

Technical Notes and Comments

The Costs and Benefits of Collaborative Research

BRUCE J. PETERSON

The Ecosystems Center

Marine Biological Laboratory

Woods Hole, Massachusetts 02543

Introduction

This essay is derived from a presentation at the Estuarine Research Federation's 1991 meeting in San Francisco; the presentation was part of the special session on Land Margin Ecosystem Research (LMER) and Long-Term Ecological Research (LTER). I thank session organizer Chris D'Elia for encouraging me to address the effectiveness of large-scale collaborative research efforts. Many of the thoughts and examples used are derived from my experiences with the Arctic Long-Term Ecological Research (LTER) project based at Toolik Lake, Alaska. Others are based on 17 years of work on collaborative coastal and global projects at The Ecosystems Center of the Marine Biological Laboratory in Woods Hole, Massachusetts. These thoughts represent my personal opinions and are not the consensus of any group. Nonetheless, I think these experiences are relevant to large group research in many settings and I hope they may be of use to research scientists and managers in their efforts to be effective and relevant in the face of unprecedented needs for solutions to environmental problems at local, regional, and global scales.

Estuarine researchers are starting a number of multiple investigator studies with plans for active collaboration among projects based in a diversity of Land Margin Ecosystems. For many it will likely mean a dramatic change in the way they participate in the scientific process. Whether or not individual scientists can adapt to the demands of large, collaborative efforts and whether or not the costs of those programs will outweigh their future benefits are points of serious concern. As an introduction to the structure of ecological research projects, I will describe the changes in our research program at Toolik Lake, Alaska, as the research has evolved since 1975 from small, loosely coordinated projects to large, tightly coordinated projects to a Long-Term Ecological Research (LTER) site and finally to the inclusion of global change research. Then

I will focus on some of the costs and benefits of projects of various size, duration, and complexity.

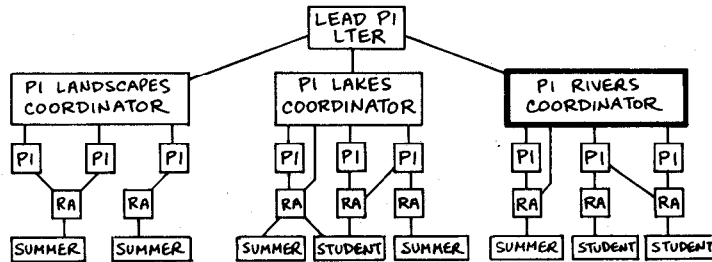
Project Size and Structure

Science is vigorously pursued by projects of very different size, and it is the experience of most of us in ecosystem studies that as we gain experience the size of our projects scale up. In Fig. 1 are examples of the seven size classes of project with which I have experience in research at Toolik Lake, Alaska, the arctic LTER site. Early in their careers, many scientists have the experience of working independently on their Ph.D. project and will remember the exciting feeling of scientific freedom to follow their own instincts and the organizational efficiency of setting their own agenda. This I label a size-class 1 project in Fig. 1.

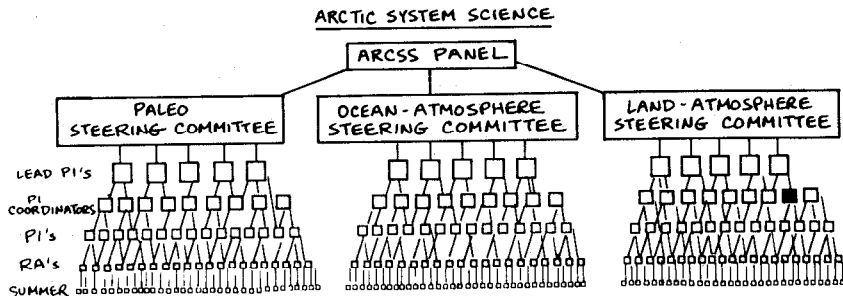
While doing almost everything oneself on a small project can be very effective, most new scientists want to have research assistants and associates as rapidly as circumstances allow. They think that they can do more with more help and don't yet realize the various costs and constraints that affect productivity as more and more people are involved and project complexity increases. Over the course of 10 yr or even less, the typical scientist moves from the Ph.D. project level to projects with 1 or 2 assistants (size classes 2 and 3) and on to multi-investigator, multi-assistant projects (size class 4) and very often participates in several of these simultaneously.

