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ISPC Commentary on CRP 1.1 Proposal

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*Document presented for Agenda Item 8:
CRP 1.1 - Integrated Agricultural Systems for the
Poor and Vulnerable in Dry Areas*

Submitted by:

ISPC

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ISPC Commentary on Proposal CRP 1.1: Integrated Agricultural Production Systems for the Poor and Vulnerable in Dry Areas (28 February 2011)

Summary

The Drylands proposal (CRP 1.1) is an example of the CGIAR transition at work. It is evident that a number of CGIAR Centers have come together with other partners to identify how best to deliver developmental impact from CGIAR efforts in major dry areas, particularly addressing the needs of poor and vulnerable people. The nature and magnitude of the agricultural development needs and underpinning constraints are addressed in the proposal. Many of the past barriers to uptake by the poor have been explored and the proposal identifies what some of the missing elements should be. The proposal spells out a set of research needs and objectives that are directly linked to the Strategy and Results Framework (SRF) of the CGIAR and the system level outcomes in the SRF. The proponents plan to build on current research, but also considered linkages with other CRPs. These strong points, however, have not led to identification of integrative biophysical research that will take place as part of this CRP. While gender issues are taken into consideration and capacity strengthening forms a core component of the implementation plan, a realistic assessment or theory of how social change will be brought about remains to be developed. The program intends to involve different kinds of partners in framing the research applicable to the specific constraints and problems in the dryland agro-ecological and social context. However, until steps towards such a participatory research framework are taken, the proposal remains at a strategic level. It is thus inevitable that detail on measurable outcomes is imprecise at this time.

The proposed research focuses on risk management strategies intended to enhance productivity through the diversification, and sustainable intensification of production systems. Combining the strengths of multiple Centers in pursuing this research program in a coordinated fashion is a clear improvement over individual Centers operating in isolation. It is also clear that the development-driven research proposed is complex, and is itself a risky agenda in the sense that research, policy and development expectations (and therefore approaches) do not automatically fit neatly together. Because it is crucial that the program is well targeted, the dry areas should be well characterized, which they are not in the proposal. This is important not only for targeting the activities of CRP1.1 but also for informing priorities in the commodity focused CRPs to ensure that use of their research outputs can have impact on the poor in dryland areas. The Program's ability to establish priorities and monitor progress also depend on using and updating data on biophysical, agricultural, demographic and social variables that characterize the target domains and serve as benchmarks to quantify change.

The ISPC finds that the proposal describes more of a CGIAR research platform for dry areas than a research plan. Many of the activities and modes of working are indicative and several aspects of the proposal can only be worked out as the CRP unfolds and engages with partners. In consequence, the proponents can promise a strategic results theme to measure impacts and cross-regional syntheses, but it cannot show in a convincing way what the value-added impacts will be from the collaborative and integrated approaches espoused. A full endorsement of the approach and budget for 2012 and beyond should await development of a more concrete proposal of activities after a further year of planning.

Recommendation: The ISPC understands that the coordinated planning of research for development activities at the agro-ecosystem level, especially those incorporating the potential contributions of a number of CGIAR Centers, is both at the heart of the anticipated CGIAR change (as highlighted by the Consortium Board) and potentially more challenging than the development of commodity- or sector-specific programs in which the CGIAR has prior experience. While the efforts of the proponents to develop new ways of doing business are commended, this CRP is not ready for approval. However, the ISPC encourages the Fund Council to provide sufficient first year funding for

further program development and re-submission of a revised proposal. The following elements should receive special attention in that revision and should be presented in greater depth:

