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HOWARD PATTEE'S THEORETICAL BIOLOGY:
A RADICAL EPISTEMOLOGICAL STANCE TO APPROACH LIFE, EVOLUTION ANDCOMPLEXITY.

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Abstract:

This paper offers a short review of Pattee's main contributions to science and philosophy. With no intention of being exhaustive, an account of Pattee's work is presented which discusses some of his ideas and their reception. This is done through an analysis centered in what is thought to be his main contribution: the elaboration of an *internal epistemic stance* to better understand life, evolution and complexity. Having introduced this core idea as a sort of *a posteriori* cohesive element of a complex but highly coherent and complete system of thinking, further specific elements are also reviewed.

Keywords: epistemological stance, epistemic cut, semantic closure, code, symbol

1. INTRODUCTION

Despite how specific and narrow it might seem, considering the amplitude of his work, it is not restrictive at all that a special issue reflecting on the thinking of Howard Pattee should center on the 'physics and evolution of symbols and codes'. As it is not surprising that Pattee himself agreed with the choice.

There are multiple ways to explain this potentially paradoxical impression, but I would like to disclose only two. First of all, it is obvious that the subject chosen is by itself big enough a problem to deserve the most dedicated survey; after all it is a question which is being investigated (along similar or totally distinct approaches and under those or other labels) by philosophers, cognitive scientists, anthropologists, linguists and, why not, even biologists and physicists. Recent or brand new interdisciplinary areas as Artificial Intelligence (AI), Artificial Life(AL), or complex systems theories are precisely addressing these kinds of subjects. In this context, Pattee's ideas are profitable (and should be more).

Secondly, it is quite indisputable that the question being addressed here marks the area where, we could say, Pattee has made his more important, original and long standing contributions. Still, related to this point, it is even more significant to realize that the issue of the 'physics and evolution of symbols and codes' is the cohesive problem where most if not all of Pattee's multiple contributions coalesce and acquire their arduous but utmost coherence.

I say arduous because the unified picture I claim is somehow lurking beneath the treatment of several different issues and all the connecting threads are not always made explicit. We have to take into account that Pattee has addressed difficult issues in more than fifty academic papers and that his contributions to science and philosophy are many and have, on their own, enough distinctiveness and substance to be considered separately. But, I think it is also important to realize that, even when he is discussing problems such as the origin of life and hereditary mechanisms; the nature of heredity, memory and their reliability; the understanding of hierarchies, control and inter-level relations; the operation of constraints and the distinction between laws and rules; the use of concepts as measurement and complementarity; the connotations of terms as symbol, code and meaning; etc.; he is always offering a definite global perspective too. Of course, the compatibility and agreement amongst the particular and diverse hypothesis and arguments defended on those articles are clear. Yet, the overall harmony is something which, perhaps, has still to be fully disentangled or, to say it better, **unfolded** and, if possible, even systematized.

Hence, I would say, Pattee's work amounts to a very rich and suggestive set of ideas concerning different questions, but likewise to a potentially robust and coherent system of thought which produces an original and fruitful approach to life, evolution and complexity. Therefore, here I will introduce and explain why a radical **epistemological stance** might be this pivotal idea around which additional proposals might be ordered. In doing that, I will try to avoid possible misunderstandings of Pattee's position by using several of his papers scattered across different domains(deliberately using some of his earlier ones to this purpose), while I expect to argue, once more, for the utility of a conscious and very careful semantic (semiotic) approach to the understanding of living systems which pays attention to the problem of origin. However, let's be clear, I do not intend at all to summarize his work or to exegetically elucidate his ideas. There is no surrogate for the direct reading of Pattee's papers.

2. A GLIMPSE OF THE VIEW FROM WITHIN.

> "The fundamental problem of biological systems theory is to understand and represent the interactions within such **epistemological systems**, that is, systems which have both a rate-dependent dynamics and a rate-independent self-description that constrains this dynamics so as to form a coherent organism." (Pattee 1978, p. 513)

Unfortunately, the task of unfolding the aforementioned coherent and complete system of thought is too large for me to tackle here (as well as for the goal and scope of this paper). Nevertheless, it is not difficult at all to introduce what I think should be the central tenet of such a system, i.e., that we should study living systems as being **intrinsically epistemological**. I think, indeed, that the idea which best represents Pattee's approach, is the need to introduce a **generalized epistemological stance** to address the study of natural phenomena once life is originated.

This is why I will center this review on his **internal epistemological stance** - a **view from within**. In particular, I will focus on Pattee's *effort to get a glimpse of this view*. Even if indirectly, of course, because, let's say it from the beginning, he is not as philosophically naive as to pretend to reach any kind of "Dingan sich". This glimpse is just an awareness from which he cannot escape once the nature of natural records is elucidated:

> "**What is a record?** I believe we must follow the reasonable assumption that the first records were in single molecules, since that is the way they occur in modern cells. The essentially new condition in this origin of life formulation of the recording or measuring problem is that no human observer, no physicist, no philosopher, nor any macroscopic measuring instrument designed by biological organisms can exist in the beginning. We imagine only the motions and interactions of the elementary matter, so we can only ask, how does matter record its own behavior without the intervention of a physicist. Or in other words: How does the motion of matter lead to *records* of these motions?
>
> Someone will probably object that the observer has not really disappeared in this formulation, and that I have only hidden the observer by imagining the existence of an objective recording process which is operationally meaningless, since it is still the human observer who decides when a record has been made. Here I shall simply admit to being a realist, that is, a person who believes that there are aspects of the world which exist independent of this observer's description of the world. I must accept as a meaningful concept supported by empirical evidence that life did not always exist on the earth, and that it was the accumulation and transmission of hereditary records at the molecular level that eventually led, only after billions of years, to observers like myself. But I must therefore add-and this is the central point- that not only matter, but also *records* existed long before physicists started thinking about matter and making large measuring devices." (Pattee 1971a,p. 310)

Of course, this is a sort of "a posteriori" unifying principle which was, somehow, already present (latent) since Pattee's very first papers and becomes more and more clear and explicit in later ones. It acquires full conceptual identity once his Semantic Closure principle is christened and introduced (Pattee 1982) and later supplemented with the statement of the need of an Epistemic Cut (Pattee1995). But it was already the inescapable answer to previously stated questions about "The physical basis of coding and reliability in biological evolution" (Pattee 1968b) or "How does a molecule become a message?"(Pattee 1969c); or those raised in earlier works on the nature of records (as in the previous quote and elsewhere).Pattee's recognition and understanding of these natural records(natural symbols) is tied with their interpretation as something inextricable from the context, i.e., as acquiring their "meaning" (always operational and only operationally discernible for Pattee) within a system, organism, ecosystem, etc. Contexts which necessarily possess material, dynamical characteristics (time-dependent and energy dissipating).

In contemporary debates belonging to different disciplines (philosophy, anthropology, psychology, ...), the classical nature/nurture dichotomy has taken manifold new faces most of them being still rather excluding (fixed/variable, inheritance/development, functional/structural, informational/dynamical, ...). This is so with the exception of the, also traditional, versions of the so called **interactionist** position and the step forward of rejecting the very dichotomy by itself(for a discussion see, for instance, Lewontin 1982, 1983; Lewontin, Rose &Kamin 1984; Oyama 1985). In this context, I would say that, for many years, Pattee has been working on a particularly rich understanding of living systems from a perspective which we could call **"internally" interactionist**, i.e., from within the organism itself. In doing so he has made a heavy use of explicitly linguistic (perhaps cognitive, but this is not so clear and should be discussed) terms and concepts to account for what he considers is the more specific feature of living systems identified as the cellular "semantic closure" arisen at the origin of life (Pattee 1982).

3. KEY INFLUENCES.

Before considering some specific areas of research where Pattee has developed his thinking, it would be fair to recall at least a few of the main influences that Pattee always acknowledges and cites once and again in his papers. Apart from fairness, this might help us to better understand Pattee's theoretical universe and how his thinking has been built.

