Smale. In the following, I shall describe the modeling practices of some 'applied topologists' and show that they soon started to diverge on many crucial points.

5. APPLIED TOPOLOGY? THE MODELING PRACTICE OF QUALITATIVE DYNAMICS, 1971-1972

Meanwhile, however, the early years of the 1970s had witnessed an important modeling activity in several disciplines by people who orbited Thom. Here, I show how, in practice, this worked out. I first focus on a symposium on Dynamical Systems held in Brazil in 1971, where Smale, Thom, and Zeeman proposed concrete examples of natural phenomena which they modeled using these new practices. I then describe the global interpretation provided by Ralph Abraham, which shows the maximal extension of Thom's program for the classification of generalized catastrophes and its fundamental limitations.

One crucial remark is here essential. To construct a metadiscourse on the modeling practice of applied topologists, and Thom in particular, is an especially slippery endeavor since, as a theory of modeling practice, Thom's theory of catastrophes attempted to do just the same thing. In addition, I have not seen sources such as drafts or scrap papers, which might have allowed me to reconstruct the *process* by which any of the applied topologists came up with their models. One therefore must be careful not to take Thom's—or, for that matter, Zeeman and his clique's—methodological affirmations at face value, and rather look at the practice as can be inferred from published articles, something which has its own limitations. In this section, I nonetheless differ from Thom's theorization of his own modeling practice in that I confront his practice with those adopted by Smale and Zeeman. I insist on the importance of their specific mathematical training for their modeling activity, as opposed to metaphysical a priori. The divergence that can be observed in practice show that not any single, overarching interpretation, and certainly not Thom's alone, may do justice to the historical phenomenon of the widespread use of topological concepts for modeling nature and society.

a) Dynamical Systems at Bahia, 1971

On August 9, 1969, Kolmogorov wrote Arnol'd that he had spent part of the day with Peixoto. Together, they discussed the organization of a whole summer school, to take place in August 1971 "for a whole month in a place which in Peixoto's word is more attractive than Rio."[183] From July 26 to August 14, 1971, the University of Bahia, Brazil, welcomed an International Symposium on Dynamical Systems, which gathered most of the experts in the theory of dynamical systems, but unfortunately not the Russians as envisaged with Kolmogorov.

Although most talks presented at Bahia addressed strictly mathematical issues, Steve Smale, René Thom, and Christopher Zeeman, invited together with John Mather to give special series of lectures, presented extensive examples of what they thought qualitative dynamics might contribute to the practice of building mathematical models. Viewed from today, this symposium showed an odd mixture of elements from chaos and catastrophe theories. Aiming at putting "under the sway of the mathematician a vast array of phenomena thus far considered beyond his reach," this book provided, Peixoto thought, "many important contributions to mathematics, both pure and applied, and exhibiting

[183] Letter repr. in V. I. Arnol'd, "On Kolmogorov," 147-149.

great conceptual unity."[184] It also betrayed seeds of divergence in the modeling practice of qualitative dynamicists, which would become apparent in course of the following years.

(i) Thom and Linguistics

"Language and Catastrophes: Elements for a Topological Semantics": this was the ambitious title of René Thom's article published in the proceedings of the Bahia Symposium.[185] It aimed at providing a geometric interpretation of language and its traditional grammatical categories such as nouns, adjectives, and verbs. With the model here constructed, Thom claimed that he could describe the emission and reception of meaning, as well as some facts of linguistic theories.

At the root of Thom's practice, lay two main activities which he often theorized in much detail. First, he isolated "objects" from a shapeless background space. This stage of modeling, Thom often called a pure *description* of "morphologies." Then, using bits and pieces of catastrophe and dynamical systems theories, he wished to provide dynamical *explanations* for these morphologies.[186] In Thom's description of this two-step process of modeling lay what has often been interpreted, most fully by Jean Petitot, as the philosophical content of catastrophe theory.

His method for isolating "objects," for constituting the morphological description of a scientific domain, rested on the vast body of observations assembled by the

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[184] M. M. Peixoto, "Preface," *Dynamical Systems: Proceedings of a Symposium Held at the University of Bahia,* ed. M. M. Peixoto (New York: Academic press, 1973): xiii-xiv.
[185] R. Thom, "Langage et catastrophes: éléments pour une sémantique topologique," *Dynamical Systems,* ed. M. M. Peixoto (New York: Academic Press, 1973): 619-654; transl. *MMM,* 214-243.

specialists of the discipline he was interested in (the experimental chaos), and also an immediate, and even naive, intuition that these were the "natural" objects to isolate from the background noise. In the specific case of linguistics, the background was a "text *T* of a language *L*." It exhibited an intrinsic division into several segments, an observed hierarchy of sentences, words, syllables, and finally irreducible elements (letters or phonemes, depending on whether one was concerned with written or spoken language).

An important feature of this hierarchical division was its *universality*, which provided the crucial hint. For Thom, whether in linguistics or in biology, universality betrayed some hidden *logos,* i.e. universal forms described topologically. In linguistics, universal features were those common to all languages; for embryology, those shared by vast classes of species.

Therefore, in his Bahia lecture, Thom spent some time defining elementary sentences, i.e. those with one, and only one, word classically called the *verb.* The verb was especially easy to identify, for, if suppressed, any elementary sentence would be profoundly affected both in its meaning and its syntax. Formal linguistics then identified phrases organized around the verb, each of which containing one or several *nouns* (subject, direct, indirect object). Nothing was even remotely original in this description. For Thom, the great success of formal linguistics was the discovery that elementary sentences could often be organized as a tree-like structure. Following this, the formal

[186] See, e.g., the first two chapters of the earlier versions of R. Thom, *Modèles mathématiques de la morphogénèse* (Pisa: Accademia Nazionale dei Lincei, 1971; Paris: UGE, 1974); and preprint in Arch. IHÉS.

school, Thom contended, then "had the quasi-Hilbertian ambition to reach a complete formalisation of the rule of syntax . . . independent of the meaning."[187]

This high ambition was dashed, Thom explained, by the aberrant structure of more complex sentences (interrogation, circumstants). Therefore, the old problem of explaining the universality of fundamental grammatical categories remained unsolved. Thom pretended to be able, using catastrophe theory, to describe "

> in the most intrinsic manner possible the structural characteristics proper to each of the functions, considered a structurally stable elements in a dynamic theory of language.[188]

In Thom's view, elementary sentences described processes involving a small number of actants whose relationship was dictated by the semantic content of the verb. Earlier he had identified simple actions represented by verbs with linear sections of universal unfoldings, and called these "archetypal morphologies."[189] The example he repeated in the Bahia lecture of the word 'to capture'. Thinking of the actants of the capture process as point attractors of a gradient system, Thom identified this action with the transverse section of the cusp catastrophe in which a two-well potential lost one minimum as it was raises above the crest. This dynamic process could indeed be described in ordinary language as one minimum capturing the other.

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[187] R. Thom, *MMM,* 216.
[188] R. Thom, *MMM,* 218.
[189] R. Thom, "Topologie et signification," *L'Âge de la science,* 4 (1968): 1-24; "Topologie et linguistique," *Essays on Topology and Related Topics,* ed. A. Haefliger and R. Narasimhan (New York: Springer): 226-248. Both are repr. in *MMM,* 166-191 and 192-213. See also chap. 13 of *SSM,* 297-330.

> In principle, all the processes described by such a "programme" corresponds to a homotopy class of paths (transversals on the bifurcation set) in the universal unfolding of a suitably chosen catastrophe.[190]

Guiding tools for Thom were biological and mathematical metaphors. "While the noun is described by a potential well in the dynamics of mental activity, the verb is described by an oscillator in the unfolding space of a spatial catastrophe."[191] Mechanisms for the genesis and regulations of lingual forms were interpreted with an odd mixture of biological and mathematical concepts, which Thom used in order to infer the semantic consequences of pursuing the analogy further.

In the case of the capture catastrophe, the section of the unfolding, representing its archetypal morphology, was dynamically produced, for example, by a Hopf bifurcation of the origin. This had the (absurd?) consequence, noted by Thom, that *the predator became its own prey.* In his Bahia talk, Thom suggested that this "confusion of actants," characteristic of the magical thought, had been eliminated gradually from the civilized human thought. He did not explained how.

Thom's practice, he often claimed, was independent of the substratum (only mathematical features mattered as opposed to the dynamics giving rise to them), but one can see that this modeling practice rather was *without* substratum. The substrata he used were left in extreme vagueness. Nouns were potential wells, but for which potentials? He did not precise. At the "deepest" level surely lay usual spacetime, but a complex hierarchy of substrata were postulated by Thom: "in all animals there is an internal psychic process

[190] R. Thom, *MMM,* 232.
[191] Abstract of R. Thom, "Langage et catastrophes," 616, not repr. in *MMM.*

E isomorphic to the space that surrounds it."[192] In the absence of precise substratum lay the most extreme difference between Thom's practice and that of other applied topologists, such as Zeeman and Smale.

