SIMULATION and the PROCESSES of CHANGE

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ABSTRACT: The magnitude and rate of the contemporary changes in our society require extraordinary measures if we intend to control them. The systems approach and thus modelling and simulation, can play a central role in putting forward alternative policies that tend to achieve the aggregated goals of society. However, in order to be an acceptable tool for this purpose, the theoretical and methodological foundations of the discipline need urgent attention. The complexity and size of the systems involved will also require powerful tools. Fortunately the research in this area provides vistas into a promising future. Especially,attention is needed for the structure-mapping requirements of societal models, as reasonable validation is otherwise infeasible. Finally the relative merits of two modelling formalisms are compared with respect to their aptitude to represent the main systemic phenomena in our unstable environment.

PROLOGUE:

Sitting in my snug study, surrounded by my favourite books and still feeling surprised by the honour to deliver the keynote speech that was to set the theme for 1980's WSC, I started looking for a motto that would at the same time express my feelings of scepticism as to the results we have been able to present the world - using our specific skill - and my honest wish to stimulate you to persevere in the search to improve the problem-solving capability of mankind. There is no better source for wisdom of this nature than the history of mankind and - to my opinion - nobody more talented to formulate it than the late Jacob Bronowsky. Let me therefore present the last sentence of his last book as a theme for this conference:

> *"We are all afraid - for our confidence, for the future, for the world. That is the nature of human imagination. Yet every man, every civilisation, has gone forward because of its engagement with what it has set itself to do. The personal commitment of a man to his skill, the intellectual commitment and the emotional commitment working together as one, has made the Ascent of Man."* (Bronowsky 1973).

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ON CHANGE:

"During the second half of the 20th century, history has taken an apocalyptic turn: our capacity to produce, to multiply, to pollute, to lay waste and to annihilate has frighteningly outgrown our capacity to control, foster and harmonize." (Wiener 1978a).
Indeed changes follow each other at such a pace and with such a degree of complexity that even the best of all possible leaders can no longer even pretend to know where we are going. The underlying reasons for these changes and their pace are not purely technological nor demographic. The (socio-political) scale of these phenomena has jumped an order of magnitude in terms of the amount of parties involved through their communication interlinkage and their economic interdependence. (E.g.: it is unthinkable in our time that a country could stop inflation locally, while the world's average rate of inflation per year is more than 6%. No country can hide - for any extended period of time - its internal problems: the media distribute to the remotest corner.)
On the other hand: *"Men find it impossible to predict the behaviour of a small interactive servomechanism with a handfull of variables, once feedback is introduced. If the transfer functions are non-linear and responses are lagged, they find the task impossible."* (Beer 1975a). This "quantum-jump" which affects the whole of mankind should not be seen, however, as a dead-end to our evolutionary process, but rather as one of the "discontinuities" that we can also observe in history (e.g. the transition from the Middle-Ages to the Renaissance): *"the unconscious phase of history is now past..... Consciousness of specialized technical methods must be balanced by consciousness of general developing forms."* (Whyte 1948).
Although we often will find it very difficult to accept, the widespread participatory turmoil - mainly among the younger generations - of our Western world is a sign of emergence of the endeavour to influence our evolution consciously. Science shall not and can not stand aside during this critical phase. because *"consciousness is a phenomenon in the zone of evolution. This world lights up to itself only where or only in as much as it develops, procreates new forms"* (Schrödinger 1958), and this last activity is first and foremost the task of science.

What then, should you ask, have systems and simulation to do with all these philosophical remarks. The key words for bridging this gap are SYSTEMS and PROCESSES. Because, although *"The Newtonian 'God' - the 'God' who made a clocklike universe, wound it and withdrew - died a long time ago.... The groundrule of that universe, upon which so much of our Western world is built, has dissolved"* (McLuhan 1969); *"Process-thinking leads the observer to recognize... a universal formative process in nature, a process in which.... forms are developed and transformed. Nature is not a chaos of particles, but a process which consists in the development and transformation of patterns (such as are evidenced in the structure of molecules, crystals, tissues, organs, and in organisms and their behaviour patterns)"* (Whyte 1948).
Apparently our simplified, 19th century, - *mechanistic* - view of the world around us as some gigantic - *optimizable* -semi-static machine (a machine whose operating parameters can change but of which we can not conceive the structure itself to be modifiable) is an anachronism. This - however - does not invalidate the possibility to conceive that same world as being a *system* of *process* with *time-varying* relations. Although the odds of practice are still against us, and the trend in the management of global affairs is towards the incremental approach (a unique - and strictly temporal - response to every stimulus),

there is no doubt in my mind that management and control based on - even limited - knowledge of the system as a set of dynamically related entities is the only approach offering us a chance to cope with the complex task of controlling our self-induced changes.