- Better characterization of target dryland systems. The proposal must define dryland areas of the developing world and identify geospatial distribution using a water balance approach that quantifies risk and severity of water shortage as the basis for categorizing regions that fall into the “reduce vulnerability” focus of SRT1, or the “sustainable intensification” focus of SRT2. Geospatial demographics on poverty and malnutrition, and on agricultural systems (crops, farming systems, livestock) can then be used as a basis for prioritization of research within and among SRTs;
- Establishment of a clear set of hypotheses as an organizing principle to help prioritize the research and results agenda. This may extend to provide sub-hypotheses for many of the research components listed under the Strategic Research Themes.
- The criteria for choice of benchmark sites and the development of relevant data to inform research requirements in both the biophysical and social sciences, and their synthesis
- A process of engagement with program partners to refine site selection and characterization and prioritize activities to be carried out, the outcomes to be expected and the assessment mechanisms for adjustment and outcome/impact assessment, working backwards from impacts to activities as suggested by the proponents
- More detail on the underpinning science and agronomic, genetic, and farming system approaches to be evaluated once the first phase has progressed
- A more comprehensive theory of how social change will result from the livelihood, gender and innovations systems approaches espoused in the current proposal.

1. Strategic coherence and clarity of Program objectives

Justification for devoting significant attention to dry areas is compelling and the nature and magnitude of the agricultural development needs and challenges are made throughout the proposal. The way in which the Strategic Research Themes are formulated suggests new ways of addressing the key issues of uptake by the poor in dry areas which aligns with the developmental needs proposed for reform of the CGIAR. The CRP1.1 proposal incorporates a potentially wide research agenda that goes way beyond simply the production of new technologies for dissemination to an unspecified group of agricultural producers.

However, the proposal is not clear about the description and characterization of its target domain. This prevents appropriate priority setting for subsequent implementation of the program. The characterization of dry areas is lacking. Justification for inclusion of irrigated areas or areas where water control and flooding are the problems is not provided, and it is critical to quantify the water balance limits that distinguishes dryland production systems that fall within the remit of CRP1.1 vis-à-vis subhumid agro-ecological zones. This will be particularly helpful in overlap with other CRPs. Inclusion of portions of the Gangetic Plain appears to be outside the normal definition of drylands and inflates the estimates of poor people potentially reached by this CRP.

Effective linkages with other CRPs would also be helped by greater clarity in exactly what is meant by the term ‘systems’. Definitions for agro-ecosystems (which include landscape elements that production system descriptions do not include) are available in FAO/WB study from 2005. This would increase the consistency across CRPs, for instance CRP 3.2 on Maize, which uses the FAO systems and CRP 6 which uses Sentinel sites. In addition, the characterization of sectors should include the non-farm sector, discussion on migration and market access that add considerable dynamism. Eventually selection of the systems across the regions should ensure inclusion of important common elements, notably related to water scarcity and risk to allow better knowledge sharing across sites.

The SRTs provide a reasonable and appropriate framework for pursuing the Program objectives. Evidence from past studies, criteria appropriate to the SRF System level outcomes and an analysis of the spectrum of situations across drylands have been used to prioritise two target systems: one on the reduction of vulnerability and risk (SRT 2) and the other on sustainable intensification (SRT 3). These are appropriate. However, there is a lack of apparent cross-cutting linkages between SRTs. The characterization and foresight work of SRT4 should be central to the CRP and designed to influence the other SRTs. The proposal presents potential options for a research agenda, but at this level the proposal is necessarily imprecise about the component research under the SRTs.

Identifying target regions is to be encouraged but the five regions appear to have been chosen largely on the basis of using sites of current research within the target regions (“Benchmark Areas”). While there is need for all CRPs to incorporate transition mechanisms from existing programs, a more rigorous attempt than currently shown should be made to apply the targeting criteria posited for CRP1.1. It would be unfortunate if accommodating existing programs of stakeholders overruled the consideration of other criteria. Of potentially great importance is the ability to use these sites as a starting point for impact assessment (including environmental impact assessment). Benchmark Areas should be chosen with knowledge of pre-existing modifications to the physical and socio-economic environments, representativeness and the possibility of research findings to other areas. Otherwise the potential for generating IPGs will be limited.

2. Delivery focus and plausibility of impact

The impact pathway is detailed for two distinct target groups: (i) those with the highest levels of absolute poverty and vulnerability, and (ii) those with greater potential for impact through market-led intensification and diversification. The basis for this distinction is that these two systems will require somewhat different approaches, and different mixes of technologies and social/institutional processes and policies. Through these two target groups, consistency with other CRPs is claimed and linkages to them are presented (Table 5, Annexes 1 and 2).

