

SCIENCE COUNCIL OF THE CGIAR

Commentary on the Challenge Program Proposal

An Oasis Challenge Program to Combat Dryland Degradation across the Developing World

SC Secretariat, 20 April 2008

The Oasis CP proposal was submitted by ICARDA and ICRISAT on behalf of the Alliance of CGIAR Centers and several major external partners, predominantly from Europe. Oasis is described as “a global research-for-development partnership proposing a concerted, leading edge effort to break new scientific ground in the fight against dryland degradation (desertification) across the developing world”.¹ As such, it would respond to the call by the United Nations Convention to Combat Desertification (UNCCD) which calls for stronger science input in its 10 year reform plan.

The SC considers that the proposal is more advanced than the former preproposal and it includes a system analysis framework which reflects the complexity of the problem. The proposal recognizes that desertification results from interplay between global and local drivers and the proponents advocate a landscape management approach. It also recognizes that in the use of drylands there is often a trade-off between various ecosystem services, food production being one. It recognizes that there is a need for flexible spatial scale analysis and long term approaches to sustainability. The proposal strongly advocates an interdisciplinary (and at times a transdisciplinary) integrative approach to this problem. The proposal states which System Priorities it aims to address. The research is proposed to be conducted through three sub-programs or K (for knowledge) streams based generally on dryland assessment, landscape management and ecosystem services; and policy and institutional options for drylands.

While there is little doubt about the importance of drylands and the need for a more ambitious research agenda to solve their problems, the SC finds this proposal disappointing in making the case. It does not provide a definition or any typology of drylands so that the scope and scale of the activities are unclear (and thus the proposed five year budget of USD66 million is difficult to evaluate). Whilst it makes a case for integrated science and management approaches to tackle complex problems, it does not adequately make the case for why dryland issues in particular should be addressed above other high priority areas relevant to the CGIAR. Much of the proposal is about quantification of the dryland problem with few on the ground interventions to address the problems. “Land users” and “learning alliances” are central to the described proposal, but how the new science will translate to effects on poverty, food security and sustainability is less well developed. The “land health” model used as a conceptual approach is not compelling - most effort would seem to be directed to diagnosis and scientific methods rather than a key development issue, potentials and constraints in dryland areas, with little in terms of remedy.

¹ Oasis is an existing partnership platform sharing many of the same concerns as the UN Convention to Combat Desertification. Several of the current projects of ICARDA and ICRISAT are relevant to this theme and some are being reoriented to bring them in line with the partnership program. Current projects which are gathered under this umbrella include such examples as the Desert Margins Program (DMP) and the Asian Development Bank-led Central Asian Countries Initiative for Land Management (CACILM). Five bodies are jointly co-proposing Oasis to the CGIAR for Challenge Programme status: CIRAD/IRD, European DesertNet, ICARDA/ICRISAT, Institute for Environment and Sustainability (IES-JRC of the European Commission), and the Sahel and Sahara Observatory (OSS).

The 3 Kstreams do not appear to have a tight and common conceptual framework for cause and effect analysis and they seem very methods driven rather than issues driven. The proposal does not adequately frame research in terms of testable hypotheses so that most of the outputs defined in all three sub-programs (K streams) are vague, not easily measured and refer more to process and tasks than to real outputs. It makes allusion to points of overlap with climate change concerns, but the topics mentioned would seem to be of lower priority in the climate change agenda. The expected synergies with CGIAR and partner research on water are not developed.

The Proposal is very ambitious and has goals that range from realistically attainable to unrealistically ambitious. The Proposal lacks structure and a proper hierarchy of tractable activities. In their efforts to aim high, to add substantially to the understanding and store of knowledge concerning land degradation, the proponents seem to have underestimated the importance of how poor land users react to the pressures they are under, and what influences the decisions they make about managing their land. Although land users occupy the center of the System Analysis Framework, as they should, there is scant reference to their position there in the rest of the Proposal, and especially in the section on Prospective Major Impacts of Oasis. It is not clear that the approach will have substantial impacts on CGIAR beneficiaries in the time course of the program.

