

**ALTERNATIVE APPROACHES TO  
NATURAL RESOURCES  
MANAGEMENT IN LATIN  
AMERICA AND THE CARIBBEAN**

**A Review of the MINGA  
Program Initiative of IDRC**

Review Team

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# MINGA review

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## 1. Key findings

The Minga Program Initiative addresses complex problems of poverty and resource degradation in the LAC context. Minga has successfully generated a coherent framework to deal with these problems, bringing together projects inherited from the past and an exciting portfolio of new projects. It has developed and should continue to exploit a niche of its own: LAC ecosystems, the benchmark approach, methods/tool development and assessment, conflict management and consortia networking. Institutional networking is probably the strongest aspect of Minga, as evidenced by the program's sustained effort to bring together different actors and perspectives in all program initiatives. Another important and unique feature and strength of Minga lies in ability to work at multiple levels, both geographically and institutionally. Minga deals with communities and regions within a nation and a continent. It also works with local peoples, NGOs, universities and advanced research institutions, not to mention local, regional and national governments. This approach gives Minga an invaluable opportunity to compare and draw lessons from the development and application of different tools, methodologies and institutional arrangements.

But there are also gaps and weaknesses, some of which we discuss in this document, together with suggestions as to how these problems can be addressed. On the whole, our view is that Minga can put greater effort into:

- researching the institutionalization of lessons learnt;
- clarifying the concept and implications of project and program cycle;
- developing strategies to compare and assess tools and methodologies and to deal with baselines both within each project and the program as a whole.

The views expressed by the two reviewers are largely based on their reading of IDRC's general policy documents; the MINGA prospectus and annual reports; IDRC's evaluation of MINGA; grant proposals and available publications from projects selected for this review; and discussions with the team leader and PI members as well.

## **2. Minga's objectives initial and revised**

Minga's prospectus objectives revolve around applied research activities that promote NRM tools (natural resource management) incorporating PMS approaches (participatory, multisectoral). Geographically, the program operates principally in well-circumscribed, environmentally fragile benchmark areas located in four rural-poor regions of LAC (Latin America and the Caribbean): the Andes, the Central American hillsides, the Amazonian basin, and coastal regions. Moreover, Minga subscribes to IDRC goals of

- multidisciplinary;
- gender sensitivity (analyzing gender *and* getting women involved);
- on-going M&E;
- capacity-building (in NRM and PMS, environmental impact analysis, regional planning, gender analysis, interest based negotiation, etc.);
- systematic information dissemination;
- resource expansion and networking at all relevant levels (involving other PIs, cofunding,
- Canada-LAC partnerships, key institutional and private sector involvement in projects, advisory assistance from universities and research institutions, etc.).

Apart from its geographic focus, a distinctive feature of Minga is that it supports a regional or ecoregional consortium approach to PMS-NRM, thereby promising closer

integration between different types of research and between research, implementation and policy uptake.

Lastly, Minga promotes comprehensive projects or project clusters that proceed through successive stages of the R&D cycle, i.e.:

- a) the synthesis of comparative findings;
- b) the development, application and dissemination of NRM tools (including baseline surveys, GIS and longitudinal surveys, remote sensing, GIS, Global Positioning System, etc.) and PMS methods (interdisciplinary curriculum development, consultations, roundtables, co-management, "mesas de concertación," PRA, collaborative watershed management, participatory planning at local and regional levels, etc.);
- c) the exploration of strategies for application (expanding scope and reach); and finally
- d) the institutional mainstreaming or scaling-up (local, regional and national; private and public) of lessons learned and validated through applied research.

These ambitious goals have been slightly revised since the prospectus, towards a more realistic appropriation of projects that can focus on one or a couple of stages, using *a regional or benchmark area to complete the PI's (PMS-NRM) research cycle or launch a new one wherever gaps are thought to exist.*

### 3. Program relevance

Both reviewers consider the Minga Program Initiative (PI) to be well conceived and well suited to its LAC context. The prospectus and clarifications brought to Minga's initial objectives provide a coherent and systematic framework to guide relevant research initiatives in LAC. They propose well-defined objectives, activities and outputs to be carried out by Minga projects, thus avoiding an ad hoc set of unrelated projects that may not be mutually complementary.

