IIASA-PC-6

International Institute
for
Applied Systems Analysis

PROCEEDINGS OF IIASA PLANNING CONFERENCE ON ECOLOGICAL SYSTEMS

September 4 - 6, 1973

VOLUME I: SUMMARY AND RECOMMENDATIONS

Schloss Laxenburg
2361 Laxenburg
Austria

TABLE OF CONTENTS

VOLUME I: SUMMARY AND RECOMMENDATIONS

VOLUME II: APPENDICES

Section Four: Participant's Commentary

Introduction

No one voluntarily defines systems analysis. It can be represented as a new concept of understanding or a new technique of managing any complex system. Or it can be claimed to be simply a new label for an established way of thinking and doing. And ecology is the same. There was a time when ecology meant the study and management of the interrelations between organisms in their environment. Now it means so many things to so many people its definition and relevance are as confused as that of systems analysis. It is ironic justice that two such indefinable subjects have been so hailed as the panaceas for the problems of an industrialized world.

And yet, the problems are real. The resources that nourish the body and spirit of man do not seem as inexhaustible or as available as they once did. And our knowledge, techniques and institutions seem too fragmented to cope. Perhaps ecology and systems analysis are needed because of the failure of a strategy which has led to such fragmentation.

That is the reason behind the International Institute of Applied Systems Analysis--to examine and design resolutions to the problems which have emerged at interfaces between institutions, and between constituencies. In order to give this impossibly broad mandate practical definition and focus the Institute, as its first act, initiated a series of ten planning conferences to draw upon the knowledge and advice of the international scientific community. The Ecological Systems Planning Conference was one of these, and, like the others, was charged to review the field and identify the pressing issues of theory and application where IIASA could play a unique role.

IIASA is a new experiment in cooperation: not just between disciplines, which is difficult enough, but between different nations and cultures as well. As a consequence, its present status, constraints and potential are changing and evolving rapidly. It was scarcely possible during the short time of the conference, therefore, for the participants to do more than analyze the state of the field. Little time could be spent analyzing the state of IIASA, nor on the critical issue of relating these two to each other so that a coherent strategy and plan of action could be designed. These latter steps were taken after the series of conferences had been completed by the resident scientists of each project--first independently and then in iteration with other project groups and the director of IIASA.

These steps have been completed and the Conference Proceedings are designed to document the process. The Proceedings are presented in two parts. The first, "Summary and Recommendations," attempts to capture the essence, but not the details of our Conference by presenting the summary minutes, the proposed research program in ecology as developed after the Conference, an overview of IIASA's research strategy, and the proposed research programs that evolved from the other planning conferences which had the closest relation to ecology. In brief, therefore, this first section represents the present state and plans for ecological and environmental activities at IIASA. The second section provides the details of our Conference agenda, invited papers, formal submissions by participants and written commentaries generated by the participants during and after the Conference.

The conference participants played a key role in the development of each of the research proposals. Moreover, as the research plans are implemented, there will be a continuing effort to be flexible and to evolve different projects and even different strategies. It will be a measure of IIASA's future worth if the scientific community will be as willing to involve themselves in this process as they were in the first planning conferences. If they are not, the grand experiment will be a failure.

October 1973

C.S. Holling
Leader, Ecological Systems Project

SECTION ONE

Chairman's Recommendations
and
Proposed Research Program

C.S. Holling
Leader, IIASA Project on Ecological Systems

Chairman's Recommendations
and
Proposed Research Program

Why IIASA Needs Ecology:

The industrialized societies have had admirable success in mobilizing the resources of our planet for the betterment of man. For all the problems of the present there is, in a relative sense, less poverty, less starvation, and less impoverishment of the human spirit and intellect than at any time in our history. And this has been achieved without any particular need to understand in detail how biological and environmental systems respond to man's intervention and developments. There have certainly been moments in the past when serious problems emerged: starvation, disease, resource depletion, and erosion are only the most obvious examples. But these have been local and transient, and solutions could always be found by a natural adjustment of nature to man or by man's development of a solution outside the confines of the problem (import new resources, export people, develop new lands). The cost of our past ignorance has been small indeed.

Now, however, signals are appearing which suggest that the successes of the past were bought at a price, and that those of the future will hold biological and environmental costs which disastrously outstrip their benefits. Problems of biological and environmental systems which were once local are now regional. Problems which were once regional are now national. And problems which were once national are becoming global. With each nation finding its problems shared by its neighbors, there is increasing difficulty in seeking outside for a solution. Moreover, within each nation, the decreasing resilience of natural systems makes the rate and extent of nature's adjustment to man slower and more tenuous. The very solutions which succeeded in the past could implicitly assume an infinite capacity for nature to adjust. The same solutions now seem to produce new problems of a scale and number greater than the original problems.

If we can no longer seek elsewhere for a solution and can no longer rely on nature's ability to adjust rapidly and effectively to human impact, then we must explore how man can adjust to nature. And adjust not only in the prohibitive or negative sense but in the positive as well, turning degradation into enhancement. This requires knowledge of how ecological and environmental systems respond to disturbance. Some knowledge is available, particularly of the physical behavior of air and water. But because of past priorities, we are appallingly ignorant of the behavior of biological systems and their relationships with the physical environment. With IIASA's goal of addressing multinational and global problems by mobilizing, synthesizing, and catalyzing scientific knowledge it cannot help but be concerned, not just with physical environmental systems, but with biological ones as well. IIASA needs ecology, for the cost of ignorance has become too high.

Why Ecology Needs IIASA

The Ecology Conference found it useful to organize its discussions along two dimensions, one indicating level of organization (roughly equivalent to geographical scale) and one representing the spectrum from fundamental studies, through practical management, to regional, national, and international policy (Figure 1). Because of the immense complexity of ecological systems, the major advances of ecology have tended to concentrate in the upper left hand portion of the Figure. There now exists a particularly rich set of data, concepts, mathematical models, and practical management experience in the more heavily shaded region of the Figure and a growing movement to analyze larger systems and to develop policy. In contrast, IIASA's goals and objectives can be represented almost exactly by an inverse version of Figure 1, with major interest in the lower right hand portion. It is the one international institution with precisely the right combination of relevant goals, prestige, independence and in-house research capability to accelerate the hesitant movement of ecosystem research and environmental policy analysis into a rigorous science of global ecology. If IIASA needs ecology, ecology even more needs IIASA in order to turn its rhetoric into substance.

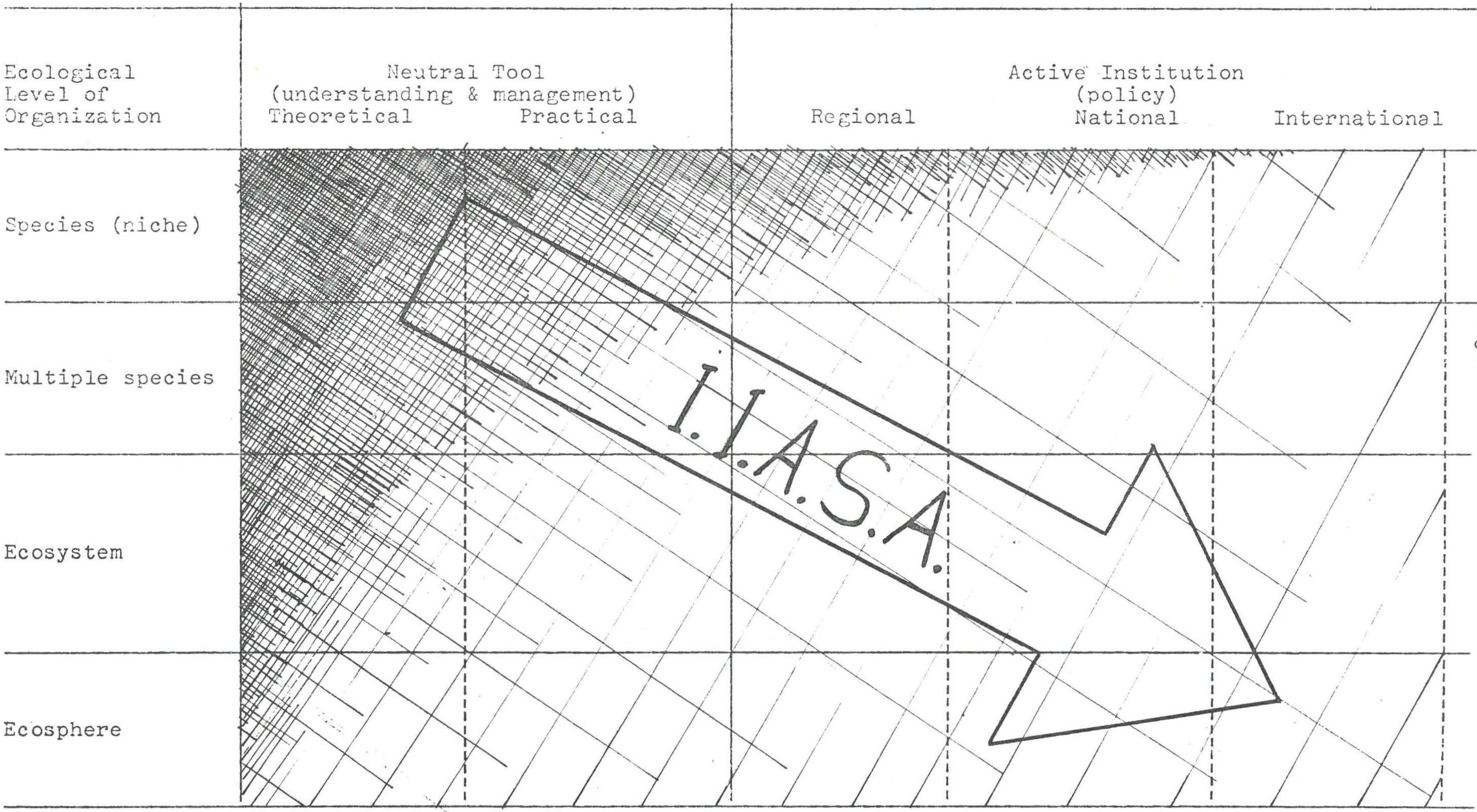
Performing the Marriage

There is now a congruence of interest between ecology and IIASA, but a mis-matching of goals, knowledge, concepts, and methodology. As a strategy to perform the needed marriage, we suggest the following.

(1) Maintain a continuing in-house theme centering on fundamental analysis of ecosystem behavior in the face of natural and man-induced disturbances. This must touch on concepts and methodology concerning complex systems behavior (stability, instability and control, resilience) and questions of dimensionality and resolution in space and time. These

Figure 1

EXISTING PATTERN OF ECOLOGICAL KNOWLEDGE



issues are absolutely central to any understanding of ecological problems, and identify the point of congruence between ecological interests and policy relevance. They also underlie every other conceptual, methodological or applied project conceivable for IIASA. Finally, they provide the one carrot which will attract cooperation and commitment from the international community of ecologists. A theme of this sort will provide continuity and the resources of creative ideas and people which can spin off into applied projects, policy research and short-term activities of IIASA.

(2) Develop a mix of one continuing project with short term pay-offs (one year or less); one with a mid-term duration (1 - 2 years); and one with a longer time horizon (3 - 4 years). The first is the continuing theme mentioned above. It would be dependent only on data in the literature and would emphasize conceptual and methodological innovation. It would require three resident ecologists (senior and junior) and two or three support staff. The second would also rely on published data and would have a specific practical focus on developing the framework to address pressing regional or global problems. The same individuals associated with the continuing project would provide the core staff, with consultants drawn internationally. The third project would require a data collection effort and thus would have to be performed in close partnership with another research institution having the necessary personnel, facilities, and expertise. A regional focus is probably the only practical one, with IIASA providing systems and modelling coordination and the mechanism to turn a national effort into a multinational one. It clearly should operate with the partial help of the resident ecologists and external consultants through one of the other IIASA applied projects (e.g., water, energy).

(3) Develop activities in which IIASA ecology personnel would perform a coordinating role: conferences on key issues, contacts with a minimum set of existing research projects, cooperation with international agencies and the preparation of state-of-the-art handbooks.

In order to implement the above strategy, there are five essential ingredients.

(i) a core of 3-4 resident ecologists (one to two year appointments) with a breadth of ecological, mathematical and policy interest and experience;

(ii) two or three support staff (programming, secretarial, technical) specifically for the ecology project;

(iii) a program development scientist with resource and environmental experience and organizational talents (ideally, on an appointment of three or more years) who would use resident scholars as consultants. Such a person is essential for continuity, for the development of creative opportunities, and for freeing resident scientists for research;

(iv) effective cross-project cooperation with IIASA;

(v) heavy use of consultants for short periods (2 days - 3 weeks).

Specific Proposals

We propose eight sub-projects for consideration by IIASA, and summarize these in Table 1. Sub-project I represents the continuing fundamental theme mentioned earlier, and can yield relatively frequent short term payoffs in concepts and methods. Sub-project II represents the ecology project's possible contribution to the Water Resources long-term effort established in conjunction with some other research institution. Sub-project III is a mid-term effort which really should be given an independent IIASA project title covering all environmental systems in which ecology activities would be a sub-project. Project IV(a) is a mid-term project to phase in during the final stages of sub-project III, and IV(b) is a mid-term project for still later development. Sub-projects V to VIII represent the coordinating activities in which resident IIASA scientists have short term responsibilities.

It is particularly important to emphasize the program development stages of these projects (the open rectangles in the Table). Sub-project I has independently already gone through this stage, but every other one needs a small or heavy planning investment. If this is to be done exclusively by the project leader, then the research activities and contributions will suffer. It is a full time, professional role and we recommend immediate negotiations to recruit a program development scientist. This position requires a mix of organizational talent and ecological experience. This person would contribute greatly to the Ecology Project as well as being an experiment for general IIASA consideration. Inevitably, his activities would facilitate the planning of other projects and we are convinced the experiment would change not just the degree of research at IIASA but the kind as well. It is as creative and innovative a role as is research itself.

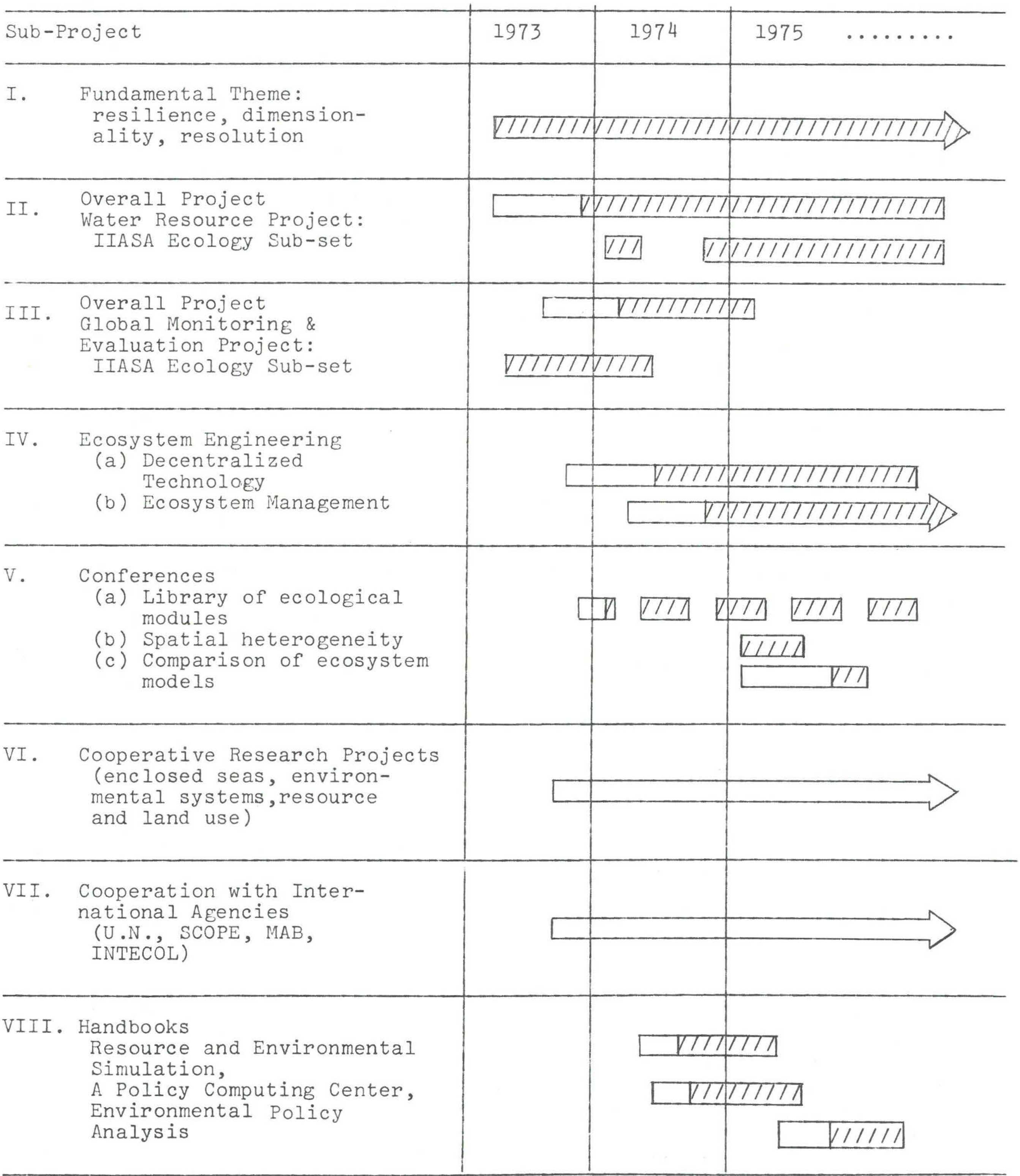
I. Analysis of Perturbed Ecological Systems

This is the basic project we have now initiated. Its

Table 1

Program Summary for IIASA Ecological Systems

(shaded rectangles = established subprojects at IIASA)
(open rectangles = project development stage)



details are covered in an annex to this document but, to summarize, we plan three stages. The first will rely on a set of models ranging from simply analytical ones (logistic, Lotka-Voltera and its variants), through several complex versions of a simulation model of an ecological system (the fresh water lake), to two land use simulation models with economic and physical as well as ecological dimensions (a recreational land use model and a model of the impact of a large hydro-electric development in Northern Canada). Each of these models shows a resilience property when parameters or state variables are perturbed, and we will use them in developing a mathematical formalization of the concept. In the second stage, we will illuminate the resilience principle by reference to the behavior of ecological, anthropological, and perhaps economic systems treated in the literature. In the third and final stage, we will design surrogate indices which can be readily measured within a framework of ecological monitoring and evaluation for a regional or global context (Sub-project III).

II. The Ecology Project's Possible Collaboration with the Water Resources Project: <u>The Alpine Areas Study</u>.

During the Conference on Ecological Systems, many delegates encouraged IIASA to develop an active role in the analysis of alpine lakes watershed systems. Approaches to the management and development of such systems are under active investigation in Austria, Italy, and France. The problems faced are ecosystem-level in scale, and both international and ubiquitous in extent. Parallel work by groups in the USSR, Canada, and USA provides a possible avenue for comparative studies and cross-fertilization of approaches.

Concern for the management and development of alpine lake areas logically comes under the purview of IIASA's ongoing Water Resources Project. And while the "systems perspective" of that Project, with its integral concern for the economic, sociological, hydrological, and chemical aspects of water resource problems is clearly the appropriate context for an alpine area study, we feel that there are a number of essentially ecological issues which could be profitably addressed in a subproject of IIASA's ecology program.

European workers have traditionally been leaders in studies of aquatic ecology and chemistry, and the individuals with whom we would hope to be collaborating in the Alpine Area Study -- particularly Drs. Pechlander and Stumm -- are among the foremost limnologists in the world today. By and large, however, the European tradition in limnology has tended to be rather narrowly professional; various specialists pursue their interests independently, and there is little inclination to work closely enough with other disciplines to make an integrated understanding of the alpine area available to planners. Consequently, present policy planning for alpine area management has made very little use of the ecological knowledge and expertise potentially available. Similarly, there has been a lack of any mechanism by which the various specialist disciplines could determine the real data needs of the resource planners.

The role which IIASA should be playing here is clearly one of integration, communication, and analysis, rather than data acquisition or fundamental research. We can act most usefully as a catalyst, bringing technical experts from a number of countries and disciplines together with responsible planning and policy officials in an environment designed to promote a practical, applied understanding of the alpine lake system.

In order to capitalize on the Conference's enthusiasm for this "catalyst" role, and to provide a stimulus for early interaction between the on-going ecological and water resource projects of IIASA, it is important that some initial activities for the Alpine Area Study be promptly initiated. We believe that the ideal vehicle for this is represented by the modelling workshop approach outlined by Dr. Carl Walters at our Ecological Systems Conference.

The program described by Dr. Walters is perhaps unique in its ability to bring systems analysis techniques and a vast amount of experience in modelling complex ecological systems together as effective tools for the resolution of specific resource management problems. Using sophisticated computer software packages and interactive terminals to keep these tools manageable, the workshop approach has a demonstrated ability to provide exactly the sort of environment required for an integrated analysis of the Alpine Area System by scientists and policy planners. Consequently, we are recommending that IIASA sponsor a modelling workshop on the Alpine Areas early in 1974, inviting a cross section of appropriate planners and research scientists, plus Dr. Walters and his team of modellers from Vancouver.

The Alpine Areas Project as outlined here stands on its own merits. Even in the event of no post workshop follow-up by IIASA, previous experience indicates that we would have significantly improved the potential for communication and exchange of ideas on the subject of alpine area development. Additionally, the project would give IIASA a chance to examine at first hand the use of Walters' workshop format as a possible way of approaching more broad-ranging problems of information communication and systems analysis.

In the longer term, it seems likely that the Alpine Area Project might well develop as a continuing focus of cooperation among several groups outside of IIASA and our own in-house activities in water resource and ecological systems.

III. Global Environmental Monitoring: A possible role for IIASA and the Ecological Systems Group.

Several delegates to the Conference suggested that IIASA might usefully engage its talents in the development of a framework for monitoring and assessment of global environmental "health." Many other research programs are dealing with various aspects of this problem, and it was clear that any useful IIASA contribution would have to be carefully integrated with these existing efforts.

Reviewing the arguments presented at the Conference, we suggest that IIASA should sponsor a planning conference with the goal of defining precisely a research program for IIASA in Global Environmental Monitoring, under the following headings:

1. Environmental Indicators (physical, chemical, biological and ecological).

2. Techniques of Measurement and Assessment.

3. Design of Optimal Network and Sampling Procedures.

4. Evaluation of Indicators

   (a) "Environmental Health Indices" to measure the present state of the environment;

   (b) "Early-Warning Indices" representing key indicators of critical importance for the near future of mankind;

   (c) "Cost-of-Ignorance Indices" to measure the degree to which mid or long-term future options are being contracted or expanded;

   (d) Projections of alternate futures using models in which the consequences of alternate patterns of development can be explored.

5. A Framework for Communication of Results.

Such a program has precisely those ingredients needed for an IIASA project--a pressing need that is recognized internationally, a specific client in the United Nations Environmental Program, a system characterization, and an existing effective review of the problem. The latter is particularly important. It is now nearing completion as a Global Monitoring and Assessment Commission of the Scientific Committee on Problems of the Environment (SCOPE). One overview document has been published (SCOPE 1: Global Environment Monitoring. ICSU 1971) and the second action plan is in draft form. These documents have gone a long way towards defining the problems, resolving some of them and identifying the gaps in knowledge. The immediate critical gaps are in defining ecological indicators, designing an optimal sampling network and developing a framework for evaluation. These are all areas of primary interest to IIASA, and existing project scientists have initiated research into some of them, for instance the Energy Project's environmental budgeting system, the Ecology Project's ecological indicators and evaluation indices and Prof. Fiering's environmental standards. In order to integrate these ongoing activities across projects and expand them around the focus of developing a "Global Environmental Monitoring System" we suggest that the Chairman of the SCOPE commission, Dr. R.E. Munn be invited to organize and chair the proposed project planning meeting early in the coming year.

Whether this full environmental monitoring project is initiated or not, the ecology project will identify a set of measurements, evaluation devices, and indicators for the biological subset.

