PROGRESS IN CYBERNETICS AND SYSTEMS RESEARCH

Volume V

Organization and Management
Organic Problem-solving in Management
System Approach in Urban and Regional Planning
Computer Performance, Control and Evaluation
Computer Linguistics

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Preface

There is no need to explain to someone who has bought a book bearing the title *Cybernetics and Systems Research* the importance of this field. And if she/he has not already been convinced of its importance, this book hopefully will do so.

It might, however, be interesting to hear how these European Meetings started, who organizes them, how they take place, in order to get some idea of the background from which this series *Progress in Cybernetics and Systems Research* arises.

It was in 1970, when a group of Austrian scientists and practitioners decided that cybernetics and systems research were sciences much too important to be practically ignored in Austria. So the 'Österreichische Studiengesellschaft für Kybernetik' (Austrian Society for Cybernetic Studies) was founded, general introductory courses in cybernetics and concentrated courses in special areas were given, and leading scientists were invited to report on their recent work in the 'Colloquia'. Besides this, several research projects were carried out and the results were published in the 'Reports'.

These activities, which my colleagues and I have undertaken since then and about which we happily inform those interested, led to the idea of inviting scientists from Austria and other countries to a conference in order to exchange ideas.

Since we did not know whether more than 20 scientists would participate, we named it 'Meeting'. And though we were not sure if anyone outside of Austria would come, we dared to call it 'European'; 'International' or 'World' seemed far too assumptive to us. Thus, in 1972 the first 'European Meeting on Cybernetics and Systems Research' took place in Vienna.

To our surprise, 82 scientists, 75 of whom presented papers, joined us, so the meeting had to be run parallely in three rooms.

Encouraged by this unexpected success, we sent out invitations to a second meeting in 1974. This time, 123 colleagues accepted our invitation, 89 papers were presented and we had to rent five rooms at the University here in Vienna.

Therefore, basing our 'modest' estimate for 1976 on an exponential assumption of the increase, we arrived at about 180 participants and some 110 papers.

To our surprise 280 papers were submitted. Therefore, we made a rather strict selection, or at least tried to, rejecting about 30% of the papers. 202 papers were accepted, which were presented by scientists from 26 European and non-European countries. Altogether, 300 scientists participated, so the conference had to be held in 8 lecture rooms at the University in parallel. Since our society is run only by a small group of scientists who do all the organizational work in their spare time—plus a part-time secretary—the organizational problems could hardly be mastered. In the meantime we decided to have the next conference (1978) with a smaller number of speakers.

The unexpected increase in the number of participants and contributors may be based on two reasons: first, cybernetics and systems research has shown itself to be—as I may call it—a 'non-disappointing science', the more one works with it, the more fascinating the possibilities for application become, especially in socially relevant areas, and therefore the expansion of the theory is all the more challenging. Secondly, the outstanding quality of the scientists who joined the Programme Committee, helped organize the symposia and chaired them. Their scientific reputation attracted many colleagues from all over the world who could exchange with them their scientific ideas and practical results.

This volume contains four of the symposia of the Meeting, each introduced by the chairman. It is more than a mere collection of the papers: many papers have been rewritten and the results of the discussion have been included.

I should like to thank all those who have helped to make this Meeting a scientific success, especially all colleagues who contributed their scientific work, and Professor Hanika and Professor Pichler who joined me in editing this volume.

I hope you will enjoy studying this book.

R. TRAPPL

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KEYNOTE INTRODUCTION

The systems view and the systems method: must they remain separate?

PROFESSOR P.B. CHECKLAND
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I face some difficulties in making some remarks which are approximately relevant to all of you, whether you are bio-cyberneticians, general systems theorists, management problem-solvers, neurobiologists, control engineers, fuzzy mathematicians or computer enthusiasts. The danger is that anything so general will be devoid of content; and this brings an added difficulty because what I wish to say is that the systems movement—300 members of which are gathered here this morning—is too ready to generalize, not ready enough to test its generalizations. That will be the content of my sermon.

I use the word 'sermon' deliberately because I'm sure you will agree that from a pulpit of this magnificence in a hall of this splendour only sermons are possible! I shall therefore extract from the later stages of my talk a quotation which can serve as the text for my sermon. Here it is; it was written more than 300 years ago by Isaac Newton in a letter to Roger Cotes:

> If, instead of sending the observations of able seamen to able mathematicians on land, the land would send able mathematicians to sea, it would signify much more to the improvement of navigation and the safety of men's lives and estates on that element.

That is my text; here is my sermon...