Minga addresses important problems for people, particularly the poor, in Latin America that depend on the use of natural resources for their livelihood: the relationship between poverty and resource degradation. It aims to provide information on tools and institutional settings to these people in order to improve their management of natural resources and to reduce poverty. There is no question that these are very relevant activities in this region, and in general the prospectus does a good job in making a case for the Minga approach to applied research. Its justification for targeting multiple stakeholders—a defining feature of the Minga program—is particularly well argued.

However, the PI prospectus neglects some issues that should be addressed to further enhance the credibility and relevance of its research activities. First of all, the general diagnosis presented in the prospectus places too much emphasis on exogenous factors

such as globalization, structural adjustment, and problems with democratization. These broader causes of poverty and resource degradation seem to preclude **local/internal factors**, such as local history and local power structures. Yet we know local power structures can be very authoritarian, and to many indigenous groups our concept of democracy can be quite alien. There is no question that global factors mentioned are playing a fundamental role. Upon further discussion with the team, it is clear that they feel that most of the projects are very strongly grounded in the local context. However, when one reads the PI prospectus this does not come across clearly.

Secondly, the evidence presented to justify the focus on hillsides, highlands, and coastal areas is a bit thin. Reading the prospectus, one wonders how the situation of resource degradation and poverty in the **targeted areas** compares with problems observed in lowlands and valley floors. The prospectus fails to offer hard data to show that the situation is worst in the former, or that other initiatives are working in the latter. Although intuitively we recognize that Minga priorities are well suited to their LAC context, more evidence should be presented regarding their validity. It may be important that intuitions be converted into hypotheses in need of factual verification. However, we think that ecoregional focus is a valid way of focusing of the program.

Thirdly, Minga should perhaps pay more attention to issues of **comparison**. Clarifications brought to Minga's two original goals (unpacked into two objectives each) provide a better operational definition of what the PI strives to achieve, with an emphasis on appropriate methods to be identified, generated, synthesized, applied, and institutionalized. PI documents should nonetheless say more about the need to actually assess and compare such tools, an evaluative exercise that is essential to achieving program objectives. Without an overall framework designed to compare tools and methodologies, how is the PI going to identify and generate "effective" strategies to apply lessons learned or demonstrate benefits of these approaches?

Fourthly, a closely-related element that may be missing is a strategy or procedures to deal with **baselines** which are fundamental to extract lessons, to compare and assess methods, and to document impacts (otherwise to be monitored and evaluated by resource users and stakeholders themselves). A clear strategy is all the more important if there is to be greater emphasis on project cycles, a shift recently announced by Minga. A program report issued in December of last year promises more staff involvement in the development of guidelines for gender analysis. Likewise Minga should develop guidelines to foster and facilitate the comparison of methods across projects and to develop baselines for Minga initiatives, generating through cross-PI collaboration a tool kit for research in this area. In our view failure to address explicitly issues of comparison and baseline research in objective 2 may jeopardize objective 3, which consists in applying and extending the lessons learned to new situations. How can we learn lessons without comparisons and then intend to apply them to new situations and institutionalize them? One possible research activity that may be useful in addressing this problem is to define more clearly for what kinds of decisions or contexts different tools can be used, paying particular attention to the relative advantages and disadvantages of tools and methods used under varying conditions.

The Minga team seems to be aware of these issues and recognizes the need to have better project design. One Minga report thus speaks of "...the need to ensure that researchers collect the necessary baseline and other data throughout the project in order to provide future evaluation efforts with sufficient background." It also states that "before the institutionalization stage, demonstration and validity of methods at local and macro levels needs to take place with recommendations regarding the contexts in which these methods are likely to have success." In our view the PI is generally moving in the right direction.

