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"PROBLEMS OF SCALE"--ISSUES IN AN APPLIED
SYSTEMS ANALYSIS PROJECT

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ABSTRACT

The evolution of the research task "problems of scale" is described. The emphasis is on the key decisions involved in the conduct of the research and the way in which perceptions of the research topic changed. The paper concludes with a discussion of the features of the style of applied systems analysis used in the task.

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TABLE OF CONTENTS

INTRODUCTION	1
EVOLUTION OF THE RESEARCH: ACHIEVEMENTS, KEY DECISIONS AND FOCI	2
Phase 1. Origins	2
Phase 2. Review of "Problems of Scale"	4
Phase 3. Preparation and Holding a Workshop	7
Phase 4. Workshop Proceedings	10
Phase 5. Research on Scale in Investment Planning	12
Phase 6. End of Task	14
THE FUTURE. WHERE SHOULD RESEARCH ON SCALE SCALE GO NEXT?	15
"PROBLEMS OF SCALE" AS AN APPLIED SYSTEMS ANALYSIS PROJECT	16
CONCLUSION	18
REFERENCES	20

"Problems of Scale"--Issues in an Applied
Systems Analysis Project

John A. Buzacott

INTRODUCTION

The purpose of this paper is to summarize and review the research task on "Problems of Scale," part of the research activity in the Management and Technology Area (MMT) at IIASA between January 1978 and July 1980.

A feature of the research task was the way in which the focus of the research changed and evolved with the development of ideas and understanding about "problems of scale." There were a number of distinct phases of the research which can be identified. Each phase was marked by a key decision which determined what was done and what was achieved. Furthermore, it seems that each phase can be characterized by a particular issue or focus which describes the participants' view of the research question.

This paper discusses

- the major achievement, key decision and focus of each phase of the research;
- future research directions arising out of the project;
- the extent to which the research can be considered to exemplify applied systems analysis.

It is written by a participant and thus reflects a personal view. However, because of the short length of stay of scientists at IIASA, recording experience on this task may have some value to the planning of other research tasks.

EVOLUTION OF THE RESEARCH: ACHIEVEMENTS, KEY DECISIONS AND FOCI

Phase 1. Origins

Focus: A General Methodology for Determining Size

In the introduction to "Scale in Production Systems" Rolfe Tomlinson described the two situations which led him to see problems of scale as a topic meriting research. In hospital planning there seemed to be no adequate analytical base for making decisions on hospital size. Similarly, when the National Coal Board was able to start planning new mines after a 25 year hiatus, he found that there was not a satisfactory analytical base for deciding on the size of the mine. There was

no generally available methodology for studying problems of scale, even though apparently similar problems appear in many industries, mistakes are widely recognized as having occurred.

There was a

widespread concern with regard to the general trend towards "giantism," and a lack of an accepted methodological approach.

Characteristic of the situations which aroused Rolfe's interest in problems of scale were:

- the need to respond to a changed environment, i.e., in both situations there was no experience in making scale decisions and the need to make such decisions arose because of a change in the environment;
- there was considerable uncertainty associated with the relationship between size, performance and cost of the technology, not only lack of knowledge but also inherent unpredictability;
- the decision concerned the size of an essentially single product facility requiring major capital investment;
- the decision did not require considering alternative locations or alternative production processes.

It was Rolfe's view that a general methodology for solving such problems of scale could be developed. By a general methodology he did not mean a common mathematical procedure, such as a mathematical programming model, nor even a formalized problem solving approach such as decision analysis. He sought an integrated and general set of procedures and analyses which could be used to arrive at decisions on scale. It should be oriented at the decision maker and his advisors.

In discussion concerning the possible research directions for the MMT area he found that there were problems of scale

concerning super tankers, electric generating units and hospitals. Thus scale seemed to be a common problem of considerable significance and hence appropriate to IIASA research.

Key Decision: Problems of Scale or Problems of Planning Major Investments

The question which arises is why look at problems of scale? Should not one look at problems of planning? Subsequently, the Industry Studies Task began to look at problems of planning--"planning for planning" within the specific context of the coal industry. So in fact research on both problems was eventually pursued.

