DYNAMICAL CONSTITUENCY: AN EPISTEMOLOGICAL ANALYSIS

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INTRODUCTION

One of the main problems of Cognitive Grammar (CG) — and of its European precursors such as Lucien Tesnière, Hans Jacob Seiler or Bernard Pottier — is to find adequate *mathematical* tools for describing the topological and dynamical information — in fact the *morphological* information — which is provided by perception, iconically encoded in cognitive image-schemas and processed by the semantics of natural languages.

Algebraic and logical formalisms are dramatically not convenient for such a task. Indeed, as far as they are discrete and symbolic, they cannot do justice to the continuous, spatio-temporal, morphological and Gestaltic aspects of the image-schemas. What we need for exploring what David Touretsky called the “iconic way” is another sort of formalism, of a topological, dynamical and physical nature.

Following René Thom's propositions in catastrophe theory and morphodynamics, I have tried to develop such a research program in semio-linguistics since 1975. It fits very well with the new trends in cognitive linguistics. We agree for example with Ray Jackendoff that there does exist a conceptual structure, that is a deep cognitive level of representation at which perception, action and language become compatible. This conceptual structure transforms the physical world into a projected world — a morphologically structured phenomenological world — endowed with a qualitative ontology.

The consequence of the fact that natural language is rooted in this morphological basis is for instance, in what regards syntactic structures, *the localist hypothesis*. It says that the possible relations of position (static and dynamic) between local domains in space-time play the role of archetypes — of basic schemes — for syntactic structures. I have explained it in details in my book "Morphogenèse du Sens".[1] It is the same idea that we find in Ray Jackendoff when he says, following Gruber, that:

[1] Petitot [1985]. Concerning the localist hypothesis, see also Petitot [1979], [1989b], [1989d].

> "In any semantic field of [EVENTS] and [STATES] the principal event-, state-, path-, and place- functions are a subset of those used for the analysis of spatial location and motion".[2]

We agree also with Leonard Talmy about the fact that *closed* grammatical classes (like prepositions, modal auxiliaries, grammatical relations, etc.) specify *semantic* contents of a very special and a very deep sort. As it is very well explained in "Relation of Grammar to Cognition" [3] and in "How Language Structures Space" [4], there exists a complex positional-topological information and a sophisticated morphological schematization which are specified by the closed class of prepositions.

> "Grammatically specified structuring appears to be similar, in certain of its characteristics and function, to the structuring in other cognitive domains, notably that of visual perception".

So, we need for cognitive linguistics a good "cognitive topology" in Lakoff's sense, and for this a good mathematical theory of the figurative schemes which organize semantic structures. This "topology" must be in fact also a dynamics. It must also include more geometry than the pure mathematical topology, for instance differentiable structures, stratified decompositions of spaces, convexity properties, transversality properties, or even metrical structures. All this belongs to what I call "*cognitive morphodynamics*".

The question is: does there exist some adequate and non trivial mathematics for cognitive morphodynamics? The response is: yes! These mathematics do exist. They are deep and sophisticated: qualitative theory of dynamical systems, bifurcation theory, singularity theory, etc. In fact, they are the mathematics used in connectionist modeling. But they have already been used in the morphodynamical approach long before the connectionist wave.

Now, the development of dynamical models for cognitive processing raises fundamental issues, some of which have already been tackled by connectionist models implementing dynamical devices. One of the most difficult challenges is the following: have dynamical models implemented in connectionist networks the capacity for adequately modeling in a purely dynamical way constituency (constituent structures) and compositionality which, classically, are modeled as symbolic ones ? At the linguistic level, the difficulty is for instance to model grammatical relations and semantic roles (in the sense of case grammars). In a nutshell, it can be formulated in the following way : if terms of sentences are modeled by attractors of some underlying dynamics, what is the

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[2] Jackendoff [1983].

[3] Talmy [1978].

[4] Talmy [1983].

dynamical status of a “syntax” relating these attractors ? *What can be a dynamical theory of syntactic constituency*?

In the last years.a great deal of works has been devoted to this question. In 1991 and 1992 two Conferences organized by Daniel Andler, Elie Bienenstock and Bernard Laks (COMPCOG I (1991) and COMPCOG II (1992)) have been held in France at Royaumont on this subject. Another Conference on *Dynamic Representations in Cognition* has been organized by Robert Port and Tim van Gelder at Indiana University in 1992. It led to reference book: *Mind as Motion*, to appear in 1995 at the MIT Press.

The particular status of dynamical explanations, contrasting with the deductive-nomological ones, has been stressed by Tim van Gelder (1992). The epistemological problems raised by a dynamical interpretation of basic theoretical concepts such as "structure", "constituency" and "syntax", have been analyzed with care by Daniel Andler (1990) and Yves-Marie Visetti (1990). They are not exclusively linguistic. They arise also in the realm of perception.

What can be a dynamical theory of syntactic constituency? The problem is difficult for at least two reasons.

a) <u>Weak CN vs Strong CN.</u>[5]

For doing syntax — deep universal and formal syntax and not English or French morpho-syntax — we need at least to distinguish :
(i) between two syntactic (categorial) types : things or objects (terms) and relations, and
(ii) between two types of relations : static and dynamic.[6]
Actually, a process described by a verb is a dynamical transformation of actantial relations. But, if we represent terms by activity patterns which are attractors of dynamical systems implemented in CN networks, the challenge becomes: how can we represent these two differences "term VS relation" and "static VS dynamic" ? It is clear that syntactic relations between attractors cannot be reduced to their mere linear superpositions. We call *weak CN* a CN which models semantic entities of *different* syntactic types by attractors of the *same* dynamical type, without taking into account the difference of their grammatical categories (this is a category mistake). For working out a response which can take up the challenge we need to strengthen weak CN into a *strong CN* which has the capacity to model different grammatical categories by mathematical entities of *different* types.

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[5] In the sequel, CN abbreviates "connectionism" and "connectionist", CNC "connectionist cognitivism", CL “classic”, and CLC "classical cognitivism".

[6] We must distinguish three meanings of “dynamical” : a physical phenomenon is dynamical when the causality of its temporal evolution is concerned; a relation is dynamical when it is temporal; a mathematical model is dynamical when it proceeds from the theory of dynamical systems (global analysis).

b) Elementary VS non elementary CN syntax.

One could think that it would be trivial to elaborate a strong CN. One would have only :

(i) to represent activity patterns (attractors) coding the terms by units belonging to some higher level layer (what is called in the literature a “localisation” through "grand-mother" neurons), and

(ii) to represent the relations by connections between these units.

We call such a solution an *elementary* one. It does not work for it projects analogically the neuronal implementation into the functional architecture. Static and dynamic relations between terms must be modeled by *dynamical relationships between attractors*, and these relations are of a completely different nature than the underlying connections they are implemented in. We call *non-elementary* a dynamical model which does justice to this principle.

The main problem to be solved is therefore the following one:

Main problem. Under the initial hypothesis that contents can be modeled by attractors, can an “attractor syntax” be worked out in the framework of a strong non-elementary CN, that is, of the theory of dynamical systems ?

In several papers (Petitot,), we have shown how this problem could be solved using bifurcation theory. We will not recall here this technical stuff. Our purpose will be to focus on more epistemological points. We will in fact analyze the debate having opposed Jerry Fodor, Zenon Pylyshyn and McLaughlin to Paul Smolensky. In early 1988, Jerry Fodor and Zenon Pylyshyn published an important paper in *Cognition* : "Connectionism and Cognitive Architecture: A Critical Analysis". It was a radical critique of the thesis held by Paul Smolensky in his paper "On the Proper Treatment of Connectionism" appeared in a special issue of *The Behavioural and Brain Sciences*. A little later, in his talk "Why connectionism is such a bad thing?", held at the Ecole Normale Supérieure in Paris, J. Fodor enhanced his criticisms.

As the debate is quite fundamental to CNC, we want to evaluate carefully its arguments. Our own arguments will seek to show :

(i) that Fodor's and Pylyshyn's criticism is essentially valid when it concerns weak CN; but that

(ii) it is not at all acceptable for a more elaborate form of CN which makes use of the full mathematical power of the theory of dynamical systems ("strong CN").

I CONNECTIONISM AND THE THEORY OF DYNAMICAL SYSTEMS [7]

1. The CNC's main thesis and its precursors.

It is well known that one of the basic ideas of CNC is that the automatisms of competence described at the symbolic level by discrete and sequential structures (symbols, symbolic expressions, rules, inferences, etc.) are formal and macroscopic "kinematical" structures which emerge from underlying microscopic "dynamics" of performance in a qualitative, stable and invariant way, and which have to be described at a subsymbolic level.[8] Classical symbolic cognitivism (CLC) is analytic and constructivist. It favours logical automatisms, conscious rules, calculus, and deductive inference. Subsymbolic cognitivism (CNC), on the contrary, is synthetic and associationist. It favours networks dynamics, intuitive performances, equilibrium positions and induction. For it, the macro entities endowed with a meaning are, at the microdynamical underlying level, global and complex distributed activation patterns of elementary meaningless local units. These scattered units are interconnected and process the data in parallel.