(ii) Zeeman and Physiology

"Differential Equations for the Heartbeat and Nerve Impulse": this was the innocuous title of E. C. Zeeman's article published in the proceedings of the Bahia Symposium.[193] In this paper, as Zeeman wrote, he "abstract[ed] the main dynamical qualities of the heartbeat and nerve impulse, and then buil[t] the simplest mathematical model with these qualities." Catastrophe theory provided "not only a better conceptual understanding, . . . but also explicit equations for testing experimentally." Zeeman's address was meant as a general exposé of the way elementary catastrophe theory could be used to build mathematical models.

Zeeman took seriously the idea that qualitative dynamics modeled *qualities*. The first task of the modeler thus was to isolate relevant qualities. This activity looked like Thom's morphological step, but was however much more shaped by mathematical concerns. The qualities Zeeman isolated were a mathematical translation of the observed features of the phenomenon that one wished to model. In the cases of the heartbeat and nerve impulse, Zeeman contended that three main dynamical qualities were displayed; they were:

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[192] R. Thom, "Topology and Linguistics," *MMM,* 199. This isomorphism between surrounding space and psychic states of course recalls Zeeman's "Topology of the Brain" which Thom cited.

(I) stable equilibrium;

(II) threshold, for triggering an action;

(III) return to equilibrium.

The third quality could be divided into two cases according to whether of not the return was smooth.[194]

Clearly, as opposed to Thom's morphologies, these were not simple physiological descriptions of the situation, but rather an idealization of a complex biochemical process. Moreover, this idealization was crucially informed by the types of behavior that qualitative dynamics was best suited for describing. Trivially, equilibrium meant 'look for attractors', while threshold was another way of saying that a catastrophe was involved. Instead of repeating observations, Zeeman abstracted from them qualities that he could simply translate into his topological language.

Then, starting from the three qualities, Zeeman derived the simplest mathematical model displaying such features. Presenting less and less simple examples, Zeeman reached a system of differential equations that exhibited the three qualities (with jump return):[195]

$$\varepsilon x' = -(x^3 - x + b), \quad b' = x - x_0,$$

where the primes denoted derivation with respect to time. By representing points on the plane with letters at opposite ends of the alphabet, Zeeman used an unusual notation

[193] E. C. Zeeman, "Differential Equations for the Heartbeat and Nerve Impulse," *Dynamical Systems,* ed. M. M. Peixoto (New York: Academic Press, 1973): 683-741; repr. *CT,* 81-140.
[194] E. C. Zeeman, "Differential Equations," 684.
[195] E. C. Zeeman, "Differential Equations," 699.

emphasizing the difference between slow and fast dynamics, i.e. between the control parameter b and the dynamical feature x he wanted to model.

For the case of the nerve impulse, a slow return to equilibrium was needed. Zeeman showed that a two-dimensional model could be used to account for this. These considerations led him to a more complicated model whose equilibrium points lay on the surface of the cusp catastrophe.[196]

$$\varepsilon x' = -(x^3 + ax + b), \quad a' = -2a - 2x, \quad b' = -a - 1.$$

It was at this point that catastrophes theory explicitly entered Zeeman's modeling practice. While he had used general arguments for deriving the simplest mathematical model exhibiting the qualities he wanted to reproduce, he needed "Thom's theorem" in order to argue that these simple models were indeed the ones he was looking for.

> Let us pause for a moment to consider what we are doing. We have found a surface of lowest degree possessing all the required properties. But is this really the "simplest" example, and is there any virtue in having found the simplest? The topologist regards polynomials as rather special, and tends to turn his nose up at so crude a criterion of simplicity as choosing the polynomial of lowest degree. Moreover, in biology of all subjects we should least expect nature to be obligating as to use polynomial equations. So perhaps we ought to consider *all possible* surfaces. Now comes the truly astonishing fact: when we do consider all surfaces, not only is this particular surface the *simplest* example, but in a certain sense it is the *most complicated* example; in other words, it is the *unique* example. Herein lies the punch of the deep and beautiful catastrophe theory created by René Thom.[197]

Besides noting the strong terms with which Zeeman lauded Thom's catastrophe theory, one may also underscore that his models were in fact constructed with catastrophes in mind. To arrive at them by purely general assumptions might have been a little deceptive since this was where Zeeman was heading in the first place.

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[196] E. C. Zeeman, "Differential Equations," 708.

Again, in a process similar to what had taken place in the case of the genericity of stable systems, the strategy consisted in substituting a purely mathematical justification for common metaphysical assumptions, namely in Zeeman's case, the postulate of simplicity. As Zeeman interpreted it, "Thom's theorem" implied that if the dynamics of the phenomena he was considering was *postulated* to depend on a potential function ψ, i.e. the dynamical equation was of the form $\varepsilon x' = -\text{grad}\psi = \partial\psi/\partial x$, and if moreover this function ψ was assumed to be generic, then the only singularities that could happen were folds and cusps. In addition, *locally* near any cusp, ψ had to be equivalent to the canonical cusp catastrophe he considered in his "simplest" model. In other words, the "simplest" model represented "the most complicated thing that could happen locally. . . . The theorem is the key mathematical fact behind our whole approach."[198]

The actual use of topology in Zeeman's modeling practice is therefore quite limited since he only used Thom's theorem in the vaguest terms in order to determine that his "simplest" models also were the only ones that locally could happen. Of course, he acknowledged that the situation might more complicated. Globally there might be more folds and cusps involved; there might be also more variables involved. A familiarity with topological techniques was essential for using more complicated catastrophes in higher dimensional spaces. But these were rarely used by Zeeman, and more rarely by his followers.

As presented at Bahia, the next step in Zeeman's procedure lay in the interpretation of the variables involved in the catastrophe theoretic representation in terms

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[197] E. C. Zeeman, "Differential Equations," 704. Original emphasis.

of some physical parameters. This was done by confronting the "simplest" model with observations, experiments and preexistent empirically-derived models. In the case of the heartbeat, experimental evidence led him to include three variables, i.e. one more than above.[199] He identified the three variables x, a, and b, respectively with the length of heart muscle fibers, a certain chemical control, and tension caused by blood pressure. Immediately, one may note the very tentative nature of this identification. For example, variations in the behavior of muscle fibers were ignored, and the nature of the "chemical control" was left in the vague ("possibly membrane potential," Zeeman simply wrote).[200]

In conclusion, Zeeman underscored the main advantages of his approach over traditional biophysical and biochemical analyses: universality, ease of adaptation, and economy of thought. Although he did not held "a very strong brief for [his] explicit equations," Zeeman contended that one should expect these equations, coming from "Thom's deep uniqueness theorem" as they did, to apply to any biological phenomena exhibiting the same three dynamic qualities. In this sense, his equations were not arbitrary, but "natural." As such, and as opposed to those based on known chemistry, they could easily be adapted to new experimental discoveries. Finally, they were economical in that, by the nature of his method, they only involved relevant parameters for the phenomenon thus modeled.

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198 E. C. Zeeman, "Differential Equations," 706.
199 Zeeman cited B. Rybak and J. J. Bréchet, "Recherches sur l'électromécanique cardiaque," *Pathological Biology,* 9 (1961): 1861-1871 and 2035-2054, and acknowledged discussions with Rybak.
200 E. C. Zeeman, "Differential Equations," 712-713.

(iii) Smale and Economics

"Global Analysis and Economics I: Pareto Optimum and a Generalization of Morse Theory": here was the technical title of Stephen Smale's Bahia article.[201] It was meant as a first approach to the introduction of time back into the equations of mathematical equilibrium economics. Smale's paper was the most mathematical of the three. It posed a specific problem expressed in mathematical terms:

> One is given real differentiable functions $u_i: W \to R$ defined on a manifold W, say $i=1,\dots,m$. What is the nature of curves $\varphi: R \to W$ with the derivative $(d/dt)(u_i \circ \varphi)(t)$ positive for all i, t? For what $x \in W$ does there exist such a φ with $\varphi(0)=x$? The critical Pareto set θ is defined as the set of $x \in W$, for which there is no such φ. The main problem is the study of θ.[202]

In the first section, Smale motivated the interest of such a mathematical problem in the case of pure exchange economy and the classical Pareto optimum. But he placed this strange warning: "The mathematician reader can skip this section if he is not interested in economics or concrete applications and the economist reader will know these things." As opposed to Thom's and Zeeman's which did not prove theorems, the main body of Smale's paper could therefore be taken as an exercise in proving theorems for the sake of pure mathematics.

Contrary to Thom and Zeeman, Smale had the good fortune of being able to rely on a rigorous, axiomatized treatment of the domain he was dealing with, provided by

[201] S. Smale, "Global Analysis and Economics I: Pareto Optimum and a Generalization of Morse Theory," *Dynamical Systems,* ed. M. M. Peixoto (New York: Academic Press, 1973): 531-544.
[202] S. Smale, "Global Analysis and Economics I," 532.

Debreu's *Theory of Value.*[203] Debreu had come to Smale with mathematical questions, and soon Smale considered that he might help.[204] His heavy-gun theory only provided Smale with an interesting mathematical problem to which he could apply the techniques of dynamical systems theory.

Therefore, the Bahia article provided a poor example of Smale's modeling practice, since he did not actually attempt at building economic models there. Like Thom and Zeeman, however, Smale saw in dynamical systems theory a reservoir of techniques that could help modeling a wide variety of phenomena.