The SC finds that a large and complex “challenge” has been described but that the proponents have not sufficiently articulated a research challenge that can best be achieved by a CP and which would have maximum impact for CGIAR beneficiaries. Therefore, the Council suggests that the proponents continue to develop the appropriate research agenda emerging from this challenge building on the past CGIAR research history on drylands and the existing related activities of ICARDA and ICRISAT. As this develops, the CGIAR could consider a CP on this topic in the future. Since the SC considers desertification as a key research challenge for the CGIAR and partners it encourages both Centers in collaboration with relevant ARIs to pursue practical engagement with land user groups in order to improve policy formulation within the CGIAR’s current dryland research portfolio.

SC comments relating to the specified criteria to assess CP proposals

Relevance of the expected outputs

The Council was encouraged to see the dryland issue set out in the following manner:

- *“Previous biophysical research has been at the plot or field level, which misses many important drivers of degradation that operate at larger scales. Better landscape management and tighter capture and recycling of natural resources, for example could enhance water, nutrient, plant and animal assets and system resilience, and benefit populations that live in similar landscapes elsewhere. Oasis will research ways to address the landscape scale, as well as cross-scale interactions that impinge upon it.”*
- *“Sustainable dryland management depends on the land use choices that inhabitants make; yet previous research rarely integrated their knowledge or attempted to understand their motivations and worldviews.”*

But the SC was disappointed to find no clear researchable issues pertaining to these problem statements. The paper does not define drylands and lacks agroecological and geographical prioritization for the research. The term “drylands” includes an enormous variation of climate and land combinations (e.g., from relatively high rainfall to very low rainfall). There should be a

priori criteria to define priority areas due to e.g., human population, potential to improve productivity, need to demonstrate infeasibility for establishing sustainable agricultural production systems, etc. The lack of a definition is important too because it is not clear why drylands should receive more attention than other types of less favoured lands (e.g. forest margins, hillsides, mountains or uplands).

It is not clear that the expected outputs will substantially further the CGIAR's objectives. Firstly, they are so broadly defined and qualitative that it is hard to gauge to what extent they would be achievable within the proposed time frame and budget – for example, Output 4, p. 26, “Shared knowledge through co-learning...”, and Output 1 p.33, “Methods for improved understanding...” (p.33) are exceedingly vague. Some of the outputs seem unachievable – for example, Output 1, p.26, “Assess dryland ecosystem services and dynamics and their effects on productivity, buffering and resilience capacity”. Though a task rather than an “output”, it is immense in scope. Secondly, the proposal gives the impression of being more concerned with advancing “*the frontier of land health assessment science*” than with achieving beneficial change.

The science is not well-focused on practical outcomes, and this sense is reinforced by the lack of explicit hypotheses in the proposal. All of the items under the heading of hypotheses are questions. What is described is predominantly based on modern tools for gathering data, coupled with statistical analysis of associations between numerous variables and the state of important attributes of the land in question. It is not obvious that such analyses will help the users of the land increase productivity while decreasing rates of land degradation. It would be necessary to interpret actual farming and grazing practices in relation to productivity and the proximate drivers of soil fertility, water productivity and land degradation, which requires more agronomic and social science expertise as well as modelling. Thus it may be expected that the described research will lead to substantial advances in the science of land assessment (KS 1); it may not add as much to what is known about soil fertility and productivity (KS 2), for the approach seems quite naive in this area.

System Priorities: The outputs align with the CGIAR System Priorities, principally with 4A (and with 1B), though the claimed connections with 4C and 4D are tenuous as the water management, agronomic and social science parts of research needed to make a difference on the ground are not clear. The paper does not go the next step and relate the agenda to the CGIAR's goal of poverty alleviation. This requires more explicit goals of improving human welfare in drylands areas.

Uniqueness of the Challenge Program as opposed to the other CGIAR research: The two proposing CGIAR Centers are already undertaking considerable research on drylands, and the state of this knowledge could have usefully been included as starting point for the proposal. The only “added value” offered by the CP to the CGIAR agenda is the notion of a more integrated scientific approach that would reach out to expertise not already available at relevant CGIAR Centers. What exactly will the integrated science lead to in terms of technologies that can significantly raise land and labour productivity? Convincing examples of any past successes, and how more integrated research per se is likely to lead to real breakthroughs, is missing. An alternative view (e.g. from mathematical programming) is that even complex systems can be constrained by just one or two binding constraints (e.g. technology or water) and relaxing those critical constraints can shift the productivity of the entire system. Most social science investigations might identify the lack of cost effective incentives to dryland technologies as a major constraint. Perhaps the more compelling argument for the greater integration of social and

natural sciences in diagnostic analysis is the need to differentiate between natural and human causes of degradation.