IV. Ecosystem Engineering

(a) Decentralized technology

A major thrust of modern industrialized societies has been toward large scale, highly centralized technology. The merits of this trend are economy of scale and greater efficiency. Such systems support management procedures, that are designed to either maximize the chances of success or minimize the chances of failure, success and failure being judged against a limited goal set.

Natural ecosystems do not appear to be organized in this way. In fact, many aspects, quite the converse is true. Ecosystems are highly inefficient. They are composed of a variety of decentralized elements. They are organized into subsystems of greatly varying size. There are a multiplicity of goals being pursued. And although the failure rate is clearly very high for certain components, the cost of failure to the whole system is low.

The organization of ecosystems provides us with an analog model for an alternative to centralized technology. A formal pursuit of this analogy would lead to methods of organizing technological systems that exhibit the properties of resilience and stability and the ability to survive under great uncertainty. A technology system organized in this way would likely share many properties with ecosystems: diversity, spatial and temporal heterogeneity, flexibility, small scale processes, and so on.

IIASA's interest in this area could involve a short, medium, and long term effort:

Short term: A specific technology could be analyzed and an alternative decentralized organization designed. A prime example would be energy utilization systems. The expertise assembled at IIASA for the Energy Supply Project makes this technology very attractive.

Medium term: Any proposal for decentralizing technology must consider the environmental, economic, social and psychological consequences. IIASA could effectively develop an interdisciplinary project to applying systems analysis techniques to this problem.

Long term: IIASA is already involved and committed to the study of decentralized management and decision organizations. The logical integration of this effort with a parallel decentralization of technology would be appropriate for IIASA.

(b) Ecosystem Management

The purpose of a research effort in this area would be to develop management techniques that would move from single species/resource exploitation to a broader community management and ideally to an ecosystem management.

Such management techniques would look to supplying the resources needed by mankind while maintaining the integrity of the ecosystem. The techniques must maintain the ecosystem resilience and flexibility in the face of gross ignorance and uncertainty, and also for the expression of a multiplicity of diverse goals.

The subproject needs considerable preplanning and cooperation with one or two existing research institutions which have an ongoing research and development activity. Fresh water or marine cultural systems are one possible focus, as is the "ecological zoo" idea of Myron Fiering in relation to turning aquatic degredation into enhancement. It could scarcely be initiated before 1975.

V. Conferences

There are many ecological problems for which data exist but no conceptual framework to apply. Several of these problem areas are ripe for a synthesis of existing views and interpretations. IIASA could play such a synthesizing role by organizing workshop-style conferences to bring together small groups of interested workers. Each conference would be centered around a specific predescribed topic, suitably bounded so that concrete results could be derived in a few days of discussion. Three examples of possible conference/workshops are:

(a) Library of Ecological Modules

The development of a module library of ecological processess was introduced at the IIASA planning conference and given widespread support. A detailed description of this proposal appears in Appendix XV of the Conference Proceedings. The present Ecology Group will undertake the development of this conference series and initiate one or two sessions during 1974.

(b) Spatial Heterogeneity

There is a growing recognition that spatial heterogeneity is of fundamental importance to the behavior of ecological systems. Many elaborate situations have been descriptively recorded but ecology lacks a basic theoretical understanding of the processes which link spatial heterogeneity with ecosystem structure and behavior. On a more pragmatic level, few methods exist to incorporate spatial heterogeneity into analytic and simulation models.

If a few individuals who are concerned with this topic could be brought together in a workshop environment for a week's time, some real progress would be made. John H. Steele would be an ideal leader of this conference.

(c) Comparison of Ecosystem Models

Some interest was expressed at the IIASA Planning Conference for a comparative study of existing ecosystem models. Two types of comparisons are suggested. First, many models exist throughout the world which describe "equivalent" ecosystems. IIASA could provide a forum to bring these models and their builders together for a comparison of techniques and results.

A second comparison could help elucidate the effect of scale and degree of detail on model performance. This could be addressed by comparing different models or by the progressive aggregation of a single model.

VI. Cooperative Research Projects

Several interdisciplinary, international research projects exist outside the sponsorship of the usual international agencies. Many of these are field oriented, data-gathering projects. IIASA could play a valuable role as a liaison between such projects and other research and interested international groups. Additionally, such projects could serve as an important source of data and consulting expertise for IIASA's own in-house research.

The identification of, and communication with these projects would be one of the responsibilities of the Program Development Scientist proposed earlier. It is clearly important that this person be scientifically qualified.

Many projects have a strong ecological aspect. They are concerned with various environmental systems, with resource utilization, and with land use.

As an example, the study of semi-enclosed seas was recommended at the Ecology Planning Conference. It was suggested that this would have multinational interest and could be applied to the Baltic, Black, Mediterranean, and even perhaps to the North Sea. A project bearing on this topic is the Controlled Ecosystem Pollution Experiment (CEPEX) being conducted by laboratories in the U.S., Canada, and the U.K. This project has both experimental and theoretical components and involves a substantial modelling effort. Links between this project and IIASA are already informally established through John Steele, who is a principal investigator. Additionally, the Canadian participation is by a team in Vancouver, British Columbia, with whom the present IIASA Ecology Group has close contacts.

VII. Cooperation with International Agencies

IIASA should develop a strong cooperation with the various international agencies which are concerned with environmental and ecological problems. Some of the more obvious ones are the U.N., SCOPE, MAB, and INTECOL, to name a few. This cooperation needs continuous involvement. If the project leader devotes himself to this, his own research efforts will suffer. It is recommended that the Program Development Scientist take responsibility for this.

VIII. Handbooks

The greatest need and potential usefulness of handbooks is in the area of resource management. The need is strong enough that many are certain to be written in any event. Some of these would be particularly appropriate for publication under IIASA's sponsorship, such as:

Resource and Environmental Simulation: A guide to the construction and use of simulation models; a guide to the various available simulation languages, packaged programs and I/O devices; a guide to techniques of communicating simulation models to the decision-making process.

A Policy Computing Center: A complete handbook on the requirements and methods of setting up a computing center for assessing problems of policy formulation, implementation, and evaluation. The guide would cover the range from theory and philosophy to hardware and staff requirements.

Environmental Policy Analysis: A handbook outlining the various practical techniques of policy analysis, but translated and applied to problems of environmental policy.

The Role of Personnel

Table II summarized the role of IIASA personnel in the proposed Sub-projects. The personnel divisions are: the Ecology Group, Other IIASA Groups, the Program Development Scientist, and Outside Consultants (including conference participants and short-term visitors). Where possible, these groups have been identified as having a major or minor role in the pursuit of each Sub-project. The extent of participation will, of course, evolve as research progresses.

Clearly, the set of projects outlined here cannot be undertaken by the present Ecology Group without contributions from other IIASA projects. Working independently, a small ecology section could contribute to Sub-project I and to a portion of Sub-projects V through VIII.

An independent IIASA project should be established to address Sub-project III, Global Monitoring. Anything short of this would not yield meaningful results. We would further recommend that the major portion of the Alpine Lake Region Study (Sub-project II) be housed in the Water Resources Project with input by the Ecology Group. Sub-project IV, Ecological Engineering, does not need special project status, but it will require a broad base of multi-disciplinary input from new IIASA personnel, as this Sub-project spans the areas of Sociology, Political Science, and Policy Management.

Table II

Role of Personnel in Proposed Projects

Sub-Project	Ecology Group	Other IIASA Groups	Program Developmt. Scientist	Outside Con-sultants
I. Fundamental Theme: resilience, dimensionality, resolution	MAJOR	-	-	minor
II. Overall Project Water Resource Project: IIASA Ecology Sub-set	MAJOR	MAJOR	MAJOR	MAJOR
III. Overall Project Global Monitoring & Evaluation Project: IIASA Ecology Sub-set	minor	MAJOR	MAJOR	minor
IV. Ecosystem Engineering (a) Decentralized Technology (b) Ecosystem Management	MAJOR	MAJOR	MAJOR	-
V. Conferences (a) Library of ecological modules (b) Spatial heterogeneity (c) Comparison of ecosystem models	MAJOR	-	MAJOR	MAJOR
VI. Cooperative Research Projects (enclosed seas, environmental systems, resource and land use)	minor	-	MAJOR	-
VII. Cooperation with International Agencies (U.N., SCOPE, MAB, INTECOL)	-	-	MAJOR	-
VIII. Handbooks Resource and Environmental Simulation, A Policy Computing Center, Environmental Policy Analysis	minor	minor	minor	MAJOR

Finally, each Sub-project will fall short of its potential without the help of the Program Development Scientist. His contribution will be especially critical to the cooperation and communication role of IIASA.

ANNEX I: Present Ecological Research at IIASA

Systems Resilience and Its Policy Consequences: An Approach to the Analysis of Perturbed Ecosystems

C. S. Holling

The purpose of the project is to explore the behaviour of ecological and resource systems, to develop techniques of measuring these properties and to relate them to a planning framework.

Objectives

(i) review theoretical and empirical analyses appropriate for ecological, resource and anthropological systems; review methologies designed to analyse their dynamic behaviour;

(ii) using a set of ecological and regional models described below, develop ways to interpret information contained within them;

(iii) on this basis, define each of the behavioural attributes of the models - particularly that of stability and resilience - and give them numerical representation;

(iv) design a policy framework that emphasizes maintaining open options and the increase of systems resilience rather than more traditional equilibrium-centred approaches;

(v) develop methods to aggregate and disaggregate our understanding of complex dynamic systems in a form useful and usable for planners and policy people.

Background

We propose to use three models we have developed as the core for our project. They represent three different classes of ecological models - one with a small number of state variables and a large number of parameters per variable developed from very rich experimental data (predator/prey system); one with a larger number of state variables (27) and a small number of parameters per variable, developed by a combination of field data and experimentation (lake ecosystem); and one a regional model similar to the preceding but developed by inter-disciplinary groups of experts from the relevant fields. Although this model is less rich in data and well-tested relationships, it has a broader representation of economic, social and policy dimensions.

In our explorations of the behaviour of these models a number of different characteristics have emerged. One of the more interesting concerns a general tendency of the modelled systems to exhibit more than one domain of "stability" or attraction around equilibrium points, trajectories or limit cycles, with at least one domain bounded by an unstable limit cycle. The feature of these boundaries is that they, rather than the area immediately surrounding the various equilibrium states, are critical to the overall behaviour of the system. Points on either side of the boundary will ultimately track to their respective predictable equilibrium states; points near the boundary are liable to be flipped across it from one domain of stability to another in the face of small perturbations. The size of the domain, and the strength of the damping forces near its bounding edge, thus in large part characterize the ability of the system to maintain a structural integrity in the face of unexpected perturbations. If the domain is relatively small then a small perturbation can flip the system into another domain, thus altering its subsequent behaviour out of all proportion to the size and duration of the perturbation applied. Moreover, the weaker the damping forces in the vicinity of the boundary, the greater the likelihood that a small perturbation will cause that boundary to be crossed, regardless of the size of the respective domains. Finally, we note that in our ecological examples the parameter values occurring in nature seem generally to produce domains that are large, with rather weak damping around the equilibrium and strong damping at the boundaries.

From an equilibrium-oriented viewpoint, then, these systems can appear rather weakly damped and quite sensitive to disturbance. But from the viewpoint of the boundary, they are immensely stable with a high degree of persistence. In a sense this is what ecologists have always been saying - that what is important is not the efficiency of such systems, but the probability of their persistence. This orientation switches attention away from events near the equilibria to the events near the boundary of stability, and it is this switch that for us is placing so much of our understanding in a very new light.

We see some interesting consequences that could emerge by applying the resilience concept to policy analysis and the planning process. The analyses described above lead to the realisation that natural systems have experienced traumas and shocks over the period of their existence and the ones that have survived have explicitly been those that have been able to absorb these changes. They have, therefore, an internal resilience related to both the size of their domain of stability and the nature of the damping forces near the boundaries of the domain. So long as the resilience is great, unexpected consequences of an intervention of man can be absorbed without profound effects. But with each such intervention it seems that the price often paid is a contraction in the domain of stability until an additional incremental change can flip the system into another state. In a development scheme this would generate certain kinds of "unexpected" consequences in response to deceptively 'minor' perturbations - a freeway that changes the morphology of a city so that the urban core erodes; an insecticide that destroys an ecosystem structure and produces new pest species. We seem now to be faced with problems that have emerged simply because we have used up so much of the resilience of social and ecological systems. Up to now the resilience of these systems has allowed us to operate on the presumption of knowledge with the consequences of our ignorance being absorbed by the resilience. Now that the resilience has contracted, traditional approaches to planning might well generate unexpected consequences that are more frequent, more profound and more global. The resilience concept provides a way to develop a planning framework that explicitly recognises the area of our ignorance rather than the area of our knowledge.

We are now, therefore, at a point where an intriguing concept has emerged, a set of models are available to explore the concept, and a policy framework is hazily emerging that emphasizes resilience and the maintaining of future open options. We are close to completing a significant study of Systems Resilience and its Policy Consequences, and plan to use the year with IIASA to bring it to fruition.

Work Outline

I Theory of Systems Behaviour

A) Review of Systems Behaviour

(i) ecological systems
(ii) economic systems
(iii) anthropological systems

B) Review of Technical Approaches to Analyzing Systems Behaviour

(i) general systems theory
(ii) control systems, frequency domains
(iii) state space

II Systems Behaviour in the Real World (analysis of examples from ecology, economics, and cultural anthropology)

A) Classification of Perturbations and Responses

(i) perturbation rates, durations
(ii) driving and state variables; parameters

B) Self-contained and reasonably homogeneous systems

C) Considerations of process analysis, temporal heterogeneity, and spatial heterogeneity.

D) Simulation study using models of ecological systems

(i) predator/prey (few state variables, many parameters)
(ii) ecosystem model (many state variables, few parameters/variables)
(iii) regional model

III <u>The Synthesis</u>

A) Summary of systems behaviour

B) Aggregation and degree of resolution of state variables (the concept of functional "roles")

C) Stability and resilience

D) Behaviour "types"

IV <u>Measurements of Resilience and Stability</u>

A) Theoretical, using models

B) Empirical, - with field examples

C) The problem of handling many dimensions...

V <u>Applications</u> - Role of technical information in decision making

A) Defining roles in the planning process

B) Information needs and contribution of each role

C) Aggregation and disaggregation of information appropriate for each role:

(i) research
(ii) development of strategic, alternatives
(iii) evaluation of alternatives at tactical and value level (resiliency and stability indicators, etc.)

D) Techniques of presentation, using examples (e.g. James Bay Development Project) -- workshops, graphical display, management 'slide rules'

(NOTE: A summary working paper written for non-mathematical ecologists is attached that briefly covers parts of topics I to IV above)

Personnel

C.S. Holling - Director, Institute of Resource Ecology,
University of British Columbia, Vancouver 8, B.C.
Systems ecologist
Married, 3 children ages 6, 10 and 12 years

Dixon Jones - Post-Doctoral Fellow (to July 1973)
Institute of Resource Ecology, U.B.C.
Physicist
Married, no children

Bill Clark - Ph.D. student in ecology,
Institute of Resource Ecology, U.B.C.
Unmarried

Zaphar Rashid - Programmer, Institute of Resource Ecology,
U.B.C.
Unmarried

SECTION TWO

Summary Minutes
of the
Research Planning Conference
on
Ecological Systems

First Day: Identifying the Issues

Introductory Remarks

Mr. Howard Raiffa, Director of IIASA, welcomed the participants to the Conference. Descriptions of the Institute history, its overall research orientation, and the specific goals and ground rules for the Ecological Systems Conference are covered in the text of his remarks included in these proceedings as Appendix II.

Mr. Raiffa then introduced Mr. C.S. Holling, Chairman of the Conference and head of the IIASA Ecological Systems Project. In his opening comments, Mr. Holling noted that ecology had come to mean many different things to different people, and that the Conference was faced with the overarching problem of developing and maintaining a tight focus for its deliberations. He asked the participants to bear in mind the distinction between ecological and environmental systems: the latter involve many considerations outside the purview of the former, and would have to remain as one of several areas of secondary interest to this Conference, though not to the research program of IIASA as a whole.

The Chairman stated the charge to the Conference as one of (1) developing a general overview of our present understanding of ecological systems, (2) assessing IIASA's particular potential for contributing to the advancement of this understanding, and (3) generating a set of specific alternative proposals for ecological research projects at IIASA. Mr. Holling stressed that he was not seeking a consensus of the Conference on these goals, but rather was interested in obtaining as free and diverse a sampling of the opinions as possible.

A "Preliminary List of Issues for Consideration" had been distributed to the participants with the pre-conference materials (Appendix XVIII). Referring to this list, Mr. Holling suggested that the Conference focus its discussions upon the following topics:

(1) Behavior-organization relationships in ecological systems;

(2) Methodological approaches to the analysis of ecosystem behavior;

(3) Human impact on ecological systems, and policy considerations relating to such impact;

(4) The problem of coupling our understanding of ecological systems to broader questions of environmental engineering; and

(5) The identification of "digestible," researchable topics for consideration by IIASA.

The Chairman next reviewed the Conference agenda (Appendix I). The first day was reserved for identifying central issues before the Conference, with no attempt at consensus or priority assignment. Problems of methodology and of model development and application were scheduled for detailed discussion on the second day. On the last day of the Conference, an effort would be made to deal with outstanding conceptual difficulties of ecosystem analysis and to develop a final set of project proposals for consideration by IIASA.

In his concluding remarks, the Chairman asked participants to submit written commentary at regular intervals in order to give him as much feedback as possible on issues discussed during the Conference. In addition, he requested all delegates to consider the developments of the Conference after their return home and forward any retrospective thoughts to him at IIASA (cf. Appendix XVI). He expressed a particular interest in receiving suggestions of potential cooperating institutions, individuals, and projects.

Invited Papers

In the first presented paper of the Conference, Mr. Holling spoke on "Resilience and Stability in Ecological Systems" (Appendix III). Summarizing his remarks, he posed the following questions for consideration by the participants:

-- Would there be any purpose in a "process inventory" or "module library" for use in ecological modelling?

-- Can we identify behavioral classes of ecological systems? Does the evolutionary history of an ecological system tell us anything about its likely response to a particular management or development scheme?

-- Should we perhaps question the paradigm of management which seeks to reduce variability in systems?

-- Does our understanding of ecological systems allow us to say anything about the appropriate scale for various sorts of system management and design? Might there not be some advantage in experimenting with a "technology of the small" as opposed to our present tendency to seek larger and larger scale "solutions?"

-- How can we go about developing indicators of systems resilience? Can we use our understanding of ecological systems to explicitly design resilience "in" rather than "out" of our environment?

The discussion following Mr. Holling's presentation amounted to a series of exploratory vignettes, rather than a sustained development of any specific topic. It is most easily summarized as a series of independent propositions, most of which were considered in greater depth later in the Conference.

1. High dimensionality poses a critical problem in applying ASA techniques to ecological systems. We can treat this in two ways -- by brute force "number crunchers" or by developing a rationale for collapsing dimensions. In the latter instance, we are faced with the problems of developing criteria for the definition of state variables. The concept of "functional roles," whereby essential elements of the ecosystem are defined by differences in trophic status, microhabitat, and size was suggested as one approach to the problem.

2. The development of a library of ecological process modules would be a great contribution to the developing science of ecological engineering.

3. Variability, both periodic and stochastic, appears to be essential to the integrity of many temperate ecosystems. An effective way of treating that variability in our models would therefore seem essential.

4. In designing indices, it is not so important to develop ones which tell us we have "crashed." The need is rather for system-level indicators which warn us of

impending difficulties. We must seek to develop a strategic alternative to our present purely reactionary approach to ecological systems management.

5. A variety of comparatively "slow" chemical and physical processes have very important effects on the behavior of ecological systems. How do we cope with time scales as different as chemical weathering and bacterial reproduction in the same models?

Mr. Brian Mar presented the second invited paper of the Conference. The text of his remarks on "Where Resource and Environmental Simulation Models are Going Wrong" is included as Appendix IV. In concluding his presentation, Mr. Mar argued that contemporary environmental modelling remains dominated by "one-man models." He suggested that a major challenge facing an organization such as IIASA consisted in shifting this dominance in the direction of truly collective multi-person modelling. In attempting this, IIASA would confront the same pitfalls which Mar and others had shown to be the causes of failure for most large scale modelling programs. The question for IIASA is "How will it be any different?" Finally, Mr. Mar cited the lack of criteria for establishing the "appropriate level of resolution" in modelling efforts as perhaps the single greatest problem which system modellers face. He held that until this conceptual/methodological impasse was satisfactorily resolved, the reductionist tendencies of most scientist-cum-modellers would continue to lead us on our present path to knowing more and more about less and less.

The discussion session, again, defied cohesive summary. The question of validation, with emphasis on the need and enormous difficulty of validation against nature, was much discussed. Several delegates, however, suggested that IIASA might attempt to provide a favorable environment for an interim step in validation through the "comparison of conflicting lies" represented by comparing alternative models of the same system.

It was suggested that no institutional framework could really do much to guarantee interdisciplinary cooperation among scientists. IIASA should search for people with a proven record of interdisciplinary "mixing."

Finally, the point was made that no proven set of techniques exists in any field for dealing with the complex and inherently stochastic systems central to ecology. It is possible that fundamental methodological research, in addition to good contact with existing methodologies, will be needed before ecologists make much progress with some of their more difficult analytical problems.

Presentation of the National Member Organization Delegates

The afternoon session and part of the second morning were reserved for the presentation of position papers and comments by the NMO delegates. A number of formal submissions providing recommendations for IIASA's research policy in ecology were received during and after the Conference and are included as Appendices IX through XV. Those focal points of the discussion not covered in the submissions are summarized below.

1. IIASA was urged to make a particular effort to identify and exploit its unique attributes; among them freedom from bureaucratic constraint, a trans-national and trans-disciplinary outlook, systems expertise, etc.

2. Ecology is a field which potentially transcends other disciplines in its perspective, but until recently ecology has considered man only as an afterthought or as a perturbation. It put man in brackets. Similarly, the social sciences have failed to look on man as part of the ecosystem. They have put nature in brackets. What we need is a science of man <u>and</u> society <u>in</u> the ecosystem.

3. Desirable criteria for selection of research projects were discussed; considerations of policy relevance, international or ubiquitous applicability, economic utility, and bearing on human problems were stressed.

4. IIASA faces a dilemma. One of its legitimate goals is to bring resident and cooperating specialists into touch with real, tangible, solvable problems. Yet in doing this, we run the risk of chosing only topics for research which can be treated in a technological manner.

5. There was a recurring emphasis on the need for IIASA to focus its efforts on a realistic number of tangible, solvable problems. Specific points to consider in this regard are:

(a) Who is the client for IIASA's work? Who is to judge its performance?

(b) Where does IIASA find its data base?

(c) What facilities and arrangements will be available for model verification and validation?

The need for a certain degree of project continuity was stressed, and several delegates warned of the dangers of committing too many resources to coordination as opposed to actual research activities.

6. ASA should be an appropriate tool for pinpointing where the "cost of ignorance" is highest in our efforts to understand the behavior of ecological systems. It should thus provide a rational way of establishing research priorities.

7. There was general agreement that a central focus of IIASA work should be the study of perturbed systems, i.e., the behavior of ecological systems under stress. This topic is covered in detail by the invited papers of C.S. Holling and H. Regier. Concern was voiced, however, that there exists a danger of becoming preoccupied with such perturbed systems to the extent of ignoring the relatively undisturbed ones which offer us potentially sensitive indicators of environmental conditions and a source for base line data and understanding.

8. The issue of global versus specific system perspective was discussed at some length. Proponents of the global view argued that both ecosystem processes and man's impact on them were truly world wide in scale and could not be meaningfully studied on a regional or subsystem basis. They stressed the difficulty of setting rational boundaries to our "system of interest" at anything less than the entire earth. The opposing camp was perhaps prepared to accept the necessity of a world view in principle, but argued that a research approach seeking to better understand the underlying nature of ecological processes and system behavior was a necessary correlate -- and perhaps precursor -- to significant modelling on a world scale. Further, they pointed out that many of our most severe ecological problems are essentially sub-global in origin and effect. In order to cope effectively with these, we must develop reasonably precise methods of modelling at a local scale.

9. It was pointed out that ecological systems will be all the more difficult to analyze effectively due to their annoying tendency to evolve. There is presently a distinct lack of productive ecological theory at the ecosystem-evolution level, and participants had little encouraging with which to fill the void.

