# CHAPTER I: INTRODUCTION

> Almost no one has the courage to do a careful anthropological study of formalism.
> —Bruno Latour.[1]

Amidst the social and political turmoil of May 1968, train workers being on strike, David Ruelle remained stuck for several hours on his way back from Strasbourg to Paris. A mathematical physicist working at the Institut des hautes études scientifiques (IHÉS), Bures-sur-Yvette, he opened an old textbook to kill time. The book was Landau and Lifshitz's classic on fluid mechanics. Ruelle disagreed with what he read.

From his objection to Landau, would come a new understanding of nonlinear dynamics. Two years later, Ruelle was ready to suggest a new explanation for why, in certain circumstances, fluid motions become "very complicated, irregular, and chaotic, [and] we have *turbulence.*"[2] Now widely known as *deterministic chaos theory,* or simply *chaos,* this mathematical understanding of natural phenomena aims at describing systems which follow simple deterministic rules, yet exhibit disorderly, apparently random behaviors. This dissertation tells the story of some of the scientists who, in a particular local context, significantly contributed to the elaboration of chaos theory.

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[1] B. Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge: Harvard University Press, 1987).

"The reason why I did not like Landau's description of turbulence," Ruelle later wrote, "is that it went against mathematical ideas I had heard in seminars by René Thom and studied [in] a fundamental paper by Steve Smale."[3] His colleague at the IHÉS, Thom had already been circulating in 1968 a first version of his still unpublished book *Structural Stability and Morphogenesis,* which introduced his views on what would soon be widely known as *catastrophe theory.* A mathematical theory, a method for modeling natural phenomena, and an ambitious philosophy of scientific knowledge, catastrophe theory provided tools for the description of systems which, depending on continuously varying internal parameters, nonetheless exhibited sudden qualitative changes of behavior. It moreover offered new ways to think about the role of mathematics in understanding the world, new ways to think about what mathematization may be for the natural and social sciences.

Most importantly for my story, however, Thom had gathered people around him at the IHÉS, attracting the best mathematicians from all over world, while striving to promote new practices for the modeling of natural phenomena. These practices were based on the most advanced mathematical techniques of topology. Among the frequent visitors of the IHÉS, mathematician Stephen Smale and his students at the University of California, Berkeley, figured prominently. From the very special encounter between these 'applied topologists', as I shall call them, and a physicist, in the local, specific, and in many ways idiosyncratic, culture of the IHÉS, a

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[2] D. Ruelle and F. Takens, "On the Nature of Turbulence," *Communications in Mathematical Physics,* 20 (1971): 167-192; 23: 343-344; repr. *Chaos II,* 120-147; *TSAC,* 57-84. Quote on p. 167. Their emphasis.
[3] D. Ruelle, *Chance and Chaos* (Princeton: Princeton University Press), 55.

transfer of topological techniques from pure mathematics to theoretical physics was nurtured. A result of this interaction was chaos.

Χαος: The ancient Greeks thought this god had been forever defeated at the beginning of times. Our universe had become a *cosmos,* not a *chaos.* It was ordered by laws and endowed with meaning, which they called the *logos.* For us mortals, the task seemed clear. In order to make sense of the world, we needed to uncover its hidden *logos.* As Thom wrote, "it is indisputable that our universe is not chaos."[4] But the old god was only sleeping. Today, he has reawakened and come back with a vengeance. His raucous name found a new incarnation in a popular scientific theory. Words do matter. By appropriating his name, *chaos theory* has acquired some of his power.

But this was not the first time the metaphor had been mobilized in a scientific context. Indeed, the Dutch chemist and physician Van Helmont (1577-1644) had borrowed the term in order to forge the word 'gas'.[5] In the twentieth century, the phrase 'molecular chaos' had sometimes been used to refer to the hypothesis of 'molecular disorder' introduced by Ludwig Boltzmann (1844-1906).[6] In 1938, Norbert

[4] R. Thom, *Structural Stability and Morphogenesis,* transl. D. H. Fowler (New York: Benjamin, 1975), 1.
[5] P. Thuillier, "La revanche du dieu Chaos," *La Recherche,* 22 (May 1991): 542.
[6] L. Boltzmann, *Lectures on Gas Theory* (Berkeley: University of California Press, 1964); and T. S. Kuhn's discussion in *Black-Body Theory and the Quantum Discontinuity, 1894-1912,* 2nd ed. (Chicago, 1987), chapter 2. There is a subtle distinction between 'molecular disorder' and 'molecular chaos'. See Paul and Tatiana Ehrenfest, *The Conceptual Foundations of the Statistical Approach in Mechanics* (Ithaca: Cornell University Press, 1959), 40-42, n. 161.

Wiener (1894-1964) had coined the very phrase 'chaos theory' for what is now much more prosaically referred to as the study of stationary random measures.[7]

Never before the 1970s, however, did chaos become a label for a worldview, picked up by mathematicians, physicists, biologists, chemists, and other scientists, and much commented on by philosophers, intellectuals, and a wide audience of cultivated people in tune with recent scientific developments. More than just a new scientific theory, chaos was a crystallization of new practices for the modeling of natural phenomena; it drew attention back to work that dated from the beginning of the century; and it was deemed a revolution in the sciences. Historical understanding of its wide popularity from the 1970s through the present requires an appeal to a wide array of scientific, institutional, cultural, and social factors which provided the conditions for its emergence.

## 1. A CULTURAL HISTORY OF CATASTROPHES AND CHAOS

This work is thus an attempt at providing a local cultural history, as complete as possible of the conditions that enabled René Thom and David Ruelle, in the context of the IHÉS, to come up with new ways of modeling natural phenomena. To achieve this goal, it tries to synthesize many levels of historical analysis. It looks at the mathematical theories grounding what have been called catastrophe theory,

[7] N. Wiener, "The Homogeneous Chaos," *American Journal of Mathematics,* 60 (1938): 897-936; repr. *Selected Papers of Norbert Wiener* (Cambridge: MIT Press, 1964) N. Wiener and A. Winter, "The Discrete Chaos," *American Journal of Mathematics,* 65 (1943), 279-298; repr. *Mathematical Review*, 4 (1943), 220. Both article also in N. Wiener, *Collected Works With Commentaries* (Cambridge: MIT Press, 1976). See B. McMillan, "Norbert Wiener and Chaos," *A Century of*

(deterministic) chaos theory, and their histories. It describes the ways they were linked historically and conceptually with the scientific disciplines from which they emerged, be they part of topology, biology, linguistics, or physics. It examines the flow of practices, as opposed to mathematical concepts and theories, and their adoption and adaptation by various actors. It studies the ways in which institutional settings, both formal and informal, played a role in the development, diffusion, and reception of catastrophe and chaos theories, and in this context, pays special attention to the history of the IHÉS. Finally, it provides clues for a concrete grounding of some resonances that seem both obvious and very hard to discuss convincingly between, on the one hand, catastrophe and chaos theories, and, on the other, some contemporary social and cultural issues. These may include such things as concerns about the social role of mathematics, which became widespread after May 68, and its political undertones, the rise and fall of structuralism, the popular successes of both catastrophe and chaos theories.

