WHY CONNECTIONISM IS SUCH A GOOD THING
A CRITICISM OF FODOR AND PYLYSHYN'S CRITICISM OF SMOLENSKY[1]

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Introduction

In early 1988, Jerry Fodor and Zenon Pylyshyn published an article in *Cognition* entitled "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" which appeared in *The Behavioural and Brain Sciences*. A little later, in his talk entitled "Why connectionism is such a bad thing.", given at the Ecole Normale Supérieure in Paris, J. Fodor continued with his criticisms.

As the entire debate is quite fundamental to the future of connectionism (CX), we intend to appraise these arguments. Our own arguments will seek to show:

(i) that the criticism by Fodor and Pylyshyn is in effect essentially valid if it concerns an extremely basic form of CX ("weak CX"); but that

(ii) it is not at all acceptable for a more elaborate form of CX which fully employs the mathematical resources of the theory of dynamical systems ("strong CX").

I. *Connectionism and the theory of dynamical systems*

It is well known that one of the basic ideas of CX is that the automatisms of competence described at the symbolic level by discrete and serial structures (symbols, symbolic expressions, rules, inferences, etc.) are formal and macroscopic "kinematical" structures (qualitative, stable and invariant) *emerging* from microscopic "dynamics" of performance which must be described at a sub-symbolic level.[2] The classical symbolic cognitivism (CL) is analytic and constructivist. It favours logical automatisms, conscious rules, calculus, and deductive inference. The sub-symbolic cognitivism (CX), on the contrary, is synthetic and associationist. It favours dynamics of networks, intuitive performances, equilibrium positions and induction. Here, the entities possessing a meaning are, at the microdynamical level, global and complex activation patterns of elementary and meaningless local units, interconnected and functioning in parallel.

This fundamental idea of CX reinforces and elaborates an idea proposed some 20 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. Hence - a deep but somewhat shocking idea - *the mathematical theory of non-linear dynamical systems and of the bifurcations of their attractors* should take over from formal logic in cognitive linguistics. Syntactic and semantic structures are then considered as analogous to the processes we meet in physics under the name of *critical phenomena* and 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 of minimisation (i.e. optimisation) of a potential function H.[3] This will then be the bifurcation theory of the minima of potential functions, i.e. *the mathematical theory of the unfolding of singularities of differentiable mappings* which will serve in the modeling of semantic and syntactic structures (cf. Thom 1972, 1980, Zeeman 1977 and Petitot 1985a, 1985 b).

Instead of being describable with formal languages where the semantics is simple and the syntax is complex, the sub-symbolic systems and processes possess, on the contrary, a simple syntax and a complex semantics. The instantaneous global state of a network S of elementary interconnected units s is given by the activation vector of these units: $x = (x_s)_{s \in S}$, where x_s takes its values from a set of values V (finite or continuous). Let $E = \{x = (x_s)_{s \in S}\}$ be the configuration space of S. 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 neighbours, define an endomorphism $\sigma : E \rightarrow E$. σ associates the successive state $\sigma(x)$ with the instantaneous global state $x \in E$ of S. In general, it encodes a considerable amount of information. It is the *iteration* of σ which defines the *internal dynamics* of the network S (cf. Petitot 1977).The stable asymptotic states of σ (its attractors) are the *internal states* of S. Their internal structure - their topology - defines their semantics. If x is an input of S (an instantaneous initial state), its trajectory $(\sigma^k(x))_{k \in N}$ will tend, in general, towards an attractor A which will be the output (the response) of S defined by x. On varying the local transition laws - for example by changing the weights of the connections of S - σ_w is modified under the action of parameters w varying in a space W, called the *control space* or the *external space*. This allows us to define inferences which are bifurcations of attractors and learning processes which consist of moving w in W until σ_w associates pre-established internal states with pre-established initial data.

The dynamics σ will be gradient if it consists in minimising an "energy" function $H : E \rightarrow R$. The models so obtained (E, H_w, W) are then exactly those which elementary catastrophe theory introduced into syntax and semantics twenty years ago (cf. Thom 1972, 1980, Petitot 1979, 1982a, 1982b, 1989b, 1989d and Wildgen 1982).

In general, the cardinal of the set E_M of the absolute minima of H is very big and a statistical approach becomes indispensable. That is why the models of statistical thermodynamics intervene in so natural and decisive a manner.

Let us consider distributions of probabilities $P(x)$ over E. For a given mean energy C, i.e. for $\Sigma_{x \in E} P(x)H(x) = C$ the distribution $G_T(x)$ which maximises the entropy $S = - \Sigma P(x)\mathrm{Log}(P(x))$ is the Gibbs distribution $G_T(x) = (1/Z_T)\exp{-H(x)/T}$ where Z_T is the *partition* function $\Sigma_{x \in E} \exp{-H(x)/T}$. When the "temperature" $T \rightarrow 0$, G_T concentrates on the minima of H, i.e. on E_M. This is a well known and fundamental physical result. Starting from this property, it is possible to derive algorithms which allow one to minimize H. One of the best known is the "simulated annealing" algorithm. It involves the construction of sequences of random variables X_n and of temperatures T_n such that:

$$\lim_{n \rightarrow \infty} P(X_n = x) = G_t(x)$$
$$\lim_{n \rightarrow \infty} P(X_n \in E_M) = 1$$

More exactly, as it is explained by R. Azencott, we are given:
(i) An exploration matrix $Q = (q_{xy})_{x,y \in E}$ satisfying the condition that for all $x, y \in E$, 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 $V_x = \{y \mid q_{xy} > 0\}$ the set of neighbours of x, this condition means that two configurations x and y, are always connectable by a chain of neighbouring configurations.
(ii) A sequence T_n of "temperatures" such that $T_n \rightarrow 0$ (cooling schedule) and such that the decrease of T_n is "slow" (slow cooling). This latter condition is expressed by the fact that $\lim_{n \rightarrow \infty} T_n \mathrm{Log} n = R$ where R is a sufficiently big constant.
(iii) A Markov chain of random variables X_n on E such that:

$P_n(x,y) = P(X_{n+1} = y \mid X_n = x) = q_{xy}\exp((H(y)-H(x))^+/T_n)$
if $y = x$
$P_n(x,x) = 1 - \Sigma_{y \neq x} P_n\ (x,y)$

($a^+ = a$ if $a \geq 0$ and 0 if not).

