René THOM

Interviews with Emile NOËL

Translation from the French, of
Prédire N'est Pas Expliquer
(To Predict is not to Explain)

Conversations on Mathematics, Science, Catastrophe Theory, Semiophysics, and Natural Philosophy

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Glossary by Prof. Alain Chenciner
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Chapter 4 of the Lisker translation was not in the French editions. We included it because it was a 1994 interview of Thom with Emile Noël and felt there was some continuity with the topics discussed in the first three chapters. "Pour une théorie de la morphogènese" (Towards a Theory of Morphogenesis) is found in Noël's book, *Les sciences de la forme aujourd'hui* and in Thom's *Oeuvres Complètes (2003).* Permission was given by Emile Noël to include this chapter.

It was our hope that Flammarion (Paris), or the IHES (Bures-sur-Yvette) would publish the English translation. Failing that it was our hope that Flammarion would give us publication rights so that we can seek another publisher.

Roy Lisker and S. Peter Tsatsanis

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TRANSLATOR'S NOTE

My first encounter with René Thom was in the winter of 1985. He'd come to the University of California at Berkeley where he gave a seminar devoted to his research. After the seminar I handed him two articles of mine, both on the topic of causation in physics. When I wrote to him later, he confessed that he'd misplaced them while traveling and asked that I send copies of them to his offices at the Institut des Hautes Études Scientifiques in France. I did so, and within the month I received his reply.

He was very enthusiastic about my paper "Algebraic Causation", and invited me to look him up should I be in France.

In June of the following year, I went to Europe to attend the 11th General Relativity and Gravitation Congress in Stockholm, which was taking place in July. Before going on to Stockholm I got in touch with René Thom and he arranged to meet me outside the École Polytechnique in Paris. We had a good conversation before going in to hear his lecture at the École Polytechnique. Most amazingly, he made reference to my paper in his talk!

When I arrived in Stockholm, his letter of recommendation for my article was essential to my being admitted to the conference free of charge, and to my being allowed to present "Algebraic Causation" in a poster session. Through some of the people who read it and discussed it with me afterwards, I was able to find the translation work that enabled me to live in Paris all through the winter of 1988-89.

During that time, I encountered René Thom many times. His kindnesses and considerations on my behalf were numerous.

Also in that year (1988), I was honored to be able to attend the Thom Symposium that was held at the Institut Henri Poincaré to commemorate his retirement from the IHES.

The news of his death was saddening to me, even more so as it came at the end of a long period in which he was rendered mentally incapacitated by a brain hemorrhage.

René Thom was that rare combination of world-class mathematician with first-class philosopher of science. It has been a great delight to me to translate the interviews in "Prédire N'est Pas Expliquer". His language and manner of expression is clarity itself for anyone possessing the basic requisites of a mathematical training. Even for those who are unable to follow all its arguments in detail, I recommend this translation to all scientific and scientifically-minded readers.

Dr. Roy Lisker
Middletown, Connecticut
July 20, 2010

EDITOR'S NOTE

My motivation in starting this project (the translation of some of René Thom's work) was to bring to the scientific community and to the general public some of Prof. Thom's philosophical and general ideas as told to interviewers who had some scientific and/or cultural background.

Many English writers of books that deal with scientific topics of interest to Prof. Thom or with topics that were Prof. Thom's creations (discoveries?) fail to mention or in some cases fail to give credit to Prof. Thom probably out of ignorance of his works. A simple example that comes to mind is *The Tipping Point* by Malcolm Gladwell who fails to mention Prof. Thom or Catastrophe Theory. What is a tipping point but a catastrophe point? Another example is a consistent failure of most science writers and scientists to state that the *attractor* concept and the word were first used by Prof. Thom in the late 1960s when he was doing work on Dynamical Systems and what later became known as Catastrophe Theory. By publishing this work (and in the near future Prof. Thom's *Paraboles et Catastrophes* (Parables, Parabolas and Catastrophes), I am hoping that his work will be better known and less ignorance of his work will be the new norm.

Roy Lisker and I began this project in the fall of 2006. I had read, in Ferment Magazine, Roy's account of a meeting with Prof. Thom in the 1980s and his partial translation of Alexandre Grothendieck's autobiography (also in Ferment) and I tried and succeeded in getting him interested in the project. It has been a great pleasure working with him and visiting him in Middletown, Connecticut these past few years. This eclectic man is interesting in his own right. The interests he has span the sciences and the arts. A book of some of his short stories may be published soon. As soon as Roy knows, I'm sure he will inform us in Ferment.

His translation is excellent and his own insights into some of the topics in the book have been extremely beneficial to me. I thank him for this.

I hope that the reading of this work will give the reader at least as much pleasure as it has given me. If there is any reality to our existence, it is found in the memories of those that survive us.

I thank my family, wife Paula and my two daughters Katherine and Linda, for putting up with my interest in Thom's work since the late 1970s. I hope they forgive me for beginning many conversations with: "Well, according to Thom..." or "in Catastrophe Theory..."

In editing this work, I may have overdone the use of commas, so I ask for understanding in advance. Excuses must also be made for translating "common" Latin phrases. I think many young people (and a few Ph. D. candidates) are not familiar with these terms. My apologies in advance if I have offended anyone. Terms of a mathematical nature or those in Prof. Chenciner's glossary have been **bolded** and references or editorial comments have been placed in

[square brackets]. The bibliographies, I hope, are complete. If I omitted a reference or did not credit a work properly, or if you have any comments on Prof. Thom's work or Roy Lisker's translation, please let me know by email. My email address is petertsa@hotmail.com and please use Thom as the subject line (just in case some of the emails end up in my "junk" folder). If there is some specific article of Prof. Thom's work that you can't get a hold of, please let me know and I may be able to help.

S. Peter Tsatsanis
Toronto, Ontario
July, 2010

INTRODUCTION

René Thom's immortality is assured in the annals of Mathematics and History. His neologisms *attractor*, *basin of attraction*, *catastrophe point* and *Semiophysics* are part of this. In 1958, he was awarded the Fields Medal, the highest honor a mathematician can attain. Jean-Luc Goddard made the film *René* about Thom and Salvador Dali created his last paintings based on Thom's Catastrophe Theory (CT).

In the annals of Mathematics, his name is associated with many concepts, theorems and conjectures. Among some of the concepts, we find *Thom class, Thom algebra, Thom space, Thom isomorphism, Thom homomorphism, Thom spectra, Thom prespectra, Thom functor, Thom stratification, Thom polynomials, Thom complexes, Thom diagonal, Thom homology operations, Thom map*, and *Thom encoding*. Among the theorems, we find *Thom isomorphism theorem, Thom isotopy theorem (lemma), Thom manifold theorem, Thom cobordism theorem, Thom splitting lemma, Thom transversality theorem* and *Thom classification theorem (of elementary catastrophes).* Many of Thom's conjectures have been proven. Some of these include the *Genericity of Stability*, *Genera of surfaces in* $\mathbf{CP}^2$ and its generalizations, the *Kähler-Thom conjecture* and the *Symplectic Thom conjecture* and *Thom's conjecture on triangulation of maps*.

These concepts, theorems and conjectures are found in Thom's fields of interest which at one time or another included Algebraic Geometry, Algebraic Topology, Differential Topology, Singularity Theory, Bifurcation Theory, Dynamical Systems Theory and CT.

His work on Cobordism, for which he received the Fields Medal, is adequately covered in [Hopf, 1960/2003], [Milnor, 1957 and 1958], [Basu et al, 2003] and the *Bulletin of the AMS*, **41**(3), 2004. It is instructive to note that [Hopf, 2003: 75], in discussing Thom's work at the Fields Medal ceremony in 1958, remarked that "only few events have so strongly influenced Topology and, through topology, other branches of mathematics as the advent of this work." On page 76, he writes, "One of the, by no means trivial, insights which Thom had obviously from the beginning was that the notion of cobordism is particularly suited for the study of differential manifolds." He concludes on page 77 with: "These ideas [on cobordism – ed.] have significantly enriched mathematics, and everything seems to indicate that the impact of Thom's ideas – whether they find their expression in the already known or in forthcoming works – is not exhausted by far." May, in [May, 1975: 215], says that "Thom's discovery that one can classify smooth closed n-manifolds up to a weaker equivalence relation of "cobordism" is one of the most beautiful advances of twentieth century mathematics.” Although it is acknowledged that the modern theory of the topology of manifolds began with H. Whitney’s work in the 1930’s,

James, in [James, 1999: 876], writes: “However, its real development began after Thom’s work on cobordism theory in 1954 …”

Thom’s work in Differential Topology and Singularity Theory is adequately covered in [Haefliger, 1988] and [Teissier, 1988]. In [Seade, 2006: 6], we read that “Thom, in 1964, gave interesting ideas for the use of Morse Theory to study foliations on smooth manifolds.” Nitecki, in [Nitecki, 1971: 28] writes about Thom’s work on transversality. He says: “The main reason for introducing transversality is its usefulness in finding generic properties of maps. This utility stems from genericity, in very general circumstances, of the property of transversality itself. The prototype of such genericity theorems, due to Thom, says that for highly differentiable maps, transversality to a fixed submanifold is generic.” Bruce and Mond, in [Bruce and Mond, 1999: ix], write: “Thom was led to the study of singularities while considering the question whether it is possible to represent homology classes in smooth manifolds by embedded sub-manifolds. With his Transversality Theorem (1956), he gave the subject a push towards a kind of Modern Platonism.” On page x, they state: “Thom saw the jet bundle as a version of the Platonic world of disembodied ideas, partitioned into attributes (the orbits of the various groups which act naturally on jets) as yet unattached to objects (functions and mappings) which embody them." Thom defined a "jet bundle" as the space of Taylor polynomials (of a specific degree) of germs of maps from one smooth

manifold to another smooth manifold. They continue on page xi with: "Thom also contributed to the idea of versal unfolding. […] The term 'versal' is the intersection of 'universal' and 'transversal', and one of Thom's insights was that the singularities of members of families of functions or mappings are versally unfolded if the corresponding family of jet extension maps is transverse to their orbits (equivalence classes) in jet space." They conclude on the same page with: "This insight, and Thom's Platonist leanings, led him to Catastrophe Theory [so named by Christopher Zeeman – ed.]. He identified and described the seven orbits of function singularities which can be met transversally in families of four or fewer parameters: these were his seven elementary catastrophes, which were meant to underlie all abrupt changes (bifurcations) in generic four-parameter families of gradient dynamical systems. […] Many of Thom's ideas in bifurcation theory and gradient dynamical systems have provided the basis for later development, and the controversy surrounding CT should not mask the importance of his contribution to the subject."

Much has been written about CT and some references are found at the end of this Introduction. What follows is a brief outline of the theory.

First and foremost, CT is a mathematical theory. Its fundamental theme is the classification of critical points of smooth functions. The essential characteristics of a smooth function can be recognized by studying its embedding in a

smooth family of functions. As Thom pointed out in his first book, *Structural Stability and Morphogenesis* (*SSM*), this fact is of extreme importance for applications since natural phenomena are always subject to perturbations. CT has as its goal to classify systems according to their behavior under perturbation. When a natural system is described by a function of state variables, then the perturbations are represented by control parameters on which the function depends. This is how a smooth family of functions arises in the study of natural phenomena. An unfolding of a function is such a family: it is a smooth function of the state variables with the parameters satisfying a specific condition. Catastrophe Theory's aim is to detect properties of a function by studying its unfoldings.

In effect then, CT provides a framework for describing and classifying systems and events where significant qualitative changes of behavior in the system are caused by small continuous changes in parameters. Within this framework, it is possible to identify the essential variables in a problem, and provide a brief (and often to the point) universal description of that behavior.

In general, CT is used to classify how stable equilibria change when parameters are varied. The points in parameter space, at which qualitative changes in behavior occur, are examples of catastrophe points. Near these points, CT can provide a canonical form for the potential, which depends only upon the number of state variables and control parameters.

The theory should apply to *any gradient system where the force can be written as the negative gradient of a potential*. The points where the gradient vanishes are the critical points and CT is concerned with the degenerate points. At these points, the Hessian (matrix of second derivatives) plays an important role.

Thom showed that near the degenerate critical points, the function can be written as a sum of a quadratic form, defining a nondegenerate subspace (a Morse part), and a degenerate subspace (the non-Morse part). The non-Morse part of the function can be represented by a canonical form called a catastrophe function. This function is the sum of a catastrophe germ, containing the non-Morse point, and a universal unfolding, which removes the degeneracy of the critical point and makes the potential structurally stable.

Thom's classification theorem (for elementary catastrophes) lists these catastrophe germs and their unfoldings for functions whose codimension is at most four. There are only seven different types of degenerate critical points for such functions – what Thom called the seven elementary catastrophes. (This list has been expanded by Arnold's Russian school.) Thom used transversality as the main tool to prove the existence of universal unfoldings. He showed that any family of potentials depending on at most five parameters is structurally stable and equivalent around any point to one of these catastrophes.

Thom created a mathematically rigorous theory that showed "the true complementary nature of the seemingly

irreconcilable notions of versality and stability, that is, *preserving identity in spite of development.* [Castrigiano and Hayes, 2004: xv, emphasis added – ed.] Thom recognized that it was this feature that would be of great importance for a theory of cognition as discussed in his *SSM.*

For Thom, CT was a methodology, and as the subtitle of his first book *Structural Stability and Morphogenesis* states that it is *An outline of a general theory of models*. These models range from theoretical biology to semiotics. In his Forward to the book *Catastrophe Theory* by [Castrigiano and Hayes, 2004], Thom writes, on page ix, that mathematicians should see CT as "just a part of the theory of local singularities of smooth morphisms, or, if they are interested in the wider ambitions of this theory, as a dubious methodology concerning the stability (or instability) of natural systems." Castrigiano and Hayes call CT "an intriguing, beautiful field of pure mathematics […]. It is a natural introduction to bifurcation theory and to the rapidly growing and very popular field of dynamical systems." (page xi) And as Thom says on page x of the Forward: "the whole of qualitative dynamics, all the 'chaos' theories talked about so much today, depend more or less on it."

And what of morphogenesis? Thom discusses some aspects of morphogenesis in Chapter 4 of *SSM*. The birth and destruction of forms was the main thread in *SSM*. Louk Fleischhacker in [Fleischhacker, 1992: 248], writes that "Thom describes in an impressive way the possibility of grasping the development of individuals of a higher form

of life." This was accomplished via his principles of morphogenesis discussed in [Lu, 1976:166-180]. The first of these principles is the assertion "that the stability of any morphogenetic phenomenon [defined mathematically – ed.], whether represented by a gradient system or not, is determined by the attractor set of a certain vector field." [Lu, l976: 171] Stability for Thom is a "natural condition to place upon mathematical models for processes in nature because the conditions under which such processes take place can never be duplicated; therefore, what is observed must be invariant under small perturbations and hence stable." [Wasserman, 1974: v] Thom's second principle of morphogenesis states that "what is interesting about morphogenesis, locally, is the transition, as the parameter varies, from a stable state of a vector field to an unstable state and back to a stable state by means of a process which we use to model the system's local morphogenesis." [Lu, l976: 175] Wasserman, on page 157, writes that "The models given by Thom are only intended to be local models for natural processes anyway; a global description is obtained by piecing together a large number of such local descriptions." Thom's third principle of morphogenesis states that "What is observed in a process undergoing morphogenesis is precisely the shock wave and resulting configuration of chreods [zones of stability – ed.] separated by the strata of the shockwave, at each instant of time (in general) and over intervals of observation time." [Lu, 1976: 179] It then follows "that to classify an observed

phenomenon or to support a hypothesis about the local underlying dynamic, we need in principle only observe the process, study the observed 'catastrophe (discontinuity) set' and try to relate it to one of the finitely many universal catastrophe sets, which would then become our main object of interest." [Lu, 1976: 180]. Even if a "process depends on a large number of physical parameters (as is often the case in applications), as long as it is described by a gradient model, its description will involve one of seven elementary catastrophes; *in particular, one can give a relatively simple mathematical description of such apparently complicated processes even if one does not know what the relevant physical parameters are or what the physical mechanism of the process is.* And the number of parameters which are involved in the physical mechanism plays no role in the description." [Wasserman, 1974: 161, emphasis added – ed.]. In Thom's words: "if we consider an unfolding, we can obtain a 'qualitative' intelligence about the behaviors of a system in the neighborhood of an unstable equilibrium point." [Castrigiano and Hayes, 2004: ix]. According to Thom, it was this idea that was not accepted widely and was criticized by some applied mathematicians "because for them only numerical exactness allows prediction and therefore efficient action." [Castrigiano and Hayes, 2004:

ix]. Since the exactness of laws rests on analytic continuation which alone permits a reliable extrapolation of a numerical function, how can the theory of structural stability of differential systems on a manifold help here?

After the work of A. Grothendieck, it is known that the theory of singularity unfolding is a particular case of a general category – the theory of flat deformations of an analytic set and for flat local deformations of an analytic set only the hypersurface case has a smooth unfolding of finite dimension. For Thom, this meant that "if we wanted to continue the scientific domain of calculable exact laws, we would be justified in considering the instance where an analytic process leads to a singularity of codimension one (in internal variables). Might we not then expect that the process (defined, for example, as the propagation of an action) be diffused (in external variable) and subsequently propagated in the unfolding according to a mode that is to be defined? Such an argument allows one to think that the Wignerian domain of exact laws can be extended into a region where physical processes are no longer calculable but where analytic continuation remains 'qualitatively' valid." [Castrigiano and Hayes, 2004: ix]

The philosophical program Thom had in mind for CT was the geometrization of thought and linguistic activity. His Natural Philosophy aspirations were centered "on the necessity of restoring by appropriate minimal metaphysics some kind of intelligibility to our world." [*Semiophysics,* p. ix] On pages 218-220 of his *Semiophysics,* he writes:

"Modern science has made the mistake of foregoing all ontology by reducing the criteria of truth to pragmatic success. True, pragmatic success is a source of pregnance and so of signification. But this is an immediate, purely

local meaning. Pragmatism, in a way, is hardly more than the conceptualized form of a certain return to animal nature. Positivism battened on the fear of ontological involvement. But as soon as we recognize the existence of others and accept a dialogue with them, we are in fact ontologically involved. Why, then, should we not accept the entities suggested to us by language? Even though we would have to keep a check on abusive hypostasis, this seems the only way to bring a certain intelligibility to our environment. Only some realist metaphysics can give back meaning to this world of ours."

Thom is the first human being to give the first rigorously monistic model of the living being, and to reduce the paradox of the soul and the body to a single geometrical object. This is one of his greatest accomplishments. Even if some aspects of his model are incomplete or wrong, he has opened up a conceptual universe by this. As he says on page 320 of his *SSM*: "But in a subject like mankind itself, one can only see the surface of things. Heraclitus said, 'you could not discover the limits of the soul, even if you traveled every road to do so; such is the depth of its form.'" And so it is with Thom's work. Its importance is, as C. H. Waddington says, "the introduction, in a massive and thorough way, of topological thinking as a framework for theoretical biology. As this branch of science gathers momentum, it will never in the future be able to neglect the topological approach of which Thom has been the first significant advocate." [*SSM*: xxxi–xxxii]

Thom's thoughts ventured in many domains including biology, linguistics and semiotics. I conclude this Introduction with a comment about each of these areas and a further comment about the current state of affairs.

In his *SSM*, pages 290-291, Thom discusses the malignity of the human attractor. In a preamble on evolution, he writes: "Let us start with the very basic objection of the finalists to a mechanist theory of evolution: if evolution is governed by chance, and mutations are controlled only by natural selection, then how has this process produced more and more complex structures, leading up to man and the extraordinary exploits of human intelligence? I think that this question has only a single partial answer, and this answer [that there is a mathematical structure guaranteeing stability – ed.] will be criticized as idealistic. […] I think that likewise there are formal structures, in fact geometric objects, in biology which prescribe the only possible forms capable of having a self-reproducing dynamic in a given environment. […] Attraction of forms is probably one of the essential factors of evolution. Each eigenform (one might even say each archetype if the word did not have a finalist connotation) aspires to exist and attracts the wave front of existence when it reaches topologically neighboring eigenforms; there will be competition between these attractors, and *we can speak of the power of attraction of a form over neighboring forms, or its malignity. From this point of view it is tempting, with the present apparent halt in evolution, to think that the human attractor is too malignant*. Of the theoretically

possible living forms only very few are touched by the wave front and actually come into being". [Emphasis added – ed.] We should heed this insightful warning and recognize that the best chance for the survival of mankind is to slow down, in any possible meaningful way, the malignity of the human attractor. The comments of [Pérez Herranz, 2000] are very insightful on this topic.

In a discussion on language, Thom writes: "Thought is then a veritable conception, putting form on the dummy actant arising from the death of the verb, just as the egg puts flesh on the spermatozoid; thus thought is a kind of permanent orgasm. There is a duality between thought and language reminiscent of that which I have described between dreaming and play: thought is a virtual capture of concepts with a virtual, inhibited, emission of words, a process analogous to dreaming, while in language this emission actually takes place, as in play." [*SSM*, p. 313]

Thom's semiotics is a huge domain and the reader is referred to his *Semiophysics*, and the works of Jean Petitot, Wolfgang Wildgen and Laurent Mottron. Mottron puts it succinctly, in [Mottron, 1989: 92], when he writes, "Thom conceives the human mind as a tracing, a simulation, an 'exfoliation' of the outside world, constrained by the same *a priori* laws as the world. Thom uses ontogenetic examples to show that *a priori* semiotics and its psychological realizations overlap, supporting a realist philosophical position. This analogy cannot be reduced to a linear causality, in the sense that the human mind should be

governed by the same laws as the world because it comes out of the world; rather, it is explained by the universality of laws governing abstract and concrete dynamic conflicts. In catastrophe-theoretic tradition, a classification is justified by the generality of its application, not by quantitative validation."

Many have written and continue to write that the mathematics community became disillusioned with CT – the latest being George Szpiro who, in *Poincare's Prize* [Szpiro, 2007: 158-159], writes: "By and large, criticism was not directed against the mathematical underpinnings of Thom's work. Rather it centered on the indiscriminate use of the theory, […], for purported applications. Soon a backlash developed, and catastrophe theory, which had promised so much but produced so little outside of pure mathematics, sank into disrepute. Nowadays one hears little of it." Authors continue to write about this myth or remain ignorant about the import of Thom's work. Very few know that he was the one who introduced the 'attractor' concept which plays such a major role in so many areas. One has to search the literature to find many applications of Catastrophe Theory – from developmental biology [Striedter, 1998], fetal heart rates [Kikuchi et al, 2000], gravitational lensing [Petters et al, 2001] to recent results for phase transitions [Bogdan and Wales, 2004], energy landscapes [Wales, 2001, 2003], biological function [Viret, 2006] and biological systems theory [Gunawardena, 2010]. Simply, if one accepts the comments that Thom

made in his Forward to [Castrigiano and Hayes, 2004: ix] (pages ix-x of this Introduction) about extending the Wignerian domain of exact laws, CT will remain useful for a long time.

For the interested reader, the books by [Woodcock and Davis, 1978], [Saunders, 1980], [Postle, 1980] and [Arnol'd, 1992] are a good beginning although Arnol'd is unnecessarily harsh on Thom because Arnol'd could not accept Thom's comments about extending the Wignerian domain to regions "Where physical processes are no longer calculable but where analytic continuation remains 'qualitatively valid'". The books by [Gilmore, 1981], [Poston and Stewart, 1978] and [Castrigiano and Hayes, 1993/2004] are geared to mathematicians and physical scientists. The books by [Zeeman, 1977] and [Thom, 1975/1989] can be read by all scientists and interested readers. They are not easy but as Thom says: "I will not deny that communication will be difficult, […], but my excuse is an infinite confidence in the resources of the human brain!" [*SSM*: xxxiii]

S. Peter Tsatsanis
Toronto, Ontario
June, 2010

CHAPTER 1

HOW DOES ONE BECOME A MATHEMATICIAN?

You majored in mathematics. Is that right?

Well, yes, it was inevitable. Having passed the courses required for the examination for a high school (*lycée)* diploma (known as the *bac*), I had to choose between a philosophy major and one in introductory mathematics. It was commonly accepted that the latter opened up more opportunities than the former. This was probably a misconception, but we believed it. Furthermore it was 1939, the very beginning of the war. Our elders who had seen World War I said to us: Try to get into the artillery; it's safer than the infantry! To be accepted into the artillery one had to know mathematics. This consideration made a significant contribution to the birth of my mathematical vocation.

But one can get a high school diploma in mathematics without becoming a mathematician.

Absolutely! Of course mathematics had a real appeal for me. Euclidean geometry in particular fascinated me. On

the other hand, I have never been terribly excited by algebra.

Is that because you can draw things, you can see pictures?

Yes, but also because it stimulates the imagination in the manner of a riddle!, whereas with regards to algebra, I always had the impression that all of its problems are either trivial or on the other hand without very many practical applications. The intellectual stimulus is therefore much less …

Aren't there some things in it which can give satisfaction?

Algebra is largely a matter of being trained in certain calculating techniques. It's a form of cramming we call *taupinage**. Students are indoctrinated with a certain number of formulae, and methodologies of calculation which enable them to solve problems, notably those they will encounter in entrance examinations. To me this is not terribly good training, although it has some value as a basic discipline.

*** Translator's note: A *taupin* is a mathematics student who is studying to enter the "Grandes Écoles". The Grandes Écoles are special elite schools in France. See Chapter 1, Note 2.**

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In other words, there is a complicated procedure of some sort, but, once it's mastered, problems are solved by merely applying it?

More or less. The problems presented in these competitive examinations do demand a bit more than that: The contestant is expected to take the initiative. However, it is true that there are a small number of key sections of these examinations where one has to be very careful not to make mistakes.

So geometry is more creative?

Absolutely. It's more important in the formation of a mathematician than algebra is. It is possible to move gradually in the presentation of its problems, something one doesn't find in algebra, a subject in which one must make a great leap from problems that can be solved by the dumb application of an easily acquired formalism, to problems such as the solution of a fifth degree algebraic equation by radicals, which, furthermore, can't be solved! In order to prove this result, a subject as difficult as **Galois Theory**[1] had to be developed. So progress is much less straightforward.

However, algebra and geometry aren't the only fields in mathematics.

They are the principal ones taught in secondary education. Arithmetic, also only goes so far, because in the next stage one confronts extremely difficult problems, those of Number Theory.

There are very simply stated problems in this subject which, to this day, remain unsolved! However, they've never been of much interest to me. Perhaps I found them too difficult. I suspect I haven't got much intuition in this area.

Thus, you found yourself drawn to geometry.

Basically, yes. That's why I chose to study introductory mathematics. I was a very gifted student, not only in mathematics, but in most of my other subjects, including literature. This made it possible for me to enter, (on the recommendation of a teacher of French, in fact), the *lycée Saint-Louis*, one of the prestigious Parisian high schools. It was around the end of 1940, when we were still under the Occupation. I was put into the courses which prepare one for the examinations of the Parisian *École Normale Supérieure*.[2] I was admitted there in 1943.

At that time, like many young people, I was interested in the foundations of mathematics, logic and set theory. In some sense I was a modernist before its time. Our teachers were almost entirely under the influence of the Bourbaki group, which was then engaged in formulating modern concepts and methods.

The history of the modernist movement in mathematics, which began exerting a real influence several years later, is a subject that needs to be taken up by sociologists studying the history of science. It's a wonderful subject: its motivation, its subsequent development and finally the totally ambiguous situation in which it finds itself today.

What is your present position on these issues?

I've promoted myself strongly as an antimodernist, primarily because the modernists have gone overboard. When, with governmental support, they sought to transform the teaching of mathematics at the primary school level, a number of pedagogical institutions were set up at every university. These were the celebrated research centers for the study of the teaching of mathematics (IREM: *Instituts de Recherche sur l'Enseignement des Mathématiques*). It was a form of proselytizing within the pedagogical community. One saw venerable old professors, who'd spent years teaching arithmetic with Cuisinaire rods, being forced to retrain. They were told: Gentlemen, what you're doing is ridiculous; you're totally ignorant of **Set Theory**, without which one can't do arithmetic. And these venerable old professors were obliged to sit on school benches and listen to arrogant youngsters tell them that they didn’t know a thing about integers!

You don’t think that this approach, with appropriate safeguards, maybe of some use in teaching children to think in mathematical concepts?

It can be good for 15 or 16 year olds. I see little benefit in introducing the notions of Abstract Algebra, such as **commutativity**, **associativity**, set theory and **power sets**, before that age. The basics ought to be learned empirically, by practice and trial and error, as it’s always been done. When I myself went to elementary school, we memorized the addition and multiplication tables. That was excellent! I am furthermore convinced that by allowing the use of a pocket calculator at ages 6 or 7, one inhibits the necessary familiarity with numbers which we develop through mental calculation. In a manner of speaking, we’ve gotten rid of the calculator inside our own heads.

How and why did you become a research mathematician?

That also happened more or less by itself. When I entered the École Normale Supérieure, I found that I was able to solve practically every problem presented to me. I was able to converse with my teachers, and they had confidence in me. When Henri Cartan, my advisor, found me a position in the *Centre National de la Recherche Scientifique* (CNRS[3]), he said: "This student appears to have a strong intuition; I am confident of his abilities". However,

I wasn't able to justify his faith in me until much later because it took me six years to finish my thesis. That's hardly dazzling. By contrast, certain mathematical minds seem almost miraculous! In the cramming classes for the Grandes Écoles, for example, some of my fellow students could solve all sorts of problems very quickly. I've never shown myself capable of such incredible feats. Perhaps my basic character is more philosophical than mathematical.

Can you be more precise? How did this manifest itself at that time?

I was definitely drawn to philosophy. I was introduced to it by our assistant scientific director, George Bruhat, who was also our advisor. When I mentioned to him that I was interested in the philosophy of mathematics, (along the lines of Cavaillès[4] and Lautman[5]), he raised his arms towards the ceiling in a gesture of despair, and cried, "Get your teacher's diploma (*aggregation*) in a hurry!!" It was excellent advice.

He thought that interest inimical to your work in mathematics?

He no doubt suffered from a common prejudice which maintains that people who express an interest in the philosophy of science are only trying to cover up for their technical deficiencies. One might call it the pedagogue's

defense reaction. To their way of thinking, persons with genuinely mathematical souls don't bother with philosophy and when they do, it's seen as some kind of aberration. That reminds me of what happened to my colleague, Alexandre Grothendieck[6]. There's a man with mathematics in his soul! Still, during the 1970s (the result, perhaps, of the events of 1968?)[7], he became a conservationist and concerned himself with ecological issues. He appears to have rejected those things which, previously, he was most passionate about.

But what is one looking for when one becomes a mathematician? What drives someone to make that sort of investment in oneself?

Investment is the right word. A word that is, for the most part, difficult, burdensome and restrictive! Once you're caught up in working on a problem, you fall into a painful state of alienation from the surrounding community. You can't think of anything else. It's something that happens automatically and there's no way to escape from it. Moreover, working for the CNRS one feels under a constant obligation to produce something! At the beginning, of course, you're not expected to produce anything sensational. You work to a certain extent on rudimentary technical matters. But that's not the way things worked out for me. My very first published work already contained a sensational discovery!

Early Works

What was that discovery?

It's difficult to describe without becoming technical. Basically one has to know what's called **Morse Theory**, reinterpreted in terms of the decomposition of a **manifold** into **cells**, rather than in the way its inventor did as a theory of **homology**. It's a way of returning a formalism, based on an algebraic algorithm, to the original geometrical situation from which it originates. Contrary to what is commonly maintained, it is geometry that gives rise to algebra.

Why is one unable to explain this in non-technical terms? What inspires you when you work on a problem of this kind?

It's worth making the attempt. The basic idea is both simple and powerful. It comes from Morse Theory. This tries to capture the essence of a global geometry by means of an elementary operation.

A topological insight may imply the existence of a space with a certain kind of global geometry. However, this will mean nothing to someone not aware of this topological insight. It becomes then a matter of inventing a method by which he will, all the same, be able to reconstruct this space. What this means in practice is a procedure for

decomposing the space into its components. The central technique of Morse Theory may be compared to the act of slicing a sausage up into small **disks**. If one is presented with the set of disks and knows how to order them in the same order in which they were sliced, you can reconstruct the sausage! There you have it.

The metric properties, such as the diameter of each of the slices, become irrelevant; one's only interested in their topological structure. Where are the boundaries of the cuts? How is the topological type changed by the process of cutting? By being able to characterize these topological types, one proceeds to classify them. These methods allow one eventually to reconstruct the global form of the space. The procedure is a natural one. Think of it as a puzzle: Putting together the space is like putting together a puzzle. In fact, it's a particularly simple puzzle. All the pieces are already ordered and they just need to be fitted together, each being pasted with the others following a scheme given by the theory.

An analytic process in other words: one cuts it up into small pieces, which are numbered…

It’s the method of Descartes: Reduce everything to constituents so simple that they are easily described. It then becomes an easy matter to reconstitute the complex from the simple. It’s what’s done these days in computers when a surface is decomposed into pixels. Every form then

becomes a block of pixels. This is much cruder than Morse Theory which is primarily conceptual, yet ultimately simpler.

You’re talking about 3-dimensional space.

Not necessarily. Morse Theory can be applied in all dimensions. The modern approach to mathematics has accustomed one to seek for results that are applicable to all dimensions, not just 1, 2 or 3 as it used to be in the past.

This concept of dimensions above the 3 that we’re familiar with is difficult to understand. Isn’t the dimension the same for everyone?

Mathematicians have no qualms about using dimensions of any size. Furthermore, the intuitive understanding needed for working with 4, 5, 6 or any number of dimensions, employs the same techniques. All of one’s intuitive perceptions can be built up from images in 2 or 3 dimensions. We’ve learned how to decompose a space of large dimensions into subspaces of smaller ones. This enables us to work with a small number of **parameters**. In some metaphorical sense, one uses the subspaces of smaller dimensions to ‘sweep clean’ the spaces of larger ones. Ultimately one can learn everything one needs to know by studying situations in 2 or 3 dimensions.

But how does one represent them?

