Dynamic, living, social and cultural complex systems: principles of design-oriented analysis

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ABSTRACT:

This chapter proposes to the discussion a set of principles for a design-oriented analysis of technico-organisational systems as dynamic, living, social and cultural systems, through eleven questions. After developing an ontological notion of complexity adequate to technico-organisational systems, these questions deal with the different theoretical, epistemological and methodological (data collecting, analysis and modelling) aspects of the knowledge of this complexity and their relation to design. The discussion appeals to many different disciplinary contributions, in empirical science, ergonomics and philosophy, and relates to ideas developed by other authors in other chapters of this book.

RÉSUMÉ

Ce chapitre propose à la discussion, à travers onze questions, un ensemble organisé de principes pour une analyse orientée vers la conception des systèmes technico-organisationnels en tant que systèmes dynamiques, vivants, sociaux et culturels. Après avoir précisé une notion ontologique de complexité adéquate aux systèmes technico-organisationnels, ces questions abordent les divers aspects théoriques, épistémologiques et méthodologiques (à la fois de recueil de données, d'analyse et de modélisation) de la connaissance de cette complexité, ainsi que leurs relations avec la conception. La discussion

fait appel à des contributions disciplinaires diverses, tant scientifiques qu'ergonomiques ou philosophiques, et est en relation avec des idées développées par les auteurs d'autres chapitres de cet ouvrage .

KEY WORDS: complexity, autonomy, technico-organisational systems, design

MOTS-CLÉS: complexité, autonomie, systèmes technico-organisationnels, conception

1. Introduction

The notion of complexity has engrossed Francophone ergonomic research for some time now, especially its core—research into work analysis (which, as a result of Ombredane & Faverge, 1955, and the avant-garde research in ergonomics of the seventies, differentiates French-language ergonomics from international Human Factors and Ergonomics while bringing it closer to new international trends like research in Computer Supported Cooperative Work). I have happy memories of discussions in the MAST group ("Modèles d'Analyse des Situations de Travail" or Models of Work Situation Analysis, formed in 1985 under the initiative of Maurice de Montmollin, see Amalberti, Montmollin & Theureau, 1991)—which was initially MASTC ("Modèles d'Analyse des Situations de Travail Complexes" or Models of Complex Work Situation Analysis)—over the question of whether or not the final "C' (for "complex") should be retained; it was finally agreed that all work situations should be deemed to be complex.

This question of complexity is also encountered in a range of other research fields. The Santa Fe Institute, for example, which was founded in 1984 by a handful of Nobel Prize winners, specialises in complexity: "Its primary concern is to focus the tools of traditional scientific disciplines and emerging new computer resources on the problems and opportunities that are involved in the multidisciplinary study of complex systems—those fundamental processes that shape almost every aspect of human life". Essentially, it combines mathematical and computer research into 'state-determined dynamical systems' and 'formal neural networks' with scientific research into fields ranging from physics to economics, and including ethology, biology, meteorology and ecology.

I will examine here the conditions under which the notion of complexity can produce results in terms of knowledge and design of technico-organisational systems. With this in mind, I will deploy the matter of complexity in a series of eleven questions to which I will seek to sketch out answers on the strength of my research experience in work analysis and work-situation design, plus my interpretation of other research experience in this field. I will conclude by a presentation of the synthesis of these answers which defines the "course of action" approach of the empirical study and design of technico-organisational systems I develop with my collaborators in university, public research and industry. As I believe it is better to open a debate by expressing a particular point of view rather than trying to circumscribe it, or to close the debate from the outset with a summary presentation, contrary to established practice, I shall

continue to speak personally rather than retreat behind a scientific 'we'.

The eleven questions are as follows.

- Question No. 1: Is complexity an epistemological or an ontological notion? Or: Does complexity characterise our ability to know 'things', or the 'things' themselves?

- Question No. 2: What is the most fitting notion of complexity for knowing and designing technico-organisational systems ?

- Question No. 3: Do the failures of methodological individualism with respect to knowledge and design of technico-organisational systems lead to methodological collectivism being adopted ?

- Question No. 4: Do the failures of methodological individualism with respect to knowledge and design of technico-organisational systems result in a return of the monopoly of behavioural data ?

- Question No. 5: Does the complexity of technico-organisational systems condemn the study of them to eclecticism ?

- Question No. 6: Does the complexity of technico-organisational systems impose a monopoly of field studies as the only place for empirical research, and, if not, what sort of empirical research does it lead to?

- Question No. 7: Does the complexity of technico-organizational systems impose a monopoly of the synthetic method ?

- Question No. 8: Can analysis of the complexity of technico-organisational systems forego dynamic semiotics ?

- Question No. 9: Does the complexity of technico-organisational systems impose a monopoly of the analytical method ?

- Question No. 10: Does the complexity of technico-organisational systems totally disqualify computo-representational models in favour of 'state-determined dynamic systems' models in respect of knowledge and design ?

- Question No. 11: Does the complexity of technico-organisational systems entail some kind of indeterminacy in its models ?

2. Complexity as an ontological notion with epistemological implications

Question No. 1: Is complexity an epistemological or an ontological notion? Or: Does complexity characterise our ability to know 'things', or the 'things' themselves ?

I propose to consider the notion of complexity as an ontological notion with epistemological consequences and not just as an epistemological notion. To put this another way, to say that a 'thing' is complex is first and foremost to characterise—or, if one wants to be more cautious, to place a bet on—the nature of that 'thing', not to characterise the relationship of the 'thing' with our ability to know, even if the nature of the thing obviously governs this ability to know. Authors who consider the notion of complexity to be an epistemological notion—who say that complexity exists only in relation to our ability to know, or, for instance, who say that a complex system is one that requires several approaches, or an interdisciplinary approach, for us to know it—do not help us make positive progress. They merely incessantly point out the limits of our ability to know here and now. In so doing, they can open the road to an ontological notion of complexity, but this is not always the case. According to Socrates well known formula, knowing that you don't know can be a good thing, but only if you are not satisfied with that state of affairs.

While subscribing to this epistemological standpoint, some authors arrive at a more positive point of view: encouraging transfers of tools and methods between domains. However, these transfers can only be achieved by taking precautions that they have great difficulty explaining, by sticking to this point of view. Keeley & Bonabeau (1992) say, for instance: "we have tried to clarify the notion of complex system, not from the scientific but from the epistemological point of view, so as to suggest a real underlying unity of science(s) of complexity. Once again, this unity does not lie in the very resemblance of all disciplines that share the field, but rather on phenomenological relationships that allow the application of methods and tools used in a particular field to another one. This is the idea of transdisciplinary concepts, which do not imply the transdisciplinarity of meanings: one must always be cautious with the phenomenological resemblances" (p. 620). Adopting the terms of these authors, I would say that to have any meaning, a 'transdisciplinary concept' must signal a 'common property' shared by the objects of the disciplines concerned. This is precisely what makes complexity an ontological notion. In the next section, I will examine the various meanings attributed to this ontological notion of complexity in order to single out the one that might best fit technico-organisational systems. In the following sections, I will show that this notion of complexity as an

ontological notion with epistemological consequences and not just as an epistemological notion has interesting consequences and is not a matter of pure academic subtlety.

3. Dynamic, living, social, & cultural complexity

Question No. 2: What is the most fitting notion of complexity for knowing and designing technico-organisational systems ?

Let us start with the first formulation of the notion of complexity in the cybernetics framework. In the chapter entitled "The Architecture of Complexity" of "The Sciences of the Artificial" (1969), Herbert Simon writes—with a pragmatic common sense that we must not allow to make us forget he won a Nobel Prize for Economics: "Roughly speaking, by complex system I mean a system made up of a large number of elements which interact in complex fashion. In such systems, the whole is greater than the sum of the individual parts, not in a metaphysical sense, but in the strong, pragmatic sense. Given the properties of the parts and the laws for their interactions, inferring the properties of the whole is no trifling matter. Faced with complexity, someone who is a reductionist out of principle can at the same time be a pragmatic holist" (Fr. transl., p. 106).

