citing relevant references and documentation.
- This CRP should be closely allied to and integrated into the system CRPs, and particularly CRP1.1.
- Highlight the new and most promising areas of research: the list of innovation initiatives and cross-learning opportunities on p122-123 are ambitious and commendable and deserve a more prominent place in the proposal, with an explanation of the value that would be generated by succeeding in each of these initiatives.
- In management and governance, a more streamlined structure is needed that provides for independence in decision making, monitoring and evaluation. (i) The Advisory Panel needs to be more appropriately structured and resourced with formal oversight by the Lead Center Board; (ii) redundancies in the Steering Committee and the Program Management Team need to be addressed; (iii) the role and authority of the CRP Director needs to be strengthened; and (iv) the CRP management functions central to the success of the program, including communications, resource mobilization, and program evaluation, need to be clarified, adequately resourced and managed.

1. Strategic coherence and clarity of Program objectives

The proposal is carefully written and well organized and reflects the experience of the participating Centers in working with GLs. It is commendable that stakeholders' consultation contributed to proposal development.

Program objectives are clear and the research plan is comprehensive, with reference to the SRF. Although the proposal rightly points out that GL cultivation and consumption are consistent with the SLOs, the justification presented is a general outline of how increasing the production of GLs could in theory contribute to meeting the four SLOs. A more detailed explanation is needed about how links between research outputs from this proposal lead to increases in production, resulting in positive changes in indicators related to the SLOs.

To support the supposition that outcomes from research on GL will address the SLOs, the proposal argues (p12) that 'Farmers both consume and sell GL crop products, granting them flexibility to optimize their livelihood strategy according to household food needs and market conditions.' And, 'A wide range of processed products from these (GL) raw materials add further value and generate important income-earning opportunities for poor people, especially women.' While these are powerful arguments in favour of research within the context of the SRF, citations given in support of these arguments are not from peer-reviewed literature.

The prioritisation framework for CRP 3.5 is based on three selection factors: regions of large historical grain legume production area; low income, food deficit countries; and numbers of poor. While this framework is helpful, considering the current state of knowledge, it is too coarse for effective prioritisation of a USD140 million investment in GL research over three years. Moreover, CRP 3.5 has done a commendable job in presenting the estimated yield losses due to many different abiotic and biotic constraints on the eight GL crops (Appendix 6). Average values for yield gaps and plausible closure of yield gaps are reported in Table 5.3.1. However, this information is not used as a basis for prioritizing research based on highest expected value in terms of impact on SLOs. Budgets appear to be based on current research investments at the respective Centers.

Projections of the increase in GL production needed to meet future demand seem overstated based on trends in pulse production and consumption in key producing countries. For example, food supply data for India (FAOstat data), the world's largest producer and consumer of GLs, shows that per capita availability of All Pulses declined by almost half since the early 1960's (22.8 kg/capita/year in 1961 vs. 12.9 kg/capita/year in 2007). There appears to be a very slight rise in availability over the past 10 years but it is hard to conceive based on that recent change that demand will grow by over 3% per year over the next 15 years. The proposal also overstates the importance of GLs relative to other sources of protein. For example, FAO data indicate that in 'Low Income Food Deficit Countries' (also examined by this proposal) that animal and fish products, as a group, are the major sources of protein (68.1g/capita/day) followed by cereals (33.2 g/capita/day) and then pulses (4.1 g/capita/day).

While we commend the proponents for performing a rudimentary yield gap analysis as a tool for identifying where the greatest opportunities may be found for increasing productivity, the methods used to make these estimates are still coarse. Efforts to improve these estimates, and the databases and methods to simulate them, are encouraged. Such yield gap analyses should focus on water-limited yield potential as the benchmark.

If a 20% yield increase over a 20% area farmed by the rural poor by 2020 is to be realistically addressed then the reasons for historic slow adoption need to be identified, understood and remedied. Elements of doing this are presented in Section 5, but the current implementation plan suggests little change from a previous *modus operandi*, which has resulted in slower than desirable adoption. Missing from the proposal is a comprehensive analysis of past successes and failures of GL improvement research over the last 30 years. The proposal makes reference to this sort of work: *"Farmers may encounter many constraints in adoption of improved technologies, especially pest and nutrient management practices, which are knowledge-intensive. These will be documented to draw lessons for future research"* (p67). This is a commendable goal but one would have expected this documentation in the proposal itself. There is much less documented success from GL improvement research compared to say, cereals, potatoes and cassava improvement research, and evidence of genuine lessons learned from previous GL research is deficient in the proposal (despite the sections titled as such). What are the main reasons behind the large gap in farmer field yields and experimentation station yields? Why don't farmers use existing improved varieties and apply recommended pest management strategies and more fertilizer and more labor to their GL crops even under marginal conditions? Are they ever likely to considering the risks and opportunity costs? How will the CRP ensure that the program is developing technologies that will be useful to and taken up by farmers? The proposal should include a separate section under each SO describing past work – success and failure – and how the current work will explore other ways and means of addressing the major constraints. Providing this rationale would help inform the Impact Pathways, which presently lack sufficient analysis and detail, generally assuming a smooth transition from research outputs to research outcomes.