A central challenge of using baselines and benchmarks is that contexts change, issues are redefined, and in many cases, baselines may be rendered outdated. It is important to recognize this problem. However, in spite of this, baseline information should be collected if a credible case for the impact of projects wants to be made. In the case of Minga, we think that basic information on poverty and resource degradation would always have to be collected, although the specific indicators may evolve through time and with the accumulation of experience. Obviously, one cannot collect everything everywhere. That is the reason for the necessity of an up-front strategy to deal with comparisons, baselines, and benchmark sites.

The last comment we wish to make regarding the general objectives of Minga concerns its **external synergies**—its relationship with other PIs and its ability to involve stakeholders in program initiatives. Although management reports that Minga has developed good synergy relations with other PIs, links with other resource management PIs could perhaps be further clarified. As with other programs, Minga documents tend to incorporate many of the objectives and priorities of IDRC appearing under the Center's guiding principles: for instance, privileging local solutions, using participatory methods, and institutional networking. As a result, the differences that lie between several of the NRM program initiatives launched by the Center sometimes appear to be a matter of nuance or geography. In actual practice, however, Minga has successfully developed and should continue to exploit a niche of its own: LAC ecosystems, the benchmark approach, conflict management and consortia networking (under conditions specific to the LAC context). It is also investing considerable energy in introducing these principles into LAC projects inherited from the pre-PI period. Institutional networking is probably the strongest aspect of Minga, as evidenced by the program's sustained effort to bring together different actors and perspectives in all program initiatives.

#### **4. Program/project scope and feasibility**

Minga's current notion that individual projects should be viewed like pieces in a comprehensive program cycle or puzzle, as opposed to model projects that meet all "ideal" requirements that proceed through all phases while receiving Center support, is a step in the right direction, away from the "monumental" approach to R&D—away from attempts to generate overly ambitious contributions to applied research in the area of NRM. Minga appears nonetheless to be caught with expectations of a holistic approach that are at odds with two real-world factors: the limited resources prevailing at both

program and project levels, and the constraints and complexity of Minga's LAC playing field. As suggested below, Minga's (and IDRC's) preference for holistic, integrated and comprehensive methodologies should be pursued but in such ways as to avoid three things: i.e., aiming beyond realistic targets and enabling environments; conflating program and project cycle; and developing project selection criteria lacking in modesty and realism.

#### *4.1 Realistic targets and enabling environments*

While concerned with issues of focus and strategic planning, Minga program documents may inadvertently convey a general expectation that all PI objectives and IDRC guiding principles be ideally met by each and every field project at some time during their research cycle (without exceeding a three-to-five year limit though). As opposed to this, we suggest that Minga (and other PIs for that matter) actively encourage projects to specify the *strategic objectives they can realistically pursue, from within the broader range of priorities and guidelines supported by Minga and IDRC*. Researchers should be invited to reflect on prevailing conditions and constraints that reduce the short-term feasibility of meeting some objectives or guiding principles otherwise promoted by IDRC and Minga. Likewise, they should explain why some strategic objectives and methods are to be preferred over others, and the extent to which the parameters of an "enabling environment" are already in place (e.g., experience with interdisciplinarity, PRA or gender studies).

The point of this exercise is not for PI and project members to promise ways to overcome observed constraints such that an enabling environment be secured and a comprehensive and holistic R&D agenda be maintained (as tends to be the case in Minga documents). The exercise points rather to the necessity for each project to clearly identify what effective and meaningful applied research contribution it can make under existing conditions and for which it may be held accountable. When working under adverse conditions, PI and project statements should be discouraged from echoing *all or too many* of the strategic goals pursued by the "new IDRC"—official objectives and all other goals appearing in the guise of guiding principles, performance targets, planned activities, expected outputs, projected reach, anticipated incidence, phases to be covered, enabling conditions to be met, etc.