By this time the scientist begins to experience some of the real differences between carrying out small, individual projects and larger, collaborative projects (Table 1). Coordinating even a handful of people requires more planning, funding, and administrative effort. And while an individual can put together a program with several small disparate sources of funding, a group requires more funding but also greater continuity and predictability of funding in order for the research to remain coordinated. In fact, the two strongest forces frustrating collaborative research are the inability of certain combinations of people to work together and the lack of funding agencies with the resources and willingness to make adequate funds available for a sufficient period of time to complete a project. In the first instance, it is difficult for most scientists to sacrifice the individual freedom to follow their own nose and to dedicate their time to group achievement. Scientists are trained to be independent thinkers, and they strongly believe in the cre-

- MODEL**
- SIZE CLASS 1 : GRADUATE STUDENT WORKING ALONE ON A PH.D. THESIS : **Ph.D. STUDENT**
 - SIZE CLASS 2 : POSTDOCTORAL STUDENT ASSISTED BY AN UNDERGRADUATE RESEARCH ASSISTANT (RA) : **POSTDOC**
RA
 - SIZE CLASS 3 : ASSISTANT SCIENTIST/PROFESSOR WORKING ON AN NSF GRANT WITH TWO RESEARCH ASSISTANTS (RA'S) : **ASST. SCIENTIST**
RA RA
 - SIZE CLASS 4 : SMALL SCALE COLLABORATIVE PROJECT: ASSISTANT SCIENTIST/PROFESSOR AS A CO-PI. WORKING ON A 3 OR 4 P.I. PROJECT WITH 2 OR 3 RA'S. : **LEAD PI** PI PI PI
RA RA RA
 - SIZE CLASS 5 : LTER OR LMER. ASSOCIATE OR SENIOR SCIENTIST/PROFESSOR WORKING AS COORDINATOR OF A PORTION OF A 12 P.I. PROJECT INVOLVING LAKES, RIVERS AND LANDSCAPE COMPONENTS. TOTAL PERSONNEL INVOLVED : ABOUT 25-50.
- SIZE CLASSES 1-4 EXIST AS NESTED WITHIN MODEL 5, OR AS SEMI-INDEPENDENT PROJECTS.



- SIZE CLASS 6 : GLOBAL CHANGE RESEARCH. "ARCTIC SYSTEM SCIENCES (ARCSS)". (ANOTHER EXAMPLE : LTER NETWORK) SENIOR SCIENTIST WORKING AS COORDINATOR/RESEARCHER TO DEVELOP A SYSTEM OF MONITORING, EXPERIMENTATION AND MODELING TO PREDICT FUTURE STATES OF THE ARCTIC SYSTEM. TOTAL PROGRAM : ~ \$15 MILLION/YEAR. TOTAL PERSONNEL : ~300-500



- SIZE CLASS 7 : EARTH SYSTEM SCIENCE. INTEGRATE ARCSS WITH OTHER GLOBAL CHANGE STUDIES. TOTAL PERSONNEL PROBABLY ~ 5000-15,000 GLOBALLY.

Fig. 1. Examples of different size classes of research projects. The author's position over time is outlined in bold.

TABLE 1. Differences between individual and collaborative group research.

| Group Research Requires |
|--------------------------------------------------|
| 1) Less individual freedom |
| 2) More planning |
| 3) More administration |
| 4) More data management |
| 5) More funding |
| 6) More continuity and predictability of funding |

ativity of the individual. In the second instance, the problem of securing funding to cover the increased levels of personnel involved in a collaborative research effort is severe. For example, a single investigator grant of 75K per year is relatively easily funded, but a multi-investigator program of 500K or more per year is not. Very few agencies or foundations will consider seriously the prospect of funding such a large effort continuously for the 5 to 10 years a large multidisciplinary environmental study normally requires. In ecosystem research this means that the large integrative efforts which could effectively address large-scale or long-term issues are normally not even attempted. When such studies are initiated, policy changes in government or in private funding agencies often dictate changes in research direction before the research questions can be effectively addressed. Finally, resources must be set aside for the increased costs of data management and administration that are needed to maintain a large collaborative effort (Table 1). Successful collaborative research requires effective administrators and project leaders in addition to talented researchers.