There are certain concepts which help one to organize one’s thinking. One of these is the idea of a **fibered space** or **fiber bundle** (it’s been around for about 50 years I think). [This was written in 1991. – ed.] One can picture a space structured by fibers. One knows what’s going on in each of the fibers and this enables one to conceptualize what’s going on in the entire space. By playing with objects of this sort one can develop one’s intuition of n-dimensional space.

Imagine a box filled with spaghetti. If each strand has the thickness of a Euclidean line one can fill a cylindrical box with an infinite number of spaghetti strands. That’s an example of a fibered structure. The box is 3-dimensional. The strands have length but no thickness, in other words they are 1-dimensional. The base of the fiber is a disk of 2 dimensions.

Length Dimension + Disk Dimension = Dimension of the entire space.

By working with this simple idea one gets a feeling for spaces in general. It’s not only the case that $1 + 2 = 3$. It’s just as easy to deal with $7 - 4 = 3$.

Let me suggest picturing the 3-dimensional spherical surface of a **ball** in 4 dimensions as an exercise in geometrical intuition. Let's begin with our normal 3-dimensional space. Pick an origin and imagine a light ray sent off from it in some direction. Suppose we find that, in

following it to the end, we find the same point obtained through following it in the opposite direction. Mathematicians describe this by saying that all such pairs of points are identified. In the final stage, we identify all these point pairs into a single point called the point at infinity. What we've done is close up our normal 3-space with a single point. This actually has the same structure as the 3-dimensional boundary of a sphere in 4-dimensions. I dare say, there's already some trouble in picturing it, isn't there?

Where is the 4th dimension of this ball?

It's easier to picture it as the **product** of other spaces. Unless, like Mr. Changeux[8], one speaks of it as a mental construction with no connection to the real world!

Let's try to make some sense of it. I've got a box and a notebook. I take a point on the cover of the notebook and pair it with a point on the surface of the box. I now treat this point-pair as a single point in a space which is, in some sense, the product of these two spaces. This new "point" therefore belongs to a 4-dimensional space.

A space in which these separate points become a single point?

That's right. A pair of points selected from 2 planes defines a point in a space of 4 dimensions. Grasping this requires an intellectual effort. Topologists who do this

are, one might say, applying "pre-logical thinking"!

Productivity: The Genesis of Catastrophe Theory

You have yet to speak of your 'motivation': What leads one to concentrate and invest so much of one's time in mathematics?

My motivation may have been social. One needs to justify to oneself the fact that the state is paying one to do something, which appears to many people to be nothing at all!

What do you mean when you speak of productive mathematics? Does this refer to applicable or applied mathematics?

It means whatever is accepted for publication. My first publications were in 1949. In my opinion, I was a productive mathematician from the period 1951-52 to about 1958-59. I didn't write many articles, but several of them continue to be cited up to the present day. It was in this period that I lay the foundations for a discipline (the English would call it a "gadget") of **cobordism**. This subject is both elegant and profound. It earned me the Fields Medal[9] in 1958. In my opinion, my productivity faded away in the years following this period.

Later, I invented what one might call a "semi-philosophy"! That's the way I describe CT. Some people consider it lousy science in the company of lousy philosophy. They may be right. However, I consider it an original contribution, something which will be valuable in the long run.

All the same, it's the aspect of your work that has created the most public interest. Is that just because its name has the word "catastrophe" in it?

The considerable amount of criticism that this terminology has aroused is largely a media phenomenon. This was far from my intentions!

In fact, there's consistent unity underlying all of my work. I've thought a lot about this subject; in my opinion, it's not a matter of philosophy. The unity to which I refer has rather to do with the concept of a boundary. This is what links CT to cobordism. We all know what a border is, the border of this table top, the edge of this wall, a frontier, and so on.

Do you make a distinction between a boundary, a frontier and a limit?

The word “limit” is a technical term. It’s not intrinsically topological. The notion of an upper limit of a series comes from Analysis. The concept of a boundary means some-

thing very different. When speaking of a limit, one has to bring in the idea of infinity. One can take the limit of an infinite sequence of numbers. A boundary is more concrete, more immediate, and more physical in fact.

I came to appreciate the importance of the notion of a boundary when I began studying the metaphysical system of Aristotle. For Aristotle, a being is whatever has separate existence. It has a boundary which sets up a separation in the ambient space. Briefly, the boundary of anything is its form. Concepts also have boundaries: The definitions of these concepts. Apart from the spaces we’re all familiar with, the positing of a definite boundary for an entity isn’t always clear to a topologist.

It’s turned out that, on the basis of this notion of the boundary, I was able to develop a number of mathematical theories that I have found useful. Afterwards, I turned my attention to **mappings**, the means by which one space can be carried into another in a continuous fashion. This led me to the study of **cusps** and **folds**, which can be given a mathematical formulation. My work was based on the investigations of an American mathematician, Hassler Whitney[10], who died a short time ago. This enabled me to set up a classification scheme incorporating the different ways a space can be carried into another one. It led me to some interesting discoveries from which I derived a number of different ways of classifying mappings. After becoming a professor at the University of Strasbourg, I started looking into physics. There were some conjectures

in pure mathematics that had been confirmed by research in geometric optics. All of these contributions were valuable.

These were the things that went into the invention of CT. There has been a simple unifying thread in all of my investigations, which has nothing at all to do with making headlines in the newspapers.

In your study of boundaries, folds and cusps, you were able to show that every space contains features which lead to breakdowns, which can be modeled …

Generally speaking, the spaces I've looked at are what are called homogeneous or locally homogeneous spaces. They are also called manifolds. Ordinary Euclidean space is a manifold. The application of constraints is what produces **singularities.** For example, ruffling the sleeve of my jacket causes several folds to appear. These do not arise from some internal mechanics in the fabric. The relevant theorem is actually quite abstract: When a space is subject to a constraint, by projecting it onto another space of smaller dimension, the target space will continuously assimilate the constraint everywhere save at a certain number of points where, in a manner of speaking, it concentrates its individuality! It manifests its resistance through the presence of these singularities. The concept of a singularity allows one to embody an entire global structure

in a single point. The subject has many fine points which await further developments.

This is a purely intellectual insight, completely insubstantial, that is to say, independent of the nature of the substance?

Although it doesn't depend on substance it may be applied to certain kinds of materials. The cardinal merit (and the greatest scandal!) of CT has been the claim that provides for a theory for accidents, of forms, of the external world, independent of substrate or substance. This claim has not won the acceptance of the scientific community.

I ended up with a list of seven **elementary catastrophes**: fold, cusp, swallowtail, butterfly, and 3 umbilics (parabolic, hyperbolic and elliptic). This notion, that there are seven different kinds of accidents, has fascinated people. Normally, in daily life one sees only a few of these. The rest can only be detected on the basis of a refined analysis.

Yet after it was taken up by other mathematicians, this theory has led to many beautiful and deep discoveries. As for myself, I was interested at first in finding applications for my ideas rather than continuing my investigations on the purely mathematical plane. Then the mathematicians came along; they drew the connection with certain techniques already being used in physics, notably those which relate quantum mechanics to classical mechanics: the **BKW (Brillouin-Kramer-Wenzel)** method. This method is based

on the study of singularities, notably **saddle points**, or height singularities. It is through these that one can recreate classical entities on the basis of quantum processes. Roughly speaking, the classical objects are associated with the singularities of the quantum processes. Everything I'm saying, by the way, is true only in a loose sense.

You've alluded to your continuing interest in philosophy; you've stated that you discovered the underlying theme of your research in Aristotle, though only in hindsight… What is the philosophical discourse that led you to CT?

In 1950, I put aside my philosophical preoccupations in order to concentrate on mathematics. This lasted until 1956-57. Afterwards came a period of depression. Whatever progress has been made on the mathematical side since then has been done by others. They produced theories whose algebraic content is so complicated that I am no longer able to follow them. And so, I handed the reins over to them.

But I had to do something! I therefore set myself the task of finding applications for the mathematical theories that I did understand. The result was CT.

It can therefore be said that it was my response to the feeling that I'd been left behind by the mainstream of mathematics (whose advancement was due in part to my own ideas) that encouraged me to look in another direction.

Since I couldn't follow what was being done, I found myself coming back to very concrete issues. All in all, and this is a classical phenomenon, mathematics was going in the direction of ever greater abstraction, that is to say, towards algebra. I've never liked algebra, and I couldn't keep up. This turned my thinking in the direction of real world applications.

CT received enormous coverage by the media between 1974 and 1975. This was followed by a somewhat vitriolic critique. Most of it came from the other side of the Atlantic, from a science establishment which did not accept this manner of theorizing.

The media phenomena collapsed like a bubble! However, there were some imitators who for a time latched onto a theory which seemed promising for their careers. When it appeared that it wasn't going anywhere, they also pulled out! The theory staged a come-back between 1975 and 1980. At that time I made an effort to respond to the epistemological criticisms I'd received. In order to meet them on their own ground I had to return, first to the philosophy of science, and then inevitably to philosophy properly speaking.

It was thus that, owing to the controversy surrounding the validity of CT, I was led to consider my position with respect to science in general, and its relationship to knowledge. This came down to a reflection on the tools that science uses to accomplish its goal.

21 HOW DOES ONE BECOME A MATHEMATICIAN?

Let's return, if it's all right with you, to the context in which you were led to develop Catastrophe Theory.

As I've already stated, I came to it quite naturally via an evolution that led me, on the basis of a problem in pure mathematics, namely, the identification of the generic singularities of a mapping, to see if my work had any relevance to physics. At that time, I was at the University of Strasbourg and engaged in some experiments in optics. A physicist colleague lent me some instruments, a spherical mirror, a prism, and a diopter. I used these to manufacture **caustics**. Through varying the position parameters, I was able to observe their deformations. That was my specific interest.

In some sense, one could say that my ideas grew out of my investigations into caustics. I really ought to say a few words about what caustics are. To fix our ideas, take a porcelain bowl which has a highly reflective inner surface. Fill the bowl with coffee, preferably very black. Then place the bowl under a lamp that gives off a highly concentrated beam coming from a point source. The rays of light, reflected off the inner wall of the bowl, will arc into a curve forming what is known as an *edge of regression* or *cuspoidal edge* in the plane of symmetry of the projected figure. It's actually a physical effect that corresponds to a theorem in mathematics. It's not all that surprising given that geometric optics isn't really a subject in physics; it's a branch of geometry. Mechanics on the other hand belongs

to physics properly speaking. I first heard this enunciated by a professor of higher mathematics at the *lycée Saint-Louis*. And what he said was true!

This led inevitably to CT. I observed that there were different types, different kinds of caustics whose existence I hadn't anticipated, and I needed to find explanations for their singularities. It took me two to three years to fully understand what was going on. Its beginnings, you see, were grounded in the observation of very ordinary phenomena.

What was the path your thinking took?

On the request of the founder of the IHES*, I left Strasbourg and returned to Paris. Since I'd been relieved of many of my teaching and administrative responsibilities, I had more leisure time. My creativity in pure mathematics had waned and I'd begun to interest myself more in peripheral concerns and practical applications.

In addition to optics, I wondered if it might not be possible to apply my ideas to certain aspects of biology. I'd finally understood where these exceptional singularities were coming from: They were connected with the fact that light rays obey the variational principle known as **Fermat's Principle**. It is because of this that caustics become the seat

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*** The IHÉS (*Institut des Hautes Études Scientifiques*) is literally and functionally, France's "Institute for Advanced Study"**

of singularities in addition to those one normally expects to find. Investigating this phenomenon led to the development of the mathematical theory corresponding to it, which is CT.

These singularities, are they particular forms which arise unexpectedly?

This raises a semantic issue. From my particular perspective, any and all discontinuities in natural phenomena should be treated as singularities. The edge of this table, the place where wood becomes air is a surface of separation, a catastrophe domain. One might call this a permanent catastrophe, one that we don't bother to pay attention to.

But the word itself presents problems. The word evokes the sense of a 'brutal transition' taking place in time, with a well defined duration. (I've recently come across the expression "vegetative catastrophe", being applied to the state of the Soviet Union before Perestroika! The phrase is attributed to Céline[11].) I've been much criticized for this choice of language.

One also finds catastrophic events in human affairs, and in social evolution …

For myself, a catastrophe is a phenomenological discontinuity. It may perhaps be an abuse of language to use

so dramatic a word for a notion that is so commonplace. But that wasn't my intention. The word was a natural choice; it was the physicists who introduced the expressions "infrared catastrophe" and "ultraviolet catastrophe" for phenomena for which the infinite series solutions of their differential equations diverge. This was a precedent for its use. But I wanted to indicate by this word the presence of some kind of underlying dynamic.

Can you think of any synonyms which convey the same message?

I have on occasion used the expression "phenomenological discontinuity". It's rather heavy, whereas the word "catastrophe" perfectly expresses my meaning. The boundary of a cloud is a catastrophe. Obviously, if this cloud is perpetually immersed in a gale, it's difficult to think of it this way. There has to be a well-defined frontier.

If the cloud hasn't got a boundary, one can't speak of a catastrophe in my sense.

The Fate of Catastrophe Theory

After several decades, what are the applications of the theory, what directions is it moving in?

Using a conventional terminology, in the sense of

applied science, (as the word 'application' is used in government documents for example), the rewards have been meager. There exists no specific area in which one might say that CT has led to the development of techniques, or tools, or methodology for solving its problems. CT is rather an interpretive scheme which allows one to understand, in a great many instances, situations which otherwise would be very difficult to describe. Such systems, being too complex to be analyzed by customary reductive methods, can't be described in any other way.

That's hardly insignificant! It gives one a way of seeing what's going on!

It's essentially more of a program, a project if you like and an eminently sensible one. Still, it has the drawback of being a qualitative theory, one based on topology, which doesn't provide quantitative bounds on the deformations being described. It does not help one to do anything. Action always has to be *hic et nunc**; there must be localization in space and time, else action is inconceivable.

It does not, therefore, allow one to make predictions?

It gives a kind of local description of a system with a space defined by the control parameters. When varied on

*** The Latin *hic et nunc* means "in the here and now".**

the basis of certain kinds of data, these parameters generate corresponding surfaces in these spaces on which one locates continuous variations and their catastrophes.

How do you explain the great enthusiasm the theory elicited at one time?

By returning to its origins. It was first presented to the public in my book *Stabilité Structurelle et Morphogénèse*[12]. It was written between 1967 and 1968 but not published until 1972. In the interval the manuscript had an underground history. It found a most enthusiastic reader in Christopher Zeeman[13]. He transferred its ideas to a more general framework, that of General Systems Theory[14].

General Systems Theory takes its departure from the notion that any system can be represented as a black box of inputs and outputs. By analyzing the correspondence between inputs and outputs, one seeks to understand the mechanisms at work inside the box. This clearly implies that CT, at least in its pure and unadulterated form, is an interpretive scheme. It does not aspire to omniscience in the way physics does. The procedure in physics is to say that the universe is governed by laws and we intend to uncover them.

Catastrophe Theory makes a simpler claim based on continuity; that is, continuity exists, as do continuous functions and continuous derivatives. One therefore examines objects described by **analytic functions** in terms

of diagrams which represent their analytic singularities. This is the underlying philosophy.

In terms of applications, Christopher Zeeman has come up with a large number of them: panic in dogs, stock-market crashes, prison riots, manic-depressive dysfunction in psychology, the behavior patterns of airplane hijackers, heartbeats in neuropsychology, and the propagation of nerve impulses[15]. These can all be described by models derived from CT. In some cases this leads to the formulation of explicit equations: This is the case in describing the propagation of nerve impulses in the squid axon. Once you've got equations, the catastrophe model isn't needed anymore.

As far as physics is concerned, CT has little to offer: The normal approach in dealing with physical phenomena is that they be described by quantitative models in accordance with physical laws. Physics only considers equations derived from these laws. It only rarely makes use of geometric intuition.

By contrast, in those areas where one can’t formulate equations but in which some kind of regularity of behavior is observed, catastrophe models can be of considerable interest. The models that Zeeman invents make such situations intelligible. One can’t use them for practical applications, or for making predictions, but they aren’t useless. If you’re a hard and fast pragmatist, you might say: “This doesn’t amount to very much because it can’t be

applied. What’s the point of understanding something if there’s no practical application?”

But our nature is constituted in such a manner that *understanding* and *acting* are not synonymous. There are many situations one comes to understand without being able to do anything about them: Like the gentleman who climbs up onto the roof of his house to watch a rising flood! One also observes situations in which there is effective action although one doesn’t understand why or how.

The discovery of aspirin is a good example! Its history is quite instructive, though I can’t guarantee the accuracy of my version of it. I gather, however, that the revelation of its properties arose from a psychological mechanism: Many people suffered from rheumatism. It had been observed that these sufferings were intensified by humidity. From these observations an association was made. I’ve asked myself, where did this association originate? Possibly from the great thinkers of the 15^{th} and 16^{th} centuries, like

Paracelsus[16], whose ideas were influenced by magic. The kinds of arguments they made suggested that in treating illnesses of this type, one should look for plants that adapt well to wet environments. Among trees, the willows are best adapted to dealing with water. This led them to distill brews made from willow leaves, and these were indeed effective against rheumatic pains, hence the origin of salicylic acid. Note that, "salex" means willow in Latin. I've no idea what this theory is worth, but it is an excellent example that shows how ideas which, on the face of them

are absurd, can still lead to positive results. Aspirin has turned out to be one of the best medicines ever invented and is used all over the world.

Isn't this the very issue on which the criticisms against you have been based?

To tell you the truth, I'm not aware of any systematic attack on my work. I've encountered crude statements of the following kind: "Thom claims that his ideas aren't subject to experimental verification, and ought to be considered nothing more than fantasies". One should not understate the influence arguments like these have had. However, they are only exploiting a play on words. These are the people who insist that everything be verified by experience. They ought rather to be making the correct distinction between 'experiencing' and 'experimentation'.* If the two words are put together, combining experience with experimentation, virtually everything I've done can be related to examples derived from daily experience. But nowadays, simple experiencing is not enough; everything must be in the form of an experiment.

Now, in my opinion, experimentation is only necessary or useful in the context of a theory which is precise enough to allow one to make predictions. The context provided by

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*** Translator's note: In French, the word "experience" also means "experiment".**

CT does not, in principle, allow for experiments which lead to practical applications. Pragmatic applications demand quantitative theories.

Could this perhaps be the difference between contemplation and action?

Could be. However, contemplations which don't end up in some kind of action, if only within oneself, aren't terribly interesting.

What can you say about the interrelation of prediction and interpretation? Could it be said that prediction is based on good formulae which provide the correct quantities, whereas interpretation can provide a frame-work for the understanding, without quantifying or predicting?

That's right. It is around this issue that one finds all the difficulties associated with catastrophe models. At times it is possible to quantify them; they then become simulations. In other cases, they are purely qualitative, and it makes no sense to quantify them. The first model put forth by Zeeman, that of aggression in dogs, is fundamentally qualitative. It was not intended to give a quantitative measure of the hostility of a dog.

It just describes the circumstances under which this hostility arises?

Zeeman has enunciated a gradual evolution in behavior which is quite interesting. But there's nothing quantitative in his model.

The Catastrophe Theory Controversy

Your work has produced various reactions, including rather severe criticisms. Are you still interested, even now, in responding to your detractors?

We're talking about two periods. The period proper to CT *per se* needs to be clarified in some detail. The theory that I originated belongs to pure mathematics. It was picked up in England where Christopher Zeeman proposed many more ways of applying the theory than I'd imagined. In my original conception, the only parameters of interest, those relevant to morphology, were those of space, more generally space-time. Zeeman was inspired by a more audacious notion when he stated that one might situate control spaces in a generalized systems theory.

CT for me was essentially grounded in qualitative discontinuities one finds in the world around us, that is to say, in forms. What one calls a form is usually, in the last analysis, some kind of qualitative discontinuity within a certain continuous background. My intention was to build a theory based on this observation. Naturally, my thinking took place in the context of ordinary space. Christopher

Zeeman added the following idea: In the general theory of systems one imagines a black box containing a system theoretically in total isolation from the external world, which can only communicate with the external world save through ways that are perfectly under one's control. One introduces matter and energy into the black box, and it is matter and energy that exit from the box. At discrete moments t = 0, 1, 2, 3 …, one introduces matter and energy into the black box according to a predetermined scheme. At the same instant one observes what is coming out. What's happening inside is being analyzed in terms of inputs and outputs. A reductionist theorist would argue: "Break open the walls of the Black box and see what's inside! Once we see what's there, we'll be able to explain how it functions." But a systems theorist cries: "No! The box can't be broken, above all when dealing with living creatures! Besides, there are many situations in which it’s impossible to break the black box.”

Which method is more productive? I’m careful to avoid taking sides. In science as it’s done nowadays, most people opt for the reductionist approach. It’s true that the general systems theory approach requires that one posses a talent for interpretation. Not everyone has that. On the other hand, such things as a precise chemical analysis with well calibrated instruments can be done by anyone who's been trained in the appropriate techniques. This is even more the case given the modern philosophical bent towards experimentation, which generates discoveries from rigidly

controlled data. Nobody's going to question the authenticity of such work, whereas once you attempt an interpretation you make yourself vulnerable to criticism.

The tendency at work today is therefore that of reducing a system to its most fundamental elements to see if one can model its dynamics on the basis of this decomposition. However, this method is beset with major difficulties. The first is that systems are often compounded from a huge number of elements. If one’s analysis is carried down to the atomic level, one quickly reaches amounts in the range of 10^{22} or 10^{23}. Modeling each of them one at a time is out of the question. Classical mechanics fails completely. Quantum mechanics also fails. Although it’s statistical in nature, it deals with phenomenon at the level of the infinitely small, and is rarely able to go beyond that scale.

The global approach proceeds in a different fashion entirely: I introduce a certain flow into my system at a time t = 0. At the same time I look at what's coming out. The operation is reiterated. Assuming that inputs and outputs can be parameterized as points in Cartesian coordinates, with x as the input value and y as the output value, I locate a point (x, y) on the plane. Reiteration of this process yields a distribution of points which can be indefinitely extended. The underlying philosophy for this approach is the following: To build a picture of the final tendencies of this distribution and, by proper interpretation, derive the simplest mechanism that will explain the emergence of these tendencies. Through extension of this

theory, I construct general hypotheses concerning the dynamics inside the box. One proceeds as follows: Assuming that the system is evolving deterministically, the introduction of an identical sequence of initial phenomena must result in the emergence of an identical sequence of outputs. The distribution of points on the graph therefore has a well-defined relationship to the states in the box. The limits to which the system tends can be interpreted as an **attractor**, a collection of limiting states for all its trajectories.

As an example, take the phenomenon of chemical equilibrium. When there is a unique equilibrium state for the variations in the concentrations of substances in a reaction, this unique point will be the attractor for the system.

One is motivated by the fact that, for a great many systems in nature, these attractors are simple objects. The dynamics may be very complex but the attractors can be relatively simple. This is the case with what are called **gradient systems**. The trajectory of a particle which has zero kinetic energy, such as the trajectory of a massless particle in which there is some energy dissipation is an example of a **gradient dynamic**. The body falls down to the lowest possible state. It is this potential minimum which serves as the attractor.

In this approach, one starts with studying a system which is determined by gradient dynamics until one has understood what is happening in this system. The configuration space is decomposed into **basins of attraction**, each with one minimum point [17] around a closed trajectory.

Think of it simply as a map with the rivers flowing into basins which, in general, are lakes or oceans. In the mathematical theory, one is usually concerned with points. The problem is reduced to one of determining which basin one happens to be in. Geographically, someone on the Langres plateau is at a loss to know if he's headed towards the rivers Saone, Meuse or Marne. [A plateau and rivers in France – ed.] If a quantity of water is poured on the ground, one would need an extremely precise map to determine the direction in which it is likely to flow.

Prediction is greatly simplified in a situation based on the dynamics of a gradient. One only needs to determine what basin one is in. As a general rule, attractors are points. Once in awhile they can be curves or surfaces; these are generally unstable and disappear quickly. If, in addition, the system's stability is assumed, then, in terms of its configuration, in any gradient system, the attractors are just points. This is a welcome development which lends itself naturally to a mathematical treatment. This is the situation that I've called "Elementary Catastrophe Theory" (ECT). This terminology has caught on.

Zeeman's idea was the following: In comparing the distribution of data in the initial space with that in the terminal space, one seeks to find the attractor of some dynamical system. There's a good possibility it's there. In almost all cases, one can interpret the limit points in terms of their classification as singularities, or cusps. These will be the minima of some sort of potential. This family of

potentials can then be studied through varying the control parameters operating in the system.

Systems depend on two kinds of variables: fast-acting variables ("internal") which parameterize the configuration space, and slow-acting variables ("external") which, in control theory, are the values determined by the experimenter. The space of the slow-acting variables is what is known as the "control space". These control parameters, temperature and pressure for example, interpreted as globally defined constants acting on the system are the ones that modify the dynamics.

Each point in the control space corresponds eventually to a certain attractor. Projecting this entire configuration onto the control space, one finds cusps, regions dominated by a single attractor. One then locates the ridges separating these regions. Sometimes it's possible, by fixing the control parameters, to determine exactly where the system is headed. That's the best kind of situation.

The contribution made by CT, and the mathematics behind it, is in the classification of what happens in these basins of attraction and their limits, under the assumption that the entire configuration is structurally stable. What this means is that the system is not qualitatively altered by minute adjustments, either of the control parameters or the input and output variables. The theory is mathematically extremely sound; yet, once one seeks to apply it, all sorts of problems arise.

Zeeman attempted to apply the theory to a broad range

of situations, taken from sociology, biology and medicine.

To manic-depression for example …

A catastrophist interprets manic-depression as a struggle between two stable regimes competing for the behavior of the individual. A friend of Zeeman's has done something similar with *anorexia nervosa*.

There was a media storm when Zeeman presented his findings at the Congress of Mathematicians in Vancouver in 1974. People were saying that although it looked like a remarkable way of modeling phenomena, it didn't appear to have any mathematical content.

The initial resistance came from across the Atlantic. It was said maliciously that the New World will never acknowledge that the Old World can invent anything new. Whatever the case, that's where the opposition started.

What was the nature of this opposition?

It converged around two issues. The first had to do with the insufficient conceptual support available for applications, by which I mean that the hypotheses required for the application of CT are too restrictive. Gradient dynamics constitutes a very special case. Even the act of throwing a set of massive bodies into space can't be described by gradient dynamics. The energy of a thrown solid object divides into two parts, potential and kinetic. Potential

energy by itself is not conservative; it gives rise to a gradient dynamics through dissipation.

Kinetic energy is different. In general, it follows what is called **Hamiltonian dynamics**, and it is conservative in the sense that there is no loss of energy due to dissipation. Their histories are very different. Either one adopts the hypothesis of a kind of infinite frictional force (equivalent to Aristotelian physics) or one assumes independence from the effects of friction, situations in which energy can be degraded but not lost. This can be extended to situations which, in the presence of friction, do not violate the conservation of energy. One considers the situation as non-thermal, and assumes that the heat generated by friction is negligible. It is then the energy itself, what is called the free energy, that diminishes.

This generated opposition.

There was also another, subtler criticism coming mostly from persons engaged in the study of dynamical systems derived directly from the laws of physics. These aren't gradient systems.

In addition, in order to use models drawn from CT, everything must be in general position, that is to say, a state of structural stability. I was lectured at, told that one must accept the universe the way it is, that it was not up me to change its laws in order that systems which are not structurally stable become so. The laws are what they are: They can't be juggled with. I ought to take the world as it is, rather than playing around with inventing mathematical

simplifications, because in fact they are just simplifications and nothing more.

Both objections seem to accuse Catastrophe Theory of not dealing with the real world.

In fact, there was also a third criticism which we found really discouraging. For theoretical reasons, CT doesn't admit quantitative, that is to say genuine, predictions. Catastrophe models aren't based on the kinds of equations that permit certain kinds of manipulations, changes of variables, perturbations, deformations. CT deals with entities that remain invariant when perturbations are introduced. These entities are qualitative, not quantitative. Suddenly everyone seemed to remember the classical dictum of Rutherford (which I cite at the beginning of *Structural Stability and Morphogenesis*): "Qualitative is nothing but poor quantitative."

It was argued that CT is insufficient in and of itself to make exact predictions, or even to supply a method for approaching a quantitative prediction, which is more serious.

It explains how things come about, but can't state when or where they will happen?

My critics said: "What you're giving us is only a metaphor." How was I to interpret this, as a reproach or as

a compliment? For my own part, I saw it as a compliment. To have created a metaphor in an area where previously there was nothing, that's already progress! But over there, everyone works with computers; they want numerical data. It has to be admitted that in this domain, CT has failed pitifully. As far as I know, no numerical model based on CT has ever yielded anything of interest. Perhaps in statistics, though I haven't looked into it.

How do you respond to the first category of objections?

On the matter of the ineffectiveness of the theory in making predictions, my detractors are justified. I admitted this myself almost a year before negative commentary started coming in from the United States. I'd already talked it over with Christopher Zeeman. He's an optimist by nature, and believed that the theory could be made to yield numerical results. This, I told him would be nothing short of miraculous.

CT belongs to pure mathematics, which has nothing to do with the phenomena happening around us, or their physical, chemical or biological attributes. To assert that pure mathematics can give quantitative results is making the claim that everything in the universe is governed by explicit quantitative laws. I doubt that the most deterministic thinker of all time, Leibniz, would have gone that far! It is not within the power of CT to provide a mathematically quantitative description of any situation which,

because it doesn't depend on the fundamental laws of physics, is inherently incapable of being given such a treatment. In fact, most people don't realize how few natural phenomena are governed by exact and precise quantitative laws. Those situations in which this is a possibility can almost be taken as a definition of physics. All other laws are approximations only.

There are a great many phenomena which result from such large numbers of interconnected causes that it must be difficult to find a model that gives an exact description and allows for predictions. One uses statistics and probabilities to establish estimates.

There is a major branch of Applied Mathematics known as Numerical Analysis that allows one to approximate outcomes in situations, which arise out of basic physics, yet which are so interconnected and complicated that one is unable to establish a precise physical description in the short term.

What then is the status of CT? You've characterized it as qualitative rather than quantitative. Couldn't this theory supply a kind of abstract theoretical framework, identifying tendencies towards this or that kind of outcome?

The word "tendency" is appropriate. What CT provides

and above all what ECT provides is a description of tendencies in conflict. But the number of tendencies has to be small, two or three, four at the outside, and I am unable to give even one example of a morphology that can be reconstructed from the competition of only four tendencies. The most one can do is to develop a *taxonomy of conflicts*. In practical terms this translates into a decomposition into basins of attraction, in a control space, in which each tendency, each attractor, defines a specific domain. What CT contributes is a morphology of the surfaces on which one jumps catastrophically from one regime to another.

What may have caused the controversy was the way in which these spatio-temporal morphologies were applied to highly complex phenomena such as social behavior and things like that.

I don't have the impression that the objections came from the human sciences or the social sciences as they are called in the U.S.A. People in those fields are only too happy when they can find a bit of mathematics to put into their data. The objections came from the applied mathematicians, specialists in the fields of **partial differential equations**, of hydrodynamics, fluid mechanics, sciences which I would characterize as "semi-hard". They don't want to trade the advantages of having equations that they can solve and which can be used to make predictions, for a softer way of making models which gives only qualitative descriptions. The reaction was professionally motivated;

and in fact, the entire profession of applied mathematicians was united in its opposition to the theory.

It's a theory which, by providing clarification, can serve the needs of philosophy.

The inherent value of the theory lies in its capacity to produce intelligible models for the sciences. And that impresses me as important. Even if we're sometimes wrong; even supposing that reality is being manipulated by an evil genie that creates intelligible appearances to deceive us. The theory is interesting primarily because it proposes a mathematical description of analogies. Analogical thinking in principle does not rest on any well defined substrate.

Analogical thinking can be applied to quite different situations, without bothering about whether one is dealing with physics, chemistry, biology or sociology.

You see the metaphor as a useful tool …

Absolutely! Konrad Lorenz[18] in his Nobel Prize speech makes an observation that made a deep impression on me when I read it a few years later. He said, *"All analogies are true"*. That's putting it in an extreme form; yet, if one modifies it by saying, *"All analogies, provided they make sense semantically, are true"*, then I think what he's saying is absolutely true. In other words, if by some effort of thought one convinces oneself that an analogy is proper, that

propriety, coming from a purely intellectual examination of the components of the analogy, implies its truth as an assertion. This is a situation in which the mental attitude expresses the truth of the analogy.

But isn't there a risk that each individual has his own way of analyzing analogies, that the collective under-standing required to transmit it in a rigorous manner is lacking?

There are situations in which analogies are totally explicit. Take the classical example of an analogy proposed by Aristotle: "*Evening* is to *day* as *old age* is to *life*."[19] This analogy assumes the form of a fraction, a proportion, an equality between two ratios. It's evident that the nexus of this analogy devolves upon the passing away of a state of affairs, the end of a lapse of time, with "evening" on the left side, and "old age" on the other. It has to do with the neighborhood of the terminal moment, the endpoint of the interval under consideration. A latent geometry fully explicates the analogy. One can find nothing to object to in it. It is endowed with a sense of conviction comparable to arithmetic.

This addresses the first class of objections. What about the second?

Let's turn to the second class. The scientific community

has continued to disparage the theory. In the days when it was popular, I received through the mail two or three catastrophist models per week representing every point of view. The most I receive nowadays is one per month. As a sociological phenomenon, the theory has foundered.

Yet it's a rather subtle sort of shipwreck, because all of the ideas which I've introduced have been incorporated into the language of common scientific discourse. Everyone talks about cusps; people know what a swallowtail catastrophe is, and so forth. These ideas are now part and parcel of the equipment of model builders. Thus although it's true that the theory has been discarded in principle, in practice it's been very successful. Of course one can't patent theorems in mathematics, and I'm not making a profit!

Which is all to the good. Mine is one of the few remaining scientific fields which are free from all commercial incentives. From that point of view at least, I strongly support the demise of CT.

In other words there are elements of the theory still being employed?

Yes.

But generally speaking the theory itself has been rejected?