Although the impact-pathways are well presented, the description of expected outcomes is weak. Along with putting together a detailed plan for research activities and expected outputs, the program needs to elaborate much better how combinations of outputs will lead to the expected outcomes and subsequently impact at scale. Currently, insufficient description is offered of the activities of the CRP to judge whether the future research outputs will materialize, and at what scale impacts might be expected. The activities are described as potential options only. As such the lists of activities and outputs include duplication (e.g. between 2.2 and 2.3, and between 3.2 and 3.3) where the difference seems to be only in scale. It is difficult to see how up-scaling can be done without analyzing trade-offs simultaneously. It will be essential to tackle head-on the potential conflicts between environmental and socio-economic goals and objectives that may be associated with specific research outcomes. Scaling up will need explicit consideration in the design of the site-specific work.

The basis on which future impact assessments can be conducted is lacking. As stated on P88, the considerable stakeholder consultations and the substantial experience among partners should allow identification of some indicators at the initial workshops and these should be recorded in the revised proposal against which progress can be measured. These of course may be modified as the program is implemented, based on experience obtained.

Development progress in resource-challenged dry areas is envisaged to follow an impact pathway with a progressive spiral of improvements that result from integrated approaches and cross-learning with multiple feed-back loops. However, given the nature of poverty and disadvantage more broadly, there is need for a greater focus on *continuous* assessment of change (both positive and negative). Learning should also reflect a more realistic assessment of past development experience and, at the same time, retain some flexibility in research approaches and focus, and perhaps have more modest expectations

of impact on food security, poverty reduction and environmental sustainability. For each specific research location, questions about the nature of social reality and including changes over time will need to be addressed. The absence of a clear theory of social change underpinning the two separate strategies (encompassing two distinctly defined target groups) tends to reduce the credibility of estimated benefits for specific categories of rural men and women in each dryland environment considered. There is also no risk analysis of potential failure for these groups, e.g. the capture of benefits by elite groups etc.

The proposal provides five examples from past CGIAR work (Boxes 1-5) of the sort of sector-specific outputs deriving from integrated approaches that the future CRP would hope to emulate. However the examples are very small in scale and not uniformly very illustrative of earlier successes. The program should include in its analyses of lessons some cases that have been implemented on relatively large scale, such as some of the watersheds in India, the project on greening the Sahel, and the Fadama program in Nigeria, often with little R4D input.

The timeframe within which change might be clearly identified is likely to be long and unpredictable, but the six + six years timeframe seems appropriate for producing significant outputs. Although the proposal itself talks about quick wins, six years alone to include out-scaling will be unrealistic, particularly regarding target systems with endemic poverty and vulnerability. It is quite unlikely that major systems changes at scale could simply be fine-tuned. Thus a very realistic timeframe should be developed and argued for.

The explicit attention paid to monitoring environmental impacts is commendable, particularly given that environmental degradation is such a crucial element of the dilemma(s) involved in pursuing AR4D in dryland areas. The central role of Benchmark Areas as venues for pursuing the research has significant promise for facilitating quasi-experimental approaches to measuring environmental changes (both positive and negative). The specific examples of impact assessment from alley cropping and index-based livestock insurance provide mixed evidence that substantial impacts will emerge in the longer term. There has been very little farmer uptake of alley cropping and the proposal should give evidence to support the hypothesis that it will achieve greater impact in this context. Simply getting an appropriate tree cover on farms might be a better way to express this objective.

The emphasis on participatory research throughout the proposal is commendable although there are challenges in applying these approaches with disadvantaged groups. The SRF describes technologies as 'blunt instruments' for reducing rural poverty that depend on changes in access to markets, credit, insurance etc. to be effective. Yet at the core of this research is technology uptake which in itself is risky with levels of risk related to specific technologies rarely being specified except in the context of controlled research situations. In line with this risk the proposal includes a program to enhance innovation capacity and the creation of an enabling environment for these target groups. There is no clear reference to the structural underpinnings of disadvantage that is regarded as central to the disadvantaged position of women. While addressing social structural issues would add another level of complexity to the research and certainly to expected outcomes in the long term, some reference to this understanding of how disadvantage is maintained or challenged would be valuable. A full proposal and description of activities would have to demonstrate therefore the means and relevant social science expertise required to produce coherent approaches.