In any case, we should keep in mind that, as Pattee makes it clear in his paper in this issue (quoting Pearson), his underlying motivation has been the question: How can we explain why life is different from physical systems if life is just matter that must follow physical laws? As he rightly notices, this is a question that, in general, bothers physicists more than biologists or philosophers. This circumstance clearly affects the reception of his work as we will see below. That question, once dissociated from any kind of vitalist answer (as he does, of course), implicitly requires a complex answer able to transcend both the reductionist physicalism, resigned to relegate the biological specifics, of most molecular biologists and the non reductive vindication of the autonomy of biology, oblivious of physics, of most evolutionists. This difficult and demanding approach, an original kind of non reductive physicalism in philosophical terms, biases its potential audience, as we will see below.

The first of his key influences is von Neumann's (1951, 1966) logic of self-reproduction and the conditions for open-ended evolution. The need for a separate description hinted in the lectures from which those publications arose is thoroughly developed by Pattee in his many papers defending the genotype-phenotype distinction as the central feature characterizing living systems.

> "The logical aspects of this fundamental evolutionary principle were understood by von Neumann (1951) in his design of a self-replicating automaton. Von Neumann's self-replicating automaton had the same basic logic that is now found in cells, although his replicating automaton was designed without any knowledge of the cellular translation code and the roles of nucleic acids and enzymes. Nevertheless it was clear to von Neumann that template replication or simple copying was, in itself, of no interest in

> either the logical or evolutionary sense, and only hereditary mechanisms which could grow in complexity had any real significance for understanding life." (Pattee 1967, p. 411-412).

Moreover, Pattee takes very seriously into account von Neumann's usually forgotten (or not fully recognized) remark on the limitations of his own (abstract, formal, merely computational) treatment of the problem:

> "By axiomatizing automata in this manner, one has thrown half of the problem out of the window, and it may be the more important half. One has resigned oneself not to explain how these parts are made up of real things, specifically, how these parts are made up of actual elementary particles, or even of higher chemical molecules." (von Neumann[Burks] 1966, p. 77).

Hence, Pattee always pays attention to the dynamical aspects of hereditary transmission and expression (protein folding in particular; time, energy, reliability, etc. in general). Not only when evaluating artificial models (v.g., Pattee 1988) but also when discussing natural systems.

Finally, von Neumann's formulation of the measurement problem (von Neumann1955) will be the basis for the treatment of measurement like phenomena and constraints in natural systems. Structures that function as measuring devices are distinguished from the phenomena they are to measure. This measurement problem stems from basic principles of Physics, and is one of the foundations for the ulterior development of the concept of epistemic cut. The same way as we could treat a measuring device as part of the general dynamics of the measured system, at the price of loosing the measurement function and its product, the record, we could as well treat any epistemic system as indistinguishable from the environment in which it is immersed, by loosing its specific agency as a subject.

This question connects directly with the second key influence by Bohr and his generalized complementarity principle that Pattee develops and applies in detail to the characterization of living systems and their capacities at all levels of complexity. What begins as a physically acceptable way to interpret and formulate the distinguishing features of life becomes a complete methodological guide applied to all areas and levels of knowledge, where no exclusive and exhaustive explanation will be easily accepted.

A third key influence which is related to complementarity is Polanyi's (1968) dual control of laws and constraints. Polanyi's view of boundary conditions (or constraints) as **harnessing** the laws of nature and, therefore, not disobeying them nor being reducible to them, will allow Pattee to develop a biological theory grounded on an illuminating understanding of the role of constraints already began some years earlier (at least in his "Physical theories, automata, and origin of life" paper (Pattee 1966)). This distinction will also contribute to form the basis (together with Simon's ideas from his 1962 seminal paper) for his own hierarchy theory being worked out in those very years(Pattee 1969a, 1971b, 1972a, 1972c, 1972d, 1973b, 1973c, 1973d).

A fourth significant inspiration is quite illustrative of Pattee's idiosyncratic choice of sources, of his reference universe. I am talking about Hertz's notion of the modeling relation, his diagram of a model (Fig.1) which is presented in the introduction to *The Principles of Mechanics* (1894).

> "We form for ourselves images or symbols [*Scheinbilder*] of the external objects; the manner in which we form them is such that the logically necessary [*denknotwendigen*] consequences of the images in thought are invariably the images of materially necessary [*naturnotwendigen*] consequences of the corresponding objects. (...) Experience shows that the demand can be satisfied and that such correspondences do in fact exist". (Hertz 1894, pp. 323-324 [1956, p. 1-2])

By idiosyncratic I want to indicate a steady pattern of selection of references consisting on mentioning, whenever is possible, the most fundamental or classical texts. This does not imply that no reference of recent, specific or particular works is given when necessary. It is only that for the more philosophical or fundamental aspects of his proposals he prefers resorting to well established classical authors, preferably physicists, and not to more or less specialized journal articles on the last detailed discussion. This probably influenced the impact of his contributions, an issue I will partially consider ahead.

Besides those four traceable influential authors there are also, at least, a couple of notions or points of view that become central for Pattee's approach. On the one hand, we find the elaboration of the concept of **selection** in all its importance and significance, i.e., as a principle alien to physical language and necessarily related to the presence of actual alternatives and to aspects of choice or decision. Notwithstanding, the significance assigned to selective phenomena, he is able not to fall prey to the pan-selectionist tide which is overflowing in current popular scientific literature (Barkow, Cosmides & Tooby 1992; Cziko 1995; Dennet 1995, to name just a few and not to mention the literature of complexity theory). On the other hand, we notice that Pattee is ready to accept a **pluralist vision of causality**, where a double causality (at least) is predicated for biology with respect to physics. This acceptance is reminiscent, again, of Polanyi's vision but also of other proposals as, for instance, Mayr's classical and influential distinction between proximal and ultimate causes (Mayr 1961 and subsequent publications). This move allows a much more open epistemological position able to encompass a truly hierarchical and complementary worldview(and is congenial with well established current positions in the philosophy of biology).

4. MAIN AREAS OF RESEARCH AND CORE IDEAS

It is not easy to disentangle Pattee's system to discern, order and catalogue those contributions "one by one" so to say, but we could, at least, indicate first his main areas of research and then some of the more important concept she has advanced. There are, at least, two main areas of research where Pattee has worked steadily and which, although would deserve detailed examination, we cannot go deeply into. I am referring to the origin of life and to hierarchy theory.

4.1 Origin of life

Pattee has worked out this subject for its indubitable relevance by itself and within biology, but also for the very clearmethodological reason (which is always present) of studying the nature of an issue at its most simple appearance. Out of a moderate disappointment with the state of the art in the origin of life research, specially regarding the nature and origin of genetic information, Pattee began a thorough (and uncompromising toward the rising post-1953 DNA centered doctrine) study whichproduced a first set of six dense papers dedicated to the question of heredity (Pattee 1961, 1965a, 1965b, 1966, 1967, 1968a). In those papers he already laid the foundations to ground his posterior thinking toward the Semantic Closure principle.

