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## **IF YOU DO BELIEVE THAT YOUR INDUSTRIAL SYSTEM IS REALLY COMPLEX, THEN... (\*)**

by Jean-Louis LE MOIGNE <sup>(1)</sup>

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*Abstract. – In order to evaluate the complexity of a large system, a typology proposed more than forty years ago by W. Weaver, (« Science and Complexity ») suggests that we differentiate models as disorganized and as organized complexity. In order to take into account the effective role of the « observing system » in the modeling process, we suggest to transform this typology and introduce a concept of « organizing complexity ». Reviewing the rich experience acquired since 1948 in modeling complex systems, we identify two basic principles: the Principle of Intelligent Action, and the Principle of Systemic Modeling. An epistemological discussion leads us to reconsider the methods and the languages often used in the modeling of industrial systems and to suggest an alternative language for handling their complexity more effectively.*

**Keywords:** Modeling of complex systems, organized and organizing models, Project design, symbolic representation, heuristic search method, intelligent systems.

*Résumé. – Afin d'évaluer la complexité de grands systèmes, une typologie proposée il y a plus de 40 ans par W. Weaver suggère que nous différencions les modèles en termes de « complexité désorganisée et complexité organisée ». Pour tenir compte du « système observant » dans un processus de modélisation, cet article suggère de transformer cette typologie et introduit le concept « de complexité organisante ». Résumant l'expérience acquise depuis 1948 dans la modélisation de systèmes complexes, nous identifions deux principes fondamentaux: Le Principe d'Action Intelligente et le Principe de Modélisation Systémique. Une discussion épistémologique nous amène à reconsidérer les méthodes et les langages souvent utilisés dans la modélisation des systèmes industriels et de proposer un langage alternatif permettant de faire face plus effectivement à la complexité.*

**Mots clés :** Modélisation de systèmes complexes, modèles organisés et organisation de modèles, conception de projets, représentation symbolique, recherche heuristique, systèmes intelligents.

### **1. HOW MANAGERS SEE THE INDUSTRIAL SYSTEM THEY HAVE TO DEAL WITH**

Is it our industrial environment which has changed since the development of operations research and of manufacturing management, or is it our conception of « the motive, the structure and the process of industrial

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management » (Tapiero, 1993) which have changed since the development of the concept of industrial systems? Some experts explain that we have to change the methods and perhaps the basic principles underlying the management of industrial systems « because » contemporary industrial systems have changed significantly (growth of internationalization, new products, new processes, new information technology, new environmental problems, reduction of product life cycles...). Some consider that a transformation of industrial management tools have previously been introduced « in order to » bring about these expected transformations of our industrial systems. We are then facing a typical « chicken and egg » situation, seeking the right means which will lead to the right ends, on the one hand and seeking the right ends explaining the choice of the right means on the other! In both cases « the basic question recurs: what shall be done next? ». (Newell and Simon, 1976).

A heuristic method dealing with these recurring problems consists in presenting them in evolutionary terms: considering the last hundred years for instance, can we identify some intelligible trend in the relationship between the state of the industrial system(s) at a given time and the principles and methods of industrial management practiced at the same time, previously, or later?

As the exercise was carried out in the past in various occasions, we can perhaps attempt to interpret them in order to reach some conclusions (or at least reach some well grounded hypotheses) providing thereby a good foundation for the design of industrial systems.

W. Weaver in *Science and Complexity* was announcing the birth of two new sciences, computer science and operations research (« the mixed team approach to operations analysis »), which « may well be of major importance in helping science to solve these complex twentieth-century problems ».

These « complex problems », explained W. Weaver, were problems that science had not been able to solve previously: considering impressive developments in the sciences ever since the seventeenth century, he observed that sciences initially dealt with « two variable problems of simplicity »: the cartesian or newtonian approach and « disorganized complexity ». The former explains mathematically the value Y by the value X of its cause. The latter approach (late nineteenth century), initiated in the physical sciences provided interpretations to « nature » of an essentially new type. Rather than study problems which involved two variables or at most three or four, some imaginative minds went to the other extreme and suggested: « let us

develop analytical methods which can deal with two billion variables ». Thus powerful techniques of probability theory and statistical mechanics were applied to deal with what may be called problems of « disorganized complexity »... ».