If Herbert Simon does not address 'metaphysics', here at least he addresses ontology (considerations on the nature of 'things'), whence he takes a new epistemological standpoint (considerations on how 'things' are known: 'reductionism out of principle' and 'pragmatic holism' at the same time, as he says himself). Along this approach from ontology to epistemology, he adds later, that "most complex systems have an arborescent structure (i.e. they break down into sub-systems that break down into sub-systems in turn, and so on) which is quasi-dissociable" (i.e. where interactions between sub-systems are slight but real), and even that "if the world contains any complex systems that are not arborescent, they must to a large extent escape our observation and our understanding" (Fr. transl., p. 129). For a complex system, having a quasi-dissociable arborescent structure implies that: 1) the short-term behaviour of each of the constituent sub-systems is roughly independent of the short-term behaviour of the others; and 2) the long-term behaviour of each of the constituent subsystems is affected by the behaviour of the others only as an aggregate effect. Making complex systems mere quasi-dissociable arborescent systems amounts to bringing the complexity down to the level of the mere complicated. Experience has

revealed the limits of this operation when dealing with complex systems involving human actors, or, more generally, living systems.

Due to this experience, this notion of complexity was recently reformulated on the basis of research into self-organisation. For the Santa Fe Institute (see above), "the word 'complexity' refers to systems with many different parts which, by a rather mysterious process of self-organisation (sic), become more ordered and more informed than systems which operate in appropriate thermodynamic equilibrium with their surroundings" (Cowan et al., 1994, p. 1). To the notion of complexity as seen in cybernetics and by Herbert Simon, therefore, is added a transforming internal dynamic, a constantly renewed story. This notion of complexity brings us closer to technico-organisational systems, but is it enough to address them? It seems to me that we must first add the presence of human actors who constitute autonomous systems, i.e. who form part of the system while at all times having their own special 'view' of the system as a whole (including themselves) and its dynamics.

This, then, puts us in line with the thinking of Humberto Maturana and Francisco Varela, with the **constructivist paradigm,** which is also called 'enaction' or 'autopoiesis of living systems' to mark its distance from other constructivist viewpoints (Maturana, 1978; Maturana & Varela, 1980, 1987; Varela, 1980, 1989; Varela, Thomson & Rosch, 1991). This paradigm opens onto, on one hand, a line of research that ranges from neurosciences to ethology and the various human sciences, and, on the other hand—and this concerns us more directly—onto a whole range of theoretical objects and approaches for knowing technico-organisational systems. It will be said then that a system is complex if, in addition to the characteristics already referred to (consisting of a large number of elements which interact in complex fashion, giving rise to self-organisation processes), it also includes autonomous systems.

Let us now take a look at a simplified definition of the notion of **autonomy** (or **operational closure**): a system is autonomous if its internal organisation is characterised by processes which: (a) are recursively dependent on each other for their generation and execution; and (b) make the system a recognisable unit in the vaster system of which it is part. This autonomy does not imply any lack of openness: an autonomous system constantly interacts with the larger system of which it is part, but this interaction is asymmetrical, i.e. it has to be relevant to its internal organisation, or, in other words, relevant from the point of view of its internal organisation. Whence we have four complementary ontological notions: **'autonomous unit', 'internal organisation' (**of the autonomous unit), **'situation'** (the

larger system of which the autonomous unit is part, i.e. not just its environment, but it and its environment), and **'structural coupling',** i.e. the relative invariants (those which last for a given period in the life of the autonomous unit concerned) of the asymmetrical interactions between the autonomous unit and the situation.

Whence we can see a first epistemological consequence: failing sufficient inroads into the neuro-psychological processes of the human actors involved, an observer can hope to know only the structural coupling between the autonomous systems they constitute and their situations. To take a classic example in ergonomics, when an operator realises a drawing of the plant where he works or the process he controls in a certain way, he gives us an insight into the structural coupling he has with us and, indirectly, with his situation. We have insight into this, and nothing else; certainly not into any 'operational image' he might have locked away somewhere in his brain, as is traditionally said.

Whence also a second epistemological consequence: even if we limit our ambitions to knowledge of the structural coupling between autonomous units consisting of human actors and their situations, we need to see the point of view of the internal organisation of the autonomous units. This requirement takes the form of an epistemological notion: **'acceptable symbolic description of the history of structural coupling',** or, in other words, the description of that history in abstract terms, given from the point of view of the internal organisation of the autonomous unit considered. Unless one has satisfactory prior knowledge of this internal organisation—which is the aim of neurosciences, but there is still a lot of ground to cover between reality and aims—one is faced with the problem of gathering empirical data on the point of view of the internal organisation of the autonomous unit. Therefore, data obtained from a purely external observation by the researcher, i.e. without any interaction with the autonomous unit concerned, or at least with negligible interaction, do not suffice. And from the moment there is interaction, including during observation, the history of the structural coupling one wants to know is modified. With respect to living systems in general—in animal ethology for example—one is restricted to inferring structural coupling from their behaviour alone. In the case of human actors and groups of human actors, and therefore of the technico-organisational systems which include them, one can to some extent go beyond this limit, as will be detailed in Section 4, by means of verbalisation by the actors, but only on the assumption that there is a link between the point of view of the internal organisation of the actors and the point of view expressed by those actors under certain theoretically founded

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conditions for recording their verbalisations. This assumption and these conditions can be summarised in : (1) the existence of what philosophers of the Phenomenological school call a "pre-reflective consciousness" of the actor at any instant of his/her activity, that is an implicit understanding of the dynamics of his/her structural coupling, the concatenation of which constitutes the actor's course of experience, the constructing process of his/her experience at any moment; (2) the possibility for actors in especially designed situations to express the content of this "pre-reflective consciousness".

To this can be added the fact that technico-organisational systems generally include different autonomous units in a given time frame: different individual actors, of course, but also, since the characteristic of autonomy is not reserved for individual actors alone, different embedded or intersecting groups of actors. As these actors live a life outside the technico-organisational systems, these systems—and therefore the different groups of embedded or intersecting actors of which they are constituted in a given time frame—change not only because of their internal dynamics, but also as a result of the life experience of the actors.

In sum, one can thus define a notion of complexity for technico-organisational systems that can be called 'dynamic, living, social and cultural'. This is what 'complexity' will be taken to mean in what follows.

4. Methodological situationism

Question No. 3: Do the failures of methodological individualism with respect to knowledge and design of technico-organisational systems lead to methodological collectivism being adopted ?

The constructivist paradigm was conceived as a global response to the empirical difficulties encountered by the **cognitivist paradigm** (or the paradigm of 'man as an information-processing system') from which both Cognitive Psychology and Artificial Intelligence as we know them today were developed. These empirical difficulties concern: a) relations between 'perception' and 'action', between 'emotion' and 'cognition', between 'communication' and 'action', between 'cognition' and 'context', and between 'body' and 'mind', and b) learning and development. In addition, the constructivist paradigm enables the stumbling blocks the cognitivist paradigm encounters when trying to deal with relationships between 'actors' and 'groups' to be overcome. I shall deal here with only the last point which clearly

shows what we gain in considering the notion of complexity as an ontological notion with epistemological consequences and not just as an epistemological notion (see Section 2). I shall refer readers to other works (the works referred to in Section 3 and Theureau, 1992) for the rest.

The cognitivist paradigm has been characterised as 'methodological individualism', both 'ontological individualism'—a "theory of the individual" (Newell & Simon, 1972, p. 10)—and 'epistemological individualism'—"the analysis of (individual) verbal protocols is a typical technique for verifying the theory, and in fact has become a sort of hallmark of the information processing approach" (Newell & Simon, 1972, p. 12). What the research carried out within the framework of this paradigm aims at—its theoretical purpose—, is "the internal mental environment" of an individual actor as a "physical symbol system", "an instance of a universal machine, the interactions of which with the environment (other actors included) being reduced to read and write operations conducted at either end of extensive processing activity" (see the critique of this idea in Hutchins, 1994, p. 371). When this research addresses groups of actors and the environment, it is either to reduce them to "an extra memory on which the same sorts of operations are applied as are applied to internal memories" (Hutchins, 1994, p. 369), or to restrict work to clinical and pragmatical studies—which nevertheless can be interesting—without any modelling prospects.