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

A theorem of Hajek says that:

$\lim_{n \to \infty} P(X_n \in E_M) = 1$ if and only if:

$\Sigma_{n=1,\infty} \exp(D/T_n) = +\infty$ where $D = \mathrm{Sup}\{D(x) \mid x \in E_{LM}-E_M\}$

In his *harmony theory*, Paul Smolensky has applied these ideas to a certain number of sub-symbolic processes and has insisted on the importance of carrying over the (von Neumann) computer metaphor to a dynamical conception (in the sense of the theory of dynamical systems) of information processing (cf. Smolensky 1986). His aim was, as we know, 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 $\underline{r}$ of values of the r_i (± 1, for example). The cognitive system interprets its environment using "knowledge atoms". Each of these knowledge atoms α is characterised by a knowledge vector $\underline{k}_\alpha$ which attributes values to each feature r_i. It is or it is not activated (we introduce an activation variable a_α =0 or 1). The α encode sub-patterns of values of the features occurring in the environment. Their frequency is encoded in their force σ_α.
(ii) Let $(\underline{r},\underline{a})$ be the state of the cognitive system and K the knowledge base defined by k_α and σ_α. We define for example H_K by:

$H_K(\underline{r},\underline{a}) = \Sigma_\alpha\ \sigma_\alpha q_\alpha h_K(\underline{r},\underline{k}_\alpha)$

with $h_K(\underline{r},\underline{k}_\alpha) = \underline{r}.k_\alpha/|k_\alpha| - k$.
(iii) We now apply the general thermodynamical method mentioned above to this particular case. This allows us to interpret the visual scene (i.e. the vector $\underline{r}$) by completion, i.e. by optimising

the *global coherence* (the consistency) of the (local) partial interpretations α : for a given representational state $\underline{r}$ and for an activation state $\underline{a}$ 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 $\underline{r}$ and each weight σ_α multiplying the self-consistency between $\underline{r}$ and α (i.e. the similarity between $\underline{r}$ and $\underline{k}_\alpha$).
(iv) This inference and decision process is then assimilated with the result of a parallel and distributed stochastic process driven by H_k.

II. *The Epistemology of a Morphodynamical Approach to Cognition*

The development of a dynamical conception - or rather, of a *morpho*-dynamical one, for we are trying to understand the emergence of syntactic-semantical *forms* - of performance raises a number of epistemological questions. In Petitot 1979, 1982a, 1985a, 1987, 1988 these are developed in detail. The most important feature of this conception consists of a radical questioning of the logico-combinatory formalist point of view on perception and language (and in general on cognition).

In a formalist conception which reduces syntax to the formal description of automatisms of competence (cf. for example the Chomskyan transformational and generative conception), all the levels of description are of the *same* formal type. Consequently, in the regression toward deep structures one ends up with abstract primitive structures which are of the same formal type as the surface structures (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 underlying dynamical mechanisms which would be those of performance. Now, the structures of perception, as well as the dynamical mechanisms of performance, undoubtedly impose certain *universal* constraints on grammatical structures. If one does not take them into account, one is forced to interpret these constraints directly in terms of genetic determinations. That is why, since 1975, we have been led to question the "evidence" according to which our ignorance of the physical foundations of mental structures forces an abstract characterisation of them. For the quick conclusion is then that structural properties of language which cannot be derived from such an abstract characterisation should be explained in innatist terms. It seemed to us, on the contrary (Petitot 1979), that the "good syllogism" was the following: (a) we do not as yet know the physical (particularly neurophysiological) bases of language; (b) but we can nonetheless posit them and thus assume the existence of dynamical processes of performance, processes from which the formal and abstract kinematical structures of competence emerge; (c) grammars formalise only certain aspects of these emerging formal structures; (d) there exist other heterogeneous aspects, coupled with perception, and which impose additional cognitive constraints on the "humanly accessible" grammars. It is such a strategy that has been allowed by the development of Thom's topological syntax in which a meaning is assimilable to the *topology* of an *attractor* of the dynamics of a neuronal network and the syntagmatic trees are trees of *bifurcations* of these attractors into sub-attractors.

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

(i) The CX 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 it concerns the structure, the architecture and the dynamical behaviour of the cognitive processes themselves.

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

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

(iv) Whereas in the symbolic formal models, all the processing levels are of the same type, in the sub-symbolic semantic models there is 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 arrive at a unified conception of cognition, it is necessary to combine the CX 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 m.sec.) determine the contents of consciousness".

(vi) As far as the linguistic rules are concerned, this presupposes in particular *the possibility of representing sub-symbolically and sub-conceptually in CX dynamical models the propositional structures of language*. This is certainly very difficult, but it is necessary. 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, the criticism of Fodor and Pylyshyn centers on this very point.

(vii) The *mathematical* universe of CX 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 sub-symbolic computation is *continuous, geometrical and differential*. Inference here is a statistical inference which optimises 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 CX dynamical systems are suited for the elaboration of a theory of schemata, of prototypicality and of categorisation. (This point has been already well-elaborated in the Thomian morphodynamical approach: cf. Petitot 1983a, 1985b, 1989a).

III. *Exposition and critique of the Fodor and Pylyshyn argument against connectionism*

1. *The general structure of the F-P argument (Fodor and Pylyshyn 1988).*

A.1. Both the CL (classical) cognitivists and the CX cognitivists, being representationalists, admit mental states encoding the properties of the external world. The CL/CX conflict is thus internal to cognitivism. It bears on a precise issue: "the architecture of representational states and processes". The CX is thus submitted to the imperative of showing that it can provide a good theory of cognitive architecture, that is, of "processes which operate on the *representational state* of an organism".

A.2. Now, there is a fundamental difference between the CL and CX paradigms. The 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 the mental representations have a combinatorial syntax and semantics, and

(ii) that the mental processes are dependent on this structure ("structure sensitive"): the operations operate on the mental representations as a function of their combinatorial structure, i.e. of their *form*.

On the contrary, according to the authors, the CX would assign semantic content only to holistic entities, without internal combinatory structure (labelled nodes symbolising the activity patterns of the network). Further, they would only admit causal relations as relations between these semantically evaluated entities. In brief, according to the authors, only the CL "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". Contrary to the CX, the CL insist that the computational operations proceed from the syntactic structure of complex symbols and that, to the extent that the syntactic relations are parallel to the semantic relations, "it may be possible to construct a syntactically driven machine whose state transitions satisfy *semantical* criteria of coherence". That is "the foundational hypothesis of Classical cognitive science".

A.3. The limits (according to the authors) of the CX are quite dramatic since the mental representations *must* possess an internal syntactico-semantical combinatorial structure. Such a constituent-structure is, in fact, necessary to explain four fundamental aspects of cognition.

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

(ii) *Systematicity*. Even if we question productivity and generativity of the cognitive capacities, we cannot reasonably put into question their systematicity, that is, the intrinsic links that relate the comprehension and production of certain expressions with those of certain other expressions. This is explicable only if there exists an internal structure of expressions allowing us to define well-formedness rules and to structurally relate various expressions.

