ABSTRACT

This is the story of a group of scientists who, in the local context of the Institut des hautes études scientifiques (IHÉS), France, contributed to the elaboration of catastrophe theory and deterministic chaos theory. Starting with a study of the role of Bourbaki's mathematics on the French intellectual scene (and especially with respect to strcuturalism), this dissertation examines the resources from topology, embryology, and linguistics used by René Thom to construct catastrophe theory. It describes the foundation of the IHÉS by Léon Motchane in 1958 and the ideology of fundamental research that shaped it. It reviews the history of structural stability for differential equations, focusing on the work of Alekandr Andronov and Solomon Lefschetz, among others, and the synthesis achieved in the 1960s by Stephen Smale at the University of California, Berkeley. These mathematical developments were used by Thom to develop new modeling practices. The IHÉS, which welcomed topologists such as E. Christopher Zeeman and Ralph Abraham, played a role in developing modeling practices based on recent advances in topology. A physics professor at the IHÉS, David Ruelle, together with Dutch mathematician Floris Takens, adapted the modeling practices of these 'applied topologists' and proposed a mechanism for the onset of turbulence, thereby introducing the concept of strange attractors. Looking at the history of fluid mechanics, I argue that Ruelle's work displaced earlier emphases on fundamental laws, like the Navier-Stokes equations, and focused on the modes of representation rather than representations themselves. A certain Bourbakization of physics and the advent of the computer shaped this evolution. Finally, focusing on convection, and taking the Rayleigh-Bénard system as a boundary system, various communities' responses to the Ruelle-Takens model are examined, in particular hydrodynamic stability theorists, phase transition physicists and Pierre-Gilles de Gennes's group, and chemical physicists orbiting Ilya Prigogine. Prior interest in developing interdisciplinary approaches for the study of turbulence helped the adaptation of a dynamical systems approach for the study of natural phenomena, greatly inspired by the work of Smale, Thom, and Ruelle.

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ACKNOWLEDGMENTS

First and foremost, I must thank the members of my reading committee, without whom I could just not have written this dissertation. Some years ago, my advisor-to-be, Norton Wise, welcomed a disoriented mathematics student and has known how to deal with the naiveté of someone with no prior historical training. Throughout my Princeton years, he has provided me with constant inspiration, guidance, and support, while I stood in admiration in front of his unflagging energy and indefatigable enthusiasm. Michael S. Mahoney was an insightful teacher, always taking great interest in my work, who never failed to encourage me with his penetrating comments. Finally, the only professional historian who, when I started with this project, had started tackling the issue of the emergence of chaos, Amy Dahan Dalmedico was kind enough to welcome me in Paris. Thanks to her I got access to invaluable archival material and was introduced to many informants. During our regular, instructive and enjoyable discussions, she shared some of her deep knowledge of the history of mathematics with me. To the three of you, I say 'Thanks' and hope that I shall go on learning from you.

Many other professors, at Princeton and abroad, helped me acquire a better sensitivity to the trade of being an historian. I would like to express special acknowledgments to Bernadette Bensaude-Vincent, Angela Creager, Gerald Geison, Charles Gillespie, Robert Kohler, Arno Mayer, Philip Nord, and Dominique Pestre. I would also like to thank Liliane Beaulieu, Catherine Chevalley, Leo Corry, Marie Farge, Emmanuel Gilquin, Michèle Lamont, Michèle Porte, Isabelle Stengers, and Spencer

Weart for having either read parts of my dissertation, or dicussed it with, or provided me original material.

Fellow students at Princeton University have been a great moral and intellectual support through my learning process, as well as the writing of the first chapters of this dissertation. I want to express special thanks to Eric Ash, David Attis, David Brock, Shelley Costa, John Detloff, Teresa Hopper, Ann Johnson, Jordan Kellman, Stuart McCook, Jakub Novak, Leo Slater, and Chuck Walton.

Based on interviews and difficulty accessible materials, this dissertation is the result of the kind generosity of many people who answered my questions or opened archives to me. More than two thirds through the completion of my time as a graduate student, I had the good luck of being given access to a rich source of archival material. Thanks to Amy Dahan Dalmedico, who introduced me to the current director of the Institut des hautes études scientifiques, Jean-Pierre Bourguignon, I spent more than two months going through the complete archival traces left by this Institute where both Thom and Ruelle spent most of their respective careers. This tremendous opportunity definitely changed the focus of the dissertation. I especially want to thank M. Bourgignon, as well as the staff of the Institute, Vedla Meyer, Helga Dernoix, and Jytte Martin.

I am grateful of the promptness with which the scientists I interviewed answered questions, letters and e-mail messages. Let me especially thank Pierre Bergé, Monique Dubois, Jean-Pierre Eckmann, Albert Libchaber, Paul Manneville, Paul C. Martin, John Mather, Christian Mira, Mauricio Peixoto, Yves Pomeau, David Ruelle, René Thom, Jacques Viret, and Arthur Wightman. Archivists and librarians have been helpful in trying to locate pertinent material, especially Hélène Nocton (Institut Henri-Poincaré), Marie-Ange Augerie and Christine Delangle (Collège de France), and Louis Cosnier and Michèle Sabourin (CNRS).

My graduate studies and the writing of this dissertation have been supported by the following organizations, whose support I am glad to acknowledge: the Natural Science and Engineering Research Council of Canada, the Social Science and Humanities Research Council of Canada, the John C. Slater Fellowship of the American Philosophical Society, the Mellon Foundation, the Program in the History of Science, and the Center of Excellence of the Council of French Studies of Princeton University.

Princeton is only as much fun as the friends you have there. Fortunately, I had to good fortune of finding many people whose company I immensely enjoyed during my stay in the United States. Here are a few of them: Paul Bogorad, Gordon Buffonge, Victoria Campos, Gabriela Cruz, Margaret Holen, Marc Potters, and Ulrich Scheven, as well as an old friend from Montréal, Karine Damar Singh. My debt to my parents, Gilles Aubin and Louise Lavoie, is of course immense. Always loving and unconditionally supportive for whatever I undertook, they deserve important credit for what I was able to accomplish here.

Finally, I dedicate this dissertation to my wife, Corinne Le Quéré, and my daughter, Marianne Aubin Le Quéré who was born while we were in Princeton. Without the constant support of the former, it simply could not have been written; without the smiles and cries of the latter it might have been completed sooner, but with much less joy. Je vous aime. Merci.

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

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[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

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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

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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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[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

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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

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[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

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[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

[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.

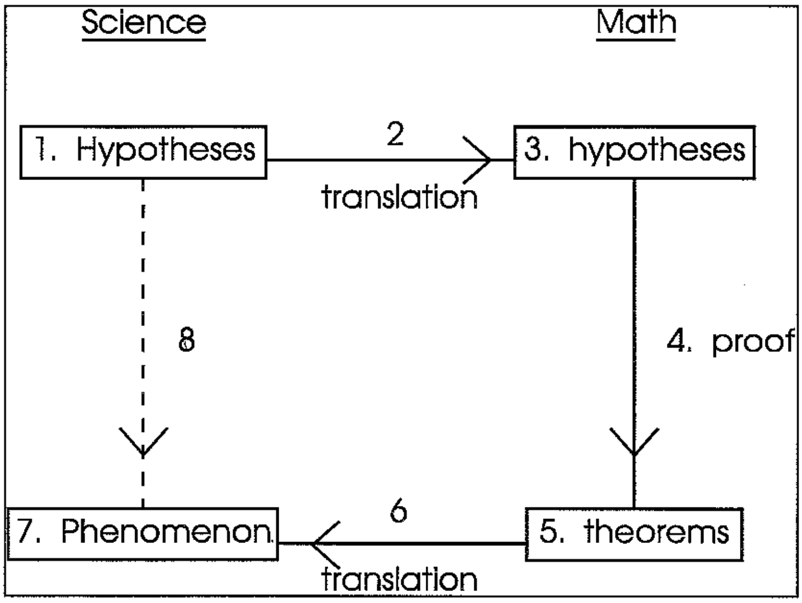

Figure 1: E. Christopher Zeeman's Pictorial Representation of the Relation Between Science and Mathematics. Redrawn by the author from a letter of Zeeman's to Steve Smale (4 October 1977).

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

[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.

CHAPTER II: STRUCTURES

Il a nécessairement vieilli, votre fictif mathématicien, il doit avoir pris du retard. Eh bien! non, Bourbaki n'a pas vieilli parce qu'il ne *peut pas* vieillir.
–Raymond Queneau.[1]

La mathématique est l'art de donner le même nom à des choses différentes.
—Henri Poincaré.[2]

1. INTRODUCTION[3]

Truly, as Queneau claimed, the prolific mathematician Nicolas Bourbaki could not grow old for the good reason that he never existed. That is, the man never did. As a symbol, on the other hand, he was, for more than 30 years, powerful enough to serve many different purposes across disciplines.[4] By looking at the various roles he played in several types of

[1] R. Queneau, "Bourbaki et les mathématiques de demain," *Critique,* 18, no. 176 (1962): 3-18; repr. in *Bords* (Paris: Hermann, 1963).
[2] "Mathematics is the art of giving the same name to different things." H. Poincaré, "L'avenir des mathématiques," *Atti del IV Congresso internzionale dei matematici, Roma, 1908,* 1 (Rome: Accademia dei Lincei, 1909): 167-182, 171.
[3] A version of this chapter is to be published in the summer issue of *Science in Context* (1997).
[4] Throughout, I use customary male pronouns to refer to Bourbaki. In doing so, I am aware of the danger of reinforcing a myth—the myth of a collective author speaking with a single, authoritative voice. But, since this chapter deals with the actual effects of this mythic image rather than the "true" history behind it, this odd usage seemed more appropriate. For Bourbaki's myth, see for example P. R. Halmos, "Nicolas Bourbaki," in *Scientific American,* 196 (May 1957): 88-97; for its functions with respect to Bourbaki's image, see below.

discourse, his rising impact among a portion of mathematicians, structuralists, and writers alike, and then his declining authority, we can study the way in which different cultural streams mingled at a node called *Bourbaki.* We will be able to put some flesh on the cliché that sometimes, somehow, ideas are "in the air."

Nothing more than a *nom de plume* chosen by a group of French mathematicians, Bourbaki nonetheless—or rather for precisely this reason—authored, from 1939 on, one of the most ambitious mathematical treatises of the twentieth century: *Les Éléments de mathématique.*[5] Raymond Queneau wrote that Bourbaki would always keep abreast of his time—he would never grow old—since a persistent rumor stated that, at age fifty, a "collaborator" of Bourbaki (as it was customary to call members of the group) would relinquish his veto over any of Bourbaki's publications, a right that would forever remain the prerogative of younger generations. In this sense, Bourbaki indeed enjoyed the rare privilege of eternal youth.[6]

But, while the mythic Bourbaki could not grow old, the myth of Bourbaki has. To reconstitute the "thought" of a group of people probably is a futile affair. This hardly

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[5] Histories of Bourbaki are to be found in L. Beaulieu, "A Parisian Café and Ten Proto-Bourbaki Meetings (1934-1935)," *Mathematical Intelligencer,* 15(1) (1993): 27-35; L. Beaulieu, "Dispelling a Myth: Questions and Answers about Bourbaki's Early Work, 1934-1944," *The Intersection of History and Mathematics,* ed. S. Chikara, S. Mitsuo, and J. W. Dauben (Basel: Birkäuser, 1994): 241-252; H. Cartan, "Nicolas Bourbaki and Contemporary Mathematics," *The Mathematical Intelligencer,* 1(2) (1980): 175-180; M. Chouchan, *Nicolas Bourbaki. Faits et légendes* (Paris: Éditions du Choix, 1995); J. Fang, *Bourbaki: Towards a Philosophy of Mathematics I.* (Hauppauge, New York: Paideia Press, 1970). The most comprehensive history is, however, still unpublished: L. Beaulieu, *Bourbaki. Une histoire du groupe de mathématiciens français et de ses travaux (1934-1944),* Ph.D. thesis (université de Montréal, 1989); *Bourbaki: History and Legend* (Berlin: Springer, forthcoming).

means, however, that it is impossible to recover what Bourbaki stood for, within the mathematical community and outside of it. For almost everyone, he served as a symbol for a strict method of axiomatization, with which, he himself claimed, he could build up the whole of mathematics on the sole basis of a few fundamental "*mother structures*" and their combinations. He thereby wished to purify mathematics from any reliance on the external world. This made the usefulness of mathematics in understanding natural phenomena hard to comprehend; it followed from a mathematical order inherent to nature, rather than the fact that, historically, mathematics had often been constructed with specific purposes in mind.

Sometime in the course of the 1970s, however, this vision, which had become dominant among mathematicians, and which Bourbaki stood for, unraveled. Let me emphasize that it was Bourbaki's *vision* that then faded away, not his goal of founding mathematics on the notion of structure, which, as we shall see below, had already begun to face serious challenges as early as the 1950s.[7] Domains of mathematics bloomed and boomed without the aid of the axiomatic method: *e.g.* the theories of catastrophes, chaos, and fractals. Although the elaboration of a satisfactory set of axioms for these mathematical theories proved challenging, if not impossible[8], mathematicians managed to dispense with it and still produce significant results enthusiastically embraced by their

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[6] The historian Liliane Beaulieu, who has worked the most extensively on Bourbaki, told me that she never came accross any written trace of this rule and that, in any case, it was breached many times.
[7] See L. Corry, *Modern Algebra and the Rise of Mathematical Structure* (Basel: Birkhäuser, 1996).
[8] See, *e.g.*, V. I. Arnol'd, *Catastrophe Theory: Third, Revised, and Expanded Edition,* transl. G. S. Wassermann, based on transl. by R. K. Thomas (Berlin, Heidelberg: Springer, [1981] 1992).

community. In addition, these recent theories were developed, not solely out of motives internal to mathematics, but rather in constant interaction with other fields of science, such as biology, physics, economics, or even structural linguistics. All of this was obviously and distinctively anti-Bourbakian.

Surveying in 1979 the mathematics of the past decade, Christian Houzel, a president of the *Société mathématique de France* in 1982, revealed to the public that "the age of Bourbaki and fundamental structures is over." While the previous period was one that had witnessed the development of powerful new theoretical tools of great generality, he noted, the 1970s were rather characterized by a tendency to revive an old interest in more concrete problems. Houzel did not venture an explanation for this reversal. "I cannot say," he simply wrote, "to what extent this [tendency] is conditioned by the internal dynamics of the development of mathematics, or by ideological currents like the degradation of science's superior image in public opinion and scientists' questioning of the social status of their practice."[9]

Houzel wisely avoided addressing a dilemma familiar to the cultural historian. The cultural history of science strives to understand the subtle connections between science and society. In order to present a compelling argument, it is however necessary to go beyond metaphors and analogies. However appealing some connections may appear, how can we assess whether enough evidence has been presented? Just how many

[9] C. Houzel, "Les mathématiciens retournent au concret." *La Recherche,* 10 (1979), 508-509; all translations are mine, except when I quote from a published translation. See also C. Houzel, ed., *Rapport de prospective en mathématiques* (Paris: Éditions du CNRS, 1985); M. Arvonny, "Quarante ans de Bourbaki. Le célèbre mathématicien est toujours immortel, mais il a vieilli," *Le Monde* (9 April, 1980): 15; and D. Nordon, *Les Mathématiques pures n'existent pas!* (Paris: Actes Sud, 1981).

astonishing coincidences will suffice for a story to be plausible? This often remains problematic. While some historians of science have recently been able to articulate convincingly such connections by focusing on social units naturally well circumscribed, the story of Bourbaki as a cultural icon in postwar France requires a much more diffuse framework, a Protean notion of cultural connection.[10]

Paralleling the trajectory of structuralism, Bourbaki's rise and decline in postwar France provides, I believe, a perfect case for which to exhibit the possibilities and limits of the cultural history of science. Here, there are indeed clear indications that the mathematicians' attitudes coincided with broad social, cultural, and intellectual movements. A reconciliation between the culture of Bourbakian mathematics and larger currents, this seems to suggest, may therefore be possible. I mean to achieve this by looking at Bourbaki as a *cultural connector.* Recall that I defined cultural connectors in Chapter I, as more or less explicit references from other disciplines, used by actors when they attempt to argue for a point or when they want to increase the legitimacy of their methods and ideas.

Bourbaki acted as a cultural connector, not because the members of the group were especially active outside of mathematics itself, but because his very name had come

[10] See, for example, M. Biagioli, *Galileo, Courtier: The Practice of Science in the Culture of Absolutism* (Chicago: University of Chicago Press, 1993); 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-

to serve as a shortcut indicating a certain attitude towards science. By invoking Bourbaki, authors signaled that they espoused some of his views—or more precisely, as I will not always state explicitly, views commonly attributed to him. How this state of affair came to be, how it varied and evolved, and within which limits, is the topic of this paper. In the following, I tell the story of the origins of the connection, its period of hegemony, and its decline. In the last part, I also mention cultural connectors replacing Bourbaki at the interface of mathematics and the philosophy of science. I emphasize the intersection of three arenas in which the name of Bourbaki often appeared: mathematics; the structuralist and postmodernist discourses; and so-called potential literature, always focusing on the points of contact between cultures.[11] With this chapter, I hope to provide a firm historical basis for placing the evolution of mathematics, and of its image, into a larger cultural context. To doing so, I will both study the culture of mathematicians in postwar France and put culture back into the cultural history of mathematics.

2. ORIGINS

a) Structuralisms: Lévi-Strauss and Bourbaki

In the aftermath of the Liberation, in 1944, France experienced a period of bubbling intellectual activity. Most prominently, the existentialists held a philosophy of *engagement,* well adapted to their troubled times, but disjointed from mainstream scientific pursuits, with the possible exception of psychoanalysis. In particular, it had no

Century Britain (1-3)." *History of Science,* 27 (1989): 263-301 and 391-449; *Ibid.,* 28 (1990): 221-261.

use whatsoever for mathematics. Although existentialism nearly monopolized intellectual debates in the immediate postwar, foundations were meanwhile being laid down for the next generation. Among the important works that came out in 1948/49—besides Braudel's *Méditerranée* and de Beauvoir's *Second Sex*—there were two more that nicely exemplified the new directions soon to be followed. Both had their genesis during the exceptional circumstances of World War II. One, Claude Lévi-Strauss's *Elementary Structures of Kinship,* is quite well known.[12] This book is widely considered as the "act of foundation" of postwar French structuralism, an approach to the human sciences extremely influential in shaping cultural and intellectual discourses in France for the next decades.[13]

The other work that I want to bring to our attention was a special issue of *Les Cahiers du Sud* published in March 1948. Edited by the mathematician François Le Lionnais, it proposed to delineate the "Great Currents of Mathematical Thought."[14] Dating from 1939, the idea for this collection was impeded by the problems of wartime communication, which intensified its French focus, and further delayed by the internment in 1944 of Le Lionnais in a German camp. Although not as famous as Lévi-Strauss's

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[11] In addition, Bourbaki's name has often been invoked by reformers of mathematical education, and their adversaries, both in France and in the U.S., an issue that I scarcely address here, but that is part of the story of the cultural connector Bourbaki.
[12] C. Lévi-Strauss, *Les Structures élémentaires de la parenté* (Paris: Presses universitaires de France, 1949); *The Elementary Structures of Kinship,* transl. J. H. Bell, ed. J. R. von Sturmer and R. Needham (Boston: Beacon, 1969).
[13] Quoted from the chronology established by A. Simonin and H. Clastres [*Les Idées en France, 1945-1988. Une chronologie* (Paris: Gallimard, 1989), 79], which has been very useful for this chapter.
[14] See the reprinted volume: F. Le Lionnais, ed., *Les Grands Courants de la pensée mathématique*, new augmented edition (Paris: Albert Blanchard, 1962); *Great Currents of*

work, it included a seminal programmatic statement by Bourbaki.[15] There, he succinctly articulated, in general terms, his overall approach to a unified science of mathematics. During the 1950s and 1960s, Bourbaki's approach would be at least as diligently followed by mathematicians as structuralism was by social scientists. More strikingly, the appeal of this famous article was based on the powerful metaphor of "mother-structures." As we shall see later, Bourbaki's structures were not unrelated to Lévi-Strauss's.

Both Bourbaki and Lévi-Strauss can therefore be viewed as having founded some sort of structuralism. But what I wish to discuss here is not so much the fact that these books can rightly be considered as sources for important currents of thought, but rather that both represented an intersection of people and ideas that would remain loosely associated until their common effacement in the 1970s. Indeed, Lévi-Strauss's book included an appendix written by André Weil, one of Bourbaki's founders and foremost collaborators.[16] On the other hand, Le Lionnais's book included, in addition to the articles written by Bourbaki, Weil, and Jean Dieudonné (another member of Bourbaki), a contribution by the famous writer Raymond Queneau, author of *Zazie dans le métro*.[17] In 1960, Queneau and Le Lionnais cofounded an influential literary group, the *Oulipo* (Workshop for Potential Literature), that explored the possibility of language in a way

Mathematical Thought, transl. R. A. Hall and H. G. Bergmann, 2 vols. (New York: Dover, 1971).

[15] N. Bourbaki, "The Architecture of Mathematics," in *Great Currents,* ed. F. Le Lionnais, I: 23-36; A different English translation of this article was published earlier: "The Architecture of Mathematics," *American Mathematical Monthly,* 57 (1950): 221-232.

[16] A. Weil, "Sur l'étude algébrique de certains types de lois de mariage (Système Murngin)," in C. Lévi-Strauss, *Structures élémentaires,* 278-285.

directly inspired by Bourbaki. It becomes apparent, therefore, that already in the immediate postwar period the discourses I want to talk about seem to have been involved in some discussion. Let us see how these relationships came into being.

b) Bourbaki: The Emergence of a Myth

On December 10, 1934, six young French mathematicians gathered in a Parisian café. André Weil had convened them with the goal of writing, collectively, a textbook of analysis, "as modern as possible."[18] Their names: Henri Cartan, Claude Chevalley, Jean Delsarte, Jean Dieudonné, René de Possel, and Weil; a few months later they formed Bourbaki.[19] The story of this meeting and the following years which saw the emergence of Bourbaki, has been told in much details by Liliane Beaulieu.

When in 1948, fourteen years and a world war later, the piece called "The Architecture of Mathematics" appeared in Le Lionnais's *Great Currents of Mathematical Thought,* N. Bourbaki was getting to be known for two main reasons: his treatise and his myth. If the reader believed the leaflet spelling out "the directions for the use of this treatise" enclosed with each published booklet, it seemed, then, that Bourbaki had embarked on a gigantic project. On the basis of the notion of *structure,* he would

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17 R. Queneau, "The Place of Mathematics in the Classification of the Sciences," in *Great Currents,* ed. F. Le Lionnais, II: 73-77. R. Queneau, *Zazie dans le métro* (Paris: Gallimard, 1959).

18 L. Beaulieu, *Bourbaki,* 147.

19 Paul Dubreil, Jean Leray, and Szolem Mandelbrojt participated to some of the next meetings. Leaving the group before the summer, Dubreil and Leray never were formally members of Bourbaki. Later in 1935, they were replaced by Jean Coulomb and Charles Ehresmann. See L. Beaulieu, *Bourbaki,* 12-13. About de Possel, see E. Gilquin, ed., *De Bourbaki à la machine à lire. Journée d'hommage à René de Possel, 1905-1974 [16 novembre 1994],* 2 vols. (Paris: Institut Blaise Pascal, 1994).

construct the foundation for all mathematics with the help of the axiomatic method. The *Éléments de mathématique* claimed to take up "mathematics at the beginning, and [give] complete proofs."[20] The modesty of the word *Éléments* in the title was misleading, the parallel with Euclid's *Elements* revealing the extent of Bourbaki's ambition. *Mathématique,* on the other hand, was, unusually for the French, singular, for this was how he had come to see mathematics as a whole.

So far, eight booklets had been published. All were devoted to aspects of algebra and topology, except for the first one to appear—in 1940, albeit dated 1939—which presented a digest on "naive" set theory (his word), a more formal treatment being announced. Together they formed the first chapters of the first books of "Part I: The Fundamental Structures of Analysis." These chapters were dealing with the bases of analysis in a very general, abstract way. As Beaulieu has documented, this emphasis departed from traditional textbooks. True, Bourbaki promised: "The general principles studied in Part I will find their applications in the following Parts." But, of course, nobody knew then, what these parts would contain or when they would appear.[21]

There was thus a significant shift between what the treatise was and what it promised to be. This shift had its origin in remnants of the initial goal of Bourbaki's members, which, as I said, was to write a textbook of analysis. At first, the group consulted physicists and applied mathematicians, such as Jean Leray, Jean Coulomb and

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[20] N. Bourbaki, "Mode d'emploi de ce traité," in *Théorie des ensembles (Fascicule de résultats). Éléments de mathématique,* Ière partie, livre I (Paris: Hermann, 1939).

[21] The (unpublished) global plan for the treatise, reproduced in Beaulieu, *Bourbaki,* 2: 104, reveals that, in 1941, Bourbaki was at least planning three other parts (functional analysis, differential topology, and algebraic analysis). Bourbaki's quote is to be found in his "Mode d'emploi." For Beaulieu study of earlier textbooks, see *Bourbaki,* 171-190.

Yves Rocard.[22] The future Bourbakis hoped that their book would be useful to students and users of mathematics as well as accomplished mathematicians. For Cartan, this even meant, at the first meeting, that algebra should be eliminated from the treatise. Later that day, Delsarte however proposed that the treatise start with "an abstract, axiomatic exposition of some essential general notions." A consensus emerged for the idea of a short "abstract packet" provided that it was "reduce[d] to the minimum."[23] Beaulieu's account shows, on the contrary, how this "packet" grew in the following years without an explicit decision to that effect. This was nearly the only part of the treatise about which major decisions had been reached before the war dispersed the Bourbakis on both sides of the Atlantic. The later parts had not been so well conceptualized yet. It was thus this abstract part that durably left its imprint on the whole project.

In the writing of the "abstract packet," between 1935 and 1938, Bourbaki developed his own style of presentation. If, at times, some collaborators had proposed texts appealing to intuition and starting with examples, Bourbaki slowly decided to work otherwise. It became customary to present definitions before examples and build general results first, relegating concrete applications to witty exercises. In his own words, Bourbaki constantly proceeded from "the general to the particular."[24] As Beaulieu writes, this "was not a sacred principle given *a priori.* Only after consultations and try-outs was Bourbaki's *exposé* progressively purified from examples."[25] But, Bourbaki knew that this mode of presentation was striking to most reader:

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[22] L. Beaulieu, *Bourbaki,* 156-161.
[23] L. Beaulieu, *Bourbaki,* 150-151.
[24] N. Bourbaki, "Mode d'emploi."
[25] L. Beaulieu, *Bourbaki,* 376.

> The choice of this method was imposed by the principal object of this first part, which is to lay the foundations to the rest of the treatise, and even to the whole of mathematics. For this, it is indispensable to acquire, to start with, a rather large number of very general notions and principles. Moreover, the necessity of demonstration requires that chapters, books, and parts follow each other in a rigorously set order. The usefulness of some considerations will thus appear to the reader only if he already possesses a rather extended knowledge, or then, if he has the patience of suspending his judgment until he has had the occasion of convincing himself of this [usefulness].[26]

This act of faith, demanded from Bourbaki's reader, was made easier by his myth. The pseudonym, as Beaulieu emphasized, certainly had its function for members of the group.[27] It helped diffuse the tensions of collective writing among eminent mathematicians who, investing time and effort, saw their work severely criticized or offhand rejected, with no hope of immediate professional reward. Indeed, authorship for Bourbaki was a complicated affair. In a letter to Jean Perrin, then Under-Secretary of Scientific Research, Szolem Mandelbrojt explained: "Each chapter, after having been . . . discussed at length, is assigned to one of us; the resulting work is seen by all, and is again discussed in details, it is always redone at least once, and sometimes many times. We thus pursue a truly collective *oeuvre,* which will present a deep character of unity." In practice, until the 1960s, it often was Jean Dieudonné who wrote the final version.[28]

From the point of view of his audience, Bourbaki's persona became a powerful guarantee of legitimacy for his authoritative pronouncements. If a group of prominent mathematicians had agreed that these were the basic structures of mathematics then it

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26 N. Bourbaki, "Mode d'emploi."

27 L. Beaulieu, *Bourbaki,* 297-306.This pseudonym came from an old student prank of the *École normale.* In 1923, the freshman class, including Cartan, attended a phony lecture culminating with "Bourbaki's theorem," the name of a French general in 1870. See L. Beaulieu, *Bourbaki,* 278-282.

surely was so. Indeed, while the literature about Bourbaki often emphasized his "polycephalic" nature, it remained discreet about who took part in the writing of his treatise.[29] It was not important to know who these mathematicians were, only that they had achieved a consensus. The myth had the effect of bolstering Bourbaki's scientific authority and hiding arguments among the group. Similarly, one should look at the rumor of Bourbaki's collaborators' retirement at age fifty as catering to the widespread belief that one's best mathematical work was accomplished in one's youth.

c) The Architecture of Mathematics

"This text deserves a special study," Jacques Roubaud recently wrote of Bourbaki's "Architecture." "There, Bourbaki quietly handles properly Neandertalian philosophical bludgeons contrasting with his usual snaky cautiousness." At the same time, Roubaud pointed out just how implicitly Bourbakist was the *Great Currents of Mathematical Thought* as a whole.[30] Even if philosophically naive, and perhaps because of this, this article was widely read.[31] Beaulieu since discovered that Dieudonné was its principal

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[28] Cf. L. Beaulieu, "Jeux d'esprit et jeux de mémoire chez N. Bourbaki," to be published. For Mandelbrojt's quote, see M. Chouchan, *Bourbaki,* 10.

[29] Among the early reviews of Bourbaki addressed to non-mathematicians, let me note: G. Bouligand, "Bourbaki s'affirme!," *Revue générale des sciences et Bulletin de la Société philomathique,* 40 (1948): 241-243; "Bourbaki en expansion," *Ibid.,* 42 (1950): 3-4; "L'École Bourbaki en face des secteurs décisifs de l'analyse," *Ibid.,* 43 (1951): 65-66; "Entre l'algèbre et l'analyse foncionnelle, Bourbaki et ses prouesses," *Revue générale des sciences et Bulletin de l'Association française pour l'avancement des sciences,* 60 (1953): 193-195; "Perspectives mathématiques," *Ibid.,* 62 (1955): 69-81; A. Lichnérowicz, "Qui est Bourbaki?," *Vie intellectuelle,* 16(2) (1948): 118-123; and E. Kahane, "La recherche collective," *La Pensée,* 21 (1948): 85-88.

[30] J. Roubaud, *Mathématique:* (Paris: Seuil, 1997), 114-132; quote on p. 123.

[31] Compare with the sophisticated defense of axiomatics by C. Chevalley and A. Dandieu, "Logique hilbertienne et psychologie," *Revue philosophique de France et de l'étranger,*

author and that it apparently was not discussed by the group.[32] For my purpose, since neither Bourbaki, nor any of his collaborators ever retracted it, we can safely take this article as it was then perceived, that is, Bourbaki's articulation of his own program.

"Mathematical science is in my opinion an indivisible whole, an organism whose vitality is conditioned upon the connection of its parts," David Hilbert had claimed in his famous lecture before the 1900 Paris International Congress of Mathematicians.[33] By 1948, however, Bourbaki noted, mathematicians were producing thousands of pages of new results every year, which, rather than linking the different branches together, increased the specialization of each subdiscipline. Universal minds, like Poincaré and Hilbert, seemed to belong to the past. Now mathematicians only hoped to master their own specialties, which possessed its terminology and methods not necessarily applicable to other fields of mathematics. Worst still, the same terms sometimes meant different things whether used in algebra or topology. The question was therefore worth raising: "Is the mathematics of today singular or plural?"[34]

For Bourbaki, as for Hilbert, there was really no choice to be made. The unity of mathematics was taken for granted, and if it was not unified, then the goal was to strive for unity. He believed that

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57 (1932): 399-411, and C. Chevalley, "Variations du style mathématique," *Revue de métaphysique et de morale,* no. 3 (1935): 375-384.

[32] L. Beaulieu, *Bourbaki,* vol. 2, 77.

[33] D. Hilbert, "Mathematical Problems." *Bulletin of the American Mathematical Society,* 8 ([1900] 1902): 437-479, authorized transl. M. W. Newson; quoted from C. Reid, *Hilbert--Courant* (New York: Springer, 1986), 82.

[34] N. Bourbaki, "The Architecture of Mathematics," 24. In the following section, I quote consistently from the version published in F. Le Lionnais, *Great Currents of Mathematical Thought,* and indicate the page number in parentheses.

> the internal evolution of mathematics has, in spite of appearances, tightened the unity of the various parts. . . . The essence of this evolution has consisted in a systematization of the relations existing among the various mathematical theories, and is comprised in an approach generally known under the name of the 'axiomatic method'.[35].

The axiomatization of various branches of mathematics was hardly anything new in 1948. But Bourbaki based his use of axiomatics on a new concept that allowed him, so he thought, to extend coherently this method to all of modern mathematics. "The common trait of the various notions designated by [the] generic name" of *mathematical structure,* Bourbaki wrote, "is that they apply to sets of elements whose nature *is not specified.*"[36] This term had first entered Bourbaki's discussions in 1936, with a sentence close to his later conception: "the object of a mathematical theory is a structure organizing a set of elements."[37] Then considered as not definable, not formally defined before 1957, the notion of structure always remained problematic for Bourbaki's enterprise, and at the same time central to his discourse.[38]

Bourbaki liked to recall that natural numbers were a structured set of elements that had lost all relation to referents. Nobody used different arithmetics to count apples, sheep, francs, or kilos. Above all, Bourbaki's archetypal example was the algebraic structure of *groups,* which had proved fruitful in algebra, as well as in physics and chemistry. In his article, he popularized the notion of group—sets endowed with a law of composition

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[35] N. Bourbaki, "The Architecture," 25.
[36] N. Bourbaki, "The Architecture," 28. His emphasis.
[37] Quoted by L. Beaulieu, *Bourbaki,* 327.
[38] See L. Corry, "Nicolas Bourbaki and the Concept of Mathematical Structure," *Synthese,* 92 (1992): 315-348; and Corry's book, *Modern Algebra and the Rise of mathematical Structure.*

satisfying three simple axioms.[39] Groups were also used by Weil in his appendix to Lévi-Strauss's book. They thus became the mathematical structure *par excellence* for non-mathematicians.

What Bourbaki did not say was that the three group axioms were not arbitrary. Slowly, mathematicians had come up with the group concept, after having been exposed through decades of experience to similar properties in many different cases.[40] With Bourbaki, following here earlier mathematicians, collections of objects that mathematics had studied for centuries were turned into mere instances of groups. For example, the set **Z** of integers (0, 1, 2, 3, etc. and negative numbers) with respect to addition, real numbers (without 0) with respect to multiplication, rotations in the plane, substitutions, etc., were all groups.

Just as we always can be sure that two apples and two apples make four apples because 2+2=4, Bourbaki showed that, using the group axioms, it was possible to prove once and for all certain properties that applied to all groups. For example, the theorem that if $x \tau y = x \tau z$, then surely $y = z$ can be proven at once for all groups.[41] Above all,

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[39] Given a set G, suppose that it can be endowed with an "operation" τ, which, to any two elements x and y of the set G, associates a third element z, corresponding to the "product" of x by y, noted $z = x \tau y$. In order for G to be a group under this operation, the following three properties have to be verified. (1) For all elements x, y and z of G, we have $x \tau (y \tau z) = (x \tau y) \tau z$ ("associativity"); (2) G possesses a neutral element e, such that $e \tau x = x \tau e = x$; and (3) each element of G has an inverse x^{-1}, so that $x \tau x^{-1} = x^{-1} \tau x = e$.

[40] H. Wussing, *The Genesis of the Abstract Group Concept: A Contribution to the History of the Abstract Group Concept,* transl. A. Shenitzer (Cambridge: MIT Press, [1969] 1984).

[41] The proof is the following: from property (3) above, we know there is a x^{-1} in G, we deduce that $x^{-1} \tau (x \tau y) = x^{-1} \tau (x \tau z)$; then, because of associativity, $(x^{-1} \tau x) \tau y = (x^{-1} \tau x) \tau z$; therefore, using (3) again, we find $e \tau y = e \tau z$; finally, applying (2), we get $y = z$. Q.E.D.

Bourbaki wanted his readers to note that "the *nature* of the elements *x*, *y*, *z* was completely irrelevant in this argument."[42] This is why structures were defined as sets of elements whose nature was arbitrary. In his conception of mathematics, Bourbaki noted, the only "mathematical objects" that remained were structures. Three great types of structures existed; they were the mother-structures of order, algebra, and topology. A whole hierarchy of increasingly complex structures, which could be built upon this foundation, formed the true architecture of the mathematical edifice, not the old disciplines of algebra, geometry, analysis, number theory, etc. Confidently, Bourbaki claimed:

> On these foundations, I state that I can build up the whole of the mathematics of the present day; and, if there is anything original in my procedure, it lies solely in the fact that, instead of being content with such a statement, I proceed to prove it in the same way as Diogenes proved the existence of motion; and my proof will become more and more complete as my treatise grows.[43]

In practice, such a construction followed the axiomatic method. "In order to define a structure," Bourbaki wrote, "one or several relations involving [the] elements are given; . . . it is then postulated that the given relations satisfy certain conditions . . . which are the *axioms* of the structure envisaged." When the axiomatic basis for a theory was in place, the rest was a game of internal logical deductions.

> To study the axiomatic theory of a given structure is to deduce the logical consequences of the axioms of the structure, *while excluding all other hypotheses* about the elements considered (in particular, any hypothesis concerning their special 'nature').[44]

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[42] N. Bourbaki, "The Architecture," 28.
[43] N. Bourbaki, "Foundations of Mathematics for the Working Mathematician," *Journal of Symbolic Logic,* 14 (1949): 1-8, quote on p.8.
[44] N. Bourbaki, "The Architecture," 28-29.

Mathematicians thus had their goals set for them. For the next few centuries, they needed only to study the logical implications of their axioms. They could do away with external inspiration. In summary, Bourbaki's general attitude expressed that "mathematics [was] an autonomous abstract subject, *with no need of any input from the real world,* with its own criteria of depth and beauty, and with an internal compass for guiding future growth."[45]

Of course, mathematicians were not abstract thinking machines, and Bourbaki acknowledged that intuition had an important role to play in research; not "the intuition of common sense, but rather a sort of divination (prior to all reasoning) of the normal behaviour [mathematicians] had a right to expect from the mathematical entities which a long association had rendered as familiar to [them] as objects of the real world."[46] This intuition was thus purely internal to the logic of mathematics; it was an immediate knowledge of structures and nothing more.

Bourbaki's axiomatics isolated mathematics from any other field of knowledge. He did away with both historical reliance on physics and foundation on logic. In 1948, Weil listed van der Pol's equation as

> one of the few interesting problems which contemporary physics has suggested to mathematics; for the study of nature, which was formerly one of the main sources of great mathematical problems, seems in recent years to have borrowed from us more than it has given us.[47]

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[45] P. D. Lax, "The Flowering of Applied Mathematics in America," *A Century of Mathematics in America,* Part II, ed. P. Duren with R. A. Askey and U. C. Merzbach (Providence: American Mathematical Society, 1989): 455-466.
[46] N. Bourbaki, "The Architecture," 31.
[47] A. Weil, "The Future of Mathematics," in *Great Currents,* ed. F. Le Lionnais: 321-336, 332.

On the other hand, Bourbaki claimed that mathematics was relatively independent from formal logic at the level of the "working mathematician," because, whatever foundational questions remained, his approach being constructive, the sole constraint was that the construction stayed free from contradiction, which was the case up to the present.[48]

Axiomatics freed mathematics from reality, or rather from errors due to the abuse of our intuition. Therefore, its utility for other sciences remained an open question for Bourbaki.

> That there is a close connection between experimental phenomena and mathematical structures seems to be confirmed in a most unexpected manner by the recent discoveries of contemporary physics; but we do not know at all the deep-lying reasons for this, . . . and we may never know them. . . . Mathematics appears on the whole as a reservoir of abstract *forms*—the mathematical structures; and it sometimes happens, without anyone really knowing why, that certain aspects of experimental reality model themselves after certain of these forms.[49]

Moreover, this position conveniently made mathematics independent from the moral choices faced by politicians, engineers, and other scientists.

> Why have some of the most intricate theories in mathematics become an indispensable tool to the modern physicist, to the engineer, and to the manufacturer of atom-bombs? Fortunately for us, the mathematician does not feel called upon to answer such questions, nor should he be held responsible for such use or misuse of his work.[50]

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[48] N. Bourbaki, *Éléments de mathématique. Livre 1: Théorie des ensembles,* Chapitre 1 et 2 (Paris: Hermann, 1954), introduction.
[49] N. Bourbaki, "The Architecture," 35-36. His emphasis.
[50] N. Bourbaki, "Foundations of Mathematics," 2.

d) Structures of Kinship

If Bourbaki was so skeptical of other sciences having a role to play in orienting contemporary mathematical research, how, then, are we to understand André Weil's collaboration with Claude Lévi-Strauss? He did, after all, write a mathematical appendix for *The Elementary Structures of Kinship.* In fact, Weil's involvement conformed to Bourbaki's philosophy. Bourbaki welcomed the application of mathematics to other fields of knowledge. The emphasis must be put here on the word *application,* which already presumes the nature of the relation between mathematics and science.[51] Bourbaki felt that mathematics should remain free from external influences; he considered that problems of application were in themselves quite unappealing, since they would not entail the development of new mathematics; but he was happy to witness the use of his mathematical theories by others, including (but rarely) his collaborators.

Perhaps only the special circumstances of World War II, which sent them both to New York City, at the *École libre des hautes études* (Free School for Advanced Studies), a university for exiles, made it possible for Lévi-Strauss and Weil to work on a common project. The anthropologist Lévi-Strauss started to work on what would become *Elementary Structures* in 1943. Soon, according to his recollections, he faced problems of Australian kinship so complex that he thought only a mathematician could solve them. He first went to see Jacques Hadamard, an accomplished mathematician in his seventies, who told him that he could not help. Lévi-Strauss then turned to Weil who worked out a

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[51] About, the abuses of the word *application* concerning mathematics, see J.-M. Lévy-Leblond, "Physique et mathématique," *Encyclopaedia universalis,* 13 (1968): 4-8.

scheme that involved groups. Hadamard's and Weil's reactions nicely encapsulate the views of their respective cohorts about the proper objects of mathematics. While Hadamard said that "mathematicians [knew] only four operations and that marriage was not one of them," Weil countered that there was no need "to define marriage from a mathematical standpoint. *Only relations between marriages are of interest.*"[52] Where for Hadamard marriage was not amenable to mathematical treatment because it was not a mathematical object, Weil could not care less, since in Bourbakist thought the nature of objects was irrelevant. Only the structure of sets mattered.

Lévi-Strauss could not have agreed more. But he had reached this conclusion by following a different route. He often acknowledged that his notion of structure was imported from linguistics. In New York he befriended the Russian-born linguist Roman Jakobson, who taught structural phonology (or phonemics) at the *École libre.*[53] The history of structural linguistics has often been told, starting with Saussure at the beginning of the century, culminating with the French structuralist movement of the sixties, *via* the interwar Prague Linguistic Circle to which Jakobson belonged.[54] The Prague linguists started to use the term *structure* around 1929.

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[52] C. Lévi-Strauss and D. Éribon, *Conversations with Claude Lévi-Strauss,* transl. P. Wissing (Chicago: University of Chicago Press, [1988] 1991), 52-53. My emphasis. See also A. Weil, *Apprenticeship of a Mathematician,* transl. J. Gage (Basel: Birkhäuser, [1991] 1992), 185.

[53] C. Lévi-Strauss and D. Éribon, *Conversations,* 41; and C. Lévi-Strauss, *The View from Afar,* transl. J. Neugroschel and P. Hoss (New York: Basic, [1983] 1985), 138-147.

[54] See, for example, J.-M. Benoist, *The Structural Revolution* (London: Weidenfeld & Nicholson, [1975] 1978); F. Dosse, *Histoire du structuralisme,* 2 vols. (Paris: La Découverte, 1991-1992); R. Harland, *Superstructuralism: The Philosophy of Structuralism and Post-Structuralism* (London, New York: Methuen, 1987); J. G. Merquior, *From Prague to Paris: A Critique of Structuralist and Post-Structuralist Thought* (London, New York: Verso, 1986); T. G. Pavel, *The Feud of Language: A*

Incidentally, Bourbaki's use of the term possibly stems from the same source. "As for the choice of the word 'structure,' my memory fails me," André Weil had to admit in his memoirs, but he ventured this explanation: "at the time, I believe, it had already entered the working vocabulary of linguists, a milieu with which I had maintained ties (in particular with Émile Benveniste)."[55] However, we should remain somewhat doubtful about this late recollection. Bourbaki adopted the term just a few months after the election of the *Front Populaire,* who popularized the phrase "réforme des structures" for its nationalization policy. Furthermore there was a history of using the term in mathematics as well. In the 1880s, Sophus Lie already talked of the "structure of a group," and Élie Cartan (Henri's father) wrote a thesis in 1894 titled *La Structure des groupes continus.* Later, in the early 20th century, Ore and Glivenko named *structures* what are now known as lattices.[56] As a consequence, a member of Bourbaki could write that one of the goals of contemporary mathematics was "the structural analysis of already known facts."[57] There is therefore no firm historical reason to assume, on the sole basis of their common name, that the mathematicians' structures and the linguists' were closely related.

Lévi-Strauss was clearly inspired by the linguists, rather than the mathematicians. "Linguistics occupies a special place among the social sciences," he wrote in 1945. "It is

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History of Structuralist Thought, transl. L. Jordan and T. G. Pavel (Oxford: Basil Blackwell, 1989).

[55] A. Weil, *Apprenticeship,* 114.

[56] A. Marchal, "L'attitude structuraliste et le concept de structure en économie politique," in *Sens et usages du terme structure dans les sciences humaines et sociales,* ed. R. Bastide, Janua linguarum, 16 (The Hague: Mouton, 1962): 63-67; see also G. Guilbaud's comment *Ibid.,* 140-141.

[57] C. Chevalley, "Variations du style mathématique," 384.

probably the only one which can truly claim to be a science."[58] During his field work in Brazil, he had difficulty denoting some Amerindian languages. Thinking that acquiring the basics of linguistics might help him, he was happy when Alexandre Koyré introduced him to Jakobson. Lévi-Strauss benefited much more than he had anticipated from this encounter. "At the time I was a kind of naive structuralist, a structuralist without knowing it. Jakobson revealed to me the existence of a body of doctrine that had already been formed within a discipline, linguistics, with which I was unacquainted."[59] What Jakobson taught in his course on phonology could be directly applied, Lévi-Strauss thought, to anthropology.

In his lectures at the *École libre,* published in 1976 with a preface by Lévi-Strauss, Jakobson investigated the union between the sound of a spoken word and its meaning, or in Saussurian terms, between *signifier* and *signified.* If previous schools had carefully studied the physiological origins of human phonemes, that is, phonetics, they had substituted "strictly causal questions for questions concerning means and ends."[60] They went back to the origins of the phenomena without having properly described them. They were thus faced, Lévi-Strauss quoted, with a "'stunning multitude of variations,' whereas explanations ought always aim at the discovery of 'the invariants behind all this

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[58] C. Lévi-Strauss, "Structural Analysis in Linguistics and in Anthropology," in *Structural Anthropology,* transl. Brooke G. Schoepf (New York: Basic Books, 1963), 31-54; 31. Originally published in French in *Word: Journal of the Linguistic Circle of New York,* 1(2) (1945), 1-21; repr. in *Anthropologie structurale* (Paris: Plon, 1957), 37-62.
[59] C. Lévi-Strauss and D. Éribon, *Conversations,* 41.
[60] R. Jakobson, *Six Lectures on Sound and Meaning,* transl. J. Mepham (Hassocks, UK: Harvester Press, [1976] 1978), 35. C. Lévi-Strauss's preface is repr. as "The Lessons of Linguistics," in *The View from Afar,* 138-147, cited above.

variety'."[61] Phonology was the structural analysis of phonemes, not in the specific forms in which they appear, but with respect to their relations to one another (usually binary oppositions) within a system. Or as Trubetzkoy, another member of the Prague Circle wrote in 1933, "phonology, universalistic by nature, starts with the [linguistic] system as an organic whole, whose *structure* it studies."[62] From the late 1920s onward, Jakobson always conceived of this idea of considering objects not for what they were, but for how they related to one another, as a general trend pervasive of all aspects of science and culture. In particular, he identified this trend as being constitutive of modern mathematics as well, exemplified by Felix Klein's *Erlanger Programm.*[63] To name this common trend, Jakobson coined the word *structuralism* in 1929.

> Were we to comprise the leading idea of present-day science in its most various manifestations, we could hardly find a more appropriate designation than *structuralism.* Any set of phenomena examined by contemporary science is treated not as a mechanical agglomeration but as a structural whole, and the basic task is to reveal the inner, whether static or developmental, laws of this system.[64]

Lévi-Strauss wanted to uncover common features among systems of kinship. How to make sense of the mind numbing variety encountered in different cultures? Who was allowed to marry whom? And why was incest, unique among this rich diversity, a universal taboo? From Jakobson's linguistics, Lévi-Strauss learned that "instead of being

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[61] C. Lévi-Strauss, in R. Jakobson, *Six Lectures,* xii; C. Lévi-Strauss, *The View from Afar,* 139. Lévi-Strauss is quoting from p. 9 of Jakobson's book.
[62] N. Trubetzkoy, "La phonologie actuelle", *Psychologie du langage* (Paris, 1933), 227-246, 233; quoted by É. Benveniste, " 'Structure' en linguistique," in *Sens et usages,* 31-39; on p. 35. My emphasis.
[63] R. Jakobson, "Retrospect (1961)," *Selected Writings I* (The Hague, Paris: Mouton, 1971), 632-637.
[64] Quoted by E. Holenstein, *Roman Jakobson's Approach to Language: Phenomenological Structuralism,* transl. C. and T. Schelbert (Bloomington: Indiana University Press, [1974] 1976), 1.

led astray by a multiplicity of terms, one should consider the simplest and most intelligible relationships uniting them."[65] An important aspect of Jakobson's structuralism indeed was his reductionist focus on the smallest unit of spoken language, the phonemes. Similarly, Lévi-Strauss emphasized elementary structures, determined by the internal dynamic of kinship, rather than the more complex ones depending on economic or political relations. For him, the first and foremost of characteristic of a structure was that "*it consists of elements such that any modification of one of them entails a modification of all others.*"[66] With the help of such a structural analysis, he reformulated, and turned around, the question of kinship. Incest was prohibited because of the necessity of alliances between clans, and not the reverse. Lévi-Strauss synthesized many anthropological traditions managing "to escape from the Scylla of thoughtless empiricism and the Charybdis of factless philosophizing."[67] Moreover, tapping into the status of structural linguistics helped him emphasize the *scientific* nature of his results.

It will be clear from the above, I hope, that there were significant differences between Lévi-Strauss's structural analysis of kinship and Bourbaki's structural view of mathematics, although they surely exhibited common features. Both aimed at unifying their respective discipline by emphasizing elementary structures. But while Bourbaki imposed systemic structures onto sets of unspecified elements, Lévi-Strauss emphasized

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[65] C. Lévi-Strauss, *The View from Afar,* 139; "Preface" in R. Jakobson, *Six Lectures,* xii.
[66] C. Lévi-Strauss, "Social Structure," *Anthropology To-Day: Wener-Gren Foundation International Symposium on Anthropology,* ed. A. L. Kroeber (New York: Columbia University Press, 1952); repr. in *Anthropologie structurale* (Paris: Plon, 1958), 303-351. Quote in chap. 15, part 1; my emphasis. In this text, Lévi-Strauss considered analogies in mathematics, taken from cybernetics, information theory, and game theory, rather than from Bourbaki.

the irreducible relations linking elements together. In the appendix of *Elementary Structures,* Lévi-Strauss underscored the distinction between Weil's analysis and his own method. Concerning one of Weil's results, he wrote that he had already reached the same conclusion by following "a *structural* analysis, and the *mathematical* analysis confirms it."[68] Most significantly in historical terms, "Weil's intuition of the potential of group theory for the analysis of kinship systems . . . turned out to have no influence on the later work of Lévi-Strauss," although he was never immune to a variety of scientific metaphors.[69] Neither were the two methods totally disjointed, as Lévi-Strauss was well aware. "This mathematical demonstration," he commented in 1988, "proceeded from principles akin to those that Jakobson applied in linguistics, since in both cases the focus moves from the terms themselves to the relationships operating between them."[70] In conclusion, we can reasonably say that the intersection of Lévi-Strauss, Jakobson, and Weil, in New York City in 1943, by crossbreeding anthropology, linguistics, and

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[67] H. Gardner, *The Quest for the Mind: Piaget, Lévi-Strauss, and the Structuralist Movement,* 2nd ed. (Chicago and London: University of Chicago Press, 1981), 40.
[68] C. Lévi-Strauss, *Structures élémentaires,* 285-286.
[69] P. Jorion and C. Assaba's comments (pp. 377-378) following an article by M. W. Barbosa de Almeida ["Symmetry and Entropy: Mathematical Metaphors in the Work of Lévi-Strauss," *Current Anthropology,* 31 (1990):367-385], an article in which the importance of Lévi-Strauss's use of mathematics is hotly debated. Weil's models were however developped by H. C. White, *An Anatomy of Kinship: Mathematical Models for Structures of Cumulated Roles* (Englewoods Cliffs, NJ: Prentice-Hall, 1963) and P. Courrège, "Un modèle mathématique des structures élémentaires de parenté," *L'Homme,* 5, no. 3-4 (1965): 248-290; repr. in *Anthropologie et calcul,* ed. P. Richard and R. Jaulin (Paris: Union générale d'éditions, coll. "10/18", 1971), 126-181. Courrège imself was an ardent Bourbakist, it should be noted, according to the romanced portrait in J. Roubaud, *Mathématique:,* 73-81. See C. Lévi-Strauss, "The Mathematics of Man," *International Social Science Bulletin,* 6(4) (1954): 581-590, for his view of the usefulness of mathematical methods; and for a critique, see A. Régnier, "De la théorie des groupes à la pensée sauvage," *Anthropologie et calcul,* 271-298 et *La Crise du langage scientifique* (Paris: Anthropos, 1974).

mathematics, helped make structuralism possible. And although the dialogue between mathematics and structuralism failed to be sustained, this fortuitous encounter was the seed for a lasting cultural connection.

3. HEGEMONY

From the postwar period to the late 1960s, the authority of structuralism in the human sciences and of Bourbakism in mathematics grew, until they achieved dominant positions within their respective domains. Both arguably peaked around 1966 only to begin a parallel decline. How are we to understand this coincidence? Were these two movements, both propagandizing the use of structure, facets of a larger trend? Did one depend on the other for its success? Or did they speak to one another? I offer here an account of the contacts between the two kinds of structuralism, which highlights the actual effects of a mostly failed discussion. Finally, by focusing on a literary group, the Oulipo, I show the impact that it could have on the outside.

a) Bourbaki's Reign

Bourbaki did not need structuralism to establish his hegemony over his discipline. He added new booklets to his *Éléments de mathématique,* more than 35 of which were published by the end of the 1970s. As early as 1951, several of the earlier volumes were revised and republished. In 1958, Russian translations started to appear. In 1966, the first volumes on *General Topology* were translated into English. Meanwhile, Bourbaki's

[70] C. Lévi-Strauss and D. Éribon, *Conversations,* 53.

programmatic article "The Architecture of Mathematics" was translated into English, Portuguese, Russian, German, and Japanese.[71]

But Bourbaki was more than just another successful author. His vision permeated all of mathematics. Some of his collaborators or students were regularly among the winners of the Fields Medal, the highest distinction for mathematicians: Laurent Schwartz in 1950, Jean-Pierre Serre in 1954, René Thom in 1958 (never himself a Bourbaki, but a student of Cartan and Ehresmann). In 1966, at the International Congress of Mathematicians held in Moscow, Henri Cartan was elected president of the International Mathematical Union for the next four years. Three of the four Fields-Medal winners were introduced by Cartan, Thom, and Dieudonné, who lavishly praised his colleague Alexander Grothendieck when he presented him with the fourth Medal in 16 years awarded to a French mathematician. When one unnamed mathematician "remembered the Bourbaki influence on two other [1966] Fields-prize winners, M. Atiyah and S. Smale, he could not help concluding that the Moscow Congress was indeed dominated by Bourbaki."[72]

If Bourbaki shaped mathematics internationally, this was even truer in France. After World War II, the Bourbakis had become established mathematicians. Henri Cartan became the statesman of French mathematics. Teaching at the *École normale supérieure* from 1940 on, he bred an entire generation of French mathematicians to whom he would

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[71] L. Beaulieu, *Bourbaki.*
[72] Quoted by J. Fang, *Bourbaki,* 58. Retrospectively, it may appear highly problematic to include Thom, Atiyah, and Smale in Bourbaki's sphere of influence, but their styles and topics certainly were, then, close to his own. For a view of Bourbaki's international dominance, see V. I. Arnol'd, "Will Mathematics Survive?: A Report from the Zurich Congress." *Mathematical Intelligencer,* 17(3) (1995): 6-10.

strongly suggest studying Bourbakist mathematics.[73] Jean Dieudonné's voice was heard by a large audience of mathematicians. From 1948 on, they had their own *séminaire Bourbaki,* which became a most prestigious outlet for research, and a pageant for job seekers. As a symbol of this rise to prominence, four of Bourbaki's founders received a substantial prize (200,000 F) from the Academy of Sciences in 1966.[74] Surely enough, the Academy would, after the usual lag, fill up with Bourbakis. By 1976, they would occupy three of the six seats of the Mathematical Section (Cartan, Mandelbrojt, and Schwartz), three more Bourbakis having been elected either as non-resident member (Dieudonné), or correspondents (Chevalley and Serre).

Mostly, from the forties onward, Bourbaki's dynamic nature oriented ambitious students towards his topics of predilection: algebraic geometry above all, but also number theory, group theory, algebraic and differential topology, a hierarchy best exemplified by Dieudonné's *Panorama.*[75] Reforms of the higher mathematical curriculum were partly inspired by Bourbaki's treatise and its message. Moreover, Bourbaki's logical rigor, his conspicuous modernity, the proclaimed exhaustively of his enterprise, and the absolute certainty of the results he exposed in his treatise, all exerted a powerful appeal for the younger generation of the cold war. Many young men who studied mathematics at the

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[73] M. Andler, "Les mathématiques à l'École normale supérieure au XXe siècle: une esquisse." *École normale supérieure. Le livre du bicentenaire,* ed. J.-F. Sirinelli (Paris: Presses universitaires de France, 1994): 351-404, esp. pp. 371-380.
[74] L. Beaulieu, *Bourbaki,* 160; *Comptes rendus de l'Académie des Sciences, Vie académique,* 263 (1966), 146.
[75] J. Dieudonné, *Panorama des mathématiques pures. Le choix bourbachique* (Paris: Gauthier-Villars, 1977).

University, in the 1940s and 1950s have testified to the subtle blend of pressure and appeal that Bourbaki exerted on the young generation.[76]

Bourbaki's dominance notwithstanding, there was room for other approaches to develop, even in France. But the following two examples show that this could be arduous. In 1958-59, when some Paris mathematicians feared that the French probabilistic tradition (Borel, Fréchet, Lévy) might be interrupted, they had to invite a French *émigré,* Michel Loève, to "sow the good seed."[77] Among his students was Paul-André Meyer who, with Jacques Neveu, would later build a French school of probability theory, a topic neglected by Bourbaki. A second example is the conference on "Forced Vibrations in Non-Linear Systems," organized by the CNRS [National Center for Scientific Research], and held in Marseille in 1964. In his introduction, the editor noted that problems concerning nonlinear systems were traditionally assigned a place within mechanics. But since new progress in functional analysis were especially exciting to him, this led the study of nonlinear systems to sit on the border of physics and mathematics: an "uncomfortable

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[76] See, *e.g.,* A. Grothendieck, *Récoltes et semailles. Réflexions et témoignage sur un passé de mathématicien,* 7 vols. (Montpellier: Université des Sciences et Techniques du Languedoc and CNRS, 1985), Fine Library, Princeton University; M. Serres, *Éclaircissements. Entretiens avec Bruno Latour* (Paris: François Bourin, 1992), 21-23; *Conversations on Science, Culture, and Time,* transl. R. Lapidus (Ann Arbor: University of Michigan, 1995), 10-11; J. Roubaud, *Mathématique:,* L. Schwartz, *Un mathématicien aux prises avec le siècle* (Paris: Odile Jacob, 1997). See also the account of the mathematical education of the Economics Nobel prize winner, Gérard Debreu, by E. R. Weintraub and P. Mirowski, "The Pure and the Applied: Bourbakism Comes to Mathematical Economics," *Science in Context,* 7 (1994): 245-272.

[77] G. Choquet, "Témoignage sur Paul André Meyer," *Gazette des mathématiciens,* no. 68 (April 1996): 13-14

situation, certainly in France."[78] Only in the late 1960s could a French school of applied mathematics develop under the leadership of Jacques-Louis Lions.[79] Meanwhile, as mathematicians looked for ways get around Bourbaki's dominance over their field, his name had begun being invoked again in the human sciences.

b) The Rise of Structuralism: The First Interdisciplinary Conferences (1956-1959)

At first, Lévi-Strauss's *Elementary Structures* was well received even among existentialists.[80] But structuralism's rise to prominence on the French intellectual scene was slower than in mathematics. Only in the late 1950s did its impact begin to be widely felt, and not before 1966-1968 did structuralism definitively replace existentialism at the zenith of French philosophy. Only then had Claude Lévi-Strauss, Roland Barthes, Jacques Lacan, and Michel Foucault taken Sartre's place; "those were the names of the 'new masters' [*nouveaux maîtres*]," among whom Louis Althusser should definitely be counted.[81] Each of them had developed new methods, borrowing from structural linguistics, which they used, respectively, in anthropology, literary and cultural criticism, psychoanalysis, history, and Marxist philosophy. None of them, however, not even Lacan who sprinkled his language with mathematical metaphors, felt the need to base his

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[78] T. Vogel, ed., *Les Vibrations forcées dans les systèmes non-linéaires,* Colloque international du CNRS, no. 148, Marseille: 7-12 September 1964 (Paris: Éditions du CNRS, 1965), 11-12.
[79] A. Dahan Dalmedico, "Polytechnique et l'École française de mathématiques appliquées," *La France des X, deux siècles d'histoire,* ed. B. Belhoste, et al. (Paris: Economica, 1995) and "L'essor des mathématiques appliquées aux États-Unis: l'impact de la seconde guerre mondiale," *Revue d'histoire des mathématiques,* 2 (1996): 149-213.
[80] See S. de Beauvoir's review in *Les Temps modernes,* 5, no. 49 (1949): 943-949.

method on Bourbaki's structures. In 1951, Lacan, Lévi-Strauss, and Benveniste had started to meet together with the mathematician Georges-Théodule Guilbaud in order to work on structures and find links between the human sciences and mathematics, without seemingly emphasizing Bourbaki's structural approach.[82] This effort, however, would not be widely felt until later.

From the late 1950s to the late 1960s, structuralism happened: it became a social phenomenon that extended much beyond four or five great "masters." Philosophers, humanists, and social scientists embraced the notion of structure as a fundamental tool for their disciplinary activities and for bridging across different sciences. It was then that Bourbaki massively appeared in the literature dealing with structure. He helped a newer generation—one that was not necessarily younger in age, but that followed into the steps of the "masters"—to grasp structuralist scholarship as a coherent whole. Seeking to define *structure,* they found Bourbaki useful. Not only did he provide a definition, either formalized in mathematical jargon, or simple enough to be used casually, he could also help gather scientific prestige for structuralism.

Following the publication of Lévi-Strauss's *Structural Anthropology*, two interdisciplinary conferences were held in 1959 with the explicit aim of mapping out the meaning of *structure.* Interestingly, notions of mathematical structures, and especially Bourbaki's, figured prominently at both meetings. On January 10-12, a symposium on the "Meanings and Uses of the Term Structure in the Human and Social Sciences," sponsored

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[81] B. Pingaud in *L'Arc,* no. 30 (March 1966). Quoted in *Les Idées en France,* ed. A. Simonin and H. Clastres, 228.
[82] É. Roudinesco, *Jacques Lacan. Esquisse d'une vie, histoire d'un système de pensée* (Paris: Fayard, 1993), 469.

by Unesco and held in Paris, met with the goal of preparing an entry for the *Dictionnaire terminologique des sciences sociales.* The stars of the conference were Lévi-Strauss, Benveniste, and Merleau-Ponty. Later that year, another conference was held, from July 25 to August 3, at Cerisy-la-Salle, where Swiss psychologist Jean Piaget was the driving force. This conference focused on the dual theme of "Genesis and Structure."[83]

Although both conferences were sponsored in part by the VIth Section of the *École pratique des hautes études,* few people participated in both. But, to the sociologist Lucien Goldmann, who did, the approaches of the two conferences differed enough to warrant a distinction between two sorts of structuralism—a distinction which I shall adopt here, more for convenience than because it reflected a fundamental division of structuralists, because I found that it often aptly overlapped with different attitudes towards the relation of structuralism to mathematics.[84] On the one hand there was the more standard "non-genetic structuralism," identified with Lévi-Strauss's, which postulated the existence of permanent and universal structures and relinquished all attempts at explaining them. On the other hand, Piaget's "genetic structuralism" strove to explain both the structures and their genesis. If at the Paris symposium the matter of genesis spurred "passionate discussions," the consensus clearly went against an absent Piaget: "The concept of structure appears as a 'synchronic' concept."[85] The lesson taken

[83] Proceedings for these two conferences were subsequently published: R. Bastide, ed., *Sens et usages du terme structure dans les sciences humaines et sociales,* Janua linguarum, 16 (The Hague: Mouton, 1962); M. de Gandillac, L. Goldmann, and J. Piaget, eds., *Entretiens sur les notions de genèse et de structure,* Colloque de Cerisy-la-Salle (Paris, The Hague: Mouton, 1965).
[84] L. Goldmann, "Introduction générale," *Genèse et structure,* 7-22; and "Le concept de structure significative en histoire de la culture," *Sens et usages,* 124-135.
[85] R. Bastide, "Introduction à l'étude du mot 'structure'," in *Sens et usages,* 9-19, on p. 17.

away from the meeting at Cerisy-la-Salle was the exact opposite: "Genesis without structure would be blind, and structure without genesis would remain empty."[86]

Contrary to the two conferences above who offered clear visions of what structuralism should be, a third meeting had been held in Paris, three years earlier, on April 18-27, 1956, whose theme already was: "Notion of Structure and Structure of Knowledge." Organized by the *Centre international de Synthèse,* under the auspices of the old Sorbonne, this conference might be characterized, I suggest, as a *non-structuralist* (and rather unsuccessful) attempt at synthesizing knowledge with the help of the notion of structure. By studying the role played by this notion in several disciplines, the organizers of the *Synthèse* week hoped to exhibit an "*isomorphism* between different sectors of knowledge," and deal with "the problem of the structure of the synthesis of the sciences." Because it raised more questions than it provided answers, this "week" remained distinctly non-structuralist: "no solution has been found; the structure of knowledge has not been defined." Lévi-Strauss's name was only once mentioned in passing; none of the other usual names (Jakobson, Lacan, Barthes, etc.) was invoked; linguistics was completely neglected. In my view, the *Synthèse* week at least demonstrates that the notion of structure was then very commonly used and that, in 1956 as opposed to 1959, it was up for anyone to grab.[87]

Given the considerable divergence between all three conferences on a number of fundamental issues, it is remarkable that, each time, mathematics played a comparable

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[86] M. de Gandillac, "Jalons pour une conclusion," in *Genèse et structure,* 337-353, on p. 347; paraphrasing a comment made earlier by Jean Desanti, *Ibid.,* 153.
[87] Proceedings were also published: XX^{e} Semaine de Synthèse, *Notion de structure et structure de la connaissance* (Paris: Albin Michel, 1957), pp. xi-xii, and xxiii for quotes.

role and was given the same kind of prominence. Because of the endorsement it could offer, mathematics exerted a universal appeal. At both 1959 structuralist conferences, participants eagerly emphasized the scientific character of their endeavor. Genetic and non-genetic structuralism tapped into the scientific prestige of biology and mathematics. But while both disciplines could offer legitimacy, the models they proposed were different. Biology served as a model for those emphasizing relations among elements of the structure; and mathematics, for those studying its systemic essence. Significantly, the Paris symposium emphasized biology, especially in the published proceedings, and the Cerisy conference considered biology in a rather inconsequential manner.

On the conceptual level, assessments of the structural view of mathematics, always emphasizing Bourbaki, were strikingly similar at all three meetings. Its most important contribution was precision. "For a mathematician, the meaning of the notion of structure offers no ambiguity at all."[88] For Jean Desanti, Daniel Lacombe, and Georges Guilbaud—who, respectively, represented the mathematicians' view at the Cerisy conference, the *Synthèse* week, and the Paris symposium—structure was both a notion and a term that had internal histories in mathematics, which culminated in, but did not end with, Bourbaki. What a structure was for Bourbaki—an axiomatized collection of relationships among elements of a set, whose nature remained unspecified—was readily acceptable for any brand of structuralism.

The debate therefore was elsewhere: it focused on whether there was a nontrivial core to the notion of structure, and in particular if Bourbaki's definition meant something

[88] J. Desanti, "Remarques sur la connection des notions de genèse et structure en mathématiques," *Genèse et structure,* 143-159, 143.

outside of mathematics. A deeper look at the two types of structuralism reveals that they diverged in their actual use of mathematics. In general, non-genetic structuralism was rather immune to mathematics. Even if at the Paris symposium, mathematics started the show with Guilbaud's presentation, no one really talked about it later (except for Merleau-Ponty), and no specific piece on the mathematical uses of structure was included in the proceedings. Whether biologist, linguist, economist, historian, psychologist, political scientist, or lawyer, each participant showed that he was quite comfortable in using structures without (explicitly or implicitly) referring to mathematics. It was moreover thought that the mathematician's unified definition hid the "splintered character" that the concept bore in the human sciences, and, in the end, might not be very useful.[89]

Paying lip service to, or totally ignoring, mathematics became a widespread attitude in (non-genetic) structuralism. When in the late sixties, a flurries of books and special journal issues dealt with the fashion that structuralism had become, introducing, explaining, or criticizing it for a wide range of readers, Bourbaki had seeped into intellectual folklore because of his high profile in the mathematical community and his alleged role in educational reforms. He had become a synonym for rigor, axiomatics, and set theory. Many authors, however, agreed that mathematics was *not really* a part of the structuralist vogue.[90] Which, of course, cannot be very surprising considering that

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[89] R. Pagès in "Compte rendu du colloque sur le mot structure," *Sens et usages,* 156.
[90] No mention of mathematics, nor Bourbaki in, for example, the special issues of the journals *L'Arc,* no. 26, devoted to "Lévi-Strauss" (March 1965), *L'Esprit,* 31, no. 322, devoted to "*La Pensée sauvage* et le structuralisme" (November 1963), 545-653; and *L'Esprit,* 35, no. 360, devoted to "Structuralismes, idéologie et méthode" (May 1967), 769-901; and the books of J.-B. Fages, *Comprendre le structuralisme* (Toulouse: Privat, 1967); O. Ducrot, T. Todorov, D. Sperber, M. Safouan, and F. Wahl, *Qu'est-ce que le structuralisme?* (Paris: Seuil, 1968), except for Sperber's chapter on anthropology, 179-

Bourbaki never informed the works of any great structuralist thinker, other than for providing an illustration for the complexity of authorship![91]

c) Jean Piaget and Genetic Structuralism

Swiss psychologist Jean Piaget, contrary to most famous structuralists, was serious about mathematics in his "genetic structuralism," as Goldmann labeled it. At the same time, Piaget's structural vision of the sciences was extremely influential. In 1959, he greatly inspired the Cerisy conference and, a decade later, he published a short survey of structuralism, which was probably read by more people than any other. From the popular series "*Que sais-je?*", the book had more than 55,000 copies printed in 1968 alone, its first year of publication. There, Piaget strongly emphasized the centrality of mathematics. "A critical account of structuralism," he wrote, "must begin with a consideration of mathematical structures."[92]

Mathematics, as the key to a structural synthesis of the sciences, was one of the suggestions that, already in 1956, emerged during the *Synthèse* week. Although it failed to gather a consensus, this view was strongly defended. Interestingly the man who argued the most forcefully for this view was none other than François Le Lionnais, editor of the *Great Currents of Mathematical Thought:*

180, and 223-228. Laconic passages in J.-M. Auzias, *Clefs pour le structuralisme* (Paris: Seghers, 1967), 9; and M. Corvez, *Les Structuralistes, les linguistes... Michel Foucault, Claude Lévi-Strauss, Jacques Lacan, Louis Althusser, les critiques littéraires* (Paris: Aubier-Montaigne, 1969), 12-13.

[91] M. Foucault, "Qu'est-ce qu'un auteur?," *Bulletin de la société française de philosophie,* 63(3) (1969): 73-104; repr. in *Dits et écrits, 1954-1988,* ed. D. Defert and F. Ewald, vol. 1 (Paris: Gallimard, 1994): 789-821, 797.

[92] J. Piaget, *Structuralism,* transl. C. Maschler (New York: Basic, [1968] 1970), 17.

> when I turn to the outer world, I everywhere see Laws of composition, neighborhoods, orders, [and] equivalencies. Here are thus four structures that, without being immoderate, we can consider as fundamental and that operate at all instant, in the domain of human realities as well as in the world of Physics. I think that if we became better aware of this, we would achieve some progress.[93]

In 1959, the Cerisy conference, unlike the Paris symposium, did not start with mathematics. But Piaget brought it up forcefully on the second day. While it cannot be said that mathematics dominated the debates at Cerisy-la-Salle, it nevertheless constantly remained in the background. Bourbaki's were prime examples of what Piaget meant by structures, *i.e.* "systems which, as systems, presented laws of totality distinct from the proprieties of their elements."[94] Indeed, if not all structures were mathematized, he "very resolutely leaned" towards thinking of them as at least potentially mathematizable.[95]

Forty years of experimentation with children had lead Piaget to believe that, at each stage of the development of intelligence, thought processes came in highly structured ways. He used one of his famous experiments as an example.[96] A child is presented with two identical balls of clay, then one is rolled up into a sausage, and she is asked whether the ball or the sausage has more clay. Typically, the emergence in a child's mind of the principle of matter conservation, Piaget contended, will follow four stages. At first, the child is likely to say that the sausage contains more clay because it is longer than the ball. Then, she inverts her reasoning, focusing on thickness. She begins to doubt her deduction. Thirdly, the child considers both directions, but is confused. She discovers the

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[93] Synthèse, *Notion de structure,* 415.
[94] J. Piaget, "Genèse et structure en psychologie," in *Genèse et structure,* ed. M. de Gandillac et al.: 37-61, 37.
[95] *Ibid.,* 54. Later, Jean Ullmo offered an extreme version: "Generally a structure can *only* be precisely defined by a group [of transformations]." *La Pensée scientifique moderne* (Paris: Flammarion, 1969), 262. My emphasis.

solidarity between the transformations. Finally, the child realizes that they are inverse, and a structure crystallizes in her mind: the matter conservation principle. For Piaget, this example showed that mental structures emerged in a sequence which was also structured. Moreover it underscored the intimate relation between structures and their genesis, so distinctive of Piaget's structuralism.

Piaget believed that the acquisition of propositional logic was crucial to a child's intellectual maturation. The mental structures enabling teenagers to think logically were themselves modeled on mathematical structures, such as the group structure. He contended that between twelve and fifteen years old, children acquired a new structure "whose influence is very striking in every domain of formal intelligence."[97] It included four types of transformations that could be applied to logical propositions: the *identical (I), negative (N), reciprocal (R),* and *correlative (C)* transformations. He took for example the proposition '*p* implies *q*' (or equivalently 'not *p* or *q*'), whose converse was '*q* implies *p*', and whose negation was '*p* and not *q*'. Its correlative was defined as the permutation of *and*s and *or*s, or equivalently the converse of the negation, *i.e.* 'not *p* and *q*'. Since each of these transformations applied twice fell back on the identity, and that taken two by two, they were equivalent to the third one ($NR = C$, $CR = N$, and $NC = R$), they formed a group of 4 elements.[98] For Piaget, this group was inscribed in our minds, enabling us to perform the most basic logical operations. The mental structures of intelligence were none other than the mathematician's structures, or at least this was the desirable ideal.

96 J. Piaget, "Genèse et structure," 44-48.
97 J. Piaget, "Genèse et structure," 40.
98 J. Piaget, *Traité de logique. Essai de logistique opératoire* (Paris: Armand Colin, 1949), 268-286.

Among all mathematical structures, the most important were Bourbaki's three mother-structures (algebraic, topological, and order structures). They "correspond to elementary structures of intelligence."[99] Once again, it was a direct encounter with Bourbaki, this time in the person of Dieudonné, that led Piaget to this belief. In April 1952, they spoke at a conference outside Paris on "Mathematical Structures and Mental Structures," in relation with the International Commission for the Study and Improvement of Mathematical Education. Later, Piaget recalled the impression that this encounter had left on him:

> Dieudonné gave a talk in which he described the three mother-structures. Then I gave a talk in which I described the structures that I had found in children's thinking, and to the great astonishment of us both, we saw that there was a direct relationship between these three mathematical structures and the three structures of children's operational thinking. We were, of course, impressed with each other.[100]

By that time, Piaget had embarked on an ambitious project, which, bluntly put, aimed at making a science out of epistemology. In 1950, he published *Introduction à l'épistémologie génétique,* in which he argued that, since the roots of the spontaneous psychological development of arithmetic and geometric operations in children paralleled the concepts used by mathematicians (but not Bourbaki yet), then the "linear order" of science extolled by Vienna Circle positivists (and Auguste Comte before) was to be replaced by a "circle."[101] For him, the "logico-mathematical" sciences, on which the rest

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[99] J. Piaget, "Les structures mathématiques et les structures opératoires de l'intelligence," *L'Enseignement des mathématiques:* 11-33 (Neuchatel and Paris: Dulachaux & Niestlé, 1955), 17.
[100] J. Piaget, *Genetic Epistemology,* transl. Eleanor Duckworth (New York and London: Columbia University Press, 1970), 26.
[101] J. Piaget, *Introduction à l'épistémologie génétique,* 3 vols., esp. tome I, *La pensée mathématique* (Paris: Presses universitaires de France, 1950), 49-50. See also J. Gayon,

of science including the sciences of man was supposed to be built, in turn rested on the structures of the human mind. Or as Léo Apostel defended with more nuance: "there are laws of thought such that, in a certain social structure and for individuals possessing certain properties, we can infallibly constrain these individuals to accept our conclusions, if they accept our premises."[102] After he met Dieudonné and studied "The Architecture of Mathematics," Piaget realized that the structures he had been talking about could be equated with Bourbaki's mother-structures. Bourbaki's structuralism therefore significantly informed Piaget's own conception of structuralism.

Piaget's genetic structuralism appeared as a unified methodology, equally applicable to logic and mathematics, physics and biology, psychology, linguistics, and the social sciences. In some respect, it was a strange view of structuralism that excluded the likes of Lacan and Barthes, harshly criticized Foucault's "structuralism without structures," and praised Noam Chomsky's work as the epitome of linguistic structuralism. Furthermore, Piaget saw a "direct adaptation of general algebra" in Lévi-Strauss's structural models, and commended Bourbaki for "subordinat[ing] all mathematics . . . to the idea of structure."[103] By emphasizing the ontology of structures, he diverged from most structuralists. "What structuralism is really after is to discover 'natural structures'—some using this somewhat vague and often denigrated word to refer to an ultimate rootedness in human nature, others, on the contrary, to indicate a non-human absolute to

"Génétique, psychologie génétique et épistémologie génétique dans l'œuvre de Jean Piaget (1896-1980): une ambiguïté remarquable," *L'Ordre des caratères. Aspects de l'hérédité dans l'histoire des sciences de l'homme,* ed. C. Bénichou (Paris: Vrin, 1989): 147-173, 162.

[102] L. Apostel, *Logique et preuve,* vol. 5 (Methodos, 1953), 305; quoted by J. Piaget, "Les structures mathématiques," 30.

which we must accommodate ourselves instead of the reverse."[104] In Piaget's conception, these two alternatives were really just one, since he saw Bourbaki's atemporal structures as rooted in the human brain.

Still, if Piaget's book satisfied a thirst for a clear, straightforward explanation of structuralism, it hardly mirrored its diversity at a time when structuralism was quickly becoming a meaningless fashion from which its initial propagandists often seemed eager to distance themselves. A sure sign that structuralism was coming to the front stage of the French intellectual landscape came when *Les Temps modernes* devoted a special issue to "its problems." In his introduction, Jean Pouillon admitted: "structuralism is indeed in fashion. Fashions have the exasperating aspect that by criticizing them one gives in to them."[105] By the late sixties, when ironically it became a popular fad, the structuralist movement was loosing all coherence, supposing it once had some to start with. For Georges Canguilhem,

> "structuralism" means nothing. . . . It is a journalist's concept, but not the concept of a scientist [*savant*], who himself knew very well that he was dealing with structures, but which he defines in a given way in mathematics, biology, linguistics, etc.[106]

Piaget's book appears, in retrospect, as a desperate effort at presenting a unified structuralism with scientific pretense.

One last-resort attempt at salvaging structuralism indeed distinguished between the true scientific uses of structures and mere ideological ones. For this reason, Piaget

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[103] J. Piaget, *Structuralism,* 17 and 23.
[104] *Ibid.,* 30.
[105] J. Pouillon, "Présentation: un essai de défintion," in *Les Temps modernes,* 22, no. 246 (November 1966): 769-790, 769.

found others who concurred in seeing modern mathematics as a prime example of structuralism. The more an author held on to the belief that structural methods offered the best hope for truly *scientific* social and human sciences, the more she would see mathematical structures as an exemplar for human structures. In 1969, Jeanne Parain-Vial explicitly made this distinction between science and ideology.[107] In order to better criticize the ideologies she attributed to Lacan, Althusser, and Foucault, she presented a panorama of scientific uses of structures, the first of which was Bourbaki's. Following a familiar strategy, the emphasis lay on the clarity of the mathematical usage. She nonetheless pointedly questioned whether human structures really were the same as Bourbaki's.

d) The Oulipo: Bourbakist Literature?

"My idea of prose was greatly influenced by . . . Bourbaki's famous treatise."[108] Indeed, social scientists and mathematicians were not alone in toying with structures. There is perhaps no more telling sign of the hegemony of structuralist modes of thought in certain French intellectual milieus and of Bourbaki's role as a cultural connector than the story of the literary group that was called *Oulipo,* an important source of inspiration for writers like Georges Perec and Italo Calvino who belonged to it.

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106 Comment in J.-M. Auzias et al., *Structuralisme et marxisme* (Paris: Union générale d'éditions, 1970), 238.
107 J. Parain-Vial, *Analyses structurales et idéologies structuralistes* (Toulouse: Privat, 1969). A similar strategy of distinguishing scientific method and ideology is at play in a critical conference held in 1967-1968, cf. J.-M. Auzias et al., *Structuralisme et marxisme.*
108 J. Roubaud, *'Le grand incendie de Londres'* (Paris: Seuil, 1989), 148.

On November 24, 1960, a peculiar semi-secret literary society was founded, mainly inspired by the mathematician François Le Lionnais, once again, and the writer and amateur mathematician Raymond Queneau.[109] On their second meeting they adopted the name "*Ouvroir de littérature potentielle*" [Workshop for Potential Literature], abbreviated as Oulipo. Their somewhat surprising premise was that, as "mathematicians and scribblers [*écriverons*], we have the right to expect that our meetings will contribute to shedding light on the exercise of our respective activities."[110] They sought to experiment with formal constraints, imposed on the production of literature. In a 1962 interview on French radio, Queneau defined potential literature as such: "The word 'potential' concerns the very nature of literature; that is, it's less a question of literature strictly speaking than of supplying forms for the good use one can make of literature. We call potential literature the search for new forms and *structures*—to use this slightly learned word—that may be used by writers in any way they see fit."[111] Once again: structures! But whose, Bourbaki's or Lévi-Strauss's?

In his "second manifesto," François Le Lionnais opted for the former. He specified that Oulipism exhibited "a syntactic structurElist perspective [*sic*]," begging his readers not to confuse this word "with structurAlist, a term that many of us consider with

109 A proud member of the *Société mathématique de France,* Queneau also published two articles in scientific journals: "Théorie des nombres: sur les suites *s*-additives," *Comptes-rendus de l'Académie des Sciences A,* 266 (6 May 1968): 957-958; and "Sur les suites *s*-additives," *Journal of Combinatorial Theory,* 13 (1972): 31-71.
110 J. Bens, *OuLiPo, 1960-1963* (Paris: Christian Bourgois, 1980), 20. This book contains summaries of the first three years of Oulipian meetings.
111 G. Charbonnier, *Entretiens avec Raymond Queneau* (Paris: Gallimard, 1962), 140; quoted (and slightly modified) by J. Lescure, "A Brief History of the Oulipo," *Oulipo: A Primer of Potential Literature,* ed. and transl. W. F. Motte (Lincoln, University of Nebraska, 1986), 38. My emphasis.

circumspection."[112] Therefore, while the Oulipians sometimes invoked Lévi-Strauss's name, thought of meeting with Foucault, and seem to have been in contact with Lacan, their main inspiration was emphatically scientific and mathematical. "We live in the middle of the 20th century," declared Queneau. "*Everything* presents a rapport with science."[113] Like Piaget, Queneau conceived of the organization of science as a circle, and there was "nothing to stop Poetry from taking its place in the centre."[114] The role of mathematics was to provide the Oulipians with abstract structures that could be imported in literature. "Mathematics," Le Lionnais added, "particularly the abstract structures of contemporary mathematics, proposes thousands of possibilities for exploration, both algebraically, . . . and topologically."[115] But how were they supposed to use these structures in writing? This would remain a constant matter of discussion, as Queneau kept pushing the mathematicians to "give" them more abstract mathematical structures to play with.[116]

Their favorite exemplars of potential literature was Queneau's stunning *Hundred Thousand Billion Poems.*[117] On the face of it, this was just a collection of ten sonnets, each comprising 14 verses, as it should. But their structure was so carefully designed that each line of a poem could be replaced by its homologue from any of the nine others,

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[112] F. Le Lionnais, "Second Manifesto," *Oulipo: A Primer*, 29.
[113] J. Bens, *OuLiPo,* 49.
[114] R. Queneau, "Science and Literature," *Times Literary Supplement* (September 28, 1967), 863-864. Note that Jean Piaget edited a volume on "logic and human knowledge" for the *Encyclopédie de la Pléiade,* whose general editor was none other than Queneau. *Logique et connaissance humaine* (Paris: Gallimard, 1967).
[115] F. Le Lionnais, "Lipo: First Manifesto," *Oulipo: A Primer,* 27.
[116] J. Bens, *OuLiPo,* 238; see also pp. 148 and 180. See F. Le Lionnais, " Quelques structures et notions mathématiques," Oulipo, *La Littérature potentielle (Créations, re-créations, récréations)* (Paris: Gallimard, 1973), 295-298.

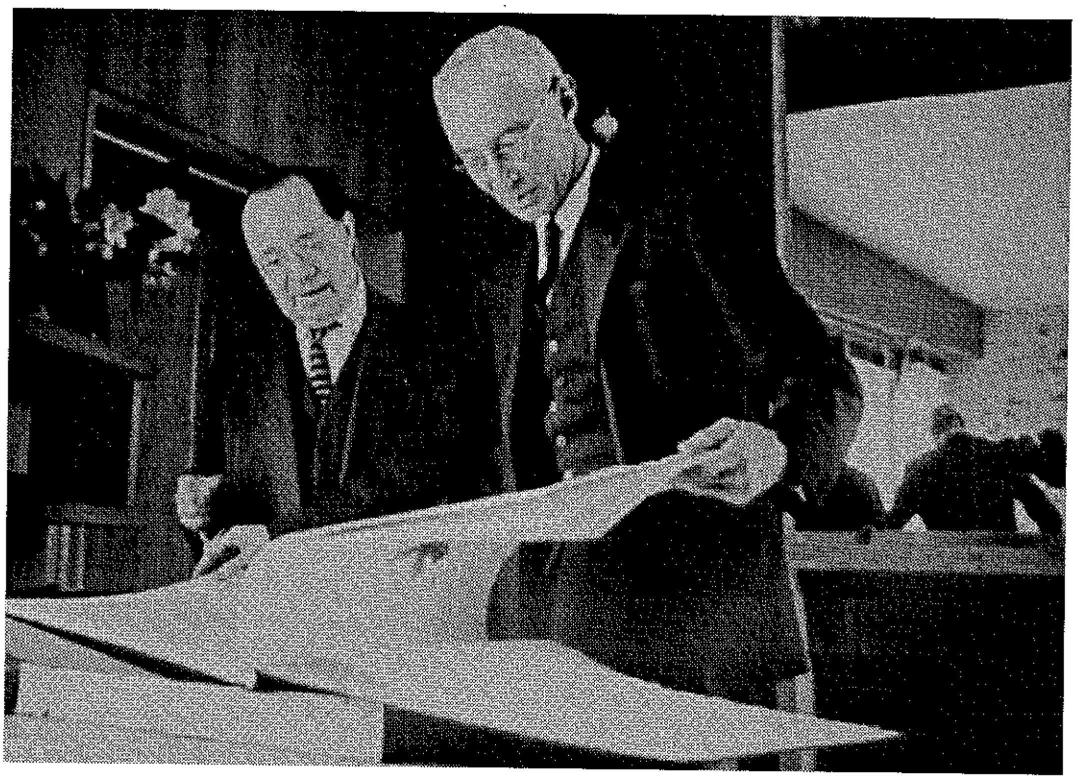

Figure 2: François Le Lionnais and Robert Oppenheimer at the IHÉS in 1963. Copyright © Arch. IHÉS.

while preserving rhythm, rhyme, and grammatical structure of the newly obtained poem.

Thus, the first four verses of the first poem:

Le roi de la pampa retourne sa chemise
Pour la mettre à sécher aux cornes des taureaux
Le cornédbîf en boîte empeste la remise
Et fermentent de même et les cuirs et les peaux

[The king of the pampas turns his shirt
To let it dry on the horns of the bulls
The canned corned beef makes the shed stink
And so are fermenting leathers and skins],

[117] R. Queneau, *Cent mille milliards de poèmes* (Paris: Gallimard, 1961).

could be turned into grammatically correct, rhyming nonsense, such as (replacing the verses above by the corresponding ones from, respectively, the sixth, first, second, and tenth sonnets):

> *Il se penche il voudrait attraper sa valise*
> *Pour la mettre à sécher aux cornes des taureaux*
> *Le Turc de ce temps-là pataugeait dans sa crise*
> *Et tout vient signifier la fin des haricots*
>
> [He bends down he would like to grab his luggage
> To let it dry on the horns of the bulls
> The Turk from that time became entangled in his crisis
> And everything comes to signify the end of beans].

The global result was a *potential* 10^{14} perfectly legitimate sonnets—much more than anyone, including the author, could hope to read in their entire lifetime! This accomplishment however is deceiving. The only mathematics that it might involve was combinatorics, disdained by Bourbaki for providing "problems without posterity."[118] More in line with Bourbaki's interests were the repeated, but rather unsuccessful, efforts made notably to exploit the notions of "intersection" of classic texts, "boundaries" of poems, etc.[119]

The Oulipo cataloged both new structures and old ones unearthed from the depth of literary history. Le Lionnais called these two activities: "synthoulipism" (synthesis + Oulipism) and "anoulipism" (analysis). While the former "examines and classifies ancient and modern texts [and] extracts from them their apparent or hidden structures and constraints," Noël Arnaud explained, the latter "invents entirely new structures . . . often

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[118] J. Dieudonné, *Panorama,* xii.
[119] J. Bens, *OuLiPo,* passim.

starting from new mathematics."[120] The Oulipians embraced history as a whole. When they discovered, Le Lionnais declared, "that a structure we believed to be entirely new had in fact already been discovered or invented in the past, . . . we make a point of honor to . . . qualify the text in question as 'plagiarism by anticipation'."[121] Did this attitude also characterize Bourbaki's oft-criticized teleological view of history?[122]

The only things that interested the Oulipo as a group were, not specific examples, but methods. In the reports of their first 40 meetings, the Oulipians never seem concerned with the message or politics of a piece of literature, and hardly ever with its esthetic quality. "The method in itself suffices. There are methods without examples. The example is an additional reward that one allows oneself," Le Lionnais mused.[123] "The very meaning of the Oulipo is to provide empty structures," Queneau concurred.[124] This is of course reminiscent of Bourbaki's distaste for application.

Nicolas Bourbaki always inspired the Oulipo, which included a few mathematicians (Claude Berge, Jacques Roubaud). Queneau once visited, in March 1962, a Bourbaki congress.[125] He helped popularize his work:

> The article that represents the intersection of these two interesting personalities . . . constitutes a subset of the issue no. 176 of *Critique* . . . according to which it is

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[120] J. Bens, *OuLiPo,* 9.
[121] F. Le Lionnais, "Second Manifesto," 31; see J. Bens, *OuLiPo,* 179.
[122] N. Bourbaki, *Elements of the History of Mathematics,* transl. J. Meldrum (Berlin and New York: Springer, [1960] 1994).
[123] J. Bens, *OuLiPo,* 81.
[124] G. Charbonnier, *Entretiens with Queneau,* 154-155.
[125] *La Tribu. Bulletin oecuménique, apériodique et bourbachique,* 56 (Congrès d'Amboise, mars 1962), 1. I thank Beaulieu for communicating me some issues of Bourbaki's internal newsletter. Interestingly, Queneau's *Exercices de style* was cited by Claude Chevalley in a rejected draft of the introduction of Bourbaki's *Theory of Sets.* I thank Catherine Chevalley for providing me a copy of this 1951 draft.

allowed to detect [*subodorer*] a few isomorphisms between Queneau and Bourbaki.[126]

Both indeed shared a common insistence on axiomatics, formal beauty vs. future utility, and especially humor, which seemed to delight Queneau. Both the Oulipo and Bourbaki were semi-secret societies founded on myths; both looked at the formal bases of their respective disciplines, and wished to rewrite its history from the current structural perspective; and both left to their members the task of producing original work based on structural approaches (new texts, new theorems). While Bourbaki always remained a significant source of structures for the Oulipo, Le Lionnais toyed with the idea of founding an *OuMathPo,* that would investigate the fecundity of their approach for mathematics. Even if some Bourbakis appreciated Oulipian prose, an *OuMathPo* seems never to have gotten off the ground.[127]

In conclusion, as he reigned over mathematics, Bourbaki became an omnipresent cultural connector across the French cultural landscape. While some writers seriously tried to adapt the Bourbakian tools to formal literature, this scarcely was the case in the human and social sciences. For some exegetes of structuralism, Bourbaki provided a compelling model, but one which social scientists could hardly live up to. Thus, almost none of them, with the notable exception of Jean Piaget, attempted to base, in a critical way, their structural method on his mathematics. Bourbaki served most usefully as a guarantor for rigor, a signal meaning that structuralism was real science. Typical of late

[126] F. Le Lionnais, in *Dossiers du Collège de 'Pataphysique,* nos. 18/19 (7 Clinamen 89 EP [vulg. 29 March 1962]), 68. R. Queneau, "Bourbaki et les mathématiques de demain," *Critique,* no. 176 (janvier 1962): 3-18.

modernist thought, structural discourses in mathematics, literature, and the social sciences were on the look for hidden essences. The meaning of the world was to be achieved either by abstracting structures from messy external appearances or by constructing them. Thus, even if the dialogue between these discourses rarely succeeded in connecting them in a meaningful way, their demise would be common. Once the attacks on self-reflexivity, metanarratives, and senseless abstraction were launched, they would ring true across disciplinary boundaries of discourses that had been used as resources for one another. But the attacks would come mostly from epistemic concerns specific to each discipline.

4. DECLINE

The decade of 1970 witnessed an effacement from prominence of both Bourbaki in mathematics and structuralism on the French intellectual scene. At the same time, Bourbaki consequently ceased to play an important role as a cultural connector, and new ones took his place. The history of this recent time however remains, for the most part, to be written. By following, in the work of Michel Serres and Bourbaki, the misfortune of the structure concept, and the subsequent rise of new mathematical ideas, such as catastrophes and fractals (which I select for their importance as cultural connectors, at the interface of mathematics, the social sciences, and philosophy), I want to suggest that, although internal dynamics or social factors could be mobilized to account for the demise of structural approaches in different disciplines, an understanding of the concordance

[127] F. Le Lionnais, "Queneau et les mathématiques," in *Raymond Queneau,* ed. A. Bergens (Paris: l'Herne, 1975), 279n; see also *Oulipo: A Primer,* 190n.3. M. Chouchan, *Bourbaki,* 9.

involves the recognition of the fact that a discussion established earlier went on with different cultural connectors with an impact that resonated widely.

a) Michel Serres: From Structuralism to Post-Bourbakism

To insist too stringently on modeling the definition of structure on Bourbaki's leads to an oddity: that "the only philosopher in France to abide by the structuralist method, defined in this way, would no doubt be Michel Serres."[128] A media figure and an idiosyncratic thinker, the philosopher and historian of science Serres nonetheless provides a useful guiding light for looking at the parallel unraveling of structuralism and Bourbakism. In a series of book, he described, mostly without footnotes, a personal evolution that should be seen here as an illustration, not a direct cause, of general intellectual shifts.

In 1961, Michel Serres, like Piaget, saw the idea of structure as stemming directly from Bourbaki's mathematics. By now, his definition should sound familiar:

> a structure is an operational set with an undefined meaning, . . . grouping any number of elements, whose content is not specified, and a finite number of relations whose nature is not specified.

Despotic, Serres insisted: "The term structure has this definition and no other." Moreover, he offered no ambiguity as to where this notion came from. This was exclusively a mathematical concept. In algebra, "it is devoid of mystery;" algebra was "the point where the content of the concept is the truest." Not that mathematicians had invented it, "only they were the first to endow it with *the precise, codified meaning* that *is*

[128] V. Descombes, *Le Même et l'autre. Quarante-cinq ans de philosophie française (1933-1978)* (Paris: Minuit, 1979); *Modern French Philosophy,* transl. L. Scott-Fox and J. M. Harding (Cambridge: Cambridge University Press, 1980), 85.

the novelty of contemporary [structuralist] methods."[129] Though he refrained from saying so explicitly, structures came, once again, from Bourbaki. Or rather, from Leibniz, read as a structuralist, and a Bourbakist, *avant la lettre.*

From this structuralism—one of the most strictly modeled on Bourbaki—Serres soon diverged radically. Not that he came to acknowledge that the notion of structure, when used in philosophy, needed a more supple form, rather he realized that "the structure . . . blew up." He espoused the radical belief that regions of order were created from a turbulent sea of chaos, and this stabilization of order, of knowledge, became his major object of study. Serres renounced structuralism on the ground that "reality is not rational."[130] In the process, his style of writing slowly evolved, mirroring his philosophical path. If his first texts were dense, tightly articulated pieces that presented his arguments rationally and structurally, his latter books were literary, almost poetic, works of philosophy that appealed more to senses and feelings than to logic. As a result, no other contemporary French philosopher, except perhaps Derrida, could be harder to summarize and paraphrase. I restrict myself, here, to a description of Serres's shifting

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129 M. Serres, *Hermès I. La communication* (Paris: Minuit, 1968), 32, and 28-29. His emphasis. This definition of structure is repeated word for word in M. Serres, *Le Système de Leibniz et ses modèles mathématiques,* I (Paris: Presses universitaires de France, 1968), 4. Serres's distinction between objects and relations between objects also stemmed from Gaston Bachelard's reading of quantum mechanics and relativity. See esp. his "Noumène et microphysique," (1931) in *Études* (Paris: Vrin, 1970); and *La Valeur inductive de la relativité* (Paris: Vrin, 1929), 98-99; both repr. in G. Bachelard *Épistémologie: textes choisis,* ed. D. Lecourt (Paris: PUF, 1971), 11 and 28.
130 M. Serres, *Hermès IV. La distribution* (Paris: Minuit, 1977), 110 and 10. See "Estime," in *Ibid,* 275-290.

relation with mathematics, and show how philosophy could, to use his own metaphor, discover "the Northwest passage" between the two cultures.[131]

Serres's early education in mathematics made a crucial impression on him and shaped his philosophy. He always claimed to belong to no school of thought. According to him, three or four professional "superhighways" could then be taken: Marxism, phenomenology, the human and social sciences, and epistemology (which was moribund). None of them appealed to him: he became a "self-taught man," a surprisingly common claim among *normaliens.* Still, he was attentive to contemporary intellectual currents, and especially Bourbakism, which had set forth one of the "scientific revolutions" he said he had the good luck to live through. In Bourbaki, Serres already saw a "structuralism—well defined in mathematics—which I sought to redefine in philosophy, long before it came into fashion in the humanities a good decade later."[132]

Aware of algebra and topology, Serres first worked, as a student, on the epistemology of Bourbaki's structures.[133] He then went back in time for his doctoral thesis, and studied their origins in the work of Leibniz. In this book, sitting on the border of history and philosophy, Leibniz was the core of a revolution revealing a complex, multicentered universe. While Serres's structural analysis seemed directly taken from Bourbaki, he acknowledged Bourbaki more as an historian than a mathematician.[134] Serres based his understanding of Leibniz on contemporary mathematics, without sinking

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[131] M. Serres, *Hermès V. Le Passage du Nord-Ouest* (Paris: Minuit, 1980).
[132] M. Serres, *Conversations,* 10. See also M. Serres, *Hermès II. L'interférence* (Paris: Minuit, 1972), 70-71.
[133] M. Serres, *Le Système de Leibniz,* 75n.
[134] N. Bourbaki, *Elements of History.* See M. Serres, *Le Système de Leibniz,* 86n for a complete collection of Bourbaki's references to Leibniz.

into naive teleology. Rather than seeing Leibniz as the precursor of modern mathematics, Serres used modern mathematical structures as a way to bring a new light to the work of Leibniz without divorcing it from its historical context. Thus could he exhibit a paradox: "Bourbaki's Leibniz is ultimately less of a Bourbakist than Leibniz himself."[135] Serres used metaphors from modern graph theory (lattices and networks) to articulate the "multilinearity" and the "plurality of orders" that characterized both the system of Leibniz in Serres's view, and his own interpretation of Leibniz's work.[136]

Such parallels between Serres's philosophy and his methodology are a constant feature of his work. At this early stage, however, the parallel was only partial. Serres relied on structural mathematics to argue for a multiplicity of possible orders. He stated that the compartmentalization of the sciences was artificial; they "form a continuous body like an ocean."[137] Local orders met and accommodated one another, but none was hierarchically superior. Auguste Comte's ladder was knocked down. Serres replaced it, not by a circle like Piaget and Queneau, but by a network, or better, several networks that intersected in several dimensions, without foundation nor center. Science was a multiplicity of orders.

But, rejecting the idea of a single order in science, Serres neared a radical questioning of his own structural method. How could he still maintain that structuralism was a unique approach to the philosophy of knowledge? Chaos loomed. At the interstices between regions of order, the networks had to be tied up with one another: some

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[135] M. Serres, *Le Système de Leibniz,* 86.
[136] M. Serres, *Le Système de Leibniz,* 16; for Serres's use of graph theory, see his *Hermès I,* 11-20.

confusion was always possible. When, like a thaw, his method broke down, Serres began to see pockets of order isolated in a sea of disorder. "Consequently, in science, there are only exceptions, rarities, and miracles. There are only islands of knowledge." Notice that this parsing of knowledge into islands had been already envisaged by René Thom in his famous article that first introduced the notion of catastrophes. Identifying determinism and structural stability as the very conditions for the building of scientific theories, he wrote that "in every natural process, one first tries to isolate those areas where the process is structurally stable, . . . islets of determinism separated by zones where the process is indeterminate or structurally unstable."[138]

Was, in Serres's view, the domain of science, including structuralism, limited to these few islands? Serres hesitated for while. Then he rebelled once again. Knowledge did not need to be restricted. The problem rested in the methods. "Structured" systems, like Bourbaki's, "of totalities without exterior, of perfect universal explanation or understanding, . . . are obsolete." The remedy was simple: "To come back to the things themselves, to mixed multiplicities, . . . not to restrain ourselves to linear sequences or . . . networks, but to treat them directly as large numbers, [or as] clouds."[139] And there was hope that new emerging sciences could help achieve this reversal of perspective.