Faced with the need for knowledge and design of technico-organisational systems, these limits on methodological individualism have led to the development of what could be called 'methodological collectivism', in terms of 'situated communicative interaction' as an extension of ethnomethodology and conversational analysis (see Lacoste herein, for instance), and in terms of 'socially distributed cognition' or 'dynamics of a socio-cultural system', as an extension of cultural anthropology (Hutchins, 1994). The price to be paid for this 'methodological collectivism' is non-consideration of the autonomous units made up of individual actors, and therefore the exclusion of both individuals and relations between individuals and groups. As recent research into the control of rail traffic (see Filippi, 1994, Theureau & Filippi, 1994, 2000c) and nuclear reactor (see Theureau et al., 2001) has shown, collectivisation in technico-organisational systems is always relative: each actor interacts with his particular situation, and does so asymmetrically, i.e. on the basis of his particular commitment to the situation, and knowledge of these asymmetrical interactions is necessary, if not sufficient, to know the dynamics of the entire system. Consequently, to be effective, ergonomic design and development

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must concern both the situation of the group and the situation of each individual actor. What is then proposed is to develop—in accordance with the constructivist paradigm—what could be called a 'methodological situationism', or the joint study, for a given period, of the individuals in situation (through individual-social theoretical objects, like 'course of action') and the different groups of actors, embedded and intersecting in situation (through social-individual theoretical objects like 'collective organisation of courses of action'). This study would lead to joint design of individual situations and the collective situation.

In the first research referred to, this 'methodological situationism' was developed as a study of the 'courses of action' of controllers and signalmen and of the 'collective organisation of courses of action' by the controllers on one hand, and by one controller and the signalmen he was in charge of, on the other. This study resulted in a proposal to design 'support situations for each sort of actor' (signalmen, controllers) and a 'support situation for co-ordination of traffic control as a whole'. But 'methodological situationism' can be developed otherwise. Pierre Vermersch and his collaborators, for instance, address the activity of the individual in situation in terms of 'private thinking' (see Vermersch, 1994). Both 'course of action' and 'private thinking' are defined in terms of 'pre-reflective activity', but the ideas of 'pre-reflectivity' they involve are partly different.

5. Observation and verbalisation methods

Question No. 4: Do the failures of methodological individualism with respect to knowledge and design of technico-organisational systems result in a return of the monopoly of behavioural data ?

In the previous section, I looked at methodological individualism, methodological collectivism, and methodological situationism only from the ontological point of view, or in other words, from the point of view of the objects they allow to study. We now have to examine the matter from the epistemological point of view. In relation with methodological individualism, Newell & Simon (1972) introduced verbalisations as data and not just as aids for interpretation of behavioural data. Their consisted of simultaneous verbalisations conceived as 'thinking aloud' by their students during problem solving in a laboratory. In response to the debate that this kind of data engendered, Ericsson & Simon (1980 and 1984) introduced a fundamental idea: the minimal theory of the observatory They say:" we must extend our analyses of the tasks that our subjects are

performing to incorporate the processes they are using to produce their verbal responses. The expansion of theories to include a theory of the measuring instruments is commonplace in physics. Experiments that involve weighing objects require at least a rudimentary theory of the pan balance. In the same way, experiments that record verbal responses of any kind need at least a rudimentary theory of how subjects produce such responses..." (Ericsson & Simon 1980, p. 216). The notion of "observatory of cognition" we proposed in Theureau (1992) draws the consequences of this fundamental idea. It links indissolubly together the data collecting methods and instruments used to study cognition (as conceived in a certain way) and their theory. It is borrowed from Milner (1989) where it was defined in order to discuss the epistemology of linguistics.

The " rudimentary theory of how subjects produce verbal responses" (or minimal theory of the data collecting methods for studying cognition, or theoretical aspect of the observatory of cognition, or, to abbreviate, theory of the observatory) that these authors propose is as follows: "The most general and weakest hypothesis we require is that human cognition is information processing: that a cognitive process can be seen as a sequence of internal states successively transformed by a series of information processes. An important and more specific assumption is that information is stored in several memories having different capacities and accessing characteristics: several sensory stores of short duration, a short-term memory (STM) with limited capacity and/or intermediate duration, and a long term memory (LTM) with large capacity and relatively permanent storage, but with slow fixation and access times compared to other memories... We assume that any verbalisation or verbal report of the cognitive process would have to be based on a subset of the information in these memories" (ibidem, p. 223). It is on the basis of this minimal theory that Ericsson & Simon (1984) consider all kinds of verbalisation methods and their relationship to simultaneous verbalisation, not just simultaneous verbalisation. Obviously this minimal theory of the observatory cannot be validated (or invalidated) by data gathered by means of the same observatory Since it is based on a memory theory, it obviously has to be abandoned when other memory theories take over, as is the case today (see Rosenfield, 1988, for instance).

When both methodological individualism and this memory theory are abandoned, should verbalisations by individual actors also be reduced to informal assistance to interpretation by researchers? Or, on the contrary, could they be developed as data, on condition that an observatory with its minimal theory have been reasonably founded by

other research? The research carried out in connection with methodological collectivism has followed the first path, whereas the research into methodological situationism we referred to in the previous section has followed the second.

Irrespective of the autonomous systems considered, data on the point of view of their internal organisation is needed. If the autonomous system is collective (a group), there has to be at least a cultural sharing by the observer; this takes place by means of verbal exchanges with the actors. But the contribution and the performance of these verbal exchanges can remain informal. This is the case of research into 'situated communicative interaction' and 'socially distributed cognition' which involve an ethnographic survey beforehand. This survey enables the researchers to share—to a certain extent—the culture of the group of actors concerned. In this context, Edwin Hutchins (quoted above) goes so far as to suggest learning the profession involved—to navigate when studying navigation, to learn to fly a light plane when studying commercial flying—and he even practises what he preaches, which takes us back to the distant origins of work analysis, i.e. to Frederick Winslow Taylor who could boast that he had worked at all the workplaces (there were more than a thousand) at Bethlehem Steel & Co. where he developed Scientific Management. Obviously, without the comments of actors, the research carried out in connection with these theoretical objects loses the greater part of the emotions and interpretations of the actors during their work. Above all, it cannot ensure that the description given of an activity is relevant to the actor who performs it.

Let us consider, on the contrary, the observatories of 'course of action', 'collective organisation of courses of action', and 'private thinking' theoretical objects (see section 4) which are in connection with methodological situationism. These observatories make use of comments by actors. Their comments are collected on the basis of hypotheses regarding the pre-reflective nature of the activity and contextual and sensorial recollection. With these comments, the theoretical objects are limited to what, in the actors' activity, is pre-reflective, what can be elicited under favourable conditions: chiefly by means of video recording and, secondarily, by means of the researcher in the case of the observatory of the course of action and of the collective organisation of courses of action; solely, but more expertly, by means of the researcher in the case of the observatory of private thinking (called 'elicitation interview').

Each of these verbalisation methods has its limits. Which implies trading off what can afford to be lost against what has to be won with respect to theoretical objects and observatories. For example, let us

consider the essential verbalisation methods, the first-level self-confrontation interview and the elicitation interview, for studying, the courses of action and their collective organisation, and the private-thinking, respectively. In a first-level self-confrontation interview, a recording of his/her behaviour is shown to the actor, preferably on the very site where his/her behaviour was observed, and as soon afterwards as possible, and equipped in so far as possible with the tools and documents he/she used. He/She is asked not to analyse his/her behaviour but to confine himself/herself to commenting on (verbalising) his actions, communications, perceptions, interpretations, emotions, and changes of focus. The appeal is made to contextual recall. This exercise is acceptable to actors only if they are afterwards allowed to take part in the analysis of this behaviour. One then goes on to second-level self-confrontation interview where the actor is in the position of a collaborator in the analysis and therefore produces not just data but also interpretations which help the researchers in their own interpretations. In the elicitation interview the actor is helped—preferably as soon as possible after the behaviour to be analysed, and without any fresh disturbance—to verbally explain his private thinking on the basis of his psychosensorial memories and his sensorial recollection. For the elicitation interview, above and beyond the differences of the theoretical objects considered, the use of video in first-level self-confrontation firstly facilitates the recall of details concerning action or communications and the accompanying perceptions and interpretations through the replay of his dynamic context by means of the 'video prosthesis' support, and secondly, hinders expression of what was constructed by means of sensorial modalities other than sight and hearing, and also expression of emotions.

Still beyond the differences of the theoretical objects considered, the observatories of 'private thinking', 'courses of action', and 'collective organisation of courses of action' do not hark back to the observatory of 'methodological individualism'. It is not just verbalisations in general that constitute data, but verbalisations gathered in the context of a recreation of a situation (obtained by means of different modalities) in conjunction with observations and recordings of the behaviour in the actual situation. These verbalisations, observations, and recordings are analysed in the same way, in terms of step-by-step construction of the activity, whereas, when Newell & Simon (1972) add observational data—e.g. the eye movements of the actors—to 'thinking-aloud' verbalisations, the verbalisations are analysed in terms of step-by-step construction of problem-solving, while the observations are analysed by statistical methods.

