CHAPTER III: CATASTROPHES

> Voici maintenant qu'après l'âge des denrées et des matières, après celui de l'énergie, nous avons commencé à vivre celui de la forme.
> —Pierre Auger.[1]

> Peut-être n'est plus capable que le mathématicien de suivre une question de forme pure.
> —George D. Birkhoff.[2]

1. INTRODUCTION[3]

In Firestone Library, at Princeton University, librarians usually throw away the paper cover of books. They are still keeping that of Alexander Woodcock and Monte Davis's popular introduction to catastrophe theory, even though it is falling apart.[4] A mere accident? Probably. But I like to think that they were struck by the quotations on the back and wished to share them with the readers. From "an intellectual revolution" to "the height of scientific irresponsibility," opinions on catastrophe theory seemed to diverge so widely that it raises a question: how can a theory prove so shapeless?

[1] "Now, after the age of materials and stuff, after the age of energy, we have begun to live the age of form." P. Auger in *Proceedings of the First International Conference on Cybernetics* (Namur, 1956); quoted and translated in G. Bowker, "How to be Universal: Some Cybernetics Strategies, 1943-70," *Social Studies of Science,* 23 (1993): 107-127, 111 and 124.

[2] "Perhaps no one is more able than the mathematician to follow a question of pure form." G. D. Birkhoff, "Quelques éléments mathématiques de l'art," *Atti del Congresso internazionale dei matematici, Bologna, 3-10 settembre 1928 (VI),* 1 (Bologna: Nicola Zanichelli, 1928): 315-333; repr. *Collected Papers,* 3: 288-306, 306.

[3] A version of this chapter is to be published in *Growing Explanations,* ed. M. Norton Wise.

Slow to be widely appreciated when it was introduced in the late 1960s by French mathematician René Thom, catastrophe theory was propelled on a wave of hype and enthusiasm during the mid-1970s, only to die out in a bitter controversy by the end of the decade. Caught in a fierce debate, catastrophe theory proved unable to survive the attacks. It now seems, for all practical purposes, to have vanished from the scene of science. But, how can so much hope last for so little time? And what, if any, can its legacy be?

a) What Ever Happened to Catastrophe Theory?

Catastrophe theory is dead.[5] Today, very few scientists identify themselves as 'catastrophists'; the theory has no institutional basis, department, institute or journal totally or even partly devoted to it. But do mathematics die? In a pioneering article on invariant theory, Charles Fisher has shown that the death of a mathematical theory is of a peculiar kind.[6] Dead mathematical theories leave behind them a corpus of theorems that usually remain true for most mathematicians. These theorems and some specific techniques find a new life, divorced from the original impulse, in other areas of mathematics and science. People educated during the time of success of the old theory are capable of broadly maintaining the same lines of thought, and even these people's radical

[4] A. Woodcock and M. Davis, *Catastrophe Theory* (New York: E. P. Dutton, 1978).
[5] Only accounts written by scientists exist concerning the history of catastrophe theory. A. Woodcock and M. Davis, *Catastrophe Theory,* which is also a good nontechnical introduction to the subject; I. Ekeland, *Le Calcul, l'imprévu. Les figures du temps de Kepler à Thom* (Paris: Seuil, 1984); *Calculus and the Unexpected* (Chicago: University of Chicago Press, 1988); and T. Tonietti, *Catastrofi: Una controversia scientifica* (Bari: Dedalo, 1983). See also J. Guckenheimer, "The Catastrophe Controversy," *Mathematical Intelligencer,* 1 (1978): 15-20; and A. Boutot's "Catastrophe Theory and Its Critics," *Synthese,* 96 (1993), 167-200.

theoretical departures are often shaped by the dead theory. This survival of some aspects of the dead theory is, I believe, what René Thom was after, when, surveying the fate of catastrophe theory, he said in 1991:

> Sociologically speaking, it can be said that this theory is a shipwreck. But in some sense, it is a subtle wreck, because the ideas that I have introduced gained ground. In fact, they are now incorporated in everyday language. . . . The notions [of catastrophe theory] have become part of the ordinary baggage of modelers. Therefore, it is true that, in a sense, the *ambitions* of the theory failed, but in *practice,* the theory has succeeded.[7]

This chapter aims at providing a first approach to the possible meanings this quotation may have. How can catastrophe theory have succeeded "in practice," while failing to live up to its original "ambitions"? Indeed, catastrophe theory provides the historian of science a first-class example of the fact that *mathematical concepts and theorems hardly are the only legacy that a mathematical theory used to model nature may have.*

True, the concepts introduced by Thom, the theorems he and his collaborators proved, have all survived more or less untouched as "a beautiful, intriguing field of pure mathematics."[8] But in the whole of this dissertation, I intend to show that the intent manifested by some authors to reduce the historical significance of catastrophe theory to the creation of an arcane corner of pure mathematics already incorporates a certain vision of the nature of mathematics and its role vis-à-vis other sciences and society. Bluntly put, to consider catastrophe theory merely as a field of pure mathematics betrays a more or

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[6] C. S. Fisher, "The Death of a Mathematical Theory: A Study in the Sociology of Knowledge," *Archive for History of Exact Sciences,* 3 (1966): 137-159.
[7] R. Thom, *Prédire n'est pas expliquer,* interview by É. Noël (Paris: Eshel, 1991), 47. My emphasis.

less Bourbakist ideology insisting on the autonomy of mathematics, holding that well defined concepts and rigorously proved theorems, incorporated within well articulated theories are the only products of mathematical practice. On the contrary, from the very beginning, René Thom envisioned much more than to create, with catastrophe theory, just another branch of pure mathematics. He wished to suggest new ways to use mathematical tools and practices in order to make sense of the world.

In fact, I show in the rest of this dissertation that the *modeling practices* of catastrophe theory have indeed survived in an altered form, and been adopted and adapted very successfully, in particular, within the framework of deterministic chaos theory. This demonstration constitutes one of the main topics of Chapters VI to VIII below. In order to demonstrate this carefully, it will however be necessary to go more in details into the mathematical backgrounds and institutional setting against which catastrophe theory was constructed than I can do in this chapter only (Chapters IV and V).

For the moment, I argue at a level between that of cultural connectors and modeling practices. As I hinted at in Chapter II, catastrophe theory became in the 1970s an important cultural connector between mathematics and some French intellectual milieus. As I show below, Thom's conception of his theory was also inspired by some cultural connections he himself drew with biology and linguistics in particular. My main point, in the following, is that it is profitable to consider catastrophe theory, almost as it was conceived by René Thom, that is, as a *theory of modeling practice*. By this, I mean that when he introduced his theory Thom had the ambition of codifying new

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[8] D. P. L. Castrigiano and S. A. Hayes, *Catastrophe Theory* (Reading: Addison-Wesley), xii.

mathematical methods for the modeling of natural phenomena. Therefore, I shall not, for the moment, describe Thom's modeling practices *per se,* but rather the way he constructed a theory of modeling practice, which was based on his practices and the connections he drew with other sciences. The result was an original philosophy of science that we need to examine carefully, before we can go into the details of the specific disciplinary and institutional contexts that made catastrophe theory at all possible.

b) Catastrophe Theory: A Theory of Modeling Practices

As described in Chapter I, I defined the term 'modeling practice' as a useful heuristic tool for the historian of science. Now, discussions about modeling practices can sometimes be articulated into a coherent discourse. Going back to Louis Althusser, I more or less identify this with what he called a "Theory of theoretical practice," (with a capital T).[9] In my view, scientists who introduce innovative theoretical and modeling practices, which go against the general consensus, sometimes feel the need to articulate, or codify, their views in the form of a theory of theoretical or modeling practice. In those time of innovation (or revolution, to use the cliché), these scientists are often perceived as acting as philosophers, which explains why we often hear that Einstein, Bohr, Mach or Newton were philosophers, as well as scientists.

[9] L. Athusser, *For Marx,* 168. Again, I differ from Althusser when I say that any modeling practice is susceptible of being codified, at least in a sketched form, in a theory of modeling practice. For Althusser, Theory (with a capital T) is "general theory, that is, the Theory of practice in general. . . . This Theory is the materialist *dialectic,* which is the same thing as dialectical materialism," *Ibid.,* 168. Elsewhere, he defines philosophy as the Theory of theoretical practice. L. Althusser and É. Balibar, eds., *Lire "le Capital"* (Paris: François Maspero, 1968), 6.

I argue that Thom's innovations are best seen in terms of modeling, as opposed to theoretical, practices, that is, that they were meant as a way to use mathematics in order to account for natural phenomena, without being necessarily constrained by the theoretical apparatus of a single discipline. In the following, I shall therefore focus on theories of modeling practice, and eschew the discussion of theories of theoretical practices, which, however, might follow a similar line.

Moreover, it is important to emphasize that theories of modeling practice, which are produced by working scientists with a specific purpose in mind, are significantly different from accounts of practice (such as my own) provided by historians, philosophers, or sociologists of science. Indeed, in a more or less complete form, scientists' theories of modeling practices offer a *prescriptive* framework for how scientific models should be constructed, while I only wish here to provide a heuristic *description* of some original modeling processes pushed forward in the sciences by Thom and others. The theories of modeling practice provided by working scientists seek to articulate explicitly some, or all three, of the elements of modeling practice that I have identified in Chapter I: raw material, means of transformation, and product-knowledge. For our purpose, it is of little importance to know what should enter such a theory, and indeed whether it is at all possible to write such a theory of modeling practice. I am content with noticing that when scientists face resistance, either from their own demands of consistency, or from scientific communities, with respect to the modeling practices they introduce, they sometimes attempt to articulate more or less explicitly their own theory of modeling practice in a coherent form.

Let me emphasize that it is hardly necessary for one model-builder in particular, or for a scientific community, to possess a well-articulated theory of modeling practice. As Althusser wrote, a theoretical practice "may well be able to do its duty as theory without necessarily feeling the need to make the theory of its own practice, of its process. This is the case with the majority of the sciences."[10] In most cases, model-builders are perfectly content to do their own work, following their own modeling practices, without relying on an explicit theory of modeling practice. Sometimes, however, they feel the need for such an explicit articulation of their modeling practice. These are usually episodes of the history of science that are interesting to study, because they reveal the inner workings of scientists' rapport with their own modeling practice.

Together with other mathematicians—Ralph Abraham, Steve Smale, and Christopher Zeeman, who often visited Thom at the Institut des hautes études scientifiques (IHÉS) in Bures-sur-Yvette, France, in the late 1960s (Chapter VI)—René Thom proposed radically new modeling practices to the physical sciences, but also and mainly to the biological and human sciences. Moreover, catastrophe theory was his own attempt at formulating a comprehensive theory of modeling practice. With it, he wished to redefine what it meant to build a mathematical model of a natural phenomenon. He offered to consider new conceptual objects as the raw material of his modeling practice, new mathematical tools as its means of knowledge production, and new interpretations of

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[10] L. Athusser, *For Marx,* 173-174. His example: Karl Marx, who never wrote a *Dialectics* which would have been his Theory of theoretical practice, *i.e.* the Theory of historical materialism. For Althusser's view on the philosophy of science, see L. Althusser, *Philosophie et philosophie spontanée des savants (1967)* (Paris: François Maspero, 1974); *Philosophy and the Spontaneous Philosophy of the Scientists, and Other Essays,* ed. G. Elliott (London: Verso, 1990).

the kind of the knowledge to which science should aspire. Roughly speaking, his experience in mathematics provided him with new means of knowledge production. This and his forays in embryology shaped his views on what should be sound raw material for theory. Finally, he found in the French intellectual context a model for his interpretation of product-knowledge. This was provided by structuralism, which he had to confront when he tackled the problems of linguistics and semiotics, and beyond which he sought to go.

The present chapter is thus an illustration of Thom's theory of modeling practice and a study of his sources of in mathematics, biology, and linguistics. Finally, I here examine the general philosophy, i.e. the theory of modeling practice, that Thom had put in place around 1975, after the main tenets of catastrophe theory had been well publicized, but just before harsh critiques made him somewhat change his views. This will provide a useful background for my later more tightly focused study of the contexts in which the modeling practices of catastrophe theory were developed, then adopted and adapted for the physical theory of turbulence.

2. WHAT WAS CATASTROPHE THEORY?

A bold and comprehensive mathematical theory aiming at explaining the dynamics of shapes in the everyday world, catastrophe theory has often been narrowly understood as a new mathematical approach able to deal with abrupt, discontinuous changes in nature: a rubber band that breaks. For Thom, however, from the very beginning, it always was much more than this.

