LONG-RANGE POLICY OPTIONS FOR IIASA

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1. Preamble

Later this month the Council of IIASA will meet to consider many issues of which the most critical will be resolution of the long-term strategy for the Institute. Determination of the optimal strategy will depend upon our judgement of many factors - the appropriate nature of our scientific program, our constraints of money and of space, the opportunities for IIASA in the world of the late 1970's. These are inherently interrelated: the research we will be able to perform will depend upon available resources, while more resources will be forthcoming for the scientific program of greater promise. This document shall present briefly these issues, the policy choices arising from them, and my personal thoughts about our options.

1.1 The Time Frame

The Institute itself is barely two years old, its scientific program less than sixteen months old. We know now far more than in October, 1972, about the potential of an international institute performing applied research in systems analysis. We cannot, however, foresee perfectly the future and the spectrum of opportunities and obstacles which it holds. We are, in short, faced with the classic situation in sequential decision-making in an uncertain environment. We must choose the optimal path for the Institute over the next twelve months keeping well in mind the longer-term future and the uncertainties it holds.

1.2 <u>The Management Framework</u>

I have argued at length in previous documents sent to the Council the need for a flexible and opportunistic research approach. We have all come to perceive operant constraints which our scientific program must observe. Rather than to belabor again these points, I list briefly here the assumptions underlying the remainder of this paper:

(a) That the management of our program must be sufficiently flexible to seize the scientific opportunity and to adapt itself to changing circumstances;

(b) That we must not be afraid to embark upon experimental forms of our program - realizing that some experiments must fail. When possible, feasibility studies should be invoked to forewarn us of failures and to reduce their consequent losses;

(c) That we must learn from our present failures and successes;

(d) That we must remain administratively and politically realistic in program selection - avoiding for instance, undertakings too long to be feasible for us and striking a fair balance between the interests of socialist and non-socialist countries.

Notwithstanding the need for flexibility, our short-term and intermediate-term programs must be as concrete as possible. They should spell out the precise nature of our research interests and the magnitudes of all proposed program components.

The detailed proposed research program for 1975 is designed to describe as accurately as possible our present set of research objectives.

1.3 Preview

Taking the assumptions above as given, the remainder of this paper will address the question of the appropriate long-term role for IIASA. The succeeding section will consider the program of in-house research while the next will discuss possible extensions of the program to include development of training materials or the organization of educational workshops. Subsequently, the possibility of engaging less formal modes of scientific exchange will be presented. A possible way to focus better and integrate our entire range of activities might be through periodic concentration by all projects on a single over-riding theme. The final section will discuss specific research tasks that might in the future be suitable for elevation to project status.

2. Primary Activities of IIASA

2.1 Research in Laxenburg

2.1.1 Introduction

The cornerstone for all scientific activities of IIASA must be a viable in-house research program in Laxenburg. We must have a basic core of scholars maintaining a level of intellectual integrity that will give ourselves confidence in our capabilities. To settle for any standards of professional work short of excellence will be to dishonor the support given us by our many prestigious national member organizations.

Without an outstanding critical mass of talent in Laxenburg, we would not be able to perform well other functions in conjunction with our intramural research. We would not be able to collaborate on a higher level with other organizations; we could serve only as an automatic relay in information exchange and not as a critical node for commentary and organization of transmitted information; we would not be in a position to identify key issues for conferences or to structure actively and to contribute to their discussions. The very spirit of international cooperation in science symbolized by IIASA must be rendered concrete in Laxenburg in a research program enthusiastically pursued by cross-cultural teams of outstanding scientists.