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6. Complementarity of the objects of study and observatories

Question No. 5: Does the complexity of technico-organisational systems condemn study of them to eclecticism?

Considering that part of activity which is pre-reflective, irrespective of how it is done: (1) one specifies the links that public communications and actions have with private emotions, interpretations, and focuses; (2) one does not make do with the present definitions of what is 'tacit' and 'implicit', 'declarative' and 'not declarative', (etc...) to characterise the competencies of actors; (3) one ensures with greater certainty that the description given of an actor's activity and situation is relevant to his internal organisation; (4) one can make recommendations concerning individual situations and the collective situation.

However, the constructivist paradigm does not in principle exclude direct study—i.e. without transiting via study of individual activity—of collective activity. As has been seen in section 2, if there is autonomy of actors, there can also be autonomy of groups, or even of cultures. The notion of structural coupling can concern collective practices equally as well as individual practices. A study of situated collective construction of activity can give rise to theoretical objects and observatories that are more parsimonious than those of course of action or private thinking, that may lose phenomena of the situated individual construction of activity to obtain easier and therefore less costly—or more parsimonious—access to its situated collective construction.

If we were to stop there, interactionist studies and studies of socially distributed cognition would appear as approaches to collective organisation of courses of action, or even to collective organisation of private thinking, which are more parsimonious, and therefore both more rapid and more limited than those which transit via analysis of courses of action or individual private thinking, but which would be sufficient in some cases and for certain aspects of the activities considered. In fact, these interactionist studies and socially distributed cognition studies also consider relatively fine phenomena of linguistic and gestural interactions which go undetected by, yet contribute to, the analysis of courses of action or individual private thoughts. Similar considerations of relative parsimony and relative fineness could be raised with respect to the comparison of studies of courses of action and studies of private thinking.

In the study of technico-organisational systems, these different approaches and their different theoretical objects and observatories

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therefore appear to be legitimate. Everything depends on both what has to be won and not lost in such a situation, for a given knowledge objective and a given design objective. This explains part of the eclecticism and syncretism, the co-existence of heterogeneous or even contradictory approaches that is currently flourishing in this sort of study (see Decortis & Pavard, 1994, for example).

Eclecticism and syncretism are certainly better than blind reductionism in the case of the study of technico-organisational systems. It seems to me though, as far as technico-organisational systems are concerned, that they can and must be surpassed today. To do this, we have to examine the conditions of complementarity between the different approaches. The notion of complementarity was raised by George Devereux, in connection with the creation of ethnopsychiatry, with respect to the psycho-analytical (and more generally psychological) approach and the cultural anthropological (and more generally sociological) approach, in order to specify the vague notion of interdisciplinarity. According to this author, "The question is never: 'When do individuals and individual phenomena stop being relevant and society and social phenomena take on overriding importance.' Nor, of course, the reverse. The real question is: 'When does it become more parsimonious to use the sociological (more collective) approach rather than the psychological (more individual) approach?' " (Devereux, 1985, p. 143). If psycho-analysis and cultural anthropology, whose complementarity has been so effectively demonstrated by ethnopsychiatry, are looked at more closely, the conditions for such complementarity can be identified: common paradigm—in this case the structuralistic paradigm—, common ground for theoretical objects and observatories respectively—in this case cultural practices and myths and the recording of their oral expression. Generalising these conditions of complementarity, it seems to me that eclecticism and syncretism in the study of technico-organisational systems can be avoided by putting the different approaches through the filter of the constructivist paradigm, i.e. by specifying their theoretical objects and observatories on the basis of this paradigm, which in return can only give more detailed insight into the constructivist paradigm. Whence the need for a scientific debate over the approaches for studying courses of action, private thinking, situated communicative interactions, and distributed social cognition, to name the approaches between which this debate already develops.

7. Constructive non experimental empiricism

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Question No. 6: Does the complexity of technico-organisational systems impose a monopoly of the field studies as the only place for empirical research, and, if not, what sort of empirical research does it lead to?

A lot of research into technico-organisational systems replies in the negative to the first part of this question, and uses studies on simulators and more generally in simulated conditions. But is this *de facto* situation or *de jure* situation? Everything depends on the way the simulated situations are constructed and what one expects from studying them.

It is important to remember the place of simulated situations in empirical study and design of office situations, in connection with 'course-of- action-centred design' (see Theureau, Jeffroy et al., 1994). The aim is to find a solution to the 'first paradox of design ergonomics'. This paradox is due to the complexity of situations and can be formulated as follows: to draw up proposals for the design of a future work situation, the courses of action in that future situation must be reliably known; yet they can be reliably known only when the future situation has been fully designed and set up; but then, the ergonomic contribution can concern only the next design process. The solution to this paradox is to iterate course-of-action study in situations that get ever closer to the future situation because they are constructed as the design process moves ahead: reference situations concerned by the design process; launch-pad situations comprising technical arrangements closer to those envisaged for the future situation than those in the reference situations; ecological experimentation in natural situation (reference or launch-pad situations); ecological experimentation and simulations on full or partial mock-ups or prototypes of the future situation; situations built from prototypes on a pilot site; new situations in the set-up phase; new installed situations.

The "full-scale" and "part-task" simulators used in the design ergonomics of control situations in nuclear reactors, aircraft cockpits, car driving, air-traffic control, navigation, surgery and anaesthesia, etc. broaden the scope of situations where it is possible to apply this iterative process for empirical analysis and contributions to design. However, as Theureau (1997a) demonstrated in relation to nuclear power plants and aeronautics, these simulated situations are often built before the simulated situations they are supposed to reproduce are sufficiently analysed, which casts a shadow over the relevance of what is simulated. In addition, "simulating is not the same as doing" (Dubey, 1997), and not everything can be learnt from simulated situations. Moreover, to know what actors import into simulated

situations from natural situations, there has to be an analysis of the natural situations; it is not possible to make do with informal analysis of natural situations and reserve formal analysis for the more easily controllable simulated situations.

Let us now consider the second part of the question: can simulator study be assimilated to experimental study, and if not, what is it? Reflections in Newell & Simon (1972) are still helpful concerning this question. These authors write that the theory of the 'human information-processing system' is "empirical, not experimental", even if it deals with data gathered in the laboratory. The justification given for this reasoning deserves mention: "Because of the strong history-dependence of the phenomena under study, the focus on the individual, and the fact that much goes on within a single problem-solving encounter, experiments of the classical sort are only rarely useful. Instead, it becomes essential to get enough data about each individual subject to identify what information he has and how he is processing it" (p. 12). According to Newell & Simon, the theory of the 'human information-processing system' is "non statistical". They say: "It is difficult to test theories of dynamic, history- dependent systems. The saturation with content—with meaningful symbolic structures—only makes matters worse. There is not even a well-behaved Euclidean space of numerical measurements in which to plot and compare human behaviour with theory. Thus, this book ("Human Problem Solving") makes very little use of the standard statistical apparatus. Theory and data are compared and some attempts are made to measure and tabulate such comparisons. But our data analysis resembles those of the biochemist or archaeologist more than those of the agricultural experimenter" (p. 13). Thus, these authors devise a renewed if not a new method of validation of theories and models which stresses systematic description of verbal protocols gathered in parallel with performance of the activity and puts classical experimentation and statistical treatment in second place. The essential instruments of this new method of validation of theories and models are the problem-solving graph and computer simulation.