> " ...we may expect that the origin of life problem will shift away from the evolution of the building blocks and the elementary operations of joining them together, to the more difficult problem of the *evolution of control* in complex organizations. This problem is more difficult because the idea of 'control' is not defined in the same sense as we can define biochemicals. (...) A live cell and a dead collection of the identical biochemicals in the same structural organization differ essentially in the amount of intermolecular control that exists in each unit. From this point of view, the question of the origin of life becomes the problem of understanding elementary molecular control processes, and of formulating a theory of the evolution of molecular control." (Pattee1965b, pp. 405-406)

For instance, already in his 1961 paper he defended the presence of some degree of pre-biotic molecular order facilitating the appearance of macromolecular sequences (well before self-organization and "order for free" ideas began to enjoy any notoriety). And L.L. Whyte in his remarkable *Internal Factors in Evolution* attentively noticed it:

> "Another line of research which is leading towards the identification of the C.C. [coordinative conditions] as they affect the genotype and its mutations is the statistical study of non-random arrangements in biological macromolecules. Recent biochemical and theoretical studies (..., H.H. Pattee, 1961) point to restrictions on the possible sequences in the linear biomolecules expressing specificity. Pattee considers that there is an important element of order in such macromolecules which is a precondition, not a consequence, of adaptive evolution." (Whyte 1965, p. 96)

In those papers he proceeds to defend the importance of conformational aspects in the sequence (tactic copolymeration) by the combination of strong and weak forces; he postulates hereditary transfer by conformation dependent propagation rules; with the introduction of non-holonomic constraints (Pattee 1966) he describes simple mechanical systems that yield primitive memory results and designs similarly functioning molecular hereditary automata as macromolecular sequence computers; he formulates plausible hypothesis for the gradual evolution of molecular and genetic controls from those primitive ones to more complex ones in the following way (a process which he explicitly considers within the context of a still lacking general theory of self-organization and in analogy to processes of learning):

> "The new approach to origin of life experiments which I am proposing, is directed at the simplest possible level of hereditary propagation in macromolecules which may arise after a stage of spontaneous chemical evolution, but well before self-replicative biological evolution, which progresses by natural selection. This intermediate level of organization I would call the stage of *molecular automata*, or to paraphrase Charles Babbage, the stage where polymers begin to 'feed on their own tails'. Evolution at this pre-replicative stage would progress initially by direct feedback selection processes which correspond logically to what we call "training" in computers or individuals. I am proposing that hereditary transfer is accomplished in growing copolymers by conformation-dependent propagation rules. Well-trained aggregations of such growing copolymers may gradually gain more self-control and less direct interaction with the environment leading finally to what we call self-replication and the completely indirect interaction with the environment which we call natural selection." (Pattee 1965a, p. 387)

In those papers he is offering, as well, practical ways to test his hypothesis, both as computer simulations and through experimental studies, which would obviously demand further development. To this respect it might be worthy to mention how these ideas fare in more recent times. In his popular review of the origin of life theories, Shapiro (1986) closes the book with a final chapter entitled "The way to the answer". In this chapter, after recalling the many experiments that have been conducted and which he has described in earlier chapters, he criticized them as well because of the methodological flaw of having a preconceived goal. In this context he introduces Pattee's suggestion as follows:

> "An almost uncharted area remains open to skilled chemical investigation: undirected prebiotic experiments. Some aspects of a study of this type have been anticipated. Isolated individuals have called for experiments that accurately simulate the complexity of a primitive earth environment. In 1963, at the Second International Conference in Florida, physicist H. H. Pattee made this point: 'For all the inevitable inaccuracies in detail, a sterile simulated seashore, with waves, tides, sand, rain, and intermittent sunlight, is amore accurate primitive earth environment than the well-defined but oversimplified reactions studied so far'. [Pattee 1965a, p. 400]" (Shapiro1986, p. 301)

A last point regarding Pattee's experimental and artificial models of hereditary order is that, seemingly, he could have succeeded where Gamow failed in the 50's when trying to use models based on stereo specificity to explain the genetic code(cf. Sarkar 1996, pp.191-194). Unfortunately, those models were not pursued much further.

In absence of acloser examination, we may conjecture that the proposal was too peripheral or it was already too late, as the Central Dogma of Molecular Biology had been strengthening for more than a decade.

4.2 Theory of hierarchies.

This is probably the area were Pattee's work has been more influential (Umerez and Moreno 1995). There is nearly no treatment of the subject since the 1970's that does not at least turn to his 1973 edited collection (see Salthe 1985 for the best treatment one decade later, and Ahl & Allen 1996 for a recent review of the state of the art). In a simplified way, it was his previous concern with the nature of molecular control together with the need to find operative ways to deal with complexity, what led Pattee to undertake the task of devising a more comprehensive hierarchical perspective. Even though he was considering control issues before (as well as after), we can identify a compact series of seven publications between 1969 and 1973 which are mainly dedicated to the exposition of his hierarchical view (Pattee 1969a, 1971b, 1972a, 1972c, 1972d, 1973b,1973c, 1973d).

Following Simon's path Pattee related hierarchical organization to a best likelihood of evolution (Simon1962, 1969, 1973) but he went further in order to explore the conditions of possibility for unconstrained open-ended evolution of a von Neumannian ancestry. Therefore, he complemented Simon's, mostly structural, kind of hierarchical order with the elucidation of a functional kind that he needed to explain control attributes (a third much less specific kind regarding classification or inclusion is of no special interest for him). He also insisted on two somehow paradoxical but crucial features of hierarchies: that systems very organized as hierarchies experiment a simplification of their behavior (i.e., Pattee 1972c) and that the limitations exerted upon the degrees of freedom at one level result in an increase at the subsequent one. Those operations are performed through material devices in the form of constraints of different kinds, mainly holonomic for structural hierarchies and non-holonomic for functional ones. The very specific control relation is introduced as the selection of alternatives.

Together with the basic distinction between laws and rules and the hierarchical perspective, the concept of physical constraint makes room for the natural acceptance of a fully material kind of downward causation (Pattee 2000; Moreno and Umerez 2000),in sharp contrast with the strong censure suffered by the concept in most philosophical schools.

4.3 Concepts and terms.

Besides those main areas of research, Pattee transplanted some well-established concepts from Physics into Theoretical Biology and Philosophy, where they are useful in what they have of more concrete and operational. Sometimes, of course, this transplant has resulted in a generalization of the intended physical meaning of those terms, but this has been always compensated with an effort to concentrate in the most "simple" instances, models or phenomena. Besides, they have been used with a conscious effort on definition supplemented with very clear and familiar examples. The rationale for this borrowing is twofold. On the one hand, there is the need for plain but expressive terms (not baroque or unnecessarily technical) with rich enough connotations as to be able to convey the full significance of new ideas (as symbol, language, control,...). This is quite usual. On the other hand, to the contrary, there is the mistrust and subsequent avoidance of some terms supposedly (quasi)technical or pertinent but misleading (as information, self-organization, emergence,...). Some of the terms he imported from Physics, such as constraint (especially non-holonomic), complementarity or measuring device are quite distinctive in Pattee's usage; whereas others imported from cybernetics or linguistics such as symbol, code, or control are more widely used.

4.4. Semantic Closure.

Now we reach Pattee's most original contribution which condenses the kernel of his thinking. The **Semantic Closure** principle derives primarily from his reflection upon relevant features of the cellular (genetic) code.

> "Looking more closely at how this comes about in the cell we see that this type of symbol-matter-function dependence is an exceptional kind of interdependence which I call *semantic closure*. We can say that the molecular strings of genes only become symbolic representations if the physical symbol tokens are, at some stage of string processing, *directly* recognized by translation molecules (tRNA's and synthetases) which thereupon execute specific but arbitrary actions (protein synthesis). The semantic closure arises from the necessity that the translation molecules are themselves referents of the gene strings." (Pattee 1982, p. 333).

Then it is also generalized to a broader abstract concept:

> "By general Semantic Closure I mean the relation between two primitive constraints, the generalized measurement-type constraints that map complex patterns to simple actions and the generalized linguistic-type constraints that control the sequential construction of the measurement constraints. The relation is semantically closed by the necessity for the linguistic instructions to be read by a set of measuring devices to produce the specified actions or meaning." (Pattee1985, p. 272).

>From the very beginning, Pattee was especially interested in making good use of physics (physical basis and conditions, quantum mechanisms, laws & constraints, actual dynamics, etc.) to "ground" (mathematical) theoretical biology (Umerez 1995), to overcome limitations of abstract models, and to balance a formalist, nonmaterial biology. This was coupled with an equally early and parallel concern regarding the too easy use, without a proper awareness, of informational and cybernetics terms in (molecular) genetics. It is interesting to note that only recently, have critics of the assumptions behind Biology, such as Stuart (1985a,b), Oyama (1985) or, differently, Sarkar (1996) (among others), denounced such metaphorical terminology.

> "I am now in position to defend my claim that the assumptions about coding and information (...) do not satisfy all of the criteria for the strong sense of reduction that I have been using above. Thus they run counter to one of the most striking patterns of explanation in molecular biology. (...) Explanations based on this model are not physicalist, in the epistemological sense in which that criterion was formulated above.