Serres dropped the structures, dropped the networks, dropped Bourbaki. New metaphors took their place. More and more, Serres drew his inspiration from Ilya

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137 M. Serres, *Le Système de Leibniz,* 16n. Serres's *thèse complémentaire,* published as *Hermès II* (Paris: Minuit, 1972), articulated this vision.
138 R. Thom, "Une théorie dynamique de la morphogénèse," *Towards a Theoretical Biology, I: Prologomena,* ed. C. H. Waddington (Edinburgh: University of Edingburgh Press, 1968): 152-166; repr. *MMM,* 13-38. Quote on p. 16.
139 M. Serres, *Hermès IV,* 11, and 39-40.

Prigogine's irreversible time and selforganizing disorder, from Thom's catastrophes, from Mandelbrot's fractals, and from the images of fluid mechanics, turbulence and chaos theory.[140] In 1982, Serres's *Genesis* argued for the introduction of the concept of a "positive chaos" into philosophy. He now envisioned the world as a turbulent fractal, mixing foreseeable regions with chaotic regimes. Serres strongly rejected structures which he now saw as the largest of the world's ordered systems. Summarizing the path he had covered, he wrote:

> Once we [philosophers] had order to conceive, new orders to construct. Then we thought through structures, with the sciences, but outside of them. . . . We conceived order under its broadest and most powerful category: a structure. . . . Thus new orders have appeared in unexpected places[;] the social sciences, literature, the history of religions, even philosophy, have been able to participate in *the algebraic festival of structure.* With it and outside it. . . . [Then,] we found ourselves in the presence of multiplicity. . . . This pure multiple is the ground of order, but it is also, I think, its birth.[141]

Then, he concluded: "Science is not necessarily a matter of one [unity], or of order, the multiple and noise are not necessarily the province of the irrational. This can be the case, but it is not always so."[142] This marked the death of a Bourbakist's dream; this was also typical of a widespread rejection of abstract structures in French thought.

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140 See Serres, *Hermès V. Le passage du Nord-Ouest* (Paris: Minuit, 1977), 99. See M. Serres, *Hermès IV,* 79ff; *Hermès V,* 40-113; and especially, *La Naissance de la physique dans le texte de Lucrèce. Fleuves et turbulences* (Paris: Minuit, 1977) and *La Genèse* (Paris: Minuit, 1982); *Genesis,* transl. G. James and J. Nielson (Ann Arbor: University of Michigan, 1995). In this last work, M. L. Assad has seen a critical tropical shift in Serres's thought. See her "Michel Serres: In Search of a Tropography," *Chaos and Order: Complex Dynamics in Literature and Science,* ed. N. Katherine Hayles (Chicago: University of Chicago Press, 1991): 278-298.
141 M. Serres, *Genesis,* 106.
142 M. Serres, *Genesis,* 131.

b) The Trouble with Bourbaki's Structures

Ironically, the structures of Bourbaki, which, as we have seen, once became a paragon of scientific rigor among mathematicians, social scientists, philosophers, and writers, had actually turned out to be quite disappointing in this respect. Despite the emphasis he put on them, Bourbaki never formally defined structures in the "Architecture." Of course, he was aware of this, as he noted that the definition he provided was "not sufficiently general for the needs of mathematics."[143] Understandable in an article written for a general audience, this omission was a glaring shortcoming for the entire edifice.

Bourbaki always intended to endow *structure* with a satisfactory formal meaning. Since he saw set theory, and structures especially, as the basis upon which mathematics should be built, the first booklet he published was his *Fascicule de résultats* on set theory.[144] But, as he was the first to admit, this summary only presented definitions and propositions from a "naive" point of view, in direct opposition with the "formalist" approach that he promised to follow in Book I. The first chapters of *Theory of Sets,* however, did not appear until 1954—fifteen years after the first *fascicule.* The chapter dealing with structures, which was announced in the leaflet spelling out the "directions for the use of this treatise" accompanying each published booklet, only appeared in 1957. It was the 22nd in the series![145]

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143 N. Bourbaki, "The Architecture," 29n7.
144 N. Bourbaki, *Éléments de mathématique,* Première partie: Structures fondamentales de l'analyse, Livre I: Théorie des ensembles – *Fascicule de résultats* (Paris: Hermann, 1939); *Theory of Sets* (Paris: Hermann and Reading: Addison-Wesley, 1968).
145 N. Bourbaki, *Ibid.,* Chapter 1: "Description des mathématiques formelles," and 2: "Théorie des ensembles" (Paris: Hermann, 1954); *Ibid.,* Chapter 3: "Ensembles ordonnés,

Ironically, although the exact circumstance of the writing of this book have still to emerge, it seems that, following this, "the old idea of 'fundamental structures' . . . disappeared from Bourbaki's vocabulary, . . . with however such a discreteness that few people seem to have noticed."[146] Indeed, during the 1960s, the emphasis on structures vanished from the new version of the "directions." With the publication of the Chapter on structure (1957), Bourbaki's enterprise was realigned. On the one hand, he was for the first time explicitly warning: "The treatise aims in no way at constituting an encyclopedia of present mathematical knowledge."[147] On the other, he began dealing with topics outside of Part I, issuing Books without serial number (Lie groups and algebras, commutative algebras).

Almost immediately, it was noticed that Book I differed markedly from the rest of the treatise. "The work of Bourbaki remains coherent after omission of this book," Michel Zisman wrote in 1956. "In some measure, it may even be considered as forming a whole distinct from the following books. Moreover, Book I is contested by some Bourbakists who are among the first who fail to understand what it brings to mathematics."[148] There were three unsatisfactory aspects about *Theory of Sets*. First, in his introduction, Bourbaki refined his vision of his axiomatic method: "the art of writing texts whose formalization is

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cardinaux, nombres entiers" (Paris: Hermann, 1956); *Ibid.,* Chapter 4: "Structures" (Paris: Hermann, 1957).

146 B. Malgrange, "À propos d'un article de J. Dieudonné," *Gazette des mathématiciens,* 3 (1975): 35-37, 37.

147 N. Bourbaki, *Théorie des ensembles,* Chapter 4: "Structures," iii.

148 M. Zisman, "Mathématiques et axiomatique, qu'apporte de nouveau Bourbaki?," *La Pensée. Revue du rationalisme moderne,* n.s., 65 (1956): 46-54, 49n.

easy to be conceived of."[149] Thus, *Theory of Sets* was *not* a formal text; and Bourbaki often resorted to natural language, as he did throughout the treatise. Second, as Zisman had noted, the other books of the treatise, as a consequence, hardly seemed to require this purported foundation at all. Finally, other foundational approaches developed since the publication of the *fascicule* in 1939 appeared difficult to integrate into Bourbaki's scheme, and perhaps even superseded his structural approach. In particular, the theory of categories elaborated by Saunders MacLane and Samuel Eilenberg after 1942 provided a suitable framework for describing general properties of objects studied by mathematicians—a framework that, following unsuccessful attempts, Bourbaki decided not to include within his own.[150]

Although this formal shortcoming in Bourbaki's work was noticed early, it had little impact. In *Les Temps modernes*'s critique of structuralism, Pouillon noted that "even in Bourbaki, . . . the definition [of structure] remains largely implicit."[151] But the criticism they addressed to structuralists in the human sciences was—discerningly—not about the problems with Bourbaki's structures. For Marc Barbut, who treated mathematical structuralism in the special issue, they were not really problematic. Mathematical structures were just so much poorer than the ones used in the human sciences that they were, more often than not, totally useless. In *Structuralism,* Jean Piaget saw category theory as the future direction of structuralism in mathematics, where the emphasis was shifting from objects to actions exerted on them. Needless to say, these developments did

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[149] N. Bourbaki, *Théorie des ensembles,* Chapter 1: "Description des mathématiques formelles," 2.
[150] M. Chouchan, *Bourbaki,* 33.

not undermine his vision, but pointed to new syntheses to come.[152] Even the Oulipian logician Jacques Roubaud was appalled by *Theory of Sets.* He said he once participated to a committee in charge of conceiving a chapter on category theory for Bourbaki, which never materialized. "In any case, it would have been bad," says Roubaud. "Fortunately, May 68 came up and everybody became occupied with other things."[153] In this context, how are we to understand the widespread appeal of Bourbaki's definition of structures?

Recently, the historian Leo Corry has carefully examined the shortcomings of Bourbaki's structures, and provided a convincing scheme to account for his important impact. He clearly noted that "*Theory of Sets* was meant to provide a formally rigorous basis for the whole of the treatise. . . . The result, however, was different: *Theory of Sets* appears as an ad-hoc piece of mathematics imposed upon Bourbaki by his own declared positions about mathematics, rather than a rich and fruitful source of ideas and mathematical tools."[154] Corry's distinction between "body of knowledge" and "image of mathematics" goes some way in explaining why Bourbaki was so powerful a symbol for practitioners of different disciplines.[155] The above has shown how the "image" projected by Bourbaki's mathematics was appropriated and misappropriated by various groups.

Ultimately, I contend, what caused Bourbaki's image to recede in the 1970s was not the debate about whether structures were a sound basis for mathematics. By then, it

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151 J. Pouillon, "Présentation," 769; see M. Barbut, "Sur le sens du mot structure en mathématiques," *Les Temps modernes,* 22(246) (1966): 791-814, 799.
152 J. Piaget, *Structuralism,* 27-28 and 143.
153 Quoted in M. Chouchan, *Bourbaki,* 124.
154 L. Corry, "Nicolas Bourbaki and the Concept of Mathematical Structure," *Synthese,* 92 (1992): 315-348, 320-321; *Modern Algebra and the Rise of Mathematical Structure* (Basel: Birkhäuser, 1996).

had become irrelevant to many people, whether they were in favor of his program or not. Typically, even mathematicians who opposed it considered the issue rather marginal. If Bourbaki's laborious effort to found mathematics on this notion had proved something, it rather was that, with it, his irrelevance had finally become obvious. By being bogged down with foundational problems, he had abandoned the real world, a critique that recalls Serres's. Some French mathematicians would soon endeavor to retrieve the concrete world, and if rigor blocked their way to progress, then they would do away with it!

c) 'Nice Visible Novelties' in Mathematical Research

In November 1968, at the first *séminaire Bourbaki* following the events of May, the Oulipian Bourbakist Jacques Roubaud distributed a witty leaflet parodying Bourbaki's humor. It announced the death of the great mathematician.[156] While this might have been premature, it signaled a new period in French mathematics. In fact, Bourbaki did not die, nor was he overthrown by a revolution. He was just too successful in making "commonplaces truly common," as Claude Chevalley wrote in 1951 in a rejected draft for *Theory of Sets.* He had fulfilled his ambition, and become less relevant.

Christian Houzel's *Prospective Report in Mathematics* offers a vivid contrast with Dieudonné's *Panorama of Pure Mathematics* published less than ten years earlier, but already at odds with current research.[157] Chapter headings included 'Mechanics and meteorology', 'Applications to biology' and to 'the sciences of man, the sciences of society

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[155] L. Corry, "Linearity and Reflexivity in the Growth of Mathematical Knowledge," *Science in Context,* 3 (1989): 409-440.
[156] Repr. in M. Chouchan, *Bourbaki,* 97.

and linguistics', etc. Where Dieudonné reserved one laconic paragraph at the end of each chapter to applications, Houzel integrated them in the body of mathematics. "This opening effort of mathematics to other sectors now seems to me essential for the survival of our science in France."[158] What had changed? The simple answer is: so much that it is impossible to know where to begin. Many factors must be mobilized to account for this change in the general outlook of the discipline. Internal developments of mathematics and science, availability of computers, renewed contacts with the Soviet Union, social demands for applications of mathematics, the popular image of science, the job market, all contributed. At least two symptoms showing that the social place of mathematics was shifting can be gathered from the *Gazette des mathématiciens,* the professional journal of the *Société mathématique de France.* First, while Lelong complained that not enough Ph.D.'s were conferred in 1961, an awareness that too few positions were available emerged in the mid-1970s.[159] Second, a commission was created in 1979 for "the defense and illustration of mathematics" at the SMF, "in the rather vague goal of popularizing mathematics and defending it against threats we began to feel."[160]

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[157] C. Houzel, ed., *Rapport de prospective en mathématiques* (Paris: Éditions du CNRS, 1985); J. Dieudonné, *Panaorama.*

[158] C. Houzel, *Rapport de prospective,* viii.

[159] See, e.g., P. Lelong, "Questions d'actualité et de prospective." *Gazette des mathématiciens,* 2(3) (1963): 1-3; B. Malgrange, "Quelques mots sur les dernières sessions du Comité consultatif," *Gazette des mathématiciens,* 3 (1975): 32-35; J.-P. Aubin and B. Cornet, "Rapport sur la situation de la recherche mathématique en France," *Gazette des mathématiciens,* 7 (1976): 18-73; M. Berger, "Note sur le flux de survie de la recherche mathématique en France," *Gazette des mathématiciens,* 13 (1980): 5-13.

[160] J. Ferrand, "Rapport d'activité," *Gazette des mathématiciens,* 14 (1980): 29. See also D. Nordon, "Sommes-nous tous pareils?" *Gazette des mathématiciens,* 10 (1978): 65-89; *Les Mathématiques pures n'xistent pas!;* and J.-M. Lévy-Leblond and A. Jaubert, eds., *(Auto)critique de la science* (Paris: Seuil, 1975).

In particular, the view of mathematics as opening to other sciences had been helped by the emergence of two avenues of research in the late 1960s and early 1970s: René Thom's catastrophe theory and Benoît Mandelbrot's fractal geometry. Catastrophe theory was an attempt at modeling discontinuous changes resulting from smooth variations of internal variables. Fractals were a geometrical description of extremely broken sets, like coastlines. Both, it was claimed, could describe natural phenomena when classical differential calculus—with which Bourbaki had started his enterprise and whose foundations he sought to secure once and for all—became useless. Both Thom and Mandelbrot strongly emphasized the need for a new mathematics tackling mundane reality.

> Many phenomena of common experience, in themselves trivial (often to the point that they escape attention altogether!) – for example, the cracks in an old wall, the shape of a cloud, the path of a falling leaf, or the froth on a pint of beer – are very difficult to formalize, but is it not possible that a mathematical theory launched for such homely phenomena might, in the end, be more profitable for science?[161]

Mandelbrot echoed this call. "Clouds are not spheres, mountains are not cones, coastlines are not circles, and more generally, man's oldest questions concerning the shape of this world were left unanswered by Euclid and his successors," among whom he surely counted Bourbaki.[162] Although overtly opposed to him, Thom and Mandelbrot knew Bourbaki very well. Mandelbrot's uncle was among Bourbaki's founders; Thom was one of the first "guinea pigs," or potential members, "carefully selected by Cartan for their

[161] R. Thom, *Structutal Stability and Morphogenesiss: Outline of a General Theory of Models,* transl. D. H. Fowler (Reading: Benjamin, [1972] 1975), 9. Hereafter *SSM.* For more on catastrophe theory, see Chapters III and VI.
[162] B. Mandelbrot, "Towards a Second Stage of Indeterminism in Science," *Interdisciplinary Science Review,* 12 (1987): 117-127, 117.

notable susceptibility to the *bourbachique* virus."[163] But they chose to develop their mathematics in a different direction.

Distrustful of the assumption that they only had to harvest the mathematical tree planted by Bourbaki, structuralists often expressed their desire to enroll mathematicians in their pursuit of a "science of man." Lévi-Strauss, in particular, predicted that mathematics itself would benefit from dealing with social science issues. "One-way collaboration," he wrote in 1954, "is not enough. On the one hand, mathematics will help the advance of the social sciences but, on the other, the special requirements of those sciences will open new possibilities for mathematics."[164] This call was widely repeated but rarely heard by mathematicians. Their stubborn deafness was informed by Bourbaki's hegemony over their field. As early as 1956, it was recognized that "Bourbaki's inclination for the absolute can have unhappy consequences. This bent sometimes leads Bourbakists . . . to exclude all other aspects of mathematics."[165] This was an "intellectual strategy," Mandelbrot wrote, but also an issue of "raw political power."[166]

Not by the least of ironies, it was Thom and Mandelbrot who answered Lévi-Strauss's call, and, in order to oppose Bourbaki, explicitly drew resources from structural linguistics. The sources of fractal geometry can indeed be traced back to Mandelbrot's

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[163] *La Tribu,* no. 8 (15 July 1945), 2. Thom attended Bourbaki's congresses at least twice: as a guinea pig in 1945, and as a visitor in 1953. *La Tribu,* no. 30 (March 1953).
[164] C. Lévi-Strauss, "The Mathematics of Man." *International Social Science Bulletin,* 6(4) (1954): 581-590, 590.
[165] M. Zisman, "Mathématiques et axiomatique," 53.
[166] B. Mandelbrot, "Chaos, Bourbaki, and Poincaré," *Mathematical Intelligencer,* 11(3) (1989): 10-12, 12. See his recollections in "Benoît Mandelbrot interviewed by Anthony Barcellos," in *Mathematical People: Profiles and Interviews,* ed. D. J. Albers and G. L. Alexanderson: 207-225 (Boston: Birkhäuser, 1985); and B. Mandelbrot, "Comment j'ai découvert les fractales." *La Recherche,* 17 (1986): 420-424.

collaboration with Piaget's group, in the late fifties, on a project to use information theory in linguistics.[167] Already in 1955, he explored the signification of, and innovations brought about by, cybernetics, game theory, and information theory, which he all placed under the heading of "structural theories."[168] As for Thom, he asked in 1972 whether "structuralist developments in anthropological sciences (such as linguistics, ethnology, and so on) [could] have a bearing on the methodology of biology? I believe this is so," he answered.[169] And much of his interpretation of the kind of knowledge produced by catastrophe theory depended on this answer. As I show later in Chapter III, Thom's morphogenesis could best be characterized as a *dynamics of structures*.[170] Although their understanding of structuralism remained rather superficial, both Mandelbrot and Thom sought to translate, and indeed surpass, structural methods in mathematics.

It might not have been impossible to reframe both catastrophe theory and fractal geometry as pure mathematics, but in their inspiration, in the way they were brought to

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[167] B. Mandelbrot, "Sur la définition abstraite de quelques degrés d'équilibre," *Études d'épistémologie génétique,* 2 (1956): 1-26; "Linguistique statistique macroscopique," *Études d'épistémologie génétique,* 3 (1957): 1-78; "Quelques problèmes de la théorie de l'observation dans le contexte des théories modernes de l'induction des statisticiens," *Études d'épistémologie génétique,* 5 (1958): 29-47.

[168] B. Mandelbrot, "L'Ingénieur en tant que stratège: théories du comportement. Une dèfinition de la cybernétique; applications linguistiques," *Revue générale des sciences pures et appliquées et Bulletin de l'Association française pour l'avancement de la science,* 62 (1955): 278-294. In 1950, the "officer cadet [*aspirant*]" Benoît Mandelbrot prepared a report on a conference on cybernetics organized by the Royal Society in London for the French Air Force. SHAA Carton 1579.

[169] R. Thom, "Structuralism and Biology," in *Towards a Theoretical Biology,* 4, ed. C. H. Waddington (Edinburgh: University of Edinburgh Press, 1972): 68- 82, 68.

[170] See also D. Aubin, "The Catastrophe Theory of René Thom: Topology, Morphology, and Structuralism," *Growing Explanations: Historical Perspective on the Sciences of Complexity,* ed. M. Norton Wise (in preparation); on catastrophe theory, see A. Woodcock and M. Davis, *Catastrophe Theory* (New York: E. P. Dutton, 1978), and Chapter III below.

bear with the world and appealed to intuition, they were deeply involved with other fields of research. Besides structuralism, Thom drew his inspiration mainly from embryology and Mandelbrot from economics and fluid mechanics. Mandelbrot crucially depended on the computer to conduct his research and let others share his powerful intuition with the help of striking graphics. As mathematicians, Thom and Mandelbrot claimed to revive forgotten traditions, especially Poincaré's qualitative work. To emphasize his own originality, Mandelbrot, in an obvious caricature, called Poincaré Bourbaki's "devil incarnate."[171] Like Poincaré, both Thom and Mandelbrot based their thinking on topology, rather than algebra; both showed disdain for mathematical rigor when it lagged behind intuition.

Their impact would subtly make itself felt in many ways. Among the descendants of fractals and catastrophes was chaos theory.[172] In 1969, the physicist David Ruelle, one of Thom's colleagues, wrote, in a book on statistical mechanics, that a Bourbakist treatment of many fields of physics was a "rewarding experience."[173] At that time, in contact with Thom's theory, he had already started thinking about turbulence. In 1971, he and Floris Takens published an article that introduced the notion of "strange attractors," and, in many ways, initiated chaos theory, a scientific theory that explicitly emphasized

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[171] B. Mandelbrot, "Chaos, Bourbaki," 11.

[172] The historical path from catastrophe to chaos has to be appreciated with nuance. Although not theoretically speaking a direct ancester of chaos, catastrophe theory was nonetheless crucial in attracting attention on the promises offered by topological approaches to the study of nature. This is one of the main topic of this dissertation. Fractals were later found quite useful to study and describe strange attractors.

[173] D. Ruelle, *Statistical Physics: Rigorous Results* (New York: W. A. Benjamin, 1969), viii.

limits to prediction.[174] The fruitful intercourse between mathematics and physics, between mathematics and the world had been resumed. Mandelbrot acknowledged the "perfect timing" of his books:

> They came out when the feeling was beginning to spread that the Bourbaki *Foundations* treatise, like a Romantic prince's dream castle, was never to be completed. . . . The Constitution phrase to insure that the group would remain eternally a cohesive young rebel was—of course—not working. In a way, the whole enterprise had become boring.[175]
>
> Brief, Bourbaki was getting old.

d) Catastrophes and Fractals as Cultural Connectors

I illustrated post-Bourbakist mathematics with catastrophe theory and fractal geometry, not because they were alone, as we have seen above with probability theory and applied mathematics, but because they acted most visibly as cultural connectors in 1970s France.[176] Admittedly, from the mathematicians' point of view, catastrophes and fractals may often have been perceived more as media fad than research avenues, especially because of Thom and Mandelbrot's remoteness from French students. This view was

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[174] D. Ruelle and F. Takens, "On the Nature of Turbulence," *Communications in Mathematical Physics,* 20 (1971): 167-192. and their "Note" in *Ibid.,* 23 (1971): 343-344; repr. Hao B.-L., ed., *Chaos II* (Singapore: World Scientific, 1990) [hereafter *Chaos II*]: 120-147; and D. Ruelle, *Turbulence, Strange Attractors, and Chaos* (Singapore: World Scientific, 1992) [hereafter *TSAC*]: 57-84.

[175] *Mathematical People,* 221.

[176] Other important cultural connectors of the late 1970s and early 1980s were cybernetics and systems theory (J. de Rosnay, *Le Macroscope. Vers une vision globale* [Paris: Seuil, 1975]; E. Morin, *La Méthode I. La Nature de la Nature* [Paris: Seuil, 1977]), self-organization inspired by Ilya Prigogine (P. Dumouchel and J.-P. Dupuy, eds., *L'Auto-Organisation. De la physique au politique* [Paris: Seuil, 1983]), and, later, deterministic chaos.

moreover boosted by the special character of Thom and Mandelbrot's books.[177] Indeed, perhaps in order to get around Bourbaki's hegemony, they published manifestos that reached beyond mathematicians and scientists.[178] This would help their wide cultural diffusion.

A channel for communication between mathematics and other cultural spheres having been established through Bourbaki, people therefore could naturally mobilize, in a similar way, mathematics critical of Bourbaki to undermine structuralism. In no other work than Michel Serres's and Jean-François Lyotard's is it clearer how Thom's catastrophes and Mandelbrot's fractals could supplant Bourbaki's structures as cultural connectors.

> Like an iceberg inverting itself, mathematics globally veered to formalism at the beginning of the century. It forsook intuition. It forgot intuition. It even, sometimes, condemn intuition . . . . Physicists or philosophers, sociologists or biologists, we all were formalists. . . . [But] here is intuition again. Here is space again. . . . Catastrophes *à la* Thom or fractals *à la* Mandelbrot. For five to ten years, again, it's been a party.[179]

Similarly, in *The Postmodern Condition,* Jean-François Lyotard played Thom and Mandelbrot against Bourbaki as a way to fault modernist structuralism. For him, the legitimacy of Bourbaki's knowledge hinged on the acceptance of statements (the axioms)

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177 This however is rather difficult to evaluate and of little consequence for my argument. See B. Malgrange, "À propos," 36; CNRS, "Rapport de conjoncture 198," *Gazette des mathématiciens,* 20 (1982): 16-110, 93, for evaluations of the impact of catastrophe theory on mathematics.

178 B; Mandelbrot, *Fractals: Form, Chance, and Dimension* (San Francisco: Freeman, [1975] 1977); R; Thom, *Structural Stability; Mathematical Models of Morphogenesis,* transl. W. M. Brookes and D. Rand (Chichester: Ellis Horwood, [1974] 1983).

179 M. Serres, *Hermès V,* 99.

to which it was subordinated.[180] This act of faith was based on power. For Lyotard, Thom's and Mandelbrot's work crucially informed new paths taken by knowledge. They were symptomatic of a

> postmodern science [that]—by concerning itself with such things as undecidables, the limits of precise control, conflicts characterized by incomplete information, '*fracta,*' catastrophes, and pragmatic paradoxes—is theorizing its own evolution as discontinuous, catastrophic, nonrectifiable, and paradoxical.[181]

The connection was not accidental. The bylaws of the *Institut des hautes études Scientifiques,* where René Thom worked, dictated that it devote some of its activities to the "methodology of the sciences of man." Always moribund compared to the other sections of the IHÉS in mathematics and physics, this section never was closer from realization than during the 1970s. Thom finally was nominated as a professor for this section in 1980 after having gathered a group of philosophers and searchers in social sciences who worked on exploring the possibilities of applications of catastrophe theory (Scheurer, Pomian, Petitot-Cocorda, Boutot, among others). By then, Bourbaki was more or less evacuated from the discourse of the social sciences, and structuralism from mathematics. Cultural connectors remained, like catastrophes and fractals, but that would quickly withered. They had benefited from the connection established through Bourbaki. But they could not sustain it. Connections would have to follow other channels.

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[180] J.-F. Lyotard, *The Postmodern Condition: A Report on Knowledge,* transl. G; Bennington and B. Massumi (Minneapolis: University of Minnesota Press, [1979] 1984), 43.
[181] *Ibid.,* 60.

5. CONCLUSION

How to account for changes in the outlook of mathematics remains a troublesome question for historians. It is not enough to exhibit striking resonance's that may have existed between scientific and cultural movements. We must locate them in history and find mechanisms able to account for them. Several strategies can be deployed. We may argue for a causal link from one to the other, for a common source, or for an extensive dialogue between them. None of these, I have shown, can well account for the subtleties of the cultural dynamics of postwar France. A dialogue between structuralism and Bourbaki's mathematics indeed took place. But on the whole, it was forced on them, unsustained and, ultimately, rather superficial, even when taking into account Piaget's serious efforts. The above however suggests that even a failed discussion can have actual effects on the fields themselves, as well as on the outside, as the Oulipo experience demonstrates.

In order to describe this cultural dynamics, I have introduced a notion of cultural connection rooted in the actors' practice and leaving them a lot of autonomy. When they used Bourbaki as a cultural connector, they had much leeway in interpreting its meaning in their own field. Still, the connection they thus established helped strengthening the successes of their respective approach in each discipline. The connection emerged from the constant, self-reinforcing call to the cultural connector, rather than from common causes. But this act of connection was not without effect. It exposed their interpretations to similar counterarguments. Once the connection was established, it became easier to replace the connector by a new one that would serve to undermine previously received ideas in both fields.

In this view, the postmodernist turn represents a change in the cultural connectors deployed, but not in the way they were used. Much more radical challenges were posed to science in the years after Mai 1968 in France, and elsewhere. Both Serres and Lyotard, just to name a few, strongly argued in ethical and moral terms. It may have been an understandable—and perhaps wise—strategy for a generation of middle-aged Frenchmen, at home or in exile, sidestepped by the events of World War II, to isolate in the pursuit of pure knowledge and distance itself from forceful attempts at controlling nature and society. But for the following generation, after Mai 1968, it seemed that totalizing science and philosophy entailed a disposition for totalitarianism.[182] By acknowledging the limits of knowledge, and by grounding it in the contemporary world, they wished to construct ethical islands of truth that would speak to the mundane reality of existence. Whether mathematicians also sought to "wage a war on totality" remains to be seen.[183] Will a detailed study of the cases of Thom, Mandelbrot, Prigogine, and French chaologists prove

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[182] About the links between totalizing systems and totalitarianism, see C. Ruby, *Les Archipels de la différence. Foucault, Derrida, Deleuze, Lyotard* (Paris: Éditions du Félin, 1989).

[183] F. Lyotard, "Answering the question: What Is Postmodernism?" (transl. R. Durand) in *The Postmodern Condition,* 82.

that a kinder science could indeed be achieved? In view of the controversy that pitted Thom against Prigogine in the early 1980s, answers will hardly be univocal.[184]

[184] K. Pomian, ed., *La Querelle du déterminisme* (Paris: Gallimard, 1990).

CHAPTER III: CATASTROPHES

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

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

1. INTRODUCTION[3]

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

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[1] "Now, after the age of materials and stuff, after the age of energy, we have begun to live the age of form." P. Auger in *Proceedings of the First International Conference on Cybernetics* (Namur, 1956); quoted and translated in G. Bowker, "How to be Universal: Some Cybernetics Strategies, 1943-70," *Social Studies of Science,* 23 (1993): 107-127, 111 and 124.
[2] "Perhaps no one is more able than the mathematician to follow a question of pure form." G. D. Birkhoff, "Quelques éléments mathématiques de l'art," *Atti del Congresso internazionale dei matematici, Bologna, 3-10 settembre 1928 (VI),* 1 (Bologna: Nicola Zanichelli, 1928): 315-333; repr. *Collected Papers,* 3: 288-306, 306.
[3] A version of this chapter is to be published in *Growing Explanations,* ed. M. Norton Wise.

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

a) What Ever Happened to Catastrophe Theory?

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

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

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

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

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

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

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

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

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

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

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

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

b) Catastrophe Theory: A Theory of Modeling Practices

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

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

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

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

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

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

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

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

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

2. WHAT WAS CATASTROPHE THEORY?

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

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

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

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

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

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

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

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

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

3. SOCIOLOGICALLY SPEAKING, A MATHEMATICIAN

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

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

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

a) Mathematical Styles: Bourbaki Against Intuition

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

b) The Mathematical Background of Catastrophe Theory

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

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

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

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

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

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

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

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

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

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

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

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

(i) Mathematical Interlude II: Singularity Theory

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

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

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

[44] M. Morse, "The Calculus of Variation in the Large," *Collected Papers,* 423-438, 423; and M. Morse, *The Calculus of Variation in the Large,* AMS Colloquium Publications, 18 (New York: AMS, 1934). See also the famous textbook by J. Milnor, *Morse Theory,* Annals of Mathematics Studies, 51 (Princeton University Press, 1963).

[45] R. Thom, "Les singularités des applications différentiables." See B. Teissier, "Travaux de Thom sur les singularités," *Publications mathématiques de l'IHÉS,* 68 (1988): 19-25; A. Haefliger, "Un aperçu," *Ibid.,* 16.

[46] For example, for a usual function f: $\mathbf{R}\rightarrow\mathbf{R}$, singular points of f are points p where the derivative of f vanishes [$f'(p) = 0$]; they can be local minima, local maxima, or flat inflection points (such as x=0, for $f(x)=x^3$).

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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[56] T. Poston and I. Stewart, *Catastrophe Theory and its Applications* (London: Pitman, 1978), 2.

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

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

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

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

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

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

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

4. TOWARDS A THEORETICAL BIOLOGY ?

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

b) 'Wad' and the Synthesis of Biology

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

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

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

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

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

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

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

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

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

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

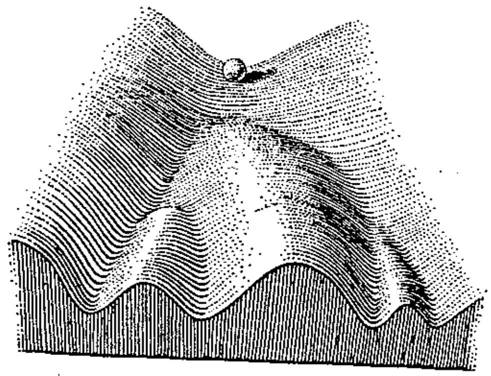


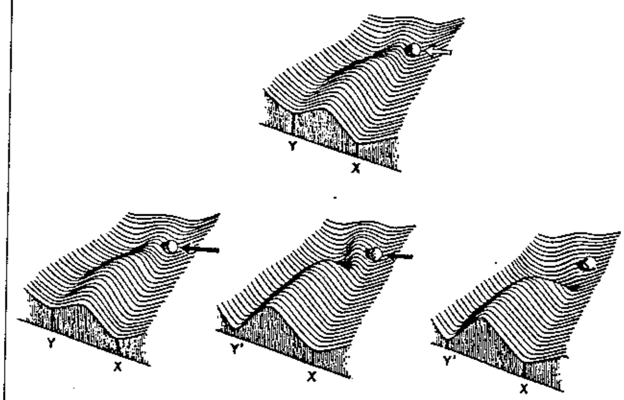
FIGURE 4
Part of an Epigenetic Landscape. The path followed by the ball, as it rolls down towards the spectator, corresponds to the developmental history of a particular part of the egg. There is first an alternative, towards the right or the left. Along the former path, a second alternative is offered;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

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

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

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

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



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

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

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

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

c) Dynamical Theories of Morphogenesis

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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95 J. Monod, *Chance and Necessity,* 89 and 95.

96 Note, however, that neither Thom nor Monod mentioned the work of the other in their writings. Indeed, Monod wished to counter vague approaches based on "general systems theory." *Chance and Necessity,* 80.

97 R. Thom, "D'un modèle de la science à une science des modèles," *Synthèse* (1975): 359-374.

98 See F. Gail, Françoise, "De la résistance des biologistes à la théorie des catastrophes," *Logos et théorie des catastrophes,* ed. J. Petitot (Geneva: Patiño, 1988): 269-279. One

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

5. TOPOLOGY AND MEANING

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

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

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

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

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

a) Man and Catastrophes

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

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

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

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

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

b) Language and Catastrophe

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

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

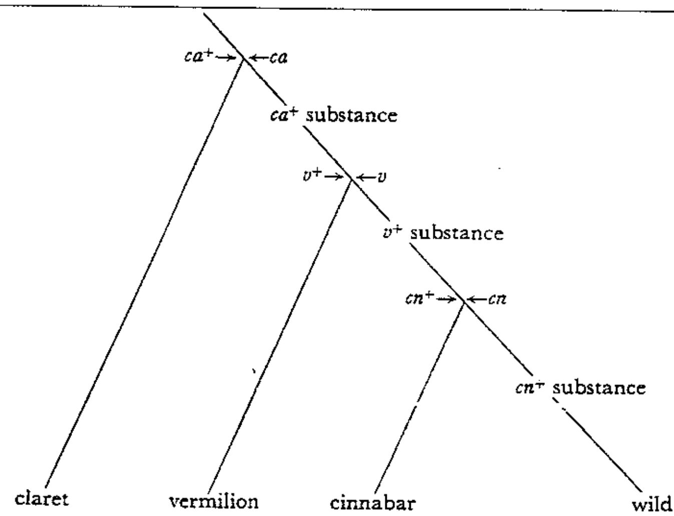

Fig. 7. The formation of eye colours in *Drosophila.* The pigment-forming process normally runs down the line through the ca^+ substance, the v^+ substance, and the cn^+ substance, to give wild type pigment. The genes, *ca*, *v* and *cn* interrupt this sequence, so that the process takes an altered course,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

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

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

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

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

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

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

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

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

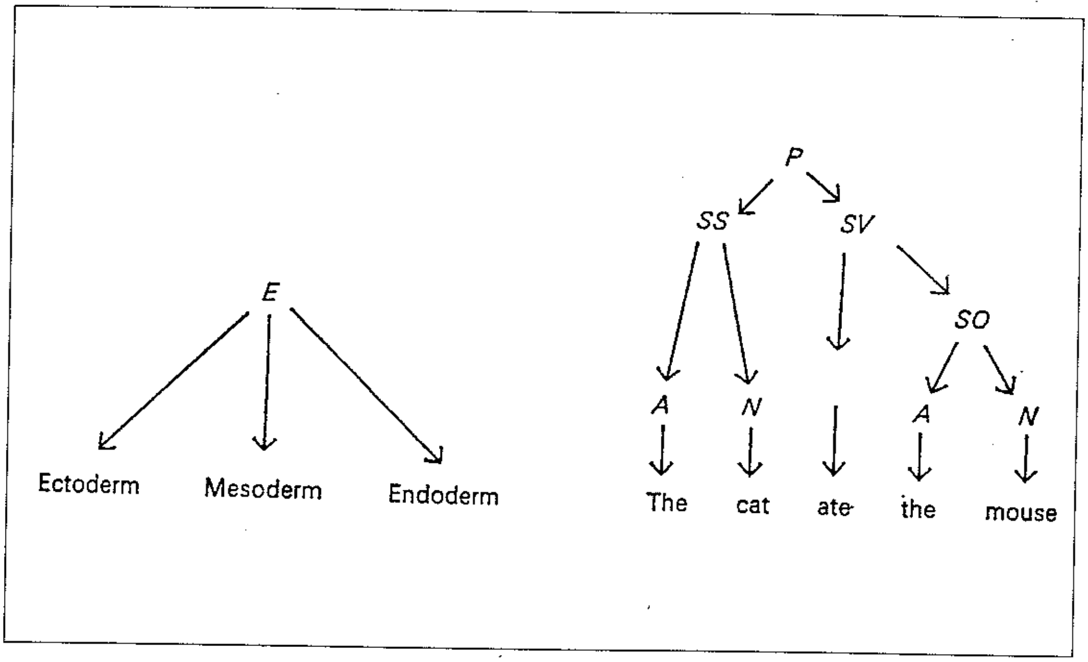

Figure 6: Thom's Analogy Between Graphs of Sentences and Development. Repr. with Permission from R. Thom, "Structuralism and Biology," *Towards a Theoretical Biology,* 4, ed. C. H. Waddington, 80. Copyright © University of Edinburgh Press.

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

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

c) Structuralism and Biology

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

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

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

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

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

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

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

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

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

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

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

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

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

(i) Nonreductionism

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

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

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

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[118] "Expliquer du visible compliqué par de l'invisible simple." J. Perrin, *Les Atomes* (Paris: Félix Alcan, 1913), Introduction.
[119] R. Thom, "Topologie et signification," *MMM,* 174.
[120] For the telelogy/mechanistic debate in 19th-century Germany, see T. Lenoir, *The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology* (Dordrecht: D. Reidel, 1982).

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

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

(ii) Forms

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

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

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[121] R. Thom, *SSM,* 159.
[122] R. Thom, *MMM* (1974 ed.), 23. Absent from later editions. More details in R. Thom, "Structuralism and Biology," *Towards a Theoretical Biology,* 4 (1972): 68-82; 73.
[123] R. Thom, *MMM* (1974 ed.), 9. Absent from later editions.
[124] Note that there is nothing absolute about the relation between the study of forms and nonreductionism. See the following historical study who focuses on the scientists' struggles to find molecular accounts of crystal shapes: N. E. Emerton, *The Scientific Reinterpretation of Form* (Ithaca: Cornell University Press, 1984).

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

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

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

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

(iii) The Mundane

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

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

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

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

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

(iv) The Logos

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

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

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

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

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

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

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

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

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

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

(v) The Qualitative

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

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

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

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

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

(vi) The Intelligibley

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

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

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

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

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

(vii) Hermeneutics

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

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

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

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

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

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

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

7. CONCLUSION

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

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

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

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

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

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

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

CHAPTER IV: FUNDAMENTAL RESEARCH

> Le milieu en quelque sorte naturel pour la recherche fondamentale est évidemment l'Université. De tous temps, et jusqu'au dernier quart du XIX° siècle, il en fut ainsi: l'ensemble de nos connaissances fondamentales n'a pas eu d'autre source.
> —Léon Motchane.[1]

1. INTRODUCTION: THE PUZZLE PLACE

In January 1973, mathematician Alexander Grothendieck, who had, three years earlier, angrily resigned from the Institut des hautes études scientifiques (IHÉS), applied for a professorship at the prestigious Collège de France. To support his candidacy, he wished the *Publications mathématiques de l'IHÉS* to print a "sketch" of his mathematical work together with a two-page biography. To this request, Jean Dieudonné, general editor of this journal, replied that while he was glad to accept Grothendieck's mathematical sketch, he saw

> no reason to publish a 'curriculum vitae'; . . . the scientific interest of such texts is null and void, and the biographical information they offer can only interest

[1] "The so-to-speak natural milieu for fundamental research obviously is the University. At all times, up until the last quarter of the 19th century, this has been so: all our fundamental knowledge has had no other source." L. Motchane, Rapport Euratom, 2. Arch. IHÉS. Is it necessary to underscore that recent historical studies of science has provided ample evidence that this statement was grossly mistaken? See, e.g., the special issue of *La Recherche,* devoted to the locales of science, ed. Dominique Pestre, 300 (1997).

> historians of science. In my opinion, [their] interest is for that matter entirely minor; if, as I believe, the history of science is first and foremost that of *ideas,* these biographical facts are mere anecdotes [*ne relèvent que de la 'petite histoire'*].[2]

Only ideas mattered for the history of science, he thought; the rest was accessory. Yet, how could Jean Dieudonné—a permanent professor of the IHÉS from its foundation in 1958 until 1964, and general editor of its *Publications mathématiques* until 1979—not have been aware of the extraordinary role that the Institute itself had played in the history of mathematics? Clearly, he was aware, for at the end of the same year he wrote to Léon Motchane, first director of the IHÉS:

> From the point of view of mathematical research, the record of the IHÉS since its foundation has more than justified the *idea* you had to create it. . . . I think future historians of mathematics will speak of the IHÉS in the years 1960-70 as one speaks of the great periods of Göttingen, in 1850-60, with Gauss, Dirichlet and Riemann, and in 1895-1910 with Hilbert, Klein and Minkowski.[3]

It is not my purpose here to dispute these comparisons, but I do want to question the reasons Dieudonné gave to explain the success of the IHÉS, or at least, to add nuances to these reasons. As the quote above shows, even the IHÉS was seen as an *idea* by Dieudonné, rather than a social institution endowed with its own culture. It is this very focus on the history of ideas that I shall question in the following. In this chapter, I describe the conditions in which this Institute was founded in 1958 and the way it functioned during the few years before René Thom and David Ruelle were hired by

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[2] Lettres de Jean Dieudonné à Nicolaas Kuiper (3/2/73); de Alexander Grothendieck aux professeurs du Collège de France, et à Jacques Tits et Jacques-Louis Lions (20/1/73); de Alexander Grothendieck à Nicolaas Kuiper (25/1/73). Arch. IHÉS. Original emphasis. Unless explicitly stated, all quotations are my translation from the orginal French. See Dieudonné's biography, Pierre Dugac, *Jean Dieudonné, mathématicien complet* (Paris: Jacques Gabay, 1995).

Motchane in 1963. By doing this, I mean to draw attention to the *social, cultural,* and *ideological* resources that the IHÉS would offer to its professors.

> It is true that you [Motchane] have had the chance to have as permanent members of the IHÉS three of the mathematicians of our time whose genius is the most powerful and most original, Grothendieck, Thom, and Deligne. Yet it was necessary to have know[n] how to attract them to IHÉS and *to give them favorable working conditions* so they could radiate their influence.[4]

According to Dieudonné, this capacity of attracting and retaining the right men, above anything else, accounted for the success of the Institute. The question I want to raise is whether it would not be more profitable and perhaps more accurate to consider the Institute as an active player in the history of catastrophe and chaos theories. Clearly, the IHÉS indeed provided conditions for the development of algebraic topology, catastrophe theory, dynamical systems theory, and deterministic chaos by attracting and retaining Alexander Grothendieck, René Thom, and David Ruelle. But, going further, we may explore its active role in bringing about the emergence of a set of original modeling practices at the heart of catastrophe and chaos theories. I am therefore concerned with the ways in which the IHÉS can be considered as a full actor of the history I am writing, just like Grothendieck, Thom, or Ruelle. My final aim is to interpret, as we historians would do for any other actor, its role in this history of chaos.

The task outlined above is too important to be done in a single chapter. In what follows, I first present a historical study of the Institut des hautes études scientifiques. Focusing on the IHÉS as an institution that shaped emerging modeling practices, this chapter examines the conditions for this institute to achieve and retain a certain stability,

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[3] Lettre de Jean Dieudonné à Léon Motchane (16/12/73). Arch. IHÉS. Translation done at the IHÉS. My emphasis.

thereby creating an atmosphere conducive to theoretical research. I focus on the ideology that animated its founders, allowing this peculiar institution, sponsored by industry and yet solely devoted to fundamental research in the most abstract sense, to exist. I also describe the efforts deployed in order to set up first-rank mathematics, physics, and humanities sections, and to promote communication among them. This gives a picture of the institution that hired René Thom and David Ruelle in 1962-63. This chapter forms the background for my discussion of the formation of Thom's research school, which I delay until Chapter VI below, and Ruelle's adaptation of the modeling practices promoted by its visiting topologists, which I discuss in Chapter VII.

2. A BRIEF HISTORY OF THE INSTITUT DES HAUTES ÉTUDES SCIENTIFIQUES (BURES-SUR-YVETTE)

On Friday, June 27, 1958, at 4 o'clock in the afternoon, a dozen men and women, most of whom were industrialists, met at the Sorbonne in the office of Joseph Pérès, Dean of the *Faculté des sciences de Paris.*[5] Boldly, they decided to go ahead and create the *Institut des hautes études scientifiques* (IHÉS), a non-profit association whose aim was "*to promote and sponsor theoretical scientific research* in the domains of pure mathematics, theoretical physics and the methodology of the sciences of man."[6] They explicitly stated

[4] *Ibid.* My emphasis.

[5] The persons present at the first meeting were Joseph Pérès, Léon Motchane, Gabrielle Reinach, Fernand Picard (Renault), Jacques Ballet (Esso Standard), Louis Devaux (Shell Française), Pierre Besse (Société des Pétroles B.P.), Pierre Braillard (Compagnie Générale de T.S.F.), Mr. Seitz (Tréfileries et Laminoirs du Hâvre), Mr. Fernique Nadau des Îlets (Gaz de France), Jean Wegbecher (representing Edmond de Rotschild), and a legal advisor Me Jean Robert. *Procès-verbal de la séance de fondation du vendredi 27 juin 1958,* 1. Arch. IHÉS.

[6] *Journal officiel de la République française,* 90, no. 165 (16 July, 1958): 6652. My emphasis. The original name proposed for the Institute was "*Institut de Recherches*

their hope to put together an institute that would form a European counterpart to the Institute for Advanced Study (IAS) at Princeton. They moreover expected that their undertaking, with the help of its director, Robert Oppenheimer, whom they named an honorary member for life, would be modeled closely on Princeton's institute.

This enterprise exhibited many peculiarities. In the land of planned capitalism, in a country where the State occupied so much of the economic space, monopolized higher education, and was the major sponsor of scientific research, a fiercely independent organization was born.[7] An entirely private initiative, "for the first time since the Pasteur Institute, ended up creating a center of international renown," devoted to "fundamental research."[8] At the same time, the industrialists who would sponsor the new institute agreed that no research subject be imposed on its scientists, that no planning at all be set

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Fondamentales" [cf. *Note,* portant la mention "strictement confidentiel," jointe à une lettre de Léon Motchane à Pierre Ailleret (7/5/58). Arch. IHÉS. Comp. to Chapter IV]. On June 4, 1958, Léon Motchane mentions the Institut des hautes études *théoriques* in a letter to Victor Weisskopf. Apparently, the definitive name was proposed by Paul Montel in June 1958 [*Hommage de André Grandpierre à Joseph Pérès* à l'Assemblée générale du 14/3/62. Arch. IHÉS]. It is mentioned for the first time on a *Note pour Francis Perrin,* dated June 10, 1958.

[7] On science policy in France around 1958, see Dominique Pestre and François Jacq, "Une recomposition de la recherche académique et industrielle en France dans l'après-guerre, 1945-1970. Nouvelles pratiques, formes d'organisation et conceptions politiques," *Sociologie du travail,* 3 (1996), 263-277; Robert Gilpin, *La Science et l'État en France* (Paris: Gallimard, 1970); and Jean-François Picard, *La République des savants. La recherche française et le CNRS* (Paris: Flammarion, 1990).

[8] *Lettre* de Léon Motchane à André Maréchal (22/11/61). Arch. IHÉS. I do not know for sure if such a statement is accurate. In any case, few academic institutions had been founded in France with private money during the last century. The precedents of the Institut Henri Poincaré, the Institut de Sciences Politiques, and the Sixth Section of the École Pratique des Hautes Études were sometimes invoked, but, all founded with the help of private foundations, they differed significantly in intent and in their realization from the IHÉS, which remained a rather unique institution in the French system.

Figure 7: Robert Oppenheimer and Léon Motchane at the IHÉS in 1963.
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for them. In brief, the sponsors were asked to write a blank check, and gladly handed it to Léon Motchane, who was named director of the IHÉS.

a) Léon Motchane and the Mobilization for Fundamental Research

"It is Motchane who took the initiative [in creating the IHÉS] and devoted himself to the quest for its means of existence."[9] It was he who wrote its bylaws "from A to Z."[10] In this light, the creation of the IHÉS naturally appears as the single-handed accomplishment of a resolute man with a vision. Léon Motchane was born in Saint-Petersburg in 1900 of Swiss parents. While in Russia, he studied mathematics and physics, but was interrupted

[9] Lettre de Paul Montel à Léon Motchane (n.d., mais reçue le 23/6/58). Arch. IHÉS.

by the 1917 Revolution. He soon left Russia, and went on with his physics studies in Lausanne, Switzerland. For a while, he then worked there as a physics *assistant*. But, in the mid-twenties, he had to find a better-paid position, and entered banking and industry, serving as consultant and administrator for various firms. He was naturalized as a French citizen in 1938, and at the start of World War II volunteered for the Army. After the 1940 defeat, he went on several missions of information for the Resistance.[11] Around 1948 that Motchane went to Paul Montel to present to him some of his mathematical ideas. Montel advised him to write up a short notice for the Academy of Sciences, and make it into a doctoral thesis. Only in 1958, after a more than thirty-year hiatus, did he obtain full-time employment in a scientific position again, as director of the Institut des hautes études scientifiques.[12]

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[10] Lettre de Léon Motchane à Victor Weisskopf (5/2/70). Arch. IHÉS.

[11] Resistance networks certainly played a role in the history of the postwar French University. "Cette préhistoire [de la VIe section de l'EPHE] donne aussi un coup de projecteur sur l'importance de la résistance universitaire souterraine – trop généralement minimisée aujourd'hui – qui apportait un prélude aux réformes d'après la Libération. [...] Cela met aussi en relief l'importance des relations personnelles, forgées dans cette période difficile." Pierre Daix, *Braudel,* (Paris: Flammarion, 1995), 249. I have very few indications that the same kind of network played any role for Motchane's enterprise, but this might be interesting to investigate.

[12] Various versions of Léon Motchane's *Curriculum vita; Mémoire de proposition pour Médaille Militaire et pour la Légion d'Honneur* (14/4/65); lettre de François Le Lionnais à Léon Motchane (12/4/65); de Paul Montel à Léon Motchane (23/4/63). Arch. IHÉS. See M. Berger, "Hommage à Léon Motchane," *Le Monde* (7 February 1990) [actually written by Louis Michel], and a more detailed manuscript in Arch. IHÉS. From 1934 to 1958, Léon Motchane published 11 notes on mathematics and theoretical physics in the *Comptes-rendus de l'Académie des sciences.* On December 17, 1954, he defended his thesis, in front of Montel, Arnaud Denjoy, Jean Favard, and Gustave Choquet; it was published as *Propriétés invariantes par convergence simple* (Paris: Gauthier-Villars, 1954). Under the pseudonym of Thimerais, he also clandestinely published, during World War II, two booklets of sociological reflections about the task ahead of rebuilding a socialist France: *La pensée patiente* (July 1943), and *Éléments de doctrine* (February 1944), both at the Éditions de Minuit.

As Dominique Pestre has shown in his history of Leprince-Ringuet's laboratory at the École polytechnique, there is a danger in telling a story that insists on the role of an institution, like the IHÉS, which is to succumb to the temptation of repeating the standard epic that belongs to the collective memory of mathematicians and physicists, as well as that of the Institute itself.[13] But, as Pestre has also insisted, neither is everything false in this memory. Although originally the idea of a single man, the IHÉS required special circumstances to come to life. It is only through a careful investigation of the social and cultural resources deployed by Motchane to promote his idea, and the constraints imposed on it, which may have affected its final shape, that we can fully comprehend the meaning of the foundation of such an institute in Paris in 1958. To go too far in this direction would however lead us away from the main topic of this chapter, which is to examine the way in which the Institute helped the emergence of a specific modeling practice. Here I examine only the social networks mobilized by Motchane in support of his project.

First, Léon Motchane convinced a part of the mathematical establishment that something had to be done in order to stop the "French hemorrhage to the USA," the "brain drain" in mathematics.[14] From France, Jean Dieudonné, Schützenberger, Benoît Mandelbrot, among others, had just left for the US in the previous decade. After spending some years in the US, Claude Chevalley had trouble getting appointed to Paris; albeit "one of the two or three greatest mathematician alive," André Weil failed to get

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[13] See Dominique Pestre, "Le renouveau de la recherche à l'École polytechnique et le laboratoire de Louis Leprince-Ringuet, 1936-1965," in *La Formation polytechnicienne, 1794-1994,* ed. Bruno Belhoste, Amy Dahan Dalmedico and Antoine Picon (Paris: Dunod, 1994): 333-356.

nominated to the Collège de France.[15] The feeling was that the French mathematical school, one of the world's best, was losing some of its most prominent representatives for lack of attractive positions to offer.[16] Something had to be done. Thus, Motchane was able to get his mentor Paul Montel (1876-1975) to head the Consultative Scientific Committee of the IHÉS, who would alone take all the major scientific decisions.[17] As dean of the Faculté des sciences de Paris until 1946 and president of the Academy of Sciences, Montel was a prestigious patron, but a rather old man, who did not participate much in the establishment of the IHÉS.[18]

For that purpose, Motchane enrolled Joseph Pérès (1890-1962), who presided over the Institute and chaired its Administrative Board until his death. Dean of the Faculté des

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[14] *Notes de séances* manuscrites, par Annie Rolland, de la séance de foundation de l'IHÉS (27/6/58). Arch. IHÉS. Cf. E. C. Zeeman, "How to Reverse the Brain Drain in Maths," *New Scientist* (4 May 1967): 263-264.

[15] About Chevalley's difficulties, see J. Dieudonné, "Claude Chevalley, 11 février 1909 - 28 juin 1984," *Annuaire des Anciens élèves de l'Ecole Normale Supéieure* (1986). The quote about Weil is from Jean-Pierre Serre, lettre à Léon Motchane (18/12/58). Arch. IHÉS. About Weil's failed candidacy at the Collège de France, where Jean Leray was preferred to him by a vote of 32 to 1, see Chapter VII below. *Assemblée générale des professeurs* (16/2/47). Arch. CdF. G-iv-l 28U.

[16] See, *e.g.*, Pierre Lelong, "Questions d'actualité et de prospective," *Gazette des mathématiciens,* 1st ser., 2, no. 3 (November 1963): 1-3. Motchane himself "criticize[d] the stinginess with which we pay intellectuals in France." Lettre de Jean Dieudonné à Léon Motchane (25/2/59).

[17] Later, known simply as the Scientific Committee. The *Statuts de l'Institut des hautes études scientifiques,* art. 11, states: "The Administrative Board [of the IHÉS] has the power to take all decisions, . . . except those of a scientific nature which come under the authority of the Scientific Committee." See art. 5, 13-16 about the roles and powers of the Scientific Committee. Arch. IHÉS. See also *Journal officiel de la République française* (22 April 1974).

[18] See Pierre Lelong, "In memoriam. Paul Montel (1876-1975)," *Gazette des mathématiciens,* no. 3 (February, 1975), 14-19. Montel presented to the Academy two notes concerning the IHÉS: "Note sur l'activité et la composition de l'Institut des hautes études scientifiques," *Comptes-rendus de l'Académie des sciences,* 254 (1962), 2257-

sciences of Paris from 1954 to 1961, Pérès also lent his prestige to the enterprise, but more importantly "ease[d] the relations between the newly created Institute and the University," which could have regarded (and sometimes did) the IHÉS as a threat to its activities. Pérès "was able to show to his colleagues that the Institut des hautes études scientifiques, far from hindering the development of the University, brought new means facilitating the progress of research."[19] Pérès also helped with establishing crucial contacts with the government, at the highest level.[20]

The Institute for Advanced Study at Princeton—"in its spirit and its structure"—always provided Léon Motchane with a model *par excellence* to which the IHÉS should aspire.[21] His brother, Alexandre Motchane, an engineer living in New Jersey, had introduced him to Robert Oppenheimer, director of the IAS. Apparently Motchane was

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2258; "Historique de l'Institut des hautes études scientifiques," *Ibid. (Vie académique),* 269 (1969), 95.

[19] *Hommage de André Grandpierre à Joseph Pérès,* Assemblée générale (14/3/62). Cf. *Éloge du Doyen Pérès par Marc Zamansky* (22/2/62). Arch. IHÉS. Also: "Cependant, dès notre fondation, une certaine méfiance s'est manifestée en France de la part de quelques institutions universitaires. La crainte d'une concurrence, la possibilité de débauchage des professeurs de la part d'un centre riche et ayant une plus grande liberté de manoeuvres que l'Université, furent probablement à l'origine de cette réserve. La présence à la présidence de notre conseil d'administration du Doyen de la Faculté des Sciences de Paris et aussi la politique rigoureusement suivie par l'Institut ont rassuré les esprits. Il est rapidement apparu que les 'emprunts' de l'Institut au personnel universitaire français se réduiraient à peu de chose." *Rapport scientifique 1958-1959* (9/2/59).

[20] *E.g.* lettre de Joseph Pérès au Général de Gaulle (27/6/58); de Joseph Pérès à André Maréchal (28/8/61), etc. Arch. IHÉS.

[21] *Note communiquée à la presse* (11/7/58). Arch. IHÉS. In fact, the bylaws of the IHÉS (art. 11, quoted above) were "more liberal" than Princeton's. Lettre de André Weil à Léon Motchane (29/9/62); de Léon Motchane à André Weil (9/10/62): "clause disant que toutes les décisions scientifiques sont du ressort du Comité Scientifique et ne peuvent pas . . . être infirmées par le Conseil d'Administration." Arch. IHÉS. About the IAS and Princeton University, see W. Aspray, "The Emergence of Princeton as a World Center for Mathematical Research, 1896-1939," *History and Philosophy of Mathematics,* ed. W. Aspray and Philip Kitcher (Minneapolis: University of Minnesota Press, 1988): 346-366.

able to convince Oppenheimer to play an important role in the founding of a "Parisian Princeton."[22] From the earliest plans remaining in the archives of the IHÉS (probably dating from the end of March 1958, but no later than June), we learn that Pérès talked with Oppenheimer in the spring of 1958 and that Motchane was already counting him among the potential members for the Consultative Scientific Committee.[23] Until his death in 1967, Oppenheimer offered unconditional support to the IHÉS, and his advice on scientific, financial, and organizational matters. Motchane adopted many a tradition from the IAS, including such English rites as serving tea and cakes at 5 o'clock![24] More importantly, Oppenheimer became something of a mentor for him in his new job as the director of a research institution. On the occasion of a trip to the US, Motchane confessed to Oppenheimer:

> There is no doubt that I come principally to see you, and, like every year, have two or three good conversations with you. By discussing with you the problems that we share, by talking in all friendliness of things and men—because the Institute is a human affair—by listening to you, I succeed in finding my course.[25]

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[22] In the years that followed its foundation, the IHÉS was widely known as the French Princeton or the Parisian Princeton. For its fundraising campaigns in the US, from 1963 to 1969, it used, with Oppenheimer's blessing, the name of "Institute of Advanced Study—Europe." Lettre de Arnaud Denjoy à Léon Motchane (20/7/58), *Séance de fondation du Comité américain [American Committee]* (11/3/64). Cf. R. P. Dubarle, "Un Princeton français: un cloître voués à la recherche," *Le Monde* (16 May 1963), 13; L. A. Zbinden, "Le Princeton de l'Europe," *Gazette de Lausanne* (4/5 May 1963).
[23] *Notice* de Léon Motchane pour Fernand Picard, directeur des Études et recherches de la Régie Renault (n.d., portant la mention manuscrite "fin mars [1958]?"). Besides Pérès and Montel, Louis de Broglie was also mentioned. He was indeed approached, but declined because of his "too heavy duties." Lettre de Louis Motchane à Louis de Broglie (7/7/58).
[24] Robert Oppenheimer said of tea: "It is where we explain to each other what we don't understand." Quoted in *Batelle Rencontres: 1967 Lectures in Mathematics and Physics*, ed. C. M. DeWitt and J. A. Wheeler (New York: Benjamin, 1968), x.
[25] Lettre de Léon Motchane à Robert Oppenheimer (24/10/62). Arch. IHÉS.

Under such patronage, Léon Motchane gathered the nominal support of many internationally renowned scientists, most of whom, however, did not directly contribute to the founding of the Institute.[26] At the International Congress of Mathematicians, held in Edinburgh in August 1958, and probably before, Jean Dieudonné and Alexander Grothendieck, eminent figures of two particularly successful generations of French Bourbakist mathematicians, accepted offers to become the first permanent professors of the Institut des hautes études scientifiques.[27] But, however successful at enrolling scientists and university administrators in supporting the Institut des hautes études scientifiques, Motchane could not set it up alone. He needed money.

b) What is Fundamental Research and Why Should Industry Sponsor It?

Before the foundation, a Committee was set up which comprised, besides Motchane, Montel, Pérès and Oppenheimer, two more members: Fernand Picard and Maurice Ponte.[28] Both occupied high-level executive positions in large French corporations. Both were seriously involved in the process of the foundation of the IHÉS from the very

[26] A *Note communiquée à la presse* (11/7/58) lists Profs. Amaldi, Niels Bohr, Max Born, Louis de Broglie, Jean Dieudonné, P.A.M. Dirac, Alexander Grothendieck, Louis Néel, and Victor Weisskopf. To which we can add Jean Leray and W. Heisenberg. Lettre de Werner Heisenberg à Léon Motchane (3/10/58); de Louis de Broglie à Léon Motchane (9/7/58); de Jean Leray à Joseph Pérès (15/7/58); de Léon Motchane à Robert Oppenheimer (24/6/58), in which Motchane asks Oppenheimer to approach Dirac, as he did Bohr. Arch. IHÉS.

[27] In the files of the IHÉS, their official agreement to become professors is lacking. The first letter from Jean Dieudonné to Léon Motchane was dated 23/6/58, in which he remarked: "Vous pouvez assurer CARTAN que nous ne dépeuplerons pas la Sorbonne!", thereby indicating that his coming to the IHÉS was already secured. On October 8, Motchane wrote to Oppenheimer about the two permanent professors. It was understood that they would start in February, 1959. Motchane's first letter to Grothendieck that is preserved is from 8/12/58. Arch. IHÉS.

beginning.[29] A physicist, *normalien* and *agrégé,* who had invented the first French radar, Maurice Ponte was vice-president of the *Compagnie générale de télégraphie sans fil* (CSF). Later in 1958, he would be drafted in de Gaulle's efforts for developing a coherent science policy at the inter-ministerial level. From the beginning a member of the *Conseil consultatif de la recherche scientifique et technique* (CCRST), also known as "les 12 sages,", he even briefly headed it in 1959.[30] "Clearly [a] positive element," Motchane noted, but understandably, with "no time to devote to the Institute."[31] While Ponte did participate in some of the early meetings and secure the financial participation of his enterprise, he seems not to have been involved so much with the actual work of organization and fundraising. At one of the last meetings prior to the foundation agreement, Ponte declared to Motchane: "I'm in, provided you take care of everything!"[32]

On the other hand, Fernand Picard, director of the research department of the nationalized automobile manufacturer Renault, actively worked at securing a financial basis for the IHÉS. Trained as an *Arts et Métiers* engineer, his general culture was only

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[28] Lettre de Léon Motchane à Francis Perrin (2/6/58). Arch. IHÉS.
[29] Lettre de Léon Motchane à Maurice Ponte (n.d., mais portant la mention manuscrite "fin mars [1958]?"), in which Motchane talked of Ponte as the "leader of our organizing committee;" notice de Léon Motchane à Fernand Picard (n.d., mais portant la mention manuscrite "fin mars [1958]?"); lettre de Léon Motchane à Fernand Picard (16/4/58). Arch. IHÉS.
[30] Presided by Maurice Letort, the first meeting of the 12 sages was held on December 13, 1958. Maurice Ponte became president of the CCRST on December 24, 1959, and was replaced by Pierre Aigrain on November 29, 1961. Arch. IHÉS. See also Antoine Prost, "Les origines de la politique de la recherche en France (1939-1958)," *Cahiers pour l'histoire du CNRS,* 1 (1988): 41-62.
[31] *Note pour le Dr OPPENHEIMER,* par Léon Motchane (septembre 1959), 2. Arch. IHÉS.
[32] Lettre de Léon Motchane à Joseph Pérès (2/2/59). Arch. IHÉS. On Maurice Ponte and CSF, see F. Jacq, *Pratiques scientifiques, formes d'organisation et conceptions politiques*

"average," according to Motchane, but he unfailingly remained "idealistic and enthusiastic about the Institute."[33] Picard enrolled his boss Pierre Dreyfus, president of the Régie Renault, in the project. A precious supporter with useful connections, Dreyfus was, Motchane wrote in September 1959, "a remarkable man with a high conscience of useful things in all areas [*dans tous les métiers*]. Thanks to him, the Institute exists: he drew all the others in."[34] Together, Picard and Dreyfus approached several corporations linked with the automobile industry, and helped obtain the support of large oil companies, such as Esso and Shell.[35]

(i) Looking for Patrons

It may not be obvious just how bold and peculiar was Motchane's gamble. Instead of trying to enroll the traditional sponsors of pure research in France, i.e. mainly the State, together with private donors, he turned to the private and nationalized industries. And to these industrialists, Léon Motchane, from the very beginning, persistently underscored the "essential" condition for the realization of the IHÉS, *i.e.* "the scientific direction of our Institute will be *entirely free and independent* from any financial influence."[36]

In the French political context especially, this was far from an obvious gamble. For the many scientists who had strong Leftist inclinations, the support of big industry for

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de la science dans la France d'après-guerre, thèse (École nationale supérieur des Mines de Paris, 1996).

[33] *Note pour le Dr OPPENHEIMER,* par Léon Motchane (septembre 1959), 2. Arch. IHÉS.

[34] *Note pour le Dr OPPENHEIMER,* par Léon Motchane (septembre 1959), 2. Arch. IHÉS.

[35] Motchane met with MM. Ballet (Esso), Kaplan (Shell), and Besse (B.P.) on 20/5/58. Note de Léon Motchane à Fernand Picard (17/6/58). Arch. IHÉS.

fundamental research was bound to be seen as a form of capitalistic control over it. Understandably, however, this objection hardly surfaces from the IHÉS archives. The only exception is a note indicating that in 1965 Grothendieck reported that Bourbaki mathematician Roger "Godement holds like Schwartz that the Institut represents the beginning of the takeover of the University by French capitalism and is decided not to have any contact with us."[37]

Actually, Motchane also looked at more traditional sources of financing, and this might well be where the feasibility of the institute project first became apparent. In a letter to Pérès, Mlle Gabrielle Reinach (1889-1970) explained that, being without direct heir, she had intended bequeathing her fortune to the Collège de France, where her father, Théodore Reinach, had taught.[38] She moreover wished to endow immediately a new chair in one the disciplines "of what we today call 'fundamental research', to the exclusion of any concern for applications." Without much of a scientific background, she confidently asserted that "since Henri Poincaré's writings, even non-mathematicians know that the progress [of the exact sciences] is possible or fruitful only when very general and disinterested abstract research is restlessly pursued." This activity, without a doubt, not only demanded great intellectual abilities, but also "much courage and character, and quite a lot of abnegation." Consequently, Mlle Reinach asked that the Collège choose not

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[36] Lettre de Léon Motchane à Maurice Ponte (n.d., mais portant la mention manuscrite "fin mars [1958]?"). Arch. IHÉS. My emphasis.
[37] Note taken by Annie Rolland (15/11/65). Arch. IHÉS.
[38] Notice that the lawyer who, at the origin, helped Motchane with the legal status of the IHÉS, Me Jean Robert, previously was Gabrielle Reinach's lawyer. Procès-verbal, Assemblée des professeurs (30/6/57). Archives du Collège de France (thereafter Arch. CdF), G-iv-m 28G*. I thank Christine Delangle and Marie-Ange Aucherie for their kind help in looking through these archives.

only professors of a sufficient scientific level, but also those that would have "the value of a moral example and would lead the young towards disinterested scientific research that our country greatly needs."[39]

Given her requirements, the faculty of the Collège de France found that there were "grave administrative difficulties" in accepting her generous offer.[40] Upon learning of the IHÉS from Motchane, Reinach decided to give the Institute an immediate gift of 15 millions *ancien francs,* and named the IHÉS her sole legatee. She became a member of the first Administrative Board, but not of the Institute after the minimum annual contribution was raised in 1959.[41]

Remembering "the generous gesture of his grandfather who founded, more than thirty years ago, the Institut Henri Poincaré, of international renown," Léon Motchane also approached "the young baron [Edmond] de Rothschild" in order to have him finance the Institute's land investment.[42] Contrary to the above precedent—or to that of the VIth Section of the *École pratique des hautes études* being set up by Fernand Braudel at about

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[39] Lettre de Gabrielle Reinach à Joseph Pérès (23/6/58), reprenant les termes d'une lettre de Gabrielle Reinach à Marcel Bataillon, administrateur du Collège de France (31/5/57). Arch. IHÉS.
[40] Procès-verbal, Assemblée des professeurs (30/6/57). Arch. CdF, G-iv-m 28G*.
[41] From 5,000F to 50,000F! *Procès-verbal de l'Asssemblée générale* (10/2/59). Mlle Reinach gave the IHÉS 50 kF in 1958, and then bequeathed her fortune to the Institute.
[42] Lettre de Léon Motchane à Albert Roncey, pour Edmond de Rotschild (6/6/58); note de Léon Motchane à Fernand Picard (n.d., "fin mars [1958]?"). Arch. IHÉS. About the foundation of the IHP, I refer the reader to Dominique Pestre, *Physique et physiciens en France 1918-1940* (Paris, Montreux: Éditions des Archives contemporaines, 1984); and L. Beaulieu, *Bourbaki,* 45-49.

the same moment—Motchane does not seem in 1958 to have solicited the Rockefeller Foundation or any other such organization.[43]

(ii) The Nationalized Sector

Léon Motchane himself felt more comfortable in asking for support from the nationalized industries producing electricity and natural gas. He also insistently solicited Francis Perrin, head of the *Commissariat à l'énergie atomique* (CEA), whose support soon became essential to allow the participation of the nationalized sector.[44] On June 2, 1958, Motchane wrote to Perrin:

> It is however extremely important that all principal industries be represented in our Institute, notably the industries of the atom, of electricity, natural gas and coal. Given the particular status of nationalized corporations exploiting these domains, it appears that the participation of the CEA as a subscriber of our Institute will make it easier, for the authorities on which the nationalized industries depend, to accept the fact that these industries largely partake in the financing of our organization.[45]

Indeed, Léon Motchane had lured Pierre Ailleret, director of research at *Électricité de France* (EDF), with the prospect of solving "the crucial problem of theoretical physics . . . namely, the structure of matter and particle theory . . . [with] a delay of a few years." Such progress entailed, as a first practical application "the direct transformation of nuclear energy into electrical energy – a transformation that would avoid any

[43] As late as 1967, Motchane writes: "nous n'avons pas l'habitude des Fondations en général." Lettre de Léon Motchane à Victor Weisskopf (7/12/67). Arch. IHÉS. On the foundation of the VIth Section, see Brigitte Mazon, *Aux origines de l'EHESS, le rôle du mécénat américain* (Paris: Éditions du Cerf, 1988).

[44] Professor at the Collège de France, Francis Perrin, born in 1901, insistedly promoted the creation of theoretical professorships at the Collège. See Procès-verbal, Assemblée des professeurs (16/3/47), G-iv-l 29E; idem (27/11/49) G-iv-l 39X; idem (5/3/50) G-iv-l 40O; idem (25/11/51) G-iv-m 4Dd; etc. Arch. CdF.

[45] Lettre de Léon Motchane à Francis Perrin (2/6/58). Arch. IHÉS.

thermonuclear reaction."[46] Given that Motchane envisaged nothing less than a solution to the fusion problem, it becomes easy to understand why the participation of EDF was made contingent on that of the CEA, whose informed opinion could be trusted. As president of the Second International Conference of the United Nations on the Uses of Atomic Energy for Peaceful Ends, taking place in September of that year, Francis Perrin certainly was an authority to be counted on.[47]

Whether or not Motchane actually thought it possible to solve the fusion problem in a few years, the question was not so much that this progress would be achieved at the IHÉS—clearly, given the nature of the institute envisaged, it would not—but rather whether there would be, in Europe, and particularly in France, at the crucial moment, "a team of trained and informed scientists [*savants*]," able to serve as "interpreters" between theory and practice, between scientists and engineers.[48] In the future, nuclear energy was never mentioned again as a possible fallout from the Institute's activities, but this conception, according to which the IHÉS help train "many interpreters capable of placing abstract structures at the disposal of those who will use them for experimental applications and practical accomplishments," would often be exploited in the following

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[46] *Note,* portant la mention "strictement confidentiel," jointe à une lettre de Léon Motchane à Pierre Ailleret (7/5/58). Arch. IHÉS. See Comp. (a) to Chapter IV.

[47] Georges Guéron, "Observations à propos de la Seconde Conférence internationale des Nations-Unies sur l'utilisation de l'énergie atomique à des fins pacifiques," *Prospective,* 2 (January 1959): 13-21. This was the time when nuclear energy started to be used commercially in France, see Syndicat CFDT de l'Energie Atomique, *L'Electronucléaire en France* (Paris: Seuil, 1975).

[48] *Note,* jointe à une lettre de Léon Motchane à Pierre Ailleret (7/5/58). Arch. IHÉS. See also Comp. to Chapter IV below.

years.[49] As we shall see below, apparently a move way from the ideal of pure, fundamental research, this emphasis on "interpreters" can be seen as having shaped the evaluation by the IHÉS of the research conducted within its walls.

(iii) Big Industry

Initial approaches looking promising, in June 1958 Léon Motchane carried out an energetic offensive to gather the financial commitments allowing the foundation of the Institut des hautes études scientifiques. Already, the participation of CSF, Renault, EDF, and three oil companies seemed a sure thing. In his letter to Francis Perrin, Motchane wrote that he now had the pledges of about ten corporations for an amount of over 100 million (*anciens*) francs, half of the goal he then fixed.[50] He, and Fernand Picard approached several other companies, securing about 200 million francs in contributions. So that Motchane wrote to Montel, on June 18: "In the presence of favorable responses that materialize, with a laudable monotony, in precise pledges, . . . we took the decision, Monsieur Pérès, Monsieur Picard and I, to proceed with the Foundation of our Institute on Friday, June 27, at 4 o'clock."[51]

Let us examine the arguments used by Léon Motchane in order to persuade a sufficient number of large corporations to finance his enterprise at a level often close to 1 percent of their total research budget.[52] In a note he sent to industrialists before and after

[49] Lettre de André Grandpierre à Pierre Messmer, Ministre des Armées (3/3/66), 2. See also les *Commentaires,* préparés pour la conférence de presse (juillet 1958). Arch. IHÉS.
[50] Lettre de Léon Motchane à Francis Perrin (2/6/58). Arch. IHÉS.
[51] Lettre de Léon Motchane à Paul Montel (18/6/58). Arch. IHÉS.
[52] The amount of 1 to 1.5% of the research budget, "much below the one commonly accepted in the United States for fundamental research," is mentioned in lettre de Léon Motchane à Pierre Ailleret (22/4/58); de Léon Motchane à Francis Perrin (2/6/58); note de

the foundation of the IHÉS, Motchane emphasized that the nature of scientific research, and of its organization, had changed in recent years.[53]

> Scientific research is not a spontaneous phenomenon of nature that flourishes in the Universities, but an activity we need to deal with, to cultivate, and which brings to a country that is abundantly equipped with [research institutions] a considerable addition of prestige and political power. . . . The true modern aspect of scientific research (which is less known to the public) consists in the fact that the work of an industrialist, of an engineer, like that of a theoretical physicist and of a mathematician, be it the most abstract, are not so far from one another, and the success of the latter becomes indispensable to the former.

Scientific research had to be cultivated, and collaborations between specialists of different fields, encouraged. With this goal in mind, modern technological applications now crucially depended on

> *Fundamental Research* in the exact sciences, by which we mean, in a restrictive fashion, the researches done, without concern for applications, in the domains of *Pure Mathematics, Theoretical Physics,* and the *Physico-Mathematical Methodology of the Sciences of Man*. . . . Alone [compared with applied science and engineering], the major problem of fundamental research, neglected for many years, has never been seriously taken up [in France], which explains for example the distressing backwardness of our country in theoretical physics.[54]

One may compare Motchane's definition of fundamental research with the one provided by a group physical and chemical experts who in 1970 concocted the VIth Plan for the French government. Noting that "a nation cannot allow to renounce to fundamental research without ineluctably vowing itself to a state of intellectual and industrial underdevelopment," they contended that the motivation of fundamental research was to "know and understand the laws of nature."

Léon Motchane pour Fernand Picard, à la suite d'une communication téléphonique entre eux deux (17/6/58).

[53] *Note pour les industriels* (Mai 1958). Arch. IHÉS. See Comp. (b) to Chapter IV.

[54] *Note pour les industriels* (Mai 1958). Arch. IHÉS. Original emphasis. Comp. (b) to Chapter IV.

> Used criteria are simplicity [and] generality. They allow to chose relatively simple problems, which will lead to the pulling out and formulation of these laws in their most profound and general form, and to the definition a simple language adapted for their analysis.[55]

In the US, and in Russia, Motchane claimed, the organization of fundamental research had been centered around institutes, like Princeton's. The solution was clear: "To gather a relatively limited number of scientists [*savants*] of great value, physicists and mathematicians, to give them all ease for work, without imposing on them teaching duties, nor any obligations."[56] While Motchane's contention about the role played by Institutes such as the IAS in Princeton may be highly contestable on historical grounds, we must note that the IAS being his model for the organization of the IHÉS, he saw the great advantages he could take away from portraying fundamental research as such.

Clearly, the mere fact that contributions to the IHÉS, up to 0.2 percent of the corporations' turnover, were tax deductible, is not enough to understand the reasons why "almost all the industrialists approached enthusiastically embraced the idea of a center for fundamental, that is, disinterested, research."[57] Motchane's arguments above hardly

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[55] DGRST, *Rapport de la Commission du 6e Plan, 1971-1974. Recherche,* tome 2 (Paris: La Documentation française, 1971), Chapitre I: "G.S. 1 - Etude de le matière et du rayonnement," 11-32. Fonds doc. CNRS. Quote on p. 11.

[56] *Note pour les industriels* (Mai 1958). Arch. IHÉS.

[57] *Commentaires,* préparés pour la conférence de presse (juillet 1958), 4. Arch. IHÉS. Corporations that joined the IHÉS in 1958-59 were: the Régie Renault, CSF, Saint-Gobain, CEA, Shell, EDF, Sovirel, Esso Standard, and Pont-à-Mousson. They were soon joined by two Italian companies: Fiat and Montecatini. See, *e.g.,* the *Résumé préparatoire* pour *Les hauts-lieux de la recherche scientifique,* un entretien de Paul Montel et Léon Motchane, avec François Le Lionnais, diffusé le 30 mars 1961 à 19h20, à RTF France III, dans le cadre de la série "La Science en Marche," 12. Arch. IHÉS. About conditions for tax deductions, cf. *Journal officiel de la République française* (28 September 1958): ordonnance no. 58-882 (25 September 1958) relative à la fiscalité en matière de Recherche scientifique et technique.

indicate a single direct return for the contributors to the Institute. He only offered their "noble and patriotic motives" as a motivation.[58]

Without questioning the patriotism of these men, I cannot help noticing that, for many who devoted a lot of time and energy to the IHÉS, a strong impression of personal gratification transpire from the records. Often with scientific training, having gone through the *Grandes Écoles,* they simply were excited by the prospect of contributing, in their own way, to the great adventure of pure science. In addition, they got to have lunch with Robert Oppenheimer himself.[59] Underscoring the personal component in the involvement of several companies was the fact that many decided to withdraw their support to the IHÉS just as they changed their administrators.

More seriously, we may underscore that these industrialists certainly also saw what a general increase on the scientific level of their country, and indeed of Europe in general, could offer in the long run in terms of pay backs for multinational corporations like theirs, who depended on high technological advances to make their profit.[60] One

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[58] *Commentaires,* préparés pour la conférence de presse (juillet 1958), 4. Arch. IHÉS.
[59] For example, Motchane writes Oppenheimer that Fernand Picard, during a trip to the US, "would be extremely flattered if you [Oppenheimer] devote a few moments to him." Lettre de Léon Motchane à Robert Oppenheimer (10/1/61). Similarly, René Grandgeorge, with Motchane, visited Oppenheimer on a trip to Princeton. Lettre de Léon Motchane à René Grandgeorge (4/3/60). When Oppenheimer came to Paris in September 1959, an busy schedule was established so that he ate with each administrator of the Board. *Note pour le Dr OPPENHEIMER,* par Léon Motchane (septembre 1959). Arch. IHÉS.
[60] I wish to thank Dominique Pestre for having explained this to me. A justification of several types of investment, depending on goals set for them, is to be found in Marcel Demonque, "Quelques réflexions prospectives sur le monde industriel de demain," *Prospective,* 1 (May 1958): 25-35 and Georges Guéron, "Synthèse des travaux," *Prospective,* 5 (May 1960): 11-77, esp. 41-43. Obviously, not all industrialists accepted to get into the boat. The vice-president of the Société d'électro-chimie d'Uginé for example argued: "According to what was recently said at the Academy, the problems of the organization of research will be taken up at a governmental level. In these conditions, it

should moreover remember that this was a period of unprecedented prosperity in the West, the height of what the French call *les Trente Glorieuses.* Some of the big industries solicited by Motchane might have felt that the necessary effort for the advancement of science demanded by him would remain a small strain on their finances. It was a small price to pay for promoting, to use Gaullist terms, a boost of France's *grandeur,* scientifically just as well as politically, which, they felt, could only help their business.

Indeed, undermining arguments in favor of narrow nationalism, the founders of the IHÉS expected from the very beginning to attract a wide European participation in its financing. Obviously, this seemed to them a natural counterpart of a participation of scientists, both as permanent and invited professors, to the IHÉS, which was supposed to overlook any kind of discrimination, including nationality, in its recruiting. The founders of the IHÉS therefore solicited industrialists from other European countries (especially Germany, Belgium, and Italy), as well as nascent European supranational structures. "It is evident that there is no room in Europe for two institutes of this kind and that, moreover, the *raison d'être* of such an organism principally resides in its universal character exceeding the framework of one nation. Consequently, as soon as it is set up, a call will be addressed to industrialists from all European countries."[61]

These European contributions would however prove extremely hard to get, especially that out of the first four permanent faculty members of the IHÉS, three would turn out to be French, but not, as we shall see below, for a lack of efforts at recruiting

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seems to me, personally, premature to take a position in one direction or another." Lettre de René Perrin à Léon Motchane (10/7/58). Arch. IHÉS.

[61] Lettre de Léon Motchane à Francis Perrin (2/6/58); and also *Commentaires,* préparés pour la conférence de presse (juillet 1958), 2. Arch. IHÉS; "Rapport Euratom."

foreigners. Only two Italian corporations (Fiat and Montecatini) would answer the IHÉS's call for a few years. On the other front, European supranational structures would prove badly designed for supporting an institution like the IHÉS, even when showing much good will (*e.g.* the case of Euratom).[62] The solution for international financing of the Institute, in the end, proved to be direct solicitation of national research councils of other European countries, but this was quite slow in the making.[63]

(iv) What Thus is Fundamental Research?

As we have seen above, Motchane's reliance on private business organizations for the funding of the IHÉS had led him to emphasize ultimate benefits that fundamental research could bring to humankind, and to the companies that sponsored it. This might seem a

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[62] In contradiction with its bylaws which forbade it from sponsoring outside research, Euratom granted the IHÉS five "research scholarships" for three years, but had to stop in 1963. On "a contourné l'obstacle . . . d'une manière peu orthodoxe" [Cf. *note de* J. C. Koechlin à l'attention de J. R. Bernard (29/3/68, CTI-N° 68/246 - JCK/JAR), Bureau du Premier Ministre]. Lettre de Léon Motchane à Jules Guéron, directeur scientifique, directeur générale des études et de l'enseignement (6/4/59); *Rencontre* de Léon Motchane et Jules Guéron à la Fondation Thiers (29/12/59). Lettre de Léon Motchane à Jules Guéron (18/2/60); de Léon Motchane à Hervé de Vitry (19/4/60); de Léon Motchane à Jules Guéron (25/6/60); de Jules Guéron à Léon Motchane (29/9/60); de Léon Motchane à Jules Guéron (3/10/60); de Léon Motchane à Jules Guéron (6/10/60); téléphone de Hervé de Vitry à Léon Motchane (25/10/60); de Jules Guéron à Léon Motchane (26/1/66). Arch. IHÉS.

[63] Britain's SRC was the first to contribute to the IHÉS, in 1970. First contacts were established through Zeeman (Lettres de E. C. Zeeman à Léon Motchane [22/6/64]; de Léon Motchane à E. C. Zeeman [30/6/64]; de E. C. Zeeman à Léon Motchane [25/8/64]). Initially, the British were more inclined to use their money in order to found a similar institute in England. Serious efforts therefore started in 1967, after the founding of the Warwick Institute, and resulted in SRC's joining the IHÉS in 1970. Lettres de Léon Motchane à E. C. Zeeman (31/8/67); de Léon Motchane à Rudolph Peierls (19/6/69); de E. C. Zeeman à Léon Motchane (14/7/69); *An Account of the Meeting between M. L. Motchane and Professor Sir Brian Flowers, Chairman of the Science Research Council (SRC):* London (27/8/69); *Proposal for the SRC to Support IHÉS,* for the meeting of the

paradoxical way to argue for fundamental research. Indeed, was not this kind of research supposed to be developed out of motives purely internal to the scientific disciplines it stemmed from? When Motchane insisted on the role the IHÉS could play in training "interpreters" between fundamental research and potential applications, was he not moving away from his ideals?

These questions are central issues for achieving a better understanding of the kind of research the IHÉS would promote. As the above shows, Motchane was always very clear about one thing: he wanted the scientists working at the IHÉS to remain totally free to study whatever they wished. But at the same time, for Motchane, as well as for the industrialists enrolled in the project, the research done at the IHÉS, no matter how "fundamental" it was, nevertheless held potential promises for future applications. Of course, nobody believed that anything readily useable by industry would come out of the IHÉS. But a premise was shared, that such fundamental research could one day prove useful.

In view of the research later conducted at the IHÉS, notably on catastrophe theory and chaos theory, I believe that this ideology of fundamental research had an effect in orienting the kind of research that would be the most highly considered by the Institute. In effect, a middle way between pure and applied science was opened. The "fundamental research" promoted by the IHÉS was to remain free from outside influences, but at the same time, highly shaped by concerns with the world. Catastrophe theory exactly was this kind of fundamental research. By opening new vistas of understanding of natural

SRC Mathematics Committee, 11 November 1969, by E. C. Zeeman (dated october 1969); etc. Arch.IHÉS.

phenomena, it showed that pure mathematical research could shape the way people grasped structures of nature, while at the same time remaining scarcely concerned with practical uses for technology. Understanding, as opposed to prediction, computation, and action, became the IHÉS's ideal for fundamental research. This entailed that, in the eyes of Motchane and the administrators of the IHÉS, the most valued areas of research to be pursued at the Institute became mathematical research on the structures of mathematics and the development of languages of great generality, physical research on the mathematical structure of physical theories, with an emphasis put on elementary particle physics, and humanist research on methodology, i.e. the structure of social sciences theories. In the first Scientific Report he presented to the Administrative Board of the IHÉS, Motchane wrote: "Faithful to our conception of fundamental research, we solicited scientists [*savants*] who are attracted and interested by *new problems of a great generality.*"[64] The IHÉS provides us with another instance where *structure* played an important role as a cultural connector.[65]

Of course, this attitude had obvious political undertones. As they were not concerned with applications, the IHÉS scientists would not be bomb builders. But, at the same time, concerned with general theories having a bearing on the world, they would not remain in the ivory tower of academic research. At least, this was the ideal Motchane was pushing for. In view of later research conducted at the IHÉS, this attitude seems to have had some concrete effects.

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[64] *Rapport scientifique, 4 juillet 1958 - 31 décembre 1959* (2/2/60), 10. Arch. IHÉS. My emphasis.
[65] For more on this, see Chapter VI below.

c) Searching for Financial Stability

(i) Legal Matters and Threat from Industrialists

The founders of the IHÉS discovered that the existence of such an institution, common in Anglo-American countries, posed a problem for French law, and required legal innovations that could only be obtained from the highest levels: the Government and the Parliament. Indeed, the contributing members formed a non-profit association ("Loi du 1er juillet 1901"), which clearly was not designed for research institutions. Indeed, as Motchane emphasized, this institute "markedly differed from a typical association, like 'the interprofessional association of the horse' whose goal was 'to develop, improve, and coordinate the production, use, and understanding of horses and mules"![66] In particular, this status did not allow the Institute to build up endowments; it limited its possibilities of receiving donations from foreign countries; it required contributions of all members to be equal; and it barred usual tax deductions allowed for national research institutions.[67] The problem was "to find a new legal formula that would allow scientific institutions to manage their funds, with no administrative obstacle whatsoever. Such a legal entity does not exist in French law."[68] For all these reasons, Motchane resolved, after a meeting with Minister André Malraux, to change the status of the IHÉS, by creating from scratch a new

[66] Note préparée par Léon Motchane en vue d'une entrevue avec Gérald Antoine (23/4/60). Arch. IHÉS.
[67] *Ibid.;* "Exposé de motifs" préparé par Léon Motchane pour M. Poignant (n.d., 1959). Arch. IHÉS.
[68] Lettre de Léon Motchane à Robert Oppenheimer (8/10/58). Arch. IHÉS.

legal entity: an "international foundation."[69] Motchane spent the first summer of the Institute's existence writing these legal texts. In September, he wrote triumphantly:

> All this is quite cheering, and it leads me to some reflections of a philosophical character, namely, that if mathematicians are led to create legal texts, why would a few unsolved mathematical problems not be proposed to State councilors [*conseillers d'État*]? Who knows?[70]

However, these were tumultuous years for the French Government. Several Ministers of Education and of Scientific Research would examine the IHÉS problem one after another, until it was decided that a declaration of Public Utility (*déclaration d'utilité publique*), decreed on March 6, 1961, would suffice.[71] Ultimately, the transformation into an international foundation had to wait until 1980.

"As it often happens at a moment when we change status," Motchane reported to Oppenheimer in April 1959, "the temptation is great for some industrialists to backtrack on the generous and liberal dispositions of our original status, which allowed us to get the support of the principal scientists of the whole world."[72] Indeed, following first attempts at negotiating a new status with the Government, Pierre Besse and Léon Kaplan, both of whom had shown enthusiasm and diligence for the IHÉS, presented Joseph Pérès with suggestions that involved important modifications of its structure and spirit. By their action, Motchane wrote, "they almost put the IHÉS down."[73] In short they proposed to "envision a larger composition for the Consultative [Scientific] Committee, and to confer

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[69] *Entretien avec M. Malraux* (17/7/58) à 16h; *Notes de séance manuscriptes de l'Assemblée générale* (13/6/59) d'Annie Rolland. Arch. IHÉS.
[70] Lettre de Léon Motchane à Paul Montel (6/9/58). Arch. IHÉS.
[71] *Journal officiel de la république française,* 93, no. 56 (7 May 1961), 2382.
[72] Lettre de Léon Motchane à Robert Oppenheimer (4/4/59). Arch. IHÉS.
[73] *Note pour le Dr OPPENHEIMER,* par Léon Motchane (septembre 1959), 3. Arch. IHÉS.

on it the responsibility of defining the field of activity of permanent and temporary professors." Moreover, they suggested that

> what we could call our *'policy of work and research'* would therefore proceed from a formal agreement, achieved in conditions to be defined, between, on the one hand, the Consultative Scientific Committee [which would be] in a way 'intellectually' responsible, and [on the other hand] the financially responsible Administrative Board.[74]

This proposal infuriated Motchane who started to count his allies.[75] He accused Besse and Kaplan of "only having a vague idea of what scientific research is and of the importance of the principles of university freedom."[76] Fortunately for him, the reformers found themselves isolated.

> Basically, no doubts are possible: to accept such principles [as proposed by Besse and Kaplan] is to backtrack short of classical academic freedom, and this simply is equivalent to liquidating our Institute. Luckily, we now have new important subscribers, and even if SHELL gets out—which nobody desires—the Institute will not be in jeopardy.[77]

From all this fuss, it resulted that Besse (and his corporation, British Petroleum) never joined the association, while Kaplan finally gave his commitment to the consensus, but always remained an inside critical voice until Shell withdrew its support in the mid-

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[74] *Rapport sur l'organisation et sur les bases scientifiques et spirituelles du fonctionnement de l'IHÉS, présentée au Doyen Pérès par MM. Kaplan et Besse en décembre 1958,* 2. Arch. IHÉS. My emphasis.

[75] *Entretien* entre Léon Motchane et Maurice Ponte (22/12/58); de Léon Motchane à René Grandgeorge (20/2/59); de Léon Motchane à Fernand Picard (26/2/59); entrevue entre Léon Motchane et Pierre Dreyfus (13/5/59). Arch. IHÉS.

[76] Commentaires de la main de Léon Motchane, suite à une copie d'une lettre de Pierre Besse à Fernand Picard (17/10/58). Arch. IHÉS.

[77] Lettre de Léon Motchane à Fernand Picard (26/2/59). In his answer [lettre de Fernand Picard à Léon Motchane (11/3/59)] proposes, in a "conciliation spirit," that the president of the Board would automatically be member of the Council, and that the latter's role be "to define the orientation of the work of the Institute, in agreement with the Administrative Board." On the copy of this letter, kept in the IHÉS archives, Léon Motchane flatly wrote "non" beside this suggestion. Arch. IHÉS.

1960s.[78] For Motchane, a liberal conception of scientific freedom always remained non-negotiable.[79]

(ii) Finances and Activities

Depending on annual corporate donations, the finances of the Institut des hautes études scientifiques therefore exhibited an acute sensitivity to fluctuations in the economic situation of the sponsoring corporations, or any loss of interest on the part of their administrators. Over the years, as its investment program progressed importantly with the acquisition of a property at Bures-sur-Yvette, in the outskirts of Paris, and as its activity likewise increased, the Institute fell "victim to [its] own success," as Motchane noted.[80] The deficit for the 1964 exercise exceeded 600,000 (*nouveaux*) francs. And in 1965, after the cancellation of several subscriptions, he pulled the alarm signal: "*I am on the brink of bankruptcy!*"[81]

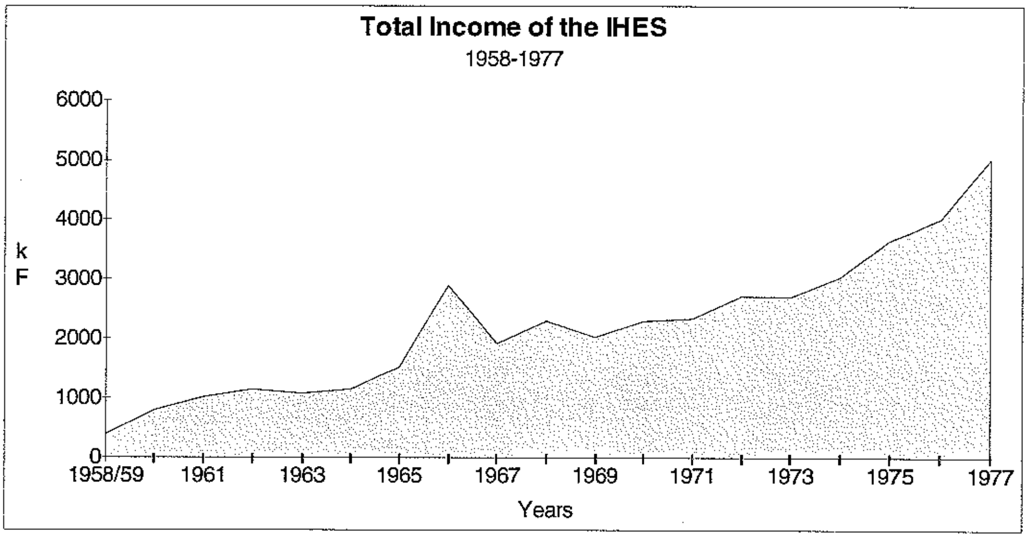
But, finally, the French State came to the rescue. In September, 1965, "the Government [took] a resolution in favor of regular help" to the IHÉS, which amounted to

[78] According to Motchane, Kaplan's role was "constantly negative." Projet de lettre de Léon Motchane à Louis Devaux (2/11/65). "[Il] essayait d'infléchir l'activité de l'IHÉS vers des programmes tracés d'avance, sous un contrôle plus étroit d'un comité où l'influence des industriels serait importante." *"Complément"* en vue de la visite de Léon Motchane à Henri Domerg (à Matignon, 19/2/68; daté 16/2/68).

[79] Léon Motchane, on the other hand, was more than willing to accept, as members of the Scientific Committee, scientists coming from research agencies financing the IHÉS, like the CEA, Euratom, the CNRS, or foreign research councils. See e.g. "Rapport Euratom" (mars 1959), 30. Arch. IHÉS.

[80] Lettre de Léon Motchane à Shepard Stone, Director of Sloan Foundation (23/5/63). Arch. IHÉS.

[81] Lettre de Léon Motchane à Frank Bowles, Program director of the Education Division, Sloan Foundation (5/2/65). Arch. IHÉS.



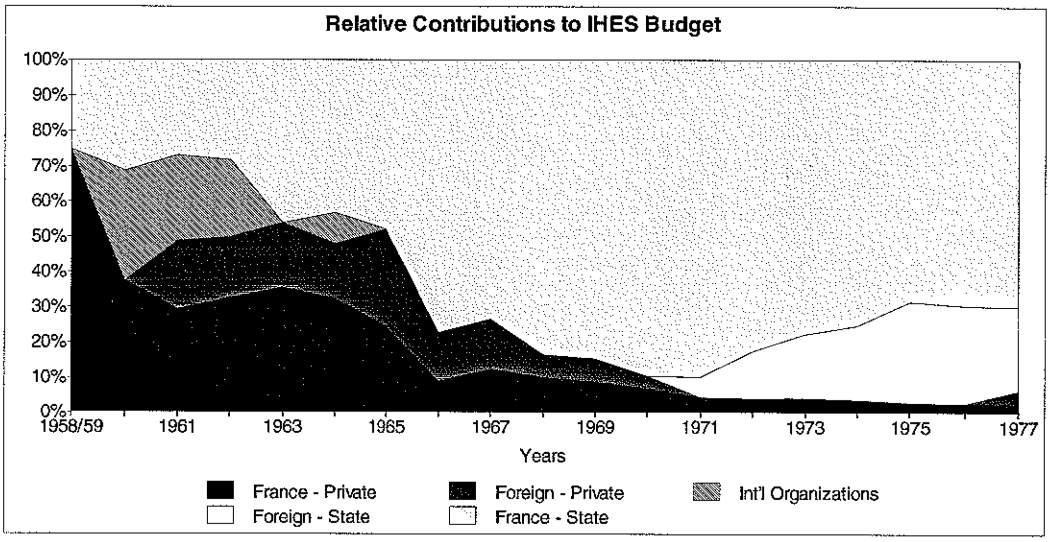
Graph 1: Evolution of the actual total income to the Institut des hautes études scientifiques (1958-1977). Arch. IHÉS.

about half of its resources for many years to come.[82] Its survival was thereby assured. In Graph 1, the peak in 1966 corresponds to an exceptional aid from the French State intended to balance earlier deficits.

Graph 1 above shows the evolution of the IHÉS budget in current francs from 1958 to 1977. This graph underscores two periods of relative stagnation: from 1962 to 1965, corresponding to the successive defection of a number of private subscribers, and then from 1967-1974, corresponding to a stagnation in the State's help, together with few infusion of money from other sources.

In order to have better view of the changing nature of the financial bases of the IHÉS, I plotted in Graph 2 the evolution of the relative contributions coming from five

[82] Lettre du Premier Ministre Georges Pompidou au Secrétaire d'État auprès du Premier Ministre, chargé de la recherche scientifique et des questions atomiques et spatiales [André Maréchal] (20/9/65). Copy in Arch. IHÉS.

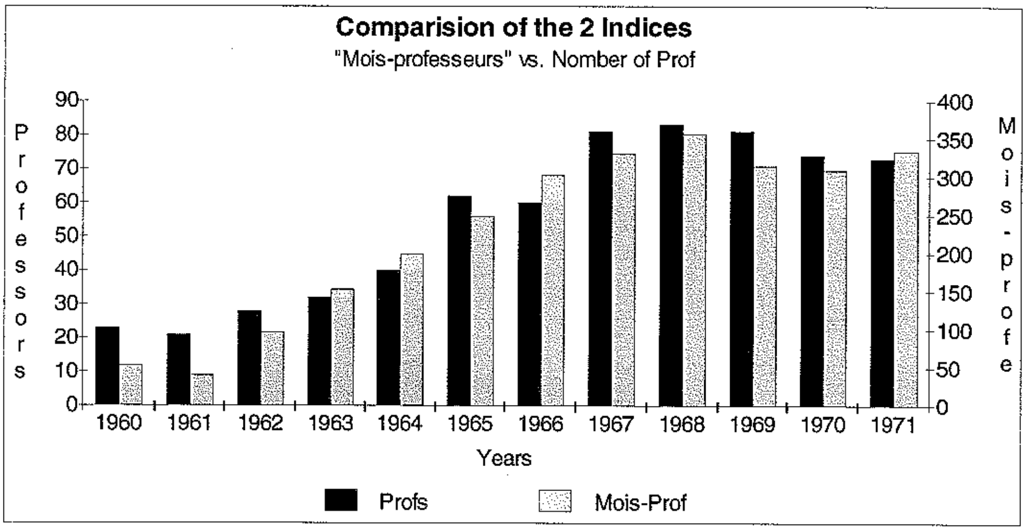


Graph 2: Relative contributions (in %) of the different types of sponsors of the Institut des hautes études scientifiques (1958-1977). Arch. IHÉS.

types of founders: the French private sector, the foreign private sector, international organizations, the foreign public sector, and finally, the French public sector.

The private sector, predominant until 1965 quickly became a lesser partner, while the French State insured the larger part of the IHÉS budget (up to 90% in 1970-1971!). Note however that nationalized industries, such as Renault, and the CEA, which were always counted as private support by the IHÉS have been here counted together with the French public sector. Support from foreign national science foundations became important starting in 1970. But clearly, the role of the French State remained predominant.

Since the level of activities at the IHÉS had not ceased to increase in 1962-1966, this situation clearly was strenuous (Graph 3). Indeed the very survival of the Institute



Graph 3: Total number of professors and total number of "mois-professeurs" at the Institut des hautes études scientifiques versus years, 1960-1971. Arch. IHÉS.

was then jeopardized. In 1965, the French State's decision to support the IHÉS directly therefore gave Motchane a little respite.

3. 'OSMOSIS' BETWEEN PHYSICISTS AND MATHEMATICIANS?

a) Statistics for Visiting Professors, 1960-1971

In Graph 3, I compared two indices that were used by the administration of the IHÉS in order to measure the evolution of the number of professors working at the Institute:[83]

(1) The absolute numbers of professors paid by the IHÉS each year, including permanent professors, invited professors, and, starting in 1965, visitors admitted without pay by the IHÉS. From the beginning, the distinction between mathematicians and

[83] The list used to compile Graph 3 below was included in Nicolaas Kuiper's *Rapport scientifique 1971* (18/5/72). Arch. IHÉS.

physicists was always clearly recorded in the IHÉS files. And (2) the number of "mois-professeurs," an index introduced by Motchane in 1963 which counted the number of months professors spent at the Institute, whose series unfortunately is incomplete.