> And around 1969, 1970 I tried going into several other things. A little bit here and there. A paper on electrical circuits. . . . Then I gave the Colloquium Lectures for the American Math Society around 1970. They were on applications of global analysis. Then I gave four talks—one in biology and one in economics—I think that was after I met Debreu, yeah, it was—and then one on electrical circuits and one on mechanics. Really there was a period there where I was just groping around a little bit.[205]

A common feature of Smale's papers dealing with applications of global analysis to the study of natural phenomena was that the models never were developed by Smale himself. He rather used preexistent mathematical models, and abstracted from them some

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[203] G. Debreu, *Theory of Value: An Axiomatic Analysis of Economic Equilibrium* (New Haven: Yale University Press, 1959).
[204] S. Smale, "Gerard Debreu Wins the Nobel Prize," *Mathematical Intelligencer,* 6(2) (1984): 61-62; G. Debreu, "Stephen Smale and the Economic Theory of General Equilibrium," *From Topology to Computation: Proceedings of the Smalefest,* ed. M. W. Hirsch et al. (New York: Springer, 1993): 131-146; and G. Chilchilnisky, "Topology and Economics: The Contribution of Stephen Smale," *Ibid.,*147-161.
[205] S. Smale in *More Mathematical People,* 314. The articles Smale cites, and some similar ones, have been published. S. Smale, " What Is Global Analysis?," *American Mathematical Monthly,* 76 (1969): 4-9; repr. *MT,* 84-89; " Topology and Mechanics," *Inventiones Mathematica,* 10 (1970): 305-331; 11 (1970): 45-64; "On the Mathematical Foundations of Electric Circuit Theory," *Journal of Differential Geometry,* 7 (1972): 193-210; and "A Mathematical Model of Two Cells via Turing's Equation," *Lectures on Mathematics in the Life Science,* 6 (1974): 15-26.

of their topological features using the tools he mastered. In a sense his modeling practice was nonexistent. Smale nevertheless could be said to have embodied a modeling practice, which consisted in topologizing existent models and therefore account for some of their general behaviors. The results he extracted from the models were thus original. Topology helped him precise assumptions hidden in the models; it helped understand some of their consequences; and it allowed him to modify them when needed. Moreover, his reliance on dynamical systems theory allowed him to make interesting suggestions as to how dynamics could be integrated into the essentially static study of equilibrium economics.[206]

(iv) Seeds of Discord?

The most obvious common feature of the three talks of Thom, Zeeman, and Smale was the insistence on building *dynamical* models. While Smale and Thom both opposed this dynamic nature of their models to dominant structural (or equilibrium) approaches, Zeeman opposed it to the biochemical processes underlying the topological features he studied. This emphasis on dynamics cannot be very surprising since they had all been concerned with qualitative dynamics for a while. The dynamics they introduced however had a different meaning from its usual physical one. It emphasized changes, but not the forces responsible for these changes. Without reducing topological features to an underlying substratum, their mathematical practices aimed at providing a dynamics devoid of forces. None of them seemed interested in studying substrata for their topological features.

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[206] About this, see S. Smale, "Dynamics in General Equilibrium Theory." *American Economic Review,* 66 (1976): 288-294; repr. *MT,* 106-112, and the papers cited therein.

Despite their commonalties, the very outlook of these three papers was quite different. With definitions and theorems, Smale's looked like the classic paper of a pure mathematician. Out of the three, Thom's article was by far the most verbose. Were it not for the many mathematical terms and symbols, it would have seemed closer to a philosophical paper than one susceptible appearing in the proceedings of a mathematical meeting. By far the longest, Zeeman's article was replete with pictures, graphs, experimental data, simple examples, and differential equations; it resembled any other work in applied mathematics.

Their respective attitude towards references to existing literature was also totally different. Except for Debreu's book, Smale's bibliography contained only mathematical works of an uncompromising technical nature. Zeeman's bibliography listed several biological articles and books; it also included a few references to catastrophe theory, none of which however concerned with mathematical research. Both Smale and Zeeman acknowledged discussions with experts from other fields. By far, Thom was the most cavalier about citations. While he explicitly cited only one article, dealing with a small mathematical result, Thom used other people's work in linguistics and mathematics without referencing it.

Referencing betrayed the relationship they had established with the fields they dove into. Thom needed previous work only to show that his categories had been used previously and to point out the failure of his predecessors. With mathematical economics, and above all Debreu's work, Smale had found a field that already used sophisticated mathematical techniques. His attitude was to translate problems already expressed mathematically into a topological language. As he had said to the statistical physicists in Chicago, he was making them "attractive to the modern mathematician, . . . brought up in the purist, Bourbakist style of education."[207] Zeeman, finally, entertained involved relationships with both experts in other domains, and their work. But as opposed to Thom and Smale, he was not taking their categories very seriously, being quite happy with vague identifications between his dynamical variables and empirically measured ones. On the other hand, Zeeman was the only one of the three who insisted on experimental confrontation, and he sometimes pressured experimenters for undertaking it.

The role of mathematics *per se* also varied greatly in their respective modeling practices. For Thom, more than a mere reservoir of metaphors, as it might appear at first sight, mathematics provided a language, his way of thinking about the problems at hand. For Smale, the mathematician proved theorems. But Smale hardly made any effort at going back from abstract topological formulations to one that non-mathematicians could easily understand. Zeeman used a wide range of mathematical techniques, which made his approach the easiest to attack by those experts who, familiar with differential equations, might not fully appreciate, like him, the importance and limitations of "Thom's theorem."

Finally, although all of these papers had a highly speculative content, one should note a different attitude concerning speculation. Thom speculated on everything: mathematics, since the general theory was not known, and the interpretation of the models. Calling for experimental confirmation, Zeeman proposed tentative models without sound physiological or biochemical principles on which to ground them. In his

[207] S. Smale, "Personal Perspectives," *MT,* 100.

paper, finally, Smale made a mathematical conjecture, admittedly informed by economic theories, but expressed in a similar way as his conjecture on the genericity of structural stability more than decade earlier.

b) Abraham: Student of Morphogenesis

Throughout the late sixties and early seventies, Ralph Abraham frequently visited the IHÉS, where, together with Zeeman, Smale, and Marsden, he was considered by the directors as furthering Thom's program in physics or topics belonging to the Third Section. In 1971, he wrote Kuiper that he wanted to come to the IHÉS "to study morphogenesis," adding: "I prefer not to teach, as I feel I am a student now."[208] Out of his stay at the IHÉS a general "Introduction to Morphology" would emerge, which had nothing to do with biology. Moreover, with Abraham, we see the most extreme justification of the interest of Thom's ideas on moral bases, which, from the point of view of cultural history, is most relevant to the wide success of catastrophe and chaos theories.

(i) Morphosophy

On March 1-5, 1972, the Mathematics Department at the University of Lyons held a meeting gathering physicists and mathematicians where dynamical issues were discussed by dynamicists then at the IHÉS (Ruelle, Abraham, Marsden, Robbin, Takens). Without doubt the most important speaker, Abraham gave a series of lectures which outlined his theory of morphology.[209]

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[208] *Rapport du Comité scientifique* (22/10/71); lettre de Ralph Abraham à Nicolaas Kuiper (4/10/71). Arch. IHÉS.

[209] R. Abraham, Hamiltonian Catastrophes," and "Introduction to Morphology," *Publications mathématiques du département de Mathématiques de l'Université de Lyon-I,*

Born on July 4, 1936, Ralph Abraham, after receiving his Ph.D. from the University of Michigan, had started, while at Columbia in the early 1960s, collaborating with Smale. In the spring of 1966, he lectured on KAM-theory in the Princeton University Department of Physics, out of which his seminal book came out.[210] According to a letter of recommendation written by Thom in 1966, Abraham had made a name for himself as "a very good specialist on many fields like Differential Topology, Qualitative Theory of Differentiable Systems." Thom however added:

> I do not think of him as an extremely original, or powerful mathematician; but he has a thorough knowledge of many modern basic theories, as proven by the fact that he wrote recently several books of great interest.[211]

In other words, not so much an original mind as far as mathematical research was concerned, Abraham was an insightful author of advanced textbooks about new mathematical trends.

At the 1969 Warwick Symposium on Differential Equations, Abraham had attempted a tongue-in-cheek prediction of the future development of the subject, in relation to what he called the "yin-yang problem."[212]

> Some large (yin [i.e. complicated]) sets of differential equations with generic properties are known, some small (yang [i.e. simple]) sets which can be classified are known, but in general the two domains have not met.

9, Suppl. 1 (1972): 1-37 and 38-114; the second article is repr. in R. Abraham, *On Morphodynamics,* 9-125.

[210] R. H. Abraham and J. E. Marsden, *Foundations of Mechanics.* For Marsden biography, see "1990 Norbert Wiener Prize in Applied Mathematics Awarded in Columbus," *Notices of the American Mathematical Society,* 37 (1990): 808-811.

[211] Lettre de René Thom à F. H. Clauser (9/11/66). Arch. IHÉS. Original English. See R. Abraham, *Linear and Multilinear Algebra* (New York: Benjamin, 1967) and R. Abraham and J. Robbin, *Transversal Mappings and Flows* (New York: Benjamin, 1967).