The picture I strive for therefore involves a wide variety of resources on which scientists draw in their innovative activities. Resources include mathematical definitions, theorems, and theories, but also the specific practice and reference people use when dealing with these mathematical resources. Resources are provided by disciplinary and institutional settings, or by the wider culture. Not all resources are

*Mathematics in America,* 2, ed. Peter Duren (Providence: American Mathematical Society, 1989): 479-491.

equally available to everyone, but it is essentially up to individual scientists to chose among those afforded by their situations.[8]

On the main questions the historian wishes to address, I believe, is: How can one coherently integrate all of these resources, used by actors in a variety of contexts and at a variety of levels? Clearly, there are flows of resources among individuals, scientific disciplines, and institutions, flows from one scientist to another, to groups of scientists, or to bodies of texts or social and cultural entities, and vice-versa. There are flows taking place within society and culture. Moreover, these flows, far from remaining unchanged, are constantly redefined, reinterpreted, and re-appropriated by various actors. But what exactly is flowing, being exchanged, transformed, adopted and adapted in processes of innovation?

To integrate this variety of resources into a single story has been difficult. Inadequacies will be apparent. One should strive to be as precise as possible when treating the flow of information, practice, or reference. In order to achieve this precision, one needs appropriate heuristic notions that can clearly identify these flows. Since I found few existing models in the historical literature, I was led to introduce new terms and notions, which to be sure overlap with many of the tools generally used by historians, but were not to be found as such in the literature.

I propose to look at practices. But, since in the cases here studied, most practices have to do with mathematical theories and models of natural phenomena, I

[8] For a discussion of resources, see M. Norton Wise, "Forman Reformed;" and "Under the Influence," both unpublished manuscripts, the latter of which was delivered at the History of Science Annual Meeting in San Diego (1997); for an earlier version of this

have chosen to call these practices, *modeling practices,* a choice I shall motivate below. What seem to flow from the wider culture to the scientific activity of an individual, and back, are more easily described as *ideas, representations,* or *metaphors.* Because these terms often are too loosely used these days, I felt the need to introduce a new one: *cultural connectors.* As I defined them, cultural connectors are more or less explicit references used by actors in the processes by which they appeal to different spheres of culture in order to argue for their own case.

Constant interactions among mathematical concepts, modeling practices, and cultural connectors take place through individuals, but they are often subtly enmeshed together. For instance scientific concepts often come with specific practices for using them.[9] Similarly, concepts labeled by such potent names as catastrophe or chaos hardly are not without giving rise to cultural resonances. Clearly, these notions (concepts, practices, cultural connectors) are never so easy to distinguish unambiguously.

In view of recent historiographies, there might be a contentious point in the picture I draw here, namely that all flows go through the individual. Has not the historiography of the last decades, picking up on the *Annales* School in particular, finally succeeded in moving away from nauseating hagiographies, which traditionally

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scheme which however does not emphasize the notion of resources, see M. N. Wise, "Mediating Machines," *Science in Context,* 2 (1988): 77-113.

[9] One may here quote, as Andrew Warwick does, Wittengstein's authoritarian stance: "'To give a new concept' can only mean to introduce a new employment of a concept, a new practice." L. Wittgenstein, *Remarks on the Foundations of Mathematics,* ed. G. H. Wright, R. Rhees, and G. E. L. Anscome, transl. G. E. L. Anscome (Oxford: Blackwell, 1967), 195e; quoted by A. Warwick, "Cambridge Mathematics and

were so common in the history of science? I would argue that individuals remain the main mediators through which practices, concepts, and cultural connectors flow, at the same time as they are the ones who can innovate with them. Historians can study these processes with the same critical stance that they adopt when following other approaches.[10] By studying these dynamical processes that take place around individuals, located in specific cultures, I hope to achieve a fuller historical picture of the emergence and development of catastrophe and chaos theories.

## 2. CULTURAL CONNECTORS

Cultural history of science ought to strive for an understanding of the subtle connections between individual scientific activities and the society in which they take place, not only at a social, institutional, and political level, but also at the more diffuse level of culture, taken in its widest sense. In order to present a compelling argument, it is however necessary to go beyond metaphors and analogies. However appealing some connections may appear at first sight, how can we assess whether enough evidence has been presented? Just how many astonishing coincidences will suffice for a story to be plausible? This often remains problematic. Some historians of science have recently been able to articulate such connections convincingly by focusing on social units naturally well circumscribed. But the study of the cultural resonances brought about by terms, like 'structures', 'catastrophe', or 'chaos', used in mathematics

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Cavendish Physics: Cunningham, Campbell and Einstein's Relativity, 1905-1911," *Studies in History and Philosophy of Science,* 23 (1992): 625-656, 625.

[10] Note that even *Annales* historians have moved back to the writing of biographies: e.g. G. Duby, *Guillaume le Maréchal, ou le meilleur chavalier du monde* (Paris: Fayard, 1984).

and the sciences but also in wider cultural discourse, requires a much more diffuse framework, a Protean notion of cultural connection.[11]

Are we to fall back on *Zeitgeist*? Vague notions such as this one have the benefit of attributing the convergence of several types of discourse to a higher level of analysis, a shared set of values, metaphors, and sensibilities, and counter claims of hegemony of one domain over another.[12] However, it is achieved only at the expense of establishing a higher level of hegemony, located in an entity that does not even exist. The mechanisms by which *Zeitgeists* arise, gain prominence, are sustained, and fade away are rarely addressed. The way they are incorporated into the thinking and practice of individuals remains mysterious.[13] Inspired by recent social and historical studies of science, I choose as much as possible to locate my explanatory heuristic

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[11] See, for example, M. Biagioli, *Galileo, Courtier: The Practice of Science in the Culture of Absolutism* (Chicago: University of Chicago Press, 1993); É. Brian, *La Mesure de l'État. Administrateurs et géomètres au XVIIIe siècle* (Paris: Albin Michel, 1994); L. Daston, *Classical Probability in the Age of the Enlightenment* (Princeton: Princeton University Press, 1988); P. Galison, "The Ontology of the Enemy: Norbert Wiener and the Cybernetic Vision." *Critical Inquiry,* 21 (1994): 228-266; S. Schaffer, "Accurate Measurement an English Science," in *The Values of Precision,* ed. M. N. Wise (Princeton: Princeton University Press, 1995): 135-172; C. Smith and M. N. Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989); M. N. Wise, "Work and Waste: Political Economy and Natural Philosophy in Nineteenth-Century Britain (1-3)." *History of Science,* 27 (1989): 263-301 and 391-449; *Ibid.,* 28 (1990): 221-261.