The expectations aroused by the theory, yes. There have

been disagreements between Christopher Zeeman and myself: He continues to be an optimist with respect to its efficacy in the construction of models and its quantitative and predictive potential. This is a territory which I've gladly ceded to my critics. My extensions of the theory have been in the domain of philosophy. There, one isn't obliged to come up with immediate results. And one doesn't have to worry about polemics.

Haven't you been reproached for promoting a theory whose applications are substrate-independent?

It's something that most people find difficult to accept, even among those who don't have a scientific mentality:

No one feels comfortable with the idea that the behavior of a solid can be the same as that of a liquid or gas. Their skepticism is justifiable. Yet for some strange reason, I've never been criticized on these grounds. Maybe it's too obvious!

Yet by taking a deeper look, one sees that CT retains its applicability, in spite of the distinctive qualities of various substrates.

A commonly cited example is that of the ridge separating the horizontal surface from the vertical surface of this desktop. The catastrophe consists in the change from a horizontal regime to a vertical regime. Their intersection is a ridge. It was made by cutting a plank that was originally continuous. The action of the saw on the

wood is the expression of an elementary catastrophe. Taken together we have what is called a *dual cusp*, something like an *anti-cusp*: The static catastrophe one observes retains the memory of the dynamic catastrophe which occurred in the making of the desk.

What this means is that solids hold the memory of all the catastrophes they've been through. From the viewpoint of their dynamics, solids aren't very interesting. In fact, they're rather static. Yet, they become interesting as the repository of actions effected in the past, which makes them of interest in the interpretation of forms. They have a memory of sorts. Liquids haven't much memory, gases even less because they take the form of the receptacle that contains them.

Does this way of ignoring the substrate encourage the development of interpretive frameworks?

The problems associated with the substrate only become significant to the degree that phenomena are moving to stasis. A morphological process comes to a halt: This produces a form that needs to be interpreted. To do so one must go back to its origins. And it is here, in the initial stages, that one can apply CT without having to be concerned about the substrate. It's easy to cut oneself with a sheet of paper, although it's not very rigid. If you slash quickly with it, you can cut yourself. It's a matter of relative velocity canceling out the relative elasticity of two sub-

stances. There is an entire area for investigation that ought to be followed up: the vanishing of specific attributes in a regime of high velocities.

What is the second category of polemics?

It comes from a new discipline in the theory of gradients which has been given the name of chaos. The field emerged between 1975 and 1980, after the notion of the attractor was proposed[20]. One of the difficulties had to do with the bifurcation problem. Unfortunately, this is a problem of enormous complexity! Even with extremely simple equations, differential equations with two parameters can produce a bifurcation diagram that is unbelievably complicated.

This ties with another issue, that of structural stability which has figured so largely in the controversies surrounding CT. I'd been working all along under the assumption that virtually all differential systems, even the ones that come from dynamics, were structurally stable. That this is true in 2 dimensions was proven for orientable surfaces by a friend of mine, a mathematician from Brazil[21]. But at 4 dimensions, one hits a wall. In 4 dimensions, it had been proven that there exist differential systems from which, by a minute perturbation of their parameters, one can obtain an infinite number of distinct topological types corresponding to each new system. In other words, there is a systematic topological instability at virtually

every point of the control space. This wrecks the theoretical foundations of CT!

I became disheartened. I'd hoped that, with the concept of structural stability, one could reintroduce a modicum of regularity in our world. Realistically, one knows that the instabilities produced by such systems are rarely visible. Their attractors have a structure articulated by extremely fine filaments, a fractal structure as one says nowadays, and the configuration of these filaments is constantly varying. Seen from a distance one can't detect any difference. Although the fine structure of the attractor is in constant flux, its global character hardly changes at all. If the mathematics doesn't need to be fine-tuned, one can retain a notion of structural stability that is similar in all respects to that of CT. There are colleagues, at the University of Nice for example, currently engaged in that direction.

As you know, chaos became very fashionable. The trend began about 12 years ago. [In the late 1970s - ed.] An old result of J. Hadamard's [22], dating from 1902, states that on a surface of genus 2, that is to say, one with two holes in it, equipped with an appropriate metric, (a constant hyperbolic metric), there will always be a pair of **geodesic** trajectories that diverge.

Otherwise stated, if the initial positions of a pair of geodesics are taken to be very close to each other, after a certain length of time, the geodesics will have diverged considerably in a statistically chaotic fashion.

For a long time his result had no repercussions in physics. The phenomenon, known as *sensitive dependence on initial conditions*, was rediscovered much later, between 1975 and 1980[23]. The data must now be expressed in statistical terms. It's become a matter of asymptotic evolutions and finding the flux averages on the space. These are the only invariants.

Such systems are called chaotic.

CHAPTER 1 NOTES

1. In a narrow sense, as in Thom's reference, *Galois Theory* is the study of Galois fields. The theory's origin was in finding roots of algebraic equations of degree 5 or higher. Methods for solving quadratic equations (Quadratic formula) were known in antiquity. Methods for solving cubic equations (Cardano formula) and quartic equations (Ferrari method) were discovered in the 16^{th} century. Attempts to find formulas for solving quintic and higher-degree equations failed until 1824 when N. H. Abel finally showed that there are no solutions in radicals for equations of degree 5 or higher. What E. Galois did in 1832 was to find necessary and sufficient conditions to be satisfied by the

coefficients of an equation for the latter to be solvable in radicals. (Galois did much more!)

In a wider sense, Galois Theory is a theory dealing with mathematical objects on the basis of their automorphisms (isomorphisms of a system of objects onto itself.) The totality of all automorphisms of an arbitrary algebraic system forms a group and the study of this group is an important and powerful tool in the study of the properties of the system itself.

A fascinating and very readable recent book by Amir Alexander, *Duel at Dawn: Heroes, Martyrs, and the Rise of Modern Mathematics*, discusses the life of Galois and labels him as the founder of modern algebra. The death of Galois marked the end

one era in mathematics and the beginning of another.

2. The *Grandes Écoles* are special elite schools for the study of engineering, science, humanities, business administration, history and other subjects. The most elite include the École Polytechnique for engineering, and the three teaching schools known as the Écoles Normales Supérieures.

Admission to them is predicated on very competitive and very difficult entrance examinations. In France, the cultural significance of these competitive examinations, with a pre-determined quota of accepted candidates, is considerable. Grueling '*classes préparatoires*' at the secondary school level are designed to prepare highly motivated students for the grandes écoles entrance examinations, or *concours*. This system has tended to instill a somewhat competitive and elitist mentality in the academic communities of these schools. (OED)

3. The CNRS is France's National Center for Scientific Research. It is the French equivalent of the National Science Foundation in the U.S.A.

4. Jean Cavaillès (1903-1944) was a French philosopher-mathematician and a leader in the French resistance and *Libération* movement in France during the Second World War. He was shot by the Gestapo in 1944. He wrote on the axiomatic method, formalism, abstract set theory and the philosophy of science.

5. Albert Lautman (1908-1944) was a French mathematical philosopher. Both Lautman and Thom acknowledge being Platonists. Thom refers to Lautman as his philosophical point of departure. For Lautman, there exist forms that are not thematized in mathematics but which organize a mathematical theory in such a way that different theories can be organized from the same form. For Thom, a mathematical theory is connected with a form which is not identical with the theory but in a certain sense it is identical with the perception form, i.e. spatio-temporal forms.

What constitutes the object for Lautman is its place in the logical form which structures the mathematical theory. In Thom's work, the mathematical object is constituted by its connection with those traits in the external object which are pregnant: connectedness, smoothness, discontinuity …

For more on Lautman's and Thom's philosophy, see the book by Svend Østergaard, *The Mathematics of Meaning*. Much of the above is taken from there.

6. Alexandre Grothendieck (1928-) was born in Germany but he is considered a French mathematician. He won the Fields Medal in 1966 for his work in Algebraic Geometry. He worked at the IHES in the 1960s and 1970s at the time Thom was there. Grothendieck has a very interesting past. The interested reader can check the following works: P. Cartier, "A mad day's work…" in the *Bulletin of the AMS*, **38** (2001), 389-408 and Allyn Jackson, "Comme apelé du néant - as if summoned from the void: The life of Alexandre Grothendiek" in the *Notices of the AMS*, **51** (2004), I,

1038-1056; II, 1196-1212. Grothendieck's mathematical output was enormous. Future generations of mathematicians will be studying his works for a long time. The French mathematician Jean Dieudonné (1906-1992) – a main figure in the Bourbaki group and an early member of the IHES – in discussing Grothendieck's monumental work *Eléments de Géométrie Algébrique* wrote: "It is out of the question to give a summary of six thousand pages. There are only a few examples in mathematics of such a monumental and fertile theory, built in such a short time and from one man." (See *The Grothendieck Festschrift*, I, Prog. Math. 86, Boston, Birkhäuser, 1990, 1-14.)

By 1986, Grothendieck had completed an autobiography, *Récoltes et semailles* (Harvests and Sowings), 1500 pages of

reflections on life and mathematics but it was never published. Parts of the book have a very strange beauty and depth. A partial English translation has been done by Roy Lisker (the translator of this book). For more information on Lisker's meeting with Grothendieck and his partial translation of *Récoltes et semailles*, see Lisker' s website:
htpp://www.fermentmagazine.org.

7. The events of May 1968 were a series of events in France that started with student unrest and led to the closing and occupation of the University of Paris (Sorbonne) by the police. This and other events led to a general strike by high school students, university students and their teachers and almost two-thirds of France's general work force. This paralyzed most of the country and led to the eventual collapse of the de Gaulle government.

Many saw the events as an opportunity to shake up the "ruling order".

8. Jean-Pierre Changeux (1936-) is a French neurophysicist and epistemologist. He is known to the public for his controversial ideas about the relationship of the mind to the physical brain. He is co-author with mathematician Alain Connes of "*Conversations on Mind, Matter and Mathematics*".

9. The Fields Medal, often described as the "Nobel Prize of Mathematics", an obvious reference to its prestige, is a prize awarded to up to four mathematicians who are not over the age of 40 at each International Congress of Mathematicians held once

every four years. René Thom along with Klaus Roth received the medal in 1958.

The medal is named after the Canadian mathematician John Charles Fields and was first awarded in 1936. Its purpose was to give recognition and support to younger mathematical researchers who have made major contributions. It is widely accepted in the mathematical community as the top honor a mathematician can receive. It comes with a small monetary award which in 2006 amounted to about 15,000 Canadian dollars.

10. Hassler Whitney (1907-1989) was an American mathematician whose work was in differentiable functions and manifolds, the theory of sphere bundles, algebraic topology, singularities of smooth mappings, analytic varieties and other

fields. René Thom had great admiration for the originality and depth of the mathematical work of Whitney. He once told his mathematician friend Mauricio Peixoto: “Of all the mathematicians I know, Whitney is the one who has the most acute sense of the differentiable. Altogether he is the American Riemann.” Thom also admitted that he was not particularly rigorous and that Whitney was one of the mathematicians that helped him maintain a fairly acceptable level of rigor. See page 87 of *René Thom (1923-2002)*, supplement to No. 103 of the Gazette des mathematicians, edited by C. Anné, M. Chaperon and A. Chenciner, SFT, Paris, 2004.

11. Thom may be referring to Louis-Ferdinand Céline (1894-1961). Céline was a French physician who became a famous and controversial writer. He visited the Soviet Union in the 1930s and after his journey wrote a scathing report on the state of the union declaring his total disenchantment with the Communist system. (Wikipedia)

12. Thom, René. *Structural Stability and Morphogenesis: An Outline of a General Theory of Models,* translated by D. H. Fowler. Reading, Massachusetts: W.A. Benjamin, 1975. The French edition was published in 1972.

This is a unique book still awaiting full recognition and applications especially in biology. The notion of a “generalized catastrophe”, introduced by Thom in this book and the book’s applications, especially in biology, need further development. This may open new vistas into those sciences in which the

destruction of symmetry or homogeneity is observed.

The article, "Speaking Volumes: René Thom's Structural Stability and Morphogenesis", by Ian Stewart in the *Times Higher Education*, 10 October 1997, gives an interesting perspective of the influence this book had on Prof. Stewart.

13. E. Christopher Zeeman (1925-) is a well known British mathematician whose major work was in topology (the unknotting of spheres in five dimensions, the proof of Poincaré’s Conjecture in five dimensions) and dynamical systems. However, he is better known for his contribution to, and spreading awareness of, a theory he named “Catastrophe Theory”. The

original work on the theory was by René Thom. Zeeman was especially active in encouraging the application of mathematics and in particular Catastrophe Theory both qualitatively and quantitatively to such areas as the physical sciences, biology, neuroscience and the behavioral sciences. He received a knighthood in 1991 for “service to British mathematics and mathematical education”. (Wikipedia)

In 1968-69, he learned about dynamical systems from Smale and Thom. Zeeman spent a year at the IHES with Thom and learned about CT. He remarked that he was very fortunate to get in on the ground floor of such beautiful new subjects. Although he and Thom had a different philosophical approach to CT, both men saw the future implications of the theory. Zeeman wanted to get his hands dirty and make predictions, and get the experimentalists to test them because he knew that the scientific community would not take the theory seriously

unless it was capable of being tested experimentally. He has said that he’s been gratified to see several predictions confirmed. Some have been refuted and others remain to be tested.

14. General Systems Theory is the transdisciplinary study of the abstract organization of phenomena in the scientific domain, independent of substrate, substance, type or spatio-temporal scale of existence. It seeks to bring together those principles that are isomorphic to all fields of scientific inquiry. General Systems Theory was established as a science by Ludwig von Bertalanffy in the 1940s. He was reacting against reductionism and attempting to revive the unity of science. General Systems

Theory brings together theoretical principles and concepts from ontology, philosophy of science, physics, biology and technology. It investigates both the principles common to all complex entities, and the models (usually mathematical) which can be used to describe them. Subjects such as Cybernetics, CT, Chaos Theory and Complexity Theory (the study of complex adaptive systems – emergence, self-organization, adaptation, artificial life, artificial intelligence, far-from-equilibrium thermodynamics, neural networks…), fall under the umbrella of General Systems Theory. (Wikipedia)

15. Zeeman, E. C. *Catastrophe Theory: selected papers 1972-1977*, Reading, Massachusetts: Addison-Wesley,1977.

It was the modeling in this book that was criticized by Smale in the *Bulletin of the AMS* in 1978 and by Sussmann and Zahler in *Synthese* in 1978. The interested reader should check Zahler and

Sussmann, "Claims and accomplishments of applied catastrophe theory" in *Nature*, **269**, 27 October 1977, 759-763 and the correspondence that ensued in *Nature*, **270**, 1 December 1977, 381-384 and *Nature*, **270**, 22/29 December 1977, 401.

16. Paracelsus (c. 1493-1541) was a Swiss physician born with the name Theophrastus P. A. B. von Hohenheim. He developed a new approach to medicine and philosophy based on observation and experience. He saw illness as having a specific external cause rather than an imbalance of the body's four major fluids: blood, phlegm, yellow bile and black bile. These fluids were thought to determine a person's physical and mental qualities by

the relative proportions in which they were present in the body. Paracelsus introduced chemical remedies to replace the traditional ones. (OED)

17. In *Structural Stability and Morphogenesis*, pages 39-40, Thom writes: "The basins associated with different attractors can be arranged in many topologically different ways ... For example, on a contour map the basins attached to different rivers are separated by watersheds which are pieces of crest lines and these separating lines descend to saddle points, where they meet like ordinary points, but rise to summits, where they may have flat cusp points. In other cases […], the mutual arrangement of two basins can be very complicated. It can happen that the two basins interpenetrate each other in a configuration that is topologically very complicated yet structurally stable; for example, in two dimensions the curve separating the two basins

can spiral around a closed trajectory. It is possible in such a case to speak of a situation of struggle or competition between two attractors…"

18. Konrad Lorenz (1903-1989) was an Austrian zoologist, animal psychologist and ornithologist. He is often regarded as the founder of modern ethology. He studied the instinctive behavior of animals and rediscovered the principle of imprinting while working with geese. He is known to the general public for getting the Nobel Prize in Physiology or Medicine in 1973 for his "discoveries in individual and social behavior patterns". In 1966, he published the book *On Aggression*, and brought ethology to

the attention of the general public. In his 1973 book, *Civilized Man's Eight Deadly Sins*, he discusses his theory of ecology.

Thom discusses some aspects related to the above in his *Semiophysics.* On pages 11-12 and 20 he writes, "[…] it is doubtful whether genetics alone would be able to code a visual form. An object in three-dimensional space has an infinity of apparent contours, and neither DNA nor any other chemical support contained in the egg would ever hold enough information to code them all. Whence the necessity of invoking *cultural* transmission, linked with the social or family organization of the community. [...] It would be as well do draw a distinction between imprint and conditioning. An imprint is a genetically programmed phenomenon producing an irreversible effect that no subsequent experiment can undo. […] An imprint is characterized by its irreversible nature, associated with a limited period of sensitivity. [...] Very strongly marked in birds, where it

was discovered long ago, this phenomenon is less evident in mammals. In man, the acquisition of language may be considered as the effect of an imprint that can be modulated by social environment; it is true that the mother tongue is mnemonically very stable."

19. See any book of Aristotle's *Poetics*, Chapter 21.

20. This was a neologism introduced by Thom. It is clear from his 1990 unpublished article, "On attractors", that the word "attractor" was used by him in 1966. Thom says that Steven Smale might have used it before then although Smale says it was

Thom that coined the neologism "attractor". The notion of a "strange attractor" came later when chaotic systems were being studied.

21. The mathematician to whom Thom is referring is Mauricio Peixoto (b. 1926). Peixoto is a Brazilian mathematician whose major work was in the study of structural stability of differential equations (dynamical systems), which was of great interest to Thom. His first contact with Thom was through Steve Smale. Smale had told Peixoto that Thom was working on a problem that was of interest to Peixoto. This was the origin of extensive correspondence and discussions that Peixoto initiated with Thom on the "closing lemma" and other related questions in dynamical systems. For Thom, the problem of stability was the following: Is the collection of structurally stable vector fields everywhere dense in a topological space B (in fact, a Banach

space – a complete normed vector space) of all vector fields? Peixoto's answer was 'yes', when the dimension of the finite or infinite-dimensional differential manifold was ≤ 2. (S. Smale and R.F. Williams answered 'no' for the dim. = 3 and Smale answered 'no' for dim. ≥ 4.) In *Structural Stability and Morphogenesis*, page 26, Thom writes, "Despite this negative answer [for dim. ≥ 3 – ed.], one must not think that the problem of structural stability has no interest in dynamics, for, even when the dimension is greater than 4, the function space B [the Banach space in question – ed.] contains at least one relatively open set where the structurally stable fields are everywhere dense."

22. Jacques Hadamard (1865-1963) was a French mathematician known for his contribution to the proof of the Prime Number Theorem. His other work included the Theory of Partial Differential Equations, Hadamard matrices and Hadamard transforms. In 1903, he gave a description of *sensitive dependence on initial conditions* which plays an important role in chaotic events. He is more known to the general public for his book *Psychology of Invention in the Mathematical Field*. In this book, Hadamard describes the mathematical thought processes and experiences of mathematicians and theoretical physicists. He described his own mathematical thinking as largely wordless, often accompanied by mental images that represent the entire solution to a problem. (Wikipedia)

23. The American mathematician and meteorologist Edward Lorenz (1917-2008) rediscovered the phenomenon known as

sensitive dependence on initial conditions in 1963 when he was investigating a set of nonlinear differential equations. Out of these investigations, the Lorenz attractor was discovered. It is a chaotic map noted for its butterfly shape. It was shown to be a strange attractor for a certain set of parameters by Warwick Tucker in 2001. Thom is thinking 1975-1980 because little was known about Lorenz's work until the 1970s.

CHAPTER 2

PHILOSOPHICAL POSITIONS

Would you call yourself a materialist?

I don’t think so. I interpret matter from an Aristotelian perspective, as a kind of continuum which is capable of acquiring form. Form may be external, visible, or internal. Internal form is, from the viewpoint of semantics, what one calls a quality. The *materia signata*[1] of Aristotle is matter devoid of qualities. As I see it, all qualities can be exactly represented, to a certain extent, as a spatial form displayed in some kind of abstract space.

What is the nature of the space that contains this quality?

Let me hazard the opinion that the base substance is something like the prime matter of Aristotle. It's a substrate with the capacity to receive the qualities of all species, modified by all their predicates. Prime matter is a kind of idealization which can quickly take on qualities and forms.

…which are essentially pre-existing?

In a certain sense, yes. But Aristotle doesn't go very deeply into his conception of prime matter. To my way of thinking it has to be associated with a continuum, a domain of extension. You might describe me as a universal topologist. I've developed a veritable metaphysic of the continuous!

If I understand you correctly, there's a kind of identification of the concept of an abstract space with that of a "materia prima"[2]…

For me, they're more or less the same thing. Obviously, the abstract spaces in mathematics are generally of an algebraic nature, **vector spaces** for example. One can perform operations over them. In some sense, they're over-qualified.

Have you always thought like that, or is this something that you've developed in connection with your research?

I couldn't really tell you exactly when I developed this kind of metaphysics. I suspect that I've never been in sympathy, in general, with the metaphysics of positivism. On the other hand, I've always been highly suspicious with regard to any metaphysics derived from religions. My position is balanced on a ridge, one that is extremely narrow. I maintain it as best I can.

In this respect I don't believe my opinions have ever

changed significantly. I come from a traditionally Protestant family. My hometown, Montbéliard, has had a unique history: It was under German domination until the Revolution.[3] In the 16th century, the dukes of Wurtemberg ran everything, and they instituted the Reformation[4] there. Our church was Lutheran. We are in the very small category of French-speaking Lutherans.

In my own family, my father was not a believer, my mother even less so. It was our grandmothers who sent us to Sunday school because it was traditional. It has strongly marked me, although in a somewhat ambiguous manner, because I was fascinated and at the same time repelled by the way in which the Bible was revealed to us.

What held my attention, in fact, were the genuinely profound aspects of the Biblical texts. One can't read, for example, Genesis, without at once being gripped by the universal appeal of the narratives, which are both highly poetic and very deep. It's evident that most of them are probably lies yet, when interpreted as myths, I claim, nothing truer has ever been said!

What repelled me was the rite: I had to stand up and clasp my hands together in prayer; these were things I felt to be quite absurd as I didn't share the feelings demanded of a believer.

Basically, what interested you was the description, the interpretation, while the things that annoyed you were the practices based more or less on make-believe…

In the final analysis what repelled me was the obligation to be engaged in a certain way. I've always rejected any kind of engagement, whether religious or political.

What makes you apprehensive? A kind of alienation?

Certainly. Furthermore, I think that the kinds of people who become engaged are those who, to some extent, lack a personality of their own. Lacking a personality, that is to say, internal resources which are, properly speaking, one's own, is precisely the situation that encourages engagement.

They seek to justify themselves by external actions?

Yes, through their social usefulness, their group utility.

Would you say that there is a connection between the description of the world you received as a child from your exposure to the Bible, and that which you've arrived at through your work in mathematics and topology?

There's no direct evidence for mathematics in the Bible. By digging more deeply into it one might uncover some connections. I'm thinking, for example, of analogies. Here's one that occurs to me: the world before the Fall, and the world afterwards, as described in Genesis; the sublunar and supralunar worlds of Aristotle; and the opposition of classical dynamics to Aristotelian dynamics. They all

express something similar: One earns one's bread by the sweat of one's brow; that speaks of the inevitability of friction, of dissipative forces, the running down of terrestrial dynamics. Yet there is also Hamiltonian dynamics, the permanence of eternity, without friction, a virtually immobilized motion!

Without reference frames, motion has no meaning …

And too many changes of reference frame stifles motion!

Can one, in reading the Bible, look for cusps and folds?

A kind of "topological semiotics" of the Bible? I'm not inclined to risk it!

Doesn't one find all sorts of catastrophes in the Bible?

That's much too simplistic! There are catastrophes everywhere. It's finding pure examples that is difficult; and these pure examples aren't terribly interesting. What I mean by that is that when the San Francisco earthquake is "explained" by the collision of the Pacific tectonic plate with the American plate, this explanation doesn't mean very much; it doesn't tell us anything. It may, I think, be a pure or irreducible description of the catastrophe, but it's of very little interest to its victim!

I understand what you're saying when you state that nothing is explained by saying that the San Francisco earthquake was caused by the collision of the Pacific plate with the American plate: One has to uncover the causes that led to the collision of the two plates. These causes are themselves derived from others. Nothing is explained by shifting the focus of the problem. Each cause leads to a precedent cause, which is not explained. Eventually one comes to a first cause, which is inherently without any explanation. And even when God is invoked, one doesn't know why He's there…

Aristotle presents an elegant solution to this problem by identifying God with the first cause, so that the problem disappears; He is eternal in both senses…

Doesn't that solution impress you as an oversimplification?

Of course. But the value of Aristotle's thought does not lie in his theory of the supralunar; its real merit, in my opinion, lies in his theory of the sublunar. When I say that I will be offending those persons who interest themselves primarily in Aristotle's divine metaphysics, (the "idea of thought", the "pure act" as it is called by the scholastics), I personally think that all this is peripheral to the real world view of Aristotle.

What is proposed by Aristotle is not only an explanation of the universe; it's also a kind of justification for the human situation, together with the essentials of proper conduct.

That is one side of Aristotle, but I have to confess that it's not what interests me. I raced through the *Nicomachean Ethics*[5] and have only browsed in the literature on the social consequences of Aristotelianism. It arouses no enthusiasm in me. In the final analysis, he proposes an ideal of wisdom and moderation that has nothing transcendent to it. It's neither the charitable ideal of Christianity, nor its devotions, nor that of political engagement.

Is it your understanding that philosophy should be able to provide us with other information, other considerations?

I'm stating bluntly, to use Aristotelian language, that from the perspective of knowledge, we are in a permanent state of deprivation. We try to fill this void, and this leads to a project of research that is without end. We go from aporia* to aporia.

From impasse to impasse in other words. Is that also true for the sciences?

*** Translator's note: The French *aporie* (aporia in English) translates to antinomy, dilemma, bafflement, perplexity.**

In a manner of speaking, yes. To begin with, philosophy, real philosophy, is quite difficult. The work involved in entering into Kant's system[6], or in understanding Heidegger[7], is, I am convinced, just as hard as the study of higher mathematics. But one encounters this mystique, that it's somehow easier to talk about Heidegger than it is to discuss **analytic extension** or the Fourier transform! It just isn't so; in fact, it's easier to master a mathematical subject, one that is relatively precise and concrete, than an extremely interconnected and ramified philosophical doctrine like Heidegger's.

One also has to do work on mastering the technical aspects. It's generally believed that philosophy doesn't demand work of this sort. To me that just isn't true. Real philosophy demands lots of technical mastery. You can't understand Husserl[8] without going deeply into his system for several months or even several years. You can have only a vague understanding of Aristotle unless you've studied four or five fundamental treatises of *the Corpus Aristotelis*; that is to say, not just read them, but made a serious effort to understand them. To my mind, it is absurd to claim that philosophy is easier than mathematics. The respective difficulties are probably not comparable, because they aren't of the same kind. But one must actively oppose this wrong-headed notion.

Of course, if you're interested in ethics, or the problems that progress in biology cause society, these are things that interest the world at large because everyone is concerned

with them to some extent. But the real problems, in the final analysis, are not those with which most people are concerned. These are problems which one can only approach after a long apprenticeship.

Would you not, all the same, try to help people who haven't got the prerequisite mathematical background to understand what you've done?

I retain my skepticism about the possibility of popularizing mathematics. Mathematics is something one learns, not something one popularizes. The ideas behind CT can't be explained to someone lacking the mathematical rudiments, by which I mean, at the level of courses of higher studies in mathematics such as one finds in the *Taupe*, or the *Diplomê d'Études Universitaires Générales* (DEUG)[9] in some mathematics department. Even that probably isn't sufficient. One has to go a bit beyond that. Otherwise, what one's doing is just so much blah-blah, talking about Heraclitus[10], conflict, and so on.

In the last analysis, popularizations are only interesting for book publishers and don't have much to do with the advancement of knowledge.

All the same, and the analogy may appear a bit free-wheeling, in order for there to be tennis champions you've got to have lots of people who know how to play tennis. Don't you think that, for a country to produce a certain

number of researchers, it is necessary that there be enough informed public opinion about what's happening in the sciences?

This topsoil theory only applies to the period before the Napoleonic era. At that time, science was something of a hobby. One didn't make a living by being a scientist. The French Revolution brought in the questionable reform of making science into a social activity underwritten by the state.

And this, you think, has made popularization less useful?

I'm not against popularizers, to the extent that they satisfy public curiosity. I don't think it's possible to do better than what one finds in magazines like *La Récherche* and *Pour La Science**. They are both of a high quality, and they appeal to persons with a large range of education, certainly in France. The presentation in them is acceptably rigorous. These are excellent mediums of popularization. I've only one criticism to make with regard *to La Récherche*, which is that it maintains a lobby of scientists whose contributions are regularly published.

*** Translator's note: La Recherche (http://www.larecherche.fr) is a high quality French science magazine. *Pour La Science* is the French edition of *Scientific American*.**

A journalist told me one day: "Above all, no controversies!" I recognized that it isn't a good idea to air disputes between scientists, above all those which reflect base considerations of commercial competition. These discussions are about giving credit here, and withholding credit there. They are often concerned with showing that what is being done by one person is somehow not as fundamental as what is being done by oneself. Most scientific disputes are of this character…

The continuous and the discrete

You say that you had developed a veritable metaphysics of continuity. This notion of the continuous underlies all of your ideas in Catastrophe Theory…

To my way of thinking, the problem of the reality of the external world is a subtle one. Whether the ground of nature is continuous or discrete is a metaphysical problem, one that I don't believe is answerable. My personal inclination is to be what one might call a "continuist", despite the fact that I emphasize phenomenological discontinuities. I basically believe in the continuous character of the universe, and of its phenomena, and the substratum for phenomena. And that is exactly the essence of CT, the relationship of apparent discontinuities to the slow progress of an underlying evolution. The problem then

becomes one of determining that evolution, and that, in general, requires the introduction of new dimensions and new parameters.

Thus, you aren't talking about the evolution of small set of discrete points but, for better or worse, a continuous evolution?

It is always some continuous object that evolves. One has to imagine what mathematicians call a **wave front**; that is to say, a surface varying as a function of time, which can deviate and, as it does so pick up various attributes, branch out, or undergo numerous transformations.

That we're designed to observe discontinuities neither proves nor refutes the material existence of discontinuity…

To say that continuity exists doesn't exclude the possibility of the action of a discontinuity on the continuous. What I object to is a modern 'received wisdom' which derives basically from computer science, which claims that everything can be reduced to bytes. Take anything at all in the universe: One can always fabricate a mathematical model for that object and then represent that model in an algebraic fashion. This can then be translated into a computer program with a certain number of bytes.

Although computer science has an enormous influence on public opinion, this doesn't mean that one must believe

that all of nature can be reduced to bytes. Consider this typical example: One has the impression that everything one sees on a television screen is continuous. But when you find out how it works, you discover that it's a matter of an effectively infinite number of cells capable of illuminating the screen which one can treat as points. This point lattice is swept by a spotlight which selectively illuminates these cells. If the notion that all of nature is like a television screen became prevalent in world opinion, one would readily conclude that, ultimately, wherever one sees continuity there is really only discreteness, discrete particles and nothing else.

The discrete is therefore tied to our modes of perception?

It's often like that: The discrete character of a transformation is a simplification created by our organs of perception. We are essentially designed to see discontinuities. They alone have meaning. It is essential to an animal that it recognize its prey: It must be both recognized and localized. Therefore, there have to exist mechanisms in the nervous system that make possible to instantaneously discriminate between what's living and what isn't. Among the criteria required for this discrimination must figure the identification of the discontinuities and general contours of the object.

Then there are activities, such as human language, which assume this discretization: Spoken language is

formed from discrete phonemes. Yet underneath these phenomena, at their foundations, lies something continuous. Although a Fourier spectrum can be very complicated, it can be continuously modified with the help of a sound synthesizer. The sound of "B" can be continuously changed into the sound of "P" by a simple transformation. But when someone listens to them he knows right away "This is a 'B', that is a 'P'". He will perceive a complete discontinuity between the two sounds; he will not be able to perceive the continuous transformation.

Does that depend on his interpretation?

It's what's called *categorical perception*: It comes from the fact that an auditory continuity is suddenly drawn to attractors, each attractor producing a specific sensation, with its own classification.

The brain, in the act of interpretation, sets up sectors …

That's what discretizes. But not everything is discretized. Space, for example, is not discretized. We've held onto a continuous intuition of it. In the same way, time appears continuous to us.

Why, in your opinion, do we discretize certain things, while retaining the intuition of continuity for others?

In the case of phonemes, the answer is simple: As language is formed by combinations of phonemes, it's important that one phoneme not be confused with another. There has to be a sharp boundary separating phonemes. Now that my hearing is going, I've come to understand that this isn't simple! I mix up phonemes and that interferes disagreeably with my wish to be understood.

Understandably, there are situations in which it's important to discretize. There are others, on the other hand, where it's important to retain continuity: The process of grabbing objects in space is based on the continuous. We have certain mobile machines, in our muscles and our articulations, which allow us, practically at every point, to touch the whole of a given domain with our finger. Continuity is represented by the actual employment of our motor apparatus. We can also estimate it through our inherent sensory makeup. When it's a matter of the way in which it deals with space, the system functions in a manner which is essentially continuous.

This is bound to outrage the neurophysiologists, but from the aspect of subjectivity, there can be no doubt that we function that way.

You've been speaking of the event as it is lived. Yet when one studies motion through the synthesis of images, it clearly depends on images which are, by definition, discrete so that motion is being analyzed image by image. It's the persistence of the image on the retina which gives us the

impression of continuity, whereas in reality we live in a world as discretized as can be imagined…

Yes, that's an interesting problem. This example is often invoked to show that our intuition of continuity is false. A motion picture consists of only a finite number of images, yet it gives us an impression of continuity: Thus, it is argued, continuity is an illusion. However, I don't think that this line of reasoning is tenable. Illusions themselves, after all, exist in some sense, possessing their own ontological status qua illusion. If there is no external continuity, I don’t see how it can be created on the inside.