These difficulties and deficiencies in addressing strategic coherence and prioritization can be greatly reduced if CRP 3.5 and its partners are diligent in efforts to identify data needs and invest in data acquisition and work together for substantial improvements in the prioritization of GL research within the CGIAR as a top-priority activity.

2. Delivery focus and plausibility of impact

The proposal has a strong gender component. Mainstreaming of gender is likely due to the pioneering and successful efforts in this area by most of the partners of CRP3.5.

The proposal is weakened by a lack of focus and should undertake research on far fewer product lines. Although initial priority-setting resulted in some crops and regions being explicitly excluded, the fact that the process concluded with such a large number of product lines (61 crop/traits for genetic improvement) indicates the difficulty of moving from

individual programs to a global program in this CRP. CRP3.5 must make the transition to fewer targets with greater probabilities of success. Some research with a low probability of success has been included - for example, continued work on bio-pesticides. It is noteworthy that a considerable body of research exists on bio-pesticides for GLs with modest impact.

The section on SO2 "*Accelerating* the development of more productive and nutritious cultivars…..") is quite strong. The traditional constraining biotic and abiotic stress factors are listed in Table 5.2.1, along with some grain quality factors. In setting crop improvement priorities, factors beyond the crop production phase need to be considered—along the entire range from input supply, through post-harvest, marketing, consumer requirements, etc. It is stated (p32) that "SO5 (value chain analysis) will help SO2 to refine breeding objectives to develop cultivars with market preferred traits". It is essential to analyse the broader social and economic environment as part of the priority setting tool for breeding objectives and activities indicated in SO2. The question needs to be asked – "even if the major biophysical constraints could be alleviated, and grain quality parameters assured by genetic improvement, would there still be factors along the value chain that would prevent production increases leading to poverty alleviation?" There is also a need for the proponents to make explicit that in the implementation phase the CRP develops a crop-specific breeding program taking into consideration the previous comments and past research experience of Centers and partners involved in GL genetics and plant breeding.

Within SO3, a number of proposed activities deserve further consideration.

- Strategies to optimize Biological Nitrogen Fixation (BNF) have been attempted for resource-poor farmers over the last 50 years, but with little positive outcome. But considering its potential, BNF R&D it is still worth pursuing if further efforts were based on an analysis of reasons for non-adoption and different approaches chartered. Previous failures in expanding the use of more effective *Rhizobium* inoculum mainly relate to marketing failures. However, there is room for CGIAR inputs in unravelling the physiological basis of environmental effects on N fixation as advocated by Giller (2009).
- "Methods to increase legume productivity and profitability through increased resource use efficiency developed, tested and promoted." This is a vague, non-targeted description of potentially useful things to do and needs to relate better to genetic improvement aspects mentioned under SO2. Similarly, the statement "Efforts will also be made to optimize water and nutrient inputs to maintain soil health and sustainability of production system" is a vague generic statement not indicating what actually will be done.
- It would seem appropriate to mention under this output some of the possible innovations for including legumes in evolving conservation agriculture approaches, where improved crop rotations may be required to help control weeds and diseases.

Improving seed systems (SO4) is an issue that goes beyond the purview of the partner institutions, and is not limited to GLs. The failures and obstacles facing seed systems in the target regions are well documented and complex. Although the proposal does recognize that poor seed systems are a constraint to GLs adoption and diffusion, it is unlikely that this project will succeed in improving seed systems on a large scale within the next decade, when numerous organizations (with arguably more relevant expertise) have been working on this issue for at least thirty years. The proposal speaks of supporting an emergent private sector in the seed industry, but it is not clear what expertise CRP 3.5 institutions have in this area. This is a key strategic factor that would need to be included in a revision of the proposal.

Improvements in value chains are proposed in SO5. It is clear that value chain opportunities have much to do with profitability—and hence with adoption—of new GL technologies. Unlike crop genetic improvements, however, changes in value chains are complicated to achieve and involve changes in marketing systems, processing, and distribution. It is not clear that the partner institutions behind CRP 3.5 are the best actors to bring about changes in GL

value chains. (Supermarket chains and other retailers might be better positioned here?) But, there is value here in terms of contributing to priority setting for SO2 and 3—if we know where the value chain opportunities are to be found, then it might shape the targets for genetic improvement or crop management techniques. This seems more realistic than the prospect of CRP 3.5 institutions helping create new markets for processed GLs or the equivalent.