(iii) *Compositionality*. There are *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 brief, if we accept an internal structure of representations then we can evidently speak of representations of the same structure, of *similar* structures, or of structures which are *related* to each other in different way. Now, according to the authors, an essential feature of CX would be the refusal of such a structure. For the CX, the cognitive systems are systems "that can exhibit intelligent behaviour without storing, retrieving, or otherwise operating on structured symbolic expressions". Certainly, the labels which mark the semantically evaluable holistic entities, 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". The CX 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 the mental representations are *distributed* over micro-features (derived by learning and extracted by multivariational analysis from the statistical properties of the stimuli samples) *does not* imply that these representations are structured. In effect, "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 CX, its "major misfortune", is in having 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 containing only 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". CX presupposes the systematic organisation 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 unquestionable 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, the CX's main criticism against the CL is not acceptable. It affirms that, for the CL, the behavioral regularities must come from explicitly encoded rules. But this is false. For the CL, numerous functions can be encoded implicitly (for example, as part of the hardware). What should be explicit are only the structures of data that the cognitive machines transform and not the rules (the grammar) of transformations.

A.6. As a consequence of all that, the CX should be rejected as a *cognitive* theory. It originates from a "bad" psychology, of an associationist nature, against which one can go on repeating the well-known traditional rationalist criticisms formulated since Kant.

A.7. The authors then conclude that the really exclusive interest of the CX 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 the CX bear on the limitations imposed on competence by the concrete constraints of performance. The material finitude of performance is the result, according to them, of an interaction between an unlimited formal competence (unlimited but finitely describable by generative rules (cf. A.3.(i)) on one side and the limited resources on the other. And, from a perspective radically opposed to the emergentist one of the CX they separate the functional architecture (the algorithms) from implementation. This is, for them, a "principled distinction". The implementation models (micro-level) are neutral with regard to the nature of cognitive processes (macro-level) and to deny this fact is to confuse structure and function with each other. It is to confuse the physical and the functional, and derive, for example, (i) from the undoubted existence of neuronal networks an associationist psychology (networks of representations), or (ii) from the not less certain anatomic distributivity of neurons 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 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) a physical localisation in instanciation. The "brain style" of the CX is quite definitely an epistemological error: "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 onto the cognitive, and in doing so, reactivates "the worst of Hume and Berkeley".

A.8. Thus, the CX "may provide an account of the neural (or 'abstract neurological') structures in which classical cognitive architecture is implemented". The symbolic structures of the CL cognitivism are physical. They are neurally encoded and instantiated and it is the physical properties instantiating them which give rise to the operational behaviour of the cognitive system. The CX arguments become valid if we interpret them as arguments in favour of a physical implementation in massively parallel networks. For example, the fact that the cognitive processes are fast, whereas the speed of the neuronal phenomena is slow, or the fact that a considerable number 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 are lending themselves for a CX implementation of the algorithms of the CL functional architecture. But if the CX models are rather to be seen as a theory of implementation, they should then give up all cognitive pretensions. They should in particular refuse to assign "a representational content to the units (and/or aggregates) that they postulate".

A.9. An argument which is not made very explicit by the authors is that "structural" necessarily means "formal-symbolic". If mental representations possess a combinatorial syntax and semantics then they are ipso facto "symbol systems". As we shall 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.*

The arguments of Fodor and Pylyshyn that we have summarized are well constructed and apparently forceful. However, we can question their real validity at different levels.

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

C2. The presentation and characterization given in A.2. and A.4. are caricatures. What is demonstrated is only the following "syllogism":
(i) a "good" CX cognitivism should be able to develop the structural hypothesis;
(ii) for intrinsic reasons, the caricature of a CX defined in A.2. and A.4. does not possess this capacity;
(iii) thus this caricature CX is "bad" as a cognitive theory.
But nothing here proves that the caricature CX can be identified with the true CX in its full theoretical power.

Let us continue to call the CX cognitivism a dynamical one - distinguishing it from a symbolic one. The central question is the following : *just as it is possible, starting from appropriate formal theories, to develop a symbolic structuralism, is it also possible, starting from the mathematical theories of dynamical systems, to develop a dynamical structuralism?* If we see the CX models as graphs of causal relations between holistic units without internal structure, then the response is trivially negative. *But these elementary models are only a 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, we can elaborate an authentic dynamical structuralism, this would not lead us to transform the CL/CX opposition into a Manichean alternative. There is certainly a higher level of functioning of the cognitive system which is symbolic in nature. But

this does not mean necessarily that there do not exist lower levels which are of a dynamical nature. The associationist psychology is certainly not sufficient, but we need not reject it totally for that reason. Logico-symbolic superstructures can very well possess associationist infrastructures. The question is not whether the CX should replace CL (or if the latter should excommunicate the former), but to find out whether *the structural hypothesis can or cannot be elaborated at the dynamical level of cognitive processes.* Such a dynamical structuralism should be clearly distinguished from the formal symbolic one. But it must be:
(i) an authentic structuralism,
(ii) a *proto*-symbolic one, i.e., one which is compatible with the symbolic level.
If one still wants to criticize it thereafter, one must develop a more refined argument (cf. C6).

C4. Here we find again the epistemological problems mentioned in II above. Fodor and Pylyshyn are deeply unaware, it seems, of the real 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 the really central issue in the point of view they are attacking. However, the physico-thermo- dynamics parallel that 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. 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 latter are thus independent of their "implementation". The very problem is to understand how an *emergent* macro-level - thus being *non*-independent - can nonetheless have a certain *autonomy* of structure. That two levels are of different types *does not* mean that they are independent and "neutral" in relation to each other. To assert this is to seriously underestimate the epistemology and ontology of emergence. The CX are thus correct when they distinguish the problem of implementation from the *intra-cognitive* problem of the emergence of a symbolic level from a dynamical sub-symbolic level. But we repeat that the CL are right when they assert that this dynamical level, in order to be considered cognitive, should be *structural.*

C5. The point mentioned above is the crux of the problem. The authors accuse the CX of assuming the systematic organisation of cognition without explaining it (A.4.). But in part, the same argument can be turned on 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 the levels (as we have observed) and by autonomising 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, then the argument is trivial : the CX is not symbolic (by definition), "hence" it is not structural, "hence" it cannot account for the structural character of cognitive processes. In fact, the true problem is as follows. 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 have an explanation, we must :
(i) model the semantically evaluable entities by - perhaps very sophisticated - *mathematical* structures of a certain type, i.e. belonging to a certain mathematical universe;
(ii) show that a theory of structures can be developed within this universe.
The question of cognitivist CX then becomes (cf. C.2 and C.3): *if certain semantically evaluable entities are modeled by the attractors of dynamical systems $\sigma_w : E \to E$, can we or can we not, within the framework of the theory of dynamical systems, develop a theory of structure?*