To maintain the quality of our scientific staff, we must formulate our long-range plans so as to enhance the attractiveness of IIASA as a place to work. We must specifically avoid long-term research commitments and administrative arrangements that make IIASA appear to be "just another agency". So far, we have been unique in our ability to accept distinguished scientists for relatively short periods of time (generally corresponding to sabbatical leaves and summer vacations) and to allow them to pursue their research unhampered by an overwhelming burden of administrative paperwork and bureaucratic infighting. Our long-range plans must include provisions for maintaining these unique qualities. When we propose the addition of activities other than research to our program, we must minimize the extent to which such activities add to scientists' administrative workload. Furthermore, as we detail guidelines for long-range development, we must be careful lest we delineate our program so precisely that senior scientists would feel intellectually cramped working at IIASA. We must strike a balance between our need for long-term direction and the perpetual necessity to attract outstanding scientists.

2.1.2 The Project Structure

Our project structure, with all its inefficiencies, is working both administratively and scientifically. The division into sub-groups of scientists results in an intimacy and informality which should be encouraged. We have some very prestigious project leaders that feel a responsibility for their groups and the research groups have a viability of their own. Some projects are admittedly better than others: more productive, more goal-oriented, scientifically deeper. Operationally, the tension of competition between projects is desirable. From a recruitment and administrative point of view it is easy to administer and decisions can be somewhat decentralized and involvement increased. Various NMO's - especially members other than the U.S. and the U.S.S.R. - can pick and choose those projects it wants most to support with personnel and with the efforts of collaborating institutions.

At first I thought it was only politically expedient to include so many different projects. Now I'm beginning to think that the large number of projects was a scientific advantage as well. It afforded us some flexibility and allowed us to begin our internal research activities relatively free from conflict about which research topics would have been included (and excluded) on the much narrower research agenda of only a few projects. During our formative years, our initial project structure has allowed us to begin work quickly and push ahead in several methodological and applied problem areas.

To maintain ourselves at the frontiers of systems science and to perform research of greatest value to our supporting members, IIASA must learn to shift its program emphasis in step with a changing world. No single set of projects can permanently capture the range of scientific opportunities most inviting for IIASA. We must be prepared to terminate projects that have completed their tasks or have outlived their usefulness. Other projects it may be more appropriate simply to shift from an intramural to collaborative status: reducing staff in Laxenburg but continuing to serve as coordinating node for continuation of the research in other instances. We may be able to coalesce projects pursuing similar research into one project. Perhaps we should establish another echelon above the project level composed of project groups: two or more projects with allied interests which would be originally linked and would better integrate their work through mutual support. In doing so, we would emphasize the work of the "super-projects" (an example of which might be the grouping of Water, Energy and Ecology in a Management of Natural Resources super-project) over that of the current projects. Although we would risk creating unnecessary bureaucracy, we would have the opportunity to achieve meaningful project integration and focus our resources on a few critical questions. Certainly, reducing the number of projects in a gradual way makes administrative sense, and I believe that scientifically, it would help us coalesce and move toward a more-integrated Institute with minimal disruption of ongoing research.

2.2 Research Versus Training

2.2.1 Initial Research Orientation

The Council has chosen to concentrate in the first years of the Institute upon the research role rather than upon the training role. This choice was so necessary that it cannot even be called wise. So few institutions anywhere in the world are presently performing interdisciplinary applied systems analysis that it would have been presumptuous for any newborn organization to attempt to teach it. No other institution performs such research from a cross-cultural perspective. Our first imperative was to prove in practice that the systems methodology could successfully be applied to the real world.

2.2.2 The State-of-the-Art Survey for the Handbook/Series

Our initial venture into training activities is measured and modest. The IIASA Handbook/Series on Applied Systems Analysis (ASA) will have the dual purpose of: (a) providing a basic state-of-the-art understanding that can serve as foundation to our subsequent research; and (b) giving scientists and practitioners elsewhere an improved picture of what ASA is, what it does, and how it optimally may be utilized.

2.2.3 Policy Option 1: Future emphasis on preparation of training materials for specialists, practitioners, or users of ASA

With our research program now well underway, we may now take advantage of our living core of researchers to create instructional materials on ASA. We could produce educational textbooks or we could be bolder and experiment with audio-visual media that would have better chance to reach a broader population. Such materials should be especially valuable for developing nations. The drawback of this option is that it would draw upon our too finite resources - of men, money, and space - and that it therefore might detract from our primary goal of successful research.