This key idea, which has been popularized under the acronym PDP (Parallel Distributed Processing) enhances in fact an idea proposed some 25 years ago by René Thom and Christopher Zeeman, according to which a meaning can be modeled, at the "macro" level, by the *topology* of an *attractor* of an underlying "micro" dynamics. These authors introduced the principle — deep but somewhat shocking at that time — that the *mathematical bifurcation theory of the attractors of non-linear dynamical systems* should take over from formal logic in cognitive linguistics. Syntactic and semantic structures was then considered as analogous to processes we meet in physics under the name of *critical phenomena*, especially with the thermodynamical phenomena of *phase-transitions*. Following Thom's proposal, if we consider the Liapounov functions on the basins of the attractors we are led to gradient like dynamics, that is, to dynamics which minimize potential functions H.[9] It was therefore the bifurcation theory of the minima of

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[7] The core of the paper developes some previous reflections (1988) published in Petitot 1991a. They were presented at the Workshop "Connectionism and Language" held at the International Center for Semiotic and Cognitive Studies of the University of San Marino in 0ctober 1989. My thanks are due to Franson Manjali for some parts of the English translation. I thank also Gertrudis van de Vijver and Barry Smith for their relevant suggestions.

[8] We use the opposition between "competence" and "performance" in its classical Chomskian sense.

[9] If X is a dynamical system (that is, a smooth vector field) on a differentiable manifold E, the attractors of X are the asymptotic and structurally stable limits of the trajectories of X. They are not necessarily punctual (that is the case for quasi-periodic movements on torus attractors) and their topology may be

such potential functions — that is the *mathematical theory of unfoldings of singularities of differentiable maps* — which became the basic tool for modeling semantic and syntactic structures (cf. Thom 1972, 1980, Zeeman 1977, and Petitot 1985 a, 1985 b).

2. CN networks and dynamical systems.

Instead of being describable by formal languages where the semantics is simple and the syntax complex, the subsymbolic systems possess, on the contrary, a simple syntax and a complex semantics. The instantaneous global state of a network S of N elementary interconnected units (formal neurons) u_i is given by their activation vector: $x = (x_i)_{i=1,\ldots,N}$, where x_i takes its values from a set of values V (finite or continuous). The main cases are $V = \{0, 1\}$, $\{-1, 1\}$, $[0, 1]$. Let $M = \{(x_i)_{i=1,\ldots,N}\}$be the configuration space of S. It is called the *internal space* of S.

The units u_i are interconnected by connections characterized by their *synaptic weights* w_{ij}. They determine the computational programm of the network. The $w_{ij} > 0$ correspond to excitatory connections, and the $w_{ij} < 0$ to inhibitory ones. In general $w_{ii} = 0$. The network computes in the following way. Each neuron receives afferent signals from its presynaptic neurons, changes its state according to a local transition law, and send an efferent signal to its postsynaptic neurons. In general, the input of the neuron u_i is defined as the weighted sum of the afferent signals:

$$h_i = \sum_{j=1}^{j=N} w_{ij}\, x_j \, .$$

The simplest hypothesis is that u_i is a threshold automaton the states of which are controled by a local transition of the form:

$$x_i(t+1) = g(h_i(t) - T_i)\, ,$$

where T_i is a threshold, and g a gain function. Typically,

- g = the Heaviside function if $V = \{0, 1\}$;
- g = the sign function if $V = \{-1, 1\}$;
- g = a sigmoid function = $1/(1+e^{-x})$ if $V = [0, 1]$.

The weights w_{ij} and the threshold T_i vary in a control space W which is called the external space of S.

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very complex (that is the case for the so called "strange" attractors). If A is an attractor of X, the dynamics is complex (ergodic, chaotic) on A. But on the complementary subset $B(A) - A$ of A in its basin $B(A)$, there always exists a real valued continuous positive function L, called a Liapounov function, which is strictly decreasing along the trajectories and which vanishes on A. So, on $B(A) - A$, the dynamics is of simple type. It minimizes a sort of energy function.

The local transition laws of an elementary unit from one state to another, considered as a function of the information that this unit receives from its immediate neighbors, define therefore an endomorphism $\mathfrak{T} : M \to M$. $\mathfrak{T}$ associates the next state $\mathfrak{T}(x)$ with the instantaneous global state $x \in M$ of S. In general, it encodes a considerable amount of information. It is the *iteration* of $\mathfrak{T}$ which defines the *internal dynamics* of the network S. The stable asymptotic states of $\mathfrak{T}$ (its attractors) are the *internal states* of S. If x is an input of S (an instantaneous initial state), its trajectory $(\mathfrak{T}^k(x))_{k \in \mathbb{N}}$ will tend, in general, towards an attractor A which will be the output (the response) of S to the input x. The key dynamical phenomenon is therefore the asymptotic capture of the (global and instantaneous) initial state of the net by an attractor. CN nets compute in a way radicaly different from that of Turing-von Neumann machines. They are dynamical computers which bifurcate from attractors to attractors.

On varying the local transition laws — for instance by changing the weights of the connexions of S — $\mathfrak{T}_w$ can be modified. The parameters w of the external space act therefore as controls on the internal dynamics $\mathfrak{T}_w$. This fundamental fact yields a basis for a theory of *learning*. The learning problem is in fact a typical *inverse problem*. The direct problem is, being given the synaptic weights w_{ij}, to find the attactors A_k of the internal dynamic $\mathfrak{T}_w$. The inverse problem is on the contrary, being given attractors A_k, to find specific weights such that the corresponding attractors include the A_k. It is a difficult one, but can be solved using some well known algorithms, such as the *backpropagation* one. These algorithms define *external slow dynamics* in the external space W.

The internal dynamics yielded by general nets can be tremendously complex. In general, attractors will have *an internal structure*, a non trivial topology. This fact is fundamental, since one can think that the topology encodes the internal intrinsic semantics [10] of the associated representations. But if the internal dynamics $\mathfrak{T}$ is gradient (see above), then this semantics reduces to a trivial one, in as much the way as, in CLC, one represents a content by a symbol. This will be the case if $\mathfrak{T}$ consists in minimizing an "energy" function $H : M \to \mathbb{R}$. We get that way the elementary catastrophic models introduced into syntax and semantics twenty five years ago (Thom 1969, 1972, 1980, Petitot 1979,1982a, 1982b, 1989b, 1989d, Wildgen 1982).

This is the case when the connections are *symmetric*. Hopfield has shown that, for $V = \{-1,+1\}$ and g = the sign function, the equations of the net are exactly those of a system *of interacting spins*. The value E of the energy H minimized by the dynamics is given in that case by the formula:

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[10] The internal intrinsic semantics must not be confused with an "external" denotative one. Using the Fodorian opposition between "narrow" and "large" contents, we can say that it concerns the narrow content of the representation.

$$E = -\frac{1}{2}\sum_{i \neq j} w_{ij}\, x_i x_j + \sum_i T_i x_i \,.$$

As far as the synaptic weights correspond to coupling constants and mix > 0 and < 0 values, we see that these nets — which exemplify the *simplest* neural nets — correspond to the *most complex* cases of spin systems, namely that of *spin glasses*.[11]

In general, the set M_{min} of minima of the energy H has a very large cardinal, and to use a statistical approach becomes therefore necessary. One introduces a "computational" temperature T and starts from an initial configuration x_0 with a high T value. This allows the system to access all the basins of E. One selects then randomly a neighboring configuration x_1. If $\Delta E < 0$ the transition from x_0 to x_1 has probability 1. If $\Delta E > 0$, it has the probability:

$$\frac{1}{1 + \exp\left(\frac{\Delta E}{T}\right)}$$

(this enables the system to overcome the thresholds). One iterates this process until the system reaches a local minimum. One lowers then T and start again.

More precisely, let us consider distributions of probabilities $P(x)$ over M. For a given mean energy C, that is for $\sum_{x \in M} P(x)\, H(x) = C$, the distribution $G_T(x)$ which maximizes the entropy is the Gibbs distribution:

$$G_T(x) = \frac{1}{Z_T} \exp\left[-\frac{H(x)}{T}\right],$$

where Z_T is the partition function:

$$Z_T = \sum_{x \in M} \exp\left[-\frac{H(x)}{T}\right].$$

When the computational temperature $T \to 0$, G_T concentrates on the minima of H, i.e.on M_{min} . This is a well known physical result. One can derive from it algorithms for minimizing H. One of the best known is "simulated annealing".[12] It is based on the construction of sequences of random variables X_n and of temperatures T_n such that:

$$\lim_{n \to \infty} P(X_n = x) = G_T(x)$$
$$\lim_{n \to \infty} P(X_n \in M_{min}) = 1\,.$$

As it is well explained by R. Azencott, we are given:

(i) An exploration matrix $Q = (q_{x,y})_{x,y \in M}$ satisfying the condition that, for all $x,y \in M$, there exists a chain (x_k) linking up x and y and such that $q_{x_k x_{k+1}} > 0$ for all k. If we call $V_x = \left\{ y \,\middle|\, q_{x,y} > 0 \right\}$ the set of neighbors of x, this condition means that two configurations x and y are always connectable by a chain of neighboring configurations.