Nevertheless, "scale" is the result or product of the investment planning decision, while "planning for planning" is concerned with the process of reaching the decision--that is, in opting for research on problems of scale, a choice was made of product over process.

Research on process can easily drift off into generalized speculation and platitudes with attempts at identifying and imposing pseudo systematic structures on decision making.

Research on product can become primarily descriptive and be more in the tradition of pure science rather than applied science. That is, it could concentrate on observing and classifying size and scale characteristics and ignore the managerial decisions which resulted in the observed phenomena.

While it would be most desirable for the research to consider both process and product and the linkage between them, the nature of IIASA and its remoteness, both physically and organizationally, from actual decision makers, suggest that the risks and consequences of research on process going astray are far higher than the likelihood of an excessive concern with the collection and analysis of data on product.

Thus "problems of scale" was a better title than "problems of planning." Furthermore, "scale" in the context of investment planning focuses attention on those situations where the magnitude of the investment is significant, where any mistakes are apparent to the community as a whole and where there can be a physical and psychological impact on the people connected with the facility resulting from the investment.

However, as the research task developed, the process aspects sometimes became of less significance and problems of scale remote from investment planning contexts were considered. Yet while a narrower title might have helped focus the research, it might then have detracted from the intellectual challenge of coming to grips with the broad and ill defined concept of "scale."

Another decision made at this time was that the emphasis would be on problems of scale rather than problems of location and scale. There is a substantial literature on the problems of location of production facilities, going back to Kantorovich's work in the Soviet Union in the 1930's, and it generally views plant scale as being primarily determined by transport costs and the size and location of markets and raw material sources. It is unlikely that IIASA could have added much to this topic. Furthermore the HSS area at IIASA had started to look at problems of public facility location. So, by and large, the location question was ignored in the IIASA research on problems of scale.

Phase 2. Review of "Problems of Scale"

Achievement:

The project began in January 1978. Initially the project had two aspects. One was a specific case study proposed by the Hungarian NMO concerning the size of generating units for a power station--200 MW or 500 MW? The other aspect was a general review of the literature on scale. This took a very broad perspective ranging from biology, through human settlements to classical economics and engineering design. However, it was decided not to continue with the case study after several meetings with the Hungarian NMO.

This phase of the research ended with the preparation of RM-78-47: "Problems of Scale"--The Case for IIASA Research by M.F. Cantley and V.N. Glagolev.

Key Decision: A General Review of Problems of Scale rather than a Specific Case Study

Although there were a number of extraneous factors which contributed to the abandonment of the case study, the initial choice between initiating a new research task with a general review of the state of the art or a specific case study seems to occur frequently.

The outcome of this decision seems to be determined partly by the background and experience of the researchers in the general area of the research and in the specific topic of the case study and partly by IIASA's perception of the eventual consumers of its research and the extent to which satisfying them is consistent with meeting the needs of the client in a specific case study.

In the Hungarian case study it would appear that the main objectives of the client were in getting some independent review of a decision and obtaining data and viewpoints from other countries' power systems on the choice of unit size. To the extent to which there are no barriers to the use of consultants from Western countries, it would seem that there are

either consulting firms specializing in electric power problems or major utilities such as TVA, Ontario Hydro or Electricite de France which have much more competence and expertize in the specific topic raised by the Hungarians than could be found in IIASA. The major unique feature of IIASA is its East-West character.

On the other hand IIASA's interest in the specific case study was to develop a general methodology for evaluating size alternatives in situations where

The long operational time span of the plant is relevant in determining the need for the investment and in which there is uncertainty in the key assumptions (quote from M.F. Cantley WN4/2.6.78)

The value to IIASA would be, first of all, in looking at a real problem which might enable us to see the real restraints under which decision makers operate rather than accepting what are inevitably the rather theoretical statements that will appear in the literature. It will, in addition, give us some general experience as a background against which we can discuss the problem with other people. Above all, it should force us to develop some concrete methodology (quote from R. Tomlinson memo June 5, 1978).