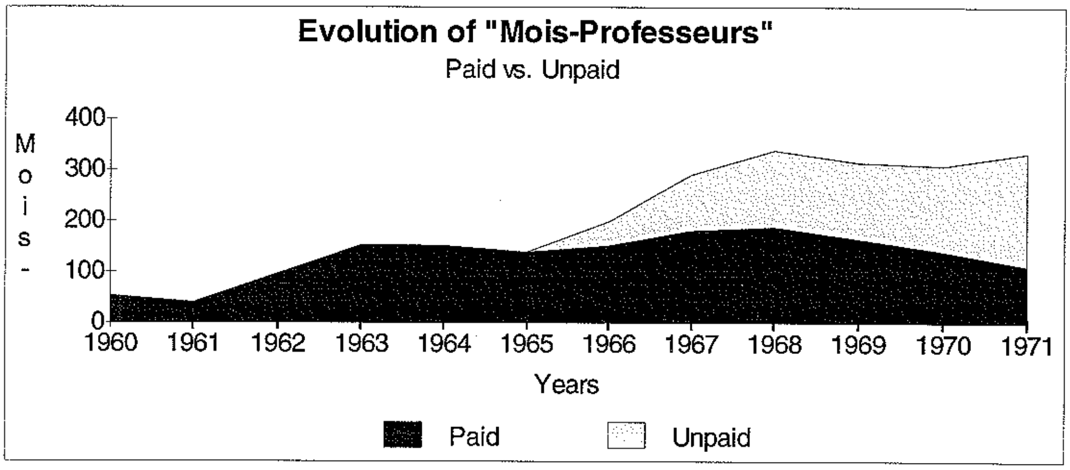
As a way to reflect the overall evolution these two indices are almost equivalent (Graph 3). The second series however underscores the low level of activity witnessed by the IHÉS in 1959-1962. The general trend is however the same for both. The activity of the IHÉS constantly increased until 1967-1968, and then witnessed a stagnation until at least 1971.

(i) Comparing Paid vs. Unpaid Professors and Visitors

To better interpret the global evolution, we may want to compare the number (or number of "mois-professeurs") of paid versus non-paid professors, as well as the number (or number of "mois-professeurs") of physicists versus mathematicians.

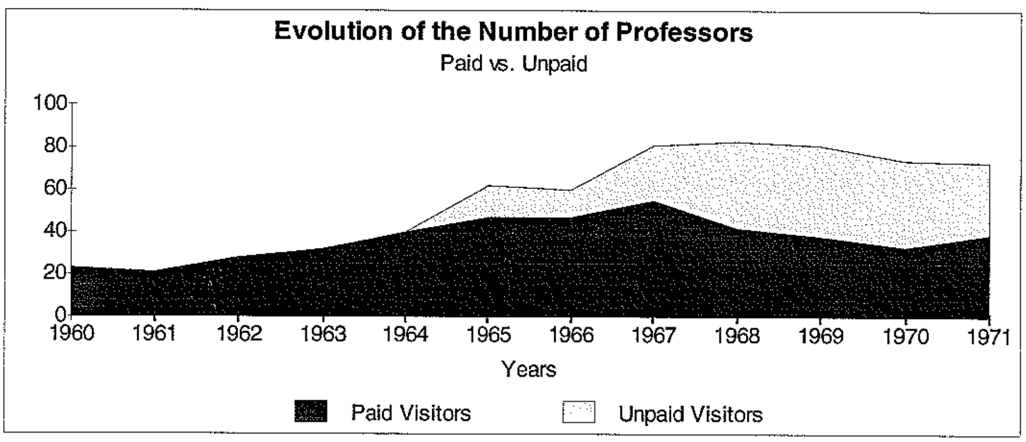
Graph 4 and 5 clearly show the increasing part played by admitted visitors (without pay from the IHÉS) as opposed to paid professors. Moreover, since admitted visitors often were graduate or postdoctoral students, whose stipend was paid by foreign universities and science foundations, they also tended to stay longer at the IHÉS.

Graph 4 gives the evolution of the number of "mois-professeurs." It graphically demonstrate the important role admitted professors were playing in 1968-1971. Considering the stagnation in the total number of collaborators working at the IHÉS, this graph underscores that the invitation budget of the IHÉS was then quickly becoming insufficient, at the same time as its repute, measured by the way it attracted unpaid researchers, was increasing.

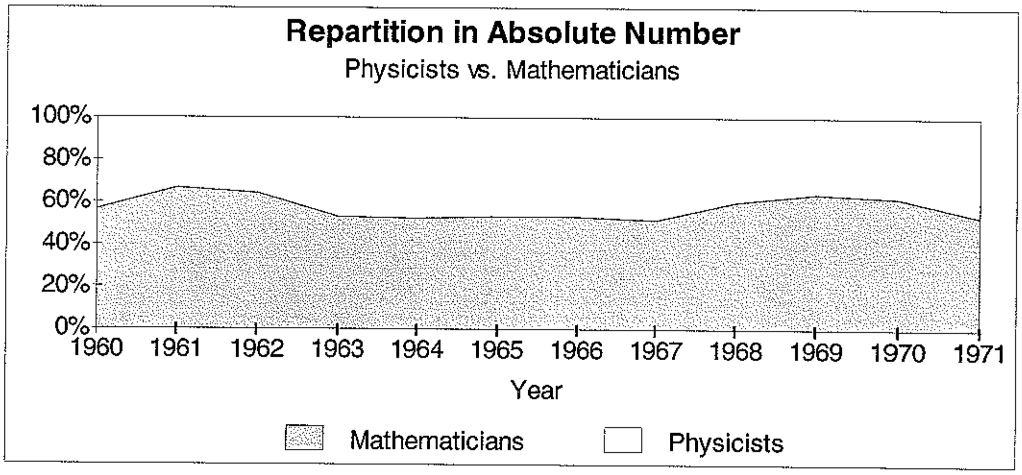


Graph 4: The number of "mois-professeurs" emphasizing the part played by unpaid admitted professors versus years, 1960-1971.

Graph 5 provides the same evolution in terms of absolute numbers of professors staying at the IHÉS. While it minimizes the importance of non-paid visitors, it however shows that these visitors started to play an important role as early as 1965.



Graph 5: The absolute number of professors emphasizing the part played by unpaid admitted professors versus years, 1960-1971.



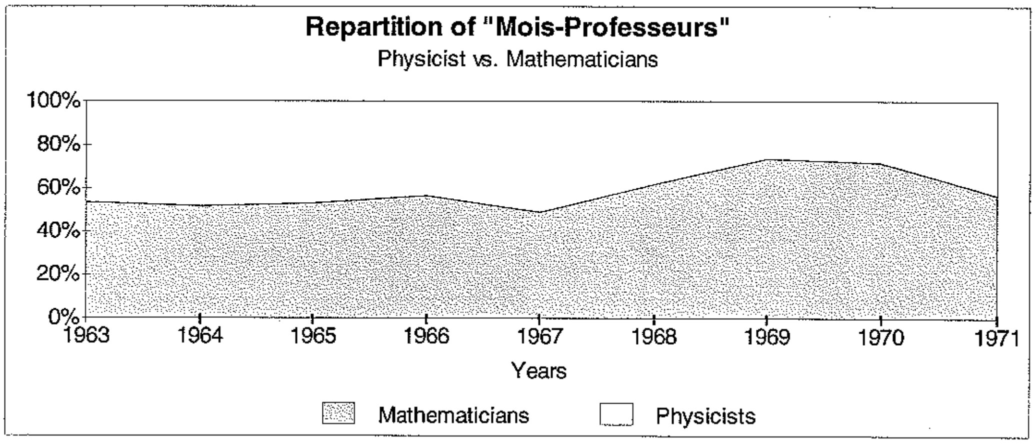
Graph 6: Percentage of mathematicians, as opposed to physicists, among the total number of professors invited at the IHÉS, 1960-1971

(ii) Comparing Physicists with Mathematicians

Graph 6 and 7 give the evolution of the percentage of mathematicians invited to work at the IHÉS. In absolute number, the proportion of mathematicians was relatively stable at around 50-60% over the whole period 1960-1971.

In terms of "mois-professeurs," however, the proportion of mathematicians, while similar to the above for 1963-1967, increased to as much as 70% in 1969-1970 (Graph 7). This graphically exhibits an important domination of mathematicians in the later part of the decade, as well as the fact that they tended to stay longer than physicist.

Let us now look more closely at the way the work was set up at the IHÉS, and at the activities that went on there during its first years, before the time when, in 1963, both René Thom and David Ruelle were hired.



Graph 7: Percentage of "mois-mathématiciens" as opposed to "mois-physiciens" spent at the IHÉS, 1963-1970. Note the scale different than in Graph 6.

b) Organizing the Work at the IHÉS

From the above, it appears that the conditions for the existence of the Institut des hautes études scientifiques hinged on an ideological premise: the success of fundamental research depended on communication between great scientists working in different disciplines of a very general character. And indeed, from the start, "a collaboration between mathematicians and physicists [was] envisaged."[84] In 1958, this hardly was a trivial statement to write; it underscored an important facet of Motchane's conception of fundamental research and foretold the kind of physics and mathematics that would be done at the IHÉS.

> The principle of establishing contacts between men whose investigation methods are . . . very different, has shown itself fruitful in the past. Today, this principle remains our only protestation against an excessive specialization, and our only

[84] Lettre de Léon Motchane à Francis Perrin (20/11/58). Arch. IHÉS.

> hope of preserving a global vision of problems, which is *precisely the key to the success of fundamental research.*[85]

The attention paid above to the problems encountered around its foundations, and to the particular nature of its legal status and sources of financing, puts us in a position to see the effects that such an organization was to have on the research conducted within its walls. I will consider two consequences of these view. The first deals with the relationship Motchane tried to foster between physicists and mathematicians at the Institute, the second with the place of the humanities at the IHÉS.

As mentioned above, the official bylaws of the IHÉS dictated that it would encourage fundamental research in "pure mathematics, theoretical physics, and the methodology of the sciences of man."[86] In practice, Motchane envisioned a division of his institute into separate sections along the lines of the "Schools" of Princeton's IAS. With Jean Dieudonné and Alexander Grothendieck starting their tenure in March 1959, the mathematical section was well under way. Hans Grauert–considered for professorship at the IHÉS, but having just been appointed to Göttingen–and André Weil, from the IAS, accepted nominations as "permanently invited professors."[87] Thus a "team" had been set up, which could "no doubt be compared to the best troops [*forces*] of the Sorbonne and of the Collège de France. But it seems difficult to find its equivalent elsewhere."[88] Indeed on

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[85] *Rapport Euratom* (March 1959), 11. Arch. IHÉS. My emphasis.
[86] *Statuts,* art. 1. Arch. IHÉS.
[87] This was an arrangement already practiced at the IAS, which involved from the part of professors a yearly short visit (1 to 2 months) at the Institute. *Rapport scientifique, 1958-1959* (9/2/59) and (2/2/60). About Weil, Jean-Pierre Serre wrote to Motchane (18/12/58): "Cher Monsieur, Vous me demandez mon opinion sur l'oeuvre de mathématique d'André WEIL. La voici: à mon avis, WEIL est l'un des deux ou trois plus grands mathématiciens vivants." Arch. IHÉS.
[88] *Rapport scientifique, 1958-1959* (9/2/59), 3.

May 19, 1959, Grothendieck began his soon-to-be-famous seminar of algebraic geometry. And scheduled (in February 1960), for the *Publications mathématiques de l'IHÉS,* was the first volume of his (and Dieudonné's) *Éléments de géométrie algébrique,* which would totally reshape the outlook of the field. Building on the prestige and strength of French mathematics—with the Bourbakis Dieudonné, Weil, Grothendieck—and securing the collaboration and sympathy of Jean Leray, André Lichnérowicz, Henri Cartan, Jean-Pierre Serre, and Claude Chevalley, the mathematical section of the IHÉS "was acquiring a personality which usually characterizes well-established scientific centers."[89]

Even in the mathematics section, however, there were few visible activities during the first years of the IHÉS. To start with, before 1963, the Institute had no seminar rooms, library, office space at its disposal, except for two offices it rented for the director and his secretary from the Fondation Thiers, in the 16th *arrondissement* in Paris. Seminars were held in a room that the Fondation made available for them every Wednesday afternoon, or at the Sorbonne or the École normale supérieure. In the spring of 1959, Wightman was expected to work in the hotel room he shared with his wife and ten year-old daughter, while, taking advantage of the nice weather, Kállen decided to work under a tree.[90] In 1959, besides Grothendieck's seminar, only John Milnor did give a few conferences.

In 1959-1960, an important portion of the mathematical activity was devoted to the *Publications methématiques de l'Institut des hautes études scientifiques.* In Motchane's words, this was not to be "a periodical mathematical journal of the usual type.

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[89] *Rapport scientifique, 4 juillet 1958 - 31 décembre 1959* (2/2/60), 11. Arch. IHÉS.
[90] Interview of A. S. Wightman by the author (28 November 1997). Hearing complaints about the lack of a library, Grothendieck is said to have declared: "We don't read books, we write them!"

The goal of this journal is to allow the very rapid publication of original memoirs of a great value."[91] In February 1960, two issues had been published and three more were in press, including Grothendieck's famous *Éléments de géométrie algébrique,* already planed to occupy 1500 pages in several issues of the *Publications mathématiques.* Algebra was the main topic represented in its pages.

In 1960, according to Motchane's Scientific Report, Grothendieck continued his weekly seminar on algebraic topology, in front of an audience of about thirty persons, including Claude Chevalley, Jean-Pierre Serre, Oscar Zariski, and Jean Dieudonné, and many young mathematicians. Proudly, Motchane underscored that "Paris—and more especially the Institut des hautes études scientifiques, the very place where [algebraic geometry] is created—has become the center of this branch of mathemetics." At the IHÉS, In 1960, several other conferences, mostly on algebraic topics, were held by important mathematicians: S. S. Chern, M. Atiyah, A. Weil, J. Tits. Moreover, Claude Chevalley, in abstentia from the Faculté des Sciences de Paris, spent the year 1960-1961 as a visitor to the IHÉS, where he delivered weekly lectures on "algebraic structures in categories."[92] Twelve mathematicians thus spent part or the entirety of the year at the IHÉS, including future Fields medalist Hironaka, and Lavrentiev, the Vice-President of the Soviet Acedemy of Sciences, who came from three weeks in order to establish scientific relations between his Academy and the IHÉS.[93]

It was only in the following year, 1961, that a certain diversification in mathematical topics occurred. Algebra ceased to occupy almost all the place. With Hans

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[91] *Rapport scientifique, 4 juillet 1958 - 31 décembre 1959* (2/2/60), 3. Arch. IHÉS.
[92] *Rapport scientifique sur l'activité de l'IHÉS en 1960* (5/5/61). Arch IHÉS.

Grauert, analysis and especially the study of several variable complex functions was emphasized by Motchane in the Scientific Report for 1961. The director of the IHÉS described the accomplishments in this domain as being similar in kind to Grothendieck's in algebraic geometry,

> namely to achieve an axiomatic exposition of these new theories and some kind of codification of methods which would allow young mathematicians to go ahead in this direction and exploit it sucessfully.

The publication of Grauert's new exposé was envisioned as part of the *Publications mathématiques.* Thus, Motchane particularly welcomed the fact that two branches of mathematics were in great development and especially that the IHÉS was "at the same time the center of research and the focus of diffusion in these two domains."[94]

Even at the very beginning, although it certainly exhibited an elitist character, and a focus on elementary structures, the IHÉS was difficult to classify according to the Bourbakist/anti-Bourbakist fault line. Indeed both clans allied in support of the IHÉS. Although tensions were perceptible even within the Scientific Committee, this circumstantial alliance persisted for many years.[95] Apparently, Motchane himself was responsible for having imposed this conciliatory attitude in the face of divisions within the French mathematical community: "it is not at all the intention of the Scientific Committee to suggest that the activity of our Institute is devoted to a single branch of mathematics, that specialization is pushed to the extreme, and that this Section is

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93 *Organisation et activités scientifiques* [1958-1961] (15/1/61), 5. Arch. IHÉS.

94 *Rapport scientifique sur l'activité de l'institut des hautes études scientifiques en 1961,* 5. Arch. IHÉS.

95 As a testimonial of tensions not often spelt out, see e.g. lettre de Léon Motchane à Jean Dieudonné (16/12/58). Arch. IHÉS.

dominated by a *chapelle*."[96] This last term, to be understood in the sense of *clique*, was then commonly used to designate different subgroups of the French mathematical community.

c) Setting up Theoretical Physics in France

The organization of the theoretical physics section of the IHÉS progressed more slowly. Besides rather vague support from prominent members of the international physics community, nothing had been set up before the foundation of the Institute. Many factors explained why the organization of this section presented greater difficulties.[97] First, Motchane himself had received his Ph. D. in mathematics, and, even though he was also trained in physics, and had published a few notes in this field, his main interests remained mathematical. Second, France's international position was much stronger in mathematics than it was in physics, especially in its theoretical branch. Motchane thought that the French situation in theoretical physics was especially poor, and, besides Francis Perrin (who also headed the CEA which was to be an important sponsor of the IHÉS), he does not seem to have pursued the collaboration of French physicists as much as that of others.[98] Finally, by the late 1950s, many career tracks, alternative to the University, were already in place for French theoretical physicists, namely at the CEA (closed to pure

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[96] *Rapport scientifique sur l'activité de l'IHÉS en 1960,* 6. Arch. IHÉS.
[97] *Rapport Euratom* (March 1959), 23. Arch. IHÉS.
[98] According to him, "on ne trouvait guère en France de physiciens théoriciens de valeur exceptionnelle, . . . ce qui ne facilite pas aujourd'hui la création à notre Institut d'un groupe de travail." *Rapport scientifique, 4 juillet 1958 - 31 décembre 1959* (2/2/60). *Commentaires,* pour la conférence de presse (11/7/58), 2. Also: "retard inquiétant de notre pays en physique théorique," *Note pour les industriels* (Mai 1958), 2. Arch. IHÉS. Comp. to Chapter IV.

mathematicians), and the CNRS (which mathematicians generally did not take full advantage of).[99]

Before October 1958, Motchane did little to set up the physics section. He then traveled to the US, the USSR, Denmark, and Italy, and took counsel with several physicists from many countries (esp. Germany, Britain, and of course France).[100] He tried, without success, to get some of them to come and work at the Institute as early as 1959 (Wigner, Weisskopf, Pauli).[101] A *Publications de physique théorique* was also envisaged, starting with review papers (of the Soviet *Uspekhi* type), with Léon Rosenfeld as the editor.[102] This never materialized.[103]

On February 21-24, 1959, this exploratory phase culminated in an international meeting of European physicists held in Paris, specifically organized by Motchane in order to help him define a general policy for setting up the physics section of the IHÉS.[104] From this meeting, three main points emerged, which seemed to go against some of the

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99 "Les mathématiciens paient actuellement, par une place très modeste au CNRS par rapport à d'autres disciplines, le civisme dont les mathématiciens ont fait preuve lors de la crise de recrutement des années 60 en s'engageant massivement dans l'enseignement." Jean-Pierre Aubin, ed., "Rapport préliminaire du VIIe Plan. Groupe GS6: Mathématiques et méthodologies mathématiques par la DGRST," *Gazette des mathématiciens,* no. 4 (1975): 13-26, 14. As late as 1980, only 6% of mathematicians were employed by the CNRS. "Schéma directeur du CNRS - Chapitres mathématiques," *Gazette des mathématiciens,* no. 15 (1980), 93-97.

100 The *Rapport scientifique 1958-1959* (9/2/59) lists several national "groups" of physicists with whom Motchane was discussing.

101 Lettre de Léon Motchane à Robert Oppenheimer (8/10/58); de Léon Motchane à Francis Perrin (20/11/58); de Léon Motchane à Robert Oppenheimer (12/12/58).

102 Lettre de Léon Motchane à Francis Perrin (20/11/58). Arch. IHÉS.

103 *Comité scientifique* (17/9/59). Arch IHÉS.

104 It gathered good, but not top-notch physicists: Abragam (CEA), Amaldi (Rome), Rosenfeld (Copenhagen), Källén (Lund), Kemmer (Edinburgh), and Guéron (Euratom). Cf. *Compte-rendu de la réunion consultative de physiciens* (21-24/2/59); lettre de Léon

orientations defined by Motchane for his institute. Physicists, more than mathematicians, cared about the spatial organization of the Institute, in terms of office space, library, etc., and its localization in the proximity of laboratories and particle accelerators (Orsay).[105] Already at the foundation meeting, on June 27, 1958, inspired again by Anglo-American traditions and the IAS in particular, it was stated that the permanent installation of IHÉS should be in a suburb to the southwest of Paris, close to the projected "campus" of Orsay/Gif-sur-Yvette and the CEA's laboratories at Saclay.[106] Anatole Abragam insisted:

> the proximity of the [IHÉS], devoted to abstract and theoretical research, to experimental physics centers equipped with modern materiel and directed by eminent experimental physicists, has a much deeper significance than just a topographical vicinity.[107]

The second point to emerge was that these physicists insisted on the necessity for established people to have young researchers around. They agreed with the principle that no teaching activity would be required from the professors, but thought (*dixit* Källén) that "the presence of valuable young persons, by the curiosity of their mind and their unexpected ways of tackling problems, might act as a stimulus."[108] A principle was adopted, which would remain an important policy of the Institute, to invite "not-yet-

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Motchane à Robert Oppenheimer (4/4/59); *Rapport scientifique, 4 juillet 1958 - 31 décembre 1959* (2/2/60). Arch. IHÉS.

105 "L'importance qu'attachent les Physiciens à l'installation matérielle de l'Institut s'explique par leurs habitudes de travail. Contrairement aux Mathématiciens dont les recherches gardent traditionnellement un caractère individuel, le travail en groupe est devenu une règle courante chez les Physiciens; ceux dont les recherches portent sur un même sujet éprouvent le besoin d'être constamment en communication; ils doivent pouvoir se réunir fréquemment et dans de bonnes conditions." *Rapport scientifique sur l'activité de l'IHÉS en 1960* (5/5/61), 7. Arch. IHÉS.

106 Another nearby site considered at the time of the foundation was close to Villacombley airport, of which no mention is made later. *Notes de séance manuscrites de Annie Rolland de la séance de Fondation de l'IHÉS* (27/6/58). Arch. IHÉS.

107 *Compte-rendu de la réunion consultative de physiciens* (21-24/2/59). Arch. IHÉS.

established young researchers, whose first works—for example the doctorate—might foreshadow a promising future."[109] By bringing young researchers to the IHÉS, this policy had an important consequence in allowing research traditions to grow out of the IHÉS.

In his written reports, Motchane did not emphasized as much a third point he had taken away from the physicists' meeting since it confirmed his own elitist conception. A policy of "pivots," or " centers of attraction," should be adopted, the physicists thought: personalities coming for a relatively long period, around whom a group could be formed.[110] Eugene Wigner, Richard Feynman, Victor Weisskopf, Res Jost, Arthur Wightman, and Leon van Hove were mentioned as possible pivots. Aside from Feynman, all of them indeed contributed to the IHÉS, which most would often visit.

> We will note that the organization of the Physics Section at first will take a different shape from the Mathematics Section. The work will be organized on the basis of teams of temporary professors. It is likely that this method will reveal which of these researchers who will be able, in due time, of occupying the position of permanent professor.[111]

Inviting temporary "pivots" while gathering teams devoted to a specific research subject was one strategy; finding the right personalities and hiring them as permanent faculty was another; fixing some research topics to be investigated was still another. Where should the emphasis be placed in the building of the section? Motchane always

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[108] *Compte-rendu de la réunion consultative de physiciens* (21-24/2/59). Arch. IHÉS.
[109] Lettre de Léon Motchane à Robert Oppenheimer (4/4/59). Arch. IHÉS.
[110] Once this term had been introduced in the scientific literature by Thom and Ruelle, the pivots will often be designated as "attractors." For example, lettre de Nicolaas Kuiper à E. C. Zeeman (29/2/72); mémo de Nicolaas Kuiper (13/9/72). Arch. IHÉS.
[111] *Rapport Euratom* (Mars 1959), 26. Arch. IHÉS.

wavered between strategies, the most important goal remaining the establishment of a favorable international reputation.

In April 1959, particle physicist E. R. Caianiello, from Naples, initiated the work of the physics section of the Institute. In May, Motchane wrote Oppenheimer that he was collaborating with a young physicist from the École polytechnique and Orsay, Louis Michel.[112] Michel was drafted by Motchane to help him organize invitations. In September, together with Leon van Hove (Utrecht), Res Jost (ETH, Zurich), and Murray Gell-Mann (Caltech), Michel was named as a permanently invited professor.[113] He would in due time become, in the official record, the IHÉS's first permanent professor of theoretical physics. But before this happened, Motchane almost succeeded in attracting physicists who would have instantaneously put the IHÉS at the center of this discipline.

On March 1, 1960, Motchane, manifestly excited, wrote to Oppenheimer: "Important events—and favorable ones for our Institute—are about to happen."[114] The first of these events was that two physicists seemed interested in coming permanently to the IHÉS: Harry Lehmann (Hamburg), and Murray Gell-Mann.[115] These were impressive

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112 Lettre de Léon Motchane à Robert Oppenheimer (21/5/59); de Louis Michel à Léon Motchane (12/6/59). Arch. IHÉS. Louis Michel was then teaching a course on the μ-meson at the Collège de France. Assemblée des professeurs (16/2/58), Arch. CdF G-iv-m 30D.

113 Van Hove was later offered a permanent position at the IHÉS, which he declined. Lettre de Léon Motchane à Robert Oppenheimer (11/7/61); *Rapport scientifique sur l'activité de l'IHÉS en 1960* (5/5/61), 8. Lettre de Louis Michel à Léon Motchane (10/12/59); *Comité scientifique* (17/9/59). Arch. IHÉS.

114 Lettre de Léon Motchane à Robert Oppenheimer (1/3/60). Arch. IHÉS.

115 Gell-Mann's name, which was not on the agenda, first came up as a possible permanently invited member at the *Comité scientifique* (17/9/59), attended by Oppenheimer. First contact was established when he visited the Collège de France in December 1959 as a Fullbright scholar. Assemblée des professeurs (16/2/58); Arch. CdF, G-iv-m 30D. Lettre de Léon Motchane à Murray Gell-Mann (9/12/1959). Harry Lehmann

prospects. Of Gell-Mann, Oppenheimer wrote that he was "universally recognized as one of the very most brilliant theoretical physicist in the world;" of Lehmann, that he "almost single-handed[ly] was responsible for the revival of a high tradition of theoretical physics in Germany."[116] For Victor Weisskopf, both perfectly fulfilled his own requirements for permanent members: "first, a thorough knowledge in the field; second, a proof of fundamental activeness and leadership; and third, young age so that we can hope that this creativeness is not yet exhausted."[117] The Scientific Committee and the Administrative Board of the IHÉS approved their nomination in the summer of 1960.[118]

Negotiations with Gell-Mann went slowly, and stumbled on several points: the legal status of the Institute, assurance of its long-term survival, delays with the permanent location at Bures-sur-Yvette, and salary matters.[119] Still, for "moral reasons," Gell-Mann was attracted by Paris. "He knows our language well. He is fond of our culture, and what is perhaps more important, the moral climate reigning in France attracts him

was suggested by Res Jost in 1959. Lettre de Léon Motchane à Robert Oppenheimer (2/10/59); de Léon Motchane à Rudolph Peierls (3/12/59). Arch. IHÉS

116 Lettres de Robert Oppenheimer à Léon Motchane (17/5/60). Arch. IHÉS. Original English.

117 Lettre de Victor Weisskopf à Léon Motchane (16/5/60). Arch. IHÉS. Original English.

118 *Comité scientifique* (3/6/60); lettres de Robert Oppenheimer à Léon Motchane (15/5/60); télégramme de Léon Motchane à Robert Oppenheimer (21/7/60). Arch. IHÉS.

119 A position of advisor at the CEA was arranged for Gell-Mann. Lettre de Léon Motchane à Jules Guéron, Euratom (13/6/60); de Léon Motchane à Victor Weisskopf (27/7/60); télégramme de Léon Motchane à Murray Gell-Mann (11/7/60); lettres de Léon Motchane à Robert Oppenheimer (26/7/60); de Francis Perrin à Murray Gell-Mann (28/7/60); de Léon Motchane à Murray Gell-Mann (5/10/60). Arch. IHÉS.

particularly."[120] Nevertheless, for several reasons, he was also reluctant to leave the US, and ultimately declined Motchane's offer.[121]

Harry Lehmann's case is intriguing since, although this fact is never officially mentioned in Motchane's yearly *Scientific Reports,* he was considered a permanent professor of the Institute from October 1962, when he first came to the IHÉS, until March 1963.[122] He then probably stated his intention of leaving to go back to Hamburg after 1964-65, which made him migrate into the visitor column.[123] Yet Lehmann actually was the first permanent professor of physics of the IHÉS. It is true that already in October 1962, when Lehmann arrived, a second permanent position had been offered to Louis Michel, who was already settled in at the Institute with a status similar to that of a

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[120] Lettre de Léon Motchane à Victor Weisskopf (27/7/60). Lettre de Murray Gell-Mann à Léon Motchane (21/7/60): "un offre aussi tentant [*sic*]. Il me faut réfléchir un peu." Arch. IHÉS.

[121] "It has been a most difficult decision to make; it was necessary to weigh the manifold attractions of the Institute, of Paris, and of my friends there against the ties that bind me to the United States and to Caltech." Lettre de Murray Gell-Mann à Léon Motchane (27/6/61). Original English. And lettre de Murray Gell-Mann à Léon Motchane (12/9/60); de Léon Motchane à Robert Oppenheimer (13/2/61); de Léon Motchane à Victor Weisskopf (9/3/61). It moreover seems that some difficulties came up from Motchane's part. On September 1, 1960, Oppenheimer cabled: "ALL EFFORTS IN OBTAINING PERMANENT PROFESSORS IN PHYSICS WILL SURELY FAIL UNLESS YOU PROMPTLY CARRY OUT YOUR PROGRAM OBTAINING RECONNAISSANCE DUTILITE PUBLIQUE AND ACQUIRE BOIS MARIE STOP BELIEVE FAILURE AT THIS POINT WOULD HAVE SERIOUS EFFECTS ON THE FUTURE OF PHYSICS AT THE INSTITUTE STOP WOULD MYSELF ADVICE THAT YOU WELCOME CONCURRENT APPOINTMENT AT UNIVERSITY AND SUPPLEMENTARY SALARY WHERE NEEDED STOP BEST GREETINGS ROBERT OPPENHEIMER." Arch. IHÉS. Original English.

[122] Lettre de Léon Motchane à Robert Oppenheimer (13/2/61); de Léon Motchane à Murray Gell-Mann (16/1/62); de Léon Motchane à Victor Wisskopf (24/3/1963); *Rapport scientifique sur l'activité de l'IHÉS en 1962* (30/4/63), 4-5. Arch. IHÉS.

[123] Lettre de Léon Motchane à Robert Oppenheimer (6/5/65): "Harry Lehmann après 3 ans à l'IHÉS retourne à Hambourg pour des raisons personnelles." Arch. IHÉS.

permanent member.[124] This was decided, at the suggestion of Francis Perrin, when it became clear that Gell-Mann would not come to the IHÉS.[125]

With Michel's nomination, the IHÉS's role in promoting the revival of French theoretical physics was confirmed. Together with Michel, a group of young French theoreticians with strong mathematical and philosophical interests temporarily staffed the Institute in 1961-63, before building more traditional careers: Roland Omnès, Jean Lascoux, and François Lurçat.[126] Michel's appointment however created some "tension" with Orsay. Following this, the dean Maurice Lévy "has always been a 'bitter foe' (his own word[s] in a letter to Oppenheimer who showed it to me) of the IHÉS since the creation of this Institute. For years he tried to annoy us."[127] Nevertheless, good relations were ultimately established with both the physicists and the mathematicians of the University at Orsay, and with the physicists at Saclay: "visits are constant and there is not a week when physicists [like] Froissart, Fotiadi, Stora, Messiah, etc. do not come to our table."[128]

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[124] Lettre de Léon Motchane à Robert Oppenheimer (10/1/61). Arch. IHÉS.

[125] Lettre de Léon Motchane à Robert Oppenheimer (9/3/61); de Léon Motchane à Victor Weisskopf (9/3/61); de Léon Motchane à Robert Oppenheimer (30/3/61); de Léon Motchane à Louis Michel (8/6/61); de Léon Motchane à Robert Oppenheimer (11/7/61); de Louis Michel à Léon Motchane (25/7/61); de Louis Michel à Joseph Pérès (27/7/61). Arch. IHÉS.

[126] Lettre de Raoul Omnès à Léon Motchane (25/7/61). Arch. IHÉS.

[127] Lettre de Louis Michel à Nicolaas Kuiper (10/11/76) avec post-scriptum (12/11/76). Original English. Cf. lettre de Léon Motchane à Robert Oppenheimer (11/5/60). Arch. IHÉS.

[128] Concernant les "relations qui se sont établies avec la Faculté des Sciences[,] cette collaboration . . . a eu – vous vous en souvenez certainement – des débuts difficiles qui avaient pour origine une certaine méfiance de l'Université envers un Institut privé nouvellement créé. À notre grande satisfaction, tous ces nuages se sont complètement dissipés." *Rapport scientifique sur l'activité de l'IHÉS en 1962* (30/4/63), 4-6; *Rapport scientifique sur l'activité de l'IHÉS en 1963* (14/1/64), 2. Arch. IHÉS.

d) Theoretical Physics or Mathematical Physics?

With Michel, and especially Lehmann, the physics done at the IHÉS took a particular flavor. In line with Motchane's early ideas about the physical problems susceptible of being treated, the emphasis was clearly put on the structure of matter, on elementary particle physics, but with a strong mathematical bent.[129] Indeed, one of the first successes of the IHÉS in physics was the gathering, in 1963-64, of Arthur Wightman, Harry Lehmann, Res Jost, Julian Schwinger, Vladimir Glaser, assisted by the young Henri Epstein, Arthur Jaffe, and Oscar Lanford, "the strongest team in the world in quantum field theory."[130]

Thus the theoretical physics done at the Institut des hautes études scientifiques took the shape of *mathematical physics.* I use the label 'mathematical physics' here to characterize a kind of physical practice which emphasized rigorous foundations of existing physical theories rather than the elaboration of new ones on the basis of empirical results. Mathematical physics could also use their important knowledge of contemporary mathematics as a way to draw special attention to basic structures of theories.[131]

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[129] Let me note here that Michel, whose main work concerned the application of group theory to particle physics, was invited to give a half-hour talk on this topic at the Moscow International Congress of Mathematicians in 1966. Lettre de I. G. Petrovskii à Louis Michel (20/10/65). Arch. IHÉS. See L. Michel, "Théorie des groupes et particules élémentaire,." *Proceedings of International Congress of Mathematicians (Moscow - 1966)* (Moscow: Mir, 1968). More than by Bourbakist (see Chapter 2), the Moscow Congress seemed to have been dominated by the IHÉS.

[130] *Rapport scientifique sur l'activité de l'IHÉS en 1963* (14/1/64), 4; *Éléments de rapport scientifique à l'assemblée* [1964] (10/2/66). Arch. IHÉS.

[131] The distinction between theoretical physics and mathematical physics has been discussed by various authors. See, in particular, D. Pestre, *Physique et physiciens,* 111ff; J.-L. Destouches, *Qu'est-ce que la philosophie mathématique?* (Paris: Gauthier-Villars, 1967), 10-12, and G. Israel, "Vito Volterra: un fisico matematico di fronte ai problemi della fisica del Novecento," *Rivista di storia della scienza,* 1 (1984): 39-72; *La*

In particular, the regular visits of Arthur S. Wightman, leader in the study of the axiomatic foundations of quantum field theory, imprinted IHÉS physics. About Wightman, Oppenheimer already wrote in 1959, "I am fairly confident that you will see a good deal of him."[132] Indeed, in the United States, this mathematical tendency for theoretical physics had less success, much to the benefit of the IHÉS. In 1968, Michel wrote from Princeton that: "Arthur [Wightman] is a great physicist. Too bad for the United States if he is not in fashion here. We will be able to have him often at Bures."[133]

Despite some efforts to counter this mathematical specialization of IHÉS theoretical physics, it was nothing but advanced when David Ruelle joined the permanent faculty in 1964.[134] This aspect of the physics promoted at the IHÉS is dealt in more details in Chapter VII below, when the conditions of his hiring are examined. For the moment, let me only note that an emphasis on mathematical physics does not necessarily imply that contacts between mathematicians and physicists were taking place, or were encouraged by the Institute. In fact, if the relative smallness of the IHÉS might have favored contacts between mathematicians and physicists, no coherent effort seemed to have been made to invite these contacts. The physics done at the IHÉS was mathematical, but this hardly meant that it was done by mathematicians, that it was done in contact with mathematicians, or even that it had anything to do with the fact that the IHÉS also included a mathematics section. Nor did it mean that mathematicians at the IHÉS were

Mathématisation du réel (Paris: Seuil, 1996). I deal with this again when I talk about David Ruelle's work in Chapter VII below.

132 Lettre de Robert Oppenheimer à Léon Motchane (26/5/59). Arch. IHÉS.

133 Lettre de Louis Michel à Léon Motchane (10/5/68). Similarly, "le fossé entre axiomatistes et les autres ne se comble pas, au contraire!" Lettre de Louis Michel, de Rochester, NY, à Léon Motchane (31/8/67). Arch. IHÉS.

more inclined to do some research in physics. With Grothendieck and Dieudonné, a Bourbakist ideology of purity surely reigned at the IHÉS.

With pains, a climate potentially conducive to a cooperation between physicists and mathematicians was nonetheless established at the IHÉS. On the basis of some of Motchane's statements in any case, the corporate members of the association were justified in believing that this cooperation was desirable for their institute. Whether it did happen was another story. Significantly, Motchane thought that the fact that mathematicians René Thom and Christopher Zeeman, a frequent visitor of the Institute, manifested some interest for physics in the early 1960s, was worth mentioning to the General Assembly. "With Thom, we see a renewal of analysts inclined for physics."[135]

At the General Assembly, on May 8, 1968, Jacques Ballet, president of Esso-Standard, remembering the original hopes for collaboration, asked Motchane: "Is there an osmosis between the two domains [physics and mathematics]?"[136] Embarrassed, Léon Motchane mumbled:

> The essential [point] for this kind of Institute is the climate that reigns. Concentration of gray matter, thanks to a climate of permanent human contacts. Everybody live together. Offices gathered in the same building, [small] Cafeteria, Tea... The whole [point] is to create a tradition. . . . It is difficult to put a number on this, but this is essential.

Overall, the concrete balance sheet that Motchane could show in response to Ballet's query was rather short: "Original work done by a mathematician in theoretical physics. A polytechnician physicist followed Thom's seminar (Math. Doctorate)." Then

[134] *Éléments de rapport scientifique [1964] à l'Assemblée* (10/2/66), 3. Arch. IHÉS.
[135] *Notes de séance manuscrites de Annie Rolland,* Assemblée Générale (23/9/64). Arch. IHÉS.

describing the work done at the IHÉS, Motchane said that this was a "difficult period" for theoretical physics. Between mathematics and physics, "specialization forbids communication."[137]

Only three years later, the situation had changed dramatically. On June 2, 1971, delivering his last scientific report to the General Assembly before he resigned as director of the IHÉS, Motchane proudly claimed that "the IHÉS [was] one of the very scarce places where physicists and mathematicians fruitfully talk to one another."[138] This reversal was due to many factors including Ruelle's and Grothendieck's changes of interest, but mainly to the attractiveness of a school of qualitative dynamics set up by René Thom.[139]

In 1968, Motchane already perceived some of the sources for the reversal. To the Administrators, he explained that, with the recent recognition of the importance of mathematical structure, a new generation of "universal mathematicians" was emerging which offered the hope of a renewed dialogue between mathematics and other sciences. "The present period is exceptionally interesting." At the IHÉS, there were "very interesting attempts, which could bring the IIIrd section about."[140] Let us now look at the unbroken chain of failures forming the history of the humanities section of the IHÉS.

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136 *Repartition entre physiciens et mathématiciens* (transcription des notes de séances manuscrites d'Annie Rolland, Assemblée générale [8/5/68]). Arch. IHÉS.
137 *Ibid.*
138 *Assemblée générale* (2/6/71)
139 I will deal with these matters in more details in Chapter VI and VII below.
140 *Ibid.*

4. 'PHYSICO-MATHEMATICAL' METHODOLOGY OF THE SCIENCES OF MAN?

In his report to Euratom, written in March 1959, Léon Motchane breached the subject of the Third Section of the IHÉS, purportedly devoted to the study of the methodology of the sciences of man. Again, the model was the IAS, but, here, "in the study of the human sciences, replacing Princeton's Historical and Archeological School, the emphasis is put on the methodological aspect in the European center."[141] While it may tempting to interpret this move away from history towards methodology as an early effect of the structuralist wave, Motchane's connection with the corporate and government worlds might more accurately account for the plans he drew for the Third Section.[142]

To understand Motchane's strategy with regard to the humanities, we must notice that this Section could serve to attract the support of businessmen. If fundamental research in mathematics and theoretical physics could offer the hope of solving the energy problem (or at least be presented as offering this hope), the Third Section could be seen as addressing the social challenges of the day. As Motchane wrote, it was on these issues that the industrialists' expertise might profitably complement the scientists'. As he wrote to René Grandgeorge, from the Saint-Gobain corporation:

> The idea that seduced eminent scientists, and the cultivated public opinion in general, was to found an Institute of advanced research, a very independent one, entirely sponsored by large corporations. . . . These are companies which . . . everyday must face scientific research problems and human problems of social organization. . . . [It will be] necessary that *a human contact between the corporate executives,* which will be the founders and the energizers of this

[141] *Rapport Euratom* (Mars 1959), 10. Arch. IHÉS.

[142] 1958 was the year Lévi-Strauss was elected to the Collège de France, and published *L'Anthropologie structurale;* 1959 saw the two conferences mentioned in chapter II above.

> organization on the one hand, *and the scientists*, which will lend it its scientific value and caution on the other [be established and maintained]. We thus conceive that the appearance of the section Methodology of the Sciences of Man, besides that of Mathematics and Theoretical Physics is not at random.[143]

On June 22, 1959, without having been previously introduced to him, and contrary to his usual manners, Léon Motchane wrote to Gaston Berger, who was *directeur honoraire de l'Enseignement* and a member of the Academy of Moral and Political Sciences, to talk about the Third Section of the IHÉS. Candidly, he admitted the difficulty he had with its organization:

> The humanities section, where the emphasis is put on the study and the confrontation of methods, might possibly be led to benefit from its scientific neighbors; but scientific disciplines will certainly be stimulated by problems posed in very different domains. . . . The organization of the first two sections presented no major difficulty, intellectually speaking. In the third section, I come against a great number of [difficulties]![144]

Berger's work, Motchane indicated, had given him some inspiration as to how to proceed.

> It seems to me that the methodological concerns that appear in 'Prospective' proceeds from tendencies analogous to ours. The posing of the question [of the methodology of the human sciences, an issue indirectly raised by *Prospective*] seemed new to me and capable of leading to profound research.[145]

Prospective was a journal, a think tank (like the IHÉS, a nonprofit association chartered under the 1901 law), and more generally an "attitude." On May 10, 1957, a group of men, from industry, State administration, and university, gathered in Paris and founded the *Centre international de prospective*. In their own words, this was a "group formed for the *study* of technical, scientific, economic, and social causes which accelerate the evolution of the modern world, and for the *prevision* of situations that could derive

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143 Lettre de Léon Motchane à René Grandgeorge (15/10/58). Arch. IHÉS. My emphasis.
144 Lettre de Léon Motchane à Gaston Berger (22/6/59). Arch. IHÉS.
145 Lettre de Léon Motchane à Gaston Berger (22/6/59). Arch. IHÉS.

from their interconnected influences."[146] Instead of ideology, methods, or philosophy, it put forward what members called an "attitude" as a way to tackle the pressing problem of man's place in a rapidly changing society.[147]

A taste for action and efficiency permeated this attitude; this was a technocratic vision of ways to conceptualize the social problems of tomorrow, and find means to foresee and face them. Therefore, this emphasis put on action, rather than understanding, made the prospective project, albeit not explicitly so, quite an anti-structuralist one.

> Each of us has one's own different and limited view of this immense and unique world where we live our existence. To be prospective is to unite these heterogeneous visions, to project them together towards the future, . . . but by raising to the human plane the problems touched, by refusing the rigidity of a purely intellectual attitude, deprived from sensitive accents.[148]

On the social rather than scientific level, this enterprise—the *Centre international de prospective*—almost exactly corresponded to the IHÉS. As we might expect, their memberships overlapped. Among the six vice-presidents of the Centre three took special care of Motchane's institute.[149]

Strikingly, while Motchane envisaged a Third Section adapting mathematical and physical methods to the social sciences, the Prospective Centre devoted much of its attention to the issues of the social consequences of scientific and technological progress.

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[146] *Extrait des statuts du Centre international de prospective,* published page numbered separately with *Prospective,* 1 (May 1958), and 5 (May 1960), 1. My emphasis.
[147] Gaston Berger, "Préface. L'attitude prospective," *Prospective,* 1 (May 1958): 1-10. See also Marcel Demonque, "Quelques réflexions prospectives sur le monde industriel de demain," and François Bloch-Lainé, "Vue prospective sur les problèmes économiques," in *ibid.,* 25-35 and 85-97.
[148] Louis Armand (president of Euratom) to the Administrative Board of the Centre international de prospective (11/12/57); quoted in *Prospective,* 2 (January 1959), ii.

The second issue of the journal *Prospective* was devoted to "The General Consequences of the New High Technologies," the fifth to "Scientific and Technological Progress and the Condition of Man."[150] They closely followed the debates at two congresses that took place in September, 1958, on the Peaceful Uses of Atomic Energy, held in Geneva, and on cybernetics, held in Namur.[151] They invited Oppenheimer in April 1958, and published one of his texts in their journal.[152]

On September 18, 1959, Motchane, Berger, and Oppenheimer met to discuss the organization of the IHÉS's Third Section. Other such meetings apparently were held, including the economist Pierre Masse.[153] But with the death of Berger in a car accident, late in 1960, the cooperation between the Prospective group and the IHÉS seems to have ended before it really started.

As with the Physics Section Motchane adopted a two-pronged strategy for the constitution of the Third Section. While drawing attention to these meetings of experts, as well as planning new ones involving "Philosophers, Sociologists, Economists and Anthropologists," he also tried to attract one or two internationally renowned scholars

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149 Arnaud de Vogüé (President of Saint-Gobain, treasurer of the IHÉS), François Bloch-Lainé (General director of the Caisse des Dépots), and Louis Armand (Euratom), already mentioned.
150 *Prospective,* 2 "Conséquences générales des grandes techniques nouvelles" (January 1959); *Prospective,* 5 "Le progrès scientifique et technique et la condition de l'homme" (May 1960).
151 Georges Guéron, "Observations à propos de la Seconde Conférence internationale des Nations Unies sur l'utilisation de l'énergie atomique à des fins pacifiques," *Prospective,* 2 (January 1959): 13-21; and G. Guéron, "Observations à propos du II[e] Congrès international de cybernétique tenu à Namur, du 3 au 14 septembre 1958," *ibid.:* 59-64.
152 See "Avant-propos," *Prospective,* 2 (January 1959): 1-9,6; and Robert Oppenheimer, " Science, culture et expansion," *Prospective,* 5 (May 1960): 79-88. The journal *Prospective* became, in 1976, *Les Futurribles.*

around whom the Section might develop. Several names were considered: biologist Roger Guillemin (suggested by Ponte), art historian Charles de Tolnay (suggested by Weil), and even Benoît Mandelbrot (suggested by Oppenheimer)![154] This list, better than any explanation, clearly shows how imprecise Motchane's ideas were.[155] The most baffling feature, however, is that these suggestions all came from scientists, and not specialists in the social sciences. As a result, De Tolnay twice visited the Institute[156]; Mandelbrot, whom Motchane had visited in the US, sent him his CV; but no one was appointed.

Another serious candidate was proposed in 1961: philosopher and historian of science Gilles Gaston Granger, who won Motchane's and Weil's support. Granger was an epistemologist, who studied the way abstract human thought was structured. He mainly planned to "define with precision the notion of *style,* justly considered as the mode of insertion of structures in concrete, individual existence."[157] But once again early contacts led nowhere.

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[153] *Organisation des sections* (n.d., 1960); *Rapport scientifique sur l'activité de l'IHÉS en 1960,* 8-9. Arch. IHÉS.

[154] Lettre de André Weil à Léon Motchane (2/3/60); de Léon Motchane à André Weil (8/3/60); de Maurice Ponte à Léon Motchane (7/3/60); *Entretien avec Maurice Ponte* (29/3/60); Lettre de Robert Oppenheimer à Léon Motchane (19/8/60); de Léon Motchane à Robert Oppenheimer (24/8/60).

[155] "Notre section des humanités n'est pas encore organisée et nous n'avons même pas une conception claire de ce qu'elle devrait être. Je pense pour ma part qu'il faudrait préférer aux sujets des personnalités." Lettre de Léon Motchane à André Weil (8/3/60). Arch. IHÉS.

[156] Charles de Tolnay gave two seminars at the IHÉS on 22 and 29 June, 1961: "Les conceptions scientifiques de Léonard de Vinci dans ses œuvres d'art;" and two more on 15 and 22 June, 1962: "Les conceptions religieuses dans la peinture de Piero della Francesca."

[157] Lettre de Léon Motchane à Robert Oppenheimer (27/11/61); de Léon Motchane à Robert Oppenheimer (24/2/62); *CV et Projets actuels* de Gilles Gaston Granger. Arch. IHÉS.

It thus appears that even if Motchane's plans remained fuzzy, a tendency emerged to envision the study of the methodology of the sciences of man as closely intertwined with other scientific concerns at the IHÉS. Social scientists were either to approach their subject with highly mathematical methods, or at least to think of their field as addressing issues of interactions between society and scientific advances.

> It is important to provide the opportunity for a collaboration between scientists and humanists, equally concerned with methodological questions and oriented towards fundamental problems," Motchane wrote, "in the hope of witnessing the emergence of new modes of research, and methods, notably in History and Sociology.[158]

How much of this remained mere rhetoric in order to entice possible sponsors is unclear.[159] Except for the year 1960, Motchane never mentioned the activity of the Third Section in any of the Scientific Reports he wrote during the 1960s.

President of the IHÉS after Pérès's death, André Grandpierre convoked two General Assemblies in 1964 to study the possibility of an American participation in the funding of the institut. On these occasions, the problem of the Third Section was raised by the members. Léon Kaplan, as always the black sheep, thought: "You will have no perenniality, no success, unless this team is set up." For Jacques Ballet, this section had to be "first rank or not at all."[160] Grandpierre suggested to "invite 1 or 2 men of a very high quality, without giving them a topic, and by seeing what they give, you may perhaps find your way." Fernand Picard, from Renault, interrupted: "No Third Section! Let us derive

[158] Lettre de Léon Motchane à Shepard Stone, Director of Sloan Foundation (23/5/63). Arch. IHÉS.
[159] Cf. *Entrevue* André Grandpierre et Léon Motchane (20/3/63) pour préparer une entrevue [qui n'aura pas lieu] avec Hallstein, Président de la Commission de la CEE: "situer l'activité de l'IHÉS en insistant sur la IIIe section."
[160] *Notes de séance manuscrites,* Assemblée générale (14/1/64). Arch. IHÉS.

the maximum from physics and mathematics. The Third Section will be the climax" of the enterprise.[161] In face of the dire financial situation of the Institut, this probably was the only reasonable course to follow.

The pressure for creating the Third Section was off Motchane's shoulders for a few years. When he would take up the plans again, after his own retirement from directorship in 1971, the internal situation would have changed. By then, Thom's school of qualitative dynamics was in full swing. Indeed, besides de Tolnay in 1961-62, there had been only one scientist invited as part of the Third Section, and it was Conrad Hal Waddington in 1966.[162] No doubt he had been invited by Thom, who, on Monday, May 2, 1966, had given a talk, titled: "Topologie comparée de la gastrulation chez les vertébrés."[163] While Motchane's goals and the sponsors' ideals for the Institut had created a climate encouraging interdisciplinarity, it was René Thom who seized the possibilities thus offered to him. By 1971, the Third Section was not conceivable without him anymore.

5. THOM'S 'DREAMS'...

In the summer of 1958, the International Congress of Mathematicians at Edinburgh provided a convenient setting for Léon Motchane to plan out the activities of the IHÉS for the first few years. In particular, Jean Dieudonné and he agreed to invite one of the new Fields Medal winners, René Thom. Among the very first mathematicians invited to the newly founded Institut des hautes études scientifiques, Thom was asked to spend the

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[161] *Notes de séance manuscrites,* Assemblée générale (23/9/64). Arch. IHÉS.
[162] *Extrait du Rapport scientifique sur l'activité de l'IHÉS en 1966* (6/4/67), 3. Arch. IHÉS.

1959-1960 academic year in Paris.[164] "I have always had much reluctance to make a decision, whatever it is," Thom replied to Dieudonné.[165] For the time being however, mostly for personal reasons, he decided not to take advantage of this offer.

Moreover, it had not escaped Motchane and Dieudonné that, having just received his Fields Medal, but at Strasbourg without a prestigious position, Thom was a natural candidate for tenure at the Institut. His name was already suggested for such a position at a Scientific Committee in September 1959. But it was then decided that no offer should be made to him before the definitive installation of the IHÉS at its campus of Bures-sur-Yvette took place.[166]

Only two years later, when the prospect of the move seemed secure, did the professors of the IHÉS mention this possibility to Thom. At Harvard, in December 1961, Grothendieck talked to him about taking Dieudonné's place. Feeling "not mathematically active enough," Dieudonné envisioned taking the position of dean of the new *Faculté des sciences* at Nice. After making sure of Dieudonné's reasons for leaving the Institut, Thom decided to accept a permanent professorship, starting in October 1963

As soon as he got to the IHÉS, Thom seized the opportunity to invite Mauricio Peixoto, a Brazilian mathematician he had met in the United States.[167] This was Thom's way of seizing the advantages that the structure of the IHÉS offered him. Starting

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[163] *Année 1966 - Séminaire et conférences,* 2.Arch. IHÉS.
[164] Lettre de Jean Dieudonné à Léon Motchane (8/10/58); de Léon Motchane à Robert Oppenheimer (8/10/58); de Léon Motchane à Francis Perrin (20/11/58); de Léon Motchane à Jean Dieudonné (23/12/58); de Jean Dieudonné à Léon Motchane (14/1/59). Arch. IHÉS. The other mathematicians invited were Shafarevich, Bott, and Milnor.
[165] Lettre de René Thom à Jean Dieudonné (6/2/59). Arch. IHÉS.
[166] *Comité scientifique* (17/9/59). Arch. IHÉS.
[167] Lettre de Léon Motchane à Maurico Peixoto (8/11/63). Arch. IHÉS.

February 7, 1964, Peixoto gave a seminar on the "Qualitative Theory of Differential Systems and Structural Stability." Since for two years Peixoto had worked in relation with Solomon Lefschetz's school of dynamical systems, at Princeton and Baltimore, where Thom was introduced to the notion of structural stability, we might guess Thom's interest in getting him to lecture at the IHÉS.[168] Equipped with this concept of structural stability, Thom would embark on an ambitious program, first with an interest restricted to pure mathematics, but soon reaching out to the general process of using mathematical concepts and techniques in order to model natural phenomena.

6. CONCLUSION

What, in the mid-sixties, were the main characteristics of the IHÉS? An institution devoted to fundamental research and sponsored by industry, it struggled to survive. Even with massive aid from the State which the IHÉS would not attain a stable financial basis until the early years of the 1970s. As a consequence it had to remain rather small, with only four permanent faculty members.

But, with both of its mathematics professors having received a Fields Medal, it had achieved a very enviable stature in the international mathematical community. The physics section, specialized in the mathematical side of theoretical physics, had an honorable reputation, but nowhere near the one of the mathematical section. The Third Section, however, remained non-existent.

Its budget for invitations, and the quality of its permanent faculty allowed the IHÉS to get leaders in their fields as visitors. Although they often sought to diversify their

[168] *Année 1964 – Séminaires et conférences,* Rapport scientifique 1964 (10/2/66), 1.

activity, Motchane and his professors largely restricted the people to which they sent out invitations to those working on fields where the IHÉS could be among the world's best.

Scientists from all over the world started to press director Léon Motchane in order to be invited to spend a few months at Bures-sur-Yvette. In fairly large numbers, students, including those from the Ecole Normale's famously influential *Séminaire Cartan,* came to seminars given by international experts. Considering their high level and the eccentricity of the IHÉS campus, this was quite an accomplishment.

It was in this context that René Thom believed the time had come to write a book. Dealing with the implications for the mathematical modeling of natural phenomena, which, in Thom's view, derived from topology and, more specifically, the study of singularities of applications and dynamical systems, this book would launch catastrophe theory. Thom thought that the mathematical concept of *structural stability* could provide general guidance in the practice of mathematical modeling. In the following chapter on the history of structural stability, we will see that great hope had often been invested in this concept.

In chapter VI, I will then come back to the IHÉS and show how, from the mid-1960S to the early 1980s, it became one of the world's major developing grounds of new modeling practices. Introduced by topologists, these practices will bear the mark of the IHÉS.

7. COMPLEMENT TO CHAPTER IV: DOCUMENTS

a) Lettre de Léon Motchane à Pierre Ailleret, Électricité de France (7 mai 1958), accompagnée d'une "Note."

Monsieur P. Ailleret
Directeur Général des Études et Recherches
Place des États-Unis,
Paris 16e.

Cher Monsieur,

Voici quelques idées sur l'orientation des études de notre Institut. Je ne vous apprends rien de neuf, mais la récente réunion à Berlin lors de la Commémoration Planck, qui a permis à plusieurs de mes amis d'avoir de longues conversations avec Heisenberg, Bogolioubov et quelques autres, confirme l'essentiel de la note. Tout cela devient actuel et important, mais je suis mauvais juge de ce qu'il faut dire et de qu'il ne faut pas dire. Ainsi, si vous estimez qu'une certaine indication sur l'orientation probable des études doit être rendue publique et pourrait vous être utile, nous pourrions la préparer ensemble si vous le désirez, d'après les éléments réels que vous trouverez dans cette note.

Veuillez agréer, [etc.]

Motchane

Strictement confidentiel.-

NOTE

Aucun sujet de recherche ne serait imposé aux savants appartenant à l'Institut de Recherches Fondamentales (I.R.F.) dont la fondation est prévue actuellement, de même que toute recherche orientée est bien entendu hors de question, la liberté de choix étant le gage principal du succès.

Cependant, la sélection de la qualité des savants réunis au sein d'I.R.F. permet d'affirmer que le problème crucial de la physique théorique va être attaqué en collaboration par des mathématiciens et des physiciens: à savoir, la structure de la matière et la théorie des particules. Tout progrès dans ces domaines signifierait qu'on a réussi à sortir de l'impasse dans laquelle se trouve actuellement la physique théorique: C'est une supposition raisonnable si l'on se donne un délai de quelques années.

Par analogie avec ce qui s'est passé pendant la période de six années précédent la guerre à savoir la mise au point du procédé de libération de l'énergie atomique résultant des études théoriques nucléaires, on pourrait se demander quel pourrait être le premier problème pratique important auquel aboutirait les études théoriques définies plus haut. La réponse est facile à donner: il s'agit évidemment de la transformation directe de l'énergie nucléaire en énergie électrique, transformation qui éviterait toute réaction thermonucléaire. C'est le problème qui est à l'ordre du jour mais qui ne pourra être résolu avant que des progrès théoriques importants soient réalisés.

On ne voit actuellement que trois endraoit [*sic*] où de tels progrès pourraient être espérés: États-Unis (Princeton), U.R.S.S. (Moscou) et Europe (Paris, éventuellement I.R.F.). La question de priorité ne jouera pas beaucoup en ce sens que les résultats d'un centre seront rapidement connus ailleurs. Cela permettrait de travailler presque simultanément aux applications.

Mais l'absence d'un tel centre qui entraînerait le manque total d'une équipe de savants entraînés et avisés serait grave car cette absence créerait un obstacle insurmontable empêchant de franchir le seuil entre la théorie et la pratique: on mettra des années à former des interprètes capables d'instruire les techniciens.

Il semble donc impensable qu'une organisation (E.D.F.) qui a la responsabilité de la production de l'énergie électrique dans ce pays soit à l'écart d'une recherche de cette nature. Non seulement ces considérations justifient une subvention importante, mais il serait également à souhaiter que dès la formation du centre, un ou deux jeunes physiciens engagés par cette organisation bénéficient de l'enseignement d'I.R.F. qui est public et ouvert à tout le monde, afin d'être capables au moment venu de servir d'interprètes entre les savants et les ingénieurs.

b) Note pour les industriels (mai 1958), par Léon Motchane, 3pp.

Note sur la fondation d'un

"INSTITUT DES HAUTES ÉTUDES SCIENTIFIQUES"

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Il y a peu de temps encore, le terme de recherche scientifique était à peine connu du grand public. On "faisait de la science" à l'Université; l'industrie s'occupait de la technique et des applications, tandis que les inventeurs étaient des gens distraits, quelquefois fous qui mouraient méconnus, dans la misère. L'aspect moderne de la recherche scientifique est relativement récent. Il est concomitant avec l'apparition d'une nouvelle conception, aujourd'hui ancrée dans l'esprit du public, à savoir que la recherche scientifique n'est pas un phénomène spontané de la nature qui fleurit dans les Universités, mais une activité dont il faut s'occuper, qui se laisse cultiver, et qui apporte au pays qui en est pourvu abondemment [*sic*] un surcroît considérable de prestige et de puissance politique. Cette représentation sociale de la recherche contient une part de vérité dont il faut tenir compte quand on place le problème sur son véritable terrain.

I – Il est bien connu que l'économie s'industrialise de plus en plus. Cela est banal pour les pays cartésiens, mais devient vrai même pour les pays de science contemplative. À mesure que les techniques s'élèvent et deviennent plus compliquées et raffinées, elle se rapprochent, par leur niveau intellectuel, des problèmes purement scientifiques. La science pure et ses applications se voisinent davantage: dans le temps d'abord, parce qu'une découverte scientifique abstraite, telle qu'une théorie mathématique nouvelle "descend" plus rapidement à travers un symbolisme physique vers une application pratique (quand elle en comporte une) du fait même de la grande multiplication et de l'abondance des techniques; dans leur niveau ensuite, car les techniques d'aujourd'hui sont infiniment plus élevées que jadis et utilisent des procédés qui, il y a peu d'années, relevaient d'expériences qualitatives et rares de laboratoire et de spéculations théoriques abstraites.

De telle sorte que le véritable aspect moderne de la recherche scientifique (celui-là moins connu du public) consiste dans le fait que le travail [2] d'un industriel, d'un ingénieur, comme celui d'un physicien théoricien et d'un mathématicien, fût-ce le plus abstrait, ne sont pas aussi éloignés les uns des autres et la réussite des derniers devient indispensable aux premiers.

2° – Cela nous amène à poser parmi tous les problèmes, celui de la Recherche Fondamentale dans les sciences exactes, par laquelle nous entendons limitativement les recherches faites sans préoccupation d'applications dans les domaines de Mathématique pure, Physique théorique, et de Méthodologie physico-mathématique des Sciences de l'Homme. Ce problème a une place à part et exige une solution de nature différente de celui de la recherche en général. En effet, la formation des cadres scientifiques d'enseignement et des cadres techniques d'industrie à l'échelle nationale incombe à l'État et se place dans le schéma général de la réforme de l'Enseignement entreprise récemment. Les recherches particulières à une branche de la physique ou de la technique, ou encore particulière à une industrie se font à l'échelon d'instituts spécialisés ou de laboratoire d'usine; son développement en France est encourageant et témoigne d'un esprit moderne chez beaucoup de chefs d'entreprise. Seul, le problème majeur des recherches fondamentales, négligé pendant de longues années, n'a jamais été repris sérieusement, ce qui explique par exemple, le retard inquiétant de notre pays en physique théorique.

Le même problème s'est posé aux États-Unis avant la dernière guerre mondiale, et a été brillamment résolu. Il n'est pas exagéré de dire, en effet, qu'une des causes de l'avance américaine dans les domaines de la physique théorique et nucléaire avec toutes ses conséquences économiques et politiques, fut en partie la création et fonctionnement de l'"Institute for Advanced Study" à Princeton, où les plus grands physiciens et mathématiciens du monde ont eu l'occasion de vivre et de travailler ensemble. Il n'est pas sans intérêt de rappeler que sa fondation remonte aux années 1931-32, et que EINSTEIN, Von NEUMANN et OPPENHEIMER y ont cristallisé les meilleures forces scientifiques du moment e[t] qu'en 1940 déjà, on entrevoyait certaines applications pratiques, dont la source peut être tracée aux travaux abstraits de recherche pure entrepris quelques années plus tôt. Les résultats obtenus ont dépassé les prévisions les plus ambitieuses. Non seulement les progrès scientifiques peu connus du grand public furent remarquables, mais le chemin **[3]** parcouru entre les connaissances les plus abstraites et leurs applications, que tout le monde connaît, s'est avéré plus court qu'on ne l'eût cru possible auparavant.

Les Russes n'ont pas procédé autrement et avec le même succès. À côté d'un grand nombre d'Instituts scientifiques où les divers aspects des sciences exactes sont étudiés, on compte quelques centres d'études de mathématiques et de Physique théorique consacrés aux problèmes les plus avancés, et où la recherche est pratiquée avec une grande indépendance.

3° – Ainsi les faits essentiels qui dictent impérativement l'organisation de la Recherche Fondamentale ont été dégagés par l'expérience – les voici:

- Il existe effectivement un problème majeur de la recherche, limité en étendue, qui se place par son objet à un niveau exceptionnellement élevé, et qui exige pour sa réalisation la participation et la formation des élites.
- Il s'agit donc de réunir un nombre relativement restreint de savants de grande valeur, physiciens et mathématiciens, de leur donner toutes facilités de travail, sans leur imposer de charges d'enseignement ni d'obligation d'aucune sorte.
- La réalisation d'un tel projet ne présente pas de difficultés matérielles insurmontables.
- Par contre la solution est subordonnée à un certain nombre de conditions morales indispensables à la réussite.

CHAPTER V: STABILITY

A vector field X on M^2 is said to be structurally stable if there is a neighborhood Δ of X in $\mathcal{B}$ such that whenever $Y \in \Delta$ there is a homeomorphism of M^2 onto itself transforming trajectories of X into trajectories of Y.
—Mauricio M. Peixoto.[1]

L'hypothèse de stabilité structurelle des processus scientifiques isolés apparaît comme un postulat implicite de toute observation scientifique.
—René Thom.[2]

A system which completely lacks stability would be a poor model for reality, as reality is *always* a perturbation of what we think it is. Thus some kind of stability is crucial.
—Robert F. Williams.[3]

1. INTRODUCTION: A HISTORY OF STRUCTURAL STABILITY

While director Léon Motchane desperately struggled to achieve financial stability for the Institut des hautes études scientifiques, René Thom, as soon as he joined its faculty, launched an ambitious program in the hope of discovering what was at the basis of the world's stability. The mathematical notion he judged could best be used for this task was *structural stability.* Thom's efforts at building around him a research school will be

[1] M. M. Peixoto, "Structural Stability on Two-Dimensional Manifolds," *Topology,* 1 (1961): 101-120, 103. First defined by A. A. Andronov and L. Pontrjagin in 1937.
[2] "The hypothesis of structural stability of isolated scientific processes is implicit in all scientific observation." R. Thom, *SSM,* 16.
[3] R. F. Williams, Review of *Dynamical Systems on Surfaces,* by C. Godbillon, *American Mathematical Monthly,* 92 (1985): 70-71; quoted by M. W. Hirsch, "The Dynamical Systems Approach to Differential Equations," *Bulletin of the American Mathematical Society,* 11 (1984): 1-64, 33.

described in chapter VI. Meanwhile, I present here a partial history of the concept of stability in the study of differential equations, which is essential background for an understanding of Thom's and, later, Ruelle's work.

The concept of structural stability was introduced in 1937 by Russian mathematicians Andronov and Pontrjagin, and taken up in the United States by Lefschetz and his younger collaborators after World War II. From the very beginning, as I describe below, a long time before Thom invested it with so much philosophical weight, the notion of structural stability had been perceived as embodying a general codification of the practice of building mathematical models of natural phenomena. Its mathematical formulation was seen as condensing general metaphysical assumptions.

As in almost every piece of historical writing on chaos, I include a chapter titled "Stability." However, my point is *not* that people were once blindly looking only for stability and overlooked chaos because of preconceived dogmas, as is often argued. This explanation is much too simple-minded and fails to explain the reasons why scientists emphasized periodic solutions in their investigation of nonlinear differential equations. More importantly, it totally misses the most important point: the very emphasis on stability, and structural stability in particular, *actually prepared the emergence of chaos in that it set up the line of questioning to which features of chaos appeared as long-sought, if somewhat surprising, answers.*

Therefore, the exploration of the stable descriptions used to model natural phenomena was a major impetus behind the modeling practices I study here. To start with, structural stability has always been conceived as a mathematical translation of philosophical assumptions about the physical stability of real systems. As we have seen, Thom's program for catastrophe theory was greatly inspired by structural stability. Similarly, American topologist Stephen Smale's whole career in the field of dynamical systems was an offshoot of his concern for finding the right kind of stability, so that "most" dynamical systems be stable under this definition.

Initially, Thom and Smale hoped that mathematical arguments could substitute for philosophical ones in the choice of systems susceptible of being used for modeling.[4] Around 1970, largely because of work they themselves had done or inspired, it became clear that mathematics could not in general guide the choice of which stable systems to use for modeling. Both Smale and Thom therefore stopped working directly on the mathematical theory of dynamical systems, and rather focused on modeling issues. They developed a new field that one might be tempted to call "applied topology" which I will address in more detail in chapter VI below.

Dealing with the conceptual history of (structural) stability, this chapter does not aim at providing original accounts of the social and cultural contexts within which each of the contributions discussed was made. Rather, noticing that both Thom and Smale made important use of work done decades earlier, I intend to provide my own re-reading of their sources as seen through their eyes. Still, I shall inject some elements of contextual analysis when necessary. But one should keep in mind that, only after Smale had

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[4] That mathematics can bypass philosophical impasses was a common theme of the 1950s; it was at play in the story of structuralism; it also figured prominently in other contexts; see, e.g. D. Lerner, "Introduction, On Quantity and Quality," in *Quantity and Quality,* ed. D. Lerner (New York: Free Press of Glencoe, 1961), 11-34, esp. 20-23.

achieved his own synthesis, could the various contributions I discuss be reinterpreted as belonging to a unified, conceptual and disciplinary setting.

In section 5 below, I discuss Smale's synthesis and the bridges he built to previous works. This section should therefore be read differently from the previous ones as it deals with the work of one of the main actors of my story. It is only from Smale's standpoint indeed that the works discussed in the first sections of this chapter become 'precursors' of Smale's own accomplishment. Finally, this chapter provides an answer to the main conundrum of the young historiography of chaos, namely the fact that chaotic behaviors seem to have been overlooked for so long. I argue that the development of the computer and of topological methods for the study of differential equations provide causes for the chaos burst that began around 1975.

2. MATHEMATICAL LAG EXPLAINS SPUTNIK, OR THE COLD WAR ROOTS OF CHAOS THEORY ?

In 1959, Princeton University topologist Solomon Lefschetz (1884-1972) presented his Final Report to the Office of Naval Research (ONR), which, for more than 13 years, had sponsored a "Project on Nonlinear Differential Equations and Nonlinear Oscillations." In his final remark, he listed the most significant mathematical contributions made by members of the project. In first place, came the work of Henry DeBaggis and of Marilia and Mauricio Peixoto on structural stability.[5] Indeed, while always a marginal concern for

[5] S. Lefschetz, "Nonlinear Differential Equations and Nonlinear Oscillations." Final Report (August 15, 1946 - September 30, 1959), Contract NONR-1858(04), Project NR043-942, p. 30. Fine Arch. For a more complete history of Lefschetz's group and some biographical information, see A. Dahan Dalmedico, "La renaissance des systèmes dynamiques aux États-Unis après la deuxième guerre mondiale: l'action de Solomon

the Project as a whole, the mathematical concept of structural stability nonetheless constantly remained on the mind of some of its members. And the story of its "ascension," as Amy Dahan Dalmedico put it, parallels the history of Lefschetz's Project itself.

> During most of World War II, the undersigned [Solomon Lefschetz], a consultant at the David Taylor Model Basin [of the US Navy], had frequent interviews with Dr. Nicholas Minorsky, in connection with the latter's production of his well-known *Introduction to Nonlinear Mechanics.* Dr. Minorsky voiced repeated regrets at the impossibility of creating in this country anything resembling the well known Institute of Oscillations in Moscow.[6]

Of course, Lefschetz recalled, a full-fledged Institute would have required immense resources. Lefschetz and Minorsky approached ONR with a more modest proposal to initiate a Project on Differential Equations, total cost to be $25,000.00 (for the first year).

This Project, as well as Minorsky's initial reports to the US Navy, explicitly emphasized the Soviet advance in the study of nonlinear differential equations which, with the notable exception of George David Birkhoff, had to a large extent been neglected by American scientists. "Many hold the opinion," Lefschetz thus wrote in 1946, "that the classical contributions of Poincaré, Liapounoff and Birkhoff have exhausted the

Lefschetz," *Rendiconti dei circolo matematico di Palermo,* ser. II, Supplemento, 34 (1994): 133-166.

[6] S. Lefschetz, "Nonlinear Differential Equations," 1. Nicholas Minorsky wrote an extensive, four-part report to the David W. Taylor Model Basin of the US Navy. Titled *Introduction to Nonlinear Mechanics,* reports #534, 546, 558, and 564 were published from December 1944 to September 1946. They can be found at Princeton, call number SK 8230.6445. Minorsky later published a book version: *Nonlinear Oscillations* (Princeton: Van Nostrand, 1962). In both the reports and the book, however, Minorsky did not emphasize structural stability.

possibilities. This is certainly not the opinion of a large school of Soviet physico mathematicians." [7]

The objectives of Lefschetz's proposal "were stated to be, on the one hand, research in the field and, on the other, the development of a group of young men who could take their place as applied mathematicians in Industry or in an emergency, in various defense organizations."[8] Educated as an engineer in France, Lefschetz possessed a sensitivity for applied problems. But above all cold war competition against the Soviet Union remained one of Lefschetz's main stated drive for his important implication in the Project. Its high level of abstraction notwithstanding, none of the technological and military consequences of the mathematical work on nonlinear dynamics were lost on him. In 1950, while looking for other sources of support, Lefschetz wrote: "I have become interested in . . . the applications of the methods of non-linear mechanics to Air Force problems in guidance and automatic controls."[9]

"Curiously enough," Lefschetz candidly acknowledged in his final report, while the goal had been to train mathematicians that could apply their skills to industrial and military technological problems, "nearly all its [younger] members remained in the academic world."[10] It therefore seems that most of the members' motivation, including

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[7] S. Lefschetz, *Lectures on Differential Equations* (Princeton: Princeton University Press, 1946), iii. For a history of Soviet research on nonlinear dynamics, see S. Diner, "Les voies du chaos déterministe dans l'école russe," *Chaos et déterminisme,* ed. A. Dahan Dalmedico et al. (Paris: Seuil, 1992): 331-370.

[8] S. Lefschetz, "Nonlinear Differential Equations," 2.

[9] S. Lefschetz to Colonel Frank J. Seiler (Oct. 19, 1950). Princeton Arch.

[10] S. Lefschetz, "Nonlinear Differential Equations," 22.

Lefschetz's, was above all academic, but they never failed, in good faith probably, to mobilize defense arguments in favor of their Project.[11]

On October 16, 1959, Solomon Lefschetz was awarded a honorary degree by the Sorbonne, in Paris, in the presence of President Charles de Gaulle. The retired mathematician took advantage of this occasion to attract people's attention to the "mathematical gap" he saw between Russia and the West. Until recently, the American lag in the study of nonlinear oscillations seemed a sorry thing, which to be sure needed to be redressed. After 1957, it became an urgent matter of national security. "Then the first Sputnik came out," Lefschetz declared in Paris. "That's when I got scared." Princeton University's press release revealingly described nonlinear differential equations as "the involved mathematical systems which underlie almost every natural movement, *including those which must be understood in order to develop more accurate rocket control systems.*" For Lefschetz, the Soviet success in guiding space rockets meant that they were even more ahead in nonlinear dynamics than he previously thought; he estimated their lead to be of "10 to 15 years." This reason, he claimed, persuaded him to leave his retirement to set up the Research Institute in Advanced Study (RIAS) in Baltimore, at the Martin Company, an aviation and missile manufacturer.[12] Of course, this only reflected

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[11] It should be noted that during the McCarthy era, Russian-born Lefschetz hardly was above being considered a suspect character by some. In 1955, one his students, John G. Kemeny, came under scrutiny for "close association with individuals alleged to be sympathetic to Communism and/or members of Communist front organizations, namely . . Solomon Lefschetz." J. Douglas Brown and Albert W. Tucker's affidavit deposition (March 2, 1955). Princeton Arch.

[12] Lefschetz's file. Princeton Arch. My emphasis. On February 7, 1960, the *New York Times* published an article about Lefschetz's worries titled "Mathematical Lag in Missiles Noted." RIAS moved to Providence in 1963; see A. Dahan Dalmedico, "La renaissance," 141.

Lefschetz's personal worries. Nothing inherent to the study of nonlinear differential equations led to such an emphasis on military and space technology. As we shall soon see, in a different time and place, namely Paris and Berkeley in the late 1960s, hopes were placed in the study of similar mathematical domains for quite the opposite reason.

Even before the start of the Cold War, as Lefschetz recalled, his proposal was quickly accepted by ONR.

> The undersigned will never forget his (only) interview with Captain Conrad [the officer in charge of setting ONR up]. At first nonplussed and puzzled, he soon called in consultation his scientific adviser, Dr. Allen Waterman . . . [who] read our short memo and exclaimed at once - 'This is just what we want'. Whereupon the matter of the project was settled in a short quarter of an hour...[13]

The activities of the Project proceeded in various ways. There was a weekly research seminar initiated by Lefschetz in 1942, an advanced course on Differential Equations, a constant flow of invited professors, postdoctoral fellows and graduate students. From 1945 to 1953, Lefschetz chaired the Mathematics Department. True to his repute of "papa daddy" for graduate students, he remained at the disposal of the Project's participants. "Suffices to say," he stated, "that the Director's office was never locked, and that he was (and has remained) infinitely accessible to one and all."[14] The Project also oversaw the publication of articles written by its members in a sub-series of the famous Annals of Mathematics Studies.[15] One of Lefschetz's foremost students, Joseph LaSalle, summarized his mentor's accomplishments as such:

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[13] S. Lefschetz, "Nonlinear Differential Equations," 2.
[14] S. Lefschetz, "Nonlinear Differential Equations," 4. It was George W. Brown who called Lefschetz a "papa daddy" in *The Princeton Mathematical Community in the 1930s: An Oral History Project,* pp. PMC3-4. Princeton Arch. AC#109 Box 40.
[15] *Contributions to the Theory of Nonlinear Oscillations, Annals of Mathematics Studies,* 20 (1950); 29 (1952); 36 (1956); 41 (1958), ed. Solomon Lefschetz (Princeton: Princeton

> It was Solomon Lefschetz who made the subject of differential equations both respectable and lively in this country, and who through his projects at Princeton and RIAS . . . made it possible with his boundless enthusiasm, inspiration, and guidance for many young people to establish deep roots in the subject.[16]

3. FACETS OF STABILITY IN THE INTERWAR: RADIO ENGINEERING, COARSE SYSTEMS, CELESTIAL MECHANICS

Most important for my purpose here was "a noteworthy adventure engaged in by the Project, [namely] the edited translation from the Russian of a classic: Andronov and Chajkin, *Theory of Oscillations*."[17] In 1931, Aleksandr Aleksandrovich Andronov (1901-1952), a student of L. I. Mandelstam, had founded a research school at Gorki.[18] The above book summarized a decade of work on nonlinear oscillations. Making a wide usage of Poincaré's, Lyapunov's, and Birkhoff's works, this book mainly addressed *dissipative* systems, as opposed to *conservative* ones privileged by previous mathematicians.[19]

University Press). A fifth volume (*Annals,* 45) was published in 1960 and edited by L. Cesari, J. LaSalle, and S. Lefschetz.

[16] J. P. LaSalle, *IEEE Memorial,* 1973; quoted in *Dynamical Systems: International Symposium on Dynamical Systems, Brown University, 1974,* ed. L. Cesari, J. K. Hale, and J. P. LaSalle (New York: Academic, 1976), iii.

[17] S. Lefschetz, "Nonlinear Differential Equations," 7. Aleksandr A. Andronov, [A. A. Witt,] and C. E. Chaikin, *Theory of Oscillations,* abridged transl. Natasha Goldskaja, ed. Solomon Lefschetz (Princeton: Princeton University Press, 1949); *Theory of Oscillators,* transl. F. Immirzi (Oxford: Pergamon, and Reading: Addison-Wesley, 1966). The name of Aleksandr Adol'fovich Witt, who disappeared during Stalinist purges, was suppressed in 1937, but reinstalled in 1959 for the second Russian edition. S. Diner, "Les voies du chaos," 342.

[18] See Amy Dahan Dalmedico, "Le difficile héritage de Henri Poincaré en systèmes dynamiques," in *Sonderdruck aus Henri Poincaré: Science et philosophie, Congrès international de Nice, 1994* (Berlin: Akademie; Paris: Albert Blanchard, 1995): 13-33, esp. 20-23.

[19] *Conservative* dynamical systems are those for which the total energy is conserved; *dissipative* systems generally include a friction term, which dissipate energy into heat.

a) Dissipative Systems and the van der Pol Equation

For a long time, dissipative systems were thought to be less interesting than conservative ones for the reason that, in the long run, they tended toward rest. Or so it seemed. As George D. Birkhoff wrote in a famous book, a "dissipative system of this type tends in its unconstrained motion either toward equilibrium or, more generally, toward the motion of a conservative system with fewer degrees of freedom."[20] When in the late twenties, Andronov tackled this problem, however, dissipative systems witnessed a period of renewed interest mainly due to the work of Balthasar van der Pol (1889-1959), an engineer at the Phillips Company in Eindhoven, Holland. By simplifying to the extreme the equation for the amplitude of an oscillating current driven by a triode, he indeed exhibited an example of a dissipative equation without forcing which nonetheless sustained spontaneous oscillations, an example of what Ilya Prigogine would later call *dissipative structures.*[21]

(i) Mathematics and Radio Problems

In a lecture given on March 15, 1947, before the Dutch Mathematical Center in Amsterdam, van der Pol recalled his original problem and how it led to more abstract mathematical concerns:

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[20] G. D. Birkhoff, *Dynamical Systems* (Providence: American Mathematical Society, 1927), 32. The type of systems he is considering here are those which are not subject to any external force, or to external force that do no work, i.e. systems that receive no external energy. Birkhoff's ideas on stability are presented below; see p. 267.

[21] B. van der Pol, "On 'Relaxation-Oscillations'," *Philosophical Magazine,* 2 (1926): 978-992; repr. *Selected Scientific Papers,* ed. H. Bremmer and C. J. Bouwkamp (Amsterdam: North-Holland, 1960): 346-360. For more on van der Pol, see G. Israel, *La*

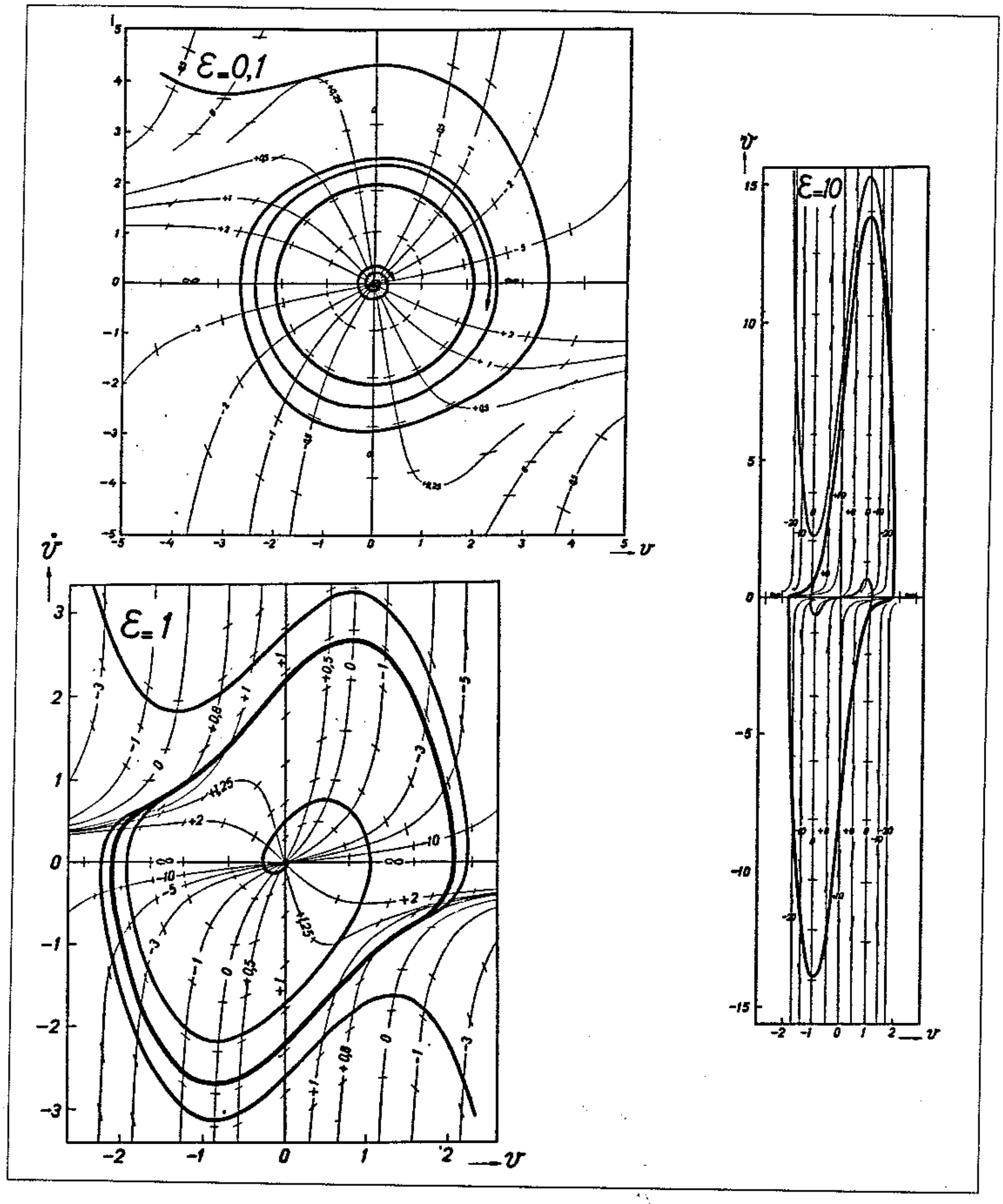

Figure 8: Flow in Phase Space for the van der Pol Equation for ε=.1; ε=1; and ε=10. Repr. with permission from B. van der Pol, "On Relaxation-Oscillations," 983-985. Copyright © Taylor and Francis.

Mathématisation du réel (Paris: Seuil, 1996), 34-51. About dissipative structures, see Chapter VIII below.

> How does a retroactive triode cause a simple electric circuit to oscillate? In 1920, I was able, by ignoring all secondary phenomena, to crystallize this perhaps most fundamental equation of all modern radio problems into the following, non-linear differential equation:
>
> $$v'' - \varepsilon(1-v^2)v' + v = 0;$$
>
> in which the constant ε>0, and the [primes] signify differentiation with respect to time, whilst v is the voltage across the oscillator circuit.[22]

This equation, van der Pol explained in 1947, had been worked out for the purpose of solving a technical problem, by making the explicit assumption that ε remained much smaller than 1.[23] There was great urgency in trying to understand this problem since triode vacuum-tube generators had then "become the standard low power source of alternating current for laboratory purposes."[24] A few years later, van der Pol impelled a new direction to his studies:

> in 1926, purely as a matter of mathematical interest, we asked ourselves whether this equation also led to interesting results in cases where ε is large, and this disclosed the theory of *relaxation-oscillations.*[25]

Analytic solutions for the van der Pol equation are extremely rare, but already in his 1926 article, van der Pol drew trajectories in phase space, that is, plotted v versus $z = v'$, for three values of ε, namely 0.1, 1, and 10. These plots clearly showed that

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22 B. van der Pol, "Mathematics and Radio Problems," *Phillips Research Reports,* 3 (1948): 174-190; repr. in *Selected Papers,* 2: 1140-1156. Originally published in Dutch in *Simon Stevin,* 25 (1947): 179-198. The quote is from *Selected Papers,* 2, 1154. The above equation is often called the van der Pol equation in the technical literature. See also B. van der Pol's review essay: "The Nonlinear Theory of Electric Oscillations," *Proceedings of the Institute of Radio Engineers,* 22 (1934): 1051-1086; repr. *Papers,* 1: 795-830.
23 B. van der Pol, "A Theory of the Amplitude of Free and Forced Triode Vibrations," *Radio Review,* 1 (1920): 701-710; 754-762; repr. *Selected Papers,* 1: 228-246.
24 E. V. Appleton and B. van der Pol, "On the Form of Free Triode Vibrations," *Philosophical Magazine,* 6th ser., 42 (1921): 201-220; *Selected Papers,* 1: 258-280. Quote on p. 258.

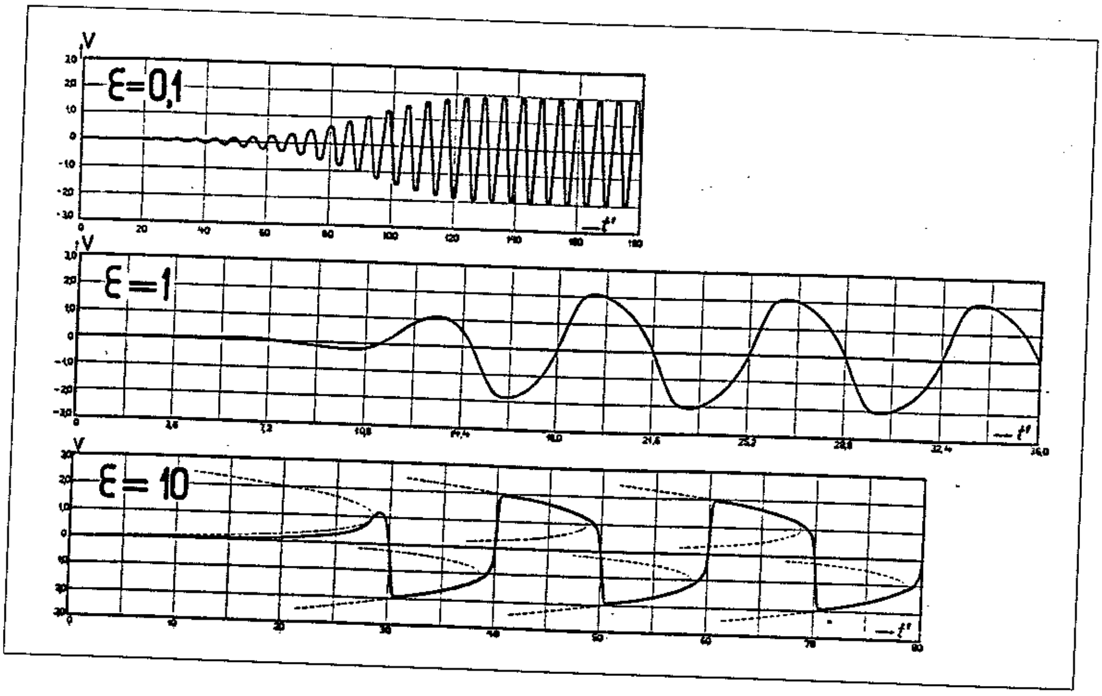

Figure 9: Solutions as Function of Time of the van der Pol Equation for ε=0.1; ε=1; and ε=10. Repr. with permission from B. van der Pol, "On Relaxation-Oscillations," 986. Copyright © Taylor and Francis.

solutions tended always to wind up around a periodic stationary solution, represented as a closed curve. In contemporary terms, this was one of the first concrete examples of an *attractor* in a physical system that was not a point (Fig. 8 and 9).

(ii) French Reception and Rocard's Insensitivity

The van der Pol equation had a stupendous career in scientific literature, among electrical engineers of course, but also among mathematicians, physicists, biologists, etc. According to van der Pol's own count, it had led, in 1947, to "at least one hundred papers and books, particularly from the Russian and French quarters."[26]

[25] S. Diner, "Les voies du chaos," 341.
[26] B. van der Pol, "Mathematics and Radio," 1150.

From Chapter II above, we may recall that, in 1948, the Bourbaki mathematician André Weil made one exception to his ideology of purity in mathematics, by listing the van der Pol equation as "one of the few interesting problems which contemporary physics has suggested to mathematics."[27] In 1928, French scientist A. Liénard generalized van der Pol's investigation by studying equations of the type:

$$\frac{d^2x}{dt^2} + \omega f(x)\frac{dx}{dt} + \omega^2 x = 0.$$

Weil's comment has to be understood in context. Indeed, a source for Liénard's interest was a memoir by Élie and Henri Cartan, to my knowledge the only one written jointly by the father and the son. Dealing with an equation very similar to Liénard's, the Cartans made it clear, with the notation they used, that the source of their inspiration lay in electrical circuit theory:

$$L\frac{d^2i}{dt^2} + (R - \varphi(i))\frac{di}{dt} + \frac{1}{C}i = 0;$$

this equation being easily recognizable as the one that describes a circuit with inductance L, condenser C, and resistance $R - \varphi(i)$ depending on the intensity of the current i.[28]

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[27] A. Weil, "The Future of Mathematics," *Great Currents*, ed. F. Le Lionnais: 321-336, 332. The van der Pol equation seemed to have gather of rare consensus in its support, since, for example, applied mathematician Theodore von Kármán selected it as his first example of an engineering problem that should be of interest to a mathematician: see "The Engineer Grapples with Nonlinear Problems," *Bulletin of the American Mathematical Society*, 46 (1940): 615-683, esp. 619-624.

[28] A. Liénard, "Étude des oscillations entretenues," *Revue générale de l'électricité*, 26 (1928): 901-912 and 946-954. Élie and Henri Cartan, "Note sur la génération des oscillations entretenues," *Annales des PTT*, 14 (1925): 1196-1204; repr. Élie Cartan, *Œuvres complètes*, part III, 1: 71-82. P. Janet, "Note sur une ancienne expérience d'électricité appliquée," *Annales des PTT*, 14 (1925): 1193-1195; summarized in *Revue générale de l'électricité*, 19 (1926): 98D.

The main question that these French mathematicians sought to answer was the following: What form should the function $f(x)$ in the Liénard equation have so that it admitted one or several stable periodic solutions?[29] Clearly, other behaviors could happen, they knew it, but for obvious reasons stemming from the radio-engineering aspects of the question, these remained unexplored. As late as the early 1970s, Maurice Roseau's course of mechanics at the Université de Paris still strongly emphasized this aspect of the question.[30]

Far from being a blind neglect of a crucial flip side of the problem, this line of research proved extremely fruitful, leading in particular to the work of Mary L. Cartwright, John E. Littlewood, and Norman Levinson on the van der Pol equation with a forcing term $e(t)$ on the right-hand side of the equation. These important contributions will be examined below.

From the point of view of the history of nonlinear dynamics in France, widespread interest for van der Pol's and Liénard's equations led to important consequences. Indeed, Yves Rocard's famous textbook on nonlinear oscillations, written in July 1940, was highly focused on the above equations, which he had considered for highly practical

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[29] See also J. Haag's work: "Sur les oscillations auto-entretenues," *CRAS,* 199 (1934): 906-909; "Sur l'étude asymptotique des oscillations de relaxation," *CRAS,* 202 (1936): 102-104; "Sur la théorie des oscillations de relaxation," *CRAS,* 204 (1937): 932-934; "Formules asymptotiques concernant les oscillations de relaxation," *CRAS,* 206 (1938): 1235-1237.

[30] See M. Roseau, *Vibrations non linéaires et théorie de la stabilité* (Berlin: Springer, 1966); *Solutions périodiques ou presque périodiques des systèmes différentiels de la mécanique non linéaire,* lecture notes (Département de mécanique, Faculté des sciences, Paris, 1970). Jussieu Lib.

purposes.[31] One of the important conclusions at which he arrived was that oscillators of a van der Pol type exhibited a behavior that was *independent from initial conditions.* After a few oscillations, they always tended towards a periodic solution, for which not only the period, as for harmonic oscillators, but also the amplitude were dictated by the equation and not the initial conditions. Knowing that Rocard's lectures at the École normale supérieure, as well as successive editions of his book, have been an important introduction to the subject for many of the French physicists who later dealt with chaos, we may conclude that sensitive dependence on initial condition was the more striking to them.[32]

(iii) A Model of Mathematical Models?

This wide interest for van der Pol's work hardly stemmed from its mathematical interest alone, but because, as Giorgio Israel recently put it, it provided a "model of models."[33] It appeared to van der Pol that relaxation-oscillations could accurately describe almost every oscillating phenomenon of nature.

> Many instances of relaxation oscillations can be cited, such as: a pneumatic hammer, the scratching noise of a knife, the waving of a flag in the wind, the humming noise sometimes made by watertraps, the squeaking of a door, a steam engine with too small flywheel, . . . the intermittent discharge of a condenser through a neon-tube, the periodic reoccurrence of epidemics and of economic crises, the periodic density of an even number of species of animals living together and one species serving as food to the other, the sleeping of flowers, the periodic

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[31] Y. Rocard, *Les Problèmes d'auto-oscillation dans les installations hydrauliques* (Paris: Hermann, 1937). See also Y. Rocard, *L'Instabilité en mécanique. Automobiles - avions - ponts suspendus* (Paris: Masson, 1954).

[32] See Y. Pomeau, "Préface," *Le Chaos. Théorie et expériences,* ed. P. Bergé (Paris: Eyrolles, and Éditions du CEA, 1988)). Yves Rocard, *Théorie des oscillateurs* (Paris: Éditions de la Revue scientifique, 1941); *Dynamique générale des vibrations,* 3rd ed. (Paris: Masson, 1960).

[33] G. Israel, *La Mathématisation du réel,* 34.

> reoccurrence of showers behind a depression, the shivering from cold, the menstruation, and finally the beating of the heart.[34]

About such a juxtaposition, the first thing to note is how ludicrous it appears! No underlying physical mechanism could unify all of the above in a single explanatory scheme. But this was not van der Pol's claim. Rather he suggested that oscillatory solutions of dissipative equations could account for all of the above. He tried to develop mathematical models for none of the above, with one notable exception. He did build a model of the heart. But, in my view, to suggest as Israel does that it actually was a *mathematical* model is somewhat misleading; it rather was an *electrical* model of the heart.[35] Van der Pol and his collaborator, van der Mark, built physical apparatuses made of electric circuits, the output of which traced curves resembling electrocardiograms. To use a fashionable term, they literally *blackboxed* the physiology of the heart, not with mathematical equations which would have been intractable, but with electrical tabletop models. That this electrical model could be expressed with differential equations, van der Pol and van der Mark hardly doubted. But as a mathematical model, their model of the heart remained the expression of a program rather than an actualization of this program.

In conclusion, van der Pol's work therefore exhibited the dual aspect characteristic of much of the mathematical research I look into in this dissertation. It combined an important advance in *qualitative* mathematical knowledge and a strong taste for *analogy* as method of mathematical modeling of the world. Historically, therefore, van der Pol

[34] Balthasar van der Pol and J. van der Mark, "The Heartbeat Considered as a Relaxation Oscillation, and an Electric Model of the Heart," *Philosophical Magazine,* 7th ser., 6 (1928): 763-775; repr. *Papers,* 1: 486-511; quote on pp. 491-492. Quoted in G. Israel, *La Mathématisation du réel,* 40.

[35] Cf. G. Isreal, *La Mathématisation du réel,* 34-51.

should not be seen as an example of those scientists who, dealing with nonlinearities—and they were many even then—remained blind to the manifestations of chaos when they should have discovered it, as Kellert would have it.[36] Rather van der Pol was someone who forged the tools for, and helped create the possibility of, looking at dissipative systems in a new light. As such, he went farther than Poincaré in a certain respect. His work was not only rediscovered by later-day chaologists but also spurred on the moment important developments contributing, at the level of both mathematical techniques and modeling practice, to the possibility of the emergence of catastrophe and chaos theories.

b) Stability in Mathematics and in Modeling Practice for Radio Engineering

As van der Pol noted in 1947, his work was well received by some Russian mathematicians, and especially Aleksandr Andronov, who was among the first to use powerful new topological techniques to study nonlinear oscillations such as those exhibited by the Dutch engineer.

(i) Coarse Systems

In a note published by the French Academy of Science, Andronov identified solutions of the van der Pol equation as examples of "self-oscillations," also found in chemistry, biology, and physics, described as such:

> These systems are ruled by differential equations that differ from those studied by mathematical physics and classical mechanics. The systems where these

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[36] S. Kellert, *In the Wake of Chaos,* 125-127. See also, J. Gleick, *Chaos,* 49. For a historical and mathematical discussion of the strange attractors to be found in the forced van der Pol equation, see R. H. Abraham, "In Pursuit of Birkhoff's Attractor," in *Singularities and Dynamical Systems,* ed. S. N. Pnevmatikos (Amsterdam: North-Holland, 1985): 303-312.

> phenomena are produced are not conservative and sustain their oscillations by drawing their energy from nonperiodic sources.[37]

More generally than van der Pol and even Liénard, Andronov considered the stationary solutions of a class of two-dimensional systems of differential equations:

$$\frac{dx}{dt} = P(x,y); \quad \frac{dy}{dt} = Q(x,y);$$

x and y being coordinates of the plane.[38] He claimed that when the system satisfied some conditions, the self-oscillations mathematically corresponded to Poincaré's limit cycles.[39] As Dahan Dalmedico emphasized:

> Andronov had the not-at-all obvious idea, almost forty years after their publication, of turning towards Poincaré's works. These had almost never been applied to concrete problems of physics or engineering.[40]

To get this result, Andronov imposed conditions on the above system of equations which he would come back to, since they amounted to structural stability. Formally, he would do this in a note, written with the blind topologist, L. Pontrjagin, and presented at the Soviet Academy of Sciences in 1937, in which they introduced what they named

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[37] A. A. Andronov, "Les cycles limites de Poincaré et la théorie des oscillations auto-entretenues," *CRAS,* 189 (1929): 559-561, 559. In this note, he refers to the work of Volterra and Lotka in biology, of Kreman in chemistry, of Eddington in astrophysics, and of Lord Rayleigh in the theory of sound, in addition to van der Pol's.

[38] By setting $z = v'$, van der Pol's equation can be rewritten as such a system of equations, namely $v' = z; \quad z' = \varepsilon(1 - v^2)z - v$.

[39] Introduced in H. Poincaré, "Mémoire sur les courbes définies par une équation différentielle," *Journal de mathématiques pures et appliquées,* 3rd ser., 7 (1881): 375-422; 8 (1882): 251-296; repr. *Œuvres,* 1: 3-84; see pp. 53-65 on limit cycles, where he introduces what is now known as the Poincaré section. See J.-L. Chabert and A. Dahan Dalmedico, "Les idées nouvelles de Poincaré" in *Chaos et déterminisme,* ed. A. Dahan Dalmedico *et al.* (Paris: Seuil, 1992), 274-305; and C. Gilain, "La théorie qualitative de Poincaré et le problème de l'intégration des équations différentielles," *La France mathématique,* ed. H. Gispert (Paris: SFHST and SMF, 1991): 215-242.

[40] A. Dahan Dalmedico, "Le difficile héritage," 22.

"coarse systems [*systèmes grossiers*]."[41] A system of the above type was said to be coarse if a small variation to it did not alter the topological character of the trajectories of its solutions. They considered the altered system:

$$\frac{dx}{dt} = P(x,y)+p(x,y); \quad \frac{dy}{dt} = Q(x,y)+q(x,y);$$

where p, q, and their derivatives remained small. The condition for coarseness expressed that there existed a one-to-one correspondence between the trajectories of the two systems, such that singular points were sent to singular points, limit cycles to limit cycles, etc. Andronov and Pontrjagin then gave, without proofs, a complete classification of two-dimensional coarse systems.