The Arctic LTER Example

Research on the tundra ecosystems near Toolik Lake on the North Slope of Alaska provides an example of how research topics evolve as the size of projects expands (Table 2). It is common in ecosystem studies for projects to progress over time from small studies with one or a few investigators toward larger efforts with more ambitious goals of understanding whole ecosystems. The accumulated data and understanding derived from the initial efforts allow the examination of more complex hypotheses. In the case of research at Toolik Lake, the LTER project of size class 5 was initiated in the twelfth year of research at the site. It also marked the first concerted effort to link terrestrial and aquatic components of the Toolik Lake research program. Prior studies had addressed questions involving ecosystem processes such as grazing or provided baseline data on nutrient and organic matter fluxes in selected terrestrial and aquatic components of the arctic landscape. Not until the current decade was the awareness of the potential

TABLE 2. Typical topics for different size projects.

| Year | Size Class | Topic |
|------|------------|-------------------------------------------------------------------------------------------------------------------------------|
| 1960 | 1 | The Limnology of Peters and Schrader Lakes |
| 1976 | 2 | Daphnia Grazing on Natural Bacteria |
| 1980 | 3 | The Carbon Budget of a Tundra Stream |
| 1984 | 4 | The Response of a Tundra River to Phosphorus Fertilization |
| 1987 | 5 | The Controls of the Exchange of Nutrients Between Land and Water |
| 1992 | 6 | Regional Estimates of the Effects of Climate Change on Methane Release to the Atmosphere |
| 1990 | 7 | Global Projects Based on GIS-Based Mechanistic Models of the Response of Terrestrial Net Primary Production to Climate Change |

role of arctic systems in global change sufficient to provide a funding rationale for scaling-up to the regional level. This work is still in the planning and pilot study stages.

Some of the benefits and costs of an LTER project are listed in Table 3. The benefits of most importance for ecosystem research are the long-term funding (6 years between renewals with a good chance for renewal if research productivity is high) and the maintenance of a documented data base at each of the 19 LTER sites in different environments. These features allow LTER projects and the LTER network to attempt types of experiments and analyses not easily addressed at other sites. There are, however, additional costs which LTER sites must attempt to cover with their grant

TABLE 3. Benefits and costs of a Long-Term Ecological Research project.

| Benefits |
|----------------------------------------------------------------------------------------------------------------------|
| 1. A longer funding horizon |
| 2. A framework of long-term monitoring and experimentation around which to focus individual projects |
| 3. The ability to address questions at the ecosystem and landscape scales that require long experiments or data sets |
| 4. A well-documented data base for addressing future as-yet-undefined questions |
| Costs |
| 1. Logistics |
| 2. Project meetings |
| 3. Communications network |
| 4. Monitoring in five core research areas, such as primary productivity |
| 5. Data base development, updating, maintenance and sharing |
| 6. GIS (ARC-INFO) data base model development |
| 7. Collaboration with other sites |
| 8. Workshop participation |
| 9. Network meeting participation |
| 10. Trips to Washington |
| 11. Preparation for site reviews |

TABLE 4. Benefits of global change research.

- | |
|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| 1. Similar to the LTER and LMER benefits, but now the questions are regional and global in scale |
| 2. The ability to participate in scientific and policy debates about future states of the earth as they may relate to environmental quality and health of the biosphere |

funds and other resources. It is clear from the experience at Toolik Lake that maintaining the logistic support, monitoring in the five core research areas, and managing the data base/GIS system fully consumes the available LTER funds. We can afford to maintain an active experimental research program LTER site at Toolik only because a variety of other research projects supported by private foundations, NSF, DOE and EPA contribute to the larger collaborative effort. This larger scale of collaboration does, however, further increase the costs of meetings, coordination and administration. Much of this coordination and administration inevitably falls to the more senior scientists in the program and tends to remove them from active participation in research. For some scientists this is a frustrating burden, while others enjoy these administrative challenges.