10. One important area of IIASA's activity should concern the development of environmental quality indicators and the establishment of criteria for defining the permissible limits beyond which ecosystems can or should not be pushed.

11. The identification and development of "critical parameters characterizing environmental quality" was seen to be a potential focus for IIASA activities. It was pointed out that such an effort would of necessity entail fundamental theoretical work on the behavior of ecological systems. In addition, however, there is a need for analyzing the design of potential monitoring systems, in order to establish criteria for the sorts of environmental parameters which could be usefully employed in a real-world monitoring scheme.

12. In many circumstances, the major obstacle to improved environmental management may be getting what we already know about ecosystem behavior into the proper hands and minds. Should we perhaps be paying more attention to matters of coordinating and administering management policies; to developing "gaming" situations which allow planners to explore the likely environmental consequences of their decisions; and to the general question of information transfer and delivery systems?

Second Day: Issues of Methodology and Application

Presented Papers

Two papers dealing with technical issues of ecosystem analysis and one on the development and application of ecological models were presented. Mr. David Goodall spoke on "Problems of Scale and Detail in Ecological Analysis;" Mr. J.H. Steele on "Patchiness in the Sea;" and Mr. C.J. Walters on "An Inter-disciplinary Approach to Development of Watershed Simulation Models." Copies of these papers are included in Appendices V, VI, and VII respectively.

Mr. Raiffa opened the discussion on the first two papers, suggesting that one major focus for IIASA's activities would be the identification and investigation of methodological "weak links" in contemporary practices of ecosystem analysis. He stressed the Institute's particular interest in exploring fundamental methodological problems common to several projects or fields.

A predominant theme of the morning concerned the high inherent diversity (alternatively read "complexity" or "dimensionality") of ecological systems and the difficulties this posed for attempts at analysis. To no one's surprise, participants generally agreed that some simplification of some sort was necessary; the debate concerned its extent and the criteria which should be employed in its execution.

Rather too much time was devoted to the sterile issue of whether in some unspecified general case, "reality" could be "adequately" modelled by less than a "complete" inclusion of detail. A more profitable line of discussion suggested that the appropriate analytical tactics for dealing with ecosystem complexity would always be shaped by the specific questions to which an analysis was directed. Two related questions were posed but remained unresolved during the conference: What tactics (i.e., methodological tools) exist for the systematic reduction of dimensionality in complex systems? What strategic considerations should apply in deciding which of these tools is appropriate for a given goal of analysis?

The treatment of spatial heterogeneity in analysis of ecological systems provided the second major focus for the morning's discourse. Mr. Steele showed that in oceanic systems physical diffusion processes could be expected to establish a "critical scale" for effective system management and control. The importance of such a concept to both theoretical and applied questions is obvious; several participants spoke of a need for investigating the range of conditions over which Mr. Steele's conclusions would hold, and wondered if the same or analogous approaches might not be fruitfully employed in the analysis of more complex situations (e.g., those involving actively dispersing organisms).

It was pointed out that the issues of diffusion in space discussed by Steele overlap substantially with those which have already occupied meteorologists and nuclear reactor engineers (among others) for some time. Also, problems of scale and spatial relationships are common to a number of applied analysis areas in which IIASA has an interest (e.g., water resources). The possibility of a multi-disciplinary mini-conference concerned with the development and consolidation of methodologies for treating problems of spatial heterogeneity was raised and met a generally favorable reception.

In written commentary received after the Conference, several participants pointed out that only a small portion of the subject matter potentially included under the term "spatial heterogeneity" had in fact been discussed. Particular reference was made to the various methods used in simulation work, and to the class of analytical approaches reflected in the section on "significance of heterogeneity, complexity, and dispersal" in P.J. den Boer & G.R. Gradwell (eds.). 1971. Dynamics of Populations (Centre for Agric. Publ. and Cos.; Wageningen, Netherlands). Such topics would certainly be considered in the proposed IIASA mini-conference.

The discussions of tactical approaches to the problem of dimensionality and spatial heterogeneity broadened into a more general consideration of the present methodological arsenal available for analysis of ecological systems. One strongly promoted viewpoint stressed that simulation constituted only one of the several classes of techniques available to ecosystem analysts. "Simulation" (or "modelling") is not synonymous with "systems analysis," despite what one might conclude from present usage in the ecological literature. An unanalyzable simulation model has no obvious advantage over an unanalyzable set of analytical equations, and the analysis of complex simulation models is presently a neglected art form in ecology.

Another line of opinion held that the analytical repertoire exhibited in the ecological literature is presently remarkably narrow. The immediate inclination of most ecologists is to cast any problem solely in terms of differential equations. Only the occasional eccentric employs a few partial differentials, some difference equations, and a bit of probability theory to good effect. After several decades of use, stability analysis remains almost without exception concerned with behavior about an equilibrium point under vanishingly small perturbations of state variables or parameters.

It is not altogether clear whether these present analytical habits of ecologists are more the product of historical accident or a considered effort to select the most appropriate tools available for the problems at hand. In any event, we might ask whether some of the other classes of techniques (e.g., set-and-graph theory, topology) popular among proponents of general systems theory have any useful role to play in understanding the workings of the ecological world.

For the afternoon session, the Conference shifted to a consideration of the development and application of models for ecosystem management. Mr. Walters described an approach to the development of watershed management models, stressing the desirability of bringing decision makers into the modelling process. A major point of his talk and the ensuing discussion was that the traditional distinction between development and utilization of management models was in many cases counter-productive. Walters contended that by intimately involving the ultimate user in the actual design and development of the management model, a large number of the difficulties pointed out the previous day by Mr. Mar could be avoided. Specifically, models developed in this manner were heavily client- and problem-oriented; they made maximal use of the expertise and intuition of those immediately familiar with the problem under study; and most important, they tended to actually improve the decision maker's ability to cope with his management problems.

Mr. Raiffa noted that there was a good deal of decision theory literature on the "gaming" aspects of such participatory modelling approaches. He was convinced of their utility as a sensitization technique, but somewhat skeptical as to their actual effect on the course of planning. He suggested that, in any event, the workshop procedures discussed by Mr. Walters should be tried out at IIASA as one possible means of improving the application potential of systems analysis. This idea was further discussed and eventually incorporated as an integral part of the proposed project on Alpine Regions described in the Day Three summary minutes.

Discussions

As Conferences are wont to do, ours had become quite muddled and diffuse by noon of the second day. Consequently, the Chairman requested that three of the delegates -- henceforth "the Committee" -- spend their lunch break attempting to set the issues raised during previous sessions in some sort of perspective; to provide an alternative to the "creative chaos" which had quite properly characterized the first day and a half of the Conference.

Following Mr. Walters' talk, the Committee advanced these guidelines for the Conference:

1. This Conference should adopt an applied systems analysis-like process for deciding upon research recommendations for IIASA.

2. The shaping of next year's decisions is our primary concern, not an evaluation of the present ecology activities at IIASA.

3. Perceptions of applied systems analysis as a neutral tool and as an active institution are complementary; both aspects deserve attention and development.

4. Decisions regarding the appropriate level of organization and resolution for a particular problem cannot generally be made on *a priori* grounds; provision must be made to reach the proper decision iteratively.

In addition, the Committee proposed Figure 1 (included in the Chairman's Recommendations) as a conceptual framework within which most of the themes raised during the Conference could be included.

Amplifying the Committee's recommendations and comments, the Chairman suggested that the remainder of the session be spent developing criteria for project selection, and a "shopping list" of specific projects for consideration by IIASA.

Participants felt strongly that project selection criteria should reflect a recognition of the particular blend of technical capabilities and political standing unique to IIASA. The proposals listed below, however, represent a sampling rather than a consensus of Conference opinion.

Proposed Criteria for Project Selection

1. Policy and client relevance, with a high potential for real-world leverage; specifically, an active interface with economics;

2. Emphasis on global problems, or local problems of ubiquitous applicability;

3. Interface with other IIASA projects and disciplines;

4. Conscious interface with policy considerations, even in fundamental research or "neutral tool" activities;

5. Common methodology; i.e., technical questions of interest to a variety of disciplines;

6. Digestible project size; this includes a freedom from data-generation requirement;

7. Potential for (catalytic) synthesis of ongoing projects; promotion of information transfer;

8. Overall continuity of effort.

The remainder of the afternoon was devoted to developing a preliminary project list for consideration by IIASA. The Chairman noted that his present goal was simply to formalize the various suggestions which had already been advanced during the Conference. Critical discussion of the resulting proposals was reserved for the following day. In order to prevent repetition, only the final list of projects as it appeared on the blackboard at the end of Day Two is presented below. The summary minutes of Day Three provide details.

Project Proposals

1. Alpine Areas

2. Interaction Between Independent Resource Policies (Fisheries, Agriculture, Forestry)

    -- Centralized versus decentralized decision
    -- Organization of large systems

3. Risk Categories, Indices of Well-Being

4. Land Utilization

5. Ecosystem Engineering

   -- The "Ecological Zoo" for environmental enhancement
   -- Technology in harmony with man and nature

6. Ecosystems

   -- Stability
   -- Instability
   -- Controllability Principles
   -- Analysis

7. Regional Resource Development Projects and Models

8. Coordination with Other IIASA Projects

   -- Energy Project
   -- Water Resources Project

9. Conferences

   -- Spatial scale and detail
   -- Module library
   -- Comparison of alternative "lies"

Third Day: Synthesis and Summary

The Chairman opened the session with a revised summary of Conference goals:

1. To identify present gaps in our studies of ecological systems, independent of IIASA's goals.

2. To determine where IIASA should attempt to locate its activities in the ASA-ecology matrix proposed by the Committee on Day Two.

3. To prepare a "shopping list" of possible ecological projects for consideration by IIASA.

Mr. Henry Regier of the Canadian delegation presented the keynote address for the closing day of the Conference. A copy of his "Conceptual Framework for a Strategy to Mobilize Ecology and Other Sciences in Order to Solve Major Problems Related to Fisheries" is included as Appendix VIII. He set forth a framework for structuring ASA efforts in two dimensions; a vertical one passing through the multiple levels of organization (resolution) at which a problem might be approached, and a horizontal one covering the specialty disciplines on which an ASA study might call. Within this context, the selection of an "appropriate level of organization" was seen as a problem resolvable in a stepwise, iterative manner.

The problems of model validation were also addressed in Mr. Regier's presentation and the ensuing discussion. The need for field replication and for alternative models and paradigms with which to contrast any particular model design was generally accepted by the delegates, but there was disagreement on matters of detail. One suggestion was advanced that a Bayesian, prior-probability approach to alternative models might be appropriate. This, however, was countered by the statement that most model failures derived from the unwarranted omission of structural elements rather than errors of parameter estimation, and a prior-probability strategy of validation was therefore meaningless. Several participants questioned whether an IIASA research project might not profitably address the entire issue of complex model validation.

At the conclusion of Mr. Regier's presentation, the Chairman asked Mr. Ananichev of the USSR, Mr. Mottek of the DDR, and Dr. Ulrich of the BRD to summarize for the Conference the specific research proposals which they had discussed with him. He noted that following these presentations he would open the floor for a critical discussion of all proposals advanced during the Conference, with a view towards fulfilling the second and third goals he had set forth earlier in the day.

Mr. Ananichev presented a detailed analysis on the subject of environmental monitoring and its possible significance for IIASA (Appendix XIII). The view emerging from his proposal and comments from other participants held that the goal of monitoring efforts should be to identify significant changes in the biosphere resulting from man's activities. It was hoped that measurement and evaluation strategies could be developed which provided a realistic lead time for reacting to incipient disruptions of the biosphere.

The major conceptual difficulty in such a program lies with the identification of sufficiently sensitive, indicative, and measurable indicators. Any success in coping with this problem will have to come from an improved understanding of the organization-behavior relationships of ecological systems. This is a subject of fundamental research at IIASA and elsewhere.

Given a set of variables to be measured and a framework for their interpretation, there remain the difficulties of actually designing the monitoring network itself in an efficient, effective manner. This constitutes a classic ASA problem, involving questions of station location and scheduling, the treatment of uncertainty in received data and so on. Further notes on monitoring are included in the Chairman's Recommendations.

Mr. Mottek summarized the material included in his formal submission in order to demonstrate that many apparently different proposals were the same or were part of a larger strategic framework. He recommended a "needs of man" orientation for IIASA projects, adding that the time had come to move from a reactive to a constructive mode in environmental engineering activities. Finally, he introduced the idea of "cost of ignorance" as a powerful aid to the establishment of environmental and ecological research priorities. Each of these topics are dealt with in greater depth in Appendix X.

Mr. Ulrich recommended that IIASA consider classes of short, medium, and long term projects for development.

He viewed short term studies as necessary to provide tangible, immediate focus for the ecology group, noting that projects in this category should be related to methodological problems (e.g., spatial heterogeneity, system resilience) which require only data readily accessible in the literature.

Medium term projects of two years or so would still have to remain dependent upon literature data, but they could have substantially more flexibility than short term studies and would not be restricted to methodological questions.

Finally, long term studies would require a substantial amount of forward planning and coordination, but could tackle major problems in cooperation with various other organizations. There would exist a definite possibility of defining data needs and coordinating necessary field studies in conjunction with projects of this sort.

Following Mr. Ulrich's comments, a general discussion on the relative merits and advisability of the various proposed projects was opened. The resulting final "shopping list" of proposals, again representing a sampling rather than consensus of Conference opinion, is provided below. Supplementary information on certain of the projects is given in Appendices and the Chairman's Recommendations.

List of Project Proposals Discussed on Third Day

1. Indices of Ecosystem Well-Being: the Design of Strategies for Environmental Monitoring

This project has a goal of determining how we may properly assess the "health" of ecological systems: it would involve both fundamental theoretical work and the applied design of efficiently scheduled worldwide monitoring systems. The latter work could build from foundations such as those covered in the recent SCOPE report on environmental monitoring. Incorporated here would be the notion of ecological "risk categories" proposed by Mr. Regier (Appendix VIII) and the "cost of ignorance" concept advanced by Mr. Mottek (Appendix X). Further details will also be found in the submission by Mr. Ananichev (Appendix XIII).

2. Ecosystem Behavior and Organization

In order to develop meaningful monitoring programs, we require a better understanding of what the measurable attributes of ecological systems really tell us about their present and likely future performance. It seems probable that ASA techniques can be profitably employed in the design of appropriate measurement variables, and in our attempts to understand more about the stability, resilience, and control properties of ecosystems. Present IIASA research on ecological systems is concentrating in this area (cf. Chairman's Recommendations).

3. Ecosystem Engineering

Working from an understanding of ecosystem behavior, we should accentuate the positive, working to improve the ability of functioning ecosystems to meet our needs (Mr. Fiering's comments on the "environmental zoo," covered in Appendix XIV are relevant here). With a slightly different focus, we might concentrate on the design of a technology which could function in harmony with -- rather than opposed to -- man and nature. Reasonable topics for inclusion here would be the scientific basis of technological forecasting, the development of socially as well as economically realistic recycling regimes. Mr. Holling's notion of a "technology of the small" would apply as well.

4. Global Assessments

These would include attempts to bring ASA techniques and IIASA's international perspective to bear on problems such as world pollution and climatological impact of human activities. Questions of multiple-use resources might be considered. A number of groups are involved in related activities, and we would have to carefully integrate our efforts.

5. Implications of Unilateral Resource Policies

IIASA might explore the large-scale implications of present unilateral national policies of resource management in fisheries, forestry, and agriculture. Such a project would logically lead to investigation of the scientific rationale for policy coordination.

6. Marine Systems

Several issues were raised, ranging from basic research on the dynamics of enclosed seas to considerations of open ocean pollution and the application of systems approaches to various law-of-the-sea problems. One aspect of this proposal is treated in greater depth in the Chairman's Recommendations.

7. Regional Resource Development

Regional development problems common to many nations could be explored. Particular attention might be directed at questions of land utilization, with emphasis on economic and social as well as ecological implications of various development strategies. Two particular projects posed under this general category are given below.

8. Alpine Areas

A cooperative project involving analysis of Alpine Area use and development was initially suggested by the Italian delegation. The proposal incorporates a regional, interdisciplinary focus and would employ an existing data base and research operation. It might center around the concept of lakes as "ecological filters," discussed by Mr. Letov, and might be developed in the context of a modelling workshop such as that described by Mr. Walters (Appendix VII).

9. Coordination with Other IIASA Projects

There should certainly exist some ecological input to several ongoing IIASA operations, particularly those concerning energy and water resources. Coordination with any epidemiological studies pursued by the medical group would also be appropriate. Summaries of current IIASA research proposals are provided as Section Three of this volume.

10. Conferences

In its information-communication and "catalyst" role, IIASA could host a series of mini-conferences or workshops on appropriate topics. Among those suggested were:

a. Spatial heterogeneity and scalar problems in ecosystem analysis (see remarks of Messrs. Steele and Goodall, Appendices VI and V, respectively).

b. Development of a Module Library (cf. Mr. Holling's comments in Appendix XV).

c. The "Comparison of Alternative Lies"; a conference on the problems of complex model validation and verification (cf. submission by Mr. Regier, Appendix VIII).

Following the development of the project proposals "shopping list," the Chairman thanked participants for their efforts, reminded them of his desire for written "afterthoughts" (covered in Appendices XVI-XX) and adjourned the Conference.

SECTION THREE

Reviews of Related IIASA
Research Efforts

A PROPOSED RESEARCH STRATEGY FOR IIASA*

(October, 1973)

I. AN OVERVIEW OF THE IIASA PROGRAM

1. Modes of Research

This document will describe a proposed research strategy for IIASA in 1974 embracing four related, but distinct, tactical arrangements for organizing the program activities. These are: A. in-house research; B. collaborative research with other institutions; C. an information agency role; and D. conference direction.

A. In-house Research

This document will place relatively heavy emphasis upon the in-house research program, giving it, perhaps, greater present importance within the overall IIASA program than, I think, will actually come to pass. This is understandable in that many sections of this paper were prepared by present, or prospective, in-house researchers who have personal interest in this facet of the program and greater familiarity with this type of activity. The three latter research modes - collaboration with other institutions, our role as an information agency, and conference direction - require a creative and flexible approach. The ramifications and details of these activities are not fully foreseen at present, hence their relatively minor role in this paper.

Yet, despite these qualifications, I must express my personal view that the in-house research is, of the four research forms, the most important. Without a solid core of top-flight researchers in Laxenburg carrying out significant

---

Prepared by the Office of the Director for consideration of the Council Meeting in November 1973.

* Restricted circulation.

systems research, the Institute will fall seriously short - in achievements, prestige, and internal morale - of the hopes set for it by its founders. Without a distinguished core of scholars, all the other proposed activities risk becoming inconsequential exercises: other organizations will not rush to cooperate with us; the expedition of information will collapse to a purely secretarial function; the conferences we organize will not attract first rate scientists.

The first prerequisite for the success of IIASA, therefore, is a healthy in-house research program. Part II of this paper will describe the plans for this program in detail. A brief description here will suffice.

We intend to bring first-rate systems scholars from many countries to Laxenburg to contribute to projects of applied and fundamental research. We want to seek inventive ways to link our projects together, as well as to arrange for individual contributions to the projects. Scientists, therefore, will be encouraged to contribute to more than one project.

We shall try to take advantage of the availability of distinguished systems scholars when the occasions arise and we shall, therefore, arrange appointments to IIASA for times ranging from one week to two years. We shall not, however, undertake projects led by men who can only manage fleeting visits to Laxenburg. Our projects will have varying time spans, but at any given moment the ongoing research should reflect the many different phases of the project's work.

We hope that the projects pursued will at all times cover a broad range of the research areas approved by the Council. It is our intention, also, that the projects should cooperate fully within their important areas of interaction, and that we should actively seek out unifying themes that will cut across project boundaries and engage joint interests. Yet the independence of the projects, of their starting points, and of their goals must also be recognized. We shall try not to overdo the unification of projects out of concern that they might all coalesce into one amorphous and inchoate mass. The phasing of the projects and of their personnel shall have the important aim of giving a sense of continuity to the in-house research program and, thereby, to the Institute as a whole.

B. Collaborative Research

During our research planning conferences this summer, the limitations upon our own resources became more and more evident as we perceived the magnitude of the many possibilities for scientific research. We must seek creatively, therefore, to draw heavily upon the resources and the contacts of our national member organizations in order to increase our research capacity.

We should also be imaginative and bold in seeking new ways to augment our resources by acting as a catalyst, as well as a coordinator, of research elsewhere. I will describe, later, some possible scenarios for this mode of activity, although I am aware that I may neglect arrangements that could prove to be highly successful.

We should experiment, review our efforts, expand those that bear fruit, and not fear to abandon those that fail. We should also bear in mind the cost of these activities in terms of the resources we have available. Even though our collaborative activities are designed to enhance our research program,they will absorb internal resources - mainly the time of the coordinating scientists. We should, therefore, not initiate collaboration that we cannot ably support and follow up adequately.

The possibilities for scientific collaboration between institutions, in different countries, are rich and, for the most part, neglected. A small number of promising arrangements are sketched out below:

i. IIASA in cooperation with other institutions on project tasks. Many projects appealing to IIASA require significant inputs of field work, data acquisition, laboratory research, or clinical experience. Such projects cannot be pursued in-house but might be pursued in partnership with institutions that can provide this type of input,and that would welcome IIASA's contribution of a multi-disciplinary, cross-cultural systems perspective.

ii. Broader institutional partnership with IIASA. It may be wise for IIASA to adopt close relationships with scientific institutions located in nations affiliated with the Institute. Possible examples are the Institute of Control Sciences in Moscow, and the Atomic Energy Commission of the U. S. A. Such relationships would feature periodic interchange of personnel, and joint research within the many topics of interest to both parties. If these institutions could help financially to support the scientists they would send to IIASA, it would effectively extend our budget.

iii. <u>Coordinating institutions from different nations.</u> Several institutions represented at our scientific planning meetings, this summer, voiced parallel interests. IIASA could play an important catalytic role in discovering common interests and in facilitating collaboration. Though the main linkage would be that of the two other institutions, IIASA could remain permanently in the background as a catalytic agent - perhaps ready to bring in other parties - occasionally encouraging inputs from its in-house staff.

C. Information Agency

The international exchange of scientific information presents problems that IIASA can help to eliminate. When embarking upon a major effort in systems analysis, it is often difficult in any one of the nations affiliated with IIASA to know what related efforts have been carried out elsewhere. One way to remedy this would be to establish, in Laxenburg, an international repository of information; yet the physical and monetary requirements for such an endeavor obviously renders it impossible. The alternative role of expediting the flow of information, however, lies well within our capacity.

This function might be played out within the following possible scenario: Scientists throughout the world could inquire about past or ongoing research within those specific areas that fall within IIASA's mandate. They might want to know what data bases, or unpublished mathematical models, or scientific software packages are available. Each of the project areas at IIASA could have at least one person able to answer questions of this nature, perhaps by mailing out a IIASA survey, perhaps by citing research references and indicating how they might be obtained, perhaps by referring the inquiry to a scientist peripherally acquainted with IIASA. In any case, assistance of this type would help scientists in many lands without significantly draining the resources of IIASA. If we are to maintain adequate coverage of the research areas prescribed by the Council, then we must keep ourselves informed of ongoing developments in those fields of research. The research planning meetings in the summer of 1973 contributed greatly to such knowledge; it will not sap our strength to share our overview with inquiring parties.

The problem of translation of languages naturally arises in this regard. IIASA would mainly provide English synopses of research, and translation into other languages would be the responsibility of the inquiring scientists. Establishment of even a minimal formal translation department is expensive, in terms of the needed physical resources. So far it has worked well to rely informally upon the linguistic abilities of junior and secretarial staff for needed translations; these people could not be put at the disposal of external parties.

Several delegates at our conferences expressed interest in strengthening a possible IIASA role as information-expediter through computer linkages. Such ties would greatly speed information-transfer and would facilitate our own acquisition of information. We could not maintain a large fund of computer-accessible information, either in a data bank, or in an extensive inventory of document references. However, we could, perhaps automatically, refer requests to other sources or to other computerized systems. There seems little doubt that computer linkages would be greatly beneficial to IIASA. Nevertheless, the costs and difficulties are also significant. The uncertainties involved are so great that the IIASA computer systems project will, in Part II below, propose a careful investigation of the possibilities.