A number of the arguments of Newell & Simon (1972)—"the strong history-dependence of the phenomena under study", "the fact that much goes on within a single problem-solving encounter", "the saturation with content"—can be used in relation to the study of technico-organisational systems on simulators or in simulated situations. They lead to what we will call a 'constructive and non-experimental empiricism' to differentiate it from the empiricism used in studies of natural situations proper. At the moment, simulator studies (like those for nuclear-reactor control, for instance) are divided

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between the ideal of laboratory experimentation and the 'constructive and non-experimental empiricism' I propose. For example, both points of view are found in the publications of OECD-Halden, an international program of simulator study of nuclear reactor control. In an assessment of 10 years of evaluation and test studies, Folleso & Volden (1993) consider that a high degree of realism was achieved to the detriment of systematic experimental control, and they "suggest to reduce some of the demands on experimental realism to gain more experimental control", starting with less realistic and more controlled studies "to demonstrate the effects of vital aspects of the system", and then using more realistic situations to test the validity of their hypotheses on a broader scale. On the contrary, Kvalem et al. (1996) envisage, as a long-term prospect for use of the HAMMLAB simulator, putting "less emphasis on strict experimental control" and more on "simulated field studies—with a higher degree of realism" which "will require the development of a different set of methods, e.g. for analysing complexity, understanding how work becomes organised, modelling the interaction between people (communication and control), etc." (pp. 17-18).

Let us look in particular at "part-task" simulators. What is new is less the reality of the "part-task" (it can be considered that traditional human factors studies concern situations of this kind) than the actual notion of "part-task" (as a simplification and reduction of the "full-scale" and not as a complication and degradation of laboratory psychological experimentation) and the fact that current computer resources allow that "part-task" simulation comes close to "full-scale" simulation, or in other words, approximates the conditions required for a pilot, aircrew, or nuclear-reactor control crew to feel 'at home' (with all the limits outlined by Gérard Dubey, 1997) instead of feeling 'out of it', in an experimental-psychology laboratory. Several considerations of cost and integration into the design process lead to studies being developed on "part-task" simulators and to their being made the essential element of empirical knowledge and of the design of technico-organisational systems. A "part-task" simulator is less expensive and is more quickly, designed, transformed, or enriched with new devices than a "full-scale" simulator. It therefore makes for easier comparison (from the point of view of activity) of design alternatives for these new devices.

To close this section, I think that the characteristic kind of complexity involved in technico-organisational systems leads first of all to construction of simulated systems developed through systematic studies in the natural situation. It then leads to development, in these simulated situations, of constructive and non-experimental empirical

studies which rank statistics in only second place and accord more importance to the systematic description of protocols (this will be discussed in the next section), both verbal and observational protocols (contrary to Newell & Simon, see section 4) and computer simulation of the activity (this will be looked at in section 10).

8. Inventive analysis

Question No. 7: Does the complexity of technico-organisational systems impose a monopoly of the synthetic method ?

The previous section spent some time examining the epistemological innovation of Newell & Simon (1972) with respect to the study of "dynamic and history-dependent systems": the role of systematic description of verbal protocols gathered while an activity proceeds. It raises the dual question of the place of analysis in the scientific procedure and of the nature of that analysis.

In the scientific procedure, what generally counts most is the synthetic method. According to Langton (in Nadel & Stein, 1991, for instance)— Langton is a biologist who promotes Artificial Life as a tool for learning about complex dynamic living systems—complexity leads to even greater reinforcement of this primacy: "The key feature of non-linear systems is that their primary behaviours of interest are properties of the interactions between parts, rather than being properties of the parts themselves, and these interaction-based properties necessarily disappear when the parts are studied independently. Thus, analysis is most fruitfully applied to linear systems. Analysis has not proved anywhere as effective when applied to non-linear systems: the non-linear system must be treated as a whole. A different approach to the study of non-linear systems involves the inverse of analysis: synthesis. Rather than start with the behaviour of interest and attempting to analyse it into its constituent parts, we start with constituent parts and put them together in the attempt to synthesise the behaviour of interest" (op. cit., p. 203). Let us disregard the confusion in this text between analysis proper we will stress below and the study of parts separated by the analysis. With respect to the complexity of technico-organisational systems as defined in section 2, the need to have a description from the point of view of the internal organisation of the autonomous systems concerned reinforces—on the contrary—the need for the analysis, and, more precisely, for inventive analysis, as we shall now see.

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As a direct consequence of this primacy of the synthetic method, in scientific research today analysis is generally conceived as applicative, as the breakdown of a whole into speculatively predefined parts. According to Timmermans (1993), the seventeenth century saw the advent of another conception of analysis, through the creation of analytical geometry by Descartes and the analysis of infinites, i.e. differential calculus, by Leibniz. Analysis was then conceived as a movement indissolubly linking questioning, breakdown of any problem into individual parts that are themselves problematical, and regression to principles. This sort of analysis, which can be called "inventive", is said to have disappeared in the 18th century as a result of a return to a form of applicative, Aristotelo-Kantian analysis which Timmermans suggests calling "analytic" or "justification" in the sense of Kant's Transcendental Analytic. For Lakatos (see Lakatos & Musgrave, 1970)— whose methodology of scientific research programmes is a chief source of inspiration to modern epistemology—the science of Newton, which is often presented as the original model of positivistic science, is rooted in this conception of 17th-century analysis and "contradicts the habitual intuition of our Popperian (from the name of Karl Popper) age which overvalues speculation". Timmermans also notes that "privileging analysis or problem solving is not just questioning, regressing from the first for us to the first in itself, but is also privileging the moment one chooses, after reflection, to determine, or to determine oneself, to put an end to one's speculative research in order to practically, concretely test and prolong it (…) Thus, the conception of analysis in classical times reveals the essence of a project which is intended to be both scientific and philosophical: to root knowledge and action, calculation and faith, reflection and decision in the same human power for invention and questioning" (op. cit., p. 5). This remark seems to me to characterise both the unending nature of the analysis of technico-organisational systems and the subjective nature— which does not mean without criteria—of the decision to temporarily stop it and turn to practice, and therefore also its necessary relationship with design, with the art of the engineer. From this point of view, Newell & Simon (1972) implement an analysis that can be called 'inventive-applicative', but with a stress upon the second adjective. It is inventive in the sense that, on one hand, analytical notions have not come from elsewhere, but have been conceived to describe verbal protocols in the problem solving considered, and on the other hand, failure is made possible by the systematic nature of the analysis performed, and can result in analytical notions being called into question. It is applicative in the sense that analytical notions are built up from a presumptive synthesis: man as an information-processing system built up with states of

information and logical operators made of productions (we shall return to this in section 10). In the work of Newell & Simon and their successors, this inventive-applicative analysis has degenerated to a large extent into strictly applicative analysis, if not into a complete absence of analysis of activities. It seems to me that the consideration of the complexity of technico-organisational systems and the failures due to this degeneration of inventive-applicative analysis should lead to a radicalisation of the inventive aspect of analysis.

This radicalisation of the inventive nature of analysis entails periodical repetition of a "temporary suspension" (to use the expression of 20th century philosopher Edmund Husserl, initiator of the phenomenological trend) of all 'constituted'—scientific or common-sense— knowledge and of all practical interests in order to consider as freely and openly as possible the technico-organisational system concerned, together with its situation of observation and analysis. But what exactly does this temporary suspension consist of in the analysis of technico-organisational systems? Firstly, I think we have to, temporarily, systematically doubt the possibility of directly transferring to work analysis the scientific results obtained by considering situations other than the actual work situations concerned, e.g. laboratory situations or interview situations outside the work situation, or by considering separate aspects of the work activity, e.g. problem solving alone or perception alone. In addition, in order to open the field of analysis as much as possible and impose on the analysis as little constraint as possible due to the spontaneous ideology of the analyst, I believe it is necessary to—temporarily, I stress—suspend one's practical interests, even the most meritorious of them (ergonomic interest in particular). Then, I consider that in work analysis it is impossible to limit oneself to the point of view of the observer, if one wishes to explain work activity and therefore contribute transformations to work situations in a scientifically founded manner. Finally, I believe that we must not take common-sense notions like 'intention', 'goal', 'sub-goal', 'task', 'sub-task', 'reasoning', 'planning', 'action', etc. as 'givens'.

However, I am obviously not saying that this constituted scientific knowledge, these practical interests, this point of view of the observer, and these common-sense notions are devoid of interest, but that the interest of a whole segment of them can and must be judged purely out of consideration of the work activity. I therefore start from a point of view very close to that of phenomenological reduction expressed in different ways, subsequent to Husserl, by all the followers of the phenomenological philosophy trend. Husserl always said that constituted knowledge, practical interests, the point of view of the

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observer and common-sense were not called into question by phenomenological reduction, but merely temporarily 'put between parentheses' in order, in particular, to develop them along new paths. This temporary suspension can—and this is important—make use of two symmetrical crutches: practical constraint—'demand'—helps in the suspension of constituted scientific knowledge; scientifico-academic constraint—the need to both discover something new and to thoroughly consult the literature which could be related to the work situation considered—helps suspend practical interests and common sense.