In a revealing footnote, they added:

> This definition of a system's roughness can be considered as that of the stability of a dynamical system with respect to small variations. . . . *This kind of stability is interesting for physics.*[42]

There lay the source of Andronov's concerns for stability. Van der Pol's equation came about by studying radio engineering and this source left its imprint on the field of nonlinear mechanics. The case of Mary Lucy Cartwright, a Cambridge mathematician who worked extensively in this field starting in 1939, will underscore this.

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[41] A. A. Andronov and L. Pontrjagin, "Systèmes grossiers," *Comptes-rendus (Doklady) de l'Académie des sciences de l'URSS,* 14 (1937): 247- 250. See also the following note: E. Leontovich and A. Mayer, "Sur les trajectoires qui déterminent la structure qualitative de la division de la sphère en trajectoires," *ibid.,* 251-254. I preferred "coarse" rather than "rough," also found in the literature, as the translation to the French original term "*grossier*" since the former was used in the 1966 edition of Andronov et al., *Theory of Oscillators,* xxix.

[42] A. Andronov and L. Pontrjagin, "Systèmes grossiers," 247-248n. My emphasis.

(ii) Stability in Cartwright's Work

Although working within a totally different mathematical tradition, Cartwright nevertheless also studied, because of radio engineering concerns, the stability of equations of the type of van der Pol's. In 1952, she explained why this was a preoccupation for radio engineers:

> To me the work of radio engineers is much more interesting and suggestive than that of the mechanical engineers. The radio engineers *want their systems to oscillate,* and to oscillate in a very orderly way, and therefore they want to know not only whether the system has a periodic solution, but *whether it is stable,* what its period and amplitude and harmonic content are, and *how these vary with parameters of the equations.*[43]

This statement is the more striking considering that Cartwright's concerns and approach always remained quite different from Andronov's. In her account of the history of nonlinear mechanics, she did not mention Andronov's work, even though she paid attention to some of the Russian contributions, such as Krylov and Bogoliubov's.

Collaborating with John E. Littlewood, Cartwright started her research in the field of nonlinear vibrations after Britain's

> Department of Scientific and Industrial Research issued a memorandum appealing for the assistance of pure mathematicians in solving the type of equations occurring in radio work, *laying emphasis on the need to know how the frequencies of the periodic solutions varied with the parameters of the equations.*[44]

Beginning "with little knowledge of the classical work of Poincaré, Liapounov and Birkhoff," she never adopted topological methods such as Andronov and Pontrjagin's.[45] She studied specific systems, admittedly with parameters that could be

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[43] M. L. Cartwright, "Non-Linear Vibrations: A Chapter in Mathematical History," *Mathematical Gazette,* 36 (1952): 80-88, 84. My emphasis.
[44] M. L. Cartwright, "Non-Linear Vibrations," 86. My emphasis.
[45] M. L. Cartwright, "Non-Linear Vibrations," 87.

tinkered with, but never ventured into topological classifications like Andronov and Pontrjagin's. Cartwright and Littlewood's case, which so markedly departs from Andronov's program, only shows how much the practical incentive for the mathematical theory could bend research programs towards studying the stability of solutions in presence of perturbations.[46]

Van der Pol certainly took notice of Cartwright's work. "A certain phase of this subject [the theory of relaxation oscillations]," did he write in 1947, "was concluded a few months ago by highly important investigations carried out by Miss Cartwright and Littlewood." That a new phase was dawning may be apparent from the fact that the van der Pol equation was one of the first one to be integrated with the help of Vannevar Bush's differential analyzer at MIT.[47]

(iii) Stability as Program and Philosophy

As opposed to Cartwright and Littlewood, Aleksandr Andronov approached the study of nonlinear vibrations not only with new mathematical tools, but also with a vast philosophical program. As Vladimir Arnol'd emphasized, the concept of coarseness appeared in Andronov's work as both a mathematically rigorous definition and a general

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[46] See her most important work: M. L. Cartwright and J. E. Littlewood, "On Non-Linear Differential Equations of the Second Order: I. The Equation $y'' - k(1-y^2)y' + y = b\lambda k\cos(\lambda t + a)$, k Large," *Journal of the London Mathematical Society,* 20 (1945): 180-189; repr. in *The Collected Papers of John Edensor Littlewood,* 1 (Oxford: Clarendon Press, 1982): 85-94; and M. L. Cartwright, "Forced Oscillations in Nearly Sinusoidal Systems," *Journal of the Institute of Electrical Engineering,* 95 (1948): 88-94. These articles are important for the prehistory of chaos since they directly inspired Levinson's paper that exhibited an instance of strange attractor, that oriented Smale's discovery of the horseshoe. See below p. 287.

idea about the type of systems useful for mathematical modeling in physics and engineering.[48]

In the lengthy introduction to his book, Andronov set out to articulate his program, which he had previously presented in front the First All-Union Conference on Auto-oscillation in November 1931.[49] Concerned, as were van der Pol and Cartwright, with real physical systems, Andronov saw the problem of the fit between the model and reality as posing itself with urgency. "In any theoretical investigation of a real system we are always forced to simplify and idealize, to a greater or smaller extent, the true property of the system."[50] The question was: how far could one go?

Clearly, many properties of physical systems were lost in any kind of idealization. Due to fluctuations, in particular, "real systems," Andronov stated, "*cannot in general be described with complete accuracy by means of mathematical relations.*"[51] This was a radical position that clearly distinguished mathematical models from physical systems, and flatly denied the possibility of a straightforward identification of the two. On top of this metaphysical reason, unavoidable fluctuations were bound to occur in any real physical system whatever their origins: quantum mechanics or the always imperfectly controlled environment.

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47 B. van der Pol, "Mathematics and Radio," 1150. About Bush's analog computer, see A. G. Bromley, "Analog Computing Devices," *Computing Before Computers,* ed. W. Aspray (Ames: Iowa State University Press, 1990: 156-199, 179-185.
48 V. I. Arnol'd, "Catastrophe Theory," in *Dynamical Systems V: Bifurcation Theory an Catastrophe Theory,* ed. V. I. Arnol'd, Encyclopedia of Mathematical Sciences, 5 (Berlin: Springer, 1994 [1986]): 207-264, 224.
49 V. I. Arnol'd, "Catastrophe Theory," 260.
50 A. A. Andronov et al., *Theory of Oscillators,* xv.
51 A. A. Andronov et al., *Theory of Oscillators,* xxv. My emphasis.

> The presence of fluctuation in real systems must indirectly be taken into account even in the theory of dynamic models of real systems. It is evident that since small random perturbations are inevitable in all physical systems, *processes which are possible only in the absence of any random deviations or perturbations whatsoever cannot actually occur in them.*[52]

Consequently, for the mathematical physicist the object of study changed. Beyond the study of a system of differential equations, and its solutions, beyond even the study of equations depending on a set of parameters, one had to deal with more general families of laws. The implication was clear: "*we have always to allow for the possibility of small variations of the form of the differential equations which describe a physical system.*"[53]

For this reason, the notion of coarseness was crucial, since only coarse systems had a chance of being useful for physics. Only they could actually model the processes taking place in the real world. Andronov and coworkers indeed required that the processes they studied "be *stable both* in relation to *small variations of the coordinates and velocities, and* in relation to *small variations of the mathematical model itself.*"[54] They went on:

> The first requirement leads to the concept of stability of states of equilibrium of the model and of the processes taking place in it, and the second to the concept of coarseness of dynamic systems. . . . Systems that are such not to vary in their essential features for a small variation of the form of the differential equations, we shall call *'coarse'* systems.[55]

One should notice here an interesting analogy between Andronov and his colleagues' reasoning and a famous statement of Pierre Duhem's (1861-1916). In his famous book, the French physicist-philosopher set forth, along with some of Poincaré's

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52 A. A. Andronov et al., *Theory of Oscillators,* xviii(note). My emphasis. See also pp. xxvii-xxix.
53 A. A. Andronov et al., *Theory of Oscillators,* xxviii. My emphasis.
54 A. A. Andronov et al., *Theory of Oscillators,* xviii(note). My emphasis.

work, Jacques Hadamard's (1865-1963) construction of geodesics on surfaces with negative curvature.[56] In this work, Hadamard exhibited a mathematical construction which displayed the property later called "sensitive dependence on initial conditions" by Ruelle, and thus described by Hadamard:

> Any change, no matter how small, brought to the initial direction of [any] geodesics . . . is enough to bring about absolutely any variation to the final outlook of the curve.[57]

For Duhem, this implied that this "mathematical deduction" could "never be utilized" in physics.[58] The reasons for which he stated this resemble Andronov's:

> One cannot go through the numerous and difficult deductions of celestial mechanics and mathematical physics without suspecting that many of these deductions are condemned to eternal sterility.
>
> Indeed, a mathematical deduction is of no use to the physicist so long as it is limited to asserting that a given *rigorously* true proposition has for its consequence the *rigourous* accuracy of some such other proposition. To be useful for the physicist, it must still be proved that the second proposition remains *approximately* exact when the first is only *approximately* true.[59]

Andronov and his coworkers apparently concurred with Duhem's analysis. They even went a step further and asserted that mathematical models, to stand a chance of faithfully representing physical systems, had to be stable, not only with respect to small variations in the initial conditions, but also in the very form of the differential equation

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[55] A. A. Andronov et al., *Theory of Oscillators,* xviii(note) and xxix.
[56] On this, see J.-L. Chabert, "Hadamard et les géodésiques des surfaces à courbure négative," *Chaos et déterminisme,* ed. A. Dahan Dalmedico et al. (Paris: Seuil, 1992): 306-330; and the original paper: J. Hadamard, "Les surfaces à courbure opposées et leurs lignes géodésiques," *Journal de mathématiques pures et appliquées,* 4 (1898): 27-73; repr. *Œuvres,* 2 (Paris: Éditions du CNRS, 1968): 729-775.
[57] J. Hadamard, *Œuvres,* 2, 772-773; quoted in J.-L. Chabert, "Hadamard," 325.
[58] P. Duhem, *La théorie physique. Son objet et sa structure* (Paris: Marcel Rivière, 1914; 1ère édition, 1906); *The Aim and Structure of Physical Theory,* transl. Philip P. Wiener (Princeton: Princeton University Press, 1954), 138.

itself. As an important consequence, Andronov neglected unstable motions and emphasized the study of stationary ones, i.e. rest, equilibrium, periodic and quasiperiodic motions, in other words Birkhoff's *recurrent motions,* which Birkhoff himself characterized as "a natural extension of periodic motions."[60]

c) Birkhoff: Conventionalism for Stability

One of Poincaré's only "true disciples" in the qualitative study of differential equations, George David Birkhoff (1884-1944), offers another approach to be contrasted with Andronov's. Like the Russian mathematician and at about the same time, Birkhoff reflected on the role of stability for the mathematical modeling of the world.[61] Instead of focusing on a single concept, like coarseness, Birkhoff adopted the more supple view that different concepts of stability could be used for different purposes, depending on the questions one wanted to answer; the choice merely was conventional.

> The fundamental fact to observe here is that this concept [stability] is not in itself a definite one but is interpreted according to the question under consideration.[62]

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[59] P. Duhem, *The Aim and Structure,* 143.
[60] G. D. Birkhoff, "Quelques théorèmes sur le mouvement des systèmes dynamiques," *Bulletin de la Société mathématique de France,* 40 (1912): 305-323; repr. G. D. Birkhoff, *Collected Mathematical Papers,* 1 (New York: Dover, 1968 [1950]): 654-672, 654.
[61] Amy Dahan Dalmedico called Birkhoff "a true disciple" of Poincaré, although they probably never met. "Le difficile héritage," 24-27. For biographical information on Birkhoff and his work, see E. T. Whittaker, "George David Birkhoff," *Journal of the London Mathematical Society,* 20 (1945): 121-128; and the introductory essays in G. D. Birkhoff, *Papers,* 1.
[62] G. D. Birkhoff and D. C. Lewis, Jr., "Stability in Causal Systems," *Philosophy of Science,* 2 (1935): 304-333, 313; repr. *Papers,* 3: 575-604, 584. One should think here of Poincaré's philosophy of science often termed "conventionalism"; see D. J. Stump, *Conventionalism and Truth: Poincaré's Mediation Between Relativism and Absolutism in Science,* Ph.D. thesis (Nortwestern University, 1988).

Birkhoff always based his reflections on Poincaré's. Although Birkhoff was trained at Chicago, "Poincaré was Birkhoff's true teacher," once said Birkhoff's own student Marston Morse.[63] Shortly after Poincaré's untimely death in 1912, Birkhoff established his reputation by proving a conjecture known as "Poincaré's last geometric theorem."[64] According to Morse, this proof "was one of the most exciting mathematical events of the era and was widely acclaimed."[65] As Poincaré had already seen, this theorem had important consequences for dynamical theories.

Having read Poincaré's *Méthodes nouvelles de la mécanique céleste,* while at Princeton in 1912, Birkhoff started to work on the field he would call dynamical systems.[66] That year, he introduced the notions of "minimal" and "recurrent" motions.[67] His work on qualitative dynamics eventually culminated in his 1927 book, much of its content having been delivered on September 5-8, 1920 at the University of Chicago. According to Morse, "History has responded to these pages on Dynamical Systems in an unmistakable way," in that it shaped much of the work done by Kolmogorov, Arnol'd, and

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[63] O. Vleben, "George David Birkhoff (1884-1944)," *Yearbook of the American Philosophical Society* (1946): 279-285; repr. G. D. Birkhoff, *Papers,* 1: xv-xxiii.
[64] The theorem states that continuous, one-to-one, area-preserving maps from the annulus to itself that rotates the points on the boundaries in opposite directions have at least two fixed points. Henri Poincaré, "Sur un théorème de géométrie," *Rendiconti dei circolo matematico di Palermo,* 33 (1912): 375-407; repr. *Oeuvres,* 6: 499-538; and G. D. Birkhoff, "Proof of Poincaré's Geometric Theorem," *Transactions of the American Mathematical Society,* 14 (1913): 14-22; repr. *Papers,* 1: 673-681; French transl. *Bulletin de la Société mathématique de France,* 46 (1914): 1-12. See also G. D. Birkhoff, *Dynamical Systems,* 163-170.
[65] M. Morse, Preface to G. D. Birkhoff, *Dynamical Systems,* 2nd ed. (Providence: American Mathematical Society, 1966), iv.
[66] O. Vleben recalled Birkhoff's reading of Poincaré; see G. D. Birkhoff, *Papers,* 1: xv-xxiii.
[67] G. D. Birkhoff, "Quelques théorèmes."

Möser on the celebrated KAM theorem.[68] As we shall see, many scientists were inspired by it in less direct ways.

"The final aim of the theory of motion must be directed toward the qualitative determination of all possible types of motions and of the interrelation of these motions."[69] In Chapter 7 of his book, Birkhoff developed a "General Theory of Dynamical Systems," going further than Poincaré and Hadamard in the topological study of curves defined by differential equations.[70] In particular, he generalized Poincaré's limit cycles, by introducing several interesting concepts that prefigured different facets of the concept of *attractor*: non-wandering, minimal, alpha- and omega-limit sets, central and recurrent motions.[71] On the basis of these definitions, Birkhoff stated:

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68 M. Morse, Preface, v; see Möser's Introduction in *ibid.* also. About the history of KAM theorem, see F. Diacu and P. Holmes, *Celestial Encounters,* chap. 5. It is also briefly discussed below.

69 G. D. Birkhoff, *Dynamical Systems,* 189. See also G. D. Birkhoff, "Recent Advances in Dynamics," *Science,* n.s., 51 (1920): 51-55; repr. *Papers,* 2: 106-110.

70 See A. Dahan Dalmedico, "Le difficile héritage," 25; G. D. Birkhoff, *Dynamical Systems,* 189-202.

71 In Birkhoff's own words, "the set W of wandering points of M is made up of curves of motion filling open n-dimensional continua. The set M_1 of *non-wandering points* is made up of the complementary closed set of curves of motions (*Dynamical Systems,* 192)." Now, finding the non-wandering set M_2 with respect to M_1, and constructing the sequence M_1, M_2, etc., we must at some point end the process with a set C of *central motions. Recurrent motions* are those which come back arbitrary close to every point of the curve of motion. They are in the set of central motions but the reverse is not necessarily true. α- and ω-*limit points* are defined as the sets of limit points as time (t) tends to - or + ∞. Nonwandering sets are in general larger than limit sets. For these, and other, definitions, see G. D. Birkhoff, *Dynamical Systems,* 191-200. Some of these were picked up in A. A. Andronov and A. A. Witt, "Sur la théorie mathématique des auto-oscillations," *CRAS,* 190 (1930): 256-258. An attractor has been succinctly defined as "an *indecomposable, closed, invariant* set . . . which attracts all orbits starting at points in some neighborhood" by P. Holmes, "Poincaré, Celestial Mechanics, Dynamical-Systems Theory, and 'Chaos'," *Physics Reports,* 193 (1990): 137-163. For more on attractors, see below.

> a first problem concerning the properties of dynamical systems is the determination of the central motion. . . . [So,] the structure of the set of central motions is of vital theoretic importance."[72]

Generally speaking, central motions were those "which all other motions tend[ed] to approach."[73] Here is the seed of future focus on attractors.

The stability of the solution curves of dynamical systems was the central concern of Birkhoff in his book. He introduced a large array of notions of stability for dynamical systems and their periodic solutions, some of which already present in the literature, some of which new: complete or trigonometric stability, stability of the first order, permanent stability ("for which small displacements from equilibrium remain small over time"), semi-permanent stability, unilateral stability (due to Lyapunov), and stability in the sense of Poisson (due to Poincaré).[74]

Although stemming out of totally different worlds—clearly Harvard mathematics department and the Gorki Institute must have been worlds apart from one another—there are interesting comparison to be made between Birkhoff's and Andronov's approaches. While both dealt with general systems of (nonlinear) differential equations, using many of the same sources (Poincaré, Lyapunov), and while both emphasized stability as a way of probing these systems, they nonetheless ended up with almost opposite views on stability. For Andronov, the practice of mathematical modeling implied that only coarse systems were of interest. Birkhoff thought that one had to dictate, by convention or by a judicious choice of problems to be answered, the kind of stability that one wanted to look at.

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72 G. D. Birkhoff, *Dynamical Systems,* 197 and 202.

73 G. D. Birkhoff and D. C. Lewis, "Stability," 309.

74 The stability of motions is dealt with mostly in chapters 4, 6, 8 and 9 of G. D. Birkhoff, *Dynamical Systems*. The quote is from p. 121.

> All that stability can mean is that, for the system under consideration, those motions whose curves lie in a certain selected part of phase space from and after a certain instant are *by definition* called stable, and other motions unstable.[75]

Interestingly, both Birkhoff and Andronov reflected on the philosophy of mathematical modeling of physical facts. Unlike future Bourbakists, Birkhoff thought that without a doubt mathematics was the language of nature, which itself should guide mathematicians' speculations.

> It will probably be the new mathematical discoveries which are suggested through physics that will always be most important, for, from the beginning, Nature has led the way and established the pattern which mathematics, the language of Nature, must follow.[76]

Like his master Poincaré, Birkhoff was a mathematical physicist; he worked on ergodic theory, wrote books on relativity, and remained ever skeptical of quantum mechanics. Furthermore Birkhoff believed that mathematics could offer guidance for other aspects of the human experience, like aesthetics or even ethics.[77] He issued repeated calls for a further collaboration between physicists and mathematicians:

> It is to be hoped that in the future more and more theoretical physicists will command a deep knowledge of mathematical principles; and also that mathematicians will no longer limit themselves so exclusively to the aesthetic development of mathematical abstractions.[78]

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75 G. D. Birkhoff and D. C. Lewis, "Stability," 332. My emphasis.
76 G. D. Birkhoff, "The Mathematical Nature," 310; repr. 919. About Poincaré's recurrence theorem and Birkhoff's use of it, see A. Dahan-Dalmedico, "Le difficile héritage."
77 First presented at the 1928 International Congress of Mathematician ("Quelques éléments mathématiques de l'art," *Atti del Congresso internazionale dei matematici, Bologna, 3-10 settembre 1928 (VI),* 1 [Bologna: Nicola Zanichelli, 1928]: 315-333; repr. *Collected Papers,* 3: 288-306), Birkhoff's theory of aesthetics inspired him many articles to be found in the 3rd volume of his *Collected Papers,* and a book *Aesthetic Measure* (Cambridge: Harvard University Press, 1933), which however I have never seen.
78 G. D. Birkhoff, "The Mathematical Nature of Physical Theories," *American Scientist,* 31 (1943): 281-310, 286; repr. *Papers,* 2: 890-919, 895.

In many ways not too far from Thom's, Birkhoff's philosophy on the role of mathematics for building models oscillated between pure Platonism and a recognition that reality is never as simple as the mathematical model. For instance, Birkhoff, admittedly with a hint of irony, wrote that Poincaré's recurrence theorem entailed that:

> within a very large but finite period of time, our article will again appear on this same subject, in this same journal, read by the same individuals, as far as one may discern, and this will happen indefinitely often.[79]

But at the same time, Birkhoff and Lewis concluded their article on the stability of causal systems by expressing a modest goal for mathematical models of the universe:

> No matter how fascinating the purely mathematical study of causal systems may be, it would seem not to be desirable to take them too seriously from a realistic point of view as applicable to the actual universe. The real purpose of physical speculation is to enable us to calculate only within certain prescribed limits of error and for reasonable intervals of time the behavior of physical systems.[80]

Birkhoff however never went as far as Andronov in doubting the possibility of accurately modeling physical systems, and indeed the whole universe, with mathematical concepts. Inspired by the famous "problem of stability" of the Sun-Earth-Moon system, Birkhoff restricted the study of stability to that of orbits lying near a periodic (or central) motion. Concerned with radio systems, Andronov imagined a more general type of stability that applied not only to solutions of a system of differential equations, but to the system itself.

In summary, for contemporaries in the 1930s, it would have been almost impossible to juxtapose Andronov, Birkhoff, Cartwright, and van der Pol, as I just did, or to think of them as belonging to a single discipline. In 1952, Mary Cartwright described

[79] G. D. Birkhoff and D. C. Lewis, "Stability," 332.
[80] G. D. Birkhoff and D. C. Lewis, "Stability," 333.

the discipline she called "nonlinear vibration," as "rather a curious branch of mathematics developed by different people from different standpoints, straight mechanics, radio oscillations, pure mathematics and servo-mechanisms of automatic control theory"[81] Summarizing his "interdisciplinary" career—before the word even existed—Balthasar Van der Pol, however, could not help voicing regrets at the lack of communication between disciplines:

> In my thirty years experience of research work I have been struck time and time again by the fact that the mathematician speaks a different language from that of the physicist. . . . This difference of languages seems to me to be one of the obstacles standing in the way of mutual understanding and appreciation.[82]

When one refrain from projecting Smale's later synthesis on the past, the remarkable fact in this prehistory of dynamical systems theory becomes, not that communication across disciplinary boundaries proved so difficult, but that it indeed sometimes took place. These few contacts however framed some common basis for the mathematical and philosophical study of stability. The lack of a stable community with clearly defined problems, tools, and social borders was one of the major reasons for the "long neglect" felt by many actors. Many people working on closely related phenomena hardly had a chance to communicate, and this created a sense of isolation. As we have seen, Lefschetz was the one who came the closest to succeeding in creating a dynamic research school, with publications, students, and a charismatic leader.

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81 M. L. Cartwright, "Non-Linear Vibrations," 86.
82 B. van der Pol, "Mathematics and Radio," 1140.

4. AS IT GOES WEST, COARSENESS BECOMES STRUCTURAL STABILITY

a) Filling Wholes

Apparently, Solomon Lefschetz—who was born in Russia—was a close friend of Pontrjagin's.[83] They worked on closely related topics, and had high regards for each other's work. While working on the translation of Andronov and collaborators' book, Lefschetz's attention could not failed to be drawn to the concept elaborated in part by his friend.

However, Lefschetz judged that *Theory of Oscillations* needed to be adapted for an American audience. "It became evident quite early that considerable condensation, paring down and editing of the original was unavoidable if its value were not to be lost to the non-Russian reader." Lefschetz therefore eliminated from the text "many lengthy and purely theoretical discussions."[84] In particular, the philosophical introduction, described above, was condensed from 18 pages to merely 2, and much of its content was lost in the process. Without this, we may venture that many of Thom's and Ruelle's ideas might have appeared less novel at the time when they were formulated. In particular, the whole discussion about coarse systems was thrown away. To redress this, Lefschetz included an appendix in his translation, which more or less repeated the terms of the note published by Andronov and Pontrjagin in 1937. Like the original though, it contained no proof.

Lefschetz had a flair for names; he had coined the word "topology" as a "snappy title" for his 1930 monograph on a topic that had until then been called, following

[83] Albert W. Tucker to J. Douglas Brown (September 30, 1959). Princeton Arch.

Poincaré, *analysis situs*.[85] Not to Lefschetz's liking, the term "coarseness" was replaced by "structural stability." He thereby shifted the attention on the property rather than the systems satisfying it. Partly as a consequence, he nearly divorced the mathematical meaning of coarseness (or structural stability) from others of Andronov's concerns. Even if, in two sentences, he drew attention to the physical significance of structural stability, Lefschetz in effect demoted Andronov's idea from a methodological guide for the study of nonlinear systems to a useful, intriguing, but marginal and rather technical mathematical concept.[86]

It is striking to note that the members of Lefschetz's group who worked on structural stability seemed to have come to Princeton with prior personal interest in it. Lefschetz himself scarcely studied it at first. The group as a whole did not devote much of its energy to it.[87] Mainly this neglect was due to the fact that, as Dahan Dalmedico emphasized, the focus of Lefschetz's school slowly evolved from an analytic study of a few cases of concrete nonlinear oscillators, catering to perceived needs of engineering science, toward a more global, and ambitious, program of classification of dynamical

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[84] A. A. Andronov et al., *Theory of Oscillations,* vi.
[85] Press Release (October 6, 1972). Princeton Arch. AC#109 Box 39. Solomon Lefschetz, *Topology,* 2nd ed. (New York: Chelsea, 1956). The German word *Topologie* was the title of a book written in 1847 by one of Gauss's students, Johann Benedict Listing. Lefschetz also introduced the term "algebraic topology" instead of "combinatorial topology." A. W. Tucker, *History of Mathematics,* Course II-1962, NSF Institute, mimeographed lecture notes by A. K. Funderburg.
[86] "The physical necessity for this [structural stability] is fairly clear; in physical systems one never knows exactly what the functions P and Q are and so one will naturally exclude systems which are affected by ever so slight a modification of these functions." A. A. Andronov et al., *Theory of Oscillations,* 337-340.
[87] Cf. S. Lefschetz, "Nonlinear Differential Equations."

systems.[88] In fact, it was the attention placed on the notion of structural stability itself by people coming from outside which impelled its direction to this evolution.

Following the publication of *Theory of Oscillations,* Father Henry DeBaggis, a young professor at Notre Dame, joined the project in 1949 for two years, a reading of the appendix having "awakened" his interest in structural stability. In Lefschetz's plain words, "DeBaggis undertook to establish a complete theory and this objective was attained."[89] This work was facilitated by members of the Project. Without the assistance of Menachem Schiffer and D. C. Spencer, who joined the Project in 1949-50 and 1949-51, respectively "and that of Lefschetz it is safe to say that DeBaggis would never have succeeded in carrying his research successfully."[90]

In any case DeBaggis finally managed to provide all proofs omitted by Andronov and Pontrjagin. He showed that a necessary and sufficient condition for a system defined on a bounded region of the plane to be structurally stable was: the system (1) had at most a finite number of singular points which can only be nodes, foci or saddle points; (2) no separatrix joining saddle points; and (3) at most a finite number of limit cycles.[91]

In plain English, structurally stable systems were *simple.* Their trajectories tended towards equilibrium or periodic solutions; and these were finite in number. Given a

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88 A. Dahan Dalmedico, "La renaissance des systèmes dynamiques."
89 S. Lefschetz, "Nonlinear Differential Equations," 15-16.
90 S. Lefschetz, "Nonlinear Differential Equations," 12.
91 To make this statement fully acceptable to a mathematician, additional technical conditions should have been imposed. See H. F. DeBaggis, "Dynamical Systems with Stable Structures," *Contributions to the Theory of Nonlinear Oscillations,* 2, ed. S. Lefschetz, Annals of Mathematics Series, 29 (Princeton: Princeton University Press): 37-59, esp. 48. Also S. Lefschetz, *Differential Equations: Geometric Theory* (New York: Interscience, 1957), 239-245; and A. Dahan Dalmedico, "La renaissance des systèmes dynamiques," 147-148.

system of differential equations, one was therefore justified in only looking for these simple solutions; provided, that is, that structurally stable systems were common enough to be of any use. And it was for this purpose only that Andronov's arguments in favor of the physical significance of coarseness now played any role.

"In the study of nonlinear problems it is difficult for the mathematician to find rich classifications of nonlinear systems which are sufficiently homogeneous in their properties to yield an interesting theory."[92] This had always been the central question for the study of nonlinear differential equations. There was no use in studying particular differential equations, except when one had a good reason to. A general method of solution seemed out of reach. And no class of equations seemed to emerge from the mathematical investigation alone. Structurally stable systems appeared to DeBaggis as a class that, resorting to Andronov's arguments, seemed rich enough to be relevant to the mathematical modeling of physical phenomena. Stability requirements "provide a clue to the restrictions a mathematician should place on his nonlinear problems."[93] In DeBaggis's work, this paragraph, certainly inspired by Andronov, played no further role than providing a justification for the mathematical study of structural stability.

b) A Density Theorem by Peixoto

Lefschetz apparently showed great interest for DeBaggis's work. In his 1957 book on the geometric study of differential equations, Lefschetz mentioned it in the introduction.[94]

[92] H. F. DeBaggis, "Dynamical Systems," 37.
[93] H. F. DeBaggis, "Dynamical Systems," 37.
[94] S. Lefschetz, *Differential Equations*. See A. Dahan Dalmedico, "La renaissance des systèmes dynamiques," 147.

The same year, he welcomed two mathematicians coming from Brazil, again with a specific interest for structural stability: they were Marilia and Mauricio Peixoto. "A man of rare enthusiasm, and most careful thinker," Lefschetz judged, "Mauricio soon made noteworthy contributions to this most delicate topic."[95] Following Andronov and Pontrjagin, DeBaggis had found necessary and sufficient conditions for a two-dimensional dynamical system to be structurally stable. Lefschetz had termed these "general systems." Tackling the problem by using a fully topological approach, Peixoto went a step further. He proved that "most" dynamical systems on the two-dimensional sphere were structurally stable. In technical terms, he showed the set of all structurally stable systems on two-dimensional manifolds was an open dense subset of the space containing all dynamical systems.[96] In other words, not only structurally stable systems were very common, but on top of this, any system could be approximated by one.[97]

This was a crucial step. Until then, the justification for studying structurally stable systems had been provided by philosophical arguments à la Andronov. It was assumed that structural stability could translate accurately more or less vague assumptions about the physical stability of systems under consideration. With Peixoto's density theorem, rigorous mathematical arguments grounded the belief that every dynamical system (in

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[95] S. Lefschetz, " Nonlinear Differential Equations," 21.
[96] A subset A of Ω is said to be *open* if every point in A is surrounded by points belonging to A; it is said to be *dense* in Ω if every point in Ω either belongs to A, or lies arbitrarily close to A.
[97] M. M. Peixoto, "On Structural Stability," *Annals of Mathematics,* 69 (1959), 199-222; "Structural Stability on Two-Dimensional Manifolds," *Boletín de la Sociedad matemática mexicana,* 5 (1960) [Proceedings of the Symposium on Ordinary Differential Equations and their Applications, Universidad nacional autónoma de México, 7-13 September, 1959], 188-189; *Topology,* 1 (1962), 101-120.

two dimensions) could always be approximated by a structurally stable one. Structurally stable systems were thus the only ones susceptible of accurately representing reality, and this for mathematical reasons alone.

It is hard to believe that mathematics alone could dictate what kind of models are to be found in nature. The following example shows plainly that, even after Peixoto's theorem, metaphysical assumptions could not be totally removed from discussions about modeling. For a long time one of the most successful mathematical models, the harmonic oscillator turns out to be *not* structurally stable. It can indeed be approximated by a slightly anharmonic oscillator (with a very small friction term).[98] In the long run, their solutions are however very different since the anharmonic oscillator, no matter how small the friction term, will always tend towards rest. The harmonic oscillator, on the other hand, will oscillate forever. Does this mean that the harmonic oscillator is useless for the mathematical modeling of reality? No, it just means that it represents an imperfect idealization of reality, a fact that was long known. Moreover, when symmetry considerations impose that energy is conserved (think of quantum field theory), the harmonic oscillator might even be an exact representation of reality.

To express his density theorem, Peixoto introduced a new word into the theory of differential equations: structurally stable systems were "generic," he wrote in 1962 (with quotation marks).[99] It was through René Thom, who had picked it up in the mid-1950s for

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[98] The anharmonic oscillator system can be written as follows: $dy/dt = \gamma y - x; \quad dx/dt = y$. The harmonic oscillator has the same form without the γ-term. In the C^1 topology used by Peixoto, the distance between the two systems tends to zero as γ does.

[99] M. Peixoto, "Structural Stability," *Topology,* 101.

his study of singularities, that Peixoto became familiar with the term.[100] The idea of using a similar concept in the theory of differential equations however went back to Poincaré (he studied the considerably different case of solutions that have probability 1 of happening, excluding exceptional trajectories from consideration), and had been picked up more or less rigorously by Hadamard, Birkhoff, Cartwright-Littlewood, and Eberhard Hopf.[101] Genericity was however a tricky concept to use, and often was the cause of much confusion.[102]

In his final report to the ONR, Solomon Lefschetz lauded Mauricio Peixoto's work in the following terms:

> Especially noteworthy is his introduction of a metric space *S* of differential equations . . . and showing that under a suitable definition one may consider the structural[ly stable] systems as dense in *S*. . . .The work of Mauricio Peixoto during his stay with the Project, his ebullient and enthusiastic attitude were so outstanding that when . . . RIAS was organized in the Fall of 1957, he was asked to join it for the following year (1958-59).[103]

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[100] See R. Thom, "Les singularités des applications différentiables," *Séminaire Bourbaki,* 7, exposé #134 (May 1956), "Un lemme sur les applications différentiables," *Boletín de la Sociedad matemática mexicana,* 2nd ser., 1 (1956): 59-71, 59-60.

[101] On Poincaré's probabilist concepts, see A. Dahan Dalmedico, "Le difficile héritage," 17; J.-L. Chabert et A. Dahan Dalmedico, "Les idées nouvelles," 296-303; and M. W. Hirsch, "The Dynamical Systems Approach," 21. See G. D. Birkhoff, *Dynamical Systems,* 197; M. L. Cartwright and J. E. Littlewood, "On Non-Linear Differential Equations," 182n; E. Hopf, "A Mathematical Example Displaying Features of Turbulence," *Communications on Applied Mathematics,* 1 (1948): 303-322, 305.

[102] See M. W. Hirsch, "The Dynamical Systems Approach," 35-36. Also see A. Weil, "Correspondence," *American Journal of Mathematics,* 79 (1957): 951-952. Written in Italian, this anonymous letter, which shows using an argument of Thom's that abusive use of genericity could lead classical Italian algebraic geometers to erroneous results, was attributed to Weil by René Thom.

[103] S. Lefschetz, " Nonlinear Differential Equations," 21-22. For a more technical discussion of the work of DeBaggis and Peixoto about structural stability, see again A. Dahan Dalmedico, "La renaissance des systèmes dynamiques," 145-148.

Starting with Andronov and Pontrjagin's article, coarseness and later structural stability had been restricted to dynamical systems with two variables. Besides simplicity, a good reason for this limitation was that these systems represented second order differential equations: the most useful ones in dynamics. Nothing in the definition provided in 1937 forbade an extension to higher dimensions. By introducing such a definition, Peixoto thus opened up vast uncharted territories. Under Stephen Smale's lead, the program of classifying structurally stable systems in *n* dimensions would provide an important incentive for studying dynamical systems in the years to come. In the process, structural stability would, for the first time, reach a wide audience.

5. SMALE'S 'BAD' CONJECTURE AND THE HORSESHOE: 'AN ADMIRABLE BATTLE'

"Smale made a bad conjecture." Thus does James Gleick begin his description of Stephen Smale's work which would lead him to forge his famous horseshoe.[104] 'Bad' is here a bad choice of word. Smale's conjecture was indeed shown (by himself!) to be faulty. But in a way it was as successful a conjecture as can be. It was the logical follow-up of decades of research on structural stability, and furthermore it led to an unprecedent boom in the study of dynamical systems. One of Smale's students, Bob Williams, described the benefits of his mentor's audacity: "he's brave enough to make the conjectures, so we got to play with them."[105]

[104] J. Gleick, *Chaos,* 45.
[105] R. F. Williams in *From Topology to Computation,* 179.

In charge of presenting Smale's work to the 1966 International Congress of Mathematicians at Moscow, René Thom emphasized his special ability of suggesting fruitful directions for mathematical research:

> If Smale's works perhaps do not possess the formal perfection of definitive work, it is because Smale is a pioneer who take risks with a tranquil courage; in a completely unexplored domain, in a mathematical jungle of inextricable wealth, he is the first to have shown the way and placed the first milestones.[106]

Late in the summer of 1958, at Princeton, Peixoto met Smale. And the latter, a topologist, showed some interest for Peixoto's work on structural stability. "I was delighted to see this interest," remembered Peixoto; "at that time, hardly anybody besides Lefschetz cared about structural stability."[107] Peixoto was hitting on a problem. Having generalized the definition of structural stability to higher dimensions, he was looking for an equivalent to DeBaggis's theorem in n dimensions. In particular, condition (2) above (see p. 276), which stated that no separatrix ran between two saddle points (the "no saddle-connection condition"), was not obvious to transpose. Smale, using a notion introduced by Thom, found a solution to this problem.

a) The Topologists' Hand

Born in 1932 in Flint, Michigan, Steve Smale received his Ph.D. in 1956 in "a new branch of mathematics called topology" with Raoul Bott at the University of Michigan.[108] That

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[106] R. Thom, "Sur les travaux de Stephen Smale," *Proceedings of the International Congress of Mathematicians (Moscow, 1966):* 25-28, 28.
[107] M. M. Peixoto, "Some Recollections of the Early Work of Steve Smale," in *From Topology to Computation: Proceedings of the Smalefest,* ed. M. W. Hirsch et al. (New York: Springer, 1993): 73-75, 73.
[108] S. Smale, "Chaos: Finding a Horseshoe on the Beaches of Rio," 2. Written for a meeting in Rio de Janeiro, in March 1996, celebrating the 45th anniversary of the National Research Council of Brazil (CNPq), this article was posted on the Web by

summer, Smale went to a topology symposium at Mexico City, where he met Thom and two graduate students from the University of Chicago Morris W. Hirsch and Elon Lima.[109] These encounters decisively shaped Smale's later involvement with dynamical systems.

The next fall at Chicago, where Smale got his first teaching position, Thom lectured on transversality theory, which generalized the notion of secant for manifolds and topological spaces. Three years later, Smale would use the notion of transversal intersection in order to solve the problem that stopped Peixoto.[110] Moe Hirsch became Smale's first, though "informal," student and, later, his colleague at Berkeley in the

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Smale himself. I do not know if it has been published. The information for this section was provided by this article and S. Smale, "On How I Got Started in Dynamical Systems (1959-1962)," (partly based on a talk given at a Berkeley seminar circa 1976), in *Mathematics of Time* (New York: Springer, 1980) [hereafter *MT*]: 147-151; "The Story of the Higher Dimensional Poincaré Conjecture (What Actually Happened on the Beaches of Rio)," *The Mathematical Intelligencer,* 12(2) (1990): 44-51; both repr. in *From Topology to Computation,* ed. M. W. Hirsch et al.: 22-26 and 27-40; and Smale's interview in *More Mathematical People: Contemporary Conversations,* ed. D. J. Albers, G. L. Alexanderson, and C. Reid (Boston: Harcourt Brace Jovanovich, 1990): 305-323. See also J. Palis, "On the Contribution of Smale to Dynamical Systems," in *From Topology to Computation,* 165-178.

109 S. Smale, "On How I Got Started," 147.

110 Cf. J. Palis, "On the Contribution of Smale," 166. In technical terms, the stable and unstable manifolds at limit sets (fixed points or limit cycles) are defined as the sets of points that tend towards the limit sets as t goes to, respectively, $+\infty$ or $-\infty$; Smale's condition was that the stable and unstable manifolds intersect transversally. For definitions, see S. Smale, "Morse Inequalities for a Dynamical Systems," *Bulletin of the American Mathematical Society,* 66 (1960): 43-49, 46-47; and Earl A. Coddington and Norman Levinson, *Theory of Ordinary Differential Equations* (New York: McGraw Hill, 1955), 330-333. About Thom's lecturing at Chicago, see S. Smale, "How I Got Started," 148, and "The Story of the Poincaré Conjecture," 29. References for transversality: see R. H. Abraham, "Transversality in Manifolds of Mappings," *Bulletin of the American Mathematical Society,* 69 (1963): 470-474; R. H. Abraham and J. Robbin, *Transveral Mappings and Flow* (New York: Benjamin, 1967); and the excellent book by V. Guillemin and A. Pollack, *Differential Topology* (Englewood Cliffs: Prentice Hall, 1974), Chap. 2.

second half of the 1960s at the time when they developed dynamical systems theory with a whole new generation of students. Regarding their exceptional complementarity, John R. Stallings once wrote: "Smale is the Mad Genius and Hirsch is the Hard Worker."[111] Finally Elon Lima, a student from Brazil, was responsible for introducing Mauricio Peixoto to Steve Smale, when the latter moved to the Institute for Advanced Study at Princeton with a two-year NSF postdoctoral fellowship late in the summer of 1958.

Why was Smale interested at all in Peixoto's work on structural stability? His domain of expertise then hardly overlapped with the field of mathematics he came to. In fact, Smale recounted retrospectively, he at once saw that topology could prove a first class tool for this study. "I was immediately enthusiastic," he wrote, "not only about what he [Peixoto] was doing but with the possibility that, *using my topology background,* I could extend his work to *n* dimensions." [112] The involvement of renown topologists confirmed his feeling. "I believe that it was the topologist's, Pontryagin and Lefschetz, hand in the subject that contributed to the fact that I was ready to listen to Mauricio."[113]

In any case, as a offshoot of the contact he had with Peixoto, Smale wrote two papers in 1959, in which he made his famous and bold conjecture. Smale suggested that the equivalent in more than two dimensions of Lefschetz's general systems—those used by DeBaggis—was a necessary and sufficient conditions for structural stability. Thom

[111] Quoted in S. Smale, "The Story of the Poincaré Conjecture," 34. Hirsch is called Smale's "informal" student by J. Palis, "On the Contribution of Smale," 175.
[112] S. Smale, "On How I Got Started," 148. My emphasis.
[113] S. Smale, "Chaos," 13. There is a bit of black humor in this statement since in 1907 Lefschetz lost his two hands in a factory accident, a tragic accident that determined his decision of becoming a mathematician instead of an engineer. See A. Dahan Dalmedico, "La renaissance des systèmes dynamiques," 133-134.

later called this class of dynamical systems "Morse-Smale systems."[114] Smale's conjecture had two parts:

> (A) It seems at least plausible that [Morse-Smale systems] form an open dense set in the space . . . of all vector fields. . . .
>
> (B) It seems likely that the conditions [for a system to be Morse-Smale] are necessary and sufficient conditions for [the system] to be structurally stable in the sense of Andronov and Pontrjagin.[115]

We therefore see that Smale's celebrated conjecture actually was a pair of conjectures. Proposition (A) generalized Peixoto's density theorem, while (B) extended DeBaggis's theorem, to higher dimensions. Together they implied that most dynamical systems were structurally stable. To sense how bold it was to suggest this, let us note, as Peixoto did in 1962, that it was not known, at the time, whether on any n-dimensional manifold, structurally stable systems even existed.[116]

Armed with Morse-Smale systems, "Smale began an admirable battle to have a global description (if only conjecturally) of 'most' of the world of dynamics, still hoping that the stable systems formed an open dense subset of it."[117] About fifteen years later, Smale acknowledged that he "was extremely naive about ordinary differential equations at that time and was also extremely presumptuous." His "overenthusiasm" had led him to suggest that Morse-Smale "systems were almost all (an open dense set) of ordinary

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114 See S. Smale, "How I Got Started,"148. See Jacob Palis, "On Morse-Smale Dynamical Systems," *Topology,* 8 (1969): 385-405. Morse-Smale systems therefore consisted of the hyperbolic dynamical systems which had only a finite number of fixed points and closed orbits as their limit sets (in this condition, limit sets were later replaced by nonwandering sets; see J. Palis, "On the Contribution of Smale," 167), and whose stable and unstable manifolds at the limit sets intersected transversally.
115 S. Smale, "Morse Inequalities," 43.
116 M. M. Peixoto, "Structural Stability," *Topology,* 101.
117 J. Palis, "On the Contribution of Smale," 170.

differential equations!"[118] In 1996—ten years after the publication and popular success of Gleick's book—Smale stated this conjecture as "chaos does not exist!"[119] Smale had however been warned not to be so bold.

> Peixoto told me that he had met Pontryagin, who said that he didn't believe in structural stability in dimensions greater than two, but that only increased the challenge. . . . If I had been at all familiar with the literature (Poincaré, Birkhoff, Cartwright-Littlewood), I would have seen how crazy this idea was.[120]

b) 'My Best-Known Work Was Done on the Beaches of Rio'[121]

In September 1959, Steve Smale presented his conjecture at the Symposium on Ordinary Differential Equations and their Applications in Mexico City. At this international conference—one of the first convened by Lefschetz's group after the end of the Project—many specialists who had at one time or another come to Princeton as part of the Project attended. But newcomers and outsiders were also present: René Thom, Georges Reeb, and, of course, Steve Smale.[122] Considering the exposure it received, it is therefore somewhat surprising that Smale's false conjecture was greeted with a certain success in the mathematical community he was addressing.

In December 1959, invited by Peixoto and Lima, Smale left for the Instituto de matemática pura e aplicada (IMPA) in Rio de Janeiro, Brazil. Shortly after his arrival, he received a letter from MIT mathematician Norman Levinson. As Smale recalled,

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118 S. Smale, "On How I Got Started," 148.
119 S. Smale, "Chaos," 4.
120 S. Smale, "On How I Got Started," 148.
121 Smale to Connick, Vice-Chancellor of Academic Affairs at the University of California, Berkeley, unpublished, but quoted in Daniel S. Greenberg, "The Smale case: NSF and Berkeley Pass through a Case of Jitters," *Science,* 154 (October 7, 1966): 130-133; and S. Smale, "the Story of the Poincaré Conjecture," 39.

Levinson "had coauthored the main graduate textbook in ordinary differential equations [which Smale had cited]. He was a scientist to be taken seriously."[123] According to Smale, Levinson's unpublished letter informed him that one could not expect Morse-Smale systems to occur so generally, and that one of his own papers already contained a counterexample for conjecture (B) above.

(i) Ancestors of the Horseshoe

Like Cartwright and Littlewood, Levinson participated in the effort spurred by World War II by studying the van der Pol and Liénard equations. Building on their work, Levinson studied in 1948 the solutions of the Liénard equation with forcing:

$$y'' + p(y)y' + y = c \sin t;$$

where y' and y'' represented first and second derivatives with respect to t. Among the solutions of the forced Liénard equation, Levinson showed that a family F exhibited a "remarkably singular structure."[124] Levinson emphasized that, contrary to relaxation

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[122] Proceedings were published in the *Boletín de la Sociedad matemática mexicana,* 5 (1960).

[123] S. Smale, "Chaos," 4. See also S. Smale, "On How I Got Started," 149. Smale is referring to E. A. Coddington and N. Levinson, *Theory of Ordinary Differential Equations.* Note furthermore that Earl Coddington acted as deputy-director of Lefschetz's Project from September 1957 to September 1958, thus just barely overlapping with Smale at Princeton. On Coddington's role in the Project, see S. Lefschetz, "Nonlinear Differential Equations," 12.

[124] Norman Levinson, "A second Order Differential Equation with Singular Solutions," *Annals of Mathematics,* 50 (1949): 127-153, 153. For his previous work in the field, see N. Levinson, and O. Smith, "A General Equation for Relaxation Oscillations," *Duke Mathematical Journal,* 9 (1942): 382-403; and N. Levinson, "Transformation Theory of Non-Linear Differential Equations of the Second Order," *Annals of Mathematics,* 45 (1944): 723-737, where he suggested that a "bad" curve of Birkhoff's type could possibly emerge from the forced van der Pol equation (p. 736).

oscillations studied by van der Pol and his followers, "most of the solutions of F are certainly *not* periodic."

As Levinson noted, similar behavior, which they qualified as "very bizarre," had already been observed by Cartwright and Littlewood just a few years before.[125] They had exhibited an infinite set of "non-periodic trajectories, of the type described as 'discontinuous recurrent [motion]'."[126] This was how Birkhoff had described recurrent motions that seemed not to be of such a trivial type as steady or periodic motions. In phase space, such discontinuous recurrent motion defined, in the notation adopted by both Cartwright-Littlewood and Levinson, a set K_0, which was connected, of measure zero (zero area), and which separated the plane in two open subsets, a bounded and an unbounded one. Moreover all motions in a neighborhood tended towards K_0 as t went to infinity. But since it had different rotation numbers for limit points of interior or exterior points, K_0 could not be a simple Jordan curve. "In fact," Ralph Abraham noted much later, such curves "are fractals."[127] In the terminology of Ruelle and Takens, they were *strange attractors* (Chapter VII).

When Cartwright and Littlewood hit upon such strange sets, which they dared not call "curves," they looked for comfort in the literature. As they wrote in their article, "our

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[125] M. L. Cartwright and J. E. Littlewood, "On Non-Linear Differential Equations," 182.
[126] M. L. Cartwright and J. E. Littlewood, "On Non-Linear Differential Equations," 183; for a description of discontinuous recurrent motions, see G. D. Birkhoff, "Surface Transformations and Their Dynamical Applications," *Acta Mathematica,* 43 (1922): 1-119, chap. 5; repr. *Papers,* 2: 111-229. See also M. Morse, "Recurrent Geodesics on a Surface of negative curvature," *Transactions of the American Mathematical Society,* 22 (1921): 84-100.
[127] R. H. Abraham, "In Pursuit of Birkhoff's Chaotic Attractor," in *Singularities and Dynamical Systems,* ed. S. N. Pnevmatikos (Amsterdam: North-Holland, 1985): 303-312, 303.

faith in our results was one time sustained only by the experimental evidence" provided by van der Pol and van der Mark.[128] Hearing through headphones the noise produced by the frequencies corresponding, or so they wanted to believe, to periodic solutions of the van der Pol equation, they made the following observation:

> Often an irregular noise is heard in the telephone receivers before the frequency jumps to the next lower value. However, this is a subsidiary phenomenon, *the main effect being the regular frequency multiplication.*[129]

The irregular noise heard by the Dutch experimenters might have been the mark of a continuous spectrum, which were later interpreted as revealing the presence of strange attractors. Let me remark that more than sixty years later, Ruelle suggested that chaotic attractors should be easy to observe experimentally in oscillating electric circuits.

> It should be possible to visualize the transition to continuous spectrum. . . . Alternatively, if frequencies are in the audible range, the transition to continuous spectrum should correspond to a change in the musical nature of the corresponding sound. These experiments . . . have not yet been performed as far as I know. Since they are easy, I strongly suggest that they should be attempted.[130]

In their search for previous observation of strange sets, Cartwright and Littlewood also found comfort in Birkhoff's work. In 1932, in the words of Abraham, "Birkhoff published a remarkable paper on *remarkable curves.*"[131] Such curves arose in the study of mappings from an annulus to itself considered in Poincaré's last geometric theorem. Birkhoff acknowledged that he was at first surprised by the existence of such curves,

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[128] M. L. Cartwright and J. E. Littlewood, "On Non-Linear Differential Equations," 182n.
[129] B. van der Pol and J. van der Mark, "Frequency Demultiplication," *Nature,* 120 (1927): 363-364. My emphasis.
[130] D. Ruelle, "Sensitive Dependence on Initial Conditions and Turbulent Behavior of Dynamical Systems," *Annals of the New York Academy of Sciences,* 316 (1978): 408-416; repr. *TSAC,* 175-184, 182. Ruelle's claim was not false since, van der Pol and van der Mark's experimental observations did not correspond exactly to what he suggested.
[131] R. H. Abraham, "In Pursuit," 303.

having for a while exploited them for a "*reductio ad absurdum*" that failed.[132] In 1935, Marie Carpentier was able to construct explicitly such a strange curve.[133] Cartwright and Littlewood suggested that their K_0 was an example of "bad" topological behavior comparable to Birkhoff's remarkable curves. But Birkhoff's and Carpentier's curves were invariant sets of an analytic mapping from the plane to the plane, *not* dynamical recurrent sets.

Using the technique of the Poincaré map, Levinson (and Cartwright and Littlewood before him) defined a transformation associated with the differential equations as such: given (y, v), where $v = y'$, and $y(t)$ is a solution of the forced Liénard equation Levinson was considering, he defined a transformation T from the plane to the plane by $T(y,v) = (y_1, v_1)$, where $y_1 = y(t+2\pi)$ and $v_1 = y'(t+2\pi)$.[134] By looking at the invariant set of T, Levinson showed what Cartwright and Littlewood had only hinted at, namely that Birkhoff's "curves" (with quotation marks) "could arise from the transformation associated with a differential equation [which previously] was not known."[135] This was the transformation that Smale would have to translate into his own geometrical way of thinking before he would come up with the horseshoe.

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[132] G. D. Birkhoff, "Sur quelques courbes fermées remarquables," *Bulletin de la Société mathematique de France,* 60 (1932): 1-26; repr. *Collected Papers,* 2: 418-443, 443.
[133] M. Carpentier, "Sur les courbes fermées analogues aux courbes de Birkhoff," *Journal de mathématiques pures et appliquées,* 9th ser., 14 (1935): 1-48.
[134] Poincaré's "first return map," which allowed to replace the study of orbits of a differential equation by that of discrete recurrences $x_{n+1} = f(x_n)$ defined on a lower dimensional surface, is discussed in J.-L. Chabert and A. Dahan Dalmedico, "Les idées nouvelles," 292-295.
[135] N. Levinson, "A Second Order Differential Equation," 129.

A striking aspect of Levinson's, as well as Cartwright and Littlewood's, analyses consisted in the realization that the behavior they observed was robust. The strange "curves" arose, not for exceptional, isolated values of the parameters in the equations, but for some continuum; they could not be perturbed away. And this is exactly what Smale reported that Levinson had written him. A counterexample to one of Smale's conjectures (conjecture (A)) could be extracted from these papers.

(ii) Smale's Geometric Translation of Levinson: The Horseshoe

With modesty, Steve Smale often emphasized that his horseshoe merely was "a natural consequence of a geometrical way of looking at the equations of Cartwright-Littlewood and Levinson", "an abstract geometrization of what [they] had found more analytically before."[136] To destroy conjecture (A), Smale had to come up with a system (and an open set of systems around it) whose limit set contained either an infinite number of the trivial limit sets he had admitted (fixed points or periodic cycles) or a nontrivial set that was neither a point nor a cycle.

"Still partly with disbelief, I spent a lot of time studying [Levinson's) paper," Smale recalled.[137] He had a hard time trying to "translate Levinson's analytic argument into my own geometric way of thinking." Eventually, he became convinced that Levinson was correct and that his conjecture was wrong. "I had guessed wrongly. But while

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[136] S. Smale, "Chaos," 5; "On How I Got Started," 149. Already in 1964, Smale acknowledged Levinson's letter: see S. Smale, "Diffeomorphisms with Many Periodic Points," *Differential and Combinatorial Topology: A Symposium in Honor of Marston Morse,* ed. S. S. Cairns (Princeton: Princeton University Press, 1965): 63-80, 64.
[137] S. Smale, "On How I Got Started," 149.

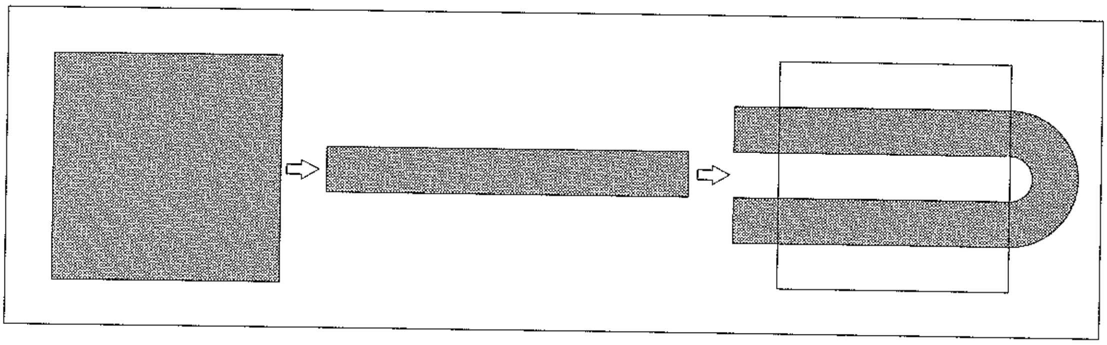

Figure 10: Smale's Horseshoe. By repeating this transformation an infinite number of times, one get a recurrent set whose section has the structure of a Cantor set.

learning that, I discovered the horseshoe!"[138] Since the horseshoe has very often been described, I refrain to do this once again.[139] Let me just say that this nonlinear mapping of the plane to the plane possessed an infinite number of periodic points, as well as nonperiodic sequences (Fig. 10).

But I do want to emphasize a few consequences of Smale's work with the horseshoe. With this counterexample, Smale showed not only that there existed diffeomorphisms with an infinite number of periodic points, but also that slight perturbations of the horseshoe exhibited the same property. For him, this showed that dynamical systems could not be generically so simple as he expected: there existed other types of limit sets, besides points and cycles. This infinite number of periodic points formed a structure similar to that of a Cantor set. This was quite complicated behavior. But was this mapping really dynamics?

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[138] S. Smale, "Chaos," 5.

[139] The original description is to be found, but in an abstract form very hard to recognize, in S. Smale, "Diffeomorphisms with Many Periodic Points," 74. For an excellent discussion of the horseshoe which is quite readable, see F. Diacu and P. Holmes, *Celestial*

In Smale's earlier work, dynamical systems remained equivalent to differential equations. If, since Poincaré, it had been possible to associate a mapping with them, the inverse process—that is, associate a differential equation to a mapping—was not obvious. Smale focused on Levinson's mapping, thus making an important conceptual shift from differential equations to mappings, homeomorphisms and diffeomorphisms, without extracting them from any system of differential equations.[140]

In fact, Smale subsumed the study of ordinary differential equations $dx/dt = f(x, dx/dt)$, or as Birkhoff had said, dynamical systems, under the study of diffeomorphisms. Smale regrouped this program under the heading of "dynamical systems," which thereby acquired a new, wider meaning. In 1962, Smale justified his reasons for doing this as such:

> It appears that usually a qualitative problem in differential equations has an analogue in the conjugacy problem [for diffeomorphisms]. This analogue is a little simpler than the original, and if solved, its solution seems to give a way of doing the original problem.[141]

He announced that he would provide details on how to do this in a following publication, "Stable manifolds for differential equations and diffeomorphisms," which was never published. Smale however supplied some clues in his famous paper "Differentiable Dynamical Systems" published in the *Bulletin of the American Mathematical Society* in 1967. An important point however remained: granted that they might be more complicated than previously conjectured by Smale, were structurally

Encounters, chap. 2; and I. Ekeland, *Mathematics and the Unexpected* (Chicago: University of Chicago Press, 1988), 70-73.

[140] Homeomorphisms are continuous mappings; diffeomorphisms are differentiable homeomorphisms with a differentiable inverse.

[141] S. Smale, "Dynamical Systems and the Topological Conjugacy Problem," 490.

stable systems dense? Could any differential system be approximated by a structurally stable one?

c) 'An Unfinished Painting with Several Superposed Sketches'

Mathematically speaking, but also on a political level, Steve Smale was a busy man in the 1960s. This might explain why, his enthusiasm for the topic notwithstanding, he worked on dynamical systems only intermittently in the few years following his stay at Rio. Nonetheless, starting from his interest in structural stability, Smale would, before the end of the decade, synthesize in an unexpected manner several older traditions, destroy hopes for genericity of structural stability, set new terms for a rich research program in dynamical systems, and establish his own school at Berkeley in later part of the 1960s. This school produced most of the workers that would shape the field in the following decades.

(i) Poincaré Again: The Homoclinic Tangle

Still at Rio, having worked out the horseshoe, Smale found in Poincaré's work an analogue of this complicated dynamic situation. "I learned about homoclinic points and Poincaré's work," he recalled in 1996, "from browsing in Birkhoff's collected works which I found in IMPA's library." He went on:

> Unfortunately, the scientific community had lost track of these important ideas surrounding homoclinic points of Poincaré. In the conferences in differential equations and dynamics that I attended at that time there was no awareness (if the conferees had known of this work of Poincaré and Birkhoff, the conjectures in my rather publicized talks would have been answered earlier). Even Levinson never showed in his book, papers, or correspondence with me that he was aware of homoclinic points. It is astounding how important scientific ideas can get lost,

even when they are exposed by leading mathematicians of the preceding decades.[142]

It was not quite true that these ideas had been totally forgotten. At about the same time, renewed interest in the theory of integrable Hamiltonian systems, such as those involved in celestial mechanics, started to be felt. Starting from a famous talk of Andrei Nikolaevich Kolmogorov (1903-1987) at the Amsterdam International Congress of Mathematicians in September 1954, KAM (Kolmogorov-Arnol'd-Möser) theory was developed, which as we have seen exploited Birkhoff's work.[143] The question Kolmogorov asked was what happened to the solutions of a conservative system when the equations were slightly perturbed. This should remind us Andronov's concerns with coarse systems.

In the course of the following decade, work inspired by Kolmogorov's theorem turned Hamiltonian dynamics, from the "hopelessly obsolete, outmoded and purely

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142 S. Smale, "Chaos," 12. For a survey of the results of Poincaré and his followers concerning homoclinic points, see Karl Gustav Anderson, "Poincaré's Discovery of Homoclinic Points," *Archives for History of Exact Science,* 48 (1994): 133-147; June Barrow-Green, *Poincaré and the Three-Body Problem* (Providence: American Mathematical Society, 1997); and F. Diacu and P. Holmes, *Celestial Encounters.*
143 Again, for an easy-going introduction, and an unsophisticated historical discussion of, KAM-Theory, I refer the reader to F. Diacu and P. Holmes, *Celestial Encounters,* ch. 5. See also the original articles: A. N. Kolmogorov, "On Conservation of Conditionally-Periodic Motions for a Small Change in Hamilton's Function" (in Russian), *Doklady Akademi Nauk SSSR,* 98 (1954): 525; transl. in *Selected Works of A. N. Kolmogorov,* ed. V. M. Tikhomirov, 1 (Dordrecht: Kluwer, 1991): 349-354 and *Chaos II,* 107-112; V. I. Arnol'd, "Small Denominators II: Proof of a Theorem of A. N. Kolmogorov on the Preservation of Conditionally-Periodic Motions under a Small Perturbation of the Hamiltonian," *Russian Mathematical Surveys,* 18(5) (1963): 13-40; Jürgen Möser, "On Invariant Curves of Area-Preserving Mappings of an Annulus," *Nachrichten der Akademie der Wissenschaften in Göttingen,* 1 (1962): 1-20. A. N. Kolmogorov's 1954 address was published (in Russian) as "Théorie générale des systèmes dynamiques et mécanique classique," *Proceedings of the International Congress of Mathematicians,*

formal branch of analytical mechanics" it once was, into "a fashionable branch of mathematics."[144] In due time, this work "appeared as the most important breakthrough in the subject [celestial mechanics] since the fundamental difficulties were first recognized by Poincaré in 1892."[145] Among the scientists whose numerical work was triggered by KAM, we may count some who would extract from simple systems extremely complicated behavior that would play crucial roles in the emergence of chaos, in particular astronomer Michel Hénon, and then physicist Joseph Ford.[146]

It was true, however, that a trench had slowly been dug between the study of conservative and dissipative systems. Smale's introduction of homoclinic points in the study of general (dissipative) systems helped a rapprochement between these fields of study, but did not close the gap. Especially with the concept of attractor soon becaming central to the study of dynamical systems, much emphasis remained placed on dissipative systems.

Considering Smale's interest in purely topological matters, on the one hand, and his pursuit of complicated structures in dynamical systems, on the other, it is hardly surprising that he could not help be attracted towards some of Poincaré's old results in the

Amsterdam, 1954, 1 (Amsterdam: North-Holland, 1957): 315-333; transl. in *Selected Works,* 1: 355-374.

144 V. I. Arnol'd, "On A. N. Kolmogorov," *Golden Years of Moscow Mathematics,* ed. S. Zdravkovska and P. L. Duren (Providence: AMS, 1993): 129-153, 132.

145 H. K. Moffatt, "KAM-Theory," *Bulletin of the London Mathematical Society,* 22 (1990): 71-73.

146 M. Hénon and C. Heiles, "The Applicability of the Third Integral Of the Motion: Some Numerical Experiments," *Astronomical Journal,* 69 (1964): 73-79. J. Ford and G. H. Lunford, "Stochastic Behavior of Resonant Nearly Linear Oscillator Systems as the Nonlinear Coupling Approaches Zero," *Physical Review,* A1 (1970): 59-70; "On the Stability of Periodic Orbits for Nonlinear Oscillators in Region Exhibiting Stochastic Behavior," *Journal of Mathematical Physics,* 13 (1972): 700-703.

later field. Indeed, working on the structural stability of gradient systems—those later used preferably by Thom—Smale "noticed how dynamics led to a new way . . . to attack the Poincaré conjecture, . . . and before long all my work focused on that problem."[147] Smale had been introduced to this problem as early as 1955. Not without difficulty, he proved the Poincaré conjecture in dimensions greater than five, which in 1966 "probably warrant[ed] his presence" at the Moscow Congress where he was awarded the Fields Medal.[148] It is to be noted that the proof of higher dimensional Poincaré conjecture became for a while a matter of priority quarrels between him and others, including British topologist E. C. Zeeman who would play a first rank role in the later development of qualitative dynamics.[149]

Poincaré's very definition of homoclinic points requires the study of (in anachronistic language, but in a form easy to recognize) stable and unstable manifolds.[150] This example together with the horseshoe therefore comforted Smale in giving away all hope for the genericity of Morse-Smale systems. In his 1962 address at the International Congress of Mathematicians in Stockholm, Smale emphasized homoclinic points, rather than the horseshoe, both of which he then saw as intimately linked. Homoclinic points provided him an example for his theorem G, which stated that there existed structurally stable systems with an infinite number of periodic points (and minimal set homeomorphic to a Cantor set). On this occasion, Smale quoted (in French) Poincaré's dramatic description of homoclinic points:

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[147] S. Smale, "The Story of the Poincaré Conjecture," 32. Smale wrote a paper "On Gradient Dynamical Systems," *Annals of Mathematics,* 74 (1961): 199-206.
[148] R. Thom, "Sur les travaux de Stephen Smale," 25.
[149] See S. Smale, "The Story of the Poincaré Conjecture."

> When one tries to depict the figure formed by these two curves and their infinity of intersections, . . . these intersections form a kind of net, web, or infinitely tight mesh. . . . One is struck by the complexity of this figure that I am not even attempting to draw. Nothing can give a better idea of the complexity of the 3-body problem and of all the problems of dynamics in general.[151]

By then, Smale had encountered still another example of complex dynamics. Witnesses are unanimous in saying that it was Thom who, in 1960, started asking whether the following diffeomorphism from the torus to itself was structurally stable: $x \to 2x+y \pmod{2\pi}$; and $y = x+y \pmod{2\pi}$.[152] This map possessed a dense set of periodic orbits. Moreover a dynamical system could be constructed starting from it, which shared all these properties.[153] The flow obtained by this system was similar to the geodesics on negative curvature surfaces, studied by Hadamard, and taken up by Birkhoff, Morse, and E. Hopf.[154] But was it stable?

(ii) A Russian Encounter

The horseshoe was first presented in internal seminars at the University of California, Berkeley, where Smale had accepted a job for July 1960. He also spoke at a conference on ordinary differential equations in Colorado Springs during the summer of 1961 (before

150 M. W. Hirsch, "The Dynamical Systems Approach," 36-38.
151 Henri Poincaré, *Méthodes nouvelles de la mécanique céleste,* 3 (Paris: Gauthier-Villars, 1899), 389. Quoted and transl. in June Barrow-Green, *Poincaré and the Three-Body Problem,* 162. Only the last two sentences were quoted by S. Smale, "Dynamical Systems and the Topological Conjugacy Problem," 494.
152 M. W. Hirsch, "The Dynamical Systems Approach," 38. J. Palis, "On the Contribution of Smale," 170-171. This map, known as "Arnol'd's cat" is discussed in V. I. Arnol'd and A. Avez, *Problèmes ergodiques de la mécanique classique* (Paris: Gauthier-Villars, 1967), 7-8 and 45-58, and I. Ekeland, *Mathematics and the Unexpected,* 50-59.
153 V. I. Arnol'd and A. Avez, *Problèmes ergodiques,* 48-50.
154 See J.-L. Chabert, "Hadamard," 329-330.

he moved to Columbia were he taught until 1964), and then in September 1961, at a meeting on nonlinear oscillations in Kiev, Ukraine, where decisive encounters took place.

In Kiev, Smale presented the horseshoe as "the first structurally stable dynamical system with an infinite number of periodic solutions."[155] He also asked whether the flow obtained from Thom's automorphism of the torus was structurally stable, conjecturing it was. In USSR, Smale met Kolmogorov and, "in his words, an extraordinarily gifted group of mathematicians: Anosov, Arnol'd, Novikov and Sinai."[156] The following year, at the Stockholm Congress, Smale was now convinced that this flow provided him with a first example of dynamical system in the form of a differential equation which was structurally stable, but not Morse-Smale. He then learned that Arnol'd and Sinai had proved the conjecture and published their result.[157] Moreover, Anosov had introduced a vast class of structurally stable flow with complicated dynamics later known as Anosov flows.[158] The first part of Smale's conjecture was definitely crushed by the horseshoe, homoclinic points, and Thom's torus map.

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[155] S. Smale, "On How I Got Started," 150. Horseshoe published in: S. Smale, "A structurally stable differential homeomorphism with an infinite number of periodic solutions," *Report on the Symposium on non-linear Oscillations* (Kiev Mathematical Institute, 1961): 365-366 (which I have never found); "Structurally Stable Systems are not Dense," *American Journal of Mathematics,* 88 (1966), 491-496; "Differential Dynamical Systems," *Bulletin of the American Mathematical Society,* 73 (1967), 747-817; repr., with additional notes and references, in *MT,* 1-82

[156] J. Palis, "On the Contribution of Smale," 170.

[157] V. I. Arnol'd and Ya. G. Sinai, "Small Perturbations of the Automorphisms of the Torus," *Soviet Mathematics,* 3 (1962): 783-787; 4: 560.

[158] See V. I. Arnol'd and A. Avez, *Problèmes ergodiques,* ch. 3. D. V. Anosov, "Roughness [i.e. Coarseness] of Geodesic Flows on Compact Riemannian Manifolds of Negative Curvature," *Soviet Mathematics,* 3 (1962): 1068-1070.

In the summer of 1961, his proof of the Poincaré conjecture behind him, Smale recalls he made "a very clean break with topology."[159] He wrote: "I announced to my friends that I had become so enthusiastic about dynamical systems that I was giving up topology." The reason he gave was that "no problem in topology was as important and exciting as the topological conjugacy problem for diffeomorphisms." This "problem represented the essence of dynamical systems, I felt."[160] His address at the 1962 Stockholm Congress was the result. It deserves that we examine it here.

(iii) What That Allowed in Mathematics?

"There is a paper of Smale that I particularly like to discuss," Smale's student Jacob Palis wrote about his teacher's 1962 address.

> When I first read it about 3 years later, I was initially amazed and taken by it. It looked to me like an unfinished painting with several superposed sketches: Was that allowed in Mathematics? It was certainly very inspiring then as well as three (and many more) years later. *Some results were announced in the paper, but mostly it made transparent his struggle at the time to reach the right concept of hyperbolic systems.* [161]

It was in this paper that Smale made clear that he was now enlarging the concept of dynamical systems to include differential equations as well as mappings (homeomorphisms and diffeomorphisms). He noted that both these problem could be seen as special cases in the study of "a non-compact Lie Group G acting differentiably on a manifold, corresponding to G=**R** and G=**Z**."[162] Structural stability could be equally well

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[159] S. Smale in *More Mathematical People,* 311.
[160] S. Smale, "On How I Got Started," 150.
[161] J. Palis, "On the Contribution of Smale," 171. Original emphasis.
[162] S. Smale, "Dynamical Systems and the Topological Conjugacy Problem," 490.

defined for both cases; Smale named the problem of defining an open dense set of diffeomorphisms the "conjugacy problem."

In the rest of the article, Smale toyed with several axioms trying to define an interesting class of diffeomorphisms forming an open dense set. Having to acknowledge nontrivial minimal sets, he nonetheless clung to his original hope that the class of diffeomorphisms defined by his axioms would coincide with the sets of structurally stable diffeomorphisms, with however a hint of uncertainty. "Although very possibly, in the final picture, [the above class] will not be the structurally stable diffeomorphisms, it seems that to date it is the best guess for such."[163]

Smale's address therefore contained at least as many unanswered questions and open problems as theorems. The last paragraph shows his enthusiasm:

> Certainly the main problems stated here are very difficult. On the other hand, it seems quite possible to us that this field may develop rapidly and already as indicated here, there have been some initial steps in this direction.[164]

But even before the end of the year, while in Lausanne, Smale was already drifting away: "I had begun to start thinking about the calculus of variations and infinite-dimensional manifolds, and this preoccupation took me away from dynamical systems for the next three years."[165] Until 1965 therefore. Until after his move back to Berkeley in 1964.

As an introduction to the course on differential topology he gave at the University of Columbia in 1962/63, Smale remarked:

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163 S. Smale, "Dynamical Systems and the Topological Conjugacy Problem," 492.
164 S. Smale, "Dynamical Systems and the Topological Conjugacy Problem," 495.
165 S. Smale, "On How I Got Started," 151.

> Recent events in differential topology indicate a change of direction is taking place, away from manifolds and toward differentiable mappings and analysis. In this course the new direction will be followed with global calculus of variations as the main goal.[166]

d) Steve Smale's Research School of Dynamical Systems

(i) 'The Heady Wonderful Years of the Mid-Sixties'[167]

In 1964, Steve Smale moved back to the University of California, Berkeley. During the 1960s, the mathematics department at Berkeley was at the center of political agitation whose main objective was to oppose the war in Vietnam.[168] Smale, who in the past had belonged to the Communist Party, got directly involved in the protests. Together with Jerry Rubin, he organized national "Days of Protest" against the Vietnam War.[169]

On the last day of the Moscow congress, where in 1966 he had received his Fields Medal, Smale held a press conference at the request of the North Vietnamese Press. He denounced the American military intervention in Vietnam as "horrible." Simultaneously, he made the parallel with the Soviet invasion of Hungary ten years earlier. "Never could I see justifications for Military Intervention, 10 years ago in Hungary or now in the much more dangerous and brutal American Intervention in Vietnam." On the same occasion, Smale protested against renewed McCarthyism as witnessed by the actions of the House

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[166] R. H. Abraham, *Lectures of Smale on Differential Topology,* Columbia University 1962/63. Jussieu Lib.

[167] N. Koppel, "Dynamical Systems and the Geometry of Singularly Perturbed Differential Equations," *From Topology to Computation,* ed. M. W. Hirsch et al., 545.

[168] See W. J. Broad, "Unabom Case Is Linked to Antiwar Tumult on U.S. Campuses in 1960's," *The New York Times* (June 1, 1996), 8.

[169] See S. Smale's interview in *More Mathematical People,* ed. D. J. Albers, et al., 305-309; and S. Smale, "The Story of the Poincaré Conjecture," 38-40.

Un-American Activities Committee (HUAC), and the internal situation in the Soviet Union, where "even the most basic means of protest are lacking."[170]

After he had left for Europe, but before his press conference, Smale was subpoenaed by the HUAC. Being already abroad, he did not learn of this before he went to Moscow. All of these events were largely reflected in the press, including the *San Francisco Examiner* and *The New York Times.* The political implications of the case became obvious when in 1966 Representative Richard Roudebush began to attack Smale on the House floor and elsewhere. "The Congressman looked into Smale's background and he's about as pink as they come," Roudebush was quoted as saying.[171]

All this created an unhealthy political situation for the National Science Foundation, which had been supporting, not only Smale's personal research, but also of his whole group at Berkeley. NSF support was withdrawn during the following year, for official reasons that had to do with Smale's management of his funds and technicalities. The charges were nicely encapsulated by the following statement by President Johnson's Science Advisor, Donald Hornig:

> The blithe spirit leads mathematicians to seriously propose that the common man who pays taxes ought to feel that mathematical creation should be supported with public funds on the beaches of Rio de Janeiro or in the Aegean Islands.[172]

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[170] The text of Smale's statement at the Moscow press conference was published in: C. Morrey, "The Case of Stephen Smale," *Notices of the American Mathematical Society,* 14 (October 1967): 778-782, 778. About this episode, see Smale's account "On the Steps of Moscow University," *Mathematical Intelligencer,* 6(2) (1984): 21-27; repr. *From Topology to Computation,* ed. M. W. Hirsch et al.: 41.
[171] C. Morrey, "The Case of Stephen Smale," 780.
[172] D. Hornig, "A Point of View," *Science,* 161 (1968): 248; quoted in S. Smale, "The Story of the Poincaré Conjecture," 39.

Eventually, after an extensive investigation by Daniel Greenberg, editor of *Science,* it appeared that all charges brought by NSF against Smale were unfounded, and that at the time of its refusal, NSF possessed evidence to that effect. NSF held a policy of "not knowingly giv[ing] nor continu[ing] a grant in support of research for one who is an avowed Communist or anyone established as being a Communist." Since this was not Smale's case, the official conclusion was that "the known facts regarding Professor Smale's activities do not constitute a basis for action."[173] Protests, the exchange of numerous letters, and campaigns in the press succeeded in having the NSF backtrack and renew, although cutting it by half, the grant of the global dynamics group at Berkeley.

Whether Smale then considered his political activities as directly bearing on his mathematical work, I cannot say. In the early 1970s, however, partly as a consequence of his contact with the IHÉS mathematicians, Smale began to concern himself with applications of his purely mathematical work. In this context, he thought, social and political questions had to be raised. In a talk delivered at the International Union of Pure and Applied Physics Conference on Statistical Mechanics, held in Chicago, in March 1971, Smale tackled these issues:

> These days especially, provocative questions confront a *socially conscious scientist* when he begins contemplate where applications of his work might lead. As one whose main work has been in pure mathematics, and who is beginning to concern himself with areas of applied mathematics such as electric circuit theory, *I*

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173 Enclosures of a letter from Leland J. Haworth to Charles Morrey (July 24, 1967), repr. in C. Morrey, "The Case of Stephen Smale," 783-784. The story of the NSF episode is chronicled in the following articles: Dan Greenberg, "Smale and BSF: A New Dispute Erupts," *Science,* 157 (1967): 1285; "Handler Statements on Smale Case," *Science,* 157 (1967): 1411; "The Smale Case: Tracing the Path that Led to NSF's Decision," *Science,* 157 (1967): 1536-1539; "Smale: NSF Shifts Position," *Science* 158 (1967): 98; "Smale: NSF's Records Do Not Support the Charges," *Science,* 158 (1967): 618-619.

wonder to what extent I should explicitly direct my work towards socially positive goals. [174]

Apparently Smale then considered directing his political activities away from anti-war protests and towards a reflection on the "many issues on the relations of the profession of a scientist to the social crises of this time." At Chicago, he did not pursue such discussion, but stated that he felt "that mathematicians and scientists in general must face these questions in a much more serious way than they have done to date (myself included)."[175] For reasons opposite to Lefschetz's, Smale certainly considered his work on global analysis as potentially reaching towards "socially positive goals," as he said.

The mathematics department at Berkeley had also been infused early in the 1950s by a noteworthy student coming out from Lefschetz's Project on nonlinear analysis. "Certain it is that upon pursuing his work [Stephen] Diliberto formed a real group of young and capable disciples at Berkeley."[176] Did Diliberto leave a lasting legacy that could still be felt in mid-1960s when Smale got there? Nothing is less certain, as Smale never acknowledged any debt to him.

However, even more than his predecessor, Steve Smale certainly did build a very strong school while at Berkeley. Thom testified to this: "he created a whole school of Dynamicists (Abraham, Pugh, Shub, Bowen, Franks, Robbins, Newhouse), a very brilliant one which completed his results on the characterization problem for structurally

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[174] S. Smale, "Personal Perspective on Mathematics and Mechanics," *Statistical Mechanics: New Concepts, new problems, New Applications,* ed. S. A. Rice et al. (Chicago: University of Chicago Press, 1972): 3-12; repr. *MT,* 95-105, 95. My emphasis.
[175] S. Smale, "Personal Perspective," 95. Note that, on November 22, 1978, Smale gave a talk at the IHÉS on "Game Theory and the Dynamics of Disarmament (resolving the Prisoner's Dilemma)." *Rapport scientifique 1978 - Séminaires et conférences,* 1. Arch. IHÉS.

stable diffeomorphisms."[177] At the *Smalefest,* a celebration for his sixtieth birthday organized by his ex-students, many, like Sheldon Newhouse, recalled with nostalgia the "atmosphere that existed in the sixties, with all the excitement and developments happening virtually every other week."[178] A partial list of his students is impressive for anyone acquainted with later development of dynamical theories: Nancy Koppel (Ph.D. 1967), Michael Shub (1967), Jacob Palis (1968), Sheldon Newhouse (1969), Rufus Bowen (1970), John Guckenheimer (1970).[179] In addition, Moe Hirsch, Oscar Lanford, and Charles Pugh were also present. A remarkable crew indeed.

(ii) Structurally Stable Systems Are Not Dense, So What Is Next?

Since Smale's conjecture had two parts, it took two step to show it false: (1) show that there were structurally stable systems with an infinite number of periodic solutions: Smale went as far with the horseshoe; 2) show that structurally stable systems were not generic. A counterexample showing the falsity of the second part of his conjecture, which Smale seems to have started to suspect as early as 1962, was published in 1966, in an article simply called "Structurally Stable Systems Are Not Dense."

There, Smale constructed an open set U of vector fields on a four-dimensional manifold such that no vector field in U was structurally stable. Smale's hope for replacing Andronov's philosophical basis for restricting the study of differential equations by a formal mathematical one was challenged.

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176 S. Lefschetz, "Nonlinear Differential Equations," 17.
177 *Note sur l'oeuvre de Stephen Smale,* par Thom (n.d., prob. 1972). Arch. IHÉS.
178 *From Topology to Computation,* 183. See also J. Palis, "On the Contribution of Smale," esp; 177-178; and N. Koppel, "Dynamical Systems," 545.

The example in this paper surely reduces the importance of the notion of structural stability," he had to admit. "One might be further discourages from studying the global qualitative theory of higher dimensional, ordinary differential equations.[180]

In Smale's view, pure mathematical rigor could not justify anymore too strong an emphasis on structural stability. But this hardly meant that his dream of finding rigorous mathematical bases for focusing on a well-defined class of dynamical systems as the only ones susceptible of being of interest for the modeling of nature had to be forsaken. Smale still believed that the study of the qualitative theory of differential equations could be "constructive." He announced he was preparing a paper "in this direction based on an axiom, which we consider to be of central importance, axiom *A*."[181]

A major synthesis of dynamical systems theory, containing a wealth of new results and promising research avenues, this paper set the ground for later studies in the field, including catastrophe and chaos theories. Smale's seminars out of which it grew were a major factor in binding the Berkeley school together. Presenting in detail the recently exhibited examples of dynamical systems with complex behavior, Smale struggled for unity. The goal still was to find "a class of diffeomorphisms which include all of the previous examples in a transparent way *and will at least have the possibility* of including an open dense subset of" all diffeomorphisms.[182]

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[179] *From Topology to Computation,* 184. See the list of Smale's graduate student in *Ibid.,* 59-63.
[180] S. Smale, "Structurally Stable Systems Are Not Dense," *American Journal of Mathematics,* 88 (1966): 491-496, 494. My emphasis.
[181] All quotes above come from S. Smale, "Structurally Stable Systems," 494.
[182] S. Smale, "Differentiable Dynamical Systems," *Bulletin of the American Mathematical Society,* 73 (1967): 747-817; *MT,* 1-82. Quote on 776. My emphasis.

A few years later Smale had to acknowledge that he had posed the problem in "too simple, too rough and too centralized" a way. "I believe now that the main problems of dynamical systems can't be unified so elegantly."[183] The "Dark Realm of dynamics," which did not fall into the domain defined by Smale's axioms, was in the next decade shown to be much "bigger."[184] Nonetheless Smale's "Differential Dynamical Systems" remained the standard reference on the topic for the many years to come and provided the founding stone for much of the later development of theories of chaos.

Given its formidable ambition of classifying, on a purely mathematical basis, dynamical systems general enough to be useful in the modeling of natural phenomena, it is striking to note how abstract, if not abstruse, Smale's program remained in those days. "I cannot recall in my four years at Berkeley having seen many actual differential equations," Nancy Koppel remembered.[185] Speaking of those years, Jacob Palis wrote:

> I like to remember the many discussions we had about dynamics and mathematics, in general, almost never about details and almost always about ideas, directions, theories and this is a concrete way.[186]

Apparently still under Bourbaki's spell, Smale thought that mathematical purity and abstractness could not be divorced from each other.

As a consequence of his search for unity, Smale defined dynamical system in the broadest way he thought possible. His stated purpose was to deal with the action of a Lie group G on a manifold M, the action being defined as "a homomorphism $G \rightarrow \mathrm{Diff}(M)$

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[183] S. Smale, "Stability and Genericity in Dynamical Systems," *Séminaire Bourbaki,* 22 (February 1970), exposé #374; repr. *MT,* 90-94. Quote on 91.
[184] J. Palis, "On the Contribution of Smale," 178.
[185] N. Koppel, "Dynamical Systems and the Geometry of Singularly Perturbed Differential Equations," in *From Topology to Computation,* ed. M. W. Hirsch et al.: 545-556, 545.

such that the induced map $G \times M \rightarrow M$ is differentiable."[187] This hardly was a language that most physicists, not too speak of specialists of other disciplines, were ready to hear.

In the early 1970s, however, as a result of his extensive contacts with the IHÉS, Smale began to talk to economists, physicists, and biologists. His approach, he acknowledged at the 1971 Statistical Physics Conference,

> may indeed be a little hard to accept for the applied mathematician trained in traditional methods. However, the approach here tends to make applied mathematics and also ordinary differential equations accessible and attractive to the modern mathematician, on who has been brought up in the purist, Bourbaki style of education.[188]

The problems of applied mathematics had to be transformed in order to cater to the expectations of Bourbakist mathematicians, rather than asking the latter to adapt their tools to the problems! For several decades, Smale thought, mathematics had developed as "very separated from applications." Acknowledging "a certain disdain for applied mathematics by many leading mathematicians," he saw "some healthy sides to this separation." But now, as a socially conscious scientist, Smale believed it "worthwhile to change this course of events," by bringing some "modernizations" to fields of applied mathematics. Although exuding condescension, this call of Smale's would be widely heard. It is true that, in this endeavor, he was greatly helped by a group of mathematicians and physicists who orbited IHÉS.

[186] J. Palis, "On the Contribution of Smale," 177.
[187] S. Smale, "Differentiable Dynamical Systems," 747.
[188] S. Smale, "Personal Perspectives," 100.

6. HISTORIOGRAPHY OF CHAOS: A QUESTION OF TIMING

"Chaos was discovered by Poincaré."[189] This statement is among the most common clichés about chaos.

Chaos has had a short history, and the history of historical accounts of it is even shorter. Some historiographical questions have nevertheless surfaced insistently. Among them, the most pressing remains the conundrum of having to explain why such a burst of activity could happen, on the basis of old mathematical ideas, very often going back to Henri Poincaré's work often seen in a new light. If the essential features of chaos, chiefly sensitive dependence on initial conditions, had been known for so long, how are we to account for the "revolution" of the last decades?[190]

Thus a theme focusing on the "nontreatment" of chaos emerged in the literature.[191] As was immediately recognized, simply to state this scarcely solved the problem. One had to explain it. Several reasons emerged: the physicists' interest in other theories than classical mechanics (relativity and quantum theories); social emphasis on stability in the scientists' training and research practices, as well as on theories which enhanced human control over nature rather then our understanding of its processes, be it for reasons that had to do with technological, philosophical, or even specifically masculine *a priori* dogmas about the nature of science.

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[189] F. Diacu and P. Holmes, *Celestial Encounters,* 78.

[190] The most important advocate for this revolutionary view was of course J. Gleick's book, *Chaos.*

[191] Present in scientists' account, this theme was best articulated by Stephen Kellert, *In the Wake of Chaos,* 119-128.

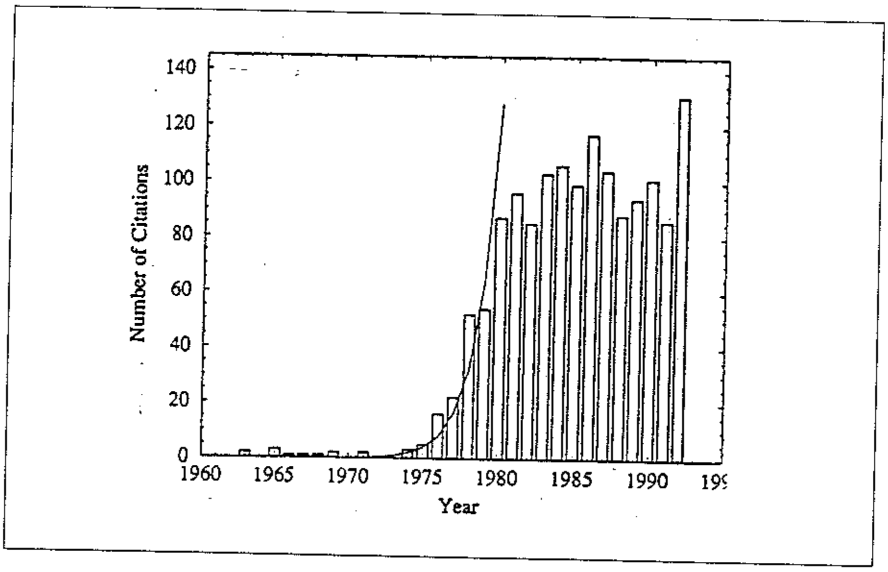

Figure 11: Citations to Edward Lorenz, "Deterministic Nonperiodic Flow," *Journal of the Atmospheric Sciences,* 20 (1963): 130-141. Repr. with permission from J. B. Elsner and J. C. Honoré, "Ignoring Chaos," *Bulletin of the American Meteorological Society,* 75 (1994):1846. Copyright © The American Meteorological Society.

(i) Traditions, Synthesis, and Topology

The above has shown that, contrary to what is often claimed, it rather seems that the quest for stability was, at least in cases of Smale (and in Thom's also, as we shall see in Chapter VI), the dominant motivation for studying those systems which, perhaps were *not* stable in the sense of Poisson (sensitive dependence on initial conditions), but still were (structurally) stable in a topological sense. The search for stability far from hindering the emergence of chaos thus crucially contributed to it.

Moreover, it is obvious from the above that Poincaré's ideas and techniques were never lost to the scientific community as a whole. Several decades of work on these ideas allowed mathematicians and physicists to go much farther than Poincaré might have thought possible. As I mentioned above, the major weakness in the social makeup of the communities that exploited these ideas lay in their dispersion. Conditions for uniting the several strands of research that were more or less directly inspired by Poincaré's qualitative theories did not exist before Smale's synthesis.

But, as Edward Lorenz noted, "one may argue that an early outburst [of research on chaos] was not *caused* by a prevailing lack of interest; it *was* the lack of interest" (Fig. 11).[192] With this striking graph, the chaos craze is made especially clear. After ten years of neglect, the number of citation grew exponentially, at a rapid pace, for about 7 years, from 1974 to 1980. It then stabilized at around 100 citations per year over the following decade. Of course, the surge of interest for Lorenz's paper, which I call the chaos craze, is not the cause, but the effect of the emergence of chaos. This lack of interest and the subsequent craze for catastrophe and chaos theories led some scientists to ponder on the roles that fashion might play in orienting their research.[193] If we take this view seriously, the question of the "nontreatment of chaos" would then amount to ask, for example: Why were miniskirt *not* in fashion before the sixties?! This would be a clearly ridiculous way of putting the nonetheless interesting historical question of determining what social changes in Western societies could be seen as being reflected the miniskirt fashion.

This analogy, no matter how stretched it might appear, offers us a way into our historiographical conundrum. Instead of looking for reason why scientists did not exploit Poincaré's ideas and techniques before—they clearly did, as the above chapter plainly

[192] E. N. Lorenz, *The Essence of Chaos,* 125. His emphasis.
[193] See, e.g., David Ruelle, *Chance and Chaos,* 71.

shows—we might try to isolate positive reasons that triggered the fashion. This is exactly what I am attempting to work out throughout this dissertation.

When in 1996 Smale recalled the context in which he was able to find his famous horseshoe, himself offered such a reason: "I was lucky to find myself in Rio at the confluence of *three different historical traditions* in the subject of dynamics (called ordinary differential equations at the time)."[194] These three traditions were: the Gorki school starting with Andronov and Pontrjagin and taken up by Lefschetz after World War II; the tradition coming out of studies of the van der Pol equation *via* Cartwright and Levinson; and the somewhat forgotten tradition of Poincaré and Birkhoff's work on dynamical systems and homoclinic points. Smale seemed to imply that by chance he could see a way to synthesize these three traditions.

Without minimizing Smale's achievements, we may wonder whether these traditions only seemed separate from his own viewpoint, which, after all, was one of an outsider. It seems true that Birkhoff's heritage was not emphasized by Lefschetz's school.[195] However, the work of Cartwright, who was invited at Princeton, and that of Levinson, were well known to members of the Project.[196] Poincaré's work was important for the Gorki school, and for Cartwright, among others; but the different concepts of Poincaré's that were exploited came from diverse, more or less connected parts his opus, which was so vast that knowledge of one part hardly precluded total ignorance of many other parts. Furthermore, Lyapunov's notions, important for LaSalle and Lefschetz, lost

[194] S. Smale, "Chaos," 12. My emphasis.
[195] See A. Dahan Dalmedico, "La renaissance des systèmes dynamiques."

their prominence with Smale's and Thom's schools. Smale's three traditions appeared to have intersected more than he thought before he took up the problem.

A major reason for Smale's success was that only with his work do we see abstract topology fully coming into play in the study of dynamical systems. With brio, he used highly abstract tools and techniques that had been developed only recently, and were extremely well codified, thanks to a large extent to the dominant Bourbakist attitude of mathematicians of all countries. These techniques, matched with some mathematicians' powerful ability for geometric reasoning, offered powerful new ways of perceiving older results. As Smale explained in retrospect:

> The more traditional way of dealing with dynamics was with the use of mathematical, e.g. algebraic, expressions. But a description given by formulae would be cumbersome [in the case of the horseshoe]. It would unlikely lead to insights or to a perceptive analysis, since that form of a description wouldn't communicate as efficiently the information in the figure. My background as a topologist, trained to bend objects as squares, helped to make it possible to see the horseshoe.[197]

(ii) The Impact of the Computer: Lorenz's Butterfly and Similar Cases

Remarkably, at about the same time as Smale was designing the horseshoe, a few scientists began to encounter other examples of nonperiodic solutions of systems of differential equations, which might have shed light on the above remaining question, but did not. The people working around Smale, Thom, and Ruelle would not become aware of them, then especially focusing on Lorenz's attractor, for a good fifteen years. Jacob Palis later recalled,

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[196] J. LaSalle, "Relaxation Oscillations," *Quarterly of Applied Mathematics,* 7 (1949): 1-19; M. L. Cartwright, "Forced Oscillations in Nonlinear Systems," *Contributions to the Theory of Nonlinear Oscillations,* 1, ed. S. Lefschetz: 149-242.

> It is curious to remark that at that very moment, a remarkable work that would remain unknown to us for the next ten years was being developed: *Lorenz was proposing the impossibility of the second half of the dream [i.e. that stable systems form a dense subset of all systems] through his still experimental robust (open) strange attractors (unstable).*[198]

Rediscovered in the mid-1970s by an applied mathematician from the University of Maryland, James Yorke, in the article that made the word "chaos" famous, the Lorenz equation was obtained from an extreme simplification of a system arising in hydrodynamics:[199]

$$\begin{aligned} X' &= -\sigma X + \sigma Y, \\ Y' &= -XZ + rX - Y, \\ Z' &= XY - bZ. \end{aligned}$$

In the early 1960s, MIT meteorologist Edward Lorenz first arrived at this set of equations, from which he extracted the butterfly attractor simply by numerically plotting the trajectories of its solutions. As an undergraduate at Harvard, Lorenz had briefly had G. D. Birkhoff as a professor. But since, he had become involved in a vast program, initiated after World War II by John von Neumann (1903-1957) and Jule Charney (1917-1981) among others, to use computers for weather prediction more efficiently.[200]

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197 S. Smale, "Chaos," 6.
198 J. Palis, "On the Contribution of Smale," 171. His emphasis.
199 E. N. Lorenz, "Deterministic Nonperiodic Flow," *Journal of the Atmospheric Sciences,* 20 (1963): 130-141; Repr. *Univ. Chaos,* 367-378; *Chaos II,* 244-255. 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. (1984): 244-251. The story of how Yorke came to be interested in Lorenz's paper, and introduced Smale's group to it, is told in E. N. Lorenz, *The Essence of Chaos* (Seattle: The University of Washington Press, 1993), 145. See in Chapter VI, however, for a discussion of the word "chaos" questioning the received idea that Li and Yorke were the first to use it.
200 I thank Amy Dahan Dalmedico for having provided me with details about the story of this program. A forthcoming publication should gather her findings.

It is useful to contrast the reasons that pushed Smale and Lorenz to come up with systems that exhibited nonperiodic solutions. For Smale, as we have seen, the main drive was to explore the structure of limit sets of differential equations. Having realized that they might be complicated, he isolated the horseshoe as a simple example.

In his later recollections, Lorenz explained why, as opposed to Smale, he had for a long time explicitly sought a system of differential equations with nonperiodic solutions. He reckoned that, in the postwar years, two distinct approaches for weather prediction could be isolated. "Dynamic meteorology" was concerned with applying the laws of physics—dynamics and thermodynamics—to the circulation of the atmosphere. This was a formidably complex problem, but the hope was that, soon, computers could help integrating systems of equations with several thousands, or even millions, of variables. On the other hand, there was also an old tradition of doing "synoptic meteorology." This approach differed from the dynamicists' in that it focused on characteristic structures of the atmosphere: high- and low-pressure systems, fronts, hurricanes, etc. Based on craft knowledge and more qualitative, the synopticians' approach was less obvious to put on a computer.[201]

A way into this problem, called "statistical weather forecasting," was provided by Thomas Malone who directed a fairly successful project at MIT. As Lorenz described it in 1993,

> the type of statistical forecasting that had received most attention was 'linear' forecasting, where, for example, tomorrow's temperature at New York might be predicted to be a constant *a*, plus another constant *b* times today's temperature at Chicago, plus another constant *c* times yesterday's relative humidity at St. Louis,

[201] E. N. Lorenz, *Essence of Chaos,* 82-83.

> plus similar terms. There were long-established procedures for estimating the optimum values of the constants *a*, *b*, *c*, etc.[202]

In 1955 Lorenz was appointed to fill in for Malone. As he recalled, statistical forecasting "was regarded by many dynamical meteorologists, particularly those who were championing numerical weather prediction, as a pedestrian approach that yielded no new understanding."[203] For practical purpose, however, the important question was whether, given sufficient computing power, linear forecasting methods could be as accurate as those of numerical dynamical meteorology.

Lorenz soon became convinced, he said, that this method was not very good. Facing opposition from the statistical forecasting community at a meeting in Madison, Wisconsin, in 1956, he proposed:

> to test the hypothesis by selecting a system of equations that was decidedly not of the linear type. I would use a computer to generate an extended numerical solution, and then, treating the solution as a collection of real weather data, I would use standard procedure to determine a set of optimum linear prediction formulas.[204]

On a Royal-McBee LGP-30, Lorenz first studied simplified meteorological equations, settling on a system of twelve equations. He first found solutions that settled down to a steady state, therefore useless as a simulation of the weather. Tinkering with parameters, he found oscillating solutions, only to realize that, using standard methods, one could obtain *perfect* linear prediction of them. "It was [around 1959] that I recognized that for my test *I would need a set of equations whose solutions were not periodic.*"[205]

202 E. N. Lorenz, *Essence of Chaos,* 130-131.
203 E. N. Lorenz, *Essence of Chaos,* 131.
204 E. N. Lorenz, *Essence of Chaos,* 131-132.
205 E. N. Lorenz, *Essence of Chaos,* 133. My emphasis.

As Lorenz wrote in 1963, it was "not obvious that such solutions can exist at all. . . . [N]onperiodic flow has sometimes been regarded as the [mere] result of nonperiodic or random forcing."[206] Had he been aware of Smale's conjectures and able to see what they meant, he might have thought it impossible to exhibit such nonperiodic behavior. A crucial difference between Lorenz and Smale's approaches however lay in the fact that while the latter was looking for *generic* complex behavior, the former only needed *one* system with nonperiodic solutions.

Not aware either of Poincaré's work, nor of Cartwright, Littlewood and Levinson's, already known to exhibit nonperiodic solutions for simpler systems, he pursued the numerical exploration of his system, and was finally able to get the long-sought nonperiodic behavior. "When I applied the standard procedure to the new 'data,' the resulting linear forecasts were far from perfect, and I felt that my suspicions had been confirmed."[207]

More importantly for the history of chaos, with this system, Lorenz observed that nonperiodic flows exhibited an "instability with respect to modifications of small amplitude."[208] In this oft-told episode, Lorenz decided to repeat an experiment with rounded-off values as its initial conditions, and found that the new output quickly diverged significantly from the original experiment.[209]

> It implies that two states differing by imperceptible amounts may eventually evolve into two considerably different states. If, then, there is any error whatever in observing the present state—and in any real system such errors seem

[206] E. N. Lorenz, "Nonperiodic Deterministic Flow," 131.
[207] E. N. Lorenz, *Essence of Chaos,* 134.
[208] E. N. Lorenz, "Nonperiodic Deterministic Flow," 132.
[209] See, e.g., E. N. Lorenz, *Essence of Chaos,* 134-136, and J. Gleick, *Chaos,* chap. 1, 11-31.

> inevitable—an acceptable prediction of an instantaneous state in the distant future may well be impossible.[210]

Lorenz's emphasis on this kind of instability may easily be understood in view of his interest in the possibility of modeling the weather on computers. He thus immediately raised a fundamental, though hypothetical, consequence of his study:

> When our results concerning the instability of nonperiodic flow are applied to the atmosphere, which is ostensibly nonperiodic, they indicate that *prediction of the sufficiently distant future is impossible by any method, unless the present conditions are known exactly.* In view of the inevitable inaccuracy and incompleteness of weather observation, precise very-long-range forecasting would seem to be non-existent.[211]

Lorenz briefly presented this result at a 1961 conference in Tokyo.[212] In his famous "Nonperiodic Deterministic Flow" paper published in the *Journal of the Atmospheric Sciences* in 1963, he studied the consequences of this very instability. This article however used another set of equations: the celebrated Lorenz system presented above. They came from a further idealization of Barry Saltzman's already idealized system of seven equations, intended to model convective fluid motion driven by heating from below.[213] In his 1963 paper, Lorenz suggested that within the limit of accuracy the solutions converged towards a pair of intersecting surfaces, indefinitely spiraling around

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[210] E. N. Lorenz, "Nonperiodic Deterministic Flow," 133.

[211] E. N. Lorenz, "Nonperiodic Deterministic Flow," 141. This and the preceding quotes is a quite lucid, early definition of sensitive dependence on initial conditions. Compare with Poincaré and Duhem. This issue was addressed more thoroughly by Lorenz in the famous talk he gave on December 29, 1972, at an AAAS meeting: "Predictability: Does the Flap of a Butterfly's Wings in Brazil Set off A Tornado in Texas?," first published as an appendix of the *Essence of Chaos,* 181-184.