It is apparent that IIASA would not necessarily provided the specific answer sought by the client. Thus it was perceived that IIASA's role is not to act as the consultant, to advise managers directly, but that it should develop methodology and approaches which can be used by the policy advisors to management. Furthermore IIASA should try and develop general approaches not just valid in the context of a single industry.

it will be important to relate the electricity generating problem to the overall question of problem of scale and to emphasize general methods. One of the distressing features of much work at IIASA is the inability to understand underlying structure, and therefore the possibility of generalization (R.C. Tomlinson ibid.).

This would appear to imply that the research should be of interest to both systems analysts in other industries and also to policy makers who want to improve their general understanding of scale and its effects.

Opting for a general survey of the topic had, at this point, several advantages. It would help to define the scope and boundaries of the topic, it would increase the competence and level of knowledge of the IIASA scientists on scale related topics and there was more likelihood that the initial results would attract interest from a broad spectrum of policy advisors and systems analysts.

The risk with a general survey of a loosely defined concept like scale is that there can be significant omissions of whole areas in the literature or that the survey can be too superficial for its intended audience. The Cantley/Glagolev RM was remarkable for its breadth of coverage and the comprehensiveness of its perspective of the topic without at any point becoming superficial or banal. There was inadequate treatment of the relation between variety of products and scale (plant versus product economies of scale) and mathematical models were not reviewed. However, this probably made the RM accessible to readers with a very wide range of disciplinary backgrounds.

Research Focus: A Set of Methodologies for Thinking about Scale

The Research Memorandum reveals changes in thinking about problems of scale, in particular:

- a recognition that in seeking generalizations it may be necessary to categorize the situations in which problems of scale are encountered by level and environment, where level means the decision makers position in the hierarchy extending from responsibility for deciding on the size of individual production machines or units to responsibility for national or international policy making. There may be a different methodology for each level;
- a broadening from the original focus on the size of major production facilities to such problems as designing organizations or creating new industries. This meant that the issue was no longer just size but also form and structure;
- the purpose of the "general methodology" was becoming less meeting the needs of a specific decision maker confronted with an immediate decision but rather the construction of broad organizational and historic frameworks within which the implications of scale decisions could be examined. Useful concepts would be developed but they would not be primarily prescriptive.

The RM, because of its variety and breadth of ideas and perspectives, probably gave the research on scale a bias towards studying the product, scale and its effects, while the Hungarian case study had had more of a concern with process-and-product.

It was probably essential that IIASA acquire this broad "state of the art" view of the topic. Although the RM could be of some value to policy makers confronted with scale problems it is likely that the main customer of this research was the institute itself. The main impression created by the RM was that of the immense breadth of the topic.

Phase 3. Preparation and Holding a Workshop

Achievement:

The next phase of the research was the preparation for the workshop held at IIASA in June 1979. Papers on both specific case studies and general discussions of scale issues in industrial context were sought, either by direct contact with people in industry, government or universities or through the invitation letter and supporting material sent to the NMO's.

In preparation for the workshop there were two studies done at IIASA:

- WP-79-42*: Scale, Protectionism and European Integration: The Structural Dynamics of Strategic Control in a Turbulent Field by Mark F. Cantley
- WP-79-43: The Scale of Ethylene Plants: Background and Issues by Mark F. Cantley.

The workshop "Size and Productive Efficiency--The Wider Implications" was attended by 55 scientists with backgrounds ranging from engineering to political science. There were 25 presentations and a great deal of vigorous and useful discussion on a wide variety of scale related issues.

One of the issues on which there was heated argument was that of learning curves and the relation between scale and learning. Following the workshop two working papers were prepared:

- WP-79-110**: "Who Learns What?"--A Conceptual Description of Capability and Learning in Technological Systems by Mark F. Cantley and Devendra Sahal
- WP-79-128***: Scale, Technology and Learning Curve by Kiichiro Tsuji.

Key Decision: How to Proceed? A Workshop, a Research Project, a Book

The RM left a number of important questions or research planning unanswered. Was research on problems of scale appropriate to IIASA with its unique mix of capabilities and limitations? Was there a potential for doing fruitful research on scale itself as a common problem or could one only do research on specific problems, like electric generation

* Much of this paper was incorporated in chapter 16 of "Scale in Production Systems."