More to the point, I believe that the origins of the current scientific fascination for the discrete are primarily operational. When computer programmers want to replicate a surface they decompose it into pixels: A grid of tiny squares is constructed, each of which receives a signal, either 'Yes' or 'No'. In this way the form ultimately reduces to an aggregate of squares. It’s a thoroughly barbarous way of reproducing a form, you understand.

But when the unit of discreteness is so tiny …

If the scale is very refined, there is a mental rectification which gives the impression of continuity. Our retinas, after all, only have a finite number of receptors, yet we have the impression that objects are continuous. If one wants to defend a Changeux-type philosophy[11], one can argue that

everything is discrete…

What’s the other kind of philosophy?

Being a mathematician has habituated me to think about the infinite. Let me ask you this: If you’ve only got a finite number of neurons, each of which has only a finite number of states, how can you think about infinity using such a machine?

How do you deal with this objection?

I respond by saying that the hypothesis itself is false, that we're something more than a finite number of neurons, each neuron having only a finite number of states. The continuous exists also at the level of the brain.

But in what sense? I'd like to believe you, but I'm obliged, when I read books, to believe that there are a hundred billion neurons at the very most in the head of a human being, combined with a certain number of connections between them.

About 10^{11} in fact. However, each neuron is in turn composed of a very large number of molecules. And if one allows the molecules to vibrate, just a little bit, you're going to be forced to take into account the position coordinates of each molecule. Suddenly you've got to deal with gigantic

dimensions, inconceivable in fact. If you then concede that the space, in which the molecule vibrates, is continuous, you've picked up continuous parameters. Continuity is something one can't escape.

But if you are intent on invoking the shallow reasoning of neurophysiologists, who argue that the neuron possesses only two states, excited and inhibited, or excited and at rest, you're not going to get very far. It should be obvious to everyone that the model of the neuron as something possessing only two states is an immense oversimplification! Neurons are very complicated objects. Their representation spaces have to have incredibly large dimensions.

The neurophysiologists have studied a little beast they call an *Aplysia**: I don't know if it's a cephalopod or a mollusk, living in the port of Marseilles or perhaps somewhere along the riverbanks in Provence. They've discovered that there are only six or eight neurons in the nervous system of this animal, which leads them to say: "Aha! It's only got six to eight neurons. At last we have a way of learning how the nervous system functions!" And they've also observed that the behavior of this animal is extremely complicated, not perhaps as much as a human being, but quite complicated all the same, much more than can be explained by the combinatoric patterns possible to six or eight neurons capable of assuming only a small number of states.

***Aplysia* is a genus of marine mollusks of the order *Tectibranchiata* ; the sea hare. (Webster's Revised Unabridged Dictionary)**

Following your reasoning, I understand that neurons are very complex entities, and that an assemblage of neurons will be correspondingly far more complicated than that. But that still isn't infinity! In effect, it's an indeterminate number or one that is unbelievably huge. But are there unbelievably huge numbers which can serve as equivalents to the infinite?

Absolutely not! But the task of determining whether the dimension of the space of states of a neuron is finite or infinite is exceedingly difficult. Given that infinite dimensional spaces are very unpleasant to work with (excepting **Fourier and Hilbert spaces** which have a universal appeal), people have a tendency to hypothesize that the systems they work with have only a finite, indeed tiny number of independent states. It's impossible to deal with too large a number of them. Discretization should therefore be seen as an hypothesis concerning the available technology, not the nature of the universe. It's been imposed by the technicalities, by algorithmic thinking.

You've said that it's unpleasant working with infinity … Why so?

Because a space of infinitely many dimensions goes beyond our intuition. Determining the dimension of a topological space can be very difficult, and I don't feel at all comfortable in infinite dimensional spaces. I know that as

mathematical objects there's nothing unusual about them, their basic properties are well understood, but I don't like living in an infinite dimensional space!

Does it fill you with anguish?

No, it has to do with my education. Some of my colleagues aren't happy unless they're working in the spaces used in functional analysis, that is to say, infinite dimensional spaces. That isn't true in my case.

From aporia to aporia

You've stated that a discretization viewpoint comes from the technology employed in the sciences. But is it not a perennial feature of scientific research, to oscillate constantly between the accumulation of small discoveries and great leaps forward?

My belief is that, ultimately, scientific activity continually returns to a central issue, a kind of *aporia at the foundations*[12]: A given science attempts to solve it. It proposes solutions that, after a certain length of time, reveal themselves to be illusory. The process begins all over again, improvements are made, until one discovers that what one has is again an illusion, and so on. The basic problem is untouched, that is to say the aporia…

Is the problem always capable of being given an explicit formulation? Or is it perhaps buried in the Unconscious, as the psychoanalysts would say?

In certain situations there are specialists who recognize the issue, but they refuse, by virtue of their biases, to confront it directly: They are unable to look at it outside of a specific formal and conceptual bias. This is true above all in the "hard" sciences, like mathematics and physics. In the "softer" sciences, it may be less true.

The fundamental aporia of biology, for example, seems to me to lie in the incongruous behavior of living matter. No matter how you look at it, living matter does not behave like inanimate matter. It's a matter of indifference that an official doctrine exists which states: "Living matter must be subject to the same laws as inanimate matter". It's still a fact that they behave differently. Thus, when a dogmatic reductionism affirms that life must be reducible to mechanism and chemistry, there results an irreplaceable loss due to a kind of violence that has been directed against one's primary intuitions; this violence, with its underlying dissatisfaction, has a deleterious effect on the activities of scholarship.

Doesn't there always exist, behind every specialized activity, some non-formulated question, ultimately metaphysical, (in the best sense of the word, naturally)?

The 'aporia at the foundations' inherent in each discipline is, indeed, very often a metaphysical problem. Read the list of Kantian antinomies, or of the Categories of Aristotle. In the past, there was a very nice book on this subject, now forgotten, entitled *Les dilemmes de la métaphysique pure* (The dilemmas of pure metaphysics) by Charles Renouvier*. Such questions continue to reign at the heart of all our modern disciplines, even in the human sciences. Obviously, the more a discipline becomes peripheral, the more specialized it becomes. As the foundational aporia is replaced by technicalities, it progressively assumes the form of a direct or concrete problem, one for which one can contemplate solutions.

Would you say that the human and social sciences are better equipped to state these problems explicitly?

I don't know. As I see it, Sociology, for example, attempts to address a fundamental dilemma, that of the stability and origins of power: What causes this phenomenon of power in human society? If one focuses on a well defined historical context, the problem can be made specific. One tries to understand why the Roman republic evolved into an empire, or how the monarchy of France gave birth, between 1789 to1793, to a republic. The funda-

*** Translator's note:** ***Les dilemmes de la métaphysique pure*** **is not completely forgotten. It can be read in its entirety at** **http://www.ac-nancy-metz.fr/enseign/philo/textesph/Dilemmes.pdf**

mental problem has only been localized, made specific.

Would it be easier to deal with this basically personal inquiry in the context of those scientific disciplines based on a collective enterprise? That may not be the case with mathematics, in which one often works in isolation, yet we know that there do exist organizations of mathematicians.

Some of the work is personal and individual, some of it is collective: That is the part that has to do with exposing one's proofs to others. In mathematics, you have to prove something to yourself first, then you have to prove it to others. If someone is highly gifted, he can write down complex demonstrations all by himself right from the beginning. This requires a strong intellect. I'm afraid that's not true in my case …

What do you mean by an intellect that is not strong?

I'm referring to people who are capable of visualizing things, but are not very good at formulating them in a manner that renders them plausible or credible to others. There's a lot of difficulty involved in translating a personally held conviction into a social accepted belief. An immense distance lies between a personal conviction and a demonstration: One can be thoroughly convinced of something yet be unable to prove it, in the technical sense of the term.

It often happens that an intuition has been formed that hasn't been made completely specific. You know that it's true, but the moment you try to formulate it, in a style acceptable to others, you risk coming up formulations that inspire no confidence because you no longer know how to prove them. That's the problem: initial formulation, final formulation; the passage from a personal intuition to its translation in the language of one's peers!

And we know that the scientific community can be pretty rough! With good reason, furthermore: It's essential to the esteem in which science is held. One's demonstrations have to be totally convincing.

It can lead to a sad situation: Mr. X can propound a conjecture that he's not able to fully demonstrate. If a Mr. Y brings him the proof of the conjecture, Mr. X may be pleased to have formulated it, but can be miserable because he was unable to prove it himself, or found some other way to demonstrate it …

Coming back to this progression from aporia to aporia, were you aware, when you began your work in mathematics that you would be embarking on an endless quest?

No, because, all the same, something has been achieved. Mathematics is, in this aspect, a science that provides fulfillment. Honestly, if one proves a single theorem in one's

life, it can be said that one is participating in a kind of immortality, however one wants to take it. An illusion, possibly …

Would you call it a metaphorical immortality?

Well, yes, but among all the fictional immortalities with which we deceive ourselves, it's still one of the most solidly based.

Is that why people go into mathematics?

Mathematicians don't experience aporias in the same way. There are some striking aporias in this subject also: One can cite, for example, the debates raging about **Gödel's Theorems**. It happens to be true that, within the most widely accepted conceptual context in mathematics, the **Zermelo-Fraenkel Axioms**[13], one can show that it is impossible to demonstrate that arithmetic is free from contradictions. There is something 'aporiatic' about this, clearly. But it's possible to get out of it by arguing that one may be able to change one's axiom systems so that things work out. In reality, it is believed by most people who are able to understand it, that the aporia is here to stay, no matter how one approaches it.

Ultimately, from the mathematical perspective, one comes to see that all the foundational analyses put forth by mathematicians, though possessing a certain local interest,

do nothing to resolve the philosophical question of where mathematical structures come from.

All in all, can you explain what motivates someone to become a mathematician and then, to go into this or that type of research? What answers would you have given to these questions when you became a mathematician?

In hindsight, I'm of the opinion that the practice of mathematics creates a particular mindset that's not possible to obtain in any other way. For me, this is its principal virtue. It enables one to see things from a perspective which is not attainable through ordinary conceptualization. I see this as its essential role. I'm not talking about methods of calculation, despite what most people believe. That portion of reality, which can be well described by laws which permit calculations, is extremely limited. It is rather the capacity for abstraction that can take concrete situations and transform them into mathematical objects; that to me is priceless.

By that capacity for abstraction do you mean that which allows one to find relational systems between objects, or a combination of structures, in situations in which the epiphenomenal or anecdotal nature of the object is less important than the number of its possible configurations?

It's a point of view I came to in the course of developing CT. It's tied to the fact that analogies are not arbitrary. Analogies and metaphors, contrary to the popular opinion that considers them flexible or just approximations, impress me as exemplifying strict relations, ones which can, in many cases, be given exact mathematical expression, even when this expression isn't interesting in terms of the mental processes which lead you to construct the analogy.

I've already spoken to you about Aristotle's analogy which compares the relationship of twilight to age to the relationship of morning to life[14]. Old age is the twilight of life; twilight is the old age of day. There are two ways of stating it, one of them being more effective than the other. I'm interested in knowing why, but what is of greatest interest is the fact that this analogy, when looked at in a certain way, is absolutely true. The formal structure of this analogy is simply this: the notion of a boundary. You're given a temporal duration and this duration has a terminus. "Evening" and "old age" are descriptions of, if I can be so bold, tubular neighborhoods of this word "terminus". The corresponding catastrophe, for me, is the fold, the place where of stable and unstable regions meet. What I find curious about this analogy is that, when one considers the geographical context in which the distinction between day and night arises, one sees that it is a great circle around the earth. Over here is the sun, the rays of which form a cylinder circumscribing the earth; over there is a meridian of contact associated with each instant. However, seen from

the viewpoint of the ray of sunlight, this contact meridian is just a fold in the sense of CT. Looked at in this way the distinction between day and night is just the expression for the presence of a fold. Such an interpretation is not so clear for life and death.

For the analogy to be fully operative that which dies must come back to life …

For it to operate in a circular fashion, yes.

An approach such as this one necessarily appeals to intuition, to the imagination. One might speak of a kind of rapprochement between artistic creation and mathematical creation. In both cases one finds a kind of tension moving one towards a formalism. You've alluded to mathematics as a way of exercising control over disorder. Artistic creativity, in its own way, also tries to bring order into things. Are you satisfied with this parallel?

The problem of aesthetics is a difficult one. I've written about it somewhat[15], but I must confess that the elaboration of a satisfactory theory would be extremely difficult. I've the impression that at the root of "the aesthetic" one finds "the sacred". What is "the sacred"? It was this question that led me to my theory of *pregnances and saliences*[16]. The original idea is that all behavior, starting with that of animals, is controlled by the fact that when the animal

perceives a form in its presence, reactions of attraction and repulsion are released with regard to that form, whether they be visual, auditory, olfactory, and so on. In even the most rudimentary cases, one finds these reactions of attraction and repulsion.

I believe that the sense of the sacred in human beings is characterized by the fact that this axis of attraction/ repulsion can, in some sense, become self-referencing through being compactified by a point at infinity. This point at infinity is precisely what we call the sacred. Stated differently, a sense of the sacred is aroused every time we find ourselves in the presence of a form which appears to be endowed with infinite power, and which is simultaneously attractive and repulsive. As these two infinities are in opposition, one becomes immobilized relative to this form: Its fascination causes motion to cease. Because such a situation is intolerable for very long, certain accommodations emerge, which relax this paralysis through the phenomenon of sacralization.

The process is elaborated along two axes: that of attraction versus repulsion and that of the sacred versus the individual. There may be a reintroduction of an interrelationship between the subject and the form which is the source of the sacral phenomenon. This can occur in two ways: either the form becomes active or the subject does. When the subject acts on the source, one is dealing with the domain of the pragmatic. Conversely, if the source acts on the subject, one is, instead, in the affective domain. It is

here that the decomposition into attraction and repulsion occurs. I picture the representation of this division of the sacred on a plane with two coordinate axes: the first goes from the pragmatic to the affective, (otherwise stated, from the pragmatic to the purely aesthetic or purely subjective, the subjective sentiment which does not react); the second is the axis of attraction/repulsion. Underlying the plane along which action unfolds is the axis of effectiveness/

ineffectiveness. It also unfolds along the attraction/ repulsion axis. Thus, there are objects that are effective and attractive (which is the case with foodstuffs, in their purest biological sense). Opposed to them are those which are useless and repulsive, like excrement. Finally there are things which can be classified as effective-repulsive and ineffective-attractive.

I believe that Art belongs in the quadrant of ineffective-attractive. The art object in and of itself is of no use, but it gives one a sensation of pleasure: It produces a certain attraction.

The effective-repulsive, by contrast, is the domain for science and magic.

Let me return to my original question: Does not the fact that, in composing an art object one is led to create order from disorder, to formalize and , in consequence call upon systems of observation and of composition, indicate a homology with the procedures of mathematicians?

In a certain sense, I believe that Art goes far beyond the procedures employed by mathematicians. Those procedures are under very tight control. They are even under social control. The artist's methods are not free of a certain amount of social control, but art objects themselves are not very susceptible to being judged by objective criteria, nor even some kind of useful sociological criteria. To my mind, it's not ridiculous to speculate that very valuable works are hidden away in attics that no one knows anything about. However, I don't believe that one finds, in the real world, foolish persons who are content to merely think in their niche without wanting to be published, nor that there presently exist results in mathematics of which we are unaware that could revolutionize science. These are only my reflections, of course.

Qualitative-Quantitative, Continuous and Discontinuous: Matter and Thought …

In our discussion of CT, you've stressed its essentially qualitative nature: It doesn't address certain required quantitative criteria. It has value in terms of interpretation, so you've stated, but it isn't predictive in the least. I'd like it if you would clarify these notions of qualitative versus quantitative. For example, you've cited the example of hostility in dogs, but you qualified this right away by the comment that it couldn't measure this hostility.

Granted. Yet, if it isn't possible to give strictly accurate measurements, is it not possible all the same, as is done in experimental psychology, to establish levels by which this hostility can be classified? Wouldn't that represent a first step towards quantification? Where is the boundary between qualitative and quantitative?

Let's go back to the quotation from Rutherford: "Qualitative is nothing but poor quantitative." For my part, I'm totally convinced that the qualitative is a great deal more than just a mediocre form of the quantitative. The whole of topology is filled, verse and chapter, with examples in support of this conviction. In what respect is a sphere different from a ball? The answer is not really quantitative. How does a circle differ from a disk? It isn't a matter of quantity, but of quality.

Topology is inherently a qualitative, not a quantitative science. Mathematics throughout its extent abounds in interesting structures which aren't quantitative. Still, there is something to be said for Rutherford's comment. It is not grossly exaggerated, it's only incomplete.

Let me phrase the question differently: Grafting onto your formulation of a 'catastrophe', could one eventually talk about a frontier between quality and quantity? Or are we speaking about phenomena so completely different that one can't consider them to be either continuous or contiguous?

There is something that resembles a frontier: continuity. For me it's geometric continuity, or topological continuity, that underlies both the qualitative and the quantitative. Yet that which distinguishes them almost immediately makes its reappearance: For a connected object (this means that any two points of the object can be joined via a continuous motion which does not leave the object), the concept of its connectivity is a qualitative one. When an object fails to be connected, it decomposes into several pieces, its connected components, and these can be counted. Owing to this peculiarity, the attribute we call connectivity has both qualitative and a quantitative aspects.

It's intrinsically qualitative, yet the notion of the quantitative is engendered the moment one withholds the attribution of connectivity to a space or object. Ultimately this is a subtle distinction. In some sense, continuity is the universal substrate for all thought and of mathematical thought in particular. But it's not possible to think effectively about anything without the existence of something discrete in the unfolding of mental processes: One uses words, sentences, and so on. The *logos**, the discourse, is always composed of discrete entities: it can be words entering in a sequence, but they are discrete words. And what is discrete automatically implies the quantitative. There exist points, and we count them; there are the words

*** Logos, from the Greek for word, reason. In Greek and Hebrew metaphysics, it is the unifying principle (of reason and creative order) of the world.**

in a sentence: They can be classified, qualitatively by virtue of their grammatical function in the sentence, but it is evident that they possess an undeniable multiplicity. My reply to your question may not be completely satisfying, but I don't think one can say much more than that.

For me, the fundamental aporia of mathematics is this opposition of discrete-continuous. At this same time, this aporia has a dominant role in all thinking.

You've said to me that, in the course of developing CT, you were trying to relate apparent discontinuities to an underlying continuity. About a century ago there existed a controversy with regard to the central nervous system: Is it continuous or discontinuous? We all know that our perceptions are discontinuous. The question was answered by anatomy: Santiago Ramón y Cajal[17] was right. Neurons are contiguous. That may have nothing to do with human psychology. On the other hand, what is your view on this matter?

To begin with, I don't agree at all with your statement that everyone knows that our perceptions are discontinuous. When I look at you, I'm seeing you in a continuous fashion!

I withdraw the term 'perception'. I really mean 'sensation': The sensory receptors function in a discontinuous fashion.

Now you're talking like a neurophysiologist! However, I fall back on my primary intuition. What gives you the right to say that a researcher in neurophysiology has more insight than my own first impressions? I reject that argument.

Yet certain things must be accepted as givens. A few moments ago you used the example of a film: Images pass in succession, and, thanks to a bit of ingenuity, an instrument, and to retinal after-imaging, one experiences continuity. But when I try to understand how the world works, I sense that there is a continuous basis upon which all things unfold in a more or less discrete fashion. Where does one place discontinuity with regards to the relationship between the continuous and the discrete?

For myself, I'm happier with the notion that the discrete is manufactured from the continuous, rather than that continuity arises from the discrete. I realize of course that the standard model in contemporary mathematics is based on the definition of number given by Dedekind[18], by what are called Dedekind cuts. This makes it possible, in theory, to construct continuity directly out of arithmetic, that is to say, on the basis of the discontinuous. However, this process is in reality highly nonconstructive. It amounts to saying: The real numbers can be constructed by taking rational numbers and bringing them indefinitely close to one another. Then if one makes a cut, that is to say, a

division of the rationals into two classes, in such a way that each rational in the first class is less than each rational in the second class, (with the understanding that the differences between them approaches zero), this can be taken as the definition of a real number.

It’s the traditional method for making Gruyère cheese*: You take some holes and start building the cheese around them. There isn’t very much cheese but there are lots of holes! Finally one ends up with no cheese at all, only holes! How is it possible, with nothing but holes, to fabricate a continuous and homogeneous paste? I must admit that it goes beyond me …

The origins of all scientific thought can be situated in the paradoxes of Zeno of Elia[19]: notably the story of Achilles and the tortoise. In it, one finds the fundamental opposition between the continuous and the discontinuous.

Science encourages us to think in a certain way: Images derived through synthesis become acceptable when they are based on “points”, pixels which are virtually imperceptible as points. Quantum Physics also has reference to a tiny minimum quantum, which is at the same time the quantum of space, of energy, and so on. These are extremely tiny entities, the smallest conceivable ones, even theoretically. Does quantum theory argue the case for a discontinuous

*** A firm tangy cheese named after a district in Switzerland, where it was first made. (OED)**

universe? I don’t know myself, but it’s often spoken of in these terms.

Let’s take an example to help our understanding. Personally, I picture quantum mechanics as a string wrapped around a drum. If the string is of infinite length, one can wrap it an indefinite number of times around the drum. A quantum is one turn around the drum. I'm aware of the fact that this is only a metaphor but it is the way in which I imagine quantum transitions. Since, in fact, we don't know, we'd like to be able to see where these things happen. The real mystery lies in the quantum effect, the quantum transition, the electron attached to an atom, jumping from one energy level to another. In theory, this has an effect on all of space, not only the local context but the solar system as well, all the way to the outer galaxies. It's beyond our comprehension.

Is it your opinion that the notion of the quantum has operational value?

Yes! It's extremely operational, but it's also unintelligible. Let's speculate a bit about the notion of the 'soft photon': When ν [frequency – ed.] is very large, in the high frequency range, the photon possesses lots of energy, and therefore tends to behave like a particle: It can be localized, one can see a trajectory, and one says that it is a superposed wave packet. It's like a grain of energy for which one can

identify a specific locale.

If on the other hand one allows ν to tend to zero then, because of the action of the Heisenberg Uncertainty Principle, the photon can't be localized anymore: It extends, in some sense, over all of space, while at the same time, because there is very little energy at any given point, its physical qualities become harder to grasp.

Theoretically, when ν is set to zero, the photon stretches over all of space and its energy at any given point is zero. A 'soft photon' is, fundamentally, an entity with very little energy, and one would like to be able to say that it's of little significance because it's energy is so small: One ought to be able to ignore it. Yet, in fact, it stretches across all of space! It's a paradox: Such an enormous object, from the spatial perspective, which can at the same time have an energy which is virtually nil. Intellectually, it's scandalous!

I, on the contrary, find it gratifying. I have no trouble imagining it. A huge amount of energy at a single point becomes something that is virtually material. Progressive dilution causes the materiality of the object to disappear, and it becomes dissipated across all of space.

But now you're introducing something like qualitative thinking by appealing to the existence of some underlying continuity. That is how, in fact, the ψ function[20] of quantum mechanics is defined: The valuation of ψ, which is to say its length, measures one's ability to detect the object.

Effectively, when ψ stretches the full length of the abscissa (the sum of the squares of its absolute values of its components at each point adding up to 1), there isn't much to see.

One could say, therefore, that in line with a certain notion of continuity, together with the possibility of defining and observing a certain number of intermediate phases, one obtains something equivalent to what are called catastrophe folds in your terminology.

I am convinced that there exists a continuous infra-particulate dynamics underlying quantum mechanics. Take for example the "Young's holes experiment"[21]. Posit a source of light. When its rays are passed through two slits, one sees interference patterns forming on a screen in front of them. This occurs even if, in a manner of speaking, it occurs one photon at a time. Therefore radiation is discontinuous. That's what one says, in fact.

Then, one must somehow be able to imagine this radiation as a process affecting all of space, which crystallizes in these holes, in some sense, then gets recombined later on! Owing to the persistence of the perturbations it's undergone, it can only manifest itself on the bright fringes; it isn't there in the dark ones. This is very difficult to conceptualize but it's not in itself so far-fetched.

When ought one to discretize? Is it done to enhance understanding? When should one persist in maintaining

the perception of continuity? Does one invoke a continuous thread in order to get a sense, or a presaging, of something, whereas discretization is used to list, understand, classify? In fact, you yourself are constantly oscillating between

both alternatives, between a discretizing attitude and a form of thinking that situates itself in a context of continuity.

Indeed. I no longer remember which mathematician it was, in the 19th century, who stated that mathematics reflects two unalterable needs of the human brain: the need to see, which can only be done in a continuous fashion and the need to understand, which can only be done finitely, hence discretely.

That sounds like the Heisenberg uncertainty relations: One can't have both at the same time.

Certainly. What one observes, one doesn't understand. The same applied to the application of Heisenberg's principle itself: It's used, but it's not understood! However, physicists, through being obliged to employ it, have developed a certain flexibility in its application.

Quantum Mechanics is far and away the intellectual scandal of the century!

What do you mean by scandal?

What I mean by this is that science, because of it, has given up the quest for an intelligible world; it really has given up! The world that thrusts itself upon us is not intelligible.

Matter and Thought

I get the impression that you've set yourself up on the line of demarcation between materialism and spirituality.

I think rather that although the celebrated antagonism between Plato and Aristotle still exists, yet in many respects, one can consider attempting to reconcile them, at least to some extent.

What's your position?

I might say that if I were to adopt an idealist or spiritualist point of view, then only thought objects would exist for me. Yet ultimately, seen in a certain way, materialism leads to the same conclusion.

In what way?

Matter is itself an object of thought. In the final analysis, I think that all of existence is an object of thought. I should qualify this by saying: It is so when analyzed initially.

But, on the other hand, we're all perfectly aware of the reality of the world that surrounds us. With regard to the material constraints that weigh on the appearance of thought, there's no doubt that they exist.

It's what is called the argument of the truncheon. Materialism is based on such an argument: If I run at you with a truncheon and strike you on the head, your thinking disappears! This argument has its merits, one can't deny it. At the same time, someone like myself who believes in the existence of forms, claims that a Platonist explanation can be given: If the truncheon stops me from thinking, it is only because it has destroyed the form of my brain; and this form is necessary in a certain sense to the actualization of those spiritual forms which are my ideas.

Which are pre-existent?

Yes, which are anterior, ontologically anterior! I think that the problem of the anterior or primitive character of one mode of existence with regards to another is not a fundamental one. Which leads me to believe even more, that what is called matter is, at base, very difficult to define. When it's regarded close up, of course, one sees the details of the texture first, then one observes the molecules and the atoms. After that one looks for the particles. Eventually, the more refinement one brings to the analysis of matter the more it appears to dissipate itself into a kind of mist. With

the final result that, if the merit of materialism consists in its according primacy to matter, by according, that is to say, primacy to the scientific existence of things, one runs the risk of getting lost in this mist.

As I see it, the real problem, is this: Rather than debating the ontological anteriority of spirit versus matter, shouldn't one really be discussing the relative anteriority between what I call *naïve* existence and *scientific* existence? Naïve existence is the level of daily reality. We are things, we speak, we have a keen awareness that we live in a universe that exists, that you and I both exist. This, let's say, is a somewhat primitive form of existence.

Along comes science which says to us: No, for in fact this desk is made up of atoms connected by relationships and by the void. And what we think is substance is, for the most part, not substantial, it's mostly empty, full of holes. Should we believe that this reality, as depicted by science, is more fundamental than that which we experience in daily life? Furthermore, the latter contains both ingredients: the solidity of matter on the one hand, and also immediate psychological evidence.

This is how, more or less, I see things. I'm tempted to say that, for me, it is this naïve reality which is anterior to scientific reality. The latter is always constructed, and its existence has the merit of what is valuable in a scientific construction: It's temporary and it's capable of extensive revision. Whereas with regards to immediate reality, one has every reason to believe that the conception we have of a

tree or a rock is not fundamentally different from those of our Paleolithic ancestors.

It's more a matter of knowledge based on direct sense experience than of a capacity for giving fundamental explanations for things.

But what do you mean by 'fundamental explanations'? It turns out that, at the level of this naïve reality, that of daily life, we have a tool which enables us to develop many explanations: that of language. Ordinary language is a means for representing things that is endowed with an enormous capacity for description and interpretation. The phenomena that science claims to explain are things that are very difficult to encounter in our daily lives. It is quite rare that a scientific theorem is capable of direct verification. In the age of Archimedes, everyone could verify his principle of specific gravity in their own bathtubs. Today we are no longer able to use this procedure of verification by immediate experience, nor even by an extended meditation on a more enriched experience. Things have gone too far, everything is much too specialized. And I think that the extent of this specialization is leading us to a certain alienation from the world that we know in its immediacy. This situation is truly serious!

CHAPTER 2 NOTES

1. Basically, Aristotle divided the nature of matter into *materia prima* (prime matter) and *materia signata* (designated matter); that is, matter of quality and matter of quantity. For an object to attain a reality for a conscious observer, the concept of the object (its quality) must combine with the observer's perception of the object (its quantity).

In his *Dictionary of Philosophy and Psychology* (1901), James Mark Baldwin defines *materia signata* as "the quantified or spatially determinate material which is peculiar to a single corporeal individual".

See http://psychclass.york.ca/Baldwin/Dictionary/L2defn.htm under the heading Latin and Scholastic Terminology (Section 9).

2. *Materia prima* (prime matter) is seen in various ways. As noted above, it is matter of quality. It is seen as the primitive formless base of all matter: mere potentiality without actuality or realization. Also, it can be seen as an incomplete corporeal substance undetermined but determinable, capable of receiving any kind of substantial form.

D. D. Runes in his *Dictionary of Philosophy* (1942) defines *materia prima* as "pure potentiality, lacking all positive characteristics".

See http://www.ditext.com/runes/m.html.

René Thom in his *Semiophysics* (pp. 166-167) writes, "Aristotle defined prime matter as the essential subject of all opposition of contraries".

3. The French Revolution started in 1789. The Bourbon monarchy in France was overthrown in 1793 but the revolution failed to produce a stable form of republican government. In 1799, Napoleon became the supreme ruler in France and declared himself an emperor in 1804.

4. The Reformation was a 16th century movement for the reform of abuses of the Roman Church ending in the establishment of the Reformed and Protestant Churches. (OED)

5. One of the treatises in *the Corpus Aristotelis*. Aristotle's ethics focus on the character of the agent ('virtue ethics') as that which is morally good or morally bad.

6. Immanuel Kant (1724-1804) is probably the greatest and most influential philosopher since Aristotle. His aim was to discover and lay out universal principles of thought applicable to the whole of mankind and for all time. In his three *Critiques*, he sets out to: a) discover and justify the principles underlying objective judgments about reality (The Categories), b) give a rational justification for ethical judgments ("the categorical imperative") and c) outline his ideas of beauty and purpose. (Philip Stokes, *Philosophy: 100 Essential Thinkers)*

7. Martin Heidegger (1889-1976) was a German existentialist whose contribution to philosophy has been highly influential. He believed that the philosophical questions of 'what there is' and "what they can know about 'what there is'", presuppose too much. He focused on the "simpler" question 'what is Being?' by which he intends that before we can ask about what sorts of properties objects might said to have, we might first look and examine, in *a priori* (knowledge of something that is known to be true or false prior to experience) fashion, what it means for something to 'be'; that is, 'why is there something, rather than nothing'? He dealt with these questions in his book *Being and Time*. (Philip Stokes, *Philosophy: 100 Essential Thinkers)*

8. Edmund Husserl (1858-1938) was a German philosopher and founder of 'phenomenology' which is the descriptive analysis of subjective processes and events that lies at the heart of all existentialist philosophies. He believed that philosophy is a rational enterprise and that it should proceed like science. Husserl's phenomenology begins with the concept of 'intentionality', that all conscious states refer to a content, though that content may or may not exist, may be abstract or particular. Husserl thought that what is crucial to philosophy is to understand all the various ways in which intentionality manifests itself. (Philip Stokes, *Philosophy: 100 Essential Thinkers)*

9. Translator's note: French students sit for their DEUG after 2 years of university study. Students have the option of leaving the university after acquiring the DEUG, (which may be

awarded with distinction), or they may proceed to obtain the *license*. The certificates obtained specify the principal areas that have been studied.

10. Heraclitus (c.?535-?475 BCE) was a Greek philosopher who believed that the dynamism between opposites was the driving force and eternal condition of the universe. To him, strife and opposition were necessary and good, for the concept of universal tension ensures that while opposites may enjoy periods of alternating dominance, none shall ever completely extinguish or vanquish the other. This universal tension ensures that change is continual, that everything is in a state of flux.

11. See Chapter 1 Notes, Note 8.

12. For more on the *aporia at the foundations*, see Thom's bibliography, "L'Aporia fondatrice delle matematiche", 1982a, and also "Thèmes de Holton et apories fondatrices", 1982/1989.

13. Ernst Zermelo and Abraham Fraenkel completed a set of axioms in 1922 now known as the Zermelo-Fraenkel Axioms. The Zermelo-Fraenkel set theory (ZF) is the standard form of axiomatic set theory and as such is the most common foundation of mathematics. By adding the Axiom of Choice to the list, the set theory is denoted by ZFC. ZFC set theory consists of the single primitive ontological notion, that of a set, and a single ontological assumption, namely that all mathematical objects are sets.

Because of Gödel's second incompleteness theorem, the consistency of ZFC cannot be proved within ZFC itself (unless it is actually inconsistent). Nevertheless, almost no one fears that ZFC harbors an unsuspected contradiction.

Drawbacks of ZFC that have been discussed in the mathematical literature include the belief that it is stronger than what is required for nearly all of everyday mathematics. Some believe that compared to some other axiomatizations of set theory, ZFC is weak, for example, it does not admit to the existence of a universal set. Others feel that it does not do justice to the way mathematics works in practice; mathematics is not about collections of abstract objects, but about structures and mappings that preserve these structures. (Wikipedia)

14. See Chapter 1 Notes, Note 22.

15. See, for example, the following references in Thom's bibliography: "Local et global dans l'oeuvre d'art", 1982b, "L'art, lieu du conflit des formes et forces", 1984 and "The Question of the Fragment", 1987.

