PROGRESS IN CYBERNETICS AND SYSTEMS RESEARCH

Volume IV

Cybernetics of Cognition and Learning
Structure and Dynamics of Socio-economic Systems
Health Care Systems
Engineering Systems Methodology

Edited by

ROBERT TRAPPL

Professor of Medical Cybernetics
University of Vienna Medical School, Austria

and

GORDON PASK

Director of Research
System Research Ltd., Richmond, Surrey, England

WITH INTRODUCTION BY

P. B. CHECKLAND

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List of authors

Barry, Prof. Patrick E., State University of New York at Stony Brook, Dept. of Electrical Sciences, Stony Brook, New York 11794, USA.

Blong, Clair K., Government and Politics Program, University College, University of Maryland, College Park, Maryland 20742, USA.

Bogner, Sue PhD., National Academy of Sciences, Institute of Medicine, 2101 Constitution Av., Washington D.C. 20418, USA.

Bojarski, Wlodzimierz, Institute of Fundamental Technological Research, The Polish Academy of Sciences, Swietokrzyska 21, 00-049 Warsaw, Poland.

Bossel, Hartmut, PhD, Institut für Systemtechnik und Innovationsforschung, Breslauerstraße 48, D-75 Karlsruhe-Waldstadt, FRG.

Bratko, Ivan, J. Stefan Institute, Jamova 39, 61000 Ljubljana, Yugoslavia.

Brix, V.H., The Pantiles, Churchland Lane, Sedlescombe, Battle, Sussex, U.K.

Burns, Tom R., University of Oslo, Institute of Sociology, Blindern, Box 1096, Oslo 3, Norway.

Cartwright, T.J., York University, Faculty of Environmental Studies, Toronto, Canada.

Checkland, Prof. Dr. Peter B., University of Lancaster, Dept. of Systems Engineering, St. Leonard's House, St. Leonardgate, Lancaster, U.K.

Cobelli, Claudio, University of Padua, 35100 Padua, Italy.

Cornock, Stroud, Leicester Polytechnic, Box 143, Leicester LEI 9BH, U.K.

Coverdale, I.L., Operational Research Service, Dept. of Health and Social Security, London, U.K.

Damm, Dipl. Ing. Erik, Technische Hochschule Darmstadt, Fachgebiet Übertragungstechnik, Merckstraße 25, 61 Darmstadt, FRG.

De Ville, Philippe, Université Catholique de Louvain, Unité de l'Institut des Sciences Economiques, 121 Parkstraat, 3000 Louvain, Belgium.

Dirkzwager, Dr. A., Psych. Research Laboratory, Vrije Universiteit, De Boelelaan 1115, Amsterdam, The Netherlands.

Doberkat, Dipl.-Math. Ernst Erich, Forschungs- und Entwicklungszentrum für objektivierte Lehr- und Lernver-fahren, Pohlweg 55, Paderborn, FRG.

England, J. Lynn, Associate Professor, Dept. of Sociology, Brigham Young University, Provo, Utah 84602, USA.

Elstob, Dr. C.M., Brunel University, Uxbridge, Middlesex, U.K.

Fraunschiel, E., Amt der Niederösterreichischen Landesregierung, 1010 Vienna, Austria.

Gaspari, Dr. Christof, Studiengruppe für Internationale Analysen (STUDIA), Berggasse 16, 1090 Vienna, Austria.

Gause, Donald C., School of Advanced Technology, State University of New York at Binghamton, Binghamton, N.Y. 13901, USA.

Gibbs, R.J., Operational Research Service of the Dept. of Health and Social Security, London, U.K.

Glanville, Dr. Ranulph, Architectural Association, 36 Bedford Square, London WC1, U.K.

Hama, Hiromitsu, Dept. of Electrical Engineering, Faculty of Engineering, Osaka City University, 459 Sugimotocho, Sumiyoshiku, Osaka, Japan.

Hanken, Prof. A.F.G., Technische Hogeschool Twente, Afdeling der Bedrijfskunde, Postbus 217, Enschede-Drienerlo, The Netherlands.

Hough, Dr. Robbin R., School of Economics and Management, Oakland University, Rochester, Michigan 48063, USA.

Kacki, Prof. Dr. hab. Edward, Technical University of Lódź, The Center of Electronic Computing

Technique, 90-924 Łódź, ul. Gdańska 178, Poland.