(My opinion: We should begin this effort in a modest way in 1975. Operationally, the easiest means of doing this is by orienting the state-of-the-art survey to investigate the types of materials needed and those that could readily be provided. Actual creation of materials should be of no more than experimental magnitude.

I believe that we should defer major production of teaching materials until two or three projects have produced major results. At that time we will have as a basis for our materials an example in which systems analysis has a visible influence on the decision making process in one or several countries. In the distant future, I hope that IIASA could produce such materials as television tapes, and computer-aided instruction courses. This would require audio-visual studios, and a cadre of professionals who would consult with the scientific staff on content but would exercise primary responsibility for the actual production of materials. Alternatively, these activities might partially be decentralized to other institutions - both scientific and commercial. Done properly, preparation of instructional materials would generate a substantial income flow that might completely cover costs. I suspect though that external subsidization in the first year of this effort will be necessary. My recommendation: that experimental investigations at a modest level be initiated in 1975.)

2.2.4 Educational Programs

We frequently have been asked if members of our staff could be made available for lecture seminars on ASA. Since our overriding concern until now had been our research program, our response had to be negative except in cases when our scientists could make special private arrangements for their vacation periods. With an established research program now underway, we should consider whether initiation of more formal training programs could be an inexpensive and beneficial spin-off activity.

Educational programs would link naturally with the provision of instructional materials described above and might be one means of bridging the gap between analyst and practitioner. They would benefit us by enabling contacts with real decision-makers of all levels who would give us practical evaulation and feedback upon our work. Like the preparation of materials, training programs would require a small number of additional, non-scientific professional staff, but should be a net money-maker after an early and short period of subsidization.

2.2.5 Policy Option 2: Educational training programs for (a) specialists, and (b) non-specialist users

(My opinion: This has undeniable potential for IIASA, but, as in the case of creating educational materials, we should start modestly. IIASA should not become a degree-granting institution for younger scientists but should address itself through workshops of short duration to middle and upper-level managers. We might start on a decentralized basis - perhaps with programs based in Japan or the USSR. Collaboration with other institutions that would handle the administrative burden might be an optimal permanent arrangement. The Handbook Survey again would be the most appropriate part of our present program to undertake initial experimental investigations of feasibility.

We should not embark upon a series of teaching programs unless we can do them well. This does not mean that we have to insist upon overnight excellence - our initial efforts will inevitably be less than ideal. If, however, our programs are of such quality that demand - and potential revenues - remain low, then they must either be upgraded or be dropped.

We must also not lose sight of the spin-off nature of such activities. We would be undertaking them: (1) because they would supplement our research by giving us direct access to decision-makers, and (2) because they would encourage us to package our research product for maximum clarity in communication. They would help us to avoid the scientist's trap of writing only for and communicating only with other scientists. Nevertheless, if preparation of educational materials or participation in training programs detracts from our research through significant preemption of resources, then they must be cut back. Our highest priority is our research program.)

2.3 Informal Scientific Exchange: Policy Option 3

As we consider long-term research options open to IIASA, we must recognize the systems component of critical world problems - arms control, law of the sea, economic trade - which we may not include in our formal research program. Increasingly, scientists in every country are advising their governments on these problems; and in many cases, I think it is a fair assessment that scientific advisors may not be fully aware of the international and "systems" implications of their advice and resulting policy decisions.

One way IIASA can ameliorate the situation is by making the Institute a home for informal scientific exchange between senior scientific advisors from many countries. For periods ranging from a few weeks to a few months, scientists dealing with a particular problem in different national contexts could come to IIASA for unstructured discussion and research. Although we would perform some background work - library research, readying computer programs, etc. - no papers would be requested, no official minutes would be recorded and there would be no pressure for written presentation of results and conclusions. Scientists would be able to share their ideas and become aware of the international aspects of major problems free from the formalities of diplomatic negotiations and without committing their governments to a policy course.