> (...) what was just said (...) can be said about our usual notion of "coding" and its associated notion of "information" as sequence." (Sarkar1996, p. 217-8)

Up to the 1990s Pattee became concerned with this issue not only in biology but also in some more recent areas such as Artificial Intelligence and Artificial Life, as well as diverse complex systems theories. Within those he has been very critical of computationalist positions. Nevertheless he has always defended, from that critical position, the importance of these code-related aspects (semiotic) in a genuine complementary relationship with purely physical ones (Umerez 1995). Thus the extant controversies with "no-information" positions in AI and Cognitive science (v.g., direct perception) or the potential ones with "similar" positions in(theoretical) biology (autopoiesis, structuralism, Developmental Systems Theory, etc.).

> "I have used the origin of life context in discussing coding and reliability because this level allows the simplest possible conception of a molecular hereditary transmission process. We have seen that even at this level the theoretical difficulties remain serious. Nevertheless **I believe that the concepts of coding and reliability will not only be useful, but also crucial at all levels of biological organization** -cellular, developmental, evolutionary, and certainly in the higher nervous activity associated with the brain. **We have used code to mean the relation between an elementary genotype and a phenotype**, that is, **a relation between a physical symbolic description and the physical object** which is actually constructed from this symbolic description."(Pattee1968b, p. 89, boldface added)

It is quite clear that this kind of terminology (code, information, program, symbol, etc.) has been largely abused in biology as, for instance, Oyama (1985) and others have shown in some detail (see Kay 1993, 2000 and Keller 1990, 1995 for diverging historical accounts regarding the introduction and role of such terminology within post-war biology). Pattee has always been aware that information-related terms and concepts do not belong to the discourse of physics and are not easily integrable with physical theories, despite what many almost self-contradictory reductionist biologists pretend. It was precisely this problem, how to encompass physical principles with hereditary records, what had prompted his reflections already in his first papers. Therefore the difference with respect to those criticisms (Sarkar, Oyama,etc.), does not rely on any kind of allegiance to current uses but, well to the contrary, on the vindication of rescuing a more fully semiotic sense (instead of promoting its abandonment). As he makes clear in the paragraph quoted ahead, code means the relation between the descriptive and the operative side, not the genetic part in isolation. Therefore, any "commanding" ability (instruction, control, determination, etc.) of genotypes, is completely dependent (complementarity, mutual relationship) on the constructive ability of phenotypic structures. Likewise, these depend on the descriptive faculty of genotypes.

To better understand this, we should bear in mind that one of his first convictions, along with complementarity, has been to claim for the consideration of the context of any system: from the inner environment of a cell to all sorts of ecosystems.

> "There is no living unit which can be considered 'living' without reference to the external environment.(...) Biologists should emphasize over and over that 'living' is unavoidably a *total ecosystem* property and not the property of an isolated collection of macromolecules. It seems to me that the central question of the origin of life is *not*, "Which comes first, DNA or proteins?', but rather 'What is the simplest possible ecosystem?'" (Pattee 1968c, p.219)

In his very discerning survey, Sarkar (1996) points out the flaws of the conceptual structure of contemporary molecular biology. We should notice that Pattee was already aware and tried to overcome some of them 35 years before, shortly after the discovery of the double helix structure and during the years when the molecular mechanisms of heredity were being elucidated. I will mention just two examples. Situating his narrative at the beginning of the60s, with the Central Dogma already in force, Sarkar concludes that:

> "The coding problem, at this point, had not only forgotten physics, but had even abandoned attention to biological specifics in preference to formal arguments about the efficiency in storage and transmission." (Sarkar 1996, p. 197)

This is something that Pattee never did, well to the contrary. As we have seen, that same decade he began writing on those "specifics". Going on with his description of the problems arising in the 60s, Sarkar points out that those theories of coding and information are, at best, incomplete by lacking any concern about dynamics (Sarkar 1996, p. 201). This is exactly what Pattee's complementarity mounts to. Besides, we have seen that he has always insisted on the importance of physical aspects to understand biological systems, precisely those dynamical aspects (time, energy, ...) that are necessary to implement codified instructions. Still quite skeptical, Sarkar even attempts to

> "...explore the potential of that forgotten alternative, that is, the possibility that an entirely different notion of "information", derived in some way from cybernetics, can be used to recapture the value of that concept for molecular biology." (Sarkar 1996, p. 202)

But he considers just the case of the operon model and remains unaware of Pattee's work. This is not the case with other recent publications within this critical scope where we find different reactions with respect to Pattee. These reactions range from Oyama's positive consideration of his aim but critical indictment of his linguistic terminology, to more confident assessments as in Keller (1995)who is not worried by the cybernetic metaphors and relates his work to are birth of developmental biology.

Oyama wonders as follows:

> "...Pattee appears to be making some important points about the explanatory insufficiency of DNA (...).Whether these points are clarified or obscured by ever more elaborate metaphors, however, by which molecules gain symbolic properties in quasi-linguistic structures, is another question." (Oyama 1985, p. 48);

To this we may positively answer that Pattee, in his "more elaborated metaphor", is already taking into account all the nuances that she is demanding when talking of programs, information, etc. Keller's assessment, referring to Pattee (and Waddington) among others, is quite eloquent:

> "However, some (distinctly nonmolecular) biologists saw a quite different and perhaps more profitable use to be made of the models the new machine had to offer. Instead of claiming the notion of information to support a highly reductionist and unidirectional causal structure, a small number of biologists interested in the complexities of embryogenesis sought in cybernetics and information theory support for a dynamic and interactive conception of organism. Although these efforts are now largely forgotten, and were not at the time followed up, they are nonetheless worth noting, if only for historical interest." (Keller 1995, p. 99)

6.- PHILOSOPHICAL DILEMMAS.

Consideration of intricate conceptual and theoretical questions regarding heredity and life and elaboration of prospective answers, made it unavoidable for Pattee to begin addressing well entrenched philosophical problems related to the nature of meaning and symbols, to knowledge, causation, information, etc., or even to some less welcomed ontological questions (such as emergence, levels of reality, etc.). He is mainly interested in defining epistemic concepts so that they apply to sub-cognitive organisms, an approach almost totally alien to philosophers.

In any case, Pattee's philosophical style is attached to discussing substantive issues, i.e., he is not fond of arguing about formal issues, analytical approaches, or philosophical technical subtleties. We could easily place him within the "natural philosophy" tradition of physicists in the past (and of scientists in general recently) or within what is beginning to be called the return of the pre-Fregean (Kitcher 1992), non-analytical way of doing philosophy of science, which nowadays is being vindicated along naturalistic lines (see Callebaut 1993).

In connection to this, Pattee's work offers us a distinctive brand of "evolutionary naturalism" with an intra-organismic dimension given by his semantic closure principle, but compatible with more functionalist and externalist versions, depending only on adaptation and natural selection. His attempt is aimed to conceive a plausible evolutionary process from which 'meaning' emerges out of sub-cognitive material symbols, beginning with the simplest kind of record until the more complex and gradually more implementation independent sign systems (Umerez 1998).

Likewise, Pattee has been forced to address some of the core issues of the problem of knowledge: the duality knower/known (subject/object) and the questions regarding the nature and accuracy of representations (realisms ofall sorts and their alternatives). His position on this topic can be seen as departing from Hertz's scheme mentioned above, enriched with an attentive reading of the theoretical physicists, in order to develop a pragmatic but realist approach. Such a difficult combination is made possible, in his case, by keeping the necessity of the epistemic cut clear, but making it flexible, not privileging any one in particular (specially not the usual one: human high order cognition). In other words, it implies a distinction between the "internal images" and the "external objects" but allows the displacement of the internal/external boundary depending on the systems, i.e., making the very boundary a matter of fact, a question of scientific inquiry. This is nothing more than the logical counterpart to the introduction of a generalized epistemological stance into the natural systems themselves (bound to living systems, i.e., diachronically considered, once life is originated; synchronically regarded, from the level of complexity of organic life on). But this extension is accomplished without blurring the role of the "observer/knower", well to the contrary, it implies assuming different systems and objects playing as "observers" at different levels. In order to preserve this solution operative it is indispensable to know at all times where the cut is being made in any particular treatment of whichever observed system. And moreover, it is imperative not to forget where it is unavoidable: us, human beings as producers of theory and other sophisticated ways to address the world, most of them, but not solely, related to language (of different kinds and levels of abstraction).