C6. If the classical cognitivists are satisfied with a formal symbolic description, it is because for them the explanation of the cognitive structures must be innatist in nature. Behind the controversy CL/CX and the conflict of arguments, behind the rationalist critique of empiricist associationism, there lies, in fact, an epistemological alternative. This was brilliantly described by Massimo Piattelli-Palmarini in his text "Evolution, Selection and Cognition: from 'Learning' to Parameter Fixation in Biology and in the Study of Mind". The argument is as follows. In all the biological domains (Darwinism, neo-Darwinism, immunology, etc.) one has progressed from *instructivist* theories ('Lamarckian') to *selective* theories. 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 taken for mechanisms of learning. This internal selection involves filtering and fixation of parameters which selectively stabilize certain possibilities amidst 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", i.e. 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 that an organism assimilates 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 this 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. The example of casual semantic roles[5], central in Fodor and Pylyshyn's argument (cf. §3), demonstrates this well. Hence the radical critique undertaken by Chomsky, Fodor and their colleagues against empiricist theories of learning by imitation, association, assimilation, induction, problem-solving, etc. Everything seems to indicate that there is a rich and subtle 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, the genetic constraints being contingent, the formal description can amount to an explanation. This is the dogma that we are criticizing. For, we repeat, these arguments seem pertinent to us only at the symbolic level. They do not imply that the innate symbolic form of the cognitive system exhausts its structure. It is perfectly legitimate to assume :

(i) that there is 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 apply the epistemological and methodological remarks formulated above to the central argument of Fodor and Pylyshyn. They consider the manner in which certain CX (Hinton, McClelland, Rumelhart) have treated a sentence like "John loves Mary". The problem is, evidently, that of the *actantial relations*[6] i.e. of "the role relations that traditionally get coded by constituent-structure". The CX mentioned above represent them by a set of activated units { + 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. For them, these descriptors combine an identity (an actant J,M) and an actantial role (S,O) and allow the representation of 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 series of inescapable difficulties which can be resolved 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". "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 retrace the initial structures. The superposition leads to an irreversible destructuring. 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 to be in a relation of "construction" –

that is to say to be related by dependence relations –, the 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*".

4. *Refutation of the main point of the F–P argument.*

R.1. The central F–P argument is valid as long as it is applied to a *weak* form of CX. We call weak CX a CX which models *uniformly* semantically evaluable entities of different syntactic types by mathematical structures of the *same* type (attractors of dynamical systems implemented in formal neural networks), *without taking into account their different grammatical categories*. We call *strong* CX a CX which has the capacity to model the differences and the relations between different grammatical categories. The question then becomes: *can a strong CX be elaborated by means of the mathematical theory of dynamical systems*? It refines the formulations given in C.2., C.3. and C.5.

R.2. Let us clarify this further. The F–P. argument denounces *a category mistake*. Its syllogism is as follows:

S1. (i) Let A_i ($i = 1,\ldots,n$) be the actants of a sentence and V the verb organising the actantial relations. Let us *model* the actants A_i by means of mathematical structures G_i of a certain type (for example by activity patterns) on which is defined an additive (associative, commutative, with neutral element and inverse elements) operation of composition +, i.e. an abelian group law (for example, the superposition of activity patterns).
(ii) Let us model the verb V by a structure F of the *same* type as that of the G_i.
(iii) Model the actantial interaction V of the A_i by the sum $+ F +_{i=1,n} G_i$.
(iv) Observation: this modeling leads to certain inescapable difficulties.
(v) Conclusion: the modeling of the actants A_i by the structures G_i should be rejected since it is experimentally refutable.

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

S2. (i) Let us symbolise the actants A_i by symbols A^*_i.
(ii) Symbolise the verb V by a symbol V^*.

(iii) Symbolise the actantial interaction V of the A_i by syntagmatic relations between the A^*_i and V^* (for example, by a syntagmatic tree of a generative grammar or of a constituent-structure grammar).
(iv) Observation: the symbolisation is good.
(v) Conclusion: it is to be accepted since it is validated experimentally.

The problem is that the first of these syllogisms (S1) is fallacious and the second one (S2) is tautological.

S1 is fallacious. In fact, it is equivalent to the triviality that the (logico-combinatorial) structures of the syntagmatic-tree type, being non-associative and non-commutative, are effectively neither associative nor commutative and cannot thus be modeled by algebraic structures of group type. Let us reproduce the argument for another theory, for example a physical theory (ultra-simple, so fictional) of elementary particles (e.p.).
(i) 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) Model the concept of interaction by another irreducible representation F.
(iii) Model the interaction of the P_i by the sum $+ F +_{i=1,n} G_i$.
(iv) Observation: this modeling is experimentally refutable.
(v) Conclusion: the modeling of the P_i by the G_i should be rejected.
In this 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. I. (iv) is trivial since the interaction of the P_i is not the same thing as the system of the free P_i to which I has been added. The inference (iv) -> (v) is completely illegitimate. It is the same with the F-P argument. The argument also denounces a category mistake: an interaction of actants is modeled by a mathematical structure F of the same type as those which serve to model the actants themselves. Fodor and Pylyshyn are then right in denouncing such a crass error in weak CX.

S2 is tautological. It is clear that if one symbolises constituents by means of formal symbols, then one can symbolise their structural relations by means of formal relations.

R.3. It is thus necessary to clarify and work out F-P's central argument. This can be done in the following manner.
(i) First of all we must gauge the *radical distance that separates a mathematical modeling from a formal symbolisation.* Modeling a certain class of natural phenomena is a matter of interpreting them by sophisticated mathematical theories which allow us to

mathematically reconstruct their properties; whereas to symbolize them, in contrast, is a matter of *formally representing* such properties. The requirement of modeling has nothing to do with symbolisation: for example a mathematical physics of elementary particle interactions has nothing to do with symbolic representations of the type $I(P_i)$ where I is an n–ary relation. The limitation of the symbolic–formalist point of view is its confusing a description by formal symbolisation with an explanation by mathematical modeling (cf. Petitot, 1982a, 1982b, 1985a, 1986b, 1986c). We now see the consequences of the argument A.9 criticized in C.5. above.

(ii) Up to now, the CX constitutes the most decisive attempt to move from a formal symbolisation to a mathematical modeling in cognitive sciences. Then, though it aims to provide an explanation only of the *proto*-symbolic structural phenomena, the lack of formalism cannot be attributed to it.