D. Conference Direction

Our research planning meetings drew attention to many areas related to systems analysis but required investments of men, money, and special expertise beyond the scope of IIASA. Because of our fortunate position as the "crossroads of international scientific exchange", IIASA is well suited to identify areas of topical interest that can serve as themes for valuable conferences. The need for an organization to play such a role as this grows, each year, as the disciplinary overlaps of problem areas increase. While it was once sufficient to rely upon international scientific societies to organize needed conferences, this has become less feasible as important problems, increasingly, do not fall neatly within the purview of individual groups.

A major problem here will be monetary. Although it is my contention that many conferences would more than justify their expenses, those expenses soon will not be able to be supported from the IIASA internal budget. We have budgeted for a dozen conferences in 1974 but are able to do this only because we will lack a full complement of scientists next year. For conferences in later years, we shall have to explore other sources of funding.

Interconnections

The conceptual segmentation needed to present the four activity modes of our research program, as presented above, is somewhat artificial. Each of the four will interact with, and will support, the others. Thus, collaborative arrangements will often be a prerequisite complement for in-house research. The conferences will be an important way of initiating, or concluding, in-house research, and will assist the information expediters in keeping up with scientific events. Conferences will also be a tool used to link collaborating institutions. The effort needed to obtain an overview understanding of a research field will benefit all four proposed modes of activity. It is precisely because the four forms of research can nurture and strengthen each other, that they are proposed, together, to form the fabric of the IIASA research program.

2. Criteria for the Selection of Projects

There are so many research topics that could be profitably pursued, that hard choices will have to be made amongst attractive alternatives. Perhaps it is best, therefore, to specify, at the outset, some of the criteria that should govern our choice of particular research activities within the broad areas of research specified by the Council. Indeed, I believe the criteria listed below have governed the choice of the broad areas of research that have been selected thus far.

a. Traditional research in the sciences usually divides phenomena into separate parts that are analyzed, in depth, by different disciplinary groups. This procedure has led, I believe, to the fragmentation of modern science. Systems analysis points in the opposite direction: fragmented pieces are put together and the composed pieces, that is the system, is then considered. One consequence of that is that systems analysis is very often of a multi-disciplinary nature. In addition, the modifier, "applied", in our title "Applied Systems Analysis", connotes a desire to tie our research to real-world problems that involve not only an analytical phase, but an implementation phase as well; thus managerial aspects should not be ignored. Our first criterion, therefore, is that the research activity should be consonant with the basic philosophy of applied systems analysis.

b. The research activity should be either global or universal in nature, where these terms are used in the following senses:

i. (Global). Where the effects of policy and the control of policy resides separately, or jointly, in nations -- for example: activities involving the oceans, the atmosphere, continental river systems, and so on.

ii. (Universal). Where activities are separately controlled by each nation, but where the problems each nation faces have a methodologically, non-trivial, communality; for example: municipal services such as solid-waste management and hospital services; management of integrated industrial enterprises; and so on.

c. The activity should be considered of <u>importance</u> to our National Member Organizations (NMO's) and mankind in general. The activity should <u>not</u> be solely of interest to either market or non-market economies, but should transcend particular economic idealogies. Results of our research should, potentially, have <u>high impact</u> and be <u>practical</u>.

d. The activity should be <u>feasible</u> for IIASA to perform. This criterion should include considerations of scientific talent, data availability, computer services, finances, and so on. However, one should keep in mind that not all research activities must be done exclusively in-house; IIASA can do collaborative research, act as a sophisticated clearing-house, and convene Conferences of experts as well.

e. Non-redundancy of our activities with the activities of other national and international organizations is of major importance. There is too much to be done merely to duplicate or replicate the work of others. But let us also keep in mind that a good deal of our work will be done in collaboration with other institutions, and this cooperation will make it feasible for us to undertake certain activities. Also, many other institutions, while working on topics that are seemingly similar to ones we might select, may not concern themselves with an integrative systems analysis approach. IIASA should look for opportunities where good work is being done elsewhere in several places,but where the pieces need to be joined for an overall broad perspective.

f. Our research accomplishments should have a phased output. We cannot expect our supporting institutions to wait years for tangible outputs, nor should we only undertake superficial consulting-type activities where the payoffs are immediately obvious. We seek a mixture of projects with different horizons for our finished products, and which will balance the risks of successful completion. If all our projects prove to be successes, then probably we will feel, in retrospect, that we were not imaginative or bold enough in our aspirations.

g. We must choose activities within each of our projects that will contribute to a sense of cohesion and integration across projects. Wherever possible, we should examine analagous problems in different areas in order to facilitate technological and methodological transfer of knowledge. This cross-project integration will also serve to make our research staff more efficient, and help interdisciplinary communications. It will also add a sense of purpose and excitement to have people working on similar projects. One word of caution: very often it is the deep problems in a given area that are peculiar to that area, and we must avoid spending time on communalities, amongst areas, that are superficial. Balances, like everything else, must be struck.

h. IIASA can play a catalytic role by pursuing some, but not too time consuming, far-out ideas with small probabilities of very high payoff. By playing this role we might spur others to pursue topics of potentially high importance to mankind. We could undertake studies of the potential costs, benefits, and risks of pursuing certain studies -- "meta-studies" if you will.

i. In closing this list we reiterate our concern to do applied types of research -- research that is concerned with real and not imaginary academic problems, and where the possible results could be implemented and could influence policy decisions.

3. Flexibility

Throughout this paper, the need for flexibility in managing the research program is stressed. Instances of this are:

i. that the planning of each individual project is structured to take advantage of attractive opportunities as they arise;

ii. that the projects lacking leaders - such ad Biological and Medical Systems, or the Design and Management of Large Organizations - have not had specific lines of study imposed upon them, although suggested areas for investigation have been outlined;

iii. that provision is made for spinning off sub-projects centering upon themes (see Part III) that attract the interest of our researchers;

iv. that many of our proposals reflect considerable input from our research planning conferences of the summer; and

v. that the planning of our program has been decentralized to the project level.

We should be bold in selecting research goals, and creative in devising organizational structures for their attainment. But this is not enough. We should be equally bold in abandoning project efforts that neither achieve their initial objectives, nor realize alternative results commensurate with their consumption of our resources - our personnel, funds, and space. This requires that we adopt a constructive attitude, if you will, toward failure.

Failure

We have embarked upon an exciting and difficult undertaking. To exploit fully, our potential, we must take chances - in some of which we will fail. Were we to experience an unbroken string of successes, I would infer, as I wrote earlier, that we had not been innovative or aggressive enough in exploring the realm of possibility. Without local failures, our global achievements might be lessened.

We must learn, then, to regard our disappointments constructively. A project staff may perform admirably, and still fall short of its goals. This realization will enable us to evaluate better all our past efforts and I hope that a clear understanding of our failures will rank among our successes. Such a perspective will - additionally and perhaps more importantly - enable us to terminate or to redirect scientific efforts, with fewer subjective and self-serving evaluations, and with fewer recriminations than would otherwise accompany such actions.

Review Processes

A primary gain from a healthy attitude toward failure will then be greater speed, and less pain in rechanneling our resources and energy. If IIASA is to strike out in new directions, it is critical that we quickly identify activities that fall short of their promises and goals. It is recognized that we cannot expect instantaneous results from any endeavor and, furthermore, that precipitate and flighty termination of projects, before they have a fair chance to prove themselves, would adversely affect morale.

Nevertheless, we cannot afford to throw good money after bad in attempts to resurrect ill-fated and irremediable projects. To this end, informal review processes will be installed to monitor the progress of our scientific teams. Seminars will present the achievements of each project to other scientific staff members, and project leaders will report, periodically, upon their progress; this procedure has already begun.

When a project appears to be a borderline proposition between continued support and abandonment, or when a project seems to be surrounded by an informational vacuum, special steps will be taken to understand its achievements, circumstances, and prospects. In some situations I can imagine, we will do better to organize a formal external review process to support the ongoing informal processes, or to obviate the tensions they cause. What I envision is a group of, perhaps, three leading scientists from three diferrent countries who would have no personal stakes, or friendships, in the projects to be evaluated. They would assess the accomplishments to date, and would estimate the prospects for future returns from the project. Their advice would assist our policy decisions upon the level and direction of future support for the project. With such an arrangement, we should reduce - even though we cannot eliminate - the hazards of placing our own evaluators and decision-makers in conflict of interest positions where their objectivity might be held in question. Perhaps, in order to remove the negative connotations of being evaluated, external review processes should periodically be invoked for all long-term projects.

Neither a Blueprint Nor a Master-Plan

In sum, then, I am saying that these papers you now hold in your hands do not pruport to be a blueprint or a master-plan for our program in 1974. We have put down, in perhaps excessive detail, a statement as faithful as humanly possible to our present set of intentions. Yet none of us can know now the circumstances in which we will find ourselves, nor the opportunities that will arise, in three or six or nine months' time. The critical choices that will have to be made over the course of the coming year should be taken upon the basis of the best information available at the decision junctures, and not to conform to a necessarily myopic plan of action from the past. For this reason I will not treat this document as a rigid commitment. More important than the details spelled out here, I consider the philosophy underlying them. Although I have striven to be as honest in recording the former as in explaining the latter,it is the planned operational details that will prove most mutable. And yet I expect our research philosophy also to shift, unpredictably, in the next twelve months, as we learn and grow.

Though the details of our program will vary to take advantage of circumstances and experience, they will accord closely with the philosophy here described, and will heed the principles agreed to in the autumn meeting of the Council All major departures in policy will be approved in advance by the Executive Committee.

4. Integration of Project and Interdisciplinary Research

It is not easy to engage in productive interdisciplinary research and many cases can be cited where groups, that aspired to do such interdisciplinary work, fractionated along disciplinary lines, so that interaction took place only at superficial levels. However, there are examples of where sophisticated exchanges between scientists of different backgrounds have taken place, and we should learn from these experiences. We believe that one of the key ingredients for productive interdisciplinary interaction is that the individuals involved should want to contribute collectively to the solution of a pressing problem; it is the problem, then, that unifies the group. We hope at IIASA that there will be many such unifying problems, and themes, which will draw our scientists together.

Each of the applied projects such as energy, water ecology, biological and medical systems, and integrated industrial systems will not only draw on the expertise of natural and physical scientists, but will need the inputs of applied mathematicians, computer scientists, behavioral and management scientists, and policy analysts as well.

In addition, we have chosen the above mentioned applied projects because of their obvious overlapping concerns and, in Part III of this report, we have tried to identify those methodological themes that are common to many of them. While we shall not work extensively on each of these themes, they are designed to cut across the projects and, hopefully, they will facilitate the interaction we seek.

We want to invite to IIASA those scholars who have demonstrated by their past accomplishments that they can work productively, in an interdisciplinary manner, on applied problems. It is too risky to invite to IIASA mature scholars who say that they want to work on such interdisciplinary projects, but who have never done so in their professional careers. Also, by getting mathematicians who have worked with behavioral scientists, and behavioral scientists who have worked with mathematicians or mathematical models, we will not have to spend man-years of time on the acculturation process.

It is important at IIASA that we not only have a mixture of disciplines (e.g. applied mathematicians, computer scientists, economists, sociologists, and so on) but that we should draw our research scholars from academic institutions, research laboratories, government agencies, and industrial firms. We need a healthy mix of backgrounds so that, collectively, we can take a broad look at the applied problems which concern us.

It is often said facetiously, around IIASA, that we have three working languages: English, mathematics, and FORTRAN. This means, in part, that the scholars from the behavioral and management sciences must be able to meet the mathematicians and computer systems programmers half-way. We will not, at IIASA, give remedial elementary mathematics courses to our non-mathematicians. A certain basic understanding of the mathematical language, therefore, must be a prerequisite for all.

There is an inherent danger, in Institutions such as ours, that the things we learn will not be communicated effectively to the outside. It is easy to write scholarly scientific papers that solve a nicely posed mathematical problem; it is not so easy to transfer, to others, attitudes, approaches, and those artistic tricks that experienced, skillful analysts employ in tackling real problems. We shall pay attention to this problem and, through internal seminars and preparation of materials, we hope to experiment with effective ways of transfering information and that elusive "know-how".

5. Introduction to Subsequent Sections

The sections below have been prepared by a number of research leaders at IIASA. We have decentralized the writing to accord with the eventual delegation of duties within the projects below. Our general principle is to decentralize as many tasks as possible, subject to the essential constraint of maintaining internal cohesion and an esprit de corps. In this instance, the shortness of time between the conclusion of our research planning meetings and the Council meeting necessitated enlisting the help of others. I hope that you will bear with the differences in style, the occasional redundancies, and possible inconsistencies; and that you will find in the end that the program descriptions are the richer for having been put down by many pens.

PROPOSED RESEARCH PROGRAM

for

ENERGY SYSTEMS

I.) The study project on energy systems as part of IIASA activities.

The study project on energy systems is a systems analysis project. It is focused on the systems aspect of energy problems. During the Research Area Meeting on Energy Systems it was attempted to identify such system problems in greater detail. Largely this means that not the sum of the components but the structure of the problem as a whole must be understood.

Traditionally this is done by working oneself through all of the details and to recognize only at the very end what the structure of the problem as a whole comes out to be. One may call this a fragmented approach.

This is not the approach for IIASA, because, among other reasons, also a very large manpower is required for such a fragmented approach.

A systems analysis approach instead tries to identify and analyze the structure and the related system problems as such. The result of such an effort could be the following:

- The structure of the problem and the various implications are early understood
- a list of discipline oriented scientific and technical questions has been articulated and explained for discipline oriented (and usually large) existing research groups with a view to take care of these questions and thereby system problems early.

The very nature of the problem necessitates therefore a constant liaison with such discipline oriented research groups and this liaison is therefore an integral part of the approach at IIASA.

Having such an approach in mind also the work within the systems project must be integrating and not fragmenting. It is therefore intended to make the writing of scenarios such an integrating factor or a clamp. A thing that can be accomplished comparatively quickly is a zero order description of a long range future energy supply for a stable, highly populated and industrialized globe.

Nuclear fission is one, albeit a very promising, among the following four options:

nuclear fission
nuclear fusion
solar power
geothermal resources.

More effort is required if the various transition stages are to be assessed and described. The nearest problem for that is the substitution of oil by synthetic hydrocarbons.

Gradually the scenario writing effort is expected to evolve and to take up the problem of reactor siting in a large scale nuclear economy.

The scenario writing approach and later the problem of reactor siting allows from the very beginning for a strong interweaving with other study projects. There are in particular the following four/five projects that form a project cluster:

- water systems
- municipal systems
- energy systems
- ecology systems

and possibly:

- technical and environmental standards.

One may look at this cluster as a system of a higher order. By such an approach it will also be possible to properly incorporate sociological and management aspects as energy will be used by someone, mostly the inhabitants of conurbations, and energy will be produced and administered by someone, mostly the utilities of such conurbations.

During the Research Area Meeting on Energy Systems the term "embedding" came out as a key for the understanding of energy problems as system problems.

If the above mentioned cluster of problems is pursued as a system of a higher order, the embedding of energy can be more readily identified and is less abstract.

In the following an outline of more specific tasks of the energy study project will be given. This should be understood as an amplification of the approach given above and not as a traditional discipline oriented approach to systems.

II.) List of specific system components.

A) Availability of natural resources for energy production

a) Mineral resources

b) Geothermal resources

c) Renewable resources

B) Energy modelling

a) Analytical comparison of energy sector models

b) Computerized information retrieval for energy sector models

c) International market models

d) Data bases

e) Uncertainty and time discounting

f) Energy demands and conservation

C) Climate and Water

a) The impact of waste heat

b) Hydrological consequences of a changed pattern of the rain cycle

c) The energy balance of the atmosphere

d) Solar power and changes of the albedo

D) Synthetic Hydrocarbon fuels

a) Technological review

b) Energy modelling for R + D priorities

c) Secondary energy partitions

d) Transition into hydrogen

e) Energy transport

f) Reactor strategies

E) Reactor siting in a large scale nuclear economy

a) Zero radioactivity release

b) The separation of Pu from the ecosphere

c) Waste disposal

d) Transports

e) Physical protection

F) Risk evaluation

a) ICRP and other literature studies

b) More specified view points

c) Quasi laws

d) Literature research on life values

e) Cultural differences

f) Perception of technological risks

G) Accountability

a) Monography on accountability in nuclear material safeguards

b) Generalized applications of accountability

H) Standards and Public Acceptance

a) Standards for a given toxin

b) Comparison of standards for toxins

c) Relating standards and risks

III.) An outline on the specific systems components.

In the following an outline on the specific systems components will be given. It must be emphasized here that this outline will be a brief one. A more detailed outline is at hand with the IIASA Proceedings on the Research Area Meeting on Energy Systems. To that extent these Proceedings should be considered as background part of this Program.

A) Availability of natural resources for energy production

a) Mineral resources

There already exists a large literature on both fossil and nuclear resources for energy production. It is necessary to review this literature analytically, and to identify the assumptions underlying these estimates. To the extent possible, the estimates from individual countries must be made comparable with each other.

It is proposed that a clear distinction be drawn between the following alternative types of forecasts:

(i) supply-oriented (those based upon geological analogies)
(ii) demand-oriented (those that assume that the availability of resources is a question of price only)
(iii) extrapolation of exploitation histories

For systems analyses, it is not enough that the available quantities of minerals be identified by their physical properties such as grade, depth, and gas pressure. It is also important that these physical characteristics be translated into economic terms. The end product would be a curve indicating the incremental costs of production as a function of the cumulative production. Without such curves, it is exceedingly difficult to evaluate the benefits for instance of research and development programs for breeder reactors, coal gasification, shale oil, etc.

b) Geothermal resources

In the case of geothermal energy, there are major discrepancies between the alternative estimates of availability. In part, these differences arise from the failure to distinguish between:

hot steam and water
the surface heat of the ocean
heat content of the earth's crust.

Just as in the case of mineral resources, it will be important to estimate the cost of production as a function of the cumulative production.

c) Renewable resources

Solar power is in most cases the source for fuels that are renewable. Wood, farm waste, photosynthesis fuel and wind power are examples for that. While it is certain that such renewable resources are too small to provide an energy supply of more than a few tenths of a Q/a ($Q \equiv 10^{18}$ BTU) such resources could well be of a regional significance. It is therefore proposed that these resources be evaluated along the same lines as in a) and b).

B) Energy modelling

a) Analytical comparison of energy sector models

A comparison will be made of a larger number of models built during the past 5 years. The comparison will include the pros and cons of alternative analytical techniques. It will also include a description of the type of fuels, the data base, temporal and spatial disaggregation and interdependencies between supply and demand. Within the US alone, 94 studies are reported in the state-of-art review of Limaye, et.al.(1972). It is proposed therefore that for an analytical comparison, there be a limit of 5 studies per country. The comparison might take the form of a journal-length article.

b) Computerized information retrieval for energy sector models

Some 500-1000 energy sector models have been built throughout the world in recent years. It is proposed that we construct a system of key-words or a standardized format for describing these models (fuel types, geographical structures, modelling techniques, etc.). This information would be maintained up to date at IIASA, and would be periodically distributed. Further work will be needed in order to ensure that this work is complementary to that being undertaken by other institutions. If successful in the energy area, this project could serve as a pilot study for information retrieval in water resources, ecology and other IIASA programs.

c) International market models

In the UK and France, models that cover a major part of the world energy market have been developed. Attention should be given to the question whether a follow-up by IIASA is appropriate. In particular, the IAEA recently completed a first version of a world market study for the introduction of nuclear power, especially in developing countries. A further thourough analysis and a constant up-grading of that study has been suggested within the IAEA. If a joint venture IAEA/IIASA materializes, IIASA would contribute to develop this market study further.

The above mentioned comparison of energy sector models might be incorporated in such a follow-up study.

d) Data Bases

It seems to be clear that the large scale acquisition of data particularly on demand and supply of energy cannot be a task for IIASA alone. However, a certain amount of data collection and compiling will be necessary. The degree of aggregation and the various accounting conventions will be a major focus.

e) Uncertainty and time discounting

Models are intended to facilitate specific decision problems, not to replicate the real world inside a computer. Inevitably there is uncertainty on the many future consequences of current decisions. Time discounting, however, makes the distant future have only a small impact upon the costs and benefits of current decisions. Koopmans and Manne have already begun work along these lines, and part of this material is in the process of being written up. Some of this relates specifically to the energy sector ("Waiting for the breeder"). Other items have a broader application. "Linear complementary methods for the computation of an invariant capital stock" is an economy-wide model. Specialized forms of this idea may be applicable to sectorial planning, e.g. through an improved rationale for amortization accounting methods.

f) Energy demands and conservation

If energy resources become depleted at a more rapid rate than they can be replaced through advances in supply technologies, it will become necessary to modify our life-styles accordingly. The modification may take the form of well-known technologies (e.g. stricter standards for heat insulation in homes, factories and offices). Over the long run, energy conservation may lead to radical changes in urban design. E.g. Dantzig's proposal for Compact City would economize on petroleum fuels for both heating and automobile transportation.

One way to implement conservation would be to increase the price paid by fuel consumers. For analyzing price responses, there are competing analytical techniques available. It seems likely that econometric and statistical trend methods may provide the most reliable basis for near-term forecasts (10-15 years in the future), but that regional planning and process analysis of new technologies as well as life style description may be needed for long-range forecasting purposes.

C) Climate and Water

As pointed out during the Research Area Meeting on Energy Systems, there exist interactions between energy, water and the climate. These must be better understood if energy is to be used at a large scale. An effort will be made to identify and analyze these interactions. Principal partners could be, among others, the British Meteorological Office near London, the Center of Atmospheric Research in Boulder, Colorado and the Hydrometeorological Service, Moscow. There are the following subtasks:

a) The impact of waste heat

Such impact has to be studied in stages. The most probable impact of waste heat is on the pattern of hydrological/rain cycle. The next stage of impact is on regional weather and climate instabilities. Only the third stage would be concerned with global changes of the climate. Such impacts arise from point sources (e.g. power plants) where large scale conversion of energy is taking place as well as area sources (industrial regions). It is of interest to identify areas on the globe that are relatively sensitive or insensitive to such releases of waste heat, and further, to identify the magnitude where such sensitivity begins to be felt. The question of placing large scale complexes for the production of secondary energy is of particular relevance here. It was suggested that these impacts could turn out to be the ultimate limitation on the production and handling of energy on sites other than in the oceans.

b) Hydrological consequences of a changed pattern of the rain cycle

The rain cycle pattern has many consequences for industry, agriculture, municipal systems and land use. A changed pattern of the rain cycle may therefore pose early limits upon the production and handling of energy. It is therefore necessary to identify these possible consequences. This leads to close links with the IIASA projects that form the above mentioned cluster of projects: water, towns and ecology.

c) The energy balance of the atmosphere

The energy exchange mechanisms in the atmosphere are completely balanced, all incoming visible light energy is ultimately radiated away into outer space in the infrared. Possibly, the disposal of waste energy could be facilitated if energy exchange mechanisms in the infrared could be identified that allow for a more direct passage of infrared radiation through the atmosphere. These are specific and detailed physics questions, but they require a systems analytical specification.

d) Solar power and changes of the albedo

The large scale harvesting of solar power is based on a change of the energy balance of the atmosphere. This relation must be studied in greater detail. The net effect of harvesting solar power can be a change of the albedo. Changes of albedo that are connected with the urbanization and utilization of large areas on the continents are also of interest.

D) Synthetic hydrocarbon fuels

It is generally believed that the world's petroleum and natural gas resources will become significantly depleted within the coming 20-40 years. Increasingly, it will become necessary to replace these raw materials with synthetic fuels. Research efforts are being directed toward modern methods of coal utilization: liquefaction, gasification and solvent refining. In addition to these technologies the use of tar sands and shale oil will also be investigated. If these resources are exploited and executed at a large scale, the side effects of such activities will have to be identified. The handling of large amounts of debris and waste and the problem of energy transport have to be included in a systems analysis investigation. From the deliberations of the Research Area Meeting on Energy Systems it can be seen that these synthetic hydrocarbon fuels cannot supply sufficient energy over the long run. This suggests nuclear energy as a source for chemical processing heat in order to stretch out the uses of fossil fuel. Connected to that is the large scale supply of hydrogen as a chemical and later the eventual introduction of hydrogen as a secondary fuel. For reducing the dependence upon fossil and especially oil resources hydrogen can gradually be used as a fuel for motor vehicles and air-planes. Hydrogen has the advantage of being a virtually non-polluting secondary fuel, similar to electricity. Its production can again be coupled to nuclear energy as a primary source of energy. From these remarks it becomes obvious that the timing of these technologies must be a point of special attention.