"In a nutshell, the argument against a strictly systemic approach has been that we are too ignorant, too divided in our aspirations, too unstable in our psychological equilibrium and too anarchic in our social organisation to attempt systemic planning. The argument against the incremental approach has been that, since it is passively adaptive, it uses only past experience to guide its responses. Since it cannot respond innovatively to unprecedented situations, it will usually provide palliative solutions rather than real cures, and tend to neglect the side-effects (externalities) of these palliative measures. Thus, while it may temporarily alleviate our most acute and critical immediate problems, in the long run it will exacerbate our fundamental disorders" (Wiener 1978b).

Therefore the choice for systemic management approaches is a necessary one. Maybe the most important facet of this approach is the setting of a course only after rational evaluation of the effects of alternative courses. This implies: the simulation connection.

ON SIMULATION FUNDAMENTALS:

"If one looks carefully at human activity, one finds that the use of models and the simulation of activity is ever present, from early childhood to old age. It often appears hidden under such phrases as Let's try the following. *We simulate by actions or by thought-experiments. In fact, we simulate so much that the distinction between model and reality may become fuzzy at times"* (Bekey 1977).

It is eminently clear, that after such a statement by an eminent modeller and simulationist, I shall not try to (re)define simulation in all its generality. Neither will I endeavour to establish if simulation is an art, a science or simply a dominant property of our cultural heritage.

I am therefore extremely aware of my (and your) limitations when - for the purpose of this keynote - I center my discussion around the concept that *simulation consists of carrying out mathematical similarity studies using computers.*

In this sense simulation is common practice in many fields of science, which all claim ownership rights to this speciality. To mention but a few: cybernetics, general systems science, engineering (especially control-), management science, operations research,econometrics and - last but not least - physics.

Being used in common by so many disciplines (almost like mathematics itself) why then, is it not a recognised field of science on its own right?

Possibly because: *"..... simulation still carries the label of an expensive, uncertain problem-solving technique that represents the 'court of last resort'...... Others, admittedly a much smaller group, are concerned with the lack of recognition of a fundamental structure - a theory - after so many years of practice."* (Nance 1980).

Being a scientist by profession, it comes natural to address the theory question first. A theory on which modelling and simulation should be based finds its fundament quite obviously in the realm of general systems research and its direct derivates. The first comprehensive introduction into this subject has been written by Zeigler (1976). Further up to date reading is provided in Zeigler, Elzas, Klir & Ören (1979) which forms a document on the latest progress

in Systems Modelling and Simulation Methodology as reflected by selected papers from the first meeting ever to concentrate uniquely on this subject.
Taking this literature as a starting point, and postulating for the time-being that there exists such a thing as similarity between an (abstract) model-in-a-computer and its real life counterpart, it seems obvious that it should eventually be possible to construct similarity laws between this abstraction and the real system, just as such laws were generated for scale-experiments in the field of physics in the 19th century (e.g. Froude 1870). Naturally in the complicated systems that we try to simulate nowadays a number of different sets of precise similarity conditions (or"rules") will have to be envisaged.
Assuming that the different aspects of the system, giving rise to different kinds of similarity rules, can be considered separately, we can proceed to discover the similarity laws for every aspect: this in order to be able to use the results of a simulation study with any degree of trust. (This does not supersede validation whose relative value will be discussed later on).
The assumption just made means that the (real) system under consideration can be decomposed into subsystems which can be analysed separately. This also means that the result of an aggregation of the analyses of the subsystems (taking into account all their interrelations) over the total of subsystems under consideration, forms a complete analysis of the original system.

At this stage I deem it to be evident that any description of a real system that does not allow this *composition* assumption to be verified, is not amenable to simulation.

In general, similarity laws that apply to simulation can be subdivided in two classes that together provide the fundamental basis for overall similarity:

1. Behavioural Similarity: in which the *real system* is considered merely as a source of data describing its behaviour, and the simulation merely a set of commands to some automation that is capable only of generating data behaviourally similar to the source data. This similarity therefore only reflects a similarity of magnitudes. The "nature" of the simulation data are on their own multi-interpretable and can only be formalised by point-to-point comparison with the real system.