One other promising area of research here relates to post-harvest losses – as GLs are highly susceptible to post harvest insect pests or fungal attack (mycotoxin-affected grain). Training of farmers in proper handling and storage has been mentioned (p96) but the CRP does not give much detail. This is extremely important as many of these technologies have been known for a long time, but little progress has been made on adoption. The proposal recognises a strong need for capacity building and there is an ambitious program of capacity building planned around each of the SOs. One weakness, however, is that there is no strategic analysis of capacity building around the key drivers for the global targets, which reinforces the need for greater focus.

The use of a 20% yield increase on 20% of the total planted legume area by 2020 as a basis for production, income and incremental protein benefits fails to make use of any discrimination across crops and their constraints and their regions. There is no detailed analysis of the contribution of the various SOs in each of the crops or regions to this target, and no justification of the allocation of resources based on any such analysis. This is a significant deficiency in the plausibility of the targeted impacts, and undermines the credibility of the targets. For example, how much of the target will be met by improved seed systems, or by improved agronomic practices and systems? Greater differentiation should be used to calculate expected yield gains for each crop and region (based on tables of yield loss, expected yield recovery, yield gap etc.) and valued accordingly.

There is little evidence provided that there have been studies of impact pathways for the major GL crops to support the schematic pathway framework in Figure 4.1, or the pathways defined for each SO. Each component of an impact pathway needs to explicitly consider how the outputs generated will address the major constraints faced by primary users, and describe a strategy to ensure effective uptake by them. The modest uptake of improved varieties and production technologies that has occurred for GLs in targeted regions of SSA and SA over the past 30 years suggests that research to better understand impact pathways and key players in the pathway would be helpful.

A strong claim is made in the proposal (on p2) that '...CRP 3.5 Grain Legumes core partners have nonetheless achieved remarkable impacts in all regions. They have helped countries to increase grain legume yields, brought destructive diseases under control, made headway against the complex problems of drought, and connected grain legumes to export markets for higher incomes.' For some legume crops, in some regions, this may well be true, but it is important that credible evidence is provided to substantiate these claims. Although the proposal highlights 12 cases of success (p24-25), many of these are yet to be realized, e.g., drought tolerant bean varieties in Rwanda and Nicaragua, *ex-ante* assessment of improved cowpea in Nigeria, or they are impacts documented on a relatively small scale, as found by the recent scoping study commissioned by the ISPC.[1] Hybrid pigeon pea, for example, has been researched for 35 years but substantial commercial uptake is yet to occur. Also, high levels of adoption of winter-sown technology for chickpea in Syria are indicated by Mazid et

[1] Tripp R. 2011. The impacts of food legume research in the CGIAR. A scoping study for the Standing Panel on Impact Assessment (SPIA) of the CGIAR Independent Science and Partnership Council http://impact.cgiar.org/sites/default/files/images/LegumesScoping2011.pdf

al. (2009) but this refers to sampling areas where winter chickpea is grown, which is a relatively small portion of the total chickpea growing area in that country.

3. Quality of science

The science underpinning the proposed research is generally sound and builds upon the experience of the participating Centers and partners. The proposal provides examples of their broad experience in innovation and the delivery of new products. This CRP would benefit, however, from identifying and focusing on a few scientific areas of global excellence (the proposal mentions these in a number of places, e.g. the chapter on innovation). The CRP could emerge as a center of excellence in these areas and develop its global scientific presence.

The 'Descriptions and Methodology' sections are reasonably well-written, and although lacking in detail, the science is sound. Some issues stand out, such as the protection of yield from disease and pests in GL production systems—an ongoing battle that requires a full arsenal (host-mediated genes, bio-control agents, vigilant IPM practices etc.) However, on this point (addressed under SO3) the proposal lacks sufficient detail on the routes to achieve solutions and appears to be relying on a continuation of what the Centers are currently doing.

While the proponents are to be commended for attempting to perform a yield gap assessment to help prioritize research, the supporting analysis given for SO3 (p65) is opaque and does not appear to have adequate underpinning physiological, ecological, or simulation to support it. Considering the importance of yield gap assessment to research prioritization and geographical emphasis, CRP 3.5 should strive to improve the capacity to perform sound quantitative yield gap assessment for GLs in target regions. Such methods and research capacity would be an important IPG.