[212] E. N. Lorenz, "The Statistical Prediction of Solutions of Dynamic Equations," *Proceedings of the International Symposium on Numerical Weather Prediction, Tokyo,* 629-635.

[213] B. Saltzman, "Finite Amplitude Free Convection as an Initiate Value Problem, I," *Journal of the Atmospheric Sciences,* 19 (1962): 329-341.

two centers without ever intersecting themselves. He described this set later to be called a strange attractor as an "infinite complex of surfaces."[214]

Trying to apply linear forecasting methods, Lorenz also constructed discrete iteration laws for the maximum reached by one of the coordinates $M_{n+1} = f(M_n)$, clearly showing that—like Smale's horseshoe—the discrete problem exhibited an infinite number of periodic, as well as nonperiodic, solutions. It was this part of the paper that drew applied mathematician Jim Yorke's attention.

In 1974, Li and Yorke presented an article at the last of the great symposia on dynamical systems, inspired by Lefschetz's spirit two years after his death. "Our initial investigations were stimulated by a fascinating series of Lorenz."[215] They stated their purpose as "to get a new glimmer of understanding of the possible analytic regularities in the chaotic, turbulent, unstable, irregular processes of such greater complexity that surround us."[216] Lorenz's system provided Li and Yorke with an example of a simple system with complicated behavior, which they naturally qualified as "chaotic" in the usual sense of the. They explained:

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[214] E. N. Lorenz, "Nonperiodic Deterministic Flow," 140. Note that, besides Birkhoff and Poincaré, Lorenz used: V. V. Nemystkii and V. V. Stepanov, *Qualitative Theory of Differential Equations* (Princeton: Princeton University Press, 1960 [1949]), translated under the auspices of Lefschetz.

[215] T.-Y. Li and J. A. Yorke, "The 'Simplest' Dynamical System," *Dynamical Systems: International Symposium on Dynamical Systems, Brown University, 1974,* ed. L. Cesari, J. K. Hale, and J. P. LaSalle (New York: Academic Press, 1976): 203-206, 203. Other articles by Lorenz on this topic to which Li and Yorke referred: "The Problem of Deducing the Climate from the Governing Equations," *Tellus,* 16 (1964): 1-11; "The Mechanics of Vacillation," *Journal of the Atmospheric Sciences,* 20 (1963): 448-464; "The Predictability of Hydrodynamic Flow," *Transactions of the New York Academy of Sciences,* 2nd ser., 25 (1963): 409-432.

[216] T.-Y. Li and J. A. Yorke, "The 'Simplest' Dynamical System," 203.

> chaotic oscillations of complicated phenomena may sometimes be understood in terms of the simple model, even if that model is not sufficiently sophisticated to allow accurate numerical prediction.[217]

Only when around 1975 mathematicians from Smale's school took up the study of the Lorenz system, would the convergence between Lorenz's numerical investigation and Smale's topological methods be realized.[218] This convergence first sparkled the chaos burst. But, as we shall see in the next chapters, much had happened in between.

As computer simulations became more and more accessible, other examples of nonperiodic solutions of differential equations surfaced quite naturally, but people had trouble finding the right conceptual setting to interpret them. Let me mention just a few examples. In 1958, five year before the publication of Lorenz's article, Japanese

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[217] T.-Y. Li and J. A. Yorke, "Period Three Implies Chaos," 985. The terms "chaos" and "chaotic regime," with due acknowledgment to Yorke and Li, as well as the reference to Lorenz, were immediately taken up by ecologist Robert May in his famous articles: "Biological Populations with Nonoverlapping Generations, Stable Points, Stable Cycles, and Chaos," *Science,* 186 (1974): 645-647; and "Simple Mathematical Models with Complicated Dynamics," *Nature,* 261 (1976): 459-467; repr. *Univ. Chaos,* 85-73; *Chaos II,* 151-159. On the first use of "chaos" in this context, see however Chapter VI below.

[218] The first works, after Li and Yorke's, done by physicists and mathematicians on the Lorenz system are: J. Guckenheimer, "A Strange, Strange Attractor," *The Hopf Bifurcation and Its Applications,* ed. J. E. Marsden and M. McCracken (New York: Springer, 1976): 368-381; D. Ruelle, "The Lorenz Attractor and the Problem of Turbulence," *Turbulence and Navier-Stokes Equations,* ed. Roger Tenam (Berlin: Springer, 1977): 146-158 [which widely circulated in preprint form]; D. Ruelle, "Statistical Mechanics and Dynamical Systems," and O. E. Lanford, III, "An Introduction to the Lorenz System," both in *1976 Duke University Turbulence Conference* (Durham: Duke University, 1977); and R. F. Williams, "The Structure of Lorenz Attractors," and O. E. Lanford, "Computer pictures of the Lorenz Attractors," both in *Turbulence Seminar: Berkeley 1976/77,* ed. P. Bernard and T. Ratiu, Lecture Notes in Mathematics, 615 (Berlin: Springer, 1977): 94-112 and 113-116.

geophysicist Tsuneji Rikitake studied coupled disk dynamos leading to the following system of equations, which strikingly resembled Lorenz's:[219]

$$\frac{dx}{dt} = yz - \mu x;$$
$$\frac{dy}{dt} = 1 - xz;$$
$$\frac{dz}{dt} = xy - ax - \mu z.$$

Explicitly, Rikitake drew attention to the fact that this system of equations could exhibit non-steady, i.e. nonperiodic, numerical solutions. The process that led Rikitake to this result recalls Lorenz's, since he suggested that oscillations of disk dynamos could provide a possible analogy with the magnetic field of the earth, and its reversal mechanism.[220] Here again a simple idealized system was used to gain insight on a complex situation, and indicated that nonperiodicity need not come from the many degrees of freedom in the system.

Similarly, Yoshisuke Ueda, a Japanese third-year graduate student in electrical engineering at Kyoto University, used an analog computer to plot solutions of a mixture of van der Pol's and Duffing's equations. He was of course searching smooth oval closed curves, and instead, on November 27, 1961, found "a broken egg with jagged edges."

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219 T. Rikitake, "Oscillations of a System of Disk Dynamos," *Proceedings of the Cambridge Philosophical Society,* 54 (1958): 89-105; and also T. Rikitake, *Electromagnetism and the Earth Interior* (Amsterdam: Elsevier, 1966), 61-64. The following equation is the consequence of equations (5-14) and (5-15), 62-63. For an accessible discussion of it, see P. Bergé et al., *Des Rythmes au Chaos* (Paris: Odile Jacob, 1994), ch. 3.

220 This suggestion came from E. C. Bullard, "The Stability of a Homopolar Dynamo." *Proceedings of the Cambridge Philosophical Society,* 51 (1955): 744-760.

This started a fruitful career in nonlinear dynamics for Ueda, which culminated in the finding of a strange attractor, christened the "Japanese attractor" by David Ruelle.[221]

The striking commonality across all these examples of strange attractors *avant la lettre*—and several other ones could be provided—is that they all came up in the course of numerical studies of simplified models for physical situations. Rather, what seems now to be the most remarkable aspect of the work of Smale, Thom and other topologists, contrary to the above, resides in the fact that they discovered all their examples *without* the help of the computer. The issue was almost never even raised in their circles. Whether they indirectly reacted to the expanding availability of computers is another, but crucial, question. Already in 1946, John von Neumann had declared:

> Our present analytical methods seem unsuitable for the solution of the important problems arising in connection with nonlinear partial differential equations and, in fact, with virtually all types of nonlinear problems in pure mathematics. . . . . [R]eally, efficient high-speed computing devices may . . . provide us with those heuristic hints which are needed in all parts of mathematics for genuine progress.[222]

With more and more urgency, it became obvious to everyone, in the last third of the century, that the computer offered a way to solve as accurately as needed any

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[221] Y. Ueda, "Strange Attractors and the Origin of Chaos," *Nonlinear Science Today,* 2(2) (1992); repr. in Y. Ueda, *The Road to Chaos,* ed. R. H. Abraham and H. B. Stewart (Santa Cruz: Aerial Press, 1992): 185-216; quote on p. 189. Ueda simulated the following equation: $v'' - \mu(1-\gamma v^2)v' + v^3 = B\cos \nu t$. To obtain Duffing's equation, replace the nonlinear coefficient of v' by a constant. See D. Ruelle, "Les attracteurs étranges," *La Recherche,* 11 (1980): 132-144.

[222] John von Neumann, in a talk at McGill University in 1946; quoted in H. R. Pagels, *The Dreams of Reason: The Computer and the Rise of the Sciences of Complexity* (New York: Bantam, 1988), 85. Already in 1908, Poincaré saw that the goal of computing with more or less rapidly converging series the solutions of differential equations, while enough for the engineer, was only a first step for a mathematician who was more interested in finding general formal procedures. See "L'avenir des mathématiques," *Atti*

nonlinear differential equation; the only question remaining was whether this solution meant anything at all. It is there that the mathematical work done on dynamical systems, using the latest of topological knowledge, became essential.[223]

Of course, the rise of a topological point of view for the study of differential equation does not answer the question of why examples of chaotic behavior, like the one so nicely exhibited by Lorenz, started to proliferate in distant domains at about the same time. This question of timing has to be solved by looking at the evolving conditions of scientific research in those years, especially with regard to the changing practices of mathematical modeling.

As a lesson of the debate between Smale and NSF, in 1967, the *Washington Star* concluded: "If only conformists need apply [to NSF], 1984 is here already. What can Stephen Smale do that a computer can't do a lot faster? And besides, a computer wouldn't talk back."[224] Nowhere had the question been so plainly put: if a computer could provide the solution to any differential equation, of what use were applied mathematicians anymore?

7. CONCLUSION

The above has shown that a geometrical point of view inspired by recent advances in topology, together with growing awareness that the advent of the computer changed the

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del IV Congresso internazionale dei matematici, Roma 1908, 1 (Rome: Accademia dei Lincei, 1909): 167-182, esp. 172-174.

[223] This point is raised by J. Möser, "Is the Solar System Stable," *Mathematical Intelligencer,* 1(1979): 65-71. I deal with this question in more detail in Chapter VII below when I talk about Ruelle.

modeling practices of scientists, were at the roots of much innovation. Moe Hirsch later clearly expressed the implications this had on the mathematician's trade:

> Contrary to some philosophers of science, accurate description of reality is not the only role for mathematics in the natural sciences. . . . Instead of precise equations, a robust *class* of equations is resorted to, no one of which is accurate, but which are plausible as a class. In this way, mathematics offers insights to the natural sciences that probably cannot be obtained in any other way.[225]

The computer made it hard for scientists to avoid facing complex, chaotic behaviors in simple dynamical systems. Topology was needed in order to show that these could not be talked away as simple artifacts of numerical analysis.

The methodological consequences of the topologization of dynamics were important, even at a philosophical level. In 1966, René Thom suggested to replace the deterministic a priori of mathematical modelers by an hypothesis of structural stability. Clearly identifying, as a simple matter of fact, sensitive dependence to initial condition as a limit to classic views about determinism, he noticed that in many examples of mundane natural phenomena (a flat disk spiraling in a free fall through air, for example), "minute variations in initial conditions may lead to very great variations in subsequent development." For Thom, the postulate that such systems were determined was "properly speaking a metaphysical position, impossible to verify experimentally." He therefore suggested to replace "the unverifiable hypothesis of determinism by the empirically

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224 In the November 9, 1967, issue of *Washington Star;* quoted by C. Morrey, "The Case of Stephen Smale: Conclusion," *Notices of the American Mathematical Society,* 15 (January 1968): 49-52, 52.

225 Comment made by M. Hirsch in *From Topology to Computation,* ed. M. W. Hirsch et al., 604. See also Morris W. Hirsch, "The Dynamical Systems Approach to Differential Equations," *Bulletin of the American Mathematical Society,* 11 (1984): 1-64, 11-14.

verifiable property of 'structural stability'."[226] In plain words, Thom expressed the belief that topology could replace metaphysics.

The process by which topologists became interested in applying their results to the modeling of natural phenomena was a crucial one taking place in 1966-1972. I have argued that the rise of what we might call "applied topology" becomes one of the most important historical phenomenon at the roots of the emergence of catastrophe and chaos theories. There, Thom's role, as well as that of his institution, the IHÉS, was central. This is what I examine in the next chapter.

[226] R. Thom, "Une théorie dynamique de la morphogénèse," *Towards a Theoretical Biology, I: Prologomena,* ed. C. H. Waddington (Edinburgh: University of Edingburgh Press, 1968): 152-166, 155; repr. *MMM,* 13-38, 16.

CHAPTER VI: QUALITATIVE DYNAMICS

Au moment où tant de savants calculent de par le monde, n'est-il pas souhaitable que d'aucuns, qui le peuvent, rêvent?
—René Thom.[1]

The next great era of awakening of human intellect may well produce a method of understanding the qualitative content of equations. Today we cannot see that the water flow equations contains such things as the barber pole structure of turbulence that one sees between rotating cylinders. Today one cannot see whether Schrödinger's equation contains frogs, musical composers, or morality—or whether it does not. We cannot say whether something beyond it like God is needed, or not. And do we can all hold strong opinions either way.
—R. Feynman.[2]

1. INTRODUCTION: THE MODELING PRACTICE, OR PRACTICES, OF 'APPLIED TOPOLOGISTS'

At the General Assembly of the contributing members of the Institut des hautes études scientifiques, on December 10, 1969, Director Léon Motchane proudly emphasized that Professor René Thom had started fulfilling the hope that he, Motchane, had placed on him when he hired him. Thom was in the process of building his own research school:

[1] "At a time when so many scholars in the world are calculating, is it not desirable that some, who can, dream?" R. Thom, *SSM,* 325.
[2] R. Feynman, R. Leighton, and M. Sands, *The Feynman Lectures in Physics,* 2 (Reading: Addison-Wesley, 1964): 41-12; quoted by R. H. Abraham, "Introduction to Morphology;" repr. *On Morphodynamics: Selected Papers by Ralph Abraham on Models for Pattern Formation Processes, Morphogenesis, and Self-Organizing Systems Showing the Evolution of the Complex Dynamical System Concept over a Fifteen Year Period,* "The Science Frontier Express Series" (Santa Cruz: Aerial, 1985), 9.

> Under the influence of Thom among others, a progressive *conversion* of topologists is being felt—a conversion toward the study of *stable* structures, forgoing purely topological themes. This problem is not new (think of Poincaré), but it has until now been treated like a poor relative, being often considered as belonging to the domain of engineers. But these problems are very close from those dealing with the stability of dynamical systems (Poinsot, Poincaré, Birchoff [*sic*]). Thus, besides Grothendieck's school of geometric algebra and all the ramified algebraic *works* spurred by it, this new tendency emerged, whose *best* representatives, Pugh, Mather, etc. ... come work with us.[3]

Starting January 5, 1970, Thom's seminar took the name of "Séminaire de dynamique qualitative." For the first time since he had joined the IHÉS, the seminar he led was devoted to a topic beyond purely mathematical themes.[4] For this seminar, he secured the collaboration of Steve Smale and his best students from Berkeley: Michael Shub, Charles Pugh, Robert Williams, John Guckenheimer, and Nancy Koppel. Other participants included David Chillingworth from University of Warwick, at Coventry, England (from the Mathematical Institute headed by E. C. Zeeman, which also came to Bures that year), and the young Floris Takens from Amsterdam.[5] Since the quality of this seminar was exceptional, it attracted other permanent professors of the IHÉS, such as Alexander Grothendieck and David Ruelle.

That year, Ruelle and Takens jointly wrote their famous paper on the nature of turbulence, which sparkled later studies on the chaotic behavior of dynamical systems in

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[3] Léon Motchane, *Notes pour le rapport scientifique 1968* (10/12/69), 2. Arch. IHÉS. Original emphasis.

[4] Thom's previous seminars were titled: "Structure des ensembles analytiques et des applications différentiables," with Bernard Malgrange (starting 13/11/64); "La stabilité structurelle des applications différentielles" (starting 8/11/65); "Propriétés tangentielles des ensembles analytiques" (starting 3/10/66); "Singularités des applications différentiables et des champs de vecteurs" (starting 9/1/67); "Analyse différentielle" (starting 6/11/67); and others on differential geometry and cobordism theory in 1968-1969. *Rapports scientifiques,* 1964-1969. Arch. IHÉS.

[5] *Rapport scientifique 1970* (2/6/71). Arch. IHÉS.

physics. That year, while at the IHÉS, Smale wrote articles where he first articulated his views on the possible uses in other fields of topological results of his dynamical systems theory. Around the same time, Ralph Abraham also visited the IHÉS and developed his own understanding of catastrophe theory. So did Christopher Zeeman.

For the first time, coalesced a new form of modeling practice of which Thom's book *Structural Stability and Morphogenesis* became the manifesto. While the mathematical developments that led to the emergence of this modeling practice partly stemmed from elsewhere, in particular Lefschetz's group and above all Smale's school, the impetus to turn these mathematical ideas into proficient tools for the modelization of natural phenomena came from the IHÉS at Bures-sur-Yvette.

Therefore, the modeling practices of those I call "applied topologists" bore the mark, not only of the discipline it came from (differential topology), not only of its main initiator (René Thom), but also of the institute where it was articulated (the IHÉS). In particular, since for financial and ideological reasons, the IHÉS could not allow incursions in the experimental domain, the idea that models had to be experimentally tested hardly surfaced at the time. As we saw in Chapter III, Thom therefore strongly argued for the total independence of his models from experimentation. Moreover, conforming with the ideology of fundamental research promoted by the IHÉS, the applied topologists' modeling practice was articulated at a highly abstract level, with the feeling that concrete applications had to be developed by "interpreters" coming from various disciplines. Throughout this period, the IHÉS remained, as an institution, focused on what it called fundamental research, i.e. pure mathematics and theoretical physics.

Most importantly, the IHÉS provided a meeting ground for, on the one hand, applied topologists and, on the other, physicists whose education and research programs were mathematically speaking sophisticated enough. A key figure in this rapprochement, David Ruelle was among the permanent faculty of the theoretical physics section of the IHÉS. Only this institution, perhaps, could have played this role. In any case, it played it magnificently for many reasons. It was small enough so that people met across disciplinary boundaries; it was prestigious enough to attract leaders in their fields; and, admittedly without much concrete effect until then, an ideology of cooperation between mathematicians and theoretical physicists permeated the discourses of its upholders.

Soon enough, however, some divergence started to be felt among the promoters of these new practices. By the second half of the 1970s, Thom, Smale, Zeeman, and Ruelle had all gone in diverging directions. Leaving mathematics and the building of particular models to others, Thom grew ever more concerned with philosophy. With some success, Smale catered his highly mathematical approach to mathematical economists who, like Gérard Debreu, had had a Bourbakist education. Very actively developing models inspired by catastrophe theory and insuring their wide promotion, Zeeman saw his approach the more harshly criticized. Developing models for turbulence, Ruelle followed an approach which, by far, was the most fruitful in terms of the consequences it had in provoking the emergence of chaos. Ruelle's work will be addressed in detail in Chapter VII. For the time being, I tell the story of how topologists, who all often visited Bures, became modelers and then disagreed on what it meant for a topologist to model natural phenomena.

2. THOM'S PROGRAM: THE EARLY YEARS, 1964-1966

Research schools in the experimental sciences have proved a useful tool for historians to think about the roles of institutions in bringing about scientific change and the emergence of new specialties.[6] To speak of a research school in mathematics is less obvious for the main reason that, at first glance, no focus on simple and rapidly exploitable experimental techniques seems possible. But, as this dissertation amply shows, the practices of mathematicians can also provide practitioners with techniques and tacit knowledge that can be learned through sustained interaction. In this chapter, I explore the possibility of thinking of Thom's school as a research school in Geison's sense.

Moreover, with the parallel development of groups working on global dynamics and catastrophe theory at Berkeley around Smale and at Warwick around Zeeman, the concept of a research network as developed by Teresa Hopper may apply.[7] Of course, no material (like in her case radium) could be exchanged in the network, but students and mathematical techniques could. The sustained interaction, not only among the leaders of these different schools, but also their students who for the most part toured these sites supports this hypothesis. But before thinking about building such school and network, Thom had to come to terms with his new, original setting where he started working in October 1963.

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[6] See in particular G. L. Geison, "Scientific Change, Emerging Specialties, and Research Schools," *History of Science,* 19 (1981): 20-40; and "Research Schools and New Directions in the Historiography of Science," *Research Schools: Historical Reappraisals, Osiris,* 8, ed. G. L. Geison and F. L. Holmes: 227-238.

[7] T. Hopper, *Science at the Boundary: Discipline Building and Community Making in Radioactivity and Early Nuclear Science, 1919-1939,* Ph.D. Thesis (Princeton University, 1997).

a) Settling in at the IHÉS

In January 1964, René Thom went to Bombay for an international colloquium on differential topology, where he talked with mathematicians John Milnor, Steve Smale, and Jürgen Möser, all of whom he of course already knew and had several occasions to meet.[8] Already in December 1957, after he had had a chance to discuss with Smale while at Chicago, Thom presented Smale's Ph.D. thesis at the *Séminaire Bourbaki*.[9] Similarly, Thom had just presented Möser's work on KAM-theory in the same setting.[10] The Bombay Colloquium was the first important conference Thom attended since he had joined the IHÉS. Director Léon Motchane specifically asked him to invite Smale and Milnor to Bures.[11] At a meeting of the Scientific Committee in October, invitations were indeed sent to Möser and Smale, while Thom still "dream[t] of a differential geometer."[12]

This was the first step in the direction of establishing close contacts between Smale's Berkeley school and the IHÉS. Strangely, Thom hardly seemed to have been the main artisan for the weaving of this link. The impetus more or less came from the Institute itself which, always on the look for the best mathematicians, could not help be enticed by men of Smale's or Möser's stature. In 1962, Smale's name had already appeared on Grothendieck's permanent list of correspondents, and as a handwritten

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[8] See *Differential Analysis: Papers Presented at the Bombay Colloquium, 1964,* Tata Institute of Fundamental Research (Oxford: Oxford University Press, 1964).
[9] R. Thom, "La classification des immersions (selon Smale)," *Séminaire Bourbaki,* 10 (December 1957), exposé #157.
[10] R. Thom, "Travaux de Möser sur la stabilité des mouvements périodiques," *Séminaire Bourbaki,* 16 (December 1963), exposé #264.
[11] Lettres de R. Thom à L. Motchane (6/1/64); de Motchane à Thom (15/1/64). Arch. IHÉS.
[12] Calabi, Chern, or Spencer. *Comité scientifique* (1/10/64). Arch IHÉS.

addition to a list of invitations proposed by Dieudonné.[13] Among the big mathematical news of those years were KAM-theory in which Möser was involved and, above all, Smale's proof of the Poincaré conjecture.[14]

(i) Singularities Versus Dynamics

For his part, starting in the mid-1950s, René Thom was mainly interested in the search for a generic classification of mappings from $\mathbf{R}^n$ to $\mathbf{R}^p$, which he undertook as a follow-up to Hassler Whitney's work. As I have describe in Chapter III, he was quickly led to focus on these mappings' singularities, and conjectured his famous classification theorem. Already in 1956, he had summarized it in a table "whose exactitude [was] not totally guaranteed," but which would evolve into his list of seven elementary catastrophes.[15]

This research program had led Thom to consider the exact same problem that Smale had called the "topological conjugacy problem" in 1961. As Albert Tucker once said, "much of topology has to do with *description* and *classification.*"[16] Thus, topologists Thom and Smale both defined their problem as the search for a topological classification of mappings. Inspired by the concept of genericity, they looked for an open dense subset (or more accurately a Baire subset) of all diffeomorphisms. But, although both were

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[13] Lettre de Alexander Grothendieck à Annie Rolland (21/2/62); *Liste d'invitations de M. Dieudonné* (30/8/62). Arch. IHÉS.

[14] See S. Smale, "The Story of the Poincaré Conjecture."

[15] R. Thom, "Les singularités des applications différentiables," *Séminaire Bourbaki,* 8 (May 1956), exposé #134; repr. *Séminaire N. Bourbaki, Volume 3, Années 1954/55, 1955/56, Exposés 101-136* (Paris: SMF, 1995): 357-369, 363-364. See also R. Thom, same title, *Annales de l'Institut Fourier de Grenoble,* 6 (1955-1956): 43-87; and "Un lemme sur les applications différentiables," *Boletín de la Sociedad matemática méxicana,* 2nd ser., 1: 59-71.

[16] A. W. Tucker, *History of Mathematics,* Course II-1962, NSF Institute, mimeographed lecture notes by Alvin K. Funderburg, 180. Original emphasis.

specialists in topology, they came to this problem from different angles. Smale attacked the conjugacy problem from the standpoint of qualitative dynamics. Following Peixoto, he focused on structural stability as a candidate for a generic property. Following Birkhoff, he chose to use the classic tools of qualitative dynamics (limit sets and soon nonwandering points), rather than singularities, as a way to characterize diffeomorphisms. Moreover, as we have seen in the previous chapter, Smale approached the problem by trying to *guess* which simple axioms could generate an open dense subset of diffeomorphisms. Instead, Thom chose to attack the more difficult problem of classifying generic singularities. The different tools Thom and Smale used in their attempts at classifying mappings led them to follow different paths.

While Smale developed a general theory of dynamical systems, Thom struggled with difficult technical concepts in order to formalize the intuition he had summarized in his table of singularities. In 1959, as a *Gastprofessor* in Bonn, he taught a course on the singularities of differential mappings, in which he conjectured that "almost all" mappings were topologically equivalent to mappings close to them.[17] Similarly, he conjectured that "almost everywhere," an infinitely differentiable mapping was topologically equivalent to a polynomial.[18] Exhibiting in 1960 a "pathological" example, Thom engaged in "the long

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[17] The maps f and g from Euclidean space E to E' are said to be *topologically equivalent* if there exist homeomorphisms h on E and h' on E' such that $hg=fh'$. If their singularities are preserved under all such homeomorphisms, the maps will be said to be *structurally stable.*

[18] His lecture notes, taken and arranged by Harold I. Levine, "Singularities of Differentiable Mappings," were widely circulated in preprint format. They were later published in the *Proceedings of Liverpool Singularities Symposium I,* ed. C. T. C. Wall (Berlin: Springer, 1971): 1-89. See Thom's introduction, 2-3.

story of the edification of the notion of stratified sets and morphisms."[19] It was his battle with these concepts that led him to wish for a differential geometer to come to the IHÉS.

In a didactic article on the theory of envelops published in 1962, Thom explained what his approach was and where he hoped it led him. Educated by Bourbakis, Thom did not hide his struggle to adapt older geometrical traditions to the dominant attitude. Recent reforms of higher education evacuated from the mathematics curricula the theory of envelops, which dealt with the set defined by the tangents of a curve. This was "quite unfortunate," Thom estimated. We may recall that at around this time he had started to experiment with caustics, which, he contended, only the theory of envelops could account for. Why was it condemned, he asked? Because of it "insufficient, non-rigorous character," he explained.

> For any professor enamored with clarity and precision, the theory of envelops, with its long list of exceptional phenomena: fixed points, singular points, stationary curves, etc., quickly becomes a torture [*tourne rapidement au martyre*]: for nothing insures that all pathological phenomena have effectively been cataloged.

With this paper, Thom's therefore aimed at providing the classic theory of envelops with the "theoretical foundations which have until then been lacking." For this, it had to be put in a more general setting: namely, singularity theory. Despite his protests against educational reforms, Thom's enterprise was a singularly Bourbakist one. Using

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[19] R. Thom, "Problèmes rencontrés dans mon parcours mathématique: un bilan," *Publication mathématiques de l'IHÉS,* 70 (1989): 199-214, 201; see also "Notice académique (1976)," Thom Arch. Thom's example was a family of mappings, parametrized by a real number k, whose topological features changed continuously with the parameter k: "La stabilité topologique des applications polynomiales," *L'Enseignement mathématique,* 8 (1962): 24-33. He gathered his results on stratification in "Ensembles et morphismes stratifiés," *Bulletin of the American Mathematical Society,* 75 (1969): 240-284.

highly technical tools (especially Ehresmann's theory of jets and stratified sets), Thom hoped that "these considerations . . . indicates the way to follow in order to get the theory out of the chaos where it is now struggling."

He distinguished clearly between two types of singularities: "stable" (or "generic") ones, which were unchanged by sufficiently small deformations, and an infinite number of unstable ones, which he assumed were nongeneric. "A sane pedagogy shall limit itself to the description of stable singularities, which are (*as far as we may guess*) in finite number." There lay the essential assumptions that made Thom's research agenda possible: stable singularities were generic and in finite number.[20] One may be struck, once again, by its similarity to Smale's convictions.

(ii) Malgrange's Preparation Theorem

During his first years at the IHÉS, Thom saw his agenda dominated by these difficult problems which, as he described John Mather's closely related work, "demanded an almost universal competence: a good topological vision, deep knowledge of analysis, a never-failing algebraic technique."[21] He did succeed in gathering a rather thriving group focusing on these problems, but soon his main interests would lead him elsewhere.

Indeed, in his Scientific Report for 1964, Motchane mentioned an "École Thom" working on the structure of analytic sets, but with few activities compared with Grothendieck's school of algebraic topology which was then flourishing. The group of

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[20] R. Thom, "Sur la théorie des enveloppes," *Journal de mathématiques pures et appliquées,* 9th ser., 41 (1962): 177-192. All quotes above are from pp. 177-178. My emphasis.

[21] Lettre de recommendation pour John Mather de René Thom à W. T. Martin, Princeton University (17/11/67). Arch. IHÉS.

mathematicians to whom Thom belonged was vaguely termed: "differential geometers and those assimilated to them, and topologists." The next year, Thom's school was demoted to a mere "group" (while Grothendieck's remained a "school"). But it now included as much as 14 mathematicians.[22]

While teaching in Bonn, Thom made a conjecture that proved easier to attack, namely that every differentiable mapping could be exactly approximated by a polynomial. This generalization of Weierstrass's preparation theorem (*Vorbereitungssatz*) was proved by Orsay mathematician Bernard Malgrange.

> I consider my duty to state that one of the main results, 'the preparation theorem for differentiable functions,' was proposed to me as a conjecture by R. Thom, and that he had to make a great effort to overcome my initial scepticism.[23]

More significantly, Thom saw a way to use the preparation theorem in order to prove Whitney's classification of the singularities of maps from the plane to the plane.[24] Starting in November 1964, Thom and Malgrange directed a seminar (Thom's first

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[22] *Éléments de rapport scientifique à l'assemblée* [pour 1964] (10/2/66); *Éléments de rapport scientifique sur l'exercice 1965* (1/12/66). Arch. IHÉS.

[23] B. Malgrange, *Ideals of Differentiable Functions* (Oxford: Oxford University Press, 1966), Introduction. See B. Malgrange, "Le théorème de préparation en géométrie différentiable," *Séminaire Henri Cartan 1962/63. Topologie différentielle.* Exposés #11, 12, 13 and 22 (February 4, 11, 25, and May 20); "The Preparation Theorem for Differentiable Functions," *Differential Analysis, Bombay Colloquium* (Oxford, 1964): 203-208. On the real axis, the preparation theorem answered the following simple question in the affirmative: Let $f(x)$ be a real, infinitely differentiable, function, on an interval; does a polynomial $P(u)$ and a diffeomorphism $u=h(x)$, $h'(x)$ never zero, exists, such that for all x in the interval, we have: $f(x)=P(h(x))$? Thom solved this problem by making a wide usage of sophisticated topological techniques. R. Thom, "L'équivalence d'une fonction différentiable et d'un polynôme," *Topology,* 3, suppl. 2 (1965): 297-307. Manuscript sent to Mr. Phillips (31 May, 1964). Thom Arch. References are: Chlander Davis, Problem 4714, *American Mathematical Monthly,* 64 (1957): 679; and N. Levinson, "The Canonical Form for an Analytical Function of Several Variables at a Critical Point," *Bulletin of the American Mathematical Society,* 66 (1960): 68-69.

[24] See Chapter III above.

seminar at the IHÉS) devoted to the preparation theorem. Sponsored by the military, this seminar dealt with stratification. Thom described it as such:

> the properties of *structural stability* of differential mappings are dealt with and in particular preliminary notions on the local differential properties of analytic sets are given. It essentially is a research seminar.[25]

For the first time, this placed the Institute at the center of essential developments occuring in analysis.

(iii) Dynamics and Structural Stability

On February 12, 1965, René Thom wrote a brief note for the director of the IHÉS. We should invite Vladimir Arnol'd, "a young Russian presently in Paris," it said. At the Institut Henri Poincaré, Arnol'd was giving a series of lectures, devoted to the ergodic properties of "unstable" motions, at the *Séminaire* of Jacques-Louis Lions and Laurent Schwartz. No doubt inspired by his own work on KAM-theory, and his encounter with Smale in 1961, these lectures were essentially published in a seminal book he wrote with André Avez.[26]

According to Thom's student Jean Petitot, the numerous conversations Thom had with Arnol'd during that spring played an important role in shaping what became

[25] Lettre de Léon Motchane à Pierre Aigrain (8/12/64). My emphasis. *Rapport scientifique 1964;* lettre de Léon Motchane à Lucien Malavard (23/11/64); contract with DRME n° 167/65 approved (2/4/65) for 20,000 F. Notification d'une commande de travaux sur mémoire, DRME: "Organisation de séminaires de physique théorique et de mathématiques" (25/5/65). Arch. IHÉS.

[26] V. I. Arnol'd and A. Avez, *Problèmes ergodiques de la mécanique classique* (Paris: Gauthier-Villars, 1967); *Ergodic Problems of Classical Mechanics* (Reading: Addison-Wesley, 1968).

catastrophe theory.[27] Even while extending Thom's singularity theory, Arnol'd remained ever skeptical of catastrophe theory. On March 7, 1967, having received a copy of Thom's *Structural Stability and Morphogenesis,* he wrote Thom: "I thank you for your biological book, where one finds *one* well-hidden page of good mathematics."[28]

Peixoto's 1964 seminar on structural stability, Thom's seminar with Malgrange, his conversations with Arnol'd all show that by that time structural stability, which Thom did not consider in his early investigations of singularities, was quickly becoming, for him and people around him, an important category of thought. Although stability had already surfaced in various forms, only in 1964 did the phrase 'structural stability' appear in Thom's work. But still, it then came up at the end of an argument, as a property exhibited by generic maps, and not as a guiding tool for his investigation.[29]

Thom's work increasingly addressed dynamics, with which he had already flirted for some time. In 1951 he was given Birkhoff's ergodic theorem as a topic for the second thesis of his *thèse d'État,* traditionally imposed by the faculty.[30] As a consequence, Thom devoted his first exposé at the *Séminaire Bourbaki* to Eberhard Hopf's work on geodesics

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[27] J. Petitot, "René Thom," *Encyclopaedia Universalis, vies et portraits,* 636-638. See V. I. Arnol'd, *Catastrophe Theory* (Berlin: Springer, 1992), x-xi, 26. At the IHÉS, on April 4, 1965, Arnol'd gave a talk titled: "La stabilité structurelle du courant géodésique." Arch. IHÉS.

[28] Lettre de V. I. Arnol'd à René Thom (7/3/67), P.S. de la copie d'une lettre du Président de la Société Mathématique de Moscou à Thom. Arch. IHÉS. My emphasis. For his critique of catastrophe theory, see V. I. Arnol'd, *Catastrophe Theory,* first published in 1979.

[29] R. Thom, "Local properties of Differential Mappings," *Differential Analysis, Bombay Colloquium* (1964): 191-202, 201.

[30] R. Thom, *Espaces fibrés en sphères et carrés de Steenrod,* doctoral thesis, Faculté des sciences, Université de Paris (Paris: Gauthier-Villars, 1952). Jussieu Lib. Defended on October 13, 1951 in front of G. Valiron (pres.), Henri Cartan, and André Lichnérowicz.

on manifolds with negative curvature.[31] This certainly helped him later take notice of Hopf's bifurcation, the rediscovery of which is attributable to Thom's conversations with Arnol'd.[32]

In 1963, Thom was chosen to present Möser's work, also at the "Bourbaki show," to use one of Tits's words.[33] There, he insisted on the following aspect: "for the first time, in the study of a diffeomorphism, the existence of a globally invariant manifold which presents a certain stability vis-à-vis perturbations of this diffeomorphism has been exhibited." This was the kind of problems Smale was interested in, but in the dissipative case as opposed to the conservative one studied by Möser. In Thom's exposé, structural stability came up as an interesting property, but not as a guideline. Moreover, the fact that Thom spoke of the structural stability of a *manifold*, which had no clear definition, indicated that he had not yet studied the concept very extensively.[34]

Thom spent the months of August and September 1965 at Berkeley, where he no doubt familiarized himself with Smale's most recent work on dynamical systems. It probably was through his interaction with Peixoto, Arnol'd, and Smale that he acquired the conviction that structural stability was an essential property for the modeling of nature, but his own previous interests had prepared him to reach this conclusion.

By 1965, the stage was almost set for Thom's theory of morphogenesis. Thom now believed that there was a great task awaiting topologists:

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[31] R. Thom, "Les géodésiques dans les variétés à courbure négative (d'après E. Hopf)," *Séminaire Bourbaki,* 2 (March 1951), exposé #28.
[32] V. I. Arnol'd, *Catastrophe Theory,* 26.
[33] Lettre de Jacques Tits à Annie Rolland (37/10/69). Arch. IHÉS.
[34] R. Thom, "Sur les travaux de Möser," 12.

> Besides classical analysis, which is essentially linear, there is the practically unexplored domain of nonlinear analysis; there the topologist may hope make an even better use of his methods, and perhaps his essential quality, namely the intrinsic vision of things.[35]

Three elements however were still missing for a new modeling practice to coalesce: Thom's assimilation of Zeeman's ideas about modeling; a satisfactory proof of his classification theorem soon to be provided by John N. Mather; and his contact with embryologist Waddington. All of these encounters took place thanks to the IHÉS.

b) Zeeman: Topology for Mathematical Modeling

On July 7, 1965, Warwick mathematician Eric Christopher Zeeman came back elated from his annual visit at the IHÉS. He wrote Motchane: "I think the single most exciting conversation I had was with Thom on his new theory of creods."[36] A most fruitful alliance was born from which catastrophe theory would emerge in the course of the following decade. Lecturing on "Piecewise Linear Transversality," Zeeman was however not counted among Thom's group in Motchane's subsequent *Scientific Report.*[37]

While Thom defined in mathematical terms what he meant by a "catastrophe," it was Zeeman who introduced the catchy phrase of "catastrophe theory."[38] Thom credited

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[35] R. Thom, "Sur les travaux de Smale," 26-27.
[36] Lettre de E. C. Zeeman à Léon Motchane (9/7/65). Arch. IHÉS.
[37] *Eléments de rapport scientifique sur l'exercice 1965* (1/12/66), 4-5. Lettre de E. C. Zeeman à Annie Rolland (13/10/64). Arch. IHÉS.
[38] The term "ensemble de catastrophe" appears for the first time in print in Thom's "Une théorie dynamique de la morphogénèse," *Towards a Theoretical Biology, I. Prologomena,* ed. C. H. Waddington (Edinburgh: Edinburgh University Press, 1968): 152-166; repr. *MMM,* 13-38; and in a rather obscure publication: "A Dynamical Theory for Morphogenesis," *Katada Symposium on Topology* (private edition, February 1967): 1-57. Thom Arch. "According to [Claude-Paul] Bruter, Thom would have found the word [*catastrophe*] in Goethe." J. Largeault, "René Thom et la philosophie de la nature," *Critique,* 36 (1980): 1055-1060, 1058.

Zeeman for using the theory to make sense of arbitrary sets of inputs and outputs, whereas Thom had thought of his morphogenetic theories as arising in four-dimensional spacetime $\mathbf{R}^4$ only.[39] As I mentioned in Chapter III, Zeeman's attempts at utilizing topology in biology predated Thom's. Audaciously he then developed many famous (and infamous) models in physics, politics, sociology, neurology, animal behavior, etc.[40] It was Zeeman who, almost single-handedly, attracted the most attention on catastrophe theory, always careful, however, of acknowledging his debt to Thom.

The archives of the IHÉS shed a new light on the interplay between Thom and Zeeman at the roots of catastrophe theory. Indeed, they show that, far from resulting from his mere reading of an "underground" copy of Thom's manuscript, Zeeman's interest in Thom's ideas emerged through constant personal contacts between the two mathematicians.[41] By the same token, Thom picked up some of his colleague's ideas. Zeeman once explained how he saw this process working out for mathematicians:

> Essentially [mathematics] is created by individuals working alone, but often the creation is inspired by talking. *Informal discussion is the secret;* . . . it is the only medium by which one can convey *intuition* without details. . . . The intuitive idea of a paper, which might take a month to read, can often be explained in 10 minutes

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[39] See Zeeman's popular presentation of "Catastrophe Theory," *Scientific American,* 234(4) (1976): 65-83; original extended version in E. C. Zeeman, *Catastrophe Theory: Selected Papers, 1972-1977* (Reading: Addison-Wesley, 1977): 1-64. See also I. Ekeland, "La théorie des catastrophes," *La Recherche,* 81 (1977): 745-754.
[40] For a compilation of his articles in several disciplines, see E. C. Zeeman, *CT;* Woodcock and Davis present several applications in an accessible manner, *Catastrophe Theory,* 76-145.
[41] R. Thom often mentioned "la carrière souterraine" of *SSM.* See, e.g., "Exposé introductif," *Logos et théorie des catastrophes,* ed. J. Petitot (Geneva: Patiño, 1988): 23-39, 33.

> on a blackboard. This is the secret of gregariousness, and of the success of the Institutes at Princeton and Paris."[42]

In short, it was thanks to these sustained personal contacts which were favored by the IHÉS's structure and setting, that emerged a modeling practice that topologists could call their own.

(i) 'The Perfect Environment Both to Think and to Write'

As early as 1961, Dieudonné invited Zeeman, who had received his Ph.D. in 1954, to come to the IHÉS. Arriving in October 1962, he gave a series of lectures on the "Foundations of Combinatorial Topology," and spent the whole spring semester at Bures-sur-Yvette. Starting an IHÉS tradition, he brought a student with him.[43]

Because of its atmosphere and the possibility to work in a quiet setting, Zeeman very much liked the IHÉS. After he came back from his first visit, he sent his "many thanks" to Annie Rolland, the General Secretary, "for creating the familyness of the Institut." Adding: "we realise (although keep it a secret) how much nicer is Bures than Princeton."[44] Having written eight papers during his stay (including 4 jointly, and 2 on physics), he could not help noticing that it had been a "tremendously rewarding year" for him. "I found the place of Bois-Marie a perfect environment both to think and to write."[45]

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[42] E. C. Zeeman, "How to reverse the brain drain in Maths," *New Scientist* (4 May 1967): 263-264. My emphasis.
[43] The student was W. B. R. Lickorish. *Comité scientifique* (8/7/61); lettres de Jean Dieudonné à E. C. Zeeman (31/5/61); de E. C. Zeeman à Jean Dieudonné (6/6/61); de E. C. Zeeman à Léon Motchane (8/1/62); de Annie Rolland à E. C. Zeeman (23/1/62); de E. C. Zeeman à Jean Dieudonné (26/3/62); de Jean Dieudonné à Léon Motchane (8/4/62); *Rapport scientifique sur l'activité de l'IHÉS en 1962* (30/4/63). Arch. IHÉS.
[44] Lettre de E. C. Zeeman à Annie Rolland (26/9/63). Arch. IHÉS. Original English.
[45] Lettre de E. C. Zeeman à Léon Motchane (26/9/63). Arch. IHÉS. Original English.

During the following years Zeeman became a fixture of the IHÉS, paying at least a month-long visit almost every spring. In 1963, following the reform of the British university system, the University of Warwick was set up at Coventry, where Zeeman accepted a chair. It was, he wrote, "a difficult decision – which I entirely made by instinct rather than by reason." Occupied with the demanding task of building a new department from scratch, he was "afraid of being drowned in administration." Therefore, he welcomed the opportunity the IHÉS offered him to work on mathematics *per se.* The "only solution is that every now & then I shall have to flee to Paris . . . to the peace of le Bois-Marie, in order to recover my wits!"[46]

Zeeman also pleased Motchane, and received from him an offer that he did not want to accept. Motchane invited him to stay for a longer period but Zeeman refused in the short term, because, he said, his image of himself was "primarily that of a teacher."[47] Because of his ability to mix highly abstract topological theories with reflections about other subjects, because he collaborated with physicists such as François Lurçat then at the IHÉS, Zeeman might have been the kind of mathematician Motchane ideally was on the look for.

(ii) Topology of the Brain

Indeed, Zeeman was unusual for pure mathematician. While he was interested in abstract theories, he also thought that they provided him with new tools to think about the world. Throughout the 1960s, Zeeman published several papers on relativity and on what he

[46] Lettre de E. C. Zeeman à Annie Rolland (18/12/63). Arch. IHÉS. Original English.
[47] Lettre de E. C. Zeeman à Léon Motchane (26/9/63). Arch. IHÉS. Original English.

called the "topology of the brain."[48] These papers were characterized by a creative use of mathematics in order to achieve better *explanations* of natural phenomena in biology and physics.

Mathematics, Zeeman believed, exhibited a number of specific characters, which distinguished it from other human endeavors. It was both an art *and* a science. As a science, "mathematics—and by that I mean pure mathematics—is the most original and most creative of all the sciences." As "the unique subject that is independent of humanity and of the universe," mathematics was "inhuman." Its result were universal; its history continuous. As opposed to other sciences, its truth was based on proofs, rather than experiments. But mathematics was also an art, because the only criterion for determining worthwhile subject matters was "*taste*" as opposed to external reality, i.e. "what is there."

> Mathematics has gradually been shaking itself free from the rest of science, and the revolution of the last two decades has witnessed the final escape. It is now recognised that we choose what mathematics to do, not for its usefulness or applicability, but for its 'elegance, intrinsic beauty, profundity, generality, simplicity, depth, subtlety and economy.'[49]

Mathematics, by detaching itself from the real world, strove for absolute, universal truths. And there lay "the outstanding quality of mathematics: . . . the final arbiter of what lives

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[48] E. C. Zeeman, "The Topology of the Brain and Visual Perception," *Topology of 3-Manifolds and Related Topics,* ed. M. K. Fort (Englewood Cliffs: Prentice Hall, 1962): 240-256; "Topology of the Brain," *Conference on Mathematics and Computer Science in Biology and Medecine (MRC, Oxford, July 1964),* (London: Her Majesty's Stationary Office, 1965): 277-311; "Causality Implies the Lorenz Group," *Journal of Mathematical Physics,* 5 (1964): 490-493; "The Topology of Minkowski Space," *Topology,* 6 (1967): 161-170.

[49] E. C. Zeeman, "Les mathématiques et la pensée créatrice." *Sciences et l'enseignement des sciences,* 5(34) (November-December 1964): 11-14. Original English from a typed manuscript "Mathematics and Creative Thinking" (n.d. [around 1962-1963]). Arch. IHÉS. Zeeman was here quoting from M. L. Cartwright, *The Mathematical Mind* (Oxford, 1975).

and dies in mathematics is 'elegance and taste'." Simplicity was the judge for this. "At the same time as complicating itself mathematics is paradoxically simplifying itself . . . in a constant process of self-refinement."

Another paradox was that, in spite of—and indeed because of—these very qualities, mathematics was nonetheless extremely useful for other sciences. Citing the classic examples of quantum mechanics and matrices, of relativity and tensors, as well as the more recent use of number theory for computer science, Zeeman contended that "sooner or later it seems that every branch of mathematics will be used by science." Using an expression later taken up by Thom, the intelligibility of the experimental "chaos," Zeeman claimed, depended on mathematics' very "power to simplify and explain."

This amounted to the standard credo of a Platonist mathematician raised in the Bourbakist age. But Zeeman went further than other mathematicians in seriously trying to work out applications of his theories to other sciences. What he did not say in the article quoted above, but expressed clearly elsewhere was that he thought not only that mathematics could help organize experimental data, but that it moreover offered new insights for some natural phenomena.[50] In his "Topology of the Brain," acknowledged by Thom as an important source of inspiration, Zeeman plainly stated his goals:

> We use mathematics to try and explain the relationship between mind and brain, between memory and anatomy, and between thinking and the electrochemical activity of the cortex. The mathematical tool used is algebraic topology, because

[50] Compare Chevalley's philosophical articles quoted in Chapter II, Ruelle's Bourbakist view of physics in Chapter VII below, and the analysis of Debreu's work in mathematical economics by E. R. Weintraub and P. Mirowski, "The Pure and the Applied: Bourbaki Comes to Mathematical Economics," *Science in Context,* 7 (1994): 245-272.

> this is a branch of mathematics *well adapted to ignore local variations and capture global properties.*[51]

This was quite an original way to envisage the classic problem of emergent properties. As Zeeman explained:

> Up till now most theories of the brain, of thinking and of memory have studied the local behaviour of molecules and neurons by using equations. It was thought that to study the global behaviour we should have to solve all the equations for all the neurons (of which there are 10,000,000,000 in each brain). Not only is this impossible, but misleading because it includes the irrelevant local randomness. The power of algebraic topology was to admit that the global activity depends entirely on the local activity, but yet, at the same time, furnish a technique for capturing only the global activity.[52]

Christopher Zeeman modeled the brain by a cube of 10^{10} dimensions, which he called "the thought cube." Each vertex of the cube corresponded to a neuron which could be in either of two states: 0 or 1. To work with such a formidably complicated construct, recent techniques of topology (homology theory) were called to the rescue. The standard problem Zeeman investigated was: How does the brain perceive a visual image starting from the nervous impulses coming from the retina?

> the message is never reconstructed into a picture, but is dissipated into a 'wave pattern' over the cortex, which bears little resemblance to the original picture." Zeeman's most important suggestion was that "the set of all such patterns has a *global mathematical structure similar to that of the sets of all pictures.*[53]

Within a certain tolerance limit in order that concepts (and pictures) be represented not by points but by small regions of the thought cube, Zeeman introduced an analogy between states of the cube and the perception of pictures. He claimed that an isomorphism existed between pictures and states of the brain, and that this could be

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[51] E. C. Zeeman, "Topology of the Brain," 277. My emphasis.
[52] E. C. Zeeman, "Mathematics and Creative Thinking."
[53] E. C. Zeeman, "Topology of the Brain," 278.

expressed mathematically. Zeeman's underlying assumption was that, granted the mechanism of the mind relied on the electrochemical interactions of neurons, new (mathematical) tools were necessary in order to understand how it worked. The "brew of electrons," as Thom wrote, provided no explanation at all.[54] Zeeman underscored that his model "was based on the well known anatomical structure of the brain."[55] But even while affirming this, he had trouble convincing biologists that his topological model—a cube in 10^{10}-dimensional space—bore any relation to the actual anatomy of the brain.[56] Zeeman's approach was moreover difficult to test in the laboratory.

> The results are expressed in geometrical language, and are *qualitative* rather than *quantitative*. This means that so far the theory to be described in this paper has attempted to *explain* phenomena rather than *predict* the measurements that experiment would obtain.[57]

Here lay a source for Thom's later pronouncement: "To predict is not to explain."[58] But Zeeman did not give up all hopes. He would "like to use the model to make predictions," he confessed, "but so far it had only given qualitative explanations of familiar phenomena."[59]

In brief, we may characterize Zeeman's approach as an exploitation of recent topological tools in order to make sense of the way the brain worked. But it did so without paying attention to the physical and electrochemical processes of neural activity. This disjunction between these processes and his topological model hardly mattered since

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[54] R. Thom, "Topologie et signification," *MMM,* 174.
[55] E. C. Zeeman, "Topology of the Brain," 291.
[56] See Sir Edward Collingwood's comment at the end of E. C. Zeeman, "Topology of the Brain," 292.
[57] E. C. Zeeman, "Topology of the Brain," 277. Original emphasis.
[58] R. Thom, *Prédire n'est pas expliquer* (Paris: Eshel, 1991). This theme was already raised in *SSM,* 5-6.

an isomorphism was postulated between the outer world (i.e. a mathematical model of our perception of it) and the structure of the brain (i.e. a mathematical model of it). As I discussed in Chapter III above, the main lesson about Zeeman's modeling practice later taken up by Thom was that recent advances in topology offered powerful new ways to model mathematically the global behavior of complex systems.

c) Convergence: The Spring of 1966

At the General Assembly of the IHÉS on April 6, 1967, Francis Perrin was intrigued by the title of a lecture given by Thom on May 2: "Topologie comparée de la gastrulation chez les vertébrés," probably a practice for the talk he would give the next summer at the Bellagio conference organized by Waddington. For the first time, he introduced the notion of "catastrophies" (as he then wrote in English). Although it did not show in his Scientific Report, Motchane was enthusiastic about it. He replied Perrin that Thom had written a book to be published by Benjamin [*SSM*], which perhaps set the first stone of the Third Section of the IHÉS.[60] Already on December 1, 1966, at an earlier General Assembly, where Motchane had presented his Scientific Report for 1965 (but which Perrin did not attend), the director had lauded, without mentioning the book, his mathematics professor:

[59] E. C. Zeeman, "Topology of the Brain," 292.
[60] *Notes de séance* manuscrites de Annie Rolland, *Assemblée générale* (6/4/67); *Eléments de Rapport scientifique sur l'exercice 1966, Année 1966 - Séminaires et conférences* (6/4/67), 2. Arch. IHÉS.

> Thom, a Fields Medalist full of genius, disgusted by topology, did remarkable things in differential geometry [and concerning the] generalization of the stability of trajectories.[61]

With the repute of the IHÉS ever growing, a foremost American algebraicist, Saunders Mac Lane, decided to visit the Institute in the spring of 1966, even though, in Thom's word, "he interested no one" there.[62] A Christmas card he and his wife sent around provides an unusual snapshot of the atmosphere reigning at the IHÉS:

> [It] has a simple but idyllically beautiful setting, coupled with hard and concentrated thinking—an excellent luncheon daily with a lively group of incisive Physicists, plus catastrophies in Mathematical Biology and schemas in Algebraic Geometry.[63]

The months of April and May 1966 had indeed witnessed a singular convergence of people and topics at the IHÉS. With Zeeman present, but still addressing issues of algebraic topology, there were, besides Thom's, two more talks that would have an important impact on the subsequent activities of the Institute. On April 19 to 22, Edinburgh embryologist Conrad H. Waddington visited the IHÉS. In May, French mathematician Bernard Morin, discussed the "Weierstrass-Malgrange Preparation Theorem According to J. Mather." The same week, Steve Smale gave a seminar on

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[61] *Notes de séance* manuscrites de Annie Rolland, *Assemblée générale* (1/12/66). The first version of Thom's book was by then finished: lettre de W. A. Benjamin à Annie Rolland (2/12/66); the revised version would not be ready before the end of May 1967: lettre de René Thom à Miss Shapiro, assistant-editor at Benjamin (14/3/67). Arch. IHÉS.
[62] Note manuscrite de Annie Rolland (25/8/65). Arch. IHÉS.
[63] Carte de Noël de Saunders et Dorothy Mac Lane à Annie Rolland (21/12/66). The Mac Lanes arrived at Bures on March 8, 1966. Note manuscrite de Annie Rolland (10/6/65) and (25/8/65); lettre de Saunders Mac Lane à Annie Rolland (3/4/66). Arch. IHÉS.

"Differentiable Dynamical System."[64] As a result, when he got back to Warwick, Zeeman wrote to Motchane that as usual the Institute had provided him

> an escape from administration and an opportunity to do mathematics again, but I find the Institute a very stimulating environment. I was particularly delighted this time to have sessions with Thom and Waddington and gain deeper understanding of Thom's book.[65]

The invitations envisaged by Thom during the year 1966 reflected his preoccupation: Ralph H. Abraham (who was teaching at Princeton at the time, after having worked with Smale at Columbia); Solomon Lefschetz (who however never came); D. Anosov (whom Thom invited upon caming back from Moscow); and John N. Mather (who had just finished his Ph.D. at Princeton).[66]

3. THE EMERGENCE OF A MODELING PRACTICE, 1966-1970

Between 1966 and 1970, the research conducted around Thom at the IHÉS was mainly directed towards achieving a proof of his old conjecture on singularities. But at the same time, Thom was moving away from purely mathematical concerns. Having written the first version of his book, he worked on developing catastrophe models for embryology and linguistics, which, as we saw in Chapter III, informed the shape of catastrophe theory. Simultaneously, Thom started to advocate it on a wide variety of stages.

Around Thom, some important scientists became interested in his ideas. Smale, Zeeman, Abraham, Ruelle, Grothendieck, and even Motchane recognized in Thom's

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64 *Eléments de Rapport scientifique sur l'exercice 1966, Année 1966 - Séminaires et conférences* (6/4/67), 2-3. Arch. IHÉS.
65 Lettre de E. C. Zeeman à Léon Motchane (5/5/66). Arch. IHÉS.
66 *Petit comité scientifique* (31/5/66); lettre de Ralph Abraham à Léon Motchane (24/10/66); de Léon Motchane à Solomon Lefschetz (2/6/66); carte postale de René Thom

approach ideas they could combine with their own previous theoretical practices in order to come up with new kinds of models. These years witnessed the establishment of an international network of scientists interested in global analysis and qualitative dynamics. Besides the IHÉS, the most important nodes of this network were Smale's group at Berkeley, and soon the Warwick Mathematical Institute headed by Zeeman. It was at Bures, however, that the push away from pure mathematics towards applications seemed the strongest.

a) The Stability of C^∞-Mappings

On November 1, 1966, Gil Hunt wrote to Orsay mathematician Jacques Deny about the Princeton people who might spend a sabbatical year in Paris in 1967-68. Among them was John Mather. "[C]onsidered by Milnor and others to be the best graduate student in several years [he] will receive his doctorate in June and would like to spend a year or two in Paris. Malgrange already heard of him."[67] Indeed, following Morin's presentation of his work, Mather was extremely welcome at the IHÉS. Motchane immediately invited him and Mather accepted to come.[68] Arriving at Bures in September 1967, Mather presented the "machinery" he had used for proving the Weierstrass-Malgrange preparation theorem

à Léon Motchane (n.d. between his trips to Moscow and Bellagio); lettre de Léon Motchane à John Mather (5/12/66). Arch. IHÉS.

[67] Lettre de Gil Hunt à Jacques Deny (1/11/66). Arch. IHÉS.

[68] Lettres de Léon Motchane à John Mather (5/12/66); de John Mather à Léon Motchane (31/1/67). Arch. IHÉS.

and suggested that it could be used for the classification of catastrophe. He was followed by Polish mathematician Stanislas Lojasiewicz's description of his own proof.[69]

Spending two years at the IHÉS, Mather wrote a series of technical papers which, not quite entirely avoiding Thom's heavy arsenal of stratified sets, did succeed in providing a satisfactory proof of his classification theorem.[70] As result, Thom wrote on November 17, 1967:

> I have the *highest* regard for John Mather. He entirely solved a problem of a great difficulty (and probably of a great importance): that of the structural stability of differentiable mappings.[71]

b) Consequences of Smale's Counterexample

Meanwhile, in the spring of 1967, previous collaborators of Smale's, important for the subsequent history of 'applied topology', lectured at the IHÉS in the *Séminaire Thom.* In particular, came Ralph Abraham (from Princeton) who talked about Anosov flows, and Nicolaas H. Kuiper. Most important was the semester-long visit of Charles Pugh (from Berkeley). He gave several talks at the Institute, and on February 27, 1967, introduced Smale's counterexample. I do not know if Ruelle attended Pugh's lectures. They predated his initial interest in dynamical systems, and while he did emphasize some of Thom's and Smale's lectures, Ruelle never mentioned Pugh's. As will be made clear in Chapter VII,

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[69] *Elements de rapport scientifique* (8/5/68), *Année 1967 - Séminaires et conférences,* 6. Arch. IHÉS. Mather used the term "machinery" in "On the Preparation Theorem of Malgrange," Princeton University preprint (April 1966).

[70] J. N. Mather, "Structural Stability of Mappings," Princeton University preprint (November 1966), both in Fine Arch.; "Stability of C^∞ Mappings," Parts I and II, *Annals of Mathematics,* 87 (1968): 89-104; 89 (1969): 254-291; Parts III and IV, *Publications mathématiques de l'IHÉS,* 35 (1968): 127-156; 37 (1969), 223-248; Part V, *Advances in Mathematics,* 4 (1970): 301-336; and Part VI, *Proceedings of the Liverpool Singularities Symposium,* Lecture Notes in Mathematics no. 192 (Berlin: Springer, 1971): 207-253.

Ruelle crucially depended on Smale's counterexample as a source for his later concerns with fluid dynamics.

Logically, Smale's counterexample—showing that that structurally stable systems were not generic—should have stricken a deathblow to Thom's hopes as put forward in *SSM.* "This result," Thom later acknowledged, "was a kind of catastrophe for my view of things."[72] He nevertheless chose to push forward for reasons he mentioned at the Moscow Congress:

> The very notion of structural stability however is far from having lost all interest: first, because there exists, in the functional space of vector fields on a manifold, a 'relatively important' generic open set of vector fields of gradient type (without recurrence) and, probably, a class of fields, defined by Smale (the so-called Morse-Smale vector fields) which exhibit recurrence (with closed trajectories) but under a benign, severely controlled form."[73]

Thom still believed that structural stability could serve as a useful guideline for his modeling practice. One reason, as he explained above, was that he emphasized gradient systems, conjectured by Max Delbrück to be at the roots of cell differentiation, which, generically, were structurally stable. This explains why much of catastrophe theory focused on elementary catastrophes. This also became a major weakness of Thom's philosophy since, clearly, not every system was a gradient system. "As we are far from understanding" generalized catastrophes, Thom acknowledged, "the models that will be presented . . . are bound to be imprecise."[74] Because of this mathematical dead end, Thom moved away from a classification of models based solely on topological premises back to philosophy.

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[71] Lettre de recommendation de René Thom à W. T. Martin (17/11/67). Arch. IHÉS.
[72] R. Thom, "Exposé introductif," 32.
[73] R. Thom, "Sur les travaux de Stephen Smale," 27.

In 1967, after a visit to the IHÉS, Abraham wrote the introduction to the lecture notes of a course on mechanics given in the spring of 1966 at Princeton University. He explicated a bit of Thom's philosophy. Using arguments similar to those of Andronov's and Lefschetz's, and citing Pierre Duhem, he explained, that the usefulness of a theory, the adequacy between models and experimental data, hinged on the a criterion of *stability.* "Although this criterion has not been discussed very explicitly by physicists, it has functioned as a tacit assumption, which may be called the *dogma of stability.*"[75] Abraham claimed that, by postulating structural stability in a specific theory, Thom offered an alternative to the dogma of stability.

c) May 68 at Bures: 'Le Bois-Marie Never Looses Her Magic'[76]

"It is useless to tell you that Bures was quiet, but you may guess how much this turmoil touched us."[77] Thus did Annie Rolland, the General Secretary of the IHÉS, comment the student revolts and general strikes that shook the country in May 1968. Its financial consequences aside, these hardly seemed to have impacted the Institute.[78] The following months witnessed a striking increase of Zeeman's and Ruelle's interest in Thom's theory.

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[74] R. Thom, *SSM,* 28.

[75] Ralph Abraham and Jerrold E. Marsden, *Foundations of Mechanics: A Mathematical Exposition of Classical Mechanics with an Introduction to the Qualitative Theory of Dynamical Systems and Applications to the Three-Body Problem* (New York: W. A. Benjamin, 1967), 3-4; this introduction is repr. in R. Abraham, *On Morphodynamics,* 1-4. See P. Duhem, *The Aim and Structure of Physical Theory,* transl. P. P. Wiener (Princeton: Princeton University Press, 1954).

[76] Lettre de E. C. Zeeman à Léon Motchane (18/8/68). Arch. IHÉS.

[77] Lettre de Annie Rolland à Jacques et Marie-Jeanne Tits (1/7/68). Arch. IHÉS.

[78] Lettre de Léon Motchane à Edgar Faure, Ministre de l'Education nationale (29/11/68). Arch. IHÉS.

On January 28, René Thom discussed "Qualitative Dynamics and Morphogenesis" in front of a distinguished audience, following the general assembly of the Société mathématique de France (SMF). During the winter term, however, most of talks at Thom's seminar, given by Thom himself, Lojasiewicz, Mather, Malgrange and Jean-Claude Tougeron, were devoted to the stability of differentiable applications and to Thom's theory of stratification.[79]

(i) Zeeman Dives In

In April, Christopher Zeeman visited the Institute for one month, leaving just before the beginning of the strikes. For the first time in years, he did not lecture. He was starting to study Thom's ideas very carefully. In August, he wrote Motchane:

> In particular, I am getting very excited about dynamical systems. The coming year we are having a year-long symposium in the subject at Warwick. . . . The following year I hear there will be much activity in Paris. [While at Bures,] I spent the whole month digging into Thom's ideas on catastrophes which was a particular pleasure, and since then I have lectured on his work in several places, and consequently I am beginning to understand it. I gave a summer course at Aarhus on dynamical systems and am devising some lecture notes giving, I hope, an elementary armchair introduction – I will send you a copy when they are finished to test out in your armchair!

Zeeman then explained how he literally got into the water. Catastrophe theory was starting directly to inform his own modeling practice.

> One facet on Thom's ideas I found very intriguing is the application to breaking waves. To the amusement of the family, I spent several hours of our summer holiday up to my neck in the sea attempting to photograph the waves as they break [!!!]. The shape is quite unlike what I had previously imagined – it is as if a thin jet stream shoots out from the crest, confirming Thom's prediction that it has to do

[79] *Rapport scientifique 1968.* Arch. IHÉS. Note that Thom became presdent of the SMF in March.

> with his hyperbolic umbilic catastrophy [*sic*]. As far as I can discover there has been no satisfactory explanation previously.[80]

In his reply to Zeeman, Motchane revealed that he also had become interested in Thom's ideas about "dynamical systems." He was reading an article by Arnol'd, and was "eager and anxious to read to receive [Zeeman's] elementary arm-chair introduction, since sitting in my arm-chair I can afford all catastrophies."[81]

(ii) The Road to Ruelle's Turbulence

From May 16 to 18, 1968, there was a meeting of mathematicians and physicists at the University of Strasbourg, where David Ruelle spoke on statistical mechanics. Jean Leray, a professor of mathematics at the Collège de France, who in his 1931 doctoral thesis had proposed a mechanism for the onset of turbulence, was also present (Chapter VII). I do not know if Ruelle discussed turbulence with him at the time, but coming back from Strasbourg, Ruelle read Landau and Lifschitz's *Fluid Mechanics.*

In August 1968, Ruelle put the final touch to a book which gathered several years of extremely successful work on the mathematical foundations of statistical mechanics.[82] Feeling done with this, he was looking for another research topic. During the fall semester, 1968, Ruelle visited the University of California, Irvine.[83] When he got there, he wrote to Motchane:

> It seemed to me that the time had come for me to try and do something other than statistical mechanics, and that my stay here was particularly appropriate for such a

[80] Lettre de E. C. Zeeman à Léon Motchane (18/8/68). Arch. IHÉS. Original English. See E. C. Zeeman, "Breaking of Waves," *Proceedings of the Symposium on Differential Equations and Dynamical Systems,* ed. D. Chillingworth (Berlin: Springer, 1971): 2-6.
[81] Lettre de Léon Motchane à E. C. Zeeman (16/10/68). Arch. IHÉS. Original English.
[82] *Statistical Physics: Rigorous Results* (New York: Benjamin, 1969).
[83] Lettre de David Ruelle à Léon Motchane (5/2/68). Arch. IHÉS.

> change. For the time being, I am therefore trying to look at some problems of hydrodynamics or, more generally, of 'dissipative phenomena' from a physical point of view analogous to Thom's. These phenomena are fascinating, but one does not see very clearly what can be done mathematically. *There is a very good chance that nothing will come out of all this, in which case I would stop there* [je tirerai une ligne], *I'll give a seminar and move on* [j'arrêterai les frais].[84]

This letter of Ruelle's shows not only that he started to think about applying Thom's ideas to fluid mechanics as early as 1968,but also that he considered the general problems of dissipative systems.[85] It moreover shows how uncertain he was about the fruitfulness of this approach.

That physics could benefit from the modeling technology develop around Thom, Smale and Mather was a natural idea to come up with. Indeed, as early as during the summer of 1967, a meeting had been held at the Battelle Research Center at Seattle in order to get mathematicians and physicists to speak to one another. Organized by Cecile DeWitt and John A. Wheeler, both relativity theorists, it gathered many IHÉS visitors. Significantly, on the mathematics side, topology was emphasized, and Thom, Smale, and Mather tried to convince physicists of the importance of their ideas.[86]

Because he saw Ruelle as attacking some of the very issues that had motivated him in founding the IHÉS in 1958, Motchane was delighted about Ruelle's new direction of research. "Your letter greatly pleased me. Specially what you said about your 'change in orientation'." He added his own advice about the problems he might face:

> If you have in mind 'dissipative phenomena' in a very general sense, there do *not exist as yet,* to my knowledge, good tools, nor adequate method to tackle the study of [these] physical phenomena. You fall back on '*the* great problem' presently,

[84] Lettre de David Ruelle à Léon Motchane (7/10/68). Arch. IHÉS. My emphasis.
[85] D. Ruelle, *Chance and Chaos,* 53.
[86] C. M. DeWitt and J. A. Wheeler, eds., *Battelle Rencontres: 1967 Lectures in Mathematics and Physics* (New York: Benjamin, 1968).

> namely: the search for a mathematical instrument *specifically adapted* to the interpretation of physical phenomena.[87]

The interest of some physicists for Thom's ideas had become clear. In March 1968, Malgrange addressed the IHÉS physicists about the stability of differential mappings, while, two weeks later, Thom spoke of "Resonances and catastrophes."[88] Meanwhile Thom was pursuing his crusade for the popularization of his ideas. In May, he participated to the *journées* on differential analysis at Rennes. In the fall, he gave talks at Orsay and the École polytechnique. Still in 1968, he published his first article on linguistics. At the same time, he was preparing the invitations for the next year, thinking of Palis, Williams, Peixoto, Smale, Shub, Milnor, etc.[89]

(iii) Motchane: Formal Structures of Real World

Besides playing an important institutional role, Léon Motchane must have also contributed to the reflections on the nature of modeling taking place at the IHÉS in the late 1960s. In his yearly *Scientific Report,* he often briefly insisted on the methodological revolution that new Bourbakist conceptions of mathematics brought about. For Motchane, Bourbaki was now allowing the mathematician to meddle with vast areas of the sciences.

> In a large number of cases, a kinship of structures in extremely diverse domains has been noticed. This allows today's mathematicians, without for all this becoming an expert in a branch that is not the object of his study, to understand its essential [features].[90]

The Bourbakist reordering of mathematics which emphasized the structure concept provided mathematicians with tools that would not suddenly make experts in a

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[87] Lettre de Léon Motchane à David Ruelle (16/10/68). Arch. IHÉS. Original emphasis.
[88] *Rapport sur le séminaire de Physique théorique* (31/3/68). Arch. IHÉS.
[89] *Petit comité scientifique* (25/6/68). Arch. IHÉS.

foreign field of them, but which enabled them to grasp the deep structures, the essence, of these other fields. In the best of cases, a dialogue could then be established between the mathematician and the expert.

In the archives of the IHÉS, I discovered a reprint of Motchane's, probably dating from around the same period, that considerably complicated some of Zeeman's reflections above.[91] Plainly, he stated that he "identified *scientific knowledge* and *structure.*" Any structure of the (physical) universe U, he argued, was induced by a structure (in Bourbaki's sense) on an appropriate mathematical space K, induced by the inverse image of the elements of F, a set grouping "all of our means of observation in a large sense," i.e. all the functions $f \in F$, $f\colon U \rightarrow K$. More concretely, an element f may represent a specific natural law, any of which being a mapping of the world to a mathematical space.

Motchane defined the "domain of scientific knowledge" as being a subset of the universe U determined by the inverse images $f^{-1}(K)$ of some mathematical spaces K. Taking the real line K=$\mathbf{R}$ as an example, Motchane asked: what subset of the universe does it represent? "This domain of the universe," he answered, "will contain physics, certain portions of chemistry and physical chemistry, but also domains belonging to other sciences that could be dealt with by using mathematics, like biology, sociology, economics, etc." This domain of the world therefore was a new concept, which did not exactly recover the "classic ideas of the exact sciences." He suggested to designate it by the outmoded term of *natural philosophy*, also picked up by Thom:

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90 L. Motchane, "Éléments de Rapports scientifique [1967] à l'Assemblée [générale du 8/5/68]," 4. Arch. IHÉS.

91 L. Motchane, "Structures formelles du monde réel," reprint (pp. 105-110). Arch. IHÉS. Unfortunately, I have been unable to locate neither its place, nor its date of publication.

> "Natural Philosophy" therefore is the domain of our observations susceptible of being studied, analyzed, endowed with structures, by the mathematical method.

As a radical consequence of this view of things, Motchane claimed, this "classification is done by the means of the method, without being too much concerned with the nature of the phenomena." This method for the modeling of the world's phenomena chopped off the universe in different domains that were defined by the kinship of their mathematical structures alone, rather than by phenomena or substrata. These vague considerations, Motchane believed, shed a new light on the process of scientific thought:

> This process makes clear the fact that the very structure of the world of phenomena—provided that this term had a precise meaning—does not influence the formation of scientific laws at all. More exactly, one should say that the scientist is scarcely concerned with this structure, knowing that he does not have a chance to grasp it [directly].

Motchane's conception of the Bourbakist impact on scientific modeling indicates that he shared similar concerns as Thom's, Ruelle's, and Zeeman's. Moreover, they underscore some of their common assumptions. They held that the ontology of the world was unreachable directly, and that our knowledge of it could be mediated by mathematical models. As a consequence, the modeling practice informed by this belief could not be dictated by the phenomena themselves. The main guideline for the modeling of natural phenomena rather were mathematical objects. The tools of global analysis, structural stability and genericity, by Motchane's inverse mapping process, revealed a whole new domain of the world that now could be modeled. This philosophy explains why, in the late 1960s and early 1970s, 'applied topologists' felt entitled to tackle so many different subjects without taking the time to become experts in any of them. Mathematics alone dictated what could be studied.

d) Mathematics versus Rhetoric: The Case against Deligne

In April 1968, a first "crisis," in Motchane's word, erupted at the IHÉS. It was triggered by Thom's opposition to the hiring of Pierre Deligne as a permanent professor of the IHÉS. Born in 1944, Deligne was a young Belgian mathematical genius who, for the last two years, had been working on algebraic geometry at Bures in close collaboration with Grothendieck. This episode highlights an aspect of Thom's view of the role of mathematics.

In May 1966, Thom, Motchane, and Grothendieck had agreed to invite Deligne to IHÉS "for 3 or 4 years."[92] Less then two years later, Grothendieck estimated that he had obtained six results "each one of which could constitute the central result of an excellent doctoral thesis."[93] Grothendieck, like Dieudonné when he presented him with his Fields Medal in 1966, lavishly compared him with none less than David Hilbert. "The mathematical center to which he will belong will become a radiating center like Göttingen."[94] Considering that Grothendieck's predominant influence on Deligne might unbalance the Mathematics Section of the IHÉS, Thom was not convinced, but proposed that the IHÉS keep him as a temporary member for still a few years.[95]

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[92] *Petit comité scientifique* (31/5/66). Arch. IHÉS.
[93] *Note sur les travaux de P. Deligne,* par Alexander Grothendieck (mars 1968). Arch. IHÉS.
[94] *Procès-verbal du Comité scientifique du lundi 1er avril 1968* (11/5/69). Arch. IHÉS.
[95] *Notes pour le Rapport scientifique 1968* (10/12/69); lettres de Léon Motchane à Pierre Deligne (20/5/68); de Pierre Deligne à Léon Motchane (22/5/68). Arch. IHÉS.

The following year, on May 11, 1969, after a particularly fruitful year for him, the matter of Deligne's hiring came up once again. But Thom remained skeptical. He was too young; he had never taught, never traveled; he was not ready for this kind of position, Thom demurred. He feared that with Deligne's nomination, he would be in minority.[96] But his main objection remained a matter of mathematical style. Deligne was very gifted, but solely in algebraic geometry.

> What counts is not really what a mathematician finds, but the problems he raises. . . . On the list of [Deligne's] works, there are only three or four [of them] that do not appear to me as pure rhetoric.[97]

Thom's position cannot be separated from the opposition to the contemporary Bourbakist-algebraicist attitude, which he expressed over the following years.[98] For Thom, any question in algebra was "either trivial or impossible to solve."[99] This underscores Thom's belief that mathematics needed to break away from its isolation. Neither a lonely queen living in an ivory tower, nor submissive servant to other sciences, mathematics was the means for achieving new intelligibility of the world.

Eventually Deligne was hired without Thom's consent. "Objectively," Motchane said, his nomination "was imperative."[100] Like Poincaré a "monster of mathematics," to

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96 "Il y aura deux géomètres algébrique contre un seul Thom [!]." *Notes de séance manuscrites du Comité scientifique* (11/5/69). Arch. IHÉS.
97 *Notes de séance manuscrites du Comité scientifique* (11/5/69). Arch. IHÉS.
98 R. Thom, "Les mathématiques modernes: une erreur pédagogique et philosophique?" *L'Âge de la science,* 3(3) (1970): 225-242; repr. *Pourquoi la mathématique?,* ed. R. Jaulin: 57-88; transl. "Modern Mathematics: An Educational or Philosophical Error?" *American Scientist,* 59 (1971): 695-699; and "Mathématiques modernes et mathématique de toujours," talk given at the Mathematical Pedagogy Congress of Exeter (1972); repr. *Pourquoi la mathématique?,* ed. R. Jaulin: 39-56.
99 R. Thom, "Modern Mathematics," 696.
100 *Notes pour le Rapport scientifique 1968* (10/12/69). Arch. IHÉS.

use Dieudonné's phrase, "Deligne proved the Weil Conjecture on July 2, 1973."[101] For this, he was awarded the extremely rarely conferred *Henri Poincaré* Medal from the Académie des sciences, and the Fields Medal at the Congress of Mathematicians in 1978. Once again, Motchane's intuition had served the IHÉS well.

e) The Network in Full Swing

In the summer of 1968, a symposium on global analysis placed under Smale's aegis was organized in Berkeley by the American Mathematical Society.[102] The next year, Zeeman's Mathematics Institute at Warwick held a year-long symposium and a summer school devoted to similar topics.[103] These meetings saw the final convergence of people and topics which would form the dynamical systems background for later excitement about catastrophe and chaos theories. As Lawrence Markus explained, the purpose of the Warwick Symposium was twofold:

> i) research – to draw together the leaders in the fields of differentiable dynamics and the more classical parts of the qualitative theory of ordinary differential equations, such as oscillation, stability and control theory, for an extended duration in an atmosphere of active and creative research.
>
> ii) education – to encourage the consolidation of the new developments in differentiable dynamics and its application after a decade of profound but rather frantic and *chaotic* investigation, and to disseminate this information among

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[101] *Communication by Directeur* (Nicolaas Kuiper), *Comité scientifique* (16/11/73); *Rapport sur les travaux de P. Deligne* (médaille Henri Poincaré), par Jean Dieudonné (9/12/74). Arch. IHÉS.

[102] Shiing-Shen Chern and Stephen Smale, eds., *Global Analysis: Proceedings of the Symposium in Pure Mathematics (Berkeley, 1968)*, 3 vols. (Providence: American Mathematical Society, 1970).

[103] D. R. J. Chillingworth, ed., *Proceedings of the Symposium on Differential Equations and Dynamical Systems: University of Warwick, September 1968 - August 1969, Summer School, July 15-25, 1969*, Lecture Notes in Mathematics, 206 (Berlin: Springer, 1971).

> mathematicians and scientists of the UK through seminars, instructional courses and schools.[104]

Pushing for greater support from the SRC for the development of differential equation studies, Christopher Zeeman was especially pleased with this:

> The Warwick Symposium and Summer School on differential equations went like a bomb, and may have interesting repercussions on the number of research students opting to go into the field.[105]

The attraction of the IHÉS was being felt especially strongly at this time. In January 1969, Zeeman wrote to Motchane a letter that could only make him proud of his achievement. "Princeton [i.e. the IAS] has been pressing me to go there, but I would much prefer to come to you, especially as I understand that both Smale and Thom will be with you."[106] At least in one field, the pupil had outdone its master. Letters asking to come and work with Thom became more and more frequent. One example was a letter Karl Sigmund, from Vienna, in which he indicated that, waiting for the publication of his book, he was reading Abraham, Smale and Zeeman.[107] In 1969-1970, the topic of Thom's seminar became "Qualitative Dynamics" at large. Collaborators included Smale's students (including Rufus Bowen, his "best student," according to a handwritten note of Rolland), Smale himself, Peixoto, Takens, and Zeeman (who was accompanied by David Fowler,

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[104] L. Markus, "Dynamical Systems – Five Years Later," *Dynamical Systems – Warwick 1974,* ed. A. Manning (Berlin: Springer, 1975): 354-365, 354. My emphasis.
[105] PS d'une lettre de E. C. Zeeman à I. M. Sneddon, Glasgow (19/9/69).
[106] Lettre de E. C. Zeeman à Léon Motchane (3/1/69). Arch. IHÉS.
[107] Lettres de Karl Sigmund à René Thom (21/5/69); de Léon Motchane à Philippe Husson (29/5/69). Arch. IHÉS.

Figure 12: René Thom Lecturing on Catastrophe Theory at the IHÉS in the Early 1970s. Copyright © Arch. IHÉS.

future translator of Thom's *SSM*).[108] For some, it must have felt as if Berkeley had moved to Bures-sur-Yvette!

[108] Lettres de Léon Motchane à R. F. Williams (17/3/69); de Nicolaas Kuiper à René Thom (16/4/69); de Floris Takens à René Thom (27/6/69); de E. C. Zeeman à Annie Rolland (10/7/69); de E. C. Zeeman à Annie Rolland (22/7/69); de Rufus Bowen à Annie Rolland, et à Léon Motchane (5/11/69); Note manuscrite de Annie Rolland (31/10/69). Arch. IHÉS.

4. EXTERNAL SUCCESS AND INTERNAL CRISES, 1969-1972

While "its situation on the scientific level seems to me more brilliant and promising for the future than it ever was," Grothendieck wrote Motchane on January 26, 1970, "the IHÉS goes through a period of crisis of which it is difficult to foresee the outcome."[109] Indeed, financial stability due to the Europeanization of its pool of sponsors was finally in view. But never this young institution had been so close to explode because of internal divisions. The crises that shook the IHÉS in 1970 were to affect the Institute deeply.

Paradoxically, it was at the same time that the attraction of the modeling practices of qualitative dynamics promoted by Thom, exerted the strongest attraction on members of the permanent faculty. Particularly striking was Grothendieck's involvement in biology based on his interest for Thom's ideas. One of the purest Bourbakists was turning to applications. This highlights the exceptional cooperation across disciplines that then characterized the IHÉS. But in order to do full justice to Grothendieck's change of interests, it is necessary to digress somewhat and go into the political issues that underlay his metamorphosis. After he quit the IHÉS, nothing would ever be the same. In the following years however, catastrophe theory had finally matured enough so that Motchane thought he could at last build the Third Section on this basis.

a) First Skirmish

The crisis erupted on October 1, 1969. In a letter to Motchane signed by the four of them, the permanent professors of the IHÉS (Grothendieck, Michel, Ruelle, and Thom) expressed their unease about some rumors that had been floating around. "We have been

[109] Lettre de Alexander Grothendieck à Léon Motchane (24/1/70). Arch IHÉS.

surprised not to learn from you" of a project for modifying the bylaws of the Institute and of the formal proposal made to Res Jost (theoretical physicist from the ETH, Zürich, and Ruelle's mentor) to become its next director. They demanded that a meeting of the Scientific Committee (CS) be convoked to discuss these matters.

At that time, Motchane was 70 years old, and might have been already looking for his successor for after his retirement, normally four years away. But the big project he was working on was the transformation of the IHÉS in order to be supported by different national research foundations in Europe. Motchane's direction style had always been authoritarian, and he was somewhat taken aback by his professors' reaction. In addition, the information they had might not have been quite exact. On November 3, he replied them: "I did not understand - and I still do not understand - [your] affirmations."[110]

Unfortunately, Grothendieck's impatience had quickly turned this lack of communication into a personal conflict. On October 24, Grothendieck escalated the dispute. Reiterating his demand for a meeting of the CS, he accused Motchane of conspiracy.

> (faithful to your bizarre principle that the most shriveled line is the shortest way between two points) you make phone calls right and left to insure to which extent it would be possible for you to spread confusion [*brouiller les cartes*].

Grothendieck expressed the wish that Motchane saw in the professors "competent, good-willing collaborators in your task of insuring the survival and continuity of the IHÉS,

[110] Lettre de Léon Motchane à Alexander Grothendieck, cc: Michel, Ruelle et Thom (3/11/69). Arch. IHÉS.

rather than a gallery to circumvent."[111] Motchane could quickly lose his temper, and on this occasion, he did.

> Your letter is of monumental thoughlessness [*un monument d'inconscience*]. How dare you, whose competence outside of mathematics is limited, all judge, give your appreciation on everything? . . . You should meditate on the parable of the straw and the beam.

A touching handwritten addendum, which I ignore whether it was sent, however tried to mend the disagreement.

> My dear Grothendieck,
>
> I would like to had a few words to this letter, and these few words may well be what is essential.
>
> It is true that friendship between men cannot be dictated; it comes and goes through unpredictable affinities. But the converse is also frequent and still more damageable when a current of sympathy is forbidden to be established for fortuitous reasons, because of one's tactlessness, or - worst still - for no reason at all.
>
> Nothing is more painful to me than to feel a hostility of which ignore the cause, and even more so when it thwarts my spontaneous feeling of sympathy. . . . While we may be different, coming from different countries, with a different education, many things, and the most important ones, unite us, for we have the same moral reactions to grave problems. Then, [there is] our shared responsibility in our common endeavor: to safeguard the Institute for the future as a refuge where man's freedom and dignity is respected. To reestablish the climate is all this for me.
>
> Sincerely yours, L.[112]

On November 12, a meeting indeed took place between Motchane and the four permanent professors whose minutes I could not find in the archives. This meeting reestablished a more healthy atmosphere at the IHÉS, Motchane promising to involve his

[111] Lettre de Alexander Grothendieck à Léon Motchane (24/10/69). Arch. IHÉS.

professors in the transformation processes. In particular, he promised to send them a copy of the budget.

b) Grothendieck, the IHÉS, and The Military

Three days later, Grothendieck wrote him an indignant letter which started another affair. "You omitted to send me a copy of the budget of the IHÉS. This 'neglect' nonetheless did not forbid me to learn that 5% of the budget of the IHÉS are presently provided by the French Military [*forces armées françaises*]." He moreover claimed that Motchane was informed since the beginning that he, Grothendieck, was not ready to keep on working at the IHÉS if a portion, no matter how small, of its finances came from a military source, and that Motchane has assured him that this was not, and would never be, the case. From this fact, "I immediately and irrevocably draw the necessary consequences on a professional, as much as on a personal, level."

Grothendieck gave Motchane an ultimatum. He asked for the written assurance that no portion of the IHÉS budget would come from the military for the following year and for as long as Motchane would remain director. "Short of receiving this assurance, I cannot keep on occupying my functions at the IHÉS." In addition, he promised to give to this question "all the publicity it deserves in my opinion."[113]

On November 24, Motchane sent Grothendieck, and the other professors of the IHÉS a long letter in order to set the records straight: "in ten years, you never one single

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112 Lettre de Léon Motchane à Alexander Grothendieck, avec addendum manuscrit (28/10/69). Arch. IHÉS. For an expression of Motchane's Gaullist socialist beliefs, see Thimerais [Léon Motchane], *La pensée patiente* (Paris: Minuit, 1943).

113 Lettre de Alexander Grothendieck à Léon Motchane (15/11/69). Arch. IHÉS.

time came to tell me about your opinions and convictions, nor to inquire of the Institute's position. . . . But of course, I did not ignore that you shared with the majority of scientists a concern for not working for war and, at the occasion of your trip to Vietnam, you were able to learn of my position on this matter which, for that matter, does not differ from yours."[114] Indeed, the archives of the IHÉS preserve little evidence to support Grothendieck's claim that he had in the past voiced his opposition to certain sources of funding.

(i) Grothendieck's Politics and Biology

In 1960 however, when Motchane discussed with the European Atomic Organization (Euratom) a possible contribution in return for the participation of one of its scientist to the Scientific Committee, Grothendieck had expressed his opposition. The reasons he gave had nothing to do with moral concerns about nuclear issues, but only questioned the principle by which Euratom's financial support was linked to its having a word to say in the scientific activities of the IHÉS.[115] Motchane might have learned from this not to involve Grothendieck in administrative matters, which according to the bylaws were none of his business anyway.

That Grothendieck had liberal views, however, was a secret for no one. During the Algerian War, "because of the internal political situation in France," he even envisaged to move to the US.[116] In August 1966, Grothendieck who was to receive the Fields Medal at the Moscow Congress, refused to go, for political reasons. When Motchane, who had

[114] Lettre de Léon Motchane à Alexander Grothendieck (24/11/69). Arch. IHÉS.
[115] Lettre de Alexander Grothendieck à Léon Motchane (3/10/60). Arch. IHÉS.
[116] Lettre de Alexander Grothendieck à Léon Motchane (22/1/62). Arch. IHÉS.

accepted the award in Grothendieck's name, delayed his handing-in of the award, in order to prepare a ceremony, Grothendieck took the delay as an affront.[117] The relationship between Grothendieck and Motchane periodically passed through several tense episodes over the years of their collaboration. In 1967, upon learning, one month after the fact that a secretary had quit, Grothendieck accused Motchane: "you seem to adopt an attitude of sabotage regarding my work."[118] Several years of laudatory *Rapports scientifiques* by Motchane however can only bear witness to the fact that he was extremely proud of the work of his professor. That he was among the greatest mathematicians of his generation was recognized by everyone.

But starting in 1967, Grothendieck became involved in political matters. In November, responding to an invitation of the Mathematical Society of Vietnam he himself had solicited, Grothendieck went to Hanoi, where saw the ravages of the war and experienced bomb alerts, during one of which a mathematics teacher was killed. Upon coming back, he gave an exposé of the mathematical life in Vietnam at the Sorbonne. Detailing, with much sympathy and some naiveté, the extreme difficulties of their daily life, he testified to the Vietnamese mathematicians' courageous efforts to pursue their

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117 Lettres de Alexander Grothendieck à I. G. Petrovskii (28/2/66); de Alexander Grothendieck à Léon Motchane (5/7/1966); and the following exchange of letters in August 1966. Arch. IHÉS.

118 Lettre de Alexander Grothendieck à Léon Motchane (18/1/67). Arch. IHÉS. A good example of what can be termed his paranoia is displayed in A. Grothendieck, *Récoltes et semailles. Réflexions et témoignage sur un passé de mathématicien,* 7 vols. (Montpellier: Université du Languedoc and CNRS, 1985). Manuscript, Fine Arch.

mathematical activity. He called for the organization of a support movement, which was to have some concrete effects.[119]

After May 1968, the mathematicians of the University of Orsay set up a committee in charge of evaluating the education and recruiting of mathematics professors, in view of addressing the "fundamental vice of the present system."[120] As a result, Grothendieck wrote a widely circulated report, which he asked *Le Monde* to publish. This highly elitist text was inspired by the Bourbakis' notion of a resonance box [*caisse de résonance*]: those "second-rank researchers" who according to Weil, "play a smaller role in [mathematics] than elsewhere, the role of a resonance box for a sound that they do not contribute to make."[121] Earlier in 1968, Dieudonné had acknowledged that "resonance boxes" might also play a role in mathematics below the 150 "great mathematicians" of the century.[122] A "Questionnaire sur la recherche" circulated by Orsay mathematicians at the end of 1967 expressed a similar point of view:

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[119] A. Grothendieck, "La vie mathématique en République démocratique du Vietnam," typed manuscript, 22 pp.; lettre de Alexander Grothendieck à Léon Motchane (29/9/67). Arch. IHÉS. In particular, it led to a trip of Bernard Malgrange and Alain Chenciner to Vietnam, in October 1974, see A. Chenciner, B. Malgrange, Lê Dũng Tráng, and F. Pham, "Les mathématiques en République démocratique du Viet Nam," *Gazette des mathématiciens,* 3 (February 1975): 26-31. For the memoirs of another activist mathematician who went to Vietnam in 1968, see L. Schwartz, *Un mathématicien aux prises avec le siècle* (Paris: Odile Jacob, 1997), chap. 11, and esp. 454-462.
[120] Lettre de Alexander Grothendieck à Hubert Beuve-Méry, directeur du *Monde* (4/6/68). A. Grothendieck, "Le maître-enseignant et le maître-chercheur dans l'Université d'aujourd'hui et de demain," typed manuscript (June 1968).Arch. IHÉS.
[121] A. Weil, "The Future of Mathematics" (transl. Arnold Dresden), in *Great Currents,* 321-336, 333; "Le futur des mathématiques," in *Grands courants,* 317-318. My translation.
[122] For his point of view, see J. Dieudonné, "Orientation générale des mathématiques pures en 1973," *Gazette des mathématiciens,* 2 (October 1974): 73-79; and J. Dieudonné, "Lettre à Marcel Berger (17/4/80)," *Gazette des mathématiciens,* 14 (July 1980): 138.

> Are useless and even harmful all the papers whose only aim is to prove the ability of the authors to solve certain exercises, or to provide these so-called researchers with positions in teaching or research.[123]

Inspired by the IHÉS's ideology, Grothendieck voiced the opinion that two akin, but distinct, roles were confused in the present situation: the mathematician had "to transmit already acquired knowledge," while at the same remaining the "intellectual creator contributing to the deepening, widening, and renewal of this very knowledge." In spite of the present confusion, the system only worked because the number of students was small enough. With the coming of mass education, the situation was changing. He believed that some professors should devote their energy to the education of future "users of mathematics" (scientists, engineers, businesspersons), rather than future mathematicians.[124] Grothendieck however loathe this type of teaching which, in his opinion, should not be imposed to the great mathematicians.

> As a general rule, except for rare exceptions, the greatest the worth of a researcher, . . . the greatest will be the effort he will have to make in order to be snatched away from [his] problems, and the difficulty he will feel to put himself into the different state of mind which the training of students who do not care at all for this science as such, demands.

Grothendieck envisioned a system where "great mathematicians" would be given a freedom comparable to the one he enjoyed at the IHÉS. The person who, "once his thesis completed, which will have demanded from him (supposing himself or his thesis advisor took seriously the traditional criteria of the worth for a thesis) a very considerable

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[123] For a summary of the above positions and comments about these issues, see P. Samuel, "Buts d'un mathématiciens," *Gazette des mathématiciens,* G2(5) (June 1970): 39-46. Quote on p. 40.
[124] Motchane also was concerned with the problem of adapting universities to mass education, lettre de E. C. Zeeman à Léon Motchane (19/8/68). Arch. IHÉS.

effort, unique in his life," the person who unfortunately was not a "great mathematician," should be charged to train mere users of mathematics.

In this text, Grothendieck wrote a surprising sentence which, taken seriously, could soon have been used to disqualify him as a serious mathematician.

> The most characteristic test to distinguish the mathematical re searcher from any species of individuals, is that when he meets one of his colleagues, . . . he starts right away to discuss neither politics, the weather, his colleagues, nor his boss, . . . but rather of mathematical problems.[125]

René Thom, who quite early stopped considering himself as "true" mathematician once voiced a similar opinion. "If one truly is a mathematician in the soul, one is not very much concerned with philosophy; and if this happens, it is a sort of derailment. I remember what happened to Grothendieck."[126] Real mathematicians could not be concerned with anything other than mathematics.

During the summer of 1970, Grothendieck engaged in an iconoclastic political crusade for raising consciousness about the responsibility of scientists. This evolution, of course, hardly was independent from the conflicts that opposed him to Motchane at the IHÉS. The most surprising, however, is that Grothendieck's metamorphosis was also due to the development of Thom's ideas. Indeed, on November 14, 1969, the day before he wrote his infuriated letter to Motchane about the military credits, Grothendieck sent a request the secretary for ordering a dozen biology books. "One of the most terrible attackers [*pourfendeurs*] of applied mathematics," Grothendieck was getting interested in

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125 A. Grothendieck, "Le maître-enseignant et le maître-chercheur."
126 R. Thom, *Prédire n'est pas expliquer,* 14-15.

biology.[127] This was an immediate consequence of the activity that was taking place around Thom.

In a long letter he sent to Motchane on January 26, 1970, Grothendieck explained what had happened.

> [W]hile in the past the relations on a scientific level among the diverse permanent [professors] of the IHÉS, between physicists and mathematicians as much as between Thom and me, were practically nil, this situation has been changing for the last few months. As it was, the 'catalyzers' have been Zeeman and [Mircea] Dumitrecu (a Romanian biologist, friend of [Valentin] Poenaru). Zeeman is making great efforts to popularize Thom's ideas on universal mathematical models (the "catastrophes") in the natural sciences, and he convinced me of the importance of these ideas and of the necessity to assimilate them.

Alexander Grothendieck later fondly remembered the ambiance of "scientific incubator [*étuve*]" which reigned at the IHÉS in 1969-1970.[128] Dumitrescu's seminar on molecular biology were sometimes attended by Ruelle and Thom, while the latter explained his geometric ideas on morphology to Dumitrescu and his audience. Grothendieck saw vast new fields of research opening to him.

> I am seriously thinking of taking advantage of the exceptional work conditions I enjoy [at the IHÉS] in order to devote a few years to acquire some basic knowledge in the natural sciences, notably in physics and biology, with the hope of later contributing to an interdisciplinary attack of certain problems, *presently all too much subjected to the sole specialists.*[129]

Grothendieck's attitude hardly pleased Motchane and contributed to the escalation of the conflict that opposed the two of them. Meanwhile, however, they had been able to reach some kind of compromise about the affair of the military credits.

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127 Jacques Baron, "Au premier rang mondial, l'École mathématique française," *Entreprise,* nos. 726/727 (9-16 août 1969), 24-33, 25.

128 A. Grothendieck, *Récoltes et semailles,* 3, 170, n.42.

129 Lettre de Alexander Grothendieck à Léon Motchane (26/1/70). Arch. IHÉS. My emphasis.

(ii) Military Credits at the IHÉS

In his reply to Grothendieck's ultimatum, on November 24, 1969, Motchane had done little to bridge the rift. He explained how himself had cleared his conscience. Grothendieck's analysis, Motchane judged, was superficial. It was common knowledge that military funding were distributed over civil budgets. To forgo all credits coming from specifically military agencies, he thought, was a Pharisaic attitude. He had adopted a solution that controlled at the level of the Institute the use made of the funds, always insisting on total scientific independence. Motchane was nonetheless ready to discuss these matters with Grothendieck, and a few days later, sent him the few documents he had demanded.[130]

Receiving Motchane's first letter, Grothendieck reiterated his accusations: "If I stayed at the IHÉS while a portion of its budget came from a [military] source, it is solely because you thought best to decide to leave me ignorant of this fact, and that I committed the unforgivable fault of trusting you on his point." He wrote that having failed to get the assurances he had asked for, he was preparing his leave from the IHÉS, albeit keeping a door open for discussion.[131]

In view of the imminent inflow of foreign money, Motchane now thought Grothendieck's concerns a "purely academic" question. While calling for a frank discussion, he insisted on the fact that the professors needed not be involved in administrative matters.

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[130] Lettres de Léon Motchane à Alexander Grothendieck (24/11/69); and (28/11/69). Arch. IHÉS.

[131] Lettre de Alexander Grothendieck à Léon Motchane (1/12/69). Arch. IHÉS.

> As you know, the absolute, without precedent, scientific independence enjoyed by the Scientific Committee is counterbalanced by its total non-interference in the administrative domains.[132]

On December 16, 1969, Grothendieck once again escalated the conflict. "I immediately stop [my] activity. In the coming days, I will come to vacate my office at the IHÉS." He did not resign, but rather asked for a leave of absence. And he insisted on attending the next meetings of the CS to discuss these issues.[133]

The same week, Motchane granted his leave to Grothendieck, but only if some conditions were respected by him. In particular, he insisted on Grothendieck's having to attend the annual spring meeting of the CS, a condition never imposed previously. Grothendieck was outraged. While saying that his intent indeed was to participate to this meeting where important issues would be raised, he stated that he had "no intention to set foot again at the IHÉS, as long as it would be supported by funds of a military origin."[134] This was a dangerous sentence to write. As Motchane replied,

> this would constitute a unilateral rupture without advance notice of contract with the Institute, whose sole immediate consequence would be pure and simple exclusion.[135]

Over the Christmas break, Motchane had however come up with an appropriate response to his professors' worries. Meanwhile, Grothendieck had obtained from his colleagues to write a joint letter to Motchane expressing their concerns regarding

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[132] Lettre de Léon Motchane à Alexander Grothendieck (12/12/69). Arch. IHÉS.
[133] Lettre de Alexander Grothendieck à Léon Motchane (16/12/69). Arch. IHÉS.
[134] Lettres de Léon Motchane à Alexander Grothendieck (19/12/69); de Alexander Grothendieck à Léon Motchane (20/12/69). Arch. IHÉS.
[135] Lettre de Léon Motchane à Alexander Grothendieck (9/1/70). Arch. IHÉS.

Grothendieck's imminent leave, and demanding that the IHÉS renounce any type of military funding. In substance, Motchane agreed to this request.[136]

He explained that the majority of the so-called military funding that the Institute had received since 1964 had been coming from the discretionary funds of the Minister of Armies, M. Pierre Messmer. This amount of 500,000 F over several years (1965 to 1967) could not, properly speaking, be labeled as military, since the Minister himself was a civil. The Minister had also contributed around 100,000 F in 1968 and 1969. These amounts represented from 13% to 4,35% of its budget. Motchane did not foresee that this exceptional help would be renewed for 1970 and after. Papers preserved at the IHÉS show that, albeit vague, Motchane's affirmations were correct.

But Motchane twisted the truth when addressing the more delicate case of the *Direction des recherches et moyens d'études* (DRME), the organism in charge of scientific research in view of military applications. Those were "the only truly military funds that the IHÉS has ever received." In 1966, indeed, the DRME had provided the IHÉS with 20,000 F, a mere 1% of its budget that year. He wrote his professors that he had had to provide a report, a requirement he found unacceptable.

What Motchane did not say, and about which he in fact lied, was that the main problems as for continuing this contract and obtaining another one from the DRME had not come from him and the IHÉS, but rather from the military agency itself. In 1964, upon arriving to the IHÉS, Ruelle had indeed written an extensive proposal for a contract to the amount of 350,000 F over two years. Finally, despite the favorable opinion of

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136 Lettres de René Thom, David Ruelle, Louis Michel et Alexander Grothendieck à Léon Motchane (19/12/69); de Léon Motchane à René Thom, David Ruelle, Louis Michel et

Pierre Aigrain, then scientific advisor to the DRME, this contract was rejected, because not fitting its ordinary framework.[137] The other contract, which was accepted, concerned research seminars in mathematics, including Thom's seminar with Malgrange and Grothendieck's. Strangely, as opposed to Thom, Grothendieck apparently was not asked by Motchane to provide a description. Motchane, on the other hand, was obligated to ask for the DRME's permission to publish its proceedings, including Grothendieck's famous *Séminaire de géométrie algébique* XIX to XXV.[138]

Neither was Motchane truthful when he insured his faculty members that "[f]aithful to the spirit that presided over the foundation of the IHÉS, . . . we never neither solicited contributions from military agencies, nor accepted those from politico-military organizations, such as NATO."[139] Indeed, while the possibility of looking for NATO support was raised at the foundation session, but apparently not pursued, the above shows that Motchane's efforts at getting DRME support were important in the dire years of 1962-1964.[140] Moreover, on several occasions throughout the late 1960s, Motchane,

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Alexander Grothendieck (8/1/70). Arch. IHÉS.

137 Lettres de Léon Motchane à Lucien Malavard (17/11/64); de Léon Motchane à Pierre Aigrain (8/12/64); de Annie Rolland à Léon Motchane (19/1/65). Arch. IHÉS. Cf. also *Note manuscrite* de Annie Rolland suite à un coup de téléphone de André Grandpierre (12/1/65), et lettre du Général Lavaud au Général René Cogny (15/5/62): La DRME "s'interdit toute action qui pourrait l'assimiler à un mécénat."