This temporary suspension also causes one to carefully consider the work-analysis situation: the relationship in the work space between one (or more) actors who not only operate but also have their own point of view on their own activity and can express it, and an observer who not only observes but can also ask questions and—to a certain extent to be carefully specified—is capable of empathising. Whence the evidence from the point of view of the scientific observer can be called into question. Think, for example, of the analysis situation of a boxing match, as French philosopher Jean-Paul Sartre suggestively presented it: "In fact, there are two, and only two, ways of following a boxing match: the inexperienced spectator chooses his favourite and takes his standpoint, i.e. he sees him as the subject of the fight, the other boxer being just a dangerous object. This amounts to making the duel a risky but solitary action, to seeing the entire match as the endeavour of just one of the boxers; boxing fans or specialists, on the other hand, are capable of switching successively—and very quickly—from one system to another; they appreciate the punches and blocks but, even if they were able to change systems instantaneously, they do not totalise the two opposing totalisations. They do give the match a real unity; they come out saying 'It was a great match', etc. But this unity is imposed on an event from the outside". (Sartre, 1985, p.14). The whole problem for the work analyst is then seen as, first of all, to possess theoretical notions, principles, and methods making it possible to describe and link together these individual totalisations and this totalisation from the outside, and secondly, to surpass and control the limits of his capacities as an empathising observer thanks to activity-recording tools, actor questioning methods, and theoretically founded methods.

9. Dynamic semiotics wanted

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Question No. 8: Can analysis of the complexity of technico-organisational systems forego dynamic semiotics ?

We have seen in section 2 that analysis of the complexity of technico-organisational systems should result in an "acceptable symbolic description of the history of structural coupling", i.e. in a description in abstract terms of that history, and therefore of the asymmetrical interactions between autonomous units (actors and groups of actors) and situations, given from the point of view of the internal organisation of the autonomous unit concerned. As a result, the problem of the relationship between semiotics and work analysis is put in a new way. The environment provides signs to be selected and interpreted and not information to the actors. Francisco Varela coined the term "in-formation", that is information built from within. Let us look back to the origins of work analysis in French-language ergonomics: Ombredane & Faverge (1955). In the context of the notion of man-machine system and with Shannon's mathematical theory of information as a modelling horizon, the work analysis they proposed sets out to identify and study the signals picked up by man and to study their relationship with the structure of the actions they help determine. After demonstrating the limits of the notion of a signal as an uncertainty reducing event, Cuny (1982) proposed moving beyond these limits by implementing the notion of sign as an element in a system of signs:

"Above and beyond the formal aspect, a message corresponds to an 'informative' content. In the case of work, the message represents a cognitive contribution to the operator .... The cognitive contribution concerned by work analysis necessarily concerns guidance of the action by the operator. It is linked to criteria of execution, to the requirements of the task as they appear to the operator .... This type of contribution is called 'useful information' in the book referred to, and the authors stress that this information "cannot be defined on the basis of the source alone, but depends on the meaning of the work" (L'analyse du travail, p. 116)" (Cuny, p. 58).

"In work, signals can be said to be good (or bad) only in reference to the signs whose actualisation they concretise. In other words, the system of signs acquired by the operator has to be characterised in order to achieve an analysis having accurate instrumental efficiency " (ibidem, p. 61).

"To the analysis method proposed in the past by Ombredane & Faverge can be added the analysis method proposed by semiologists, which deals with the structure and functioning of signs acquired and yet to be acquired" (ibidem, p. 63).

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Let us leave aside the eclecticism and syncretism evidenced by such addition of two analysis methods derived from two theories whose principles are radically different, and whose limits we flagged in section 5, and let us concentrate on the essential: the Saussurian conception of the sign which is expressed in this proposition by Cuny (1982). It should be remembered that although the customary notion of sign evokes a letter, a word, a signpost, a smoke (as a sign of fire, the traditional example in logical text books), etc., the Saussurian (from the name of Ferdinand de Saussure) notion of sign has complicated the picture: it involves a signifier (Sr) / signified (Si) dyad related to a referent (R). Its hypotheses are: the arbitrariness of the sign, i.e. of the relationship between sign and referent; and the need for three categories (real = referent; thought = signified; intermediary between real and thought= signifier). If one has an interest in psychology or information technology, to this can be added the hypothesis of a state of information (I) and of its modification (I') by the operation of signification.

If this proposition of Cuny (1982) has found very few developments in work analysis, it is of course due to the lack of knowledge of semiology that prevails in ergonomics and occupational psychology, but it is also due to the limits of the Saussurian notion of the sign when it comes to addressing the asymmetrical interactions between actors and situations. Essentially, these limits are due to: 1/ the fact that the signifier is cut off from the dynamic situation in which it appears to the actor; 2/ the fact that the signified is reduced to a concept, and is powerless to reflect the emotions, focuses, actions, and communications of the actor, not to mention his learning and development at every instant (for more, see Theureau, 1992, pp. 183-188). Fortunately the Saussurian notion of sign is not the only notion of sign to have been produced by semiology. The Peircean notion of sign which is constantly cropping up in various forms, obliges us to count to three: it is a triad of Object (O) (which is in the category of the Possible) / Representamen (R) (which is in the category of the Actual, of the shock) / Interpretant (I) (in the category of the Virtual, of the law). In addition, it introduces a possibility of rebound: the Interpretant can become the Representamen of a new sign ad infinitum.

Whether dyad, triad, or n-ad, all can be interpreted as summarising an underlying dynamics: dyadic dynamic between process of perception of the signifier and process of construction of the signified; triadic dynamic between process of construction of the Possible ('Object'), process of disturbance, of shock ('Representamen'), and process of interpretation ('Interpretant'). This is how Pinsky &

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Theureau (1987) proposed a first dynamic semiology inspired by Peirce, including four elements, the first three of which (Object, Representamen, Interpretant) adding greater detail to the Peircean triad, while the fourth one, called the 'Interpretative Representamen' actualising here and now the Interpretant. This dynamic semiology was then enriched, firstly by categorisation of Objects, Representamens, and Interpretants (in Theureau, 1992), then by the introduction of notions of 'Commitment to the situation', 'Potential actuality', and 'Frame of reference', and by replacement of the notion of 'Interpretative Representamen' by that of 'Elementary course of action unit' (in Theureau, Jeffroy et al., 1994). Finally, with the notion of hexadic sign (in Theureau, 1997b and 2000a), of which the preceding notion can be considered to be a simplification, assuming certain notions are redefined, the picture has merely been detailed and complicated even further in order to better address learning and development.

This is not the place to look more closely at this particular dynamic semiology, its hypotheses, and the philosophical and empirical background to its construction. What should be stressed is that, for it to be analysed, the complexity of technico-organisational systems requires a dynamic semiology of any kind, obviously on condition that it saves the essential phenomena concerned. Its development along various paths would add to the current three main trends of semiotics (which are essentially: generalising from the sign to the symbol, by logical abstraction of semantics; proceeding transversally between different systems of signs involved in communication; extending the scope of the linguistic sign from the word to the text) a fourth trend: considering the sign as the building block of meaning in the asymmetrical interactions between actors (and, more generally, autonomous units) and situations.

10. Dialectics between inventive analysis and inventive synthesis

Question No. 9: Does the complexity of technico-organisational systems impose a monopoly of the analytical method ?

It might be asked if inventive analysis should not be accompanied by inventive synthesis, as Leibniz suggested in the immediate follow-on to Descartes, and as the etymology of the Greek *analusis* and *sunthesis* suggests: in weaving, these terms mean to undo the warp (let the fabric unravel) and bind the warp together (weave), respectively. To examine this, we have to philosophise some more.