But the « really important characteristic of the problems of this middle region, which science has neglected lies in the fact that these problems, contrasted with disorganized complexity points out to the essential feature of « organization ». In fact one can refer to this group of problems as those of « organized complexity ».

Introducing, in 1948 *organized complexity*, W. Weaver illustrated it through numerous examples, « What makes an evening primrose open when it does? »... to « How can one explain the behavioral pattern of an organized group of persons such as a labor union or a group of manufacturers...? » And he concluded: « These new problems, ...require that science makes a third great advance ». Science must, over the next 50 years, learn to deal with these problems of organized complexity ». Further, there is much general evidence, and two recent instances, especially promising. These instances pointed to were the computer and the management sciences (seen as an interdisciplinary operation analysis).

Nearly fifty years later, can we consider that W. Weaver was right in his diagnosis and in his prescription?

As often in such cases, the answer appears mitigated: Yes and No. « Yes » because one may consider that if science has not yet achieved this « third advance », it is because all the scientists, (even computer scientists and management scientists, including operations researchers) have not read and discussed enough the W. Weaver's thesis; they are still devoting much more effort to problems of simplicity and of disorganized complexity (that is to say to advances in mathematics and statistics), rather than to problems of organized complexity, which seems much more difficult to formulate in quantitative terms. And « no », because the concept of organized complexity, which was defined by W. Weaver as problems « with a sizeable number of factors which are interrelated into an organic whole » does not characterize all the problems of « the middle region, between simplicity and disorganized complexity ». In this « middle region », problems often appear to be more complex than those of a sizeable number of factors interrelated into an organic whole: this is particularly the case of problems we deal with when we consider socio-economic systems in general, and industrial systems and their management in particular. No evidence supports the definition of such

organic wholes and the list the factors interrelated in such wholes. When we are fortunate enough to establish such a clear definition (in order to design and to simulate a finite model), no evidence supports the assumption of the stability of such a definition and of the corresponding finite model.

H. A. Simon, (today the unique Nobel-Prize winner for his scientific contribution to operations research and management science) underlined, in « The new science of management decision », first published in 1960, the evidence that people in industrial as well as in administrative organizations, can identify some of their current problems in terms of « well-structured problems » or « programmed decision » (p. 45):

« They are repetitive and routine, to the extent that a definite procedure (or model) has been worked out for handling them... » (p. 46). Those well-structured problems are generally formulated with a « sizable number of factors which are interrelated into an organic whole »; the most popular type of structured problem in industrial management is certainly today « the optimization problem », such as linear programming which is often presented as a perfect example of a new victory of science dealing with « the complexity of the third type » according to W. Weaver. But according to H. A. Simon, the decision-making processes involved in a social organization – industrial or not – never deals exclusively with structured problems. The « whole complex of intelligence and design activities that preceded... the final act of approval of the step » is to be considered. Not the « whole organism » (which one?), but the « complex whole of intelligence and design activities », that H. A. Simon calls the « non-programmed decision » or the « ill-structured problem ». We can identify some « organized » structures which will describe such an ill-structured problem (« The structure of ill-structured problems », Simon 1973-1977, p. 304), but we shall discover that these structures lie in the cognitive organization of the design processes of the problem, and not in the social organization facing it. And even if we can develop our « intelligence » of these cognitive design processes, we know that this does not lead to a unique and certain « optimizing » solution, but may be to some feasible « satisficing » solution. The definition of the problem has been displaced from the « observed system », seen as a natural system of an « organized complexity » (or modelled in a stable complicated structure of interrelated factors), to the « observing system » (Von Foerster, 1981), seen as a cognitive system of an « organizing complexity », or designed as a functional and evolving intelligible model, adapted to the ends of the modeler.

The experiences accumulated by model builders of industrial systems (and also, perhaps mainly, in others fields unanticipated by W. Weaver, such as human communication therapies, ecological system, planning and design, research on immunological systems, architectural design,...) lead us to a « fourth advance » in dealing with complexity. Subsequent to an era of the « paradigm of simplicity », of disorganized complexity, of organized complexity, we have may be faced with a paradigm of « organizing complexity » as we shall define below.