In other words, the question is whether all research parameters recommended by IDRC/MINGA are to be given equal priority in all circumstances. Can a workshop such as the Washington conference in CBNRM be undertaken as an effective means for generating/diffusing CBNRM tools and methods but with modest expectations regarding their uptake by the World Bank? Multiple objectives must be pursued as interrelated problems of poverty and environmental degradation rarely lend themselves to simple panaceas. But holistic solutions to complex problems should not be espoused with so much optimism and naively as to ignore the enormous constraints and obstacles faced by LAC researchers and institutions.

## 4.2 Project selection criteria

One central task of any PI is to generate, communicate and apply project selection criteria and related research guidelines. This task raises issues of scope, feasibility and appropriateness as they apply to research parameters established by the donor(s). As already noted, we suggest that these parameters could place more emphasis on considerations of strategic focusing and pragmatic flexibility.

On the whole Minga's PMS-NRM selection criteria are appropriate in that they build upon the strengths of expertise and research institutions available in LAC countries. Caution should be taken, however, not to exclude potential contributions from recipients other than strong institutions operating in benchmark areas. The comprehensiveness of Minga's prospectus plan need not be passed on to complex, incremental research projects involving multiinstitutional consortia, strong leadership, full-fledge multistakeholder participation, and long-term longitudinal analyses of elaborate benchmark data bases. Although we view benchmark areas as very useful tools for Minga, some goals that form an integral part of the Minga puzzle might be better served through highly-focused, lower-risk R&D activities requiring smaller grants and shorter-term commitments. Simply stated, Minga's primary aim should be to make a significant difference, under conditions that will permit change to actually occur. This flexible scaling of Minga activities might make it easier for the program to cover all four ecoregions announced in its prospectus, with some rationale as to where larger-scale projects and related RSAs are more likely to succeed and where smaller-scale initiatives (sometimes with less-experienced scientists) are worth pursuing.

Minga illustrates the increasing complexity of IDRC-supported projects that promise holistic multidisciplinary, long-lasting R&D capacity-building, high-quality publication output, institutional networking (within and beyond national boundaries), dynamic multistakeholder involvement, and effective policy uptake designed to alleviate problems of social inequity and environmental degradation alike. In our view, Minga should maintain these program objectives but not expect projects to pursue all of them with equal energy let alone equal success. Strategic decisions regarding project goal priority and feasibility must be made less project-planning lapse into buzzword rhetoric. Program guidelines should invite projects to *speak frankly* to goals already achieved; short-term objectives that cannot be realistically contemplated and those that can; the relative weight that should be given to publications over other outputs; the preferred ways in which research is to be utilized and "mainstreamed"; lessons learned from failures to achieve some objectives; shifts in planned activities; reasons as to why project goals should deviate from PI priorities (within reasonable limits); and so on. An overview of Minga's on-going activities and appropriations indicates that pragmatic and strategic considerations are already an integral part of what Minga actually does. Management thus praises Minga for having attained a good balance between program focus and responsiveness to regional needs. We suggest that this successful experience be fed back into a more realistic, flexible and dynamic conceptualization of project selection criteria.



### 4.3 Conflating program and project cycles

The last Annual Report submitted by Minga (June 1999) announces that "Benchmark Areas are places where we aim to complete cycles of research. Such cycles consist of problem setting, baseline definition, development and application of tools and approaches, demonstration of their efficacy, via indicators and evaluation, and institutionalization via use and uptake of the results. A research cycle might be completed by contributions from one or more projects. This approach forces us to ask, when considering a new benchmark area, whether there exists an enabling environment for the whole cycle." This seems to indicate a shift from comprehensive benchmark area projects to projects that show clearer focus and stronger project design. Projects located within benchmark areas can "specialize" in particular research cycles. Benchmark areas should nonetheless proceed through all phases of the research cycle while receiving Center support.

The benchmark concept is central to Minga planning and should be maintained. Still, Minga documents should perhaps be careful to avoid confusing program plans with project cycle. The confusion is not merely semantic or academic. It implies for instance that Minga will consider the institutionalization process as the final stage of its own program cycle, a stage that presupposes earlier Center involvement. Without this prior involvement (preferably from the start), PI opportunities to support projects that study/experiment with institutionalization practices based on previous contributions from other parties or actors may be entirely missed. The end result is to put off "institutional (policy-uptake) studies" to the end of the program cycle, with the assumption that program and project cycle must coincide.