Kaihara, Shigekoto, M.D., Hospital Computer Center, University of Tokyo Hospital, 7 Hongo, Bunkyo-ku, Tokyo 113, Japan.

Kiselev, Prof. A.S., Head of Scientific Center of Psychiatry, 23 Kropotkinskystr., Moscow G-34, USSR.

Klabbers, Dr. Jan, Social Systems Research Group, University of Nijmegen, Erasmuslaan 16, Nijmegen, The Netherlands.

Kremer, Dr.-Ing. H., Technische Hochschule Darmstadt, Fachgebiet Übertragungstechnik, Merckstraße 25, 61 Darmstadt, FRG.

Kunz, Prof. Philipp R., Dept. of Sociology, Brigham Young University, 183 FOB, Provo, Utah 84602, USA.

Laszlo, Charles A., Division of Health Systems, University of British Columbia, Vancouver, B.C., Canada.

Legler, Harald, Institut für Systemtechnik und Innovationsforschung (ISI), Breslauerstraße 48, 75 Karlsruhe-Waldstadt, FRG.

Lorimer, Prof. Kenneth V., 18 Adys Road, East Dulwich, London SE15, U.K.

Lyszkowski, Dr.-Ing. Tadeusz, Technical University of Łódź, The Center of Electronic Computing, ul. Gdańska 178, 90-924 Łódź, Poland.

Machiraju, N.R., Algorithms in Systems Inc., Northridge, Calif., USA.

Manderscheid, Prof. Ronald W., Mental Health Study Center, National Institute of Mental Health, 2340 Univ. Blvd., E., Adelphi, Maryland 20783, USA.

Mantz, Dr. ir. M.R., Trompenbergerweg 7, Hilversum, The Netherlands.

Millendorfer, Dipl. Ing. Dr. Hans, Studiengruppe für Internationale Analysen (STUDIA), Berggasse 16, 1090 Vienna, Austria.

Milsum, Prof. Dr. John, Division of Health Systems, Health Sciences Center, University of British Columbia, 4851 College Highroad, Vancouver 8, B.C., Canada.

Mitchell, Prof. Dr. P. David, Dept. of Education, Sir George Williams Campus, Concordia University, Montreal, Quebec H3G 1M8, Canada.

Niewierowicz, Dr.-Ing. Tadeusz, Technical University of Łódź, The Center of Electronic Computing Technique, ul. Gdańska 178, 90924 Łódź, Poland.

Nowakowska, Maria, Laboratory of Psychology and Action Theory, Institute of Organization and Management of the Polish Academy of Sciences, Warsaw, Poland.

Nurmi, Prof. Hannu, University of Turku, Department of Political Science, Aurakatu 14B, 90100 Turku 10, Finland.

Owen, Dr. Philip James, California State College, Stanislaus, 800 Monte Vista Avenue, Turlock, Calif. 95380, USA.

Pask, Prof. Dr. Gordon, Systems Research Ltd., 2 Richmond Hill, Richmond, Surrey, U.K.

Pavesic, Mag. Dipl.-Ing. Nikola, Faculty of Electrical Engineering, University of Ljubljana, Trzaska 25, 61000 Ljubljana, Yugoslavia.

Pichler, Univ.-Prof. Dr. Franz, Universität Linz, 4045 Linz-Auhof, Austria.

Pillmann, Dipl.-Ing. Werner, Kegelgasse 19/3, 1030 Vienna, Austria.

Popovic, Dr. Bojan, University of Belgrade, Economic Institute, POB 402, 11000 Belgrade, Yugoslavia.

Raman, Prof. S., Dept. of Epidemiology and Community Medicine, University of Ottawa, Ottawa, Ontario K1N 9A9, Canada.

Reusch, Prof. Bernd, Universität Dortmund, Institut für Informatik, 46 Dortmund, FRG.

Reuver, Dr. H.A., Technische Hogeschool Twente, Afdeling der Bedrijfskunde, Postbus 217, Enschede-Drienerlo, The Netherlands.

Robles, Gabriel, California State University, Northridge, Dept. of Health Science, Northridge, Calif. 91324, USA.

Schindler, Dr. Nikolaus, Institut für Völkerrecht und Internationale Beziehungen, Universität Wien, Universitätsstraße 2, 1010 Vienna, Austria.

Schuler, Albert, Institut für Industrielle Elektronik der Technischen Universität Wien, Gußhausstraße 27-30, 1040 Vienna, Austria.

Silvio, José F., Universidad Central de Venezuela, Caracas, Venezuela.