(iii) In order to refute the F–P argument, the first question to be answered is whether, in the case of syntactic structures, there are two authentically structural levels which do correspond to the dynamical and symbolic levels respectively. In a number of works (see bibliography), I have tried to show that such is the case. Underlying the strictly grammatical level of grammatical relations which are adequately describable 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.

(iv) Now, we notice that the F–P argument is *neutral* with regard to this difference of levels. It has to do in fact with the actantial roles *and only refers to the "geometry" of structures* where "the geometric relations are themselves semantically significant". Thus it is legitimate to apply the observations regarding actantial syntax elaborated in R2. Whence the question: *If the actants A_i of a process are modeled by attractors G_i of a dynamical system, is it then possible, within the framework of the theory of dynamical systems, to elaborate a theory of actantial interactions, that is, in fact, a theory of the verb?*

(v) Let us develop further this question. As in section I above, let us assume that the dynamics are of the gradient type and let us therefore replace the attractors G_i by the minima m_i of a potential function f.

QUESTION : *If the actants A_i of a process are modeled by the minima m_i of a potential function f, is it then possible, within the framework of the dynamical theory of potential functions, to* *elaborate a theory of actantial interactions, that is, in fact, a theory of the verb?*

The response given by P. Smolensky in his replies to Fodor and Pylyshyn is insufficient. It has only to do with semantics (componential analysis) and not with syntax (constituent-structure). Actually, we need another type of response.[7]

IV. *Elements of a dynamical structuralism for connectionism*

Not only is the response to the question positive, but it was given already 20 years ago by René Thom, and has been developed in a very detailed manner in Thom 1972, 1980, Petitot 1982a, 1985a, 1988, 1989b, 1989c, 1989d, Wildgen 1982. We shall restrict ourselves here to a very brief summary of these works.

As already mentioned, it was René Thom who was the first to elaborate a *topological actantial syntax* based on the idea that the minima of potential functions could be interpreted as the *actants* of a process. From there, he went on to show how a theory of the verb could be developed, by considering a verb as an *interaction* between actants. The basic idea is to embed the potentials f in families f_w parametrized by control parameters w varying in an *external space* W.[8] The f_w are, therefore, deformations of f. In other words, we consider the product $E \times W$ of the internal space E by the external space W as a fibration $\pi: E \times W \rightarrow W$ over the external space W, and we consider the "vertical" potentials $f(x,w) = f_w(x)$. The CX models have also made use of this idea, but only for the theory of learning (W is then a space of parameters which vary very slowly the synaptic weights of the connections of a network). Here, it is used in a completely different manner: for modeling the syntactic categorial difference between actant and verb, we introduce connectionist networks $f_w: E \rightarrow E$ fibered over a base space W.

Let $(f_w)_{w \in W}$ be a deformation of potentials. We assume that we can define the notion of *qualitative type* of f_w. In the differentiable case f and g are qualitatively equivalent if they are conjugated by diffeomorphisms of their source and their goal:

$$\begin{array}{ccc} E & \xrightarrow{f} & R \\ h \downarrow & & \downarrow h' \\ E & \xrightarrow[g]{} & R \end{array} \qquad g = h' \circ f \circ h^{-1}$$

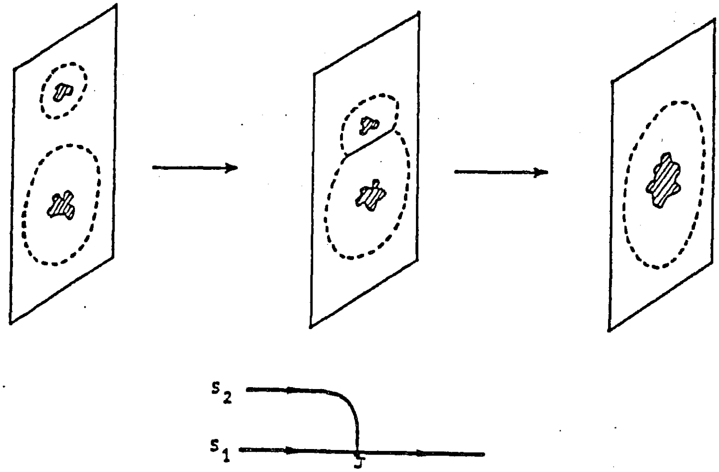

Figure 1. A trivial example of an actantial graph: the capture of S_2 by S_1.

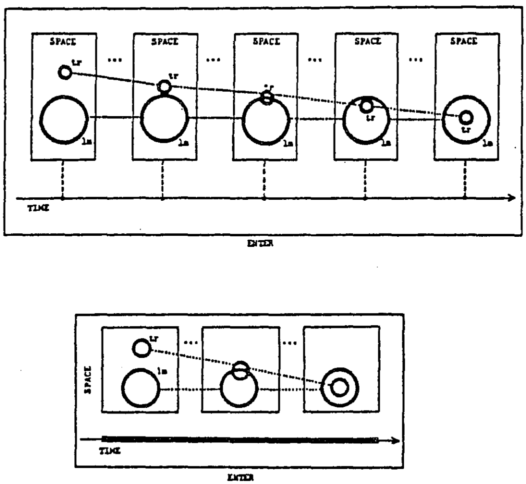

Figure 2. Langacker's representation of "enter" in cognitive grammar (cf. Langacker 1987).

One says that $w \in W$ is a *regular value* of the control if for all w' close enough to w, $f_{w3} = f_w$. This means that f_w is a *structurally stable* potential. Let U be the set of regular values. By definition, U is an *open set* of W. Let K be the complementary closed set $K = W - U$. K is the set of *singular values* of the control. One can show that in the good cases, K has a "good" geometry (called a stratification) which partitions W into regular domains.

Consider now, in the external space W, a *directed path* g crossing K in a singular point s, and let us interpret g *temporally*. When w moves along g, f_w models a process. "Before" the crossing of s, f_w defines by its minima a certain configuration of relations between actants. "After" the crossing of s, f_w defines another configuration of relations between these actants, since the *qualitative* type of f_w has been transformed. At the crossing of s, there is a *transition* of configuration, that is to say an actantial *interaction*, an *event* involving a change of the actantial relations.

Let us consider, for example, the simple case of an interaction where an actant S_1 captures another actant S_2, and suppose that the process is a spatio-temporal process *perceptually* given (as for example "the balloon enters the room", "the cat enters the bag"). As suggested by Thom, and, more recently, also by Marr or Langacker, we shall reduce the actants to topological balls or blobs, i.e. to spatial domains with a delimited, perceptually salient, boundary. These topological actants maintain between each other topological (Gestalt-like) relations of separation, of localisation in the same neighbourhood, and of inclusion. When time varies, these topological relations are deformed and the topological domains enter into interaction (cf. Fig. 1)[9].