Almost entirely an original construct of Thom's, catastrophe theory slowly matured throughout the sixties. From 1964 to 1968, on his own account, Thom worked on an ambitious book, a real manifesto in fact, that would reveal his unconventional ideas to the world. This book, titled *Structural Stability and Morphogenesis,* was not published until 1972, due to its publisher's financial trouble.[11] For this reason, catastrophe theory was first presented in two articles, both published in 1968. To the proceedings of a Theoretical Biology Symposium, held at Bellagio, Italy, Thom contributed "A Dynamical Theory of Morphogenesis," and for the French journal *L'Âge de la science,* he wrote "Topology and Meaning."[12] The first article was concerned with biology. The second paper addressed issues of semiotics, to which Thom would devote much effort in the following years, and linked his thinking about modeling practices to the current vogue in French thought: structuralist semiotics. Not content with introducing a new mathematical language and exploring its consequences in some areas of science, Thom also conceived of his book, and both of these articles, as *exposés* of an original philosophy of science, indeed a true "natural philosophy."[13] The subtitle of his book, "An Outline of a General

[11] R. Thom, *Stabilité structurelle et morphogénèse* (Reading: W. A. Benjamin; Paris: Édiscience, 1972; reed. InterÉdition, 1977); *Structural Stability and Morphogenesis,* transl. by D. H. Fowler (Reading: Benjamin, 1975); hereafter *SSM.*

[12] R. Thom, "Une théorie dynamique de la morphogénèse," in *Towards a Theoretical Biology,* 1, ed. C. H. Waddington (Edinburgh: Edinburgh University Press, 1968); and "Topologie et signification," in *L'Âge de la science,* 4 (1968). Both are reprinted in R. Thom, *Modèles mathématiques de la morphogénèse. Recueil de textes sur la théorie des catastrophe et ses applications* (Paris: U.G.E., coll. "10/18, 1974; reed. Christian Bourgeois 1980); *Mathematical Models of Morphogenesis,* transl. W. M. Brookes and D. Rand (Chichester, Ellis Horwood, 1983), 1-38, 166-191; hereafter *MMM.*

[13] See, *e.g.,* R. Thom, "Towards a Revival of Natural Philosophy," *Structural Stability in Physics,* ed. by W. Güttinger and H. Eikemeier (Berlin: Springer, 1979): 5-11. See also J. Largeault, "René Thom et la philosophie de la nature." *Critique,* 36 (1980): 1055-1060; and *Philosophie de la nature 1984* (Créteil: Université Paris XII Val-de-Marne, 1984).

Theory of Models," reveals the extent of his ambitions: to sketch out his own theory of modeling practice.

A striking paradox raised by Thom in 1968 illustrates his epistemological concerns.[14] Consider an eroding cliff and the developing egg of a frog. In the former case, suppose that we know later microclimatic conditions and the geological nature of the soil, then our knowledge of the physical and chemical forces at play will be excellent. Even then, it is impossible to predict the future shape of the cliff. As for the egg, however, Thom contended that, although knowledge of the substrate and developmental mechanisms is sketchy, we can still be pretty sure that it will end up as a frog! This paradox, in Thom's view, showed that a blind reliance on reductionist arguments had little to say about the forms of nature. Clearly, a new method was needed: one that would focus on shapes, account for their stability, and explain their creation and destruction.

For Thom, catastrophe theory supplied this method. In summary, its goal was to understand phenomena of the world by approaching them directly, rather than relying on traditional reductionist methods. Its main concern was the creation and destruction of shapes and forms, but more precisely forms as they arise in the world, at the mundane level of everyday life. Catastrophe theory posited the existence of a mathematically-defined structure responsible for the stability of these forms, a structure that Thom called the *logos* of the form, and consequently he rejected the notion that the universe was governed by chaos or chance. The models built with the help of catastrophe theory were inherently qualitative, not quantitative, which meant that they were not suited for action or prediction, but rather aimed at describing, and intelligibly understanding, phenomena of the world. Finally, Thom recognized that catastrophe theory was not a proper scientific theory, but rather a method or a language that could not be tested experimentally, and therefore was not falsifiable in Popper's sense. These themes will be further developed at the end of this chapter.

One might be surprised that I have hardly mentioned mathematics. Indeed, catastrophe theory was elaborated on the basis of, and importantly shaped by, Thom's mathematical experience and concerns as we shall see in Chapter VI. However, he considered that catastrophe theory went far beyond mathematical techniques. When viewed as a theory of modeling practice, the mathematical tools used by catastrophe theory becomes secondary. Mathematics made Thom's thought possible, but it did not subsume it. With catastrophe theory, Thom proposed, not just new mathematical models applicable in embryology, but a modification of the common understanding of the mathematical modeling of natural phenomena.

3. SOCIOLOGICALLY SPEAKING, A MATHEMATICIAN

At the source of catastrophe theory, we find a man who still "sociologically" defines himself as a mathematician.[15] Born in 1923, René Thom likes to say half-jokingly that he owes his professional orientation to his parents' advice. They "had lived through the First

[14] R. Thom, "Une théorie dynamique," *MMM,* 15.
[15] R. Thom recounts his memories in two published interviews: *Paraboles et catastrophes. Entretiens sur les mathématiques, la sciences et la philosophie,* interview by G. Giorello and S. Morini (Paris: Flammarion, 1983 [Milan: Il Saggiatore, 1980]) and *Prédire n'est pas expliquer.* See also his "Problèmes rencontrés dans mon parcours mathématique: un bilan," *Publications mathématiques de l'IHES*, 70 (1989): 199-214, and his "Exposé introductif" in *Logos et Théorie des catastrophes. À partir de l'oeuvre de René Thom.* Actes du colloque international de Cerisy-la-Salle, septembre 1982, ed. by J.

War [and] told us: Try to be an artillery man. They are less exposed than the infantry!"[16] You needed mathematics to qualify for the artillery: René Thom passed his "*bachot de mathélèm*" (high school degree in elementary mathematics) in 1939. More seriously, he recalls a "decisive encounter with Euclidean geometry" during his *lycée* years. The effects of his attraction to what he calls "the geometric mode of thought and type of proof" are still present many years later.[17] However, his geometric, intuitive vision of mathematics was opposed to the dominant trend.

a) Mathematical Styles: Bourbaki Against Intuition

Too young to be drafted in 1939, René Thom went on with his education during the Occupation, first at the *lycée Saint-Louis,* then at the *École normale supérieure* starting in 1943, where he experienced "the excitement born with Bourbakist ideas."[18] Some Bourbakis, already by the late 1930s important members of the French mathematical community, were among Thom's professors. As I described above, Bourbaki "was a symbol . . . of the triumph of abstraction over application, of formalism over intuition."[19] He made "mathematics appear as a polished monolith, built purely deductively."[20] As we saw above, Bourbaki did not reject geometry so much as the intuitive approach to

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Petitot (Geneva: Patiño, 1988): 23-39. There, he wrote that "sociologically" *(sociologiquement)*, he was a mathematician.

[16] R. Thom, *Prédire n'est pas expliquer,* 9.

[17] R. Thom, "Exposé introductif," 24. See also his interview in *Hommes de sciences: 28 portraits,* ed. M. Schmidt (Paris: Hermann, 1990).

[18] R. Thom, "Problèmes rencontrés," 200.

[19] L. Beaulieu, *Bourbaki. Une histoire du groupe de mathématiciens français et de ses travaux (1934-1944),* thèse de l'université de Montréal (1989), 1.

[20] G. Birkhoff, "Current Trends in Algebra," *American Mathematical Monthly,* 80 (1973): 760-782, 772. Emphasized in the original text.

Euclidean geometry, upon which Thom's mathematical intuition and philosophy were built.

Thom's opinion of Bourbaki is thus quite ambivalent. One of Bourbaki's most successful students, he clearly praises Bourbaki for introducing into France the mathematics of the Hilbert school in Göttingen. But David Hilbert's message was that two tendencies were present in mathematics:

> On the one hand, the tendency toward *abstraction,* seek[ing] to crystallize the *logical* relation inherent in the maze of material that is being studied, and to correlate the material in a systematic and orderly manner. On the other hand, the tendency toward *intuitive understanding,* foster[ing] a more immediate grasp of the objects one studies, a live *rapport* with them, so to speak, which stresses the concrete meaning of their relations.[21]

For Thom, Bourbaki had clearly chosen the first path, thus failing to keep Hilbert's mathematics alive. "It is a bit like if at the time of Vesaleus, when the method of dissection eventually imposed itself, one had wanted to identify the study of human beings with the analysis of cadavers."[22] Bourbaki's ascetic ultraformalism killed mathematics.

Thom therefore knew Bourbaki very well. As mentioned above, he had once been one of their "guinea pigs," but says that he literally fell asleep during the lectures.[23] Nevertheless, he was learning! It is Thom's early achievement to have been able to reconcile his powerful geometric intuition with Bourbaki's mathematics. In 1946, Thom

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[21] D. Hilbert and S. Cohn-Vossen, *Gometry and the Imagination,* transl. P. Nemenyi (New York: Chelsea, 1952 [1932]), iii. Their emphasis.
[22] Interviewer's comment, with which Thom agrees, in R. Thom, *Paraboles et catastrophes,* 24.
[23] R. Thom, *Paraboles et catastrophes,* 23; and also A. Haefliger, "Un aperçu de l'oeuvre de Thom en topologie différentielle (jusqu'en 1957)," *Publications mathématiques de l'IHES,* 68 (1988): 13-18, 15.

moved to Strasbourg with his mentor Henri Cartan who oriented him towards a field of mathematics that was then rapidly developing: differential topology. This, in part, motivated Thom's ambiguous assessment of Bourbaki. Multidimensional spaces, which form the subject of topology, are difficult to visualize intuitively. It is then that systematic, rigorous and formal thought, however boring and counterintuitive it might be, is incomparably useful. Thom mastered these technical means (algebraic and topological) offered by Bourbaki's edifice. Indeed, he mastered them well enough to obtain powerful mathematical results, which, according to the mathematician Jean Dieudonné, "the modern rise of differential topology."[24] In 1958, the Fields Medal, the highest distinction for a mathematician, was awarded to René Thom in recognition for this work.

Thom's powerful intuition was already at work. In his tribute to Thom, the mathematician Heinz Hopf clearly identified his strengths. This was a time in which topology was in a "stage of vigorous . . . algebraicization," he wrote.[25] Not only had algebra been found to provide "a means to treat topological problems," but also "it rather appears that most of the problems themselves possess an explicitly algebraic side." This algebra/topology divide then informed the mathematicians' image of their discipline. As Hermann Weyl had put in 1939: "In these days the angel of topology and the devil of

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[24] J. Dieudonné, *Panorama des mathématiques pures. Le choix bourbachique* (Paris: Gauthier-Villars, 1977), 14. See Thom's paper "Quelques propriétés globales des variétés différentiables," *Commentarii Mathematici Helvetici,* 28 (1954): 17-86.
[25] H. Hopf, "The work of R. Thom" in *Proceedings of the International Congress of Mathematicians* [Edinburgh: August 1958] (Cambridge: Cambridge University Press, 1960): lx-lxiv; this and the following quotes are from pp. lxiii-lxiv.

algebra fight for the soul of each individual mathematician." But in 1952, he was reported as having acknowledged: " I take it all back."[26]

Still, for Hopf, a danger lurked in algebraicization of topology, namely the danger of "totally ignoring the geometrical content of topological problems."

> In regard to this danger, I find that Thom's accomplishments have something that is extraordinarily encouraging and pleasing. While Thom masters and naturally uses modern mathematical methods and while he sees the algebraic side of his problems, his fundamental ideas . . . are of a perfectly geometric-*anschaulich* nature.

Thom was able to use Bourbaki's powerful algebraic methods in order to solve topological problems without loosing sight of their *anschaulich,* or intuitive, character. Because of Thom's original approach, Heinz Hopf predicted that the effect of Thom's future ideas would "not be exhausted for a long time."[27] An ardent 'catastrophist,' Tim Poston, later vividly contrasted Thom's style with a more traditional approach *à la* Bourbaki.

> Some mathematicians go at their work like engineers building a six-lane highway through the jungle, laying out surveying lines, clearing the underbrush, and so on. But Thom is like some creature of the mathematical jungle, blazing a trail and leaving just a few marks on his way to the next beautiful clearing.[28]

Indeed, Thom would come to a conception of rigor in mathematics as counter-intuitive and counter-productive. "Absolute rigor is only possible in and by insignificance."[29] It was, in any case, only an ideal goal, never achieved in practice. True to his preference for

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[26] H. Weyl, "Invariants," *Duke Mathematical Journal,* 5 (1939): 489-502, 500; and A. Borel in "Responses to 'Theoretical Mathematics: Towards a Cultural Synthesis of Mathematics and Theoretical Physics,' by A. Jaffe and F. Quinn," *Bulletin of the American Mathematical Society,* 30 (1994): 178-207, 180.
[27] H. Hopf, "The work of R. Thom," lxiv.
[28] Tim Poston, quoted by A. Woodcock and M. Davis, *Catastrophe Theory,* 16.

meaningful wholes over insignificant details, Thom held that rigor hid the essential. "Rigor," he wrote, "is essentially a *local* property of mathematical reasoning."[30] In any case, it always followed. "Rigor, in mathematics, essentially is a question of housekeeping [*intendance*]."[31]

(i) Mathematical Interlude I: Thom's Cobordism Theory

This section shows in greater mathematical details the intuitive approach, allied with profound knowledge of Bourbakist methods, that guided Thom in his work on *cobordism theory,* for which he was awarded the Fields Medal in 1958.[32] As Heinz Hopf said, cobordism was especially important because of the way Thom mixed topological and algebraic approaches in the classification of manifolds. In the following, I define a few concepts, central to either *topology* or *algebra,* two of the three-legged bases of Bourbaki's mathematics, in order to illustrate Thom's mathematical work. Briefly, Thom's cobordism theory enable him to construct *groups* Ω^n out of *equivalence classes* of *manifolds* of dimension n, and classify these groups.

Topology is a generalization of geometry, which studies spaces with the degree of generality that is appropriate to a specific problem. One central concern of topology is to

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[29] R. Thom, "Mathématiques modernes et mathématique de toujours," *Pourquoi la mathématique?,* ed. R. Jaulin (Paris: Union générale d'éditions, 10/18, 1974): 39-56, 49.
[30] R. Thom, "Modern Mathematics: An educational or Philosophical Error?" *American Scientist,* 51 (1971): 695-699, 697. Origin. publ. in French in *L'Âge de la science,* 3(3) (1970): 225-236.
[31] R. Thom in *Entretiens avec "Le Monde",* 3. *Idées contemporaines,* interview by J. Mandelbaum (Paris: La Découverte, 1984), 80; and R. Thom, "Mathématiques modernes," 52.
[32] R. Thom, "Quelques propriétés globales;" see also R. Thom, "Sous-variétés et classes d'homologie des variétés différentiables," *Séminaire Bourbaki,* 5 (February 1953), exposé #78; and "Variétés différentiables cobordantes," *CRAS,* 236 (1954): 1733-1735.

study the properties of spaces that do not change under a continuous transformation, i.e. translation, rotation, and stretching, without tearing. One such property is expressed by the concept of *dimension:* a curve is one-dimensional; a surface has two dimensions; ordinary space, three; and the space-time of general relativity theory, four.