Another interesting article concerning dance is, "Life Scores", an interview of Thom by Laurence Louppe, 1994.

16. R. Thom's *Semiophysics: a sketch*, 1990 (originally published in French by Intereditions, 1988) deals in Chapters 1 through 5 with the theory of saliences and pregnances. On pages vii-viii Thom writes: "The hypothesis put forward here is that only certain configurations of elements make sense and can be used as a

basis for an intelligible construction that allows linguistic description. It's a question of picking out of the spectacle of stable elements in the shape of balls that will interact through contact, merge together, separate, be born and die (fade away) like living beings. These are *salient* forms. Such beings will also be able to interact at a distance thanks to the invisible go-betweens like light and sound. If morphology presented only a tangled mass of teeming and ramifying forms, then it would be difficult to discern meaning in it – except by assimilating it to luxuriant plant proliferation or the chaotic disorder of a raging sea. In this direction we find what I call *pregnances*, propagating from salient form to salient form which they invest as they go; the invested form consequently suffers a change of state (figurative effect) and can, as a result, re-emit the pregnance which may or may not have been modified, (the "coding" effect)."

In his theory of saliences and pregnances, Thom saw "conditions both necessary and sufficient for an onto-morphology to be intelligible".

17. Santiago Ramón y Cahal (1852-1934) was a Spanish histologist, physiologist, physician and winner of the Nobel Prize in Physiology or Medicine in 1906. He is one of the founders of modern Neuroscience and is most famous for his studies of the fine structure of the central nervous system. Using a histological staining technique developed with Camillo Golgi, Ramón y Cajal postulated that the nervous system is made up of billions of separate neurons and that these cells are polarized. He suggested

that neurons communicate with each other via specialized junctions called "synapses". This hypothesis became the basis of the *Neuron Doctrine*, which states that the individual unit of the nervous system is a single neuron. Electron microscopy later showed that a plasma membrane completely enclosed each neuron, supporting his theory. Ramón y Cajal also proposed that the way axons grow is via a growth cone at their ends. He understood that neural cells could sense chemical signals that indicated a direction for growth. He published over 100 scientific works and articles especially on the fine structure of the nervous system and especially of the brain and spinal cord, but including also that of muscles and other tissues, and various subjects in the field of general pathology. (*Nobel Lectures, Physiology or Medicine 1901-1926* and Wikipedia)

18. Richard Dedekind (1831-1916) was a German mathematician who did important work in abstract algebra, algebraic number theory and the foundation of the real numbers. His most important work was in the foundations of Number Theory. The axioms of the Natural Number System (now known as the Peano Axioms) were formulated by Dedekind in his 1888 essay "What Numbers Are and Should Be". His best known contribution was the rigorous definition of irrational numbers as classes of fractions by means of 'Dedekind cuts'. (Wikipedia and *World Great Mathematicians,* edited by G.R. Chatwal et al)

19. Zeno of Elea (c.?490-?430 BCE) was a Greek philosopher known for his paradoxes. These are the first recorded examples

of argument by the logical technique of 'reductio ad absurdum' (literally, reduction to absurdity) in which the opponent's view is shown to be false because it leads to a contradiction. He defended the view that the common sense notions of change and plurality are illusory. He did this by developing a series of paradoxes to show that they lead to very uncommon, non-sensical conclusions, thereby proving that they cannot represent the true nature of the world. For Zeno, the true nature of reality is an unchanging, indivisible whole. Although many future philosophers (including Kant) tried to find answers to Zeno's paradoxes, none were entirely successful. Only by using set-theoretic mathematics, which abandons the Euclidean definition of a line as a series of points, has a reasonably satisfactory answer to Zeno has been found. (Philip Stokes, *Philosophy: 100 Essential Thinkers*)

20. The ψ wave function (psi read as psee) is the wave function of a quantum particle. Schrödinger's equation predicts how ψ develops in time. The square magnitude of ψ gives the probability; ψ itself is the probability-amplitude or "psi field". It is a basic principle of quantum theory that quantum probabilities for events are found by adding probability-amplitude waves (psi fields) for all possible paths and then squaring.

21. Translator's note: Also known as the 'two-slit experiment'. Feynman has stated that all of quantum mechanics is contained in the interpretation of this experiment.

CHAPTER 3

ABOUT SCIENCE

You've talked about your need to study epistemology because you had to be able to reply to the objections raised against Catastrophe Theory. In what respect was epistemology needed?

It's because the sorts of criticisms I was receiving were epistemological in nature. I'd studied the philosophy of science a bit before getting into the study of philosophy in general that I was just telling you about.

I'd gone beyond troubling myself with the validity of CT to a more general interest in the relationship of all the sciences from the standpoint of knowledge. It was then that I started to develop my critical stance vis-à-vis the so-called experimental method, as well as the naïve faith that is generally placed on the virtues of the kinds of experiments on which progress is based. I've already talked about this. I've also stated to you that experimentation does not enable one to exercise control unless it is accompanied by a theory, one which provides the tools needed for extrapolation and making predictions.

Now, however, I found myself up against a stumbling block: In a science such as biology, for example, people reject the essential role of the imagination in the formulation of theory. Theorizing, from my standpoint, is linked to the possibility of immersing reality in an imagined virtual world, endowed with generative properties which permit forecasting.

Is this also valid for mathematics?

I assert that mathematics is, in its essence, fanciful! Unless one takes the materialist position of Changeux. Are you acquainted with his book that appeared a few months ago: *Matière à pensée*?*

You're referring to his discussions with Alain Connes[1]?

Yes; I was not impressed by their dialogue, since the arguments presented on both sides seem to come from some kind of faith. It's two certitudes in collision. Obviously, I would be more in sympathy for Connes' position than that of Changeux. However, in essence, their discussions aren't saying very much.

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* *Matière è pensée* **(Food for Thought) was translated into English by M. B. de Bevoise as** *Conversations on Mind, Matter and Mathematics*. **The French edition was published in 1989.**

More sympathy you say because Connes is a mathematician, or because you, to some extent, share his ideological assumptions?

It is certainly true that I've remained very much a mathematician. I'm therefore more in sympathy with someone like Connes, with whom I share the same discipline, than with Changeux. I also happen to think that the excessively narrow materialism of Mr. Changeux doesn't take one very far. I am of the school of those who maintain that, even in the sciences, introspection and thought experiments play indispensable roles. All major theoretical advances, in my opinion, have arisen from the capacity of their inventors to "get into the skin of things", to be able to empathize with all entities of the external world. It is this kind of identification that transforms an objective phenomenon into a concrete thought experiment.

Do you have any personal examples to recount of such introspective efforts?

I'm not a physicist. It's not the same thing in mathematics: Here one is in a context of methods, of mathematical tools, just like someone who has mechanical tools at his disposal. One employs a formal method in exactly the same way that one uses a pair of scissors to cut a sheet of paper; it's the same kind of thing. In both cases, a certain kind of spatial intuition is required. It comes from the phenomenon of universal continuity, which leads in the first case to formalization and in the second case to the customary geometrical instruments. In my opinion, it is in this domain that one succeeds in bringing about the union between these two modes of understanding, that which is called objective, and that which is intuitive and introspective.

Another look at relating the qualitative to the quantitative

In terms of method, of one's conception of the work involved in scientific research: relating the rigorous to the approximate, would you say that it is in physics, where quantitative models based on formulae work well that one can speak about rigor, whereas the methods you are advocating are more approximate?

The physicists talk like that. It brings us back once again to the famous quotation by Lord Rutherford that I've already cited: *"Qualitative is nothing but poor quantitative."* This exactly expresses the position you've stated: A qualitative description is a badly done quantitative description. My response to you before was that there is always a topological aspect in qualitative descriptions. Topology is a branch of mathematics that deals with formal properties of configurations that have nothing to do with spatial magnitudes. The topological analysis of a situation has a qualitative content that isn't quantitative. It is in this sense that CT is qualitative, not quantitative.

So, according to you, a physicist might think in that way. To return to epistemology, I've observed in a number of colloquia that there exists a certain rivalry, even a certain antagonism, between the positions taken by physicists, their way of conducting experiments, and those taken by mathematicians. Alain Connes, who was mentioned a moment ago, made a similar remark to J.-P. Changeux: Mathematical concepts often prefigure theoretical physical concepts from a great distance, sometimes putting in their appearance even before the physical phenomenon is identified.

You've touched on a universal problem, that of observation. Can one, from a phenomenal landscape, recognize a thing or object unless one already has a conception of it? It's as simple as that. If one doesn't have a concept of an object, one can't recognize it. Or one can limit one's remarks: Here one sees a wave, there a small valley, a tiny crevice, a hole. But these accidents are almost topological in character. They aren't quantitative. The ability to recognize an object in general, some entity in an empirical landscape, is always, in my opinion, dependent on conceptualization.

I believe I've heard mathematicians go further than that:

There are those who assert that the mathematical instrument is already present beforehand, ready to serve the needs of physical science. That the metaphors you're using invoke mathematical objects, whereas others speak about mathematical entities, must certainly have meaning.

I repeat; it seems to me that one can't observe anything unless one has a previous concept of it. However, experimentalists are able to argue that, starting from a preexistent set of concepts, observation or experiment may modify this conceptual system, and require one in some sense to diverge in other directions, thus allowing for the creation of new concepts. This is completely defensible. And it's certainly possible to find examples.

However, if one looks at the way in which scientific theories arise, one reaches the conclusion that the imaginative construction of concepts has generally preceded the data derived from experience. Most people try to make short work of this problem by brutally asserting that it is the dialogue between experience and thought that makes for progress. The metaphor of a dialogue is a pretty one, but one ought to try to understand how it unfolds.

It's my belief that the vast majority of mathematical concepts come from within. I totally reject the hypothesis that mathematical concepts can have an experimental origin, or of mathematical principles suggested by one's experience. One may cite, indeed, the concept of the Fourier Transform. Yet, this arose from the need to quantify something that existed before it was developed: the properties of musical instruments. Here one is always dealing with oscillators which emit sounds having a spectrum. One needed to understand how this spectrum was organized. In this sense, the theory of the Fourier Transform, fundamentally, derives from the study of vibratory phenomena, that is to say, musical instruments.

Where does music come from? Not from mathematics, I would guess. Still, the study of melody and harmony undoubtedly figured among the great sciences of Ancient Greece. The fact that one can associate consonant chords to the simple ratios of lengths of strings played a role.

Thus you think that the nature of mathematics is interior, that is to say that it evolves out of itself, that the enrichment of emerging chains of concepts constitutes progress. Mathematics is self-inventing …

I would rather say that it is engendered through a kind of inner dialectic which only progressively comes up to the surface. Millennia may be needed for a mathematical concept to accumulate all of its true riches.

Certain epistemologists have claimed that the Ancient World didn't have the concept of a function. Others assert the contrary: In ancient Greece one had the laws of static equilibrium enunciated by Archimedes. Aristotle proposed a law of inertia that was incorrect. All the same, it's very likely that the concept of a function did not then exist. Its appearance can essentially be dated to the 17th and 18th centuries, a period in which mathematicians were beginning to work with algebraic polynomials. Starting from these polynomials, it was possible to construct polynomials of a more general character. But it would appear that the rigorous definition of a function wasn't stated until 1695, by Leibniz. And then! What a magnificent instrument it has turned out to be for expressing the implications of the concept of determinism by scientific law! Before the emergence of the function concept, it was virtually impossible to define what one meant by determinism.

Did physics develop in step?

Absolutely. Otherwise stated, scientific progress is always dependent on the presence of an intellectual instrument which allows one to express correspondences and regularities between phenomena.

This raises a question in my mind: Are there any limits to mathematical discovery? Perhaps such a question belongs to metaphysics. Stated differently, is the number of concepts finite or infinite?

One need only look at **Cantor's theory of transfinite numbers** to recognize that the number of concepts is infinite, indeed of a terrifying infinity! However, the problem is that these transfinite numbers of Cantor are objects that are have no mathematical application. My colleague Dieudonné[2] stressed this point: These were objects that fascinated the mathematicians in the latter part of the 19th century. Today one realizes that they are completely inaccessible. One's dealing with a sort of delirium: These entities were the fruits of a delirious imagination. But they do exist: There can exist a kind of mathematics virtually devoid of content, just as other forms of mathematics are highly meaningful.

What do you mean by empty mathematics? Do you mean mathematics that can't be applied? Whereas, meaningful mathematics eventually enters into some experimental science?

It's a bit like that, certainly. There is a kind of mathematics that is contrived through simple extrapolation, arising from the internal generative properties of a structure. Suppose I take all of the positive integers 1, 2, 3… The operation +1 never stops. It takes one right to infinity. What is the ontological status of a number which is so great that it has no physical realization?

Benveniste[3] has been heavily criticized for his theory of water. He's made the claim that water has a memory, even when diluted by a factor of 10^{110}! The argument goes as follows: Since the number of particles within the known universe is not as large as this number, it's something like 10^{70} [about 10^{80} – ed.], such a dilution can't possibly be per-formed in our universe. I don't know how to evaluate this kind of reasoning, but it's certainly true that very large numbers create a kind of vertigo.

Whatever the virtual nature of the abundance of mathematical concepts, are you claiming that it underlies a material universe that is more or less fluid, more or less distant?

Yes; however, I believe that there is a basic core of mathematics that one is obliged to learn if one wants to be able to use it. This basic core is composed of exactly those things which have gone into the construction of physical laws. It supplies the means to represent the mathematical essence of this world.

On the nature of mathematical entities …

You often refer to 'mathematical entities'.

One may speak of structures or systems of association of the sort that in the past were called categories. There are Aristotelian categories, Kantian categories and so on. Something in this spirit is involved, I think, in mathematics, when speaking of mathematical structures. The status of such objects is obviously very difficult to explicate, because one hesitates between an explanation that can be considered purely psychological, (given that these things are in our heads, in our synapses, so that if these didn't exist, the entities wouldn't exist either), and another explanation which treats them as being objectively real. My personal feeling is that this is a misguided way of looking at things, and that the existence one ascribes to these things is probably derived through abstracting from concrete things. Nevertheless, these abstractions are so ubiquitous that one is forced to recognize that in some sense they are present everywhere in the real world.

Some of your colleagues don't hesitate to claim that these mathematical entities may exist even before physical experience, and that physics itself comes out of these concepts. Furthermore, you yourself have said similar things.

That's true. In my opinion, the thought experiment, in many cases, can go much further than experimentation in the technical sense of the word. The best proof of this is, in fact, that the ideas we entertain about matter differ little from those proposed by the pre-Socratic philosophers 2,500 years ago. We've been able to go further because we've developed the appropriate mathematics, that is to say, structures which, in themselves, are mental.

One is therefore talking about a kind of progressive elaboration, given that there is a definite continuity

between the mathematical conceptions of the ancient Greeks and those of today. There have been a certain number of things that have, in some sense, enriched their concepts.

I see things a bit differently, in the sense that, even if one were to adopt a purely materialist perspective, claiming that mathematical structures are merely the acquired residues of our cerebral activity, it is nonetheless true that these cerebral activities have not always existed. They've been created by an organism which forms them, and their formation is not exclusively the result of a molecular code, as the biologists claim. Laws of a physical nature are always present in biological morphogenesis, particularly in that of the brain. These laws can be given an abstract formulation, in the sense that when they are formulated in a manner which allows one to exercise control over them, it is always an abstract formulation. The truth of the matter is that one cannot escape the necessity of admitting abstract entities into the organization of reality.

Platonic ideas exist in a virtual universe: Might one treat mathematical entities in a similar fashion?

Mathematical ideas are products of our brains to the extent that we think them. But, since they exist even when we don't think about them, they must exist somewhere apart from our memories: They exist, I would say, else-where; they manifest themselves in a great many concrete situations.

They already exist, in other words, before they've been discovered?

Certainly! And they are actualized in some sense in specific cases, on this or that appropriate substrate. This is the old idea of participation which one finds already in Plato and which remains, I think, the correct interpretation. This is not incompatible with Aristotle's notions of matter and form, whereby matter is subordinated to form.

You've talked about the notion whereby psychic activity is produced by the brain, as stated by Changeux. Picking up again on this hypothesis, there are some who claim that, in the same fashion that mathematicians invoke mathematical entities, there is a soul …

I find something very satisfying in the Aristotelian conception of the soul as the form of the body. I would like to believe, and Aristotle is very formal on this point, that the soul cannot be separated from the body: This is explicitly stated in *De Anima**, and it's for this reason that he's been called a materialist. Yet from the other side, the body's capacity for being the support of a soul is something

*** One of the treatises of the *Corpus Aristotelis*.**

that is presented as a law of a formal character, precisely associated with a form in the morphological sense, that is to say, organized in a spatio-temporal way. The entirety is evidently tied to the form of the flows traversing the organism: blood circulation, neuronal. Generally speaking, these are metabolic. All of this is form for me, deriving from a resident form of an organizing character: the soul.

However, the structure, that which is somehow intrinsic to the resident form, is itself the end product of a formal structure, derived from that gigantic object which one gets by considering the form of all of the molecular and physical movements of our organism, which is of an extraordinary complexity! From the molecular perspective … The number of molecules in 22.4 liters is 6.023×10^{23}, Avogadro's Number. The number of molecules in the human body is of the order of 3×10^{27}. In order to represent the configuration diagram of the movement of these molecules, one needs a space of at least twice 3×10^{27} dimensions… that's rather big! And the convinced materialist, for the most part, makes appeal to the properties of matter under the assumption that they can be known, whereas, in fact, they can't be! Yet, the reasons for the existence of the properties of matter remain an enigma. Scholars are not in the habit of admitting their ignorance! It is true all the same that the phases of matter (solid, liquid, gaseous) still await a complete theory that takes account of them. Someone told me recently that even the solid state, with the crystalline state as prototype, has no fundamental explanation at the level of the laws of quantum mechanics. It's easily explained why such a state is possible, but there is no formal demonstration showing that it can't just as well be something else. In particular, the stability of the crystalline state has not been demonstrated in general.

Finally, consider the following example: Rule a plane by equidistant horizontal and vertical straight lines. This produces an equidistant configuration of vertices of squares. This can indeed be stable for an interaction potential between atoms which are placed at these vertices. But alongside this configuration you can have another one based on a series of hexagons. What is needed to convince one that the atoms will take up the configuration by squares rather than the one by hexagons? Responding to this question is difficult: In fact, I don't know of a satisfactory answer to it. Then, when one moves to 3 dimensions, it all becomes horribly complicated! It is believed that such things are understood; yet, in fact, they are not. Not to speak of chemistry where even the notion of a chemical bond is unclear. I told a friend of mine one day that in attempting to explain life by chemistry one ends up explaining *obscurum per obscurius**! I'm not sure he recognized the quotation …

The simplistic illusion of the materialists comes from the belief that we're in possession of all the laws. That's not

***Latin for explaining an obscure thing by something even more obscure.**

true! It's far from the case!

Even the fundamental ones? Rather than laws, what we have are a certain number of formalizations which explain the "how" of things …

Even that "how" poses difficulties. We aren't about to go all the way back to the Big Bang and the concentration of plasma into gluons, hadrons, and so on. That would really be swimming around in the modern mythology!

I take the naïve approach in these matters: I believe that one ought to take one's point of departure from ordinary macroscopic reality, which is familiar to all of us: the reality that you have, that I have, that is shared by this box on my desk. And, if we deny *a priori** all validity to this reality, we find ourselves condemned to solipsism** or to doctrines so subjective that one must consider them lunatic! One's point of departure must inevitably be this immediate realism, and it is from this that one has to construct those scientific entities which, alone, will allow us to penetrate more deeply into the organization of things. One shouldn't try to turn everything topsy-turvy in order to demonstrate the existence of this fountain pen, invoking the fact that I perceive it thanks to my retina, within a bi-laterally

******A priori* **is Latin for reasoning from a premise to logical conclusions. It is deductive or presumptive knowledge.**

******* *Solipsism* **is the view or theory that the self is all that can be known to exist. (OED)**

symmetric body, immersed in a thick intertwining of neurons and synapses. Between you and me furthermore, if you deny the existence of this fountain pen, how is it you're seeing it? As I tell my neurophysiologist friends: Why do you insist that I have faith in the reality of neurons and synapses, if you're denying me the reality of this pen?

Salience and Pregnance[4]

You've characterized Quantum Mechanics as "the greatest intellectual scandal of the 20th century", since it has obliged science to renounce the intelligibility of the world. Do your concepts of salience and pregnance help us in rendering it intelligible?

With regards to salience, its meaning is clear right away: A form is salient if it can be distinguished from its background. There is always a frontier that limits the object and separates its interior from its background. Discontinuity is somehow inherent in the notion of salience. Ultimately, it is only discontinuities that are propagated, and this is a paradox. (It's just occurred to me to think about it this way …) Pregnance belongs rather to the world of the continuous; yet, it is also the support for the propagation of entities, like sound, light and so forth. The discourse, the *logos* itself, is carried along through sonorous vibrations, which, though fundamentally not discrete in nature yet contain discrete elements.

One recognizes sounds on the basis of phonological discontinuities.

We don't live in a one-dimensional universe. The only universe in which a discontinuity can't be propagated is that of the straight line, the one-dimensional continuum. If you mark a point on a straight line, it is where it is: It's not going, in principle, to propagate. If it propagates, it stops being a point.

But if you take a 2-dimensional object like the interior of the circle, and place yourself at a point on the circumference, you could say that a discontinuity was being propagated because there is a tiny arc of the circle that passes by this point.

I derived the theory of pregnance, you know, from the study of animal behavior. The phenomenon of Pavlovian conditioning is a fundamental manifestation of pregnance. When a little bell is tinkled at the ears of a hungry dog before it's given a piece of meat, and the experiment being repeated a certain number of times, then merely tinkling the bell will cause the animal to salivate.

I interpret that as meaning that this tinkling, taking place within an interval on the time axis (there are silences before and after) fulfills the role of a salience. It's been saturated, invested with the alimentary pregnance that is carried by the piece of meat. The latter is a pregnant form for the hungry dog, and this pregnance behaves to some extent like a contamination fluid, investing the sensory forms close to the source forms, either in temporal-spatial continuity, or in similitude.

This fluid seeps into the field of phenomena by the cracks which are the salient forms. The fluid has very specific properties. An association like that of the meat with the tinkling of a bell will not persist unless it's reinforced. Being artificial, it disappears, unless reinforced by the experimenter. It exists in spite of associations which are rooted in nature and which, owing to this fact, are permanent: This is the basis of language.

The distinction between the human and the animal comes from the fact that the latter possesses very few pregnances: hunger, fear, sexual desire. These pregnances, however, are extremely adaptable: They can infiltrate a great many salient forms. This investment, however, is always reversible; it is never definitive, with the possible exception of the phenomenon of maternal imprinting, which has the largest number of the characteristics of irreversibility.

There is a certain amount of ambiguity in the concept of salience. Typically, a salience is visual: We can see that things are distinct from their background. But at night all cats are grey. Basically, salience is dependent on a light source that illuminates the object; and it is, ultimately, the irreversibility of the emitted radiation which is reflected, or diffused, by the object. It enters in through the eye and excites my retina. Thus, from the standpoint of physical processes, the manifestation of salience is dependent on a pregnance that originated from some external source.

Pregnance seems to resemble a source of energy which nourishes certain effects that become salient.

The concept of energy, in many respects, appears to me to be a kind of conceptualization of an undifferentiated pregnance.

What does it mean when you state that pregnance can be diffused[5]?

An enormous step was made, relative to the animal psyche, at the origins of the human psyche, even if the discontinuity may have been a subtle one. In the case of the meat and the investment with alimentary pregnance of the tinkling of a bell, one is dealing with a phenomenon which is, in principle, purely subjective, that is to say, relative to the dog which has been thus conditioned. Objectively, as a sound form, the tinkling of the bell hasn't got a thing to do with a dog's hunger: This is an association which belongs to the biological domain, and to the responses deriving from the interpretation of the subject. But it is a fact that a great many physical agents in the world play the same role as animal pregnances. I think one should search for their origins in the olfactory aspect of animal pregnances.

In the most primitive animals, pregnances are essentially of a chemical character, thus related to molecular diffusion. In general, they aren't visible, and they can only be transmitted through odors. However, the animal is conscious, all the same, of the fact that these forms come from a source. And since they are pregnances, this means that it's a simple matter of returning to this source: the pregnance is attractive.

This is the case with pheromones in insects: To invite copulation the female diffuses a perfume, a series of molecular messages which can be perceived by the male at considerable distances even when diluted to infinitesimal amounts. Once the male has received these molecules, he orients himself immediately – through the action of a chemiotactism* with respect to a concentration gradient of the subject – and moves towards the source. Between ourselves, detecting a gradient involves subtleties; both memory and a sense of orientation are needed.

But you've indicated a process of orientation based on a quantitative perception.

Of course. All gradients are quantitative. But they possess qualitative aspects. This biological process (ulti-

*** Chemiotaxis or chemotaxis is the tendency of cells to migrate toward or away from certain chemical stimuli. (Mosby's Dictionary of Medicine…)**

mately, chemiotactism is inherent in life) doesn't require the presence of synapses or a nervous system: The free cell, isolated, is already capable of tropisms* without difficulty; we're really talking about something very primitive in animate matter.

But I want to reconsider the passage from subjective pregnances of the Pavlovian type, to the objective pregnances of physical fields, before going on to sociology.

The possibility of giving an explanation arose when a way was developed to objectivize the pregnances, when it was realized that certain salient forms could be invested by entities with the same effects as those of the biological pregnances.

Here is a simple example: the kinetic moment of a solid object. If a solid body is moving towards your own organism, the movement of this body and its mass have a major biological significance: One immediately executes a movement of avoidance, a recoil to avoid the shock, which comes from the interpretation of the trajectory of this body. The kinetic moment, to the extent that it is directed towards us has, in a manner of speaking, both an objective definition and a subjective significance.

A moment arrives when, after he's witnessed the collision between two solid bodies, the thinking subject becomes, in some sense, the object of a feeling of empathy

*** Tropism, in Biology, is the turning of all or part of an organism in a particular direction in response to an external stimulus. (OED)**

for one or another of the two bodies (observe that the word "body" applies both to the stone and the human body: this situation is typical). The optimists identify themselves with the impinging body, while the pessimists identify with the impinged body, a distinction that, in principle, ceased to exist after Newton and Galileo, but which we continue to make nevertheless.

One can enunciate kinematic criteria which, in a collision, allow one to distinguish between the impinging and the impinged object, except for certain rigorously symmetrical situations that are the exception. It is because of this distinction that the concept of the kinetic moment has both objective and subjective aspects. In some sense, it has been generalized by scientific conceptualizing, first by empirical observation then by science properly speaking; and so, certain entities with the characteristics of pregnance have been objectified.

Take color. It's obviously something that incorporates both subjective and objective aspects: The characteristic red of a hot object has simultaneously both an objective and a subjective value.

Finally I've reached the conclusion that, at the fundamental level, our minds must confront the world structure which, at its origins, consisted of salient entities. These entities emitted pregnances which other salient entities were able to receive or capture. In that context, effects were produced that one may call figurative, which might have induced the invested entity to re-emit the pregnance, or, depending on the situation, one that was slightly different. In reality, what we've got here is the ultimate source for all cases of systems in interaction.

This recalls the dialogue on the relationship of background to form …

I don't see it that way. Paul Valéry[6] has said: *"Le fond n'est que forme impure"* (The background is only form in an impure state). What this means precisely is that when an entity is fractally disassociated, that is to say, it breaks into ever smaller and smaller pieces until these elements become so small that they are no longer perceptible, one has geometrically transformed a form into a background. It is this which I tried to give a name to as a "generalized catastrophe". In the absence of theorems, this terminology has died out.

As I explained a moment ago, the salient entity is framed; it's endowed with a definite form through a discontinuity, that of its boundary. This very profound idea is the key to understanding Aristotelian physics. There are citations from the *Physics* of Aristotle that I could give you, but they're unimportant … For him, the form of a physical object is something like its boundary; in the abstract meaning of the word, the *eidos** is also something like a form placed within an abstract space, together with its boundary. It possesses intelligible matter, which is in some sense contained within its definition; it's virtually the same word as *oros**, which means boundary. This is quite remarkable.

To define is to speak of frontiers, to draw frontiers?

It is, in effect, a delimiting by frontiers. And, in our opinion, this intuition contains something very profound.

Thus it isn't possible to have contiguity save between definite entities defined by frontiers, that is to say, discretized entities?

It is possible for entities to undergo defective transformations. They may, as stated by Aristotle, enter into a state of deprivation, the Aristotelian *steresis*. I conceive of this deprivation as a kind of wound: The boundary of a boundary is empty. This becomes, in mathematics, the great axiom of topology and of differential geometry, but it also expresses the notion of spatial integrity of the boundary of an organism. Deprivation is mutilation; it's the blood that spurts forth, and so on. It is the gap in form; form becomes deficient, and this affects its stability and its permanence.

In your progression from mathematics to epistemological reflection, by way of Catastrophe Theory, you

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*** *Eidos* is Greek for form, type or idea.**
O*ros* is for limit or boundary.

found yourself drawn to morphogenesis: In some sense you made a transition from catastrophism to morphology …

That's long past. I became interested in forms through the study of caustics. Then I underwent a revelation at the museum of Poppelsdorfer Castle in Bonn, where I saw models of frog embryos undergoing gastrulation, a beautiful kind of geometry, which I tried to interpret in terms of an unfolding of wave fronts in an appropriate space which can be projected onto ordinary space. It was from this moment, essentially, that I began to apply such ideas to embryology.

Is it possible to generalize? Can't one, ultimately, investigate the origins of all forms, and all formalisms, in this manner?

One can, in fact, pose the problem of morphogenesis for every kind of form, not only for living organisms. What happens is that the determination of most of the forms of inanimate objects present difficulties which, in principle, do not derive directly from CT.

To begin with there are the forms about us of which we are very conscious of, such as instruments, tools, furniture, dwellings. Catastrophes exist, but they are bound up with memories of various kinds. Consider the edge separating the ceiling from a wall; this is a line of discontinuity, of direction, from the standpoint of the materials utilized. Its origin is easily understood: It comes from the conflict between the necessity of having a vertical wall to support a horizontal surface.

It is the conflict of the horizontal with the vertical that creates the line that concerns us. The conflict is, in some sense, a formal one, finding its realization at a given moment, in the mind of the architect; this realization, when socially codified, translates into a process of manufacturing pillars and planks. It is not at all mysterious since we are very familiar with the psychic mechanism which engenders the object.

But this does not change the fact that, at the beginning, there was a conflict between two fundamental gradients: The conflict of a horizontal polarity with a vertical direction, which, in ordinary space, are dual.

For animate objects in their natural setting, it is very difficult to lay down simple rules which allow one to understand the creation of form. Consider plants: We know that they emerge from seeds that push and ramify, and produce leaves. The laws which enable one to describe this engendering are actually quite well known at the present time, but they aren't very rigorous, not enough to be able to vary the environmental factors independently. In reality, plants share many common structures, yet with an enormous amount of individual variability.

There also, when one attempts to go more deeply into the nature of these mechanisms, one encounters considerable difficulties. A description by appeal to laws is possible.

There is an immense body of literature on plant morphology, which in fact brings in rather sophisticated mathematics, the **Golden Section** for example. If one wants to explain the form, one must go down to the cellular level, even to the molecular level, and this entails a complexity that goes way beyond our understanding.

Polemics

At this stage, it impresses me that your reflections have overstepped mathematics into philosophy. And this transition has aroused much criticism…

Very little in fact. On the one hand, because the philosophers are not displeased to see that some of their problems are being addressed, even if they aren't being solved, and cast, more or less, into mathematical language; on the other, because the only real objections I've encountered have been precisely those of a philosophical nature, coming for example from defenders of Metaphysics.

I have been told by some people: Your intention is to reduce the individuality of a being to its connectivity; but the fact that a body has the form of a ball is not a sufficient criterion for establishing the individuality of its psyche. This is true! I am prepared to recognize that having a body in the shape of a ball does not suffice to determine the individuality of a certain psychological type.

Yet, it is a necessary though not sufficient condition for us to be the kinds of living creatures that we are. If the ball is broken in two, the mental structure disappears …

I don't know to what extent this implies the manifestation of an underlying materialism. I tend to believe rather that it is a proof of the importance of form. In discussions on this subject, the standard argument of the materialists, as I've already mentioned, is the bludgeon metaphor (I refer you again to the book of Connes and Changeux); "You are claiming that, through mathematics, you enter a world of abstraction which is not material? My opinion is that when I will crush your skull with great blows from my bludgeon, your intelligible entities will disappear! "

This is incontestably true, yet it doesn't convince me: If our brain is capable of accommodating so many platonic structures, it must be because of their existence within this magma of neurons, synapses and other "doo-dahs"! To the extent that the forms disappear, it follows that the Platonic entities will also disappear!

All this does is indicate that these entities are sometimes in need of a structure of a different nature, yet, one that is formed, endowed with a form, a *materia signata* as Aristotle says, or as he has been interpreted, perhaps, by his Latin readers.

When you talk about a ball, you're using a metaphor. Are you referring to a limited body, an inside and outside?

It's even more precise than that; it's a topological ball. It is not a full torus, like a blown up inner tube.

The digestive tract suggests that there are two different kinds of interior in the human body …

In fact, I've been embroiled in numerous arguments with people on this subject. I obstinately maintain that the inside of the intestine is inside the organism and that, indeed, it implies the existence of a frontier region with teeth, tongue, sphincters, and so on, to separate the interior from the exterior.

It is in precisely this case that one can speak of an interior which is more interior than another…

That's true, perhaps, but it's difficult to state correctly. The interior of the digestive tube is intermediate between the regime of the flesh external to the tube, and that of the outside world.

Your employment of the metaphor of a ball refers to a material space identified in some sense by its own proper limits and by its boundaries. If the frontier is broken, there is nothing left.