7. LIMITATIONS

Before trying to assess the impact of Pattee's work on science and Philosophy, it would be pertinent to supplement the preceding condensed review of his contributions with an equally succinct survey of the limitations of such endeavor.

For instance, it is quite easy to notice an unfortunate lack of philosophical dialogue, in particular with the standard community of philosophers of science. Needless to say in this respect that, until very recently, while the analytic tradition was still dominant (at least in practice), it has not been an easy task for a theoretical scientist to attempt to get his ideas through a linguistically oriented philosophy. Remarkably, this has been an experience common to other researchers, especially those worried about biological issues, as Mayr, for instance, is fond of denouncing (Mayr 1976, p. 357; Mayr 1982, pp.: 73-76; Mayr, 1988, pp. v-vii, 1-2). In the case of the 1988 book, together with the repeated complaint, Mayr is able to confirm the optimistic side of which he already had a glimpse in the previous ones:

> "Since the Scientific Revolution, the philosophy of science has been characterized by an almost exclusive reliance onlogic, mathematics, and the laws of physics. But in recent years we have witnessed a laudable state of ferment in the field. (...) The philosophy of biology is characterized by its emphasis on concepts and their clarification. (...) Alas, the older generation of philosophers of science still ignore this insight. Too many of them persist in dissecting with the sharpest tools of logic the grueness or not of emeralds or the blackness or not of ravens. Fortunately, the younger philosophers of

biology entirely agree with the scientists that a careful analysis of the underlying concepts has primacy in philosophy over exercises in logic." (Mayr 1988,pp. v-vi)

We could still lament that this dialogue has not been tried harder with this "younger" generation of philosophers, as Mayr calls it (looked at from his almost century long life), at least inasmuch to keep the connecting threads that somehow were established formerly with those biologists or philosophers prone to attend to this point of view already in the 60's and 70's.

Something similar happened, I am afraid, concerning the progressively more scarce conversation with, at least partially, common positions in Biology. Since those previously mentioned years, when there existed a more fluent relation, the connections have been kept with some heterodox positions (autopoiesis, self-organization, dynamical theories, systemic positions, ...), but less with more standard(albeit not totally orthodox) though likely congenial biologists in several areas beyond the hardened core of molecular biology (this could include evolutionary theorists of a more pluralist bent as Mayr, Lewontin, Gould, etc. or other researchers as Margulis, Buss, D.S. Wilson, etc...). Of course, they are not motivated by the same questions as Pattee and relating the partial aspects of their theories which do indeed overlap is not always an easy task. Especially considering that, those that do not concentrate in the tiny molecular problems of Biology, do often simply discard as irrelevant any insight which might come from physics.

A more general source of limitations, encompassing those mentioned above, is linked to an arduous academic institutional placement. Here we have a downright example where an accomplished "trans/inter-disciplinarity" is a source but of trouble. This situation, despite all its current glamour, implies an increased difficulty in passing one's message through, turning the explorer into an outsider to the rest of the disciplines. Most instances of successful interdisciplinarity are grounded either on a good integration within the discipline of departure which enables one to venture far away by offering a safe place to return; or else on having amore or less well established area of research available as the destination of such transgressing incursions.

Pattee's case is a much more complicated one for he probably took his chances uncommonly far off from biophysics (when biophysics itself wasn't as firmly established a discipline as today) into promising areas of biological research (origins of life, nature of genetic information, automata theory, theoretical biology at large, etc.) which were virtually swept away by Molecular Biology and mathematical Population Genetics1. Besides, he later came upon a receptive field, that of Systems Science, which was specially appropriate because of its basic and fundamental transdisciplinary character despite the lack of a solid institutional framework. But, even within that milieu, the chances of pursuing a theoretical inquiry of biological systems(serious and demanding) instead of resorting to more applied or engineering like systems approaches (or at least to computer science) were becomingscant. Later on, related new fields as Artificial Intelligence or Cognitive Science and, especially, Artificial

Life and the whole area of Complexity studies (already in the90s) will offer new opportunities but, in a sense, too narrow ones for the breadth of Pattee' thought.

There is one further issue: the difficulty of establishing an "intermediate" position, a sensible middle point between extreme positions. This is well exemplified in the crucial "Cell Psychology" paper (Pattee1982) for instance, where neither information processing nor direct perception are given primacy but instead a complementarity of both is defended. The same pattern is manifested in the general picture of biology that Pattee entertains where both genetic (informational) and metabolic (dynamical) aspects are tied together within a multilevel complementarity, or both natural selection and self-organization are combined to explain related but diverse features of living systems. Or we could repeat here his non-reductionist claim of the importance of physics for biology, an intermediate path that only recently is being pursued again from diverse theoretical perspectives.

A very similar point is hard to evaluate but might arise as a limitation: the absence of a monograph, a book format exposition of this thinking, the difficulty of not offering a complete vision with everything tied together (but we should perhaps wonder if this is a fault, i.e., whether keeping one's system of thought open and unbound is wrong).

Regarding the development of the crucial theory of the Semantic Closure, there is perhaps some shortness of the temporal dimension, of the evolutionary dimension, which is present, of course, but has not been elaborated as thoroughly as the synchronic dimension.

Considering more specific issues that might indicate possible shortcomings, we should mention the problems associated with conducting experiments, developing applications, or running models. For instance, Pattee has put forward several proposals of empirical (real or simulated) tests in different areas which he has not been able to perform or built: he presented the suggestion of a different kind of prebiotic experiments which were not conducted (Pattee 1965a, b; Shapiro 1986, p.301); he achieved the formal description of simple "hereditary" automata which were not built (Pattee 1961, 1966, 1968a); he contributed to the description and computer simulation of ecosystems (Conrad & Pattee, 1970)in the early 70's, long before anything similar would be attempted in Artificial Life, which he did not personally pursue (they were followed up by Conrad, i.e., Conrad 1981, see 1983for further references).

Most probably there are plenty of reasons why those attempts were not carried out, some imputable to the very proposals, some to Pattee himself, and others (most) to the heterogeneous conditions of possibility required to be met for the successful development of a scientific hypothesis. This is not the place to discuss research policies or which ever other conditionings that affect effective pursuing of lines of research. But, as any practicing scientist knows (at least intuitively though they do not often admit it), they are not restricted to the putative "quality", truthfulness, plausibility or potentiality of results of the hypothesis. Besides this common sense discernment, more recently, both the history and the philosophy of science (and specially the area of *science studies* or social studies of science, SSS) is showing the fruitfulness of paying thorough attention to all kinds of practical circumstances (institutions, financing sources, dominant views and prejudices of all sorts, legitimate or spurious interests, places of publication, etc.). In this sense, it would take a detailed sociological and historical survey of each of those alleged failures in order for us to be able to assess them.

8. INFLUENCE.

Before anything else, we should stress the extreme importance of Pattee's teaching, i.e., his most direct legacy. Over the years Pattee has dedicated a good amount of attention to the instruction of researchers who have contributed to develop various aspects of the core questions that have always interested him. He has been able to attract students from different backgrounds who have later pursued diverse specific lines of research, but always departing from and pursuing Pattee's general approach. Just to mention a few, we could choose, for instance, the Pattee Group (as they labeled it) that met in the late 80s: Peter Cariani, Michael Kelly, Eric Minch and Dennis Waters. There have, of course, been others earlier and later, as the editors of this issue Michael Conrad and Luis Rocha, as well as other contributors (Vahe Bedian, Cliff Joslyn, and Eileen Way). This does not mean that anything like a scientific or philosophical school was formed, but that the underlying motivation and the kind of problems characteristic of Pattee's thinking are being worked out, within their own disciplines, by people scattered around both geographically and academically.