When a mathematician is faced with the problem of characterizing a space that is locally isomorphic to a Euclidean space with dimension n, he or she uses the notion of *manifold.* An n-dimensional manifold is thus a space M, such that there is a neighborhood V around each point p of M in one-to-one correspondence with a subset W of $\mathbf{R}^n$. The study of manifolds is called *differential geometry,* and the classification of all manifolds of a given dimension is an important problem of topology.[33] It is also possible to define *manifolds with edges.* If the manifold with edges has n+1 dimensions, than the edges are n-dimensional manifolds. For example, a sheet of paper folded into a cylinder have edges that are circles; a manifold with three circles as edges looks like pants.

Let us also define *equivalence relations* and *equivalence classes.* An equivalence relation ~ over a set S is defined so that, for all a, b and c in S, the three following properties are satisfied: (1) reflexivity: $a \sim a$; (2) symmetry: if $a \sim b$ then $b \sim a$; and (3) transitivity: if $a \sim b$ and $b \sim c$, then $a \sim c$. The equivalence class $[a]$ of an element a of S is

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[33] For example: (1) A circle is a one-dimensional manifold, and so is the union of any number of non-intersecting circles. (2) A sphere—e.g., the Earth—hardly is distinguishable from a plane when standing very close to it; mathematicians say that a sphere is locally isomorphic to the plane, thus it is a two-dimensional manifold.
(3) Einstein's general relativity explains gravity by assuming that space-time is a curved four-dimensional manifold.

the subset of S that contains all the elements b that are equivalent to a, *i.e.* all b's in S that are such that $b \sim a$.[34]

With the above definitions, one is now in position to describe Thom's cobordism theory. He defined two manifolds M and N, both of dimension n, to be *cobording* (in French, *cobordantes,* from *bord* = "edge") if there is a manifold P of dimension $n+1$ so that M and N form its edge. He then showed that cobording manifolds formed an equivalence class. For example, one circle is cobording with the manifolds consisting in the non-intersecting union of two circles, because it is possible to unite them with the pants-shaped two-dimensional manifold with edges.

Thom realized then that the set Ω^n of all these equivalence classes formed a group, the group operation being defined as the non-intersecting union of manifolds representing the equivalence class. Moreover, exploiting modern formalism (homology, homotopy, and orthogonal Lie groups), and with the help of Jean-Pierre Serre, Thom identified the structure of those groups as being that of usual groups. He found that (Thom also provided partial results for higher dimensions):

$$\Omega^0=\mathbf{Z};\ \ \Omega^1=\Omega^2=\Omega^3=0;\ \ \Omega^4=\mathbf{Z};\ \ \Omega^5=\mathbf{Z}_2;\ \ \Omega^6=\Omega^7=0.$$

It is worthwhile to note that if M is cobording with N, then it is possible for M to evolve in time and become N. Thus cobordism is the study of possible continuous transformations of a given shape. Retrospectively, Thom also saw it similarly: "The

[34] For example: (1) the ordinary equality = between numbers is one such equivalence relation; (2) every integer is either even or odd, we can define an equivalence relation so that for a and b in $\mathbf{Z}$, $a \sim b$ if both numbers are odd, or both are even. We thus get two equivalence classes: [0] and [1], which are, respectively, the set of all even, and odd,

problem of cobordism . . . is of knowing when two manifolds can be deformed one into the other without encountering a singularity in the resulting space, at any *moment* in this deformation."[35] The example of a circle becoming two circles can, very crudely of course, model cell division.

b) The Mathematical Background of Catastrophe Theory

But we have got ahead of ourselves. In 1946, after his *agrégation,* René Thom moved from Paris to Strasbourg with a stipend from the *Centre national de la recherche scientifique* (CNRS). From 1946 to well into the 1950s, the Alsacian capital hardly corresponded to the provincial exile that French professors had to endure before, if successful, they could trek back to Paris. In addition to Thom's thesis director Henri Cartan being there, Charles Ehresmann directed a *séminaire de topologie,* where several renowned foreign mathematicians were invited. There Thom heard Hassler Whitney (1907-1989) present his work on singularities of mappings from the plane to the plane in 1950.[36] Thom also became acquainted with Morse theory, named after the American mathematician Marston Morse (1892-1977), who studied the relation between the topology of spaces and the singularities of real functions defined on them.[37]

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numbers. The set $\mathbf{Z}_2=\{[0], [1]\}$ with the addition defined as such $[a]+[b]=[a+b]$, is also a group defined in Chapter II.

35 R. Thom, "Exposé introductif," 27. My emphasis.

36 R. Thom, "La vie et l'oeuvre de Hassler Whitney," *Comptes-rendus de l'Académie des sciences – La vie des Sciences,* 7 (1990): 473-476.

37 Thom's first published article was on Morse theory: "Sur une partition en cellules associée à une fonction sur une variété," *CRAS,* 228 (1949): 973-975. His major work on cobordism made good use of Morse theory as well.

From his stay in Strasbourg, Thom therefore drew resources that were congenial to his attack on the problems of *singularity theory,* which, with Morse and Whitney, he can be considered as having founded. Just like "living beings," Paul Montel wrote in 1930, "functions are characterized by their singularities."[38] Montel considered that the study of their *singular points* allowed to investigate the individual characteristics of functions. For Thom, trying to make sense of multi-dimensional spaces, singular points were a blessing. He once discussed "a philosophical aspect" motivating the emphasis placed on their study in a way that clearly shows his topological intuition. "A space is a rather complex thing that is difficult to perceive globally." It was however possible to project it on the real line in order to study its structure. "In this flattening operation, the space resists: it reacts by creating singularities for the function. The singularities of the function are in some sense the vestiges of the topology that was killed: . . . its screams."[39] In 1955, he published his first article on singularities, which as we shall see in Chapter VI underlay most of his research activities for the following years. Thom knew that he had found a great topic: "There is hardly any doubt, in conclusion, that the study of the local properties of singularities of differential applications opens the door to an extremely rich domain."[40] From his work on singularity theory, Thom adapted mathematical tools that would help him develop catastrophe theory: the concepts of *genericity* and of *structural*

[38] P. Montel, "Sur les méthodes récentes pour l'étude des singularités des fonctions analytiques," *Bulletin des sciences mathématiques,* 2nd ser., 56 (1932): 219-232; 219.
[39] R. Thom, "Exposé introductif," 26.
[40] R. Thom, "Les singularités des applications différentiables," *Annales de l'Institut Fourier de Grenoble,* 6 (1955-56): 43-87, 87.

stability, as well as a classification of singularities which would later become a list of the seven *elementary catastrophes.*[41]

The concept of genericity, used by Italian algebraic geometers since the beginning of the century, became a crucial mathematical tool of catastrophe theory. Thom had spent the 1951-52 academic year at the Graduate College in Princeton. In the spring, he met the Bourbaki Claude Chevalley, who was then at Columbia. The idea of extending the use of genericity to differentiable structures dates from a "memorable discussion" he had with him. "I quickly perceived that this phenomena of 'genericity' was an essential source for our present worldview."[42]

In 1960-61, Thom spent a year in Baltimore with the nonlinear dynamics group, which, under the direction of Solomon Lefschetz, was reviving interest in the qualitative study of ordinary differential equations. In particular, Lefschetz had introduced the concept of *structural stability* from Russia.[43] This concept also was central for the development of catastrophe theory, the title of Thom's first book being *Structural Stability and Morphogenesis.* The conjunction of mathematical concepts of genericity and structural stability would guide Thom's research program in singularity theory, as he arrived at the IHÉS in 1964. They would also form the mathematical technology used for

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[41] R. Thom, "Les singularités des applications différentiables," *Séminaire Bourbaki,* 8 (May 1956), exposé #134.
[42] R. Thom, "Mémoire de la théorie des catastrophes," in R. Thom, M. Porte and D. Bennequin, *La genèse de formes.* Thom Arch.
[43] R. Thom, "Exposé introductif," 31. On Lefschetz, see A. Dahan Dalmedico, "La renaissance des systèmes dynamiques aux États-Unis après la deuxième guerre mondiale: l'action de Solomon Lefschetz," *Rendiconti dei circolo matematico di Palermo,* ser. II, Supplemento, 34 (1994): 133-166; and Chapter V below.

the foundation of the modeling practices he promoted with catastrophe theory. As we shall see later, he had by then already started to look at possible applications in physics.

(i) Mathematical Interlude II: Singularity Theory

The projection that René Thom described in vivid terms to show the importance of the study of singularities (p. 127) was called a Morse function. It was a smooth mapping f from an n-dimensional manifold M to the real line $\mathbf{R}$, satisfying some additional technical property. As Thom conveyed, one of Morse's crucial results allowed "the determination of the relations between the topological characteristics" of M and the singular points of f.[44]

Thom started to be interested in the properties of the set of singularities of multivariable functions, during the summer of 1955.[45] Consider a smooth differentiable mapping f from $\mathbf{R}^m$ to $\mathbf{R}^n$, or more generally from an m-dimensional manifold M to an n-dimensional manifold N. Then, a point p in M is a *singular point* of f if there is a direction along which the derivative of f at p vanishes.[46]

The name of the game then was, as often in modern mathematics, to classify and characterize singularities. For an arbitrary mapping f and arbitrary manifolds M and N, the

[44] M. Morse, "The Calculus of Variation in the Large," *Collected Papers,* 423-438, 423; and M. Morse, *The Calculus of Variation in the Large,* AMS Colloquium Publications, 18 (New York: AMS, 1934). See also the famous textbook by J. Milnor, *Morse Theory,* Annals of Mathematics Studies, 51 (Princeton University Press, 1963).
[45] R. Thom, "Les singularités des applications différentiables." See B. Teissier, "Travaux de Thom sur les singularités," *Publications mathématiques de l'IHÉS,* 68 (1988): 19-25; A. Haefliger, "Un aperçu," *Ibid.,* 16.
[46] For example, for a usual function f: $\mathbf{R}\to\mathbf{R}$, singular points of f are points p where the derivative of f vanishes [$f'(p) = 0$]; they can be local minima, local maxima, or flat inflection points (such as x=0, for $f(x)=x^3$).

classification problem was very hard.[47] Thom limited his study to low-dimensional spaces *M* and *N,* and to *structurally stable* mappings, which means that they keep the same topological character for a small perturbation of the mapping. He hoped structurally stable mappings to be very common, so that every mapping was either stable or very close to one: in mathematical parlance, they were *generic.*

In the above example of real functions, a generic singular point p was such that the second derivative of f at p was nonzero: $f''(p) \neq 0$. Morse theory showed that using an appropriate change of variable $x \to y(x)$, such that $y(p) = 0$, then f could be written as $f(y) = \pm y^2$ in a small neighborhood of the singular point p. This completely classified the generic singular points for real functions: there was, essentially, only one kind of singularity that could occur. In Thom's language, this singularity would soon be defined as a catastrophe called the *fold.*

Whitney completely classified the singularities that "a good approximation" of any mapping from the plane to the plane were allowed to have.[48] This can be visualized as follows. Imagine a surface S (a sheet for example) that we project on a plane underneath it. The surface S is just a different parametrization of the plane. So, we are faced with Whitney's problem: find the generic singularities of f: $\mathbf{R}^2 \to \mathbf{R}^2$. Often, there is no problem; there is a one-to-one correspondence between the points of S and those of the plane below. But, it might happen that you have a fold, close to which two points from the

[47] See H. Whitney, "Singularities of Mappings in Euclidean Spaces," *Symposium internacional de topología algebraica* (Mexico City: Universidad Nacional Automa de México & UNESCO, 1958): 285-301.

surface are projected onto the plane no matter how close to the fold you get; this is a singularity. You might even encounter isolated points around which, locally, three points of *S* are projected onto the plane; these are *cusp* singularity. These two are the only local singularities that would survive small readjustments of the sheet, *i.e.* perturbations of *f.*

Thom's elementary catastrophe theory basically extended this classification to higher dimensions, but with a slight difference. In *Structural Stability,* Thom recognized that the essential characteristics of a smooth function could be analyzed by studying its embedding into a smooth family of functions. He called this family of functions $F(x,u)$, such that $F(x,0) = f(x)$, an *unfolding* of the function f [here, x and u are multidimensional vectors]. "The goal of catastrophe theory is to detect properties of a function by studying its unfoldings."[49]

There were an infinite number of unfoldings for a given function f. The question was to know if there was one capturing the essential information about all unfoldings of f. Such an unfolding, when it existed and the number of dimensions of the variable u was minimal, was called *universal.* The fold and the cusp, discussed above, were universal unfoldings of $f(x) = x^3$ and x^4, respectively.