2. Structural Similarity: a model is considered similar in structure to the real system if it truly reflects the way in which the real system "operates" to produce its behaviour. The composition assumption than entails that this type of similarity should also be valid for (partial) decompositions of real systems and model. (More details on this subject can be found in Elzas 1979a).

From the above it should have become clear that we can only *claim* to gain more understanding of the functioning of a real system through simulation if a minimum of structural similarity is achieved in the model. No amount of data put forward to support a strictly behavioural simulation can change its restricted applicability or: *"It is important to understand very clearly that strengthening a particular technique - putting muscles on it - contributes nothing to its validity"* (Weizenbaum 1976a).

Fundamental research in the similarity domain is urgently needed if some of the uncertainty surrounding the results of simulation is to be removed.

Karplus (1976) has shown that simulation can only solve three (interrelated) fundamental types of problems. These are:

a. Assuming that knowledge is available on a system and its input vector, compute its resulting output characteristics *(Analysis/Prediction)*
b. Assuming knowledge of input vector and resulting output characteristics, establish the nature of the system *(Synthesis/Identification)*
c. Assuming knowledge of the system and the (desired) output characteristics, compute the corresponding (necessary) input vector *(Management/Control)*.

(Note that the term vector is used here for a set of time-trajectories).

It is remarkable that the general approach to the solution of these problems displays iterative characteristics (One usually solves problem a. for a large set of trial systems to find the solution to b., and solution of c. more often than not entails iterative solution of sets of a. and b. problems).
Unfortunately right now we are still stumbling forwards in trying to solve the problems of Prediction validly for any larger system, while the problems of change require us to be proficient in the most difficult area: Management & Control (Elzas 1978a). Serious doubts can, by the way, be expressed as to the possibility of solving c.-type problems a priori for any system containing adaptive elements (like e.g. human beings).

The nature and fundamental aspects of simulation also depend to some extent on the purpose of the exercise. Those who work in the field of Artificial Intelligence often speak in this context about working modes. These modes do not only describe the way in which the computer is used, but also the relation between this use and human activity. Three working modes can be recognised (Weizenbaum 1976c):

Simulation Mode :	understanding/designing/planning by imitation
Performance Mode:	designing/controlling based on whatever (goal-oriented) principle one can discover
Theory Mode :	designing (abstract) theories based on analogies (practical entities being mere models of theories).

It can be stated that, just as the inventions of the early pioneers in technology, the origin of simulation and modelling hails from imitation of apparent reality.
Modern simulation, however - although the dividing line between the old and the new is not absolute - is much closer to an operation in the performance mode. It is a systemic, interdisciplinary and experimental activity, dealing with the structure and the behaviour of large and complicated systems in which alternative behaviours are studied in accordance with intuitive and/or theoretical expectations with respect to goal-achievements.
Clearly, I do not believe in simulation as a general problem solving technique, whatever any enthusiast might say. What is more: I would like to postulate that there are no such things as "general problems". As long as many of us still have to spend their valuable time - for whichever relevant reason - in solving irrelevant problems partially, we still stand a

reasonable chance of success with our idiosyncratic approach to experiment with possible problem solutions, as long as we keep the perspective on the total system involved.

All this sounds commonplace, but there are sufficient examples of nearsighted problem solving in the news every day to keep us wide awake and active.
There are scores of (partial) wonder-solutions offered to mankind everyday, that cannot even stand the test of (total) system validity performed by simple mental calculation. Let me give you a few extremely simple examples:

Very recently an important (German) electrical industry announced on the European newsmedia that the cure of the world's energy crisis could be found in establishing energy-farms of large arrays of photo-voltaic cells in tropical desertic areas. It claimed that in this way about 40% of the global energy needs could be provided in the 1990's. Quick (mental) verification brings out the following: today's global energy needs are approx. $1.5*10^{12}$ Watt/hour. Using the highest performance photo-voltaic cells that we know today (as used in aerospace) one would need an area of approx. $1.7*10^5$ km^2 of desert completely covered with cells. (All of the Sahara were it flat and empty). The consequent change in albedo of this desert area would moreover be so large, that even if the occupied area was smaller (e.g. fivefold increase of performance of cells), the prevailing climate in Europe (with a high pressure area above the Sahara) would be seriously disturbed, etc.
Another example of a similar kind comes from the field of computer technology. The (cryogenic) Josephson gates will provide us in the near future with logic working at Gigarates, but: within the almost 40-year -old Von Neumann architecture. (For a clear critique of this architecture see Backus 1978). You will, no doubt, find yourself lots of funny mental games to play with nearsighted problem "solutions"! (A few hints: reflect on total conversion of the US energy supply to coal or wind-energy, a complete return to artisanal industry -"small is beautiful"-, or a hard-line policy towards all foes).
So, what is wrong with the fundamental structure of simulation, if we are able to disprove - by a simple mental exercise - the results reached after massive application of computer power? Nothing much except ourselves. (Elzas 1978b).
"The methods become clear once the stereotypes are overthrown, the cause of the problem is revealed, and the need to design viable systems is accepted - whole books are available about how to proceed." (Beer 1975b).

ON MODELLING FORMALISMS:

To be quite frank: the main problem in our field today is the difficulty for every modeller to get rid of the *ingrown stereotypes* that have been with him since his schooling days in the basic aspects of simulation.
Ören (1979) lists and describes eight different modelling formalisms that are with us today. What is cause for serious concern is that only in exceptional case one finds a simulation practitioner that transcends the boundary from one formalism to the other. In most cases the situation is even worse: the problem is not one of sticking to a formalism stereotype but to a specific simulation language. The case is well posed by Nance (1980):

"Language differences, or modelling differences, are not bad as such, but when an interpretation becomes perceived as a theory, confusion arises as to the primacy of concepts and the generality of application. The seemingly independent and concurrent development of several Simulation Programming Languages during the 1960-68 period magnified the confusion caused by the 'interpretation/theory inversion'."

Reading Nance's report one discovers that the malady he registers is even far more widespread and has wider consequences than he shows. The whole family of system-dynamics and continuous simulation formalisms and languages is left out of consideration, and yet the proven confusion of time and state relationship formalisms is shown to be almost catastrophic in the realm of discrete simulation alone.

All of us who thought that only the rift between continuous and discrete simulation worlds was a matter of major concern, are in for a sobering surprise when digesting Nance's first report in a series on this subject. Even the prestigious Summer and Winter Simulation Conferences display clearly the extent of the gap within the world of formalisms.

Is the gap real?

Is it the result of actual differences in the nature of the systems described?

Zeigler (1979) has shown without a shadow of any possible doubt that this phoney gap is - at the utmost - a product of our own stereotyping and not more than a difference in which *aspects* we associate with the *entities* we wish simulate. So: *There is no longer any excuse to keep holding on to our trusted modelling stereotypes and to pretend that formalisms, that are only different aspects of one and the same system, have the level of rigorous - and intrinsic - theoretical system-nature differences. Keeping our nose to our accustomed - stereotype - track will in the end result in the final discrediting of simulation as a tool, through the domination of interpretation disputes.*

A lack of vehicles (that is: Simulation Programming Languages - SPLs) for description of systems with different aspect-associated formalisms is no excuse either: there are at least three widely used SPLs that provide facilities for formalism mixing (e.g. GASP, SIMULA, SIMSCRIPT).

True, these vehicles all display some flaws in that they still have some peculiar numerical problems under certain circumstances or do not provide for the whole range of discrete and/or continuous formalisms. But as long as there is no strong user base which really makes full use of these advanced features (and complains to the makers about apparent flaws), improvement will stand no chance.

This phenomenon is not unique to simulation but - alas - widespread in the entire field of computer usage: *"All too often we dwell upon making mechanisms for productivity which, like birth control, are most practicable for our neighbours, not for ourselves."* (Negroponte 1977).

The "closing of the gap" in fact only depends on two changes:

a. Finding, enumerating and standardizing the different modelling formalisms, so as to be able to provide the right interfaces to allow transit from one formalism to the other

b. Leaving behind us the concept that a language,in which we can formulate models for simulation on a computer, should also cater for conventional algorithmic (if you wish: procedural) computer programming. In other words: making the transition from SPLs to MOLs (Model Oriented Languages).

There is an - I must admit not widely known - international committee that is trying to do just that, and which I have the honour to chair. The acronym of the committee aptly describes its proposed function: CISMOL (Committee for International Standardization of Model Oriented Languages).