(My opinion: At first glance, informal scientific exchange appears to be an option that might add substantially to our administrative burden and our space "squeeze". I do not believe this to be the case at all: the exchange workshops should be self-financing, perhaps with national governments paying a fee which would cover our overhead. Thus, while the exchange would require effort and space on our part, the requirements would not be met at the expense of our research program.

Since the administrative preparation for an exchange workshop should be extensive, I would recommend against scheduling one in 1975, but I suggest we think about hosting such a seminar in the summer of 1976. In the meantime, we should evaluate potential topics with an eye toward selecting a topic which meets governments' needs <u>and</u> which might be included in our own general activities or offer a needed perspective to our formal research projects.)

2.4 <u>"An International Year": Policy Option 4</u>

Our current research format calls for extended periods of activity on many fronts. The typical pattern of project development is the buildup of a critical mass of moderate size for an expected duration of several years. Alternatively, we might implement a more concentrated research program whereby we develop research themes toward "International Years" on various applied topics. For instance, research on a topic might be initiated with 10% of our total manpower and budget. After a year or two of preparation and background research, the project (or topic) would be allocated perhaps 30% of available manpower and resources for the International Year (or Years) on that topic. Other projects would be expected to devote as much as half their effort to the theme, and we would actively seek to coordinate parallel research efforts in several different countries. Following the period of the International Year, the theme would again be allocated about 10% of our resources for a year or so in order to follow up on initiatives of the International Year, perhaps decentralizing further research to collaborating institutions.

(My opinion: For an International Year to be a successful catalyst of world scientific opinion, the administrative and scientific preparation for it would require time and effort. Before IIASA can embark on such a venture, we must solidify our scientific reputation and build stronger ties with other scientists within our member nations.

The scientific advantages of an International Year could be immense. A burst of energy in any of several applied areas would attract sufficient attention for many of our hopes for international scientific cooperation to be realized. However, I recommend that if we decide in favor of an "International Year" approach, we experiment with it rather than rushing to make it standard policy.

I would suggest if we sponsor an International Year, we sponsor it beginning in 1977 at the earliest, leaving time for us to prepare our internal organization by planning the necessary reduction in our project structure and cultivating our liaison with external institutions.)

2.5 IIASA "Affiliates": Policy Option 5

The "in-house" research activities described in the options listed above connote research only in Laxenburg. Yet, challenging opportunities exist for IIASA to assemble a research team to work on a problem outside Laxenburg. Such teams could draw on expertise in Laxenburg, but their primary tasks would be problem solving within one country or cultural context. The precise role the team chooses to play could vary. At one extreme, the team could perform a consultant's role - establishing itself in a country to deal with a particular problem. On the other hand, the affiliate could mobilize the scientific resources of the country to develop national potential for ASA. In either case - or in the more likely event of the affiliate performing some combination of consultant and mobilizer roles - affiliates would be initiated only with the moral and financial support of the "host" country.

(My opinion: IIASA affiliates could be a mechanism through which we move in several of the research directions I discuss below. For instance, affiliates would provide excellent opportunities for devoting more of our attention to the problems of developing countries. They could afford us the chance for more dialogue with decision makers and for more "real world" problem solving. However, I am hesitant to endorse the IIASA affiliates option because it presents the danger of rapid multiplication of the administrative burden in Laxenburg. My own suggestion is that IIASA be ready to spawn research ventures in various countries with a clear understanding that such ventures quickly become administratively independent and part of the scientific establishments of those countries. While we could encourage collaboration between IIASA and affiliates, IIASA would serve as the center of an international network of systems analysis groups rather than the manager of numerous affiliate organizations.)

3. Research Directions

Inseparable from questions concerning the selection of research activities are the questions concerning the broad research directives within which specific research activities are planned. Is the present mix of "global" and "universal" problems appropriate? Should IIASA deal more aggressively with problems of developing countries? These are questions we must consider carefully, for the decisions we take on these issues will provide the framework within which specifics of our research program will be decided. They shape the broad course of scientific activity for the intermediate and long-term future.