[12] Albeit much more sophisticated, Michel Foucault's *epistemes* also fall in this category. See *The Order of Things: An Archeology of the Human Sciences* (New York: Pantheon, 1970); and *The Archeology of Knowledge,* transl. A. Sheridan (New York: Pantheon, 1972).

[13] An example of how much confusion can derive from an unsophisticated understanding of processes of cultural connection is provided by a recent book: A. Sokal and J. Bricmont, *Impostures intellectuelles* (Paris: Odile Jacob, 1997). I should emphasize that the confusion I myself deplore lies in their method, which prevents them from seeing the cultural meanings of the loose use of mathematics they criticize, and not the confusion of the authors they discuss.

tools at the level of actors. This choice displaces the causal agency from discourses to the actors themselves. Instead of passively receiving cultural 'influences', scientists actively forge cultural connections.[14] Cultural connectors, as I define them below, provide such a heuristic tool that may be useful in describing cultural resonances.[15]

Cultural connectors are more or less explicit references used by actors when they attempt, by drawing on parallels, analogies, metaphors, or full-fledged theories, to argue for a point, to strengthen the meaning of their work, or to increase the legitimacy of their methods and ideas.[16] Cultural connectors carry whole sets of meanings and practices which more or less happily flow between spheres of culture.

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[14] On the dangerous use of the notion of 'influence,' see M. N. Wise, "Under the Influence," unpublished manuscript; and "The Enemy Without and the Enemy Within," *Isis,* 87 (1996): 323-327.

[15] For a concrete example of my use of cultural connectors, see the following paper, which partly overlap with Chapter II below: D. Aubin, "The Withering Immortality of Nicolas Bourbaki: A Cultural Connector at the Confluence of Mathematics, Structuralism, and the Oulipo in France," *Science in Context* (Summer 1997).

[16] I thank David C. Brock for having suggested this term to me. He and M. Norton Wise recently argued that postmodernism could be seen as a cultural connector between "postmodern quantum mechanics" and contemporary culture. See "What is the Meaning of 'Postmodern Quantum Mechanics'," *Growing Explanations: Historical Perspective on the Sciences of Complexity,* ed. M. N. Wise (in preparation). The above definition however is a re-appropriation of my reading of their paper, as well as of the many discussions I had with them. They may not agree totally with what I suggest here. Other notions introduced by historians and philosophers partly overlap with that of cultural connector, namely "mediating machine" (M. N. Wise, "Mediating Machines," *Science in Context,* 2 (1988): 77-113); "collective statements [*énoncés collectifs*]" (A. Boureau, "Proposition pour une histoire restreinte des mentalités," *Annales. Economie, société, civilisation,* no. 6 (1989), 1491-1504); "wandering concepts [*concepts nomades*]" (I. Stengers, ed., *D'une science à l'autre. Des concepts nomades* [Paris: Seuil, 1987]); "trading zones" (P. Galison, *Image and Logic: A Material Culture of Microphysics* [Chicago: University of Chicago Press, 1997], esp. 803-844); "boundary objects" (S. L. Star and J. R. Griesemer, "Institutional Ecology, 'Translations', and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39," *Social Studies*

Superficially or not, cultural connectors enter widely different types of discourse and acquire their strength through constant reinforcement.

Cultural connectors therefore have two aspects that need special emphasis. First, they are used at a variety of levels: People establish connections through personal contacts, either ephemeral or winding up in intense collaborations; or by their citations, which are innocuous metaphors or essential concepts for thought or legitimacy; or by borrowing, translating, adopting and adapting whole bodies of knowledge into a new setting; or finally, by associating different cultural spheres in the context of a third discourse, often a philosophical enterprise, or even in the media. All of these levels are important. Second, although the diversity of levels at which cultural connectors are employed is bound to make the connection seem superficial, cultural connectors become more potent because they are used over and over again, and they link durably the spheres of culture they connect. A single instance of connection is not enough; it must be picked on, expanded on, argued for and against, etc. The connection becomes so widespread that an historical account can usefully be given not only for the plugging-in of the cultural connector, but for also its disintegration, not taken as an event, but as a process, in which actors are playing the central role.

## 3. MODELING PRACTICES

In science studies and recent historiography of science 'practice' has emerged as a healthy antidote to a theory-dominated vision of the development of science, and

*of Science,* 19 (1989): 387-420.); and "immutable mobiles" (B. Latour, *Science in* therefore has often been equated to *experimental* practice.[17] It will not come as a surprise, I imagine, that even those scientists, whose work mainly consists in imagining new theories and models, adapting old ones to new purposes, or investigating their theoretical consequences, follow some practice, in much of the same sense as experimenters working in the laboratory follow their own. These practices involve skills, or tacit knowledge, acquired through training, apprenticeship, and experience. And just as experimental practices, they are rarely articulated by practitioners themselves into a coherent vision. They can also differ from one individual to another.[18] It is worth emphasizing the obvious, namely, that there is more than one way of doing theory, more than one way of building models.

There is already an array of options offered by the literature in order to discuss the practice of scientists who mainly do theoretical work.[19] This confusing

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*Action*).

[17] Studies of experimental practices are to be found, e.g., in the contributions to D. Gooding, T. Pinch, and S. Schaffer, eds., *The Uses of Experiment: Studies in the Natural Sciences* (Cambridge: Cambridge University Press, 1989); A. E. Clarke and Joan H. Fujimura, eds., *The Right Tool for the Job: At Work in Twentieth-Century Life Science* (Princeton: Princeton University Press, 1992); Andrew Pickering, ed., *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992); R. E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Practice* (Chicago: University of Chicago Press, 1994); M. N. Wise, ed., *The Values of Precision.*

[18] About individual variations of experimental practices, see K. Jordan and M. Lynch, "The Sociology of a Genetic Engineering Technique: Ritual and Rationality in the Performance of the 'Plasmid Prep'," *The Right Tool for the Job,* ed. A. E. Clarke and J. H. Fujimura: 77-114.

[19] See L. Althusser, *For Marx* (New York: Vintage Books, 1970), 165-174; A. Pickering, *The Mangle of Practice: Time, Agency and Science* (Chicago, 1995), chapter 4; A. Pickering and A. Stephanides, "Constructing Quaternions: On the Analysis of Conceptual Practice," in *Science as Practice and Culture,* 139-167; A. Warwick, "Cambridge Mathematics and Cavendish Physics: Cunningham, Campbell and Einstein's Relativity, 1905-1911," *Studies in History and Philosophy of Science,* 23 (1992): 625-656; 24 (1992): 1-25. See also C. Rosental, *L'émergence d'un*

multiplicity of meanings presents the danger of eroding the benefits of the discussion. In the following, I will try to make clear distinctions between different types of non-experimental practices. They are: *conceptual practice,* a term introduced by Pickering and Stephanides, *theoretical practice,* introduced by Althusser, and employed recently in a very different sense by Warwick, and *modeling practice,* a term I introduce here in an attempt to better capture the relation of catastrophe theory and chaos, or of topologists and physicists.