Consider a (physical) system whose dynamics is controlled by a potential function $V(x)$, where x describes the state of the system.[50] If friction forces are large enough, the

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48 H. Whitney, "On Singularities of Mappings of Euclidean Spaces. I. Mappings of the plane into the plane," *Annals of Mathematics,* 62 (1955): 374-410; repr. in *Collected Papers* (Berlin: Birkhäuser, 1992): 370-406.
49 D.P.L. Castrigiano and S.A. Hayes, *Catastrophe Theory.* See *SSM,* 29-34; and *MMM,* 59-77.
50 Gradient dynamics was already considered as an application in R. Thom, "Généralisation de la théorie de Morse aux variétés feuilletées," *Annales de l'Institut Fourier,* 14, no. 1 (1964): 173-189; 188-189.

state of the system x should always be very close to a minimum of V, that is, a singular point. Imagine a ball sitting at the bottom of a valley. Suppose that $V(x) = x^3 + ux$. Then the only stable equilibrium position was at $x = |u/3|^{1/2}$, for u negative. If, however, u varied, or in other words, if there was an internal control parameter slowly, but continuously varying, the state of the system could suddenly change drastically. Indeed, as u approached 0, the minimum became flatter until it vanished at u=0 . . . at which point the *catastrophe* occurred and the ball fell to infinity. The power of catastrophe theory is to say that, locally, every similar situation could be described by this simple potential.

The tricky part of this program was to find universal unfoldings. A heavy arsenal of functional analysis and algebraic topology was needed for Thom, Malgrange and Mather to be able to finally establish the list of seven catastrophes conjectured by Thom. In particular, they used the notion of map germs, and the jet theory of Charles Ehresmann, Thom's professor in Strasbourg.[51] I describe this work in more details in the institutional setting of the IHÉS in Chapter VI.

c) 'A Beautiful, Intriguing Field of Pure Mathematics'

The relationship between catastrophe theory and mathematics is a contested one. On the one hand, the mathematician John Guckenheimer aptly wrote that *SSM* "contains much of interest to mathematicians and has already had a significant impact upon mathematics, but

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[51] See R. Thom, "Sur la théorie des enveloppes," *Journal de mathématiques pures et appliquées,* 9e sér., 41 (1962): 177-192. His reference for jet theory is C. Ehresmann, *Introduction à la théorie des structures infinitésimales et des pseudo-groupes de Lie,* Colloque CNRS, 52 (1953). See R. Thom, "La théorie des jets et ses développements ultérieurs," in C. Ehresmann, *Œuvres complètes et commentées,* 1, ed. A. Ehresmann, *Cahiers de topologie et géométrie différentielle,* suppl. 1 and 2 (Amiens, 1984): 523-525.

[it] is not a work of mathematics."[52] On the other hand, authors of recent textbooks often feel the need to stress its mathematical nature. One started by emphasizing that "Catastrophe theory is a branch of mathematics."[53] Another asserted that this branch had in fact been "discovered" by Whitney in 1955, and transformed "into a 'cultural' tool" by René Thom.[54]

Historically, it is indeed true that Thom's mathematical experience made catastrophe theory possible and shaped the outcome of Thom's theory of modeling practice. As early as 1967, he divided catastrophes into two categories on the basis of his mathematical knowledge: the seven *elementary catastrophes* arising in simple systems; and *generalized catastrophes,* which lived in more complex spaces.[55] Recall that catastrophes were abrupt changes caused by smooth variations of the internal conditions of a system. Generalized catastrophe arose when there was loss of a global symmetry in the system. Thom wrote very little about them, since the mathematical basis for their classification was lacking. As for elementary catastrophes, they were those sudden discontinuities that occurred in systems whose dynamical behavior was controlled by a gradient (or potential). The image "of a ball rolling around a landscape and 'seeking' through the agency of gravitation to settle in some position which, if not the lowest

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52 J. Guckenheimer, review of *SSM, Bulletin of the American Mathematical Society,* 79 (1973): 878-890. The title of this section is a quote from D. P. L. Castrigiano and S. A. Hayes, *Catastrophe Theory,* xii.
53 A. Majthay, *Foundations of Catastrophe Theory* (Boston: Pitman, 1985), 1.
54 M. Demazure, *Catastrophes et bifurcations* (Paris: Ellipse, 1989), 167.
55 R. Thom, "Une théorie dynamique," and *SSM.* See also Figure 1, for a list of the seven elementary catastrophes. For the date, R. Thom, "Problèmes rencontrés," 203.

possible, than at least lower than any other nearby" was offered by T. Poston and I. Stewart in order to help understand this dynamics.[56]

One of the most powerful results from singularity theory, which made catastrophe theory at all possible, was a complete classification of the elementary catastrophes that arose in a system described by less than four internal parameters. In this case, Thom conjectured that only seven elementary catastrophes existed: the *fold, cusp, swallowtail, butterfly,* and the three *umbilics.* By the early 1970s, this conjecture was fully proved by Bernard Malgrange and John N. Mather.[57] It would later be widely known as "Thom's theorem." Elementary catastrophe theory made it certain that, if the above conditions were fulfilled (gradient dynamics and a small number of parameters), the abrupt changes in the system, unless not generic, had to be locally described by one of Thom's elementary catastrophes.

While Christopher Zeeman's exploitation of Thom's theorem made the international fame of catastrophe theory, it barely touched on Thom's own vision for his theory. Too tight a focus on this theorem betrays his philosophy and misses the point of his most important innovations for the practice of modeling, a fact that was recognized by some catastrophists: "It is not Thom's *theorem,* but Thom's *theory,* that is the important

[56] T. Poston and I. Stewart, *Catastrophe Theory and its Applications* (London: Pitman, 1978), 2.
[57] Two recent books are essentially dedicated to a pedagogical reproduction of this proof: M. Demazure, *Catastrophes et bifurcations* and D.P.L. Castrigiano and S.A. Hayes, *Catastrophe Theory.* T. Poston and I. Stewart present an intermediate-level explanation of the notions that articulate this theorem, see their chapter 7 in *Catastrophe Theory,* 99-122.

thing: the assemblage of mathematical and physical ideas that lie behind the list of elementary catastrophes and make it work."[58]

Thom emphatically concurred with this view. He granted that his philosophy was made possible by new advances in topology and that mathematical concerns importantly shaped his theory.[59] Surely, qualitative mathematics, some of which he had contributed to develop, some of which was cruelly lacking for the moment, were, or would have been, quite beneficial for catastrophe theory. But, generally speaking, these mathematical tools were just one of the facets of the general method of scientific inquiry that was catastrophe theory.

> Catastrophe theory is a not a theory that is part of mathematics. It is a mathematical theory to the extent that it uses mathematical instruments for the interpretation of a certain number of experimental data. It is a hermeutical theory, or even better, a methodology, more than a theory, aiming at interpreting experimental data and using mathematical instruments whose list is, for that matter, not *a priori* defined.[60]

Catastrophe theory was, in Thom's view, more philosophical than mathematical. This philosophy was grounded in part in Thom's mathematical practice. The most casual reading of Thom's work reveals that his thought was framed by mathematical language. His emphasis on shapes and qualitative theories can be directly traced back to his work on topology, where measurements are eschewed, and on singularity theory, where global properties can be extracted from the local study of critical points.

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[58] T. Poston and I. Stewart, *Catastrophe Theory,* 7.
[59] R. Thom, *SSM,* 159. See above, p. 166.
[60] R. Thom, *Paraboles et catastrophes,* 98. See also R. Thom, "Le statut épistémologique de la théorie des catastrophes," *Morphologie et imaginaire, Circé,* 8/9 (1978): 7-24; repr. *AL,* 395-410.

But Thom did not come up with catastrophe theory until he experimented with biological theories. These are at least as important as his mathematical practice in explaining catastrophe theory. In fact, it was from his reading of embryology textbooks that he came up with the notion of *attractor,* which figured so prominently in the modeling and experimental practice of chaos.

4. TOWARDS A THEORETICAL BIOLOGY ?

Overlooking beautiful Lake Como, in the village of Bellagio, Italy, stands Villa Serbelloni owned by the Rockefeller Foundation. There, on August 28, 1966, a select group of computer scientists, mathematicians, physicists, and, of course, biologists (but hardly any molecular biologist!) gathered in order "to explore the possibility that the time [was] ripe to formulate some skeleton of concepts and methods around which Theoretical Biology [could] grow."[61] There also, René Thom presented a noted contribution where he introduced the notion of catastrophe. How did he come to be invited at a biology conference? What did he present exactly? And what relation did this have with catastrophe theory?

a) From Pure Mathematics to Theoretical Biology, 1960-1968

In 1963, consecration came for René Thom in the form of an offer by Léon Motchane, the founder of the IHÉS, to join the faculty of this research institution. There, Thom had no teaching obligation, and could devote most of his time to research. He accepted, but only slowly to move away from mathematics and venture into disciplines, like biology and linguistics. A definitive reason for this shift of interest probably does not exist. Maybe his new situation at the IHÉS had something to do with it: "I had more leisure time, I was less preoccupied by teaching and administrative tasks. My purely mathematical productivity seemed to be declining and I began to be more interested in the periphery, that is, to possible applications."[62] Perhaps he finally succumbed to a taste for philosophy that he had neglected since his *lycée* years because of the demands of a mathematical career.[63]

For all his success, Thom seemed to have found mathematics hard to practice, and somewhat insatisfying. "If you don't need to work in mathematics for a living you need much courage to do it, because, in spite of all, mathematics is difficult!"[64] He especially loathed putting the final touch to his papers, many of which remained as manuscripts in his files. As we shall see in Chapter VI, one thing is however certain: he did not immediately abandon all concern with pure mathematics. Throughout the 1960s, he published a few articles on singularity theory, in which he introduced many concepts that inspired more conventional mathematicians.[65] It is a sign of Thom's exceptional intuition that he was able to do so without always spending the time and energy necessary to present them with the polish that the generation brought up by Bourbaki asked for.

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[61] C. H Waddington, Preface, *Towards a Theoretical Biology,* 1.
[62] R. Thom, *Prédire n'est pas expliquer,* 27.
[63] "Lorque j'ai dit [à George Bruhat, sous directeur scientifique lors de sa Taupe] que je m'intéressais à la philosophie des mathématiques, dans la direction de Cavaillès et de Lautman, il a levé les bras au ciel en s'écriant: 'Surtout, passez-moi rapidement votre agrégation!'" R. Thom, *Prédire n'est pas expliquer,* 14, see also *Entretiens avec des mathématiciens (L'heuristique mathématique),* by Jacques Nimier (Villeurbanne: IREM, 1989), 96-97.
[64] R. Thom, "Exposé introductif," 27. He also said: "I never mistook myself for a mathematician," *Paraboles et catastrophes,* 29.
[65] R. Thom, "La stabilité topologique des applications polynomiales," *L'Enseignement mathématique,* II, 8 (1962): 24-33; and "Ensembles et morphismes stratifiés," *Bulletin of the American Mathematical Society,* 75 (1969): 240-284.

In any case, with the help of the physicist P. Pluvinage and his assistant M. Goeltzene from Strasbourg, Thom began in 1960 to experiment with caustics—those luminous outlines that are formed, for example, by sunlight in a cup of coffee.[66] Starting with a problem he approached for its mathematical interest, that is, the classification of generic singularities, he asked whether his models were general enough to find applications in physics. Still under Bourbaki's spell, very few mathematicians in France were then raising this sort of question, although they proclaimed the universality of their structures. With singularities proving so fruitful in mathematics, Thom wondered whether they would be just as useful in the study of the physical world.

Armed with a few instruments (a spherical mirror, a prism, a dioptometer), Thom, Pluvinage, and Goeltzene constructed several caustics and studied their perturbations. The rays reflected by the spherical mirror formed a luminous curve with a cusp: a singularity! "This cusp has the marvelous property of being stable. If the orientation of the light rays is slightly changed, one sees that the cusp subsists. *This is the physical effect of a theorem of mathematics.*"[67]

Having stumbled upon unexpected behavior in optics, Thom then turned to biology. The only explanation he gave for his new interest is a retrospective story. In 1961, he visited the Natural History Museum in Bonn. There, he hit upon a plaster model of the gastrulation of a frog egg. "Looking at the circular groove taking shape and then

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[66] About the catastrophe theory approach of caustics, see M. V. Berry, "Les jeux de lumières dans l'eau." *La Recherche,* 9(92) (1978): 760-768.
[67] Thom, *Prédire n'est pas expliquer,* 27. My emphasis.

closing up, I saw . . . the image of a cusp associated to a singularity. This sort of mathematical 'vision' was at the origin of the models I later proposed to embryology."[68]

Thom also recalled that around 1962-63 he was struck by the fact that some mathematical models in biology seemed to exhibit facets of his theories: first, a proposal by the physicist Max Delbrück in 1949, to the effect that cell differentiation could be explained in terms of transitory perturbations of the cell's chemical environment; second, Christopher Zeeman's articles on the "Topology of the Brain," in which he pointed at the possibilities of using topology to model biological phenomena.[69] Further stimulation came from discussions with biologists among his colleagues (Philippe L'Héritier and Etienne Wolff) and with Zeeman, who frequently was visiting the IHÉS.

In his "Preface" to *SSM,* Thom singled out four biologists as his precursors. In addition to D'Arcy Thompson's (1860-1948) classic *On Growth and Form,* he mentioned two other "physiologists": Jakob von Uexküll (1864-1944) and Kurt Goldstein (1878-1965).[70] Thom found in these authors a way of treating organisms as wholes, a

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[68] Thom, *Paraboles et catastrophes,* 45. *Gastrulation* is the process by which the first internal layer of cells is formed in an animal embryo.

[69] Thom, "Exposé introductif," 30. M. Delbrück's comment in *Unités biologiques douées de continuité génétique* (Paris: CNRS, 1949): 33-34, transl. in *MMM,* 29-31. E. C. Zeeman, "Topology of the Brain", *Mathematics and Computer Science in Biology and Medecine,* sponsored by the Medical Research Council [Oxford, July 1964], (London: Her Majesty's Stationery Office, 1965): 277-292. Zeeman's work before and after he took up catastrophe theory will be examine in Chapter VI.