The main lesson to be drawn from these modeling experiences is perhaps a methodological one: the interaction between the systems observed and their model builders is not neutral. Cognitive processes involved in modelling an observed phenomenon is becoming an important part of the model: the resulting model is more a model of this observing model than a model of the observed phenomena: we cannot easily separate the « observed system » and the « observing system », and all we know about the observed system is what we know as an observing system. The key feature of this understanding of the « complexity of complex systems » is that complexity is, perhaps, more associated to the design process of models rather than to the system itself. Thus, we cannot ignore the complexity of our own designing system (the cognitive observing system) if we purposefully intent to model « another » system. Usually, we perceive ourself as acting in this « other » system. Thus, considering that we are modeling at a purely cognitive meta-level (Van Gigch, 1987) we attempt to avoid the difficulty of our position. But as we observe that those interactions modify both the observed system and ourself acting as observer within this observed system, we are never sure that our cognitive meta-observing system is not also transformed by this process. The biologist Th. Dobzhansky (1962, p. 391) expressed the intelligibility of our knowing process in distinctive terms:

« When changing what he knows about the world,  
 Man changes the world that he knows,  
 And changing the world in which he is living,  
 Man changes himself » <sup>(2)</sup>.

This discussion leads us to a new conception of complexity interweaving the two « views » which express any model of any modelled phenomenon: « All what is said, is said by an observer to another observer who may

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<sup>(2)</sup> My translation in english of the french translation of the original english text. I thank the readers who can send me the original Dobzhansky.

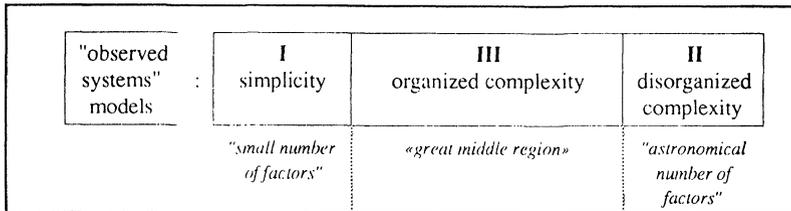
be himself », said H. Maturana (quoted by H. Von Foerster, in Segal 1986, 1990, p. 83); the resulting complexity of the observed system is known only through its model designed by an observing system which might see itself as acting in some complex manner.

This formal distinction between the observed and the observing system suggests a common definition of their complexity: P. Valéry defined (in his *Cahiers*) complexity as « the essential unpredictability » (« imprévisibilité essentielle »), and he added later that this essential unpredictability is nevertheless « intelligible ». If we apply this definition to the « observed system », (assuming that it exists before it is observed), we can consider that a complex or unpredictable observed system is *a priori* « uncountable »: one cannot count with certainty its different parts or behaviors, hence one cannot describe its whole behavior with certainty. Symetrically, a non-complex observed system (either simple or complicated) is considered as having a finite number of parts or of behaviors, so that the number of feasible combinations can be exactly determined: the occurrence of each can be fully described. If we now apply this distinction to the « observing system », we can see it as a « closed model » or as an « open model »; or as able to design closed or open models of the phenomenon they describe... Since the aim of the observing system is to design « intelligible » models, we can consider that closed models aim to be completely and effectively predictive models, (usually expressed in computable algorithmic terms), and that open models aim to be understandable, helping the system to control or to manage the phenomena modelled (usually expressed in programmable heuristics terms). In H. A. Simon's well known distinction (1955) observing systems of the closed type have an « optimizing behavior », which are not perceived as really complex (although eventually very complicated); and observing systems of the open type, have a « satisficing behavior » (which is not completely foreseeable since the system can design several feasible solutions).

These two views of complexity – or non-complexity – of any model seen as the model of an hypothetical observed system and of an observing system, can now be associated in order to enlarge the definition of the complexity of a phenomenon which is modelled and proposed fifty years ago by W. Weaver (who, in 1948, was not familiar with the distinction between the model and the modeling process, or between the observed and the observing system, a distinction initiated by quantic physics and developed later on in the cybernetics and in the systems sciences).