### a) Modeling Practices: A Definition

In thinking about modeling practices, I have found it useful to go back to some older definitions provided by Louis Althusser, but taking them very much out of context. In "On the Materialist Dialectic," an essay written in 1963 and published in his famous book *For Marx,* Althusser gave the definition:

> By *practice* in general I shall mean any process of *transformation* of determinate given raw material into a determinate *product,* a transformation effected by a determinate human labor, using determinate means (of 'production').[20]

These are four elements that should indeed enter any discussion of practice: raw materials, means of the transformation, end-products, and labor. When talking about specific practices, the fourth element, labor or the agent of production, is crucial. Practice requires a bearer. I shall always assume that practice is the reflection of someone's membership in a well-defined social group. At the same time, a practice

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*théorème logique,* Doctoral Thesis (École de mines, Paris, 1996); and L. Hodgkin, "Mathematics and Revolution from Lacroix to Cauchy," *Social History of Nineteenth-Century Mathematics,* ed. H. Mehrtens, H. Bos, and I. Schneider (Stuttgart: Birkäuser, 1981): 50-71.

has to be embodied in a person with all his or her idiosyncrasies. Thus, practices are always actualized by particular people, either individually or as members of social or institutional groups. This means that although this element of practice is, in my view, the most important, it also is the only one that remains constant. People have the possibility to choose among different raw materials, means of transformation, or end-products, but they will always remain people. This means that cultural history is about people and the choices they are facing.

Althusser also introduced different kinds of practices (economic, social, political, etc.), further observing: "The existence of a *theoretical practice* is rarely taken seriously." But it did exist. Like any other practice it transformed a "raw material (representations, concepts, facts) which it is given by other practices."[21] However, theoretical practice "end[ed] in its own *product:* a *knowledge.*"[22]

When dealing with the product of a modeling practice, instead of speaking of a "product-knowledge" as Althusser does, we shall say that the product of a modeling practice is what the practice itself—or rather model-builders who follow the practice—consider to be 'knowledge'. Obviously, the kind of knowledge produced by a specific modeling practice does not need to be considered knowledge by people using other (modeling) practices. A modeling practice, however, includes tacit rules about what to consider proper knowledge, and what to reject.

Modeling practices are thus defined as the actual processes by which scientists transform some given material, selected by them, into a product which they hope will

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[20] L. Althusser, *For Marx,* 166. His emphasis.
[21] L. Althusser, *For Marx,* 167.

be considered knowledge. Just as the modeling practice informs the kind of product-knowledge that is acceptable, the tacit rules that one follows in the selection of the raw material susceptible of supporting a model is also part of one's own modeling practice. And, of course, the "means" used in order to achieve new knowledge are also unwritten aspects of the modeling practice. In other words, a specific modeling practice will inform all aspects of "the position, examination, and resolution" of a modeling problem.[23] I thus define a specific modeling practice by the assumptions that are used in order to start modeling (what to study, which data to consider, etc.), the tools that are used during the process (specific mathematical techniques, but also tables, lists, graphs, computer programs, etc.), and the sort of predictions or explanations that it will provide. Modeling practices, in this sense, are the set of all the techniques that enable scientists to build models of natural phenomena. At each stage of the practical process, resistance is usually encountered, and model-builders must accommodate it. They rethink their assumptions, go back to the original question, and sometimes fiddle with the modeling practice itself.

### b) Practice, Practices, and Conceptual Practice

However, as Andrew Pickering was already aware, there is a certain confusion and ambiguity in the contemporary uses of 'practice' among historians, sociologists, and

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[22] L. Althusser, *For Marx,* 173.
[23] L. Althusser, *For Marx,* 165. There is a difference between my scheme and Althusser's. The "position, examination and resolution" of a theoretical problem actually *is* what he calls a theoretical practice. For me, they are informed by the specific modeling practice of the person who decides to solve a problem (because one

philosophers of science.[24] Pickering noted that there were at least two common, and very distinct, uses of 'practice' in the literature. While *practice*-1, let me say, referred to the "work of cultural extension," as Pickering defined and used it in all of its generality, *practice*-2 "relates to specific, repeatable sequences of activities on which scientists rely in their daily work."[25] I find it useful to distinguish between *practice*-1 as a process without a specific human actor, and *practice*-2 as an actualization of this process at a particular time and place, by a particular human actor, himself or herself placed in a specific context as a member of some social group. As Pickering noted, *practice*-2 admits a plural, while *practice*-1 does not.

So, although Pickering himself has recently discussed *conceptual practice* as the work of extension of conceptual realms of science (mathematics or the modeling aspects of natural sciences), little has been done to understand how specific modeling practices are formed and transformed, adopted and adapted by theoreticians and model-builders in concrete cases. In the *Mangle of Practice,* for example, Pickering discusses the conceptual practices that informed Hamilton's construction of quaternions.[26] True to his focus on *practice*-1, however, Pickering analyzes conceptual practice as a process, during which Hamilton makes "associations," encounters "resistances," and seeks to "accommodate" them. I do not want to argue

knows from one's modeling practice that there are good chances that the problem is solvable, using one's modeling and theoretical practices).

24 Theories of practice already have a long history behind them. See especially P. Bourdieu, *Esquisse d'une théorie de la pratique* (Geneva: Droz, 1972); and a strong and pointed critique of recent theories of practice by S. Turner, *The Social Theory of Practice: Traditions, Tacit Knowledge and Presupposition* (Cambridge: Polity, 1994).

25 A. Pickering, *The Mangle,* 3-4.

with this description of practice as a process (*practice*-1). I will argue, however, that Pickering's scheme will not help us to conceptualize modeling practices as they are lived by the scientists (*practice*-2).

Similarly, Althusser's whole discussion of practice referred to processes and, as such, were similar to Pickering's *practice*-1. Again, my use of 'practice' is more along the lines of Pickering's *practice*-2, but I adopt, and adapt to my purpose, Althusser's definitions, with the caveat that the process in question is to take place in specific conditions, and carried out by individual human actors. I thus place 'practice' within a humanist setting that goes against much of Althusser's (as well as Pickering's) framework. I also differ from Althusser in intent. While my use of 'modeling practice' may illuminate some aspects of the history of mathematical modeling, he had more ambitious aims.[27]

### c) Theoretical Technologies

The phrase 'theoretical practice' has recently been taken up by Andrew Warwick and Claude Rosental, although their uses of it have little to do with Althusser's. By remaining closer to what theoreticians and mathematicians actually do, Warwick and

[26] A. Pickering, *The Mangle,* chapter 4. See also A. Pickering and A. Stephanides, "Constructing Quaternions."

[27] In this text, Althusser wished to demonstrate the scientific status of materialist dialectics, and solve the so-called 'demarcation problem' between the sciences and the rest. He expressed his aim as such: "science has to be defended against an encroaching ideology," he wrote. "[W]hat is truly science's and what is truly ideology's has to be discerned, . . . [and] the true theoretical practices that socialism, communism, and our age will need more and more, [have to be] established." *For Marx,* 172.