[70] R. Thom, *SSM,* xxiii. He cites J. von Uexküll, *Bedeutungslehre* (J. A. Barth, 1940), transl. *Mondes animaux et monde humain* (Paris: Gonthier, 1963); and K. Goldstein, *Der Aufbau des Organismus: Einführung in die Biologie unter besonderen Berücksichtigung der Erfahrungen am kranken Menschen* (Nijhoff, 1934), transl. *The Organism: A Holistic Approach to Biology Derived from Pathological Data in Man* (New York: American Book Co., 1939). Thom was struck by the latter's description of psychological pathologies as being "catastrophic," see the 1st French ed. of *SSM.* About Uexküll and Goldstein, see

nonreductionist approach to biology, which could provide mechanisms accounting for the finality of living beings. Above all, Thom was impressed by the writings of the fourth man he cited: British biologist Conrad Hal Waddington (1905-1975). Indeed, when Thom first introduced his theory of morphogenesis, he claimed that it stemmed for two sources:

> On the one hand, there are my own researches in differential topology and analysis on the problem called structural stability. . . . On the other hand, there are writings in Embryology, in particular those of C. H. Waddington whose ideas of 'chreod' and 'epigenetic landscape' seem to be precisely adapted to the abstract schema that I met in my theory of structural stability. [71]

This acknowledgment of Thom's—that his catastrophe theory derived also from biology, rather than having been just applied to it—was rarely taken seriously by those who commented on catastrophe theory. That all of them were mathematicians, and none of them biologists might explain this asymmetrical attribution. But it is at the interface with biology that Thom would develop a mathematical picture of competition between *attractors* in dynamical systems—a picture that would become one of the cornerstones of catastrophe theory, and beyond this, of chaos theory.

b) 'Wad' and the Synthesis of Biology

According to Waddington, the main problem of biology was to account for the characteristics that defined living organisms: form and end. "How does development produce entities which have Form, in the sense of integration or wholeness; how does evolution bring into being organisms which have Ends, in the sense of goal-seeking or

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A. Harrigton, *Reenchanted Science: Holism in German Culture from Wilhelm II to Hitler* (Princeton: Princeton University Press, 1996).

[71] R. Thom, "Une théorie dynamique," 152; *MMM,* 14.

directiveness?"[72] Organisms retained their shapes in spite of the fact that matter was continuously flowing through them. Development always ended up in the same final state, after having passed through the same stages. These problems of organization were the fundamental questions, only to be solved by a synthesis of evolution, embryology, and genetics.[73] Waddington believed that genes were the major causal factor for development, but at the same time never denied the influence of the rest of the organism. Thus, he thought that, while part of the answer lay in genetics, the main focus of study should be, not the genes themselves, but the nature of the causal relationship between the organism and its genes. For this science, he coined the name of *epigenetics.*[74]

Being "stuck" with a biological order "in which there [was] an inescapable difference between the *genotype*—what is transmitted, the DNA—and the *phenotype*—what is produced when the genotype is used as instructions," the epigenetician's task was to come up with mechanisms that could explain the phenotype in terms of the genotype.[75] But Waddington cautioned against careless oversimplifications. There was an "'atomistic' metaphysics" among geneticists: "It set out from the assumption of the existence of

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[72] C. H. Waddington, *The Strategy of the Genes: A Discussion of Some Aspects of Theoretical Biology* (London: George Allen & Unwin, 1957), 4, 9. On Waddington, see A. Robertson, "Conrad Hal Waddington," *Biographical Memoirs of Fellows of the Royal Society,* 23 (1977): 575-622; D. Haraway, *Crystals, Fabrics, and Fields: Metaphors of Organicisms in Twentieth-Century Developmental Biology* (New Haven: Yale University Press, 1976); and R. M. Ponsot, *C. H. Waddington ou l'évolution d'un évolutioniste,* thèse de doctorat (Université de Paris I, 1987), 3 vols.
[73] C. H. Waddington, *Principles of Embryology* (London: Allen & Unwin, 1956).
[74] C. H. Waddington, "The Basic Ideas of Biology," *Towards a Theoretical Biology,* 1: 1-32; 9. See also C. H. Waddinston, *Organisers and Genes* (Cambridge: Cambridge University Press, 1940).
[75] C. H. Waddington, "The Theory of Evolution Today," in *Beyond Reductionism: New Perspectives in the Life Sciences* [Alpach: 1968], ed. by A. Koestler and J. R. Smythies (London: Hutchinson, 1969): 357-395; 363.

single genes, and it asked, at first, what does *A* do and later, what controls whether gene *A* is active or not?"[76] But this approach did not work in general. "There is a whole series of processes in which the various genetic instructions interact with one another and interact also with the conditions of the environment in which the organism is developing."[77] For example, he had found that some 40 different genes affected the development of the wing of *Drosophila* (Fig. 5).

Epigenetics had two main aspects: changes in cellular composition (cell differentiation), and in geometrical form (morphogenesis).[78] In all cases the development of an organism followed definite pathways, always the same, and resistant to change. The description of these pathways and the genetic influences on them was thus a major task of epigenetics. Waddington introduced in 1939 an intermediary space between the genotype and the phenotype, which he called the *epigenetic landscape.* It combined, in a unique visual representation, all the development paths, which were pictured as valleys (Fig. 3).[79] The epigenetic landscape had no physical reality, but it helped visualize the various processes of development.

> Consider a more or less flat, or rather undulating, surface, which is tilted so that points representing later states are lower than those representing earlier ones [Fig. 3]. Then if something, such as a ball, were placed on the surface it would run down towards some final end state at the bottom edge. . . . We can, very

[76] C . H. Waddington, *The Evolution of an Evolutionist* (Edinburgh and Cornell University Presses, 1975); quoted by A. Robertson, "Waddington," 597-8.
[77] C. H. Waddington, "The Theory of Evolution Today," 364.
[78] C. H. Waddington, "The Basic Ideas of Biology," 11.
[79] S. F. Gilbert has examined the source of this idea: see his "Epigenetic Landscaping: Waddington's Use of Cell Fate Bifurcation Diagrams," *Biology and Philosophy,* 6 (1991): 135-154. The epigenetic landscape first appeared in *An Introduction to Modern Genetics* (New York: MacMillan, 1939), and was treated extensively in *Organisers and Genes,* and in *The Strategy of the Genes.*

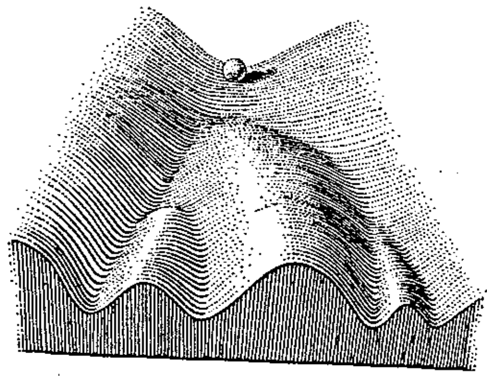


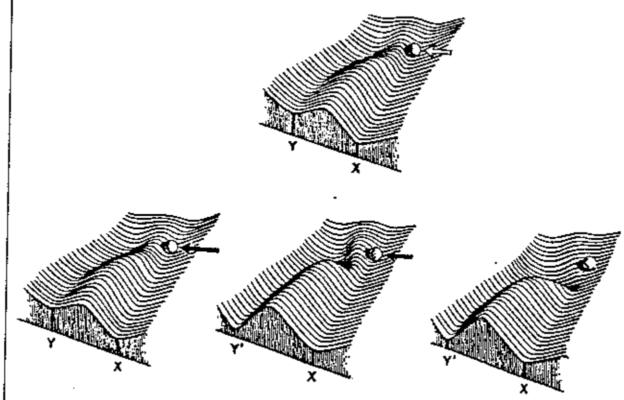
FIGURE 4
Part of an Epigenetic Landscape. The path followed by the ball, as it rolls down towards the spectator, corresponds to the developmental history of a particular-part of the egg. There is first an alternative, towards the right or the left. Along the former path, a second alternative is offered; along the path to the left, the main channel continues leftwards, but there is an alternative path which, however, can only be reached over a threshold.

Figure 3: Waddington's Epigenetic Landscape. Repr. from C. H. Waddington, *The Strategy of the Genes,* 29.

> diagrammatically, mark along its one position to correspond, say, to one eye, and another to the brain, [etc.].[316]

The image of the ball on a surface is of course reminiscent of the potential functions of catastrophe theory. Moreover, the canalizations formed on the epigenetic landscape had the property of being stable, in the sense that after a small perturbation in its trajectory, the ball tended to go back to the slope along the valley bottom. These stable pathways of change, Waddington called *creodes,* and later *chreods.*[317] They were the minimum points of a potential function unfolding in time. In his work on *Drosophila* during the 1930s, Waddington had studied the switches that can occur among several

[316] C. H. Waddington, *The Strategy of the Genes,* 29.
[317] From the "Greek roots *χρη,* it is necessary, and *οδος,* a route or path." C. H. Waddington, *The Strategy of the Genes,* 32.



'Organic selection' (the Baldwin effect) and genetic assimilation. The diagram above shows part of an epigenetic landscape, with a main valley leading to the adult character *X* and a side branch leading to *Y*; the developing tissue does not get into the *Y* path unless an environmental stimulus (hollow arrow) pushes it over the threshold. The three diagrams below show ways in which the 'acquired character' *Y* might become incorporated into the genotype. On the left, the original environmental stimulus is replaced by a mutant allele (dark arrow) which happens to turn up; this is 'organic selection'. On the right are two modes of 'genetic assimilation'. In the central one, the threshold protecting the wild type is lowered to some extent, but there is an identifiable major gene which helps push the developing tissues into the *Y* path. On the right, the genotype as a whole causes the threshold to disappear and there is no identifiable 'switch gene'. Note that in both the genetic assimilation diagrams there has been a 'tuning' of the acquired character, i.e. the *Y* valley is deepened and its end-point shifted from *Y* to *Y'*.

Figure 4: Switches in the Epigenetic Landscape. Repr. from C. H. Waddington, *The Strategy of the Genes,* 167.

development paths. In the sequence of events, if a gene was active at a particular moment then the eye had a different tint of red. At the switches an important phenomenon took place. The ball had to chose among several pathways (Fig. 4). René Thom would see in this a topological change occurring in the set of minima (singularities) that the potential function possessed: it was a *catastrophe!*

When Waddington organized the Bellagio Symposium, biology was in flux. The discovery of the structure of DNA by Watson and Crick in 1953 was becoming the measuring stick against which all biological models had to be tested. For molecular biologists, theoretical biology had to wait until one could provide it with the right molecular answers. Waddington also felt that answers to biological problems should be molecular. But more importantly, they should address the important questions of biology.[82] He hardly felt compelled to modify his epigenetic theories in view of molecular biology.[83] Bluntly, he asked: "Do you have to wait till you can reduce to the molecular biology of the dogma in a single leap or is there anything useful to do meantime?"[84] Finding something useful to do now was the goal of the Bellagio Symposium. For this, Waddington counted on the abilities of scientists from various fields. "After all," he wrote, "I am a biologist; it is plants and animals that I am interested in, not clever exercise in algebra or even in chemistry."[85] After the introduction of Thom's theory, Waddington would proudly recall that as early as 1940 he had called for a "biologically useful topology."[86]

c) Dynamical Theories of Morphogenesis

In his "dynamical theory of morphogenesis," Thom introduced a biochemical model of cellular differentiation. The problem of accounting for differentiation had puzzled many generations of embryologists. Independently, Waddington and Delbrück proposed that gradients in the concentrations of some postulated chemical substance might account for the phenomenon.[87] In their schemes, the cell (or its enzymes) was constantly processing chemical substances so that the different concentrations changed in a complex way—

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[82] C. H. Waddington, "Theoretical Biology and Molecular Biology," *Theoretical Biology,* 1, 104.
[83] C. H. Waddington, *The Strategy of the Genes,* 10.
[84] C. H. Waddington, "Theoretical Biology and Molecular Biology," *Theoretical Biology,* 1, 103.
[85] C. H. Waddington, *The Evolution of an Evolutionist,* quoted in A. Robertson, "C. H. Waddington," 599.
[86] C. H. Waddington, *Organisers and Genes,* 132.
[87] See "Correspondence Between Waddington and Thom," *Theoretical Biology,* 1, 166-179.

given by coupled, nonlinear equations. In a biological system, a *flux equilibrium* was eventually reached, that is, concentrations remained stable even though chemical substances always flowed through the cell. Waddington and Delbrück considered that there were several stable regimes that the system could achieve. The classification of these stable regimes became, in Thom's scheme, the description of the morphologies of the system. Hence one of his most innovative ideas: *to consider a system, even physical ones, in terms of the different end points it can achieve,* which he translated as a study of forms in nature. It expressed in a mathematical language adapted to the physical sciences the concept of finality in biology.

Thom called these different stable regimes of the system, *attractors.* They were region of the configuration space that were stable under the dynamical equations of the system—i.e. once you are in this region, you cannot get out—and such that any configuration close enough to an attractor would approach it asymptotically. The *basin of the attractor* was a region containing the attractor inside of which any initial condition fell back to it. Of course, Thom was well aware that to achieve a complete topological description of attractors and basins of a general system would be a difficult, but imaginable, task.

In the case of a local system, where, e.g., the concentration of chemical substances was given at each point of space and time, the attractors could well differ from point to point. Thus the domain of space that was under study—e.g., the cell—was divided in several regions associated with different attractors. These regions were separated by surfaces that Thom called "shock waves." Using Thom's theorem, he could establish that in the case of gradient dynamics, these separating surfaces could only present a small number of singularities, which were elementary catastrophes.