That's why I've recently been returning to Aristotle. I can easily accommodate his definition of an entity as something whose presence is separated from its environment. The boundary of an entity is exactly its form, that which supports form, its manifestation. One can go further, of course, and say that for living creatures the form is not limited to external form, as it is for a statue: It's also the forms of all the internal organs. There is, in fact, an internal form which prolongs, in some sense, the external form. Furthermore, in embryology, this is contrasted continuously on the basis of the external form. This is a temporal continuity between the tissues formed in the interior, and the initial shell of the blastula, as the embryologists would put it.

But in reality form is never simple, and is always composed of a number of other forms …

This touches on a problem of great difficulty: that of the parts. What is a part of an entity? There is an aporia of Aristotle's which to the present day remains a matter of considerable subtlety. In the case of the animals that interested Aristotle, he posed the problem of knowing if one part, in the traditional sense, the paw for example, or wing, or beak, should be deemed an entity, in the metaphysical sense that he ascribes to the word *Ousia**. It is reasonable to suppose that, in his spirit, that these would

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* ***Ousia*** **is Greek for being or substance. It is the most crucial of Aristotle's categories by means of which to describe a natural object.**

not be accorded full status as entities, because they are subordinated to the effective actualization of the global organism.

One can, however, distinguish between different parts of the body, diverse kinds of organs?

I've recently given quite a lot of time to this question. In my recent book, *Semiophysics*, I revisit Aristotle's reflections. He introduces two interesting ideas: that of the homoeomerous part and that of the anhomoeomerous* part. I would like to tell you something about the very mathematizable concept of homoeomerous. (I have at least this advantage over Aristotle, of having received a better education in topology!)

With a space containing a number of different qualities, a qualitative space, like a space of colors, it's possible to define an equivalence relation between two points: A point x is equivalent to a point y if I can cut out a small neighborhood of x and a small neighborhood of y, and I can continuously transfer the neighborhood of x onto the neighborhood of y, in such a manner that, in the course of this displacement, the qualitative structure of the space undergoes no change; this defines the equivalence relation between the two points. For good spaces, these have

*** Homoeomerous parts are members of segmental systems that are differentiated from each other and anhomoeomerous parts are overlapping or interconnected members of segmental systems.**

classical definitions in mathematics through very good equations. One can show that this relation is, in fact, to within a margin of error, equivalent to a decomposition into pieces, but pieces of strictly decreasing dimensionality.

Consider, for example, a polyhedron in 3 dimensions: Take a cube, a die for instance. If you use this method for classifying points, you will discover that there are 4 classes of points: those inside the die, those which lie on a face, those on the edges and those on their vertices.

This produces four classes of points. We next look at the connected components of these classes: I call these the ***strata.*** It is this method by which the space under consideration can be stratified.

Polyhedra can be stratified in this manner. The cube has one stratum of dimension 3 (the interior), 6 strata of dimension 2 (the faces), 12 strata of dimension 1 (the edges) and 8 strata of dimension 0 (the corners). In this fashion one has, in some sense, decomposed the morphology into ***equisingular*** pieces along the length of each piece.

This procedure is of particular value for biology, provided one doesn't look at things too closely. One can decompose a face into several elements, eyes, eyelids, eyelashes, front, nose, mouth, and so forth. One runs into difficulties however when, starting from this purely geometrical definition, one tries to reconstruct the parts of the body as we know them in the ordinary sense.

Take the hand: The thumb, from the standpoint of skin, cannot be considered as a separate entity. When one states that the thumb is a part of the hand, it's easy enough to define the boundary between the thumb and the rest of the hand at the level of the skeleton: The thumb is separated from the index finger by the metacarpal; and the articulating surface between the meta-carpal and the index finger is a boundary that can be cleanly designated because it is functional.

Yet around it one finds the tendons; they are connected by a whole series of ligaments. Then, enveloping everything one finds the protective cover that constitutes the skin. How then does one designate the frontier between the thumb and the hand?

Aristotle does not preoccupy himself explicitly with this kind of problem, but he introduces it alongside of his concepts of homoeomerous and anhomoeomerous when he says that an homoeomerous part is composed of several anhomoeomerous parts. One normally considers the organs as members with a definite individuation, a certain individuality, but composed of several parts.

Aristotle came up against a small difficulty with the intestinal membrane: It is relatively homogeneous in its aspect as a membrane, but all the same, it is separate from the interior of the flesh, and from that point of view, it serves as a kind of differential between the two. Is it an homoeomerous part or an anhomoeomerous part?

Contemporary biologists consider such questions a simple matter of semantics, therefore without interest. But in fact, these questions are deep, because linguistic studies have shown that the way in which the organs of humans and animals are named differs very little from one language to another. There is an almost complete isomorphism between the different ways of naming the parts of the body. Why is that? Why do we feel the need to designate a hand, an arm, a forearm? Yet almost every idiom contains these distinctions.

No doubt it's because these different parts possess sufficiently precise morphological characteristics that all cultures find that they need to name them…

Aristotle's reply is in his book on the parts of the body. He says, essentially, that the anhomoeomerous are the active parts, the working parts. And indeed this is the decisive factor which decides the unity of a bodily part in the ordinary sense: Basically, it comes from the direction of functionality and not that of structure. From the aspect of structure, on the contrary, one finds the phenomenon of polymorphism. Functionally, however, there is unity. The great problem is one of recovering this unity … above all when one is considering functions which are already integrated, like respiration, circulation, digestion, and so on.

It's not at all difficult to draw up a table of all the major biological functions. All of them are in response to banal constraints, a black box with entrances and exits. One classifies them in accordance with their nature; that may be their material makeup, or it may be more subtle than that, their form. And if the classification is material, one classifies them further by the phases of matter. After this classification is made, one already has almost all major physiological functions: respiration, which is at the same time the intake and expiration of air; buccal alimentation, the ingestion of solids and liquids; excretions in all 3 phases… It's all quite standard. There are in addition several other functions which are strictly biological, reproduction, irritability, and so on. The latter is not really a function, but a characteristic of all living matter.

At the present moment, I am the president of the *Francophone Society of Theoretical Biology**. This is coming to an end soon. But what's being discussed here doesn't interest biologists very much at present …

Is this the logical extension of your topological and morphological interests?

I've always been fascinated by problems of morphology. A good half of the contents of my first book is taken up with, no doubt prematurely, models for embryology. I think that this is justified in principle, but arriving at a sufficiently accurate description is difficult to achieve. This touches furthermore on another great difficulty in CT, that

*** Société Française de Biologie Théorique (SFBT)**
http://sfbt.lami.univ-evry.fr/fr/index.htm

of the substrate. CT, in its unadulterated form, is substrate independent. It is concerned with forms, independently from the nature of the medium in which they arise.

What are some of the most recent disagreements?

They don't have to do with positions that are related in any fundamental way with CT. They are rather directed at positions that I've taken at the philosophical level.

On the one hand, there is the discourse on determinism that's been unfolding in the magazine *Le Débat* since the 1980s. "*Halte au hasard, silence au bruit!*" (Stop chance! Silence noise!)[7] is an article I wrote directed primarily against the followers of Prigogine who were proclaiming the death of determinism in science. I persist in maintaining this position because I believe that science is, in its essence, determinist. It's sometimes obliged to deal with indeterminate situations, but always reluctantly.

I think I've heard you speak of deterministic chaos.

That had to do with the automorphisms of the torus, defined by the matrix: $\begin{bmatrix} 2 & 1 \\ 1 & 1 \end{bmatrix}$ and by the formulae $x' = 2x + y$ and $y' = x + y$. A torus is the product of two circles, and these formulae map the torus into itself. Such a transformation is called "chaotic". I find it something of an outrage that something that can be defined by 4 numbers and two vertical bars should be called chaotic. It's an abuse of terminology!

Other disagreements have to do with obstacles that I've encountered in scientific circles. For example, my ideas on embryology aren't those that resonate among biologists at this moment. Their virtually unanimous complaint can be summed up as follows: If you have suggestions or ideas on this subject, demonstrate them with experiments! We'll open our laboratories to you. Give us ideas for setting up experiments, we'd be very happy to do them.

Therefore, with my back up against the wall, I entered upon the study of modern biology, and I was absolutely dumbfounded to discover the manner in which work is being done in this field, or let us say, the manner in which it's being conceived. I'm not saying that the work is improperly done: They're doing a good job. Yet nowadays, when you try to explain something in embryology to these people, unless you've got a gene to show them, an enzyme or some kind of gadget, they won't listen to you.

And what would you like to propose to them?

My proposal to them is to formalize embryology in terms of abstract entities. Science exists to the extent that one is able to immerse reality in a controlled virtual representation. It's through the extension of reality by a more comprehensive virtual representation that one's able to study the constraints which determine the propagation of the real through the enveloping representation space.

Mechanics is no different. There is the space of all possible positions and all possible velocities of a solid object, then, the product space with time. You're given the trajectory and then you introduce the real inputs which are those of initial positions and other initial givens. What the formalism contributes is the whole trajectory that will be traversed in reality. The representative point is the injection of the real into the representational.

Isn't this level of complexity difficult to imagine?

I wrote an article entitled *Ambiguités de la complexité en Biologie*[8] (Ambiguities of complexity in biology), in which I reproached the biologists for gargling on this word "complexity", although biology is filled with many simple things. In certain respects, complex organisms can behave quite simply. I posed the following problem: "If one compares the movements of a cat to the way in which a cell propels itself through emitting its pseudopods, which of these impresses you as being easier to understand?"

Given that we are composed of cells, we ought to be infinitely more complicated than a cell in isolation. Still, we find that it's much easier to understand the way a cat moves than to understand the displacement of a cell, above all, because the cat always walks on four paws, whereas the cell can emit any number of pseudopods in any direction, at least in theory.

It's obvious to me that some kind of enormous psychological impediment is present in the minds of people who refuse to even look at the autonomy of certain levels of intelligibility in biology. They insist from the start that it isn't so. When they look very closely at the way one bone moves relative to another, they ask: What guarantees the synchronization of a given articulation with another one? How are they made up, what are the neurons, the dynamic motors of walking …? This way one always falls right away into complications. I readily agree that all this is complicated. But if one is looking for complications, one always finds them.

According to you, there exist simple ways of finding and setting up explanatory schemes, and thus, perhaps, progressively …

One should move progressively towards one's goal. Alongside the reductionist approach, which starts at a very minute level and tries to reconstitute the organism, there is, to my mind, an inverse approach which consists in starting with the large organic structures to which we give ordinary names. From this qualitative decomposition of the organs which we've inherited from language, (possibly through the activities of butchers and meat-eating traditions), the naïve anatomical decomposition, one ought to be able, by further discoveries in physiology, to achieve increasingly more accurate descriptions of local structures, those in particular which relate to their embryological genesis.

This is my ambition, and my program.

Let me give you a typical example. In the age of Vesalius[9] and Harvey[10], the heart was considered to be a kind of pump that injected blood into tubes known as the blood vessels. There is thus this analogy with tubes and a pump that was sufficient to make the functioning of the heart intelligible. It was said that our lungs were a kind of bellows. Understanding the need to have air in the body, only came with advances in our knowledge of chemistry. One had to acquire, from Lavoisier[11], the distinction between oxygen and nitrogen, before one could understand the necessity for oxygen. Then one had to imagine the living organism as a kind of heat machine. All of this, fundamentally, came from outside biology.

These approaches are analogical, metaphorical…

That these are metaphors is true, but I believe that there is as well a common core, a mathematical core underlying metaphor, which explains in what sense it is correct to say that the heart is a pump.

Once again, as Aristotle tells us, it isn't nature that imitates art, but art that imitates nature. It is because we possessed the scheme of a pump implicitly in our own hearts that we were ultimately able to construct the technology of the pump. And today, people are claiming that the brain is a computer! It goes on …

You've stated that our ordinary language is sufficient for explaining a certain number of states of consciousness. Yet, difficulties arise when one tries to use this language for the transmission of mathematical approaches. Why is this so?

The difficulty is mainly psychological. We aren't accustomed to it; we apprehend linguistic formalism and mathematical formalism as disjoint domains of psychic activity. I myself am inclined to say that it is the linguistic domain which is fundamental. The mathematical domain has a peculiarity, specific to itself, which is tied basically to the use of geometrical images: The ability to "spatialize" things, and to have transformation groups acting in these spaces. This is, at heart, the essence of mathematics.

Well then, there is a phenomenon in mathematics known as *iteration*, that is to say, the possibility of repeating the same thing over and over again indefinitely. The prime example comes from arithmetic: one counts by constantly iterating the same operation. The objects produced, however, are all qualitatively distinct. No integer is like any other integer. There is actually something rather paradoxical in this: Mathematics is built upon a sort of intrinsic monotony; structures can be generated indefinitely, yet, from another perspective, from this monotony, there arise qualitative distinctions, a whole qualitative universe.

And here we have arithmetic on the one hand, which I personally find rather dull, and on the other hand one has topology, the geometric and topological objects which I'm passionate about! The problem of understanding mathematical objects consists in returning to familiar mental entities, then trying to find correspondences between mental operations and these mathematical entities. Some of these mental operations can be modeled, or simulated by mathematical objects.

One can, for example, return to the mathematical roots of analogy. Here's an example: Acceleration and deceleration are essentially the same thing, only not in the same direction. Likewise, an acceleration and a derivative are the same. Thereby, one sees how a calculus, like the differential calculus, makes it possible to define very diverse objects from the standpoint of sensible perception. From that point of view, acceleration and deceleration are very different. However, one can find a single mathematical formula for calculating these two kinds of motion.

Perspectives on Research

What is your overview of science at the present moment, and of the relationship of the sciences to mathematics? What are the interesting directions in research, given the state of humanity at the present moment?

I think that it would be a vain endeavor to speculate on the ways that science may develop. I've always had the impression that it is, at any given moment, a form of exploration, and to the extent that there is something to find, scientists will find it. The nature of what is discovered can be very interesting or it can be totally insignificant. But it facilitates the work of scientists, (that is to say, outside of the activities of specialists who have a personal stake in this issue), that scientific results not be referred to as insignificant. In this way one continues to investigate everything that one can, and only afterwards interpret what comes out.

Could things be done differently?

I don't believe so. However there has been, in my opinion, a considerable downgrading in what was formerly referred to as the academic milieu. Those persons who constitute it have the benefit, (apart from certain material advantages), of quite a lot of social prestige. This has been degraded, perhaps because of errors made by members of the academic community, who've shown a tendency to allow themselves to be seduced by the siren song of the media. But there is also the fact that the college examination and college diploma are no longer considered necessary for success in life.

It's still probably true when it's a matter of escaping extreme poverty, but to "succeed" in the full meaning of the word, one needs more than a university education.

The great success stories of today come from other sources. Because of this fact, the academic value system has become devalued sociologically. People don't see any need to make a great intellectual effort, to devote 10 to 15 years of their lives to difficult studies unless they're guaranteed adequate compensation for it.

But, what about the orientation of research? What is the effect of these tendencies on the orientation of research?

Indeed, research has become, in some sense, a way of providing a career to many people who would not in the past have been able to qualify for an academic career.

What's going on in the sciences today is very institutionalized. And, in fact, that word is insufficient: It's regimented; it presumes a sort of regimentation. There do exist research structures, it's true. But their regulation only deals with the material aspect of things, research careers, promotions, and so on. The orientation of research in and of itself is not a research objective that's taken very seriously.

There is a kind of sociological determinism at play here. The kinds of research being done by people – I'm thinking of disciplines like biology – are essentially being directed by the traditions of the laboratory. These have the equipment, the "resources" as biologists would call them. With the acquisition of resources and locales, the experimentation done in them has to justify the investment, make it profitable. So it becomes, to an important extent, the machinery and the resources that determine the direction of research, and not the underlying theory.

Isn't the situation quite a bit different in fundamental physics?

Yes, of course, because in that case the expenses are so enormous that a theoretical justification has to be in place before society allows such large amounts to be invested. It's different from what goes on in the space program: Outer space grabs the public. Sending out a probe like the Voyager, which will take close-up photographs of the planets, which can later be looked at, their features appreciated and debated, which stimulate conversation, that's highly satisfying… Whereas the construction of an enormous accelerator of 30 km [18 miles – ed.] in diameter, for the purpose of finding evidence for the existence of this or that particle, may be of great interest to specialists, but very few others. It's difficult to convey the value of the return on this to the general public.

And yet you've qualified Quantum Mechanics as the "great intellectual scandal of the 20^th^ century"!

I'm under the impression that if enough money were to be invested in trying to make Quantum Mechanics intelligible, as is being invested in the construction of large accelerators, we would have succeeded in finding a model for explaining it that is intellectually satisfying. This may be an idle speculation, but I harbor a suspicion that the specialists told themselves: "We've got a functioning theory, so don't try to go beyond it, don't look too closely at the implications of the formalism."

What you call a functioning theory, is that one which allows one to make correct predictions of a certain number of things?

It's a theory which, in some sense, creates its own experimentation through a kind of generating principle inherent in its formalism. It therefore becomes difficult to attack from the customary scientific point of view. We've reached the point at which physicists need theories to produce actual phenomena that can be kept under control. This is the case in high energy physics, the mechanics of elementary particles. In fact, this is a curious situation: People seem to be motivated by the need to produce a theory basically in order to justify their experiments. These experiments have become so costly that it's become absolutely necessary to come up with a theory. It's unacceptable that humanity be expected to spend such large sums merely for the satisfaction of a few individuals. From this point of view biology doesn't present the same problems: As a discipline, it doesn't cost so much. That's why it doesn't bother with theory: One can do one's experiments without it, and without having to worry about one's laboratory being called to account.

What did you mean to say, exactly, when you stated that science has renounced intelligibility?

All I'm saying, simply, is that if science is reduced to a collection of recipes that work, one's intellectual situation is no better than that of a rat which, on pushing a lever, will cause some food to drop into its bowl. The pragmatist interpretation of science brings us to the situation of the rat in its cage.

Do you exempt mathematicians and physicists from this picture?

With regards to the physicists, one cannot deny that they've worked hard to 'synthesize' their recipes. All of them come out of a certain number of principles which can be expressed mathematically: Newtonian Mechanics, Quantum Mechanics, Relativistic Mechanics. Principles have been derived from them which allow one, at least in theory, to deduce laws applicable to a wide range of phenomena. At the level of this formalization of science, the attempt to provide a synthesis becomes a metaphysical quest, equivalent to claiming that what exists comes from a God who creates and organizes everything. This responds to a spiritual need: The tendency to unify is a fundamental spiritual need.

And the other disciplines?

The other disciplines don't have these intellectual difficulties. Science, in the large, is not intellectually difficult. This may come to you as something of a surprise but, with the exception of mathematics and physics, science presents no intellectual difficulties.

You were telling me that you consider philosophy to be something which is not simple, whereas ethics is. You've stated that the real problems are not those which preoccupy the world.

I'm not convinced that ethics is a branch of philosophy. We're still subject to that former ideology which equates philosophy with wisdom. It's not entirely misguided to think that philosophy may confer a certain degree of wisdom, which is perhaps its only justification…

It's certainly true that the Ancients considered the acquisition of knowledge basically in terms of an ascent towards wisdom. This idea should not be totally rejected. Knowledge that is fully aware and well assimilated can, I think, establish a framework which allows one to attain to a certain kind of wisdom.

But when it comes to ethics, one is always dealing with concrete issues: abortion, the use of embryos in research, certain kinds of medical therapies, things like that. It's extremely difficult to take a position without having a doctrine which sets up a moral code …

The insights that science or philosophy can bring to such questions aren't all that fundamental. They can give one an appreciation for the constraints to which the facts are subject, as well as the benefits to be derived from them. That is to say, a kind of accounting … Unfortunately, things are complicated, there is always some kind of new element that completely ruins one's expectations, despite the good intentions of the positions taken …

A "Map of Discernment"…

In the days when Catastrophe Theory was in its glory, I had lunch with the psychiatrist Jacques Laçan[12]. I'd been invited by the Master, and he'd encouraged me to talk freely all through the meal, about my views on mathematics, about my career, about the evolution of my mathematical ideas, on my relationship to the "mathème"*. I'm not sure I understand what the "mathème" is!

For his part, he said practically nothing. At the end of

*** The mathème is a concept introduced in the work of the 20th century French psychoanalyst Jacques Laçan. They are formulae, designed as symbolic representations of his ideas and analyses.**

the meal I came up with a phrase that made him sit up. I said to him: "Truth is not limited by falsity, but by insignificance. " He fell into a reverie and said, "That gives me pause, that gives me pause..." There you have it: I connected with the Master! This statement, which I tried to explain in an article[13], is best explained by making a drawing (see page 179), a kind of "Carte du Tendre"[14].

At the base, one finds an ocean, the Sea of the Insignificance. On the continent, Truth is on one side, Falsehood on the other. They are separated by a river, the River of Discernment. It is indeed the faculty of discernment that separates truth from falsehood. It's Aristotle's notion: the capacity for contradiction. It's what separates us from animals: When information is received by them, it's instantly accepted and it triggers obedience to its message. Human beings, however, have the capacity to withdraw and to question its veracity.

Following the banks of this river, which flows into the Sea of Insignificance, one travels along a coastline that is slightly concave: Situated at one end is the Slough of Ambiguity; at the other end is the Swamp of La Palice[15]. At the head of the river delta, one sees the Stronghold of Tautology: That's the stronghold of the logicians. One climbs a rampart towards a small temple, a kind of Parthenon: that's Mathematics.

To the right, one finds the Exact Sciences: Up in the mountains that surround the bay is Astronomy, with an observatory topping its temple; at the far right stand the giant machines of Physics, the accelerator rings at CERN*; the animals in their cages indicate the laboratories of Biology. Out of all this, there emerges a creek that feeds into the Torrent of Experimental Science, which flows into the Sea of Insignificance.

To the left is a wide path climbing towards the north-west, up to the City of Human Arts and Sciences. Continuing along it one comes to the foothills of Myth. We've entered the kingdom of anthropology. Up at the top is the High Plateau of the Absurd. The spine signifies the loss of the ability to discern contraries, something like an excess of universal understanding which makes life impossible.

It's something I've done to amuse myself, but it reflects something real, I think: The *Logos*, the possibility of representation by language, only comes into play for humanity in a rather limited number of situations, between what I call Cosmos and Chaos. Cosmos in its most absolute form is the cemetery. Here one finds utter tranquility, the Calm of Insignificance, the Nothingness of Insignificance.

At the top, in contrast, one finds the unleashing of the Chaos, of cosmic forces. They are always present and capable of threatening us In the face of danger, the opposition True/False disappears; as do, likewise, at the level of the Insignificant, the truths of the axioms of mathematics. They turn into conventions. One can well

*** CERN is the European organization for nuclear research (Centre Européene pour la Recherche Nucléaire).**

change any axiom from true to false. The opposition of truth to falsehood is thus transformed into the manipulation of the context. As this is variable, the True/False dichotomy fades into insignificance.

At the top, this opposition disappears rather abruptly on this mountain chain, because it is up there that humanity is subject to the unleashing of the forces of nature that threaten it: One's forced into making an immediate response. If someone cries "Fire!" in a crowded auditorium, one doesn't stop to investigate if the assertion is true or false … so there, also, the true/false distinction disappears.

This distinction is only really of importance in the narrow strip of the basin of the River of Discernment. Above is the Chaos of natural forces; below is the peace of the Void*. Between the two, there is a sort of crescent that can be upended so that one can picture it as a canoe floating on the turbulence of the forces of nature. Above is the eternal calm of the skies … By inversion of the y-axis, there is the serenity of the Void.

This gives a fairly precise idea of the role of language as the support of what Heidegger has called *Sorge* (concern, or care). He claimed that existence is bound up with the emotion of anxiety, to the necessity of reacting to dangers that threaten us. This interpretation of mine of the thought of a metaphysician is perhaps too concrete, but it's a

*** See the diagram on page 180. This diagram is taken from the article given in Note 12, p. 173. It was not included in the French editions of this book.**

genuine phenomenon. The *logos* only exists where there is danger; yet, this can be conceptualized and therefore handled in terms of acquired knowledge, so that at the same time it is neutralized.

Next, moving to a higher level of abstraction, one begins to manufacture linguistic entities which do not correspond to real things, which therefore have nothing threatening about them, thereby producing the play of language, of logic and tautology, a certain kind of philosophy, a certain kind of epistemology. That's where the River of Discernment runs into the Fortress of Tautology, into the sewers. It's become invisible, but, at the surface it can smell pretty bad…

This series of interviews was opened with the question: What motivated you to become a mathematician? My final question is: What motivates you today? What do you hope to accomplish? What role would you like to play, in particular for arriving at a better understanding of the world?

My tendency is to reply that I've never felt the desire to "play a role". That's never been an ambition of mine, I believe. And, if it does happen that I've often taken up controversial positions (even provocative at times), it's been less from any desire to take action than because I've always had a strong reaction against intellectual dishonesty. It seems to me that one finds examples everywhere. It's therefore been difficult for me to restrain myself.

Apart from that I have no ambitions. I'm not certain I've ever had any… I've never given a thought to shape my life in this way or that. I've allowed myself to be driven by events. I've never lacked subjects to interest me! But I relate to them as to a kind of nourishing environment. It's not the result of any deliberate choice.

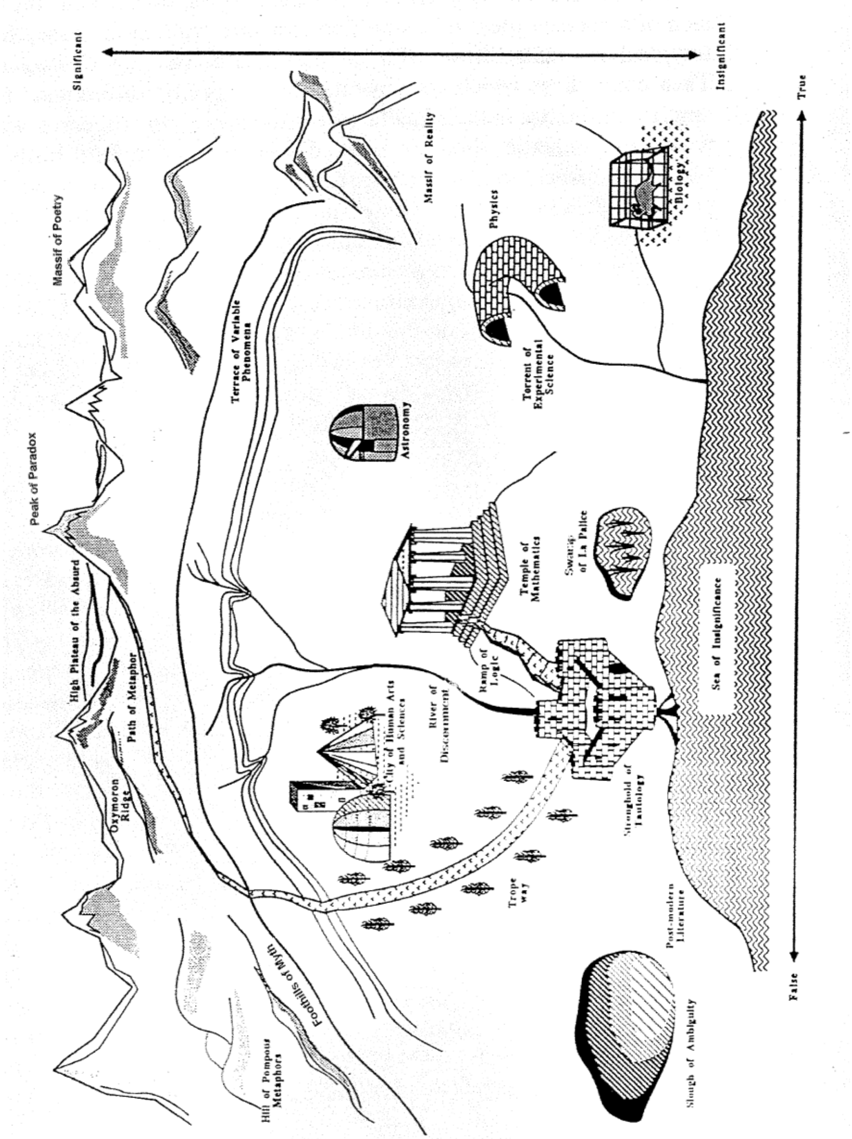
There are many questions that preoccupy me. Among them there are some mathematical questions I'd like to see resolved in the years left to me. It may be nothing more, perhaps, than a matter of curiosity.

About the general situation of the human race on this planet, I think, as many do, that we have to attain a plateau of Zero Population Growth as soon as possible, much as one finds in primitive societies which survive in unforgiving environments … As Levi-Strauss[16] puts it, we have to "refrigerate" our humanity, transform society to a chilled state. It may be less stimulating than a "hot" society, but, well… I've made these comments to many people: The economists are all convinced that the economy has to keep growing. For them, it's a bad thing if the economy is stationary, or regresses. I've the impression that our societies are trying to get around this problem by creating fictive needs for fictive goods. This comes at a high price for humanity. A kind of production is thereby developed, but around goods that tend to have a psychological or emotive nature.

And what about meditation?

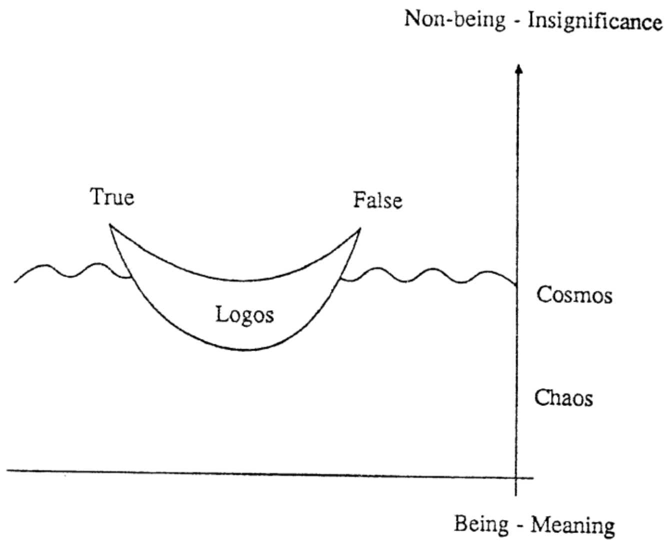
Meditation? Sure; it's a good thing. But meditation alone!… It seems to me that one has to achieve a state of unique individuality, to a degree of asceticism which I personally have never been able to attain. All in all, I'm content with presenting the small number of ideas I'm able to have, and I've been gratified by the reactions they evoke. I suffer much more from silence, from the absence of any reaction, than I do even from disagreeable or critical ones.

However, it's only normal. Ultimately, important ideas find only few echoes in the world. To quote Nietzsche: "New ideas always come in the talons of a pigeon…"



Map of Discernment.

Adapted from the Diagram on page 403 of [Thom, 1989/**1992**].



The domain of the Logos

Adapted from Fig. 3 on page 406 of [Thom, 1989/**1992**].

CHAPTER 3 NOTES

1. Alain Connes (1947-) is a French mathematician and a leading specialist in operator algebras. He won the Fields Medal in 1982 for his work in operator algebras, in particular von Neumann algebras. He obtained partial results on the classification of these algebras as intrinsic algebraic and topological objects. He is also a leading expert in noncommutative geometry and is currently applying his work in many areas of mathematics and theoretical physics including number theory (the Riemann Hypothesis), differential geometry and particle physics.
(Wikipedia)

2. Jean Dieudonné (1906-1992) was a leading French mathematician, a main figure in the Bourbaki group and an early member of the IHES. He is known for his research in abstract algebra, functional analysis and for editing Grothendieck's monumental work *Eléments de Géométrie Algébrique.* He is also known as a historian of mathematics and wrote a very interesting book, *A panorama of pure mathematics, as seen by Bourbaki*. In Chapter A (II) on Differential Manifolds and Differential Geometry, he wrote, " about 10 years ago [early 1970s – ed.], Thom developed some extremely interesting ideas on the possibility of applying the theory of singularities of differentiable mappings to the qualitative study of physio-chemical and biological phenomena and even to linguistics…" (p. 32).

3. Jacques Benveniste (1935-2004) was a French immunologist who contributed to the description of the structure of the *platelet activation factor* and its relationships with histamine. He was the center of a major inter-national controversy in 1988 when he published a paper in *Nature* reporting results that seemed to indicate that the configuration of molecules in water may be biologically active. A journalist coined the term "water memory" for this hypothesis and this is to what Thom is referring.

The reader should check Wikipedia for more information on the controversy and a paper by Benveniste "From 'Water Memory' effects to 'Digital Biology'…" answering his critics. It is found at http://www.digibio.com/cgi-bin/pl?nd=n3.

4. See Thom's *Semiophysics*, especially Chapter 1. Thom wrote extensively on Salience and Pregnance. Some of his papers in English listed in the bibliography are the following: "Animal psychism vs. human psychism", 1981/1983; "Saliency and Pregnances" or "Contribu-tion "1983a/1992; "Transitivity Continua and Prototypicality", 1983b; "Cyclical Structures in Semiotics", 1983c/1989; "Organs and Tools: A Common Theory of Morphogenesis", 1984/1986.

5. For a more complete discussion of the "diffusion of a pregnance", see [Mottron, 1987].

6. Paul Valéry (1871-1945) was a French polymath – poet, essayist and a philosopher fascinated by science. As a poet, today, he is considered one of the last of the French symbolists. In addition to his fiction (poetry, drama and dialogues), he wrote many essays and aphorisms on art, science, history, letters, music and current events.

Thom uses some of Valéry's aphorisms in his book *Structural Stability and Morphogenesis* (*SSM*). The following were translated by Roy Lisker:

- **Pour nous autres Grecs, toutes choses sont formes …**

"For us latter-day Greeks, everything is an ideal form …" (I think he's referring to the Platonic theory of ideas or ideal forms. - RL) (In Valéry's *Eupalinos*, page 12 in *SSM*.)

On page 20 in *SSM*, Thom writes: "If Paul Valéry said 'Il n'y a pas de géométrie sans langage' (There can be no geometry without language.), it is no less true […] that there is no intelligible language without a geometry, an underlying dynamic whose structurally stable states are formalized by the language. As soon as a formal model is intelligible, it admits a *semantic realization*, that is, the mind can attach a meaning to each of the symbols of the system."