Our decisions should result from deliberation rather than coincidence, and to assist our decision process, I outline below the salient issues surrounding answers to some of the major questions concerning long-term research directives.

3.1 Research Option 1: More Global Programs

IIASA's original research agenda included both "universal" problems (those problems like delivery of urban emergency services faced by many countries within unique contexts) and "global" problems (those, like pollution of the oceans and the atmosphere, faced jointly by many countries). Quite honestly, our initial research may have tilted slightly toward the universal. Now, as we plan for the future, we must decide whether to maintain our original orientation or to move deliberately from universal to global problems.

> (My opinion: Since the delineation of our original research agenda, global problems including food and agriculture, population, and economic relationships between supplier and "consumer" nations, have increasingly come to the center of world attention. In the process, two additional factors: (1) the increasing recognition by many nations of their interdependence and need for cooperation; and (2) the necessity for common understanding as a background for this cooperation have so sensitized the world environment that I believe IIASA must increase its global orientation.

This belief is reinforced by our own success in opening communications between institutions in member countries. Now, we may be able to deal effectively with universal problems by way of our "clearinghouse" function and afford more of our manpower to global issues. In 1975, we have the opportunity to assess our role in one global problem area, food and agriculture, since that area is funded as a General Activity. I recommend that we use this opportunity to plan future involvement in food and agriculture. In addition, I urge that we use General Activities as a "home" for the assessment and planning of future IIASA involvement in other global problem areas. One area into which we could easily move is the analysis of catastrophes. Such research would integrate well with our developing work on resilience and could proceed along the following themes:

(a) the case of distorted climates;

(b) the case of a disturbed ecosystem;

(c) the case of large radioactive releases;

(d) the case of biogenetic catastrophe;

(e) the case of disrupted food supply;

(f) the case of disrupted supply of power, water or essential raw materials.

Another area suitable for early IIASA involvement would be "global monitoring", an effort in which our current Ecology, Water, Computer Science and Methodology projects could cooperate. My recommendation is that in 1975 we explore these and other areas so that we can make a concerted effort to shift to more global concerns in 1976 and beyond.)

3.2 Research Option 2: More Problems of Developing Countries

As we recognize the critical, global nature of problems such as population and food production, we find ourselves face to face with problems of the "developing" as well as the "developed" world. For our first two years, our research topics have been generally of more interest to industrialized rather than to non-industrialized nations. The question for the future is whether our research program should evolve to devote increasing attention to problems of the developing world.

(My opinion: The factors weighing in favor of our devoting increasing attention to the "developing" world are threefold: First, many of the problems faced most directly by the developing nations (population, food and agriculture) are simply too serious for us to stand aloof. They will require international action including considerable input from developed countries if they are to be abated and eventually solved. Second, the developing nations are eager for the types of technical assistance ASA can offer. Finally, many of the concerns of the industrialized nations can be dealt with by technology and scientists within those nations, and we can encourage this through the formation of collaborative networks.

Of course, in devoting more attention to the developing countries, we encounter a host of difficulties which will be resolved only by a slow transition from our current research activities. If we add problems of developing countries to our existing research program, we risk diffusing our scientific activity to the point that we will make few significant contributions in any field. If we choose to devote less attention to the developed world, we will be forced to abandon work in which some of our current NMO's are most interested. As well, in taking on problems of developing countries too quickly, we risk premature involvement in politically sensitive questions.

My recommendation is that we move towards the inclusion of the problems of developing countries, but that we do so on a cautious timetable. Our initial steps should be in the direction of research topics of interest to both developing and developed countries. Food and agriculture certainly falls within this category, as does the problem of emergency relief during catastrophes. Research in these areas can begin in the near and intermediate term future, after which experimental work on more sensitive problems might begin.)

3.3 Research Option 3: More Politically Sensitive Areas

As we pondered the content of our original research program, we consciously avoided including politically sensitive questions such as law of the sea and arms control. I think we were correct in assuming that such issues would threaten the fabric of the young Institute. As we now ponder the long-term research plan, we must reconsider the inclusion of politically sensitive questions in our program.