MINGA requires that projects be shown to pay off through the institutionalization of research innovations. This should not mean, however, that program and project initiatives should devote resources to this effort not until they reach the final stage of each benchmark research cycle. Early-stage research on the institutionalization process, *irrespective of where previous inputs (such as problem definition or baseline gathering and analysis) come from, inputs that are likely to come from multiple partners in any case*, can generate worthwhile results and provide useful insights into what strategies other projects should contemplate when attaining sufficient maturity. If Minga already feels it does not need to be involved in the full cycle, then we suggest that the cycle concept be clarified along those lines.

## 5. Program gaps and project weaknesses

Minga internal documents speak of program gaps in the following areas:

- research phases or outputs other than research tools and methods (institutionalization and scaling-up objectives are the least covered);
- non-agricultural sectors (forestry, mining, water);

- the use and assessment of PMS methods other than "mesas de concertacion" (GIS, decision support systems, etc.);
- social-environmental indicators and complex ecosystem monitoring.

Regarding the first point, although we gather this may not be the intended view, Minga documents give the impression that tools and methods (objective 2) must first be secured if they are to serve as building blocks to achieve Minga's longer-term, incremental objectives such as scaling-up and policy-uptake. As already pointed out, this standardized-cycle approach to Minga R&D should be eschewed with greater clarity such as to allow all phases of the Minga cycle to be studied concurrently, through project appropriations that focus on institutional-policy activities already in progress (PSM-NRM projects that may have no prior IDRC involvement).

Gaps in particular sectors such as mining should be remedied through new appropriations, such as Negotiation and Decision for Mining Communities in Latin America. More is said about the need for more baseline information and ecosystem monitoring in our assessment of projects selected for this review (see below).

Minga documents also mention five problems generally (though not equally) observed at the project level:

- some difficulties in introducing Minga concepts and strategies into projects inherited from the pre-PI period;
- conceptual frameworks lacking in interdisciplinarity and the use of systems frameworks and spatial perspectives (e.g., CODESU, Central American Hillside, Condesan);
- insufficient multiple stakeholder participation in networks and consortia;
- weak links between information gathering and analysis and its subsequent use by decision-makers (policy uptake issues);
- relative absence of gender perspective and related methodologies.

In our view appropriate measures designed to remedy these five weaknesses have already been identified. They include additional workshops and training in PMS-CBNRM; coevaluation activities; direct assistance from POs, the Gender Support Unit (in Ottawa and Montevideo), CIAT and Minga-supported sabbaticants; increased university and government involvement; research support from Canadian researchers in the field of regional analysis and planning; and second-phase readjustments to project planning. Other possible measures such as methodological guidelines or "tool kits" were discussed in section 4.

## 6. Project assessment

We turn to a discussion of six Minga initiatives and the contribution and lessons they bring to program objectives. Three projects have been selected to illustrate Minga efforts to facilitate syntheses of experience with collaborative approaches to NRM and related conflict management practices. They include the Washington Conference and book on Conflict and Collaboration (6.1); the Red de Manejo de Conflictos Socioambientales (6.2); and Bellanet's proposal for a Virtual Workspace for Small Grants Programs and Dialogues on NRM in LAC (6.3). Baselines allowing comparison between different projects or different phases of a project are critical to the program. We have therefore chosen to select two hillside projects (6.4), which should provide a good comparative perspective on issues of multistakeholder involvement and the identification, development and application of research tools or methods. Other projects proposing a collaborative strategy and well-suited to our discussion of baseline comparison issues include the Ecuadorian Resource management project (6.5) and the Competitive Grants Program for Research on Monitoring and Evaluation for Natural Resource Management Projects and Programs in LAC (6.6).