I

American advertizing executives talk of 'running an idea up the flag pole' in order to see whether anyone salutes. It has been true for some years now that whenever a 'systems approach' is run up the flag pole, most people salute. No one ever is heard boasting that they do *not* use a systems approach, and cogent attacks on the idea of systems thinking are all too rare. I was delighted, therefore, recently to come across some words of Jacques Monod, Nobel Prize-winning molecular biologist:

> ...What I consider completely sterile is the attitude, for instance, of Bertalanffy (but he is not the only one), who is going around and jumping around for years saying that all the analytical science and molecular biology does not really get to interesting results; let's talk in terms of general systems theory. Now I was struck by this term and I talked to some systems theorists and informationists and so on, and they all agree that there is not and there cannot be anything such as a general systems theory; it's impossible. Or, if it existed, it would be meaningless. [1]

Alas, this criticism is not developed and justified; it remains an assertion. But even so it is the kind of assertion we in the systems movement need more of. We need to dispel the complacency which is typical of the systems movement, because it could be that attacks on a 'systems approach' are rare because it is devoid of content, as Monod implies, because it is not worth attacking. We ought to be disturbed that practically everybody is in favor of a 'systems approach', and especially so because most enthusiasts would find it difficult to explain just what a 'systems approach' consists of.

The fact that a large number of people are in favor of an intellectual abstraction which is very ill-defined gives rise to a marked gap between, on the one hand, the enthusiasts and, on the other, people like Jacques Monod who find it meaningless. This gap is a gap between what systems thinking is *said to be* and what it is *seen to do*. It is a gap we in the systems movement ought to try to close, for two reasons. Firstly, matching what the systems view *is* to what the systems method *does* potentially provides a program for the systems movement, a program whose execution will ensure that a 'systems approach' is more than a rallying cry. Secondly, it would be greatly to the advantage of the systems movement if we could persuade serious potential critics like Monod that the systems outlook is serious enough to be worth attacking seriously.

I believe we can make progress in closing the gap, in bringing together the systems view and the systems method, by examining *what a systems approach is*, which means examining where it comes from—its intellectual source—since this, I believe, shows us what systems thinkers ought to be *doing*.

II

I take it as indisputable that the systems movement is a part of the broad sweep of the science movement. Systems thinking is, or is supposed to be, a variety of scientific thought. This being so, we can best take a view of what systems thinking is by seeing it in the context of the history of science.

Science is characteristic of Western civilisation, and arose together with philosophy, from which it was at first indistinguishable, in 6th Century B.C. Greece. We now see Thales, Anaximander and the other Ionian philosophers as the founders of the science tradition because when they suggested that there must be a single component, a unitary stuff, from which the world was constructed they were founding a tradition of rational argument. Critical discussion, without recourse to myths or the supernatural, characterized Greek science, and has remained a prime characteristic of science as an organized human activity. But although Aristotle worked as a marine biologist, and founded a tradition of observation, the Greeks did not contribute in a major way to the experimental tradition that we see in science. That derives more directly from the medieval clerics who struggled with the problems of inductive argument—such as Grosseteste and William of Ockham—and, later and most dramatically with the experimentalists like Galileo and Gilbert who contributed to the scientific revolution of the 17th Century which culminated in Newton's world picture: rationally argued, experimentally verifiable and expressed through the generalising power of mathematics. Since the 17th Century, the exponential rise in the activity of science has virtually created our world. What we have learnt most clearly during this period of the exploitation of science is: firstly, that science is an unprecedentedly powerful means of finding things out, a highly successful 'learning system'; secondly, through the downfall of the Newtonian world picture and its replacement by Einstein's model in which space is no longer an absolute framework, we have learnt that all scientific knowledge is provisional, and that at any moment of time the scientific knowledge we have is simply the best-tested knowledge: it may be replaced by future conjectures which survive more stringent tests.

What the history of science teaches us is that science consists of rational thinking applied to experience, especially the kind of experience obtained in the special kind of controlled observations we call 'experiments'. We may characterize science, in fact, as a learning system, in terms of three crucial characteristics: reductionism, repeatability and refutation. [2] Science is *reductionist* in the sense that in experiments we isolate a small part of the world in the laboratory and investigate just a few variables under controlled conditions. And it is reductionist in the sense which derives from William of Ockham; using the principle known as Ockham's Razor ('do not multiply entities unnecessarily') we seek to explain the results in the most simple way, using as small a number of concepts as possible. *Repeatability* is the criterion which the results of experiments must satisfy if they are to be accepted as scientific knowledge; this is concisely expressed in Ziman's definition of science as 'public knowledge'. Finally, science makes progress by subjecting conjectures to experimental testing, retaining those which best survive the tests—Einstein's theory, for example, being preferred to Newton's precisely because it can encompass all Newton's results *and*, for example, some apparent anomalies in the motion of Mercury which defeat Newton's formulation. The aim of the exper-