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[11] See e.g. Mézard et al. [1987].

[12] Cf. Azencott [1988].

(ii) A sequence T_n of "temperatures" such that $T_n \to 0$ (cooling schedule), the decreasing of T_n being sufficiently "slow". This later condition is expressed by the fact that $\lim_{n\to\infty} T_n Log(n) = R$ where R is a sufficiently large constant.

(iii) A Markov chain of random variables X_n on M such, that for $y \neq x$:

$$P_n(x,y) = P(X_{n+1} = y \mid X_n = x) = q_{x,y} \ \exp\left[-\frac{\left(H(y)-H(x)\right)^+}{T_n}\right]$$

(where $a^+ = a$ if $a \geq 0$ and $= 0$ else), and

$$P_n(x,x) = 1 - \sum\nolimits_{y \neq x} P_n(x,y) \ .$$

A configuration x is a *local* minimum of H ($x \in M_{locmin}$) if $H(x) \leq H(y)$ for all $y \in V_x$. Its depth $D(x)$ is then defined as the minimal height of the thresholds (the saddles) which limit its basin of attraction.

A theorem due to Hajek says that:

$$\lim_{n\to\infty} P(X_n \in M_{min}) = 1$$

iff $\sum_{n=1}^{n=\infty} \exp\left(\frac{D}{T_n}\right) = \infty$, where $D = Sup\left\{D(x) \mid x \in M_{locmin} - M_{min}\right\}$.

When the synaptic weights become *asymmetric* there exists no longer an energy function and the dynamics is in general extremely complex. Steve Renals et Richard Rohwer have considered systems:

$$x_i(t+1) = g\left(r \sum_{j=1}^{j=N} w_{ij} \ x_j(t)\right)$$

where r is the slope of the sigmoid function g. They have retrieved that way many classical routes towards chaos, and in particular, for $r \in [12, 14]$, the period doubling subharmonic cascade of Coullet-Feigenbaum-Tresser.[13]

H. Sompolinsky, M. Samuelides and B. Tirozzi have also investigated such systems when N becomes very large and when the asymmetric synaptic weights w_{ij} are random variables with mean value = 0 and variance = w^2/N.[14] For the critical value $rw = 1$, they present a *phase transition* from a convergent regime to a *chaotic* one.

Many similar results show that it is henceforth possible to give a rigourous mathematical and physical status to the deep Thomian idea that the mental contents can be modeled by complex attractors of complex dynamical systems implemented in neural nets.

3. Harmony theory.

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[13] Cf. Renals, Rohwer [1990].

[14] Cf. Sompolinsky, Crisanti, Sommers [1988], Doyon, Cessac, Quoy, Samuelides [1993] et Tirozzi, Tsodkys [1991].

In his harmony theory, Paul Smolensky (1986) applied these ideas to many subsymbolic processes and stressed the importance of going beyond the (von Neumann) computational conception towards a dynamical conception of information processing. His aim was to link up the higher levels of cognition with the lower levels of perception. For example, in the interpretation of a visual scene we can suppose that the cognitive system undergoes the following operations.

(i) Let $(r_i)_{i \in I}$ be a set of representational features which "constitute the cognitive system's representation of possible states of the environment with which it deals". A representational state is thus a vector $\boldsymbol{r}$ of values of the r_i (±1, for example). The cognitive system interprets its environment using "knowledge atoms". Each of these atoms α is characterized by a knowledge vector $\boldsymbol{k}_\alpha$ which attributes values to each feature r_i. It is or it is not activated (we introduce an activation variable $a_\alpha = 0/1$). The atoms α encode sub-patterns of values of the features occurring in the environment. Their frequency is encoded in their force σ_α.

(ii) Let $(\boldsymbol{r},\boldsymbol{a})$ be the state of the cognitive system and let K be the knowledge base defined by $\boldsymbol{k}_\alpha$ and σ_α. We define H_k by:

$$H_k(\boldsymbol{r},\boldsymbol{a}) = \sum_\alpha \sigma_\alpha q_\alpha h_k(\boldsymbol{r},\boldsymbol{k}_\alpha)$$

with $h_k(\boldsymbol{r},\boldsymbol{k}_\alpha) = \boldsymbol{r}.\boldsymbol{k}_\alpha / |\boldsymbol{k}_\alpha| - k$.

(iii) We now apply the general thermodynamical method sketched above to this particular case. This allows us to interpret the visual scene (i.e. the vector $\boldsymbol{r}$) by completion, that is, by optimizing the *global coherence* (the consistency) of the (local) partial interpretations α: for a given representational state $\boldsymbol{r}$ and for an activation state $\boldsymbol{a}$ of the cognitive system, H is a sum including a term for each knowledge atom α, each term being wheighted by the force σ_α of $\boldsymbol{r}$ and each weight σ_α multiplying the self-consistancy between $\boldsymbol{r}$ and α (i.e. the similarity between $\boldsymbol{r}$ and $\boldsymbol{k}_\alpha$).

(iv) This inference and decision process is then indentified with the result of a parallel and distributed stochastic process driven by H_k.

4. Some epistemological issues concerning the "morphological turn".

4.1 The morphodynamical agenda.

The development of a morphodynamical explanation of performance raises a number of hard epistemological issues (Petitot 1979, 1982a, 1985 a, 1987, 1988). The most important feature of the "morphological turn" consists in a radical questioning of the logico-combinatorial formalist point of view on perception and language (and more generally on cognition).

In a formalist conception which reduces syntax to a formal description of competence automatisms (see for example the Chomskian transformational and generative conception), all the levels of description are of the *same* formal type. Consequently, in regressing from surface structures towards deep structures, one ends up with abstract primitive ("atomic", "kernel") structures which are *of the same formal type* as the surface ones (for example syntactic trees). It is thus impossible, on the one hand, to establish their link with the structures of perception, and on the other hand, to understand their genesis and their emergence in terms of the underlying dynamical mechanisms of performance. Now, the structures of perception, as well as the dynamical mechanisms of performance, impose certain *universal* constraints on grammatical structures. If one does not take them into account, one is committed to interpret them as genetic constraints.

That is why, since 1975, we have argued against the CL Chomskian "evidence" that our ignorance of the physical foundations of mental structures forces us to an abstract characterisation of them. The conclusion of such a "bad" syllogism is that structural properties of language which cannot be derived from such an abstract characterisation must be explained in innatist terms. It seemed to us, on the contrary (Petitot 1979), that the "good" syllogism was rather the following:

(a) we do not as yet know the physical (neurophysiological) bases of language ;

(b) but we can nonetheless hypothesize them and thus assume the existence of dynamical processes underlying performance, processes from which emerge the formal and abstract kinematical structures of competence;

(c) formal grammars formalize only certain aspects of these emerging formal structures ;

(d) but there exist other aspects, linked with perception, which impose additional cognitive constraints on the "humanly accessible" grammars.

It is such a strategy which was provided by Thom's topological syntax, where a meaning is coded by the topology of an attractor, and where the syntagmatic trees are trees of bifurcations of attractors into sub-attractors.

4.2. The PTC agenda.

Very similar questions have been recently tackled by P. Smolensky. Adopting a dynamic point of view in semantics and an emergentist one in syntax (emergence of formal, discrete, sequential structurally stable structures) he has very clearly presented their bases, their characteristics and their epistemological consequences. Let us recall briefly the essential points of his article "On the Proper Treatment of Connectionism" (Smolensky 1988a) :

(i) The CN level is neither the conceptual and symbolic CL level nor the neuronal one. It does not concern the implementation of cognitive algorithms in massively parallel machines, but rather the structure, the architecture and the dynamical behaviour of the cognitive processes themselves.

(ii) We cannot model the performances of *intuitive* knowledge on the assumption that the intuitive processor applies, simply in a unconscious way, sequential programs of formal rules. The processes at work are not adequately modeled in the framework of the CL symbolic paradigm, where symbols denote external objects (denotative semantics) and are operated upon syntactically by application of rules.

(iii) In a dynamical CN model, the units possessing a semantics are complex patterns of activity distributed over many elementary units. This conception of semantics is characteristic of the CN approach.

(iv) Whereas in the symbolic formal models, all the processing levels are of the same type, in the subsymbolic semantic ones there exists a semantic shift:

> "Unlike symbolic explanations subsymbolic explanations rely crucially on a semantic shift that accompanies from the conceptual to the subconceptual levels".

(v) In order to get a unified conception of cognition, it is necessary to combine the CN and CL approaches. The rules consciously applied by the conscious rule interpreter will be then interpreted as structurally stable emergent regularities:

> "patterns of activity that are stable for relatively long periods of time (of the order of 100 ms.) determine the contents of consciousness".