Although data is provided on the numbers of accessions currently in CGIAR germplasm collections (Table 5.1.1), it is recommended that this be extended to include worldwide accession numbers for GLs, to give a more complete picture of the existing genetic diversity (for example, the USA soybean germplasm collection has more than 20,000 accessions).

4. Quality of research and development partners and partnerships

Intended partners and partnerships are carefully considered and appropriate. CRP 3.5 - unlike other CRPs where the partnership strategy is more generalised - commendably proposes to focus on and invest in a core group of activities, including capacity building and knowledge sharing, that aim to increase the value of partnerships in achieving impacts. More specifics about building and maintaining partnerships would be helpful but, by treating partnerships as one of the SOs, it ensures that at program management levels there will be at least one member of the team responsible for the effectiveness of the partnership strategy.

Specific examples of opportunities for partnering with other CGIAR Centers are provided (p23-24) and these offer a useful way of monitoring progress towards achieving effective collaboration within CRPs. CRP 3.5 benefits from being among the last group of proposals drafted, so that the proponents can be expected to better visualise potential interactions with other CRPs, as listed in Chapter 9. As such, one would have expected to see closer integration with the system CRPs, particularly CRP1.1. It is not evident that there was sufficient collaborative preparation across the two dryland institutes involved. In the original formulation of the SRF, GLs were not a subject matter for individual CRPs but would have been part of the systems programs. It is still appropriate to ask how these "minor" crops will be handled and their relationship to the system's programs in the overall CGIAR portfolio.

For non-CGIAR partners, the list appears to be lacking for SSA. Only Ethiopia is listed, yet a large majority of the work will target SSA and will be conducted there. The ISPC recommends that additional partners in SSA be sought. More private sector partners are also

essential for achieving impact (e.g. in seed production and delivery). The private sector will be especially important in production and delivery of *Rhizobium* inoculants, but there is no mention of linkages or partnerships with the private sector towards this goal.

5. Appropriateness and efficiency of Program management

There are similarities in the management and governance structures of the CRPs 3.5 and 3.6 where ICRISAT is the lead-Center and many of the ISPC's observations on these two CRPs are the same. The proposed structure for CRP3.5 management and oversight includes:

- A Steering Committee (SC) of approximately 12, initially comprising the "top leaders (or their designates) of the major partners—including regional/sub-regional organizations, IARCs, NARES, ARIs and private sector organizations…" (p131), to oversee strategic direction, monitoring of overall performance, and improvements to operational mechanisms
- The CRP Director, whose duties include external communications and research mobilization
- A Research Management Team (RMT), comprising the coordinators of the six SOs as well as the research directors from key partners not represented by coordinators
- An R4D Advisory Panel of six to 10 members to provide input and advice primarily to the RMT.

The lead Center, ICRISAT provides an unspecified range of financial and management services to the CRP and its DG acts as chair of the SC for an initial period. No executive office or program management staff other than the CRP Director is described in the proposal. The six positions dedicated to coordination of the strategic objectives are budgeted to spend 25% of their time on the responsibilities that attach to the RMT. This limited time means that they can hardly be considered managers of the strategic objectives much less a management staff for the overall program, the challenges of which should not be overlooked.

Both the SC and the RMT are problematic. Each is essentially wholly representative of the primary partners. All primary partners are represented on the SC. Each is guaranteed a spot on the RMT. The roles of both the SC and the RMT in priority setting and resource allocation fail to provide any formal space for independent, disinterested decision making; instead they have significant potential to preserve the status quo. The impulse behind the structure may be to build transparency among partners and enable consensus but the effect is to create a drag on the potential for genuine leadership and innovation. Between the SC and the RMT there is little incentive to move past the aggregation of existing projects, partnerships and funding that characterize the start up of the CRP to create a program with its own priorities that has the capacity to attract the influence and resources needed to advance its goals.

Although the R4D Advisory Panel offers a mechanism for engaging scientific and development advisors from outside the partnership circle, it is primarily an input to the RMT with the potential for additional interaction with the SC. It has no formal or informal relationship with the ICRISAT governing board. Finally, its name subtly but effectively signals its standing in the structure—it is a panel, not a committee, and its members are described as being part of a "pool." The budget allocation for this panel is further proof of the intended limits of its role.

The CRP Director has not been given the scope of work or sufficient authority to manage a program with a projected annual operating budget in the range of USD40 to 50 million. The fact that the position will be internationally recruited and compensated accordingly does not offset the limited conception of the position. The Director is expected to serve as the public representative of the CRP, helping to raise its profile and the value of its work, to lead partner/donor relations, and to be active in resource mobilization. For all that, the position does not appear to have any authority—to appoint a management team or to evaluate the

performance of team members, to provide genuine leadership for the achievement of the program's strategic goals, or to shape ongoing planning.