Although the committee has only just started its work and still is a long way removed from reaching its goal, I consider myself unusually lucky to be able to work together with a great number of competent people whose names carry a more prestigious sound than mine in the simulation world. Therefore I have high expectations as to the resolution of the formalism confusion as we experience it today.

ON TOOLS:

"In the environment of the virtuoso machine, one needs only remember (and worry about) one of the few domains of consensus about creativity, namely, the suspension of critical judgment in moments of collaborative effort to find that for which you do not know you are looking" (Negroponte 1977).

It should be clear, from this quotation, that to my opinion the ideal tool (a combination of hardware, software and data) is an "intelligent" medium that acts as a depository of the knowledge and experience of a multitude of toolmakers and users in order to enhance creative approaches for the solution of complex problems.

In the context of the problem we face, affecting the very functioning of our society, considerations as to speed and cost are irrelevant when compared to the essential problem-solving capabilities needed.

A brief look around our immediate, daily, operating environment clearly shows that - until now - simulation has been forced to make use of readily available tools, mainly designed with other main purposes in mind. This does not mean that we cannot make use of the tool-expertise acquired in other fields of science and technology. It does mean, however, that the time is past in which we can afford to bend our theories and formalisms to fit within the straight-jacket of the limited tools which are - more or less accidentaly - available in our immediate neighbourhood. Fortunately, since the middle of the 70's, research has been restarted on - theoretically well founded - advanced tools (hard- and software) for complex system simulation.

Within the scope of this keynote I am, unfortunately, not able to do justice to all those that are active in this field. So, instead of trying to offer you an exhaustive enumeration, I will limit myself to mentioning a few examples which - to my personal opinion - are outstanding examples of the new trend towards theory-based tools.

Hardware:

Two main research avenues have become visible in novel simulation hardware design:

a. General system-structure oriented architectures
b. Data-structure oriented architectures.

In class a. two mainstreams can be observed. One of these streams is based on the notion that while the LSI/VLSI-technique allows the production of relatively fast, and moderately accurate, microprocessors for reasonable prices, "conventional" analog circuitry of sufficient accuracy and speed is no longer economically feasible it could be feasible to imitate analog computer architecture with discrete circuitry, while adding some advantages of stored program concepts. It has been shown that such an approach allows the scope of this architecture to be widened in terms of classes of problems that can be solved in principle. (Cyre, Davis, Frank, Jedynak, Redmond, Rideout 1977; Elzas, Smeenk 1977). These designs, however, are only based on an extremely limited subset of modelling formalisms and probably do not have a future as general system-oriented machines.

A second avenue has been opened during the last five years, by a group of researchers centred around the Delft University of Technology. This group is circulating a research proposal at this moment in which a parallel simulator is proposed that reflects the state of the art in concepts for total system support in the modelbuilding/experimenting/modelbase area. (Dekker, Kerckhoffs, Vansteenkiste, Zuidervaart 1980).
Although it seems too early at this moment to be able to judge the merits of such a venture, the concept fits well into the requirement for more theory-based tools.
Two design concepts that fall in the second class are interesting enough, to my opinion, to be mentioned here.
The first one of these is based on the idea that a necessary and sufficient data-model can be defined in order to obtain standardized data-entities (comparable in structure to a Simula-Class for example). This (semantic) data-structure, for which the associated architecture can be described in the form of mapping operations, has been shown to be implementable in a specific parallel architecture organized around operations executed on data streams. The architecture concept implies that the physical structure of the machine directly reflects the (internal) data-structure. In simulation terms: this is a formalism-based machine, but structured in such a way that if a unique, standardized, formalism-structure can be defined it could well be used to advantage as a general system simulation tool of high performance. (Giloi, Berg 1977).
The second data structure oriented machine that comes to my mind, is one that is in operation today and that directly reflects the structure of multi-variable functions in its highly parallel architecture.
This machine is based on a central,multi-port,fast-data-memory to which a number of specialized pipe-lined processors (each with their own program memory) are associated. Although the machine has originally been designed with specific (aero-space) applications in mind, the architectural concept allows also in this case to build a general formalism oriented device by the addition of more (special-purpose) processors. (Gilbert, Howe 1978).
A concluding remark on hardware could be that specific simulation hardware designs only have a chance of success if they are soundly based on the fundamental concepts of system simulation <u>and</u> are able to handle extremely complex and large model descriptions with which one can experiment at sufficient speed to keep model-experimenter interaction at a rate adapted to the human time frame.