There are the following subtasks:

a) Technological review

A technological review must be made for the various options to use coal, shale oil and tar sands.

b) Energy modelling for R+D priorities

By employing the techniques described in the chapter on energy modelling, technical priorities should be evaluated. The capital investments and R+D requirements will be identified.

c) Secondary energy partitions

With the same techniques the economical consequences of changing the partition between the electrical form of energy and non-electrical forms of energy will be studied.

d) Transition to a hydrogen economy

This study will focus upon the implications of a gradual transition from hydrocarbon based fuels to synthetic fuels using hydrogen and finally to pure hydrogen.

e) Energy transport

In this context, the problem of energy transport will be evaluated. Particularly in the case of electricity and to a lesser extent for non-electrical fuels the transport over truly large distances will be reexamined.

f) Reactor strategies

The connections of the production of hydrocarbons and/or hydrogen to nuclear energy leads to the problem of reactor strategies. Up to the present the build-up of reactor populations, the interaction of the fuel cycles of the various reactor types and fuel cycle facilities has been studied, taking into account only electricity generation. It is necessary to reconsider these reactor strategies. Reactor and fuel cycle strategies will be re-evaluated therefore with the prospect of providing primary energy for other uses than the production of electricity. This again may be of particular interest if the above mentioned cooperation between IAEA and IIASA materializes.

E) <u>Reactor siting in a large scale nuclear economy</u>

This problem is expected to evolve from scenario writing and to serve as a clamp for the study project. But even so it has a number of aspects that must be studied in line with the other tasks of the program. These are the following:

a) Zero radioactivity release

It is necessary to specify quantitatively the degree to which radioactivity must be isolated within nuclear facilities. A special case are the reprocessing plants. The timing of this problem is particularly important. Some releases are of legitimate concern within the next years, others only within the next decades, some possibly after a century or more.

b) The separation of plutonium from the ecosphere

This is a special but major problem and therefore requires special attention. The long half life of Pu leads into methodological problems of dealing with time periods that exceed any human experience.

c) Waste disposal

This problem is connected to that of a) and b). Thus far, the treatment of this problem has been along the lines of traditional engineering. In view of the required reliabilities and the long time spans involved, an attempt must be made to at least identify if not analyze the methodological problems that are involved.

d) Transports

If nuclear power is to be produced in large complexes the transport of irradiated and other nuclear material will be largely reduced or eliminated. Instead, it will be necessary to transport energy in secondary form (electricity or hydrogen). Further, the pattern of land use and the design of large industrialized areas, or in other words, the problem of centralization versus decentralization is connected to the problem of transports. The systems implications of transport in the context of sector siting shall be identified and analyzed.

e) Physical protection

The protection of sensitive parts of nuclear installations against acts of sabotage leads to methodological problems. The systems analytical aspects will be identified.

The evaluation of nuclear installation siting will be more effective if the above mentioned cooperation between the IAEA and IIASA comes into effect.

F) Risk evaluation

During the Research Area Meeting on Energy Systems the task of risk evaluation was formulated and put into perspective. In the foreground of the attention are biomedical risks and their quantification for the purpose of computing risk/benefit ratios.

This line of research is relatively new. The following subtasks can be identified:

a) ICRP and other literature studies

A good starting point could be the publications of the International Commission on Radiological Protection (ICRP). In these publications, there is constant reference to the evaluation of risk/benefit ratios. Such a step is in principle required for the establishment of standards. This would be followed by an exhaustive survey of the literature in many disciplines and the establishment of contacts with other researchers with common interests. Many disciplines are involved and only little or no contact exists between these disciplines.

b) More specific viewpoints

Points to be considered in risk evaluation studies include:

- components of risk, e.g. frequency and magnitude, self-inflicted or common
- the effects of rare but large catastrophes
- life values
- the relationships between risk and expected benefits for both individuals and social groups (i.e. how much benefit is required to change the perception of a given risk from "unacceptable" to "acceptable")
- the study of historical precedents in risk acceptance.

c) Quasi laws

Ch.Starr has suggested the existence of quasi laws that relate acceptance, benefits and risks. A further study of these laws seems appropriate and could be readily combined with the initial literature survey and a data gathering effort.

d) Literature research on life values

A large body of literature exists on life values (ranging from studies concerned with the present value of future earning to the results of self insurance questionnaires).

e) Cultural differences

The examination of cultural differences in risk acceptance would also be both profitable and interesting. A cursory comparison of accident statistics from Austria and the United States has recently revealed differences in social response which seem culturally dependent.

f) Perception of technological risks

Finally, work should be started on adapting techniques used for quantifying the perception of personal risks to gathering new information on the perception of technological risk and on quantifying the relative importance of specific factors influencing the perception of a particular risk.

Again, this line of research would be most appropriate for a joint project of IAEA and IIASA.

G) Accountability

a) Monography on accountability and nuclear material safeguards

An effort must be made for the analysis and unified representation of the decision theoretical work performed in the case of the safeguards accountability. Two main aspects (material balances and verification by random sampling) are of general mathematical interest. They may possibly be applied to further and quite different control problems.

The essential parts of such a work would be

(1) Principle of material balance
Measurement errors and random losses. Comparison of book and physical inventories. Significance thresholds. Sequence of inventory periods. Problem of starting inventories. False alarm rates.

(2) Verification of the material balance by means of independent measurements. Random sampling. Strategies of inspection authority and of plant operator. Saddlepoints.

(3) Combination of both the foregoing aspects. Different categories of strategies. Guaranteed probability of detection as a function of false alarm rates, potential diversion, effort.

(4) Remarks on sequential decision theoretical problems in this field.

(5) Applications.

This work should be done in collaboration with members of the Institute who are interested in methodological research, especially in the field of game and decision theory.

b) Generalized applications of accountability

One method of handling the embedding of energy is accountability of materials and in particular of pollutants. There already exists an operative global accountability system. This is the IAEA nuclear material safeguard system whose design was accomplished by a considerable systems analysis effort.

An attempt shall be made to benefit from this experience, and to consider the material flow of substances (SO_2, NO_x, particulates, possibly CO_2 and others) that are connected with the production and handling of energy.

This attempt is to be made in collaboration with the ecology project of IIASA.

H) Standards and public acceptance

More than ever it has become clear that virtually all technologies are accompanied by toxic effects of widely different characteristics and/or by risks. If these technologies are introduced on a large scale, these toxic effects and risks can change from secondary order effects to first order effects and can thus become the overriding concern. Typically this is handled by the establishment of standards.

This leads to the following subaspects:

a) Standards for a given toxicity

The establishment of standards for a given toxicity has a scientific side and the side of public acceptance in line of what has been said above on benefit/risk ratios. As mentioned before, the deliberations of the International Commission on Radiological Protection are a prominent example for that. While the scientific side of the problem must be dealt with by existing traditional scientific methods, it is the other side of the problem that requires a systems analytical treatment.

b) Comparison of toxicity standards

It is necessary to compare and cumulate in systems analytical evaluations the toxic effects of widely different characteristics. This leads into the methodological problem of comparing "apples and oranges". Work is required here!

c) Relating standards and risks

Not only widely different toxics but also toxics and risks have to be interrelated. This amplifies the above mentioned methodological problem.

Standards and Public Acceptance probably come out to be one of the first major themes that go through all of IIASA's activities. It remains to be seen whether this has also organizational consequences. In any event this task leads to a heavy interweaving with water, towns and ecology.

IV) Priorities, timing, conferences, manpower.

As outlined in the first paragraph it will be scenario writing that couples the energy system study together.

This project approach implies that not all of the above outlined system components can be attacked in parallel, the manpower that is available is too small for that, and a parallel approach has inherently the danger that one may fall back to a traditional discipline oriented working style. Therefore, in the more immediate future, that is 1974, emphasis will be given to the following tasks:

Energy modelling, climate and water,
standards and public acceptance.

But to the extent necessary for the writing of scenarios also all other system components will be considered. During the year of 1974 the following tasks will be phased in:

Natural resources,
synthetic hydrocarbon fuels.

The task of accountability requires little manpower and will not be a major line of attack and the task of risk evaluation probably needs a careful start and is a more long range venture.

It is expected that scenario writing which functions as a clamp for the study project will evolve into the siting problem of reactors and similar installations. It is the intention that all studies have at least first order results not later than by the end of 1975.

The required manpower can be divided into three categories. The category "Long term appointees" may refer to scientists who are at IIASA for a year or more. The category "Short term appointees, consultants" may refer to senior scientists who come for a few weeks or senior consultants who come for shorter but possibly more frequent visits. And the category "Juniors, assistants" may refer to junior scientists who work with IIASA either on a part time basis or any other arrangement. The figures that are given in the table refer only to 1974.

During 1974 one major conference is envisaged. This conference should touch on all of the tasks of the program but with one particular problem as a focus. One or two smaller conferences with more limited participation should also be envisaged. One of these more specialized topics could be risk evaluation. Modelling could be the other topic. It must be realized, however, that it is too early to take a firm commitment on the details of these conferences.

The required manpower in manyears is given below:

Long term appointees	10
Short term appointees, consultants	3
Juniors, assistants	8

PROPOSED RESEARCH PROGRAM

for

WATER RESOURCES

1. Introduction

It is useful to divide our problems into categories; the human mind finds it much easier to attack those problems which are organized into some sort of structure. But it is not clear how the divisions should be made in any given problem. For example, in water-resource planning, we might classify problems according to their parent discipline--e.g. hydrology, hydraulics, environmental sciences, mathematical programming, stochastic processes, optimization, economics, political institutions, etc. This is a very appealing scheme because it is one with which we are familiar and comfortable; it is the one which governs the way books are stored in the library, and the way most universities departmentalize the professors and their budgets. But it does not encourage interdisciplinary activities which cut across traditional lines, and consequently, because IIASA is not a university interested primarily in extending knowledge about various phenomenological concepts, we offer another (orthogonal) proposal for classification.

Many studies of the allocation of scarce goods or resources can be layered into three hierarchical tiers. At the bottom, on level I, we have problems of short-term resource management. It is the operational level at which the technical and economic environment as well as the structure of demand and the value of standards are exogenously determined or given de facto. Typical problems in the respective applied project fields are, for example:

- how to store, distribute and control waterflows within an existing river basin to meet a given demand vector under technical and economic constraints represented by the existing facilities and economic conditions (Water Resources);
- how to manage pollution problems in a given environment, with given pollutants and given pollution standards (Ecology);
- how to manage a given city with its existing industrial, social, cultural and traffic structure and its present population (Municipal Systems);
- how to use and possibly optimize the existing production capacities of a given energy system operating mainly with fossil fuels (Energy Systems).

In a water resource context, this would include work in distribution, storage and control of water systems. For example, basin-wide or even nation-wide studies fall into this category. They would include the use of large programming models, system simulation, synthetic inflow traces, variable economic parameters, and constraints on system performance and water quality with the understanding that they generally be introduced as exogenous variables. We could concentrate on agricultural needs imposed by specific crops, industrial needs imposed by specific processes, and recreational needs imposed by specific populations. Interchanges with our Municipal, Ecology and Energy Projects would be encouraged.

Our work will include a major effort in water resource modelling at the basin level. We propose to construct a control-oriented model which will focus on the operation of existing facilities, with exogenous demands, supplies, standards, operating rules and economic parameters. We will experiment with systematic analysis to identify those variables to which multi-dimensional system response is sensitive. We will calibrate this work by applying the model to basins for which detailed studies already exist. The detailed proposal for such a model is offered in Section 2, all of which is devoted to this model, because this large effort is the direction in which our work will be initiated. Methodological inputs would include control theory, optimization techniques, simulation, and mathematical programming.

1.1 Our work will attempt to model as closely as possible real-world conditions and constraints. On the other hand, we recognize that large and cumbersome models are not always useful at all levels of inquiry.
We propose to emphasize the highest form of systems analysis--paring the problem to its essential elements, those to which the solution is most sensitive, and investigating the remaining relationships by simpler, perhaps analytical, means. We plan to encourage the development of powerful models whose solutions show the consequences of major technologic,

economic and institutional influences, hoping that these solutions might guide the development of more appropriate detailed planning and design models. These are known as Didactic Models.

While much remains to be done in perfecting allocation models, most work is now directed at flood and drought events, at stochastic models which adequately reproduce the low probabilities associated with extrema, at generating schemes for superimposing these extrema on the system, and at routing models for moving the flows (and their suspended and dissolved pollutants) through the hydraulic system.

Water quality will be introduced in several ways. First, we will consider the role of the polluter. (1) What treatment technology is available, at what price, and at what level of reliability? (2) What are the prospects for substantial change in this technology in the near and distant future? (3) What are the social consequences of rigid standards? Will there be significant unemployment if firms elect to shut down rather than adhere to the standards? (4) Are options for storage and dilution available? (5) Are process changes feasible?

Second, we will consider the prospects for reducing pollution by imposing standards on the ambient flows rather than the various effluents.

Third, we will adopt (i) the matrix methods of Thomann for routing degradable pollutants (particularly nitrogenous and carbonaceous BOD) through major rivers, (ii) other widely accepted forms of mixing and dispersion for routing conservative pollutants through systems, (iii) special functions for routing radioactive wastes (including the problems introduced by scour, deposition in benthic deposits, etc.), and (iv) special functions to accommodate thermal budgets.

Fourth, we will experiment with large-scale alternative methods and placement of treatment, of non-treatment (bypass), and of demand reduction.

All of these circumstances and cases will be represented by functional forms and modules which are subject to precipitous, not merely incremental, change. We do not propose merely to optimize the system by inquiring after the consequences of marginal changes; we welcome widely-based challenge to our traditional notions.

Level II includes regional or perhaps continental problems, or problems whose resolution demands a set of policies and institutional interchanges for specifying large-scale investments. Here we must deal with political infrastructure, persistent regional pollution, large-scale social and psychological issues (e.g., population dynamics and control), migration and urbanization, water and waste-water facilities as instruments of national or regional policy, negotiations among various economically powerful vested interests, and problems of incentive, coalition and coercion on a national scale.

On this level, which may be called the Investment or Restructuring Level, the question is how to restructure the present resource-management system in order to anticipate reconcile, solve and alleviate some of the future tensions, inconsistencies and potential conflicts resulting from autonomous development which can be extrapolated from present trends in Water Resources Management, Energy Systems, Ecological Systems, Urban Systems, etc. This level is perhaps the most important policy-making level, having the closest and most sophisticated interconnections in all of the fields enumerated. Here again the questions in all project areas are very similar, e.g.:

- what kind of investments are needed to create water systems, reservoirs, sluices, dams, treatment facilities, etc. to meet a future changing and stochastic demand, subject to quality standards resulting from interaction of economic, institutional, social factors (Water Resources);
- what kind of investments and policy measures are needed to regulate the population flow; creating new urban/suburban centres, resuscitate old ones, etc. (Municipal Systems);
- what kind of ecological policies, embodied in investment projects, can be made to reach a new or resuscitate an old ecological equilibrium (Ecology);
- what kind of energy producing technologies should be chosen and invested in to change the present energy structure to be consistent with the future ecological, environmental, etc. requirements (Energy).

In effect, we admit three classifications of "environment," and models on level II deal with changes in these environments. These include investments to change i) resource availability,

ii) water quality, and iii) political and institutional aspects.

For resource availability we will study combined preliminary-planning-design models which show the connection between initial approximations to optimal design and final studies, usually by simulation of dynamic, stochastic, risky systems. Initially we do not envisage a major effort in this area.

1.2 For water quality, one possible approach is The Environmental Zoo. (Memo of 30 August, Fiering to Raiffa)

1.3 In addition, we propose to study lake eutrophication.

The subject of environmental standards is an Integrating Theme for IIASA projects and is described in another portion of this prospectus.

1.4 With respect to political and institutional aspects, we propose to utilize our competetive advantage based on an international environment and a large staff of experts in management. We could develop a systematic diagnostic procedure for determining who makes the decisions, and under what forms of pressure and influence, in various countries. Such a project would be useful to agencies who wish to plan a project, and to organizations who wish to block one. It is eminently capable of generalization, and developing communities might learn much from the catalogued experience of other countries.

On level III we introduce problems of a world-wide (or global) scale. Here we must be concerned with inventories over the entire world, with population growth and trends, with technologic projections and extrapolations of supply and demand as they are determined thereby, and with interchanges among other sectors of the economy and among users of renewable and (virtually) unlimited resources. We must accept massive amounts of aggregation, statistical consolidation and description, gross specification of standards, and, most importantly, the limitation of our inquiry to those control variables which reflect major investment policies of an international scale.

Here on the level of global aspects, we must deal with long term, world-wide problems. These must be approached by very aggregated models, using statistical and eventually simulation methods. All the long-term global and regional supply and demand forecastings, extrapolations etc. belong here. For example, these include the water resources field, the energy field, a long-term overall population flow models with given gravity points of the Municipal Field, and projected long-term trends in Ecology. In other words this level assumes the continuation of the present autonomous development in the various fields investigated. The research done on this level will detect significant tensions and inconsistencies among the regions and among the fields of Water Resources, Energy, Ecology and Urban Systems.

1.5 It is estimated by Colas ("Producing Fresh Water from the Sea," L'Eau, 49, 1962) that there are some $1.3x10^9$ km^3 of water in the oceans and seas, and an additional $27x10^6$ km^3 in the polar ice caps. Of the total fresh water resource,

6.9×10^5 km^3, some 3% or 2.2×10^4 km^3 is in the atmosphere while the remainder is split about equally between surface and subsurface storage (with each accounting for some 3.3×10^5 km^3). One can continue to parse these figures into finer and finer subdivisions but the more interesting statistic is that about 4.5×10^5 km^3 falls annually as rain and snow, with about 25% on the land and 75% on the seas, so that the average detention time is (6.6/4.5/4)= about 6 years. In the United States, current estimates suggest that consumptive losses account for about 8% of the precipitation. This suggests that one problem with regard to water is its maldistribution in time and space; a small portion of the waters which reach the land is re-used many times, so that a second problem is the continuing and often cumulative degradation of water quality. IIASA should study both of these. It is naive to think that we are running out of water in a gross way. But redistribution in time and space requires enormous capital investments and subsequent utilization of energy which, inevitably, increases the environmental pollution burden. Thus water utilization is inextricably bound up in problems of a global scale, intersecting energy, ecosystems, thermal budgets (and their consequences with regard to transportation and development along coastal districts), etc. A suitable analysis of these intersections and interfaces requires a systems-oriented study, which is not described in our current budget but may become a future project.

1.6 Finally, we will immediately undertake retrospective studies to guide our research and to help calibrate our models at all stages of their development. We propose to deal with three basins: Vistula (or Tisza), Delaware (or Potomac) and Trent.

2. LSS Model of Level I

2.1 General

The complex use of water resources is quite a difficult problem of control of the Large Scale System (LSS) and is the subject of the Systems Analysis (SA) application.

The most important aspects of the problem are:

1) hydrological - identification of the region S of the watershed, distribution of sources and sewers, estimation and prediction of water resources under usual or normal meteorological conditions and under extrema (floods, droughts, etc.), and the hydrological potential of their control;
2) hydrodynamical - study of stream-bed dynamics, of wave propagation and of those transport and diffusion processes which govern the movement of sewage and waste materials;
3) geographical - identification of water users, location of cities and towns, structure of population and dynamics of its growth and migration, location of industry, power stations, agriculture and transport systems in S, identification of the

parts of the region S available for regional developments, use of water resources as a lever for control of this development;

4) economical - identification of features of regional economic development and current and projected water consumption by all users, and the consequences for proposed cities and towns with regard to water consumption;
5) ecological - formulation of requirements for ecological equilibrium in S, which is necessary for society or for programmed migration to new equilibria;
6) technological - definition of the main features of those additional investments which might be made for water resources control purposes, i.e. for the construction of new buildings, control reservoirs, communication lines, irrigation channels, pipe lines, pumping stations, water treatment plants, etc.;
7) social - investigation of the economic and social interests of water users, formulation of criteria for computing social costs and benefits, estimation of potential influence on the life of human communities and on natural habitats;
8) constitutional - identification of the basic structures of legislation (national and international) for dealing with multi-purpose water use, operational management, responsibility, monitoring, weather modification, environmental standards, and supervision of ecological equilibrium.

2.2 Main Goal of the Project

The main goal of the project's research is:

- development of a better scientific understanding of the problem of water resource allocation;

- development of the systems analysis methods which allow us to take into consideration all the above-mentioned aspects of the problem and provide decision makers with a better basis for planning and day-to-day operational control policy of large scale water resources to satisfy human needs and demands under conditions of ecological equilibrium;

- application of the methods to particular examples in order to demonstrate their effectiveness.

Demands will be established by a variety of institutional, political and priority-oriented factors. In addition to these exogenous demands a range of economic considerations will be imposed so that allocations can be made among competing users. This suggests the use of vector-valued optimization analysis, game theory, and related techniques for deciding upon optimal distribution of the total resource.

All uses will be subject to scrutiny, and to such constraints as are indicated, for maintenance of water quality. A variety of standards will be imposed (if they are available) and the developmental consequences will be elaborated. We hope to learn, for example, whether certain levels of water quality simply cannot be attained while prescribed amounts of energy (produced in accordance with some prescribed technology) are required. Water quality will be introduced in several ways. First, we will consider the role of the polluter. (1) What treatment technology is available, at what price, and at what level of reliability? (2) What are the prospects for substantial change in this technology in the near and distant future? (3) What are the social consequences of rigid standards? Will there be significant unemployment if firms elect to shut down rather than adhere to the standards? (4) Are options for storage and dilution available? (5) Are process changes feasible?

Second, we will consider the prospects for reducing pollution by imposing standards on the ambient flows rather than the various effluents.

Third, we will adopt (i) the matrix methods of Thomann for routing degradable pollutants (particularly nitrogenous and carbonaceous BOD) through major rivers, (ii) other widely accepted forms of mixing and dispersion for routing conservative pollutants through systems, (iii) special functions for routing radioactive wastes (including the problems introduced by scour, deposition in benthic deposits, etc.), and (iv) special functions to accommodate thermal budgets.

Fourth, we will experiment with large-scale alternative methods and placement of treatment, of non-treatment (bypass), and of demand reduction.

All of these circumstances and cases will be represented by functional forms and modules which are the subject to precipitous, not merely incremental, change. We do not propose merely to optimize the system by inquiring after the consequences of marginal changes; we welcome widely-based challenge to our traditional notions.

The water resource project will emphasize connections to other segments of society in depth how these connections can be represented within the framework of a decision model in the best way. We will try explicitly to construct connections which realistically reflect institutional constraints, potential technological changes, substitution of one resource to another, etc. We will not be limited to specific functions for which it is necessary only to change a parameter or two.

We will also explore simpler didactic models which strip most of the detail so as to focus on the profundities of the connections and interfaces between sectors of the model. This apparent simplification is, generally speaking, deceptive but the formal and numerical reductions we seek are permissible only when we can be certain that the requisite assumptions are justifiable. This certainly requires extraordinary perception, insight and experience in water-resource system modelling.

2.3 The Two Parts of the Program

The program has:

- a general methodological (theoretical) part;
- an applied part, including a retrospective case study of a particular example of the large river basin (Delaware, Potomac, Vistula, Trent).

This document aims to describe the main concept and program of the research project and the proposed stages of its development. The description has been based on a principle of SA, which we shall call Basic One and states the following:

> "A Large Scale System, as an ordered set of different types of compatible and integrated elements -- as equipment, energy sources, materials, money, people, and others -- should operate in time and in space as one well-designed mechanism subjected to the feedback control principle."

For practical reasons the program will be divided into a number of subprojects in accordance with the accepted classification of the main aspects of the problem. All the subprojects will be linked together through the central water resources allocation model.

2.4 Block Diagram of the LSS

A block diagram of the LSS is to be studied and is shown in Figure 1. It includes:

- a watershed area and water resources;
- the water uses (industry, cities and population, an energy

system, agriculture, a transportation system, environment and a recreation area;

- a bank.

It is assumed that each user has to be provided with a water waste treatment plant (WTP).

Interconnections between subsystems are indicated by arrows.