Scaling-Up To Global Change Research

Most ecosystem scientists choose not to participate in global change research since the scale seems daunting and the issues intractable. However, our ability to understand how the biosphere will respond as climate change accelerates will need to be founded on understanding of the controls of ecosystem processes at a variety of time and space scales. Probably most ecologists have more to contribute than they realize. One daunting aspect is the size and complexity of the planning and administrative work involved in getting these efforts off the ground (size class 6 in Fig. 1). Often the workshops, planning and program document preparation take several years of effort even before the first research proposals can be written. The number of meetings and committees involved can be overwhelming to individuals who are already busy. The total costs of participation in this process are always much greater than the customary travel reimbursement for workshops.

There are benefits of participation in global change research since it allows study of regional and global ecological systems which are beyond the scope of most ecological research (Table 4). However, the main benefit of participation in global change research may be that it provides a forum for scientists to participate more actively in shaping the way man chooses to change the world. In much basic ecological research, if one chooses not to par-

ticipate, the research simply isn't attempted and there may be relatively little apparent consequence. In the global climate change arena, policy decisions about food and energy generating systems are going to be made which will affect the future of the biosphere whether or not there is any organized input from environmental scientists. With appropriate and systematic research, scientists will not only learn about the controls of ecological processes at large scales but will also be in a position to predict how climatic changes may affect large regions of the earth well before these changes actually occur. My view is that such predictions will be always uncertain but that the degree of uncertainty can be narrowed significantly with timely research efforts. Even our ability to perceive change in its earliest stages will be enhanced by timely research. Considering the potential costs of not knowing how changes in global temperature and CO₂ will alter agricultural, forest, and oceanic productivity, the costs of the research are relatively small. It is a puzzlement to me why such research remains chronically underfunded to the extent that the costs of planning, administering and coordinating the programs consume most of the resources available.

Research Initiatives

The phenomenon of attempting to obtain funding for environmental research with new large-scale, long-term research initiatives is growing. The growth is occurring because initiatives that can be attractively packaged and sold in the political arena are required to provide funding to address environmental issues and to help alleviate the erosion of funds available for basic environmental research. There is no question that applying ecological understanding and research to questions of regional and global significance is appropriate. However, scientists must be aware that increasingly massive research initiatives may not be the only, or the most effective, way to conduct or foster research. Arguments are made that these initiatives foster basic research in two ways. The first is that basic research will be needed to answer the question well and the second is that once the initiative's lifetime expires the funds can be reallocated to basic research. I believe both rationales are questionable.

Research initiatives with strong overarching questions are directed by top-down thinking and management. The research teams are addressing a question thought to be important by a workshop or panel that has hammered out a compromise statement of goals. To answer the question the research group will need to employ the best available tools for the job but are limited to currently

available techniques. There is usually not time or sufficient funding to investigate new breakthrough approaches if the promised product is to be attained within the allotted time. Thus, the process excludes those scientists whose research focuses on making new tools available rather than applying already developed approaches. Or, even more damaging, the process entrains the best and most creative scientists but insists they perform routine tasks in their area of expertise. The data needs and time pressures of the program work to exclude basic research from these top-down programs designed to address environmental issues.

I think most people are not aware of how ruthlessly exclusive the process of mounting a research initiative can be. Initially, in the first planning steps, initiative statements are grand and inclusive to gain broad consensus and approval. The estimates of funds to be requested are high enough to give hope to many. Only much later, when most of the potential participants have been eliminated by the proposal review and funding processes, does the actual much reduced scope of the research opportunity emerge. The majority are left to attempt to mount the next initiative or to compete for increasingly scarce basic research funds.

The concept that initiative funds will revert to and enhance core basic research programs when the initiative has run its course is likely to be misleading for two reasons. The first is that now we have phased initiatives where, as one initiative is phased out, another scales up. The second is that funds from initiatives of ten-years duration will scarcely offset inflation when eventually folded back into core basic research budgets that are declining in real dollars annually. A strong basic research program requires its own compelling funding rationale.

Balancing Large and Small Research

The growth of large collaborative research programs poses a dilemma for individual scientists and for science as a whole. We are all aware of the increasing tension between small science and big science especially as big science has grown and gobbles up most of the "new" money. I think there are reasons to think critically about these issues that go beyond simple apprehension about who gets the money.