(vi) As far as the linguistic rules are concerned, this presupposes in particular the possibility of representing subsymbolically and sub-conceptually in CN dynamical models the propositional structures of language. This is certainly very difficult, but it is necessary. Now, according to Smolensky, a constituent structure can be obtained for the patterns of activity possessing a conceptual semantics when considering them as *superpositions of subpatterns* (constituents). As we shall see, this is the Achille's heel of the PTC agenda, and the criticism of Fodor and Pylyshyn focuses on this very point.

(vii) The mathematical universe of CN models is not that of formal languages and of Turing machines. It is that of dynamical systems, that is, of the qualitative theory of differential equations (global analysis). The subsymbolic computation is *continuous, geometrical and differential*. Inference here is a statistical inference which optimizes the fit with the input (harmony theory):

> "macro-inference is not a process of fixing a symbolic production but rather of qualitative state change in a dynamical system, such as phase transition".

(viii) The CN dynamical systems are suited for the elaboration of a theory of schemata, of prototypicality and of categorisation. (This point also has been already well-elaborated in the Thomian morphodynamical approach : see Petitot 1983 a, 1985b, 1989 a).

II. FODOR'S AND PYLYSHYN'S ARGUMENTS AGAINST CONNECTIONISM

1. The general structure of the F-P arguments.

Let us begin with a summary of Fodor's and Pylyshyn's main points.

A.1. Both classical (CL) and connectionist (CN) cognitivists are *representationalist.* They both admit mental states encoding the properties of higher level cognitive processes and, therefore, CLC/CNC debate is internal to cognitivism. *It concerns a precise issue : "the architecture of representational states and processes"*.[15] The CN paradigm is thus committed to show that it can provide a good theory of the cognitive architecture, *that is, of "processes which operate on the representational states of an organism"*.

A 2. Now, there exists a fundamental difference between the CL and CN paradigms. CL cognitivists assign semantic content to expressions, and admit between semantically evaluable entities, not only causal relations but also, *structural relations*. They consider it to be characteristic and essential :
(i) that mental representations share a combinatorial syntax and semantics, and
(ii) that mental processes *are dependent on this structure* ("structure sensitive") : operations operate on the mental representations as a function of their combinatorial structure, that is, of their syntactic form.

On the contrary, according to the authors, CNC would assign semantic content only to holistic entities, without internal combinatorial structure (labeled nodes symbolizing activity patterns). Further, they would only admit *causal* relations between the semantically evaluated entities. In a word, only CL cognitivists

> "are committed to a symbol-level of representation, or to a "language of thought", i.e. to representational states that have combinatorial syntactic and semantic structure".

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[15] In this section, all quotations are from Fodor, Pylyshyn [1988].

Contrary to CNC, CLC insists upon the fact that computational operations act on the syntactic structure of complex symbols and that, as far as the syntactic relations are parallel to the semantic ones,

> "it may be possible to construct a syntactically driven machine whose state transitions satisfy *semantical* criteria of coherence".

Such is "the foundational hypothesis of Classical cognitive science".

A.3. According to the authors, the limits of CNC are quite dramatic since mental representations *must* possess an internal syntactico-semantical combinatorial structure if they are to explain four fundamental aspects of cognition.

(i) *Productivity and Generativity*. As all natural languages, the "language" of thought is able to generate an indefinite number of expressions from finite means. Consequently, there must be rules of generation, and this presuppose an internal structure of the expressions.

(ii) *Systematicity*. Even if we challenge the productivity and the generativity of cognitive capacities, we cannot reasonably challenge their systematicity, that is, the intrinsic links relating the comprehension and the production of certain expressions with those of certain other expressions. Systematicity is explainable only if there exists an internal structure of expressions providing well-formedness rules and structural relations between different expressions.

(iii) *Compositionality*. There exist *semantic* transformations (a "covariance") between systematically related expressions (like "John loves Mary" and "Mary loves John", or like, "being a brown cow", "being brown" and "being a cow", etc.). The principle of compositionality according to which the semantic properties of constituents are independent of the context can be only understood if there exists a syntactico-semantic constituent-structure.

(iv) *Inferential coherence*. The relations of logical similarity between different inferences presuppose the same conditions.

A.4. In a nutshell, if we accept an internal structure of representations, then we are legitimate to speak of representations sharing the same structure, of similar structures, or of structures which are related to each other in different ways. But, according to the authors, an essential feature of CNC would be to reject such a structure. For CNC, cognitive systems are systems:

> "that can exhibit intelligent behaviour without storing, retrieving, or otherwise operating on structured symbolic expressions".

Of course, the labels which identify semantically evaluable holistic entities, will have, in general, a constituent-structure, but the processual dynamics of the system is *not* determined causally

> "by the structure – including the constituent-structure – of the symbol arrays that the machines transform".

CN graphs are *not* structural descriptions of mental representations, but specifications of purely causal relations.

> "The intended interpretation of the links as causal connections is intrinsic to the theory".

> "A network diagram is not a specification of the internal structure of a complex mental representation. Rather, it's a specification of a pattern of causal dependencies among the states of activation of nodes".

On the other hand, the fact that mental representations are distributed over micro-features extracted by multivariational analysis from the statistical regularities of the stimuli samples) *does not* imply that they are structured. Actually,

> "you have constituent-structure when (and only when) the parts of semantically evaluable entities are themselves semantically evaluable".

> "Complex spatially-distributed implementation in no way implies constituent-structure".

The main error of CNC, its "major misfortune", is to have confused a componential analysis of micro-features with a combinatorial structure.

> "The question whether a representational system has real-constituency is independent of the question of micro-feature analysis".

> "It really is very important not to confuse the semantic distinction between primitive expressions and defined expressions with the syntactic distinction between atomic symbols and complex symbols".

In short, from the moment when the semantically evaluable entities (nodes, activation-patterns, etc.) are conceived of as *atomic* and *holistic* Gestalts related only by causal relations, it becomes impossible to account for the fundamental features of cognition, namely, productivity, generativity, systematicity, compositionality, and inferential coherence (cf. A.3.).

> "The connectionist architecture (...) has no mechanism to enforce the requirement that logically homogeneous inferences should be executed by correspondingly homogeneous computational processes".

CNC presupposes a systematic organization of cognition. But it should also be able to explain it.

> "It's not enough for a connectionist to agree that all minds are systematic ; he must also explain *how nature contrives to produce only systematic minds*".

Hence the final verdict :

> "The only mechanism that is known to be able to produce pervasive systematicity is classical architecture. And (...) classical architecture is not compatible with connectionism since it requires internally structured representations".

A.5. Further, according to the authors, CNC's main criticism against CLC is not acceptable. It claims that, for CLC, the behavioral regularities must come from explicitly encoded rules. But this is false. In fact, for CLC, several functions can be encoded implicitly (for example, as part of the hardware). What should be explicit are only the data structures that the cognitive machines transform and not the rules (the grammar) of transformations.

A.6. As a consequence, the CN perspective should be rejected as a *cognitive* theory. It relies upon a "bad" associationist psychology against which one can repeat the well-known rationalist criticisms formulated since Kant.

A.7. The authors conclude that the only real interest of CNC is to provide an alternative theory of *implementation* for the classical functional architecture. They stress the fact that most of the arguments put forward by CNC bear only on the limitations imposed on competence by the concrete constraints of performance. According to them, the material limits of performance result from an interaction between an unlimited formal competence (unlimited but finitely describable by generative rules (see A.3.(i)) on the one hand and the limited resources on the other. Adopting a functionalist perspective radically opposed to the emergentist CN one, they separate drastically the functional architecture (the software algorithms) from its implementation (the hardware). This is, for them, a "principled distinction". The (micro-level) models of implementation are neutral with respect to the nature of cognitive (macro-level) processes and to deny this fact is to confuse structure and function.

Such a confusion leads to catastrophic consequences. For example,

(i) from the evident existence of neuronal networks one will conclude to an associationist psychology (networks of representations), or

(ii) from the not less evident anatomic distributivity of neurons one will conclude to a functional distributivity of the mental representations themselves (componential analysis in micro-features), or

(iii) from the reinforcement of the connection of two neurons by their co-activation one will conclude to associationist statistical models of learning, or still

(iv) in the other direction, from a functional locality (position of a symbol in an expression, for example) one will conclude to a physical localisation in instanciation.

The "brain style" of CNC is quite definitely a dramatic epistemological error : it makes

> "the implicit – and unwarranted – assumption that there ought to be similarity of structure among the different levels of organisation of a computational system".

It projects the neuronal level onto the cognitive one, and so doing, reactivates "the worst of Hume and Berkeley".

A.8. Thus, the CN stance may only provide

> "an account of the neural (or "abstract neurological") structures in which classical cognitive architecture is implemented".