### 6.1 Washington conference and publication

The Washington conference and publication currently in print (*Conflict and Collaboration in Natural Resource Management*, D. Buckles ed.) have brought together an impressive array of CBNRM expertise from different parts of the world (250 participants from over 60 countries) and generated a useful tool: a well-designed book integrating IDRC-funded case-studies and concept papers commissioned by the Center. IDRC and its partners were responsible for about 25% of the case studies and 20% of the plenary presentations planned for the event. Though labor-intensive (100 persons days of IDRC staff time spanning over 8 months), these results have been achieved at a modest cost to the Center and with effective cross-PI cooperation (with CBNRM). All indications are that the Conference and the book have enhanced and will contribute to raising the profile of IDRC partners and promoting the international visibility and credibility of CBNRM practices and philosophy. Other objectives pursued—raising IDRC's profile among other donors and influencing WB thinking and WBI's training agenda—seem not to have been attained. Senior Bank staff were conspicuously absent at the event, and Minga documents mention a tendency on the part of WBI to give little recognition to cofunding organizers (IDRC and Ford Foundation) and to co-opt CBNRM philosophy into serving the cause of decentralization à la World Bank.

The Washington event raises questions of multi-institutional strategy. More specifically, what are the pros and cons of activities involving cofunding institutions whose agendas and "partnering politics" may be too far removed from IDRC philosophy? Should the Center envisage future projects involving long-term partnership with the World Bank in the hope of having some impact on the WBI's agenda, with payoffs to be assessed over time? Alternatively, could projected partnerships with CIDA and the SDC be more compatible with Minga objectives?

## 6.2 *The Red de Manejo de Conflictos Socioambientales*

The Red de Manejo de Conflictos Socioambientales assists Central American researchers in documenting and disseminating more systematic and sustainable means to manage natural resources and related conflicts. The network purports to facilitate and disseminate the analysis and synthesis of case studies of collaborative strategies adopted by Mesoamerican communities confronted with conflicts and crises in NRM. Activities and outputs consist of tools for analysis; case-studies; local, regional and national workshops; participation in key international fora (e.g., the Washington conference); network expansion and consolidation; capacity-building through cross-visits and training; a bulletin (first issue in January-February 1999); and some publications. Through these activities the project seeks to build up tools, expertise and networks needed to provide guidance to local communities, university groups and public institutions in advancing collaborative approaches to natural resource conflict resolution. The project began in early 1998 and involves several CA research institutions, at a total cost of \$230,000 to IDRC.

Case studies facilitated by this project (e.g., WBI book, Copan, Honduras) make a good-quality contribution to research in the field of natural resource conflict management. The network also appears to be successful at making CBNRM information, tools and training available to Latin American researchers interested or involved in developing and applying Minga principles to their own research activities. Impact attained beyond these immediate gains is hard to assess. Documents made available to the reviewers suggest that the project is putting considerable emphasis on academic-NGO networking activities and research capacity-building. There is little indication of other Minga priorities (research productivity, multiple stakeholder involvement and institutional mainstreaming at local, regional or national levels) having already been attained.

## 6.3 *Bellanet proposal*

The Bellanet proposal (Virtual Workspace for Small Grants Programs and Dialogues on NRM in LAC) is a well-focused, modest-budget contribution to Minga's small-grant program, enhancing its visibility through advanced Internet-based technology. While applied to Minga activities, the project also aims at testing and developing a general model for efficient and transparent SGP management, a model to be transferred to institutions in Latin America and the Caribbean. In our view, this well-designed experiment in on-line management practice illustrates the actual flexibility shown by Minga towards projects that fit perfectly well into its general agenda but that do not necessarily meet all requirements of a monumental, comprehensive approach to benchmark resource management in LAC.

## 6.4 *Sustainable Hillsides Agriculture and Farmer Participatory Research for Sustainable Management of Honduran Hillsides I & II*

The two projects have generated a wide array of documents. They include project proposals, final reports, workshop proceedings, a training manual, at least one presentation in an international conference, and brochures for farmers. Documents mention several training and networking CIALs established by projects in their respective areas. Most of the research documents submitted to the reviewers can be described as "gray" literature material of variable quality and which interested outsiders may find hard to access. There are no peer review papers aimed at the scientific

community or references to papers in preparation or already submitted for publication. There is no policy brief or document aimed at policy makers either. However, the latest Annual Report 1999 mentions a book in preparation that will synthesize the project experiences, thus filling some of these gaps.