Weaver's definition of « organized complexity » of an observed system assumed that it had to be seen (fig. 1) as the « great middle region » (III) on an axis with two extremes:

- on the left, the small observed system, containing a small number of factors, easy to calculate (the area of « simplicity », I).
- and on the right, the large observed systems, containing an astronomical number of factors (the areas of « disorganized complexity », II).



**Figure 1. – The W. Weaver's typology of Systems Models.**

If we combine these two definitions of model complexity, seen both as the model of an observed system, and as the model of its observing system, we can enlarge our definition of complexity taking in account both « organized complexity » as viewed by W. Weaver and « organizing complexity » as we perceive it today trying (including lessons of multiple experiences in modeling complex systems) (fig. 2).

OBSERVING SYSTEMS MODELS	OPEN MODELING	<b>III</b> NON-LINEAR COMPLICATION or ORGANIZED COMPLEXITY	<b>IV</b> ORGANIZING COMPLEXITY
	CLOSED MODELING	<b>I</b> LINEAR COMPLICATION or SIMPLICITY	<b>II</b> DISORGANIZED COMPLEXITY
		COUNTABLE FACTORS MODELS	UNCOUNTABLE FACTORS MODELS
OBSERVED SYSTEMS MODELS			

**Figure 2. – The generalized W. Weaver's typology of systems modeling.**

This enlarged presentation of « the paradigm of complexity modeling » will provide a new intelligence of the cognitive modeling process:

(a) If the model builder considers the phenomenon as « deterministic », (*i.e.* to be explained by the « law » which governs its behavior), he shall search for some finite or closed model which describes this law: the well-known example of the three Kepler's laws, completely describing behaviors of the planets around the sun, through a mechanical or quasi linear model (without any important internal loops between the interrelated factors), highlights this classical case. H. A. Simon showed (in *How complex are complex systems*, 1976), the strength of the model of the « hierarchical organization » for the design of such linear systems, assuming that the modeled phenomenon can effectively be described through some long « chain of simple reasons » (R. Descartes), without any noticeable loops inside the chain. The analytical cartesian methodology is today universally known as the best tool for designing such closed models of closed phenomena: the « four precepts » of the *Discours de la Méthode* assess it in definitive terms. W. Weaver was right when he called it « method of simplicity », since the explicit aim of the analytical cartesian method is to divide into simple factors the complicated linear or quasi-linear chain which describes the whole (or closed) phenomenon. Operations Research in its first stages mainly used this methodology for modeling the known industrial systems to be managed in a classical manner (classical today, for us). In its second stage, when it was called « systems analysis », it did not change its « closed modeling » methodology, but it moved towards larger and larger phenomena to be modeled: the number of factors remained countable and computable, but it became so large that model building needed powerful computers to model them or, more correctly, to program them: the well known case of *Industrial Dynamics* (Forrester, 1962) highlights one of the cases of Systems Analysis: as we can now anticipate, this conception of (complicated) « systems analysis » has nothing to do with the contemporary conception of (complex) « systems modeling », except the word system. In case of analysis, system refers to a closed and to a stable set of interrelated factors. In case of modeling (or as H. A. Simon said, 1969, « design »), system means an open evolutionary and teleological conjunction of actions, an « organization » (as E. Morin suggested, 1977).

(b) If the model builder considers the phenomenon as determined only at a macroscopic level by some probabilistic law, he will search for some statistical laws, which may explain its presumed average behavior in some intelligible manner: the development of statistical mechanics, of

thermodynamics and of the so-called mathematical theory of communication, illustrates the now classical modeling tools developed for describing the behavior of such large systems. Operations Research emphasizes its own progress in areas such as industrial quality control, pointing out to its ability to use statistical techniques. Similarly, we expect today to master the new tools of « chaos theory » in order to apply them to some problems of industrial organization; although we are not sure that there are many areas of disorganized complexity in the manageable manufacturing units we can consider!

(c) If the model builder is aware of the fact that his own cognitive model for designing processes affects the modeling of observed phenomena, that is to say if he cannot separate the model and the modeling process, or the product and the production process. If he can only describe this complex modeling system, he departs the simpler area of closed modeling methods and enters the area of the open and purposeful methods, « the whole complex of intelligence and design activities » as H. A. Simon claims.