Software:

There are two different tasks in the modelling/simulation context that are amenable to treatment by software. These tasks are: model construction/structuring/documentation and model experimentation/execution/evaluation. More and higher tasks are conceivable as pointed out in Ören (1980).

Detailed information can be found in Zeigler's state-of-the-art lecture for this conference (Zeigler 1980).

Let me, however, stress here that progress is being made in the specific direction of Negroponte's "virtuoso machine" in the area of both tasks mentioned above.

The first item that comes to my mind in this context is ESP (an Entity Structuring Program) built by Belogus (1980) under the direction of Zeigler. This is clearly a product which falls under Ören's Cybernation category (Ören 1980). Fortunately, you will not have to wait long to get to know this tool as Zeigler has included an example of its capabilities in his text.

A product belonging to the area of the second task-category, but still in its definition phase, is GEST (Ören 1978a; Ören, Den Dulk 1978; Elzas 1979b). The purpose of GEST is to provide a multi-formalism, methodology oriented, model description language that at the same time has all the (known) features available that are needed for the execution of simulation experiments with built in safeguards as to robustness of operation.

ON ACCEPTABILITY:

In the terms of Ören (1979) I will only consider here the issues on *Acceptability of a Model (and its simulation results) with Respect to the Real-System.*

A large number of papers too numerous to be referenced here) have discussed the necessity and the techniques for the *Validation* of models. Most of these papers imply that a *Validated Model is a Credible Model.*

This - alas - is an axiom which is completely based on our experience with technical systems, which are by definition mechanistic in nature (see discussion of this notion earlier on in this keynote).

Although our experience with computer-based simulation is at the most 40 years old, we have much longer experience with models and their validation. For more than half a century it has been common practice in econometry to take the general (mostly linear) equations suggested by some economic theory, quantify the coefficients of the variables in the equations by fitting them (e.g. by regression) to past data and use the model so *particularized* to forecast economic trends or to judge the sensitivity of the economy to different policy-scenarios (see a.o. Coats, Parkins 1977).

Although such a model can be considered as to have been validated, it is nonetheless far from credible or even acceptable.

"Wenn es etwas gibt, das einen Laien auf dem ökonomischen Gebiet der Mut geben kann zu einer Meinungsäusserung über das Wesen der beängstigenden wirtschaftlichen Schwierigkeiten der Gegenwart, so ist das hoffnungslose Gewirr der Meinungen der Fachleute (If there is anything at all that can give a layman the courage to express his opinion about the nature of the disquieting problems of today's economy, it certainly is the hopeless muddle of the opinions of the experts)." (Albert Einstein, in an article published in 1929, ref. Einstein 1955).

There are even completely undecidable problems in model validation (certainly when a structural similarity is not achieved). As an example let us look at R.E. Overstreet's experiments with models of the so-called "prisoners dilemna". (The dilemma is the following: 2 criminals are incarcerated after committing a crime. They both know that if they both admit their culpability they will get 3 years in jail, if neither confesses they both will get only 1 year, if only one confesses he will be freed in order to secure the conviction of the other who will then get a sentence of 5 years. The prisoners are interrogated separately.)

Overstreet proposed 7 different models of behaviour, that he then proceeded to compare with a large sample of real situations consisting of repeated experiments with the same set of people. Overstreet's model took into account the learning effect that would surely be present in his validation experiment. Three models were slightly special.,

The Naïve model in which the same prisoner would always give the same answer, the Contingent model in which the prisoner would always try to invert the outcome of the trial - basing himself on his knowledge of the last trial - and thirdly the Chance model which implied a completely random choice of answers.

It is clear that Naïve and Contingent represent exact opposites. The validation experiments, however, gave no basis at all on which to decide which of the models fitted reality best. Naïve and Contingent were statistically completely equivalent, Chance was worse in any case! (Overstreet 1971). Apparently *"In applying the theories one should take into account the categorical differences of inanimate systems that scientists and engineers are so much familiar with, and the systems having humans as components"* (Ören 1978b).

If validation of systems with time-varying structure is so difficult (and can be proved nonsensical at times), how is one to proceed in constructing models that achieve at least some degree of credibility?

Some sources advocate to proceed by consensus in such a situation, using some kind of high-level opinion poll process, just as we do this for predicting the outcome of presidential elections. We all know, by now, the relative merits of any scheme of this kind.