Stravs, Alexis K., ETH Zürich, Betriebswissenschaftliches Institut, Zürichbergstraße 18, Postfach 8028, Zürich, Switzerland.

Strezova, Z., Scientific Center for Social Information Systems, Opaltchenska St., 112 A, POB 10, 1233 Sofia, Bulgaria.

Tancig, Peter, Institut 'Jozef Stefan', Jamova 39, 61001 Ljubljana, Yugoslavia.

Taschdjian, Edgar, 109-50 117th Street, S. Ozone Park, New York 11420, USA.

Thomas, Prof. Raymond E., University of Bath, School of Management, Claverton Down BA2 7AY, U.K.

Till, Dr. Peter, Technische Universität Wien, Institut für Industrielle Elektronik, Gußhausstraße 27, 1040 Vienna, Austria.

Trappl, Univ.-Prof. Dr. Robert, University of Vienna Medical School, Schwarzspanierstraße 17, 1090 Vienna, Austria.

Uster, Hans, Swiss Federal Institute for Technology, Dept. of Behavioral Sciences, Turnerstraße 1, Zürich, Switzerland.

Yamashita, Prof. Dr. Kazumi, Dept. of Electrical Engineering, Faculty of Engineering, Osaka City University, 459 Sugimotocho, Sumiyoshiku, Osaka, Japan.

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 Pask and Professor Pichler who joined me in editing this volume.

I hope you will enjoy studying this book.

R. TRAPPL

Contents

KEYNOTE INTRODUCTION

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

PROFESSOR P.B. CHECKLAND
Department of Systems,
University of Lancaster, UK

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 experiments can thus be viewed as an attempt at the *refutation* of the hypotheses embodied in the experiments.

Science, as a learning system characterized by reductionism, repeatability and refutation, is manifestly successful. It can point to demonstrable successes, where consulting astrologers or reading the entrails of slaughtered goats cannot. But, nevertheless, it is out of the limitations of science that the systems movement arises.

Science is at its most powerful in the investigation of the physical regularities of the universe, phenomena such as the properties of light, magnetism, the laws of chemical combination, etc. The method is stretched to the limit as the complexity of the phenomena studied increases. It is clear, for example, that the bold program of the pioneers of social science—to establish the laws of society, to set alongside those of physics—has not been fulfilled; questions of methodology in the social sciences are still crucial issues. Perhaps it is not surprising that systems thinking, self-conscious thinking in terms of 'wholes', should arise in the science of biology which in complexity is intermediate between physics and chemistry on the one hand and the social sciences on the other. Systems thinking, thinking in terms of wholes and their properties, appears to be an appropriate weapon whenever the investigation is concerned with densely-connected whole entities which show properties described as 'emergent', that is to say, characteristic of the level of complexity being studied and without meaning in terms of lower levels. If I may quote a summarizing statement from a paper currently in the press [2]:

'Thus while the Weltanschauung of science is that the world consists of groups of phenomena which may be investigated by the method of science, the counter-Weltanschauung of the systems movement is that the world consists of a complex of wholes which we term 'systems'. The systems thinker assumes that the worl will exhibit emergent properties at virtually all levels of complexity and that it will be useful to examine the world in terms of the wholes which exhibit those properties, and to develop principles of 'wholeness'. The long-term program of the systems movement may be taken to be the search for the conditions governing the existence of emergent properties and the wholes which exhibit them.'

The systems movement can thus be seen as a part of the science movement, but one which hoped to make progress in understanding complex phenomena by trying not to be reductionist, by trying to analyze in terms of wholes which, compared with the components which comprise them, show emergent properties. One might say that the reductionism of the systems thinker consists of saying 'I will analyze and explain in terms of components which are themselves coherent wholes'. The intention is not to dispense with analytical reduction, but to create an intellectual weapon which is complementary to it.

Given this view of the nature of systems thinking, as a response to some problems in science, can we surmise why progress has been so slow? Can we obtain from the history of science any indications of what the systems movement ought to do?