(d) At this stage we can consider two different attitudes: either the model builder supposes that he is designing an open model of a phenomenon which is assumed to be effectively closed (countable): we shall see that it was probably this attitude that W. Weaver was considering when he defined the « models of organized complexity », or he assumes that he is designing an open model of a phenomenon which is possibly open, that is to say that it can exhibit some unforeseeable behavior emerging at any moment. (The case of our usual perception of living phenomena, especially human living phenomena such as love or passion). I suggest to call the models designed according to this second attitude, models of « organizing complexity », borrowing this wording to H. Von Foerster inventing the « observing system » concept, to E. Morin developing the « paradigm of Eco-Auto-Re-Organization », and, initially, to P. Valéry who wrote in his *Cahiers*, in 1920: « The organization, the organized thing, the product of this organization, and the organizing are « unseparable » » <sup>(3)</sup>.

If the organization of the model, of the modeled phenomenon and of the modeling system can be perceived as both « organized and organizing... complexity », we can assume that the experience we have been gaining since

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<sup>(3)</sup> « L'organisation, la chose organisée, le produit de cette organisation et l'organisant sont inséparables », *Cahier 1*, p. 562.

1948 <sup>(4)</sup> in modeling organized complexity will enrich our ability to model organizing complexity and that the experience we have been gaining more recently, since the seventies <sup>(5)</sup> in our collective learning-process helping us to deal with models of organizing complexity, will in return progressively enrich our ability to model organized complexity. The second part of this article will briefly sketch some lessons to be drawn from this collective experience in complexity modeling.

## 2. « MODELING IS A PRINCIPAL TOOL FOR STUDYING THE BEHAVIOR OF COMPLEX SYSTEMS »

Some of those experiences in complex-systems-modeling were discussed some years ago by H. A. Simon in a paper first presented at the International Institute for Applied Systems Analysis (Laxenburg, Austria) at its 15th Anniversary Conference: « Forty years of experience in modeling systems on computers, which every year have grown larger and faster, have taught us that brute force doesn't carry us along a royal road to understanding such systems. Nature is capable of building, on the scale of microcosms or macrocosms or any scale between, systems whose complexity lies far beyond the reach of our super computers, present or prospective. Even in environments as artificial and constricted as the game of chess we are faced with numbers on the order of 10 raised at the 120th power. The combinatorics of such numbers are almost beyond our imagining, and certainly beyond our capabilities for computation. Modeling, then, calls for some basic principles to manage this complexity... » (Simon, 1990, p. 7).

### Two basic principles of the modeling of complex systems

What can we say about those principles? H. A. Simon suggested that we distinguish between the two types of interest that we usually have in model building: modeling for prediction and modeling for prescription.

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<sup>(4)</sup> In 1948, the following articles were published: W. Weaver, « Science and Complexity », « Cybernetics or Control and Communication in the Animal and in the Machine », of Norbert Wiener, the founder of Cybernetics, « The Mathematical Theory of Communication », of C. Shannon introduced by W. Weaver, *Etude thermodynamique des phénomènes irréversibles* by I. Prigogine and *Administrative Behavior, a Study of the Decision-Making Process in Administrative Organization*, by H. A. Simon. The last two were published in 1947, but the three other texts were also written and partly published before 1948!).

<sup>(5)</sup> The key texts to which we refer today when we discuss our experience in « organizing complexity » modeling were known if not published after 1970: Bateson (1972), Von Foerster (1973-76), Morin (1973, 1977), Atlan (1972), Varela (1975-1979),...

He discussed the weaknesses and the failures of modeling techniques for predicting the time paths of large systems, from demography to economics; and considered the specific case of predictive models in social system: « Such systems have numerous feedback loops, but that, in itself, is not the problem. We constantly model with success engineering systems that are full of passive feedback loops. But the feedback loops in social systems are not passive but predictive. Each of the participants may be trying to forecast the behavior of others actors and of the system in order to adapt his or her behavior advantageously ». Considering the answers proposed to deal with this problem, from von Neumann and Morgenstern game theory to the Muth-Lucas theory of rational expectation, H. A. Simon conclude that « these impressive theories have provided us... a demonstration of the deep intractability of the problem » (p. 10).