Program management appears to have no staff dedicated to it but relies on ICRISAT for unspecified management support. Although the proposal demonstrates a nuanced understanding of the value of both communications and knowledge sharing, and the differences between them, no ideas are presented as to how a more externally focused communications strategy designed to raise awareness about dryland GLs and build interest at a global level will be coordinated or managed. All of the resources for communications and knowledge sharing are embedded within a strategic objective. The assertion that "the program's communication action plan [will be implemented] at all levels and be carried out by many of those involved in the R4D work" (p133) suggests that eventually nobody may be in charge. A comparable challenge can be anticipated in resource mobilization.

Assigning both of these important tasks to the CRP Director and then expecting the program to acquire capacity on an *ad hoc* basis is unrealistic. It is possible to subcontract for backroom functions like financial services and HR; it is much more difficult to subcontract for an ambitious communications program or professional resource mobilization, particularly if the Centers continue to maintain corporate identities and seek resources for programs that fall outside of the CRP.

6. Clear accountability and financial soundness, and efficiency of governance

The total budget for the project over three years is projected to be USD137 million, which includes a funding gap of USD33.5 million. Although each of the CGIAR Centers is assigned a portion of the funding gap, the presentation of the budget by SO and by region (tables 14.2 and 14.3) does not indicate where funding gaps are anticipated to occur in the program. It is therefore not possible to see where a potential shortfall will have the greatest impact, nor is a contingency budget presented that illustrates how resources will be allocated in the event that the additional funds are not raised.

The CRP Management Budget allocates a significant percentage (40%) of its USD2.4 million budget to meetings that enable the full representation and participation of partners at three points in the program's governance and management (SC, RMT, Global and Regional Coordination Meetings). The Advisory Panel is provided with approximately USD20-25,000 a year to support the participation of its pool of six to 10 advisors. The imbalance is indicative of an inherent problem with the structure.

The Advisory Panel has the potential to bring together expertise and perspectives of value to the program and provide a more independent level of planning and oversight than currently exists in the proposal. The Panel's role is to "provide independent guidance on strategic planning, new R4D opportunities, and research progress across the CRP agenda". It is proposed to have six to 10 members appointed by the SC based on recommendations by the RMT.

The proposal does not envision the Panel meeting as a group on any consistent basis, rather the Panel is intended to provide the program with a pool of experts who can be tapped a few at a time to participate in meetings of the research team, or occasionally the SC. Aside from a three-year term for appointments to the Panel, there is no other structure proposed—no regular meeting as a Panel; no leadership structure; no link, formal or informal, to ICRISAT's governing body. Although there is a reference to its role in evaluation of the CRP's performance, there is no realistic way it could effectively fulfil this function given its lack of structure and support. As noted, the budget for supporting the work of the Panel is minimal.

CRP 3.5's management structure has two bodies that are insufficiently independent, and one without the mandate and structure to be effective or fully useful. As a result, the program

needs to establish a mechanism that can support its accountability, increase the transparency and independence of decision making, and reduce any potential risk that of affirming the status quo at the expense of the program's potential impact.

At present, the Centers and other partners are given adequate opportunities to observe the program and strengthen it through the involvement of their research staff on the management team as well as participation in twice yearly global and regional coordination meetings. The SC as described would seem to be superfluous and counterproductive.

With that in mind and to strengthen the management and governance of CRP 3.5, the following recommendations are offered:

- Strengthen the structure and terms of reference for the Advisory Panel to give it a more substantial role in monitoring and evaluation, and in recommending program priorities and resource allocations. Provide a mechanism that allows a DG or equivalent from one of the primary partners to be a member of the Panel, in addition to the DG of the lead Center who can serve *ex officio*.
- Establish a chair for the Panel, elected from among the members of the Panel, who has reporting links to ICRISAT's DG and board chair on the progress of the CRP.
- Eliminate the SC and redistribute its proposed functions to the Advisory Panel, the RMT, or the CRP Director as appropriate.
- Strengthen the role and authority of the Director sufficiently for the incumbent to lead and manage the program in an effective way. The evaluation of the Director's performance (and future recruitment) should include the chair of the Advisory Panel. The reporting relationships between the Director and the members of the RMT should also be strengthened to increase the ability of the Director to manage for performance.
- Identify more clearly the management activities that will be undertaken by the program office or management unit to assure that functions central to the success of the program, including communications, resource mobilization, and program evaluation, are adequately resourced and managed.