The symbolic structures of CLC are of course physical ones. They are neurally encoded and instantiated and it is their physical implementation which cause the operational behaviour of the cognitive system. The CN arguments become therefore valid if we interpret them as arguments in favour of a physical implementation in massively parallel networks. For example, the fact that cognitive processes are fast, whereas neuronal phenomena are slow, or the fact that a considerable amount of forms (words, faces, etc.) stocked in memories can be quickly recognized, or still, the continuity, fuzziness, approximation and structural stability properties of cognitive processes, all these facts support the thesis of a CN implementation of the algorithms making up the CL functional architecture. But if the CN models are rather to be seen as concerning only implementation, then they should surrender their cognitive claims. They should in particular refuse to assign "a representational content to the units (and/or aggregates) that they postulate".

A.9. A key argument which is not made explicit by the authors is that "structural" necessarily means "formal-symbolic". If mental representations share a combinatorial syntax and semantics then they are ipso facto "symbol systems". As we will see, it is this *formalist dogma* — the dogma of logical form — which is the Achille's heel of all their arguments.

2. Comments : the problem of a dynamical structuralism.

F-P's arguments are well constucted and apparently forceful. However, we can question their effective validity at different levels.

C1. Arguments A.1. and A.2. (as regards the characterization of CLC), A.3., A.5. and A.8. (except for its conclusion) are, we think, excellent and convincing. But they *do not* imply a rejection of CN as a cognitive theory. They simply impose to it certain constraints and additional requirements (as explained in A.1.) : to be able to develop what we will call the *structural hypothesis*.

C2. The presentation and characterization of CNC given in A.2. and A.4. are caricatures. They "demonstrate" only the following "syllogism" :
(i) a "good" CNC should be able to develop the structural hypothesis;
(ii) for intrinsic reasons, the caricature of CNC presented in A.2. and A.4. does not fulfill this requirement;
(iii) therefore CNC, whatever can be its further developments is *de jure* a "bad" cognitive theory.
But of course, nothing proves that the caricature can be identified with the full theoretical power of CNC. It is why the argument is dogmatic.

Let us continue to call "dynamical" the CN cognitivism. The central question is the following:

Question: *just as it is possible, using appropriate formal theories, to develop a symbolic structuralism, is it also possible, using the mathematical theories of dynamical systems, to develop a dynamical structuralism* ?

If we reduce a priori all possible CN models to graphs of causal relations between holistic units lacking internal structure, then the reponse is of course trivially negative. *But these* *elementary models are only a very tiny part of the mathematical theory of dynamical systems*. We shall return to this point later. It is essential.

C3. Even if we could accept that, contrary to the assertion of the authors, it is possible to elaborate an authentic dynamical structuralism, this would not lead to transform the CL/CN opposition into a Manichean alternative. There is certainly higher processing levels of the cognitive system which are of symbolic nature. But this does not entail that there do not exist lower levels which are of a dynamical nature. Associationist processes are certainly not sufficient to explain the structure of cognition, but they can be nevertheless necessary. Logico-symbolic superstructures can possess associationist infrastructures. The question is not whether CNC should overcome CLC (or if the later should excommunicate the former), but to find out whether *the structural hypothesis can or cannot be already elaborated at the dynamical level of cognitive processes*. Such a dynamical structuralism must be clearly distinguished from the formal symbolic one. It must be :

(i) an authentic structuralism,

(ii) a *proto*-symbolic one, that is, one which is compatible with the symbolic level.

If one still wants to criticize it, one must develop more refined arguments than the F-P ones (cf. C6).

C4. Fodor and Pylyshyn seem deeply unaware of the true nature of the *emergence* of a macro-level from a micro-level. By separating the functional level of algorithms from the level of implementation, they disregard what is really the central issue in the point of view they are attacking. However, the physical parallel they suggest should have incited them to more circumspection.

> "The point is that the structure of 'higher levels' of a system is rarely isomorphic, or, even similar, to the structure of 'lower levels' of a system. No one expects the theory of protons to look very much like the theory of rocks and rivers, even though, to be sure, it is protons and the like that rocks and rivers are 'implemented in'".

The argument is fallacious. Physics is radically anti-functionalist. In physics, the relation between micro-levels and macro-levels is a matter of emergence. No physicist would separate the levels and postulate, as the authors do, that micro-levels are "neutral" in relation to macro-levels and that the later are thus independent of their "implementation". The "form/ matter" opposition, which is of Aristotelian origin, has been eliminated in modern science. The very physical problem here is to understand how an emergent — and therefore *non*-independent — macro-level can nonetheless present a certain *autonomy* of structure. That two levels are of different nature *does not* implies that they are independent and "neutral" in relation to each other. To assert this is to seriously misunderstand the epistemology and ontology of emergence. CN cognitivists are thus right when they distinguish the problem of implementation from the *intra-cognitive* problem of the emergence of a symbolic level out of a dynamical subsymbolic level. But we repeat that CL cognitivists are right when they assert that this dynamical level, in order to be considered cognitive, should be *structural.*

C5. This last point is the crux of the problem. The authors attack CNC because it assumes the systematic organisation of cognition without explaining it (A.4.). But the same argument can be turned against them. For they themselves *do not* explain this systematicity. They only *describe* it formally. By reducing the performance constraints to the material concreteness of implementation, by separating, conformally to their functionalist perspective, the levels and by autonomizing competence they can surreptitiously identify a formal logico-combinatorial description of competence with the development of the structural hypothesis. *But this identification is possible only if we assume the thesis A.9. according to which structural ≡ symbolic (logico-combinatorial).* But, if we admit this equivalence, then the argument becomes trivial : CNC is not symbolic (by definition), "hence" it is not structural, "hence" it cannot account for the structural character of cognitive processes.

A formal symbolic description of mental representations and of mental processes is clearly possible. But as such, it should not be confused with an explanation. To get an explanation, we must :

(i) model the semantically evaluable entities by *mathematical* structures — perhaps very sophisticated — of a certain type, that is, belonging to a certain mathematical universe;

(ii) show that a theory of structures can be developed within this universe.

The question of CNC then becomes (cf. C.2 and C.3):

Question : *if semantically evaluable entities are modeled by attractors of dynamical systems, is it or not possible, within the framework of the theory of dynamical systems, to develop a theory of structure* ?

C.6. If CL cognitivists content themselves with a mere formal symbolic description, it is because the explanation of cognitive structures must be for them of an innatist nature. Behind the CL/CN controversy and the conflict of arguments, behind the rationalist critique of empiricist associationism, there lies, in fact, an epistemological alternative. It was brilliantly sketched by Massimo Piattelli-Palmarini in his (1988) paper "*Evolution,* *Selection and Cognition : from 'Learning' to Parameter Fixation in Biology and in the Study of Mind*". The argument goes as follows.

In all biological domains, one has progressed from *instructivist* ("Lamarckian") theories to *selective* ("Darwinian") ones. In every case, both experimentally and theoretically, one has arrived at the conclusion that there cannot be a transfer from the structure of environment to the organism, and that only mechanisms of *internal* selection can be mechanisms of learning. This internal selection involves filtering and fixation of parameters which selectively stabilize certain possibilities among a *very rich* universe of *genetically determined* possibilities. From the *instructivist* point of view, the genetic constraints are poor and structuration comes from general capacities, such as adaptations, resolutions of problems by trial and error, etc. For the selective point of view, on the contrary, the genetic constraints are essential, the structuration is strongly innate and modular, and adaptation is replaced by "exaptation", that is, by the fact that the characters can be selected independently of all adaptive value, even if, later on, they acquire such a value. For the selective thesis, the impossibility for an organism to assimilate external structures is a nomological one : it is nomologically improbable that

> "structures external to the organism might possibly be 'internalized' through a 'learning' process";

it is, however, nomologically very probable that

> "a process of selection, of triggering and parameter-fixation, acting on a vast, profligate and highly articulated repertoire of innate structures may prove to be the most productive explanatory hypothesis" (Piattelli-Palmarini, 1988 : 23).

It is such an innatist and selective point of view which is now further developing in the domain of the cognitive sciences, in syntax as well as in semantics. Hence the radical critique undertaken by Chomsky, Fodor and their colleagues against empiricist theories of learning by imitation, association, assimilation, induction, problem-solving, etc. Many results seem to indicate that there exists a rich syntactico-semantic architecture of language whose universality is of a genetic origin :

> "our species innately possesses a rich, specific, modular and highly articulate capacity for language, organized around certain universal 'principles'".

This cognitive capacity would be independent of perception and action. It would manifest

> "a very intricate and closely inter-dependent process, full of 'deductive' consequences that are known to each of us in a totally unconscious way".

That is why, genetic constraints being contingent, a formal description can amount to an explanation.

It is this formalist dogma we are criticizing. For these arguments are relevant only in the symbolic framework. They do not imply at all that the innate symbolic form of the cognitive system exhausts its structure. It is indeed perfectly legitimate to assume :

(i) that there exists an objective content on which this form operates;

(ii) that a *dynamical* functional architecture can also be innately constrained.