His discussion of the experience we have gained in prescriptive modeling is comforting: « Our practical concern in planning for the future is what we must do « now » to bring that future about. We use our future goals to detect what may be irreversible present actions that we must avoid, and to disclose gaps in our knowledge that must be closed soon so that choices may be made later. Our decision today requires us to know our goals, but not the exact path along which we will reach them »... that is to say, in methodological terms, that « intelligent approximation, not brute force computation, is still the key to effective modeling ».

### **The principle of intelligent action**

We need intelligence, not computation, to identify our modeling goals, and we need more intelligence to anticipate the recursive effect of the means we are choosing to reach a given goal, on the transformation of this goal; new goals which in turn will perhaps suggest the choice of new means... In modeling our actions in complex systems, we know only that our present goals are not final goals, but intermediate goals. So how can we rationally determine the means, or the behavior, which will meet our goals, if we do not know what those final goals are? The recursive nature of the relationship between means and ends transforms our familiar conception of rationality: the deductive or substantive rationality we were accustomed to exercise for reasoning our model-building is no longer adapted to the cognitive process of the modeler dealing with a complex system. H. A. Simon popularized the concepts of « procedural rationality », or of « bounded rationality » to describe those « natural » forms of reasoning that human beings easily use when they are designing their next « intelligent action »

(Natural logic, as opposed to formal logic: *see* Lakoff, 1972/76, Grize 1983/1989). Perhaps we should insist here on a frequent misunderstanding of this concept of « bounded rationality »: it is often translated into french by the wording « rationalité limitée » (« limited rationality »), and correspondingly interpreted in terms of « weak rationality » (the « strong rationality » being in this case the « substantive, or perfect, or objective rationality »!). « It was a strategic error (in 1956) to defer to orthodoxy by renaming the topic as « bounded » rationality, thus implying the existence of an ideal, unbounded form... (It) construes a contradiction in terms « the economist R. Marris observes (in Simon *et al.*, 1992, p. 198), who prefers to it « intelligent rationality ». It seems to me that the term « bounded rationality » intends to name « internalized rationality », the mode of reasoning which is entirely designed within the cognitive modeling systems. H. A. Simon, in a recent paper (1992, p. 26), characterized it as « the ability to calculate correct actions inside the skin ». Bounded rationality is not « limited », it is entirely generated in the cognitive system, which has not to acquire elsewhere (in a book of formal logic for instance) a specific and limited procedure to reason correctly. Rationality is « bounded » when it is its own controller, able to deal with the recursive processes between the design and the choice of means and of ends, between its controlled and its controlling system.

The renewal of our conception of rationality involved in the modeling of complex processes has contributed to re-open the door to well known... and often forgotten since a century... methods of reasoning; such as rhetorical and dialectical reasoning, from abduction to retroduction and transduction, able to develop « plausible reasoning » (G. Polya, 1952) through many *heuristic search* processes (Newell and Simon, 1976). Old methods which make it possible to deal with those « strange loops » that we often find in the cognitive process involved in complex system modeling, describing the recursive relationship between the observed system and the observing system, between the autopoietic process and its active environment, between the invention of new means and the evocation of plausible ends, between action and reflexion, between the map and the territory (« If the map is not the territory », Korzybsky, 1931, we know many situations where the territory is becoming like its map). H. A. Simon and A. Newell have not discussed often the lessons learnt from this understanding of our cognitive ability to model and to reason such recursive relationships. But H. Von Foerster (1960, 1975, 1976, dans 1981), F. Varela (1975 in 1979, 1991), and mainly E. Morin (in the four volumes of *La Méthode* 1977, 1980, 1986, 1990), have often shown the feasibility of such reasoning processes, more