** Subsequently published as RR-80-42, December 1980.

***This paper was incorporated in chapter 6 of "Scale in Production Systems."

planning or industrial restructuring, in which scale was a significant factor?

Nevertheless, there was a general feeling that there was a great deal of experience in dealing with scale problems in industry and elsewhere but that this experience was scattered and uncoordinated. Given IIASA's unique situation it could perform a valuable role if it were to try and bring this experience together and draw attention to the commonalities. This suggested that some sort of conference or workshop would be appropriate. The workshop might also help decide whether there should be a book on scale and whether specific research projects appropriate to IIASA could be identified.

Several alternative workshop formats were considered: One focusing on case studies in a number of industries, or one attended by specialists from a wide variety of different disciplines. The risk with the latter was that it would not go beyond vague generalities so a mixture of the two was proposed--there would be reports of a number of case studies but there would also be the disciplinary experts to provide comment and point out the wider significance.

The alternative to a workshop was that IIASA do a research project on some specific scale decision or begin writing a book on scale, preferably a handbook to advise people on how to approach problems of scale, although the content and format of the RM were more suitable as the basis of a textbook about scale. Factors involved in making scale decisions, such as complexity, flexibility, risk and social consequences to individuals had not been considered in detail in the RM. It was felt that a book should wait until after the workshop, which could provide useful raw material, specific case studies and fill in any gaps in the RM.

It is likely that one of the implicit objectives of the workshop was to determine the extent to which scale was recognized as a valid problem for systems analysis and to obtain the recognition of prominent experts that it was a legitimate field of research.

The project on ethylene done in parallel with the workshop preparation was valuable in enabling IIASA's interest in scale to achieve some recognition in the industry. The strongest industry representation at the workshop came from the chemical industry. On the other hand there was no representation from electric utilities--no doubt the impression given by the RM of IIASA's work was that it was too general to be of relevance. This experience suggests that it might have been advisable for IIASA to have done a couple more industry specific reviews like the ethylene study--enough to demonstrate to industry some knowledge, competence and awareness of its specific problems.

The workshop was successful in achieving most of its goals. The papers demonstrated the comprehensiveness of the RM as there were few ideas that had not been foreshadowed by it.

There were quite a number of papers with case study material although only one paper (Betts) had much discussion on the process of making scale decision. Most of the other papers focused on the result of scale decisions (the product). For example, the paper by Fisher of GE which suggested that utilities had built too large generating units did not discuss at all the nature of the relationship between manufacturers and utilities and how this might have led to the utilities over optimistic expectations of the reliability of large units.

However, the workshop was not particularly successful in identifying directions for further research. "Free advice is too often worth its cost" as one participant quoted from J.M. Keynes. There were a large number of problems identified but the general impression was that every participant saw his own particular problem as a problem of scale. There was no agreement on whether these problems had anything in common.

Research Focus: Is There Any General Methodology for a Meaningful Subset of Scale Problems?

The nature of the workshop was such that the topic broadened still further. The diversity of backgrounds and interests of the participants resulted in a great deal of spontaneous discussion, sometimes quite heated, and leading to a synergism of ideas. The broadening occurred in a variety of ways:

- it was pointed out that scale had also to be considered in the context of multi product systems. This led to the recognition that scale means more than size; it also involves structure and degree of specialization;
- the overall theme of much of the general discussion was the way scale is related to change. This occurs in two ways. As the environment or the available technology changes so does the appropriate scale of plant. But furthermore, the ability of an organization to respond to these changes is in turn affected by the organization's size and structure. The most heated discussion concerned two issues, does the concept "turbulence" enable one to define the changeability of the environment and can the learning curve be used to predict the way in which the performance characteristics of technology change with time.

The broadening of "problems of scale" made it obvious that problems of scale were ubiquitous and diverse. There was felt to be a general need for some sort of taxonomy of all the different problems and nobody thought that the same methodology could be used for determining aircraft size and for deciding on national trade and industrial policies.