3. The main point of the F-P argument.

Let us come now to the central argument of Jerry Fodor and Zenon Pylyshyn. They consider the way in which some CN cognitivists (Hinton, McClelland, Rumelhart) treated a sentence like "John loves Mary". The problem is, evidently, that of

> "the role relations that traditionally get coded by constituent-structure".

They accept therefore with great fair-play the conception of syntax which is the least symbolic and the most akin to the CN sensibility, namely that of *case grammars*. But they stress the fact that, to be acceptable and amenable, CNC must provide a good CN account of the semantic roles which select cases. This is the main problem : to model in a CN framework what European linguistics and semiotics call *actantial relations*.[16]

The CN cognitivists mentioned above represent actantial relations by a set of activated units such as {John-subject; +loves; +Mary-object}, where the descriptors *J-S*, *L*, *M-O* are labels of holistic units *without* internal syntactic structure and *without* structured inter-relations. These descriptors combine an identity (an actant *J*, *M*) with an actantial role (*S*, *O*) and represent the syntactic structure of the sentence in a set-theoretic manner. Fodor and Pylyshyn can easily show that such a representation immediately leads to a lot of inescapable difficulties which can be solved only by a "grotesque" proliferation of the number of descriptors :

> "the idea that we should capture role relations by allowing features like John-subject thus turns out to be bankrupt".

[16] We use here the gallicisms "actant", "actantial", and "actantiality" for what concerns the case semantic roles.

> "It is of course, no accident that the connectionist proposal for dealing with role relations runs into these sorts of problems. Subject, object and the rest are classically defined *with respect to the geometry of constituent-structure trees*. And the connectionist representations don't have constituents".

If we just superpose additively the activated holistic entities in order to account for the sentences, then it becomes, for example, impossible to account for the relation between *J-S* ; +*L*; +*M-O* and *M-S*; +*L*; +*J-O* (argument of systematicity, cf. A.3.(ii)).

> "This consequence (...) offers a particularly clear example of how failure to postulate internal structure in representations leads to failure to capture the systematicity of representational systems".

Further, in the case of a conjunction of sentences, it becomes impossible to retrieve the initial structures. The superposition leads to an irreversible destructuring (what is called the "superposition catastrophe").

This is really the decisive point :

> "when representations express concepts that belong to the same proposition, they are not merely simultaneously active, but also *in construction with each other*".

And, in order to be in a relation of "construction" — that is, to be related by dependence relations —, representations should be constituents of more complex representations (cf. the arguments A.2 and A.3).

> "Representations that are 'in construction' form parts of a geometrical whole, *where the geometrical relations are themselves semantically significant*".

The main problem is therefore to build-up what we will call *a configurational definition of case roles*.

Of course, for the CL paradigm, the problem of a configurational definition of actantial relations is *a priori* solved by the use of formal and combinatorial symbolic structures. But this does not entail at all that every such configurational definition *must* be, for *de jure* reasons, of a symbolic nature.

4. Towards a geometry of syntax.

We see that Fodor's and Pylyshyn's critiques against the possibility of working out a connectionist theory of high level cognitive abilities — and in particular a theory of syntactic constituency and compositionality — are based on two thesis.

a. A thesis concerning the internal constituent structures — that is the internal form — of mental representations.

The structures they consider are principally case-structures where semantic roles (the "actantial relations") are "in construction with each other". These semantic roles "classically defined *with respect to the geometry of constituent-structure trees*" are geometrical *and* syntactical relations which constitute the roots of constituency. Here, constituency is more basic than combinatorial compositionality.

b. A thesis concerning the necessary symbolicity of constituent structures and the reducibility of constituency to combinatorial compositionality.

For the authors, representations can manifest internal constituent structures if and only if they are symbolic representations. Their internal constituency is therefore a combinatorial one analogous to that found in formal languages. The "geometrical" form of constituency is therefore reduced to a pure combinatorial one.

Now, the point is that thesis (a) does not entail thesis (b). Actually, there exist many natural structures which are not symbolic but present nevertheless constituent structures. The most evident case is that of atoms and molecule in quantum mechanics: electronic orbitals provide a typical example of constituents which are not symbolic in nature and can be dynamically modeled as solutions of partial differential equations. Therefore, if constituent structures are *natural* structures they don't need to be necessarily symbolic.

In the perceptive realm, another spectacular example is provided by the use of anisotropic diffusion equations in computational vision. They are able to carry out in a unified way two contradictory operations on images: smoothing and segmenting! They yield very powerful bottom-up and data-driven algorithms which can achieve a morphological constituent analysis without any symbolic means.

If we admit thesis (a) without admitting thesis (b), the nature of the problem raised by the F-P arguments changes radically. It becomes to know whether the main concepts of relation, structure, constituency and compositionality can be mathematically interpreted in a purely dynamical framework. Is it possible to work out a "syntactic geometry" of structures in the CN context ?

III. REFUTATION OF THE MAIN POINT OF THE F-P ARGUMENT AND THE MAIN PROBLEM.

R.1. The central F-P argument is valid only as far as it is applied to a *weak* CN. Remind that we call weak CN a CN which models uniformly semantically evaluable entities of different syntactic types by mathematical structures of the same type, without taking into account their differences of grammatical categories. On the contrary, we call strong CN a CN which has the capacity of modeling the differences and the relations between different grammatical categories.

R.2. Let us clarify this a little bit further. The F-P argument points out *a category mistake*. Its "syllogism" is as follows :

Syllogism S1:

(i) Let A_i ($i = 1,..., n$) be the actants of a sentence and let V be the verb organizing the actantial interactions. Let us *model* the actants A_i by means of mathematical structures $\mathcal{A}_i$ of a certain type (for example activity patterns) on which is defined an abelian (associative, commutative, with neutral element and inverse elements) operation of composition $\oplus$, that is an abelian group law (for example, the superposition of activity patterns).

(ii) Let us model the verb V by a structure $\mathcal{V}$ of the *same* type as that of the $\mathcal{A}_i$.

(iii) Let us model the actantial interaction V of the A_i by the sum $\mathcal{V} \bigoplus_{i=1}^{i=n} \mathcal{A}_i$.

(iv) Experimental observation: such a modeling strategy runs into unavoiable difficulties.

(v) Conclusion: the modeling of the actants A_i by the structures $\mathcal{A}_i$ should be rejected since it is experimentally refutable ❒

Further, the authors oppose this syllogism to another one aimed at showing the superiority of CLC.

Syllogism S2:

(i) Let us *symbolize* the actants A_i by symbols A^*_i.

(ii) Let us symbolize the verb V by a symbol V^*.

(iii) Let us symbolize the actantial interaction V of the A_i by syntagmatic relations between the A^*_i and V^* (for example, by a syntagmatic tree produced by a generative grammar or a constituent-structure grammar).

(iv) Empirical observation : such a symbolization is "good".

(v) Conclusion : it has to be accepted since it is experimentally valid ❒

The problem is that the first syllogism (S1) is fallacious and the second (S2) tautological.

S1 is fallacious.

In fact, it is equivalent to say that the (logico-combinatorial) structures of syntagmatic-tree type being non-associative and non-commutative (and therefore non abelian), they cannot be modeled adequately by abelian algebraic structures of group type.

Let us mimic the F-P argument for another theory, for instance a physical theory (ultra-simple, so fictional) of elementary particles (e.p.).

(i) Let us model the free e.p.'s P_i ($i = 1,\ldots, n$) by irreducible representations G_i of the Poincaré group in an Hilbert space.

(ii) Let us model the concept of an interaction V between the P_i by another irreducible representation F.

(iii) Let us model the interaction of the P_i by the sum $F \bigoplus_{i=1}^{i=n} G_i$.

(iv) Empirical observation : such a modeling strategy runs into unavoiable difficulties and is experimentally refutable.

(v) Conclusion : the modeling of the P_i by the G_i must be rejected ❐

In such a case the fallacy is striking. One has made a category mistake in confusing the concept of *interaction* in (ii) and (iii) with an additional free e.p. (iv) is trivial since an interaction of P_i is not the same thing as the system of the free P_i to which V has been added. The inference (iv)⇒(v) is completely illegitimate.

It is the same case with the F-P argument. It also points out a category mistake : an interaction of actants is modeled by a mathematical structure $\mathcal{V}$ of the *same type* as those $\mathcal{A}_i$ which are used to model the actants themselves. Fodor and Pylyshyn are then perfectly right in denouncing such an error in weak CN, but nevertheless their drastic and dramatic conclusion is fallacious. The only correct conclusion is that, if actants are dynamically modeled by attractors, then verbs expressing interactions of actants cannot be modeled by attractors of the same type.

S2 is tautological.

It is clear that if one symbolizes constituents by means of formal symbols, then one can *a priori* symbolize their structural relations by means of formal relations. But, as we have already seen, such a formalization does not explain at all the relations.

R3. It is thus necessary to clarify and work out further F-P's central argument. This can be done in the following way.