138 Lettre de Léon Motchane à Jacques Dubois (21/6/65). Lettre de Léon Motchane à Lucien Malavard (23/11/64); contract with DRME n° 167/65 approved (2/4/65) for 20,000 F. Notification d'une commande de travaux sur mémoire, DRME: "Organisation de séminaires de physique théorique et de mathématiques" (25/5/65). Arch. IHÉS.

139 Lettre de Léon Motchane à René Thom, David Ruelle, Louis Michel et Alexander Grothendieck (8/1/70). Arch. IHÉS.

140 Lettre du Général René Cogny, Commandant en Chef en Afrique Centrale, à François Le Lionnais (15/5/62). Arch. IHÉS.

helped by François Le Lionnais, attempted to transform the exceptional support of the Minister of Armies into a permanent subvention.[141]

On January 8, 1970, with the promise that foreign subventions should come in the near future, Motchane gave his "moral assurance" to the permanent faculty members that the IHÉS would not receive, nor solicit funds from "military or pseudo-military" sources. This was the most he could do, and it satisfied Grothendieck and the others.[142] The incident was closed.

(iii) Jalousie? Better a Good Divorce than a Bad Union[143]

During the spring term, 1970, the Minister of Armies however informed the IHÉS that he was granting another subvention of 80,000 F. The finances of the IHÉS remaining stretched, the administrators accepted the subvention, even if it hardly represented more than 3,5% of its budget.[144] On March 5, in order not to cut their invitation budget, while allowing Grothendieck to stay, the physics professors, Michel and Ruelle, asked that these funds be exclusively devoted to their Section.[145] President André Grandpierre of the

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[141] E.g. Lettre de André Grandpierre à Pierre Messmer (3/3/66); *Notes sur les rapports de l'IHÉS avec le Ministère des Armées and the DRME* (9/3/66); de Henri Domerg à Léon Motchane (18/4/67); de Léon Motchane à Jacques Ballet (12/1/68); de Jacques Ballet au Général Fourquet, délégué général à l'Armement (22/2/68): "Expriment le souhait que l'aide du Ministère des Armées devienne permanente et prenne la forme d'une subvention annuelle." *Note sur les relations de l'IHÉS avec le Ministère des Armées* (14/5/68). Arch. IHÉS.

[142] Lettres de Alexander Grothendieck à Léon Motchane (13/1/70); de Alexander Grothendieck, Louis Michel, David Ruelle et René Thom à Léon Motchane (16/1/70). Arch. IHÉS.

[143] Lettre de Jacques et Marie-Jeanne Tits à Annie Rolland (13/6/70). They wrote: "un bon divorce vaut mieux qu'un mauvais ménage." Arch. IHÉS.

[144] *Notes manuscrites d'Annie Rolland, Assemblée générale* (15/12/70), where president Jacques Ballet said: "Toujours sur la corde raide." Arch. IHÉS.

[145] Lettre de Louis Michel et David Ruelle à André Grandpierre (3/5/70). Arch. IHÉS.

IHÉS replied that this solution was not administratively possible, but by informal agreement, they could be attributed only to physicists.[146] Failing to gather the support of his colleagues to oppose in block the acceptance of this subvention, judging the IHÉS's compromise as being "incompatible with the responsibility of a scientist [*savant*] vis-à-vis society," Grothendieck resigned from the IHÉS on May 25, 1970.[147]

But the affair of the military credits might have only been a façade. Personal conflicts between Grothendieck and Motchane soon resumed after the January arrangement. Moreover, Grothendieck's avowed interests, as well as his activities, had perhaps started diverging from what Motchane had in mind for him. This incident reveals some other aspects of the atmosphere reigning at the IHÉS in 1970, which helped shape the modeling practices being promoted at the same time by Thom and his clique.

As early as January 26, Grothendieck wrote a long letter to Motchane in order to make his position clear for the future. There, as described above, he talked about his new interest for the natural sciences. But mostly, he reproached Motchane of not having involved the faculty enough in the administration of the IHÉS, especially with regards to the choice of the new director and the change of status linked with the Europeanization of the IHÉS. In unmistakable terms, he also accused him of incompetence, "due to the overwork and nervous fatigue [*usure*]" of the twelve tears he spent as the head of the IHÉS. He was showing him the door. "Since in a few years at the latest, we will have to

[146] Lettre de André Grandpierre à David Ruelle (4/5/70). Arch. IHÉS.
[147] Lettres de Alexander Grothendieck aux membres du Comité scientifique (25/5/70); de Alexander Grothendieck à Léon Motchane (9/6/1970). Arch. IHÉS.

leave to others the task of pursuing this enterprise (If it survives until then), is it not better that you accept it right now?"[148]

Indeed, other problems between Motchane and the faculty, already brought up during the previous semester, had started to occupy the foreground. On January 18, the four permanent professors, without Deligne just officially hired, again addressed a joint letter to the director. It concerned the change of status of the IHÉS and the choice of its next director. The professors asked for the establishment of formal procedures for the CS, so as to keep a consultative voice in these negotiations. Again, the atmosphere was rapidly deteriorating.[149]

"We always lose something when a private association is transformed into an organization whose patrons are States."[150] The professors were endeared to the bylaws Motchane himself had written for the IHÉS, and the prospect of important changes scared them. For Motchane, this was a "true wind of craziness [which] started to blow among the permanent [professors]."[151] But soon, he felt that the cohesion among the professors was not so strong as he feared.

> The "united front" of the permanent [professors] seems shaken up. I saw MICHEL this morning, who does not blindly walk with GROTHENDIECK anymore. THOM of course remains correct and normal.[152]

On November 30, 1969, E. C. Zeeman, answering a verbal offer by Motchane, had written that, for the time being, he did not want to be considered for directorship, but that

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[148] Lettre de Alexander Grothendieck à Léon Motchane (26/1/70). Arch. IHÉS.
[149] Lettre de Alexander Grothendieck, Louis Michel, David Ruelle et René Thom à Léon Motchane (18/1/70); de Léon Motchane à René Thom (28/1/70). Arch. IHÉS.
[150] Lettre de Léon Motchane à Victor Weisskopf (5/2/70). Arch. IHÉS.
[151] Lettre de Léon Motchane à André Grandpierre (13/2/70). Arch. IHÉS.
[152] Lettre de Léon Motchane à André Grandpierre (18/2/70). Arch. IHÉS.

he might reconsider his decision at the end of Motchane's mandate in 1974.[153] In February, as a way to insure the future of the IHÉS, Motchane intensified the search for his successor. His preference went to Res Jost, while Zeeman remained his second choice.[154] But the former apparently was not interested.

On April 6, 1970, at 10 o'clock, a fateful meeting of the CS took place, attended by the five permanent professors, Motchane, Montel, and British physicist Rudolph Peierls.[155] A motion was unanimously voted in favor of formally asking Zeeman to become the next director of the Institute. Motchane then explained how negotiation with foreign governments were going. Invitations for the next year were discussed.

Suddenly, Grothendieck mentioned his desire of inviting molecular biologist Mircea Dumitrescu for the whole year. He had already given a seminar at the IHÉS, underscoring, Grothendieck thought, the "positive character of the emergence of common interests among permanent [professors]," upon which other professors more or less protested. Grothendieck had solicited the opinion of eminent biologists in favor of Dumitrecu.[156] Thom's opinion however was not as favorable. The biologist possessed

[153] Lettre de E. C. Zeeman à Léon Motchane (30/11/69). Arch. IHÉS.
[154] Lettres de Léon Motchane à Rudolph Peierls (5/2/70); de Léon Motchane à Victor Weisskopf (5/2/70). Arch. IHÉS.
[155] This memorable session of the CS was reconstituted starting from the *notes de séances manuscrites de Annie Rolland*; and *Procès-verbal du Comité Scientifique du 6 avril 1970* (dated 29/5/70), which however differ on some points. Also *Lettre* de Léon Motchane à E. C. Zeeman (22/4/70). Arch. IHÉS.
[156] Lettres de Alexander Grothendieck à E. Wollman (12/3/70); de E. Wollman à Alexander Grothendieck (17/3/70); de Alexander Grothendieck au Dr. C. Ropartz (26/3/70); de C. Ropartz à Alexander Grothendieck (2/4/70). Dumitrescu's doctoral thesis in medical sciences, *Genetic Mechanisms of Evolution and Analysis of the Consistency of Some Usual Biological Concepts*, prepared while he was at the IHÉS, was defended at Bucarest on June 17, 1971. See *Rapport sur les activités scientifiques à l'IHÉS en 1972—*

"very original [and] advanced conceptions, some of which are perhaps valuable, but [he was] too speculative for an experimenter and too experimental for a speculative." But Thom did not mind his coming to the IHÉS. Motchane however seemed opposed to it. His reasons were not exactly clear. Perhaps did he not think that Grothendieck should devote so much energy to biology; more probably, as he said during the meeting, did he feel uncomfortable with inviting a biologist when this clearly was not the role of the IHÉS. True, Waddington had been invited by Thom to spend three days at Bures in 1966, but the present proposal had a totally different scale.

Since the handwritten minutes taken by Annie Rolland and the proceedings established by Motchane differ at this point, what happened next is not totally clear. In any case, Ruelle suggested that, since the Institute was supposed by its bylaws to promote research in "all discipline connected with" its main fields of inquiry, Dumitrescu could be invited to organize a "pluri-disciplinary seminar on the relations between mathematics and physics." According to the minutes, Motchane agreed to this formula, upon which Grothendieck interjected that he had always considered him an "arrant liar [*un fieffé menteur*]." Motchane was enraged. Grothendieck said: "If you were looking for a grave incident, you got it!" Following this, Rolland simply wrote "Strife [*Bagarre*]."

Three days later, at 9:35, the CS went on discussing modification to the bylaws. "M. Grothendieck left the session at 10:05. He had been reading Bourbaki for the whole

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Ouvrages de mathématiques et de physique élaborés à l'IHÉS (February 1973). Arch. IHÉS.

time."[157] The relations between him and Motchane had suffered beyond repair. From then on, Motchane would work on ways to get rid of Grothendieck.

On April 14, Peierls wrote that he thought Motchane had not appreciated "the strength of the feeling amongst the permanent members of the Institute, and their near-unanimity in spite of the divergence in their view on some matters." Peierls did not see that there was enough ground to dismiss Grothendieck. True his letter stated vague intentions of devoting himself to subjects other than mathematics. True his attitude at the CS had been reprehensible. But this was no ground for firing him.

On April 22, Motchane exposed Grothendieck's case to the Administrative Board of the IHÉS. I have not found the proceedings, nor the minutes of this meeting in the archives, but I hit upon non-dated handwritten notes prepared by Motchane for an exposé which I suspect took place on this occasion. For Motchane, after the crisis linked with military credits, there was a "violent action of G trying to take control, to kick me out."[158] Apparently, a *motus vivendi* was obtained, Motchane declared, but a desire remained "to install an *oligarchy*." In his view, Grothendieck's departure had become unavoidable, since Thom and Michel would otherwise leave. Motchane believed that this crisis hid something deeper. Grothendieck's crazy ideas [*lubies*] were dangerous. For him, Thom's

[157] *Notes de séances manuscrites du Comité Scientifique de Annie Rolland* (9/4/70). Arch. IHÉS.

[158] *Notes manucrites de Léon Motchane* (n.d.). Here is the complete text: "Nov. 69 = crise des crédits militaires. Solution? Position de l'IHÉS. Crise liée aux transformations. Pourparlers avec les allemands, lié avec ma retraite: action violente de G essayant prendre le dessus, me mettre dehors, joue un rôle[?] de dictateur, opposition de G et [?]... En apparence un motus vivendi est obtenu mais le désir d'installer une oligarchie. Quelques profs sont venus me trouver pour dire que c'est imparable = le départ de G devient nécessaire. Raison[?] Thom et M. partiront[?]. Cette crise cache quelque chose de plus

success in biology had "engendered a humility" from Grothendieck's part. Obviously, he, or Motchane, had to leave. It was in this context that the administrators accepted the subvention from the Minister of Armies.[159] Since not anymore devoting his energy to mathematics, Grothendieck was dispensable.

Besides the personal reasons that Grothendieck might have had, the attraction of Thom's ideas, and the activities going on around him, thus were powerful enough to contribute greatly to his quitting not only the IHÉS, to which he had contributed so much, but also mathematical research. Was Grothendieck envious of Thom? The count of their approximate number of lines in the *Science Citation Analysis,* as I found in the archives of the IHÉS, would hardly substantiate this claim. Above everyone else, Grothendieck had 520 lines, while Thom and Ruelle, respectively, only had 95 and 119.

The best analysis of the crises that shook the IHÉS in 1969-1970 may well have been that provided by Peierls. Having just married his daughter, he wrote that Motchane found himself in "the familiar situation of the parent who is taken by surprise by the fact that his children have grown up and has to adjust himself to this new situation."[160] The Institute had acquired a life of its own. Motchane could leave it the next year, to be replaced by Nicolaas Kuiper, a Dutch mathematician specializing in fields close to

profond = Ces lubies peuvent être dangereuses = Thom biologie = a engendré une humilité[?]."

159 Communication de Léon Motchane (15/5/70). Arch. IHÉS.

160 Lettre de Rudolph Peierls à Léon Motchane (12/4/70). Arch. IHÉS.

Thom's. He was chosen only after Christopher Zeeman and André Maréchal both declined.[161]

As for Grothendieck, a new period in his life began during which he got involved in anti-war, then ecological, activism. In June 1970, he founded at Montréal the movement *Survivre,* (later, *Survivre et vivre*), which counted, besides Grothendieck's own highschool nephew, among its French members and fellow travelers Claude Chevalley, Pierre Samuel, Denis Guedj, Daniel Sibony, all of whom would play prominent roles in debates about the social role of mathematics during the next decade.[162] Attempting to use the International Congress of Mathematicians, held in Nice in August 1970, to make mathematicians more aware of their responsibility vis-à-vis society, Grothendieck only succeeded in infuriating his old colleague Dieudonné.[163] This marked the sad end of a very successful mathematical career.

c) The Birth of Catastrophe Theory

Just as reminder that the political situation was not tense only for IHÉS scientists, let me quote from a letter sent by Oscar E. Lanford on May 7, 1970. At Berkeley, he also found

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[161] Lettres de E. C. Zeeman à Léon Motchane (30/6/70); de E. C. Zeeman à André Grandpierre (30/6/70); de E. C. Zeeman à Annie Rolland (30/6/70). About Maréchal, lettre de David Ruelle à Léon Motchane (12/11/70). Arch. IHÉS.

[162] A. Grothendieck, *Récoltes et semailles,* tome 3, n.6, p.143. A. Grothendieck, "Responsabilité du savant dans le monde d'aujourd'hui: le savant et l'appareil militaire," typed manuscript, 63 pp. Arch. IHÉS. See e.g. D. Sibony, "À propos des mathématiques modernes," *Tel quel,* 51 (1972): 87-103; repr. *Pourquoi la mathématique?,* ed. R. Jaulin, Robert (Paris: UGE, 1974): 100-130; and P. Samuel, *Séminaire "Mathématiques, mathématiciens et société."* Publication mathématiques d'Orsay n° 86-74.16 (1974). Jussieu Lib.

[163] A. Grothendieck, "Reportage: Le Congrès international des mathématiciens de Nice (1-10 septembre 1970)," typed manuscript., 7 pp. Arch. IHÉS.

himself in the midst of a political crisis. The Governor has just closed the University of California for four days "to preempt Faculty-students strike against Nixon's adventure in Cambodia." Lanford acknowledged that: "Evidently, no one finds much energy to spend on scientific work."[164]

Apparently, despite all the strife, this was not so much the case at the IHÉS. In 1969-1970, the Institute witnessed an unprecedented convergence of dynamicists. The following years would see these relations being sustained. In particular, Smale and his students may sometimes have found the atmosphere of Bures-sur-Yvette more conducive to work than Berkeley's. It was while he was at Bures that Smale wrote one of his first papers, titled "Topology and Mechanics," which did not deal with purely mathematical themes.[165] A modeling practice inspired by Thom's was attracting the best representative of dynamical systems theory. Important contributors would be, among others, Ralph Abraham, Jerrold Marsden, Christopher Zeeman, David Ruelle, and René Thom himself.

In 1971, Motchane proudly stated that "the IHÉS is one the very rare places [in the world] where physicists and mathematicians successfully interact."[166] Throughout the 1970s, sometimes under Thom's impulsion, and more and more under Ruelle's, the IHÉS remained one of the main research centers where dynamical systems theory, and of course catastrophe theory, were pursued always with an eye intensely focused on applications, not only in physics, but also in biology, linguistics, economics, and psychology. Taking

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[164] Lettre de Oscar E. Lanford, III, à David Ruelle (7/5/70). Arch. IHÉS. In October, Ruelle witnessed that the "political atmosphere was heavy" in California: Lettre de David Ruelle, from Irvine, à Léon Motchane (14/10/68). Arch. IHÉS.

[165] *Rapport Scientifique, Année 1970 - Travaux de mathématiques et de physique théorique,* 1. Arch. IHÉS. It was published in *Inventiones Mathematica,* 10 (1970): 305-331; 11 (1970): 45-64.

up his directorship on October 1, 1971, Nicolaas Kuiper wrote: "M. Thom keeps on attracting the attention of young mathematicians and others on many open problems and he is an 'attractor' and a source of inspiration for many visitors." He greatly inspired Zeeman's and Abraham's applied work. At the same time,

> the work of M. Ruelle is of mathematical and physical interest and contributes to the coherence of the scientific activities of the IHÉS, in particular between physics and mathematics, an exceptional thing in the world."[167]

Throughout these years, René Thom traveled the world "to spread the gospel," giving talks on a wide variety of topics (mathematics, biology, linguistics, and physics) to a wide variety of audiences.[168]

In 1970-1971, Thom devoted many sessions of his seminar to his "Mathematical Models of Morphogenesis," writing up the first chapters of his second book first published as a pocket book in 1974. In the following years, Thom held a Monday seminar whose designation varied from applied global analysis to structural stability and bifurcation theory, or qualitative dynamics. In particular, in February and March, 1972, Thom's seminar welcomed several talks given by Abraham, Marsden and Ruelle, on hydrodynamics, Smale's econometric theories, and bifurcation theory.[169]

But despite all his attraction power, Thom hardly seem to have been interested in building a true research school. Too much of a dreamer and moving away from pure mathematics, he lacked the necessary administrative skills. True, a young French

[166] *Note pour le rapport scientifique 1970* (2/6/71) Arch. IHÉS.
[167] *Note pour le rapport scientifique 1971* (18/5/72). Arch. IHÉS.
[168] "je m'arrêterai à Buffalo (Center for Theoretical Biology), et à Chicago, pour y répandre la bonne parole sur la Morphogenèse." Lettre de René Thom à Léon Motchane (21/6/67). Arch. IHÉS.
[169] *Rapports scientifiques* (1971 to 1973). Arch. IHÉS.

mathematician working on his Ph. D. thesis, Alain Chenciner, was especially attracted by Thom's ideas in the early 1970s. Thom invited him to speak at his seminar, and was on his thesis committee.[170] Unfortunately, Chenciner remained an exception, and Thom was not able to build at Bures anything approaching Smale's Berkeley global analysis group of the late 1960s.

On September 25-28, 1972, a "catastrophic seminar" was held at Bures-sur-Yvette, celebrating "Catastrophe Theory and its Applications."[171] Coinciding with the publication of *SSM,* which had been awaited for so long, this symposium may well be considered as the official birth of catastrophe theory as such. Besides talks by Abraham, Smale, Takens, Zeeman, and a series by Thom, it also included talks by French mathematicians, B. Teissier (a frequent speaker in Thom's seminars), D. Thillaud, and F. Pham, which may serve to underscore the attraction of Thom's ideas, but mainly the difficulties of integrating them within the dominant Bourbakist mold. In 1973, Thom recruited another man from the French quarters in the person of Ivar Ekeland, who would play an important role in the diffusion of catastrophe theory in France.

In 1974, in numerous lectures, Thom addressed issues related to qualitative dynamics, differential topology, catastrophes and the human sciences, language and catastrophes, and theoretical biology. That year, other people who would be quite vocal in

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170 A. Chenciner gave talks were "Catastrophes élémentaires de Corang deux: Ombilics" (25/1/71), "L'ombilic parabolique" (1/2/71). His thesis, *Sur la géométrie des strates de petites codimensions de l'espace des fonctions différentiables réelles sur une variété,* doctoral thesis (Faculté des Sciences d'Orsay, Université de Paris), was defended on June 6, 1971, in front of H. Cartan (president), J. Cerf, L. Scwhartz, and R. Thom.

171 *Journées sur la théorie des catastrophes and morphogenesis: Summaries and References Concerning the Given Lectures.* Lettres de Nicolaas Kuiper à Floris Takens et

their defense of catastrophe theory for the first time spoke at Thom's seminar. In particular, one notes Claude-Paul Bruter's talk on solitary waves, and Jean Petitot's on "The Pictorial Representations of the Myth of St. George."

Two other factors besides Thom's own reticence, further hindered the establishment of a true research school in qualitative dynamics at Bures-sur-Yvette. First, not being a university, the Institute had no obligation to accept students (in fact, Thom could not head a thesis committee). Therefore, although students were always present and welcome at the IHÉS, either as auditors for seminars or, more rarely, as paid visitors, their flow seldom was important enough. Second, albeit not requiring anything close to the financial needs of an experimental facility, the constitution of a research school would at least have involved the payment of a few salaries, while throughout its existence, the IHÉS struggled to keep afloat. There was therefore always a question of how best to distribute scarce resources. When new credits started coming from different national science foundations in the early 1970s, a debate between Ruelle and Motchane underscored that a feeling, shared by Thom, existed among the faculty that too much was spent on the salary of permanent professors as opposed to that of visiting scientists.[172]

Overall, the variety of topics treated around Thom probably was too wide to avoid a feeling of spreading out. Only a mind such as Thom's might have been able to keep up with it, but only at the crucial cost of often remaining superficial. "In a sense," as Zeeman put it, "Thom was forced to invent catastrophe theory in order to provide himself with a

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autres (7/2/72); de René Thom à Nicolaas Kuiper (5/7/72 et 31/2/72) where the "séminaire catastrophique" is mentioned. Arch. IHÉS.

canvas large enough to display the diversity of his interest."[173] With the lack of a clear focus, Thom's school might have been bound to remain an exciting maelstrom with no actual impact on the practice of mathematically modeling natural phenomena. In short, to use a word dear to Motchane's ideology of fundamental research, Thom needed "interpreters" in different disciplines for adapting his ideas, for producing concrete modeling practices. The IHÉS, especially with Ruelle's impulse, provided a perfect place for this to happen in physics. But, as their interests and methods diverged, the contacts between Thom and Ruelle became sparser. Catastrophe theory, as a theory of modeling practice, was born at the IHÉS, but perhaps was it only destined to die in infancy.

d) A Research School for Thom in the Methodology of the Sciences of Man?

This needed not to be so. After his retirement in 1971, Léon Motchane engaged in the new project of, at long last, setting up the Third Section of the IHÉS. Already in 1958, he had wanted his institute to include a Section dealing with the "Methodology of the Sciences of Man." The very day he left the directorship of the IHES to Kuiper, on October 1, 1971, Motchane wrote to Chen Ning Yang, professor of theoretical physics at SUNY, Stony Brook, about the Third Section. Again, we may note that he was addressing a physicist rather than a specialist in the humanities and social sciences. Believing that the time had "come to try some new experiences in the research of human sciences,"

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[172] Lettres de René Thom à Léon Motchane (12/5/70); de David Ruelle à Léon Motchane (12/4/71); de Léon Motchane à David Ruelle (3/5/71); de Louis Michel à David Ruelle (28/4/71). Arch. IHÉS.

[173] 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.

Motchane solicited Yang's advice on the directions that such experiences should take. His letter appeared somewhat jumbled, and, as far as I was able to determine, received no answer. It however shows that he now thought of this Section as one where the mathematicians' and physicists' tools, their "structures," might be used for attacking problems in biology, sociology, and linguistics. In particular, one was to look "for some hints to a structural stability. The recent work of Thom and others," he added, "gives us some hope."[174]

Motchane hoped that the activities of the different Sections, including the humanities, be formalized. In January, 1972, however, the members of the Scientific Committee expressed the opposed opinion. . On February 7, Ralph Abraham introduced Smale's theories in economics. Thom and Michel suggested that a meeting be organized on economics, and on March 3, 1972,economists Gérard Debreu and Werber Hildebrand came to the IHÉS.[175] These modest activities, in addition to the "catastrophic symposium" of the next September, were felt by Kuiper as a partial fulfillment of the IHÉS mandate in the humanities.

For Motchane, it seemed that multidisciplinarity had become essential in order to broaden the financial basis on the IHÉS. During the following summer, having been named on the Administrative Board of the Institute, he warned Kuiper:

> In the future it will not be accepted that government's money is spent on scientific work, in case that work does not contain components in the direction of human

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[174] Lettre de Léon Motchane à C. N. Yang (1/10/71). Arch. IHÉS. Original English.
[175] *Rapport scientifique, Année 1969 - Séminaires et conférences,* 5. Arch. IHÉS.

> sciences. . . . Too great specialization will not be accepted. . . . If we want to obtain a high level, this entails multidisciplinary activities.[176]

Motchane discussed the matter further with Thom and Michel, and on October 19, 1972, he wrote to Jacques Ballet, the new president after Grandpierre's death in July, that the moment had come to set the Third Section up. He suggested that Thom, who had accepted, be nominated as permanent professor in the methodology of the sciences of man. In a note written at the same moment, he explained that setting up the Third Section was becoming imperative in view of "the tendency to establish pluridisciplinarity as a general rule in Universities and research institutions and the disposition to favor budget priorities in the same direction." The work of Thom and others represented, for Motchane, "real progress" that had come in the last few years. The IHÉS should capitalize on its success.[177]

On October 6, at meeting between Motchane and Thom, a commission including them both as well as Lévi-Strauss was envisioned to study the feasibility of the Third Section.[178] For Thom, the principal difficulty was to find the right persons for this endeavor.

> It is difficult to find [in the human sciences] elements that are simultaneously *serious* and *brilliant.* One will perhaps find good, serious specialists (like Debreu in Mathematical Economics) or, with more difficulty, brilliant, but not too serious, people (like certain fashionable structuralist, not to name anyone). . . . Sooner or later, we will have to chose between the 'serious' and the 'brilliant'. Our scientific training would surely make us prefer the former to the latter. But the necessity of

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[176] *Rapport d'un entretien téléphonique Nicolaas Kuiper-Léon Motchane* (19/6/72). Arch. IHÉS.

[177] Lettre de Léon Motchane à Jacques Ballet (19/10/72); *Note de Léon Motchane* (23/10/72). Arch. IHÉS.

[178] N. Kuiper, *Note sur les activités de l'IHÉS dans d'autres domaines (Méthodologie des sciences de l'homme)* (14/11/72). Arch. IHÉS.

> impressing potential contributors might lead us temporarily to carry out the converse policy.[179]

In this case, Thom noted, a problem raised by Louis Michel would make this solution difficult to contemplate. Indeed the theoretical physicist incisively noted that the Yvette valley truly was a "desert" as far as the human sciences were concerned. How, then, could the IHÉS hope to attract a flamboyant intellectual humanist in the French tradition?

Motchane was almost alone in truly wanting to devote much effort to this matter. While Kuiper accumulated arguments working against a formalization of the IHÉS activity in the humanities, Thom surprisingly remained unreceptive to the idea. He found that "the social sciences as a science, or better what concerns their methodology, are practically non-existent." But "content with the present situation," he preferred "the silent, humble way of development."[180]

This sheds light on Thom's attitude regarding the establishment of a research school around himself. For all his projecting the image of a public intellectual, in the French sense of the term, Thom tried to remain, to use his expression, a "serious" scientist more committed to the building of abstruse theories expressed in highly technical mathematical language, if not always using its techniques, rather than to the gathering of an entourage of devoted disciples. Because of his scientific training, Thom would not commit the same mistakes as those he called "fashionable structuralists."

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[179] Lettre de René Thom à Léon Motchane (10/11/72). Arch. IHÉS.
[180] N. Kuiper, *Note sur les activités de l'IHÉS dans d'autres domaines (Méthodologie des sciences de l'homme)* (14/11/72). Arch. IHÉS. Original English.

In December, the Administrative Board of the IHÉS charged Motchane, Kuiper, and Thom of examining the question of the Third Section. The report Kuiper produced on September 26, 1973 voiced the opinion that a clearer conception of the position of the Third Section had to be found. He opposed Thom's nomination on the ground that his theories still needed "considerable work before they are ready for more general 'consumption' by scientists of other disciplines." In short, he was proposing to let die the issue. Thom agreed to give more rigor, "i.e. by not accepting any crazy people anymore," to his theoretical biology seminar. He indicated that he was looking for a permanent professor to occupy a chair in the Third Section.

> The ideal case would be to find a personality coming from the Human Sciences per se (Sociology, Ethnology, Linguistics?) but with a solid mathematical training, and the ability to apply mathematics judiciously. . . . No need to say that I have not yet found this rare bird.[181]

In this circumstance, it is not surprising that nothing was done regarding the Third Section until the early 1980s.[182] In summary, the above has shown that while Thom hardly was interested in establishing a research school either focusing on mathematical problems or applications of catastrophe theory to the human sciences, the IHÉS nonetheless served as a catalyst for the development of new modeling practices that attracted the attention of some important mathematicians, including Grothendieck and

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[181] R. Thom, *Note sur la 3e section* (27/9/73). Arch. IHÉS.

[182] Thom only became Professor of mathematics and methodology of the sciences of man in 1980. *Comité scientifique* (15/3/80) and *Conseil d'administration* (5/5/80). With the help of Jean Petitot, he then elaborated a detailed research program for the humanities. R. Thom, *rapport sur la 3e section;* and *Programme de recherche présenté par Jean Petitot dans le cadre de la 3e section* (n.d. [1980]). Arch. IHÉS. About Petitot's early involvement with Thom's catastrophe theory, see the two special issues of *Mathématiques et sciences humaines,* 15(59) (Fall 1977); and 16(64) (Winter 1978).

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.

[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

[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 $i=1,...,m$. 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.

[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

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[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.

[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.

[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

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[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

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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

[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.

[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.

CHAPTER VII: STRANGE ATTRACTORS

> I am an old man now, and when I die and go to Heaven there are two matters on which I hope for enlightenment. One is quantum electrodynamics, and the other is the turbulent motion of fluids. And about the former I am really rather optimistic.
> —Sir Horace Lamb.[1]

> Si, jetant une pierre dans une mare, vous désirez savoir ce qui se passe, il vaut infiniment mieux faire l'expérience et la filmer, que d'essayer d'en faire la théorie; les meilleurs spécialistes de l'équation de Navier-Stokes seraient incapables de vous en dire plus.
> —René Thom.[2]

> We cannot hope that the old ape in us, clever as he may be, has direct comprehension of abstract physical or mathematical questions.
> —David Ruelle.[3]

1. INTRODUCTION: A NEW ALTERNATIVE FOR THE MODELING PRACTICE OF PHYSICS

For centuries, physicists aimed at unveiling laws of nature. Marching into the steps of Sir Isaac Newton, they exploited the second law (*F=ma*) with great success. Just as

[1] At a meeting of the British Association for the Advancement of Science in London in 1932, as recalled by S. Goldstein, "Fluid Mechanics in the First Half of This Century," *Annual Review of Fluid Mechanics,* 1 (1969): 1-28, 23.

[2] "If you want to know what happens if you throw a stone into a pond, it is infinitely better to do the experiment and film it than to try to formulate a theory about it: the finest specialists in the Navier-Stokes equations would certainly be incapable of telling you more about it." R. Thom, "Une théorie dynamique de la morphogénèse," *Towards a Theoretical Biology, I: Prologomena,* ed. C. H. Waddington (Edinburgh: University of Edinburgh Press, 1968): 152-166, 154; repr. *MMM,* 13-38, 15.

Newton had uncovered the dynamical equations governing the motion of planets in heavens, physicists in the first half of the nineteenth century were able to derive from first principles mathematical relations for fluid flow. Although, except for a few simple (and simplistic) cases, it was rarely possible to exhibit exact solutions to the Navier-Stokes equations, as they came to be known, this derivation had become an inescapable part of the classical physics curriculum. The Navier-Stokes equations provided, it was believed, the foundation for any theoretical understanding of hydrodynamic phenomena. It was therefore a shock when, in 1971, two outsiders, a physicist specializing in statistical mechanics and a mathematician who studied dynamical systems, published a controversial article "On the Nature of Turbulence" claiming nothing less than a new "mechanism for the generation of turbulence," especially since the authors, as opposed to the current practice, never explicitly wrote down the Navier-Stokes equations.[4]

This chapter aims at providing an account of the changes in physical modeling which made it possible that a new model of the onset of turbulence could be proposed without its authors ever feeling the necessity of mentioning the law found a century and a half earlier by Claude Louis Navier and Sir George G. Stokes. Inspired by René Thom's ideas, conceived and written at the Institut des hautes études scientifiques in the spring of 1970 by the French physicist David Ruelle and the Dutch mathematician Floris Takens, this article is remarkable for several reasons reaching beyond its

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[3] D. Ruelle, "The Obsession of Time," *Communications in Mathematical Physics,* 85 (1982): 3-5, 5.

introduction of the famous notion of *strange attractors,* which was to have a very bright future. Above all, Ruelle and Takens's article supplies both a *symptom* and a *direct cause* for crucial changes that have been widely affecting the modeling practice of theoretical physics ever since.

Based on first principles coming from either molecular hypotheses or continuum mechanics, the partial differential equations of physics acquired, in the course of the nineteenth century, an almost ontological status.[5] A telling and much studied instance of this process, which can be seen as originating in Fourier's analysis of heat flows, is provided by the rise of the notion of a field, which ultimately subsumed the ether under an abstract set of differential equations written down by Maxwell and his followers.[6] For Maxwell and Boussinesq, the complex diversity of behaviors exhibited by solutions to partial differential equations reinforced ontological commitments to them.[7]

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[4] 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 from the abstract on p. 167.

[5] For an analysis of the dual basis for the derivation of mathematical laws based on two sets of hypotheses, molecular or continuous, see Amy Dahan Dalmedico, *Mathématisations. Augustin-Louis Cauchy et l'école française* (Paris: Albert Blanchard; Argenteuil: Éditions du Choix, 1992).

[6] See e.g. J. Z. Buchwald, *From Maxwell to Microphysics: Aspects of Ectromagnetic Theory in the Last Quarter of the Nineteenth Century* (Chicago: University of Chicago Press, 1985); B. J. Hunt, *The Maxwellians* (Ithaca: Cornell University Press, 1991). See also J. Fourier, *The Analytic Theory of Heat,* Transl. A. Freeman (Cambridge: Cambridge University Press, 1878).

[7] J. C. Maxwell, "Does the Progress of Physical Science Tend to Give any Advantage to the Opinion of Necessity (or Determinism) over that of the Contingency of Events and the Freedom of Will?" repr. *The Scientific Letters and Papers of James Clerk Maxwell,* 2, ed. P. M. Harman (Cambridge: Cambridge University Press, 1995): 814-823; V. J. Boussinesq, "Conciliation du véritable déterminisme mécanique avec l'existence de la vie et de la liberté morale" (1878); repr. *Cours de physique*

New modeling practices emerged in the late nineteenth-century with the development of statistical mechanics under Maxwell and Boltzmann's lead, which offered the hope of basing the understanding of fundamental laws of physics on the molecular hypothesis again. Quantum mechanics and the investigation of inter-molecular forces provided a further pull in this direction. Later, mainly after World War II, statistical approaches were further developed with much success. But the question of the relation between microscopic, molecular theories and macroscopic, continuous differential equations always spurred passionate debates. As far as macroscopic physics was concerned, the exploitation of fundamental laws, derived from general principles and expressed by differential equations, only partially justified by statistical and quantum mechanical considerations, remained the physicists' dominant foundation for their modeling practice.

In this context, the turbulence problem for fluid mechanics was a distressing one. If, in traditional histories of physics, the discovery of an equation has often been the culminating point, we might contend that the history of turbulence started with the equation. Indeed, only when this equation existed did turbulence become a theoretical problem. On the one hand, there was every reason to believe that the Navier-Stokes equations provided a faithful description of classical fluid flows. On the other hand, it was an experimental fact that extremely complex flows arose when the fluid was

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mathématique de la Faculté des sciences, Compléments au Tome III. Paris: Gauthier-Villars, 1922; and H. Poincaré, *cience et méthode* (Paris: Flammarion, 1908). About this, see I. Hacking, "Nineteenth-Century Cracks in the Concept of Determinism," *Journal of the History of Ideas,* 44 (1983): 455-475; and M. A. B. Deakin, "Nineteenth-Century Anticipations of the Modern Theory of Dynamical Systems," *Archive for History of Exact Science,* 39 (1988): 183-194.

submitted to intense external stress; this complexity was called turbulence. The turbulence problem lay in the relation between fundamental equations and their solutions. In his own flowery way, French hydrodynamicist Joseph Kampé de Fériet used the common metaphor of an inaccessible mountain peak to express the problem:

> On one side, on a peak covered with perpetual snow, there waves in the wind, in loneliness and silence, the flag of the Navier[-Stokes] equations; an unfathomable abyss divides this icy summit from the ground on which is pouring the incessant rain of experimental results. It is on this ground, sometimes slightly boggy because of the abundance of rain, that mathematical models are tentatively elaborated; . . . but is it not premature to brand these models as theories of turbulence?[8]

To bridge the chasm dividing the Navier-Stokes equations from feasible experiments or known solutions, was the "turbulence problem."[9] The above quote also exhibits an interesting distinction between theories and models of turbulence, a distinction that the study of Ruelle and Takens's article and its reception among fluid dynamicists will underscore.

In their heart, physicists had always recognized that fundamental laws were but the beginning of a theoretical solution to any problem. But for a long time, when unable to solve the equations explicitly, physicists had few mathematical tools which still could have enabled them to account for natural phenomena in a satisfactory

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[8] J. Kampé de Fériet, in *Mécanique de la turbulence. Colloque international du CNRS de Marseilles, 28 August - 2 September, 1961,* ed. A. Favre (Paris: Éditions du CNRS, 1962). See also Philippe Delache's 1977 cartoon reproduced in U. Frisch, *Turbulence: The Legacy of A. N. Kolmogorov* (Cambridge: Cambridge University Press, 1995), 253.

[9] F. Noether, "Das Turbulenzproblem," *Zeitschrift für andgewandte Mathematik und Mechanik,* 1 (1921): 125-138.

manner.[10] Historically, Ruelle and Takens's article signaled the reencounter of physics with qualitative mathematics. It would help to initiate a powerful alternative to the endless quest for the final law of nature. Instead, more and more physicists started to look anew into mundane phenomena, without relying too heavily on fundamental laws. These laws, they began to think, might be unreachable with certainty, but they hoped nonetheless to provide deep theoretical explanations for experimental data.

This chapter examines the conditions that enabled Takens and especially Ruelle to attack the turbulence problem with some success and to come up with new modeling practices. Their argument is summarized and contrasted with other alternative pictures for the onset of turbulence. Their special situations at the IHÉS shaped the way they saw the possibility of adapting mathematical techniques of dynamical systems theory to the study of turbulence. The earlier career of Ruelle in statistical physics is then seen as providing the ground on which this new modeling practice could grow.

In a second part, this chapter tries to place the Ruelle-Takens model within a long-term survey of the history of turbulence. This part is meant to underscore the changes in modeling practice that this model afforded. The history of the relationship between the fundamental equations of fluid mechanics and turbulence is briefly reviewed. It is then explained how a certain subdiscipline of fluid mechanics, called

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[10] A important research path that, unfortunately, I am *not* pursuing here could be called the *big science* of fluid flows. With large computers and wind tunnels, the modeling of fluid flows in concrete situation was very different in practice than anything before.

hydrodynamic stability theory, was best suited to accommodate Ruelle and Takens's approach.

Briefly, Ruelle's new alternative for physicists' modeling practice displaced the emphasis often put on specific models or fundamental laws of nature, in order directly to tackle classes of models. "Contemporary rational thinking goes through successive ontologies in name of the epistemic reality," Simon Diner once wrote.[11] Without resolving the conundrum of the nature of the relationship existing between fundamental laws and observation, this new practice made models cheap and dispensable, and rather focused on some essential topological features of observed behaviors which were assimilated to the structural, yet dynamical, characteristics of classes of models. In short, some physicists *stopped looking at specific representations of nature in order to study the consequences of the mode of representation itself.*

2. THE NATURE OF TURBULENCE: THREE ALTERNATIVES

The article published in 1971 by David Ruelle and Floris Takens "investigate[d] the *nature* of the solutions of [the Navier-Stokes equations], making only assumptions of a very general nature on [the equations]."[12] It provides an excellent probe for examining this significant shift in the modeling practice of some physicists. They introduced a method characteristic of an attitude, inspired by René Thom in particular, that would become widespread. For Ruelle and Takens, it was not so much the

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[11] S. Diner, "A Renewal of Mechanism: Toward an Instrumental Realism," *Dynamical Systems: A Renewal of Mechanism,* ed. S. Diner et al. (Singapore: World Scientific, 1986): 273-284, 281.

detailed structure of the Navier-Stokes equation that mattered, but the very fact that fluids could be described, with an amazing degree of precision, by dissipative differential equations. From this fairly general starting point, and several other technical assumptions which they did not even care to derive from the fundamental equation, Ruelle and Takens were able to redefine the nature of turbulence and "give some insight into its meaning, *without knowing [the Navier-Stokes equations] in detail.*"[13] Quite decisively, they also made *qualitative* predictions that could be tested *in vitro* or *in silico,* that is, by numerical simulations of fluid flows.

Originally rejected by a referee of the *Archive for Rational Mechanics and Analysis,* Ruelle and Takens's article had to be published in a journal of which Ruelle himself was an editor.[14] At first, according to Ruelle, the response from the physics community to these "controversial ideas on turbulence" was slow to come and cold.[15] This is not difficult to understand considering the extreme technicality of the mathematics involved in the article from the point of view of physicists, and the heresy against Landau's widely accepted model. Besides, for those who penetrated the mathematical technicalities, the crucial reliance of their argument on the concept of *generic solutions* was rather hand-wavy. As described in Chapter V, this matter was

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[12] D. Ruelle and F. Takens, "On the Nature," 168. My emphasis.
[13] D. Ruelle, "Méthodes d'analyse globale en hydrodynamique," *TSAC:* 1-56, 7.
[14] Ruelle, *Chance and chaos,* 56, 63. Interview of David Ruelle by the author (7 February 1997).
[15] Only 7 citations in the *Science Citation Index,* other than from the authors themselves, for the years 1971-1974. See Graph 8 and D. Ruelle, "Turbulent Dynamical Systems," *Proceedings of the International Congress of Mathematicians* (Warsaw, August 1983), 275; *Chance and chaos,* 66.

far from being settled in 1970. The following shows that Ruelle and Takens's use of this concept very much relied on the practices they observed around René Thom.

a) The Argument of Ruelle and Takens's Paper

In 1971, David Ruelle and Floris Takens suggested, but did not show rigorously, that when a fluid was subjected to increasing external stress, it went through a succession of bifurcations, where different modes of vibration—i.e. different frequencies—appeared.[16] So far, this merely was a rephrasing of the model proposed by Lev Landau in 1944 and, independently, by Eberhard Hopf in 1942-1948. But Ruelle and Takens went on to suggest, albeit once again without providing a rigorous demonstration, that this bifurcation sequence had to stop after the manifestation of three different modes, because a "strange attractor" appeared in a "generic" manner, and the fluid motion ceased to be quasiperiodic.[17] Strictly *aperiodic motion* was the new definition they proposed for turbulence.

One striking feature of Ruelle and Takens's article was that they did not feel the need to write down the Navier-Stokes equations explicitly. As we have seen previously, as early as the fall of 1968 Ruelle had hoped that his approach could bear on any problem concerning dissipative systems, and not only fluid motions. Their

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[16] Many notions, including the Navier-Stokes equations (see p. 509), from fluid mechanics and dynamical systems theory that are used here but may not be already clear to the reader are later introduced in the course of this chapter. They are however not needed at present.

[17] In 1978, the Ruelle-Takens scenario was deemed to arise after the appearance of only two modes; S. Newhouse, D. Ruelle, and F. Takens, "Occurrence of Strange Axiom *A* Attractors Near Quasi-Periodic Flows on T^m, $m \geq 3$," *Communications in Mathematical Physics,* 64 (1978): 35-40; repr. *TSAC,* 85-90.

omission of the Navier-Stokes equations probably was intended to underscore the fact that their argument remained independent of its precise form.

> The time evolution of a velocity field [for a fluid] is given by the Navier-Stokes equations
> $$\frac{dv}{dt} = X_\mu(v)$$
> where X_μ is a vector field over H [the space of velocity fields $v(x,t)$]. For our present purposes it is *not* necessary to specify further H or X_μ.[18]

The parameter μ represented the external stress applied on the fluid. In the case of motion through a pipe, or past an object, a unique parameter depending on the physical characteristics of the flow—the so-called Reynolds number *Re*—was the only one determining the global behavior of the motion, i.e. whether or not it was turbulent. For other situations, like convection when a fluid is heated from below, the Raleigh number *Ra* played a similar role. As is explained below, the determination of critical values of these parameters, at which the properties of the motion changed, had been, for almost a century, the subject of numerous theoretical and empirical studies.

But Ruelle and Takens were not interested in particular critical values of the parameter μ, only the general features of motion as this parameter increased. When $\mu=0$, there was no external stress; but because of friction, the fluid would always come to rest as time went to infinity. Transitory motion was not their concern. The parameter μ represented external forces, so that when it was nonzero, the fluid could, despite friction, be maintain in a state of perpetual motion. For small μ, the motion

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[18] D. Ruelle and F. Takens, "On the Nature," 168. My emphasis. Compare with the Navier-Stokes equations on p. 509.

tended towards a *stationary motion,* that is, the velocity field remained constant for all time: $v(t;\mu) \equiv v_0(\mu)$.

At a certain critical value μ_1, the flow went though a bifurcation, which Ruelle and Takens identified as a the *Hopf bifurcation.* This meant that the velocity field ceased to be independent of time, but started to oscillate at a given frequency ω_1. The flow was periodic. In phase space, this was expressed by saying that, while for stationary flows a fixed point existed which was an *attractor* for the system, when the oscillatory mode appeared, this point 'exploded' into a closed curve.

At a further critical value of the parameter μ, a second bifurcation occurred which gave rise to a second frequency ω_2 of oscillation, and so on. This quasiperiodic behavior was the picture conjectured by Landau and Hopf. When the value of the parameter μ increased sufficiently, a situation arose in which "the fluid motion becomes very complicated, irregular and chaotic, we have turbulence."[19] But how could one describe this "chaotic" flow?

As the title of Ruelle and Takens's paper indicated, what was at stake was the very nature of turbulence. For Hopf, and especially Landau, the quasiperiodic flow that resulted from the appearance of several oscillatory modes was turbulent. Ruelle and Takens claimed that the quasiperiodic case was not *generic* for general dissipative dynamical systems. From this, they concluded that it had no chance of being observed and that one had to look elsewhere for a "mathematical explanation of turbulence."[20]

[19] D. Ruelle and F. Takens, "On the Nature," 168.
[20] D. Ruelle, "Strange Attractors as a Mathematical Explanation of Turbulence," *Statistical Models and Turbulence: Proceedings of the Symposium at the University of*

Ruelle and Takens's model for the onset of turbulence was dependent on their exploitation of three main sources. First, their reliance on concepts stemming from the qualitative study of dynamical systems made plain how much their explanation of turbulence was dependent on the work of Thom, Smale, and his students. Second, the emphasis with which they studied the Hopf bifurcation, and the techniques they used to do so, were novel. As will become apparent, Hopf's pair of articles had by and large fallen into oblivion. Ruelle and Takens's article was crucial in drawing attention back to them and linking them with Landau's scheme for turbulence. The third important strain lay in Ruelle's earlier work. In the following, these three strains will be examined. Like for most of Chapter V above, there will be no pretense of presenting an original history. Rather, this will convey, with an infusion of relevant contextual elements, a re-reading of the sources that were important for Ruelle and Takens when in 1970 they wrote their famous article.

b) The Quasiperiodic Model for the Onset of Turbulence

(i) Physics à la Landau

As was said in Chapter VI, David Ruelle's interest in fluid dynamics surely went as far back as 1968. And his inroad into the field was provided by the famous textbook *Fluid Mechanics* by Landau and Lifshitz. A Russian specialist in fluid mechanics who wrote one of its only existing modern histories, G. Tokaty expressed his strong admiration for this book as such:

California, La Jolla, 1971, ed. M. Rosenblatt and C. van Atta, Lecture Notes in Physics, 12 (Berlin: Springer, 1972): 292-299.

> in my personal opinion and experience, among the more recent contributions to fluidmechanics [*sic*], . . . by far the most outstanding in essence and beautiful in mathematical form was the great book *Mekhanika Sploshnykh Sred* [The Mechanics of Continuous Media] by Landau and Lifshitz, Moscow, 1954; it is difficult to imagine a professional pleasure superior to that experienced while reading this book.[21]

Ruelle did not seem to have enjoyed reading Landau and Lifshitz's book as much as Tokaty, but he did find a gem in it. "I worked my way slowly through the complicated calculations that these authors seem to relish," he recalled, "and suddenly fell on something interesting: a section on the onset of turbulence, without complicated calculations."[22] In retrospect, it seems easy to notice that Landau's suddenly verbose prose was indicative of a conjecture. But he had nevertheless deemed this conjecture important enough to publish it in a separate note, included in the 1944 volume of the Proceedings of the Soviet Academy of Sciences.[23]

Little is known about the conditions in which Lev Landau came up with his model for the onset of turbulence. In most biographical writing about him, the World War II years do not seem to be well documented. It is only mentioned that his Moscow Institute was evacuated to Kazan where he worked on defense-related problems, and in particular the detonation of explosives.[24] It would be interesting to

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[21] G. A. Tokaty, *A History and Philosophy of Fluidmechanics* (Henley on Thames: G. T. Foulis, 1971), 223.

[22] D. Ruelle, *Chance and Chaos,* 53.

[23] L. D. Landau, "On the Problem of Turbulence," *Doklady Akademi nauk SSSR (C. R. de l'Académie des sciences de l'URSS),* 44 (1944): 311-314; repr. *Chaos II,* 115-119; and L. D. Landau and Evgueni Lifshitz, *Fluid Mechanics,* chap. 3, (Oxford: Pergamond, 1959); *Mécanique des fluides* (Moscow: Mir, 1971); first Russian ed. 1954.

[24] See, e.g., A. Livanova, *Lev Landau,* transl. I. Sokolov (Moscow: Mir [1978], 1981), 39; A. I. Akhiezer, "Recollections of Lev Davidovich Landau," Physics Today, 47 (June 1994): 35-42; and G. Gorelik, "Lev Landau, Prosocialist Prisoner of the Soviet

know whether his theory of turbulence stemmed from consideration of this kind of applications. And although he seems to have expressed contempt for mathematical physics—he called it "mathematical lyricism"[25]—it would also be important to know the extent to which he was in contact with the Soviet mathematicians already discussed, namely Andronov, Kolmogorov and their followers.

A most important feature to notice about Landau's article was his reliance on arguments of a very general nature, which already made them potentially applicable to situations much different from the turbulence problem. No doubt Lev Davidovich Landau (1908-1968) impressed his mark on many, if not all, portions of theoretical physics in the twentieth century. Landau's contribution to the turbulence problem may not have been as spectacular as in other fields, but it provided a concise, intuitive picture of the mathematical mechanism responsible for turbulence. In addition to his theoretical work, Landau built a successful school of theoretical physics and trained a whole generation of students, for whom he wrote, with Evgueni Lifshitz, the famous *Course in Theoretical Physics,* of which the book on fluid mechanics was one volume. This course remains one of the most comprehensive syntheses of this field in the twentieth century. "I am the last universal physicist," Landau was quoted as saying after Enrico Fermi's death in 1954, and many indeed agreed with him.[26]

The paper published in 1944, can therefore be seen as a manifestation of Landau's universality. Indeed, as opposed to most previous works on the onset of

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State," Physics Today, 48 (May 1995): 11-15; Karl Hall, "Moral Economy of Soviet Physics Circa 1937," HSS Meeting in Atlanta (November 11, 1996).

[25] A. Livanova, *Lev Landau,* 69.

[26] A. Livanova, *Lev Landau,* 11-12.

turbulence in fluids, Landau's explanation of this phenomenon relied on general arguments and applied to every situation where turbulence arose in fluids. It was an actualization of a practice Ruelle called "Physics à la Landau."[27] It consisted in modifying the linearized theory by including the first nonlinear terms in the perturbation expansion, and sometimes, as in the case of turbulence, exhibiting new and interesting qualitative features.

In his 1944 paper, which was included almost untouched in his book with Lifshitz, Landau investigated the behavior of the solutions of the Navier-Stokes equations for Reynolds numbers slightly above critical values. Using old arguments about the stability of solutions, he contended that an oscillatory perturbation to the stationary solution was bound to arise, past a critical value of *Re*. In fact, he had to acknowledge that there was no theoretical foundation for this phenomenon. Only experimental data indicated that it was so.[28] In any case, Landau's exploitation of perturbation techniques allowed him to give an intuitive, but mathematically-informed justification for the fact that the motion ceased to be stationary for Reynolds numbers larger than a critical value. Moreover, this justification was easily transposable to any other case where such an oscillatory behavior could be obtained from a stationary solution.

Boldly, Landau guessed, with no computation to back him up, that this process would repeat *ad infinitum,* with new modes constantly appearing as the Reynolds number went up.

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[27] D. Ruelle, "Idéalisation en physique," in *Logos et théorie des catastrophes,* ed. J. Petitot (Geneva: Patiño, 1988): 99-104, 103.

> In the course of the further increase of the Reynolds number, new and new periods appear in succession, and the motion assumes an involved character typical of developed turbulence. For every value of *Re* the motion has a definite number of degrees of freedom; in the limit, as *Re* tends to infinity, the number of degrees of freedom becomes likewise infinitely large.[29]

In the 1954 book version of the argument, Landau again avoided deducing the appearance of the second frequency, much less the following ones, from the equations of motion. He wrote that this study should be attempted along the same line as for the first oscillatory mode. In a revealing footnote, he added: "But [it] has not been [done] in any case, due to exceptional mathematical difficulties."[30] As the process was repeated over and over again, new frequencies were supposed to appear in such a way that physical parameters of the fluid flow were given by an expression of the form:

$$x(t)= f(\omega_1 t,...,\omega_k t)$$

where the frequencies $\omega_1,...,\omega_k$ were not rationally related. Then Landau defined turbulence as being this complicated quasiperiodic behavior. "In this way, for $Re>Re_{\mathrm{crit}}$ the motion quickly acquires a complex, inextricable character. Such a motion is said [to be] *turbulent.*"[31]

During World War II, as is well known, another Soviet scientist made a fundamental contribution to the study of turbulence which shaped much of the work of the succeeding decades. In 1941, Andrei Kolmogorov published his famous article on the statistical theory of developed turbulence. As it was concerned with the global features of fluid flows at very high Reynolds numbers (so-called *developed*

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[28] L. D. Landau and E. Lifshitz, *Mécanique des fluides*, 127.
[29] L. D. Landau, "On the Problem of Turbulence," 314.
[30] L. D. Landau and E. Lifshitz, *Mécanique des fluides*, 131n. My translation.

turbulence), and not at all with the mechanism responsible for the transition to turbulence, it will not be dealt with it in any detail.[32] But, since the roots of Ruelle and Takens's theory lay in a confrontation between fluid mechanics and dynamical systems theory, one may wonder what a mathematician of Kolmogorov's stature, who had deep knowledge in both hydrodynamics and qualitative dynamics, and who directed a seminar on just these two topics together in the late 1950s at the Moscow State University, might have thought of Landau's scheme. According to his student Vladimir Arnol'd, Kolmogorov did not believe in Landau's theory and as early as the late 1950s made fun of Landau's torus: "Apparently, he [Landau] did not know of any other dynamical systems."[33]

(ii) The Hopf Bifurcation

One of the most important effects of Ruelle and Takens's article, as shown by the citation analysis summarized in Graph 8, was to draw the attention of a wide circle of scientists back to a pair of forgotten papers written in the 1940s. As two specialists of the stability theory approach of turbulence, Daniel Joseph and David Sattinger, wrote at about the same time as Ruelle and Takens: "Hopf's paper appeared in the *Sach.*

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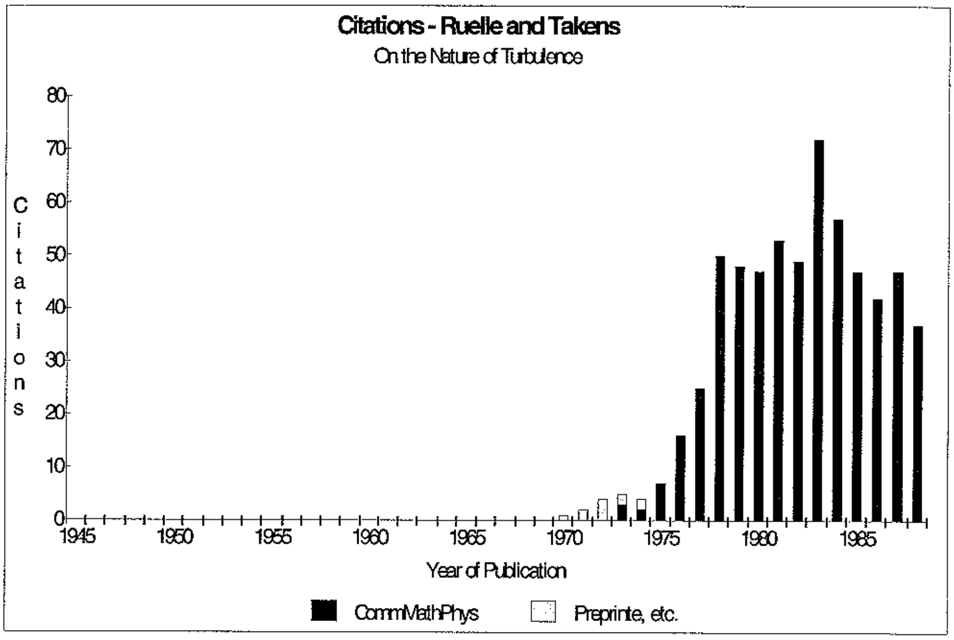
[31] L. D. Landau and E. Lifshitz, *Mécanique des fluides*, 131. My translation, and my emphasis.

[32] On Kolmogorov's contribution to the theory of developed turbulence, which lies outside of the scope of the present study, I refer to a careful introduction to the subject: U. Frisch, *Turbulence.* A quick historical treatment is to be found in M. Farge, "Évolutions des théories sur la turbulence développée," *Chaos et déterminisme,* ed. A. Dahan Dalmedico, et al.: 212-245. See also his original paper: A. N. Kolmogorov, "Local Structure of Turbulence in an Incompressible Fluid at Very High Reynolds Numbers," *Doklady Akademi Nauk SSSR,* 30 (1941): 299-303.



Graph 8: Citations to David Ruelle and Floris Takens, "On the Nature of Turbulence," *Communications in Mathematical Physics,* 20 (1971), according to the *Science Citation Index,* 1949-1988.

Akad. der Wiss. of 1942 and, unfortunately, is not generally available. . . . His results and techniques do not appear to be widely known."[34]

The citation analysis of David Ruelle and Floris Taken's 1971 famous article allow us to assess Ruelle's own perception that it was slow to be appreciated. Graph 8 shows the evolution in the number of citations to this article. Knowing that the paper was rejected by the referee of the first journal it was sent to, we might suspect an underground career. In order to best account for initial reactions to the preprint, I have

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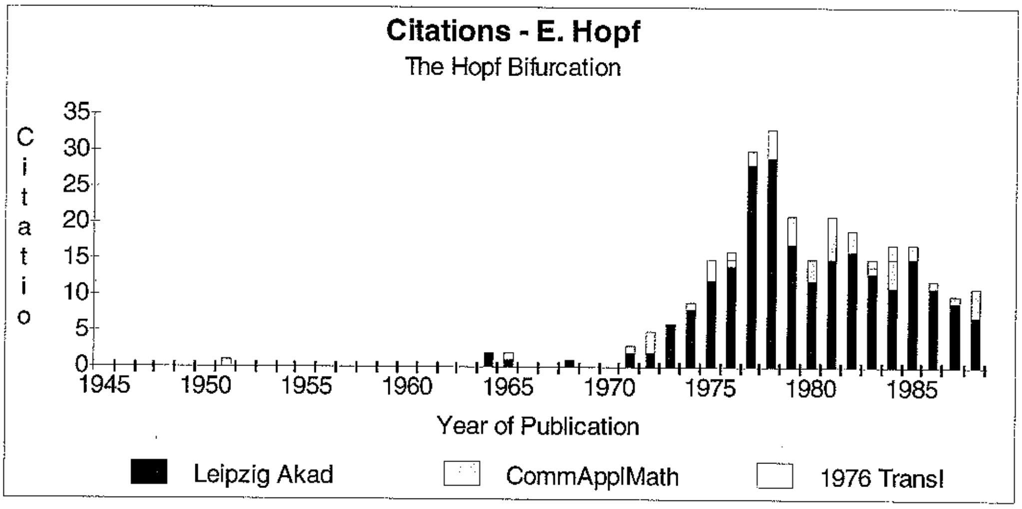
[33] V. I. Arnol'd, "On A. N. Kolmogorov," *Golden Years of Moscow Mathematics,* ed. S. Zdravkovskan and P. L. Duren (Providence: American Mathematical Society, 1993): 129-153, 130.

[34] D. D. Joseph and D. H. Sattinger, "Bifurcating Time Periodic Solutions and their Stability," *Archive for Rational Mechanics and Analysis,* 45 (1972): 79-109, 81.



Graph 9: Citations to Eberhard Hopf, "Abzweigung einer periodischen Lösung," and "A Mathematical Example," and also the translation: "Bifurcation of a Periodic Solution," according to the *Science Citation Index,* 1945-1988.

included citations to Ruelle's work labeled as "to be published", "unpublished", "personal communication", and "preprint," during the period 1970-1974, with no insurance, however, that they specifically referred to Ruelle and Takens's work.

Even by taking into account its potential underground trajectory, the career of the Ruelle-Takens article was quite slow to take off, indicating that its reception was not a trivial affair. The period that saw the biggest surge in interest was 1975-1978, thus mirroring almost perfectly the reception of Lorenz's paper, with a shorter latency period (Fig. 11).

Citation analysis also establishes that Ruelle and Takens's paper was crucial for having brought attention on Hopf's work on bifurcation theory. In graph 9, I plotted the evolution of the number of citations that both of his articles dealing with the subject received in the period from 1945 to 1989. It is remarkable that these papers were hardly ever cited before 1971. Hopf's 1948 paper was mentioned only once, in 1951 by fluid mechanicist G. Batchelor, while his 1942 Leipzig paper was cited, for the first time, 22 years after its publication, by Courant Institute mathematician Jürgen Möser. This was two years before Thom referred to the Hopf bifurcation in *Structural Stability and morphogenesis.* It becomes quite obvious that it was the success of the Ruelle-Takens hypothesis that made Hopf's.

In his two articles, Eberhard Hopf (1902-1983) introduced and studied a *bifurcation* [*Abzweigung*], now known as the Hopf bifurcation (Fig. 13).[35] Modestly, he pointed out that he scarcely thought there was anything new in his result, emphasizing that the methods had "been developed by Poincaré perhaps 50 years ago."[36] Indeed, Henri Poincaré tackled a very similar problem in *Les Méthodes nouvelles de la mécanique céleste* published in 1892. He studied the following equation, reminiscent of Ruelle and Takens's version of the Navier-Stokes equation:

$$\frac{dx_i}{dt} = X_i(x_1,\dots,x_n); \quad \frac{\partial X_i}{\partial t} = 0.$$

Poincaré then posed his problem as such: "Suppose that, in the [above] equation, the functions X_i depend on a certain parameter μ; suppose that in the case μ=0, we were

[35] E. Hopf, "Abzweigung einer periodischen Lösung von eine stationären Lösung eines Differentialsystems," *Berichten der Mathematisch-Physischen Klasse des Sächcischen Akademie der Wissenschaften zu Leipzig,* 94 (1942): 1-22; "Bifurcation of a Periodic Solution from a Stationary Solution of a System of Differential Equations," transl. L. N. Howard and N. Kopell, in *The Hopf Bifurcation and Its Applications,* ed. J. E. Marsden and M. McCracken (New York: Springer, 1976): 163-193; and E. Hopf, "A Mathematical Example Displaying Features of Turbulence," *Communications on Applied Mathematics,* 1 (1948): 303-322.

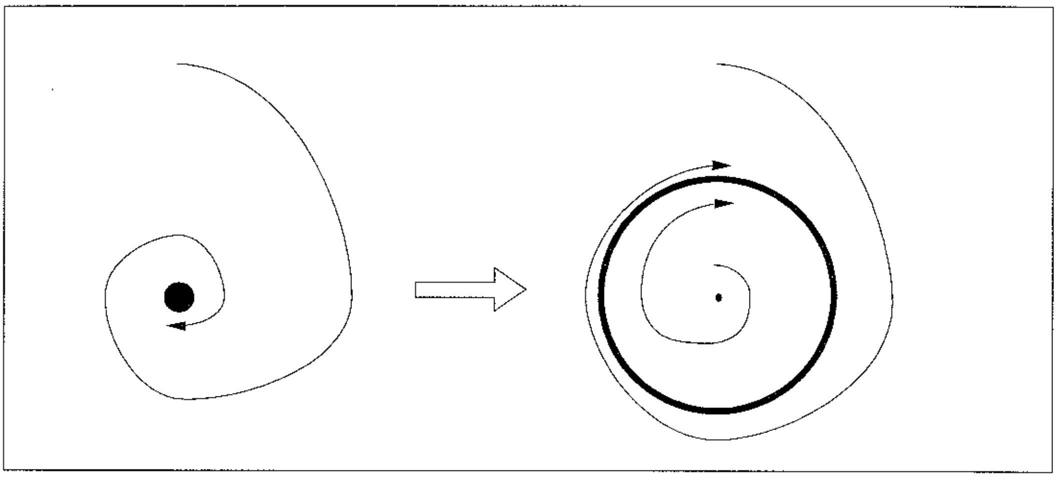


Figure 13: The Hopf Bifurcation of a Point Attractor into a Close Trajectory. Redrawn from R. Thom, *SSM,* 97.

able to integrate the equations, and that we thus noticed the existence of a certain number of periodic solutions. In which conditions will we have the right to conclude that the equations still exhibit periodic solutions for small values of μ?"[37] As opposed to Hopf however, Poincaré had not used the word bifurcation to describe this situation.[38]

Still, Hopf believed his results to be "not without value" because they bore on non-conservative systems, which Poincaré did not consider. It must also be noticed that this bifurcation was extensively discussed by Aleksandr Andronov already in the

[36] E. Hopf, "Bifurcation," 168.
[37] Henri Poincaré, *Les Méthodes nouvelles de la mécanique céleste,* 1 (Paris: Gauthier-Villars, 1892), 81.
[38] He reserved the word bifurcation for another case: H. Poincaré, "Sur l'équilibre d'une masse fluide animée d'un mouvement de rotation," *Acta Mathematica,* 7 (1885): 159-380; repr. *Œuvres,* 7: 40-141. See O. Gurel, "Poincaré's Bifurcation Analysis," *Bifurcation Theory and Applications in Scientific Disciplines,* ed. O. Gurel and O. E. Rössler (New York: New York Academy of Sciences, 1979): 5-26.

early 1930s.[39] Vladimir Arnol'd later wrote that the neglect of Andronov's contribution in the 1970s may have been partly his fault since he failed to emphasize it when he started talking—to Thom especially—about the Hopf bifurcation in the mid-1960s.[40] Nevertheless, for the mathematicians and physicists who took up Hopf's work where he had left it in 1948, "Hopf's crucial contribution was the extension from two dimensions to higher dimensions," apparently not considered by Andronov, who mainly worked in two dimensions.[41]

Born in 1902, Eberhard Hopf received his Ph.D. from Berlin University in 1925 for a dissertation in real analysis.[42] From 1926 to 1930 he then worked at the *Astronomische Rechenzentrum* at Berlin University. Like Poincaré and Birkhoff, he approached the study of differential equations starting from astronomical concerns. Hopf then spent the next six years in the United States at the Harvard Astronomical Observatory and then at the Massachusetts Institute of Technology (MIT), Cambridge, Mass., where he might have been in contact with George D. Birkhoff.

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[39] V. I. Arnol'd, "Catastrophe Theory," 229-230, in which are cited: A. A. Andronov, "Mathematical Problems of the Theory of Self-Oscillations," *The First All-Union Conference on Auto-Oscillations, November 1931* (Moscow: GTTI, 1933): 32-71; and A. A. Andronov and E. A. Leontovich, "Some Cases of Dependence of Limit Cycles on Parameters," *Uschen. Zap. Gor'k. Univ.,* no. 6 (1939): 3-24. See also S. Diner, "Les voies du chaos déterministe dans l'école russe," *Chaos et déterminisme,* ed. A. Dahan Dalmedico et al., 342.
[40] V. I. Arnol'd, *Catastrophe Theory,* 35.
[41] J. E. Marsden and M. McCracken, "Preface," *The Hopf Bifurcation and Its Applications,* ed. J. E. Marsden and M. McCracken (New York: Springer, 1976), ix.
[42] Information for the following paragraphs was gathered from: M. Denker, "Eberhard Hopf: 04-17-1902 to 07-24-1983," *Jahresbericht der deutschen Mathematiker-Vereinigung,* 92 (1990): 47-57; and A. Icha, "Eberhard Hopf (1902-1983)," *Nieuw Archief voor Wiskunde,* 4th ser., 12 (1994): 67-84. Significantly, earlier tributes to Hopf did not emphasize his work on the Hopf bifurcation; see, e.g., P. M. Anselone,

In 1936, he was appointed at Leipzig University to fill the chair of the late Leon Lichtenstein, the founding editor of the *Mathematische Zeitschrift.* Going back to Germany when it was ruled by the Nazis, Hopf's move was resented by some Cambridge mathematicians. Throughout those years, Hopf worked on fields of mathematics that were closely related to physical concerns, more precisely the theory of partial differential equations and ergodic theory. In this later field, one most note that using the work of Birkhoff and von Neumann, he proved the ergodicity of surfaces of negative curvature.[43] This was the work that René Thom studied for his doctorate, and presented at the Bourbaki Seminar in 1951.

In 1942, Eberhard Hopf was drafted in the German war effort and called to serve at the *Luftfahrtforschungsinstitut* in Einrig near Munich. Was it in contact with applied fluid dynamics that he became interested in bifurcation theory? One thing is certain: already in 1942, Hopf's paper briefly pointed out the possible application to fluid dynamical cases, a field that he had started considering in the early 1940s.[44]

Appointed to Carathéodory's chair at Munich in 1944, he left in 1947 for the Institute for Mathematics and Mechanics at NYU, directed by Richard Courant (later to become the Courant Institut), "imported . . . as a paper clip scientist for the U.S.

"In honor of Professor Eberhard Hopf on the occasion of his seventieth birthday," *Applied Analysis,* 3 (1973): 1-5.

[43] See especially, E. Hopf, *Ergodentheorie*, Ergebnisse der Mathematik und ihrer Grenzgebiete, 5(2) (Berlin: Springer, 1937), where a unified treatment of this young field of analysis was introduced. For a history of this theory, see A. Lo Bello, "On the Origin and History of Ergodic Theory," *Bollettino di storia delle scienze matematiche,* 3 (1983): 37-75.

[44] E. Hopf, "Ein allgemeiner Endlichkeitssatz der Hydrodynamik," *Mathematische Annalen,* 117 (1941): 764-775.

Army." He stayed abroad because, he said, "in the USA, there's more time for research."[45]

It was while at Courant's Institute that he wrote his second paper dealing with the Hopf bifurcation, this time strongly emphasizing the turbulence problem. It was published in the Institute's own *Communications in Applied Mathematics,* in an issue where "all the papers . . . represent[ed] results which were obtained at the Institute . . . under contract with the Office of Naval Research of the US Navy."[46]

In 1948, Hopf took up a teaching assignment with the U.S. Navy and then joined the Institute of Mathematics at Indiana University (in order to work with Clifford Truesdell), where he stayed until his retirement in 1972. He spent the last decades of his mathematical career working on hydrodynamic problems. But rather than exploiting the Hopf bifurcation, he chose to develop further Jean Leray's ideas, also cited by Ruelle and Takens, and introduced below. Says his biographer, Hopf's "interest in hydrodynamics and turbulence . . . [was] based on his deep understanding of differential equations."[47]

(iii) What Hopf Did and What He Did Not Do, Compared with Ruelle and Takens

There are many similarities between Hopf's original pair of papers and Ruelle and Takens's. From today's point of view, Hopf's work can be seen as already drawing the connection between turbulence and bifurcation theory, as exhibiting notions similar to

[45] M. Denker, "Eberhard Hopf," 48. About the Courant Institute, see A. Dahan Dalmedico, "L'Institut Courant: le bastion des maths appliquées," *La Recherche,* 300 (1997): 106-111.
[46] *Communications on Applied Mathematics,* 1 (1948).

that of attractors and genericity, and, like Landau, as explaining turbulence in terms of an accumulation of frequencies. There also were obvious differences between Hopf's work and Ruelle and Takens's. To confront them is a worthwhile exercise since it underscores where the originality lay in Ruelle and Takens's treatment of turbulence.

To start with, the appearance of a periodic solution from a stationary one in a system of the form $dx/dt=F(x,\mu)$ forms the basic result of Hopf's 1942 paper. The method Ruelle and Takens later used to prove Hopf's theorem hardly departed from Hopf's. Moreover, already in 1942, examples taken from hydrodynamic situations were on Hopf's mind. In fact, he used classic results about the stability of solutions of hydrodynamic equations as a way to orient his investigation of the bifurcation:

> Since in nature only stable solutions can be observed for a sufficiently long time of observation, the bifurcation of a periodic solution from a stationary solution is observable only through the latter becoming unstable. Such observations are well known in hydrodynamics. For example, in the flow around a solid body; the motion is stationary if the velocity of the oncoming stream is low enough; yet if the latter is sufficiently large it can become periodic.[48]

In 1942, Hopf also mentioned the case of Taylor-Couette flows, an example also briefly raised by Ruelle and Takens.[49] In 1948, Hopf's second paper was exclusively concerned with hydrodynamics and turbulence. Of course, the first equations to be found in this article dealing with hydrodynamics were the Navier-Stokes equations. But, like Ruelle, Hopf saw that his approach could be applied to problems beyond turbulence. "There is no doubt, however, that many characteristic

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[47] M. Denker, "Eberhard Hopf," 48.
[48] E. Hopf, "Bifurcation," 167.
[49] E. Hopf, "Bifurcation," 169n.

features of the hydrodynamic phase flow occur in a much larger class of similar problems governed by non-linear space-time systems."[50]

Like Ruelle and Takens, Hopf too parametrized the Navier-Stokes equation in an abstract way by using the letter μ, but it was proportional to the inverse of Ruelle and Takens's own μ. So, for μ large, he wrote, "the only flow observed in the long run is a stationary one (laminar flow). This flow is stable against arbitrary initial disturbances." He clearly expressed that this flow represented "a single point in Ω [i.e. the phase space]."[51]

For smaller μ, the common understanding then was, Hopf contended, that "the turbulent flow observed instead displays a complicated pattern of apparently irregularly moving 'eddies' of various sizes." While Leray had suggested in the 1930s that turbulence was due to a loss of regularity of the solutions (see below p. 472), Hopf argued that there existed a smallest size of eddies, so that, on a small scale, the flow seemed laminar and "the regularity of the flow would never be doubted."

Instead "the qualitative mathematical picture which the author conjecture[d]" was the following. In the long run, the solution corresponded to a manifold $M(\mu)$ in phase space which was invariant under phase flow. This manifold had a finite number of dimensions $N(\mu) = \dim M(\mu)$. For μ large, M was a point and N=0. After the first bifurcation at μ_1, M became a one-dimensional Poincaré limit cycle. Then for μ_2, μ_3, etc. smaller and smaller, $N(\mu)$ increased at each bifurcation. Unaware of Landau's work on turbulence, or not seeing the connection, Hopf believed his model for

[50] E. Hopf, "A Mathematical Example," 304.

turbulence "to be the first of its kind." [52] He emphasized that it was in *qualitative* accord with turbulence, but not in a *quantitative* one. From today's perspective, it is easy to see that this scheme overlapped with Landau's, but this was expressed in the totally different language of qualitative dynamics. It is therefore not surprising that David Ruelle, aware of both Landau's model and dynamical systems theory as it was being developed at the IHÉS in the late 1960s, would see this similarity.

From a retrospective standpoint, many concepts used by Ruelle and Takens, with which they came in contact only through the latest developments in qualitative dynamics, are also to be found in Hopf's articles. He worked in phase space as a convenient way to visualize the solutions of the Navier-Stokes equations. Like Ruelle and Takens, he wondered: "What is the asymptotic future behavior of the solutions, how does the phase flow behave for $t \rightarrow \infty$?"[53] This proto-notion of attractors, which however lacked certain of its important characteristics, can easily be recognized as Birkhoff's ω-limit sets.

Moreover, Hopf used a proto-notion of genericity already present in the work of Poincaré and Birkhoff on ergodic theory, with which he was well acquainted. "Stability here means that the 'majority' of phase motions tends for $t \rightarrow \infty$ toward $M(\mu)$. We must expect that there is a 'minority' of exceptional motions that do not converge toward M."[54] But, as opposed to his predecessors and successors, he made no attempt

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[51] E. Hopf, "A Mathematical Example," 304-305.
[52] All quotes above are from E. Hopf, "A Mathematical Example," 304. Note that Landau was not cited by Hopf.
[53] E. Hopf, "A Mathematical Example," 304.
[54] E. Hopf, "A Mathematical Example," 305.

at giving a mathematical definition of what was meant by "majority" and "minority" when dealing with phase motions.

What therefore could Ruelle and Takens do that Hopf had not? Well, the second Hopf bifurcation, in which the limit cycle is 'inflated' into a torus, had been conjectured by Hopf, but not rigorously derived. Using Poincaré section techniques, by then a classic tool of qualitative dynamics, Ruelle and Takens showed that the second Hopf bifurcation could indeed occur. Generally, the outlook of Ruelle and Takens's article was also quite different from Hopf's. While Hopf's papers elegantly suggested his results, after thirty years of Bourbakism, Ruelle and Takens's looked much more rigorous, too much so for physicists, but as we shall see, not enough for mathematicians.

Of course, Ruelle and Takens's most original suggestion was that the picture conjectured by Landau and Hopf was not generic, and that an open set of strange attractors, a notion they then introduced, had to exist in the vicinity of any quasiperiodic flow involving at least four oscillatory modes. Ruelle and Takens's exploitation of recent results in dynamical systems theory, and more importantly, their adaptation of what I have called the modeling practices of applied topologists, will be the topic of section 3 below. Another possible explanation of turbulence that Ruelle considered was Jean Leray's. This case highlights a wholly different mathematical approach of the turbulence problem, much more informed by analysis.

c) Leray: Turbulence as Irregularity

On October 22, 1969, mathematician Jean Leray of the Collège de France spoke at the Institut des hautes études scientifiques. The title of his talk was: "Turbulent Solutions to the Equations of Fluid Mechanics."[55] This had been the topic of his doctoral thesis in 1933, supervised by French fluid mechanist Henri Villat.[56] Since the mid-1930s, however, Leray had spent little time working specifically on this problem. He rather chose to devote much of his time to the study of partial differential equations and topology in more abstract ways. As Ruelle and Takens only referred to Leray's 1934 article in *Acta mathematica,* we may therefore suppose that he essentially presented the same views when he addressed the physicists of the IHÉS, 35 years later.

(i) Turbulent Solutions

In his doctoral thesis, Leray conceived of turbulence as the breaking down of the Navier-Stokes equations at a certain point. Faced with the problem of establishing the existence of solutions to the Navier-Stokes equations, Leray considered an integral equation, due to the Upsala physicist C. W. Oseen, whose solutions did not need to be

[55] *Rapport scientifique, Année 1969 - Séminaires et conférences* (2/6/70), 6. Arch. IHÉS. At the time Ruelle first started to become interested in fluid mechanicshe met with Leray, on 16-18 May 1968, at the *Sixième rencontre entre physiciens et mathématiciens de Strasbourg,* where the later spoke of Feynman's integrals. I however ignore whether they discussed turbulence on this occasion.
[56] J. Leray, "Études de diverses équations intégrales non linéaires et de quelques problèmes que pose l'Hydrodynamique," *Journal de mathématiques pures et appliquées,* 12 (1933): 1-82; "Essais sur les mouvements plans d'un liquide visqueux que limitent des parois," *Journal de mathématiques pures et appliquées,* 13 (1934): 331-418; "Sur le mouvement d'un liquide emplissant l'espace," *Acta Mathematica,* 63 (1934): 193-248.

differentiable.[57] In doing so, Leray succeeded in using the most recent techniques of topology and the theory of functional sets in order to solve a physically-motivated problem, but at a cost:

> We tried to establish the existence of a solution to the Navier-Stokes equations corresponding to a given initial state: we succeeded only by renouncing regularity of the solution at some conveniently chosen instants.[58]

Leray thus defined "turbulent solutions to the Navier-Stokes equations" as irregular solutions to Oseen's integral equation. The relation of "turbulent solutions" to what physicists called turbulence, however, was not obvious.[59] This work on the theory of equations and fluid mechanics was highly praised by Villat and Lebesgue, and in 1934, Leray received the well-endowed Henri de Parville Prize from the Academy of Sciences.[60]

Leray's suggestion was totally different from what Landau and Hopf would propose ten to fifteen years later. Both had assumed that the Navier-Stokes equations would apply at any Reynolds number whatsoever. "There must exist, in principle, for all problems," Landau and Lifshitz postulated, "an exact stationary solution of

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[57] C. W. Oseen, "Sur les formules de Green généralisées qui se présentent dans l'hydrodynamique et sur quelques unes de leurs applications," *Acta mathematica,* 34 (1911): 205-284; and several articles published in the *Arkiv för matematik, astronomi och fysik,* from 1906 to 1919. Oseen already suggested that irregular solutions represented turbulence, see P. Appel, H. Begin, and H. Villat, "Développements concernant l'hydrodynamique," d'après l'article allemand de A. E. Hove, *Encyclopédie des sciences mathématiques pures et appliquées,* French ed. J. Molk and P. Appel, original German ed. F. Klein and C. H. Müller, tome IV, vol. 5 (Paris: Gauthier-Villars, 1914; repr. Jacques Gabay, 1993), 181.

[58] J. Leray, "Sur le mouvement," 245.

[59] J. Leray, "Sur certaines classes d'équations intégrales non linéaires," *CRAS,* 194 (1932): 1627-1629, 1629. See J. Leray, "Problèmes non-linéaires," *L'Enseignement mathématique,* 35 (1936): 139-151, 149.

hydrodynamical equations. . . . These solutions formally exist for any Reynolds number."[61] Neither Landau nor Hopf made any remark that might indicate that he had considered Leray's explanation of turbulence at the time. It was not that Leray's proposition was disproved by any later findings, but rather that no evidence was offered that his hypothesis—a radical one since it implied that the fundamental equation of fluid mechanics was no longer true after some critical point—was needed in order to account for turbulence.[62] But it is worth noticing here that, in the 1950s, Eberhard Hopf would choose to pursue Leray's approach instead of the bifurcation explanation he suggested in 1948.[63]

Leray's hypothesis ran against most of the history of turbulence in that it supposed that the Navier-Stokes equations ceased to describe fluid flows faithfully after a certain critical value, and had to be replaced by an integral equation.[64] This

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60 See *CRAS,* 199 (1934): 1479; and Lebesgue and Villat's praises in Paul Fallot's report, Assemblée des professeurs (24/11/46). Arch. CdF. G-iv-l 27Bb.

61 L. D. Landau and E. Lifshitz, *Mécanique des fluides,* 126. My translation from the French.

62 As late as 1991, Marie Farge would contend that Leray's "hypothesis has been up until now neither confirmed nor refuted." M. Farge, "Évolutions des théories," 223.

63 Among many papers, see E. Hopf, "Remarks on the Functional-Analytic Approach to Turbulence," *Hydrodynamic Instability,* ed. G. Birkhoff, et al. (Providence: AMS, 1962): 157-164.

64 There is much that could be said about Leray's work on turbulence and how it fits with long-term trends in the history of fluid mechanics, and especially with the interplay of mathematics, physics and engineering in the French context of the interwar period. This effort was done around Villat and sponsored in large part by the Ministry of Air: "one of the first examples [in France] of a scientific and technological policy." See P. Mounier-Kuhn, "L'enseignement supérieur, la recherche mathématique et la construction de calculateurs en France (1920-1970)," *Colloque Enseignement supérieur et formations technico-scientifiques supérieures en Lorraine - XIXe-XXe siècles, Metz, décembre 1995,* 8. See P. Mounier-Kuhn, "Un programme technologique national: la Mécanique des fluides," *Programmes Villes et institutions scientifiques, Rapport final,* ed. A. Grelot and M. Grossetti (CNRS PIR Villes, 1996).

suggestion could be seen as stemming from the work of Yves Rocard, whom we have already encountered studying self-oscillations (Chapter V). In 1929, the young Rocard gave a series of ten conferences at the Institut de mécanique des fluides at the Sorbonne, in which he proposed to examine the relationship of hydrodynamics with the kinetic theory of gases.[65] The molecular hypothesis, together with the statistical methods introduced by Maxwell and Boltzmann, allowed him to recover the Euler and Navier-Stokes equations, which were thereby confirmed as being consistent with contemporary beliefs about the molecular structure of matter. Boundary conditions could however have the effect of changing the dynamical equations of fluids. He concluded that "the molecular hypothesis . . . imposed by reality, already leaves the classical frame of hydrodynamics." This conclusion, he added, "takes nothing away from the mathematical interest of the problems of viscous fluid hydrodynamics, but *one should not expect ever to see a definitive concordance of [hydrodynamics] with data.*" The turbulence problems—imposed by experiments, Rocard noted, and not theory—might necessitate going beyond the classical theory of Navier and Stokes.

> That viscous fluid hydrodynamics itself possessed the power to solve, to treat such problems, this had hitherto not appeared to involve the shadow of a doubt, so great was the confidence in the value of this discipline to adapt to facts. Now *there is ground for us to be more worried, more skeptical* with respect to these possibilities and . . . it seems clear that it is on the contrary by

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I thank Pierre Mounier-Kuhn for having provided me these two texts. On the Pérès-Malavard computing machine for hydrodynamics, see J. Pérès, with L. Malavard, *Cours de mécanique des fluides (fluides parfaits, aile portante, résistance)* (Paris: Gauthier-Villars, 1936); and L. Malavard, *Applications des analogies électriques à la solution de quelques problèmes de l'hydrodynamique* (Paris: Blondel de la Rougery; Gauthier-Villars, 1934).

[65] Y. Rocard, *L'Hydrodynamique et la théorie cinétique des gaz* (Paris: Gauthier-Villars, 1932).

> leaving the framework of classical hydrodynamics that difficulties of this kind could be tackled.[66]

By undermining the validity of the Navier-Stokes equations, Rocard's speculations might have provided the ground on which Leray's model could be built.

Even while unhappy with the scheme proposed by Landau and Hopf, David Ruelle quite bluntly dismissed Leray's ideas, too. "While such a breakdown [of the validity of the Navier-Stokes equations] may happen, we think that it does not necessarily accompany turbulence."[67] He just did not want to deal with the complicated issue of the existence and uniqueness of the solutions to the Navier-Stokes equations: "Turbulence has probably nothing to do with these difficulties."[68] This later field had been, and would be, the object of numerous studies; it was the concern of many applied mathematicians, without much to show as an end result of decades of research as far as physical problems were concerned. Ruelle just deemed it irrelevant, since he could propose an explanation which only relied on the hypothesis that turbulent flows were the solution of a dissipative nonlinear equation, and not necessarily of precisely the Navier-Stokes equations.

(ii) What Use for the Theory of Equations? Existence and Uniqueness Theorems

David Ruelle therefore deemed a whole thriving branch of (applied) mathematics, namely the search for existence and uniqueness theorems in the theory of partial

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66 Y. Rocard, *L'Hydrodynamique,* 149-150. My emphasis.
67 D. Ruelle and F. Takens, "On the Nature," 57. In his Lausanne lecture notes, Ruelle only cited the conclusion of Leray's paper, which I quoted above. D. Ruelle, "Méthodes d'analyse globale," 5-6.
68 D. Ruelle, "Strange Attractors as a Mathematical Explanation," 295.

differential equations, as of little importance for the study of turbulence. This attitude was in a striking opposition to the views expressed by Szolem Mandelbrojt when, on February 16, 1947, he recommended Jean Leray's candidacy for the chair on Theory of Differential and Functional Equations at the Collège de France.[69] In his defense, Mandelbrojt claimed that formal integration of differential equations had lost the importance it once had.

> Even supposing that a [formal integral] could be obtained, one would have, in order to grasp the properties of the solution thus computed, much more difficulty than by simply starting from the fact that it satisfies the given equation. In other word, progress is rarely achieved by adopting this formal point of view, as it is now called with some contempt, which for that matter is rather justified. [70]

Mandelbrojt then claimed that the first property that one needed to establish was that the solution to an equation with given boundary conditions, *existed*. The second was that it was *unique*. Only after "these two problems having been solved, do we need study the general propoerties of the solution."[71] Strikingly, physicist Ruelle totally reversed mathematician Mandelbrojt's order of priority.

The approach to the study of differential equations which focused on existence and uniqueness theorems had a long history in mathematical physics. This history regularly exhibited misunderstanding between mathematicians and physicists. Its classic expression was that "in general, existence theorems have little interest for the

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[69] A founding member of Bourbaki whom he had since left, Mandelbrojt was Benoît Mandelbrot's uncle. For biographical information about Mandelbrojt, and in particular his involvement with Bourbaki, see L. Beaulieu, *Bourbaki,* 384 and passim.
[70] Exposé de S. Mandelbrojt, Assemblée des professeurs (16/2/47). Arch. CdF. G-iv-1 28f.
[71] Exposé de S. Mandelbrojt (16/2/47). Arch. CdF. G-iv-1 28f.

physicist."[72] And Rayleigh confirmed this in 1916, when, reviewing the fourth edition of Horace Lamb's *Hydrodynamics,* he wrote:

> 'existence theorems,'... though demanded by the upholders of mathematical rigour, tell us only what we knew before, as Kelvin used to say.... What is strange is that there should be so wide a gap between [the physicist's] intuition and the lines of argument necessary to satisfy the pure mathematician.[73]

Leray's work in hydrodynamics could be taken as a counterexample for this widely shared view.[74] Here existence theorems seemed to imply that turbulence was due to the breaking-down of the classic Navier-Stokes equations. On the contrary, Ruelle and Takens's hypothesis was simply a reaffirmation that existence theorems were secondary to the physicist's concern with the behavior of solutions.

In view of Bourbaki's dominance of postwar French mathematics, it is somewhat surprising that Leray was chosen over another candidate who was none other than André Weil. Not only one of the most prominent Bourbakis, Weil was, by

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[72] V. Volterra, "Drei Vorlesungen über neuere Fortschrifte der mathematischen Physik," *Arkiv der Mathematik und Physik,* 22 (1914): 97-181; repr. *Opere matematiche,* 3 (Rome: Accademia nazionale dei lincei, 1957): 389-470, 441. Quoted by J. Gray, "Mathematics and natural Science in the 19th Century: The extraordinary Success of the Classical Approaches. Poincaré, Volterra, Levi-Civita, Hadamard," *Colloque International d'histoire des mathématiques, Luminy Marseilles, September 1997.* See also G. Israel, "Vito Volterra: un fisico matematico di fronte ai problemi della fisica del Novecento," *Rivista di storia della scienza,* 1 (1984): 39-72.

[73] Lord Rayleigh, Review of H. Lamb, *Hydrodynamics,* 4th ed., *Nature,* 97 (1916): 318; repr. *Scientific Papers,* 6 (Cambridge: Cambridge University Press, 1899-1920): 400-401. Quoted by S. Goldstein, "Fluid Mechanics," 3.

[74] "You people spend much time and much wit to show the existence of solutions whose existence is often evident to us for obvious physical reason," Theodore von Kármán imagined an engineer saying to a mathematician. "Tooling up Mathematics for Engineering," *Quarterly of Applied Mathematics,* 1 (1943): 1-6, 4. On the other hand, Leray's methods for proving existence and uniqueness theorems were those taken up by Eberhard Hopf, and further developed by, among others, the founder of an important school of applied mathematics in postwar France, J.-L. Lions, "Les équations de Navier-Stokes," *Séminaire Bourbaki,* 11(3) (May 1959): exposé #184.

then, already recognized worldwide as a first class mathematician.[75] About Weil, Mandelbrojt simply remarked in 1947 that he was "a very great mathematician, but [that] there [was] no point in talking here of his work, since there [was] no link between Weil's work and the title of the chair created."

This shows that the relation between pure and applied, Bourbakism and anti-Bourbakism is no simple matter when dealing with immediate postwar France. Many factors informed the preference expressed by the Assembly of Professors at the Collège de France. We must note that Leray had not been Mandelbrojt's first choice. Indeed, the procedure for hiring new professors at the Collège de France was a two-step process. When funds for a chair became available, the initial decision that professors had to make was how to rename it, and only at a further meeting did they suggest people to fill it. In practice, however, the naming of the chair was the moment when decisions were really taken, as it often was the case that professors presented candidates at the same time as they argued for their chair. On November 26, 1946, Mandelbrojt expressed his preference that the chair replacing Paul Langevin's, after his retirement, be named "General Analysis and Calculus of Probability" and filled by Maurice Fréchet. In his proposal, Mandelbrojt argued for the daring abstraction of Fréchet's method, which, recalling Bourbaki's, focused on the study of elements of as general a nature as possible.[76]

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[75] In June 1940, the Rockefeller Foundation asked G. D. Birkhoff and S. Lefschetz to designate the French mathematicians to be rescued from the debacle. Out of 9 names, only Weil (who was Jewish) and Henri Cartan belonged to the younger generation. L. Beaulieu, *Bourbaki,* 387.

[76] Proposition de la création d'une chaire d'Analyse générale et calcul des probabilités, par S. Mandelbrojt, Assemblée des professeurs (24/11/46). Arc. CdF. G-iv-l 27Cc.

A biologist, Paul Fallot dared to oppose Mandelbrojt, who was the only mathematician present at this meeting. He presented a masterful defense for a chair devoted to the theory of equations and to be filled by Jean Leray, whom he knew from having been his colleague at Nancy before the war.[77] Arguing that this topic of mathematics was hardly represented in Paris, Fallot underscored its fundamental importance as a "mathematical tool" which, for lack of necessary improvement, often blocked the progress of mathematical physics. Having shown the importance of the theory of equations for mathematics and other sciences, Fallot listed Leray's accomplishments, where his work on hydrodynamics figured prominently. Leray's work was remarkable for the new methods he used for the solution of differential equations encountered in nature. Moreover in the difficult circumstances of the war, Leray had also exhibited a gift for abstract topology. For Fallot, Leray's main strength lay in his ability to marry abstraction to practical concerns.

No doubt Jean Leray's war experience also played a role in his election at the Collège de France in 1946-1947. In Fallot's words, this was "a topic that one is almost ashamed of having to tackle, since it concerns not only the domain of Science." From 1940 to 1945, Jean Leray had spent five years in a German camp, where he put together a captive university. He refused to buy his freedom by becoming professor at

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One should not go too far in identifying Fréchet with Bourbakism. I leave this responsibility to Mandelbrojt. See his debate with Daniel Lacombe in Synthèse, *Notion de structure et structure de la connaissance* (Paris: Albin Michel, 1956): 97-135. In addition, far from being an unconditional promoter of abstraction, Fréchet worked on building a calculating machine at the Institut Henri-Poincaré in 1939-1940. On this, see P. Mounier-Kuhn, "L'enseignement supérieur."

[77] Rapport de M. Paul Fallot sur la chaire de "Théorie des équations différentielles et fonctionnelles," Assemblée des professeurs (24/11/46). Arch. CdF. G-iv-l 27Bb.

the University of Berlin. This experience seems to have pushed him further to the side of pure mathematics. During his captivity, "Jean Leray pursued his research, but took care to leave aside questions of mechanics, which could have interested the enemy."[78]

"The history of Science shows," Fallot contended, "that great advances in Mathematics always, or almost always, had as their starting point the necessity of finding new methods of computation in order to account for phenomena that old methods were powerless to analyze." Significantly, the professors of the Collège de France, by 26 cast ballots against 8, preferred the methods explored by Leray to Fréchet's abstraction.[79] In effect, they voted at the same time against Fréchet's earlier attempts at numerical computations, which in the postwar era would provide new methods *par excellence* for the integration of equations.

3. DYNAMICAL SYSTEMS IN THE RUELLE-TAKENS MODEL

As early as the 1920s, George D. Birkhoff wrote that "topology deserves to obtain a more prominent position in physical theories than it has yet obtain."[80] It took a while for this to happen. But we may contend that if Ruelle and Takens were able to make a dent in such a formidable problem as turbulence, it was mainly because they were in a good position to capitalize on recent developments in topology, and especially in the

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[78] F. Lot, "Jean Leray, aventurier de l'abstrait," *Figaro littéraire* (6-12 mars 1965), 11. However, Leray taught a course on compressible fluid in 1946 where he dealt at length with wings of airplanes: *Mécanique des fluides compressibles.* Cours du Centre d'études supérieures de la mécanique, section des fluides compressibles (1946). Jussieu Lib. Note the similarity of Leray's experience as a war captive with Fernand Braudel's, see Pierre Daix, *Braudel,* (Paris: Flammarion, 1995).
[79] Procès-verbal, Assemblée des professeurs (24/11/46). Arch. CdF. G-iv-l 27V. Rapport de M. Paul Fallot. G-iv-l 27Bb.

theory of dynamical systems brought about by Thom and Smale. The physicist's eye of David Ruelle, after having been in contact with these developments, could perceive the problem with Landau's scheme. It was unrealistic because *non-generic.* Infinite odds were against it. Before this could be shown rigorously, however, there was a lot of work to be done.

a) Thom, Smale, and the Concept of Attractors

(i) Acknowledgments

"The authors take pleasure in thanking R. Thom for valuable discussion, in particular introducing one of us (F.T.) to the Hopf bifurcation. Some inspiration for the present paper was derived from Thom's forthcoming book [*Stabilité structurelle et morphogénèse*]." Thus read the acknowledgments in Ruelle and Takens's article.

When the paper was written, in the spring of 1970, the IHÉS was indeed being visited by many mathematicians important for the development of what in Chapter VI I have called the modeling practice of applied topologists. Ruelle later remembered :

> The reason I did not like Landau's description of turbulence in terms of modes is that I had heard seminars by René Thom and studied a fundamental paper by Steve Smale called "Differentiable Dynamical Systems." . . . The former is my colleague at the Institut des Hautes Etudes Scientifiques near Paris, and the latter makes frequent visits there. From them I had learned the modern developments of Poincaré's ideas on dynamical systems, and from these developments, it was clear that the applicability of the mode paradigm is far from universal.[81]

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[80] G. D. Birkhoff, "The Mathematical Nature of Physical Theories," *American Scientist,* 31 (1943): 281-310, 310; repr. *Papers,* 2: 890-919, 919.

[81] D. Ruelle, *Chance and Chaos,* 55. Note that his use of the word paradigm is here a clear reference to T. S. Kuhn's ideas: "I am not an uncritical believer in Kuhn's ideas; in particular, they appear to me of little relevance to pure mathematics. The physical

Ruelle and Takens's most original innovation was the concept of a *strange attractor*, for which they obviously relied on the concept of *attractor* as it was being defined in dynamical systems theory. While having a long prehistory that could go as far back as Poincaré and Birkhoff, the attractor concept was very recent.

(ii) Attractors

Arguments involving attractors were pervasive in Thom's work on catastrophe theory. While Ruelle and Takens made it famous, it was Thom who actually already drew attention to the Hopf bifurcation in *Structural Stability and Morphogenesis.*[82] Because their structural stability was immediate, Thom rarely considered attractors that were more complicated than points. But the limit cycle appearing after a Hopf bifurcation provided him with an example of what he called a "generalized catastrophe."[83] Confidently, Thom also raised the possibility of a second Hopf bifurcation from cycle to torus. In the first edition of his book, it was when dealing with the Hopf bifurcation that Thom revealed his ignorance of Poincaré, attributing the source of the word 'bifurcation' to Hopf.[84] Noticing, clearly as a consequence of Ruelle's work, its possible relevance for turbulence, Thom moreover used the Hopf bifurcation to model the phenomenology of mitosis.

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concepts of *modes* and *chaos* seem, however, to fit rather well Kuhn's description of *paradigms.*" *Ibid.,* 177n7. His emphasis.

[82] R. Thom, *SSM,* 97-100, 108, 263-264, and 283.

[83] As Thom never was very clear about what he meant by a generalized catastrophe in mathematical terms, whether he actually considered the Hopf bifurcation as one of these may therefore be open to debate. Nevertheless, it is clear that he considered it to be a catastrophe, which was not elementary since not stemming from gradient dynamics. R. Thom, *SSM,* 97, 103.

[84] R. Thom, *SSM,* 1972 ed., 105. Corrected in later eds.

Nevertheless, the use of the Hopf bifurcation in itself which was little noticed in Thom's work. His greatest innovation lay in the description of this process in terms of *attractors.* It is not clear whether Smale or Thom first introduced this important concept in the theory of dynamical systems. "Each says the other invented it."[85] Actually, the first appearance in print of the word 'attractor' in this context is due to neither Thom nor Smale. In September 1959, Pinchas Mendelson, from the Polytechnic Institute of Brooklyn, gave a talk "On Unstable Attractors" at the Symposium on Ordinary Differential Equations and their Applications, in Mexico City, a meeting attended by both Smale and Thom.[86] No definition of attractor was then given and it was used as a synonym for a critical point which was the only minimal set of the ω-limit of the dynamics. This was very close to the definition later coined by Thom and Smale, and taken up by Ruelle and Takens: "A closed subset A of the non-wandering set Ω is an attractor if it has a neighborhood U such that [the ω-limit set of U is] A."[87]

Indeed, Thom's manuscript for *Stabilité structurelle et morphogénèse,* which I have seen at Princeton University and which clearly was revised between 1966 and

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[85] Bob Williams's comment in *From Topology to Computation,* ed. M. Hirsch et al., 183. See their first definitions in R. Thom, *SSM,* 39; and S. Smale, "Differentiable Dynamical Systems," 786. At least once, however, Thom personally claimed responsibility for the term. R. Thom, "Problèmes rencontrés dans mon parcours mathématique: un bilan," *Publications mathématiques de l'IHÉS,* 70 (1989): 199-214, 203.
[86] P. Mendelson, "On Unstable Attractors," *Boletín de la Sociedad matemática méxicana,* 5 (1960): 270-276.
[87] D. Ruelle and F. Takens, "On the Nature," 170. For more on the early use of attractors, see R. F. Williams, "One-dimensional Non-Wandering Sets," *Topology,* 6 (1967): 473-487.

1969, included many instances of the use of the word 'attractor' as well as derived notions, such as the basin of an attractor. But it did *not* include the definition finally provided in 1972.[88] In a few places, the term "minimal sets" first used by Thom, was crossed out and replaced by "attractors."[89] The attractor concept thus came to carry a basic significance in Thom's modeling practice:

> Every object, or physical form, can be represented as an *attractor C* of a dynamical system on a space *M* of *internal variables*.[90]

Similarly, the term attractor was also defined in Smale's famous review paper "Differentiable Dynamical Systems." But it then appeared only on the 40th page of the paper, and was used only briefly, in particular to introduce the attractor that Ruelle and Takens would take up as an example of a "strange attractor." Shown to Smale by Jürgen Möser, this set was only brought up as an example of a nontrivial attractor, locally the product of a Cantor set and a manifold.[91] In his review paper, Smale's goal remained the classification of flows, and not that of their attractors. After the publication of Ruelle and Takens's work, the latter would become the goal of many 'chaologists' in the next generation.