Even in the context of the problem of determining process plant size. Bela Gold was doubtful that the time was ripe for a general methodology. He felt that there were no general theories or empirical findings on the scale potential of industry. He thought that there was a need to concentrate on specific cases and only try very limited generalization of specific sub-groups of problems. He emphasized the diversity of managerial goals and the diversity of technology and suggested that, because scale decisions were just one set of decisions in the overall firm strategy, perhaps scale decisions could not be isolated and examined on their own.

Phase 4. Workshop Proceedings

Achievement:

The writing of a book based on the workshop proceedings was the next major activity of the task. About half the papers presented at the workshop were included. However, as well as these papers a number of chapters were written by IIASA staff. Some were reports on the discussion and attempts at relating the discussion to the literature, while others were intended to provide adequate introductory and summary material so that a comprehensive perspective could be given to the topic of problems of scale and possible directions for further research.

The book "Scale in Production Systems " edited by J.A. Buzacott, M.F. Cantley, V.N. Glagolev and R.C. Tomlinson will be published by Pergamon Press and should appear in early 1981.

Key Decision: What To Do with the Conference Proceedings? A Compendium of Papers, a Book Based on the Proceedings, a Book on Scale

In making this decision there were several factors to be considered:

- a compendium of papers would not have required much scientific time and could have been completed rapidly. However it meant ignoring all the discussion and most participants had felt that the discussion had been the most exciting feature of the workshop;
- a book based on the proceedings required a substantial amount of scientist time in order to decide what to do with the discussion and develop a suitable structure for the book. However, as long as it was based on the proceedings it would be limited in scope of coverage to those scale topics which were mentioned at the workshop. There were limits to the extent to which authors could be asked to rewrite their papers;

- a book on scale, either a handbook or a textbook required the development of the necessary research base and it was not clear that this existed in sufficient depth. Because of the variety of scale topics which arose at the workshop it was not clear what the scope of a handbook, in particular, should be. Thus this option was not seriously considered.

The choice between the compendium of papers and a book based on the proceedings required some consideration of the functions of each alternative.

A compendium of papers would:

- (1) provide authors with a means of circulating their paper under IIASA's auspices;
- (2) demonstrate IIASA's interest in the subject.

A book based on the proceedings would:

- (1) through its organization provide a framework for thinking about problems of scale;
- (2) enable the discussion on the major aspects to be summarized and related to the wider context and literature;
- (3) serve as a basic reference and starting point for future research on scale.

The decision was for a book based on the proceedings. Only about half the papers presented at the workshop were included. Selection was based on a mixture of space, quality, and a desire to maintain a reasonable East-West balance. The major time requirement was in the analysis of the discussion, trying to identify the major issues and also to find suitable quotes which captured the essence of what was sometimes a long and rambling argument. A number of papers were almost completely reorganized and rewritten in order to improve the clarity of exposition.

In retrospect it seems that the book based on the proceedings suffers from the limitations imposed by the scope and quality of the original papers. It might not have required much more time to have produced both a compendium of papers and a book on scale, although such a book would have required an unusual structure in order to incorporate the workshop discussion and its vigour and directness (and even conflict). Indeed, an audio presentation, linking extracts from the discussion by a commentary and having a structure like a radio documentary might well have been the best way to make the discussion available to the interested audience.

Research Focus: Who Could be Helped by Developing a General Methodology?

It is evident that following the workshop it was necessary to provide a clearer structure for thinking about scale problems. The organization of "Scale in Production Systems" suggests three major categories of problems: the size of plant and equipment, the structure and size of organizations, scale issues affecting national industrial and trade policies. Initially anyway, each category of problem must be examined separately and at present any methodology developed should be specific to just one category.

However, although the book appears to have been successful in developing a taxonomy and surveying significant issues in deciding on scale its concluding chapter raised a number of issues that proved difficult to resolve. Even if a general methodology for making scale decisions can be developed, and the book suggests that many of the components of such a methodology already exist, who would be interested? Who is the client for research on problems of scale? If such a general methodology existed it would certainly help policy makers who wish to influence the behavior of firms as it would give them a means of testing alternative policies (e.g., how would tax incentives affect investment in new plant). But would individual decision makers and their advisors use it? Or would they always want a methodology which is situation specific? Although this question was answered optimistically in the book, a more neutral position, waiting to see what happens when such methodology exists, would be more realistic.