This is expressed by Langacker in the following manner (cf. Fig. 2)

By a technique of *contour diffusion* (well-known in mathematics and physics and also in visual psychology, cf. Blum 1973, Psotka 1978 and Koenderink and Doorn 1986), it is easy to associate with a configuration S_1/S_2 a potential G for which the boundaries of S_1 and S_2 are the two connected components of a same level section (cf. Petitot 1989c). It is G which expresses the *holistic cohesion* of the Gestalt S_1–S_2 (cf. Fig. 3).

When the time t varies, G is deformed. The family G_t represents the process of capture as a deformation of potentials. But it is easy to see that the *syntactic* information encoded in G is preserved if we reduce the topological domains to *points*, and even if G is reduced to a potential defined on an internal space of dimension 1 (cf. Fig. 4 and 5).

We thus obtain the *actantial graph* of capture, which is an

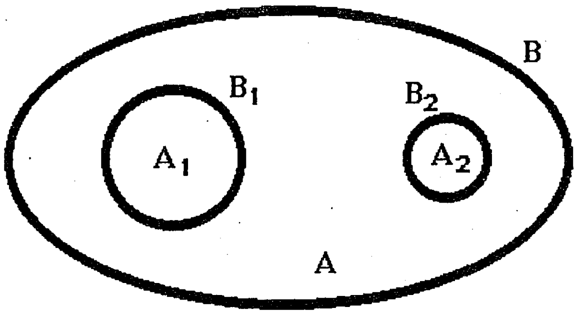

FIGURE 3-a. A configuration of two actants S_1 and S_2 with boundaries B_1 and B_2 in a domain with global boundary B.

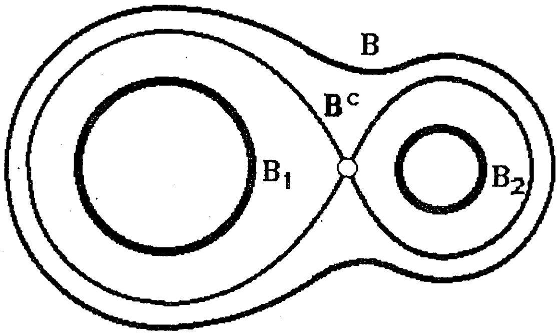

Figure 3-b. The contour diffusion process between B_1+B_2 and B. Notice the critical level section B^c with its critical saddle point.

archetype of the topological syntax. This archetype is derivable from what is called in singularity theory *a universal unfolding*. It schematizes the *topological semantics* of the verb "enter" (cf. Fig. 6).

The theory of universal unfoldings of singularities of potential functions allows for the generation of a number of other archetypes and the development of a topological analysis of the verbal semantic content. "By interpreting the stable local [minima] as actants, it is possible to give the qualitative structure of the catastrophes [conflicts and bifurcations of minima] a seman-

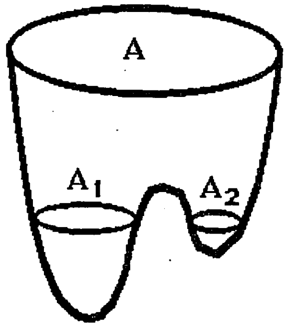

Figure 3-c. The generating potential of the contour diffusion process 3-b.

tic interpretation, expressed in ordinary language. We thus obtain what I consider as the universal structural table which contains all the types of elementary sentences" (Thom 1980: 188). These *Thomian schemata of actantial interactions satisfy all structural properties required by Fodor and Pylyshyn.*

To link up these perceptual situations with general semantics, we can make use of what is called in linguistics the *localist hypothesis*, according to which the spatio-temporal interactions between spatio-temporal actants have served as a matrix, in the course of the evolution of language, for the actantial relations in general (cf. Hjelmslev 1935 and Petitot 1979, 1985a, 1989c). This hypothesis has also been advocated recently by R. Jackendoff, R. Langacker and G. Lakoff.

The brief observations above, when developed technically, show that, it is effectively possible to construct a topological theory of syntax - of actantial relations and of verbal valence - within the framework of the theory of dynamical systems (theory of bifurcation of attractors), and particularly within the framework of the theory of gradient systems (theory of universal unfoldings of singularities of potential functions). This solves, in principle, the problem set down by Fodor and Pylyshyn, and refutes their main argument to the extent that we are able to construct CX visual models for the distribution of topological balls in space.

To sum up: the F-P argument is definitively valid only for weak CX. A strong CX *can* be developed. But to realize that it is not enough to model the semantically evaluable entities by

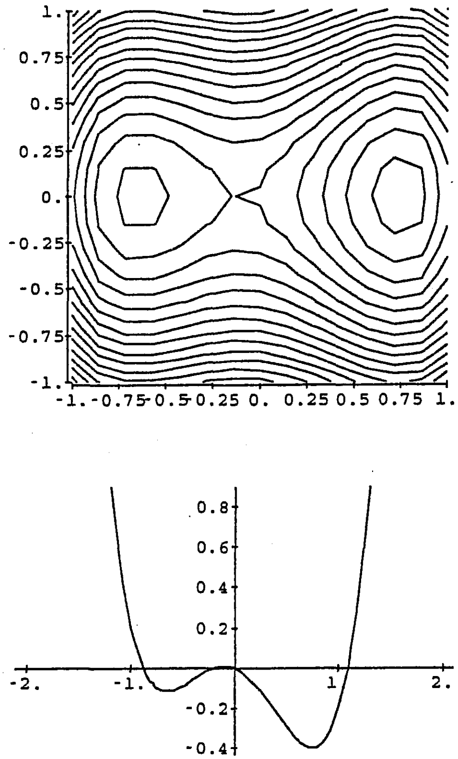

Figure 3-d. The level sections of an algebraic form of the generating potential 3-c: $f(x,y) = x^4+y^2-x^2-0.2x$ (the graph of the one-dimensional potential $x^4-x^2-0.2x$ is also shown).

attractors of dynamical systems. We must also employ the theory of bifurcations and conflicts of these attractors. This theory (which constitutes an important aspect of contemporary qualitative dynamics) allows, among other things, the modeling of the categorial difference between the actants and the verb.