Besides that, Pattee's work is and has been largely influential also in print. It is quite easy to find references to his work in monographs or articles, especially in those dealing with issues of complexity, hierarchies or complementarity. Therefore, a pertinent caveat to be made here is that even the most superficial survey ofcitations to his papers would be beyond the scope of this review and would excessively elongate its bibliography. Such a survey should at least cover work ranging from Whyte's approving citation mentioned ahead or Waddington's and other authors' references in the context of the *Towards a Theoretical Biology* symposia to, for instance, the recent book by Margulis and Sagan (1995).

8.1.- Biological science.

At the beginning of his academic career in the late1960s and the early 1970s, Pattee participated in some of the most important transdisciplinary conferences of the time dealing with frontier issues: Second International Conference on "The Origins of Prebiological Systems" in 1963(with Oparin, Haldane, Bernal, Pirie, Fox, etc., papers published in 1965(Fox, 1965)); the symposia "Towards a Theoretical Biology" at Villa Serbelloni, Lake Como in 1966, 1967,1968 and 1969 organized by C.H. Waddington under the auspices of the International Union of Biological Sciences (I.U.B.S.) and sponsored by the Rockefeller Foundation (Waddington 1968-1972); the

Interdisciplinary Symposium on "Hierarchical Structure in Nature and Artifact" in 1968 (Whyte, Wilson & Wilson 1969); the informal colloquium organized by E.W. Bastin and D. Bohm in Cambridgein 1968 on "Quantum Theory and Beyond" (Bastin, 1971); etc.2

This will be a constant mark of Pattee's production (inter/transdisciplinary character from the very beginning): applying brand new advances in automata theory and computer science together with a robust physical perspective to ardent biological issues as those of the origin and organization of life and organisms. This will be reflected on the variety and kind of places of publication of his papers and will eventually affect the chances of having an attentive and answering audience.

In this context, we should not overlook the importance of Waddington's *Towards a Theoretical Biology* symposia, not only for Pattee himself or for the evaluation of the repercussion of his presentations, but to the general development of a dedicated area of research focused on issues of complexity, in current terminology. Pattee was one of the few participants, besides Waddington himself, who attended the four meetings of the series and gave a paper at all of them. There Pattee, as early as 1966, delivered a key paper which is already entitled "The physical basis of coding and reliability in biological evolution" (Pattee 1968b). Two more papers on "Physical problems of heredity and evolution" (Pattee 1969b) and on "The problem of biological hierarchy" (Pattee 1970) followed up the issues, which are resumed in the last one of the series on "Laws and constraints, symbols and languages" (Pattee1972b). These papers are among the most cited ones of his production. Waddington himself, in the epilogue to the series, takes ground on Pattee's proposals:

> "Now the living world, as Pattee points out, is founded on a dualism of a kind which can be regarded in this way. At a relatively simple level, we have the genotype, of relatively unreactive DNA, in which no one could workup much interest were it not that it can act as a symbol coding for the much more reactive phenotype, the proteins which perform operations on their surroundings. It is on the phenotypes that our immediate interest is directed as natural predatory members of the biosphere. So is that of natural selection; (...). Returning to Pattee's essay; he argues that a symbol can only function as such when it is part of a system of symbols. A word must be a word in a language. To his own question 'How does a molecule become a message' he answers that it is inadequate to say 'when it codes for some other molecule in which we are more interested', as a nucleic acid may code for a protein; such a reply tacitly assumes the existence of ribosomes, polymerases, activated aminoacids and so on—a whole linguistic apparatus, a grammar. The 'structures mediating global simplicity' which we have to search for in the theory of general biology are, then, perhaps profitably to be compared with languages; based on the primary biological disjunction between genotype and phenotype as the analogue of symbol-symbolized, but going as much beyond it as a structured grammar is beyond a single word. And once we have a language, we can have a metalanguage; we can define one word in terms of a set of other words."(Waddington 1972, pp. 285-286)

8.2. Philosophy of biology.

In the 1960s, when Pattee was already concerned with theoretical issues regarding Biology (especially new concepts, terms and ways of thinking that were being developed in those years of molecular biology success), there was not really any Philosophy of Biology as such, besides the theorizing made by the very biologists (quite satisfactory, by the way), except Beckner (1959), Goudge(1961), or some papers on reductionism issues or philosophical debates regarding aspects of evolutionary theory (for instance, Grene 1959, Schaffner 1967, Scriven 1959). Hull's pessimistic appreciation in 1969 is quite illuminating.

By the 1970s something began to change, a full-fledged Philosophy of Biology grew as a separate field of research yet firmly rooted in contemporary philosophy of science and, as such, manifesting its best lesson, i.e., that a philosophical account of any scientific issue should fulfill as its first requisite a proper scientific acquaintance with the subject matter. Unfortunately for Pattee, this developing field was still centered around questions of Evolutionary Biology (regarding its structure as a theory, the status of natural selection, the concept of species, the units of selection, the alleged choice between lawful descriptions or historical narratives, etc.) or dealing with issues of reductionism (in general or specific cases as that of Mendelian to molecular genetics) and the autonomy or provincialism of biology. Most questions regarding aspects of origins and nature of life itself, inheritance, or development were neglected for many years in this approach, and those were precisely the ones which attracted Pattee's interest and examination3. Therefore, hopes for a Theoretical Biology in the lineage of Waddington's attempt were still unfulfilled.

Probably, only now do we witness the growth of new and more suitable trends where his work could be best incorporated. I would, at least, mention the following:

- Historical and philosophical interest on Molecular and Developmental Biology is increasing within established Philosophy of Biology (as we have previously seen with Kay, Keller or Sarkar to name just a few);
- Alternative views in Biology (Structuralism, Developmental Systems Theory, Thermodynamic and Informational approaches, ...) are getting a larger share of discussion (to get a glimpse of further work and previous references, see two recent collections of papers: van de Vijver, Salthe & Delpos 1998and Chandler & van der Vijver 2000);
- A wholly renewed area of Biosemiotics is beginning to be recognized (i.e., Hoffmeyer 1997, Salthe 1993).

Therefore we could conclude that, since 1969,

> "Pattee's question of how a molecule becomes a message has not yet been answered, but something else of note has begun to happen." (Keller 1995, p. 110).

8.3.- Other areas.

Even if they are not the main target of this review, we should not finish this point without mentioning the related areas of Cybernetics and Systems Science, Artificial Intelligence and Artificial Life, and the new cluster forming around theories of complexity. This is undoubtedly the composite area where Pattee has exerted a greater deal of direct influence. This, of course, does not soften any degree of peripherality regarding Biology or Philosophy:

> "The simplicity/complexity issue has only been scratched at the surface, philosophically speaking, and then usually by people who are rather peripheral in the profession like Simon (...) and Pattee (...)." (Callebaut1993, p. 232)

Anyway, as we have mentioned above, his work on hierarchy theory remains among the best state of the art in Complex Systems Theory and is also very influential in other fields such as Ecological Science (v.g., Allen & Starr1982) and Sociology (in this case together with the idea of complementarity).

His contribution to AI and cognitive science debates has been difficult due to the extreme dichotomy established among extreme computationalist and dynamicist perspectives, where no space is left in between for more complex positions. Nevertheless, a whole new area of research centered around issues of complexity (properly deserving Simon's "Sciences of the Artificial" denomination), which goes beyond AI and includes Artificial Life, offered new opportunities. Pattee was invited to the first AL conference where the paper he delivered (Pattee 1988) was assigned a distinguished position in the published proceedings and has also been included in the first, and only, anthology on the Philosophy of Artificial Life (Boden,1996). Unfortunately, his clear-cut distinction between simulations and realizations and his emphasis on the strict conditions (including symbol-matter relation, evolvability, etc.) to be fulfilled by AL models, earned him the reputation of a hard skeptical critic of the whole enterprise.