3. Quality of science

The general hypotheses relating to the needs of the dry areas are convincing. The most important constraints are identified, particularly regarding areas/systems with the deepest endemic poverty and most vulnerable populations. It is recognized that development strategies in vulnerable systems will have entry points related primarily to livelihood strategies rather than productivity *per se*. The science-based approaches and linkages proposed through local institutions in benchmark sites to

development programs addressing issues of social, financial and other capital, institutional support programs and capacity strengthening are appropriate.

The actual science which will be undertaken is not well described, although attempts were made in the text to show the room for innovative research to be designed. It is thus very difficult at this stage to assess the quality of the science which will be undertaken. In the absence of a predetermined detailed agenda for research, presentation of a range of clear testable hypotheses for each SRT would allow better assessment of the potential quality of the science. For instance the proposal raises several significant, testable hypotheses that could underpin much of CRP1.1, and these should be given emphasis in an overarching framework, with sub-hypotheses linked to each Output. One broad hypothesis is that (pg 22) “*some dryland production systems have been identified as having the greatest potential to increase food production. Sub-Saharan and North Africa, West, Central and South Asia and the dry Andes in particular have large productivity gaps, where relatively quick wins would be possible (Cooper et al. 2009).*” A follow-up hypothesis (also from pg 22) is that “*These areas have not yet benefited substantially from research innovations. One of the major reasons why research has not delivered more to drylands is that research has mostly been reductionist: conducted on isolated single components of an agro-ecosystem, while farmers, communities and policy makers operate in complex systems, with high levels of integration of many components.*” Note that to test the hypothesis of Cooper et al (2009) requires the kind of biophysical database that is needed to delineate the two major dryland categories and to prioritize research opportunities.

Whilst the dry areas characterizations are largely technical, this ambitious research program would require a range of research approaches. Several on social research are described in the proposal (participatory research concepts expanded to whole value chains; community-led integrated adaptive research; social learning). For each specific research location, questions about the nature of social reality and including changes over time, will need to be addressed. The absence of a clear theory of social change underpinning the two separate strategies for two distinctly defined target groups tends to reduce the credibility of estimated benefits for specific categories of rural men and women in each dryland environment considered.

Whilst many of the approaches seem sound, some of the descriptions of how systems/agro-ecosystem approaches will be adopted are rather superficial. The descriptions on sustainable intensification options (3.2) are also disappointing at this stage: a lot of options are already known or are being developed in the CRPs that have more of a commodity orientation. Due regard needs to be paid to information already collected and synthesised. It would be relevant for CRP1.1 to enhance understanding of the context in which the different options might work and how the rapidly changing environment (biophysical and economic) influences the risk of success in the future. That said, the recognition of the need for out-scaling to deliver IPGs is welcome and output 3.3 on trade-offs does offer a useful analysis.

There is the perennial question of what levels of impacts may be derived from research in pastoral and steppe lands *per se* rather than the diversification of farming systems in rainfed areas (although these are argued simultaneously in the discussion on target system 2, p. 23). The interaction between systems is likely to be a fertile research area, including the transition and tensions between transhumance and sedentary systems, and particularly conservation agriculture and fodder supplies for livestock.

SRT 4 with its emphasis on measuring impact is welcome; including Output 4.3 which indicates how lessons learnt on impact will feed back in to the design of the other SRTs. Given the evolutionary nature of this CRP and need for continual learning of lessons, it might be useful to consider how a peer-review process of the proposals at the project level might be conducted by the Steering Committee.

The intention to select benchmark sites for the four to five focus regions, some with contrasting satellite sites is welcomed. The issue will be to do this at the appropriate scales and in an efficient and cost effective manner (and Tables 3 and 4 are a good beginning). There are indications that CRP1.1 will make use of existing sites where CGIAR and relevant stakeholders are operating and future benchmark or sentinel sites, as are also being anticipated by CRPs 6 and 7. It will be necessary for the CGIAR to optimise the choice of such sites across the CRP portfolio. However, the site selection for this CRP needs to confirm the targeting of truly dryland areas.