Rosental present a picture of theoretical practice inspired by recent studies of experimental practice.

Warwick's appropriation of the term 'theoretical practice' is especially germane to my discussion of modeling practice. In his essay on Cambridge mathematical physicist Ebenezer Cunningham's use of relativity as a way to further his own research agenda, Warwick has emphasized the role of the "practical skill-base" for understanding how English physicists coped with Einstein's innovations. He argues that the "theoretical technologies" used by Cunningham (educated in the Mathematical Tripos at Cambridge University) should be the starting point for our understanding of the interest he showed in Einstein's article, *not* the other way around. Thus, Warwick displaces the agency, from Einstein's paper to those who chose to comment on it.

Inspired by "revisionist studies in the history of experiments" which focused on the culture-specific aspects of experimental practice, Warwick has drawn attention to "what might be called the skill- or practice-ladenness of theory."[28] This recognition, he argues, has two implications for the historian of mathematical physics. It draws attention on the actual practices used by theoreticians and to the local cultural resources mobilized for this practice. "[W]e must differentiate between the idealized conceptual schema of a general theory, and the piecemeal steps actually followed by physicists in solving particular problems." He therefore employed the term "*theoretical technology* to describe pieces of theoretical work that are not constitutive

[28] A. Warwick, "Cambridge Mathematics," 631-633.

of a general theory, but which are used to solve particular problems and which are taken for granted by members of a local community."[29]

While Warwick's scheme puts a healthy emphasis on theoretical techniques (which we may equate with Althusser's means of production), it barely touches upon other aspects of Althusser's conception of practice emphasized above, namely raw material and end-product. While dealing with scientific innovation, the picture presented by Warwick significantly differs from mine in the sense that it eschews dealing with innovation in the practice itself. His practices are characteristics of groups of people and reproduce themselves from master to pupil. "The process by which [taken-for-granted theoretical] practices *are* actually transmitted remain to be investigated by historians of physics."[30] More generally, the process by which innovation occurs in such practices has scarcely been tackled by historians of science.

In the following, whenever the term 'theoretical practice' shall be used, it will be in the restricted sense of the activities described by Warwick as the use of "theoretical techniques." I have introduced the term 'modeling practice' in order to articulate a more global picture of the role of practice when mathematicians and scientists build models in order to describe or explain natural phenomena.[31] Moreover, while restricting the use of 'theoretical practice' to the activities taking place within a

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[29] A. Warwick, "Cambridge Mathematics," 630. His emphasis.
[30] A. Warwick, "Cambridge Mathematics," 630.
[31] Note that a clear definition for *mathematical models* hardly is an easy thing to come up with. This is not an important concern for me, however, since the very meaning attributed to the word 'model' is a part of what I call modeling practice. For a tentative definition of models, see G. Israel, *La Mathématisation du réel* (Paris: Seuil, 1996), 17-20.

specific disciplinary culture, I apply the term 'modeling practice' to those activities aimed at bridging disciplinary boundaries.

### d) The Modeling Practice of 'Applied Topologists'

The focus of this study shall be the modeling practices of a group of topologists. They came to the activity of building models for natural and social phenomena later in their professional life, after having been trained in topology as pure mathematicians, and having spent their first decades of work addressing questions that had little to do with the world outside of mathematics. Above all I focus on three mathematicians: Stephen Smale, René Thom, and Christopher Zeeman, the last an English mathematician who became one of the most ardent promoters for using catastrophe theory in the sciences. These three mathematicians had frequent personal interactions with one another, especially in the environment provided by the Institut des hautes études scientifiques. Thom's book *Structural Stability and Morphogenesis* provided them with a forceful manifesto that articulated a vision of what their modeling practice should be.

Briefly put, these applied topologists were on the lookout for topological features in systems they wished to study. This meant that they became interested in qualitative descriptions, and mainly argued in terms of discontinuities, shapes, or forms. They started their modeling activity with identifiable phenomenological features that they wished to describe using topological tools and techniques. How best to reduce these topological features to a postulated underlying mechanism, always explicitly present and well defined in the case of physical phenomena, remained the source of much disagreement among them.

Mainly, they were after descriptions, or as they often claimed, mathematical *explanations*, for the ways in which such qualitative features might evolve as external parameters varied. There could be different types of "bifurcations," which made the forms they studied change. They wished to construct a sufficiently solid mathematical theory so that they would be able to classify all possible bifurcations that could occur in a particular situation—and this, without having to rely on reductions to particular mechanical models. The main technical tools used by applied topologists therefore included parts of bifurcation theory, singularity theory, and dynamical systems theory. But one should hasten to add the crucial observation that the mathematical theories listed above were concurrently developed, partly by Thom, Smale, and Zeeman, and often with the explicit intent of using them in model-building situations. In these cases, the pure/applied divide that may be found in the later literature is a reconstruction of a process that intimately mixed the two aspects.

This modeling practice produced qualitative descriptions, predictions, or explanations for the changes in topological features of systems, bypassing too a specific reliance on mechanical reductions. Here again, interpretation of the results remained open to discussion. And as we shall see, controversies erupted among applied topologists about the explanatory status of their models.

In the end, their modeling practices might have remained rather sterile if a mathematical physicist, who was in close interaction with them, had not adapted them to concerns more specific to physics. Because of his special situation at the border of physics and mathematics, David Ruelle was able to reframe the practices of the applied topologists in ways which made them more palatable to a wide audience of physicists, in particular by providing a way to connect them with experiments. This interaction will reveal a richness in "the patterns of mathematization" that may contribute to a better understanding of the role mathematics has played in science and culture during the second half of the twentieth century and conversely, of the effect that role has on mathematics.[32]

## 4. 'PATTERNS OF MATHEMATIZATION'

Through its focus on innovation in the modeling practices employed by mathematicians and physicists, this dissertation is intended as a contribution to a cultural history of mathematization—a history which for the most part remains to be written.[33] Far from providing a general picture of the mathematization process in the history of science, however, this study has a more modest intent. By explaining a detailed example, I wish to raise questions about the complex dynamics of mathematization in the second half of the twentieth century, and to add some texture to further discussions.

If it is true that in the nineteenth century, the process of mathematization was affected by two diverging trends, the twentieth century has witnessed a convergence of these two trends. These trends have been, on the one hand, the emergence of physical theories that were not relying on mechanical analogies, and, on the other, a parallel increase in the autonomy of mathematics with respect to physics. Prior to the

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[32] This expression was used by M. S. Mahoney, "Pattern of Mathematization," unpublished manuscript.

nineteenth century, the fit between mathematical models and physical systems was to be "expected," since, as Mahoney emphasized, "the two had been constructed in tandem."[34] Things started to change, however, in the course of the nineteenth century, because of the emergence of strong tendencies pulling mathematics and the sciences using mathematical arguments further apart from one another.[35] On the one hand, non-mechanical mathematical models emerged, such as Fourier's treatment of heat dynamics, and statistics as a way to mathematize the social and biological sciences.[36] On the other hand, mathematicians started at the same time to assert the independence of their endeavor from physics and metaphysics, a movement that culminated with David Hilbert's (1862-1943) and Nicolas Bourbaki's structuralist program well into the twentieth century.[37] So, one witnessed a parallel increase in the autonomy of mathematics with respect to physics and of physics with respect to mechanics.