The documents reviewed reflect the strength of these projects, which are the engagement of many different stakeholders in the projects, and the implementation of methodologies to deal with conflict, particularly in watershed management. We applaud the inclusion of documents that are candid about the shortcomings of project implementation from the point of view of the stakeholders, such as the document entitled "Estudio sobre el comité local para el desarrollo sostenible de la Cuenca del Río Tascalapa (CLODEST) Yorito, Honduras." This shows honesty and willingness to learn from mistakes, which is highly commendable.

However, these documents also reflect project weaknesses. For one thing we were unable to find any document that presented a conceptual framework for the projects. There are no documents that explicitly describe the baseline conditions of the projects, besides providing very general descriptions of the sites and the situations they are currently faced with. In fact the evaluation of phase I of the Honduran Hillsides project for phase II (97-1001-01) points out that baseline information was collected, but not organized in a way that can contribute to project evaluation, a problem to be addressed in phase II.

Given this, it is hard to see how impact will be assessed. It could be argued that impact comes at a later stage only, yet there is no document that offers a strategy and methodology to document impact. Nor is there a document that reflects on what methods should be used to carry out comparative analysis. For example, in the Technical Report for the project Improving Agricultural Sustainability and Livelihoods in the Tropical American Hillsides, June of 1998, activity A.7.1 deals with learning from experiences in multi-stakeholder NRM in several countries. A document regarding this activity is included in one of the annexes. The document lacks not only a conceptual framework for the comparisons of the different experiences but also the indicators or variables to be used for comparing experiences. This suggests an *ad hoc* approach to exercises in comparison and lesson learning.

Another key method used in these projects is the CIAL. The projects promote the creation of CIALs without providing any evidence that they make a difference. If CIAL impact is the object of study, then there should be a strategy designed to test what difference they may or may not make. For example, the fact that CIALs established in Colombia and Central America show different levels of participation and accomplishments offers an important opportunity to investigate factors that influence participation and outcomes. Such a research could be very useful in learning more from the CIAL methodology. While the comparison of different experiences is stated as a goal, there is no explanation of what variables, indicators or factors will be used to carry out such a comparison. Statements such as "CIAL methodology contributes to better technology development (a more relevant and quicker evaluation process, as well as improved adoption and/or adaptation), applicable directly at the farm and community levels" (Project Proposal for Phase II of the Honduran Hillsides) are not backed up with any hard evidence.

In summary, the strength of these projects lies in their reach. They have engaged different partners and stakeholders to address difficult problems, both in Colombia and Central America. Efforts to develop and apply tools for conflict resolutions in watersheds are most promising. However, conceptual parameters are generally missing and strategies to compare methods and generate baselines are somewhat deficient. Our concern is that these projects may have real impact in the long run but may not be able to document such effective impact in any credible way. On the positive side, the PI team seems to be aware of these shortcomings. This is clear from the appraisal of the Honduran Hillside proposal for Phase II, where the evaluators recognized the problems documenting baselines and evaluating impacts in Phase I. These problems are to be addressed in Phase II.

The PI should perhaps require from each project it funds (a) documents that present their general conceptual parameters; (b) a strategy to deal with the comparative analysis of methods and tools (including institutional experiments); and (c) the precise ways in which impact will be documented and how baselines are going to be used.

### *6.5 Resource Management (Ecuador) I & II*

These projects have generated a wealth of documents. They include project proposals, final reports, workshop proceedings, a book and book chapters. Although many of the documents are also "gray" literature, there are more formal outputs for a scientific audience, such as the books and book chapters. There are no peer review papers or references to papers submitted or in preparation. The scientific quality of many of the documents seems nonetheless adequate. There are no policy briefs, but we presume they will emerge from phase II of the project.