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Let us go back to Spinoza's critique of Descartes in the name of the primacy of synthesis over analysis by using Deleuze (1968) who gives us a suggestive picture. Along this author, Descartes's preference for analysis stems from the fact that, according to him, "we have clear and distinct knowledge of an effect before we have clear and distinct knowledge of its cause" (op. cit., p. 140). On the contrary, for Spinoza: "it is possible for us to start with clear and distinct knowledge of an effect; but this way we will only ever achieve clear knowledge of the cause, i.e. we will know nothing about the cause beyond what we consider of the effect, and we will never obtain adequate knowledge" (ibidem, p. 141). Along Deleuze, then: "Descartes means: the synthetic method claims to always attain knowledge by the cause, but it does not always succeed .... (it) is over-ambitious; but it gives us no means of knowing the real causes. In fact, it starts from confused knowledge of the effect and moves up to abstracts that it wrongly presents as causes; this is why, despite its pretensions, it contents itself with examining causes through effects. The analytical method, on the contrary, has more modest ambitions. But because first of all it develops a clear and distinct perception of the effect, it gives us the means to infer real knowledge of the cause from this perception; this is why it is able to show how the effects themselves depend on their causes. The synthetic method is therefore legitimate only on one condition: when it is not applied alone, when it comes after the analytical method, when it is based on prior knowledge of the real causes. The synthetic method does not allow us to know anything by itself; it is not a method of invention; it finds its utility in the exposition of knowledge, in the exposition of what has already been 'invented' " (ibidem, p. 143). On the contrary, "for Spinoza, the synthetic method is the only real method of invention, the only method of any value in the order of knowledge" (ibidem, p. 145), because it is 'reflective' (it allows us to know our competence to understand beyond the ascertained effect), 'constructive and genetic' (due to the use of mathematics, we can deduce, generate the effects from the assumed causes).

In the modern scientific approach, such a 'reflective, constructive and genetic' synthetic method essentially involves what is conventionally called modelling, an important variant of it being computer simulation. And the critique of the analytical method insists on the difficulties it has generalising, going beyond the particular and the limits of the data analysed. Whence—since I have already justified the development of inventive analysis in section 7—a more specific focus to the question addressed here: must or can the analysis of technico-organisational systems go hand in hand with a modelling approach implementing the available mathematical and computer

tools; and if so, under what conditions (particularly those of theoretical and practical interest)?

Putting the question this way amounts from the outset to considering that analysis and synthesis must be the two essential moments of a dialectic, that harmony between them can result only from a process during which the contradictions between their results appear and are resolved or give rise to provisional compromises. This amounts to proclaiming the end of the 'miracle' accomplished by Newell & Simon (1972). Let us go back and look at it to appreciate the extent of the work to be accomplished for inventive analysis of technico-organisational systems to blend harmoniously with a new synthesis. This 'miracle' accomplished by Newell & Simon is that their analytical notions of state of information, of operator of information processing and production, constitute not only the basis for the description of protocols, of analysis, but also the basis of synthetic modelling. The 'pre-established harmony' (to use Leibnizian terms) between analysis and synthesis has thus been achieved. What the end of this 'miracle' marks for the study of technico-organisational systems is not the death-knell of the synthetic method, but the need to associate inventive analysis and inventive synthesis, or, to put it differently, descriptive generalisation and a priori deduction, phenomenology and mathematics, hermeneutics and modelling, but to do it without the help of a miracle, through research work.

For if one restricts oneself to inventive analysis, to descriptive generalisation, to phenomenology, to hermeneutics, one risks *being* restricted to description, or even to a-theoretical clinical studies. If one restricts oneself to inventive synthesis, to a priori deduction, to mathematics, to modelling, one risks finishing with fallacious localisation of the concrete (to use the expression of Alfred North Whitehead, co-author with Bertrand Russell of "Principia Mathematica", in Whitehead, 1978). Whence the need to develop both sides of these couples in parallel, without fear of contradictions. Obviously, for this dialectic to work, the analysis has to be 'modelling', it has to sufficiently mathematicise its descriptive notions, which in fact presupposes a more or less implicit or explicit, more or less important contribution of mathematics to the analysis. It was in this way that a scientific congress which always used to focus on the "mathematicising of formless doctrines" (Canguilhem et al., 1972) concluded that one can never mathematicise a formless doctrine, but that one can mathematicise a doctrine which has stressed—but not monopolised—descriptive generalisation and, inversely, develop the concretisation of a doctrine which has stressed—but not monopolised— mathematicising.

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11. Synthetic models design between scientific knowledge and design

Question No. 10: Does the complexity of technico-organisational systems totally disqualify computo-representational models in favour of 'state-determined dynamic systems' in respect of knowledge and design ?

We have just seen the need for dialectics between the analytical method and the synthetic method in empirical study of the complexity of technico-organisational systems. Broadening the matter of dialectics to all aspects of empirical study and design, and to conclude, we ask: Is it possible, and under what conditions?

As soon as the notion of complexity of Herbert Simon and of cybernetics is abandoned, it is obvious that computo-representational models have a limited, even negative heuristic value. That is why the researchers of the Santa-Fe Institute, Francisco Varela, and many more, develop synthetic models in terms of 'state-determined dynamic systems'. However, on one hand, the current research works which come closest to the study of technico-organisational systems deal with a complexity of a much lesser degree in terms of both quality and quantity (see: Port & Van Gelder, 1995; Smith & Thelen, 1993; Thelen & Smith, 1995). On the other hand, as Barthélémy & al. (1996) and Stewart (1998) have demonstrated, 'state-determined systems' are by their very construction non-constructive and incapable of fully reflecting the characteristic autonomy of living systems, and therefore have to be surpassed. If, therefore, synthetic models in terms of 'state-determined systems' possess some heuristic value for the empirical study of technico-organisational systems, it is achieved by judicious reductions, and less for their predictive capacity than for their limited heuristic capacity and as 'humility injectors' (to use the expression of David Pines, in Cowan, Pines & Meltzer, 1994, p. 650).

This real but limited heuristic value of synthetic models developed in terms of 'state-determined systems' thus brings them close to computo-representational models as they are conceived by the other authors of this book (Pavard, Benchekroun, Rognin, Salembier and Zouinar, Decortis). Once their limits have been stipulated, thanks in particular to inventive analysis and, more generally, a notion of adequate complexity, these computo-representational models (which could be called 'everything happens as if within those limits' models) can be used to better define certain hypotheses and the relations between them (the 'as if') and to facilitate empirical validation (the answer to the question: Does it really happen 'as if'?). With these restrictions, they therefore do have a real heuristic value.

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If we broaden the matter to design, in addition to their limited heuristic value, such computo-representational models have a practical value which the aforementioned authors stress particularly: because current computer systems are essentially designed in computo-representational terms, computer specialists need man-machine dialogue design models to guide their design work, even if the activity of men in situation and their asymmetrical interactions with their situation is fundamentally foreign to them. This is what, in Theureau, Jeffroy et al. (1994), was called the second paradox of design ergonomics, and which led to a clear distinction being made between analytical theoretical models (and one could add 'and synthetic theoretical models') and practical design models, the former serving to enhance the second and to define the limits of their relevance. It seems to me that such a distinction is absolutely necessary for development of empirical study and design of technico-organisational systems.

12. In what sense indeterminacy characterises the models of technico-organisational systems

Question No. 11: Does the complexity of technico-organisational systems entail some kind of indeterminacy in its models ?

Henri Poncaré, the forerunner of the mathematics of 'state-determined dynamic systems' distinguished three reasons to mention chance and probabilities in Physics (Poincaré, 1991): (1) when "a very small cause, which we can't perceive, determines a dramatic effect, which we can't miss, and then we say that this effect happens by chance", the main case being when these small causes are "small differences in initial conditions that we can only know approximately"; (2) when there is a "complexity of causes" we are not able to fully determine, case "often associated with the smallness of causes"; (3) when we put "deliberate limits on our inquiry about the antecedent situation", a classical example being that of the chain of events resulting in a tile falling from a roof and killing a passer-by. The characteristics of autonomy of technico-organisational systems result in a fourth reason: (4) the limits of the observer's point of view, due to a neglect of this autonomy or to the limits, intrinsic or circumstantial, of the verbalisations collected from the actors. These distinctions are helpful to define the kind of indeterminacy which is present both in analytical models and in synthetic models.

In analytical models: (1) autonomy implies a dramatic role of the initial conditions, i.e. of the history of each individual's activity up to

the instant considered (for example, this kind of indeterminacy is included in the notions of 'Commitment to the situation', 'Potential actuality', and 'Frame of reference' as components of the hexadic sign, alluded in section 9); (2) it implies also a complexity of causes in the dynamics of the actors' state, situation and culture (included in the definition of the theoretical object 'course of action'); (3) the necessary deliberate reduction of the theoretical objects studied, individual-social or social-individual (see section 4) results in a further indeterminacy; (4) even when the limits of the observer's point of view versus the existence of pre-reflective consciousness and the possible appeal to it are considered, the observer's point of view is difficult to exceed. First, the pre-reflective consciousness has its own limits. Second, adequate verbalisation methods are time consuming and need special conditions difficult to fulfil in industry. In synthetic models, the situation is even worse, due, first, to the necessary further reductions operated and, second, to the considerations of operationality, time-saving, and so on, especially when these models are built and validated/falsified in relation with design processes.