9. CONCLUSION.

We can not but end up saying that none of the problems that interest Pattee have been solved using only physical laws, e.g., origin of life, origin of codes, origin of coherent enzymatic controls, origin of semantic closure, etc. Pattee has helped largely to clarify them but his contribution would be greater if more biologists and philosophers realized that these are fundamental problems that must be solved to make sense of life.

A fair amount of work remains to be done to develop this collection of ideas in manners and ways suitable to a wide range of prospects. We might mention at least three methodological and one substantive realms (among others) where this work should be pursued by willing researchers:

a. A careful task of disentangling, unfolding and working out in detail these ideas and their potentialities. This should be aimed at getting some rather systematic, explicit structure.

b. A double attempt at connecting those ideas with other areas of research in order to establish some basis for further work and, also, to find out which of their terms, ways of research and traditional formulations of shared problems could be bridged over easily and cogently. Those should be tried, apart from Biological and Complexity Sciences, with Philosophy and History of Biology, Philosophy of Science, Evolutionary Studies, Social Studies of Science.

c. A more difficult effort is to push for the due recognition and some legitimate credit assignments. This might be vain, but worth trying.

d. Finally, a substantive effort should be made in order to answer Pattee's questions and to develop his ideas. This attempt should aim at obtaining models and elaborate concepts that can be profitably used in the Sciences as well as in the building of a philosophical view.

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NOTES

1. His personal career is clearly indicative of this general difficulty and of the more specific bias against theory in biology. First the Biophysics program at Stanford, which he helped to establish, and then the Center for Theoretical Biology at SUNY Buffalo, where he was research professor from 1971 to 1975 (together with James Danielli as its head, Robert Rosen and Ludwig von Bertalanffy), were retrenched because of that bias.

2. It may be worth noting that this kind of meetings and areas of research, with the qualified exception of the first one, have not got any continuity.

3. His 1971 paper on "Physical Theories of Biological Coordination" was reprinted in 1976 in one of the first anthologies in the Philosophy of Biology (Grene & Mendelsohn 1976).

FIGURES

Fig. 1: Hertz's diagram of the modeling relation

```
                    logical
Image    ------------------->  Image
  I                              I
Object   ------------------->  Object
```

material

BIBLIOGRAPHY

Ahl, V. & Allen, T.F.H. (1996) *Hierarchy theory*. New York: ColumbiaUniversity Press.

Allen, T.F.H. & Starr, T.B.(1982) *Hierarchy: Perspectives for ecological complexity*. Chicago: TheUniversity of Chicago Press.

Barkow, J.H., Cosmides, L. & Tooby, J.(eds.) (1992) *The adapted mind*. Oxford: Oxford University Press.

Bastin, T. (ed.) (1971) *Quantum theory and beyond*. Cambridge: Cambridge University Press.

Beckner, M. (1959) *The biological way of thought*. New York: Columbia University Press.

Boden, M.A. (ed) (1996) *The Philosophy of Artificial Life*. Oxford: Oxford University Press.

Callebaut, W. (ed.) (1993) *Taking the naturalistic turn*. Chicago: The University of Chicago Press.

Chandler, J. & van de Vijver, G. (eds.) (2000) *Closure*. New York: New York Academy of Sciences.

Conrad, M. (1981) Algorithmic specification as a technique for computing with informal biological models. *BioSystems* **13**: 303-320.

Conrad, m. (1983) *Adaptability The Significance of Variability from Molecule to Ecosystem*. New York:Plenum.

Conrad, M. & Pattee, H.H. (1970) Evolution experiments with an artificial ecosystem. *Journal of Theoretical Biology* **28**: 393-409.

Cziko, G. (1995) *Without miracles*. Cambridge, MA: The MIT Press

Dennet, D.C. (1995) *Darwin's dangerous idea*. New York: Simon &Schuster.

Fox, S. (ed.) (1965) *The origins of prebiological systems*. New York: Academic Press.

Goudge, T.A. (1961) *The ascent of life*. Toronto: University of Toronto Press.

Grene, M. (1959) Two evolutionary theories(parts I & II). *British Journal for the Philosophy of Science* **9**:110-127 & 185-194.

Grene, M. & Mendelsohn, E. (eds.) (1976)*Topics in the Philosophy of Biology*. Dordrecht: Reidel.

Hertz, H.R. (1894) Introduction to *The Principles of Mechanics*. In J.E. Mulligan (ed.) *H.R. Hertz(1857-1894). A Collection of Articles and Adresses* (1994), New York, NY: Garland Publishing [Reprint 1956, New York: Dover].

Hoffmeyer, H. (1997) *Signs of meaning in the universe*. Bloomington, IN: Indiana University Press.

Hull, D. (1969) What philosophy of biology is not. *Journal of the History of Biology* **2(1)**: 241-268.

Kay, L.E. (1993) *The molecular vision of life*. Oxford: Oxford University Press.

Kay, L.E. (2000) *Who wrote the book of life? A history of the genetic code*. Stanford: Stanford University Press.

Keller, E.F. (1990) Physics and the emergenceof molecular biology. *Journal of the History of Biology* **23(3)**: 389-409.

Keller, E.F. (1995) *Refiguring life*. New York: Columbia University Press.

Kitcher, P. (1992) The naturalists return. *Philosophical Review* **101**: 53-114.

Lewontin, R.C. (1982) Organism and environment. In H.C. Plotkin (ed.) *Learning, development, and culture*, New York: Wiley, pp.151-170.

Lewontin, R.C. (1983) The organism as the subject and object of evolution. *Scientia* **118**: 65-82.

Lewontin, R.C., Rose, S. & Kamin, L.J. (1984) *Not in our genes*. New York: Pantheon Books.

Margulis, L. & Sagan, D. (1995) *What is Life*. New York: Simon &Schuster

Mayr, E. (1961) Cause and effect in biology. *Science* **134**:1501-1506.

Mayr, E. (1976) *Evolution and the diversity of life*. Cambridge, MA: Harvard University Press.

Mayr, E. (1982) *The growth of biological thought*. Cambridge, MA: Harvard University Press.

Mayr, E. (1988) *Toward a new philosophy of biology*. Cambridge, MA: Harvard University Press.

Moreno, A. & Umerez, J. (2000) Downward causation at the core of living organization. In *Downward causation*, P.B. Anderson, C. Emmeche, N.O.Finnemann & P.V. Christiansen (eds.), Aarhus(DK): Aarhus University Press,pp. 99-117.

Oyama, S. (1985) *The ontogeny of information*. Cambridge: Cambridge University Press.

Pattee, H.H. (1961) On the Origin of Macromolecular Sequences. *Biophysical Journal* **1**: 683-710.

Pattee, H.H. (1965a) The Recognition of Hereditary Order in Primitive Chemical Systems. In S. Fox (ed.) *The Origins of Prebiological Systems*, New York: Academic Press, pp.385-405.

Pattee, H.H. (1965b) Experimental Approaches to the Origin of Life Problem. In F.F. Nord (ed.) *Advances in Enzymology, vol. 27*, New York: Wiley,pp. 381-415.

Pattee, H.H. (1966) Physical Theories, Automata and the Origin of Life. In H. Pattee, E. Edelsack, L. Fein & A. Callahan (eds.) *Natural Automata and Useful Simulations*, Washington DC: Spartan Books, pp. 73-104.

Pattee, H.H. (1967) Quantum Mechanics, Heredity and the Origin of Life. *Journal of Theoretical Biology*, **17**: 410-420.

Pattee, H.H. (1968a) Automata Theories of Hereditary Tactic Copolymerization. In A.D. Ketley (ed.) *The Stereochemistry of Macromolecules, vol. 3*, New York: Marcel Dekker, pp. 305-331.

Pattee, H.H. (1968b) The Physical Basis of Coding and Reliability in Biological Evolution. In C.H. Waddington (ed.) *Towards a Theoretical Biology 1, Prolegomena*, Edinburgh: Edinburgh University Press,pp.67-93.

Pattee, H.H. (1968c) Comments by H.H. Pattee. In C.H. Waddington (ed.) *Towards a Theoretical Biology 1, Prolegomena*, Edinburgh: Edinburgh University Press, pp.: 219-220.