Conclusion

The following assertions of Fodor and Pylyshyn are not scientifically acceptable : "so far as we know, there are no worked-out

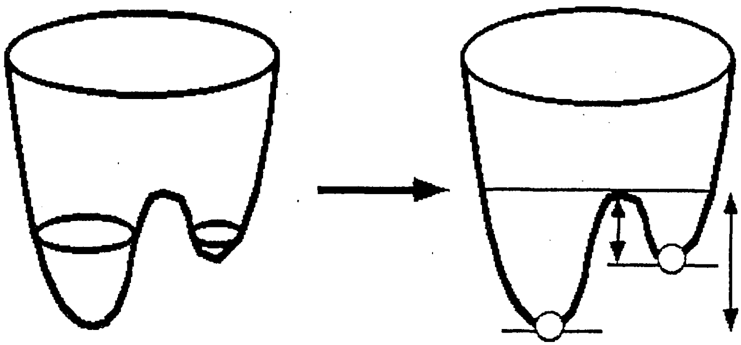

Figure 4. The "syntactic" equivalence between the generating potential of Fig. 3-c and the complete potential where the actants are coded by the minima.

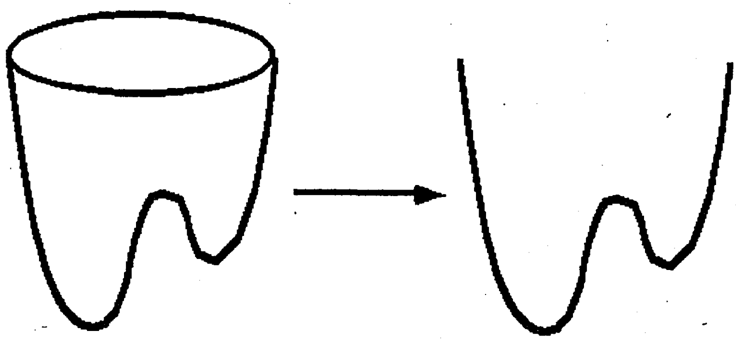

Figure 5. The "syntactic" equivalence between the potential of the figure 4 and the potential defined over a one-dimensional space.

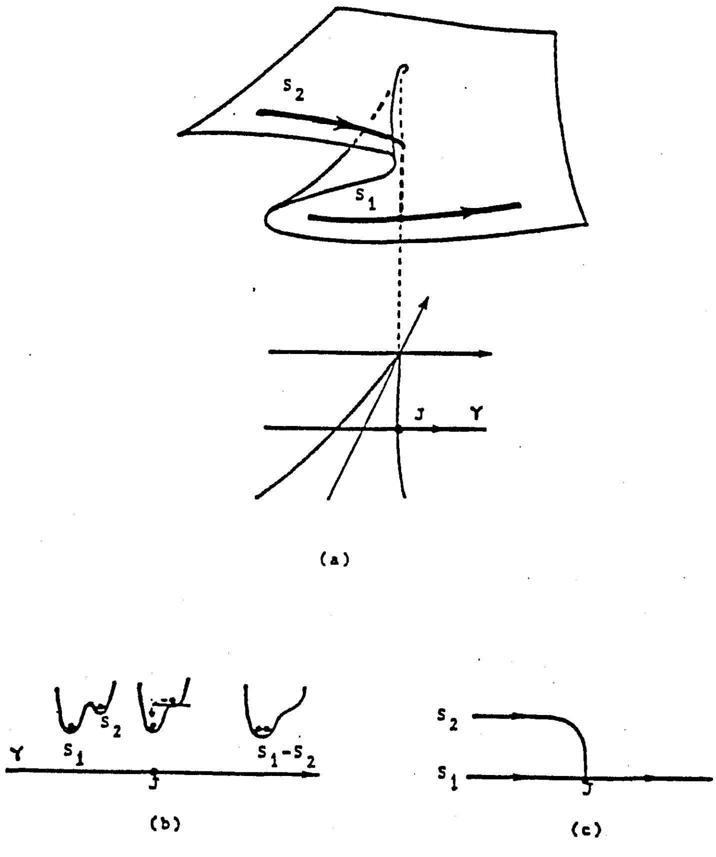

Figure 6. The actantial graph of capture can be generated by the cusp catastrophe.
(a) The path γ in the external space W.
(b) The temporal evolution of the actants (minima of f_W).
(c) The corresponding actantial graph.

attempts in the Connectionist literature to deal with the syntactic and semantical issues raised by relations of real-constituency"; "there doesn't seem to be any other way [than what is sketched in III.3 above] 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"; "there are no serious proposals for incorporating syntactic structure in Connectionist architecture".

Not only have such "worked-out attempts" and "serious proposals" existed for a long time already. They are, moreover, naturally derived from the mathematical universe chosen by the CX modeling. That is why connectionism is such a "good" thing.

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NOTES

1. This paper summarizes certain reflections of 1988 published in Petitot 1989c. It was presented at the Workshop "Connectionism and Language" held at the *International Center for Semiotic and Cognitive Studies* of the University of San Marino in October 1989. My thanks are due to Franson Manjali for having prepared the English translation. I thank also Gertrudis Van de Vijver and Barry Smith for having suggested relevant corrections.
2. We use the opposition between "competence" and "performance" in its classical Chomskian sense.
3. If X is a dynamical system (i.e. 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 and their topology may be 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) in 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, in B(A)-A, the dynamics is of simple type. It minimizes a sort of energy function.
4. For a philosophical criticism of this dogma, see Mulligan et al. 1984.
5. "Casual" in the sense of Case grammars.
6. We use the gallicisms "actant" and "actantial" to denote the

semantic roles of a case grammar linguistic description of a state of affairs. Actantiality is a fundamental concept of european linguistic traditions.

7. For current developments (June 1990) in the debate, cf. Fodor-McLaughlin 1990, Visetti 1990 and Andler 1990.
8. The potentials f are real-valued smooth functions $f:E \to R$, which are defined on a differentiable (i.e. a smooth) manifold E called the *internal space*. In the product space E×R, the graph of f - which is the subset $\{(x,y) \mid y=f(x)\}$ of E×R - is like a "landscape" above E, the value f(x) of f at x being the height of the "landscape" above x. At the qualitative level, what is essential are the *critical points* of f and their critical values. A point x of E is critical for f if gradient(f)=0 at x. When they are not *degenerate* (i.e. when they are not a fusion of many simpler critical points) critical points are minima, maxima or saddle points.
9. For more details, see Petitot forthcoming A and B.

BIBLIOGRAPHY

Andler, D. (1990). Connexionnisme et cognition: à la recherche des bonnes questions. *SC* 1990, 95-127.

Blum, H. (1973). Biological Shape and Visual Science. *Journal of Theoretical Biology*, 38, 205-287.

Fodor, J., Pylyshyn, Z. (1988). Connectionism and Cognitive architecture: A critical analysis. *Cognition*, 28, 1/2, 3-71.