(i) First of all, we must be aware of *the distance that separates a true mathematical modeling from a mere formal symbolization*. Modeling a certain class of natural phenomena is to interpret them by sophisticated mathematical theories which allow to *reconstruct* their properties *mathematically*; whereas to symbolize them, in contrast, is to *represent* such properties *formally* . The requirement of modeling has quite nothing to do with symbolization: for instance a mathematical physics of elementary particle interactions has nothing to do with symbolic representations of the type $V(P_i)$ where V is an n-ary relation. The main limitation of symbolic-formalist points of view in cognition is their confusing of a formal description with a mathematical explanation (see Petitot 1982a, 1982b, 1985a, 1986b, 1986c). We see here the dramatic consequences of the argument A.9 criticized in C.5. above.

(ii) Up to now, CNC constitutes the most decisive attempt to move from a formal symbolization to a mathematical modeling in cognitive sciences. And, as far as it aims to provide an explanation only for *proto*-symbolic structural phenomena, the lack of formalism cannot be imputed to it.

(iii) We can refute F-P's arguments if we can answer positively the question whether, in the case of syntactic structures, there are *two* structural levels which do correspond respectively to the dynamical and symbolic ones. In a number of works, I have tried to show that such is effectively the case. Underlying the strictly grammatical level of grammatical relations (which are quite adequately described in terms of symbolic structures: syntagmatic trees, etc.) there does exist, in fact, the level of actantial relations where the actants are defined by their semantic (casual) roles and where the verbs express the actantial interactions. The differences between formal grammars and case grammars are well known. Now — it is the main result of these works — it is possible to work out a dynamical theory of actantial grammars.

(iv) But, we noticed that F-P's arguments are *neutral* with respect to this difference of levels. They concern as well the actantial roles as the grammatical relations *and only refer to the "geometry" of structures* where "the geometric relations are themselves semantically significant". Thus we may apply the results of § R2 regarding actantial syntax. Whence the question:

Main question. If the actants A_i of a process are modeled by attractors $\mathcal{A}_i$ of a dynamical system, is it possible, within the framework of the mathematical theory of dynamical systems, to elaborate a theory of actantial interactions — that is a theory of the verb ?

(v) Let us develop further this question. We have seen that, in many dynamical models, the situation can be greatly simplified if one makes the hypothesis that the dynamics X defining the attractors $\mathcal{A}_i$ admits a global Lyapunov function or, even more simply, that X is gradient : $X = -\mathrm{grad}\, f$. The $\mathcal{a}_i$ are then the minima m_i of the potential function f.

Simplified main question. If the actants A_i of a process are modeled by the minima m_i of a potential function, is it possible, within the framework of the dynamical theory of potential functions, to elaborate a theory of actantial interactions — that is a theory of the verb ?

The mathematical challenge is therefore to develop a theory of *interactions of attractors*. What we call an *attractor syntax*. René Thom, Wolfgang Wildgen, Per Aage Brandt and ourselves have shown that bifurcation theory provides adequate tools for solving it.

IV. FROM CONNECTIONIST BINDING TO A CONFIGURATIONAL DEFINITION OF SEMANTIC CASE-ROLES.

Since 1988, many CN cognitivists have proposed strategies for taking up Fodor's and Pylyshyn's challenge. One of the most interesting, but still insufficient, is Smolensky's idea of using the *tensorial product* operation.[17]

1. Smolensky's tensorial product.

Smolensky's main idea is to take for granted the CL finitist and combinatorial view of symbolic structures and to *represent* them in a CN way — in much the same way as one represents abstract groups in linear groups in the well known group representation theory. To do this, he first adopts a case conception of syntax and thinks of syntactic structures as composed of three sorts of entities:

(i) semantic case-roles r_i ;

(ii) fillers f_j ;

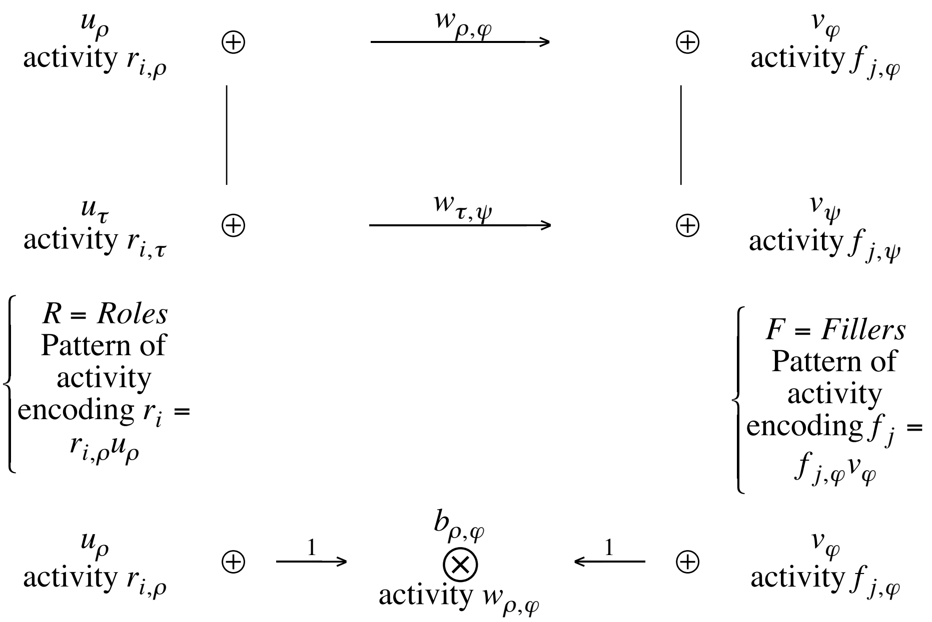
(iii) binding relations between roles and fillers.

He supposes then that the roles and the fillers are *already* represented in a CN (local or distributed) way and solves the problem of representing *the binding relations* using the linear device of tensorial product.

Suppose that the roles r_i (resp. the fillers f_j) are vectors belonging to the vector space V_R (resp. V_F) of the global states of a network R (resp. F). Let u_ρ (resp. v_φ) be the units of R (resp. F). One connects R and F using connections $u_\rho \leftrightarrow v_\varphi$ with Hebbian

[17] See Smolensky [1990] and Visetti [1990].

weights $w_{\rho,\varphi} = \sum_i r_{i,\rho}.f_{i,\varphi}$ where $r_{i,\rho}$ (resp. $f_{i,\varphi}$) is the activity of the unit u_ρ (resp. v_φ) in the global activity pattern of R (resp. F) representing r_i (resp. f_i). The tensorial product device consists in introducing *new* units $b_{\rho,\varphi}$ between R and F, $b_{\rho,\varphi}$ being connected by two weights = 1 to u_ρ and v_φ and having $w_{\rho,\varphi}$ as activity (see figure).



It is easy to see that we get that way a CN implementation $R\otimes L$ of the tensorial product $V_R\otimes V_L$ with basis $b_{\rho,\varphi} = u_\rho\otimes v_\varphi$. With $w_{\rho,\varphi} = \sum_i r_{i,\rho}.f_{i,\varphi}$, the state of $R\otimes L$ becomes :

$$\sum_{\rho,\varphi} w_{\rho,\varphi}\, b_{\rho,\varphi} = \sum_{\rho,\varphi}\left(\sum_i r_{i,\rho}.f_{i,\varphi}\right) u_\rho \otimes v_\varphi = \sum_i \left[\left(\sum_\rho r_{i,\rho}u_\rho\right)\otimes\left(\sum_\varphi f_{i,\varphi}v_\varphi\right)\right] = \sum_i r_i \otimes f_i$$

We get therefore a *representation* $\Psi : S \rightarrow V$ of a set S of structures in the state space of a network, where a representation is defined in the following manner. We suppose that there exists a *role decomposition* F/R of S that is a truth function which assigns to each pair (f_j, r_i) the truth value of the predicate $f_j \,|\, r_i$ on S : "f_j fills the role r_i in $s\in S$". We suppose as given a CN representation of the fillers/roles bindings $\Psi_{bind} : \{f_j \,|\, r_i\} \rightarrow V$ and we define the representation of the role decomposition F/R by the map :

$$\Psi : S \longrightarrow V$$
$$s \longrightarrow \sum_{\{(f_j,r_i)\,|\,(f_j\,|\,r_i)(s)\}} \Psi(f_j \mid r_i) \qquad .$$

In a tensorial product, r_i (resp. f_j) is identified with an activity pattern $r_{i,\rho}u_\rho$ (resp. $f_{j,\varphi}v_\varphi$) and $f_j\,|r_i$ is identified with $r_i \otimes f_j$. As far as a structure s is a conjunction of $f_i\,|r_i$ and a conjunction is represented by addition, we get finally :

$$\Psi(s) = \sum_i r_i \otimes f_i \quad .$$

Paul Smolensky has shown in a detailed and convincing manner that this type of procedure allows to represent in various ways operations and transformations on symbolic structures. According to him, this shows that it is possible to integrate

> "in an intimate collaboration, the discrete mathematics of symbolic computation and the continuous mathematics of connectionist computation".