It seems to me characteristic of science that its practitioners have on the whole shown a considerable lack of theoretical interest in its method and philosophy and that this has been a positive advantage to the development of science. Scientists have always been intensely interested in *problems, puzzles, or paradoxes,* and have been anxious to take for granted the philosophy of the activity. The systems literature, however, sags under a heavy load of untested assertions, models and conceptualizations. When the Aristotelian world picture was replaced by that due to Galileo and Newton, science took an immense step forward; it happened because motion was taken to be a problem. According to Aristotle motion could be sustained only if there were a local source of motion continuously at work. In the case of an arrow flying through the air Aristotelians had to assume that the air pushed out of the way by the front of the arrow rushed round to the back to provide the push. The flight of a projectile, they conjectured, was a straight line followed by a vertical fall to earth. Now, the point is that the new way of examining such things—experimental science—came to see that these conjectures were simply not true. Galileo discovered that the flight of a projectile was a parabola. Science took up motion *as a problem*, and by solving it changed mankind's way of looking at the universe. Science has always tackled problems; but where are the major problems with which the systems movement is currently engaged? And what kind of testable conjecture is emerging which will allow the problems to be solved? The answers to both questions are bleak ones.

Three hundred years ago Isaac Newton, in a letter to Roger Cotes, wrote:

> 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 navi-

gation and the safety of men's lives and estates upon that element.

In modern language this is a call to 'action research'; I think it is highly apposite today. Addressing this Conference two years ago James Miller suggested that what was needed in the systems movement was less talk and more data, a greater readiness to formulate hypotheses and to submit them to test [3]. That is still the challenge; and finally I would like briefly to indicate one way of trying to face it: the way in which the Department of Systems at Lancaster has been trying to meet it.

III

James Miller's group is concerned with one particular kind of system, namely 'living systems', and their approach is to formulate empirically testable hypotheses about the nature of such systems.

At Lancaster our concern has been with 'real-world problems'. By this phrase we mean problems of decision which arise unasked, which we find ourselves facing, as opposed to the laboratory problem which the scientist can select, define and constrain to suit his inclinations, interests and resources. The problem or puzzle at the foundation of our work is the fact that society is not good at tackling real-world problems, and that science, for all its power in other directions, seems not able to help. Real-world problems are frequently a perceived mis-match between 'what is' and 'what might be', and in this sense are 'management' problems defining this term broadly. The body of knowledge known as 'managment science' has clearly not been very successful so far in providing ways of tackling real-world problems. Certainly that is the case if we judge by the low opinion of management science held by most real-world managers. So we took as a problem the fact that real-world problem-solving was astonishingly difficult; our approach was to try to use systems ideas in tackling such problems; and our anticipation, or hope, was that out of the work would come some system-based methods of problem-solving and, perhaps, a systemic base for an improved management science.

The choices of problem and means of approaching the problem has restricted our attention to systems of a particular kind. Where Miller has concentrated on 'living systems' our focus has been on a kind of system which has not always been recognized as such. We call a connected set of purposeful activities a 'human activity system'. Much of our effort has turned out to be an exploration of the nature of such systems.

In the method of approach adopted we hope to have avoided the danger of merely *talking about* real-world problem solving by making the research 'action research'—of the kind we hope Newton would have approved. We have tried to work within problem situations with problem owners who wanted problems solved. We try to be involved in action-taking, even though this means that research aims formulated before the event cannot always be followed. Action research has to follow where the action leads. The advantage of this has been that it provides in the long term—over a number of studies—a criterion by which the systems content of the work can be judged. If in a large number of studies we can persuade a number of problem owners of different types that problems have been alleviated or solved, or at least that insight has been gained, then we may surmise that some of the virtue lies in the systems thinking used in the problem-solving! This is by no means a sharp criterion, cut and dried; but we, like everyone else working in systems containing human beings, have found it extremely difficult to find a way of formulating testable systems hypotheses. By taking part in the action of problem-solving we have at least insured that we have been using systems ideas, not merely talking about using them.

Our problems tackled in systems studies have been varied. They have covered problems within organizations large and small, both user-supported and public-supported, and they have covered problems not organization-bound. Problem owners have ranged from chief executives in industry, to middle managers, to public authorities, to a charity organization, to a community action group.

All these varied experiences have only one thing in common: that they have encompassed problem situations in which we have tried to use systems ideas in problem solving. What every systems study has had in common with all the others, indeed the only thing they all have in common, is: methodology. So it is inevitable that the outcome of the work has been some principles of systems method—a systems-based *methodology* for problem-solving [4]. What has been most interesting about this outcome has been the fact that its emergence from an action research program has forced us to recognize that methodology is in itself of little value because it is, on its own, untestable. (Success might be due to extraneous reasons, and failure might be due simply to incompetence in applying the methodology!) In order to judge methodology it is necessary to take