Quality of science and qualifications of the research team

The project proposal offers an insight into the state of the art. There is some reference to the estimated degree of degradation to date and acknowledges that the information is scanty. In taking up this challenge, one would have expected some reference to similar undertakings (e.g. LADA and DeSurvey). The case for complex-system science is made, but one of the larger players in this field (the Santa Fe Institute) goes unmentioned. Reliance on the budding ESSP, the leading agency for the Climate Change CP, appears overly optimistic. The use of the syndrome analysis concept to characterize the various desertification phenomena at a regional level is questionable as to date only two global syndromes for desertification (the Aral Sea Syndrome and the Sahel Syndrome) have emerged. More likely such characterization will be possible only once the necessary datasets are collected at the finer scale, i.e. in the final phase of the project and thus will not be a diagnostic tool.

For the causal pattern analysis the proposal refers to the proximate and underlying driver concept for analyzing desertification processes (as per the LUCC project), but the proposal does not identify the successor project, the Global Land Project, as a potential partner in the CP. The proposal refers to the MEA and Malthusian paradigm as well as the counter-paradigm of inherent instability of the drylands and the adaptive responses of its population. There seems to be no recognition that in some cases there are few options other than to reduce the intensity of use of drylands.

The proposal uses the dryland development paradigm's (DDP) 5 general principles that should define the desertification research agenda. Amongst the slow drivers that cause gradual change however the proposal should differentiate between human induced land degradation (LD) and climate induced LD which call for different mitigating strategies, even though some of the adaptation strategies may be similar.

Though interesting, the "land health" analogy is not compelling: the health sector deals with a single species whereas land is rather ill defined in comparison. The Land Health Approach describes a process for research rather than its content and it is not clear how the DDP and Land Health Approach will be married in the actual research proposed in the Kstreams. Further, the deliverables from the land health approach are unlikely to impact research outputs within the life of the proposal if "*Sentinel sites will be sampled at 5 to 10 year intervals as it is difficult to detect changes in land health at shorter intervals*".

The research approach: Fundamental scientific aspects, and applied and adaptive stages of the research:

The Kstreams do not fully measure up to the promise in the introduction and to a certain extent reflect the interests of the proponents. Thus, the land health assessment is an exercise comparable to LADA or DeSurvey, two programs that already attract considerable public resources with a similar aim. The statement that "*Human and socio-economic dynamics ...interact with environmental vulnerability in typical patterns*" seems meaningless and thus the notion that syndromes at a finer scale can be defined is rather speculative. Moreover, land health surveillance is a truly challenging undertaking and it is questionable whether this is a CGIAR responsibility. The sentinel sites are a good idea, if more thought is placed on selection, but should be a long-term commitment well beyond the CP and will be costly. There is no real indication of scale in the research, similarities or differences in sentinel sites etc. and thus it is not clear what the

development and poverty impacts will be. Most of the large scale work only involves assessing degradation, not finding actionable solutions.

The hierarchical nature of this problem is not reflected in the listed questions, so that the questions for Kstream 1 might require further reflection e.g. starting with what defines land degradation (if it is to be mapped, by what criteria?); how does one separate human induced land degradation from natural or global change induced degradation? the role of or the impact on biodiversity of the land? etc..

Kstream 2 addresses the interventions to deal with the problem at the landscape scale. The proposal takes a limited view of what constitutes ecosystem services and rapidly converges to farms and fields. The biological component (except soil biodiversity) is lacking. Similarly, the advanced tools that are listed to tackle this Kstream are soil focused. In the water services area Oasis is likely to overlap with CPWF, but little mention is made of this. The water use efficiency emphasis at the farm level is rather conventional. A more interesting concept is that of rainfall use efficiency (RUE), which is controversial and requires some novel thinking worthy of a CP. Carbon and nutrient rangeland services and climate services in the dry areas are interesting but lack the greater vision shown in the first part of the proposal. One would have expected the issue to be approached from the point of view that land use has to be tailored to land attributes in such a way that ecosystem services (including food production) are optimized on a landscape level (considering all stakeholders). This will require a bio-economic modeling approach that will require substantial effort. Some of the elements of such an endeavor are alluded to in Kstream 2, but the approach is unbalanced and too soil-centric.