Theoretically such a procedure could naturally work; however, the semantic problems inherent to the method are such that we need not even begin to try. Let me offer you an anecdote to illustrate this: A young lady visits her M.D. and says: "I would like to get a fourth child." Says the M.D.: "I do not see any problem with that, considering that you already have three thriving youngsters." "Yes", says the lady, "but I am extremely worried since I read in the newspaper that every fourth child that is born is Chinese!"

Naturally we could try to operate by consensus in a multidisciplinary team of experts, but:

> *"Thus we come to manage an oversimplified model of the world*
> *that exists only in the mind of the consensus*
> *instead of the real world.........out there"* (Beer 1975c)

Experience in the last few years has shown that the team we most need is not necessarily a multidisciplinary one, but certainly one consisting of competent people that have a talent for analysis and synthesis and moreover a lot of common sense. But that is not all, the modelbuilding process itself should comply at least to the following rules:

- the model should be transparent and usable by others than the original drafters
- the goals of the modelling process should be clearly and explicitly stated
- verifiable parts of the model should be validated
- any structure discernable in the real system should be painstakingly reflected in the model
- claims as to applicability of the model should be critically evaluated

- the model should be incorruptible (robust wrt. its inherent presuppositions)
- the model should be used in an objective way (Elzas 1978b).

Finally: *"We must always be sure that any lessons we learn about a modeled entity by studying its model would still be valid if the model were removed"* (Weizenbaum 1976b).

ON the MANAGEMENT of CHANGE:

"If I should press these views on men strongly inclined to the contrary, how deaf would they be to it all! Stone deaf, no doubt - I said - and no wonder! To tell the truth, it seems to me that you should not offer advice which you know will not be considered. What good could it do? How could such a bold discourse influence men whose minds are prepossessed and deeply imbued with contrary aims? Such academic philosophy is not unpleasant among friends in free conversation, but in the King's council, where official business is being carried on, there is no room for it." (Thomas More 1551).

After the publication of "Limits to growth" (Meadows 1972) - of which a few million copies were sold in the Benelux alone - and the impact of the Energy crisis in 1973, a great number of groups all over the world suddenly started to get interested in modelling the very complex multifaceted systems which influence global and national policies. The movement got such an impetus that e.g. in The Netherlands a group of leading experts coming from as disparate fields as econometrics, computer science and sociology was brought together in order to discuss the possible actions that the government and industry should undertake in the aftermath of this publication. The upheaval did not stop at this: Her Majesty Queen Juliana convened a special meeting in her palace in Amsterdam, to which representatives of all major ecology, conservation, industrial, political and scientific organisations were invited in order to achieve a free exchange of ideas on the subject at hand. A permanent national Energy Advisory Committee was formed, with as its main task the study of alternative forms of energy supply. It should be noted that nothing of the kind happened after the publication of "Mankind at the Turning Point" (Mesarovič, Pestel 1974).

Possibly because the first publication (based on a relatively simple and therefore far from complete model) had the merit to clearly show causal relationships, structural similarity with "known" phenomena and feedback loops. In more general terms: *in the management of change the witness and the steersman is more interested in the structural aspects than in accurate predictions whose background he can not fathom.*

There is more mathematical (or if you wish system - theoretical) foundation in this statement than what meets the eye.

The (econometry-based) predictive techniques that are mostly used for such analyses have serious limitations. These limitations are caused mainly by three reasons: the (mathematical) nature of the models, the environment in which they operate and their utilisation mode. Typical examples of these models can be found in Coates, Parkins (1977) and Norman (1977).

On their nature: *"The major problem is dealing with changing parameters. Most time-series models (stochastic, ergodic, markovian) describe the behaviour of stationary processes, assuming constant parameters. But changing parameters constitute a major management problem which cannot be assumed away"* (Howland, Colson 1978).

The stationary process approach entails that the dynamic characteristics of the transition of one state to the other (and that is what change is all about) are neglected. This was not much of a problem in the situations in the past where the excursions from one state to the other entailed relatively small changes in the total socio-economic situation. However, now that not only national systems are in an almost constant state of flux, but also the international scene knows several major upheavals per year and the national and international socio-political scenes have become very closely tied together, there is no more basis for applying steady-state philosophies for modelling these phenomena which are extremely dynamic. In other words:
"This theory turns out to repudiate one of the few cybernetically valid mechanisms for self-regulation that the economy has ever had. It turns out to entail that all growth is both minuscule and uniform, which cannot be true." (Beer 1975d).