These diverging movements nonetheless gave rise to a new unifying vision of the role mathematics could play in the modeling of the phenomena of nature and of

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[33] Giorgio Isreal took note of this neglect and offered some reasons that may account for it in *La Mathématisation du réel,* 202-203.
[34] M. S. Mahoney, "Patterns of Mathematization."
[35] Obviously, questions about the professionalization of various scientific activities directly bear on the emergence of these tendencies: see, e.g., H. Mehrtens, H. Bos, and I. Schneider, eds., *Social History of Nineteenth-Century Mathematics*; and C. Jungnickel and R. McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein* (Chicago: University of Chicago Press, 1986).
[36] See, e.g., I. Grattan-Guiness, *Convolution in French Mathematics, 1800-1840* (Basel: Birkhäuser, 1990); and T. Porter, *The Rise of Statistical Thinking, 1820-1920* (Princeton: Princeton University Press, 1986).
[37] See H. Mehrtens, *Moderne Sprache Mathematik: Eine Geschichte des Streits um die Grundlagen der Disziplin und des Subjekts formeler Systeme* (Frankfurt: Surkamp, 1990); and L. Corry, *Modern Algebra and the Rise of Mathematical Structure* (Basel: Birkhäuser, 1996).

the human world. A key figure for the development of this vision was John von Neumann (1903-1957).

> One may very schematically describe his 'solution' by saying that, for the old reductionism, von Neumann substituted a kind of *neoreductionism.* Its key was *the central role of mathematics,* considered as a purely logical and deductive schema, constituting the new form of scientific reasoning. . . . For the mechanical analogy, he substituted the *mathematical analogy.*[38]

Von Neumann's "solution" consisted in transferring the axiomatic method, promoted by Hilbert, to realms where mathematics was used as a tool of understanding (quantum mechanics, economics, etc.). "Axiomatics thus provides a new framework for the development of scientific practice, . . . which can go ahead quietly and by taking no risk."[39] Mathematical theories, axiomatized for the sole purpose of grounding their accuracy on rigorous reasoning, and generalized to the extreme in order to expose their most fundamental structures, found applications in an unprecedented variety of contexts—a fact that made mathematicians stand in awe of "the unreasonable effectiveness of mathematics in the natural sciences."[40]

This attitude however promoted a pragmatic, utilitarian, and, one might even say, agnostic, vision of the role of mathematics in the modeling of natural and social phenomena. Von Neumann later developed this approach in parallel with axiomatics, but with different intent. "The sciences," von Neumann contended, "do not try to explain, they hardly even try to interpret, they mainly make models."[41] This was

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[38] G. Israel, *La Mathématisation du réel,* 198. His emphasis.
[39] G. Israel, *La Mathématisation du réel,* 198.
[40] E. P. Wigner, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," *Communications in Pure and Applied Mathematics,* 13 (1960): 1-14.
[41] J. von Neumann, "Methods in the Physical Sciences," *Collected Works,* 6, ed. A. H. Taub (Oxford: Pergamon, 1961-1963), 461; quoted by A. Dahan Dalmedico, "L'essor

indeed far from what Israel has called von Neumann's "neoreductionism." In this view, science abandoned its goal of understanding or explaining the world to become a reservoir of more or less accurate descriptions, without insisting so much on the construction of coherent, unitary wholes. At the same time, the privileged mathematical tools and techniques for modeling changed.

> One went from the theory of individual functions to the study of the "collectivity" of functions (or functional spaces); from the classical analysis based on differential equations to abstract functional analysis, whose techniques referred above all to *algebra* and to *topology*. . . . The *mathematics of time,* [i.e. differential equations] . . . was defeated by a *static and atemporal* mathematics.[42]

After World War II, these trends were furthered, especially in the US, by the development of a professional community of applied mathematicians, the tremendous increase in calculating powers provided by computers, and lessons derived from the mobilization of mathematicians in the war effort.[43] Wiener's cybernetics, Shannon's information theory, and Bertalanffy's general systems theory provided tools and frameworks to think about the use of mathematical analogy for modeling, which found wide audiences.[44] Simultaneously, the success of axiomatic mathematics for modeling promoted a worldwide Bourbakist hegemony over the selection of what

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des mathématiques appliquées aux États-Unis: l'impact de la Seconde Guerre modiale," *Revue d'histoire des mathématiques,* 2 (1996): 149-213, 179.

[42] B. Ingrao and G. Isreal, *The Invisible Hand: Economic Equilibrium in the History of Science,* transl. I. McGilvray (Cambridge: MIT Press, 1990), 186-187. Their emphasis.

[43] About this, see A. Dahan Dalmedico, "L'essor des mathématiques appliquées."

[44] N. Wiener, *Cybernetics: or Control and Communication in the Animal and the Machine,* 2nd ed. (Cambridge: MIT Press, 1961); W. Weaver and C. E. Shannon, *The Mathematical Theory of Communication* (Urbana: University of Illinois Press, 1949); L. von Bertalanffy, *General Systems Theory: Foundations, Development, Applications* (New York: George Brazillier, 1968).

were considered the most prestigious branches of mathematics. An insistence on the "purity" of the mathematical endeavor became a prominent element of the mathematicians' discourse.

In the 1960s, however, the fit between the most axiomatized branches of mathematics and fields where mathematical tools and techniques were used had become problematic. The applied topologists I study in this dissertation, using Bourbakist mathematics in order to undermine the Bourbakist project, would loudly promote a return to more intuitive, geometric ways of doing mathematics. In the words of Smale, the need for a new "mathematics of time" was now starting to be felt.[45] Mathematical descriptions, they claimed, ought to provide explanations of the world they lived in. In 1977, one of the applied topologists discussed above, Christopher Zeeman, thus introduced a pictorial representation for this relationship between mathematics and the sciences (Figure 3):

> In applied [catastrophe theory] there are *descriptions* and *explanations*. By an *explanation* I mean a diagram 8=1234567: [Figure 3]. By a *description* I mean just the part 567. Most of Thom's models (and his philosophy of analogy supports this) are descriptions. However where possible it is good science to try and extend descriptions into explanations, and I have tried to do this with several of my models.[46]

New patterns of mathematization changed the way scientists attempted to explain nature.

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[45] S. Smale, *The Mathematics of Time: Essays on Dynamical Systems, Economic Processes, and Related Topics* (New York: Springer, 1980).
[46] Zeeman to Smale (4 October 1977). His emphasis. Copy in the archives of the IHÉS [hereafter Arch. IHÉS.]. See a similar figure Zeeman included in *Catastrophe Theory: Selected Papers, 1972-1977* (Reading: Addison-Wesley, 1977), 267.