As often happens in articles containing a concept appealing to the imagination, Ruelle and Takens did not formally define strange attractors in 1970.[92] They

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[88] R. Thom, *SSM,* 38-40; *Stabilité structurelle,* 1972 ed., 56-57.
[89] See the discussion of the Hopf bifurcation, R. Thom, *SSM,* 98.
[90] R. Thom, *SSM,* 320; quoted by M. W. Hirsch, "The Dynamical Systems Approach," 29.
[91] S. Smale, "Differentiable Dynamical Systems," 786-788.
[92] D. Ruelle and F. Takens, "On the Nature," 170; "Méthodes d'analyse globale," 13, 23. They also used terms like "bizarre attractor" and "vague attractor." Only the latter, introduced by Thom, was defined in too technical a fashion to comment on this here. R. Thom, *SSM,* 27 and 39. Already used in the 1966 version of the manuscript. See

illustrated what they meant by strange attractors by using Smale's example. In practice, they included all attractors that were neither fixed points, nor limit cycles, nor quasiperiodic sets.

In studying dynamic processes in terms of attractors, the most important concept introduced by Thom actually was not the attractor itself, but rather the *basin of an attractor,* which Smale did not use in 1967. Indeed, the definition of an attractor provided by Thom and Smale could be seen as merely an insightful combination of Birkhoff's classic definitions of nonwandering, ω-limit, and minimal, sets, all well known to most people working on dynamical systems. But by defining the basin of an attractor A as the set of all points whose ω-limit set is A, i.e. which are attracted to A (this basin being the neighborhood U appearing in Ruelle and Takens's definition above), Thom allowed the cutting-up of the base space into several parts that could be studied separately.

As was shown in Chapter III, the proper context in which to understand this notion of attractor is in relation to Waddington's epigenetic landscapes. In this case, the concept of basin is immediate, as indicated by Thom's metaphor:

> on a contour map the basins attached to different rivers are separated by watersheds, which are pieces of crest lines, and these separating lines descend

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simple introductions in D. Ruelle, "Strange Attractors," *Mathematical Intelligencer,* 2 (1980): 126-127; *TSAC,* 195-206; "Les attracteurs étranges," *La Recherche,* 11, (1980): 133-144; and C. Grebogi, E. Ott, and J. A. Yorke, "Chaos, Strange Attractors, and Fractal Basin Boundaries in Nonlinear Dynamics," *Science,* 238 (1987): 632-638. A good popular exposition of strange attractors can also be found in Gleick, *Chaos* and Kellert, *In the Wake of Chaos.*

to saddle points, where they meet like ordinary points, but rise to summits where they may have flat cusp points.[93]

Focusing on the dual concepts of attractors and their basins allowed a program to be imagined. This program could be described as the classification of attractors of dynamical systems and the way they interacted with one another. It was a direct analogue of Thom's catastrophe theoretic program. No doubt it presented a formidable task, but the task could be contemplated.

b) Modeling Practices at the Institut des Hautes Études Scientifiques

Clearly, Ruelle's association with "applied topologists" was for him a major source of inspiration. But many historical treatments of chaos theory, except for some written by mathematicians, have tended to obscure this fact and especially the "inspiration" Ruelle acknowledged taking away from catastrophe theory. As Thom's works remained quite controversial for the following decades, this neglect might be interpreted as an instance of careful purification, conscious or not, of controversial sources by working scientists.[94]

But what I want to claim more. That Ruelle and Takens's work relied on *concepts* introduced by Smale and Thom in dynamical systems theory is clear simply from looking at their joint paper. I contend, however, that Ruelle's *modeling practice* was crucially shaped by close contact with the activities that went on around Thom at

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[93] R. Thom, *SSM,* 39-40. See illustration 25 of the plaster models built by Marcel Froissart for Thom, in *SSM,* illus-x.

[94] A telling example of this attitude is provided by a recent article of Ivar Ekeland's in which while comparing catastrophe theory with chaos, he totally obscured the fact that they depended on one another for their historical genesis: "La théorie des catastrophes. Relu 20 ans après par son auteur." *La Recherche,* 301 (1997): 89.

the IHÉS. In this way, his situation at Bures-sur-Yvette, not only provided him an opportunity to learn about new developments in a mathematical theory that were only starting to become known outside a small circle, but showed him ways in which these new mathematical tools could be used in the concrete practice of building models for natural phenomena. Briefly put, the modeling practice Ruelle and Takens adapted to the problem of turbulence could be described as follows: (1) *isolate topological features* in some process, in this case the onset of turbulence; (2) explore the way these features can, or cannot, *bifurcate* under various circumstances, relying on the postulate of genericity; and (3) provide *explanations* based on mathematical results.

In addition, Ruelle's position at the IHÉS was directly responsible for his meeting with Floris Takens. Born in 1940, Takens was a mathematician of the University of Groningen in the Netherlands. Just a few years earlier, he had received his Ph.D. from the Mathematisch Instituut of the University of Amsterdam, working with Nicolaas Kuiper, who we may recall was interested in topics close to Smale's, Thom's and Zeeman's, and would become the second director of the IHÉS in 1971. Working on the singularities of differentiable mappings and vector fields, Takens and Kuiper had been in contact with the IHÉS and had invited John Mather in 1969.[95] Following a suggestion of Kuiper's, Thom invited Takens to spend the 1969-1970 academic year at Bures-sur-Yvette.[96] His resulting paper with Ruelle was the only

[95] F. Takens and J. Mather files. Arch. IHÉS.
[96] Lettres de Nicolaas Kuiper à René Thom (16/4/69); de Floris Takens à René Thom (27/4/69). Arch. IHÉS.

incursion Takens, who remained a specialist in the theory of dynamical systems, ever made into physics.[97]

As Takens acknowledged in his joint paper with Ruelle, even his knowledge of the Hopf bifurcation was due to Thom. In fact, Takens's contribution, like that of the IHÉS in general, again confirms that more than mere abstract concepts, it was practices that were in the process transferred from mathematics to physics.

In order to make this claim clearer, let me compare the modeling practice exhibited by Ruelle and Takens with those examined when looking at the almost contemporary 1971 Bahia Symposium. Recall that at this occasion Smale, Thom, and Zeeman had presented exemplary exposés of their modeling practices with respect to the exploitation of what they called global analysis or catastrophe theory. In Chapter VI, the fact that they already differed significantly in the ways they chose to model natural phenomena has been made explicit. Their practices of mathematization disagreed on the manner in which one should identify physical parameters with topological features. In brief, while Smale relied on a discipline that was already highly mathematized, Thom and Zeeman were quite cavalier in this respect. The mathematical techniques they used were very close to one another, but clearly distinct: Zeeman using the formal theory of elementary catastrophes, Thom a vaguer theory of generalized catastrophes, and Smale general topological methods. When trying to interpret their mathematical results, their attitude again showed divergences. Zeeman looked for differential equations that could be experimentally tested, Thom

[97] See e.g. F. Takens, "Singularities of Vector Fields," *Publications mathématiques de l'IHÉS,* 43 (1974): 47-100.

made great claims but fell back on vague philosophical arguments to back them up, and Smale more prudently refrained from drawing important conclusions from the "theorems" he had partly proved.

From their article, we may infer that, of the above three, Ruelle and Takens's modeling practice was closest to Smale's. The substratum they had to deal with was rather uncontroversial. It was given by the velocity field of fluid flows governed by the Navier-Stokes equations. But just as for Zeeman's case, the identification could not be immediate: in particular, the phase space of this velocity field was infinite-dimensional, which posed a problem as far as the techniques of Smale's dynamical systems theory were concerned. They proved the legitimacy of the reduction from an infinite number of dimensions to two dimensions, but not the more complex cases of three and four dimensions.[98] Briefly, while the substratum was straightforwardly identified, the pertinence of reducing it to a low-dimensional manifold was not obvious in mathematical terms. Here they relied on common assumptions about the onset of turbulence—Landau's degrees of freedom—without attempting to derive them from the Navier-Stokes equations.

Generally speaking, it might be worth emphasizing a truism, namely that a mathematization of natural phenomena can only succeed in the direction afforded by available mathematical tools. Leray's irregularity hypothesis for turbulence stemmed from his ability to prove, or not, existence theorems for the Navier-Stokes equations; Ruelle and Takens bold assumption was required by the modeling practices they wanted to exploit, which forced them to work in low-dimensional spaces, because only there were the tools available. Neither Leray's nor Ruelle and Takens's model for turbulence was never rigorously proved or disproved. Changes in ontological beliefs were thus crucially informed by limitations on available mathematical techniques, and to be fair, in Ruelle and Takens's cases, by experiments (Chapter VIII).

c) Strange Attractors and Genericity

Ruelle and Takens made one crucial conceptual innovation when they introduced the notion of a strange attractor, which however always remained hazy for mathematicians.[99] Due to the presence in the neighborhood of the multidimensional manifold suggested by Hopf of an open set of "strange attractors," they claimed, the quasiperiodic scenario had no chance of being observed. But again, this innovation in dynamical systems theory was mediated though the practice of using such concepts as attractors and genericity.

The core of most of Smale's, Thom's and Zeeman's practices, both in mathematics and in their modeling activity, was the identification of generic properties. Even while picking up the term from Italian algebraic geometry, Thom was aware that it was a slippery concept. "The adjective 'generic' is used in mathematics in so many senses that to restrict its usage within the framework of formal theory is probably unreasonable." Between 1969 and 1972, he acknowledged that Smale had made a welcome clarification when he restricted the use of the adjective to properties of the topological space, rather than to the points of this

[98] F. Takens and D. Ruelle, "On the Nature," section 5, 176-178.

space.[100] As Thom then emphasized, the use of genericity was an art that was difficult to make rigorous. This very point would become one of the weaker, and much criticized, parts of Zeeman's whole approach in using catastrophe theory for modeling natural phenomena.

Ruelle and Takens's reliance on genericity therefore was another place where they had to adapt not only a *concept* from dynamical systems theory to the turbulence problem, but also a *practice,* which was mediated through their close interaction with Thom's school at the IHÉS. First, they noticed that Smale's example was stable under small pertubations. From which, they concluded that "the existence of such a 'strange' attractor therefore is not a non-generic pathology."[101]

By showing that in the neighborhood of quasiperiodic motions in more than 3 dimensions a generic set of such strange attractors existed, Ruelle and Takens felt entitled to pronounce that complicated quasiperiodic motions, i.e. with more than three frequencies, could not physically occur. They redefined turbulence as aperiodic fluid motion. But one should note the tentativeness of their language when they made such a proposition.

> For $\mu>0$ we know very little about he vector field X_μ. Therefore it is reasonable to study *generic* deformations from the situation at $\mu=0$. In other words we shall ignore possibilities of deformations which are in some sense

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[99] See, e.g., M. W. Hirsch, "The Dynamical Systems Approach to Differential Equations," *Bulletin of the American Mathematical Society,* n.s., 11 (1984): 1-64, 30.
[100] R. Thom, *SSM,* 35n.1. The middle sentence referring to Smale's suggestion was absent from the manuscript. See S. Smale, "Differentiable Dynamical Systems," 748.
[101] D. Ruelle, "Méthodes d'analyse globale," 13; D. Ruelle and F. Takens, "On the Nature of Turbulence," 171. In 1971, Ruelle acknowledged that "the notion of genericity . . . is not very satisfactory when physical applications are considered." See "Strange Attractors as a Mathematical Explanation," 293n.

> exceptional. This point of view could lead to serious error if, by some law of nature which we have overlooked, X_μ happens to be in a special class with exceptional properties. It appears however that a three-dimensional viscous fluid conforms to the pattern of generic behavior which we discuss.[102]

As stated before, Ruelle and Takens's approach made no use of the particular form of the Navier-Stokes equations. "Of course something is known of this structure, and also of the experimental conditions under which turbulence develops, and a theory should be obtained in which these things are taken into account."[103] But they would not attempt it. Commenting of Ruelle and Takens's work, a hydrodynamicist, Manuel Velarde, argued that this was

> a point of *philosophy*: . . . without arguing about their relevance [*i.e.* of Navier-Stokes equations] to physics and more specifically to the study of turbulence, I ought to confess we can forget about them here.[104]

This attitude had the advantage of being potentially applicable to other cases of dissipative systems, in particular to some oscillating chemical systems (the so-called Belousov-Zhabotinski reaction), which Ruelle explored in a following article.[105] Characteristic of the modeling practices of applied topologists, it would

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[102] D. Ruelle and F. Takens, "On the Nature," 168. Italics are original; underlined words are phrases are emphasized by me. In 1971, Ruelle wrote: "The possible generic types of asymptotic behavior of . . . vector fields have not been completely classified. It seems however that, apart from attracting critical points and attracting closed orbits, the behavior described [as follows] is typical: complicated and apparently erratic with sensitive dependence on initial conditions." See "Strange Attractors as a Mathematical Explanation," 294.
[103] D. Ruelle and F. Takens, "On the Nature," 176.
[104] M. G. Velarde, "Steady-States, Limit Cycles and the Onset of Turbulence. A Few Model Calculations and Exercises," *Nonlinear Phenomena,* ed. T. Riste: 205-247, 210. My emphasis.
[105] D. Ruelle and F. Takens, "On the Nature," 176; see D. Ruelle, "Some Comments on Chemical Oscillations," *Transactions of the New York Academy of Sciences,* series II, 35 (1973): 66-71; repr. *TSAC,* 109-115. On these chemical systems, see A. T. Winfree, "Rotating Chemical Reactions," *Scientific American* (June 1974): 82-95;

exert an important attraction for the new alternative in the modeling practice of physicists as promoted by Ruelle during the following decade.

As the bottom line, Ruelle and Takens proposed a new definition of turbulence. This was how they exploited the mathematical results derived from their topologization of the problem of turbulence. The mathematical theory grounding it remained shaky. As late as 1981, an early supporter of their model had to acknowledge that:

> in spite of its mathematical character, the Ruelle-Takens approach is still mathematically speculative in the sense that it is based on some concrete conjectures about the Navier-Stokes equations, conjectures which are so far supported only by indirect evidence, not by any solid and precise analysis of the equations themselves.[106]

Indeed remaining very mathematical in its outlook, Ruelle and Takens's paper ended up relying on numerical and experimental results before it became acceptable to a majority of physicists.

In conclusion, one should note that Ruelle and Takens's model exhibited features which, given the right conditions, might be observable. They proposed nothing less than a redefinition of turbulence, based on a rigorous mathematical property of the solution: *aperiodicity*. This property would be a bit tricky to detect in a noisy experimental situation. Nonetheless, in his subsequent paper dealing with chemical oscillations, Ruelle offered a precise direction for experimental research on the onset of turbulence. In this picture, only a limited number of frequencies should

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Blair Johnson, *Nonlinearity, Irreducibility, and Emergent Properties: A Short History,* Senior Thesis (Princeton University, 1994).

appear before the power spectrum became continuous as a consequence of aperiodicity. Experimenters only had to search for this kind of continuous spectra. Finally, Ruelle and Takens's model, if correct, would exhibit an extreme "sensitiveness to initial conditions," which that should be detectable experimentally, as well as analytically from the Navier-Stokes equations.[107] This property was not shared by Landau's theory of turbulence.

> Landau's turbulence, . . . people say, is inadequate to account for experimental data. Thus the usual dogma in the physicists' community that chaos or turbulence arises either from interaction of an infinite number of degrees of freedom or from an external noise . . . is just over![108]

But this had to await confirmations based on numerical calculations and experiments in the lab (Chapter VIII).

4. DAVID RUELLE, THE 'MONSTER': THE CAREER OF A MATHEMATICAL PHYSICIST

Albeit a permanent professor at the Institut des hautes études scientifiques, David Ruelle could well have not attended Thom and Smale's seminars. He might not have perceived their interest as far as theoretical physics was concerned. As explained above, three main strains of research lay behind Ruelle and Takens's paper: the quasiperiodic picture of turbulence (Hopf and Landau), dynamical systems theory (Smale and Thom), and mathematical physics. This section addresses the third of these strains by focusing on Ruelle's earlier career. A closer look at his mathematical

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[106] O. E. Lanford, "Strange Attractors and Turbulence" in *Hydrodynamic Instabilities and the Transition to Turbulence,* ed. H. L. Swinney and J. P. Gollub (Berlin: Springer, 1981): 7-24, 7.
[107] This phrase is first used by D. Ruelle in "Some Comments," 70; *TSAC,* 114.
[108] M. G. Velarde, "Steady States," 208.

physics, and especially the circumstances which led to his hiring at the IHÉS in 1963, clarify why he was in a position both to see the relevance of Thom's and Smale's ideas and to adapt them for the specific concerns of theoretical physicists.

a) Still Another Mathematical Physicist?

It has been said that mathematical physics, after a glorious time in early twentieth-century, notably with Poincaré, had disappeared, leaving theoretical physics in its place. This is incorrect. Mathematical physics enjoyed rather happy years in the 1950s and 1960s. In particular, this period saw, under the principal impulse of Arthur S. Wightman, a considerable development of constructive or axiomatic quantum field theory, using Laurent Schwartz's distributions. This highly mathematical branch of theoretical physics was also importantly developed in Europe, especially around Res Jost, professor at the *Eidgenössische Technische Hochschule* (ETH) in Zürich.[109]

It was from this domain that David Ruelle came. Born in 1935, he received a doctorate from the Université libre of Brussels, but mainly working under the direction of Jost. Ruelle then spent two years at Zürich, before leaving for the Institute for Advanced Study (IAS) at Princeton for another two years (1962-64). During his stay in the US, he started working on statistical mechanics, trying to take advantage of

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[109] In 1982, Ruelle expressed his admiration for his mentors as such: "The relation between physics—real physics—and mathematics—real mathematics—has not been as easy one in the last thirty years. It took vision to see that this relation is possible and fruitful now. . . . Res Jost in Zürich, Freeman Dyson and Arthur Wightman in Princeton had that vision, and made many other share it." D. Ruelle, "Large Volume Limit of the Distribution of Characteristic Exponents in Turbulence," *Communications in Mathematical Physics,* 87 (1982): 287-302, 287; *TSAC,* 295-310, 295.

the sophisticated mathematical techniques used in axiomatic quantum field theory in order to establish rigorously some general results.

But Jost and Wightman were among the physicists who visited the IHÉS soon after its foundation, in 1959. In October 1961, a handwritten note, added to an invitation list discussed during a Scientific Committee of the IHÉS (which included Montel, Oppenheimer, Motchane, and the future permanent professor of theoretical physics Louis Michel), bore Ruelle's name. Following a suggestion of Jost's, it was proposed that he should be invited.[110] But Ruelle did not come then, no doubt preferring to go to Princeton.

On his more or less yearly pilgrimage to the United States, during which Léon Motchane never failed to go to Princeton to visit the director of the IAS Robert Oppenheimer, who was among the founding members of the IHÉS. During his trip in 1962, Motchane discussed with Ruelle the possibility of attracting him to Bures-sur-Yvette.[111]

His colleagues' opinion concerning Ruelle were laudatory: Wightman, C. N. Yang and Jost (who wanted to keep him at Zürich), all strongly recommended him. Motchane thus envisioned the creation of a permanent professorship for him at the IHÉS. He was young and dynamic; he knew well American and European researchers in his field; he might be just what was needed for developing the IHÉS. The only shadow, Motchane explained, was that he was a bit too mathematical in his approach. "Obviously, he is a theoretician of a mathematical type, but very recently, he did some

[110] *Comité scientifique* (17/10/61). Arch. IHÉS.
[111] Lettre de Léon Motchane à David Ruelle, à Princeton (2/5/63). Arch. IHÉS.

work on statistical mechanics, which indicates a great variety of interests."[112] To Oppenheimer, Motchane wrote:"[Ruelle's] somewhat formal, mathematical orientation obviously fits with the atmosphere reigning here, but one could wonder whether this specialization of our Physics Section in a single direction is a good thing."[113]

Two points are important: Ruelle had just started to work in a field that was new to him, and he did mathematical physics. We shall see below that these two points probably are not without relation to one another. At this time, foreign physicists who belonged to the Scientific Committee of the IHÉS distrusted the increasing specialization of the Institute solely in mathematical physics. When in 1965 the opportunity presented itself to hire Vladimir Glaser as a permanent professor, Oppenheimer voiced his concerns: "the faculty of your Institute should have at least one, and preferably more than one, physicist concerned with the actualities of present experimental exploration of fundamental physical problems." Motchane took notice that according to Oppenheimer, Peierls and Weisskopf, "in theoretical physics, the physics should be forgotten."[114]

Moreover, to hire Ruelle at that time—he was 28 years old—was a risk. Jost, his ex-mentor testified to this ten years later: "David Ruelle was very young, and working in a new field, and above everything with tools of such penetrating rigour that he was once likened by a famous physicist not to a fellow theoretician but to a

[112] Lettre de Léon Motchane à Victor Weisskopf (24/3/63). Arch. IHÉS.
[113] Lettre de Léon Motchane à Robert Oppenheimer (27/7/63). Arch. IHÉS.

monster [*sic*]."[115] English theoretical physicist Rudolph Peierls, just coopted in 1963 into the Scientific Committee of the IHÉS, and who hardly knew mathematical physics, stated:

> I am certainly familiar with the reputation of Dr. Ruelle, though I have not met him personally and have not had an occasion to study his paper in any details. . . . [H]is papers are not very easy to read. . . . Ruelle is concerned with the rather abstract and formal side of [theoretical physics] and this applies also to some extent to Lehmann and to Michel [the other two physics permanent professors].[116]

But Ruelle strongly impressed those among his elders who fathomed his work. Weisskopf "very strongly" commended him to Motchane. "To my mind, Ruelle is just the right man for you and you should go all out to get him."[117] Res Jost pressed Motchane to do all in his powers to bring Ruelle back to Europe, the more so since Ruelle had caught Princeton's interest.[118] When Motchane asked for the advice of the director of the IAS, Oppenheimer's secretary replied that he and his colleagues thought "highly enough of Ruelle to be considering him for a professorship here."[119] This could not fail to alarm Motchane: "I want at all price to avoid all that could be taken for competition between our Institutes."[120]

Finally, although Oppenheimer and Ruelle had inded discussed the possibility for the latter to remain at Princeton, Ruelle finally decided to accept Motchane's offer

114 Lettres de Léon Motchane à Robert Oppenheimer (6/5/65); de Robert Oppenheimer à Léon Motchane (20/5/1965); de Léon Motchane à Robert Oppenheimer (15/3/65). Arch. IHÉS.
115 Lettre de Res Jost à Nicolaas Kuiper (18/1/74). Arch. IHÉS.
116 Lettre de Rudolph Peierls à Léon Motchane (6/7/63); de Léon Motchane à Rudolph Peierls (26/6/63). Arch. IHÉS.
117 Lettre de Victor Weisskopf à Léon Motchane (27/6/63). Arch. IHÉS.
118 Lettre de Res Jost à Léon Motchane (20/6/63). Arch. IHÉS.
119 Lettre de Verna Hobson à Léon Motchane (9/7/63). Arch. IHÉS.

in September 1963. He joined the IHÉS in October 1964. In accepting his offer, Ruelle warned Motchane that although he was working in a new field, statistical mechanics, "I maintain a great interest in field theory to which I intend to come back." [121] But he never did.

b) Ruelle, Statistical Physics, and the Military

As soon as he arrived at the IHÉS, Ruelle wrote a research proposal for the *Direction des Recherches et Moyens d'Études* (DRME), the Gaullian organization which sponsored most defense-related research in France. Following the defection of several important subscribers, the IHÉS faced major financial hardships which jeopardized its very survival.[122] The Ruelle contract would have furnished a non-negligible amount of 350,000 F for two years, or about 10% of the budget of the Institut, allowing Motchane to breath a bit easier.[123] The proposal was titled "Convergence Theorems and the Existence of Phase Transitions in Statistical Mechanics." We thus see that Ruelle was well engaged in statistical mechanics. But his approach was special. Indeed, as Motchane wrote:

> by modern analytic techniques (functional spaces, Banach spaces), he establishes convergence theorems for different thermodynamic functions. . . .

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[120] Lettre de Léon Motchane à Robert Oppenheimer (27/7/63).
[121] Lettres de Léon Motchane à David Ruelle (2/5/63); de David Ruelle à Léon Motchane (15/5/63); de Léon Motchane à David Ruelle (10/9/63), de David Ruelle à Léon Motchane (16/9/63). Arch. IHÉS.
[122] Notes de séances manuscrites de l'Assemblée générale (23/9/64); projet de lettre (non-envoyée) de Léon Motchane à Pierre Chatenet (18/12/64); lettre de Léon Motchane à Frank Bowles (5/2/65). Arch. IHÉS. See Chapter IV above.
[123] Rapport du Conseil d'administration à l'Assemblée générale (23/9/64); lettre de Léon Motchane à Lucien Malavard (17/11/64); de Léon Motchane à Pierre Aigrain (8/12/64). Arch. IHÉS.

> An original aspect of RUELLE's approach on these problems of statistical mechanics was to show the analogy of his formalism with the one of axiomatic quantum field theory.[124]

Ultimately, albeit initially accepted by Pierre Aigrain who then was scientific advisor for the DRME, Ruelle's research proposal was rejected. It was rejected because it did not fit the normal work sponsored by the DRME ["*la ligne de travail normale de la DRME*"].[125] The IHÉS intended to do "rigorously pure research sponsored in a perfectly disinterested spirit [*esprit de mécénat purement désintéressé*]." Ruelle and Michel were ready to provide arguments to justify military interest in these researches, but, ultimately—especially since discussions with the Prime Minister's Cabinet about a direct sponsorship from the Government were turning out favorably—the directorate of the IHÉS decided to "show extreme intransigence about the principle."[126]

c) The Structure of Physical Theories: The Bourbakization of Physics?

Ruelle's work in statistical mechanics would nevertheless prove truly exceptional. According to Jost, "David Ruelle for the first time—one hundred years after Ludwig Boltzmann and about 70 years after Willard Gibbs—finally laid down the mathematical foundations of statistical mechanics."[127] In 1968, Ruelle would collect his results in a book called *Statistical Physics: Rigorous Results.* In the introduction,

[124] Lettre de Léon Motchane à Lucien Malavard (17/11/64). Arch. IHÉS.
[125] Lettre de Annie Rolland à Léon Motchane (19/1/65). Cf. également Note manuscrite de Annie Rolland suite à un coup de téléphone de André Grandpierre (12/1/65), et lettre du Général Lavaud au Général René Cogny (15/5/62): La DRME "s'interdit toute action qui pourrait l'assimiler à un mécénat." Arch. IHÉS.
[126] Note manuscrite de Annie Rolland suite à un coup de téléphone de André Grandpierre (12/1/65). Arch. IHÉS.

Ruelle clearly expressed his deep belief in what a rigorous mathematical approach brought to the theoretical study of physics. It was a "rewarding experience," he wrote. "[M]athematical analysis gives to the physical world a new structure and meaning. The knowledge of this structure and meaning constitutes an understanding of the 'nature of things' as deep as we can hope to be." Several domains of physics were then (1968) very interesting and open to "insightful" mathematical treatment, Ruelle contended, even while repudiating his origins: "An exception to this statement may be relativistic quantum mechanics, largely because of 'overgrazing,' but there are also vast areas of *terra incognita.*" He expressed his admiration for the Bourbakist work.

> The progress of mathematical physics could be significantly promoted, in the author's opinion, by the availability of results of important mathematical theories in concise form and without proofs, in the spirit of Bourbaki's 'Fascicules de Résultats'.[128]

These reflections offer us a means to grasp some of the reasons why Ruelle, other than because of his great personal talent, could successfully jump laterally from one field to another. "I like to change rather often my centers of interest. But one can find in all my works a constant feature: the striving for mathematical rigor in the exposition of physical theories."[129]

In the late 1960s rigor often served as a synonym for a Bourbakist attitude. In other words, Ruelle's emphasis on rigor should be interpreted as an indication that a structural approach informed his modeling practice on physics. We may well here

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127 Lettre de Res Jost à Nicolaas Kuiper (18/1/74). Arch. IHÉS.

128 David Ruelle, *Statistical Physics: Rigorous Results* (New York: W. A. Benjamin, 1969), vii-viii.

129 "La nature de la turbulence. Une interview de David Ruelle," *CNRS-Info,* special issue "L'École françaises du chaos," n.d [1989], 12-13.

recall Motchane's contemporary pronouncements (Chapter VI), when he contended that the kinship of structures in extremely diverse domains allowed the mathematician, without becoming an expert, to grasp the essential features of the scientific domains he invested.[130] As mentioned in Chapter IV, this structuralist attitude could be seen as a fundamental character of the ideology pushed forward by the Institut des hautes études scientifiques, both in its structure and function.

The Bourbakist reordering of mathematics, and above all the emphasis put on the concept of a mathematical structure, much more than the actual concept as such, was supposed to allow the mathematician or the mathematical physicist, without becoming an expert in a foreign field, to grasp some of its deep structures, its essence. In the best cases, this should set the ground for a fruitful dialogue with the experts. In Ruelle's case, as opposed to most of the applications of catastrophe theory suggested by Thom and Zeeman, this dialogue took place. Even for physicists, Bourbaki's structures were thus an important cultural connector. I will return to this issue below

5. A LONG-TERM DISCIPLINARY SURVEY OF THE TURBULENCE PROBLEM

From the point of view of the historian of science, turbulence represents a challenge, and this might be the reason why secondary literature has remained extremely scarce on this topic.[131] Because of this scarcity of secondary sources, we need very cursorily

[130] Léon Motchane, "Éléments de Rapports scientifique [1967] à l'Assemblée [générale du 8/5/68]," 4. Arch. IHÉS.

[131] I have only been able to find one somewhat recent monograph devoted to the history fluid mechanics, traditionally included in histories of rational mechanics: G. A Tokaty, *History and Philosophy of Fluidmechanics.* Significantly, this book was

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to trace back the evolution of modeling practices at play in the history of fluid mechanics, and especially when trying to relate observed turbulent phenomena with the Navier-Stokes equations, which was called the "turbulence problem."[132]

Manifestly, progress in the history of turbulence has been nonlinear. The methods that have been attempted in order to relate the observed phenomenon of turbulence with the Navier-Stokes equations have been so diverse that conceptual unity has been impossible to achieve. New approaches and new means of computation regularly opened new avenues of research. Methods for dealing with the turbulence problem involved the theories of partial differential and integral equations, functional analysis, qualitative dynamics, statistical theories, energy methods, ergodic theory, numerical computations, empirical identification of coherent structures, finite element analysis, etc. Regularly, these methods proved disappointing for those in search of general theories of turbulence.[133]

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written by a practicing scientist. However, see also Marcel Nordon, *Histoire de l'hydraulique. 2. L'eau démontrée* (Paris: Masson, 1992).

[132] A noteworthy exception to the lack of attention historians have manifested towards fluid mechanics in the twentieth century is provided by G. Battimelli, "The Mathematician and the Engineer: Statistical Theories of Turbulence in the 20's," *Rivista di storia della scienza,* 1 (1984): 73-94.

[133] Some of the most historically-minded articles used for this section are the following: P. Appel, et al., "Développements concernant l'hydrodynamique"; J. L. Synge, "Hydrodynamical Stability," *Semicentennial Addresses of the American Mathematical Society* (New York: AMS, 1938): 227-269; J. von Neumann, "Recent Theories of Turbulence," unpubl. report to the ONR (1949); repr. *Collected Works,* 6, ed. A. H. Taub (Oxford: Pergamon, 1963): 437-472; S. Goldstein, "Fluid Mechanics in the First Half of This Century," *Annual Review of Fluid Mechanics,* 1 (1969): 1-28. H. W. Liepmann, "The Rise and Fall of Ideas in Turbulence," *American Scientist,* 67 (1979): 221-228; and G. T. Chapman and M. Tobak, "Observations, Theoretical Ideas, and Modeling of Turbulent Flows—Past, Present, and Future," *Theoretical Approaches to Turbulence,* ed. D. L. Dwoyer, et al. (New York: Springer, 1985): 19-49.

In late nineteenth century hydrodynamics was perceived as being part of rational mechanics and closely linked with applied mathematics. It then opened to a wide array of concerns, attracting the attention of physicists and engineers especially. Practical problems raised by the development of air flight only furthered this trend. More and more, fluid mechanics became a subject studied by many communities with diverging goals. Unifying these different branches, embodying distinct modeling practices, posed a difficult conceptual problem which confronted several generations of students of turbulence.

In 1969, summing up a half-century of work on fluid mechanics, Sydney Goldstein, from Harvard University, could not help noticing the dispersion of his field:

> Fluid mechanics is a part of applied mathematics, of physics, of many branches of engineering, certainly civil, mechanical, and aeronautical engineering, and of naval architecture, and geophysics, with astrophysics and biological and physiological fluid dynamics to be added.[134]

Similarly, George K. Batchelor recalled that when founding the *Journal of Fluid Mechanics* in 1956, he felt that there was "a three-way split of literature on fluid mechanics" among theoretical and mathematical papers, experimental and observational papers, and those dealing with application. In 1981, he contended that this journal, meant to bridge the gap, had only been partly successful in doing so.[135]

Given the variegated nature of the field, what is presented here can only serve as a rough—and very partial—survey of the turbulence problem. My goal is to

[134] S. Goldstein, "Fluid Mechanics," 4.
[135] G. K. Batchelor, "Preoccupation of a Journal Editor," *Journal of Fluid Mechanics,* 106 (1981): 1-25.

understand the conceptual setting in which the model suggested by Ruelle and Takens could insert itself and underscore the changes in modeling practices that it represented.

a) Fluids Are Described by the Navier-Stokes Equations

In order to evaluate the radical changes in modeling practices represented by the Ruelle-Takens model, with its skepticism towards the ontological status of the Navier-Stokes equations, how the turbulence problem constantly cast doubts on this fundamental law is briefly reviewed. The aim is twofold. First, in a somewhat cavalier fashion, history serves as an introduction of essential concepts of fluid mechanics, such as the Navier-Stokes equations and Reynolds numbers. Second, and most importantly, a sense of the always shifting nature of the modeling practices in hydrodynamics is briefly conveyed. The changing ontology of fluids will be seen to go hand in hand with the modeling practices available to the specialists.

(i) Euler's Equations

Around 1755, Leonhard Euler (1707-1783) contended:

> However sublime are the researches on fluids which we owe to the Messrs. Bernouilli, Clairaut, and d'Alembert, they flow so naturally from my two general formulae that one cannot sufficiently admire this accord of their profound meditations with the simplicity of the principles from which I have drawn my two equations, and to which I was led immediately by the axioms of mechanics.[136]

[136] L. Euler, "Principes généraux de l'état d'équilibre des fluides"; "Principes généraux du mouvement des fluides"; and "Continuation des recherches sur la théorie des mouvements des fluides," *Opera Omnia,* 2nd ser., vol. XII (Lausanne: Orell Füssli Turici, 1954): 2-132; first published in 1757. The above quote was translated by A. P.

Euler is generally taken to have been the first to analyze the dynamics of fluids by applying Newton's second law to the study of flows. He wrote down a simple set of differential equations that admittedly governed the flow of a fluid in terms of its density and velocity field, an impressive reduction of prior disparate results to the laws of mechanics.

"By this discovery," Joseph Lagrange later wrote, "*all fluid mechanics was reduced to a single point of analysis,* and if the equations involved were integrable, one could determine completely, in all cases, the motion of a fluid moved by any force."[137] Following Lagrange, one might be inclined to believe that the problem was solved. Unfortunately, Euler's equations were nonlinear, and indeed have turned out to be quite difficult to solve analytically up to this very day. Lagrange was well aware of these difficulties as he immediately added: "unfortunately [Euler's equations] are so rebellious that up until now they have only been solved for very limited cases." Similarly, Euler had already been forced to admit: "If we cannot achieve complete knowledge of fluid motions, it is not to mechanics and to the insufficiency of the known principles that we should ascribe the cause, but analysis itself here fails us."[138]

In retrospect, this difficulty in using Euler's equations to account for observations stemmed from two sources: the intractability of the nonlinear equations

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Yonschkevitch, *s.v.* Euler, *Dictionary of Scientific Biography,* ed. C. C. Gillespie, 4 (New York: Charles Scribner's Sons, 1978), 481.

[137] J. Lagrange, *Mécanique analytique* (Paris, 1788; repr. Paris: Jacques Gabay, 1989), sec. X, 436. My translation and emphasis.

[138] Quoted in P. Costabel, "La mécanique des milieux continus," *Histoire générale des sciences,* ed. R. Taton, tome III, vol. I, 2nd ed. (Paris: PUF, 1981), 99.

and the neglect of an important characteristics of real fluids. As G. A. Tokaty, one of the rare historians of fluid mechanics, remarked in series of mixed metaphors:

> Leonhard Euler was not a contributor to, but the founder of, Fluidmechanics [*sic*], its mathematical architect, its great river. . . . But the beautiful trousers he tailored had no buttons, they failed to include viscosity. The buttons were provided by Claude Navier.[139]

(ii) Navier and the Molecular Hypothesis

Indeed, Euler's assumptions, basically equivalent to neglecting friction inside the fluid, eventually turned out to be too crude to provide a realistic description of most liquids. In 1822, Claude Louis Navier (1785-1836) modified Euler's equations by introducing a dissipative effect, later called *viscosity* (denoted ν below).[140]

> The considerable or total differences that, in some cases, the natural effects present with respect to the results of known theories [Euler's], show the necessity . . . of taking into account certain molecular actions that principally manifest themselves in phenomena of motion.[141]

Navier thus hypothesized that a force existed between fluid molecules that was proportional to their velocity relative to one another. "In a fluid in motion, two molecules approaching one another repel one another more strongly, and . . . two molecules that get further apart repel one another less strongly than they would if their actual distance remained constant."[142]

From this basis, Navier derived the laws of motion for the particles of an incompressible fluid (i.e. with constant density), which he expressed as a set of three

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[139] G. A. Tokaty, *A History of Fluidmechanics,* 73 and 88.
[140] Claude Louis Navier, "Mémoire sur les lois du mouvement des fluides," *Mémoires de l'Académie des sciences,* 6 (1823): 389-440.
[141] C. Navier, "Mémoire sur les lois du mouvement," 389.
[142] C. Navier, "Mémoire sur les lois du mouvement," 391.

differential equations. In a modern vectorial form, these can be reduced, for incompressible fluids, to the following single expression:

$$\frac{\partial \mathbf{v}}{\partial t} + (\mathbf{v} \cdot \nabla)\mathbf{v} = -\frac{1}{\rho}\,\mathrm{grad}\, p + \nu\, \Delta\mathbf{v};$$

where $\mathbf{v}=(v_x,v_y,v_z)$ represents the three spatial directions of the velocity of fluid particles at each point; $p(x,y,z)$ is pressure, and ρ is density, assumed constant. Together with the continuity equation (div $\mathbf{v} = 0$, in the case of an incompressible fluid), found by Euler and unmodified by Navier, the description would become widely known as the Navier-Stokes equations. Since then, they have been the basis of every theoretical description of fluids.

A significant difference existed between the method Navier used to derive this equation and Euler's analysis. In conformity with the principles of the Laplacian school of physics, Navier considered the forces acting on a single molecule in the fluid and derived the equations of motion for these molecules. On the other hand, Euler made no hypothesis as to the composition of the fluid, and based his consideration solely on the average speed in small elements.[143] The certainty of Navier's equations therefore hinged on the acceptance a particular hypothesis concerning the nature of intermolecular forces. In the late 1820s, a fierce debate, concerning the then closely related topic of elasticity in solids, rooted Navier against Siméon Denis Poisson (1781-1840), a close follower of Laplace, who had also derived

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[143] R. Fox, "The Rise and Fall of Laplacian Physics," *Historical Studies of Physical Sciences,* 4 (1974): 89-136. See also C. C. Gillespie, R. Fox, and I. Grattan-Guiness, *s.v.* Laplace, *Dictionary of Scientific Biography,* 15, Suppl. 1 (New York: Charles Scribner's Sons, 1978): 273-403; and A. Dahan Dalmedico, *Mathématisations.*

a similar set of equations starting from different hypotheses.[144] Navier's proposal was indeed somewhat heterodox for Poisson for two reasons. What mattered for Navier was not the exact form of the forces between molecules, but rather those arising when equilibrium was disturbed, assuming that they canceled each other at rest. Furthermore, in Navier's scheme, the force between molecules depended on their relative velocity, rather than their position.

This debate highlights the fact that the assumptions made by Navier involved a certain degree of arbitrariness. "Poisson never seemed content with purely mathematical models as description of the underlying physics. He wanted to provide explanations, not descriptions."[145] For Navier, molecules hardly represented more than material points—centers of attractive and repulsive forces suitable for calculations—which allowed him to bypass some of the problems plaguing the Laplacian school. Indeed, by insisting on always having attractive actions between molecules, the progress of this school's program was hindered by infinite densities. Therefore, Navier's assumptions were already one step away from a specific reliance

[144] C. Navier, "Note relative à l'article intitulé: 'Mémoire sur l'équilibre et le mouvement des corps élastique', page 337 du tome précédent," *Annales de chimie et de physique,* 2e sér., 38 (1828): 304-314; S.-D. Poisson, "Réponse à une note de M. Navier insérée dans le dernier cahier de ce journal," *Ibid.,* 435-440; C. Navier, "Remarques sur l'article de M. Poisson, insérée dans le cahier d'août, page 435," *Ibid.,* 39 (1828): 145-151; S.-D. Poisson, "Lettre à M. Arago," *Ibid.,* 204-211; and C. Navier, "Lettre à M. Arago,"*Ibid.,* 40 (1829): 99-107. C. Navier, "Postface au débat avec Poisson," *Bulletin des sciences mathématiques de Férussac,* 11 (1829): 243-253. On the Navier-Poisson debate, see A. Dahan Dalmedico, *Mathématisations,* 266-273.
[145] D. H. Arnold, "Poisson and mechanics," in *Siméon-Denis Poisson et la science de son temps,* ed. M. Métivier, P. Costabel, and P. Dugac (Palaiseau: École polytechnique, 1981): 23-37, 35.

on the nature of such forces, since only variations away from equilibrium mattered, and not the equilibrium itself.

(iii) Stokes: The Robustness of Partial Differential Equations

In fact, English physicist George Gabriel Stokes eventually published in 1845 an article showing that molecular assumptions were unnecessary in order to derive Navier's equations.[146] Already on November 27, 1843, Adhémar Barré de Saint-Venant (1797-1886) had also read a note at the Académie des sciences in which he derived Navier's equations "without making suppositions about the magnitude of attractions and repulsions between molecules as a function of either their distances or their relative speed."[147] With this step, the Navier-Stokes equations became more reliable than any specific assumption concerning molecular forces. Indeed the very molecular hypothesis could be done away with.[148] In a manner that recalls Fourier's treatment of heat, the fundamental tool for the description of fluids became the differential equation, rather than specific suppositions about ultimate constituents of fluids and interactions between them.

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[146] G. G. Stokes, "On the Theory of the Internal Friction of Fluids in Motion, and of the Equilibrium and Motion of Elastic Solids," *Transactions of the Cambridge Philosophical Society,* 8 (1845): 245; repr. *Mathematical and Physical Papers,* 1 (Cambridge, 1880; New York: Johnson Reprints, 1966): 75-129. See A. Dahan Dalmedico, *Mathématisations,* 291-294, 429-430; and C. Smith and M. N. Wise, *Energy and Empire,* chap. 4.

[147] "sans faire de suppositions sur la grandeur des attractions et répulsions des molécules en fonction, soit de leurs distances, soit de leurs vitesses relatives (1240)." A. B. de Saint Venant, "Note à joindre au Mémoire sur la dynamique des fluides, présenté le 14 avril 1834," *CRAS,* 17 (1843): 1240-1243.

[148] It was G. G. Stokes who noted that Saint-Venant's "method does not require the consideration of ultimate molecules at all." G. G. Stokes, "Report on Recent

However, neither was Stokes's treatment free from assumptions. Stokes's was a geometrical derivation that assumed that Navier's equations needed, in the words of Osborne Reynolds, "to involve no other assumption than that the stresses, other than that of pressure uniform in all directions, are linear functions of the rates of distortion."[149] In other words, Stokes made an hypothesis of a "daring simplicity" to the effect that internal pressures were directly proportional to the velocities, which could only be valid for small velocities. "Hence although [the Navier Stokes equations] may apply with great accuracy to cases of slow motion, we have no assurance of their validity in other cases."[150] Therefore, the possibility existed that his derivation of the Navier-Stokes equations might not be valid for 'turbulent' motions, which admittedly involved large velocities for the fluid.

The name of the game then became solving the Navier-Stokes equations, together with the continuity equation (div v = 0) and boundary and initial conditions—solutions which would provide the exact time evolution of fluid flows. However, the Navier-Stokes equations maintained the nonlinear character of Euler's

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Researches in Hydrodynamics," *Reports for the British Association for the Advancement of Science* (1846), Part I; repr. *Papers,* 1: 157-187, 184.

[149] O. Reynolds, "On the Dynamical Theory of Incompressible Viscous Fluids and the Determination of the Criterion," *Philosophical Transactions of the Royal Society,* A186 (1894): 123-164; repr. *Papers on Mechanical and Physical Subjects,* 2 (Cambridge: Cambridge University Press, 1901): 535-577. See G. G. Stokes, "On the Theories," 88ff.

[150] H. Lamb, *A Treatise on the Mathematical Theory of the Motion of Fluids* (Cambridge: Cambridge University Press, 1879), 221. On later grounds to believe this hypothesis, see H. Lamb, *Hydrodynamics,* 575. J. L. Synge emphasized the "daring simplicity" of Stokes's assumption in "Hydrodynamical Stability," 231.

equations, and even more than the latter, they turned out to be extremely unmanageable mathematically, except for a few simple cases.[151]

b) The Turbulence Problem: From Hydraulics to Physics

Turbulence was *not* simple, not from the standpoint of the Navier-Stokes equations. To show their distress in face of this formidable problem, fluid dynamicists liked to cite Horace Lamb's comparison, the epigram of this chapter, between quantum electrodynamics and the turbulent motion of fluids. "About the former I am rather optimistic," Lamb remarked.[152] And he was right. While QED was solved by Feynman, Schwinger, Dyson and Tomonaga in the 1950s, while particle physics and quantum field theory witnessed impressive advances in the following decades, barely a dent was made in the problem of turbulence, despite considerable efforts. "Hydrodynamics," Ruelle contended in 1981, has "remained somewhat in the backwaters of the scientific storm of this century."[153]

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[151] It was Arnold Sommerfeld who, at the Rome International Congress of Mathematicians in 1908, insisted on the nonlinarity of the Navier-Stokes equation as being the source of the difficulty in interpreting theoretically turbulent phenomena: "Ein Betrag zur hydrodynamischen Erklärung der turbulenten Flüssigkeitsbewegungen," *Atti del quarto congresso internazionale dei matematici in Roma 1908,* ed. G. Castelnuovo, 3 (Rome, Accademia dei lincei, 1909): 116-124, 118.
[152] Quoted by S. Goldstein, "Fluid Mechanics," 23. Also in Paul C. Martin, "The Onset of Turbulence: A Review of Recent Developments in Theory and Experiment," *Statistical Physics: Proceedings of the International Conference [Budapest, August 1975],* ed. L. Pál and P. Szépfalusy (Amsterdam: North-Holland, 1976): 69-96.
[153] D. Ruelle, "Differentiable Dynamical Systems and the Problem of Turbulence," *Bulletin of the American Mathematical Society,* 5 (1981): 29-42, 30; repr. *Proceedings of Symposia in Pure Mathematics,* 39(2) (1983): 141-154; *TSAC,* 233-246, 234.

(i) Early Studies of Turbulence: Poiseuille, Darcy, Boussinesq, etc.

The first sign that the fundamental equations written down by Navier, Saint-Venant, and Stokes did not resolve the problem came from practical hydraulic works and experiments. George Stokes had derived his equations in order to deal with the paradigmatic case of the resistance that a solid body opposed to the flow of liquids. There also difficulties linked with 'turbulence' arose, but careful experimental studies of problems of this kind were harder to perform.[154] As for pipes, since the work of Navier in 1838, the accepted law stated that the square of the resistance R of water flowing in pipes was proportional to the mean velocity U ($R^2 \propto U$).[155]

An ex-Polytechnician and a medical doctor teaching physics at the Faculté de Médecine of the Sorbonne, Jean-Louis Poiseuille (1799-1869) undertook, in the 1840s, to test experimentally this law of Navier's for reasons that had to do with the study of blood flows in capillary veins. He had several very narrow glass tubes built (of diameters from .013 mm to .65 mm) and studied the resistance they opposed to water flows. These experiments appeared to challenge Navier's law, since they showed that resistance was directly proportional to velocity ($R \propto U$).[156] Poiseuille thought that his experiment cast doubts on Navier's whole approach.

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[154] Experiments dealing with the resistance opposed to ship motions always were plentiful. In the interwar period, many likewise investigated this problem in relation with wings of airplanes.
[155] C. Navier, *Leçons à l'École des ponts et chaussées* (Paris, 1838), no. 108.
[156] J.-L. Poiseuille, "Recherches expérimentales sur les mouvements des liquides dans les tubes de très petits diamètres," *CRAS,* 11 (1840): 961-967; 1041-1048; 12 (1841): 112-115; ""Recherches expérimentales sur le mouvement des liquides dans les tubes de très-petits diamètres," *Mémoires des savants étrangers,* 9 (1846); V. Regnault, et

To meet this challenge, several propositions were introduced to modify the Navier-Stokes equations in order to find the correct fundamental law.[157] But by the 1860s, Émile Mathieu and Joseph Boussinesq proved that Navier's resistance law was "*de facto* independent from all parts of his memoir relative to internal friction," i.e. from the Navier-Stokes equations.[158] For Saint-Venant, Poiseuille's experiments became a severe restriction imposed on the equations of hydrodynamics: there was "no need to adopt formulae of so strange a complication."[159] In fact, Poiseuille's experiments were turned into a crucial confirmation of Stokes linear hypothesis and the Navier-Stokes equations for a wide range of cases and were used to determine the viscosity of water as a function of temperature.[160] Indeed, the Navier-Stokes equations had successfully stood up to the challenge.

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al., "Rapport sur un Mémoire de M. le docteur Poiseuille, etc.," *CRAS,* 15 (1842): 1167-1186.

[157] See A. B. de Saint-Venant, "Sur l'hydrodynamique des cours d'eau," *CRAS,* 74 (1872): 570-577; 649-657; 693-701; and 770-774, esp. pp. 655-657. Reports of the Academy of Sciences were published in Combes, et al., "Rapport sur un Mémoire de M. Maurice Lévy, relatif à l'hydrodynamique des liquides homogènes, particulièrement à leur écoulement rectiligne et permanent," *CRAS,* 68 (1869): 582-588; and A. B. Saint-Venant, Delaunay, and J. Bertrand, "Rapport sur un Mémoire de M. Kleitz, intitulé Études sur les forces moléculaires dans les liquides en mouvement, et applications à l'hydrodynamique," *CRAS,* 74 (1872): 426-438, 430. Kleitz's memoir was not recommend for publication.

[158] A. B. de Saint-Venant, "Sur l'hydrodynamique des cours d'eau," 577. É. Mathieu, "Sur le mouvement des liquides dans les tubes de très-petits diamètres," *CRAS,* 57 (1863): 320-324; J. Boussinesq, "Mémoire sur l'influence des frottements dans les mouvements réguliers des fluides," *Journal de mathématiques pures et appliquées,* 13 (1868): 377-424; and Serret, O. Bonnet, and A. B. de Saint-Venant, "Rapport sur un Mémoire de M. Boussinesq, présenté le 27 juillet 1868 et relatif à l'influence des frottements dans les mouvements des fluides," *CRAS,* 67 (1868): 287-289.

[159] A. B. de Saint-Venant, "Sur l'hydrodynamique des cours d'eau," 697.

[160] "On voit donc que les expériences de M. Poiseuille démontre l'exactitude des formules de Navier." J. Boussinesq, "Théorie des phénomènes constatés par les expériences de M. Poiseuille," *CRAS,* 65 (1867): 46-48, 48.

Of course, practical incentive were never lacking for a careful study of water flows in pipes. "There are few branches of physico-mathematical sciences [which are] more important as far as constant application to practic[al matters] than that dealing with the motion of water in pipes."[161] In 1858, a long article was published by Henri Darcy, an *inspecteur général des Ponts et chaussées,* in which he studied the influence of the diameter of water pipes extensively.[162] Darcy also questioned the validity of the assumption that fluid flows remained parallel to the pipes, yet an exact solution of the Navier-Stokes. "*Ruptures,* eddies [*tourbillonnements*] and other complicated or oblique motions, which must greatly influence the intensity of friction, arise and develop more in large sections."[163] Darcy fell back on Poiseuille's results only when he supposed that the diameter of his pipes was small. For the first time, a clear distinction between two types of fluid motion was emphasized.[164]

In 1867-1877, Valentin Joseph Boussinesq (1842-1929) tackled the difficult problem of the flow of water in large pipes. A specialist of this "*intimate* mechanics . . . which is that of actual things of the earthly world, and whose beautiful and noble study is, by and large, more arduous than that of the planetary world," Boussinesq went further than anyone before him in the "beautiful and difficult science" of

[161] D'Aubuisson to F. Arago (1 October 1829); quoted in H. Darcy, "Recherches expérimentales relatives au mouvement de l'eau dans les tuyaux," *Mémoires présentés par divers savants à l'Académie des sciences de l'Institut impérial de France,* (2) 15 (1858): 141-403, 144-145.
[162] H. Darcy, "Recherches expérimentales."
[163] H. Darcy, "Recherches expérimentales," 322.
[164] H. Darcy, "Recherches expérimentales," 215 and 354. "L'opinion que le 'tourbillon' est la base du changement dans la loi de la résistance a déjà été formulée par [Saint-Venant in 1851]; elle a été admise par J. Boussinesq et G. G. Stokes. H. Darcy signala

hydrodynamics.[165] In particular, he carefully studied the experimental results of Poiseuille and Darcy, and tried in 1868 to derive the distinction between the two types of motion in a theoretical fashion, starting from the Navier-Stokes equations. The first sentence of the long memoir he devoted to this topic in 1877 therefore stated as a matter of fact that:

> Fluids move in two different ways, according to whether they flow in very narrow pipes or in spaces with sections comparable to that of large pipes or uncovered canals.[166]

In his memoir on running waters, Boussineq made the radical suggestion, following Saint-Venant, that the viscosity coefficient that entered the Navier-Stokes equations might vary in space and time and with the geometry and size of the sections of the pipe. This suggestion notwithstanding, the flow of water remained, and for long, "a distressing enigma [*énigme désespérante*] against which distinguished spirit stumbled in vain."[167] But one notes that by then, the validity of the Navier-Stokes equations as the adequate basis for the description of turbulent flows had ceased to be questioned.

également ce passage du régime régulier au régime irrégulier." P. Appel et al., "Développements," 199.

165 Manucript of Saint-Venant's proposition of Boussinesq for a chair at the Academy of Sciences (4 January 1886). Also, J. Boussinesq, *Notice sur les travaux scientifiques de M. J. Boussinesq* (Lille: L. Danel, 1880 and 1883). Arch. AdS.

166 J. Boussinesq, "Essai sur la théorie des eaux courantes," *Mémoires des savants étrangers,* 23(1) (1877): 1-680; preceded by O. Bonnet, Phillips and A. B. de Saint-Venant, "Rapport sur un Mémoire de M. Boussinesq présenté le 28 octobre 1872 et intitulé 'Essai sur la théorie des eaux courantes'," [publ. in *CRAS,* 76 (1873): 924-943]. See also J. Boussinesq, "Essai théorique sur les lois trouvées expérimentalement par MM. Darcy et Bazin, pour l'écoulement uniforme de l'eau dans les conduites," *CRAS,* 71 (1870): 389-393.

Once again in this chapter, we encounter a fascinating case of interaction between men coming from a variety of backgrounds. Engineers, medical doctors, physicists, and mathematicians, all exchanged theories, experimental results and practices at a node represented by the Academy of Sciences of Paris. Limited by the topic of this dissertation, one can only hope that more historical study will be undertaken on this topic. For the time being, the conclusion which should be drawn from this cursory account of the scientific investigation of water flows in pipes is the following: after half a century of research, the "distressing enigma" was still there, but the conviction that the Navier-Stokes equations represented the basis of any theoretical explanation of the phenomena was considerably reinforced.[168]

(ii) Osborne Reynolds's Experimental Discovery of Turbulence

Systematic study of turbulent flows, however, only began with the British physicist Osborne Reynolds (1842-1912) around 1876.[169] By importing to England the work done on the Continent, by adopting a physical, empirical attitude and merging hydraulic and mathematical studies, Reynolds greatly changed the outlook of the subject. "The English teach mechanics as an experimental science," Poincaré once

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[167] A. B. de Saint-Venant, "Sur l'hydrodynamique," 774; quoted by J. Boussinesq, "Essai sur la théorie des eaux courantes," 6. About Prandtl's use of Boussinesq's suggestion, see G. Battimelli, "The Mathematician and the Engineer," 84-86.
[168] One should note however that boundary conditions (the so-called no-slip boundary condition which was later adopted) were very much still a matter of intense debate. See P. Duhem, *Recherches sur l'hydrodynamique,* 2 (Paris, 1904): 79-95, for a history of boundary conditions for viscous fluid.
[169] Experiments on pipes were also performed by H. Hagen in 18??, but his theoretical interpretation was not convincing and his experiments did not receive much notice at the time. G. H. L. Hagen, *Abhandlungen der Akademie in Berlin* 1854, éd. 1855,

wrote; "on the Continent, it is still exposed more or less as a deductive, *a priori* science. The English are right, this goes without saying."[170]

To start with, Reynolds established on the basis of extensive experimentation that "the internal motion of water assumed one or the other of two broadly distinguishable forms."[171] Furthermore, he identified a single parameter that controlled the behavior of the fluid. His experiments showed that this parameter, later named the *Reynolds number,* possessed a critical value at which the motion changed its form.

Although a most cursory observation of fluid flows could reveal that turbulent motions were, on the face of it, very different from smooth ones, it was Reynolds's achievement to distinguish them unambiguously. "A clear surface of moving water has two appearances," he wrote, "the one like that of a plate of glass, in which objects are reflected without distortion, the other like that of sheet glass, in which the reflected objects appear crumpled up and grimacing."[172] Reynolds assumed that these two characteristics corresponded to two distinct types of motion, which later came to be labeled as *laminar* and *turbulent,* although Reynolds preferred the terms "direct" for the former and "sinuous" for the latter. The first occurrence of this "very

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mathematische Abhhandlung, 17. On this, see M. Brillouin, *Leçons sur la viscosité des liquides et des gaz* (Paris: Gauthier-Villars, 1907), 196ff.

170 H. Poincaré, *La Science et l'hypothèse* (Paris: Flammarion, 1918), 110. My translation. On laboratory culture and theoretical practices in late nineteenth-century England, see A. Warwick, "Le laboratoire Cavendish: A Cambridge, deux mondes s'opposent," *La Recherche,* 300 (1997): 70-75.

171 O. Reynolds, "An Experimental Investigation of the Circumstances which Determine whether the Motion of Water shall be Direct or Sinuous, and of the Law of Resistance in Parallel Channels." *Philosophical Transactions of the Royal Society,* 174 (1883): 935; repr. *Papers,* 2: 51-77, 52; for date, see p. 58.

descriptive term"—i.e. turbulence—was attributed to William Thompson, later Lord Kelvin, by Horace Lamb.[173]

This distinction between "direct" and "sinuous" flows was provided by experiments, *not* theory. From March to April 1880, Osborne Reynolds experimented with glass tubes which would lead him to this conclusion. He was assisted by Mr. Forster, of Owens College, who built several glass tubes of varied diameters fitted with trumpet mouthpieces, so that the water from a tank above might enter without disturbance. As water was made to flow through the tube, a colored streak was added to clear water. What Reynolds and Foster observed was the subsequent appearance of the streak. "The general results [of their observations] was as follows:–

> When the velocities were sufficiently low, the streak of colour extended in a beautiful straight line through the tube. . . .
>
> As the velocity was increased by small stages, at some point in the tube, always at a considerable distance from the trumpet or intake, the colour band would at once mix up with the surrounding water, and fill the rest of the tube with a mass of coloured water. . . .
>
> On viewing the tube by the light of an electric spark, the mass of colour resolved into a mass of more or less distinct curls, showing eddies.[174]

The central question for Reynolds was whether there was a clear way to distinguish between these two kinds of motion. "Did steady motion hold up to a

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172 O. Reynolds, "An Experimental Investigation," 52.
173 H. Lamb, *Hydrodynamics*, 6th ed. (Cambridge: Cambridge University Press, 1932; repr. 1993), 664. See W. Thomson, "On the Propagation of Laminar Motion Through a Turbulently Moving Inviscid Fluid," *B. A. Report* (1887): 386-495; *Philosophical Magazine,* 24 (1887): 342-353; repr. in Kelvin, *Mathematical and Physical Papers,* 4, ed. J. Lamor (Cambridge: Cambridge University Press, 1910): 308-320. On p. 311, Thomson defined "the average velocity of the turbulent motion," but not turbulence itself.
University of Cambridge for 1888 (p. 321)."

critical value and then eddies come in?" he asked. "Did the eddies first make their appearance as small and then increase gradually with the velocity, or did they come suddenly?"[175] Reynold's experiment seemed to settle these questions in favor of a sudden appearance of turbulence. There was a critical speed below which motion was steady, and above which it became "sinuous." The critical speed depended on the experimental setting; it varied with the radius of the pipe and with the water temperature, which had an effect on its density and viscosity.

With his experiments Reynolds found that the appearance of "sinuous" motions, or eddies in the colored streak, occurred in all cases at a critical value of a single dimensionless parameter. Considering U as the mean speed along the tube of a fluid with viscosity μ and density ρ, and a single parameter D characterizing linear dimensions of the tube, say its diameter, Reynolds defined the parameter as follow:

$$Re = \frac{\rho D U}{\mu}.$$

Depending on how he defined "criticality," that is, on which observation Reynolds used in order to locate the change of character of fluid motion, experiments showed the value of the critical Reynolds number for the appearance of "sinuous" motion lying between 2000 and 12,000.[176] The consistency of this value in several

[174] O. Reynolds, "An Experimental Investigation," 59-60.
[175] Questions 4 and 6, in O. Reynolds, "An Experimental Investigation," 57-58.
[176] O. Reynolds, "On the Dynamical Theory," 536. In his 1883 paper, Reynolds expressed his results using Poiseuille's law: $P = (1+\alpha T+\beta T^2)^{-1} \propto \mu/\rho$, α=0.0336 and β=0.00221, T being the temperature of the water. In this case, he found P/Uc=43.79, c being the diameter of the pipe, corresponding to Re~12,000, as the value at which steady motion broke down. The law of resistance, however, also changed, at a critical speed, from being proportional to mean water velocity. In this case, the critical point

experimental situations proved, Reynolds thought, "not only the existence of a critical velocity at which eddies come in, but that it is proportional to the viscosity and inversely proportional to the diameter of the tube."[177]

Reynolds considered that this observation of "sinuous" motion in water and the relation between critical velocity, diameter, and viscosity stood "prominently forth, as to invite or defy theoretical treatment." Indeed, although apparently founded by Stokes on sound first principles and successfully tested for "direct" flow, theory was conspicuously unable to account for unsteady motion in water. "The theory of hydrodynamics has so far failed with the slightest hint why it should explain these phenomena [of sinuous flow] encountered by large bodies moving at sensibly high velocities through water, or that of water in sensibly large pipes." Did responsibility lie with "some fundamental principles of fluid motion of which due account has not been taken in the theory"?[178] Were the Navier-Stokes equations to be modified in order to account for "sinuous" motion?

Reynolds thought unlikely the prospect of finding anything faulty in the Navier-Stokes equations. Still, he believed that "they might contain evidences which had been overlooked, of the dependence of motion on a relation between the dimensional properties and the external circumstances of motion (55)." Indeed Reynolds noticed that the dependence of tube resistance on the velocity of the flow

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was also only a function of *P/Uc* which was then equal to 278. This later value was consistent with earlier experimental results by Darcy (1857) and Poiseuille (1845), and is also equivalent with $Re \sim 2000$. See Reynolds, "An Experimental Investigation," 60 and 74.

177 O. Reynolds, "An Experimental Investigation," 75.

178 O. Reynolds, "An Experimental Investigation," 52-53.

was distressing in the sense that it seems to imply a dependence on absolute dimensions. In an early example of dimensional analysis, he noted that the ratio μ/ρ of the viscosity over the density was a "quantity of the nature of the product of a distance and a velocity (54)."[179] The Navier-Stokes equations also confirmed that the ratio of the nonlinear term $(\mathbf{v}\cdot\nabla)\mathbf{v}$ to the viscosity terms $\nu\Delta\mathbf{v}$ was proportional to the Reynolds number *Re*.

> Of course without integration the equations only gave the relation without showing at all in what way the motion might depend upon it. It seemed, however, to be certain, if the eddies were due to one particular cause, that integration would show the birth of eddies to depend on some definite value of [*Re*].[180]

For Reynolds, the next theoretical step therefore should have been to deduce the critical value of the Reynolds number starting from the Navier-Stokes equations. At first, however, Reynolds did not present such an investigation, which he noted would have "involved the integration of the equations for unsteady motion in a way that has

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[179] O. Reynolds, "An Experimental Investigation," 54-55. Reynolds's own reconstitution of the path that led him to this simple idea might be of interest for the history of dimensional analysis: "It is always difficult to trace the dependence of one idea on another. But it may be noticed that no idea of dimensional properties . . . occurred to me until after the completion of my investigation on the transpiration of gases, in which was established the dependence of the law of transpiration on the relation between the size of the channel and the mean range of the gaseous molecules (54)." Historical references for dimensional analysis are the following: Rayleigh, "Presidential Address," *British Association Reports* (Montréal, 1884): 1-23; *Papers,* 2: 333-354, 344; "The Principle of Similitude." *Nature,* 95 (1915): 66-68; repr. *Papers,* 6: 300-305; A. Vaschy, "Sur les considérations d'homogénéité en Physique," *CRAS,* 114 (1892): 1416-1419; and E. Buckingham, "On Physically Similar Systems: Illustrations of the Use of Dimensional Equations," *Physical Review,* 4 (1914): 345-376. See also P. W. Bridgman, *Dimensional Analysis* (New Haven: Yale University Press, 1963).

[180] O. Reynolds, "An Experimental Investigation," 55.

not been accomplished, and which, considering the general intractability of the equations, was not promising."[181]

When Reynolds came back to this problem in 1894-1895, he tackled the theoretical problem of the determination of critical Reynolds numbers, and introduced statistical methods.[182] While both of these approaches would be widely followed later, it is only necessary for my purpose to discuss theoretical attempts at evaluating critical Reynolds numbers. Apparently, this line of research was triggered by the fact that "the stability or instability of the steady motion of a viscous fluid" had been proposed by William Strutt, Lord Rayleigh, as the subject for the Adams Prize of the University of Cambridge for 1888.[183] Well undertaken by the work of British physicists of Thomson's and Rayleigh's stature among others, this subject of stability theory would evolve into a thriving subdiscipline of fluid mechanics which attracted the attention of the likes of Lorentz,[184] Sommerfeld, and Heisenberg. Following Reynolds and his contemporaries, the evaluation of critical Reynolds numbers was accomplished for a few simple cases, known as the Poiseuille, Couette, and Bénard flows, which became paradigmatic of stability theory.

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[181] O. Reynolds, "An Experimental Investigation," 57.
[182] O. Reynolds, "On the Dynamical Theory."
[183] Rayleigh, "Further Remarks on the Stability of Viscous Fluid Motion," *Philosophical Magazine,* 38 (1914): 609-619; repr. *Papers,* 6: 266-275, 267. William Thomson [Lord Kelvin], "Broad River Flowing Down an Inclined Plane Bed," *Philosophical Magazine,* 24 (1887): 188-196, and 272-278; repr. *Papers,* 4, 321-330 and 330-337, 321. About the problem of the stability of fluid motion for Thomson, see C. Smith and M. N. Wise, *Energy and Empire,* chap. 12.
[184] See H. A. Lorentz, "Ein allgemeiner Satz, die Bewegung einer reibenden Flüssigkeit betreffend, nebst einigen Andwendungen desselben," *Abhandlungen über theoretischen Physik,* 1 (1907): 43-71.

c) Stability Theory: The Conceptual Unit Challenged by the Ruelle-Takens Model

In 1953, as a graduate student, Russel J. Donnelly asked Lars Onsager at Yale University what stability theory was: "He informed me that hydrodynamic stability was a small field of physics carried on by 'a small crew'. The crew members were identified as Chia-Chiao Lin (at MIT), Subrahmanayan Chandrasekhar (at the University of Chicago) and Geoffrey Ingram Taylor (at the University of Cambridge)."[185] The history of stability theory went as far back as Georges Stokes in the first half of the nineteenth century and it had been the subject of much controversy up until Lin's impressive synthesis in 1945-1955.[186]

At the basis of stability theory lay the assumption that the Navier-Stokes equations provided the correct description of turbulent, as well as laminar flows. Since it was an experimental fact that the laminar solution ceased to be observed when the Reynolds number went though a certain critical value between 1,000 and 100,000 depending on the geometry of the arrangement, there had to exist other solutions which were realized only at large Reynolds numbers.

> It is only reasonable to infer from this that the laminar flow, while still a solution, ceases to be a stable one, or at least the most stable. It is plausible to conclude that the turbulent flow represents one or more solutions of a higher stability, and that these come into existence or at least acquire their higher stability, only for high values of Reynolds' number.[187]

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185 R. J. Donnelly, Review of *The Life and Legacy of G. I. Taylor* by George Batchelor, in *Physics Today,* 50(6) (1997): 82.
186 Some historical remarks and references are to be found in P. G. Drazin and W. H. Reid, *Hydrodynamic Stability* (Cambridge: Cambridge University Press, 1981).
187 J. von Neumann, "Recent Theories," 439.

In 1945, Chia-Chiao Lin, a student of Theodore von Kármán at Caltech, summarized the "final aims" of most of the work in stability theory as follows:

> 1) The first aim of stability theory is to determine whether a given flow (or a given class of flows) is ultimately unstable for sufficiently large Reynolds numbers.
>
> 2) The second purpose is to determine the minimum critical Reynolds number at which instability begins. . . .
>
> 3) Finally, we want to understand the physical mechanism underlying the phenomena by giving theoretical interpretations and experimental confirmations of the results obtained from mathematical analysis.[188]

With the goals of stability theory stated as such, one sees that the model suggested by Ruelle and Takens only addressed the first of Lin's purposes. In addition, the modeling practice they introduced was at odds with many of those used by stability theorists.

In the following, going back to the beginning of the century will show the successes and controversies that surrounded stability theory. Again, we will pay attention mainly to the modeling practices involved, to the confidence put on the Navier-Stokes equations, and not so much the specific context for each contribution. Lin's synthesis will show that his approach, while resolving long-standing controversies, became irrelevant for the turbulence problem and was almost immediately superseded by new nonlinear methods. These nonlinear methods were those directly challenged by the picture suggested by Ruelle and Takens.

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188 C.-C. Lin, "On the Stability of Two-Dimensional Parallel Flows," Quar*terly of Applied Mathematics,* 3 (1945): 117-142; 218-234; and 277-301, 1; repr. *Selected Papers,* 117.

(i) Ancestors and Controversy

Already in 1843 George G. Stokes conjectured that among the causes for the "discrepancy between theory and observation," besides internal friction, a possibility existed that flows might be "unstable." In certain circumstances, he suggested, it might happen that a flow "though dynamically possible, nay the *only* dynamically possible when the conditions which we have supposed are accurately satisfied, is unstable, so that the slightest cause produces a disturbance in the fluid, which accumulates . . . till the motion is quite perturbed."[189] Although close in its expression to sensitive dependence on initial conditions, this statement should not be confused with an anticipation of chaos. Rather Stokes insisted on the well known fact that some solutions of differential equations may be unstable, as for a ball rolling on an edge which might fall on either side.

Introduced by Rayleigh in 1880, a general method, called the method of small oscillations, was favored for the investigation of the stability of small perturbations of a stationary flow.[190] A solution $(\mathbf{V}, p)$ was said to be stationary if it satisfied the time-independent Navier-Stokes equations:

$$(\mathbf{V}\cdot\nabla)\mathbf{V} = -\frac{1}{\rho}\text{grad } p + \nu\Delta\mathbf{V}.$$

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189 G. G. Stokes, "On Some Cases of Fluid Motions," *Transactions of the Cambridge Philosophical Society,* 8 (1845), 105; repr. *Papers,* 1: 17-68, 53-54. Noticed by O. Reynolds, "An Experimental Investigation," 55.

190 See, e.g., the following review articles: F. Noether, "Das Turbulenzproblem," 128-131; J. L. Synge, "Hydrodynamical Stability," 235ff. Rayleigh, "On the Stability, or Instability, of Certain Fluid Motions," *Proceedings of the London Mathematical Society,* 11 (1880): 57-70; repr. *Papers,* 1: 474-487.

To study small disturbances of the stationary motion, solutions of the following form were studied:

$$\mathbf{v}(x,t)= \mathbf{V}(x)+\varepsilon\mathbf{v}'(x,t)+ \text{ higher order terms in } \varepsilon;$$

where ε was small for small Reynolds numbers. Then the equations statisfied by **v**' could be derived by subsituting the above *Ansatz* in the Navier-Stokes equations and neglecting terms of higher order in ε, which amounted to a linearization of the equation. The method of small oscillations consisted in supposing that **v**' had the following form:

$$\mathbf{v}'(x,t)= e^{\sigma t}\mathbf{F}(x).$$

The question of the stability or instability of the perturbation was then reduced to the question of computing whether the real part of σ, in general a complex number, was or was not positive. In the positive case the magnitude of the disturbance would grow exponentially. An infinitesimal variation from the stationary solution would then become, after a certain time, large enough to be observed. In general, a critical Reynolds number Re_{crit} could be computed, below which the motion was stable with respect to small oscillatory disturbances, and above which it became unstable. What then would be the observed solution of the Navier-Stokes equations, however, received no answer.

This approach to stability was very difficult to put in practice and had obvious limitations. First, it required that an exact solution to the Navier-Stokes equations be computed to start with. This theory "can only be attempted in cases when one possesses a special solution, however we know how rare are the exact solutions of motions in viscous fluids."[191] There were more or less three classes of flows where the techniques of stability theory had been applied: the Poiseuille flow, characteristically involving flows in pipes or canals of different sections driven by a pressure gradient; the Couette flow, involving flows between surfaces, typically coaxial cylinders or parallel planes, moving at a constant speed relative to one another; and the Bénard convective flow, in which a layer of fluid was heated from below. Each of these different types of flows could also be studied in two dimensions, which led to significant simplification, but left the results open to criticism.

Second, a number of different assumptions, all of which could be questioned, entered into the above procedure and led to contradictory results for the plane Couette flow. In particular, in the 1910s, following Arnold Sommerfeld's lead, Ludwig Hopf and R. von Mises concluded that the flow remained stable for all Reynolds numbers.[192] This result was difficult to test exprimentally, however, and seemed a "surprising result from a physical point of view."[193] In other words, between two infinite plates moving at a constant relative velocity, turbulence should not develop! In 1923, in his doctoral thesis, Werner Heisenberg used different approximation

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[191] H. Villat, *Leçons sur les fluides visqueux,* recueillies et rédigées par Julien Kravtchenko (Paris: Gauthier-Villars, 1943), 423. See also Villat's early work, "Sur quelques progrès récents des théories hydrodynamiques," *Bulletin des sciences mathématiques,* 42 (1918): 43-60; 72-92; and his thesis: "Sur la résistance des fluides," *Annales scientifiques de l'École normale supérieure,* 28 (1911): 203-311.
[192] A. Sommerfeld, "Ein Betrag zur hydrodynamischen Erklärung;" R. von Mises, "Kleine Schwingungen und Turbulenz," *Jahresbericht der Deutschen Mathematiker-Vereinigung,* 21 (1912): 2416248; "Beitrag zum Ozscilationsproblem," *Festschrift Heinrich Weber* (Leipzig and Berlin, 1912): 252-282; L. Hopf, "Der Verlauf kleiner Schwingungen auf einer Strömung reibender Flüssigkeit," *Annalen der Physik,* 44 (1914): 1-60.
[193] J. L. Synge, "Hydrodynamical Stability," 261.

methods (similar to the Wentzel-Kramers-Brillouin method later used in quantum theory) to derive a critical Reynolds number for the same case. His method was harshly criticized by Noether and spurred a long-standing controversy that was not resolved until C.-C. Lin's work in the late 1940s.[194] This controversy had two important effects. First, it once again cast doubt on the validity of the Navier-Stokes equations; and second, it made people wonder about the pertinence of the scheme of small oscillations for the determination of critical values. It seemed that one was entitled to concur with William Orr's gloomy diagnosis: "It would seem improbable that any sharp criterion for stability of fluid motion will ever be arrived at mathematically."[195] Other methods were thus developed, most notably energy methods and Ludwig Prandtl's boundary layer theory.[196]

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[194] W. Heisenberg, "Über Stabilität und Turbulenz von Flüssigkeitsströmen," *Annalen der Physik,* 74 (1924): 577-627 [his doctoral thesis (Munich, July 1923)]; repr. *Gesammelte Werke/Collected Works,* ser. A, 1, group 1, introductory essay by S. Chandrasekhar and H. Rechenberg. About Heisenberg's thesis and the following controversy, see J. Mehra and H. Rechenberg, *The Historical Development of Quantum Theory,* 2 (New York: Springer, 1982), Section I.7: 49-63. For his critique, see F. Noether, "Zür asymptotischen Behandlung der stätionären Lösungen im Turbulenzproblem," *Zeitschrift für angewandte Mathematik und Mechanik,* 6 (1926): 232-243, 242. It was Heisenberg himself who later noted the similarity of his method with the WKB approximation: "On the Stability of Laminar Flow," *Proceedings of the International Congress of Mathematicians, Cambridge, Mass., 1950,* 2 (Providence: AMS, 1952): 292-296.

[195] W. McF. Orr, "The Stability or Instability of the Steady Motion of a Fluid. Part II: A Viscous Liquid," *Proceedings of the Royal Irish Academy (Dublin),* A27 (1907): 69-138. Similarly, Rayleigh declared in 1916: "One can hardly deny that [theoretical hydrodynamics] is out of touch with reality." Cf. his review of Lamb's *Hydrodynamics, Papers,* 6: 401.

[196] For energy methods, see F. Noether, "Das Turbulenzproblem," 131-133; J. L. Synge, "Hydrodynamical Stability," 263-266. About boundary layer theory, see L. Prandtl, "Über Flüssigkeitsbewegung bei sehr kleiner Reibung," *Proceedings of the Third International Congress for Mathematics* (1904): 484-491; *Führer durch die*

(ii) Success with Taylor-Couette Flow: Sequence of Instabilities

In 1923, stability theory witnessed a "conspicuous triumph in the work of G. I. Taylor [1886-1975]."[197] This work has been emphatically praised by many a fluid dynamicist. "One of the most influential investigations of 20th-century phyics," according to Donnelly, Taylor's paper was seen as a definite proof for the correctness of the Navier-Stokes equations and a definitive settlement in favor of the no-slip boundary conditions.[198] "It was a tour de force which, more than any other single paper, established hydrodynamic stability as a distinct field."[199] Contrary to Ruelle and Takens's later views, Taylor contended:

> It seems doubtful whether we can expect to understand fully the instability of fluid flow without obtaining a mathematical representation of the motion of a fluid in some particular case in which instability can actually be observed, so that a detailed comparision can be made between the results of analysis and those of experiments.[200]

The particular case he selected was that of a liquid contained between two coaxial cylinders, rotating with different frequencies. As opposed to the infinite planes considered before him, such motion would be readily observable in a carefully designed experiment. In addition, Lord Rayleigh had worked out a criterion for

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Stromungslehre, 3rd ed. (Braunschweig: Fr. Vieweg und Sohn, 1942); *Guide à travers la mécanique des fluides,* transl. A. Monod (Paris: Dunod, 1952).

[197] J. L. Synge, "Hydrodynamic Stability," 228. See G. I. Taylor, "Stability of a Viscous Liquid contained between Two Rotating Cylingers," *Philosophical Transactions of the Royal Society (London),* A223 (1923): 289-343.

[198] About Taylor's work, see R. J. Donnelly, "Taylor-Couette Flow: The Early Days." *Physics Today,* 44(11) (1991): 32-39; where the above quote is to be found.

[199] G. K. Batchelor, *The Life and Legacy of G. I. Taylor* (Cambridge: Cambridge University Press, 1996), 88.

[200] G. I. Taylor, "Stability of a Viscous Liquid," 290.

inviscid fluid.[201] In the 1880s, this type of experiment had been performed by H. R. A. Mallock before the Royal Society, and Maurice Couette for his doctoral thesis.[202] Interestingly, Couette undertook his experiments not in order to show the stability of the motion, but rather to answer the following "fundamental question": "Is the interior friction coefficient [i.e. viscosity] a well-defined physical quantity?"[203] This question naturally came up as a consequence of Boussinesq's suggestion of considering viscosity as a function of space and geometry, as well as of the physical characteristics of the fluid. Couette's experiments confirmed that the motion assumed one of two regimes: "the first exactly conforms to the simplest integral of the Navier[-Stokes] equations; the second does not conform to these integrals."

Not content with providing a complete theoretical study of the stability of Couette flows, notably using Bessel functions, Taylor also performed careful experiments which agreed with his theory and provided further directions for research. Like Couette before him, he indeed noticed that the first instability was not the only one. Between steady motion and fully developed turbulence, many different instabilities arose one after the other. With his glass cylinder, Taylor observed that the first instability involved the formation of rolls perpendicular to the axes of the cylinders. He provided a complete theoretical derivation of these solutions. He moreover noticed that at higher Reynolds numbers spirals and other types of vertical

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201 Lord Rayleigh, "On the Dynamics of Revolving Fluids," *Royal Society Proceedings,* A (1916): 148-154; repr. *Papers,* 6: 447-453.
202 M. Couette, "Sur un nouvel appareil pour l'étude du frottement des fluides," *CRAS,* 107 (1888): 388-390; "Études sur le frottement des liquides," *Annales de chimie et de physique,* 6th ser., 21 (1890): 433-510. [His thesis.]
203 M. Couette, "Études sur le frottement," 433.

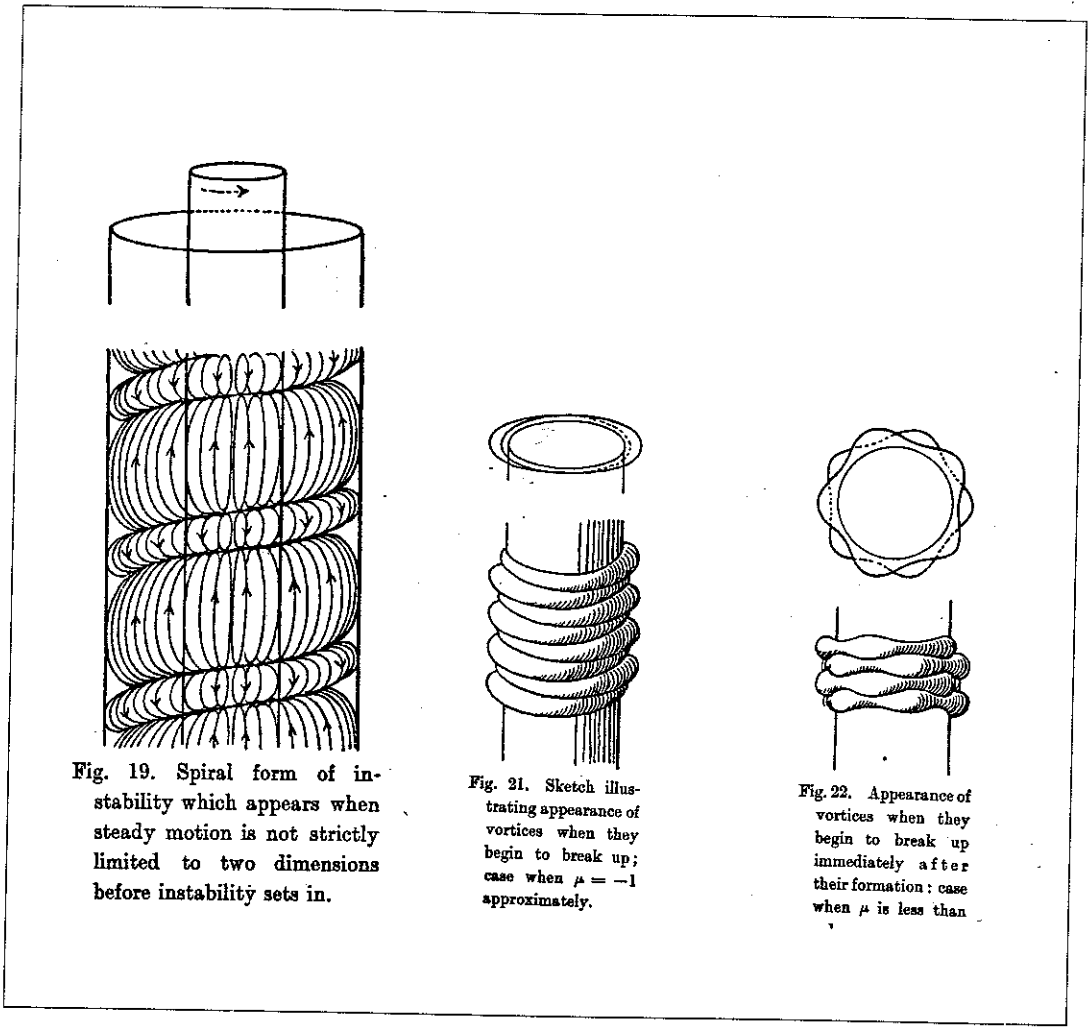

Figure 14: Secondary Oscillation Observed by G. I. Taylor in the Couette Flow. Repr. with permission from G. I. Taylor, "Stability of a Viscous Liquid," 326 and 343. Copyright © The Royal Society in London.

disturbances could appear. These experiments provided the ground for Hopf's and Landau's works in which they considered instabilities as involving appearances of frequencies in a sequence (Fig. 14).

Taylor's observation was crucial in undermining the arguments for an oscillation between the two regimes (steady and turbulent) which had been proposed by Couette in 1890.[204] In 1898-1899 and 1899-1900, Marcel Brillouin gave a course on fluid mechanics at the Collège de France where he took up Couette's proposal.[205] Carefully reviewing previous experiments and even repeating them and performing new ones with the help of Henri Bénard—it was at this occasion that Bénard famously studied convective flows[206]—Brillouin had to acknowledge that the theory explaining the passage from one regime to another "was scarcely sketched." His admiration for Reynolds notwithstanding, Brillouin did not insist on critical Reynolds numbers but was rather inclined to believe that the transition was progressive.[207] G. I. Taylor partly confirmed this view. The transition to turbulence was not sudden, but rather involved a succession of instabilities appearing one after the other.

Taylor's success with the Couette flow provided new grounds for feeling more confident about the theory of hydrodynamic stability. One telling example of this renewed interest was that at the celebration of the first fifty years of the American Mathematical Society this topic was the sole contribution on "application of mathematics." According to J. L. Synge, who delivered the address, its preparation "involved a difficult decision," because the subject of applied mathematics was so

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204 M. Couette, "Études sur le frottement," 478-480.
205 Marcel Brillouin, *Leçons sur la viscosité,* esp. Livre II, chapitre IV: "Le régime de Poiseuille et le régime hydraulique. Passage d'un régime à l'autre," 196-224.
206 H. Bénard, "Les tourbillons cellulaires dans une nappe liquide I: Description générale des phénomènes," *Revue générale des sciences pures et appliquées,* 11 (1900): 1261-1271; "Les tourbillons cellulaires dans une nappe de liquide transportant de la chaleur par convection en régime permanent," *Annales de chimie et physique,* 7th ser., 23 (1901): 62-144.
207 "Dans un même tube de verre le passage d'un régime à l'autre n'a pas lieu brusquement, à partir d'une vitesse déterminée, mais il existe une période troublée où les deux régimes sont possibles et alternent avec une fréquence plus ou moins grande." M. Brillouin, *Leçons sur la viscosité,* 196.

vast. He chose hydrodynamic stability because he felt this was a topic that was well formulated mathematically, interesting not only to the mathematician but also to the physicist and the engineer, and involved still unsolved problems.[208]

Taylor's work notwithstanding, many fluid dynamicists felt that the successes of stability theory were at best relative. In 1930, Henri Villat, a student of Brillouin whom we have met as Leray's mentor, wrote that the theoretical study of the transition to turbulence was in its infancy. "Here are huge fields of research where workers will be able to exert their sagacity and where experimenters will often open the way to mathematicians."[209] In the early 1940s, Caltech mathematician and engineer Theodore von Kármán (1881-1963), a Hungarian émigré, endeavored to draw the mathematicians' attention to many nonlinear problems, difficult on a mathematical level, but crucial for engineers. For him, therefore, hydrodynamics and aerodynamics, his main fields of research, provided clear examples of nonlinear engineering problems where advanced mathematical methods were required. But he did not deal with the turbulence problem, barely noting that:

> [it] has been discussed mathematically by several authors by means of linearized equations without reaching a satisfactory agreement with experiment. The adequate treatment of the nonlinear equations is bound to contribute essentially to the solution of this important problem.[210]

Similarly, John von Neumann contended in 1949:

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[208] J. L. Synge, "Hydrodynamical Stability," 227. See G. I. Taylor, *Proceedings of the Fifth International Congress of Applied Mathematics, Cambridge, Mass., 1938* (New York: J. Wiley & Sons, 1939): 304-310.
[209] Henri Villat, *Mécanique des fluides* (Paris: Gauthier-Villars, 1930), vi-vii. [Cours de l'École normale d'aéronautique.]

> The summing up of several decades of stability theory appears to be this: The stability theory proved to be mathematically much more difficult than might have been originally expected. . . . [Its relation] to experiment has so far not been a very satisfactory one.[211]

Manifestly, at the end of World War II, classical analytic methods had reached a limit. The theory of hydrodynamic stability was in need of some good cleaning-up. It would however be achieved at a crucial cost: in the process it lost much of its relevance as a way to understand turbulence.

(iii) Synthesis, but Insignificance?

In fact, Taylor's very observation of a sequence of instabilities indicated that stability theory was but the beginning of the theoretical study of the onset of turbulence. Like von Kármán, von Neumann believed that stability theory had to become fully nonlinear in order to answer questions about the nature of turbulence:

> The stability theory could at best only determine when the laminar flow breaks down and turbulent flow becomes possible. It will not describe, however, what the properties of the developed turbulent flow are. This linear, 'small perturbation' theory must obviously be complemented by a non-linear theory of large deviations from the laminar pattern. Or to put it more directly: *A complete non-linear theory of the general solutions of the Navier-Stokes equations is called for.*[212]

In 1944, Chia-Chiao Lin, a student of von Kármán, started to publish his work that would end up establishing solid foundations for stability theory, as summarized in his acclaimed 1955 monograph.[213] Building on Synge's work, he thereby confirmed

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[210] T. von Kármán, "The Engineer Grapples with Nonlinear Problems." *Bulletin of the American Mathematical Society,* 46 (1940): 615-683, 664n. See also "Tooling up Mathematics for Engineering." *Quaterly of Applied Mathematics,* 1 (1943):. 1-6.
[211] J. von Neumann, "Recent Theories," 441.
[212] J. von Neumann, "Recent Theories," 441. My emphasis.
[213] C.-C. Lin, *The Theory of Hydrodynamic Stability* (Cambridge: Cambridge University Press, 1955).

the results of Heisenberg's 1923 thesis and rehabilitated the field of stability theory. What is the most noticeable in his work is that he made crucial use of the first computers available in order to recover Heisenberg's prediction that the plane Couette flow was indeed unstable.[214] However, Lin clearly resented this state of affairs: "It would be highly desirable if the instability of the classical motion could be proved without resorting to such heavy calculations."[215] As with every previous success, Lin's synthesis only furthered belief in the validity of the Navier-Stokes equations.

Subramanayan Chandrasekhar, the famous Indian physicist from the University of Chicago, was the third "crew member" who, according to Onsager, was embarked in the 1950s on the boat of hydrodynamical stability theory. He was a master at combining delicate experimentation,[216] numerical computations,[217] theoretical derivation, and an uncanny physical intuition. But mostly, Chandrasekhar's work showed the ability of Lin's methods to account for a wide variety of cases. Above all, Chandrasekhar explored the stability of convective flows, when combined with rotational motion or electromagnetic fields.[218]

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[214] See C.-C. Lin, "On the Stability of Two-Dimensional Parallel Flows." *Quaterly of Applied Mathematics,* 3 (1946): 117-142; 218-234; 277-301. Repr. *Papers,* 1: 1-68.
[215] C.-C. Lin, *The Theory of Hydrodynamic Stability,* 31.
[216] See a picture of his hydromagnetic laboratory at the Fermi Institute for Nuclear Studies, Chicago, in figure 12 in S. Chandrasekhar, "Thermal Convection: Rumford Medal Lecture 1957," *Daedalus,* 86 (4) (1957): 323-339; repr. *Selected Papers of S. Chandrasekhar,* 4 (Chicago: University of Chicago Press, 1989): 163-191, 180.
[217] For example, Chandrasekhar acknowledged von Neumann's help in having numerical work done on the IAS machine in S. Chandrasekhar, "The Stability of Viscous Flow Between Rotating Cylinders in the Presence of a Radial Temperature Gradient," *Journal of Rational Mechanics and Analysis,* 3 (1954): 181-207; repr. *Selected Papers,* 4: 107-133.
[218] The summary of his work is presented in *Hydrodynamic and Hydromagnetic Stability* (Oxford: Clarendon, 1961).

But at the same time as Lin settled the theoretical bases and Chandrasekhar explored their ramification, stability theory ceased to be as relevant as before, as von Neumann's remark above indicates, since it only dealt with the first instability. Lin himself was the first to acknowledge this: "The problem of the transition to turbulence . . . is in practice more important than that of stability of laminar motion."[219] But Lin admitted that he could not deal with this "more important" problem with the methods he had chosen to exploit. Indeed, a result of James Serrin's, painfully obtained following these methods together with energy methods, might be taken as symptomatic of the lack of relevance of hydrodynamic stability theory. In 1959, Serrin was able to determine a "universal stability criterion" showing that fluid flows were always stable for Reynolds numbers $Re < 5.71$, which should be compared with the values of the order of 1,000 to 10,000 obtained in most experimental situations![220]

Chia-Chiao Lin's modeling practice involved linear assumptions, reliance on computer calculations, and a refusal to interpret turbulence. In particular his basic assumptions for linear stability theory were two: first, he only considered infinitesimal disturbances and not finite ones and second, he "assume[d] that, for small disturbances, the [Navier-Stokes] equations may be linearized; that is, we shall neglect terms quadratic or higher in the disturbances and their derivatives."[221] This practice would be challenged by the next generation, who would endeavor to build a

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[219] C.-C. Lin, *The Theory of Hydrodynamic Stability,* ix.
[220] J. Serrin, "On the Stability of Viscous Fluid Motions," *Archive for Rational Mechanics and Analysis,* 3 (1959): 1-13.
[221] C.-C. Lin, *The Theory of Hydrodynamic Stability,* 1.

nonlinear theory of hydrodynamic stability without resorting to the computer. These were the scientists who had to react to Ruelle and Takens's suggestions.

(iv) Nonlinear Stability Theory

"As engineering science progresses," von Kármán contended in 1943, "the need for more exact information and the necessity to get nearer and nearer to physical reality, forces us to grapple with many nonlinear problems."[222] The nonlinear study of hydrodynamic stability could be taken to have started with "a capital memoir" by J. T. Stuart, from the National Physical Laboratory, Teddington, Middlesex, in 1958.[223] In this work, his ambition was important; he hoped that by considering nonlinear disturbances, stability theory could again claim to provide an account for turbulent phenomena in Poiseuille, Couette, and Bénard flows.

> A more fundamental objective [of the theory of hydrodynamic stability] is to understand how, and under what circumstances, turbulence may arise from laminar instability. . . . It is clear that the stability problem in its general form must be considered to be non-linear, because the equations of motion [i.e. Navier-Stokes] are non-linear.[224]

Significantly, Stuart noticed the "interesting suggestion concerning the development of turbulence from the growth of small disturbances" which had been advanced by Lev Landau in 1944. Indeed, from the point of view of stability theory,

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[222] T. von Kármán, "Tooling up Mathematics," 5.
[223] J. T. Stuart, "On the Non-Linear Mechanics of Hydrodynamic Stability," *Journal of Fluid Mechanics,* 4 (1958): 1-21. Gérard Iooss called Stuart's paper "a capital memoir" in his doctoral thesis: *Contribution à la théorie non-linéaire de la stabilité des écoulements laminaires,* thèse, Paris-VI (1971), Jussieu Lib. Note that there will be an important blind spot in the account of nonlinear stability theory provided here, namely the contributions of several Soviet scientists, and in particular V. I. Yudovich. For some references, see D. Ruelle and F. Takens, "Note Concerning our Paper," *TSAC,* 83-84.

Landau's was the first method to tackle nonlinear disturbances.[225] Stuart believed that this suggestion could even account for experimental observations that seemed to belie it:

> "there are cases of flow in which turbulence develops rather suddenly as the Reynolds number is raised, and in these cases one might infer that the critical Reynolds numbers are close together."[226]

In the early 1970s, most of the more mathematical contributions to nonlinear stability theory appeared in the *Archive for Rational Mechanics and Analysis.* Since this journal rejected Ruelle and Takens's paper, it is interesting to look at what it was publishing concerning the onset of turbulence around the same time. "The editor did not like our ideas," Ruelle recalled, "and referred us to his own papers so that we could learn what turbulence really was."[227] Founded by Clifford Truesdell in the late 1950s, the *Archive* "nourishes the discipline of mechanics as deductive, mathematical science in the classical tradition and promotes pure analysis, particularly in contexts of applications."[228] In 1970, it was edited by James Serrin, from the University of Minnesota, who specialized in the study of the Navier-Stokes equations and the stability of their solutions.[229] The editorial board included hydrodynamic stability theorists, such as D. D. Joseph and C.-C. Lin, applied mathematicians, such as J.-L.

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224 J. T. Stuart, "On the Non-Linear Mechanics," 2.
225 G. Iooss, *Contributions,* 7-8.
226 J. T. Stuart, "On the Non-Linear Mechanics," 5-6. Let us note here that Stuart's work, like Lin's, involved numerical computations performed by Miss S. W. Skan (see P. 21).
227 D. Ruelle, *Chance and Chaos,* 56.
228 Statement of intent of the journal published in each volume.
229 Note that the University of Minnesota, whereto James Serrin attracted both Daniel Joseph and David Sattinger, seems to have played an important role in the

Lions, but also mathematicians coming out of Lefschetz's school on nonlinear differential equations, like L. Cesari. The journal mainly dealt with hydrodynamics, elasticity, and thermodynamics, as well as the theory of ordinary and partial differential equations. More occasionally, it also included papers on various fields of mathematical physics, such as electromagnetism, relativity, or celestial mechanics. Its general philosophy was to print papers dealing with mechanics, in an uncompromisingly rigorous fashion. The standard form of its articles was strictly mathematical, with definitions, theorems, and proofs. Differential equations were the most common expressions to be found in its pages. Apparently, this was an outlet well suited for Ruelle and Takens's article. So, why was it rejected?

In order to understand this rejection one must take notice of the fact that in 1970-1972 several papers appeared in the *Archive* that dealt with the problem of (nonlinear) stability of fluid flows. Their authors were Gérard Iooss, Daniel Joseph, and David Sattinger.[230] Both Sattinger and Iooss came from a mathematical background: the former from the University of California in Los Angeles and the

development of nonlinear stability theory by maintaining contacts between engineers and mathematicians.

[230] G. Iooss, "Théorie non linéaire de la stabilité des écoulements laminaires dans le cas de « l'échange des stabilité »," *Archive for Rational Mechanics and Analysis,* 40 (1971): 166-208; D. H. Sattinger, "Bifurcation of Periodic Solutions of the Navier-Stokes Equations," *Ibid.,* 41 (1971): 66-80; "Stability of Bifurcating Solutions by Leray-Schauder Degree," *Ibid.,* 43 (1971): 154-166; D. D. Joseph and W. Hung, "Contributions to the Nonlinear Theory of Stability of Viscous Flow in Pipes and Between Rotating Cylinders," *Ibid.,* 44 (1971-1972): 1-22; D. D. Joseph and D. H. Sattinger, "Bifurcating Time Periodic Solutions and their Stability," *Ibid.,* 45 (1972): 79-109; G. Iooss, "Existence et stabilité de la solution périodique secondaire intervenant dans le problème d'évolution du type Navier-Stokes" *Ibid.,* 47 (1972): 301-329. See also D. D. Joseph, "Stability of Convection in Containers of Arbitrary Shape," *Journal of Fluid Mechanics,* 47 (1971): 257-282.

second from Paris.[231] They attacked the problems of hydrodynamic theory with the goal of providing solid mathematical foundations, first to the linear theory, then to show that the nonlinear theory fell back on the results of the linear theory as far as the first instability was concerned.[232] Vladimir Arnol'd's characterization of his own work in hydrodynamics may be well suited as a description of stability theory in the early 1970s: "following N. Bourbaki's call, I endeavored always to substitute blind calculations to the lucid ideas of Euler's."[233]

For Iooss, but also for Sattinger, if the long-standing controversy about stability theory had showed one thing, it was the need to be extremely clear about the functional spaces with respect to which a given flow was stable, or not.[234] They both developed heavy mathematical apparatuses that were extremely clear about the functional spaces they considered, and studied well-defined operators acting on these spaces. At the same time, they inserted themselves within the disciplinary tradition of stability theory, emphasizing Lin's synthesis and Landau's (and, in the case of Sattinger, Hopf's) suggestions as an indication of how to go beyond linear theory.

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231 Sponsered by the ONERA, the French aerospacial research agency, G. Iooss defended his Ph. D. thesis on March 3, 1971 in front of jury composed of J.-L. Lions (president), J.-P. Guiraud (his main advisor), and A. Avez. See G. Iooss, *Contributions.*
232 See, in particular, D. H. Sattinger, "The Mathematical Problem of Hydrodynamic Stability," *Journal of Mathematics and Mechanics,* 19 (1970): 797-819.
233 V. I. Arnol'd, "Sur la géométrie différentielle des groupes de Lie de dimension infinie et ses applications à l'hydrodynamique des fluides parfaits," *Annales de l'Institut Fourier de Grenoble,* 16(1) (1966): 319-361, 319.
234 D. H. Sattinger, "Stability of Nonlinear Hyperbolic Equations," *Archive for Rational Mechanics and Analysis,* 28 (1968): 226-244; "On Global Solutions of Nonlinear Hyperbolic Equations," *Ibid,* 30 (1968): 148-172.

Mainly, they set themselves the goal of investigating, with the most formal mathematical tools of functional analysis and topology, the stability of the Navier-Stokes equations typically written in the following form:

$$\frac{du}{dt} + L_\lambda u - M_\lambda(u) = 0;$$

where u represented the velocity field, and L_λ and M_λ respectively were linear, and nonlinear, operators depending on a parameter λ representing typically the Reynolds number.[235] Basing themselves on the theory of ordinary differential equations, which was the starting point of dynamical systems theory, they nonetheless insisted on the specificity of partial differential equations, such as Navier-Stokes.

During those years, Iooss's and Sattinger's most remarkable achievement was the study of the bifurcation of periodic solutions of the Navier-Stokes equations and their stability.[236] In Ruelle and Takens's language, this consisted in the second Hopf bifurcation. Without any contact with the flamboyant groups at Berkeley, or Bures-sur-Yvette, Iooss and Sattinger of course did not consider their problems in terms of attractors. Rather, they investigated the existence of a correctly behaved solution to the Navier-Stokes equations. It must be noticed, however, that Sattinger, who was knowledgeable about the work done at the Courant Institute of New York University, often framed his discussion in terms of bifurcation theory.[237] Finally, it should also be

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[235] See G. Iooss, *Bifurcation et stabilité,* Lecture notes for a course at Paris XI-Orsay (1972-1973); Jussieu Lib. Of course, the exact form of the equation varied slightly from one author to another, and even for the same author.

[236] Again one should mention Yudovich's name in this respect.

[237] He cited J. B. Keller and S. Antman, *Bifurcation Theory and Nonlinear Eigenvalue Problems* (New York: Benjamin, 1969).

emphasized that this work always remained purely mathematical and that, as opposed to their predecessors in stability theory, it involved no numerical computation whatsoever.

Compared to the above, Ruelle and Takens's article, no matter how mathematically arduous for the average physicist, was rather informal. The Bures pair remained sloppier about the characterization of the functional spaces they used. Moreover, the theorems they proved were either already well known, or based on unjustified assumptions as far as hydrodynamic flows were concerned.[238] From this shaky mathematical basis, they then speculated much further from stationary solutions than any stability theorists dared to venture, whether working in the nonlinear domain or not.