- **J'ai vu bondir dan l'air amer les figures les plus profondes.**

"I've seen the deepest images leaping in the bitter air."

(In Valéry's *Le vin perdu*, page 92 in *SSM*.)

- **La vie n'a pas les temps d'attendre la riguer.** "In life, there isn't time to wait for rigor."

(In Valéry's *L'idée fixe*, page 280 in *SSM*.)

In his *Semiophysics* (p. 139), Thom writes, "There are already so many known facts in biology awaiting, if not explanation, at least 'representations' as Valéry would have said, that I hardly need to add more facts to this common horde. Biology is not physics; there is not 'in biology' any generativity other than the (biological) reproduction of vital forms. All experimentation rests, of necessity, on 'artifacts'. A knowledge of pathology, […], does not necessarily imply an understanding of the normal process." Valéry, in his *Cahiers* (I.829), wrote, "Explanations have always been sought, when all that one could try to invent was representations."

On pages 165-166 in his *Semiophysics,* Thom writes, "It is my hope to contribute some elements involving aspects not easily appreciated by specialists for whom the problem of the relationship between mathematics and reality has never been posed except as a 'philosophical' one (in other words, as Paul Valéry put it so famously, a problem one might refrain from considering), rather than as the essential problem that it really is. What is involved here is the aporia constituted by the relations between continuous and discrete."

Thom also contributed a paper to a colloquium on Valéry. "La modélisation des processus mentaux; le 'Système' valéryen vu par un théorecian des catastrophes", 1982/1983. In the paper, Thom discusses, amongst other things, Valéry's diversion of scientific notions (Catastrophe Theory) into "unscientific" ones.

7. The article "Halte au hasard, silence au bruit!"was first published in *Le Débat*, **3**, pp. 119-132. What followed was a debate in other issues of *Le Débat* with a concluding piece by Thom, "Sur le déterminisme: En guise de conclusion", in *Le Débat* **15**, pp. 115-123. The journal *SubStance*, #40, 1983, published the articles in English beginning with "Stop chance! Silence noise!" and concluding with Thom's "By way of conclusion".

Thom was thought, by some, to be inflammatory and anti-collegial; the paper he wrote was a type of put-up or shut-up piece. Thom's argument is that the universe, for him, remains eminently knowable. He considered the *nouvelle science* of the period anti-scientific because it is anti-deterministic. On page 19 of his paper in *SubStance,* we find: "Chance [...] is an entirely empty, negative concept, therefore without scientific interest.

Determinism, on the contrary, is a source of fascinating richness – for the one who knows how to scrutinize it."

The replies to Thom's arguments by philosophers of science and scientists Edgar Morin, Ilya Prigogine, Henri Atlan, Michel Serres, Jean Largeult and Antoine Dachin were vigorous and argumentative. All the papers were translated into English in *SubStance*. In the concluding piece, Thom responds to his critics and remarks on page 83 that "Determinism, when it is scientific, that is to say, accessible to all, and theoretically intelligible to all, is then an instrument of liberation."

The debate continued and other material is presented in the book *La querelle du déterminisme. Le Débat,* edited by K. Pomian. For this book, Thom wrote another concluding piece "Postface au débat sur le déterminisme". This has not been translated into English.

8. The article "Ambiguïtés de la complexité en Biologie" was written in 1985. It is found in *Archives Internationales de Physiologie et de Biochimie*, **94**(4), Nov. 1986, pp. 101-110. It is also in Thom's *AL*, pp. 219-231 with a short introductory note and in [Thom, OC, 1985, 7].

9. Andreas Vesalius (1514-1564) was a Flemish anatomist thought of as the founder of modern anatomy. His major work, *De Humani Corporis Fabrica,* written in 1543, contained accurate descriptions of human anatomy, but owed much of its great historical impact to the woodcuts of his dissections. (OED)

10. William Harvey (1568-1657) was an English physician who discovered the circulation of the blood. In his book, *De Motu Cordis*, written in 1628, Harvey described the motion of the heart

and concluded that the blood left through the arteries and returned to the heart by the veins after it had passed through the flesh. (OED)

11. Antoine-Laurent Lavoisier (1743-1794) was a French scientist thought of as the father of modern chemistry. He caused a revolution in chemistry by his description of combustion as the combination of substances with air, more specifically with oxygen. (OED)

12. Jacques Laçan (1901-1981) was a French psychoanalyst, psychiatrist, and doctor. Laçan's 'return to the meaning of Freud' (with a renewed concentration upon the Freudian concepts of the unconscious, the castration complex, the ego conceptualized as a mosaic of identifications) profoundly changed the institutional face of the psychoanalytic movement internationally. The Seminars of Jacques Laçan, which started in 1953 and lasted until his death in 1981, were one of the formative environments of the currency of philosophical ideas that dominated French letters in the 1960s and '70s, and which has come to be known in the Anglophone world as post-structuralism, though it would be a mischaracterization to label Laçan as only a post-structuralist. The centrality of language to any psychoanalytic work was paramount. His work has a strong inter-disciplinary focus, drawing particularly on linguistics, philosophy, and mathematics, and he has become an important figure in many fields beyond psychoanalysis, particularly within critical theory, and can be regarded as an important figure of Twentieth-Century French Philosophy. (Wikipedia)

13. The article is “Entre Chaos et Cosmos, le Logos, ou Le vrai, le faux et l’insignifiant”. It was translated into English as “The True, the False and the Insignificant or Landscaping the Logos” by Vendla Meyer. The statement is on p. 399 of the English version. The English version is found in the *Poznan studies in the philosophy of the sciences and the humanity (26): Idealization IV: Intelligibility in Science,* Dilworth C., ed., Amsterdam, Rodopi, 1992, pp. 399-406. The French version is found in [Thom, *OC*, 1989, 8.1].

14. Translator’s note: In the French version of this book, it is “Une carte du sens”, a play of words on the “Carte du Tendre”: An allegorical “map” of the romantic emotions, drawn and described in the writings of Madame du Scudery (1654). It is instructive to compare René Thom’s diagram with the original Carte du Tendre that can be seen at:
http://lettres.ac-rouen.fr/francais/tendre/tendre.html

“Sens” in French has many meanings. Cassell’s French-English dictionary has sense, feelings, judgment, wit, intelligence, meaning and many more. In some of Thom’s works, “sens” has been translated as “meaning”. The problem with translating “sens” as “meaning” is that in this context, Thom is using the word in such a way as to implicate the faculty that determines if something has meaning, somewhat in the way we use the expression “common sense”. By using “discernment”, I am emphasizing the faculty of ascribing meaning.

15. Translator’s note: La Palice is a pun on the feminine noun “lapalissade”, meaning ‘a statement of the obvious’.

16. Claude Lévi-Strauss (1908-2009) was a French anthropologist who developed structuralism as a method of understanding human society and culture. Outside of anthropology, his works had a large influence on contemporary thought, in particular on the practice of structuralism. (Wikipedia)

F. Dosse, in his *History of Structuralism, Vol. 2*, discusses more on Lévi-Strauss' structuralism, Thom's work and the work of Jean Petitot, a student of Thom, who showed that all the major structuralists were realists who saw structure as an integral part of reality, and who claimed an identity between knower and knowable. In particular, see pages 372-373 and 398-399.

CHAPTER 4

TOWARDS A THEORY OF MORPHOGENESIS[1]

(1994)

In an article about theories of morphogenesis you wrote: "It is not possible to give a simple answer to the question 'What is a form?'". Coming from someone like yourself, who has done some serious work on the study of forms, this is somewhat disturbing. How, then, should we introduce our conversation?

Well, first of all, by considering the power that forms exercise over us, or their importance in the domain of technology. Conversely, there is the power we have over forms, our capacity for storing and deforming them …

What does one mean by the "power of a form"?

I think that everyone has some intuitive notions about power. I will not risk trying to give it a definition; let's just say that it is potential for influencing the behavior of someone else.

So, forms affect our behavior?

Unquestionably. In certain instances, forms can have a considerable influence on their environment. It is not so much the form in itself that has power, rather it is its immersion in an energy flow which is, in some sense, directed, oriented or channeled, depending on the circumstances. Owing to this, it is possible, through interaction with the form, to attain a concentration of that energy upon a well-defined point with useful properties for humanity.

The simplest example is, let's say, the magnifying glass which, by focusing sunlight, can set a piece of paper on fire. There are many other examples, too, such as the helix. A helix turning in the wind supplies an energy flow. The form of the helix transforms the velocity of the wind into an angular velocity around an axis which, for certain people, can be infinitely more interesting than the form of the wind.

There are also the subjective and aesthetic influences that forms can have on us.

Yes. That's understood; one has all the problems associated with the attractive and repulsive aspects of forms. Here, however, we are treading upon a domain which comes out of the analysis of the effects on the subject of forms as things in themselves. The receptor can suffer what Aristotle has called an insufficiency. For example, a starving predator has an insufficiency. He seeks to capture and devour an external prey. This is the simplest and, I believe, the most fundamental biological example of what I call a “pregnance”. It’s precisely because beings under stress are subject to a pregnance that an external form can assume power over a being in a situation of need.

Doesn’t the power which we claim to have over forms come from the possibility of deforming them? Of course, one must also take into consideration the capacity to know, understand and analyze them …

Of course. Yet prior to that, there is a problem, which one might call sociological, of codification. As I’ve said, forms are given to us empirically, experimentally, from observations on the external world. So, in order that a form becomes an object of science, one must, in some sense, delineate it by means of a formalism which is external to it. It’s only by this transcription in a code which is external to the object itself that one can hope to create a theory of forms.

Beginning first, I suppose, by classifying and cataloguing them.

Cataloguing them, that’s correct, by cataloguing them. The initial task is therefore one of mimicry. One must be able to transcribe the form to an image in order to make several identical or similar copies of these images in accordance with well-established procedures. Photography is the paradigm for this.

But is not the copy always an approximation?

Yes. Generally, the copy is something more in the nature of a projection. A photograph is a projection of the 3-dimensional figure of an object onto a sensitive film and, ultimately onto paper. This reduces its dimension.

This is a method of describing form through simplification. The simple form represents, approximately, or geometrically, a more complex one.

The ideal situation would be one in which there existed a notational procedure for forms, such that one would only have to use one symbol to represent an extremely complicated 3-dimensional object. To me this is an illusion. In fact, one cannot represent a highly complex object by a simple formal structure.

As was pointed out by Paul Valéry, this is the way Socrates defined entities. What is the difference between a geometric entity and some arbitrary entity? When comparing the shape of a mollusk shell to that of the smoke from a fire, the first is called a geometrical object while the second isn't. The first is readily coded in a simple manner by an appropriate formalism, while the latter is beyond our means of description.

However, when analyzing a form, it's possible to notate it, to copy it. But isn't it also possible to construct forms by using a given number of symbols?

Indeed, however the process of notating is normally done through pixels. The surface on which the form is displayed is cut into tiny squares called pixels. Afterwards, one notes all the little squares which intersect the form. In the long run, it's possible to code color, shading, brilliance, and so forth. Using this method of notation, it's possible to reconstitute a form which is not the original form, yet, which is so close an approximation that one's subjective impression is essentially identical.

I presume that mathematics provides a great many ways of analyzing form, notably by topology. There's been enough progress made in this area to allow for refined calculations and the use of different modes of representation. At present, however, questions are being raised about the morphogenetic approach. It's far from being the dominant viewpoint of modern science. Right now scientists tend to consider qualitative analysis as merely 'bad quantitative'. They persist in being analytic and reductionist. Science is geared towards prediction rather than interpretation. What future do you envisage for the

morphogenetic approach in the framework of modern science?

Reductionism has a major failing: It destroys the form itself. The aim of a morphogenetic analysis, in theory, is to describe a form in such a manner that it is possible to reconstruct it. But when you put a substance into a test-tube for the purpose of studying it, you must introduce reactants in order to see what's going on, thereby completely destroying the internal structure of the object.

This objection to reductionism is constantly cited. If, therefore, one wishes to save the form, one ought not to proceed by means of a chemical analysis. One's procedures must show far more respect for the internal structures. Certainly, there are biochemists who will tell you that they're able to completely grasp all the secondary and tertiary configurations of proteins, and to visualize their interaction.

But this can only be done within a small locale. If someone's intention is to portray the respective configurations of 10^{30} molecules, one can imagine that he will run into serious problems, if only those involved with coding them. Thus, the reductionist approach is limited right from the beginning because of the enormous number of objects that have to be considered, precisely because they are so very small. One ought to be using a method that sets up collective equivalence classes, which are much cruder than the molecular analysis in vogue today.

It preserves the form, agreed, but does it also make it possible to read inside it, to know what the form contains?

Speaking historically, I ought to point out first that, up until the modern period, that is to say, this century [20th – ed.], biologists classified animals in terms of their organs. Classification by internal organs is morphologically crude. Nonetheless, it's still very valuable because it is the only way to enable one to define homology between organisms. All of our zoological classifications, taxonomies, are based on the notion of homology.

One has to be able to say when two organs, O_1 in space E_1 and O_2 in space E_2, are homologous. And, it isn't possible to classify, to make sense of a notion such as biological organization, without this concept of the equivalence of two organs through this kind of correspondence. A more refined analysis extends these correspondences, both morphologically and topologically.

This was very well stated by D'Arcy Thompson in his book *On Growth and Form*[2]. He'd done a translation of Aristotle's book *History of Animals*, and he'd discovered that this was what Aristotle had in mind when speaking of *equivalence by excess* and *equivalence by default* between animal organs and animal species. To my way of thinking, this was a conceptual discovery of the first magnitude. Previously, people did talk about homology. For centuries they debated the idea of homology, with some people claiming that such an organism was homologous to some other organism. But these were matters of personal choice, and never led to a precise theory. These issues could only be properly addressed through considerations of a topological nature.

There are similitudes of forms and similitudes of functions. Do these notions ever come together?

The English biologist Owen[3], you know, said that two organs were homologous when they stood in the same relationship to other organs, even if they exhibited variations in both form and function. Owen allowed for the possibility of variations in the internal forms of organs, and in their functions, but he clung to the possibility of a global framework which he could define in an intrinsic way. But if you want to be accurate, you must effectively proceed in another fashion; starting from what Aristotle called the homoeomerous part, which is to say phenomenologically homogeneous contexts. A homoeomerous part is a context such that, if I've got two points A and B of the homoeomerous part, then, there is a small spherical neighborhood V_A of A and a small spherical neighborhood V_B of B such that the interiors of these spheres can be mapped onto each other in a compatible fashion, together with their phenomenal properties.

Does that mean, for example, that for Aristotle the entire musculature is homoeomerous?

Indeed, yes. Marrow is homoeomerous. Blood is homoeomerous. The anhomoeomerous parts, to the contrary, are characterized by the fact that one finds partitions in them which separate phenomenologically different milieus.

Those are called anhomoeomerous?

Yes, they're the parts of the body which ordinary language describes: the head, the neck, the appendages, and so on.

Composed of several differing tissues …

That's it. There is, furthermore, a problem which has arisen in modern biology which Aristotle anticipated – he didn't solve it but he envisaged it: In any anhomoeomerous part, there will be several homoeomerous parts. In other words, in order that a milieu be able to act within the organism, it can't be homogeneous. There have to be, as one would put it today, several distinct compartments separated by membranes. And, it is the role of these membranes which has become an enormous problem for modern biology: Why should there be membranes?

More generally, one can restate the question as a need to know if one should speak of one or of several theories of morphogenesis. Is the morphogenetic approach itself

homogeneous? Is it applicable to the elaboration of theories?

I said previously that the morphogenetic approach, as I see it, belongs to a branch of pure mathematics, that is to say, the theory of algebraic or semi-algebraic varieties. To rigorously encode a spatial form, it has to be definable by an equation, or an inequality; let's say a system of inequalities, comprised of a finite number of symbols, numbers, what have you. These numbers have to be definable by some finite process; therefore, they are rational numbers; and this leads immediately to a very strict, perhaps abnormally limited, conception of descriptive methodology.

A finitist program for description is indeed very proscriptive; yet, it suffices to describe a huge number of forms, and these are forms with "good" properties. One knows how to classify them. Their phenomenology has a name. One calls it "equisingularity". Points are classified on the basis of their equisingularity. If they are equisingular, this means that they constitute what I've called a stratum, and it is this which corresponds, biologically, to the notion of homoeomerous. Thus, one has here a correspondence which is entirely new, I believe, between mathematics and biology that can be exploited.

This confirms the opinion that you're proposing a qualitative description of the world. It's in opposition to

the scientific approach; let's say the classical approach, which adheres uniquely to what is quantifiable and predictable. Would you say that, in this qualitative/quantitative dialectic, morphogenetic theories are complementary or antagonistic to the reductionist approach?

I would say that they are frankly antagonistic, because the quantitative approach, by itself alone, has the great failing of situating itself in the perspective that unlimited precision is possible. The qualitative approach, as I understand it, does not require unlimited precision. The classes of basic biological tissues, for example, make up a finite catalogue. Obviously, if one looks at things close up, one is led to further decompose these classes into (to fix our ideas) cells.

Ask a biologist the following question: How many distinct kinds of cell are there in a living creature; let's say a human being? You'll get some surprising answers: Some of them will say there are a finite number, others an infinite number. There is no philosophy of typology; yet, it's easy to understand that any system of classification must take into account qualitative considerations of form which go beyond quantity.

Here's a simple example: Draw q points on a blackboard, where q is an integer between 10 and 20. Connect these points by edges – you can even orient the edges. In doing this, you've created an oriented graph which might represent a dynamical system for example, or certain qualitative aspects of a dynamical system.

Okay; it's not the number of points which one considers in the classification of an oriented graph, it's the structure of the graph itself, that is to say, its form as a topological object – or, if you like, an algebraic object – but with a topological description. What one has here, is a kind of qualitative irreducibility relative to quantity.

A certain kind of autonomy?

A certain kind of autonomy, yes. In the collection of point pairs in your graph, you are making a distinction between those which are connected and those which aren't. This makes for a qualitative distinction.

In your opinion, does this give a better description of the world than that provided by modern science?

I don't think there's any opposition in principle, because I believe that any sincere person, any sincere scientist would admit that a description of a system by means of an oriented graph is a scientific description – crude, but scientific.

But the irreducibility of a qualitative description is something that's very important. I'll give you another example in an area in which it constantly arises, that is to say, Darwinism. Darwinian natural selection is based, essentially, on the hypothesis of gradualism, that is to say, that in a single generation, by virtue of continuity, the child doesn't differ significantly from its parents. The variations in the child can't be very great. Quantitatively they've always small.

The problem is, however, that a small quantitative alteration can be an enormous qualitative one. Consequently, the effect of a mutation can be very great, even if from the quantitative aspect it is small. I think that this opposition between the quantitative and the qualitative illustrates well that there is a specificity associated with quality, associated in an exact way to the functional capabilities of the transformed object, and that these functional capabilities of the transformed object are not of a quantitative, but of a qualitative nature.

Only a small number of genes distinguish a human being from a chimpanzee.

Exactly.

Are you an optimist about the ability of the approach you are proposing to win support over the coming years?

Not right away, certainly not. But there are, I think, a number of thinkers who are beginning to understand it.

And this will be beneficial to scientific research in general?

I like to think so.

Addendum by René Thom

At the beginning of my book *Structural Stability and Morphogenesis*, I sketched out a general program for constructing a dynamics of forms, which would explicate the succession of forms as a function of the forms themselves in so far as they are geometric objects.

This approach has only been sketched in my works. It's clear at first sight that a succession of spatial forms cannot be predicted without a mathematical theory that can be applied to it. Now, the only effective mathematical method for making predictions is that of analytic extension, the great invention of the mathematicians of the 19th century (Cauchy, Weierstrass, Riemann). My only contribution has been to consider a particular aspect of analytic extension, based on understanding the *flat deformations* at one of the points of a collection of singularities. This operation allows one to limit the kinds of deformations that a form can undergo when an unstable singularity is stabilized.

In this sense, it was (and still is) of indisputable interest. In other words, the aim of CT has been to specify exactly what effect an accidental singularity can have on a pre-existing analytic object. This algorithm allows one to give a precise meaning to the general notion of 'metamorphosis': When a form F_1 metamorphoses into a form F_2, although F_2

will differ from F_1, they still maintain the same formal structure. This is the aporia being debated in the Platonic dialogue, *Parmenides.*

I believe that, with appropriate restrictions on the substrate, this program can be developed, and that the insight it can shed on the intelligibility of biological processes (embryology, in particular) can be considerable. The most extensive treatment of it in my work is to be found in my book *Semiophysics*. Obviously, to be open to these methods one should not have the "one model fits all cases" reaction of a modern biologist: "Everything is in the genes", since in my models there are no genes. Modern biology suffers from many blind spots.

CHAPTER 4 NOTES

1. This chapter was not in the French editions. I included it because it was a 1994 interview with Emile Noël and I felt there was some continuity with the topics discussed in the first three chapters. "Pour une théorie de la morphogènese" (Towards a Theory of Morphogenesis) is found in Noël's book, *Les sciences de la forme aujourd'hui*. On page 9 of his Introduction, Noël writes, "The interviews assembled in this volume correspond to the series of radio broadcasts with the same title conducted by *France-Culture*. In the course of making the transcription from oral to written form, they have been revised and corrected, with an eye to conserving the conversational spontaneity."

See also the Introduction of this book, especially pages vii-x.

2. D. W. Thompson (1860-1948) was a Scottish biologist, mathematician, classics scholar and author of the 1917 monumental work *On Growth and Form*. This strikingly original work has been very influential and has enchanted and stimulated generations of biologists, artists, architects and mathematicians and others working on the boundaries of these disciplines.

The central thesis of *On Growth and Form* is that biologists of his day overemphasized the role of evolution, and under-emphasized the roles of physical laws and mechanics, as determinants of the form and structure of living organisms.

Thom has said that the biologists of this day overemphasize the role of genes, and underemphasize the roles of everything else.

On Thompson's work, Thom writes: "That we can construct an abstract, purely geometrical theory of morphogenesis, *independent of the substrate of forms and the nature of the forces that create them,* might seem difficult to believe, especially to the seasoned experimentalist used to working with living matter and always struggling with an elusive reality. This idea is not new and can be found almost explicitly in the classical book of D'Arcy Thompson *On Growth and Form,* but the theories of this innovator were too far in advance of their time to be recognized." [*SSM,* p. 8]

3. Richard Owen (1804-1892) was an English biologist, comparative anatomist and paleontologist. He synthesized French anatomical thought, especially from Georges Cuvier and Geoffrey Saint-Hillaire, with German transcendental anatomy. Owen gave us many of the terms still used today in anatomy and evolutionary biology, including "homology" which for him meant the same organ in different animals under every variety of form and function. Taking homology to its conclusion, Owen reasoned that there must exist a common structural plan for all vertebrates, as well as for each class of vertebrates. He is probably best remembered today for coining the word *Dinosauria* and for his outspoken opposition to Darwin's theory of evolution by natural selection.

See also Chapter 5 of Thom's *Semiophysics*.

(Wikipedia and www.ucmp.berkely.edu/history/owen.html

GLOSSARY

The text and drawings of the glossary are the work of Alain Chenciner, at the time, Professor at the University of Paris VII.

(Editor's note: The exercises have been incorporated into the text and topics not mentioned in Chapters 1-4 have been omitted.)

Although the terms in this glossary are defined on **manifolds** of all dimensions, the illustrative examples for surfaces, and even of embeddings are all in $\mathbf{R}^3$, the symbol used by mathematicians for the 3-dimensional space of our daily experience [ordinary Euclidean space – ed.]. There are two exceptions: respectively, the description of the **topological sphere** of dimension 3 (see **fibered structure**) and that of the hypercube of dimension 4 (see **product)**.

Furthermore, the functions under consideration are always "height functions", that is to say, restrictions of a coordinate system in $\mathbf{R}^3$ to a surface embedded in $\mathbf{R}^3$. An example would be the function $(x_1, x_2, x_3) \rightarrow x_3$. The complexity of the function is reflected by the complexity of the embedding.

One is advised to look at the diagrams, and to read the commentary only as a last resort. Although we do not want to be seen as illustrating a notorious invention of the *Oulipo* literary movement, we allow ourselves the right to use, in each of these articles, without paraphrase, any words defined in the other articles. A more technical synthesis can be found in these articles: "Singularities of differentiable functions" and "Dynamical systems" in the *Encyclopedia Universalis.* (In French – ed.)

[The *Oulipo*, a peculiar semisecret literary society, was founded in 1960, inspired mainly by the mathematician François Le Lionnais and the writer and amateur mathematician Raymond Queneau. Their somewhat surprising premise was that, as "mathematicians and scribblers, we have the right to expect that our meetings will contribute to shedding light on the exercise of our respective activities." See Aubin's article, 1997, page 320 – ed.]

Manifold

A manifold is a generalization of the ideas of curve and surface: it is a space that locally resembles a space of n dimensions, $\mathbf{R}^n$ (see **product**). A manifold can be defined in terms of an **atlas** composed of **charts**, each of which describes a section resembling a piece of $\mathbf{R}^n$ and is defined by **local coordinates**. (See Figure 1.)

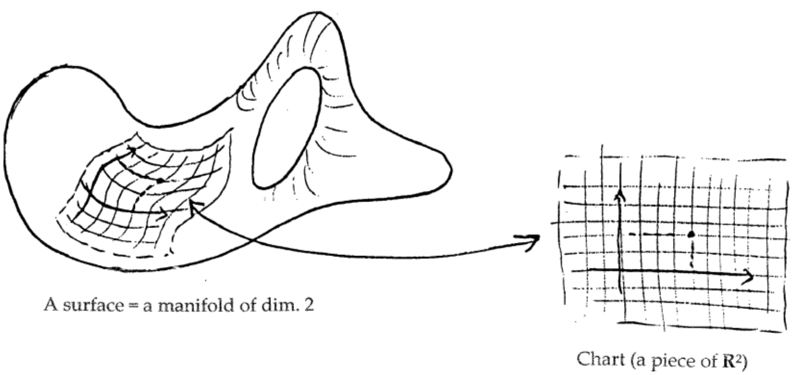


Figure 1

We will refer to manifolds as either **topological**, or **differentiable of class $\mathbf{C}^k$**, depending on the properties (simply continuous or differentiable k times) of the mappings between their charts. The latter case allows global extension of the differential calculus on $\mathbf{R}^n$ to the manifold.

A manifold may or may not have a **boundary**, that is to say, a subset which is, either topologically or differentiably a manifold of one less dimension. Boundaries themselves do not have boundaries: the fact that "*the boundary of the boundary is empty*", is the fundamental concept of Algebraic Topology. (See Figure 2.)

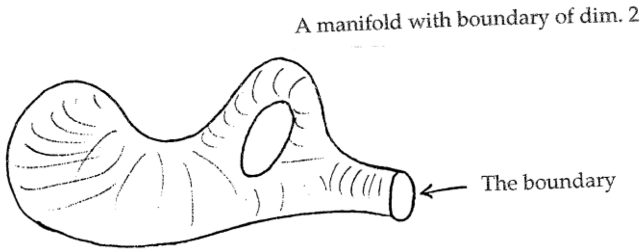


Figure 2

Topological (respectively differentiable) sphere of dimension n

One uses this to refer to any **manifold** which can be continuously (respectively differentiably) deformed into the set of points situated at unit distance from the origin of a Euclidean space $\mathbf{R}^{n+1}$, of dimension (n+1), or "standard n-sphere" S^n. The (n+1) coordinates $(x_1, x_2, \ldots, x_{n+1})$ of each point of S^n, satisfy the equation $\sum^{n+1} x_i^2 = 1$. S^n is also the boundary of the disk D^{n+1} of dimension (n+1), that is to say, the set of points at a distance ≤ 1 from the origin of $\mathbf{R}^{n+1}$. (See Figure 3.)

The boundary of a cube is a topological sphere of dimension 2, which the cube itself is a disk of dimension 3. A *stereographic projection* supplies a chart (see **manifold**) of the complement of the projection point, and allows one to represent S^n as the union of a point, (sometimes notated as the "point at infinity", ∞) and the space $\mathbf{R}^n$. (See Figure 4.)

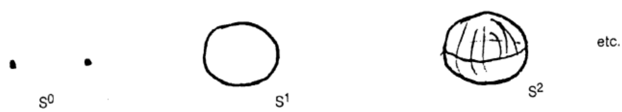


Figure 3

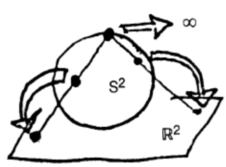


Figure 4

Figure 5 (on the next page) shows how S^2 can be obtained as a product. (See **product** and, for S^3, see **fibered structure**).

In the same way, S^3 can be obtained through gluing two "solid tori" at their boundaries (one glues the parallels of one to the meridians of the other): $S^3 = S^1 \times D^2 \cup D^2 \times S^1$.

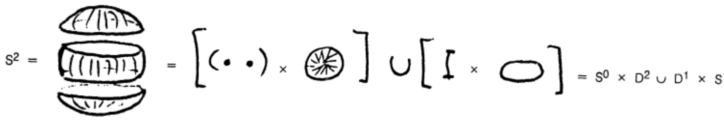


Figure 5

Product of Two manifolds (See Figure 6 on the next page.)

In the product of two **manifolds** A and B, each point is represented by 'generalized coordinates' (a,b), coupling an element a of A with an element b of B. The mappings $P_1 : (a,b) \rightarrow a$, and $P_2 :(a,b) \rightarrow b$ are called the first and second 'projections'.

Iterating products can be used to generate spaces of higher dimension, in particular $\mathbf{R}^n$, where each point is represented by n coordinates from the real line **R**. The same definition, of course, covers products of any pair of sets.

Fibered structures (Fibrations)

These are structures which locally resemble **products** but which may also possess a "global torsion". (See Figure 7 on page 219.)

Observe that, in the case of the Moebius strip, there is no continuous analogue of the second projection P_2. However, if one removes a fiber, such a projection exists and one gets to a new **product** (this can be verified with a pair of scissors).

Tori

A torus is the product of two circles (topological or differentiable 2-spheres). As a topological circle can be obtained through gluing together the end-points of a segment, one can represent the method of constructing a torus by means of a rectangle. (See Figure 8 on the next page.)

If one tries to do this in 3-space with a flat rectangle, one will have to allow for some folds and cusps due to the curvature.

The torus $S^1 \times S^1$ is the boundary of the "solid torus" $S^1 \times D^2$ or equivalently $D^2 \times S^1$.

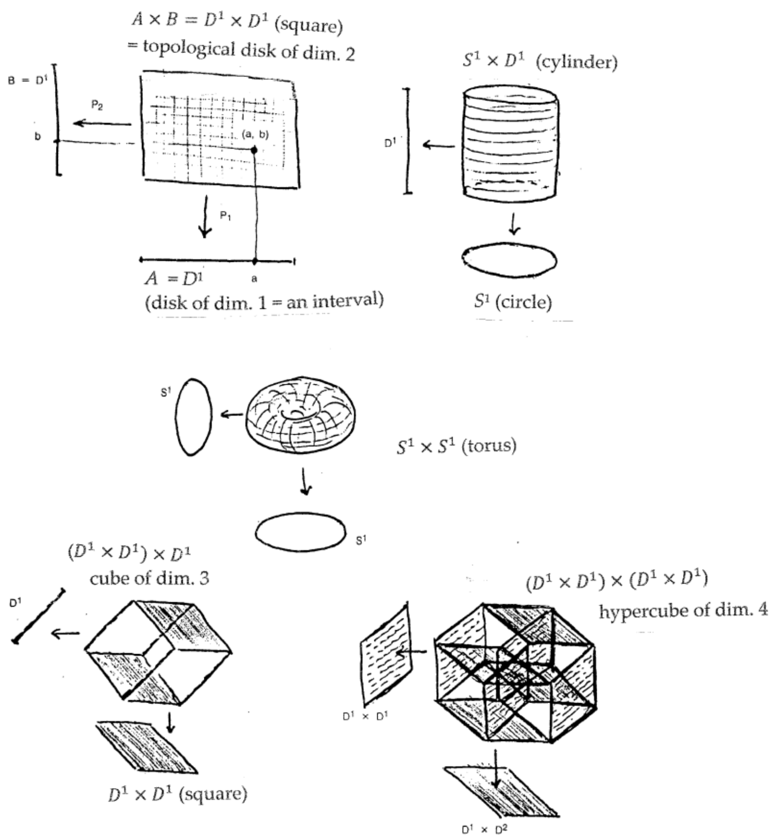


Figure 6

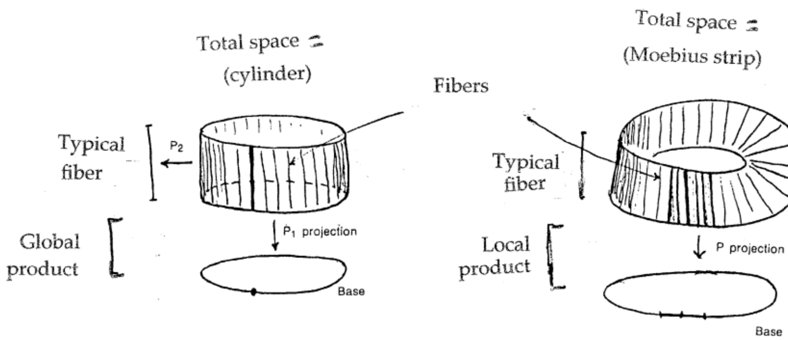


Figure 7

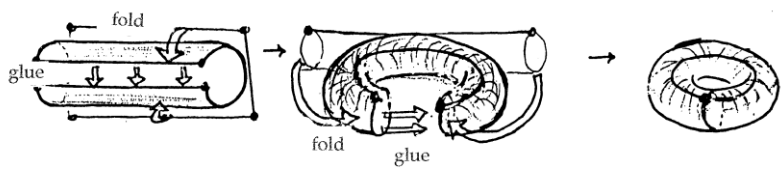


Figure 8

Figure 9 below shows some distinct embeddings of a torus in 3 dimensional space.