Fodor, J., McLaughlin, B.P. (1990). Connectionism and the problem of systematicity : Why Smolensky's solution doesn't work. *Cognition*, 35, 183-204.

Hjelmslev, L. (1935). *La catégorie des cas*. Wilhelm Fink Verlag: München [1972].

Jackendoff, R. (1983). *Semantics and Cognition*. MIT Press: Cambridge.

Jackendoff, R. (1987). *Consciousness and the Computational Mind*. MIT Press : Cambridge.

Koenderink, J.J., van Doorn, A.J. (1986). Dynamic Shape. *Biological Cybernetics*, 53, 6, 383-396.

Langacker, R., (1987). *Foundations of Cognitive Grammar*. Stanford University Press.

LTC (1989). *Logos et Théorie des Catastrophes* (J. Petitot ed.). Patino: Genève.

MC. (1990). Modèles connexionnistes,(D. Memmi and Y.M. Visetti, eds.). *Intellectica*, 9-10.

Mulligan, K., Simons, P. M., Smith, B. (1984). Truth-Makers.

Philosophy and Phenomenological Research, 44, 287-321.

PDP (1986). *Parallel Distributed Processing* (D.E. Rumelhart, J.L. McClelland eds.). MIT Press : Cambridge.

Petitot, J. (1977). Centrato/Acentrato. *Enciclopedia Einaudi*. Einaudi: Torino. II, 894-954.

Petitot, J. (1979). Hypothèse localiste et théorie des catastrophes. *TLTA* (1979) 516-524.

Petitot, J. (1982 a). *Pour un Schématisme de la Structure*. Thesis, 4 vol.. Ecole des Hautes Etudes en Sciences Sociales : Paris.

Petitot, J. (1982 b). Sur la signification linguistique de la théorie des catastrophes. *Mathématiques et Sciences humaines*, 79, 37-74. Ecole des Hautes Etudes en Sciences Sociales : Paris.

Petitot, J. (1983 a). Paradigme catastrophique et Perception catégorielle. *Recherches Sémiotiques*, (RS/SI), 3, (3), 207-245.

Petitot, J. (1983b). Théorie des catastrophes et Structures sémio-narratives. *Actes Sémiotiques*, V, 47/48, 5-37.

Petitot, J . (1985 a). *Morphogenèse du Sens I*. Presses Universitaires de France : Paris.

Petitot, J. (1985 b). *Les Catastrophes de la Parole*. De Roman Jakobson à René Thom. Maloine : Paris.

Petitot, J. (1986 a). Epistémologie des Phénomènes critiques. *CAMS Reports*. Ecole des Hautes Etudes en Sciences Sociales: Paris.

Petitot, J. (1986 b). Thèses pour une objectivité sémiotique. *Sémiologie et Sciences exactes* , 42/43, g1- g23. Degrés: Bruxelles.

Petitot, J. (1986 c). Structure. *Encyclopedic Dictionary of Semiotics*, (Th. Sebeok, ed.), 2, 991-1022. Walter de Gruyter: New-York.

Petitot, J. (1987). Sur le réalisme ontologique des universaux sémio-linguistiques. Sémiotique en jeu (M. Arrivé, J.C. Coquet eds.) 43-63. Hadès-Benjamins : Paris-Amsterdam.

Petitot, J. (1988). Eléments de dynamique modale. Poetica et Analytica, 6, 44-79. Aarhus University.

Petitot, J. (1989 a). Morphodynamics and the Categorical Perception of Phonological Units, *Theoretical Linguistics*, 15, 1/2, 25-71.

Petitot, J. (1989 b). On the Linguistic Import of Catastrophe Theory. *Semiotica*, 74, 3/4, 179-209.

Petitot, J. (1989 c). Hypothèse localiste, Modèles morphodynamiques et Théories cognitives : Remarques sur une note de 1975. *Semiotica*, 77, 1/3, 65-119.

Petitot, J. (1989 d). Catastrophe Theory and Semio-Narrative Structures. *Paris School of Semiotics* (P. Perron and F. Collins eds.), 177-212. John Benjamins : Amsterdam.

Petitot, J. (1990). Le Physique, le Morphologique, le Symbolique.Remarques sur la vision. *SC* 1990, 139-183.

Petitot, J. (forthcoming) A. Modèles morphodynamiques pour la grammaire cognitive et la sémiotique modale. RS/SI (*Semiotic Inquiry*), 9, 1-2-3, 17-51.

Petitot, J. (forthcoming) B. Schémes morphodynamiques et grammaire cognitive : de René Thom à Ronald Langacker. *Modèles linguistiques*.

Piattelli-Palmarini, M. (1988). Evolution, Selection and Cognition: From "Learning" to Parameter-Fixation in *Biology and in the Study of Mind*. MIT Center for Cognitive Sciences.

Poston, T., Stewart, I (1978). *Catastrophe Theory and its applications*. Pitman Publishing : London.

Psotka, J. (1978). Perceptual Process that May Create Stick Figures and Balance. *Journal of Experimental Psychology* (Human Perception and Performance), 4 (1) 101-111.

Pylyshyn, Z. (1986). *Computation and Cognition*. MIT Press: Cambridge.

SC. (1990). Sciences cognitives : quelques aspects problématiques, (J. Petitot ed.). *Revue de Synthèse*, IV, 1-2. Paris: Albin-Michel.

Smolensky, P. (1986). Information Processing in Dynamical Systems: Foundations of Harmony Theory. *PDP* (1986), I., 194-281.

Smolensky, P. (1987). The constituent structure of mental states: A reply to Fodor and Pylyshyn. *Southern Journal of Philosophy*, 26, 137-160.

Smolensky, P. (1988 a). On the Proper Treatment of Connectionism, *The Behavioral and Brain Sciences*, 11, 1-23.

Smolensky, P. (1988 b). Connectionism, constituency and the language of thought. *University of Colorado Technical Report*.

Thom, R. (1972). *Stabilité structurelle et Morphogenèse*. Benjamin: New-York, Ediscience: Paris.

Thom, R. (1980). *Modèles Mathématiques de la Morphogenèse*. Christian Bourgois : Paris.

TLTA (1979). Théories du Langage, *Théories de L'Apprentissage* (Massimo Piattelli - Palmarini ed.). Le Seuil : Paris.

Visetti, Y. M. (1990). Modèles connexionnistes et représentations structurèles. MC 1990, 167-212.

Wildgen, W. (1982). *Catastrophe Theoretical Semantics*. Benjamins: Amsterdam.

Zeeman, C. (1977). *Catastrophe Theory* . Addison-Wesley: Massachusetts.