[212] R. Abraham, "Predictions for the Future of Differential Equations," *Proceedings of the Symposium on Differential Equations and Dynamical Systems,* ed. D. Chillingworth (Berlin: Springer, 1971): 163-166.

Putting references to Oriental philosophy aside, this way of thinking overlapped with Smale's earlier efforts. Consulting the *I Ching,* he prophesied that the answer was no! "As it had become clear through the esoteric Buddhist principles of Karma and Transcendence that the yes-no question formally posed was too restrictive," he rather asked:

> How will the subject evolve in the course of the next year? In view of the theories propounded by Professor Thom, it seems that in place of a steady approach of the two domains we should expect a number of bifurcations, and fruitful investigations of new domains.

From the same source, he correctly inferred that, in the future, there would be a clear dominance for yin, i.e. the exploration of systems with complicated dynamics or in other words chaos. He outlined what should the plan for future study of this domain.

> By concentrating on what is important without being too ambitious, by continuing to work hard and with determination, and by studying closely and learning from the many counter-examples of the past, progress will be made towards peace and harmony in differential equations.

Whether peace and harmony would ever be achieved remained doubtful, but Abraham's 1972 course at Lyons indicated that the ambitious program still attracted him.

Indeed, Abraham intended his "Introduction to Morphology" as the pursuit of Thom's and Smale's classification efforts for differential systems. This course exposed an ambitious mathematical theorization of modeling practice. According to him, Thom had introduced in the sciences a program of "morphometrization of phenomenological processes with controls."[213] Correspondingly, there was a phenomenological philosophy of science, "which I call *morphosophy,*" and which Abraham traced back to the *I Ching* and Plato.

Abraham mainly wished to devote his lectures to *morphometry,* which dealt with the parametrization of observations using mathematical structures. In order to do so, Thom had introduced some special cases, which Abraham called *metamodels.* In other words, Abraham contended that mathematics provided a foundation for metaphysics. Thom's reservoir of metamodels was a set of mathematical theories that justified the use mathematical technologies for the modeling of natural phenomena.

Beyond elementary catastrophe theory, Abraham went the furthest in attempting to classified what Thom had named generalized catastrophes. He recognized that recent developments in the theory of dynamical systems made Thom's initial focus on structural stability insufficient. A rigorous mathematical classification was lacking.

> As catastrophe theory [i.e. bifurcation theory for vector fields] is now barely in its infancy, it is impossible to describe morphodynamics as a mature theory.[214]

(ii) Chaos

The present consensus has it that it was Li and Yorke's 1975 paper, titled "Period Three Implies Chaos," that introduced this powerful term into science. As I discussed in Chapter V, this paper certainly played an important role in the history of chaos, in particular by drawing attention to Lorenz's article. After its publication, chaos was adopted as a widespread label for processes that were starting to be studied extensively. In their article, however, Li and Yorke did not define 'chaos' and used the term rather casually. The

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[213] R. Abraham, *On Morphodynamics,* 14.
[214] R. Abraham, *On Morphodynamics,* 29.

scientist who first used it in a more precise sense was biologist Robert May who, after having talked to Li and Yorke, defined "chaotic regimes" for iterated functions.[215]

Strikingly, when mentioning the case of "universal catastrophes" containing open sets in 1972, Abraham used the term 'chaos' to label this situation. One may moreover notice that Ruelle and Takens had also described the turbulent regime using the word "chaotic," a rather natural word for turbulence. "For sufficiently large [external stress], the fluid motion becomes very complicated, irregular, and *chaotic,*" did they write in 1970, "we have turbulence."[216]

Although he later attacked the use of this term, it may well be Thom himself who, much to his dismay, introduced this word into the scientific language. Mentioning hydrodynamic turbulence, Thom indeed defined chaotic morphology in the 1971 printing of *Modèles mathématiques de la morphogénèse.* It may happen, Thom wrote that the catastrophe set is dense in some subset of the dynamical space, in which case "we might have reasons to say that the morphology is *chaotic.*"[217] This definition differs from the

[215] T.-Y. Li and J. A. Yorke, "Period Three Implies Chaos." *American Mathematical Monthly,* 82 (1975): 985-992; repr. in Hao B.-L., *Chaos,* 1st ed.: 244-251. May, Robert. 1974. "Biological Populations with Nonoverlapping Generations, Stable Points, Stable Cycles, and Chaos." *Science,* 186: 645-647

[216] D. Ruelle and F. Takens, "On the Nature of Turbulence," *Communications in Mathematical Physics,* 20 (1971): 167-192, 167. My emphasis. Note that the same term was used in Ruelle's lecture notes at Lausanne, dating from the summer 1970: "Méthodes d'analyse globale en hydrodynamique," *TSAC,* 5 and in D. Ruelle, "Some Comments on Chemical Oscillations," *Transactions of the New York Academy of Sciences,*35 (1973): 66-71, 70; *TSAC,* 114.. As an example of the naturalness, in this context, of the word "chaos," already matched with the qualifier "sensitive" later to be much used in the phrase "sensitive dependence on initial conditions," one may cite Theodore Schwenk, *Sensitive Chaos: The Creation of Flowing Forms in Water and Air,* transl. O. Whicher and J. Wrigler (London: Rudolph Steiner, 1965).

[217] R. Thom, *Modèles mathématiques de la morphogénèse,* 1971 ed., 2. Original emphasis. Also in R. Thom, "Topological Models in Biology," *Towards a Theoretical*

one that has been adopted in later years. But Abraham's lecture notes underscored that Thom's use of the term had been noticed, and that minds might have been ready for it.

Abraham considered Thom's "chaos" as a being part of the classification program for generalized catastrophes. Although the equivalent of the Thom-Mather theorem did not exist for the dynamical systems case, Abraham described a "zoo of archetypal catastrophes." In a "fictitious" classification of dynamic catastrophes, he included the "Smale horseshoe catastrophe," the "Hopf catastrophe," and the "Ruelle-Takens excitation catastrophe."[218]

(iii) Is Mathematics Worth Doing?

Concluding his lectures, Abraham gave reasons for what he considered as the most important consequence of Thom's "morphosophy." It was, he contended, "a potential step toward the reintegration of wisdom [into science] so badly needed for our survival, and social evolution." Certainly, Abraham was one of those "socially conscious scientists" Smale was calling for. His own interest for catastrophe theory and, later, chaos theory were a consequence of his concerns with social issues.

> On these years [1967-1972], I have been deeply concerned with the social problems of the world, the misuse of technology, and the basic question: *is mathematics worth doing*. My experiences and reflections during this period leave me with the conviction that much of mathematics, including the ideas of this lecture in particular, are part of the intellectual wealth of mankind, essential to our evolution and survival. I may add, however, the qualification that relevance and value of this kind of wealth is dependent upon applications, which up to now, have been shamefully neglected. All wealth can be used, misused, or neglected.[219]

Biology, 3, ed. C. H. Waddington (Edinburgh: University of Edinburgh Press, 1969): 89-116, 103.

[218] R. Abraham, *On Morphodynamics,* 49-51.

[219] R. Abraham, "Hamiltonian Catastrophes," 1. My emphasis.

Citing such esoteric scientists and philosophers as Buckminster Fuller, René Guénon, and Oliver Reiser, Abraham claimed that "the crises of the modern world have penetrated the scientific community with great force."[220] Following these authors, he thought that science and technology were both cause and cure for the present crisis. Eastern philosophy, he believed, gave "some foundations for socially responsible and valuable scientific research." His "position of these matters, at present," Abraham avowed, "is represented by this paper."[221] But nothing insured that they would be used to further these laudable goals.

> I am also painfully aware of the awful potential in the misuse of knowledge of such power, but its seems to me better to go ahead, rather than stay here. Therefore, I have tried to create an opening to a new science with this introduction to Thom's ideas. I hope it will help some to go on, and thus also all of us.[222]

Clearly, Abraham's distress could only be received by mathematicians with varying degrees of skepticism, as best expressed by Michael Shub's following poem:

> <u>For Ralph</u>
>
> Concerned with forms we gain a healthy
> disrespect for their authority, as
> [Bob] Dylan and Donovan mingle with
> Dynamics in minds meeting answers,
> the Book of Changes intruding with
> equanimity in our lives, to questions

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220 R. Abraham, *On Morphodynamics,* 12. He cites B. Fuller, *Operating Manual for Spaceship Earth* (Carbondale: Southern Illinois University Press, 1969); R. Guénon, *The Crisis of the Modern World,* transl. M. Pallis and R. Nicholson (London: Luzac, 1962); and O. Reiser, *Cosmic Humanism: A Theory of the Eight-Dimensional Cosmos Based on Integrative Principles from Science, Religion, and the Arts* (Cambridge, Mass.: Schenkman, 1966).
221 R. Abraham, *On Morphodynamics,* 12.
222 R. Abraham, *On Morphodynamics,* 77-78.

we have not yet thought to ask:
What if Ω meant more to us than politics and death?[223]

Nowhere is it clearer than in Abraham's lecture notes that catastrophe theory, for some, held the promise of the coming of socially conscious scientific research. How far could we say these ideas were shared by scientists who dealt with catastrophe and chaos theories is a tricky question. However, this identification of a research program with a science that could be socially positive certainly played a role in the diffusion of catastrophe and chaos theories in the late 1970s and early 1980s.