Project proposals and the final technical report of phase I are to be noted for the thoroughness of the information presented. There is excellent baseline information and an analysis of the present conditions in the watershed of El Angel. Again, these projects do a very good job at engaging multiple partners and stakeholders. The level of interdisciplinarity is also commendable. Finally, the projects make a good case for their relevance and contribution to the Minga program objectives.

An explicit strategy to evaluate project impact in Phase II is nonetheless lacking. This is less of a problem with Phase I, which is more of a diagnostic. In the proposal for Phase II, objective 3 mentions the identification of critical indicators for impact and a monitoring plan (3.2) and the elaboration of baseline indicators of development (3.6). This is reassuring, yet the very thorough and complete analyses done in Phase I would lead us to think that project members would have a clearer idea of what indicators they should use and how to go about documenting the actual project impact. Although the project plans on doing close M&E and has the kind of baseline information needed to do impact assessment, researchers could be more explicit as to how they intend to do this. Two questions should thus be addressed: (a) how would project success affect stakeholder behavior and resource management practices, and (b) how can positive results be convincingly documented?

Another potential problem as mentioned earlier, is the acritical incorporation of CIALs in project plans. Phase II plans on establishing at least 15 CIAL-type research activities. Yet there is no hard evidence presented that CIALs do make a difference in the patterns of

adoption of technologies at the community level. On this point, see our discussion of the Hillside projects.

#### *6.6 Competitive Grants Program for Research on Monitoring and Evaluation for Natural Resource Management Projects and Programs in LAC*

RIMISP documentation is relatively limited since it is relatively recent and consists mostly of a project proposal and web site material. This project is very well justified and does address program objectives. The project acknowledges many of the concerns raised above with respect to impact assessment methods, comparative analyses of methods and approaches, and the use of related indicators. Other Minga projects could learn from discussions such as the one appearing under point 5 of the background and problem statement. This is the section that addresses issues of selection of indicators of equity, sustainability and efficiency; ex ante diagnosis and baseline study of the dynamics of NRM in project areas and control areas as well; etc. Other projects stand to gain from the lessons learned in this project.

Furthermore, the Training and Exchange Program as described in the Annual Report 1999 should address some of the deficiencies pertaining to baseline comparisons for Minga Benchmark Areas.

### **7. Conclusions**

The Minga Program Initiative (PI) is well conceived and well suited to its LAC context. Its objectives provide a coherent and systematic framework to guide relevant research initiatives in LAC. The program proposes well-defined objectives, activities, and outputs to be carried out by Minga Projects, thus avoiding an ad hoc set of unrelated projects that may not be mutually complementary.

Minga addresses complex problems of poverty and resource degradation in the region. This is reflected in the diverse portfolio of projects it funds. The PI strives to apply a holistic, multidisciplinary, and multistakeholder approach to these problems. While Minga's preference for holistic, integrated and comprehensive methodologies should be pursued, three things should be avoided: aiming beyond realistic targets and enabling environments; conflating program and project cycle; and developing project selection criteria lacking in modesty and realism.

Minga has successfully developed and should continue to exploit a niche of its own: LAC ecosystems, the benchmark approach, methods/tool development and assessment, conflict management and consortia networking (under conditions specific to the LAC context). It is also investing considerable energy in introducing these principles into a LAC projects inherited from the pre-PI period. Institutional networking is probably the strongest aspect of Minga, as evidenced by the program's sustained effort to bring together different actors and perspectives in all program initiatives.

Minga is moving towards a focus on completing cycles of research. This seems to indicate a shift from comprehensive benchmark area projects to projects that show clearer focus and stronger project design. We considered this shift to be a positive move; all the same, the benchmark area concept is central to Minga planning and should be

maintained, we view the latter move as positive. Finally, given the program focus on developing and applying research instruments, it is our view that there should be clearer strategies to compare and assess these tools and methodologies and to deal with baselines both within each project and the program as a whole. These strategies must be refined if proper lessons are to be extracted, if methods are to be compared and assessed, and if impact is to be effectively documented.