He thus introduced the following global scheme. Starting with a local singular situation in a dynamical system, he could say that ulterior catastrophes were contained in the "universal catastrophe space" associated with the singularity. For example, if one started with a local critical cusp situation, the only other catastrophes that could occur later in time were folds. Of course, all of this was local in a topological sense: it meant that, between the cusp and some finite limit in time, only folds could be encountered. But there was no way of knowing how large this limit was; it could be as small as one wishes as long as not zero. It could even be impossible to detect; hence Thom's reluctance to accept that catastrophe theory could be submitted to experimental control (see below).

In his theory, Thom saw "a mathematical justification for the idea of 'epigenetic landscape', suggested 20 years ago by Waddington."[88] This was not a mere gesture; the ideas of attractors and of conflict between attractors had been almost word for word described by the biologist .

> [1] At each step [of development] there are several genes acting, and the actual development which occurs is the result of a balance between opposing gene-instigated tendencies. [2] At certain stages in the development of an organ, the system is in a more than usually unstable condition, and the slightest disturbances at such times may produce large effects on later events. . . . [3] An organ or tissue is formed by a sequence of changes which can be called the 'epigenetic paths'. . . . And also each path is 'canalized,' or protected by threshold reactions so that if the development is mildly disturbed it nevertheless tends to regulate back to the normal end-result.[89]

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[88] R. Thom, "Une théorie dynamique," 158; transl. in *MMM,* 19.
[89] C.H. Waddington, *Organisers and Genes,* quoted in A. Robertson, "Waddington," 593.

Although he hardly knew enough mathematics, Waddington agreed with Thom. He claimed that Thom had "shown how such ideas as chreods, the epigenetic landscape, switching points, etc.,—which previously were expressed only in the *unsophisticated language of biology*—can be formulated more adequately."[90] Both Thom and Waddington clearly saw the advantages of having each other's theories reinforcing their own.

Thom not only proposed models for cell differentiation, but also for morphogenesis, hence the title of his book, and other biological processes (regulation, reproduction, predation, etc.).[91] In all of these cases, changes in the shape of an embryo, or some of its parts, were interpreted as arising from elementary catastrophes. Since all of his models were fairly undeveloped, and apparently *ad hoc,* their usefulness could be questioned. Moreover, Thom seemed to indulge in teleology, an accusation that he did not reject.

> But, I hope to have shown the following: even if you allow yourself all of the facilities of teleological thinking, you are still very far from explaining development. For embryology is full of enigmatic structures, of transient morphologies, which do not seem to have the slightest usefulness.

Catastrophe theory provided an explanation of these structures, by describing, independently of DNA, "the basic and universal constraints of stability imposed on

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[90] C. H. Waddington, "Foreword," *SSM,* xxi. My emphasis. See also his "The theory of Evolution," 367.

[91] R. Thom, *SSM,* ch. 9-11, 161-279; R. Thom, "Topological Models in Biology," in *Theoretical Biology,* 3 (1970): 89-116; also in *Topology,* 8 (1969): 313-335; R. Thom, "A Global Dynamical Scheme for Vertebrate Embryology," *Lectures on Mathematics in the Life Sciences,* 5 (1973), Mathematical Questions in Biology IV: Proceedings of the Sixth Symposium on Mathematical Biology [December 1971]: 1-45.

epigenetic mechanisms."[92] Thom therefore never answered the question that prompted Waddington in imagining epigenetic landscapes and chreods, that is, the link between development and genetics. Contentious, Thom went on: "only a mathematician, a topologist, could have written [this article], and the time may be very near when, even in biology, it might be necessary to think."[93]

It is striking to contrasts Thom's writing on biology with another famous French scientist whose work would in the early 1970s reach a broad audience, namely molecular biologist and Nobel-Prize winner Jacques Monod. A chapter of *Chance and Necessity,* first published in 1970, was devoted to the problem of spontaneous morphogenesis of living organisms. But the picture Monod presented was almost totally opposed to Thom's. Indeed Monod explained his aims as such:

> In this chapter I wish to show that this process of spontaneous and autonomous *morphogenesis* rests, at bottom, upon the stereospecific recognition properties of proteins; that is *primarily a microscopic process* before manifesting itself in macroscopic structures. . . . But we must hasten to say that this "reduction to the microscopic" of morphogenetic phenomena does not yet constitute a working theory of phenomena. Rather, it simply set forth the principle in whose terms such a theory would have to be formulated if it were to aspire to anything better than simple phenomenological description.[94]

As opposed to Thom's reduction of morphogenetic processes to a certain mathematical idealism, Monod argued for the "principle" of reducing them to molecular

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[92] R. Thom, "A Global Scheme," 44.
[93] R. Thom, "A Global Scheme," 44.
[94] J. Monod, *Chance and Necessity* (New York: Knopf, 1971), 81 and 88. My emphasis. On Monod's work in molecular biology, see A. Creager and J.-P. Gaudillière, "Meaning in Search of Experiments and Vice-Versa: The Invention of Allosteric Regulation in Paris and Berkeley, 1959-1968." *Historical Studies in the Physical and Biological Sciences,* 27 (1996): 1-90.

interaction. As the quote above indicates, this was nothing more than a "principle," and certainly not a full theory. But Monod put a great deal of faith in this principle. "

> I for my part remain convinced that only the shape-recognizing and stereospecific binding properties of proteins will in the end provide the key to these [morphogenetic] phenomena. . . . In a sense, a very real sense, it is at the level of chemical organization that the secret of life lies, if indeed there is any one such secret.[95]

Emphasizing the molecular and chemical properties of the substratum, the forces acting between organic macromolecules, and quantitative studies of them, Monod's discourse strikingly sound as an anti-Thom one, or conversely, Thom's as an anti-Monod diatribe.[96] Just as uncompromising, René Thom emphasized that no theoretical explanation was conceivable in biology without the aid of mathematics.

> In such a view of scientific explanation, there should not exist other theorization than mathematical; concepts used in each discipline, not susceptible of gathering a consensus around their use (let us think, for example, of the concept of information in Biology), should be progressively eliminated after having fulfilled their heuristic function. In this view of science, only the mathematician, who knows how to characterize and generate stable forms in the long term, has the right to use (mathematical) concepts; only *he, at bottom, has the right to be intelligent.*[97]

In this context, one is hardly surprised by the fact that Thom's theory had, in the long run, little impact on biology.[98] However, his forays into embryology provided Thom with crucial intuition about ways to study dynamical systems with finality. In no small

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[95] J. Monod, *Chance and Necessity,* 89 and 95.
[96] Note, however, that neither Thom nor Monod mentioned the work of the other in their writings. Indeed, Monod wished to counter vague approaches based on "general systems theory." *Chance and Necessity,* 80.
[97] R. Thom, "D'un modèle de la science à une science des modèles," *Synthèse* (1975): 359-374.
[98] See F. Gail, Françoise, "De la résistance des biologistes à la théorie des catastrophes," *Logos et théorie des catastrophes,* ed. J. Petitot (Geneva: Patiño, 1988): 269-279. One

sense, his introduction of the concepts of attractor, and the even more important concept of the basin of an attractor, can be seen as stemming from his involvement in biology. I shall come back to these issues in Chapter VI and VII, and show the ways in which these two concepts and the practices of their use were adapted to the study of physical systems.

5. TOPOLOGY AND MEANING

Having pointed out the relevance of topological concepts and practices for the modeling of biological phenomena, René Thom saw no reason to stop there. He himself proposed catastrophic models for the physics of phase transitions and geology.[99] Since the early 1970s, however, his main fields of research, besides philosophy, have been linguistics and semiotics. His evolution through these fields is the easiest to follow since the last chapters of *SSM,* devoted to them, kept changing from his 1966 manuscript to the 1977 French edition. A sequence of articles also shows his progression.

With his incursion into the human sciences, Thom was bound to confront structuralism. Never himself a structuralist *per se,* Thom was attracted by this movement. With some adjustments, his theories could be made to fit into structuralist modes of thought. But, since he began to work on linguistics so late, catastrophe theory was only mildly affected by structuralism in practice. In those years, however, increasingly faced with a strong opposition to his ideas about modeling, Thom also began to ponder the epistemological foundations of catastrophe theory, as well as the philosophy of science in

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may note the more ambivalent position defended by François Jacob, "Le modèle linguistique en biologie," *Critique,* 30(322) (1974): 197-205.

[99] R. Thom, "Phase Transitions as Catastrophes," in *Statistical Mechanics: New Concepts, New Problems, New Applications,* ed. by S. A. Rice et al. (Chicago: University of

general. In his attempts at articulating the kind of knowledge that his theory was producing, Thom made the clearest usage of structuralist resources.

a) Man and Catastrophes

In his manuscript of *SSM,* Thom's chapter 13 is called "L'homme." It would be published with some substantial additions under the title "From Catastrophes to Archetypes: Thought and Language." The original chapter aimed at extending the techniques and assumptions of catastrophic models of morphogenesis to human thought processes and societies. He actually developed few of the models he suggested. Always a mathematical terrorist, Thom used mathematical notations and language only to express vague correspondences among neurological states, thoughts, and language.

His basic assumption was that there existed a few "functional chreods," later to renamed "archetypal chreods," which expressed simple biological actions: to throw a projectile, to capture something, to reproduce, etc. These chreods had been internalized in the human brain, whose mental activity (*activité psychique*) was identified with a dynamical system. By analogy with the epigenetic landscape, Thom postulated that this psychological system was divided among basins and attractors, the most important being stable chreods isomorphic to external ones, the latter playing a role in biology. "The sequence of our thoughts and our acts is a sequence of attractors, which succeed each other in 'catastrophes'."[100]

Chicago Press, 1972): 93-107; R. Thom, "Tectonique des plaques et théorie des catastrophes," *Astérisque,* 59/60 (1978), 205.

[100] R. Thom, manuscript for *SSM,* sect. 13.3.C.

Thom then claimed that language translated the mental attractors of our brain. There was a mental atlas of dynamic chreods that existed and was common to all human beings, and even to animals. An idea was expressed as a mental attractor. When one wished to formulate a sentence expressing an idea, it was mathematically projected onto a space of admissible sentences, where several attractors competed. One was eventually chosen, and the sentence was uttered. All of this was manifestly vague and programmatic. Thom needed to elaborate his ideas. He would do so after his encounter with structuralism. Conversely, he drew on structuralist thought to articulate the accomplishment of catastrophe theory and his own epistemology.

b) Language and Catastrophe

In the Parisian intellectual climate of the late 1960s, René Thom had to encounter structuralism, especially since he was thinking about the catastrophes of human languages. As early as 1968, he noted that "the problem of meaning has returned to the forefront of philosophical inquiry."[101] Since he saw this quest as one of Heraclitus's, this return of the sign pleased him. Nevertheless, semiotics was first introduced in Thom's work, not as a quest in itself, but as a method for biology. He had of course come upon the Saussurian notions of *signified* and *signifier.* In the context of his biological concerns, Thom considered them as congenial to the goals of epigenetics, which were to find the connections between genetics and embryology. "Is not such a discipline which tries to specify the connection between a global dynamic situation [the organism] (the 'signified'),

[101] R. Thom, "Topologie et signification," *L'Âge de la science,* 4 (1968); repr. in *MMM,* 166-191.

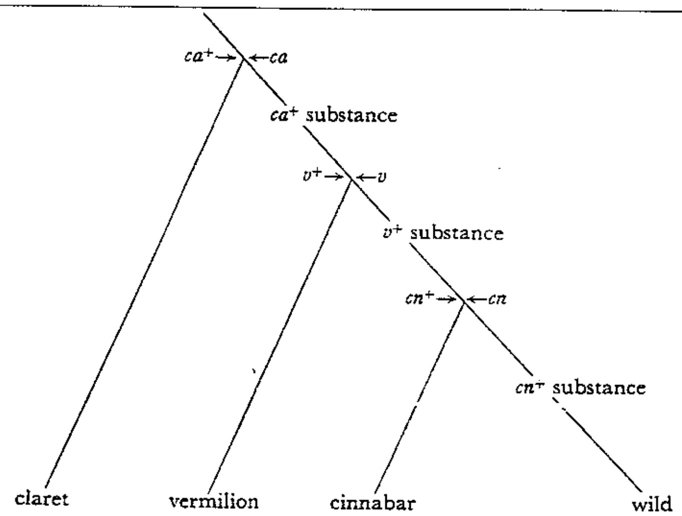

Fig. 7. The formation of eye colours in *Drosophila*. The pigment-forming process normally runs down the line through the ca^+ substance, the v^+ substance, and the cn^+ substance, to give wild type pigment. The genes, *ca*, *v* and *cn* interrupt this sequence, so that the process takes an altered course, to give claret, vermilion or cinnabar pigmentation.

Figure 5: Waddington's Switching Diagram. Repr. with permission from C. H. Waddington, *Organisers and Genes* (Cambridge, 1940), 77. Copyright © Cambridge University Press.

and the local morphology in which it appears [DNA] (the 'signifier'), precisely a 'semiology'?"[102] He expressed his whole method for catastrophe theory as a problem of semantics. "The decomposition of a morphological process taking place in $\mathbf{R}^m$ can be considered as *a kind of generalized m-dimensional language;* I propose to call it a *'semantic model'*."[103] He would later push this intuition further, but first, he noticed the analogy between the theoretical tools of structural syntax and epigenetics.