Phase 5. Research on Scale in Investment Planning

Achievement:

With the completion of the workshop proceedings research was begun on a number of specific topics connected with scale effects in planning major investment. This resulted in three working papers:

- WP-80-83: Scale and Process Innovation: The Adoption of the Basic Oxygen Process by Canadian Steel Firms by John A. Buzacott
- WP-80-116: The Effects of Uncertainty in Generation Expansion Planning--A Review of Methods and Experiences by Kiichiro Tsuji
- WP-80-117: The Dynamics of Scale, Technological Substitution and Process Mix by John A. Buzacott and Kiichiro Tsuji.

Key Decision: How to Complete the Research Task? Research,
Task Force or Workshop

With the proceedings completed and the decision having been made that the research task would end in mid 1980 the question of how to complete the research task arose.

Although the workshop had extended the problem of scale much beyond the original investment planning context and identified significant problems in other contexts the available time and manpower resources suggested that the research should return to investment planning problems and look at the effects of technological change and uncertainty about the future.

However, there were several alternative ways of conducting the research. One was to have a workshop or task force meeting. It could focus on the problems within the context of a particular industry and try and use the meeting to obtain case study material on how managers actually make these decisions and how they allow for uncertainty. IIASA could offer a East-West contact. Preliminary contacts indicated interest from the East and smaller Western countries but no enthusiasm from the large Western countries. It is likely that this reflects differences between the perceptions of benefit from such a meeting and there is a strong risk that IIASA's goals might not be met. This alternative was abandoned when it became apparent that there would be too little time to evaluate the results of such a meeting and incorporate it in the research.

Had a workshop or task force considered the general problem the purpose of the meeting would have been to try and use the NMO's and other contacts to identify informed people and collect information from a wide variety of sources, hoping that the publicity for the meeting would flush it out.

However, it now seems that such a general meeting would not be successful unless there were specific results of IIASA research to present and to serve as a means of getting the interest and understanding of other people on the nature and objectives of the research task. Furthermore the existence of IIASA research results would have helped identify what sort of people should come to the meeting.

So the research pursued the approach of developing models in order to improve understanding of specific situations on the role of uncertainty about future demand and on the impact of technological change on the size of major capital investments.

Research Focus: Is Scale Really the Problem?

In both cases considered, the results of the research suggest that while scale is an important issue, it is not the central problem. Thinking about scale leads to new insights on

other problems and these insights are more significant than the results on scale itself.

To elaborate:

The problem of determining size when there is uncertainty about future demand leads to the same problems as any decision making under uncertainty. That is, the decision will be affected by the choice of criterion (e.g., minimum expected cost, min-max cost or minimum regret) and also by the way in which the decision maker describes his uncertainty about the future (i.e., the set of possible events and their timing), acquires information about the occurrence of these events and uses this information to modify his perceptions about the probability of future events. Concepts such as reliability, flexibility and robustness can be incorporated within the general decision making under uncertainty framework although focusing on the size decision gives insight into many of the issues. Different views on the appropriate size could reflect the use of different criteria and perceptions and one of the roles of the systems analysis is to elucidate these differences. Yet a separate methodology of scale is not required, the methodology will be based on the literature on decision making under uncertainty.

The study of the effect of technological change on scale decisions showed that the scale characteristics of the new technology have a significant effect on the way it is adopted. In particular, as long as the maximum size of plant which is considered feasible is constrained by limited experience with the technology and the problems of scale up, then the new technology will be restricted in its adoption. It will only suit a particular niche, sometimes only firms in a limited range of size and growth rate. The focus on scale leads to an understanding of technological change and the diffusion of innovations. However, the real problem, what policy instruments can be used to influence the process of technological change still remains, even though looking at scale has led to new insights.