(My opinion: Questions such as controlled use of nuclear power and law of the sea are, quite honestly, too interrelated with our current research to be excluded from our program indefinitely. Yet, the threat they present to our future has only abated, not disappeared. I believe we must move cautiously toward the day when politically sensitive questions will be an integral part of our research structure. To exist in a political vacuum is to become irrelevant, but to embark too soon on research into very sensitive areas is to risk the deterioration of our scientific integrity into ideological debate. We must seek a compromise, assessing carefully which questions we can or cannot address and still maintain our basic scientific orientation. We should not become politically embroiled at the expense of our scientific progress.)

3.4 More Client Orientation

Throughout my discussion of options for research activities and directions, there have been references to increased contact with decision makers. An example is the Handbook/Series and option of training materials, with which we are moving towards a limited dialogue with the policy makers who utilize ASA. More fundamentally, we must decide the extent to which IIASA will seek to implement its research results in actual planning and decision-making contexts. In delineating a research direction, we must establish guidelines for future decisions on the selection of clients and the policy problem on which we will work.

(My opinion: Ideally, I believe we would maintain an ongoing dialogue between analyst and an array of potential clients so that we could share the decision-maker's implementation problems, and he could share our analytical approach. In reality, we are a long way from such a dialogue, and we are painfully aware that developing it is a tedious process. We have few, if any, examples to follow.

We should not become a consulting firm with rigid deadlines; yet we would be wise to incorporate some aspects of the consulting routine into the conduct of our research. I urge that when we seek "clients", we not prostitute our scientific standards for "easy money". As we develop a dialogue with decision-makers, we should remain financially independent so as to preserve our scientific stature.)

3.5 More Concern for Implementation

Regardless of our decision on the selection of clients, we cannot escape the fact that our research thusfar has been dominated by technological concerns. Little attention has been devoted to the social and institutional problems which will be encountered in the implementation of our results. Our capacity to deal with the myriad national, regional and municipal agencies responsible for energy, water, environmental, and urban management is all too limited. One of the challenges we must face in planning for the future is strengthening our managerial perspective in order to complement our growing capacity to model physical realities.

(My opinion: We should develop our ability to cope with social, managerial and institutional problems as early as 1975. I recommend that we increase our complement of social scientists (sociologists, psychologists, economists), lawyers and management experts in LOP and within the applied projects. I suggest a planned increase in LOP manpower from 5 to 10 man-years in the next two years. Through integrative activities, the management perspective should permeate the applied projects. The closer we move toward the presentation of scientific results and the more we include global and/or politically relevant problems in our research program, the more we will need to understand the social and institutional implications of the technological changes we propose.)

4. Closing Thoughts

The considerations that have been raised in this paper cover many questions upon the long-term future of IIASA which cannot all be resolved in November, 1974. This is a sequential decision process in which we must be prepared to learn as we progress and to take advantage in our actions of our growing knowledge.

The essential minimum of decision and commitment to be made later this month concerns our program for 1975. We have proposed a modest extrapolation of ongoing activities that recognizes our constraints of finance, space, and manpower.

Even if the formal decisions taken this month relate only to the coming year, we must think hard about our longer-range future. This document has - for reasons of conciseness - dwelt only upon the scientific aspects of the future, but the organizational implications of our research decisions must also be considered. The long-term financial basis of our program, the way in which the program should be housed in Laxenburg, and the strategy of our staffing are all directly connected to our scientific decisions and must be so resolved. Also important to our scientific appointments, the way in which we maintain linkage with our scientific alumni, and our hopes for development of the Laxenburg community. Each of these issues could be extensively debated and innumerable imaginative options generated. It is our misfortune that time is only finite and that our main discussions cannot be deflected from the critical decisions that must be made for the 1975 program. These decisions should, however, be taken in the light of our hopes and plans for a more distant future. This paper attempted only to present concrete examples of policy alternatives that we may wish to include in those hopes and plans.