6. DIVERGENCES AND CONTROVERSIES, 1974-1977

Mathematics rarely gets into the news. And even rarer are media controversies centered around mathematical theories. While chaos theory may have received quite a lot of publicity lately, few other mathematical theories have aroused so much media attention, so grandiose claims, and so devastating critiques, than catastrophe theory in 1974-1977.

At the heart of the debate lay much confusion on the nature of the modeling practices that catastrophe theory intended to codify. The always ambiguous relation between mathematics and the mathematical modeling of the world only exacerbated the confusion. This afforded a wide variation in opinions regarding the practical and philosophical implications of catastrophe theory. As we have seen, divergences were already apparent in the early 1970s. An examination of the further unfolding of these debates provides a better understanding of the implications for modeling that catastrophe and chaos theories had.

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[223] M. Shub, "For Ralph," *Proceedings of the Symposium on Differential Equations and Dynamical Systems,* ed. D. Chillingworth (Berlin: Springer, 1971): 167.

At this point, however, the history of the applied topologists' modeling practices becomes enmeshed with traditional stories of catastrophe theory. Because it has already been the object of much comment, I will focus on debates among Thom, Smale, and Zeeman, using part of the unpublished correspondence of the latter two, and refer to these debates only as far as they directly impacted their modeling practices.[224]

a) Media Success: The Vancouver Congress in 1974.

"Catastrophe theory is a method discovered by Thom of using singularities of smooth maps to model nature."[225] Zeeman's talk at the 1974 International Congress of Mathematicians at Vancouver made a splash. By introducing several concrete examples of application of catastrophe theory in several domains of the natural and social sciences and even the frequency of riots in prisons, Zeeman contended that this theory offered two "attractions."

> On the one hand it sometimes provides the deepest level of insight and lends a simplicity of understanding. On the other hand, in very complex systems such as occur in biology and the social sciences, it can sometimes provide a model where none was previously thought possible.[226]

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[224] For surveys of the different approaches to catastrophe theory, I recommend the elementary discussions in A. Woodcock and M. Davis, *Catastrophe Theory* (New York: E. P. Dutton, 1978); J. Guckenheimer, "The Catastrophe Controversy," *Mathematical Intelligencer,* 1 (1978): 15-20; and the more technical survey T. Poston and I. Stewart, *Catastrophe Theory and its Applications* (London: Pitman, 1978).
[225] E. C. Zeeman, "Levels of Structure in Catastrophe Theory Illustrated by Applications in the Social and Biological Sciences," *Proceedings of the International Congress of Mathematicians (Vancouver 1974),* 2, ed. R. D. James (1975): 533-546; repr. *CT,* 65-78.
[226] E. C. Zeeman, "Levels of Structure," 533.

As early as 1971, Zeeman had begun popularizing catastrophe theory.[227] But it was after his presentation at Vancouver that it really achieved general popularity, both among scientists and in the mainstream media. Thom later recalled that, while at Vancouver, Zeeman had to repeat "an extremely brilliant presentation" of catastrophe theory in front of journalists who painted an highly promising picture of it.[228]

Reviewing *SSM,* British physicist C. W. Kilminster compared Thom's book to Newton's *Principia*: both "lay out a new conceptual framework for the understanding of nature, and equally both go on to unbounded speculation"[229] British biologist Brian C. Goodwin wrote:

> "the book gave me a sense of liberation and enlightenment akin to what I imagine Ptolemaic astronomers may have felt when offered Copernican heliocentric geometry.[230]

Unbounded praise, especially those coming from reporters or directed to unqualified audiences, created an unstable situation for catastrophe theory. On the one hand, very hopes were raised, while, on the other, all models proposed by Zeeman and Thom were highly tentative. At the same time, the mathematical theory was still extremely limited, in that only low-dimensional catastrophes for gradient systems had been classified; only elementary catastrophe theory had achieved anything resembling a

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227 See, e.g., E. C. Zeeman, "Geometry of Catastrophes," *Times Literary Supplement* (1971): 1556-1557.
228 R. Thom, "Mémoire de la théorie des catastrophes," *La genèse des formes. Prix LVMH pour l'art,* 17. Thom Arch.
229 C. W. Kilminster, "The Concept of Catastrophe" (review), *London Times Higher Education Supplement* (30 November 1973). Quoted in A. Woodcock and M. Davis, 59.
230 B. C. Goodwin, "Mathematical Metaphor in Development," *Nature,* 242 (1973); 207-208.

complete, rigorous status. Too high hopes with too little to back them up had made a backlash seem unavoidable. But Thom's attitude did not help.

b) The Thom-Zeeman Debate

As of the beginning, Thom and Zeeman embodied slightly different modeling practices with regards to catastrophe theory. In 1973, Thom wrote, for the Warwick graduate student journal *Manifold,* a short provocative survey of the "Present state and future perspectives" of catastrophe theory.[231] It was "a fascinating mixture of tantalising hints and deeply profound remarks about mathematics and science, spiced with a few provocative cracks at experimentalists, and garnished with some fairly wild speculations," Zeeman replied the following year.[232] Thom answered Zeeman's reply with a text to which Zeeman added further footnotes.[233]

This debate between two men who shared so much both in their practice and philosophy with regards to the use of mathematics to understand the world, might not have been consequential. But when Zeeman's models came under harsh attacks from the part of applied mathematicians, Thom's earlier position allowed him to take his distance from Zeeman. Later in his life, Thom seemed to have regretted his timidity. "If I have to formulate a regret, it is no doubt not to have, at the time, more firmly defended the

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[231] R. Thom, "La théorie des catastrophes: état présent et perspectives." *Manifold,* 14 (1973); repr. *Seven Years of Manifold, 1968-1980,* ed. I. Stewart and J. Jaworski (Nantwich: Shiva, 1981); and *Dynamical Systems, Warwick 1974*, ed. A. Manning (Berlin: Springer, 1975): 366-372.

[232] E. C. Zeeman, " Catastrophe Theory: A Reply to Thom," *Manifold,* 15 (1974); repr. *Dynamical Systems, Warwick 1974*, ed. A. Manning (Berlin: Springer, 1975): 373-383, 373.

possibility of a fundamentally *qualitative science.*"[234] Indeed, Thom began his 1973 survey by claiming:

> We cannot consider catastrophe theory as a scientific theory in the usual sense of the term. . . . [W]e must consider it as a *language,* a method, which allows to classify, to systematize empirical data, and which provides these phenomena with the beginning of an explanation that makes them intelligible."[235]

He would soon claim that catastrophe theory was nothing less—and nothing more—than a "state of mind."[236] If even for Thom the very status of catastrophe theory was so vague, how could anyone else make sense of it?

Thom then launched a very harsh attack at experimenters, especially in biology. He attributed their rejection of, or at least their indifference toward, catastrophe theory to "the psychological chasm that separates the present-day biological approach from any theoretical thought." Biology was, he deplored, "a cemetery of facts."[237]

As far as catastrophe theoretic models were concerned, Thom proclaimed, there was little hope of ever achieving experimental confirmation for them. Arrogantly, the theoretician (Thom) was saying to the experimenters (molecular biologists):

> You must convince yourself that the progresses in Biology depend less on an accumulation of experimental data than on a widening of the capacity of mental simulation of biological facts, on the creation of a new 'intelligence' among Biologists.

The only external control over catastrophe theoretic models that one should expect was "an esthetic feeling of intellectual economy." Thom argued that hopelessness came

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[233] R. Thom, "Answer to Christopher Zeeman's Reply," *Dynamical Systems, Warwick 1974*, ed. A. Manning (Berlin: Springer, 1975): 384-369.
[234] R. Thom, "Mémoire," 20. Thom Arch. Original emphasis.
[235] R. Thom, "Etat présent et perspectives," 366. Original emphasis.
[236] R. Thom, "Structural Stability, Catastrophe Theory, and Applied Mathematics," *SIAM Review,* 19 (1977): 189-201.

from ignorance of the nature of the parameters, such as morphogenetic gradients. This might have constituted an opening towards experimental investigation of the biochemical nature of these gradients. But Thom argued against this, being content with saying that they might exhibit a "kinetic nature, and thus elude biochemical analysis techniques."[238]

In all his models, Zeeman restricted himself to elementary catastrophe theory. If this choice allowed him to back up his claims about the simplicity and universality of his models, it had the obvious drawback of not relying on the general philosophy propounded by Thom. As opposed to the latter, Zeeman held on to the dream of basing his modeling practice on a rigorous, mathematical theory. In face of the incompleteness of catastrophe theory, and indeed indications that it might never be completed for general dynamics, this position was weaker and more open to attacks.

Therefore, for Zeeman, the lack of interest manifested by biologist was easily understandable. Their insistence on providing experimental tests for catastrophe theoretic models was a "simple insurance policy." If one was not ready, or able, to penetrate the details of the proof of the classification theorem—"I must confess it took me several years to achieve this objective myself," Zeeman wrote—then, experiments were an easy way to verify the models. In plain words, Zeeman opposed Thom about the usefulness of experimentation:

> any theory must face up to the classical scientific method of prediction, experiment and verification. I see no reason why his theories should be sacrosanct on the grounds of being qualitative rather than quantitative.