Attention given to the strengthening of innovation systems and linkages to policy actions is very welcome. However, this approach requires particular competencies and partners to be included with sound track records in these kinds of research approaches, particularly integrating policy and social science issues with the agro-ecological issues. Many of the participating Center-based research teams are staffed by individuals who have made significant contributions to the development of research in dry areas. As such, the capabilities of the research teams have been well-demonstrated but this proposal calls for another level of social sciences research and development. Promoting livelihood strategies that feature (partial) exit from agriculture might be the best course of action for some particularly difficult agro-environments. However, the participating Centers' comparative advantage may not be in identifying (or even promoting) non-agricultural alternatives. For example, Payments for Environmental Services schemes are identified as being one potential alternative, but there is really no elaboration on how such schemes would amount to anything more than income support transfer payments to residents fortunate enough to live in areas associated with such schemes.

4. Quality of research and development partners and partnership management

The proposal's partnership strategy and management reflect the inherent value as well as the complexity of the proposal's underlying integrated systems approach. The number and missions of the participating CGIAR Centers model this approach, and the proposal itself details the effort to identify the ways in which it will relate to and integrate other CRPs.

In the broader discussion of proposed and potential partners, partners and stakeholders are considered together, rather than defined or circumscribed by their financial contribution to the CRP or their relative standing within a hierarchy of organizations. While some partners are critical for their ability to invest in the research or leverage its results, many more are critical because of their proximity to the ground and their ability to provide near term input or demonstrate long term impact. While the obvious partners are cited, there is an assumption that the most promising configuration of partners will be revealed as the work itself moves forward. This notion, of building the plane while flying it, is undoubtedly true of all programs intent on achieving results, but it was admirable to include such a frank assertion of uncertainty in the proposal.

The proposed program management includes a number of formal mechanisms, including the Steering Committee and Regional Stakeholder Advisory committees, which will bring partners and stakeholders together on a systematic basis. In addition, the budget includes significant allocations for partner engagement, particularly on the part of the two largest participating Centers (ICARDA and ICRISAT). Nevertheless, given the importance of communications and knowledge sharing to the success of the partnership strategy, the lack of a more detailed description of these activities and how they will align to further the goals of the program is problematic. While the overall design may emerge from the proposed inception workshops, some elements of the strategy and the need for related investments are surely evident even at the start of the program. The proposal makes the case for investing heavily in face-to-face communications, but simply itemizes all the other necessary approaches that will be needed. The lack of a separate budget for program management, including communications, is a further handicap to assessing whether partnership management will be as effective as it needs to be at the CRP level.

Particularly with respect to the out-scaling segments of the impact pathways, some consideration should be given to changing the balance of identified partners by incorporation of more clearly strategic partners, e.g. a multi-national development bank, an international private sector seed company, a global Foundation etc. The absence of IFPRI is notable, particularly given the strong emphasis on social research and the importance of policy to outcomes.

A large number of institutions with pre-eminence in addressing the research themes have been involved in developing this proposal. However, an explicit analysis of alternative suppliers of research is missing – developed country research and development institutions, international NGOs, etc. All the CGIAR Centers with activity in dry areas are included without a discussion of their comparative advantage in this particular program. The leadership arrangements need to reflect the specific competencies required from research management and oversight bodies to steer a successful program of a kind where the CGIAR does not have much prior experience or demonstrated success.

5. Appropriateness and efficiency of Program management

CRP 1.1 adopts a multi-level structure for program management that includes mechanisms for consultation, general program administration and oversight, and independent research review. Program management is organized principally along regional lines, where interdisciplinary teams implement activities and deliver results within each of four strategic research themes. ICARDA serves as the lead Center for CRP 1.1, with coordination of each target region assigned to an appropriate participating Center.

The single largest challenge facing effective management of the program is to maintain the structure's many moving parts. There is a welcome effort to quantify the time required to perform a role in this structure effectively and to extend accountability for performance beyond the CRP Leader. The four Independent Scientific Advisors (ISAs) accept an appointment that involves 30-45 days a year. A regional coordinator is considered a part-time appointment with performance evaluated annually by the Steering Committee. The CRP Leader is expected to remain active in research for 10 percent of his/her time.