This view is clearly anti-eliminativist. Smolensky does not want to *reduce* all symbolic structures and processes to CN ones. He wants to *represent* in a CN way these symbolic descriptions in order to explain "higher thought processes".

For instance, in a recent joint work with G. Legendre and Y. Miyata [18], he applies this strategy — of understanding

> "how symbolic computation can arise naturally as a higher-level virtual machine realized in appropriately designed lower level connectionist networks" —

to binary trees. Let r_{x_i} be the *positional* roles in a binary tree with nodes x_i. A tree s with atom f_i at node x_i is represented by the tensorial product $s = \sum r_{x_i} \otimes f_i$. This representation is in fact a *recursive* one. The x can be coded by binary strings using the code 0 = "left child" and 1 = "right child". Let $r_{x0} = r_x \otimes r_0$ and $r_{x1} = r_x \otimes r_1$. Using such a binary coding, a tree s becomes represented by a vector of the vector space $\bigoplus_{k=1}^{k=\infty} V_R^{\otimes k} \otimes V_F$ where V_R is generated by r_0 and r_1. Smolensky shows how to implement a programming language such as LISP in such a framework. He gives also a CN representation of Context Free Grammar theory.

2. Dynamical binding.

With regards to the implementation of the binding relations between roles and fillers, we want also to mention the *dynamical binding* by means of synchronized oscillatory neural groups developed by Gerald Edelman, Christof von der Malsburg,

[18] Smolensky et al. [1992].

Charles Gray, Wolf Singer, Elie Bienenstock, and Lokendra Shastri.[19] The key idea is that the terms of sentences (objects) are internally represented and encoded by distributed arrays of rythmic neural assemblies — oscillators — and the bindings by processes of *synchronization* — phase locking — between such oscillators. In this perspective,

> "reasoning is the transient but systematic propagation of a *rythmic* pattern of activation, where each *phase* in the rythmic pattern corresponds to an object in the dynamic or short-term memory, where bindings are represented as the *in-phase* or *synchronous* firings of appropriate nodes, where long-term facts are subnetworks that act as temporal pattern matchers, and where rules are interconnection patterns that cause the propagation and transformation of rythmic patterns of activation" (Shastri-Ajjanagadde (1990), p. 14).

Using such oscillatory patterns and phase-locking processes, one becomes able to embody dynamical bindings in *the fine temporal structure* of firing patterns in the brain.

This idea is akin to the works concerning the role of *fast* synapses in neural networks, that is the possibility for a neural network to change non-adiabatically its synaptic weights during its transient functioning.

We see that the way by which one can bind a role label with a filler term rises effectively some fundamental issues. But these are not central for taking up F-P's challenge. Indeed, the main problem is that of the *configurational* definition of roles which can substitute for the classical role *labels*. In such a configurational definition, roles are identified with *positions* — places — in configurations of positions. Of course, they have to be filled by fillers, but the key difficulty is to elaborate an effective CN theory of such positional relations *without taking for granted any prior CL representation of them.*

As was strongly stressed in the article "On Variable Binding and the Representation of Symbolic Structures in Connectionist Systems", CNC must

> "find ways of naturally instantiating the sources of power of symbolic computation within fully connectionist systems".

Such a CN instantiation is much more than a mere CN implementation of a symbolic stuff. It is an "extended version of connectionist computation".

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[19] See e.g. Edelman (1987) and Sporns, Tononi, Edelman (1991), von der Malsburg-Bienenstock (1986), Bienenstock (1992), Shastri-Ajjanagadde (1990). Synchronization of oscillatory neural groups is also used for modeling constituency relations (what is called the *labeling hypothesis*). This rises very technical mathematical problems. See e.g. the works of Yoshiki Kuramoto, Hiroaki Daido, George Bard Ermentrout and Nancy Kopell. There is now strong evidence concerning the possibility of explaining that way the cognitive Gestalt laws (see e.g. Engel *et al*. [1992]).

This is the core of the main problem because it is the "syntactic geometry" of the internal form of constituent structures which defines the semantic relations characterizing the roles. For solving it, it is not sufficient to binds noun fillers and case-frame slots of verbs, the roles being themselves tensorial product of semantic verb features and case-roles. We must be radical, and *instantiate* — and not only implement or represent — the concept of role in a purely dynamical way.

3. The core of the debate : the need for a configurational definition of the roles.

In their response to Smolensky's response, Jerry Fodor and Brian McLaughlin [20] reconsider the systematicity problem and the fact that:

> "cognitive processes are causally sensitive to the constituent structure of mental representations" (p. 185).

They summarize their main point claiming that:

> "all we really need is that propositions have internal structure, and that characteristically, the internal structure of complex mental representations corresponds, in the appropriate way, to the internal structure of the propositions that they express" (p. 187).

More precisely, they introduce a condition (C) which "expresses a *psychological law* that subsumes all systematic minds":

(C) "If a proposition *P* can be expressed in a system of mental representations *M*, then *M* contains some complex mental representation (a "mental sentence") *S*, such that *S* expresses *P* and the (classical) constituents of *S* express (or refer to) the element of *P*" (p. 187).

Condition (C) plus the fact

> "that mental processes have access to constituent-structure of mental representations"

allows to explain the cognitive systematicity of the mind.

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[20] Fodor-McLaughlin [1990].

Against this theoretical background, Fodor and McLaughlin can provide an evaluation of Smolensky's tensorial product device. Their main criticism is that it is impossible to retrieve from tensorial product representations and from abelian superposition operations a constituent-structure whose constituents are endowed with a *causal* status. Indeed, in a vector space the choice of a basis and hence of a vector decomposition is *not canonical*. Every vector decomposition is therefore *counterfactual* and the constituents (components) it generates *cannot* have causal efficiency.

We think that this negative argument is essentially right even if it is over-drastic. For instance, it is true that there exists no canonical basis in a vector space V (that is, V has a non-trivial symmetry group, the linear group GL(V)). But, nevertheless, the vector space V_R of the states of a network R does possess a distinguished basis, namely that defined by its units. In that case, vector decompositions are not counterfactual operations. But notwithstanding, F-M's criticism points out a major difficulty which can be expressed in the following manner.

We have seen that for Smolensky, the basic problem of a CN theory of symbolic structures is that of the *binding relations* between roles and fillers. He succeeded in solving this problem, but in a way which replies to only *half* of F-P's challenge. Indeed, it says nothing about the possibility of reaching in the CN framework a configurational definition of actantial roles. Moreover, it takes for granted a symbolic pre-definition of the roles. As was stressed by Yves-Marie Visetti (1990), in the tensorial product approach "the associative conception of memory as a relaxation to a preferential state" together with "the concept of attractor as an intrinsic meaningful state" disappear.[21]

Now, we have just seen that the problem is not only to represent semantic roles as local or distributed activity patterns of some appropriate network. It is also to give a correct CN account of the relations of actantial interaction which are involved in syntactic structures. *These relations are not binding relations*. They concern the roles independently of their fillers. The PTC agenda which, according to Smolensky,

> "consists in taking [the] cognitive principles and finding new ways to instantiate them in formal principles based on the mathematics of dynamical systems"[22]

must also be applied to the configurational definition of the actantial roles.

In some sense, it is such a requirement which is stressed by Fodor and McLaughlin when they claim that in order to build up a CN theory of constituency and systematicity one must :

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[21] Visetti [1990], p. 186.

[22] See Smolensky's contribution to COMPCOG I [1991].

(i) find “some property D, such that if a dynamical system has D its behavior is systematic”;
(ii) “make clear what property D is”; and
(iii) “show that D is a property that CN systems can have by law” (p. 201).

But suddenly, the authors become dogmatic. They state that such a requirement is impossible to satisfy. Even when Smolensky stresses the fact that constituent structures do exist in physics (for instance a molecule with its atomic nucleus and its electrons), they reply that

> “since *being a representation* isn’t a property in the domain of physical theory, the question whether mental representations have constituent-structure has no analog in physics” (p. 200).

Of course, it is then very easy for them to conclude

> “that Fodor and Pylyshyn’s challenge to connectionists has yet to be met. We still don’t have *even a suggestion* of how to account for systematicity within the assumptions of connectionist cognitive architecture” (p. 204).

This claim resumes some previous ones in the 1988 paper:

> "so far as we know, there are no worked out attempts in the Connectionist literature to deal with the syntactic and semantical issues raised by relations of real-constituency" (p. 22);

> "There doesn't seem to be any other way to get the force of structured symbols in a Connectionist architecture. Or, if there is, nobody has given any indication of how to do it" (p. 24);

> "There are no serious proposals for incorporating syntactic structure in Connectionist architectures" (p. 67).

However, these peremptory judgements are not at all true. Indeed, let us recall again that the concept of *bifurcation* of attractors provides an effective property D, and that it permits therefore to work out a CN attractor syntax and a dynamic theory of constituency. It is a rather technical stuff. But this is another story. Our purpose here was only to discuss the epistemological issue of dynamical constituency.

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