The two main arrows represent:

- the investment policy line;
- the feedback control line.

Two kinds of input are applied to the system:

- legislation for water resources use;
- some kind of external disturbances, affecting almost each subsystem.

2.5 Interdisciplinary Character of the Project

The block diagram shows that the project has interdisciplinary character and closely touches upon many branches of science -- geology, meteorology, hydrology, physics, chemistry, mathematics, technology, agronomy, biology, ecology, economics, sociology, and even some aspects of political science.

The total amount of knowledge available in these disciplines should be used in order to achieve the main goal mentioned in Section 2.2. It means that organization of the IIASA inter-disciplinary water resources team should be provided.

2.6 The Strategy of the Scientific Research Development

The strategy of the scientific research development begins with the composition of the fundamental review of the state of the art of the problem of "The Complex Use of Water Resources."

The review should be spread over all aspects of the problem mentioned in Section 2.1.

The review will provide us with background information about the progress which has been achieved since the time of King Hammurabi.

Special attention will be paid to the mathematical modelling of economic activity of the LSS (Figure 1) in connection with the definition of the day-to-day operational policies of water resources management and the economic effectiveness of the feedback control principle's application. This principle has not only an economic but also a very fundamental historical background, as is demonstrated by the following passage:

> "The first recorded evidence of human activity in the field of feedback control comes from Babylonia and is about 4,000 years old. In an era about 2,000 years before the birth of Christ, Babylonia had the most advanced civilisation on the face of the earth. By means of a highly developed irrigation system using waters from the Tigris and Euphrates rivers, the Babylonians achieved an agricultural productivity that enabled them to support the highest population density known up to that date. Their yields per acre were not

exceeded until modern times. Modern engineering has its roots in the irrigation systems built and operated in ancient Babylonia. In irrigating his land the Babylonian practiced feedback control since he consciously regulated the moisture content of the soil by opening and closing ditches. The recorded evidence for this is contained in the set of laws codified and inscribed on stone by order of King Hammurabi approximately 2,100 years before Christ. The code of Hammurabi comprising 282 laws is the oldest known law code in the world. A number of these laws relate to irrigation, and a typical one is translated as follows: 'If anyone open his ditches to water his crop, but is careless, and the water flood the field of his neighbor, then he shall pay his neighbor corn for his loss.' Archaeologists believe that Hammurabi promulgated very few new laws in establishing his code, but merely wrote down what had long been the law and custom of the land. How far in advance of his code irrigation had been practiced is not definitely known. We can only conclude that the laws of King Hammurabi contain the first known evidence of legal penalties for the malfunction of a human operator in a feedback control system."

In the first part, dealing with the problem of formulation, the research strategy should be based on experience dating back as far as Babylonia and continuing up to the present day. The second part of the research strategy, namely the solution of the problem, should be based on the application of up-to-date mathematical tools and digital computers.

The strategy could be implemented through design of the mathematical model of the LSS mentioned in Section V and through careful identification of all its parameters.

The model will be studied in cases when:

- it is deterministic;
- the random variables are provided with necessary information about the law of probability distribution;

- the random variables are uncertainties.

2.7 The Core of the Control Problem

The core of the control problem includes the following tasks:

a) ordering of all subsystems,
b) establishment of their compatibility,
c) design of the LSS multilevel hierarchy,
d) definition of the initial conditions, supremal goal of the LSS and infimal goals of the subsystems,
e) definition of admissible alternative ways of achieving the supremal goal,
f) determination of the information available and information needed for control operations and information processing,
g) clarification of the supremal and infimal decision unit structure and coordination of activity between them, and
h) clarification of the sensitivity of the problem solutions to different circumstances.

Special attention should be paid to the estimation of risk in systems operation in the presence of uncertaintites and to the recommendations for control policy which provide minimal losses.

When the possibilities of the control reservoirs have been defined, the concern of the control problem is to provide operational procedures for decision makers, for all situations in the subsystems and their admissible environmental behavior.

2.8 Modelling

Insofar as the given LSS cannot be treated in a purely analytical manner, its investigation can be implemented with a Digital Computer (DC). Modelling will clarify the principal LSS quantities, the functional character of their causalities, the possible local feedback control loops; influence of time delay elements and their interconnections and sensitivities in different situations.

The aims of modelling are:

- to find all of the LSS admissible trajectory behavior and the influence of admissible control policies;
- to quantify the effects of all types of disturbances; and
- to find the best policy for maintaining the LSS.

The modelling can be done by formalization of the LSS model and selection of an appropriate language and information processing using the DC. Modelling permits the estimation of scalar or vector valued criteria and the discovery of the LSS optimal control policy.

During the stage we shall be able to take into consideration the model that grows in size and complexity interconnections among sectors. For example, are new constraints introduced if regional or national energy production shifts from a thermal to a nuclear basis, and if the water resources are thus required to convey spent radioactive wastes? If so, how do these constraints interact with criteria imposed by political, institutional, economic, ecological, and other interests? All questions of this kind we shall try to settle during the stage.

2.9 Improvement of the Model

We can enlarge or iterate the model design after completing it in the simplest case. The goal of modification is to introduce into the model any innovations, particularly those which are being planned by regional authorities. Mathematical formalization of these innovations can be done by the same methods which will be used in the first iteration. Further, we must find all new random variables and associated probability distributions.

Infrequent disaster events -- e.g., drought or excessive water during spring thaw -- must be taken into special consideration because the reliability of the LSS is closely related to the cost of the LSS, modes of its operation and maintenance.

2.10 Innovations and Singularities of the Project

Attempts will be made to introduce some innovations and singularities into the project which will emphasize its methodological originality and practical value.

1) The mathematical model of the LSS will be a substantial generalization of the water resources allocation model which has been used in the Soviet Union*. Generalization will be spread over water use technology and estimation of the economical effectivenes s and competitiveness of this technology inside of regional development. This means that the model will indicate the possible ways of reducing one kind of production to provide the extension of another.

2) The model will include some combination of multistage decision processes with continuous ones. This kind of combination has never been described up to date. The combination poses an important question about coordination and compatibility of continuous and multistage decision policies of control.

3) Through the selection of the water resources control policy, which will be established by means of the vector-valued optimization technique, the project will meet, as far as ever possible, a particular and sometimes controversial economic interest of different agencies, societies and even of private water users.

---

*S.B. Elahovsky, V.L. Roitburd, S.I. Sorokina, V.I. Buyakas. "Methods for Solution of the Operational Problem for Water Resources Allocation and Their Comparative Evaluation" -- Proceedings of an international symposium, "Water Resources Systems," Prague 1972.

4) As it could be seen from Figure 1, the project has a considerable intersection with the other IIASA projects like Energy, Municipal and Ecological Systems. We shall be able to take into consideration all the possible recommendations out of the Energy, Municipal and Ecological Groups about possible styles of water use determined by possible developing of the projects in the future.

   A special kind of intersection coming out from Professor M. Fiering's proposals concerning "Environmental Zoo" (the Memorandum of 30 August from M. Fiering to H. Raiffa) and "Lake Eutrophication" (the Memorandum of 3 September from M. Fiering to H. Raiffa).

   Another intersection with the project of Large Scale Organizations coming from suggestion of Dr. Emsellem, Dr. D. Clough, and Prof. Y. Iwaza dealing with the institutional infrastructure and political aspect of water resources use.

We could develop a systematic diagnostic procedure for determining who makes the decisions, and under what forms of pressure and influence, in various countries. Such a project would be useful to agencies who wish to plan a project, and to organizations who wish to block one. It is eminently capable of generalization, and developing communities might learn much from the catalogued experience of other countries.

This would include a study of bodies of law governing water use; organizations for the administration of laws; organizations for operational control of the water system; unofficial organizations of citizens, private enterprises, technical associations, and other political pressure groups; multi-person decision processes, including public hearings, bargaining and conflict management; institutional arrangements for co-operative management at both a diplomatic and an operational (civil service) level.

In a more general approach we may have three kinds of another linkages among our research projects:

a) similarity in methodology (attacking different fields with the same methods on the basis of structural analogy, e.g. traffic flow, water flow, population flow, electric power systems, etc.).

b) similarity in the time and space dimensions of the problem (global, regional, micro-problems; long- and short-run problems, etc.) which allows the integration of different projects either on a regional or temporal basis. This is the way to develop a global model or a model of a certain geographical region.

c) interfaces, overlapping fields, interconnections and supplementary fields among parallel research projects (energy - water pollution - waste heat disposal - cooling).

5) The project meets the most important societal demands of all research work:

- it is a universal enough and has a pressing importance at the present time;

- it is of practical value;
- it has some degree of scientific originality;
- it is solvable by a small group of scientists within a resonable period of time.

3. Stages of the Program and Presentation of the Results

3.1 Stages

First Year:

Study of the relevant literature;

Data collection, especially relating to the example selected;

Visits of IIASA national member organizations;

Preparation of a technical report about the state of the art concerning the water resources problem.

Second Year:

Identification of the LSS methods;

Data processing and preparation of the example;

Report on the mathematical model of the LSS.

Third Year:

Definition of goals of the LSS operations and alternative means for achieving them;

Selection of the LSS social performance criteria;

Report about the statement of the research problem.

Fourth Year:

Modelling of the prepared example with possible iteration of its model and estimation of the social benefit criteria;

Report on results of modelling.

Fifth Year:

Preparation of a monograph about "Complex Use of Water Resources".

3.2 Expected Results

We expect the research program to produce the following results:

1. A general methodological statement of the problem, including: initial data handling, selection of goals and alternative means of their realization, selection of social performance criteria, and methods of SA for research into the problem.

2. Methods of research into the problem which consider the innovations indicated above; modelling of the example and recommendation of the optimal control policy for its operation.

3. A monograph to be written in the form of a handbook entitled "Complex Use of Water Resources."

3.3 Manpower Needs

Project Titles	Man-Years per Year					Total
	1	2	3	4	5	
LSS model on level 1 (Sect.2) Didactic models (Section 1.1)	7	5	4	6	5	27
Biological problems:(Sect.1.2) (Sect.1.3) Infrastructures and Institutions (Section 1.4)	2,5	4	4	4	4	18,5
Retrospective Studies (Section 1.6)	0,5	1	2	0	1	4,5
	10	10	10	10	10	50

Mathematicians with considerable experience in control science (or systems analysis)	3
A mathematician - probabilist for ½ term	1
An economist-sociologist for ½ term	1
Hydrologist	1
Chemist-Biologist	2
Engineer	1
Ecologist	1
Programmers for ½ term	2
Total for full term:	10

It is supposed that this team will work closely with other IIASA groups and international research institutions concerned with the problem. Appropriate satellites of the group in national member organizations must be organized.

3.4 Program Budget Estimate for 10 Member Team for 5 Years

Salary 10 @ $ 25.000 x 5 Years	$ 1.250.000
Travel Expenses for 2 trips @ $ 4.000	80.000
One 5-day Conference at IIASA for 30 people @ $ 200	30.000
Cost of Modelling	90.000
Miscellaneous Materials	50.000
Total:	$ 1.500.000

The budgetary cost of the Project should be the subject of special attention at the Finance Committee Meeting.

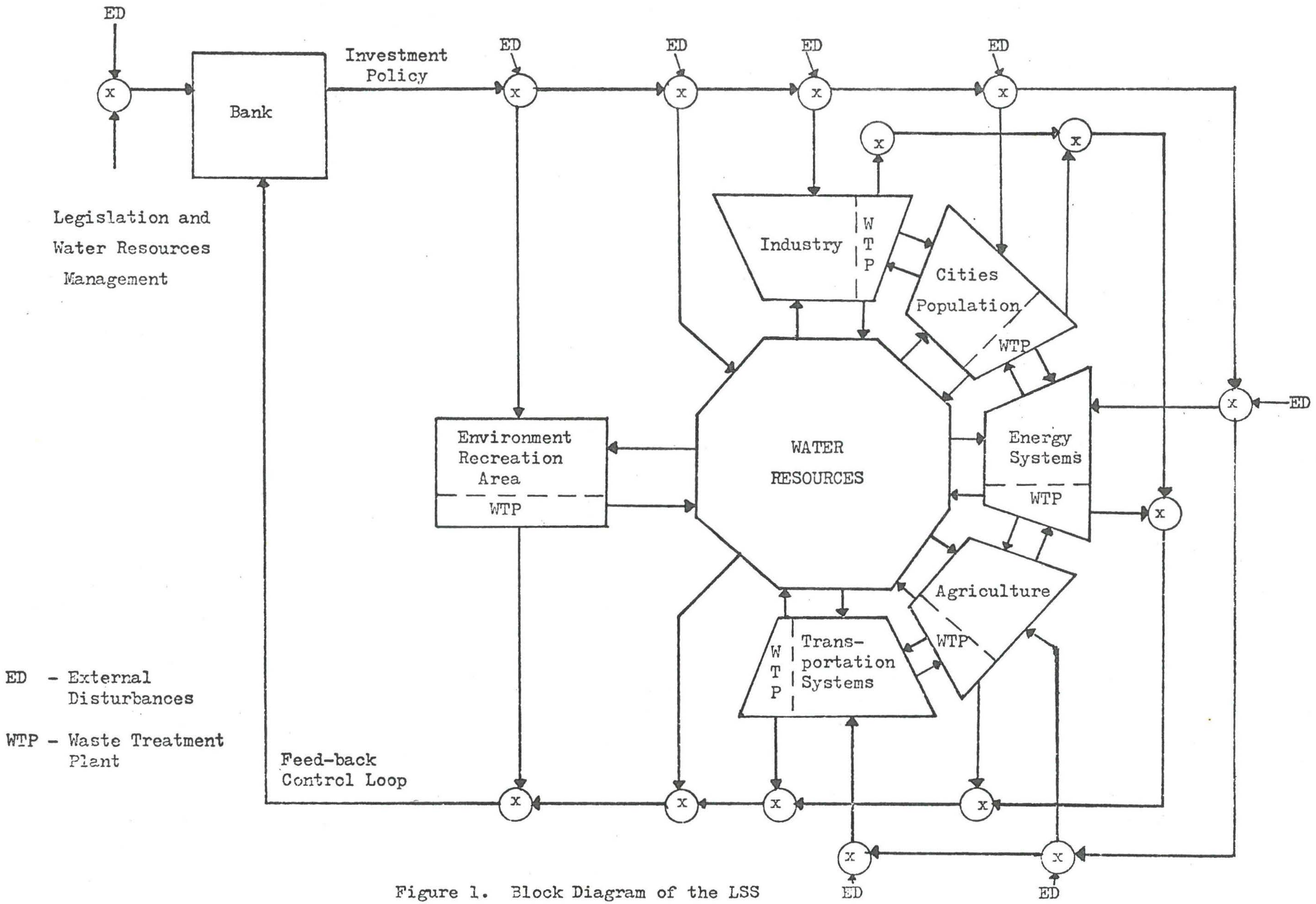


Figure 1. Block Diagram of the LSS

PROPOSED RESEARCH PROGRAM

for

MANAGEMENT OF URBAN SYSTEMS

(1) Introduction

Two streams of work are planned, one at the scale of national settlement systems and policies, and one at the scale of the metropolitan region and its key components. Both will have long- and short-run outputs, but the main weight in the long term will rest most heavily on the former.

The whole program is intended to stress fundamental work necessary for rational policy development, together with methodological problems encountered in the analysis, design and implementation of urban policies and management strategies. The program is also intended to take advantage of the unique capabilities represented in IIASA's constitution and in the other projects.

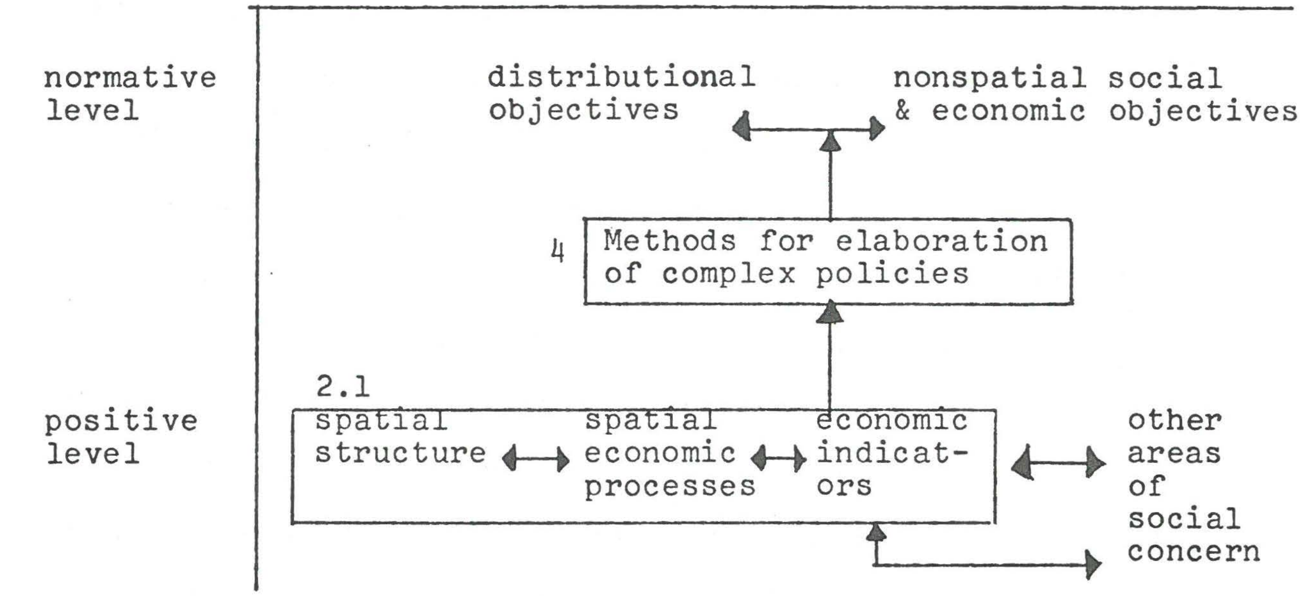
Components of the research agenda are necessarily dependent on the vagaries of staffing, and upon synergies yet to be developed with other IIASA projects.

(2) National Urban Policy Problems

Most advanced nations now have explicit policies regarding the spatial distribution of population and economic activities, which may be referred to summarily as urban or settlement policies. They are concerned not with individual cities but with the dynamics of a whole system of places, large and small, together with their functional relationships, spatial structure, and territorial specialization. As presently formulated, most such policies seek to moderate the growth of the largest places in favour of accelerating the development of the medium-sized, often with a special emphasis on redressing regional economic disparities.

(2.1) The theoretical basis for spatial objectives, however, is weak, fragmentary, and in need of a multidisciplinary synthetic approach. In particular, the relation between spatial distribution objectives at national and macro-regional scales and such commonly accepted nonspatial goals as economic growth and development, social mobility and opportunity, equity and justice, environmental quality, and the like, is woefully undeveloped. A considerable if inconclusive literature exists for each side of the question, but a reliable relation is missing. An explication of the causal links between indicators of the state of economic development and the spatial structure of the economy in both socialist and market economies is the key to understanding the larger process in both and will be the major long-run theoretical task in this area. Figure 1 indicates the place of this work, as well as another task outlined below (see 4), in that more general process of moving from understanding (positive level) towards prescription (normative).

Figure 1: Location of sub-projects 2.1 and 4



(2.2) To set the stage for this long-term work, the first step will be a descriptive inventory of national settlement policies in member nations, enumerating goals, rationales for and against policy alternatives, and the tactical instruments or levers being used to implement these policies. Continuing sources of information, both governmental and academic, will have to be developed for each country through the NMOs.

(2.3) Simultaneously, a review paper is to be prepared on work to date on theoretical and operational models of national urban systems. The two are expected to be the principal background papers for an invitational workshop on national urban policies in late 1974.

(2.4) The major follow-on activity, beginning in the second half of 1974 and continuing at least through 1975 will be the elaboration, at a conceptual level, of a general synthetic model of the settlement system of a modern macro-regional economy. In form, the model will be a positive (i.e., non-normative) impact predictor which deliberately preserves the major value judgments as exogenous interventions. It is not proposed to make the whole model operational for a given urban system, but rather to use it as a diagnostic/prognostic device for identifying degrees of sensitivity among policy levers, and as a guide to more narrowly focussed empirical analysis. Initiation of work on the following examples will depend on circumstances 20 months hence, including particularly the availability of researchers and possible synergies with other IIASA projects.

(2.41) Experience to date with these types of models indicates that spatial distributions of economic activity and the definition of more or less "efficient" distributions are very sensitive to what might be generally termed as spatial linkage costs. Hence a general method of assessing the impact of technological developments in transportation and communication systems can be made available, provided only that the operating characteristics and substitution elasticities of the relevant technologies can be estimated.

(2.42) Migration patterns are seen as the aggregate of individual or household choices whose determinants are for the most part expressible within the model. Thus, while the experimental calibration of this part of the general model would require a culling of the very large literature on migration, no extensive empirical work on migration behaviour per se is foreseen. Rather, we would aim to predict the evolution of an urban system through manipulated changes in the factors giving rise to migration streams. Again, structuring the problem in this fashion allows application in both socialist and non-socialist economies.

(2.43) A long-term shift in the occupational structure of the labour force in advanced economies, and more importantly, a shift in the locus of social and economic power, has been taking place for some decades and will likely continue. This shift (a component of the end-state which is variously called 'post-industrial society,' the 'leisure economy,' or more accurately, the 'information economy') is away from direct resource exploitation and manufacturing industries, to some extent away from personal service activities, and towards the quaternary, or high-level information management and decision-making, sector. This long-term trend has important consequences for settlement pattern, among many other things, and the impact of changing mixes of primary - ... - quaternary activities, with their differing locational requirements as types of economic activities, can be identified in principle through the medium of the proposed general conceptual model.

(2.44) The changing composition, location, and magnitude of expenditures on public goods in the urban and spatial linkage infrastructures should be deducible (again, in principle) from the proposed model. Exemplary calibrations to real regional systems should be possible in 1976 and later, should circumstances then warrant it.

(2.45) The general specification of size, function, and linkage to the regional economy of planned new communities might also be treated within this framework. It is an interesting problem, as the design of new cities is likely to continue growing in importance in member countries, and since some designs have proved in practice to be failures owing to a lack of consideration of these 'role' questions.

(2.46) A number of quite general problems will arise to pique the interests of IIASA's applied mathematics and methodology group, chief among them being the problems of autocovariance in spatial forecasting and local-versus-global optimization in hierarchical systems whose parts are evolving at different rates.

(2.47) Finally, for this indicative list at any rate, the general model will serve as both springboard and reasonableness check (clamp) for exercises in scenario writing about the future of settlement systems. It is anticipated that futurist speculations, ours as well as others, may be rewritten as scenarios and checked against the model, and that the model itself will give rise to other scenarios, all of which should be immensely suggestive of fruitful detailed empirical studies.

(3) Metropolitan Management: Strategy and Tactics

At the scale of the town or urban region, there are a large number of interesting problems. It is not due to a dearth thereof that the major emphasis of this urban research agenda is at the macro-regional scale, but rather because (a) IIASA's limited resources dictate a carefully focussed approach if meaningful results are to be obtained, and (b) there are already many institutions working on metropolitan problems, whereas IIASA can make an important and unique contribution to the macro-level concerns, However, the conceptual approach to settlement systems leads directly to and helps illuminate some interesting urban-scale problems.

The general approach is that the allocation of functions to localities that flows from consideration of the urban system as a whole generates (and helps specify) certain classes of "local" problems. Assume for the moment, without worrying about implied teleologies, that the purpose of an urban system is to satisfy a list of conflicting objectives such as those set out in Section 2. This means, among other things, that the matching of supply and demand for goods and services should satisfy marginal utility constraints, which then becomes one of the principles underlying a management strategy. But one is dealing with systems characterized not by static equilibria, but continuous deformation (though perhaps with equilibrium-seeking as a constant if unattainable feature). This means that there will inevitably be mismatches, probably more severe as the size of the unit of observation decreases. There will always be a demand, therefore, for 'coping' tactics, particularly with respect to the supply of public goods. The location of service facilities -- parks, schools, hospitals, etc. -- the routing of service networks, the scheduling of activities, and so forth, in fact a myriad of the rather mundane labour-intensive activities of any local government, have been subjected to OR techniques in a number of places. Moreover, although the potential areas of application are numerous, the number of underlying mathematical models is small.