To be successful, scientists must be creative of new ideas, new techniques, new instruments, and new answers to problems. Creative scientists achieve breakthroughs in understanding which improve life for all of us. Uncreative scientists repeat the patterns of observations of others hoping to discover something new but adding little to the total store of knowledge and understanding. Uncreative sci-

COSTS AND BENEFITS OF COLLABORATIVE RESEARCH

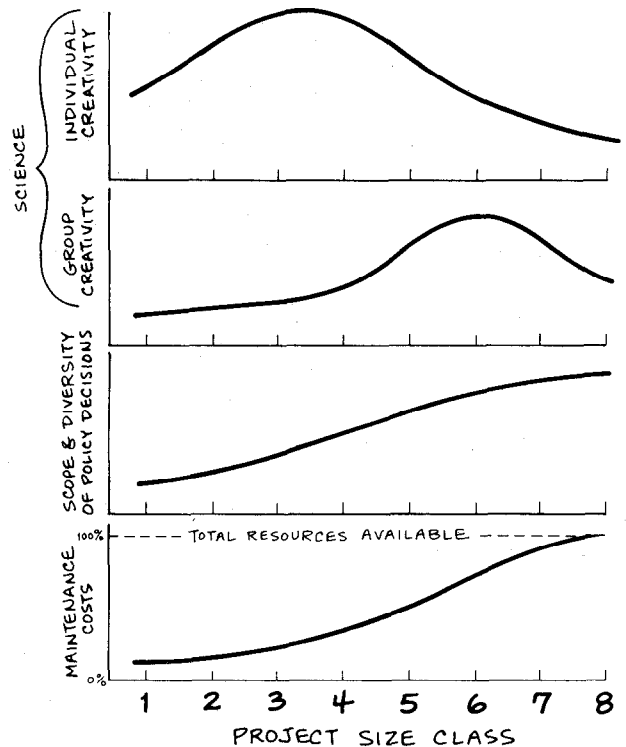


Fig. 2. Hypothesized relationships between scientific creativity, the scope of policy decisions, project maintenance costs, and research project size.

ence dominates when the funding is hard to get and science becomes conservative. Uncreative science dominates when scientists are too busy or too unfocused to work effectively. Uncreative science may be fostered by large collaborative research initiatives unless we can become more successful in controlling the costs and maximizing the benefits of collaborative research programs.

My experience suggests that there are relationships between scientific creativity and the size and structure of research projects that deserve careful consideration. The creativity of individual scientists is increased by interaction with other scientists but only up to a point (Fig. 2). As the size and complexity of projects increases, a disproportionate level of effort must be devoted to activities that are tangential to science though necessary to the maintenance of the projects. The justifications for accepting this hypothesized decline in the creativity of individual scientists are that the creativity of the group of researchers is enhanced as project size continues to increase and that the group can address scientific and policy issues of greater scope

(Fig. 2). However, it is probably true that even group creativity will asymptote or decline as very large projects can become inefficient and hinder scientific achievement. The maintenance costs of the largest of the collaborative research efforts, even not counting institutional overhead, probably exceed the funds going to research. At the largest scales, little original or creative work can be attempted as the goal requires strong top-down direction and control to address the big question.

One common misconception is that individual scientists and small research teams are not effective in addressing big questions. I believe many big questions should be addressed by individuals or by small teams performing funded in-depth syntheses of extant information prior to any large field effort. Resources are currently wasted because field research programs are initiated before all the relevant data can be brought to bear on the problem in spite of the fact that it is far more cost effective to think than to mount a massive program. Without such in-depth thought and planning, funded research initiatives risk failure since they may lack the most appropriate conceptual model of the issue at hand.

Ultimately, scientists must help society address

the question of what research programs must we support to address environmental issues not yet perceived or articulated. Few new initiatives focus on this research area. A wise society does not prepare to answer only today's questions. A society that has lost the ability to articulate and to understand the argument that basic research is a valuable resource in its own right is a society in trouble because the tools to explore future questions and frontiers will not be available when they are needed most. The time and effort required to address the next generation of questions will be overwhelming unless prepared minds are already working on a broad diversity of questions and topics. The most valuable and cost-effective research investment that we can make may well be a strengthening of our commitment to investigator-initiated basic research.

For every dollar of new money we invest in new topical initiatives, we should invest a matching amount in growth of basic research programs where ideas are generated by scientists rather than by government agencies and workshops. We should be investing at least as much in our ability to address the issues of tomorrow as we currently spend on the hot topics of today.