often qualitative rather than quantitative (Bartoli and Le Moigne, 1994), more « symbolic » than « numerical », more « joining than separating »: « It was long ago noticed and established that man, in his activity, practice and cognition, only joins and separates... But further investigation reveals that the two acts, joining and separation, do not play an equal part in the activity of man or occupy in it an equal place: the act of joining is primary, the other derivative; the one can be direct, the other is always only a result » (Bogdanov <sup>(6)</sup>, 1921-1980, p. 63-64). Summarizing briefly those various comments about the reasoning processes involved in the modeling of complex systems, I suggest to sum them up under the heading indirectly suggested by A. Newell and H. A. Simon in their « Turing Lecture » (1976), of « the Principle of Intelligent Action » (which is to be seen as contrasting with the famous « Principle of Sufficient Reason » assessed by Leibniz, to which all model builders of complicated linear systems refer to justify their analytical reasoning methods). The principle of intelligent action (Le Moigne, 1992) says that a reasoning process is able to design an adaptative answer to a given situation: its « intelligence » is in its ability to adapt itself, its next behavior, to its immediate ends, by heuristic searches. We understand that the experiences gained by some research on artificial intelligence and heuristic programming, artificial life, artificial engineering and other simulations of complex behaviors, are progressively enriching our ability to reason in complex modeling. I assume that operations researchers will recognize today that H. A. Simon and A. Newell were right when they claimed in 1958 (and later in 1987) that « Artificial Intelligence (will be) the next advance in operations research ». But I have the feeling that operations research has not yet explored the whole area of complex systems modeling opened by the development of artificial intelligence and related sciences of cognition: is it not still « limited » to some familiar cases of « organized complexity »? and to ignores to a large extent cases of qualitative, « organizing complexity ».

### **The principle of systemic modeling**

« The meditation of an object by a subject has always the form of a project » <sup>(7)</sup>: this beautiful definition of the modeling process proposed by

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<sup>(6)</sup> The Russian A. Bogdanov (1873-1928) is probably the first founder of our contemporary « sciences of complexity ». His *Tektology, universal science of organization* (1913-1920) remained practically unknown during fifty years. The English translation of his *Essays in Tektology*, by G. Gorelik, in 1980, gives us now a first useful introduction to this great thinker.

<sup>(7)</sup> « La méditation de l'objet par un sujet prend toujours la forme d'un projet » (*Le Nouvel Esprit Scientifique*, 1934, p. 15).

G. Bachelard gives us another key for effective modeling of complex (or open) systems. We must admit that the observing and knowing subject (the model builder), as any human being, has a project, usually complex, aggregating multiple and evolutionary goals, and that this complex project, be it explicit or not, leads his cognitive activities for designing and simulating some symbolic (or artificial) models of the observed phenomenon itself, perceived or not as complex. H. A. Simon (1990, p. 13) concluded that in our modeling exercises, « we will do a better job if, before we begin, we ask what our goals are - what questions we are trying to answer ». The development of systemic modeling since the late 1970s (Le Moigne 1977-1994, 1986, in UNU, and 1990) is entirely built on this basic assumption: to model an identifiable phenomenon, natural and artificial, is not to analyze an object in order to discover its invariant structure; to model is to design a project interweaving the actual goals of the designing system and the presumed goals of the modeled phenomenon. Systemic modeling starts from key questions of the old rhetorical *inventio*: « What is done? What should be done, for what, in which context? Becoming what? », and not from the modern questions of analytical modeling: « From which parts is it done? ». Rather paradoxically, the practitioners of industrial systems' modeling over the last forty years, have experienced the analytical rather than the systemic questioning: they have apparently often forgotten the lessons suggested by N. Wiener's *Cybernetics* (1943, 1948) creating the powerful modeling tool known as the « teleological black box »: do not focus on the organs but on the functions, do not focus on the structure but on the functioning, do not focus on the internal circuitry but on the external behavior. Instead of building a « truly objective model » of this industrial system, try to design some intelligible model » of its observed and expected behaviors, a « projective model » of this manageable industrial system.

And if we assume that this industrial system is really complex, do not try to manage it as such: by definition, complexity – « essential unpredictability » – is un-manageable! We may « deal with » it more than manage it! May we notice here a curious cultural difficulty coming from the translation from english into french: « to deal with » is usually translated by « to manage » (« manager »), but we loose a part of its meaning (in english « to deal with » is not exactly « to manage »). I think that the formulation proposed by D. Génelot in his book (1992): *Manager dans la complexité* (« to manage « within » complexity ») is probably a good trade off. It may help people to remember that there is *a priori* no good project management model for managing really complex projects such as the « Channel Tunnel »;

and that the models we shall design *a posteriori* cannot be used in other occasions: so many unforeseeable events can occur « in » such complex environments. We may model, step by step, the cognitive process of the design of our next modeling step, and gain lessons from this experience; but we cannot expect to refer to a « well-structured » model of this complex industrial system and of its very complex management!