(3.1) Therefore, as a specific and relatively short-term effort, it is proposed that a special sub-project be devoted to producing a handbook on general methods, with case studies of actual applications stressing implementation tactics. This should be a high-visibility short-term project with concrete markets in mind. Discussions with the International Union of Local Authorities and the organizers of the UN Conference/Exposition on Human Settlements (CONFEX) should precede initiation of this sub-project.

(3.2) "Form must ever follow function," said Louis Sullivan. He was, of course, ignored then and ever since by his fellow architects, but there are many studies which demonstrate that the organization of urban space and built environments are reflections of the dominant technologies of their times. (In fact, one of the generic coping problems is making do with physical armatures dating from past technological eras, as anyone who drives in downtown Vienna can attest.) There are at least three areas of impending or conceivable technical change which may have strong impacts on urban form, and which are suitable for study at IIASA. Their suitability stems from their pre-emptive, strategic posture with respect to policy formulation, as opposed to the reactive tactical approach of Section 3.1. Zero-order approaches to these problems would also take the form of scenarios or prospectives.

(3.21) Urban form in the energy economy of the late 20th century and beyond is liable to be radically different than at present. It is anticipated that a joint sub-project with the Energy group will be defined which will seek to map versions of the city of the future under strong single-objective conditions, such as the minimization of aggregate energy demand.

(3.22) Another 'single-objective,' though richer, problem is the design of urban form broadly conceived to maximize the resilience and dynamic stability of the resulting system. This represents a translation into urban planning terms of the very powerful concepts being formulated in the Ecology project; capitalizing on that strong foundation will be a high priority within the Urban project, with work scheduled to begin in earnest in mid-1974. If successful, the most important contribution of this work would be to help shift the mind-set of present-day economic and physical planners away from single-point 'optimal' or equilibrium-centered modes of thought.

(3.23) A second area for in-house collaboration, and this strongly involving the Water and Ecology as well as Energy groups, will examine the question of the siting of energy production and transmission facilities.

(3.24) A third area, also involving those groups but especially the Water Resources Project, is the problem of solid-waste management. Recycling and other waste management technologies are especially worthy of concern in the municipal management area, and, like a number of the other activities suggested here, could be an application of the IIASA integrating theme, Standards. IIASA's contribution to this concern needs to be studied, however, in relation to the considerable work now going on in member countries and international organizations on the topic.

(3.3) These and a number of other topics* could be examined in holistic fashion through a case study of an appropriate urban region. An opportunity to collaborate with the government of Canada and the United Nations may arise through CONFEX, which is to be held in Vancouver in June 1976. As one of its contributions to CONFEX, the Canadian government may commission a case study of the development and prospects of the numerous settlements in Canada and the U.S. which border on the Straits of Georgia and Puget Sound and which are now in some respects beginning to face the same problems and to function in an integrated fashion. Should this work be commissioned, the Project would seek an active collaborative and supporting role.

(3.4) A component of most national urban strategies is the establishment of new towns and cities. Recent experience and designs for the future will be a major feature of CONFEX. We have already suggested that a contribution may be made to new town planning by drawing attention to the macro-regional systems aspects of the problem (2.45); the same may be true with respect to the organization of

---

*Utility of modes of international regional planning; testing certain hypotheses about flows in a dispersed conurbation and about the operational definition of life-styles; scale and convergence effects in regional urban agglomerations.

municipal services. We would now suggest a project, using CONFEX as a baseline, which would offer a systematic context for the evaluation of new town designs made by others, and which would also be the vehicle for examining the organizational problems in implementing new and possibly radical designs requiring large investments and accommodations on the part of other components of the national urban system. This part of the work should be elaborated in conjunction with the IIASA project on the design and management of large-scale organizations.

(4) Practical Methods for Elaboration of Complex Urban Management Strategies

This is a quite general problem, transcending scale and the present working definition of the Urban project. At the same time, systematic study of the problem in the urban management sphere is of critical importance to the coherence of the theme. In one sense, it is a special aspect of the problem of the design and management of large-scale organizations, but with the constraint that the roles played by various actors in the process is sharply defined in governmental decision making.

Translating research into policy is far from an automatic process. The design and comparison of alternative strategies for information flow in decision hierarchies has been studied using systems methodologies,
in some of the socialist countries and more recently in

North America. Should the Urban project be in a position to offer appropriate scholars and cases to a joint project with other IIASA groups, it will be delighted to do so. (See Figure 1.)

(5) Program Planning

(5.1) The components of this project include in-house research, possible collaborative research with other institutions, information collation and dissemination, and selected conference/workshop activities. These activities are arrayed in time as shown in Figure 2. Since not all the proposed activities may in the event be capable of realization, the crucial highest-priority sub-projects are underlined on the Figure, and those of lowest priority placed in parentheses.

(5.2) CONFEX will be a major opportunity to demonstrate the fruits of IIASA's urban and urban-related research. Pending some definition of the ground rules by the United Nations, several of the specific sub-projects suggested here ought to aim for at least interim conclusions and proper documentation for presentation to the world community in Vancouver in 1976.

(5.3) In order to assure that program planning continues to take advantage of the best talents available in member

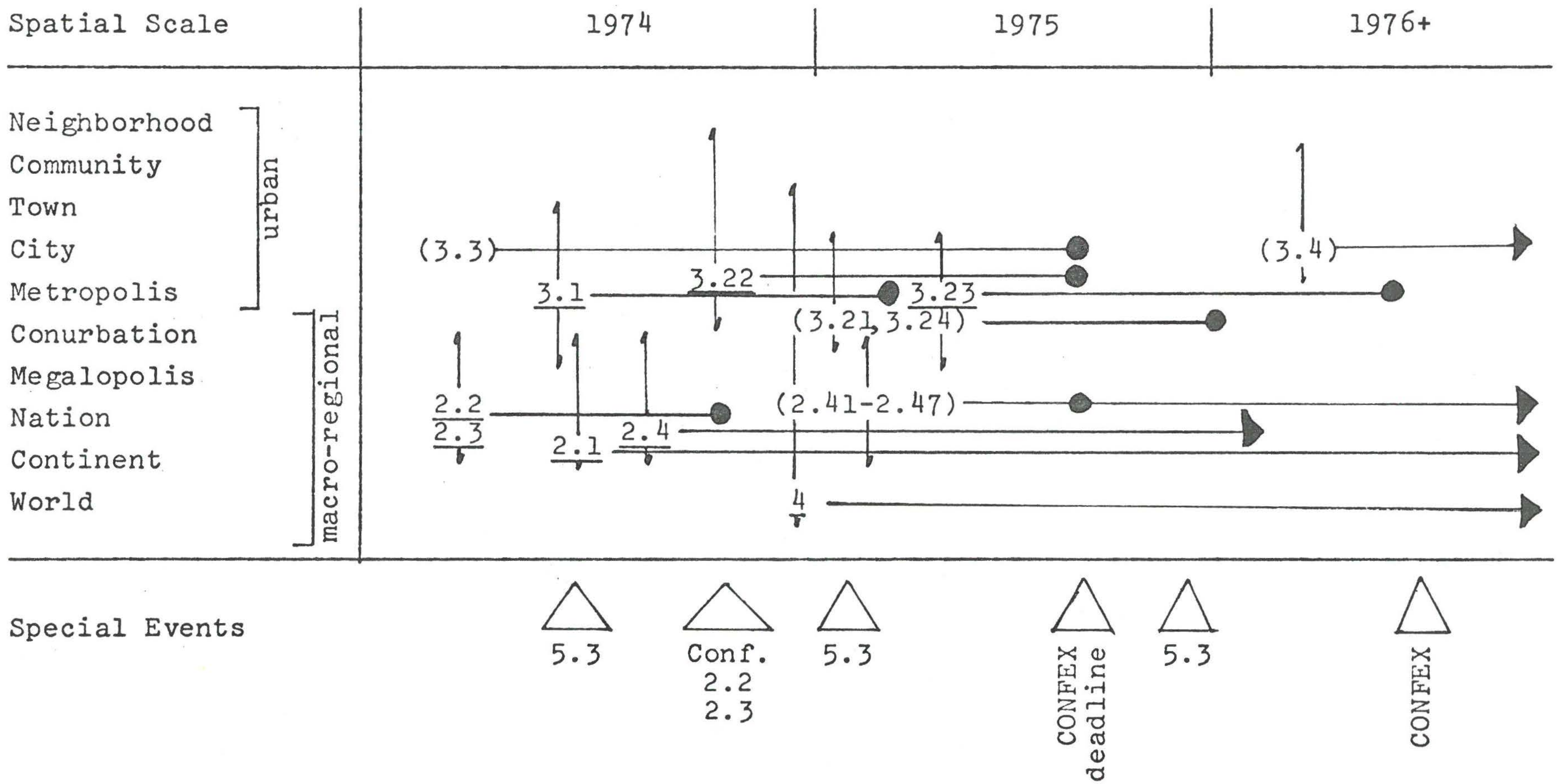


Figure 2: Rough Guide to Urban Project Activities

countries, it is proposed to create an advisory council to the urban project, consisting of 5 or 6 leading world scholars whose names will be suggested to the Director by the project leader. It is anticipated that the group would meet once or twice a year for periods of 5 to 10 days to review and criticize work done internally and help in the continuing elaboration of research plans. Carefully selected, the members of this advisory council should also aid in scientific recruitment and in maintaining scholarly relations with member nations.

(5.4) Clearly, a statement like the present one is merely a snapshot in time of a continuous research planning process. Succeeding statements may be expected to integrate ever more successfully the concerns and criticisms of colleagues in other Projects, urban scientists yet to be appointed, and members of the Advisory Council.

PROPOSED RESEARCH ACTIVITIES

in

COMPUTER SYSTEMS AND COMPUTER SCIENCE

I. Introduction

Computers are the principal physical tool for implementing the results of Applied Systems Analysis (ASA) and for research in the area. Computer Science and Artificial Intelligence are among the basic methodologies underlying systems analysis. We propose an integrated program of support, development and research in these areas, which should greatly assist the research and operational capability of the Institute.

There is no doubt that the primary function of the Computer Section, as well as its raison d'etre, is to support the research program of the Institute. On the other hand, if a high standard of service is to be achieved and maintained, it is necessary to study on a scientific basis how to accomplish this goal. This is the secondary, but no less important, function of the Section.

The proposal is divided into five main sections. This section describes the role of advanced computers and Computer Science in the IIASA program. The second section describes the four proposed activities: computer services, networking, information systems and supporting research. The third section briefly discusses other activities such as surveys, conferences, etc. The fourth section discusses the selection of a computer for IIASA and the fifth section presents timing and manpower information.

The mission of IIASA is to play a leading role in applying systems analysis to social problems. The central problem in planning for this role is how to get the maximum effect from IIASA's limited resources. The overall strategy is to have IIASA coordinate other activities and to carry out selected in-house projects of high expected impact. The Computer Science/ Computer Services effort can greatly assist in both areas. We propose a high-quality interactive computer system connected to a variety of other computers and a software development effort aimed at furthering the basic aims of IIASA.

There has been overwhelming agreement among staff members and conference attendees on the desirability of interactive computing for IIASA, even at the expense of some pure computing power. The advantages, in general, of interactive computing are widely accepted in the field (Section IV contains a brief review of the arguments). The most important advantage to IIASA is the increased productivity of scientific personnel. Some suggested projects in other IIASA areas, such as interactive modelling, require an interactive system. There are also important advantages for the administration of the Institute itself. To carry out the research proposed below, IIASA would require a medium-scale computer system. A greatly reduced, but viable, program could be carried out with a large mini-computer system. The computer acquisition problem is further discussed in Section IV.

The overall aim of the CS/CS program is to provide support for and contribute to IIASA goals. We can make the computer system itself one of the reasons for scientists to come to IIASA and one of the common ties among projects.

II. Computer Service/Computer Science Activities

The program is presented in four main sections for convenience. In fact, the plan is to have a coordinated effort involving CS/CS work and other IIASA projects.

A. Computer Services

We expect that every group in the Institute will make use of Computer Services. There should be interactive consoles available to whoever needs one. The consoles will be connected to an in-house computer system. The capabilities of this system will depend on machine purchase decisions, but it will include at least the following. There will be a direct interactive language (e.g. BASIC or FORTRAN) for small computations. There will be an editor for the preparation of documents, data and programs, and there will be a capability for entering a task in the job stream of one or more large computers. Possible additional services include the ability to build interactive models in various domains and to carry out research on high-level communication with decision makers.

The Computer Services will also include programming services. These will cover a broad spectrum running from programming advice to the construction of systems programs to meet IIASA requirements. There will certainly be a need for applications programmers to support research in energy, ecology, water, etc. While some of these programmers will be attached to one area, there should be others available from a pool. A small group of applications programmers is an efficient use of manpower and should serve to transfer techniques among areas. Further, we would hope to have the same people do systems programming and applications programming on different occasions. An important related aspect of Computer Services will be consultation and training. The computation program proposed here will evolve into one of the more sophisticated centers in the world. An important function will be to help the scientists of IIASA learn to take maximum advantage of these capabilities.

The next two sub-sections - Networking and Information Services - can be viewed as development efforts to extend the computer services at IIASA.

B. Networking

Networks of computers, connected by high speed lines, are becoming an increasingly important factor in large systems. To some extent, computer control of a very large system inherently requires a computer network. In addition, networks are an important tool for gaining access to remote hardware and software and for exchanging data. Thus, networks of computers are important to IIASA both as a tool and as part of the application of systems analysis to very large systems. An additional point is that IIASA appears to be the ideal institution to pioneer in the formation of networks including socialist and non-socialist nations.

The networking development at IIASA will proceed in several steps. The first step is to connect the Institute's machine with a large computer, probably the CYBER 73/74 of the University and Technical University of Vienna. This is needed for other work anyway and will provide initial experience in multi-computer usage. As soon as possible, an additional link should be established to a large machine of the IBM 370 series.

The next step will be to connect the IIASA machine to several different computers in different nations. The hardware for doing this should be designed to be generally applicable, and suitable communication protocols should be established. This effort will also involve significant interactions with the telecommunications establishments of various countries. This development will allow IIASA scientists to use the facilities of several machines and will open up a number of research problems.

Even with a functioning physical network, it may not be practical for one person to use several machines. Each machine has its own log-in procedure, command format, etc. We should have a program in the IIASA computer which masks most of these idiosyncrasies from the user. A more difficult task involves data and format conversion from one machine to another. Eventually, one would like the ability to build models with components on several machines.

Another research area is switched or store-and-forward networks to replace the fixed lines used at this stage. Still more problems arise in connection with information services, described next.

C. Information Systems

IIASA is deeply committed to being a center of information exchange. Handbooks, surveys, conferences and clearing house functions involve handling vast amounts of information. Much of the work involved can be automated and the machining of this material opens new scientific possibilities for IIASA staff and national member organizations. There is a great amount of established technique for handling information; IIASA should innovate only where there is an important advantage for other Institute programs.

The CS/CS group in cooperation with Scientific Services can provide many useful services at moderate cost. We envisage information services including a number of straightforward functions, such as the maintenance of selective mailing lists. There should be files on personnel, contacts, organizations, related projects, etc. available. The Institute may or may not choose to automate various accounting functions. There is a large body of techniques for document production, ranging from text editors to spelling checkers and document compilers which format, fill in references, make indices, etc. IIASA should plan to make use of such programs as soon as possible.

It is important to note that neither general information retrieval systems nor general management information systems are sufficiently well developed. Many of the systems that are available require some notion of computational procedure from the users - this should not be a problem in the IIASA environment. It is expected that these information services will be quickly adopted as they have been in other research environments.

One area for IIASA innovation is in multiple machine information systems. After we have gained some experience with networking and with the information services available locally, we could begin to explore the use of files on several machines. In its simplest form, this project is just a particular case of network computing. A user can process files on a remote machine using the software of that machine. But that is quite burdensome; different machines tend to have different ways of saying essentially the same thing. We should be able to provide the IIASA user with a language which will allow him to process files on all connected machines with little extra effort.

This general file processing system is within the state of the art but has not, to our knowledge, been done elsewhere. IIASA might be the first institution to develop such facilities. In any event, there are still more ambitious goals for a multi-machine information service. At the next level one might provide conversion routines for units, number representation, standard currencies, etc. The long range goals include the ability to understand data files in different formats, at different aggregation levels and with different but equivalent classification schemes. This will require fundamental advances in Computer Science as suggested in the next section.

D. Artificial Intelligence and Computer Science

These fields are very wide and IIASA cannot attempt to build an internal competence in them. We propose that a small number of researchers, in selected areas, be invited to IIASA as opportunities arise. The Institute may, in addition, sponsor conferences, surveys, etc. on topics of interest.

One important set of research topics arises in connection with advanced information systems. In order to deal intelligently with data bases having different classification schemes, a program must incorporate some notion of the *meaning* of the terms involved. This problem of treating general knowledge is considered to be among the most basic in Artificial Intelligence. Professors Butrimenko (USSR), Klics (GDR), and McCarthy (USA) have proposed that IIASA pursue this problem of semantics as an area of continuing scientific interest to many national member organizations. An understanding of semantics will also be necessary for a project to communicate naturally with decision makers in some problem domain. An important related topic is the organization of collections of algorithms. This has considerable practical importance and is a subject of great current research interest.

Another area of some importance to IIASA is the study of the complexity of algorithms. Basic results are being obtained on the best possible computation time for tasks like sorting and matrix multiplication. Since the size of problems approachable by systems analysis depends on the speed of various algorithms, IIASA has a considerable interest in these results.

Most complex problems are solved by people using non-algorithmic or heuristic techniques. There has been a great deal of work applying heuristic techniques to domains such as game playing, chemistry and medical diagnosis. There should be work on combining heuristic techniques (e.g. for linearization or aggregation) with more vigorous methods. One can envisage an interactive decision system for some area combining human, heuristic and mathematical decision making. IIASA might eventually undertake such a project and should surely support research on its components.

III. Other Activities

We expect that the CS/CS group will participate in a number of auxiliary activities in addition to those mentioned above. There will certainly be cooperation with other agencies in preparing surveys and reports, holding conferences, etc. In fact, there is an effort underway to produce a survey of computer aided design (CAD) in connection with IFIP and others. This should be completed within a year and should be suitable for incorporation in the Handbook of Applied Systems Analysis.

When the technological forecasting effort gets underway, we expect that forecasting of computer hardware and software technology will be an important component. The interaction with other IIASA groups should be extensive. There will be many joint projects in addition to the ones mentioned in Section II.

IV. Some Ideas about Computer Facilities for IIASA

Like a number of other advanced research communities, IIASA needs a modern time sharing system with an adequate file system. The list of places which operate such systems is quite impressive, and there is overwhelming evidence that no one who has been introduced to such facilities ever willingly returns to the old batch processing-type of computing. Nevertheless, it is interesting to analyze the arguments in favour of such a time-sharing system at IIASA. Some arguments are of a general nature and some are more specific to IIASA.

A. General Arguments

In turn these arguments can be broken down into two classes: one deals with the improvements in the programming tasks and the other with the improvements to the general environment.

A.1 Programming tasks

There are basically three kinds of improvements which a time-sharing system provides:

(a) The first kind is of quantitative nature: program preparation is much easier because of the ability to create and manipulate files. Most systems have sophisticated text editors which will for example enable one to replace an identifier by another one throughout a program with one command, and so forth.

Also, on-line syntax checking saves a large amount of time, for instance by preventing dead runs.

More generally, these systems allow substantial savings in time for a number of reasons: one of them is the elimination of cards and all overhead associated with them. The presence of terminals in individual offices will cut down on the commuting time between offices and the computer center. On the whole, turnaround time decreases by an order of magnitude.

(b) These <u>quantitative</u> improvements are so large that they generate <u>qualitative</u> changes. For example, because the turnaround rate is so much faster, scientists experimenting with computer models will tend to design their experiments differently: instead of designing complicated experiments to maximize the information they get out of one computer run, they will tend to design many simple experiments, thus making better use of their time.

Also, the possibility of interaction changes the nature of the programs being written: it is now possible to design interactive programs, delay parameter specification on models, etc.

(c) Then new ideas get generated because of these richer aids to scientists: for example, people will get different intuitions about a phenomenon by watching a live simulation on a graphic display terminal.

A.2 Services

General efficiency is increased by the availability of general sophisticated services such as a message service, editing and publishing facilities, information and documentation services, etc. A list of such services in existence at the Stanford Artificial Intelligence Laboratory is included as sub-section D.

B. Specific Arguments

IIASA's scientists will be of general high caliber who will stay at the Institute for relatively short periods of time; therefore, this time will be very valuable. Therefore, the increase of productivity provided by sophisticated computer services will be of prime value.

IIASA will have a leading and educating role in a number of areas and it is very important that it be equipped with the most modern and advanced computer services, so that the great benefits of such services may be made known to a larger number of scientific communities. There is already considerable interest in local universities in having students do dissertations on IIASA facilities. This is a significant source of cheap, high quality talent.

It is also worth noting that such a system would very easily host some computer projects which IIASA might like to undertake such as a general information system to access various data banks, or a computer aided design project. It would also be relatively easy to interface with other systems.

C. Proposal

We propose that IIASA have an in-house time sharing computing facility of medium-large scale (such as PDP/10/TENEX or a GE 645/Multics or a IBM 360-67/MTS). Here are some arguments for this proposal:

(a) In-house versus remote access

A large number of terminals is needed so there is at least a need for an in-house message switching system. The arguments for having all the system in-house are of two types:

- Economic: renting large file storage such as will be necessary for IIASA's needs appears to be exhorbitant at present rates.
- Reliability: computing facilities are so central to an institute such as IIASA that controlling them is vital. The risks involved in having little control or no control can be extremely high: for example, losing a project's or the whole of IIASA's files can be a major disaster. Also, the change of a computer system over which IIASA has no control may result in prohibitive costs for IIASA.

(b) Mini-computer versus medium computer

Current and anticipated mini-computer configurations will support at most thirty terminals, twelve being a more realistic number for good response. A mini-computer will also not adequately support a large file storage system. Therefore, a mini-computer would not be adequate for long term purposes at IIASA.

Now we argue that it is uneconomical to change computers after two or three years. Software development costs turn out to be a major budget item, and they are not significantly lower for a small computer. In fact, they would be much higher, since mini-computers come with a small amount of software, as opposed to the systems above mentioned which have a tremendous body of available code (utility packages, software tools, etc.). Acquiring one of the systems mentioned with a minimal configuration of core and disk file storage, then expanding it with the needs of the Institute, would therefore seem to be the right policy.

There are several projects that will not be feasible with only a mini-computer at IIASA. These include interactive modelling, medium to large linear programs, multi-machine information systems and large local files. In addition, the ability of the research staff to build models and experiment with methods will be reduced.

There are also a number of financial and other non-scientific considerations in machine purchase. The problem is exceedingly complex and is the subject of considerable current effort. The Computer Group is preparing technical recommendations for purchase in both classes of machine: medium scale and mini-computer. These will be ready for the November meetings of the Financial Committee and Council.

D. Some Services on the PDP-10 at Stanford A.I. Laboratory

Interactive languages
Interactive de-bugging aides
Graphics packages, picture input-output
Circuit design system

Text processing - almost all papers, etc. done in machine

Editors, context ability
Spelling checker
Text justification
Document compiler (format, references, etc.)
Font composer (for xerographic printer)
Search any file by keywords

Message processing

Local; direct and broadcast
Arpa net
From Associated Press News Service

Many programs for games, art, etc.
Personnel, Publications, etc. on files
System status - resources, who is where, etc.
All documentation in system
"HELP" command for each subsystem
Associated Press News Service + retrieval system
Accounting for lunchroom, sauna, etc.

This is all in addition to the primary research capability of the system.

V. Personnel and Timing

A. The personnel required will be as follows

Computer Services:	1 director 1 business manager
Information Services:	1 first-rate professional
Networking:	1 first-rate scientist capable of designing the hardware and software; 1 person to solve the political problems involved (This should not completely tie up these people)

Research:	1 or 2 scientists as available.
Programmers:	We should staff with people capable of working on all these projects as well as projects in other areas. I would expect the total number to be about ten. They should not be as expensive as scientists. Under certain conditions we will need 1 or 2 hardware people as well.