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[237] R. Thom, "Etat présent et perspectives," 369.
[238] R. Thom, "Etat présent et perspectives," 370-371.

As we have seen with the Bahia Symposium, while developing his models by using the "deep" significance of "Thom's theorem," Zeeman always aimed at quantitative models—differential equations—defined on a substratum that should be as tangible as possible, and leading to predictions that could be confronted with experiments. Of course Zeeman acknowledged that the dynamical variables he used seldom were clearly identified, even in physical cases where it should have been the least problematic. For the breaking of waves, for example, he confessed: "I do not yet see how to identify the catastrophe variables with the classical variables of hydrodynamics."

Nevertheless, even in the case of sociology, Zeeman entertained the hope that such an identification should be possible, and thus the models testable experimentally. Exposing a rough model, based on the cusp, for the strength of political opinions in a society, he thus suggested that data might be "possibly collectable by a suitably designed questionnaire." The two ways in which catastrophe theory might impact sociology, and for that matter any other science, was "in the design of experiments, and the synthesis of data."[239]

Thom remained unmoved by Zeeman's criticisms. Catastrophe theory, Thom contended, might suggest different models for the same phenomena. Experiment might give you criteria to choose, but perhaps not. Then only "a subjective feeling of elegance, of mathematical or conceptual economy may decide." This approach only told you that a model might be *preferable* to another, but not that one is *true*, while the others are *false*.

> This kind of vagueness for the choice of models is felt by scientists of strict positivist or Popperian opinion . . . as an overwhelming objection against the scientific claims of CT. Needless to say, I do not share this prejudice: for me, the

[239] E. C. Zeeman, "Reply to Thom," 375-377.

> scientific status of CT is founded *on its internal, mathematical consistency,* which allows making deductions, generating new forms from another set of forms, thus allowing in some favorable cases qualitative predictions, and in general realising a considerable "reduction of arbitrariness" in the description.[240]

In no veiled terms, talking of "precarious quantitative modelling," Thom attacked Zeeman's presentation of his own models with a "didactic warning:"

> when presenting CT to people, one should never state that, due to such and such theorem, such and such a morphology is going unavoidably to appear. In no case has mathematics any right to dictate anything to reality.

In effect, Thom was already invalidating Zeeman's whole modeling practice, predicting that this might cause a "backlash" among "positivist-minded Scientists."[241] For Thom, catastrophe theory offered no more than a tool that could be used "to clear all sciences of old, biologically deeply inrooted concepts, and replace their fallacious explanatory power by the explicit geometric manipulation of morphogenetic fields." With panache, he added: "The only possible theorisation is Mathematical."[242] The problem of course lay in the fact that the mathematics for his program did not exist.[243]

c) The Twofold Way: The Heart of Modeling Practices

In 1975, by introducing the notion of "the two-fold way of catastrophe theory," René Thom brought a welcome clarification to his own conception of catastrophe theory. Turning a major weakness into "one of the nicest features of catastrophe theory," he argued that the variety of levels of rigor allowed by his theories had to be clearly

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240 R. Thom, "Answer to Zeeman's Reply," 384-385. My emphasis.
241 R. Thom, "Answer to Zeeman's Reply," 387.
242 R. Thom, "Answer to Zeeman's Reply," 389.
243 The next year, in 1975, Thom would admit that "in pure mathematics, CT seems to have now reached a stage where its future looks very uncertain." R. Thom, "The Two-

distinguished. There were two extreme "ways" in which catastrophe theory might be applied:

> Either, starting from known scientific quantitative laws (from Mechanics or Physics), you insert the CT formalism (eventually modified) as a result of these laws: this is the *"physical" way.* Or, starting from a poorly understood experimental morphology, one postulates "a priori" the validity of the CT formalism, and one tries to reconstruct the underlying dynamics which generates this morphology: this is the *"metaphysical" way.*[244]

This dichotomy had been present in Thom's work for some time already. He expressed more or less the same when he contrasted the "reductionist" approach with the "structural" one.[245] But with the twofold way, I contend, Thom argued in terms of practice, rather than in terms of philosophy.

This twofold way of thinking about catastrophe theory therefore offers us a powerful inroad into the conceptualization of modeling practice.[246] As described in Chapter I, modeling practices include many things. One has to select the phenomena one thinks amenable to the tools one uses; one must be in a position to make sense of the results one achieves. Thom's twofold way clearly stated that, only by using topological techniques such as those provided by catastrophe theory, nothing insured that a same practice was shared.

Fold Way of Catastrophe Theory," *Structural Stability, the Theory of Catastrophes and Applications,* ed. P. Hilton (Berlin: Springer, 1976): 235-252, 236.

[244] R. Thom, "The Two-Fold Way of Catastrophe Theory," 235. My emphasis.

[245] See, e.g., R. Thom, "Structuralism and Biology," *Towards a Theoretical Biology,* 4, ed. Conrad Hal Waddington (Edinburgh: University of Edinburgh Press, 1972): 68-82; *MMM,* 1971 ed., 14-18; and "La linguistique, discipline morphologique exemplaire," *Critique,* 33(322) (March 1974), 235-245. See Chapter III.

[246] This way of thinking seems to have been adopted, in particular, by G. Israel, *La Mathématisation du réel* (Paris: Seuil, 1996).

The main difference between the two ways lay in the relationship one entertained with the dynamical variables of the substrate. In some cases—the "physical" way—these were given by well-developed bodies of preexistent theories. A topologically-inspired modeling practice attempted to find the morphologies exhibited by the solutions to specific dynamical equations. The knowledge extracted from such an analysis was not an explicit solution, but an inventory of allowed phenomena. The objection raised by specialists of different fields was then that often this knowledge could be inferred directly from the equations and that there was no need for the heavy apparatus of catastrophe theory. Studies in chaos theory however demonstrated that, in some cases, this approach could be fruitful (Chapter VII and VIII).

On the other hand, the "metaphysical" way started with "an empirical morphology" with no clear theoretical principle to support it. In this case, all was open to the speculation of the modeler. One had to define, in the case of elementary catastrophe theory which Thom preferred, the morphogenetic fields, the "chreods," and interpret them as local fields of some catastrophe. Then a dynamical explanation of the processes from one catastrophe to another might be attempted. Catastrophe theory became a theory of analogy, offering, "for the first time since Aristotelian Logic, a new way of constructing and interpreting analogies."[247] This was an infinite distance away from standard modeling practices.

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[247] R. Thom, "The Twofold Way," 250.

d) Critiques and Attacks: A Social Phenomenon?

By moving towards the second "way," by acknowledging that it seemed to him "far more promising than the first, if less secure," Thom further detached his modeling practice from that of other modelers: Zeeman, Smale and chaos theorists.[248] The controversy regarding Thom's modeling practice went back a long way. Already in 1971, Saunders Mac Lane wrote of Thom:

> There is some controversy as to the significance of his work, but it excites interest everywhere, for example and especially in the Department of Theoretical Biology at my own university [University of Chicago].[249]

In 1971, at his IHÉS seminar, Thom raised Guckenheimer's objections to catastrophe theory.[250] A student of Smale's, who had visited the IHÉS in 1970, John Guckenheimer published his objection in the proceedings of the 1971 Bahia Symposium. In essence he noticed that the relation between the unfolding of potential functions and the bifurcations of gradient system was not clear. That one could move from one to the other was an hypothesis that lay at the very heart of Thom's elementary catastrophe theory. "The point which we raise here is that the *mathematics* of the situation is not sufficient to justify this assumption."[251]

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[248] R. Thom, "The Twofold Way," 235.

[249] Lettre de Saunders Mac Lane to Harrison Brown (23/2/71). Arch. IHÉS. Original English.

[250] *Rapport scientifique, Année 1970 - Séminaires et conférences,* 6. Arch. IHÉS.

[251] J. Guckenheimer, "Bifurcation and Catastrophe," *Dynamical Systems,* ed. M. M. Peixoto (New York: Academic, 1973): 95-109, 96. Original emphasis. I do not go in the details, but let me mention the important finding, which was that a vector field existed, which was a structurally stable perturbation of grad *f*, without being equivalent to the gradient of any function in the universal unfolding of *f*. See Smale's comment about this objection in his review of E. C. Zeeman, *CT,* in *Bulletin of the American Mathematical Society,* 84 (1978): 1360-1368; rep. *MT,* 128-136.

This point was well taken by Thom, but he did not think that it posed an insurmountable challenge to his general theory.[252] The question Guckenheimer raised was linked with a more general one concerning the relation between mathematics and the modeling practice afforded by catastrophe theory. It emphasized the difficulty in achieving anything resembling Mather's theorem for the stability of mappings in the more intricate, and probably more important, case of general dynamical systems. Guckenheimer remained pessimistic about whether this could be achieved.