Thus the tentative conclusion is that scale is an important issue which has been to a large extent, ignored in the past. Scale itself is not the problem but thinking about scale leads to insights into other problems, such as decision making under uncertainty, technological change or national industrial and trade strategies.

Phase 6. End of Task

The "Problems of Scale" task ended at the end of July 1980.

Key Decision: The End or Should Research on Scale Continue?

The decision that research on scale should end was made as a result of the Advisory Committee meeting in October 1979. In the light of the amount of specific research results then existing and the concreteness of the research directions proposed at that time, the decision was probably reasonable. Unfortunately, the decision was made in the middle of the process of analyzing the workshop and writing the proceedings. The workshop did, in the end, contribute to a greatly increased understanding of the concept of scale--but not until the proceedings had been written, all the different ideas sorted out and a framework for them provided. The advisory committee occurred when this process was quite incomplete and hence the decision was almost certainly made with an inadequate understanding of the scope and potential for future research. It would have been more appropriate to defer the decision until the proceedings were completed.

THE FUTURE. WHERE SHOULD RESEARCH ON SCALE GO NEXT?

The knowledge obtained about scale during the research project is by now such that it would be possible to achieve the original objective of writing a handbook on scale for decision makers and their advisors concerned with making major capital investments in homogeneous product industries. However, now that the scientists connected with the task have left IIASA it is unlikely that this will ever be done, even though such a handbook might have created a role for IIASA in transferring the methodology to a variety of industrial contexts and contributing to the interchange of ideas between East and West.

Yet the experience in thinking about scale in investment planning could be of value to other projects at IIASA. For example, with the present knowledge and understanding of scale it would have been possible to contrast the Lovin's small scale renewable energy scenario with conventional large scale fossil and nuclear energy scenarios. As another example, focusing on the scale decision enables the implications of some of the broader issues in technological planning and decision making under uncertainty to be understood and clarified.

Even so, it is probably not worth starting the task again; although it is to be hoped that there will be sufficient memory in IIASA so that if any research task is concerned with investment planning it will remember that scale is important.

Nevertheless the research on problems of scale has indicated that there are a number of other problems which could well be particularly appropriate to IIASA and in each of which scale is a significant issue.

These are:

- Technological Substitution, Diffusion of Innovations, Sectoral Specific Industrial and Trade Policies. All these problems are linked by a common methodological base. That is, they require models of the determination of plant size and choice of process and of the allocation of the required production between different plants which have as their basis the size/cost/performance characteristics of the technologies. These models can be used to understand the effects of policies for promoting innovation or for improving industrial performance.
- The Role of Small Firms--Productivity and Market/Product Specialization. The "Ideal" Firm Size Distribution. One of the fascinating sidelines of the scale research was an observation by Don Daly at the workshop that the relationship between productivity and size of enterprise is quite different in the East to the West. Small firms in the West are relatively much more productive than their counterparts in the East. The explanation of this phenomenon can probably be gained by looking at the issue of plant scale versus product scale in the multi-product enterprise. Small firms in the West probably achieve high productivity by specialization in either product or market. If the basis for this can be further developed it would suggest guidelines for industrial structure and the "ideal" firm size distribution. Since the actual distributions differ so much between East and West it might be a good project for IIASA, particularly if the focus is on policies which affect the firm size distribution, such as mergers, monopolies, or industrial restructuring, and hence affect economic performance.
- Organization Size, Structure and Performance--The Impact of Information Technology. The structure of organizations is affected by the methods and technologies used for control, communication and information processing. An appropriate project for IIASA's information technology task would be to look at the impacts on organization structure. This should result in increased understanding of scale as structure-and-size because information technology widens the feasible alternatives for organizational structures.