From *La Méthode* of E. Morin (1977-1990), to the *Models of bounded rationality* of H. A. Simon (1982), we have today a rather elaborate formulation of those principles of systemic modeling: we know today that we are not constrained to express our models only through the forms of numerical structures. We rediscover the impressive resources of our symbolic capabilities: natural language and artificial drawing are powerful symbolic tools well adapted to the design of models of complex systems, and they can be memorized and computed as effectively as numerical systems. The example of Leonardo da Vinci's invention of the helicopter may illustrate this point: in his *Cahiers*, we can see for instance the cognitive design process operating between some sentences in natural language (list of symbols) and some drawings (graphical symbols or *disegno*) progressively elaborated to describe steps of the modeling process.

The engineering of the modeling of complex systems (and project) can be seen as the engineering of symbolizing and memorizing: we need to develop as H. A. Simon said (1969-81), some « science of design » in order to master such engineering of intelligence and design. Instead of starting from applications such as CAD/CAM/CIM, MRPI, MRPII and other GRAI or MERISE tools, we can start from the formulation of our modeling goals and then to try to enrich them, in order to express them through new symbol systems, be there graphical, litteral or numerical. The classical answer today, when an industrial system modeler is asked about his goals is to say that he always wants first « to integrate » everything (manufacturing and marketing, monitoring and control, maintenance and production, quality and productivity,...). He confesses that he usually does not succeed because things and people have changed between the time he designed the integrated industrial models, and the time the system was using it. So we can wonder whether the constraint is to deal only with this unique goal of integration? If he assumes that he will never succeed, he might consider some other parallel goals, such as adaptability, flexibility, conviviality in human relationships. He would then have to represent, through some symbolic systems, the other characteristics of the system, in order to express also those other goals:

H. A. Simon (1960) proposed to call this key step of the modeling of complex systems the « intelligent stage » in complex system modeling.

**3. SO, IF YOU DO BELIEVE THAT YOUR INDUSTRIAL SYSTEM IS REALLY COMPLEX...**

If we try to briefly summarize this rather epistemological discussion of the modeling of complex systems - and more specifically here, of complex industrial systems - we can perhaps suggest a new list of keywords used by a model builder working on his first draft: they are presented in the table below, contrasting the usual keywords found in classical operations-research modeling with the new (and in fact very old, from Protagoras to G.B. Vico) keywords suggested by our discussion of the modeling of complex systems, organized and organizing ones.

Some might consider that they did not master this other language well enough when studying the modeling techniques during their years at school? But it is conceivable to effectively use a language developed for the description of simple or linear sets, to model in intelligible terms those industrial systems that are perceived as organized and organizing complex systems? Is our aim to apply standard methods, old or new, or is it to develop some intelligent adaptative behavior?

Instead of:	we may speak of:
<p>the Analysis of the Object is described by the Structure of a closed set of elementary organic parts separated from its environment by a clear and simple boundary.</p> <p>This structure has to be self-explaining the potential optimizing controlled behavior (optimized in reference to a unique and exogeneous goal) of the Operations run by the Object, which in turn strictly determines the list of information to be processed</p> <p>in strictly logical or numerical terms by the control unit which maintains the closed set of parts inside its boundary.</p>	<p>the Design of the Project is expressed by the Organization of an open System, by some active functional processors related to their environment, by some rather ambiguous and multiple interactions.</p> <p>This organizing model may help to understand the actual satisficing intelligent behavior (referred to multiple, evolving and endogeneous goals) of the Operations run by the System. which teleologically suggests</p> <p>some new symbolic computable representations</p> <p>along some heuristic search procedures elaborated by the intelligent decision-making subsystem which invents some new adaptive interactions.</p>

« Intelligence, concluded J. Piaget (1937, p. 311), organizes the world by organizing itself », « by crystallizing experience into an interaction of

an experiencing subject and the object of experience » (E. Von Glasersfeld 1981, 86). A rather stimulating project for the intelligent model builder of complex industrial systems: to understand the recursive interaction between their cognitive modeling activities and the symbolic models they built... in order to understand... their own evolving project.

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