As we have seen, mathematics alone, at least in the form of a rigorous classification of all possible cases, had ceased to play an important role in Thom's conception of catastrophe theory. Like Andronov, Thom indeed preferred to fall back on philosophical arguments explaining why the mathematical categories he used were the most useful for creating and interpreting models of the world.

From the above, it hardly seems surprising that a full-fledged attack on catastrophe theory would become public in 1977.[253] It was led by applied mathematician Héctor Sussmann of Rutgers University, a disenchanted promoter of catastrophe theory.[254] Sussmann gave a critical talk at the 1976 Meeting of the Philosophy of Science Association.[255] Word came out of his critique in the April 15, 1977, issue of *Science.*[256] Sussmann, and his associate Raphael Zahler, later published two contentious articles, in

[252] R. Thom, *MMM,* 1971 ed., 72.
[253] However, see J. Croll, "Is Catastrophe Theory Dangerous?," *New Scientist,* (17 June 1976), 630-632.
[254] H. J. Sussmann, "Catastrophe Theory," *Synthese,* 31 (1975): 229-270.
[255] Lettre de H. J. Sussmann à E. C. Zeeman (14/10/77). Copy in Arch. IHÉS.
[256] G. B. Kolata, "Catastrophe Theory: The Emperor Has No Clothes," *Science* (April 15, 1977), 287, 350.

Nature and the philosophy journal *Synthese.*[257] At issue was not the validity of the theory as a branch of pure mathematics, there carefully distinguished as the *theory of singularities;* only its use, especially in the social and biological sciences, was harshly criticized.

> We are excited about the prospects of new applications of mathematics, and concerned that many will be disenchanted with all modern mathematics when they discover, as we have, that catastrophe theory is a blind alley.[258]

Their critique focused on the misuse of mathematical tools, above all so-called "Thom's theorem," used to justify arbitrary extrapolation. This attack was directly aimed at Zeeman's modeling practice. "Catastrophe theorists have," Sussmann and Zahler wrote:

> – misused the basic mathematics in ways that lead to indefensible arguments;
> – offered models which are based on unreasonable assumptions and which lead to implausible conclusions;
> – made predictions which are frequently vacuous, tautologous, vague, or impossible to test experimentally.[259]

Strong indictments indeed! Albeit about many social aspects including style and presentation, Zahler and Sussmann's argument against catastrophe theory was based on what they perceived as breaches to the proper practice of scientific modeling. They simply could not accept the modeling practices of catastrophe theorists, because they felt that their assumptions were "unreasonable," their use of mathematics "indefensible," and their predictions useless, or even wrong. Nor were they willing to see catastrophe theory

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[257] R. S. Zahler and H. J. Sussmann, "Claims and Accomplishment of Applied Catastrophe Theory," *Nature,* 269 (1977); 759-763. H. J. Sussmann and R. S. Zahler, "Catastrophe Theory as Applied to the Social and Biological Sciences: A Critique" in *Synthese,* 37 (1978), 117-216. A. Woodcock and M. Davis, *Catastrophe Theory,* and T. Tonietti, *Catastrofi,* study the scientific, philosophic and social aspects of the controversy.

[258] R. Zahler and H. Sussmann, "Claims and Accomplishments," 759.

[259] H. Sussmann and R. Zahler, "Catastrophe Theory," 118.

as introducing a theory of modeling practices. For Sussmann and Zahler, it was only "an attempt to approach science by trying to impose a preconceived set of mathematical structures upon the world, rather than by means of the experimental method."[260] As Zeeman was the most willing to confront his models with the laboratory, this last point might have been somewhat unfair.

In social terms, Thom often contended that catastrophe theory threatened the interests of applied mathematicians. While this claim has never been satisfactorily substantiated by anyone, we may still notice that the authority and status of applied mathematicians in the academic world were built upon the belief that their modeling practices were the most suitable way of constructing the mathematical models that other realms of science were longing for. But Thom and Zeeman were asking to revise these modeling practices in favor of new ones that were hard to learn. So, said Thom, "it was a corporate reaction: the whole community of applied mathematicians rose up against [catastrophe] theory."[261] Thom went as far as claiming that "the interests of the computer industry [were] perhaps not entirely foreign to this state of affair."[262] Nevertheless, the question raised by Sussmann and Zahler was there to stay: what scientific use, if any, could be found for a qualitative, nonpredictive mathematical theory? The net result was the progressive abandon of catastrophe theory, until it almost died from a lack of practitioners.

Whether or not we may interpret, like Thom, the attacks on catastrophe theory as a clash between scientific communities, an important fact remains: Smale, whom we have

260 H. Sussmann and R. Zahler, "Catastrophe Theory," 208.
261 R. Thom, *Prédire n'est pas expliquer,* 45.

seen as someone who contributed to the emergence of a set of modeling practices of which catastrophe theory was a representative, also bitterly opposed Zeeman on this very issue. In this case, I find myself in the odd position of arguing, against a scientist, for the inadequacy of too simplistic a sociological argument to explain the fall from grace of catastrophe theory. Much more study is needed to establish this claim.

e) The Smale-Zeeman Debate

On February 22, 1977, Stephen Smale addressed an embarrassed letter to Christopher Zeeman: "I guess it is about time I wrote to you of the latest, including my thoughts, on catastrophe theory and your role especially." In effect, Smale told Zeeman that he wanted to be clear on the critiques he had concerning his modeling practice.[263] Inviting Sussmann to speak at Berkeley in January 1977, in front of 150 people, Smale painted an "energetic canvassing of Sussmann's paper," which was quite devastating for Zeeman's approach.[264] "His talk had an impact here; you had no defenders," Smale wrote Zeeman.[265]

By that time, the mathematics department at Berkeley was engaged in series of seminars, which proved very important for the chaos theory of turbulence.[266] The

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262 R. Thom, "Structural Stability," *SIAM Review,* 196.
263 Lettre de S. Smale à E. C. Zeeman (22/2/77). Copy circulated by Zeeman in Arch. IHÉS.
264 Lettre de E. C. Zeeman à S. Smale (4/8/77). Arch. IHÉS. Smale had already given critical talks about catastrophe theory at the University of Chicago in 1974, and at the Aspen Institute of Physics in 1975. See S. Smale, Review of E. C. Zeeman, *CT,* in *Bulletin of the American Mathematical Society,* 84 (1978): 1360-1368; rep. *MT,* 128-136, 128.
265 Lettre de S. Smale à E. C. Zeeman (22/2/77). Arch. IHÉS.
266 See the proceedings: J. E. Marsden and M. McCracken, eds., *The Hopf Bifurcation and Its Applications* (New York: Springer, 1976); and A. Chorin, J. E. Marsden, and S.

modeling practices promoted there were closer to Ruelle's (Chapter VII). At Berkeley, the catastrophe craze, if it had ever occurred, was over. Chaos was taking its place, and the Berkeley seminars were important in promoting it.

In a series of letters from February to October, 1977, later circulated by Zeeman, a debate took place between him and Smale, which highlights many aspects of the divergence in modeling practice among 'applied topologists'. Smale had "a lot of mixed feelings" in writing to Zeeman. At first, he made two main accusations concerning the picture Zeeman had painted of catastrophe theory, and his lack of involvement with the scientific domains to which he was offering models.

> I think you (more than anybody) have created a false picture of catastrophe theory, which has for the moment made it the most fashionable thing in mathematics in the popular mind. I believe this will collapse.

Smale's most severe grief against Zeeman concerned his practice. "*[Y]ou have entered a subject starting with the model, not starting with what that subject required.*" Smale accused Zeeman of not being involved enough with the subject to which he was suggesting models.

> I believe also you have suffered a major weakness of Thom['s] on these things; jumping from application to another, never staying long enough to meet the weaknesses of that application; [n]ot going enough into that particular subject, its traditions, its experiments. . . . Also on the question of experimental verification, your taking the role of mathematician, not responsible for such questions, put me off.

Was Smale's criticism fair, especially when himself could have been accused of committing similar crimes? Smale's modeling practice, as described above, was not devoid from a tendency to jump from one application to another, without much concern

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Smale, organizers, *Turbulence Seminar: Berkeley 1976/77,* Lecture Notes in for experiments, but as opposed to Zeeman, Smale was always careful to pull the focus of his papers back to his own strengths, that is, pure mathematics. More than Zeeman's, Smale indeed tried to avoid stepping on other people's turf.

In July, Zeeman received Sussmann's visit at Warwick. At first, Zeeman did not taken Smale's criticisms very seriously.[267] After Sussmann's visit, Zeeman claimed that the applied mathematician had agreed that Zeeman's mathematics was correct, that his use of Thom's theorem was justified, and that the only criticism remaining was about his usage of biological words. This is related to Zeeman's often loose identification between dynamical variables and biological ones.[268] However, none of Sussmann's admissions ever appeared in print.

In September, Smale added more precision to his critiques of Zeeman's use of catastrophe theory. He raised three specific points, the last being a repetition of earlier concerns: the relation between local and global inferences allowed by elementary catastrophe theory; Zeeman's "super-great claims for cat. Theory;" and his ignorance of the history of applied work and works of others.[269] Smale also wrote that he thought that the Hopf bifurcation alone lay "deeper" than Thom's elementary catastrophe theory.