"PROBLEMS OF SCALE" AS AN APPLIED SYSTEMS ANALYSIS PROJECT

The research on "problems of scale" was characterized by some unusual features. In contrast to conventional systems analysis work with its focus on the solution of a specific problem and a well defined process of modeling and analysis (such as described in Miser (1980)) this research was characterized by:

- an emphasis on gaining understanding of particular decision making situations. Particularly during the workshop there was a concern that the research focus on actual situations and the real motivations of decision makers;
- the progressive change in the problem addressed. At times there may have been a tendency for diffuseness but with improved understanding a clearer focus generally emerged;
- the recognition that study of a decision at the level of the firm could not be divorced from wider policy issues and indeed the research may have provided more new insight into these wider policy issues than into the original firm level issues. The research emphasized the interconnection and interrelationship of national and firm level decision and policies.
- a relatively unsophisticated use of formal models. It was only in the final phase of the research that models were used at all. Their role was to guide and clarify thinking about broader issues. There was no claim that the models were a comprehensive and sufficient representation of reality.

All the same, it must be recognized that the approach used in this project was more risky than "conventional" systems analysis. There was a not insignificant probability that the project might have produced only trivial results. Yet an institute such as IIASA has to be prepared to try alternative systems analysis approaches. There are enough failures in past systems analysis projects for it to be apparent that a variety of approaches have to be tried and used.

Furthermore, in some respects the style of this project was well suited to IIASA. Its successive phases were marked by a change from focusing on a specific situation, to the generation of a broad diversity of ideas, to the creation of an opportunity for discussion and even conflict among a broad cross section of people, to the sober evaluation of the issues involved in the conflict and then to the clarification of some of these issues. While the "sober evaluation" is perhaps best done by one person, and the clarification of issues can be done by a loose linked group of scientists, some of the other phases, such as the organization of a workshop and the preparation of discussion material, require a team of people drawn from diverse backgrounds and with strong interaction. This variability in the number of scientists connected with a project, and the desirability that the process keep on repeating itself, means that this style is most unsuited to institutions where scientists have permanent, life time careers. Because of IIASA's unique capability to vary the number of scientists working on a project the style is well suited to the institute. However, it is important that there be some continuity over the duration of the project and the difficulties of managing and evaluating the research must be recognized. Rather than a steady stream of results, progress comes in leaps or jumps and each such step requires careful evaluation.

However, one of the difficulties with scientific research is that there is often considerable time delay until the research is evaluated by the rest of the scientific community and its significance appraised. These long delays in appraising research quality are not normally of such concern in academic environments when the researcher is likely to stay in one position for many years and his achievement will be evaluated over a 3-5 year period. At IIASA the team can be dissolved and there is no carry forward of its experience, with the virtual impossibility of reestablishing the group if on evaluation it seems attractive.

Indeed, in my view there are some basic weaknesses in the methods of research planning and evaluation at IIASA. There are some research projects, and scale was one, where it is by no means clear at the outset as to what the research entails and what the likely research results would be. All the same the topic appears to be of some significance. Thus it would appear to be desirable to decide on certain milestones in advance at which the project should receive scientific evaluation by at least one or two assessors external to IIASA. This in turn implies that there needs to be a more comprehensive research proposal for each project than exists in the present research plan. The proposal should identify what research could be done, the likely research results and whom the research results would benefit or interest. It should specify the achievement or event milestones at which there should be more formal reviews. This proposal can then be subjected to some internal evaluation.

In the case of the scale project the RM was essentially such a research proposal. Insofar as there was a decision to continue the project on completion of the RM it was based on the expectation that the workshop would result in specific research suggestions and that it would enable, through its cross disciplinary interchanges, generalizations about scale to be made.

Yet, in retrospect it was not realistic to expect the attendees at the workshop to come up with specific suggestions for research appropriate to IIASA. Apart from their lack of familiarity with the institute's capabilities, it is inevitable that they see the research needs from their own personal perspective.

The completion of the proceedings was a milestone at which there should have been a scientific review and such questions asked as: How good is the work? What is the potential for further research achievement? Who will the research benefit?

CONCLUSION

It will not be until the book "Scale in Production Systems" appears that it will be possible to have much feeling for the success of the project in contributing to overall scientific knowledge.

Yet, each of the scientists involved in the task seems to have found the topic of considerable intellectual stimulation and an experience which broadened his understanding of systems analysis. The variety of ideas about scale and the situations in which it seems important led to a general knowledge of a wide variety of topics which is bound to influence the directions of our future research.

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