Pattee, H.H. (1969a) Physical Conditions for Primitive Functional Hierarchies. In L.L. Whyte, A.G. Wilson & D. Wilson (eds.) *Hierarchical Structures*, New York: American Elsevier, pp. 161-177.

Pattee, H.H. (1969b) Physical Problems of Heredity and Evolution. In C.H.Waddington (ed.) *Towards a Theoretical Biology 2, Sketches*, Edinburgh: Edinburgh University Press, pp. 268-284.

Pattee, H.H. (1969c) How Does a Molecule Become a Message?. *Developmental Biology Supplement*, **3**: 1-16

Pattee, H.H. (1970) The Problem of Biological Hierarchy. In C.H. Waddington(ed.) *Towards a Theoretical Biology 3, Drafts*, Edinburgh: Edinburgh University Press, pp. 117-136.

Pattee, H.H. (1971a) Can Life Explain Quantum Mechanics? In T. Bastin(ed.)*Quantum Theory and Beyond*, New York: Cambridge University Press,pp.307-319.

Pattee, H.H. (1971b) Physical Theories of Biological Co-ordination. *Quaterly Reviews of Biophysics*, **4(2/3)**:255-276 [Reprinted in M. Grene & E. Mendelsohn (eds.) (1976) *Topics in the Philosophy of Biology*, vol. 27 of *Boston Studies in the Philosophy of Science*, Dordrecht: Reidel].

Pattee, H.H. (1972a) Physical Problems of Decision-Making Constraints. *International Journal of Neuroscience*,**3**: 99-105.

Pattee, H.H. (1972b) Laws and Constraints, Symbols and Languages. In C.H.Waddington (ed.) *Towards a Theoretical Biology 4, Essays*, Edinburgh: Edinburgh University Press, pp. 248-258.

Pattee, H.H. (1972c) The Evolution of Self-Simplifying Systems. In E.Laszlo (ed.) *The Relevance of General Systems Theory*, New York: Braziller, pp. 33-41.

Pattee, H.H. (1972d) The Nature of Hierarchical Controls in Living Matter. In R. Rosen (ed.) *Foundations of Mathematical Biology, vol. 1*, New York: Academic Press, pp. 1-22.

Pattee, H.H. (ed.) (1973a) *Hierarchy Theory. The Challenge of Complex Systems*. New York: Georges Braziller.

Pattee, H.H. (1973b) The Physical Basis and Origin of Hierarchical Control. In H.H. Pattee (ed.) *Hierarchy Theory*, New York: G. Braziller, pp.73-108.

Pattee, H.H. (1973c) Postscript. Unsolved Problems and Potential Applications of Hierarchy Theory. In H.H. Pattee (ed.) *Hierarchy Theory*, New York: G. Braziller, pp. 131-156.

Pattee, H.H. (1973d) Physical Problems of the Origin of Natural Controls. In A. Locker (ed.) *Biogenesis, Evolution, and Homeostasis*, Heidelberg: Springer Verlag, pp. 41-49.

Pattee, H.H. (1978) Biological Systems Theory: Descriptive and Constructive Complementarity. In G.J. Klir (ed.) *Applied General Systems Research*, New York: Plenum, pp. 511-520.

Pattee, H.H. (1982) Cell Psychology: An Evolutionary Approach to the Symbol-Matter Problem. *Cognition and Brain Theory*, **5 (4)**:325-41.

Pattee, H.H. (1985) Universal Principles of Measurement and Language Functions in Evolving Systems. In J.L. Casti & A.Karlqvist (eds.) *Complexity, Language, and Life: Mathematical Approaches*, Berlin: Springer, pp. 268-281 [Reprinted in G.J. Klir (1991) *Facets of Systems Science*, New York: Plenum, pp. 579-592].

Pattee, H.H. (1988) Simulations, Realizations, and Theories of Life. In C.Langton (ed.) *Artificial Life I*, Redwood City, CA: Addison-Wesley, pp.63-77.

Pattee, H.H. (1995) Artificial Life Needs a Real Epistemology. InF.Mor‡n, A. Moreno, J.J. Merelo & P. Chac—n (eds.) *Advances in Artificial Life*, Berlin: Springer, pp. 23-38.

Pattee, H.H. (2000) Causation, control, and the evolution of complexity. In *Downward causation*, P.B. Anderson, C. Emmeche, N.O. Finnemann & P.V.Christiansen (eds.), Aarhus, DK: Aarhus University Press, pp.63-77.

Polanyi, M. (1968) Life's irreducible structure. *Science* **160**: 1308-1312.

Salthe, S. (1985) *Evolving Hierarchical Systems*. New York: Columbia University Press.

Salthe, S. (1993) *Development and Evolution*. Cambridge, MA: MIT Press.

Sarkar, S. (1996) Biological information: A skeptical look at some central dogmas of molecular biology. In S. Sarkar(ed.) The philosophy and history of molecular biology: New perspectives. Dordrecht: Kluwer, pp. 187-231.

Schaffner, K.F. (1967) Antireductionism and molecular biology. *Science* **157**: 644-647.

Scriven, M. (1959) Explanation and prediction in evolutionary biology. *Science* **130**: 477-482.

Shapiro, R. (1986) *Origins*. New York: Summit Books.

Simon, H.A. (1962) The architecture of complexity. *Proceedings of the American Philosophical Society* **106**: 467-482 [Reprinted in H.A. Simon (1969) pp. 192-229].

Simon, H.A. (1969) *The sciences of the artificial*. Cambridge, MA: MIT Press.

Simon, H.A. (1973) The organization of complex systems. In H.H. Pattee(ed.) *Hierarchy Theory*, pp. 3-27.

Stuart, C.I.J.M. (1985a) Physical Models of Biological Information and Adaptation. *Journal of Theoretical Biology***113(3)**: 441-54.

Stuart, C.I.J.M. (1985b) Bio-informational Equivalence. *Journal of Theoretical Biology* **113(4)**: 611-36.

Umerez, J. (1995) *Semantic Closure*: A guiding notion to ground Artificial Life. In *Advances in Artificial Life*, F. Mor‡n, A. Moreno, J.J. Merelo & P. Chac—n (eds.), Berlin: Springer, pp. 77-94.

Umerez, J. (1998) The evolution of the symbolic domain in living systems and Artificial Life. In *Evolutionary Systems*, G. van de Vijver, S.N. Salthe & M. Delpos (eds.), Dordrecht: Kluwer, pp. 377-396.

Umerez, J. & Moreno, A. (1995) Origin of Life as the First MST – Control Hierarchies and Inter level Relation. *World Futures. The Journal of General Evolution* **45**: 139-154.

van de Vijver, G., Salthe, S. & Delpos, M. (eds.) (1998) *Evolutionary Systems*. Dordrecht: Kluwer.

von Neumann, J. (1951) The general and logical theory of automata. In *Cerebral mechanisms in behavior*, L.A. Jeffress (ed.), New York: John Wiley& Sons, pp. 1-31.

von Neumann, J. (1955) Mathematical foundations of quantum mechanics. Princeton, NJ: Princeton University Press.

von Neumann, J. [ed. by A.W. Burks] (1966) *Theory of Self-Reproducing Automata*. Urbana, ILL: University of Illinois Press.

Waddington, C.H. (ed.) (1968-1972) *Towards a Theoretical Biology* (4vols.). Edinburgh: Edinburgh University Press & Chicago, ILL: Aldine.

(1968) vol. 1. *Prolegomena*.

(1969) vol. 2. *Sketches*.

(1970) vol. 3. *Drafts*.

(1972) vol. 4. *Essays*.

Waddington, C.H. (1972) Epilogue. In *Towards a Theoretical Biology* (vol. 4. *Essays*), C.H. Waddington(ed.), pp. 283-289.

Whyte, L.L., Wilson, A. & Wilson, D. (eds)(1969) *Hierarchical Structures*. New York: American Elsevier.

Whyte, L.L. (1965) *Internal Factors in Evolution* . New York:G.Braziller.