In both kinds of models, the indeterminacy involved is relative to our ability and efforts to know and their circumstances, and not absolute. Therefore, it can be reduced through a progress in observatory and in theory that *de jure* precede modelling proper. Let's take two examples from a series of studies of collective control of nuclear power plant in accidental situations simulated in a full scale simulator (see section 7) which illustrate these two kinds of progress and their association. Let's consider first the observatory. In a first study, instead of having only a transcription of observational data of discrepancies between actions and procedures plus a recording of a collective debriefing, we introduced as a complement a transcription of a video recording with one camera. This progress in the observatory allowed a first progress in intelligibility, but essentially limited to the reactor operator's course of action (Theureau et al, 2000, Theureau, 2000b). In a second study, we introduced a transcription of a video recording with two cameras plus brief self-confrontation interviews of the supervisor and reactor operator. From such a transcription resulted the possibility of a new progress in intelligibility associated with a systematic analytical modelling of the supervisor's and reactor operator's courses of action (Theureau et al., 2001). Observatory counts, but also theory. In Theureau (2000b), using a piece of data collected in the first study, we showed that taking the theoretical means to deal with the operators' individual-social activity (through the notions of 'Commitment to the situation', 'Potential actuality', and 'Frame of reference' as components of the hexadic sign) results in an intelligibility of the data concerning the interactions between the
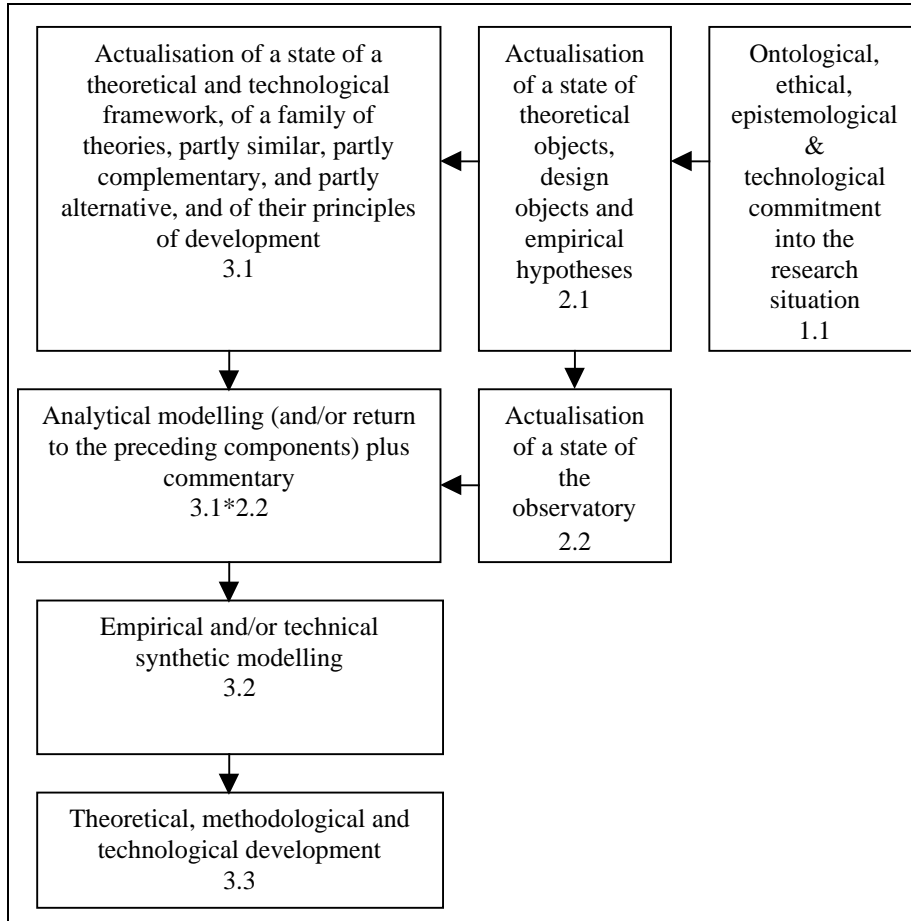
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supervisor and reactor operator that can't be attained by the kind of methodological collectivism coined as "socially distributed cognition" (Hutchins, 1994). In the second study (Theureau et al., 2001), Pierre Vermersch deepened these results by clarifying the dynamics of the allocation of attentional resources of these two operators. Finally, the observatory of this second study appeared insufficient to fully validate/falsify these notions and hypotheses, and so on…

13. Conclusion

Separating the questions that have to be answered, as we did up to now, has the enormous advantage to allow separate reflection and progress concerning one or the other of these questions. But, these questions originated in fact while building a global approach, through theoretical speculation, empirical studies, scientific litterature and debate with other researchers. This global approach, the course of action approach, ought to be presented here, at least roughly, in order to further stimulate the discussion. Along this global approach, in a research study realised at a given moment concerning a given technico-organisational system or family of technico-organisational systems, different movements are involved, which belong to the following structure (Figure 1), that of hexadic sign, which is its main theoretical notion (Theureau, 2000a).

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- Figure 1 -

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In this figure, the rationale and details of which we cannot present here, the arrows point to an order of the definitions of the different movements, i.e. dependency links between these movements. This order means that realising one of these movements implies: (1) having an implicit or, better, explicit idea about the preceding movements; (2) having for a horizon the following movements. Let's consider these different movements one by one in relation with the eleven questions above.

(1.1) Before defining theoretical objects, i.e. relevant reductions, for the study of individual-social and social-individual activity in technico-organisational systems, one has ontological, ethical, epistemological commitments. It is the place where questions about paradigms and definitions of complexity arise (sections 3 and 4), but also questions about the ethics of the research that we didn't tackle here.

(2.1) The theoretical objects (sections 3, 4 and 6) are specified as objects of the research study at hand, and different empirical hypotheses are made concerning these objects.

(3.1) Competing theories, included relevant mathematical theories, or elements of these competing theories, are considered. We give privilege, but not monopoly, to one of them (a part of which we presented briefly in section 9). It is important to notice that this movement is also the place of the first step of inventive synthesis (sections 8 and 10): the choice of a mathematical model to concretise through the empirical synthetic modelling movement.

(2.2) An observatory is specified, along with the theoretical hypotheses founding it (sections 5, 6, 7). It ought to be independent of moment (3.1) as indicated by the arrows. We saw through an example in section 12 that progress in theory and observatory are independent, but should be associated to improve the resulting analytical modelling.

(3.1*2.2) It is the pivotal movement of the analysis of the data collected, both applicative and inventive, resulting in an analytical modelling (section 8). It sets up a dialectics between analytical and synthetic modelling, with a primacy given to an inventive analysis realised with synthetic modelling as its horizon (section 10). By adding "commentary", we mean empirical and/or practical results obtained through the analytical modelling process, but waiting for a future modelling.

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(3.2) Here is the place of two kinds (empirical and/or technical) of synthetic modelling, depending on the orientation of the study (sections 11 and 12). This association in the same movement of empirical synthetic models and practical design models (synthetic by nature) of a technico-organisational system (or a family of such technico-organisational systems) stresses, on the one hand, the continuity between synthetic modelling of a given technico-organisational system and design of a new technico-organisational system, on the other hand, the similar epistemological role they play.

(3.3) The whole process results more or less in some kind of theoretical, methodological and technical progress.

On the whole, this global approach gives the answers to the eleven questions presented above a structure. This structure gives the primacy to ontology over epistemology (section 2), to theories or, at least, to theoretical hypotheses, over methods and models, to inventive analysis realised with synthetic modelling as its horizon over synthetic modelling itself.

Finally, the diagram presented (Figure 1) summarises a 'research programme' the 'heuristic power' and 'growing capacity' of which (notions developed in Lakatos, Musgrave, 1970) have to be evaluated at every moment. The discussion we would like to open with these eleven questions and with this summary of this global approach for the study and design of technico-organisational systems might be a part of this evaluation.

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