B. Timing

1. Computer Services: For the first year, the group will be mainly concerned with bringing up the IIASA system and aiding users with specific problems. In the second year work should begin on interface routines to ease the use of multiple systems. It is not possible at this time to predict the nature of problems to be encountered or how quickly the service components will come up to the standards described in earlier sections.

2. Networking: The timing here is fairly well worked out. The first two connections should be functioning before June 1974. In summer 1974 we will hold a workshop of one to three months on IIASA and other international networks. This will have two functions: detailed design of the second stage of the IIASA network, and formulation of a ten year plan in more general terms. The organization of this workshop is well under way.

3. Information systems: This will be an ongoing development program driven by Institute needs and the availability of proven software. There are some small projects underway. We propose a working group including personnel of Scientific Services, CS/CS and outside but local consultants, to coordinate the introduction of new services.

4. Research: This will be handled opportunistically. The most promising current topics are semantics and the development of combined heuristic and mathematical programming models.

PROPOSED SERVICE AND RESEARCH ACTIVITIES
in
METHODOLOGY

General

The primary mission of the scientists in the Methodology Section is to help support and service the methodological needs of the applied projects. But at the same time, if we want to attract good people, it is crucially important that the methodologists should have a vitality of their own. They should not be, merely, a service section, but they should actively seek to identify and examine those key methodological problems that are common to several of our applied research projects. Ideally, methodological activity and interests should flow naturally from the applied research we are pursuing.

It will be impossible to pursue in any depth a large number of these methodological problems. But let us keep in mind that the Methodology Section has available the same options we have alluded to earlier. They could:

a) pursue in-house research;

b) identify topics for collaborative research at IIASA and other institutions;

c) facilitate the exchange of information--the so-called "sophisticated clearing-house" or "agency" role;

d) convene international Conferences that are addressed to certain clearly defined methodological issues.

We have already identified the following three primarily methodological areas for in-house and collaborative research efforts:

1. Design and Management of Organizations
2. Optimization of Large Scale Systems
3. Computer and Information Systems.

These three research areas will be discussed separately in this report.

In addition, as a by-product of the Conference we have already held on our basic research areas, and of discussions at internally-held seminars, we have identified several integrating methodological themes that are discussed in some detail in Part III of this report. All of these themes will not be pursued at once because of lack of personnel, but they form a menu of topics that can help focus the research activities of our methodologists. In some cases it will only take one person--or even a part of a person--to fulfill a modest coordinating agency-type role for one of these themes. Other themes will simmer on a back burner and, occasionally, additional ingredients might be added in terms of interest, manpower, outside funding, attractive collaboration, and so on to make it desirable for us to intensify our research activities in one area or other.

Organization of the Methodology Section

If an individual researcher has skills that will be used exclusively by one applied research area (e.g. a hydrologist working for the Water project), then he will be assigned, naturally, to that area. There are other individuals whose work clearly cuts across our different applied research areas and they will be assigned to the Methodology Section. There will be other individuals that might be assigned in fractional amounts to the Methodology Section and to various applied areas. Lest there be some misunderstanding, let me say at the outset that we are seeking ways of achieving flexibility and integrability. If, to take an extreme example, a leader of one of our applied projects has certain methodological skills that are needed by another project, then he should, time permitting, play the methodologist's role for a while; it then becomes a purely administrative matter how we allocate his time in our program budget. I hope, however, we can be relatively relaxed about this type of ex-post budgeting--even though our administrators will say it's impossible. The point we are making here is that not all methodology will be done by individuals who wear methodological labels.

We have made a survey of the types of methodological skills and of the discipline oriented expertises that we should have access to. This bank of skills need not all be available in house. Some skills can be covered by means of consultants readily available in Vienna and surrounding areas, others may be located at more distant places but they should be clearly identified and have already expressed a willingness to help. Perhaps many of these contacts can be scholars who have at some time or other spent time at IIASA. Obviously this type of network will take time to mature.

For illustrative purposes let us take one methodological area such as probability and statistics. In some universities there are departments of probability and statistics with twenty or so members and a good number of courses they offer can only be taught by one man in the department. Obviously we can not hope to get coverage of this level of detail. Nevertheless a resident probabilist or statistician at IIASA may be sufficiently knowledgeable to help formulate methodological problems, know where to find things in the literature, and know whom to contact in order to get more detailed professional advice and help.

The bank of skills we need to draw on can be conveniently divided into four broad categories.

1. <u>Applied Mathematical Sciences</u>:

   General applied mathematics, optimization techniques (mathematical programming, control theory, heuristic programming, probability theory, stochastic inference, decision theory, stochastic control (inventory theory, queueing, etc.), game theory, simulation gaming, and so on.

2. <u>Computer Sciences</u>:

   Computer systems (hardware, software, telecommunications), information sciences (information retrieval, document preparation and handling); complexity of algorithms, data and control structures, artificial intelligence (natural language, heuristic search, machine perception); man-machine systems (including aids to decision makers) and so on.

3. Behavioral, Managerial and Policy Sciences:
Economics (general theory and econometrics), sociology, social-psychology, psychology, demography, organization theory, management information systems, management control systems, program budgeting, policy analysis, history, international law, and so on.

4. Engineering Sciences:
Biology, chemistry, physics, geology, meteorology, and so on.

Let me reiterate that in no way do we imply that the above list is in any sense complete nor that we intend to get depth coverage in-house of all these methodological topic areas. However, we expect that the above list represents the types of methodological skills that various applied projects will need. In order to get some reasonable coverage we shall need a few methodological generalists as well as specialists.

Handbook on the State-of-the-Art of Applied Systems Analysis

We quote from our pamphlet "Background Information Provisional Research Strategy."

> This project [Handbook] would review, synthesize, and critically evaluate the present status of ASA. It would prepare annotated bibliographies (designed for different audiences), compile pertinent expository papers, and would commission the preparation of other papers designed to clarify and summarize areas of importance. Its results would be published in the form of a handbook, which could be supplemented from time to time (perhaps in a journal form) with expository accounts of new developments and with in-depth studies of important areas. Such a handbook would provide a useful basis for discussion of the research areas of IIASA, would facilitate the development of common terminology, and may serve as background material for conferences and symposia on selected subjects. It would have an important educational role in the development of ASA.

It is proposed that the administrative responsibility for the preparation of this Handbook and related activities be under the supervision of the Methodology Section. A series of small conferences, both with in-house personnel and with the potential editors for this project, is scheduled to take place in November and December of 1973. A progress report will be prepared by December 15, 1973.

Proposal for Research on Optimization of Large Scale Systems

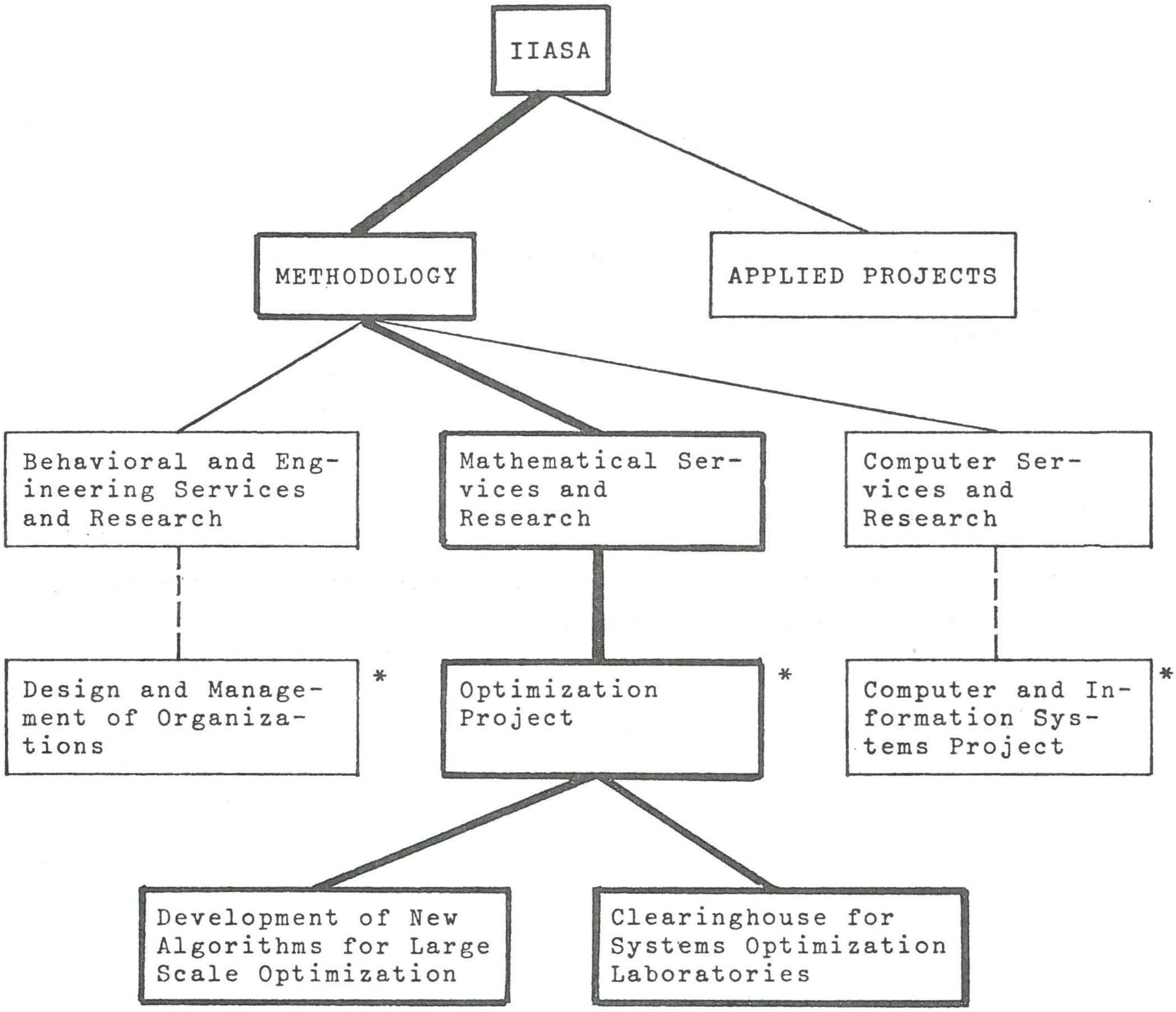
The methodological group at IIASA provides mathematical and computer services to the applied projects as well as performing research on its own. Within the methodology group there will be three primary research areas:

1. Design and Management of Organizations
2. Optimization of Large Scale Systems
3. Computer and Information Systems

This report gives a proposal for the second of these areas. It is emphasized that research into other topics of optimization theory not covered by the project could (and should) be carried out by individuals in the methodology group.

As will be seen later, it is proposed that the Optimization Project consist of two parts - a team working to develop new techniques for handling large scale problems and a group performing the role of a clearinghouse for computational experience in algorithms for solving large scale problems.

It is important to relate the large scale optimization project to other efforts within IIASA. The following chart is intended to make these relationships clear:



Recommended Manpower for Mathematical Services and Research

It is expected that the people in this group will divide their time evenly between project or individual research and provide in-house services such as advice on modelling and computer aids.

---

* Indicates methodological areas of sufficient importance to embrace project endeavors of their own.

Mathematical Programming (Linear, Nonlinear Programming and Combinatorial Optimization)	4 men
Optimization Clearinghouse	2 men
Control Theory and Dynamic Programming	2 men
Statistics and Stochastic Models	1 man
Game Theory and Sequential Decisions	1 man
Numerical Analysis	1 man
Total	11 men

Of the above, about half will be application and computer oriented with the remainder of a theoretical nature.

The Views of the Participants in the Research Planning Conference on Optimization and Control of Complex Dynamics Systems held October 3-5

The participants were quite clear that the problem of successfully applying systems analysis to real world problems lay in the ability to solve large-scale optimization problems, both in mathematical programming and control theory frameworks. Many speakers mentioned the vital importance of integer programming both per se and because discreteness often provided a small but very difficult component of large systems. The problem of decomposing and aggregating large systems was repeated by many participants. It was felt that the group should research into developing new procedures for decomposition and aggregation, and having done so, communicate with modellers to teach them how best to formulate problems so that the resulting optimization problem could be solved.

It was a common feeling that although a wide body of theoretical literature exists in the area of large-scale optimization, what little actual experimentation with the implementation of these algorithms exists, is badly documented. It was therefore emphasized that if IIASA could play the role of a clearinghouse for computational experience in large-scale optimization algorithms, systems analysis would benefit greatly. Although it would be even better to have a systems optimization laboratory in-house which could systematically compare algorithms on representative models, it was felt that the budget constraints of the Institute at the present time may not be sufficient except as it contributes directly to solving models in the applied areas.

Suggestions for Research

There are two separate roles outlined above:

1. Research into the development of new techniques for solving large-scale optimization problems.
2. Providing a clearinghouse for computational experience with existing procedures.

While they are to be a part of the same project and may have a certain overlapping of staff, they should be considered as distinct subprojects.

The following two sections of the report give the proposals for each of these two areas.

PROPOSAL FOR RESEARCH IN THE DEVELOPMENT OF TECHNIQUES FOR SOLVING LARGE-SCALE OPTIMIZATION PROBLEMS

It is likely that all the major projects will produce large-scale optimization problems having linear, nonlinear, integer or control structure, and in some cases, aspects of all four.

Some examples of how large-scale problems arise are listed below.

A. INVESTMENT PLANNING (INTERTEMPORAL ALLOCATION): Problems of aggregate economic planning for a (developing) country, present an exploitable special structure that has been studied intensively and has great potential. Related structures occur in problems of dynamic programming and optimal control. Related but more complicated structures arise, for example, in problems of plant location and time-phasing, and in investment planning in general in the firm.

B. DECENTRALIZED ALLOCATION: The origin of the modern methods of decomposition, and still one of the major areas of application, is the class of decentralized allocation problems, in which scarce resources are to be allocated among several otherwise independent enterprises or "division". Closely related is the class of problems of two-stage allocation under uncertainty, for which in the linear case, it is known that the dual problem is one of decentralized allocation. It is of particular importance to realize that the "divisional subproblems" may themselves be of a special structure (e.g. a transportation problem) which can be exploited.

C. ENGINEERING DESIGN AND OPTIMIZATION: A variety of engineering design and process optimization problems present specially-structured mathematical programs for which the structural features are highly dependent on the process being studied. Problems of this type illustrate the need for a flexible and comprehensive software package from which components can be drawn to build up models of very complex systems.

D. ENERGY AND WATER SYSTEMS: Up to the present the production, transmission and distribution of water and energy have been considered mostly as a fragmented problem. Now both energy and water are being considered as a system embedded in socio-economic system in which risks, effects on ecology, economic resources all play a role. Models of these complex systems give rise to large-scale optimization problems.

E. PHYSICAL, BIOLOGICAL, AND ECOLOGICAL SYSTEMS: A number of problems in the physical sciences (e.g. X-ray crystallography) and biological sciences (e.g. models of body processes) present specially-structured mathematical programming problems. An extreme example are models of ecological systems in which the many and varied relationships among the components again require a flexible and comprehensive software package.

F. URBAN PLANNING: Coordinated planning of the many component subsystems (e.g. transport, recreation, education, etc.) of an urban environment presents a complex systems optimization problem for which ordinarily the most powerful and flexible methods are required.

G. LOGISTICS: Coordinated logistical support for any large industrial (e.g. warehousing and transport) activity normally presents a system optimization problem of considerable size and complexity, but with exploitable structural features.

H. TRANSPORTATION SYSTEMS: Various problems concerning the design of transportation systems can be formulated as network optimization models of a combinatorial nature. These models typically have very special mathematical-programming structures for which highly efficient algorithms can be devised.

Much work has already been done in the linear programming case where problems having special structure have been studied; for example, network problems arising in transport planning, "block-diagonal" problems arising in decentralized allocation problems, "staircase" problems arising in dynamic investment planning, economic growth models and optimal control, "column generation" problems arising in production scheduling and elsewhere and general problems having the property of sparseness. There has also been work on the problem of decomposing a large problem into smaller sections from whose solution the solution to the large problem may be derived.

The aim of this project is to develop techniques such as these, not only for linear problems, but for the general problems that are also encountered in large-scale systems.

The three main areas for research are summarized below.

1. Decomposition Techniques

This area will contain the main thrust of the research, improving techniques for solving specially structured problems ("staircase" and the like) and constructing procedures for "nearly decomposable" problems. In this general class one finds a macro-structure which would be perfectly decomposable into independent subproblems except for the presence of a relatively few connections (and therefore interdependencies) among the subproblems. The development of efficient algorithms for nearly decomposable problems is a major area for research and one for which the range of applications is enormous. Its successful conclusion may require the development of general methods for highly connected systems such as Benders decomposition method for mixed integer programming.

One preliminary task in the development of decomposition methods would be the construction of an efficient taxonomy for system structures. This task is only partially complete. The major taxonomic features that are well understood can be described briefly as follows. First, there is a large and important class of problems whose special structure permits the design of an efficient algorithm based directly on this structure. Usually, duality and compact representation schemes play a key role in the design of the network problems, problems with upper and lower bound constraints, and a number of nonlinear problems (geometric programming, fractional programming, variable-factor programming, etc.).

Often problems with these special structures occur as subproblems in larger systems and it is therefore important to have available efficient, tested, and documented routines for these problems which are easily callable.

A task related to decomposition is the complementary technique of aggregation which simplifies systems to make optimization easier.

2. Integer Programming and Combinatorial Optimization

Many large-scale problems contain a subset of variables for which non integer values make no sense; for example, with zero-one decision variables. In addition, it is possible to formulate a number of important, but difficult, problems of a nonlinear, non-convex and combinatorial character as mixed integer linear programming problems. Using, for example, Benders decomposition method requires a pure integer programming algorithm as a subroutine. Therefore, research should be carried out to examine the nature of the discrete subproblems occurring in large-scale problems, and to provide codes for their solution.

Problems of aggregation, stability, sophisticated look-ahead rules in Branch & Bound, and bounding all need considerably more research in order to develop efficient algorithms. Powerful new bounding techniques using Lagrangian Dual problems warrant further study and implementation.

3. Large Scale Control and Dynamic Programming Problems

Many of the problems encountered at IIASA will be formulated in a control theory framework, so some research should be conducted into the handling of large-scale problems of this type.

Complex system modelling and identification of general problems is important together with study of problems having special structure such as:

- hierarchical structures common in the area of managing organizations,
- tandem structures found in industrial processes such as a chain of chemical reactors,
- cooperative structures in economical models (e.g. Leontief),
- PERT graphs and many others.

The optimization of these problems is, of course, very much related to the decomposition theory of the first section, and the need for synthesis and aggregation is also apparent.

The tools for optimization in this area are generally of a non-classical nature with techniques such as the Maximum Principle Dynamic Programming and Linear and Nonlinear Programming in Banach spaces.

All these areas have applications within IIASA; for example, in Complex Water Systems, Integrated Electrical Power Systems, and Design and Control of Management Systems.

Proposal for a IIASA Clearing-House for System-Optimization Laboratories

It is proposed that IIASA serve in a coordinating role fostering the development (in various countries) of system optimization laboratories and that IIASA collect and disseminate information about software availability, the results of experiments on optimizing representative models by various laboratories, individuals, and groups, or from published reports.

It is not proposed (because of budget limitations) that IIASA itself, at this time, set up a formal System-Optimization Laboratory for systematic comparison of various methods of optimization. Instead, it is envisaged that IIASA will carry out this role in a less formal way as part of its "Optimization of Large-Scale Systems Project". This project, in the course of its work, will develop or acquire software for the solution of large-scale models of water, energy, eco-systems, etc. This will bring about in a natural way a two-way flow of information between the methodology activity and the clearing-house activity just described.

The Need for System Optimization Laboratories:

We now describe why IIASA should foster the development of system optimization laboratories in various countries.

Background: From its very inception, it was envisaged that techniques like linear programming would be applied to very large, detailed models of economic and logistical systems. Kantorovich's 1939 proposals, which were before the advent of the electronic computer, mentioned such possibilities. Since the introduction of the Simplex Method of linear programming in 1947, the power of the methods of mathematical programming, and the range of effectiveness of its applications, have grown enormously. In the intervening decades the methodology has been extended to include non-linear and integer programming, dynamic programming and optimal control, and a host of other types of optimization problems. The range of applications has been extended from simple allocation problems to an enormous variety of problems in intertemporal allocation and investment planning, engineering design and optimization, and scientific studies of physical, biological, and ecological systems. There is, in fact, no end foreseeable to the applications of mathematical programming, to a number of important (and crucial) optimization problems.

Society could benefit greatly if certain total systems can be modeled and successfully solved. For example, crude economic planning models of many developing countries indicate a potential growth rate of GNP of 10% to 15% per year. To implement such a growth (aside from political difficulties) requires a carefully worked out detailed model and the availability of computer programs that can solve the resulting large-scale systems. The world is currently faced with difficult problems related to population growth, availability of natural resources, ecological evaluation and control, urban redesign, design of large-scale engineering systems (e.g., atomic energy, and recycling systems), and the modeling of man's physiological system for the purpose of diagnosis and treatment. These problems are complex, are urgent and can only be solved if viewed as total systems. If not, then only patchwork, piecemeal solutions will be developed (as it has been in the past) and the world will continue to be plagued by one crisis after another caused by poor planning techniques. For solutions, these problems require total system planning, modeling and optimization.

Need for Laboratory Testing: The optimization of large-scale systems is technically an extremly difficult subject. Historically, starting with U.S. Air Force problems in 1947, linear programs were formulated to solve just such systems. These problems involved systems of interlocking relations involving many planning periods, functional units, types of personnel and supply. It led to thousands of equations in many thousands of unknowns. This was beyond computational capabilities. It was necessary to severely restrict the class of practical problems to be solved. Starting around 1954 a series of purely theoretical papers began to appear on how to efficiently solve large systems and by 1970 they numbered about 200. There was little in the way of implementation. Exceptions were the out-of-kilter algorithms for network flow problems proposed by Ford and Fulkerson (1958) and the "decomposition principle" of Dantzig and Wolfe which had been tried out with variable results (1960). On the other hand a more modest proposal of Dantzig and Van Slyke (generalized-upper bounds) has been very successful (1967). Apparently a great deal in the way of empirical testing of ideas is necessary and this has not been easy to do because the test models have to be complex to be pertinent and cost a great deal of money to program and solve. Therefore progress has been slow up to recently.

The purpose of system optimization laboratories, accordingly, would be to support the systematic development of computational methods and associated computer routines for the optimization of large-scale systems. The ultimate objective of the development effort would be to provide an integrated set of computer routines for systems optimization.

Research Projects of a Systems Optimization Laboratory

A major activity of System Optimization Laboratory would be the development of software packages for systems optimization. This development effort could proceed on two different levels. First, a major activity would be the completion of a macrolanguage for organizing and calling routines in the software package. Mainly this could be an extension of a macrolanguage Mathematical Programming Language (MPL) under development. The second major activity could be the programming, testing, and documentation of algorithms for decomposition and special structures, including experimentation with alternative algorithms, and testing of algorithms on practical problems.

Computer routines would be thoroughly documented, tested on standard problems, and written in a format compatible with and callable by the macro-language.

Development of Software that exploits model structure: It is the nature of human activity, and in large part of the physical world as well, that large and complicated endeavors are organized as systems of interrelated parts, and indeed, as systematic hierarchies of interrelated sub-systems. Such systems typically exhibit special mathematical structures. These special structures permit numerical analysis and optimization via methods that exploit the special structure, whereas general-structure methods would be infeasible if the problem is of the size normally encountered in practice. The extension of the range of applications of mathematical programming is, therefore, most promising for pressing world problems involving total system optimization discussed earlier since they exhibit special structures.

Recommendation:

To model and to solve a host of pressing world problems in the areas of population, food, energy, water, ecology, urban development, it is recommended that several System Optimization Laboratories be established. Solving large-scale systems cannot be approached piecemeal or by publishing a few theoretical papers. It is a complex art requiring the development of a whole arsenal of special tools. Large-scale system optimization requires laboratories where a large number of test models, computer programs, and special software tools are assembled in a systematic way. It is further recommended that software developed in the research be thoroughly tested on representative large-scale systems optimization problems arising in practice and be made freely and publically available to users of government, science and industry.

It is further recommended that IIASA serve as an international information center for these laboratories fostering their development in various countries, collecting and disseminating information about software availability and the results of experiments on representative problems.