102 R. Thom, "Topologie et signification," *MMM,* 169.
103 R. Thom, "Topological Models in Biology," 103.

In 1970, René Thom presented a more sophisticated catastrophe-theoretical model of language.[104] His goal was to explain the syntactical structure of atomic sentences (basically, with one verb), in terms of their meaning. He was struck by the resemblance between the tree-shaped graphs that L. Tesnière used to analyze the structure of sentences, and Waddington's chreods (Fig. 5 and 6).[105] If indeed you strip the epigenetic landscape of the out-of-equilibrium position, you get a switching diagram, looking like a tree. In Tesnière's view verbs were sentences' centers of gravity. They became, in Thom's view, the catastrophic attractors of cerebral activities, words being chreods. He developed a visual representation of the verbs associated with spatio-temporal activities by using sections of elementary catastrophe surfaces. This was, he would say 20 years later, a "geometrization of thought and linguistic activities."[106] The main benefit of such an analysis was to establish a map from signified to signifier, which went against the Saussurian "dogma" about the arbitrariness of the sign. Classifying syntactical structures into 16 categories, Thom claimed that "The topological type of the interaction determines the syntactical structure of the sentence which describes it."[107] Meaning and structure were no longuer independent.

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[104] R. Thom, "Topologie et linguistique," *Essays on Topology and Related Topics (Dedicated to G. de Rham),* ed. by A. Heafliger and R. Narasimhan (Berlin: Springer, 1970): 148-177; repr. *MMM,* 192-213.

[105] L. Tesnière, *Élements de syntaxe structurale* (Paris: Klincksieck, 1965).

[106] R. Thom, *Semiophysics: A Sketch,* transl. Vendla Meyer (Redwood City: Addison-Wesley, 1966), viii.

[107] R. Thom, "Topologie et linguistique," *MMM,* 197. See a figure of his 16 archetypal types in *SSM,* 307.

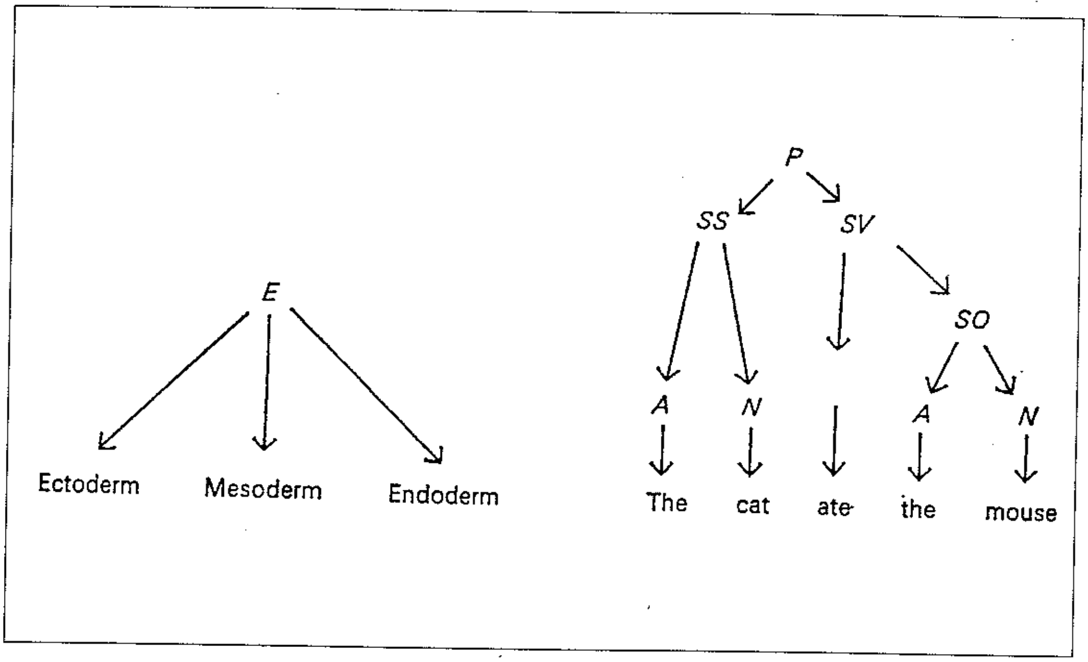

Figure 6: Thom's Analogy Between Graphs of Sentences and Development. Repr. with Permission from R. Thom, "Structuralism and Biology," *Towards a Theoretical Biology,* 4, ed. C. H. Waddington, 80. Copyright © University of Edinburgh Press.

Thom's theory of sentence construction went beyond common structuralist algebraic ideas. Against Chomsky, he noted that "of all the actantial schemes predicted by algebraic theory, only certain of them are realised in biological morphology, or in the syntax of a simple sentence." He thus asked: "In the light of what criteria is that 'choice' made?"[108] Simply, structures of sentences reflected the chreods of thoughts, themselves modeled on biological ones. They were dictated by Thom's idealistic exploitation of mathematics.[109]

[108] R. Thom, "Topologie et signification," *MMM,* 183.
[109] In Chapter VI, I describe in more details the modeling practice actually adopted by Thom in linguistics, and contrast it with that of other topologists, close to him, who used topology to model natural phenomena.

c) Structuralism and Biology

As we saw in Chapter II, Thom confronted structuralism head on in 1972: "Can structuralist developments in anthropological sciences (such as linguistics, ethnology, and so on) have a bearing on the methodology of biology? I believe this is so."[110] He indeed had a particular vision of what structuralism was—a view singularly reminiscent of his own modeling practice.

> The task of any structuralist theory is: (1) to form a finite lexicon of elementary chreods; (2) to build experimentally the 'corpus' of the empirical morphology [stable aggregations of frequent elementary chreod]; (3) to define 'conditional chreods', objects of the theory; (4) to describe the internal structure of a conditional (or elementary) chreod by associating a mathematical object to it, whose internal structure is isomorphic to the structure of the chreod.[111]

In Thom's view, his morphogenetic theories and structuralism reinforced each other. Molecular biologists were prone to interpret the living order in terms of the DNA code. For Thom, this was wrong-headed, because if indeed biology could be seen as a semantic model, it was a dynamical, multi-dimensional one. Language was a semantic model of dimension one: how could spatial processes of biology be described by it?[112]

In contact with the knowledge produced by structuralist linguistics, which was loudly defending its scientific character, Thom extracted a philosophy of science that would be up to the task of making sense of the knowledge his approach had produced, and not only in the human sciences. Henceforth, Thom would point at two approaches to

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[110] R. Thom, "Structuralism and Biology," *Towards a Theoretical Biology,* 4 (1972): 68-82, 68; transl. in first French ed. of *Modèles mathématiques de la morphogenèse* (1974), but absent from later eds.
[111] R. Thom, "Structuralism and Biology," 70.
[112] About a contemporary attempt at articulating a multidimensional structuralism, see a book by one of Thom's followers, P. Scheurer, *Révolutions de la science et permanence du réel* (Paris: PUF, 1979).

scientific knowledge that were susceptible of providing explanations for the phenomena of the world: the *reductionist* approach, and the *structural* one.[113] Following a principle of economy in science, both approaches aimed at simplifying the description of empirically observed morphologies, or natural phenomena. But the structuralist approach refused to do so by attributing causal effects to factors that were external to the empirical field. The only admissible causality was structural.

Thom had obviously modeled his "structural approach" on the linguists' claims to knowledge production. He viewed some human sciences as successful at building a nonreductionist theories, especially, formal linguistics and Lévi-Strauss's structural analysis of myths. They held a "paradigmatic value: they show the way in which a purely structural, morphological analysis of a empirical data can be engaged."[114] It would indeed be absurd, Thom contended following Lévi-Strauss, to base linguistics on reductionist assumptions. "It would consist in an attempt at explaining the syntactical structure of a sentence of words by an interaction of phonemes of a phonologic character."[115]

Thom could now articulate his own interpretation of the kind of knowledge produced by catastrophe theory. An *explanation,* he said, was "any theoretical process

[113] Thom calls this second approach: "*l'approche structurale.*" We must note a difference between French qualifiers: *structurel* (as in 'stabilité structurelle': simply the translation of an English phrase) refers to actual structures, while *structural* refers to structures as syntax, susceptible to be realized in several instances of actual structures. See J.-M. Auzias, *Clefs pour le structuralisme* (Paris: Seghers, 1967), 18.
[114] R. Thom, " La science malgré tout...," *Encyclopaedia universalis,* 17, Organum (1975), 6.
[115] R. Thom, "La linguistique, discipline morphologique exemplaire," *Critique,* 30(322) (March 1974): 235-245., 239.

whose result is to lessen the arbitrariness of description."[116] In practice, knowledge produced by catastrophe theory should be more economical than a simple description of facts. Like Piaget, Thom saw a serious epistemological problem in structuralism, namely that it could not account for the emergence of its structures, because, historically, structuralist linguistics was synchronic, i.e. static in time. But, Thom believed that nothing prevented linguists of conceiving time as another dimension of space-time: "we can make a structural theory of the changes of forms, considered as a morphology on the product space of the substrate space by the time axis."[117] Indeed, catastrophe theory provided a way for building a dynamic structuralism, which would explain the emergence of structure. Like he had done with Bourbakism, Thom used structuralist practices in order to undermine the very project of structuralism.

6. SHAPES, *LOGOI*, AND CATASTROPHES: THOM'S THEORY OF MODELING PRACTICE

The above have shown how Thom constructed a modeling practice which, roughly speaking, used topologically-informed means of transformation, biologically-inspired raw materials that he adapted to mathematical practice, and structuralist interpretations of the kind of knowledge produced by catastrophe theory. From the first version of *SSM* in 1966 to his publication of philosophical articles intended for a wide array of audiences, Thom also worked at what can best be termed as a theory of modeling practice. In the following, I shall describe its general gist and its philosophical undertones.

[116] R. Thom, "Rôle et limites de la mathématisation en sciences," in *La Pensée* (October 1977): 36-42.
[117] R. Thom, "La linguistique," 240.

(i) Nonreductionism

René Thom was among those who loudly contested the success of reductionist science. That science in the twentieth century had been mainly a reductionist enterprise was a commonplace. In their efforts to understand the world—or, more precisely, pursuing the Laplacian dream, to predict its future course—scientists have followed Jean Perrin's ideal: "to explain complex visible things with the help of simple invisible things."[118] Thom contended that this approach was far from having lived up to its promises. "The Universe is nothing more than a brew of electrons, protons, [and] photons," he wrote. "How can this brew settle down, on our scale, into a relatively stable and coherent form far from the quantum-mechanistic chaos?"[119] In raising this question, Thom was engaging the old debate of materialism vs. vitalism, mechanism vs. teleology, and more recently, reductionism vs. holism.[120]

For Thom, physicists overreached themselves when they claimed to be able to explain the everyday world. "Realization of the ancient dream of the atomist—to reconstruct the universe and all its properties in one theory of combinations of elementary particles and their interactions—has scarcely been started." Thom adamantly opposed dogmatic reductionism:

> this primitive and almost cannibalistic delusion about knowledge, [which demands] that an understanding of something requires first that we dismantle it,

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118 "Expliquer du visible compliqué par de l'invisible simple." J. Perrin, *Les Atomes* (Paris: Félix Alcan, 1913), Introduction.

119 R. Thom, "Topologie et signification," *MMM,* 174.

120 For the telelogy/mechanistic debate in 19th-century Germany, see T. Lenoir, *The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology* (Dordrecht: D. Reidel, 1982).

> like a child who pulls a watch to pieces and spreads out the wheels in order to understand the mechanism.[121]

But he did not altogether reject it. Reductionism was a valid approach to knowledge, but an imperfect one, which was unachievable at the practical level. The only place it worked, Thom claimed, was in the example of a perfect gas. In this case, however, "there is no morphology."[122]

(ii) Forms

René Thom's theory of modeling practice was indeed grounded on a study of morphology. "Reality presents itself to us as phenomena and shapes."[123] His program was to make the morphologies of our day-to-day reality the object of a dynamical science of shapes. In a given domain of experience, his modeling practice could be summarized as such: find out the shapes that are usually encountered; establish a list of these shapes, according to their topologic character; and find the underlying dynamics that governs their emergence and destruction.[124]

Thom took his cue from British biologist D'Arcy Wentworth Thompson, who had recognized the morphological problems arising in the physical sciences. Thompson confidently believed that physics was—roughly—up to the task of explaining these morphologies.

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[121] R. Thom, *SSM,* 159.

[122] R. Thom, *MMM* (1974 ed.), 23. Absent from later editions. More details in R. Thom, "Structuralism and Biology," *Towards a Theoretical Biology,* 4 (1972): 68-82; 73.

[123] R. Thom, *MMM* (1974 ed.), 9. Absent from later editions.

[124] Note that there is nothing absolute about the relation between the study of forms and nonreductionism. See the following historical study who focuses on the scientists' struggles to find molecular accounts of crystal shapes: N. E. Emerton, *The Scientific Reinterpretation of Form* (Ithaca: Cornell University Press, 1984).

> The waves of the sea, the little ripples on the shore, the sweeping curve of the sandy bay between the headlands, the outline of the hill, the shape of the clouds, all these are so many riddles of form, so many problems of morphology, *and all of them the physicist can more or less easily read and adequately solve.*[125]

Listing similar natural shapes, René Thom disagreed that traditional physics could do it:

> Many phenomena of common experience, in themselves trivial (often to the point that they escape attention altogether!) – for example, the cracks in an old wall, the shape of a cloud, the path of a falling leaf, or the froth on a pint of beer – are very difficult to formalize, but is it not possible that a mathematical theory launched for such homely phenomena might, in the end, be more profitable for science [than large particle accelerators]?[126]

Catastrophe theory, from the beginning, was thus an attempt at formalizing in rigorous mathematical language the dynamics of forms. And in *Structural Stability,* the first seven chapters gave an outline of a general theory of morphology, which would be applicable to all problems of shape.