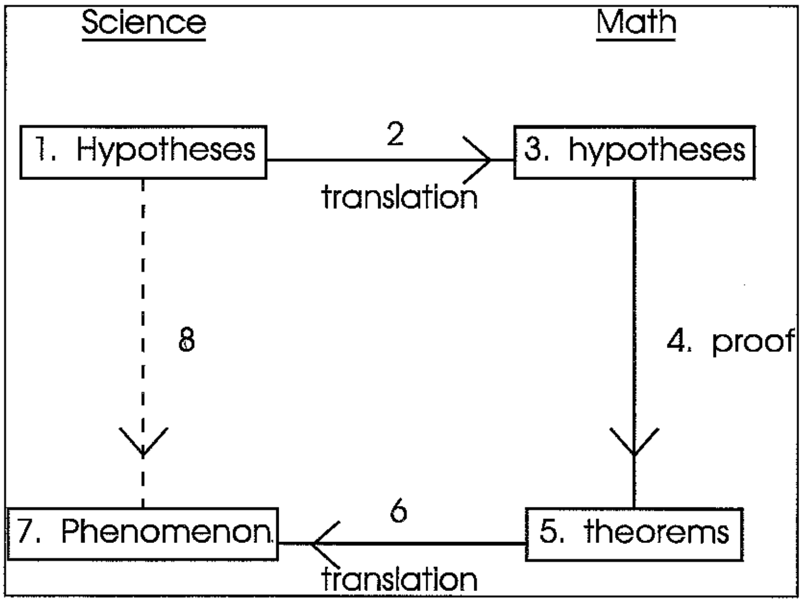


**Figure 1:** E. Christopher Zeeman's Pictorial Representation of the Relation Between Science and Mathematics. Redrawn by the author from a letter of Zeeman's to Steve Smale (4 October 1977).

## 5. SOURCES AND CONTENTS

This dissertation is mainly based on the exploitation of original archival materials, interviews, and published sources. The popularity of catastrophe theory and chaos has generated a vast literature of a more or less rigorous historical nature.[47] Journalistic

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[47] With no pretense of being exhaustive, I list here some of the sources of this type which have been the most important to me: J. Gleick, *Chaos: Making a New Science* (New York: Viking, 1987); M. W. Hirsch, "The Dynamical Systems Approach to Differential Equations," *Bulletin of the American Mathematical Society,* n.s., 11 (1984): 1-64; and F. Diacu and P. Holmes, *Celestial Encounters: The Origins of Chaos and Stability* (Princeton: Princeton University Press, 1996). Among accounts and recollections written by first-hand actors let me cite: D. Ruelle, *Chance and Chaos;* P. Bergé, Y. Pomeau, and M. Dubois-Gance, *Des Rythmes au chaos* (Paris: Odile Jacob, 1994); I. Prigogine and I. Stengers, *Order out of Chaos: Man's New Dialogue with Nature* (New York: Bantam, 1984); R. Thom, *Paraboles et catastrophes. Entretiens sur les mathématiques, la sciences et la philosophie,*

accounts of the theories, as well as a rather vast popular literature, often include snippets of relevant historical information.[48] Another rather large category of published sources consists in attempts at drawing the philosophical implications of these theories, and their cultural resonance.[49] Some edited books, including contributions from scientists, philosophers and historians, offer more sophisticated inroads into the history of chaos theories.[50] Finally, there was Amy Dahan Dalmedico's preliminary article on the revival of dynamical systems theory in postwar US.[51] Of course, the main source of published materials I have exploited simply were the articles, in journals or conference proceedings, and the books that scientists write

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interview by G. Giorello and S. Morini (Paris: Flammarion, 1983); and R. Thom, *Prédire n'est pas expliquer,* interview by Émile Noël (Paris: Eshel, 1991).

48 Let me just list a few books I found particularly interesting: I. Stewart, *Does God Play Dice? The Mathematics of Chaos* (Oxford: Basil Blackwell, 1989); A. Woodcock and Monte Davis, *Catastrophe Theory* (New York: E. P. Dutton, 1978); I. Ekeland, *Mathematics and the Unexpected,* transl. I. Ekeland (Chicago: Chicago University Press, 1988); J. Briggs and D. F. Peat, *Turbulent Mirror: An Illustrated Guide to Chaos Theory and the Science of Wholeness* (New York: Harper & Row, 1989); and J. L. Casti, *Searching for Certainty: What Scientists Can Know about the Future* (New York: William Morrow, 1990).

49 I note especially: S. H. Kellert, *In the Wake of Chaos: Unpredictable Order in Dynamical Systems* (Chicago: University of Chicago Press, 1993); N. K. Hayles, *Chaos Bound: Orderly Disorder in Contemporary Literature and Science* (Ithaca: Cornell University Press, 1990); and A. Boutot, *L'Invention des formes. Chaos, catastrophes, fractales, structures dissipatives, attracteurs étranges* (Paris: Odile Jacob, 1993).

50 S. Diner, D. Fargue, and G. Lochak, eds., *Dynamical Systems: A Renewal of Mechanism* (Singapore: World Scientific, 1986); and above all A. Dahan Dalmedico, J.-L. Chabert, and K. Chemla, eds., *Chaos et déterminisme* (Paris: Seuil, 1992).

51 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.

for a living.[52] By and large, however, the cultural history of catastrophe and chaos theories remained to be told.

Chapter II, titled "Structures," is a cultural study of the background in French mathematics and intellectual movements (including structuralism and the literary group Oulipo) on which catastrophe and chaos theories could be built. In this chapter I deal the most fully with cultural connectors in the case of Bourbaki. Not only did the group of mathematicians and their concept of a mathematical structure provide an essential background for the understanding of French mathematical culture from the 1940s to 1960s, but also one for a better appreciation of views about the social role of mathematics in a wide array of cultural arenas.

Chapter III, "Catastrophes," provides an introduction to René Thom's vision of catastrophe theory. It examines the intellectual resources he could draw on in mathematics, biology, and linguistics when he attempted to formalized a theory of modeling practice of his own, which, either explicitly or implicitly, was taken up by many of the actors discussed later. This chapter also provides a summary of Thom's early philosophical views, before around 1975. While the goal of these two chapters is to set the stage for the intellectual encounter between Thom and Ruelle, the following two examine the institutional and conceptual backgrounds against which this encounter could take place.

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52 Especially useful were the two following books which collected many seminal papers: P. Cvitanovic, ed., *Universality in Chaos* (Bristol: Adam Hilger, 1989 [1984]); and Hao B.-L., ed., *Chaos* Singapore: World Scientific, 1984; 2nd ed. *Chaos II* (1990). I could also count on a bibliography containing more than 5,000 entries compiled by S.Y. Zhang, *Bibliography of Chaos* (Singapore: World Scientific, 1991).