Very often modern economy-, political- and other policy-evaluation models are based on a definition of "scenario's" and subsequent optimisation efforts. In this approach the scenarios have to be specified in full detail beforehand. The effects then studied are mostly limited to the elements described in the scenarios. There is almost no structural possibility to include interaction effects unknown at the time the scenarios were defined. Because of this the scenarios studied might completely miss a reality which can occur at a later date, but which is "unconceivable" at the time the original scenario-driven model is set up. In other words: scenario-driven models tend to behave like self-predicting prophecies and are mainly dependent on the background of their makers. *The important consideration that humanity should strive to control its own scenario is lost in this context.* This is *not* a theoretical disadvantage, but can have far-reaching socio-political implications in the sense that the population will feel driven, rather than in control, and the *decision makers (who - more often than not - have to leave scenario-construction to experts) will not feel confidence in the resulting models nor will abide by the scenario's and policies which are presented to them.*
Here again the key-word is structural mapping of the real system into the model, so that the effect of changes in the inner structure of the system can be evaluated on the model, comprehension can be enhanced, and eventually, control possibilities assessed. (Not only in the sense of controlling driving forces, but also in the sense of adapting structures.)
"In spite of the difficulties of solving them, the problems of adapting to change continue to grow, and a 'paradigm shift' from optimizing to adaptive models is indicated" (Howland, Colson 1978).

At this stage one should face the fact that applying the best of our knowledge, the most competent of our people and the best modelling techniques will not - in itself - imply that we can manage change.
"At present, the most obtrusive outcome of the system we have is gross instability of institutional relationships....." (Beer 1975e).
The time of the Stalin , Mussolini, De Gaulle, etc., are over. Modern history has shown that coping with change is not a matter anymore of merely strong leadership. Entire mankind faces the dilemma. Novel social structures will have to be created in order to create the framework in which large numbers of people can participate in the necessary adaptation to the new circumstances. Time cannot be turned back.
These novel structures have clear implications for the structuring of the societal models which should help to manage change.

In trying to assess how one can create the right environment for participation in this unprecedented venture, the notion of a Meta-Market is put forward by Wiener (1978c). The rationale behind it is based on the notion that goals for any society should be negotiable from the individual up to the highest democratic level, because:
"Goal-setting ought not to be a once-for-all decision but a continuous self-sustained process; the more reason to leave it to the individual" .(Wiener 1978d).
It would naturally be inconceivable that an individual goal which would have widespread side-effects can be granted a-priori to any citizen. The individual goals have to be aggregated (probably with compromises) into ever higher social-organisation levels. (e.g. from the workers to the team, from the teams to the factory, from the factories to the industrial sector, from the sectors to the national scale.)

In the same way global goals (achieved through aggregated compromises) will have to be decomposed back to the individual level.
In such a (not unconceivable) participatory social organisation it seems rational to give every party at every hierarchical échelon the opportunity to weigh the relevant compromises with a model adapted to the adequate level of hierarchy. This then clearly implies models structured after the societal organisation and fitting together in the same way. Although the described modelling part still belongs in the realm of futurology, the described organisational structure has already been in operation for many years in the labour unions of several European countries (e.g. Sweden, Denmark, Holland).

I can well imagine that many of you will feel a chill running down your spine when being presented with such an involvement of modelling in society. The problem nevertheless is that: *"Acceptable Man is competent no longer. But Competent Man is not yet acceptable. Here lies the dilemma"* (Beer 1975f).

EPILOGUE:

Reaching the end of this keynote "manifesto" I am very aware of the fact that a large majority of this audience will either consider me as a charlatan, a naïve academic (which is worse?) or a science fiction writer. I claim that neither description fits me, and that I am extremely aware of the stress I have imposed on your praiseworthy desire to safeguard your scepticism. I therefore should maybe offer my heartfelt apologies, but: *"Overselling the science which offers one hope in a doom-laden situation is a venial sin. It is better than selling science short in a desperate attempt to look dignified and mature"* (Beer 1975g).

There is one final, modest, hint that I would like to pass on, worded by a man of our time, the playwright Bertolt Brecht:

"But you, listeners to this story
Impress on your memory this wisdom of the Ancients:
That all things belong to those that take them to heart,
Thus let the children belong to the motherly in order to thrive,
Thus let the wagon belong to the good coachman, that it be driven well,
And let the valley belong to the irrigator, to bear fruit." (Brecht 1955).

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