Kstream 3 places the land degradation research effort in a policy/institutional context. Little is said about how this will be done, although a constraint analysis for improvement will be undertaken. Most of these are likely to be in the policy and institutional sphere, but some include technological R & D. This is depicted in a hierarchical approach diagram (that could serve the entire program if it included the bio-physical components). The diagram is optimistic in that constraints are never insurmountable. This unfortunately is not true and abandoning land sometimes is the only option. Trade-off analysis is included here (but it should have been part of Kstream 2 as well). Common sampling frames and the integration of the bio-economic modeling done in this group with the hands-on activities of the other Kstreams will be required.

There is no technology development component in any of the three KStreams other than improved land management practices. Unless one assumes farmers and communities are currently operating in irrational ways in managing their resources, how can one be sure that there are going to be big enough breakthroughs to shift dryland production frontiers on the scales needed? Given there are important and ongoing demographic changes in many drylands – changing population densities, ageing, feminization, etc. – this ought to be an important topic for research in Oasis as it will certainly affect the likely uptake of improved SLM methods.

The proposal provided does not obviously draw on past drylands experience of ICRISAT and ICARDA or the DMP, or on past development projects by governments, donors and NGOs. There are a number of good existing socio-economic studies that successfully integrate measures of resource condition and change with socio-economic drivers although previously this has been easier to do at farm, community or small region levels where holistic and matching data sets can be collected. This literature identifies many of the key development issues and constraints in dryland areas: including weak infrastructure, poor market access, drought risk, weak institutions

for managing collective action and common properties, often state intervention /ownership of forest and grazing lands, and finding technology options that are sufficiently profitable to attract the interest of farmers and pastoralists. These kinds of constraints may offer a better explanation of why many past efforts to develop drylands have failed than the lack of more integrated science. There would seem to be more scope for building on past research undertaken at various scales and reflecting such understanding in the priority setting process, in the research design, and in the choice of study sites. Progress in complex adaptive systems relies to an extent on trial and error, on doing experiments, on launching pilot programs, on being opportunistically flexible. The account of KS3 exudes an understanding of such systems, but reviewers remain doubtful that the program will advance fundamental knowledge of them as claimed.

For diagnostic work, the cause and effect relations implied by the systems framework need to be formalized into testable models. This would require the capacity to implement action plans at grass roots levels that can subsequently be monitored and evaluated. Either Oasis must have funds set aside for this purpose, or it must partner with development organizations and NGOs that fund and implement the required investments in agreed ways.

While the main parties in the program are well-qualified to pursue parts of the research agenda, the Proposal offers no evidence that highly skilled expertise outside Europe, USA, and Africa has been consulted or that the “rollout process” will involve partners from other continents.

Strategy for utilizing and applying results

Delivery mechanisms are clearly defined for KS3 and should be successful if the proponents succeed in working closely with policy makers and other development partners, because whatever is to be delivered will have been jointly developed and be attractive to the users. The accounts of KS1 and KS2 contain no explicit plans for delivery, but list users (on pp. 23 and 28 respectively). KS1 mainly aims to influence national planning and policy departments. There is little evidence in the account of K2 that interactions between land users and the professionals involved will match that in K3. How the outputs of the proposal will influence land users remains hard to discern.

The integration of data (“*a novel land health cyberinfrastructure for Oasis*”) will need to be highly selective as this can quickly overwhelm the CP. The proposal is cursory in the way data are to be linked and what type of data is useful in this regard. It is, for instance, unclear why soil infrared spectral data, that are as yet difficult to interpret, are singled out. The idea of biometric centers of excellence sounds appealing, but would make sense only after an extensive period of data collection and some established protocols on what needs to be analyzed and how.

In relation to impacts, the “profitable bio-reclamation strategies for dry-land areas ...” (P6, point 8) is the only benefit listed that pertains to actual improvements in human welfare, and it is limited to specific areas in West Africa. Much more significant goals for reducing poverty and environmental degradation in drylands around the world would be expected by prospective donors to such a program.