Chapter IV, titled "Fundamental Research," is an attempt at sketching the peculiar character of the IHÉS both in terms of material resources and of its intellectual goals. Devoted from the outset, in 1958, to the pursuit of fundamental research in pure mathematics, theoretical physics, and "the methodology of the sciences of man," the IHÉS was first thought of as relying on, and made possible by, the sponsorship of big industry. This led its founding director, Léon Motchane, to take the difficult, and perhaps contradictory, position of arguing for the concrete benefits one could get out of "pure", "disinterested", or "fundamental" research. Motchane succeeded in this enterprise by endowing his Institute with a rhetorical emphasis on cooperation between fields of research and with a structuralist attitude that aimed at isolating the essence of mathematical and physical theories so that they could reach the highest level of generality possible. The specific character of the IHÉS, I argue in the following chapters (especially in Chapter VI), made it possible for catastrophe and chaos theories to emerge from within its walls through exchanges of modeling and theoretical practices among permanent professors and visitors.

In order to achieve this on a more concrete level, Chapter V, "Stability," looks back at the history of stability within the theory of differential equations. By trying to look at this history through the eyes of one of the main developers of this theory in the 1960s, Steve Smale, this chapter emphasizes his role in thoroughly infusing the study of differential equations with the most recent topological methods available. Chapter V therefore shows that, far from being a dogma that prevented mathematicians, physicists, and engineers from recognizing chaotic behavior in dynamical systems, as has often been argued, the widespread focus on stability in fact provided the necessary tools for the recognition of chaotic behavior. In short, chaos was the answer to the questions raised by the search for stability. Centering on Smale's role in the synthesis of many stands into a new branch of mathematics he identified as dynamical systems theory, however, I limit my historical study to his own sources. Far from being an internalistic history of a branch of mathematics that, really, did not exist as such before Smale reconstructed it as a way to achieve his own synthesis, this chapter provides my own re-reading of the sources important to Smale in order to set the stage for his own contribution to the story I here tell.

In Chapter VI, "Qualitative Dynamics," I return to the IHÉS and show how Thom's mathematical program was actualized by taking advantage of the conditions he found at the IHÉS. This chapter explains how his own research program in mathematics intersected with Smale's. By attracting many important mathematicians to the IHÉS, Thom succeeded in drawing attention to the problems he was studying, and Chapter VI tries to assess whether it makes sense to speak of Thom's group at the IHÉS as a research school. Focusing on the interactions that took place at the IHÉS between several groups of scientists, the special atmosphere reigning there is described. This influx of visitors to the IHÉS made it possible for many of them to start looking at the implications that Thom's ideas may have had on their own modeling practice. The similarities and differences one may find in Thom's, Zeeman's and Smale's modeling practices are examined in detail. Later controversies that erupted around catastrophe theory among applied topologists are also discussed in order to underscore the bounds within which their modeling practices varied.

The crucial encounter between Ruelle and Thom, the crucial transfer of modeling practices from applied topologists to physicists takes place in Chapter VII, entitled "Strange Attractors." The three main strains can be followed in order to get a clearer view of the historical sources of the model for turbulence proposed by Ruelle and his co-writer Floris Takens: namely prior models for the onset of turbulence, the modeling practices promoted by applied topologists, and Ruelle's earlier career as mathematical physicist at a time when this profession was not very popular. In a second part, Ruelle and Takens's models are compared with a long-term view on the history of the turbulence problem in fluid mechanics, so that the innovation in modeling practice suggested by Ruelle and Takens become clearer. A particular community of mathematically-minded fluid dynamicists that, seemingly, should have been the first to react to Ruelle and Takens's proposal, at first remained unconvinced by their argument. Following different modeling practices, it is argued, made it harder for them to see the import of the Ruelle-Takens model.

The ultimately successful reception of the Ruelle-Takens model for the onset of turbulence followed another route. This story is provided by the core of Chapter VIII called "Chaos." Simple ideas about the diffusion of a new proposal to other disciplines, or the simplistic picture of a hypothesis that was confirmed by experiments, be they performed in the laboratory or on the computer, will not account for the much more complex and interesting process that then took place. This chapter instead tries to reconstruct a portion of the path by which an article came to be considered as seminal. Focusing on Rayleigh-Bénard convection as a "boundary system," which could be tackled by a variety of professional groups of scientists, each of them with their specific approaches and tools, this chapter shows that the Ruelle-Takens model was taken up as a way to promote the development of interdisciplinary analogies, an interest which preexisted Ruelle and Takens's proposal. In short, this clearly indicates that the conditions for the emergence of an interdisciplinary approach, such as the one that would be embodied in chaos theory, was not the consequence of the reception of the Ruelle-Takens model by physicists but rather a crucial condition for the reception of this model. Modeling practices of later 'chaologists', far from being a straightforward adoption of a 'dynamical systems approach' à la Smale, Thom, or Ruelle, were rather an adaptation of dynamical systems modeling practices to experimental results. In effect, one might contend that these modeling practices promoted a type of mathematical modeling informed by experimental results, rather than mathematical theories only.

Several other themes run between the lines of the main argument given above. One may find at least three such themes which are more prominent than others. First, while Bourbaki was severely criticize by the promoters of catastrophe and chaos theories who always stressed applications, it is argued that the global axiomatization of mathematics, which Bourbaki was perceived as having pushed forward, may also be thought of as having provided a significant push in the direction of an original use of mathematics in the activity of building models for natural phenomena.

Second, the advent of computers shaped the development of these mathematical theories in crucial, but not always obvious ways. Even for people, such as Thom, Smale and Ruelle, who never really used the computer in their practical work, this event provided the ground on which they began to think about the goals of using mathematics in order to describe natural phenomena. In Chapter VIII, the role of the computer becomes even more apparent as numerical experiments provide clues for the adequacy of Ruelle-Takens. Moreover, I show that the computer also had a significant impact on the kind of experiments people could envision. More than a mere enhancement of computing power, the computer became, to use one of Norton Wise's phrases, a tool to think with.

Finally, the social and political contexts in which catastrophe and chaos theories emerged often remain close to the main line of argument. At several points, questioning the role of science in society, and especially its military side, surfaces among the important concerns of the scientists I follow. This should not be seen as coincidental, although more time and sources will be required to follow up on this fascinating issue.

Generally speaking, one might profitably imagine the structure of this dissertation as similar to that of an hourglass whose metering center represents the crucial intellectual encounter between Thom and Ruelle, which occurred in 1968-1970 as described in Chapter VI and VII. In the first chapters, broad cultural arguments are tackled, and then the dissertation focuses more tightly on institutional and disciplinary settings against which this encounter took place. At the end of Chapter VII and in the next one, the scope is broadened again in order to look at the repercussions of the Thom-Ruelle encounter on fluid mechanics and physics more generally. At this point, a more complete history of chaos would need to incorporate other strands that cannot be included here since they would necessitate works parallel to this one.