The first three years of the CRP are dedicated to uncovering and defining the most effective research activities, benchmarks and partnership strategies to achieve the goals of the program. Given this substantial period of potential flux, the Steering Committee, ISAs and Research Management Committee provide a good framework for evaluation, priority setting and resource allocation during that initial three-year period.

The Steering Committee composition is described in general terms but does not indicate the criteria for inclusion of partners and stakeholders, other than the participating CGIAR Centers, and there is no suggestion that terms of some kind might be used to facilitate turnover in the either Steering Committee or the ISA. A governance and management review is proposed as well as a comprehensive external evaluation of the CRP [p.89], but it is not clear whether the Steering Committee or the ISAs will be responsible for commissioning such evaluations.

No budget is provided for the direct costs of program management or for the achievement of CRP-level convening and communication functions. The proposal states that CRP 1.1 “has a simple and cost effective management mechanism that will rely almost entirely on the capabilities of participating centers [p.69].” This strategy appears with some variation in many of the CRP proposals to the Fund Council. However, management is not a negligible element of a proposal's success.

While all management functions are assumed to be cost sensitive, the proposal acknowledges that the effective use of Regional Stakeholder Advisory Committees might be hampered because of the expense of convening five different regional advisory bodies on a regular basis. The proposal envisions using technology and other means to bridge this challenge, and relying on the chairs of the

RSACs to provide the Steering Committee periodically with direct feedback and advice. In a program that must be exquisitely sensitive to how partners are engaged and the extent to which their participation is honoured, it will be important not to over-promise or under-deliver on the most visible, formal platform the proposal identifies for partner input and advice.

CRP Leader: CRP 1.1 provides an explicit and well rounded executive function for the general management and administration of the program. The CRP Leader has a role that parallels that of a Center DG. Although there is the usual accommodation for the legal and fiduciary responsibilities of the lead Center, the recruitment, appointment and performance evaluation of the CRP Leader rely on the Steering Committee with the lead center providing the formal mechanism for hiring the CRP Leader, and reporting to the Consortium Board.

In addition to the expected management and administrative responsibilities, the CRP Leader is also expected to serve as the public representative of the CRP and is given explicit responsibility for resource mobilization as well as partner and donor relations. The proposal also envisions that the position will be held by a “world-leading scientist” to give the CRP credibility and influence [p. 70]. Recruitment of a “world-class leading scientist who is also good at managing the complex partner and donor relations is a tall order. Experience elsewhere suggests that the skills required for effective coordination and management should be paramount, whilst ensuring that there is at least one world-leading scientist on the Steering Committee.

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

The balance of the budgets between Centers is interesting – the emphasis on ICARDA and ICRISAT is expected because of their mandates, but given the number of times that livestock and forestry are mentioned as an important part of dry area systems, the relatively modest budgets for ILRI and ICRAF and lack of budget for WorldFish (explained in part) are surprising. Presumably the relevant technologies will be picked up from the other CRPs in which they are active, but it is difficult at this stage to envisage how the team will ensure a balanced discussion at the systems level.

In general, the independence of the Steering Committee could be enhanced by describing its role more specifically to distinguish it from management as well as considering whether its leadership and composition should be adjusted to minimize the potential for conflicts of interest. Although the ISA is clearly intended to assure independence and accountability, the Steering Committee should also be seen as a mechanism for increasing the confidence of donors and partners in the CRP’s value and impact.

- Instead of establishing the lead Center’s DG as the standing chair of the Steering Committee, that position should include a term of service, and a mechanism for election from among the committee’s members.
- In order to maintain a functional size for the Steering Committee, the non-Center members should be given a reasonable, non-renewable term (3 years). This would increase the opportunities among stakeholders to participate and also minimize the *quid pro quo* quality the present proposal implies.

The ISA is small but also powerful. The proposal does not indicate the term of the appointment, although it does include an important expectation about performance in specifying the number of days an advisor would be expected to dedicate to the assignment. Establishing a term for the appointment provides for additional accountability, and also enables the scientific expertise of the panel to adapt to the needs of the CRP.