(iii) The Mundane

It is one thing to focus on forms, it is quite another to focus on the specific ones listed above. But just as Thom questioned the pertinence, to the everyday world, of explanations in terms of electrons, he also noticed that science was quite unable to account for "the froth on a pint of beer," and many such things with which we are, paradoxically, so

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125 D. W. Thompson, *On Growth and Form* (Cambridge University Press, 1948 [1916]), 10; my emphasis. Note that Thom placed this quotation in front of his Introduction in *SSM,* 1. But he dropped the last part where Thompson so confidently asserts the success of physics.

126 Thom, *SSM,* 9. The allusion to particle accelerators was added for the second French edition of *Stabilité structurelle et morphogénèse* (Paris: InterÉdition, 1977), 10. See Chapter II above.

familiar. French mathematician Benoît Mandelbrot, the inventor of the notion of fractals, shared this concern. "Clouds are not spheres, mountains are not cones," etc.[127]

If Mandelbrot saw himself as a new Euclid, Thom thought that he was picking up a broken line of thought just where Heraclitus had left it. Around 500 BC, Greek philosopher Heraclitus already noticed the difference between knowledge and understanding. "Many people do not understand the sorts of things they encounter! Nor do they recognize them even after they have had experience of them, though they themselves think [so]."[128] In Heraclitus's fragments, Thom indeed found some inspiration for his own philosophy. When christening some of his elementary catastrophes *swallowtail,* or *butterfly,* Thom applied Heraclitus's precept to figures impossible to visualize in three-dimensional space.[129]

(iv) The Logos

Once the problem is posed as such: find a scientific description of natural forms, even though they arise from just a "brew of electrons," the next pressing question is about the *stability* of such forms at our scale. Returning to his quarrel with reductionist physics, Thom noticed that "although certain physicists maintain that the order of our world is the inescapable consequence of elementary disorder, they are still far from being able to furnish us with a satisfactory explanation of the stability of common objects and their

[127] B. B. Mandelbrot, "Towards a Second Stage of Indeterminism in Science," *Interdisciplinary Science Review* 12 (1987): 117-127,117. See Chapter II.
[128] Heraclitus, *Fragments,* transl. T. M. Robinson (University of Toronto Press, 1987), 19, fragment 17.
[129] "Whatsoever things are objects of sight, hearing, and experience, these things I hold in higher esteem." Heraclitus, *Fragments,* 39, fragment 55.

qualitative properties."[130] In other words, the physicists are not able to *understand* the morphologies of the world in terms of atoms.

For Thom, the explanation lay in an ideal mathematical structure.

> The stability of a form rests definitively upon a structure of algebraic-geometric character . . . endowed with the property of *structural stability* with respect to the incessant perturbations affecting it. It is this algebraic-geometric entity that I propose, recalling Heraclitus, to call the *logos* of the form.[131]

For Heraclitus, the λογος was the "true discourse according to which everything happens. It was the truth of this world."[132] Thom attributed a *logos* to each form; it was "a formal structure which insures its unity and stability." One may note here that he was indeed applying Jean Perrin's precept, except that Thom's "simple invisible things" were mathematical structures as opposed to atoms. For all his structuralist talk, Thom's philosophy is well captured by the term "neoreductionism" with which Giorgio Israel characterized von Neumann's approach.[133]

Thom soon felt that he had to emphasize that he studied morphology without regard to the substrate. In his manuscript, written in 1966, he had made no mention of this.[134] Coming from a topology background, he believed in the universal relevance of his mathematics. But after having presented his theory to an audience of biologists, he underscored its autonomy from specific material bases.

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[130] R. Thom, "Topologie et signification," *MMM,* 174.
[131] R. Thom, "Topologie et signification," *MMM,* 174-175.
[132] M. Conche, in Heraclitus, *Fragments* (Paris: Presses Universitaires de France, 1986), 65.
[133] G. Isreal, *La Mathématisation du réel* (Paris: Seuil, 1996), 198.
[134] R. Thom, *SSM,* manuscript, Fine Library, Princeton University, 13-14, and compare with *SSM,* 8-10.

> The essence of our theory, which is that a certain knowledge of the properties peculiar to the substrates of the forms, or the nature of the forces at work, may seem difficult to accept, especially on the part of experimenters.[135]

Again, Thom placed himself as heir to D'Arcy Thompson, who, "in some pages of rare insight, compared the form of a jellyfish to that of the diffusion of a drop of ink in water."[136] The only thing that Thompson lacked, Thom contended, was a formal foundation in topology, which, with the abstract structure of the *logos,* provided the basis for an explanation of morphogenesis without relying on material properties. Mathematics was the only external element that was called upon. The lesson was that there were other methods of knowing than pure materialist pursuit. Thus, if there was an idealistic trend in Thom's thought, it lay in a Platonic belief, common among mathematicians, in the existence of mathematical objects. "The hypothesis that Platonic ideas give shape to the universe," he wrote in 1970, "is the most natural and, philosophically, the most economical."[137]

(v) The Qualitative

There was a backdrop to this all-encompassing vision. Based on topology, which abandoned all reliance on geometric measure, Thom's method was not suited to numerical analysis, to measurement. It had to remain qualitative, and not quantitative. Traditionally, this was a serious problem for a theory. Thomas Kuhn wrote in 1969: for a scientist "probably [one of] the most deeply held values concern predictions; . . . quantitative

[135] R. Thom, "Une théorie dynamique," 153; *MMM,* 14.
[136] R. Thom, *SSM,* 9. D. W. Thompson, *On Growth and Form,* 72-73 (1961 ed.).
[137] R. Thom, "Modern Mathematics," 697.

predictions are preferable to qualitative ones."[138] On this respect, he concurred with Lord Kelvin's authoritative pronouncement:

> Where you can measure what you are speaking about and express it in numbers, you know something about it, and when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind. It may be the beginning of knowledge, but you scarcely in your thought advanced to the stage of science.[139]

René Thom nonetheless saw the qualitative aspect of catastrophe theory in a positive light. He thought that those who really wanted to understand the world had to rid themselves of "the intolerant view of dogmatic quantitative science."[140] By recalling Rutherford's dictate—"Qualitative is nothing but poor quantitative!"[141]—Thom wished to show the common prejudice against qualitative theories. But, "what condemns these speculative theories in our eyes," he wrote, "is not their qualitative character but the relentlessly naive form of, and the lack of precision in, the ideas they use." Now, he claimed, everything had changed since he could "present qualitative results in a rigorous way, thanks to recent progress in topology and differential analysis, for we know how to define a *form.*"[142] Catastrophe theory was the *rigorous* way to think about quality.

(vi) The Intelligibley

Intelligibility of the world was the benefit, and the ultimate goal, of René Thom's approach. Always contentious, he wrote: "One of the causes for the stagnation of science

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[138] T. S. Kuhn, "Postscript – 1969," *The Structure of Scientific Revolutions,* 2nd ed. (Chicago: Chicago University Press, 1970), 185.
[139] Quoted in S. A. Rice and P. Gray, *The Statistical Mechanics of Simple Liquids* (New York: Interscience, 1965), ix.
[140] R. Thom, *SSM,* 159
[141] R. Thom, *SSM,* 4, for example.
[142] *Ibid,* 159

is that science has basically forgotten its primary vocation, . . . which was to make us understand reality."[143] He defended Descartes against Newton.

> Descartes, with his vortices, his hooked atoms, and the like, explained everything and calculated nothing; Newton, with the inverse square law of gravitation, calculated everything and explained nothing.[144]

Again and again, Thom opposed explanation to prediction, intelligibility to control, understanding to action. But it is rarely clear exactly what he means by *explanation.* Ultimately, he believed that a theory would be totally intelligible when the theory itself would be able to decide on its own validity: "*a theory of meaning whose nature was such that the act itself of knowing is a consequence of the theory.*"[145] While Thom never claimed that catastrophe theory could live up to this feat, he nevertheless thought that it made the world more intelligible.

(vii) Hermeneutics

Thom often insisted that catastrophe theory was not a proper scientific theory. It was a language, a method. Nowhere was this more evident than when he confronted the delicate question of experimental control. He always admitted that an experiment that would falsify, or for that matter confirm, his theories was in principle impossible.[146] This problem was inherent to the qualitative nature of catastrophe theory. His theory could

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[143] R. Thom, " La science malgré tout...," *Encyclopaedia universalis,* vol. 17, Organum (1975), 6.
[144] R. Thom, *SSM,* 5.
[145] R. Thom, "Topologie et signification," *MMM,* 170. Italics in the original text.
[146] See his later debate with Abragam at the Académie des science apropos the experimental method, R. Thom, "La méthodologie expérimentale: un mythe des épistémologues (et des savants?)," *CRAS. Série générale: la vie des sciences,* 2(1) (1985): 60-68; repr. *La Philosophie des sciences aujourd'hui,* ed. J. Hamburger (Paris: Gauthier-Villars, 1986).

eventually provide the basis for the elaboration of a quantitative model, and as such, susceptible of experimental control. But in general, the mathematics needed to do so was not yet invented. And even if it were possible to analyze mathematically the dynamical processes that insured the stability of a form, "this analysis is often arbitrary; it often leads to several models between which we can only choose for reasons of economy or mathematical elegance"[147]

But, once again, according to Thom, this serious drawback was not fatal. He indeed saw at least two reasons to justify scientists' interest in his theory. First, catastrophe theory, as a language for science, questioned the traditional "qualitative carving out of reality . . . into the big disciplines: Physics, Chemistry, Biology."[148] The theory would integrate this taxonomy of experience into "an abstract general theory, rather than blindly accept[ing] it as an irreducible fact of reality."[149] Second, as a theory of modeling practice, catastrophe theory would substitute itself to the "lucky guess" that had hitherto based all model construction in science. "The ultimate aim of science is not to amass undifferentiated empirical data," he wrote, "but to organise this data in a more or less formalised structure, which subsumes and explains it."[150] On the path towards a "General Theory of Models," catastrophe theory showed the way of the future.

As a Theory of modeling practice, catastrophe theory therefore was a radical departure from prior views on model-building. To use his theory in constructing a

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[147] R. Thom, "Une théorie dynamique," *MMM,* 21.
[148] Le "découpage qualitatif de la réalité . . . en grandes disciplines: Physique, Chimie, Biologie." R. Thom, *SSM* (1972), 323. The English translation of *SSM,* 322, misses Thom's point here.
[149] R. Thom, *SSM,* 322.
[150] R. Thom, "Une théorie dynamique," *MMM,* 22.

scientific model meant, for Thom, to start with shapes, forms, and morphologies as they appear, to identify their topological features, and forbid oneself all unnecessary reliance on substrate and forces. It also meant to adopt sophisticated mathematical methods to develop an intelligible, qualitative model of the phenomena in question. Lastly, and perhaps more importantly, it meant to abandon all previous notions of quantitative knowledge, and embrace the idea that knowledge of the world could be gained by a qualitative description. We shall see in Chapter VI how these ideas were actualized by Thom and scientists close to him.

7. CONCLUSION

With catastrophe theory, René Thom believed that he was breaking away from centuries of reductionist thinking. He developed models for biology, linguistics, and semiotics displaying his vision of a holistic science. He introduced a new modeling practice and tried to codify its epistemological rules. Based on his mathematical experience, catastrophe theory used topology as a resource for grasping a world of qualities and shapes. Embryology suggested to him a new starting point for theory, namely the ends of a dynamic process: its morphology. Thom never argued for the intrinsic superiority of his method, but rather for its greater capabilities at explaining the world as we perceived it. The models produced by catastrophe theory were not supposed to reflect the world as it is, but to explain its structure in the most economical way, which Thom believed was the accomplishment of structuralism. Catastrophe theory provided "schemes of intelligibility. And this seems quite valuable to me."[151]

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[151] R. Thom, *Prédire n'est pas expliquer,* 45-46.

In developing catastrophe theory, Thom introduced important mathematical concepts and attempted to extend them beyond their rigorous limits. In doing so, his speculations were often rejected by mathematical communities. His insistence on denying the possibility of experimentation was met with suspicion by practicing biologists.[152] Finally, it was the non-genericity of structural stability for non-gradient systems which discredited the general ambitions of catastrophe theory. As for elementary catastrophe theory applied to the physical sciences, it did not seem to explain anything that was not already known.

Thom's program was however richer than just concepts, models, theorems and theories. His modeling practice presented some appealing aspects that would be taken up by 'chaologists'. Using Thom's concept of attractors and his geometric vision of dynamical systems, David Ruelle and Floris Takens showed in 1971 that the attractor that was usually assumed for turbulence was not structurally stable, and thus introduced the notion of *strange attractor,* which would found chaos theory.[153] But, contrary to Thom's philosophy, their prediction was successfully submitted to the verdict of experiments, in the laboratory and on the computer. This would make the difference. I deal with these issues in Chapters VII and VIII below.

However, in order to grasp Thom's modeling practices, one needs to go beyond the level of his own discourse. Clearly, one should look in more detail at the structure and culture of the IHÉS, which made the encounter between Thom and Ruelle possible. In the

[152] See F. Gail, Françoise, "De la résistance."
[153] D. Ruelle and F. Takens, "On the Nature of Turbulence," *Communication in Mathematical Physics,* 20 (1971): 167-192; and their "Note" in *Ibid.,* 23 (1971): 343-344; repr. *Chaos II,* 120-147; *TSAC,* 57-84.

following chapter, we shall see that the IHÉS was quite an idiosyncratic institution, which played a definite role in creating conditions propitious for Thom to develop his catastrophe theory of modeling practices, allowed his frequent interactions with other topologists working on qualitative dynamics, most notably Steve Smale, and set the stage for the adaptation of Thom's modeling practices to a new conceptual setting by Ruelle. This context is described in Chapter VI below. Before I do this, I explore the mathematical context for Thom's program in qualitative dynamics, in Chapter V.