Collaborative arrangements and beneficiaries involvement in research

The expected partnerships between Oasis, CGIAR Centers, NARS and ARIs are sketched generically but not expounded beyond the proposing entities. The proposal has been developed by a writing group and the proposal reviewed by representatives of sub-regional networks. The writing group is still not representative of the wide spread of organizations that have

considerable experience and complementary expertise to the CGIAR e.g. ARIs of Australia, Canada, USA, and Brazil which address dryland issues, and can add practical experience to the academic. It is difficult to determine if the range of stakeholders are as closely involved with the development of this proposal as with other Oasis activities.

Allusion is made to possible links with the putative Climate change CP. Throughout the proposal climate variability and change are cited as key threats to sustainable production in drylands. However no reference is made to the impact of climate related risks (droughts) at different scales of resolution, and the proposal does not include activities oriented to assess and manage these climate-related risks. KStream 2 includes activities to *“predict how land degradation affects seasonal and decadal climate change and vice versa for different regions and different typologies of land degradation”*. Although this is an interesting scientific question, the linkages and actual risks of climate variability and change with sustainable agricultural production are much more important for the CGIAR. Given the likely development of a climate change CP and the consideration that most green house gas emissions from agriculture are associated with deforestation, nitrogen fertilizers, ruminants and paddy production, investing efforts in measuring greenhouse gas emissions from drylands seems an issue of secondary importance for Oasis.

The proposal states that the initial modus operandi will be to commission research. It lays out a participatory process for diagnosis, implementation and evaluation under K Stream 3. The potential for synergies and benefits from the partnerships is strong, but, without careful management, transaction costs could be stifling given that there are so many partners operating over such a wide range of disciplines and scales. Indeed, the two-dimensional System Analysis Framework, could reasonably have a third dimension added to it to encompass the roles of the R&D community, for they themselves are part of the complex adaptive system that this Framework sets out to portray.

Governance and management

The proposal is generally clear on the governance arrangements that would be put in place were it to be successful. The Proposal describes how Oasis will be formulated as a joint venture between ICARDA and ICRISAT (as the legal entities) and the program partners. The Joint Venture Agreement will specify roles and responsibilities and be agreed by the respective Center Boards and Board Chairs. A Program Steering Committee and advisory bodies are proposed, together with their expected expertise and periods of rotation. The PSC will operate autonomously on science program matters but the PSC must adhere to the policies and procedures outlined in the Joint Venture Agreement. The Oasis Challenge Program Director would be an ex officio member of the PSC. The Chair of the convening Centers would appoint the Chair of the PSC. Day to day management of the CP will be through a Program Management Team composed of the CP Director and 4 K Stream Leaders. The proposal suggests that institutional issues will be handled according to the rules and regulations of the convening Centers. IPR policy and procedures are mentioned together with human resource and financial policies as being specified in the JVA to be drawn up were the proposal to be successful and presumably closely aligned with the policies of one or both convening Centers. More detailed aspects of administrative support are not clear from the proposal. A Gantt chart of activities and milestones for the first five years of the putative project is provided. Year one focuses on program organisation and methods, site selection would begin in year two of the proposed plan.

Performance evaluation and impact assessment

The proposal alludes to the use of bench mark sites, baselines and indicators of performance but

these have yet to be established. The Proposal anticipates that the program would commission external reviews after 4 and 9 years. Peer review of prospective partner proposals is also anticipated. Regional Coordinators are described as having roles in management, monitoring and evaluation and cross site assessments. Thought might be given to some representation of end users on the PSC. Relegating end users to an advisory committee does not ensure that their voice will be taken seriously if conflicting objectives arise. Impact indicators have not specifically been identified, and the section on impact refers to the general intentions of the CCD in relation to knowledge and context specific translation of results by land users for applicability. "Learning Alliances" are advanced as an important element of extending new knowledge and capacity building.

Budget and finance

An indicative budget of approximately USD 11-14 million per year has been proposed (with a five year cost of USD 66 million). Research operations and equipment make up approximately half the total. The scope and scale of the research expected is not defined. It would be important to distinguish more accurately the differences between funds supporting current Oasis projects and personnel and the completely new Program and its cost centers to avoid confusion. A business plan is not provided but the proposal indicates general donor interest in support of dryland research, some of which is already captured by the existing Oasis arrangement, including grants to ICARDA and ICRISAT. There is substantial donor support currently for Oasis and land degradation/rehabilitation research in dry areas. The Proposal includes several letters of support from regional organisations and the Secretariat of the Convention to Combat Desertification which welcomes a scientific knowledge approach which would underpin the workings of the Convention.