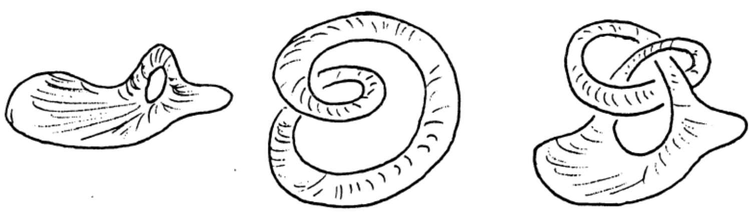


Figure 9

C^k functions on a C^k differentiable manifold

With each point x of the **manifold**, one can associate a real number f(x), the "value" of the function f at the point x, in such a manner that, in each of the charts of an atlas (see **manifold**), f will be represented by a C^k function over $\mathbf{R}^n$, that is to say a function that can be well approximated in a neighborhood of each point by a polynomial in n variables of degree k.

The set of points x with the same value c = f(x), is called a *level line*, (think of relief maps which show the level lines of the function "height above sea level".). As stated earlier, we will always be concerned with functions of this type. (See Figure 10.)

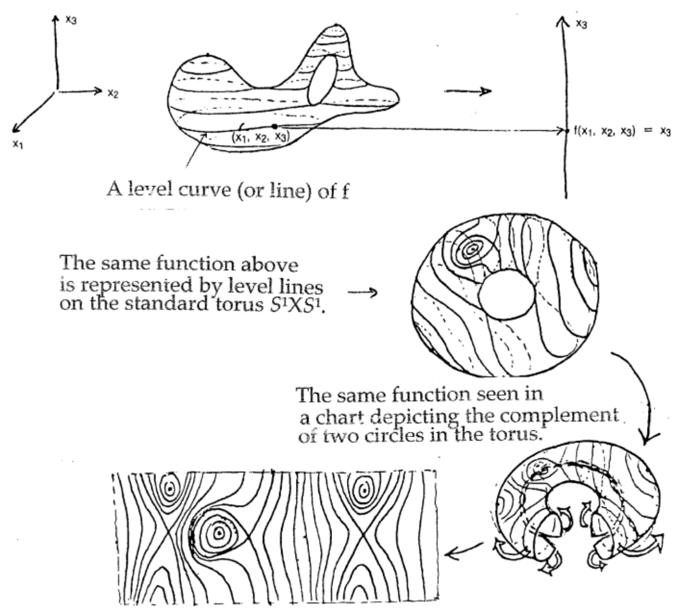


Figure 10

Critical points (or singularities) of a function of class $C^{k\geq 2}$ on a manifold: Morse functions

Let *Q* be the surface, or graph, of a function rotated, as René Thom was fond of doing, by 90°. What happens when one projects this surface onto a portion of the real line? Such a projection is called a mapping. (Mapping is a synonym for function.) The ***critical points*** are the points of resistance, the points which "cry out", those where the level lines undergo a qualitative change, in the neighborhood of which the function f can no longer be represented by a 1−1 fibration.

In Figure 11 (next page), these are the points where the plane tangent to the surface is parallel to the plane $x_1 O x_2$ or in other words, the points that are abominably degenerate. What one has is a *constant map*: one that sends an entire manifold onto a single point. However, if by a very slight "perturbation" of the function f one can deform it to another function for which all the critical points are "non-degenerate, one says that f is a **Morse function**. In the neighborhood of each of these non-degenerate critical points, f will behave like a second degree polynomial. This is why f must be at least of class C^2 or more which, in the case of a surface, is of the following three types: local minimum, saddle point and local maximum. (One uses as chart – see **manifolds** – in the neighborhood of the critical point, the projection π of the surface onto the plane $x_1 O x_2$; the surface then becomes, locally, the "graph" of the composed function $g = f o \pi^{-1}$, of f in this chart.) (See Figure 12 on the next page.)

To simply the notation, one can assume that the projection of the critical point on the plane $x_1 O x_2$ is at the origin; it can always be sent there by a translation. Observe that , at a 'regular point' (non-critical), a local model is simply the projection $g(x_1, x_2) = x_1$,

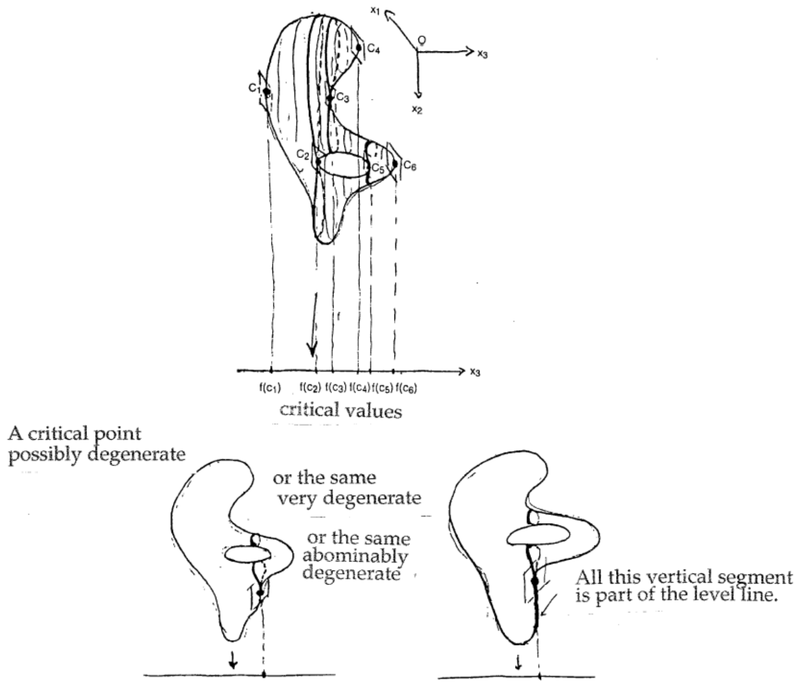


Figure 11

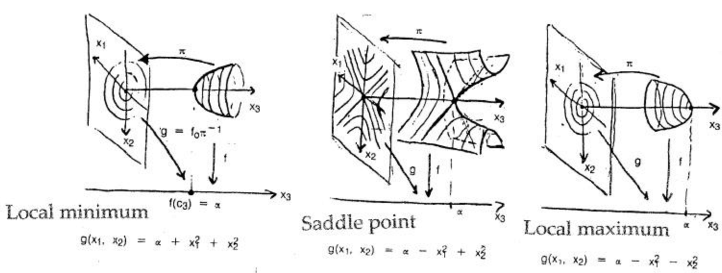


Figure 12

whereas in our first example of a degenerate critical point, the local model is of the form $g(x_1, x_2) = \alpha - x_1^2 - x_2^2$.

We can show that, in accordance with the sign of $\varepsilon \neq 0, g(x_1, x_2) = \alpha - x_1^2 - x_2^2 + \varepsilon x_2$ has either 0 or 2 critical points, all non-degenerate. (See Figure 13.)

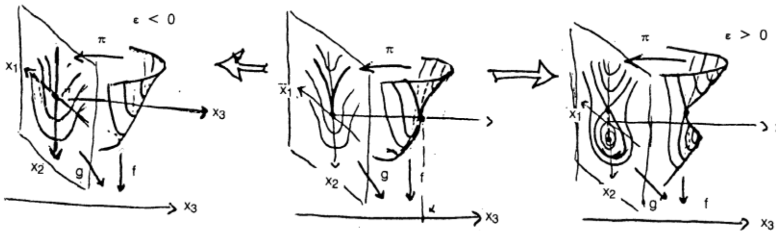


Figure 13

Gradient lines and index of non-degenerate critical points

In the case of a "height function", gradient lines are the lines of 'greatest slope', oriented by increasing height, along which the function increases at the greatest rate. The fact that they are perpendicular to the level lines shows how this notion is derived from the measurement of lengths on a surface, where it is the Euclidean distance of a curve on the surface, but treated as a curve in $\mathbf{R}^3$.

The union of the gradient lines which meet at a non-degenerate critical point is a **topological disk**, (see **topological sphere**) minus its boundary (see **Morse Theory in the sense of Thom**). The dimension of this disk is the **index** of the critical point. The disk itself is known as a **Thom cell**, or **descending sheet** of the critical point. (See Figure 14 on the next page.)

Replacing f by -f changes the index i to 2−i, and the descending sheets to ascending sheets.

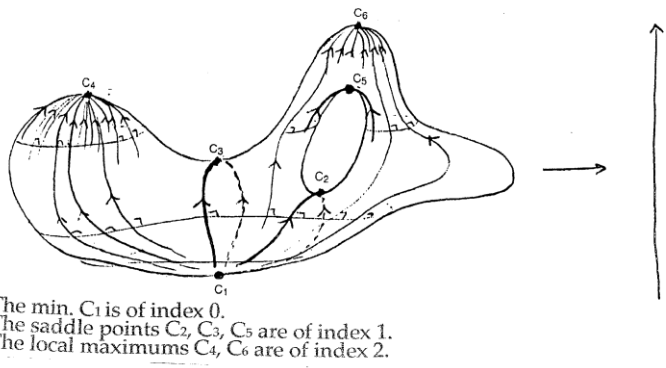


Figure 14

Morse Theory in the sense of Morse

Developed by Marston Morse starting in 1925, it owed its inspiration to the work of George David Birkhoff. Its goal is to extract as much information as possible about the topology (that is to say, the global form) of a **differentiable manifold**, from knowledge of the **Morse function** on that manifold.

Morse Theory in the sense of René Thom

This refers to a decomposition into “cells” (disks without boundary) associated to a Morse function on a manifold (see **gradient lines**). (Also, see Figure 15 on the next page.)

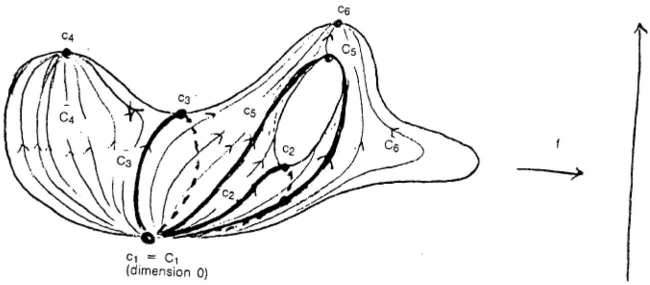


Figure 15

Morse Theory in the sense of Smale

This method consists of constructing a manifold through the addition of successive **handles**, glued to the boundary of what has already been constructed. This is the approach that led Steven Smale to the proof, during the 60's, of the Poincaré Conjecture in dimensions greater than 4: if a manifold without boundary, of dimension equal to or greater than 6, has a Morse function with at most 2 critical points, it is a **topological sphere**. (See Figure 16, next page.)

Cobordism

Thom has described it as a kind of art by which one smoothes angles in all directions. To be precise, one starts with a "cone" on a manifold, which is a union of segments joining an exterior point to a point on a manifold, producing a resultant surface that can be treated as another manifold. (See Figure 17, next page.)

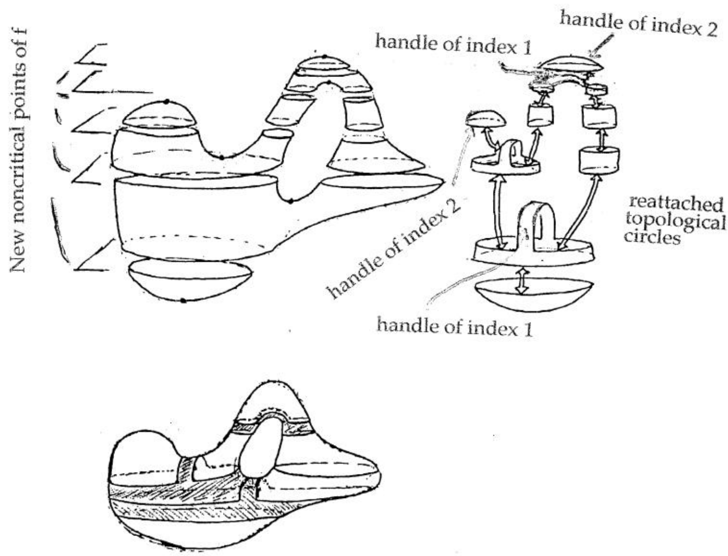


Figure 16

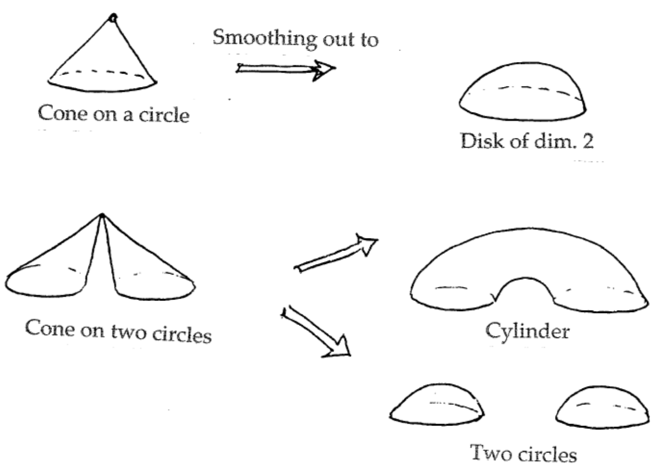


Figure 17

One says that two surfaces are **cobordant** if there exists a manifold of dimension 3 whose boundary is the (topological) union of these two surfaces. The examples above are the cobordisms, respectively, between a circle and the null surface Ø, and between two circles and one circle. (See Figure 18.)

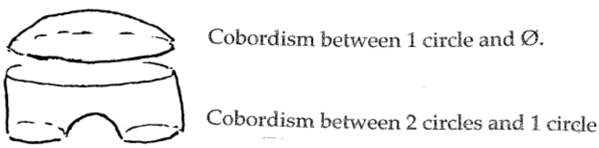


Figure 18

Generic singularities

We've already learned about the singularities (or **critical points**) of a function on a **manifold**. More generally, one can define the singularities of a mapping from one manifold onto another. For example, a singularity of a mapping from one surface to another is a point in the neighborhood of which a system of **local coordinates** (see **manifold**) is not carried onto another system **of local coordinates**.

A generic singularity is a singularity into which any singularity may be transformed by an arbitrarily small perturbation of the domain. Generic singularities of functions on manifolds are the **non-degenerate critical points** (see Figure 11 at the end of **critical point**). The generic singularities of mappings of a surface have been studied by Whitney, whose work had a major influence on Thom. (See Figure 19, next page.)

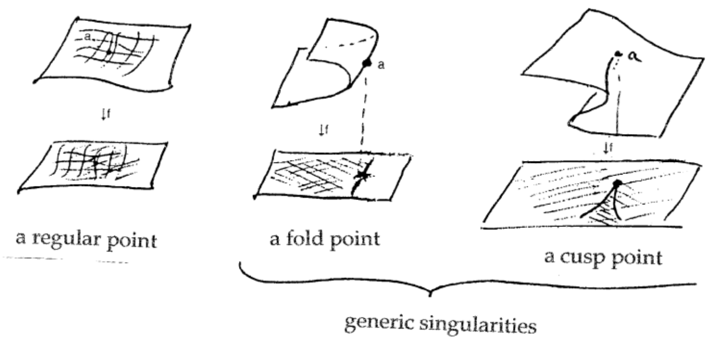


Figure 19

For functions and mappings between surfaces, the generic singularities are structurally stable: a sufficiently small perturbation does not change their character (see also ***analytic objects***).

Homological Algebra

This is a methodology that allows one to distinguish local and global properties of a manifold.

For example, a "small" ***loop*** (that is to say, a **topological circle**) traced on a surface is the boundary of a **topological disk**. A "large" loop is not necessarily such a boundary. (See Figure 20, next page.) From the viewpoint of cobordism, two loops which are boundaries of pieces of a surface are "equivalent". (See Figure 21 on the next page.)

Differential systems, gradient dynamics, Hamiltonian dynamics
(See Figure 22 on the page 230.)

Through following the integral curves for a given period of time t, one defines a mapping φ_t of the surface onto itself. When

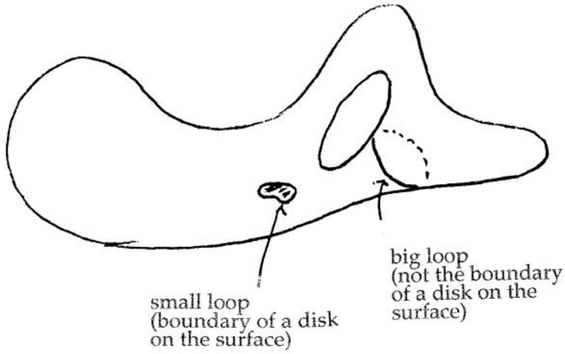


Figure 20

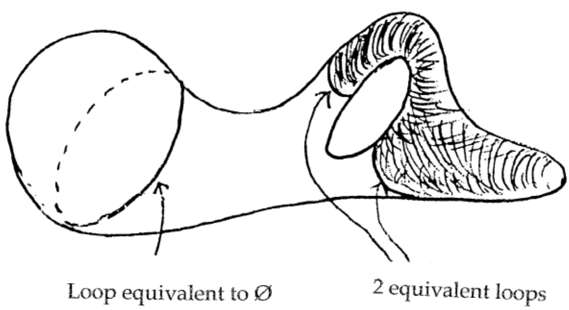


Figure 21

this is carried out for all t, one obtains a family of mappings, the ***dynamical system*** *associated with a vector field*.

The universe of differential systems divides into two parts:

1. ***Dissipative systems***, of which the paradigms are ***gradient systems***, having for integral curves the ***gradient lines*** of a function (if one treats the negative of this function as a kind of energy, one can interpret its behavior through time as a kind of dissipation.) (See Figure 23 on page 231.)

2. ***Conservative systems***, of which the most important ones are those with a **Hamiltonian dynamics** (see Figure 24 on page 231) obtained from a gradient dynamics through rotating each vector by $\pi/2$: then the energy is conserved. This characterization is valid for surfaces. In even dimensional spaces, the $\pi/2$ rotation must be made in different directions: this is known as a ***symplectic gradient*** operation. For example, the Hamiltonian system describing a pair of uncoupled harmonic oscillators of the same period has for its manifolds of constant energy (f = constant) 3-dimensional spheres.

For another example, see **geodesics**.

It is part and parcel of Thom's viewpoint that, in general, a differential system can be described on two levels: a rough description of the gradient type, and a more refined description of the conservative type (see also **basins and attractors**).

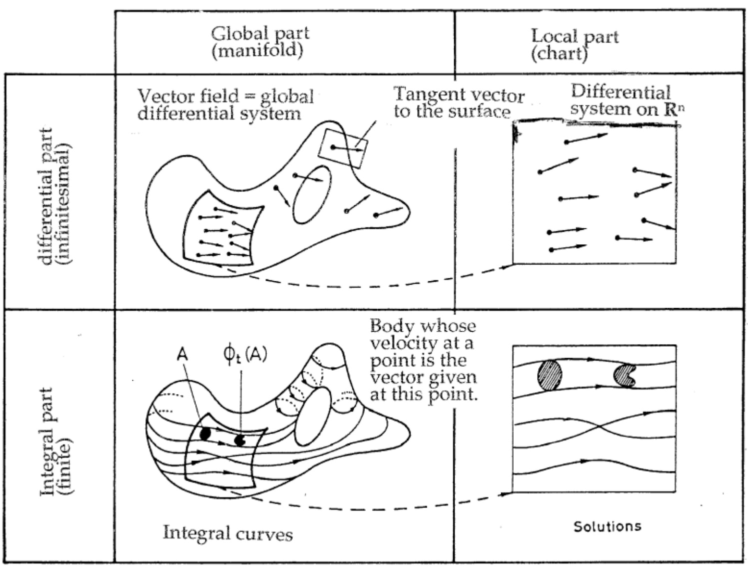


Figure 22

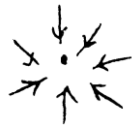


Figure 23

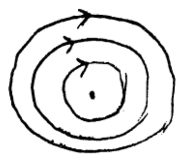


Figure 24

Basins and attractors

One example of the decomposition of a manifold into **basins of attraction** is supplied by the decomposition into Thom cells associated with a Morse function. The differential system is defined by the gradients of f_i, the attractors are the local maxima (see **gradient lines**, **differential systems**, **Morse Theory in the sense of Thom**).

The integral lines of points belonging to a cell of dimension 2 converge towards the local maximum associated with each cell. More generally, the **attractors** are sets of integral curves characterized by the fact that the integral curve at (almost) every sufficiently close point will converge to them. Their topological structure is very simple in dimension 2, but extremely complicated in dimension 3. As for the basins of different attractors, their borders can overlap in a very complex fashion, and need not cover the entire manifold.

Black box (See Figure 25.)

Figure 25

*WKB (Wenzel, Kramers, Brillouin***)**

This method, dating from about 1925, furnishes asymptotic solutions of differential or partial differential equations.

Heisenberg Principle

The more light one shines on an electron to discover which hole it passes through, the more photons bombard it, and the more the electron is perturbed. For a marvelous description of the Uncertainty Principle which forbids the simultaneous measurement of the position and momentum of a particle, see the *Feynman Lectures in Physics,* Volume III.

Fermat's Principle

Light moves along the path that minimizes the length of the "optical path" between two points. An example is refraction. (See Figure 26, next page.)

Caustic

A caustic is an envelope of light rays. (See Figure 27, next page.)

See the ‘cusp singularity’ as described in the section **generic singularities**.

Explanation: Consider the surface produced by light rays, placing each of them at a height which is a function of their inclination: the caustic is the ***apparent contour*** of this surface, the image of the ***singularities*** of the projection on the horizontal plane. (See Figure 28 on the next page.)

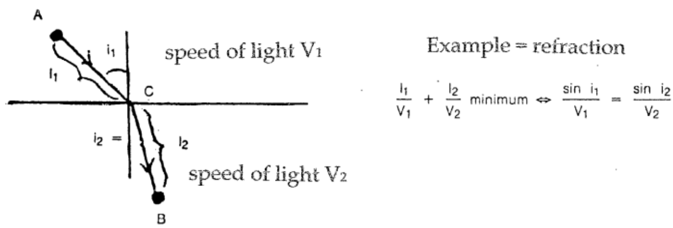


Figure 26

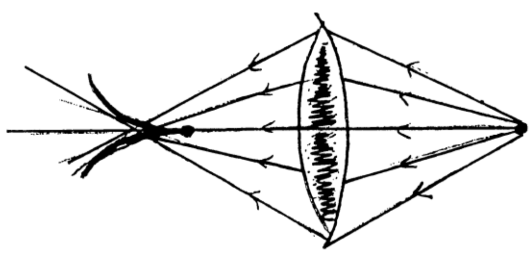


Figure 27

Hyperbolic metric

A model for the non-Euclidean geometry of Lobatchevsky, in which one measures lengths in terms of the ***optical path*** (see **Fermat’s Principle**), in a half plane formed by an accumulation of thin strips, in which the speed of light is equal to their height. The **geodesics** (that is to say, the “straight lines” of the geometry) along which the light rays move, are defined by the law of refraction. (See Figure 29 on the next page.)

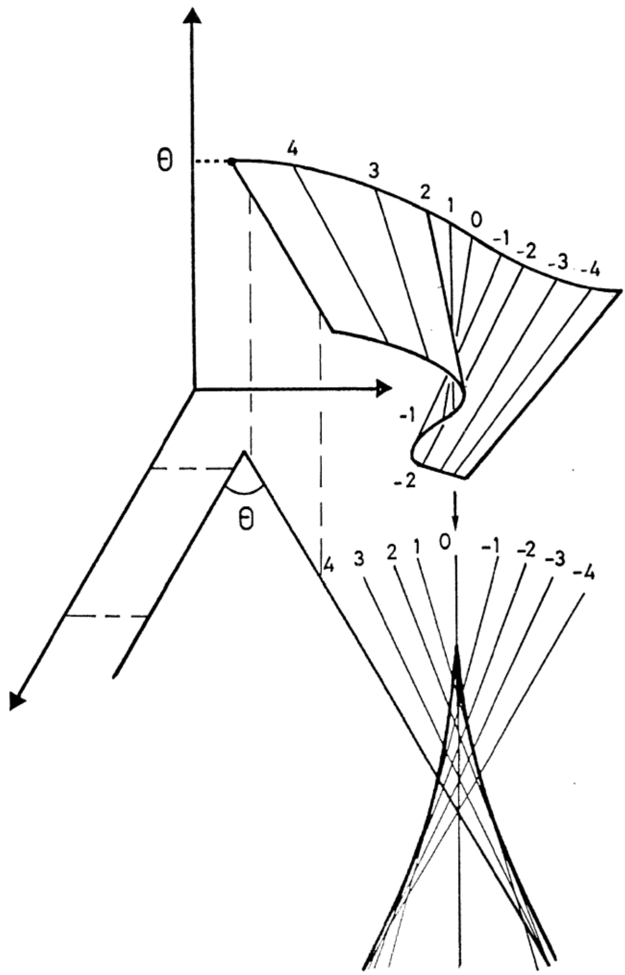


Figure 28

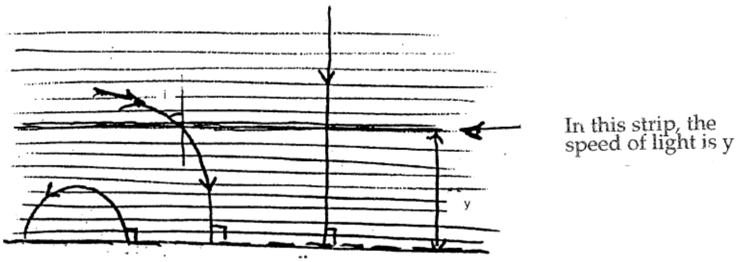


Figure 29

The study of the behavior of geodesics (for more complicated manifolds) was initiated at the end of the 19^{th} century by Jacques Hadamard, later built upon by Morse. It is extremely complicated: most of the geodesics pass arbitrarily close to every point on the surface, and in a direction arbitrarily close to any given direction. This is a typical example of an ***ergodic system***. As rightly observed by Thom there's nothing "chaotic" about this. On the contrary it shows that a perfectly ordinary dynamics can become infinitely complex!

Geodesics on a surface

Choose a metric on a surface, that is to say a method for measuring the length of curves drawn on the surface. This might be the induced metric of an embedding of the surface into $\mathbf{R}^3$, or some other metric.

A **geodesic** is then a curve γ drawn on the surface which possesses the following property: given two points a and b on γ and sufficiently close to each other, the length of the arc between a and b is less than or equal to that of any other arc drawn on the surface between a and b:

1. Straight lines are the geodesics of the Euclidean metric (ordinary length) on $\mathbf{R}^3$;

2. The great circles are the geodesics on the round sphere;

3. On a torus of revolution, equipped with a metric inherited from $\mathbf{R}^3$, the geodesics are already very complex. (See Figure 30.)

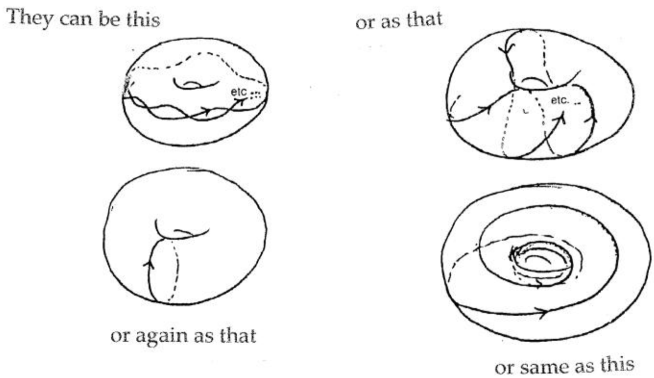


Figure 30

The geodesics on a surface in $\mathbf{R}^3$, equipped with the metric induced by $\mathbf{R}^3$, are the curves followed by a particle constrained to move along the surface not subject to the force of gravity. This is the fundamental example of a **Hamiltonian system** (defined on a manifold of dimension 4, the space of the vectors tangent to the surface under consideration).

Hilbert Space

Hilbert space is like n-dimensional Euclidean space, but of infinitely many dimensions: each point is identified by a vector with infinitely many coordinates $\boldsymbol{x} = (x_1, x_2, \ldots, x_n, \ldots)$. The distance from the origin is given by a generalized version of the "Pythagorean Theorem": Distance $[0, x] = \sqrt{x_1^2 + x_2^2 + \ldots + x_n^2 + \ldots}$. The "points" of this space are those for which this distance is finite. The paradigmatic example is the ***Fourier Space***, whose "points" are periodic functions integrable over a period, and the coordinates are the Fourier coefficients (harmonic amplitudes).

Cantor's transfinite cardinal numbers

We will say that two sets are "equivalent" when their elements can be placed into 1−1 correspondence. A transfinite number is an "equivalence class" of sets. The equivalence classes of finite numbers are the ordinary numbers 0, 1, 2,…. As for the others, there are many, many, many! (See Figure 31.)

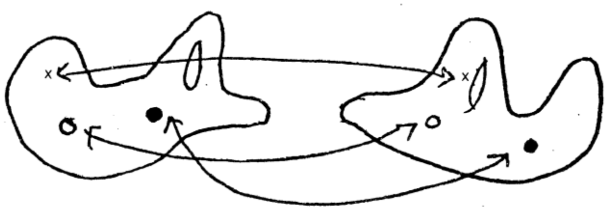


Figure 31

Analytic objects, stratification, equisingularity

A portion of $\mathbf{R}^n$ defined by one or several equations is not always a **manifold**. The "form" of such an object can be extremely complicated, which is usually the case when the equations are simply differentiable. However, in the case of polynomials (or, more generally analytic functions (roughly speaking, polynomials with real coefficients of infinite degree), the resultant forms are remarkably well structured: they allow for **stratification** (another cherished notion of Thom), decompositions into manifolds of differing dimensions (the ***strata***), which admit of a geometric analysis. To be useful, these stratifications have to satisfy certain conditions going back to Whitney, which guarantee the **equisingularity** of the object throughout the length of each stratum: in the neighborhood of each point of the stratum, the landscape is the same.

Figure 32 shows the *swallowtail* (one of the ***elementary catastrophes*** of Thom).

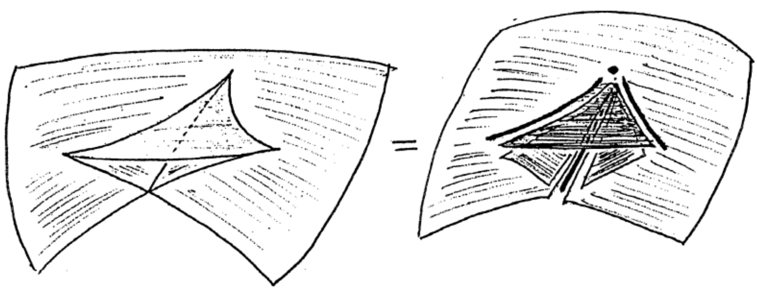


Figure 32

Observation*:* A regular object, like a **manifold**, also admits stratifications, such as a decomposition into Thom cells (see **Morse Theory in the sense of Thom**).

In the same spirit, the classification of functions on a manifold, according to the types of critical points, is achieved through the **stratification** of the “space” (of infinitely many dimensions) of all functions: the crudest ***strata***, the union of which fills "almost" the whole space, contain the **Morse functions**, with distinct critical values (see **critical points**), that is to say, the **structurally stable functions** (see **generic singularities**).

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This is a very complete version of Thom's published and unpublished works. There are a few articles not published in *OC* and Prof. Porte has been made aware of that by this editor.

Prédire n'est pas expliquer (Paris: Eshel, 1991)

This is a book of conversations with Emile Nöel. A corrected edition was published in 1993 by Flammarion, Paris and this edition is found in [*OC*, 1991, 1]. The Lisker translation is based on the IHES edition. The working title is *Prediction is not Explanation*.

Apologie du Logos (AL), (Paris: Hachette, 1990)

This is a collection of Thom's articles from 1970 to 1989. Each article has a short introduction.

Esquisse d'une sémiophysique, (Paris: InterEditions, 1988)

This book was translated into English by Vendla Meyer as *Semio Physics: a sketch – Aristotelian Physics and Catastrophe Theory*. It was published in 1990 by Addison-Wesley, Redwood City, CA. We will refer to this book as *Semiophysics*.

Paraboles et catastrophes (*P et C*), (Paris: Flammarion, 1983)

This is a book of conversations with Giulio Giorello and Simona Morini. It was a translation of the Italian book *Paraboli e Catastrofi* published in 1980 by Il saggiatore, Milan. The translation into French was by Luciana Berini and was completely reviewed by Thom. The French version has been translated by Roy Lisker (2010). The working title is *Parables, Parabolas and Catastrophes*.

Modèles mathématiques de la morphogénèse, (Paris: C. Bourgois, 1974)

A re-edited and augmented edition was published in 1980. This edition with some small changes was translated into English as *Mathematical Models of Morphogenesis (MMM)* by W. M. Brookes and D. Rand and was published in 1983 by Ellis Horwood, Chichester, England.

Stabilité structurelle et morphogénèse, (Reading, MA: Benjamin, 1972)

A second edition, with some changes, was published in 1977. The 1972 edition, with some small changes, was translated into English as *Structural Stability and Morphogenesis (SSM)* by D. H. Fowler and published in 1975 by the same publisher. A 1989 edition, with a new preface by Thom, was published by Addison-Wesley, Redwood City, CA.

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"Stop chance!, Silence Noise!", *SubStance*, #**40** (1983): 11-21.

1980b/1983 "Sur le déterminisme: En guise de conclusion", *Le Débat* **15** (1980): 115-123. Also in [*OC*, 1980, 2.1].
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1982a "L'Aporia fondatrice delle matematiche", In *Enciclopedia Sistematica* ***XV*** (Torino: Einaudi,1982: 1133-1146). Also in [*OC*, 1982, 1].

1982b "Local et global dans l'oeuvre d'art." In *De la catastrophe,* (Genève: Centre d'Art Contemporain, 1982: 41-53.) Also in *AL*, pp. 101-117 and in [*OC*, 1982, 2].

1982c/1983 "La modélisation des processus mentaux; le 'Système' valéryen vu par un théorecian des catastrophes", In *Fonctions de l'esprit. Treize savants redécouvrent Paul Valéry,* (Paris:

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