On October 4, Zeeman wrote a lengthy letter carefully responding to Smale's criticisms and presenting his philosophy for the modeling of natural phenomena.[270] Concerning the Hopf bifurcation, Zeeman contended that it was part of generalized catastrophe theory. For Smale, however, this generalized portion of catastrophe theory did

Mathematics, 615, ed. P. Bernard and T. Ratiu (Berlin: Springer 1978).
[267] Lettre de E. C. Zeeman à S. Smale (17/4/77). Arch. IHÉS.
[268] Lettre de E. C. Zeeman à S. Smale (4/8/77). Arch. IHÉS.
[269] Lettre de S. Smale à E. C. Zeeman (4/9/77). Arch. IHÉS.

not exist since the mathematics was not complete. Concerning the lack of reference to previous works, Zeeman confessed:

> I agree that I have often ignored the work of others, but this is because I am a slow reader with a poor memory, and consequently ignorant in many subjects. I apologize for this, but do not see how to remedy it in my lifetime, other than to take humble notice every time I am corrected.

Like Thom, Zeeman clearly distinguished between description and explanation. He even draw a little graph to explicit the difference.[271]

> I do indeed, as you say at the beginning of your letter, "use ECT to make global inference", but wherever I do it as an *explanation,* rather than as a *description,* I *prove* the globality rigorously from mathematical hypotheses, that are themselves translations of acceptable scientific hypotheses. I claim that, in so doing, I am doing rigorous mathematics & good science.

The global inferences in Zeeman's modeling practice were dictated by his use of "Thom's theorem." This was the basis for the repeated criticism that he had mathematics dictating how reality should be modeled. In Zeeman's view, his inferences were always motivated by the theorem, using "suitable mathematical hypotheses, themselves translations of scientifically-acceptable scientific hypotheses."[272]

Zeeman went to Berkeley in the fall of 1977, and had an explanation with Smale, which, apparently did not convinced the latter. Indeed, the next year, Smale published a devastating review of Zeeman's *Catastrophe Theory* in the *Bulletin of the American Mathematical Society.* He did not accept Zeeman's laziness as an excuse for neglecting

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[270] Lettre de E. C. Zeeman à S. Smale (4/10/77). Arch. IHÉS.
[271] Lettre de E. C. Zeeman à S. Smale (4/10/77). Arch. IHÉS. See Chapter I.
[272] Lettre de E. C. Zeeman à S. Smale (4/10/77). Arch. IHÉS. Zeeman later published a similar defense against Smale's attacks: "Controversy in Science: On the Ideas of Daniel Bernouilli and René Thom," *Nieuw Archief voor Wiskunde,* 4th ser., 11 (1993): 257-282.

previous literature. Discussing a model introduced by Isnard and Zeeman for the levels of military activity in a society, Smale wrote:

> There is much theory and data from history and social sciences relevant to the model of Zeeman and Isnard. None of this finds its way into the paper directly or indirectly save for brief references to Tolstoy on calculus in *War and Peace* and [Konrad] Lorenz, *On Aggression.*[273]

Smale also made painfully clear that only elementary catastrophe theory was mathematically complete (saying that Guckenheimer's objection made even this claim open to debate). Generalized catastrophe theory did not exist as a mathematical theory. Therefore, it could not be used as a guideline for the practice of building models. He found the repeated appeal to the "deep classification theorem of Thom" both misleading and dangerous. This was "mystification."

In summary, while he still lobbied for the use of modern calculus and geometric techniques in the sciences, Smale thought that catastrophe theory had not lived up to its promises, and was calling for more study on dynamical systems theory. Smale's modeling practice was now informed by what was going on around the nascent theory of chaotic systems. There, no classification theorem was in sight, but there was much mathematical work to be done in studying the structure of strange attractors, and much experimental work to be attempted in trying to find them in nature. As chaos studies developed, this later direction was to be widely followed by mathematically-inclined scientists. As Zeeman wrote, the mathematical soundness of his catastrophe theoretic approach was not relevant anymore. History was deciding in favor of chaos.[274]

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[273] S. Smale, *MT,* 132.

[274] "I won on mathematical grounds & he [Smale] won on historical grounds." Lettre de E. C. Zeeman à Nicolaas Kuiper (14/12/77). Arch IHÉS.

7. CONCLUSION

In this chapter I described the important activity that took place around René Thom at the Institut des hautes études scientifiques, roughly from 1964 to 1977. I made clearer the interplay between Thom's earlier research program in pure mathematics and the works of Arnol'd, Malgrange, Mather, Smale, and Zeeman. All of them played an important role in helping Thom to refine his more ambitious goal of reforming the modeling practices of the sciences.

From the above, it appears that the IHÉS, as an institution, had a significant part in shaping the outcome of Thom's catastrophe theory. On the positive side, the renown of this institute helped Thom get into close contact with leaders of the fields he touched upon. Because of its structure and size, it moreover provided a perfect meeting ground for topologists looking for applications of their skills to the modelization of natural phenomena and mathematically-inclined physicists. There were few places in the world where such a process could have happened. One may here recall the words of Ralph Abraham, as reported by James Gleick:

> When I started my professional work in mathematics in 1960, . . . modern mathematics in its entirety—in its *entirety*—was rejected by physicists, including the most avant-garde mathematical physicists. . . . These people [i.e. physicists and mathematicians] were no longer speaking. They simply despised each other. Mathematical physicists refused their graduate students permission to take math courses from mathematicians: *Take mathematics from us. We will teach you what you need to know. The mathematicians are on a terrible ego trip and they will destroy your mind.* That was 1960. By 1968 this had completely turned around."[275]

On a negative side, however, the IHÉS, of course with Thom's support, also forbade some directions to be explored. In particular, no experimental work could be even

[275] Quoted in J. Gleick, *Chaos,* 52. Original emphasis.

envisaged in this setting. Moreover, computer facilities at the IHÉS were *nonexistent.* As late as 1973, numerical computations had to be done at Orsay![276] This condemned all such attempts in advance. Albeit showing some indirect, but not often explicit, concerns for the possibilities created by the computer, the activities at the IHÉS therefore remained purely theoretical until very late in the story.

Was thus Thom's a research school in the classic sense of historians of science? Besides the purely nominal question, this query helps us locate some of the IHÉS's institutional weaknesses that hindered a successful diffusion of its modeling practices. Using the subjective technique published by Geison in 1981, one may attribute about 8 pluses (corresponding to its favorable features) to Thom's school, and perhaps more to Smale's and, later, Zeeman's.[277] The main reason for this difference is that the IHÉS was not part of the university system. So, although intermittently present, students did not constitute a primary feature of this elitist institution. Also, the always-tight budget of the Institute further hindered the development of an important, permanent research group at Bures-sur-Yvette.

Thom's school may be considered a failure in that catastrophe theory, failing to gather a community well focused on some research agenda, was rejected. With Thom's tendency to work on so many, spread out topics, his personal character did not help the success of his school. For personal, institutional, and financial reasons, Thom's school therefore always had to rely on other breeding groups for the production of a generation of followers.

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[276] *Notes de séance* de N. Kuiper, *Comité scientifique* (24/3/73). Arch. IHÉS.

In the above, I have however argued that the modeling practices he promoted were widely taken up by others. Zeeman's research school did produce an important, more tightly connected group of mathematicians who became quite prominent. The demise of catastrophe theory around 1977 was however detrimental to its final fortune. Smale's school was even more successful in training a generation of competent mathematicians with a clear focus, but Smale himself chose to forgo purely mathematical concerns around 1970 in order to deal with applications. As a result, his dynamical systems school was not sustained, as such although many of his students became major developers of the mathematical techniques of chaos theory. Of course, much more research is needed in order to get a clearer picture of their respective history. As a net result, however, sustained communication among Bures, Berkeley, and Warwick lay the ground for the establishment of a flourishing research network focused on the exploration of qualitative dynamics. Of this network, I showed that, when we focus on modeling practices as opposed to purely mathematical concerns, the IHÉS provided the initial glue.

In addition, the modeling practices explored by this research network were to have a wide impact on many fields of science. Of course, they had to be accommodated and adapted by members of different communities, but the initial work pushed forward by members of the network was frequently exploited. Its indirect impact was therefore enormous, as Floris Takens witnessed:

> Exaggerating somewhat, one can say that where applied mathematicians used to be confined to investigate the equations, and their solutions, given by the accepted

[277] G. L. Geison, "Scientific Change," 24. He lists 14 criteria for the success of a research school.

> mathematical models for the different phenomena, the work of Zeeman showed a much more liberal attitude towards the choice of these models.[278]

This is quite an exaggeration, indeed. But by replacing Zeeman's name by members of the network, this affirmation becomes closer to the historical truth. The process by which the network's modeling practices were, not only received, but *adapted,* in other disciplines is in no way simple. In the next chapter, I explore how Ruelle and Takens's theory of turbulence came out from the activities of the network, how the physicist's concerns pulled them in a sidewise direction, and finally how it could be confronted with an already-well established subspecialty within fluid mechanics, called *stability theory.*

[278] F. Takens, "The Work of Professor Sir Christopher Zeeman FRS," *Nieuw Archief voor Wiskunde,* 11 (1993): 251-256, 256.