The modeling practice of the stability theorists involved a clear identification of functional spaces and the operators involved, and not, like Ruelle and Takens, topological features of attractors. The mathematical methods used were part of the classical tradition of functional analysis, well represented in the pages of the *Archive,* as opposed to global analysis and dynamical systems theory. They were not interested in classifying systems, since they already had their firm starting point in the Navier-Stokes equations, which remained unquestioned. Finally, since they were looking at solutions and the stability of disturbances, even if they had had the mathematical apparatus available to go beyond the study of the stability of periodic solutions, which they did not have, they would have been incapable of seeing something like a strange attractor. Aperiodic solutions would have been very difficult to distinguish from quasiperiodic ones without the topological tool of the attractor and the practices that went with it.

Looking at fluid mechanics as a "deductive, mathematical science in the classical tradition," Iooss, Joseph, and Sattinger were hardly in a position to appreciate something like the Ruelle-Takens model. But mainly, one may suspect that the reason why the editor of the *Archive* rejected this paper was that stability theorists then showed little interest for turbulence. For years, stability theorists had endeavored to prove the stability of laminar flow because this was a problem it could address; its practitioners had all but forgotten about turbulence!

6. RECEPTION OF THE RUELLE-TAKENS MODEL BY STABILITY THEORISTS; RECEPTION OF STABILITY THEORY BY RUELLE

Nevertheless, stability theorists' concerns were already close enough to Ruelle and Takens's, so that some dialogue could be established. Even if it involved translation and misunderstanding, this dialogue was an important factor contributing to a wide recognition of the Ruelle-Takens model. In the following, the first confrontation between the two approaches is examined as it took place at the Battelle Research Center in Seattle during the summer of 1972. The consequences that his work with Takens had on Ruelle's career in the years following their paper, and its consequence for the stability theorists' later careers are drawn.

[238] An exception to this was to so-called "Central Manifold Theorem." Cf. O. E. Lanford, "Bifurcation of Periodic Solutions into Invariant Tori: The Work of Ruelle and Takens," *Nonlinear Problems,* ed. I. Stakgold, et al.: 159-192.

a) Confrontation at Battelle

When Gordon Battelle died in 1923 he bequeathed his large fortune to found an institute devoted to the pursuit of "the social and economic benefits to be derived from scientific research and from the making of discoveries and inventions." Playing a key role during World War II in the metallurgy of uranium, the Battelle Memorial Institute expanded greatly in the postwar years, with a total staff of nearly 6500. In 1967, at its Seattle research center, a first meeting was held in which physicists and mathematicians were invited to exchange ideas, and which included an important French delegation, as well as addresses by Mather, Thom, and Smale.[239] It quickly became a tradition at Battelle to organize this kind of meetings with nearly equal representations from mathematics and areas where mathematicians might have something to contribute.[240]

From July 3 to 28, 1972, Battelle Seattle Research Center welcomed another Summer Institute, this time devoted to "the mathematical analysis of nonlinear problems in the physical and biological sciences." The meeting focused on four areas: biology, statistical mechanics, hydrodynamics, and chemical reaction engineering. Already in 1972, a "theme" emerged to the effect that "disparate branches of science generate common mathematical problems of nonlinear analysis."[241]

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239 C. M. DeWitt and J. A. Wheeler, eds., *Battelle Rencontres: 1967 Lectures in Mathematics and Physics* (New York: Benjamin, 1968), x-xi.
240 Note that a meeting on catastrophe theory was held at Battelle on April 21-25, 1975. P. Hilton, ed., *Structural Stability, the Theory of Catastrophes, and Applications in the Sciences: Proceedings of the Conference Held at Battelle Seattle Research Center 1975,* Lecture Notes in Mathematics, 525 (Berlin: Springer, 1976).
241 I. Stakgold, D. D. Joseph, D. H. Sattinger, eds., *Nonlinear Problems in the Physical Sciences and Biology: Proceedings of the Battelle Summer Institute, Seattle,*

In particular, David Sattinger and Daniel Joseph spoke at great lengths about bifurcations and stability in hydrodynamics, while Oscar E. Lanford, III, introduced the Ruelle-Takens model. A student of Arthur Wightman, who had brought him to the IHÉS in 1963, Lanford was then working in the Mathematics Department at Berkeley. Collaborating with Ruelle on statistical mechanics, he had been invited to come to the IHÉS in 1966-1967.[242] In 1972, at Battelle, we encounter him as a missionary for Ruelle and Takens.[243] Besides introducing the Poincaré map, which apparently was unknown to Joseph, Sattinger, and Iooss (who was not present at Battelle), Lanford proved "Ruelle-Takens theorem." As opposed to Ruelle and Takens's own paper, however, Lanford's had little to do with turbulence and strange attractors.

But the message nonetheless got to Joseph and Sattinger. And they were in the best position to see both the interest and limitations of the Ruelle-Takens proposition. They both found the suggestion very stimulating. In fact, they could even claim to have seen a similar phenomenon, with a more careful identification of the circumstances in which it might happen than Ruelle and Takens's.[244]

> The transition to turbulence through repeating branching [succession of Hopf bifurcations] cannot, however, be the relevant description in the case of subcritical bifurcations. In this case, the time periodic solution which

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Juley 3-22, 1972, Lecture Notes in Mathematics, 322 (Berlin: Springer, 1973), preface.

242 Lettres de Léon Motchane à Robert Oppenheimer (27/3/63); de David Ruelle à Léon Motchane (25/7/66); de Léon Motchane à David Ruelle (20/9/66); de Oscar E. Lanford à Léon Motchane (23/11/66). Arch. IHÉS.

243 O. E. Lanford, "Bifurcation of Periodic Solutions."

244 D. D. Joseph, "Remarks about Bifurcation and Stability of Quasi-Periodic Solutions which Bifurcate from Periodic Solutions of the Navier-Stokes equations," *Nonlinear Problems,* ed. I. Stakgold, et al.: 130-158, 151; D. H. Sattinger, "Six Lectures on the Transition to Instability," *Nonlinear Problems,* ed. I. Stakgold, et al.: 261-287, 268.

> bifurcates from the steady solution is unstable from the start and an arbitrary initial disturbance of the steady solution either decays or is attracted to something else, perhaps a stable 'turbulent solution'.[245]

Since, in the "subcritical" scheme, the periodic solution after the first bifurcation was unstable, this obviously was very different from what Ruelle and Takens had in mind. But, at the same time, the much more careful description of bifurcations that Joseph and Sattinger were able to provide had caught the eye of Ruelle. The appendix of Ruelle and Takens's paper was sent to be typed by an IHÉS secretary on July 15, 1970.[246] There, for the first time, Ruelle made the connection with stability theory, citing the work, much of it published in the *Archive for Rational mechanics,* done on the Taylor and Bénard problems by people such as Yudovich, Welte, Fife and Joseph, all well known stability theorists. A dialogue between specialists and applied topologists (such as hardly ever occurred with catastrophe theorists) could take place.

b) Ruelle and the IHÉS After Ruelle-Takens

On June 4, 1973, David Ruelle wrote to Kuiper to ask him to invite Serrin, Joseph, and Sattinger to the IHÉS, at the same time as he asked for Bowen and Lanford. Clearly, he was intending to spend more time on the theory of dynamical systems. But, this had not been the case prior to 1973.

In interview, Ruelle now says that his article with Takens was but a small incursion into a foreign field, which he was not sure he wanted to pursue much further. To a degree, the archives of the IHÉS confirm this. In 1970-1971, however,

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[245] D. D. Joseph and D. H. Sattinger, "Bifurcating Time Periodic Solutions," 106.
[246] Arch. IHÉS. See the appendix "Bifurcation of Stationary Solutions of hydrodynamical Equations," in D. Ruelle and F. Takens, "On the Nature," 189-191.

when Ruelle spent the academic year at the IAS in Princeton, he went on a tour in order to spread the word about his new model for turbulence. Most notably, he spoke at Boston, where he met Harvard physicist Paul C. Martin who remembers telling him about Edward Lorenz's work;[247] and at Indiana University in January 1971, where Eberhard Hopf was still teaching. He participated in the conference on "Statistical Models and Turbulence" held at La Jolla in July 1971. He also gave series of lectures on turbulence at Boulder and Brandeis. His model was not always well received, since Ruelle recalls C. N. Yang, from SUNY, Stonybrook, joking about his "controversial ideas about turbulence."[248]

But clearly, at that time, Ruelle hardly considered this new orientation in his research as something that should shape his invitation strategy for the IHÉS. Indeed, during most of the spring term 1971, he clashed by mail with Motchane's ambition of hiring as many as three more permanent professors of mathematics. Feeling that "invitations in domains that interest me are sacrificed," Ruelle voted against all three nominations.[249] Coming back to Bures after having spent the year at Princeton, Ruelle envisaged, with the presence of Eliott Lieb, a year 1972-73 with an emphasis on statistical mechanics. In January, he proposed a "grandiose program" for 1973-74; he planned to invite "the big people of constructive field theory, which is probably the

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[247] In the interview of Paul C. Martin conducted by the author (7 May 1996), it was not exactly clear when this happened. It might have been later (in 1973 or even 1975), since Ruelle did not start addressing the Lorenz attractor before the summer of 1975. Martin however remembers that it took a while for Ruelle to see the relevance of Lorenz's work.

[248] D. Ruelle, *Chance and Chaos*, 66.

hottest thing in mathematical physics nowadays." In February, he was in favor of organizing an "astrophysical year" for 1974-75 in collaboration with the CNRS, contending that "one can think that, in the future, the most active developments of theoretical physics will be obtained in, or via, astrophysics." None of these projects had much to do with his work on dissipative systems.[250]

The only exception was the invitation Ruelle sent to Jerrold Marsden for the academic year 1971-1972.[251] A student of Wightman's at Princeton University, Marsden had written his thesis "with much inspiration from Ralph Abraham" in 1967-68. A great "note-taker," he helped Abraham prepare the publication of his famous lecture notes.[252] Having read one of V. I. Arnol'd's papers in fluid mechanics, he moved to Berkeley where he attended Smale's seminars, teamed up with David Elbin, and started to work on hydrodynamics.[253] This was not a popular field for theoretical physicists at the time; he remembers having been told to "stop wasting [his] time."[254] Like Lanford and Ruelle, Marsden was the kind of mathematical physicist who could

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[249] Lettre de David Ruelle à Léon Motchane (25/6/71). Offers were made to Bomberi, Langlands, and Armand Borel, all of whom rejected the offer. See *Comité scientifique* (25/6/71). Arch. IHÉS.

[250] *Rapport du Comité scientifique* (22/10/71); *Petit Comité scientifique* (10/1/72); *Petit Comité scientifique* (7/2/72). Arch. IHÉS.

[251] *Comité scientifique* (28/6/70). Arch. IHÉS.

[252] R. H. Abraham and J. E. Marsden, *Foundations of Mechanics* (New York: W. A. Benjamin, 1967). It was Wightman who, in interview, called Marsden a "great note-taker."

[253] V. I. Arnol'd, "Sur la géométrie différentielle des groupe de Lie de dimension infinie et ses applications à l'hydrodynamique des fluides parfaits," *Annales de l'Institut Fourier de Grenoble,* 16(1) (1966): 319-361.

[254] By Wigner or Wheeler, he was not sure. See J. Marsden's acceptance speech in "1990 Norbert Wiener Prize in Applied Mathematics Awarded in Columbus," *Notices of the American Mathematical Society,* 37 (1990): 808-811, 810.

build bridges across disciplines. But Marsden was not part of the stability theory community.

"I think that Marsden's visit will be very valuable from a scientific viewpoint," Ruelle wrote to Motchane; he "should in particular interest Thom."[255] Indeed, in the spring term of 1972, while Abraham was also at the IHÉS, Marsden gave several talks in Thom's seminar, in which he addressed the issue of the "Onset of Turbulence."[256] Marsden also used the opportunity of being at the IHÉS to work on papers in which he studied the Hopf bifurcation, and reviewed different models for the onset of turbulence including Ruelle and Takens's.[257]

That same year, the theoretical physics seminar of the IHÉS welcomed a talk by Paul C. Martin, who had showed an early interest for the Lorenz model, seeing it as an instance where turbulence set in suddenly without following Landau's scheme. Like Ruelle a specialist in axiomatic quantum field theory, statistical mechanics, and the many-body problem, Martin was spending the year at Saclay and Orsay in de Gennes's group. His study of phase transitions led him to believe that he could have something to say about the onset of turbulence. On April 12, 1972, he gave a talk in the Ruelle-Michel seminar entitled: "Schwinger-Feynman Techniques in Classical

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[255] Lettre de David Ruelle à Léon Motchane (8/12/70). Arch. IHÉS.
[256] R. Abraham's seminar in Thom's applied global analysis seminar (22/2/72) was "Hydrodynamic Bifurcations according to Ruelle and Takens;" D. Ruelle (28/2/72): "Bifurcations with Symmetry;" J. Marsden (13/3/72): "The Onset of Turbulence." *Rapport scientifique 1972.* Arch. IHÉS.
[257] J. E. Marsden, "A Survey of Some Recent Applications of Global Analysis to Hydrodynamics," *Quatrième rencontre entre mathématiciens et physiciens, 1-5 mars 1972* 4(2), Supplement 2 to *Publications du département de mathématique de l'Université de Lyon-1,* 9 (1972): 194-207; "The Hopf Bifurcation for Nonlinear Semigroups," *Bulletin of the American Mathematical Society,* 79 (1973): 537-541.

Fluid Dynamics."[258] One should further note the presence at the IHÉS in 1972 of O. E. Lanford, who however spoke on statistical mechanics.

Meanwhile, in 1972, Ruelle gave only two talks devoted to "turbulence et attracteurs étranges" (at Moscow and in Israel), while delivering many more on statistical mechanics. That year, he nonetheless wrote two papers dealing with the Hopf bifurcation.[259] Very different from one another, these two papers show that Ruelle's incursion in the field of dynamical systems was turning into an actual change in orientation. This time accepted by the *Archive for Rational Mechanics,* his paper on "Bifurcations" was a more formal treatment of the Hopf bifurcation, but significantly it scarcely dealt with phenomena which are today associated with chaos and turbulence. Formal bifurcation theory did not allow him to go further.

The other paper, on chemical oscillations, is much more interesting from the point of view of the history of chaos. There, a number of important themes recurrent in later works on chaos theory were raised and argued for the first time. Having visited Prof. B. Chance in his group on chemical oscillations at the University of Philadelphia, Ruelle grasped the possibility of applying the Ruelle-Takens model to a totally different case. One should note that Sattinger had also remarked on the similarity of hydrodynamic and chemical stability, as early as 1971.[260] These parallels

[258] Interview of P. C. Martin by the author (7 May 1996); *Rapport scientifique 1972.* Arch. IHÉS.
[259] D. Ruelle, "Bifurcations in the Presence of a Symmetry Group," *Archive for Rational Mchanics and Analysis,* 51 (1973): 136-152; "Some Comments on Chemical Oscillations;" *TSAC,* 91-108 and 109-115.
[260] D. H. Sattinger, "Stability of Bifurcating Solutions," 165; where he cited G. R. Gavalas, *Nonlinear Differential equations of Chemically reacting Systems* (New York: Springer, 1968).

were one of the themes of the 1972 Battelle Summer Institute, where Grégoire Nicolis was present representing Prigogine's School in Brussels, which was interested in the topic.[261]

But Ruelle went further than this; he also put forward two important characteristics of turbulence as he saw it. Whether manifested in fluids or chemical reactants turbulence involved nonperiodic solutions and "sensitiveness to initial conditions."[262] These properties opened the door for careful experimental confirmation of his theories. The conclusion was clear to Ruelle:

> The bifurcations that lead to such "turbulent" solutions are difficult to study mathematically, but turbulent time behavior should be easy to recognize when it occurs experimentally. This behavior might easily be overlooked in chemical systems as "messy, unusable data." The phenomenon is in fact respectable.[263]

Ruelle, it seems, was now expecting a confirmation of his model not from theory, but from experiments.

Only during the spring of 1973 did Ruelle finally propose, with Thom's support, that a year starting in the spring of 1975 be devoted to "Dynamical systems, statistical mechanics, and turbulence" at the IHÉS. Coming back from Moscow, Ruelle also suggested that Soviet mathematicians Arnol'd and Sinaï be considered for permanent professorships at the IHÉS, although under the conditions reigning in the

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[261] See e.g. I. Prigogine, *Introduction à la thermodynamique des processus irréversibles* (Paris: Dunod, 1968), chap. 8, where chemical oscillations, Bénard oscillations, and the Lotka-Volterra equations were treated as instances of instability. About this, see Chapter VIII above.
[262] Sensitivity to initial conditions was already mentioned by D. Ruelle, "Strange Attractors as a Mathematical Explanation," 293.
[263] D. Ruelle, "Some Comments," 70; *TSAC,* 114.

USSR, this was difficult to envision. Finally, his inroad into the theory of dissipative dynamical systems had begun to shape Ruelle's duties at the IHÉS.

Simultaneously, he came in contact with Rufus Bowen, whom we may recall as "Smale's best student." On June 4, 1973, Ruelle wrote to Kuiper in the letter mentioned above that he wanted to invite him to the IHÉS: "I also had useful scientific contacts [while in California, from April to August 1973], particularly with Bowen. . . . He impresses me very much, and I am in favor of making soon a firm and attractive offer [to him]."[264] In fact, the case of Bowen is revealing of the passing of the initiative from Thom to Ruelle in the pursuit of the best use of dynamical systems theory for the study of physical systems. Indeed, just the year before, it was to Thom that Bowen had written in order to be invited to the IHÉS (although, in the end, he chose not to come).[265] With Bowen, Ruelle would soon publish the article marking his full involvement in dynamical systems theory.[266] This paper marks the meeting point of his earlier work on statistical mechanics and his new concerns for dissipative systems, which would set the direction of his future research, and provide an axis around which the activities of the IHÉS in this domain would be built.

It was in the same letter to Kuiper that Ruelle suggested that James Serrin, Daniel Joseph, and David Sattinger be invited to the IHÉS. At the meeting of the Scientific Committee on November 16, 1973 the impulse was definitely given for

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[264] Lettre de David Ruelle à Nicolaas Kuiper (4/6/73); *Comité scientifique* (24/3/73); *Rapport scientifique 1973*. Arch. IHÉS.
[265] Lettres de Rufus Bowen à René Thom (24/2/72); and (24/5/72). *Comité scientifique* (14/4/72). Arch. IHÉS.
[266] R. Bowen and D. Ruelle, "The Ergodic Theory of Axiom *A* Flows," *Inventiones Mathematicae,* 29 (1975): 181-202; *TSAC,* 153-174.

developing the activities of the IHÉS around the themes of dynamical systems, turbulence, and statistical dynamics, but now under Ruelle's lead rather than Thom's,. Moreover, David Sullivan, from MIT, then visiting at Orsay and the IHÉS, was now considered as the more likely prospect for the creation of a new position.

> The range of interest and our expectation of necessary moves to new fields in the course of life seems greater with Sullivan [as opposed to B. Mazur, and J. Mather, also considered]. Sullivan attacks hard and most fundamental problems, and does aim at finding the e[ss]entials. He has a great technical power at the same time and uses it.[267]

With the offer of a position, he became interested in "the theory of singularities of complex manifolds and in dynamical systems. Both of these topics," Kuiper wrote, "presently are in full activity, notably in the Paris region and more especially at the IHÉS (Thom-Ruelle-A'Campo)."[268] Sullivan accepted the offer and began at the IHÉS in September 1974.[269] Clearly, the IHÉS was now launched as a major center in the theory of dynamical systems, still envisioned in its mathematical aspects, but also with respect to its relevance for mathematical physics, particularly turbulence and statistical mechanics.[270]

In the later part of the 1970s, the IHÉS thus became one of the breeding grounds from which chaos theory emerged by the end of the decade. But at the same time, the landscape became much broader with infusions from a wide array of backgrounds. This central issue in the emergence of chaos will be partly addressed in

[267] Mémo, by N. Kuiper; *Comité scientifique* (16/11/73). Arch. IHÉS.
[268] Lettre de Nicolaas Kuiper à François Le Lionnais (24/4/74). Arch. IHÉS.
[269] *Comités scientifique* (19/4/74). *Compte-rendu* (dated 8/5/74). Arch. IHÉS.
[270] Let me furthermore note a talk by C. Foias, from Budapest, on November 5, 1974 on "Connections entre les équations de Navier-Stokes et la théorie de la turbulence." *Rapport scientifique 1974.* Arch. IHÉS.

Chapter VIII below. But before it will be useful to sketch out the activities of Joseph, Sattinger, and Iooss, who would play a role in this emergence.

c) Stability Theorists in the Age of Chaos

Gérard Iooss, Daniel Joseph, and David Sattinger had too fine a knowledge of the bifurcations involved in hydrodynamic systems, and of the differences of each particular situation, to convert wholly to the Ruelle-Takens model. Each of them would go on working on the problem and achieve imposing synthetic pictures of nonlinear stability theory. At the same time, they worked on building linkages between this theory and related ones which explored bifurcations and loss of stability in many physical systems. In this sense, they all became actors in the emergence of chaos.

With his interest in bifurcation theory, it was only fitting that David Sattinger, from the Mathematics Department of the University of Minnesota, was the first of the three to come to the IHÉS. He came as one of the "attractors" for the "year" on dynamical systems and ergodic theory planned for the spring semester, 1975, at the same time as Lanford and Bowen, among others.[271] Sattinger was also the first of the three to publish a monograph dealing with their common endeavor.[272] Consisting of lecture notes for a course given at the University of Minnesota in 1971-72, this book barely touched with the Ruelle-Takens model.

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[271] Sattinger spoke "On the Free Surface of a Viscous Fluid in Motion," on May 14, 1975. *Rapport scientifique, Année 1975 - Séminaires et conférences,* 8. Arch. IHÉS.
[272] D. H. Sattinger, *Topics in Stability and Bifurcation Theory,* Lecture Notes in Mathematics, 309 (Berlin: Springer, 1973).

As early as 1967, Clifford Truesdell had started pushing Daniel Joseph, from the Department of Aerospace Engineering and Mechanics at the University of Minnesota, to write a book on the *Stability of Fluid Motions.* "The theory of stability has developed so rapidly since 1967 that the book I might then have written would now have much too limited a scope."[273] In 1976, he finally published a two-volume compendium of hydrodynamic stability theory. Interestingly, Joseph acknowledged Sattinger, for reminding him that stability theory was part of mathematics, and Fritz Busse, for reminding him that it was part of physics as well. Restricting the Ruelle-Takens model to a very small part, Joseph's book dealt in detail with all simple cases of fluid flows: Poiseuille, Couette, Bénard, the flow past a sphere, etc.

His comments about Hopf, Landau, and Ruelle-Takens were particularly revealing of stability theorists' attitude in the face of the Ruelle-Takens model. In a long note, he deplored the fact that this model "left vague" many characteristics of the flow. Joseph observed that it rested on some restrictive assumptions, needing "justification at many points," especially since catalogues of attractors for ordinary differential equations, not to mention the Navier-Stokes equations, "still elude[d] analysis." In summary, Joseph thought that it was "a step forward to think of non-periodic, phase-mixing attractors in the description to turbulence." But it clearly was far from supplying the universal "mechanism for the generation of turbulence" that Ruelle and Takens had claimed. In particular, it led to no description of the spatial features of turbulence and forgot about the wholly different possibility of subcritical

[273] D. D. Joseph, *Stability of Fluid Motions,* Springer Tracts in Natural Philosophy, 27-28 (Berlin: Springer, 1976), 1: v.

bifurcations.[274] Already considered by Ruelle in 1973, Joseph was formally invited to spend the academic year 1979-80 at the IHÉS. He did indeed come in 1980-81, but spent most of his time, either at Orsay with Roger Tenam, or at Nice with Gérard Iooss.[275]

Their relative skepticism concerning the Ruelle-Takens model notwithstanding, both Sattinger and Joseph played a definite role in the emergence of chaos. Joseph, for example participated in the Gordon Research Conference on "Dynamical Instabilities and Fluctuations in Classical and Quantum Systems." Organized by Paul C. Martin and Jerry P. Gollub and held in New Hampshire on July 19-23, 1976, it was "aimed primarily toward the review of specific time-dependent non-equilibrium phenomena on which experiments are being performed, and an assessment of whether there are helpful mathematical techniques that can be brought to bear." This conference might be interpreted as still another of the birth places of chaos theory.[276] Both Sattinger and Joseph also spoke at the famous conference on

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[274] D. D. Joseph, *Stability of Fluid Motions,* 58-60. Ruelle and Takens's quote is from the Abstract of "On the Nature," 167.

[275] Lettres de David Ruelle à Daniel Joseph (11/1/78); de Daniel Joseph à David Ruelle (10/2/78); de Roger Tenam, Orsay, à Daniel Joseph (2/5/78); de Daniel Joseph à Nicolaas Kuiper (24/5/78); de Nicolaas Kuiper à Daniel Joseph (5/4/79); *Comité scientifique* (23/9/78); *Comité scientifique* (10/3/79); *Assemblée générale* (8/5/78): "l'ouverture voulue par Monsieur Ruelle va dans la direction de la mécanique et de l'Hydrodynamique. La visite du Professeur Daniel Joseph (Minnesota) pour l'année académique 78-79 nous donne un espoir dans cette direction." Arch. IHÉS.

[276] Among those present, one notices G. Ahlers, R. P. Behringer, R. Bowen, B. Derrida, R. J. Donnelly, M. J. Feigenbaum, J. P. Gollub, J. Guckenheimer, H. Haken, D. D. Joseph, E. L. Koschmieder, A. Libchaber, P. C. Martin, J. B. McMaughlin, G. Nicolis, H. L. Swinney, G. Toulouse, R. Williams, and A. T. Winfree. Memo from P. C. Martin and J. P. Gollub to all speakers and suggested chairman (July 6, 1976). I thank P. C. martin for providing me this letter, the program of the conference, and a list of people who attended it.

"Bifurcation Theory and Applications," held in October 1977 under the auspices of the New York Academy of Sciences.[277]

Gérard Iooss, who was the most mathematically-minded of the three, never quite achieved the same international status as Joseph and Sattinger in early chaos conferences (perhaps because he often wrote in French). In 1972-73, he taught courses at Orsay where he did not consider the Ruelle-Takens model. In 1975, however, directly inspired by this model, he studied analytically the bifurcation of periodic solutions into an invariant torus.[278] Having moved to Nice, he started collaborating with Alain Chenciner, a student of Thom's, moving towards a dynamical systems analysis of the onset of turbulence. In Nice, a "meeting between physicists and mathematicians about non-linear problems and their applications" was organized in September 1977 by J. Coste, P. Coullet, and A. Chenciner. There, Iooss carefully reviewed the Ruelle-Takens scenario, noting that much computation and experimental work remained to be done in order to check it rigorously.[279] In a 1979 monograph on bifurcation theory, written for a course he taught at the University of Minnesota in the fall of 1977, he did not dwell on Ruelle-Takens. But in 1981, he was one of the

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[277] D. H. Sattinger, "Spontaneous Symmetry Breaking in Nonlinear Problems," and D. D. Joseph, "Factorization Theorems and Repeated Branching of Solutions at a Simple Eigenvalue," *Bifurcations Theory and Applications in Scientific Disciplines,* ed. O. Gurel and O. E. Rössler (New York: New York Academy of Sciences, 1979): 49-63; and 150-167.

[278] G. Iooss, *Bifurcation et stabilité;* "Bifurcation of a Periodic Solution of the Navier-Stokes Equations into an Invariant Torus," *Archive for Rational Mechanics and Analysis,* 58 (1975): 35-36; "Sur la deuxième bifurcation d'une solution stationnaire de systèmes du type Navier-Stokes," *Archive for Rational Mechanics and Analysis,* 64 (1977): 339-369.

organizers of a summer school in Les Houches devoted to the "Chaotic Behaviour of Deterministic Systems."[280] Like Joseph and Sattinger, Iooss had become a chaologist, albeit maintaining some level of skepticism.

For stability theorists, then, the Ruelle-Takens model remained at best an interesting suggestion which they sometimes exploited. But it did not solve the problem of turbulence because its mathematical foundations remained somewhat shaky. Their modeling practice received an infusion from dynamical systems theory and they started to look for attractors, but the problems were to be solved case by case, by a careful analytic study of the equations, still using the methods of functional analysis: existence and uniqueness theorems. I believe that this historical process was what Ruelle was describing when he wrote:

> the "theory of chaos" . . . is a specific bag of tools. . . . In the favorable cases, the new ideas are integrated and accepted (so that we know that turbulence is chaotic . . .). Then each domain of research resumes its own individuality, and can no longer be called "a branch of the theory of chaos." The whole process takes a few years. Very roughly I would say . . . from 1971 to 1986 for turbulence.[281]

For the general community of physicists, however, the lesson to be drawn from the Ruelle-Takens model was different. Numerical and laboratory confirmations seemed sufficient to show the fruitfulness of the approach. It had introduced an alternative

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279 G. Iooss, "Bifurcations successives et stabilité," *Journal de physique,* 39 (1978), Colloque C5 ["Rencontre entre physiciens et mathématiciens sur quelques problèmes non linéaires et leurs applications"]: 99-105.
280 G. Iooss, *Bifurcation of Maps and Applications* (Amsterdam: North-Holland, 1979; G. Iooss, R. H. G. Hellerman, and R. Stora, eds., *Chaotic Behaviour of Deterministic Systems: Les Houches Summer School, Session XXXVI, 1981* (Amsterdam: North-Holland, 1983).
281 D. Ruelle, "Introduction," *TSAC,* xiv-xv.

modeling practice for physicists that was there to stay. This process, a complex one, will be the topic of Chapter VIII below.

7. CONCLUSION: BOURBAKI AND THE COMPUTER

The above summarily traced back shifting commitments in favor of the Navier-Stokes equations by various groups of scientists working on the turbulence problem. Once it was recognized that even in very simple cases, some exact solutions of the Navier-Stokes equations could not be describing certain observed flows, this problem always posed a challenge to the belief that these equations were a fundamental law of physics. Successes confirmed this belief; difficulties and controversies made the foundation shakier. Ultimately, commitments to the Navier-Stokes equations stemmed from a variety of evidence, taken away from widely different theoretical approaches, experiments, and numerical computation.

But throughout this history, the final aim of finding an equation that would faithfully describe the dynamics of fluid flows was almost never questioned. One thought of tinkering with the Navier-Stokes equations, but never that such a description of fluid flows could be intrinsically faulty. What the model suggested by Ruelle and Takens suggested, however, was much more radical, and this was shared by some of the later stability theorists' approaches. In short, they proposed that topological methods might be powerful enough to bypass the problem. The Navier-Stokes equations might—or might not—be a totally faithful description of fluid flows. This did not matter anymore. The only portion of the prior beliefs that remained was that some differential equation, endowed only with the few necessary properties, did indeed accurately describe the flow, but one did not need to know it in order to extract fruitful information about turbulence. This was the meaning of "genericity," which would often be replaced by "universality" during the following decade.

Instead of being based on a specific equation, the modeling practice of some scientists working on turbulence began to be founded on experimentally observable topological features which were robust with respect to variations of the no-longer-so-fundamental differential equation. This entailed a change in the basic building blocks of mathematical models for the onset of turbulence. They now were topological features, as opposed to analytic solutions of an equation. This of course implied that the mathematical techniques used to study the onset of turbulence became topological, coming from dynamical systems theory or functional analysis. Finally, the consequences derived from a topological study of fluid flows were interpreted as a better understanding of turbulence, even while specific mechanisms for the generation of turbulence remained elusive. In short, this modeling practice dispensed with prior aims at avoiding turbulence. Understanding replaced action as the main goal of model-building for turbulence.

"The general lesson" of a decade of work on deterministic theories of turbulence, David Ruelle contended in 1983,

> seems to be that hydrodynamical systems at the onset of turbulence behave very much as generic differentiable dynamical systems in finite dimension. Simple systems of differential equations with arbitrarily chosen coefficients, when studied by digital or analog computers, yield data so analogous to those of hydrodynamical experiments that it is not possible to tell them apart.[282]

[282] D. Ruelle, "Differentiable Dynamical Systems and the Problem of Turbulence;" *TSAC*, 238.

In this situation, what then was the use of the Navier-Stokes equation? This conclusion of Ruelle and Takens was the result of three main strains: earlier models for the onset of turbulence; the adaptation of concepts and practices from dynamical systems theory for which Thom was a major mediator; and Ruelle's earlier commitment to a theoretical physics relying on technical mathematical methods inspired by a Bourbakist attitude. Their suggested model departed in practice from earlier ones. A major lesson was that the topological, structural, yet dynamical study of bifurcations and attractors, as opposed to reduction to ultimate molecules or fundamental laws, could have a high reward.

The following testimony of Leo Kadanoff, a physicist from the University of Chicago who was rather a late-comer to the study of chaos, is revealing of the change in attitude with respect to specific models.

> [A]t a crucial moment, Bob Gamer, a colleague in Chemistry, asks why I am devoting so much work to a particular model system. His implication is that *the model is not so real as to be of practical interest, and perhaps not so deep as to have real intellectual interest*. . . I know that his implied criticism is right. So I resolve to learn something new. The new subject I find is dynamical systems theory.[283]

Significantly, this kind of remark had already been made more than fifty years before by George D. Birkhoff:

> At a time when no physical theory can properly be termed fundamental—the known theories appear to be merely more or less fundamental in certain directions—it may be asserted with confidence that ordinary differential equations of dynamical origin will continue to hold a position of the highest importance.[284]

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[283] L. P. Kadanoff, *From Order to Chaos. Essays: Critical, Chaotic, and Otherwise* (Singapore: World Scientific, 1993), 386. My emphasis.

[284] G. D. Birkhoff, *Dynamical Systems* (Providence: AMS, 1927), vi.

In the 1970s, a certain disenchantment with reductionist approaches started more and more to touch physicists, not all of them of course, but a significant portion of the profession. As Princeton physicist Philip Anderson once contended:

> The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe. In fact, the more the elementary particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very problems of the rest of science, much less society.[285]

Fundamental laws, some began to think, were not so certain anymore. But more importantly, they felt they could provide meaningful models that did not rely on a perfect knowledge of God's mind, to use one of Einstein's phrases. The dynamical systems approach, and soon chaos theory, could lead to profound understanding of natural phenomena, without much ontological commitment to fundamental equations. The question is why this happened.

No simple answer will suffice. In this dissertation, it is argued that new modeling practices stemmed from a wide variety of sources, ranging from technical innovation in mathematics to more diffuse cultural factors. From the above discussion two hypotheses surface, which hardly settle the question. First, it has been tempting, by studying Ruelle's career trajectory, to conclude that a certain 'Bourbakization' of theoretical physics allowed the emergence of a new alternative for the physicist's modeling practices. Second, we may think that a very important change in the material conditions for the practice of theoretical physics made itself felt, namely the rising availability and use of computers.

A feeling among mathematicians that their methods allowed them to understand scientific problems, sometimes more deeply than the specialists themselves, is hardly new, as the following statement of Garrett Birkhoff will make explicit:

> During the [second world] war I found that my ability to diagnose fluid mechanics even with a rather limited knowledge of it . . . was very useful to both the Army and the Navy. I decided that if opportunity permitted after the war I would try to see what could be done scientifically . . . to treat some of the questions.[286]

From the early 1960s to the early 1980s, at least, the Institut des hautes études scientifiques was a singular place where a fruitful dialogue between mathematicians and physicists, resulting in a sharing of concepts and practices, was established and sustained. Physicists used the latest mathematical tools available, organized in a Bourbakist way. An intense activity on the part of some physicists took place, aimed at studying physical systems with the greatest generality possible. Bourbaki wished to do away with most properties of the real numbers when he wanted to establish some theorem that only required, for example, the group structure. Similarly, Ruelle and Takens endeavored, in their famous paper, to uncover the structure (almost in Bourbaki's sense) of turbulence as such, without relying on unnecessary, superfluous properties of the Navier-Stokes equations. In this sense, one may speak of a Bourbakization of the physicist's modeling practice.

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[285] Quoted in M. M. Waldrop, *Complexity: The Emerging Science at the Edge of Chaos* (New York: Simon & Schuster, 1992), 81. See also P. W. Anderson, "La grande illusion des physiciens," *La Recherche,* 11 (1980): 98-102.
[286] G. Birkhoff's interview, *Mathematical People: Profiles and Interviews,* ed. D. J. Albers and G. L. Alexanderson (Boston: Birkhäuser, 1985), 13.

Of course, one cannot go too far in this direction. This new alternative for the physicist's modeling practice was not the consequence of just one paper by Ruelle and Takens, no matter how seminal it may have been. Many physicists did not flirt with Bourbaki. The next chapter, which examines the way in which Ruelle's innovations would be taken up by a variety of scientists, will plainly show this. Moreover, principal propagandists for alternative modeling practices, such as Thom, started very early on to break free from Bourbaki's dogmatism, and with good reason since they actually looked for original ways to escape the ivory tower of pure mathematics.[287]

On the other hand, changing conditions for the modeling of natural phenomena—the rising availability of computers—played a definite role even for those who chose not to rely on them. Again, the case of Ruelle provides an interesting perspective one this impact of computers. Even if he never used them in any crucial way, Ruelle clearly saw, as early as 1964, that theoretical physics had to change its ways confronted with

> an event which no doubt shall have profound repercussions on the evolution of statistical mechanics, [namely] the development of electronic computers. These allow not only to treat numerically problems up until now unreachable, but also to reproduce the evolution of systems [involving] a few hundreds of particles, allowing their "experimental" study.

The lesson Ruelle took away from this "event" was that techniques of approximation had to be adapted to the new situation, "or die." But what interested him most was that even "techniques for the rigorous study" of statistical mechanics

[287] Let me mention that an attempt at formalizing physical way in a strict Bourbakist way was made by a philosopher: W. Stegmüller, *The Structuralist View of Theories: A Possible Analogue of the Bourbaki Programme for the Physical Science* (Berlin: Springer, 1979).

had to be adapted, "for calculators will be more and more able to provide convincing, if not rigorous, answers to the problems of statistical mechanics." That is, Ruelle felt that the study of the conditions in which numerical solutions were, or were not, meaningful was becoming a major task for the mathematical physicist. One had to ponder the way problems were posed in theoretical physics.[288] As Ruelle wrote much later, the computer as such was no solution:

> The use of modern computers . . . has had for example a considerable impact in the studies of hydrodynamic turbulence. But scientific progress requires specific ideas and methods like those constituting the "theory of chaos."[289]

Already in 1964, it seems that Ruelle geared his research program towards a better understanding of the mathematics fundamental to the modeling practices of theoretical physicists, so that the computer be used more efficiently in their discipline. "The question," Ruelle asserted, "will soon be raised to know whether it is the calculator or the researcher that is the tool of the other."[290]

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[288] All of Ruelle's quote above are taken from: Lettre de Léon Motchane à Lucien Malavard (17/11/64), which included Ruelle's own description of his project for a contract with the DRME. Arch. IHÉS.
[289] D. Ruelle, "Introduction," *TSAC,* xiv.
[290] Lettre de Léon Motchane à Lucien Malavard (17/11/64). Arch. IHÉS.

CHAPTER VIII: CHAOS

La cause en est un je ne sais quoi, et les effets en sont effroyables. Ce je ne sais quoi, si peu de chose qu'on ne peut le reconnaître, remue toute la terre, les princes, les armes, le monde entier.
Le nez de Cléopâtre: s'il eût été plus court, toute la face de la terre aurait changé.
–Blaise Pascal, *Pensées,* no. 162[1].
Our findings showed that there was a form of predictible order in the randomness of turbulent motion. There was, as my father had taught me, a natural law guiding the chaos.
—Theodore von Kármán.[2]

1. INTRODUCTION

From June 30 to July 4, 1975, around a hundred physicists, the great majority of them being French, gathered in Dijon for a symposium devoted to the study of "Physical Hydrodynamics and Instabilities." The only American speaker at the Dijon Symposium, Harvard physicist Paul C. Martin was in no way what we might call a specialist in fluid mechanics. However, his talk was among the very few where the Ruelle-Takens model was discussed as one of the "illuminating and suggestive"

[1] "The cause is a *je ne sais quoi,* and its effects are appalling. The *je ne sais quoi,* which is such a tiny thing that we cannot even recognise it, rocks the world, thrones, armies, the whole of the creation to their foundation. The nose of Cleopatra: if it had been shorter, the face of the whole earth would have changed." Transl. M. Turnell (London: Harvill, 1962), 133.

[2] T. von Kármán, with L. Edson, *The Wind and Beyond: Theodore von Kármán, Pioneer in Avation and Pathfinder in Space* (Boston: Little, Brown, 1967), 139.

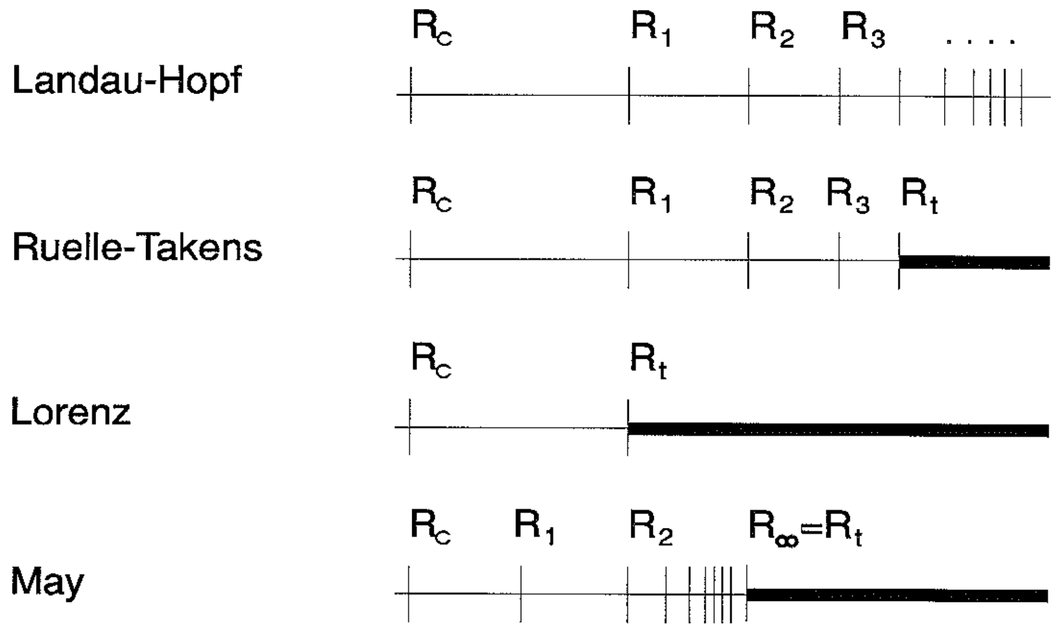

Figure 15: Landau's Picture for the Onset of Turbulence, and Three Alternatives. Redrawn from P. C. Martin, "Instabilities, Oscillations, and Chaos," Fig. 4, C1-60.

models for the onset of turbulence of the last few years. But, five years after the pair of IHÉS scientists had made their suggestion, Martin's talk was hardly an enthusiastic 'response' to it.

> The basis for their proposal [Ruelle and Takens's] seems somewhat arbitrary to me but that may reflect my ignorance. Their prediction seems to agree with calculations by John McLaughlin and myself, for a problem of the Bénard type. While our calculations seem to be in semiquantitative accord with the experiments and to exhibit features of the Ruelle-Takens picture, the agreement could be fortuitous.[3]

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[3] P. C. Martin, "Instabilities, Oscillations, and Chaos," *Journal de physique,* 37, *Suppl.,* Colloque C1 (1976): 57-66, 60; where the proceedings of the Dijon Symposium were published. See also another similar talk he delivered in Budapest during the same summer: P. C. Martin, "The Onset of Turbulence: A Review of Recent Developments in Theory and Experiment," *Statistical Physics: Proceedings of the International Conference,* ed. L. Pál and P. Szépfalusy (Amsterdam: North-Holland, 1976): 69-96.

One of the reasons why Martin did not feel more confident about the Ruelle-Takens model was that, as he wrote, "it is not inevitable." Indeed, he had uncovered a model studied by Edward Lorenz more than ten years earlier (Chapter V), which, according to Martin, was the simplest "counterexample" to the view that the onset of turbulence had to follow the path suggested by Ruelle and Takens, which therefore might not be as "generic" as they had claimed.

Instead of exhibiting the successive appearance of oscillations, Lorenz's model suggested that a *sudden* transition to turbulence from a stationary solution was possible, contrary to Ruelle and Takens's claim. Moreover, Martin noted, experiments performed by Günter Ahlers, from Bell Labs, exhibited a behavior that "was reminiscent of the Lorenz model." By mentioning the work of Fritz H. Busse, from the Institute of Geophysics and Planetary Motion at UCLA, Martin made the connection with physicists who had long experience in studying convection both experimentally and theoretically. Finally, by pointing out the possibility of a third picture for the onset of turbulence, which "may not have much bearing on fluid dynamics, but . . . bears mention," namely the bifurcation cascade studied by Robert May, Tien-Yien Li, and James A. Yorke, Martin reintroduced the word "chaos" into France.[4] In short, in this remarkable talk, Martin put in place the necessary connections between different fields of science, and distinct groups of scientists, which would be the core around which chaos would emerge in the years to follow

[4] As we have seen in Chapter VI above, the often heard assertion that Li and Yorke first came up with the term "chaos" can, at least, be debated on sound historical bases.

(Fig. 15). For the very first time, a single picture encompassed three of the now classic ingredients of chaos theory, namely the works of Lorenz, May, and Ruelle-Takens.

a) Reception of the Ruelle-Takens Model

This chapter deals with the reception of the Ruelle-Takens model by the various communities that would find in it a useful resource to help forge a common language to speak about instabilities in fluids and elsewhere. It explores the convoluted ways in which this paper came to stand as a seminal one. It shows the complex alliances that made it possible that concepts and practices from dynamical systems theory massively entered physics.

At first glance, one might be tempted to say that Ruelle and Takens's model remained controversial for physicists as long as it was neither verified experimentally, nor based on sound, rigorous mathematical bases they could understand. The mathematical foundations for the model, as we saw in Chapter VII, remained unclear well into the 1980s. But around 1974, two "crucial experiments" were performed which provided some grounds to believe that the phenomena predicted by Ruelle and Takens were indeed observed in reality. In one of these experiments, done by American physicists Harry L. Swinney and Jerry P. Gollub at the City College of New York, the transition to turbulence in the Taylor-Couette flow was observed. They determined that the fluid indeed went through a few bifurcations where new periods appeared successively. Then, the transition to aperiodic behavior occurred suddenly, indicating that, out of the four pictures mentioned by Martin, Ruelle and Takens's was the closest to the observed behavior of Taylor-Couette flows.[5]

The other 'experiment' that seemed to confirm the Ruelle-Takens model consisted in numerical computations of the convective instability. It was performed by graduate student John B. McLaughlin at Harvard with the help of his advisor, Paul Martin, and seemed to be in "semiquantitative agreement" with the model suggested by Ruelle and Takens. Indeed, they noticed that in general the transition to turbulence was sudden, contrary to Landau's scheme. For them, however, this was only a partial confirmation of Ruelle and Takens's model.[6]

To use Morris Hirsch's phrase, it would seem that the "dynamical systems approach" to the study of turbulence was thereby vindicated.[7] Starting in 1975, a flurry of experimental results, theoretical studies, and numerical experiments would only further this trend. A *chaos constellation,* let me say, emerged that would select Ruelle and Takens's model as one, but only one, paradigmatic exemplars. This constellation, formed of groups of scientists coming from a wide variety of backgrounds, would promote the study of turbulence and various other phenomena with the help of modeling practices making a wide usage of theoretical technologies

[5] J. P. Gollub and H. L. Swinney, "Onset of Turbulence in a Rotating Fluid." *Physical Review Letters,* 35 (1975): 927-930; repr. *Univ. Chaos,* 143-145; H. L. Swinney and J. P. Gollub, "Transition to Turbulence," *Physics Today,* 31(8) (1978): 41-49. Recall that the Taylor-Couette flow is that of a fluid contained between two coaxial cylinders which are rotating at different angular velocities. See Chapter VII.
[6] J. B. McLaughlin and P. C. Martin, "Transition to Turbulence of a Statically Stressed Fluid," *Physical Review Letters,* 33 (1975): 1189-1192; "Transition to Turbulence in a Statically Stressed Fluid." *Physical Review A,* 12 (1975): 186-203.
[7] M. W. Hirsch, "The Dynamical Systems Approach to Differential Equations," *Bulletin of the American Mathematical Society,* n.s., 11 (1984): 1-64.

specific to dynamical systems theory. Strange attractors did indeed provide an explanation for turbulence!

In the following, we shall see that the process by which modeling practices of dynamical systems theorists were adopted and adapted by physicists was a bit more complicated and interesting. Indeed, I shall show that the modeling practices adopted by chaologists were just as much informed by the experiments as they were by dynamical systems theory. Chaologists thereby forged what we may call an *experiment-based mathematical modeling practice* using concepts from qualitative dynamics.

This chapter deals with the reception of the Ruelle-Takens by physicists, showing that this reception was not a 'response'. One well-studied instance of the reception of a controversial theory is provided by reactions to Einstein's 1905 paper on relativity theory. In thinking about its introduction at Cambridge, historian Andrew Warwick recognized that:

> the term 'response' is rather misleading because it seems to attribute Einstein's work with *active* capacity to command the attention of the reader. . . . I shall claim that these commentators [of Einstein's] should not be regarded as mere passive respondents to the appearance of a new and novel theory, but as working physicists who, for whatever reason, *actively* identified the work of a little-known physicist as relevant to their own research.[8]

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[8] 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, 628-629. His emphasis. For a collection of essays on the reception of Einstein's relativity theory, see T. F. Glick, ed., *The Comparative Reception of Relativity* (Dordrecht: Reidel, 1987). For another excellent social study of reception in science, dealing with J. J. Thompson's 'discovery' of the electron and the role this "myth" played in the history of physics, see B. Lelong, "Personne n'a découvert l'électron," *La Recherche,* 303 (November 1997): 80-84.

Indeed, it is crucial to attribute agency to the right persons. Ruelle and Takens could vocally promote their model if they wished to, but, like Einstein, they had no ability to command their listeners to adopt their frame of thinking nor their modeling practice. One important aspect of Warwick's study was his focus on a local culture. In studying the emergence of the chaos constellation, which took place in the eighth decade of the twentieth century, local communities are not as naturally defined as in the case of relativity theory at the beginning of the century. National and institutional settings seem to play a smaller role in the age of preprints, frequent international conferences, and air travel. However, local cultures do develop around research topics, practices, and bounded, albeit international, communities.

b) Rayleigh-Bénard: A Boundary System

Far from being a *consequence* of Ruelle and Takens's proposal, the constitution of the chaos constellation hinged on a preexisting desire on the part of diverse groups of scientists to develop a common framework for dealing with phenomena of turbulence and instabilities. The Ruelle-Takens model, as well as others exhibiting sensitive dependence on initial conditions, provided them with well adapted tools and practices to push their desire for interdisciplinarity forward.

Indeed, one of the most fascinating and intriguing features, striking even to early 'chaologists', was this very communication across disciplinary boundaries that was then established. Emphasizing this aspect of the history of the emergence of chaos, Saclay physicist Pierre Bergé, who designed experiments useful for the recognition of chaotic behaviors in fluids, once declared:

> among the general qualities which I am pleased to recognize in the study of dynamical systems, there is a *very enriching collaboration* between theoreticians and experimentalists and—even more remarkable—between mathematicians and physicists.[9]

In order to better capture the nature of this "collaboration," focusing on a particular physical system, which I shall call a *boundary system,* will be useful. A simple case of convection, this system was known as the Bénard system, mentioned in Martin's talk above, and more often as the *Rayleigh-Bénard system.* In the early 1970s it was studied experimentally by Günter Ahlers, theoretically by Fritz Busse, and numerically by Paul Martin and John McLaughlin. Moreover, it was the starting point for Edward Lorenz's famous model. As is described below, the Rayleigh-Bénard system was simple enough so that its essential features could be discussed by various groups of scientists, yet by following very different approaches.

As such, the Rayleigh-Bénard system recalls a heuristic notion introduced in 1989 by Susan Star and James Griesemer: *boundary objects.*

> Boundary objects are objects which are both plastic enough to adapt to the local needs and the constraints of several parties employing them, yet robust enough to maintain a common identity across sites. They are weakly structured in common use, and become strongly structured in individual-site use.[10]

Even though in Star and Griesemer's view boundary objects "may be abstract or concrete," it seems difficult to think of the convection problems raised by Rayleigh-

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[9] P. Bergé's comment in P. Bergé, Y. Pomeau, and C. Vidal, *Order Within Chaos: Towards a deterministic Approach to Turbulence* (New York: John Wiley & Sons; Paris: Hermann, 1984), 267.

[10] 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 of Science,* 19 (1989): 387-420, 393.

Bénard experiments as an 'object'. Inspired by the notion of boundary objects, I shall nonetheless call the Rayleigh-Bénard system a 'boundary system'.

By focusing on the Institut des hautes études scientifiques of Bures-sur-Yvette, the previous chapters have presented a very partial view of the history of the emergence of chaos. Here, I intend to redress this bias somewhat in a summary fashion. By looking at Rayleigh-Bénard as a boundary system that provided means of communication among groups of scientists insisting on extending analogies from their fields of specialty to the study of hydrodynamic instabilities, I show the wider context in which the Ruelle-Takens model was inserted by various actors. Much more could, and should, be said about the contributions to the history of chaos raised here. This chapter provides only a few hints about how we may understand the formidable activity that went into the discussion of a variety of themes soon seen as closely connected within the chaos constellation.

c) Structure of the Chapter

Because of the number of scientists belonging to different interacting groups that enter this chapter, its structure is considerably more complicated than previous ones. To focus on a single boundary problem is useful. But this focus is not enough to transform the complex dynamics among these groups of scientists into a neat linear story.

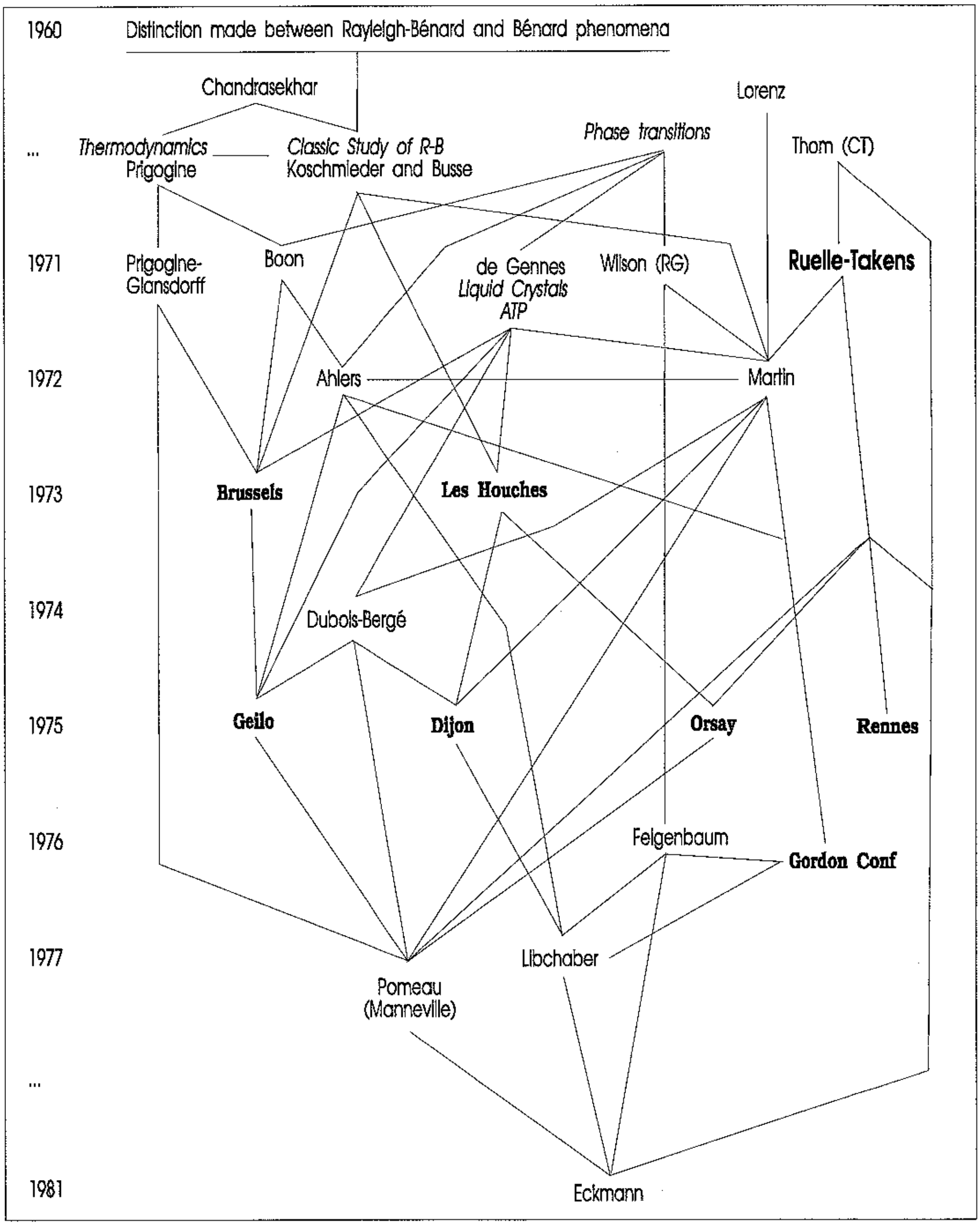

Figure 16: A Schematic View of the Contents of Chapter VIII, showing the interplay of important scientists, research subjects, and conferences, in relation with the Rayleigh-Bénard system and turbulence. RB stands for Rayleigh-Bénard; RG for the renormalization group; and CT for catastrophe theory. Not all connections are drawn and some omissions are obvious (stability theorists, Gollub-Swinney). Dates are merely indicative.

In figure 16, a schematic view of the content of this chapter is represented, which makes the complexity of the interconnections painfully clear. It shows the interplay that took place among the most important scientists mentioned below, conferences where some of them gathered, and research subjects they were working on. Lines between scientists correspond to references or, when horizontal, collaborations. Lines linking scientists and conferences indicate participation in these conferences. Lines between conferences convey a continuity of agendas. Lines connecting research subjects to scientists indicate that they drew from previous involvement in a discipline. Obvious omissions in the figure only reinforce the desperate complexity of the relationships established at that time. In particular, neither stability theorists who interacted with Prigogine and Ruelle, nor the experimental work of Swinney and Gollub are included, because the tight focus on the Rayleigh-Bénard system excluded them. Similarly the important work of Hermann Haken, Michel Hénon, Christian Mira, and Joseph Ford have all been omitted in the figure as well as in the Chapter.[11]

In 1960, a distinction was made between two types of phenomena occurring in convection which triggered renewed interest for the study of the Rayleigh-Bénard system. A few years later, Ilya Prigogine noticed a formal analogy between instabilities in hydrodynamics and nonequilibrium thermodynamics (especially

[11] In the US, I mention that the actions of Joseph Ford should be more closely studied as establishing collaboration across disciplines. "In the 1970s Joe [Ford] conceived the idea of disseminating 'Nonlinear Science Abstracts.' He personally collected, organized, typed, copied and distributed volumes of abstracts of dynamics papers, and thereby *forged a global, interdisciplinary community.*" T. Uzer, Boris Chirikov,

emphasizing oscillating chemical systems): a passage from stable equilibrium situations to symmetry-breaking, self-organizing flux equilibrium situations, which he called *dissipative structures.* He and his collaborators began to build bridges with hydrodynamicists, stability theorists like those discussed in Chapter VII, and mathematicians working on dynamical systems theory.

In parallel, during the late 1960s and early 1970s, physicists had made important experimental and theoretical progress in the study of various kinds of phase transitions, most importantly by using renormalization group methods introduced by Kenneth G. Wilson in 1971. Here again, analogies between different types of physical systems were the main object of study. Furthermore, the analogy with phase transitions provided incentives to study hydrodynamic instabilities. In particular, Pierre-Gilles de Gennes and his group were facing problems involving both phase transitions and hydrodynamic instabilities in the study of liquid crystals.

In 1973, the above three groups (nonequilibrium thermodynamicists, hydrodynamicists, and scientists from de Gennes's liquid crystals group) gathered in Brussels at a conference organized by Prigogine. As the left-hand side of figure 16 indicates, many of the connections among specialists working on different subjects were already in the process of being established when Ruelle and Takens's paper appeared in 1971. No one at the 1973 Brussels conference mentioned the Ruelle-Takens paper, but, as will become obvious, the activities of these three groups, as well

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Franco Vivaldi, and Giulio Casati, Obituary for Joseph Ford, *Physics Today,* 48(10) (1995): 88. My emphasis.

as their desire to approach hydrodynamic instabilities in an interdisciplinary way, set the ground on which the Ruelle-Takens model could be planted.

The above figure also underscores Paul Martin's role as a crucial mediator for the Ruelle-Takens paper. By drawing on a variety of sources, he included the paper in a mesh of vaguely related studies, thereby drawing the attention of many. Only after 1975 did Ruelle and Takens's article began to be cited in fairly large numbers (Graph 8), something that is made manifest in the figure by the fact that most direct connections with Ruelle-Takens were drawn in later years.

After 1975, the figure includes merely a few of the many scientists who worked on the Rayleigh-Bénard system. It however emphasizes the wide array of resources coming from classical hydrodynamicists, physicists of de Gennes's group, and scientists close to Prigogine, used by the scientists who by the end of the decade tried to synthesize the achievements of the new "chaos theory" by using concepts and practices from dynamical systems theory. In particular, the work of Yves Pomeau and Paul Manneville, as well as that of Jean-Pierre Eckmann, have been singled out as particularly significant because they illustrate the ways in which dynamical systems were adapted by physicists.

By focusing on the Rayleigh-Bénard system, I hope that I have constructed a coherent story of the way in which Ruelle and Takens's 1971 article finally achieved the status of a seminal paper for the deterministic theory of turbulence. We shall thus see that, far from being what caused the emergence of chaos, the physicists' adaptation of methods stemming from dynamical systems theory was the result of complex alliances, which latched on Ruelle and Takens's paper as a common reference. In the following, classic theories of the Rayleigh-Bénard system will be reviewed as an introduction to the renewal of experimental and theoretical interest occurring in the 1960s. Then, the boundary nature of the Rayleigh-Bénard systems will be examined, as it was tackled by different groups of scientists who brought a whole variety of experimental and theoretical techniques to bear on it. Special attention will be paid to the French context where interdisciplinary studies of turbulence appear to have been helped by definite political desires. After 1975, the study of turbulence with the help of theoretical tools stemming from dynamical systems became widespread. The role of David Ruelle and of the IHÉS will be seen as being less prominent than earlier. A few significant contributions, by French and Swiss physicists, have been singled out as particularly helpful in articulating the ways in which "a dynamical systems approach" can indeed be said to have been adopted. We shall however see that it differed markedly from Ruelle and Takens's own modeling practice.

2. CLASSIC RAYLEIGH-BÉNARD: EXPERIMENTS AND THEORIES

Consider a horizontal layer of liquid inside a cavity which is heated from below. In response to heating, the lower strata of the fluid expand and become lighter than the overlying strata. The warmer and lighter layers at the bottom tend to rise, while the cooler and denser top layers tend to sink. Thus described, convection is responsible for many large-scale atmospheric and oceanic phenomena, as well as the roiling of a heated broth. In a scientific way, it expresses the popular wisdom: "Heat rises." This was known by 1797, and surely much before, when Count Rumford studied the transport of heat.[12] To describe the phenomenon, the term *convection* was introduced by William Prout in 1834.[13]

Prior to the late 1960s, the Rayleigh-Bénard problem was considered as having been essentially solved by Lord Rayleigh in 1916. Although convection played an important role in many fields of engineering and science, very few physicists thought of it as worthy of study. Those who did, like Subramanayan Chandrasekhar, mainly focused on adding complexity to the problem, by studying the effect rotations, magnetic fields, or a combination of the two had on it.[14] For physicists, the simple Rayleigh-Bénard problem was solved.

By 1978, the situation had changed considerably. In Grenoble, a Symposium devoted solely to convection welcomed 57 papers presented by 65 participants coming from 15 different countries.[15] What happened in between was of course chaos. But as we shall see, while it certainly played a role, the emergence of chaos can hardly be taken as the cause for increased interest in convection. Indeed, the emergence of

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[12] Count Rumford, "The Propagation of Heat in Fluids," *Collected Works,* 1, ed. S. C. Brown (Cambridge: Harvard University Press, 1968): 117-284, 124. Secondary account: S. C. Brown, "Count Rumford Discovers Thermal Convection," *Daedalus,* 86 (1957): 340-343.

[13] "We venture to propose the term *convection* (*convectio,* a carrying or converging), which not only expresses the leading facts, but also accords very well with the two other terms (conduction and radiation)." W. Prout, *Bridgewater Treatises,* 8 (1834), 65, ed. by W. Pickering; quoted by S. Chandrasekhar, *Hydrodynamics and Hydromagnetic Stability* (Oxford: Clarendon, 1961), 71. For an easy, accessible introduction, see M. G. Verlarde and C. Normand. "Convection," *Scientific American,* 243 (July 1980): 92-108.

[14] S. Chandrasekhar, "Thermal Convection," *Daedalus,* 86 (October 1957): 323-339; repr. *Selected Papers,* 4: 163-191.

[15] E. J. Hopfinger, P. Atten, and F. H. Busse, "Instability and Convection in Fluid Layers: A Report on Euromech 106," *Journal of Fluid Mechanics,* 98 (1979): 217-240. Held from 11 to 14 September, 1978.

chaos could be just as well be seen as stemming from the intense study of the Rayleigh-Bénard problem, undertaken for reasons that had little to do with an enthusiastic embrace of a "dynamical systems approach." The two went hand in hand.

In the present section, I briefly recall the classic experiments and theories dealing with convection before I turn to the increase of interest that took place from the late 1960s to the early 1970s. At that time, many experimenters and theoreticians carefully reviewed the classic theory of convection and attempted to extend it beyond the linear domain. In the following section we shall see how Rayleigh-Bénard was simultaneously taken up by several scientific communities, each bringing their specific concepts and practices to bear on the problem, albeit with very different intentions.

a) Classic Problems of Convection

As is briefly mentioned in Chapter VII, Henri Bénard was a French physicist who performed experiments on fluids for a course given at the Collège de France by Marcel Brillouin at the turn of the century. Bénard was among the first to study the behavior of a thin layer of liquid, about a millimeter in depth, when heated from below, the upper surface being in contact with air at a lower temperature.[16]

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[16] H. Bénard, "Les tourbillons cellulaires dans une nappe liquide: I. Description générale des phénomènes," *Revue générale des sciences pures et appliquées,* 11 (1900): 1261-1271; "Les tourbillons cellulaires dans une nappe liquide transportant de la chaleur par convection en régime permanent," *Annales de chimie et de physique,* 7th series, 23 (1901): 62-144. See also M. Brillouin, *Leçons sur la viscosité des liquides et des gaz* (Paris: Gauthier-Villars, 1907).

Experimenting with liquids of different viscosity, he observed in all cases the formation of a striking pattern of hexagonal cells.[17]

This appearance of order in the Rayleigh-Bénard system was a phenomenon that never failed to captivate those who tackled the system for the first time. "A fascinating aspect of the Rayleigh-Bénard instability," Bergé thus wrote in 1975, "consists in the existence of a remarkable periodicity—or if one prefers the existence of a perfect order—in the organization of the convective cells, an order that cannot fail to surprise the one who sees it for the first time."[18] This spontaneous ordering often led scientists to the wildest speculations.

To start with, Bénard thought that physicists ought to be more ambitious in their pretension to understand nature, and this spontaneous emergence of organization struck him as having potentially important applications for the life sciences.

> I think it is impossible not to concern oneself with what the consequences [of this phenomenon] might entail from the point of view of biological theories. . . . Purely physical research, such as this, might perhaps have some interest in the eyes of scientists who do not despair of reducing the complex phenomena of life to the general laws of inorganic nature.[19]

In view of more recent attempts at applying models such as this one to biology, made by Thom or Prigogine, Bénard's statement may be seen as yet another early

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[17] It seems that James Thompson, Lord Kelvin's older brother, was the first to observe these convection cells; see his quote in Chandrasekhar, *Hydrodynamic Stability,* 71; and J. Thompson, "On a changing tessellated structure in certain liquids," *Proceedings of the Philosophical Society of Glasgow,* 13 (1882): 464-468. See also M. G. Velarde, " Hydrodynamic Instabilities (in Isotropic Fluids)," *Fluids Dynamics,* ed. R. Balian and J.-L. Peube (London: Gordon and Breach): 469-527.
[18] P. Bergé, "Aspects expérimentaux de l'instabilité thermique de Rayleigh-Bénard," *Journal de physique,* 37, Colloque C1 (1976): 23-33, 24.
[19] H. Bénard, "Les tourbillons cellulaires," *Annales,* 144.

anticipation of the modeling practices discussed in this dissertation.[20] Conversely, we might contend that it was the emphasis from the beginning on potential biological applications that drew their attention to the Bénard phenomenon.

The first theoretical account of the phenomenon was provided by Lord Rayleigh who applied his method of small oscillations to convection problems. "The present is an attempt to examine how far the interesting results obtained by Bénard in his careful and skillful experiments can be explained theoretically."[21]. With these words, James William Strutt, Lord Rayleigh, opened the 1916 paper, which has since shaped modern views about convection.

Rayleigh explained convection in terms of an imbalance of forces. The force causing the bottom lighter layers to rise was called *buoyancy* and it increased with the difference of temperature. Opposed to it, there was a dissipative force, or friction, due to viscosity. When the temperature gradient between the bottom and the top was small, the forces canceled. Heat was propagated by diffusion only. No current was created and the liquid stayed immobile. Convection arose when buoyancy overcame viscosity. The relative importance of these two forces, Rayleigh showed in a manner recalling Reynolds's work, was measured by a pure dimensionless number *Ra*, later called the *Rayleigh number:*

$$Ra = \frac{g\alpha d^3 \Delta T}{\kappa\nu};$$

[20] Thom mentioned the Bénard phenomenon in relation with biological ordering in *SSM,* 108 and 234.

where g was the gravitational acceleration, d and ΔT, respectively, the distance and the difference of temperature between the plates, α the thermal expansion coefficient, κ the coefficient of thermal diffusion and ν the kinetic viscosity, the latter three quantities being physical characteristics of the fluid. Rayleigh's theory predicted the existence of a critical Rayleigh number Ra_C, depending on the geometry of the cavity but not on further physical properties of the fluid, at which a stationary convective current was set in motion. The boundary nature of Rayleigh's criterion was obvious quite early on, as it was used in a variety of domains. It helped people study global meteorological phenomena, convection in stars, and plate tectonics. In the cases that concern us, the onset of convection occured at a critical Rayleigh number of the order of 2000.

However, it was not until the 1950s that it was realized that Rayleigh had provided an account for a phenomenon quite different from the one observed by Bénard.[22]

> We are saying that Bénard convection in the limit of an extremely thin fluid layer under thermal gradient has nothing to do with Rayleigh's instability criterion described above! Indeed Bénard cells can be induced from heating from above! Or by a horizontal heating if the fluid layer is vertical![23]

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[21] Lord Rayleigh, "On convective currents in a horizontal layer of fluid when higher temperature is on the under side," *Philosophical Magazine,* 6th series, 32, (1916): 529-546; also *Scientific Papers,* 6, (Cambridge, England, 1920), 432-446.

[22] M. J. Block, "Surface Tension as the Cause of Bénard Cells and Surface Deformation in a Liquid Film," *Nature,* 178 (1956): 650-651; and J. R. A. Pearson, "On Convection Cells Induced by Surface Tension," *Journal of Fluid Mechanics,* 4 (1958): 489-500.

[23] M. G. Velarde, "Hydrodynamic Instabilities," 476.

With a view to exploring the possibility of growing crystals in space, the experiment was even carried out in zero gravity on board the Apollo XIV spacecraft, and gave rise to the same hexagons.[24] Further studies showed that the Bénard phenomenon was almost entirely due to surface tension. It became customary among hydrodynamicists to call the problem of explaining the appearance of hexagonal cells the Bénard problem, while reserving the term 'Rayleigh-Bénard' for qualifying convection problems in terms of loss of stability. "It seems fairly well established nowadays [1973] that for standard . . . fluids, hexagons . . . appear when surface tension is involved in the problem, . . . whereas rolls and only rolls are the structure of Rayleigh convection."[25] The experimental system best designed to study this particular phenomenon included an upper plate above the liquid, so as to minimize the effect of surface tension.

b) The Hydrodynamicists' Approach to the Rayleigh-Bénard System

(i) Experiments: 'How the Onset of Convection Actually Occurs'

A somewhat arcane corner of physics, which may not have been among its most exciting topics, Rayleigh-Bénard convection had nonetheless generated a huge literature. Reviewing it in 1973, Madrid physicist Manuel G. Velarde wrote: "Over 500 (five-hundred—there is no mistake in this figure) article have been published . . . on Rayleigh-Bénard convection or similar [phenomena]."[26] Velarde added:

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[24] M. G. Velarde, "Hydrodynamic Instabilities," 479.
[25] M. G. Velarde, "Hydrodynamic Instabilities," 479.
[26] M. G. Velarde, "Hydrodynamic Instabilities," 514.

> In fact, too many papers have been published . . . leading to much confusion since the publication of Lord Rayleigh's paper, just because people did not try to repeat Bénard's experiments under different conditions and/or did not want to contradict the beautiful and masterly analysis of Lord Rayleigh.

In a footnote, Velarde added: "besides Block, I take the opportunity of mentioning that E. L. Koschmieder is an important exception to the general rule!".[27] Indeed, in the 1960s, as experimenter at the College of Engineering and the Center of Statistical Mechanics and Thermodynamics of the University of Texas, Austin, Koschmieder had undertaken to review carefully, and perform again, delicate experiments on the Rayleigh-Bénard system.[28] Once again, let me quote from Velarde's informative review, which, using the experimenter's arid language, explained what then interested Koschmieder:

> Koschmieder has explained to me in a private conversation how the onset of convection actually occurs. His "boat" is a cylindrical container. The upper plate is a colorless sapphire of 13.23 cm. diameter and 0.508 cm. thickness. The lateral boundaries of the "boat" are made of lucite. The sapphire has a thermal conductivity 300 times that of lucite and that of the liquid (silicon oils, liquid depth also 0.508 cm.) but only one-tenth of that of the bottom plate, made of copper. The rolls appear as follows: (i) they originate near the wall. . . . (ii) *timing:* If R_c denotes the critical temperature difference, the first [circular] rolls appears at around $0.6R_c$. The second one at $\sim 0.74R_c$, the third at $\sim 0.85R_c$. By $0.95R_c$ the plate is almost filled and just at R_c the entire plate is filled with rolls. . . . (iv) at R_c he observed 13 rolls, then this number is maintained up to $1.3R_c$, but at $2.2R_c$ there are only 12, and at $3.4R_c$ only 11 rolls remained. For $6R_c$ only 10.[29]

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[27] M. G. Velarde, "Hydrodynamic Instabilities," 477-478.
[28] See his own reviews, E. L. Koschmieder, "Bénard Convection," *Advances in Chemical Physics,* 26 (1974): 177-212; and "Stability of Supercritical Bénard Convection and Taylor Vortex Flow," *Advanced in Chemical Physics,* 32: *Proceedings of the Conference on Instability and Dissipative Structures in Hydrodynamics [Brussels, 1973],* ed. I. Prigogine and S. A. Rice (New York: John Wiley, 1975): 109-133.
[29] M. Velarde, "Hydrodynamic Instabilities," 479-480. His emphasis.

The dryness of this description underscores two of the essential concerns of the experimenter: first, the materials and configuration of the experimental system; and second, the spatial organization of rolls as the temperature difference, or equivalently the Rayleigh number, was increased. When compared with the above, the pertinence of the Ruelle-Takens model is far from being immediate. Indeed, even tough they emphasized topological features, Ruelle and Takens had nothing to say about *spatial* characteristics of the onset of turbulence. At the level of generality they claimed for their model, there was little they could say about specific problems. On the other hand, the flows described by Koschmieder, even though the liquid was not still, were all stationary. He did not even consider here the appearance of the first oscillatory mode. But still, in Koschmieder's observations, there were plenty of puzzling features to be accounted for by an ambitious theoretician. However, this was a difficult problem that did not seem to offer much reward.

(ii) Theory: No 'Real Breakthrough in Understanding'?

Rayleigh's computation of the critical temperature at which convection arose amounted to a linear stability analysis of the Rayleigh-Bénard system, similar to Taylor's later study of the Couette flow. The nonlinear theory of the Rayleigh-Bénard system was tackled by hydrodynamic stability theorists, in particular by Daniel D. Joseph.[30] But as Velarde wrote in his 1973 review:

[30] D. D. Joseph, "On the Stability of the Boussinesq Equations," *Archive for Rational Mechanics and Analysis,* 20 (1965): 59-71; "Nonlinear Stability of the Boussinesq Equations by the Method of Energy," *Ibid.,* 22 (1965): 163-184; "Stability of Convection in Containers of Arbitrary Shape," *Journal of Fluid Mechanics,* 47 (1971): 257-282.

> Quite to my dismay I must confess that the solutions to the simplest non-linear Rayleigh[-Bénard] problem . . . has not yet appeared in the literature! Not much, in fact, has actually been advanced (73 years after Bénard's original experiments!) since the original and simple analysis of Rayleigh. . . . This obviously shows the mathematical difficulties involved in such simple physical problems.[31]

In spite of the "over-production of publications on a particular subject," there had not been "any real breakthrough in understanding" concerning the Rayleigh-Bénard system.[32] Was this solely an effect of "mathematical difficulties"? This situation raises an interesting question, as was already noticed by some early chaologists:

> It is striking to note that twenty years ago [i.e. 1964] little more was known about [turbulence] than at the beginning of the nineteenth century when Navier was setting down the equations governing the flow of a fluid. . . . And yet fluid mechanics is a domain easily accessible to experiment: no laboratory machinery comes anywhere close—in complexity and in cost—to the accelerators used to study subnuclear particles! Despite its banality, this observation raises questions which historians of science will one day have to address: that of the underlying causes (circumstantial or epistemological) of the relative stagnation, in a discipline which has never lacked for practical and economic motivation.[33]

Some of these claims cannot withstand even a superficial historical look. As shown in Chapter VII, to claim that little more was known about turbulence in 1964 compared to the 1820s is grossly exaggerated. As later examples in this Chapter likewise demonstrate, the experiments that served to ground the chaotic behavior of fluid flows at the onset of turbulence crucially depended on resources that were far from being available in the 1820s, such as computers, lasers, and liquid helium. Despite claims to the effect that these experiments were of "nineteenth-century style,"

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[31] M. G. Velarde, "Hydrodynamic Instabilities," 514.
[32] M. G. Velarde, "Hydrodynamic Instabilities," 514.

they needed modern technology in order even to be envisioned.[34] Most significantly, it was only after Ruelle and Takens had *redefined* what turbulence was, only after their approach was recognized as perhaps relevant, that people could claim that "real breakthroughs" in the study of turbulence had been achieved.

While it might have been a sound marketing strategy for physicists to underscore that up until they came onto the stage "fluid turbulence remains one of the most fascinating and poorly understood phenomena in macroscopic physics," to expect that a general, simple theory of turbulence could indeed be built to account for widely different cases seems an unjustified *a priori* belief.[35] Indeed, if stability theorists had demonstrated one thing in decades of studies, it certainly was that the onset of turbulence could arise in a variety of cases, which seemed to call for a whole range of mathematical techniques. For them, it was doubtful that one should expect "real breakthroughs." Turbulence was hardly a problem to be approached by simple unified techniques; it was a phenomenon to be tackled cautiously in special, well defined situations.

Nevertheless, one may legitimately ask: why did physicists get generally excited about such classic problems as Rayleigh-Bénard during the 1970s? The present Chapter will show that a lot of work and energy went into the *intéressement* of physicists in fluid dynamical problems.[36] For the courageous ones who tackled the

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[33] P. Bergé, et al. *Order Within Chaos,* xiii.
[34] J. Gleick called Libchaber a "nineteenth-century style" experimenter. *Chaos,* 192.
[35] J. B. McLaughlin and P. C. Martin, "Transition to Turbulence in a Statically Stressed Fluid," *Physical Review A,* 12 (1975): 186-203, 186.
[36] On the notion of *intéressement,* see M. Callon, "Some Elements of a Sociology of Translation: Domestication of the Scallops and Fishermen of St. Brieuc Bay," *Power,*

Rayleigh-Bénard problem before this process was completed, it could offer only small intellectual and professional rewards in return for giant headaches. Moreover, their ways of thinking about the problem would remain of little interest to chaologists, hence the feeling of the latter that nothing had advanced very much before they entered the stage.

At around the same time as Koschmieder experimented with his convection "boat," Fritz Busse and coworkers endeavored to provide a theoretical understanding of this rich array of observations in Rayleigh-Bénard systems.[37] Building on the work of Malkus and Veronis,[38] Busse worked out a nonlinear perturbation theory of the Rayleigh-Bénard problem, along the lines of those discussed in Chapter VII for stability theorists. His approach, however, was much more physical than theirs. That is, he tackled head-on difficult differential equations using classic perturbation methods, most often without using the analytic arsenal that Daniel, Sattinger, and Iooss mobilized. In a series of papers written between 1962 and 1974, Busse could nonetheless establish a whole 'zoo' of instabilities arising in different cases, depending on the Rayleigh number *Ra* but also on the so-called Prandtl number of the fluid.[39]

Action, and Belief: A New Sociology of Knowledge, ed. J. Law, Sociological Review Monographs, 32 (1986): 196-233.

[37] Busse's 1962 Ph.D. dissertation, *Das Stabilitätverhalten der Zellular Konvektion bei eindlicher Amplitude,* from the University of Munich was transl. by S. H. Davis, and published in the *Rand Reports,* LT-66-19 (Santa Monica: Rand Corporation, 1966).

[38] W. V. R. Malkus and G. Veronis, "Finite Amplitude Cellular Convection," *Journal of Fluid Mechanics,* 4 (1958): 225-260.

[39] See fig. 13 in F. H. Busse, "Non-Linear Properties of Thermal Convection," *Report on the Progress in Physics,*41 (1978): 1929-1967. The Prandtl number is a dimensionless characteristics of the fluid studied: $Pr = \nu/\kappa$, where ν is the kinetic viscosity and κ the thermal diffusivity. Obviously, according to Rayleigh's criterion it

As described above, Koschmieder's observations being mostly qualitative, it was in no way easy to derive quantitative predictions from the theory which could then be checked with observations.

> At the beginning, a particular author does not aim at rigorous mathematics, . . . does not aim at predicting a figure in all quantitative aspects. Rather than that, the attitude is more towards modifying the mathematical description in such a sensible, but relevant, way, that at the end of the calculation a qualitative or semiquantitative agreement (as good as possible) is obtained with the known experimental results. Prediction is also provided but of measurable quantities! Any extremely good agreement with experimental results, however, would then be suspicious![40]

One such quantity, the *Nusselt number,* was defined as the ratio of the observed heat flux through the Rayleigh-Bénard cell over the one predicted for pure conduction through still liquid.

In the early 1970s, Ruby Krishnamurty, from the Geophysical Fluid Dynamics Institute at Florida State University, observed the slope of the curve when the Nusselt number was plotted against the Rayleigh number, for different Prandtl numbers. At a Rayleigh number around $12R_c$ (for $10<Pr<10^4$), she found that the slope changed, which following Busse she interpreted as a transition from two-dimensional rolls to

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has no effect on the appearance of convection rolls, but may influence the development of secondary instabilities. For Busse and coworkers' most important papers, see A. Schlüster, D. Lortz, and F. H. Busse, "On the Stability of Steady Finite Amplitude Convection," *Journal of Fluid Mechanics,* 23 (1965): 129-144; F. H. Busse, "The Oscillatory Instability of Convection Rolls in a Low Prandtl Number Fluid," *Journal of Fluid Mechanics,* 52 (1972): 97-112; R. Clever and Fritz H. Busse, "Transition to Time-Dependent Convection," *Journal of Fluid Mechanics,* 65 (1974): 625-645; and his review paper cited above "Non-Linear Properties of Thermal Convection." Experimental evidence of a dependence on the Prandtl number for the onset of oscillatory convection was first provided by G. E. Willis and J. W. Deardorff, "The Oscillatory Motions of Rayleigh Convection," *Journal of Fluid Mechanics,* 44 (1970): 661-672.

[40] M. G. Velarde, "Hydrodynamic Instabilities," 516.

more complicated three-dimensional structures.[41] As important as these results were, their obvious drawback was that evidence was merely global and indirect, and there was no quantitative way to check directly the fine structure of rolls, as described by Busse and his coworkers.

In conclusion, from the late 1960s to the early 1970s, the study of the Rayleigh-Bénard problem by hydrodynamicists, taken in the most restricted sense, had somewhat progressed. And this had happened before any mention of the Ruelle-Takens argument was ever made. Taking Velarde's critiques below as a hint, we might conjecture that people working on Rayleigh-Bénard had begun to meet regularly, so that research foci became more precisely defined. Velarde complained in 1973:

> Nowadays scientists are forced to become literary businessmen rather than in-depth productive researchers. In my opinion, the *over-abundance* of literature can only confuse, rather than clarify, the real issues. Perhaps one should go more to meetings and discussions instead of writing papers.[42]

Velarde himself was much impressed by his personal contact with Koschmieder and other such meetings were perhaps important. At the same time, however, analogies with phase transitions started to emerge.

3. RAYLEIGH-BÉNARD: A BOUNDARY SYSTEM

The noble effort described above, on the part of marginal physicists, would provide a crucial ground on which would be built much of the later interest in the Rayleigh-Bénard problem, on the part of outsiders to the field of fluid dynamics *per se*. Among

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[41] R. Krishnamurty, "On the Transition to Turbulent Convection," *Journal of Fluid Mechanics,* 42 (1970): 295-320; "Further Studies on the Transition to Turbulent Convection," *Journal of Fluid Mechanics,* 60 (1973): 285-303.
[42] M. G. Velarde, "Hydrodynamic Instabilities," 514.

these outsiders who, in the early 1970s, found in Rayleigh-Bénard a boundary system which could be tackled using analogies and methods coming from their own practice, we find Ahlers, Martin and McLaughlin, Prigogine and his schools, de Gennes and those he encouraged to study hydrodynamic instabilities.

a) Rayleigh-Bénard as Dissipative Structure

"According to us, this example [i.e. the Bénard system] is an especially enlightening example of the *degree of unification* that our method allows us to achieve between problems pertinent to thermodynamics and hydrodynamics."[43] Paul Glansdorff and Ilya Prigogine published in 1971 a book which, like Thom's *Structural Stability,* would reach a rather large audience despite its technicalities. In it, the Rayleigh-Bénard problem (which they most often conflated with the Bénard problem) provided one of the main examples of the extension of their method, stemming from nonequilibrium thermodynamics, to other systems.

(i) Prigogine: From Irreversible Thermodynamics to Rayleigh-Bénard

As early as 1955, Prigogine had started extending his study of irreversible thermodynamics, which he had tackled for his Ph.D. thesis in the 1940s, to the nonlinear domain, with applications to biology in view.[44] "Only recently," he wrote in

[43] P. Glansdorff and I. Prigogine, *Structure, stabilité et fluctuations* (Paris: Masson, 1971), 3. My translation and emphasis. See however the English version of this book, *Thermodynamic Theory of Structure, Stability, and Fluctuations* (New York: John Wiley, 1971).

[44] I. Prigogine, *Introduction to Thermodynamics of Irreversible Processes* (Springfield, IL: Charles C. Thomas, 1955; 2nd ed. New York: Wiley, 1962). This book is a rewriting, with many additions, of his thesis published as *Étude thermodynamique des phénomènes irréversibles* (Paris: Dunod; Liège: Desoer, 1947).

an interesting appendix on nonlinear effects, "have some results which belong to the treatment of such effects been published and much remains to be done. It would seem worthwhile, however, to include a preliminary account of these results here, for they indicate the directions in which further progress may be possible."[45]

This was indeed a direction he pursued. In 1968, Prigogine's book was translated back into French. "On the occasion of this translation, the author was kind enough to recast and complete the preceding edition, by devoting the last two chapters to the study of systems evolving far from equilibrium."[46] Chapter 7 consisted in an expansion of the above appendix, which insisted on oscillating chemical reactions. These reactions, as had been exhibited by Soviet chemists Belousov and Zhabotinsky in 1950s, instead of tending towards an equilibrium state, exhibited oscillatory behaviors, which, in retrospect, can be seen as limit cycles, much like oscillatory solutions in hydrodynamic systems.

For Prigogine, these chemical systems, which he approached starting from the analogy with hydrodynamic instability, were particularly interesting in that they exhibited a spontaneous appearance of order in time and space under the influence of dissipation.[47] Already in 1967, in collaboration with Glansdorff, Prigogine had thus

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About Prigogine's work, see J.-P. Brans, I. Stengers, and P. Vincke, eds., *Temps et devenir. Autour de l'Oeuvre d'Ilya Prigogine* (Geneva: Patiño, 1988).

[45] I. Prigogine, *Introduction to Thermodynamics* (1955), 94. On Prigogine's general program in 1962, see I. Stengers, *Cosmopolitiques,* 5, *Au nom de la flèche du temps: le défi de Prigogine* (Paris: La Découverte/Les empêcheurs de penser en rond, 1997), chap. 2, 33-59.

[46] I. Prigogine, *Introduction à la thermodynamique des processus irréversibles* (Paris: Dunod, 1968), vii-viii.

[47] In Chapter 8, titled "Order and Dissipation, Prigogine contended: "This problem presents a great interest since it is intimately linked to important biological problems

distinguished between two kinds of structures in matter, those arising in equilibrium (e.g. crystals), and those arising in out of equilibrium conditions, which he called "*dissipative structures.*"[48] A few years later, he defined them as follows:

> beyond a critical level dissipation can become an organizing factor, destabilize the disordered state, . . . and drive the system to an ordered configuration. Hence the term *dissipative structure.*[49]

As he saw it already in the late 1960s, dissipative structures bore directly on hydrodynamic problems. "Methods of the nonlinear thermodynamics of irreversible processes lead us to discuss hydrodynamic instabilities in a mode very close to that of phase transition."[50]

In 1965, a conference, organized by, notably, Prigogine, was held at the University of Chicago, with Chandrasekhar in attendance. A strong emphasis was put on the unifying prospect of using variational techniques in many fields of science, from statistical mechanics to hydrodynamics. The analogy between hydrodynamic

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such as the mechanism of biological clocks." *Introduction à la thermodynamique* (1968), 128. For an interesting account, inspired by Prigogine, of the link between oscillating chemical reactions and chemical clocks, by people who would become fervent chaologists in the late 1970s, see A. Pacault and C. Vidal, *À chacun son temps* (Paris: Flammarion, 1975).

[48] I. Prigogine and P. Glansdorff, "On Symmetry-Breaking Instabilities in Dissipative Systems," *Journal of Chemical Physics,* 46 (1967): 3542-3550, 3550. For a philosophical discussion, see A. Boutot, *L'Invention des formes. Chaos, catastrophes, fractales, structures dissipatives, attracteurs étranges* (Paris: Odile Jacob, 1993); and "Structures dissipatives et catastrophes: la redécouverte du monde sensible," *Revue philosophique de France et de l'étranger,* 178(2) (1988): 171-209.

[49] I. Prigogine's Introduction, in *Proceedings of the Conference on Instability and Dissipative Structures in Hydrodynamics. Advances in Chemical Physics,* 32, ed. I. Prigogine and S. A. Rice (New York: John Wiley, 1975), v.

[50] I. Prigogine, *Introduction à la thermodynamique* (1968), 129. See also P. Glansdorff et I. Prigogine, "On a General Evolution Criterion in Macroscopic Physics," *Physica,* 30 (1964): 351-374.

instability and phase transition was forcefully emphasized, using convection as a test case.

> If concepts such as thermodynamic potential could be generalized to cover non-equilibrium situations involving mechanical convection, it would become possible to discuss hydrodynamic instability problems in a fashion quite similar to the way in which phase transitions are discussed in classical thermodynamics.

This kind of interdisciplinary approach, far from being obvious to achieve on a social level, however promised to offer great intellectual rewards, as the editors wrote using scientific metaphors:

> The conference brought together scientists working in rather different areas. One might have thought that this heterogeneity would have led to phase separation. But the thermal energy generated by the discussions was so large that a real mixing process on both the hydrodynamic and molecular levels occurred.[51]

Without using the term 'bifurcation', which he would later adopt, Prigogine explained in 1968 the analogy that he was seeing.[52] Prigogine explicitly linked oscillating chemical reactions with the limit-cycles studied by Lotka and Volterra in the case of population dynamics, whose thermodynamic aspects had been studied by his students René Lefever and Grégoire Nicolis. In particular, he described phenomena in terms directly inspired by hydrodynamics: There was a parameter *R*

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[51] R. J. Donnelly, R. Herman, and I. Prigogine, eds., *Non-Equilibrium Thermodynqmics, Variational Techniques, and Stability* (Chicago: University of Chicago Press, 1966). See their "Foreword," v-vi, for the above quotes. The Bénard problem was discussed by P. H. Roberts, from the University of Newcastle on Tyne: "On Non-Linear Bénard Convection," *Ibid.,* 125-162.

[52] "For sufficiently large values of the affinities [a control parameter here for a chemical system, but this could equally be applied to the Rayleigh number] characterizing the stationary state, there appears an instability. This instability leads to new stationary states. . . . The most fascinating aspect of this instability perhaps lies in

representing the relative concentration of reactants, exhibiting a critical value R_c, which separated a region of aperiodic behavior ($R>R_c$) from one of oscillatory behavior ($R<R_c$).

In these conditions, it is no surprise that Prigogine tackled the Rayleigh-Bénard problem. In his view, there was a "great merit" in thermodynamic and hydrodynamic approaches in that both "introduced a 'reduced description' and a 'simplified language' generally sufficient for the study of macroscopic systems," as opposed to microscopic descriptions involving too many particles to be usefully treated.[53] Having been introduced to the Rayleigh-Bénard problem through Chandrasekhar's book, Prigogine was impressed by the fact that both before and after the instability a macroscopic description remained possible.

Inspired by his work on oscillating chemical reactions, he contended that his thermodynamic methods could greatly help to understand the physical reasons for the instability. He had in view a general, unified method for dealing with all kinds of phenomena of instability. "Various problems, until now treated by entirely different procedures, at least in appearance, now become accessible by a *new unified method.*"[54] Using variational principles, Prigogine and his collaborators derived integrals which included a balance between dissipative and convective effects, whose sign dictated the stability or instability of the Rayleigh-Bénard system. He clearly underscored the analogy: Bénard's hexagons were dissipative structures.

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the fact that it breaks symmetry." I. Prigogine, *Introduction à la thermodynamique* (1968), 131.

[53] P. Glansdorff and I. Prigogine, *Structure, stabilité et fluctuations,* 1.

[54] P. Glansdorff and I. Prigogine, *Structure, stabilité et fluctuations,* 3. My emphasis.

(ii) Instability and Dissipative Structures in Brussels, 1973

Around that time, Prigogine arranged to have a joint appointment in Brussels and at the University of Texas, Austin, in the Center of Statistical Mechanics and Thermodynamics, where E. L. Koschmieder also worked. From their encounter, Prigogine's interest in hydrodynamics only increased. In 1973 he organized a Conference on Instability and Dissipative Structures in Hydrodynamics at Brussels, where Koschmieder, Prigogine and his collaborators, and physicists from an Orsay group working on liquid crystals (whom I shall discuss below), tried to isolate the commonalties of their respective approaches. At this conference, Prigogine expressed most fully the "analogies" that he saw at play between different dissipative systems:

> The purpose of this volume is to present a number of problems involving hydrodynamic instabilities from the standpoint of irreversible thermodynamics of dissipative structures. We hope that the *analogies* with chemical kinetics and the existence of common underlying ideas in all phenomena involving the emergence of order in a previously disordered medium will stimulate further research in these fascinating areas.[55]

Besides his study on dissipative structures, Prigogine saw recent developments of the mathematical analysis of nonlinear differential equations as a useful advance. Inspired by these studies, he started to speak of bifurcations. To problems of instability, Prigogine contended "one may apply the powerful tools of the qualitative analytical-topological theory initiated by Poincaré, continued by Andronov, and completed to perfection by Thom [*sic*]."[56]

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[55] I. Prigogine's Introduction, in *Advances in Chemical Physics,* 32, ed. I. Prigogine and S. A. Rice, vi. My emphasis.

[56] G. Nicolis, I. Prigogine, and P. Glansdorff, "On the Mechanism or Instabilities in Nonlinear Systems," *Advances in Chemical Physics,* 32, ed. I. Prigogine and S. A. Rice: 1-11, 2. On the relation members of Prigogine's saw between dissipative

Citing David Sattinger's work as a review of bifurcation theory, Prigogine saw fluid mechanics as a traditional testing ground for new mathematical approaches:

> Fluid mechanics, which was the first field to show, more than a century ago, the emergence of patterns of order, has long been developed independently of irreversible thermodynamics and fluctuations. On the other hand, it has always been the privileged field where new mathematical techniques and ideas were tried and applied.[57]

It thus was partly through his interest in fluid mechanics, an interest which was a direct consequence of the analogies he detected with chemical and thermodynamic phenomena, that Prigogine got interested in recent developments in dynamical systems theory, including Thom's catastrophe theory. These develoments provided resources that he could mobilize in order to further his own research agenda, but, in fact, he never embraced a topological point of view.

Prigogine and coworkers identified three characters of dissipative structures which bore on instability problems in hydrodynamics, and Rayleigh-Bénard especially: the nonlinear character of the basic equations; the existence of a "set of parameters related to the *distance from equilibrium,*" possessing some critical values;

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structures and catastrophe theory, see G. Nicolis and G. Auchmurty, "Dissipative Structures, Catastrophes, and Pattern Formation: A Bifurcation Analysis," *Proceedings of the National Academy of Sciences of the USA,* 71 (1974): 2748-2751. One may note here that Nicolis had given a talk at the 1972 Battelle Summer Institute, mentioned in Chapter VII, where stability theorists Joseph and Sattinger were definitely introduced to the Ruelle-Takens model by Lanford. See G. Nicolis, "Mathematical Problems in Theoretical Biology," *Nonlinear Problems in the Physical Sciences and Biology,* ed. I. Stakgold et al. (Berlin: Springer, 1973): 210-230.
[57] I. Prigogine's Introduction, in *Advances in Chemical Physics,* 32, ed. I. Prigogine and S. A. Rice, vi.

and finally the appearance of ordered patterns.[58] In this context, they suggested that two "outstanding questions" were raised:

> 1. Is it possible to analyze rigorously the *mathematical mechanisms* behind these transitions and, in particular, is it possible to provide a general classification of the situations which may emerge beyond instability?
>
> 2. What is the *microscopic origin* of these transition?[59]

These two questions indeed raise a fascinating dichotomy between mathematical and microscopic explanations for bifurcations. As we have seen in previous chapters, the latter were most often disregarded in the modeling practices of applied topologists, albeit provoking some intense controversies among them. Contrary to Thom's and Ruelle's approaches, Prigogine aimed at integrating both kinds of explanations within a common, general, and unified understanding of instabilities.

(iii) Order through Fluctuations: Debate with Thom

Indeed, under the famous label "order through fluctuations," Prigogine articulated a view in which microscopic fluctuations might, in certain cases, give rise to ordered macroscopic features. "To the indifferent [molecular] chaos of equilibrium, room was added for a creating chaos evoked by the Ancients, a fecund chaos from which different structures can emerge."[60] In 1980, following the publication of Prigogine's

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[58] G. Nicolis, I. Prigogine, and P. Glansdorff, "On the Mechanism or Instabilities in Nonlinear Systems," 1-2. Their emphasis.
[59] G. Nicolis, I. Prigogine, and P. Glansdorff, "On the Mechanism or Instabilities in Nonlinear Systems," 2.
[60] I. Prigogine and I. Stengers, *La nouvelle alliance. Métamorphose de la science,* 2nd ed. (Paris: Gallimard, 1986), 243. My translation. See however the English translation *Order out of Chaos: Man's New Dialogue with Nature* (New York: Bantam, 1984), chap. VI devoted to "order through fluctuations." This view greatly inspired Michel

popular account of his theories, written in collaboration with Isabelle Stengers, René Thom would harshly denounce this view as an "antiscientific attitude *par excellence,*" which "outrageously glorified chance, noise, 'fluctuation'."[61]

This episode highlights the conflict between the modeling practices of those who did not despair of grounding qualitative dynamical analyses on a careful reductionist study of the substrata from which dynamical phenomena arose, and those, like Thom, who eschewed such an approach as counterproductive and even antiscientific. Although Thom argued on general philosophical grounds that "nothing in Nature is *a priori* unknowable," his critique was an issue of modeling practice.

> Only a careful study of the bifurcation of these 'strange attractors'—according to the modern terminology—will allow us to see more clearly through [these phenomena]; everything else is literature and—I am afraid—bad literature.[62]

For Prigogine, on the contrary, "fluctuation and intelligibility are not more opposed to one another than determinism and randomness, but form the framework for questions that are not concerned anymore with the distinction between the macroscopic and the microscopic as a fact, but integrates it as a problem."[63]

Serres, *La Naissance de la physique dans le texte de Lucrèce. Fleuves et turbulences* (Paris: Muinuit, 1977). About "order through fluctuations," see also Prigogine's interesting reactions to Thom's paper at the 1971 Statistical Physics Conference in Chicago: *Statistical Mechanics: New Concepts, New Problems, New Applications,* ed. S. A. Rice, K. F. Freed, and J. C. Light (Chicago: University of Chicago Press, 1972): 106-107; as well as Prigogine Nobel Lecture, repr. "Time, Structure, and Fluctuations," *Science,* 201 (1978): 777-785.

61 R. Thom, "Halte au hasard, silence au bruit," *Le Débat,* 3 (1980): 119-132; repr. *La Querelle du déterminisme,* ed. K. Pomian (Paris: Gallimard, 1990): 61-78, 61-62.

62 R. Thom, "Halte au hasard," 63 and 74.

63 I. Prigogine and I. Stengers, "Préface à la seconde édition," *La Nouvelle Alliance,* 2nd ed. (1986), 18. See also Prigogine's response to Thom: "Loi, histoire... et désertion," *La Querelle du déterminisme,* ed. K. Pomian: 102-112.

Extending over the whole decade of the 1980s, the debate occupied many people. In his fiery piece of 1980, Thom accused Edgar Morin, Henri Atlan, and Jacques Monod, together with Prigogine and Stengers, of being representative of "a French popular epistemology."[64] Framed in terms of a quarrel about the role of determinism in contemporary science, this debate is indicative of the difficulty of integrating new modeling practices into "normal science." Written in 1981, David Ruelle's contribution to the debate seems a conciliatory one. "I endeavored to show two things: on the one hand, that the determinism of natural laws does not exclude fantasy, the fortuitous, the impredictible and, on the other hand, that this unpredictibility cannot be converted into a predictible consequence of astral influences or psychic forces."[65] Between total randomness and determinism, a middle road existed, which was chaos, emphasizing the limits to predictibility. This would be the main message taken away from chaos.

(iv) Prigogine and Ruelle: Noiseless Turbulence

At the 1973 Brussels Conference, Prigogine underscored the consequence that his approach to phenomena of hydrodynamic instability had in reframing these questions. Pointing out the analogies among hydrodynamic instability, transitions leading to dissipative structures and phase transitions, Prigogine and his co-authors mused:

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[64] See K. Pomian, ed., *La Querelle du déterminisme,* for a collection of contributions by Stefan Amsterdamski, Henri Atlan, Antoine Danchin, Ivar Ekeland, Jean Largeault, Edgar Morin, Jean Petitot, and David Ruelle, most of whom we should note were followers of Thom's.

[65] D. Ruelle, "Hasard et déterminisme: le problème de la prédictibilité," *La Querelle du déterminisme,* 153-162; first publ. in *Traverses,* 23 (November 1981).

> One is surprised by the fact that, in spite of such strong analogies, the general belief in fluid dynamics is that the onset of convection or turbulence, or any other similar kind of instability, is principally triggered by the boundary conditions or by some external stimulus. This attitude, however, masks the *essential mechanism of instability.*

They went on:

> Suppose that we drive a fluid very carefully up to the Bénard threshold and that by suitable isolation we avoid the influence of any spurious disturbance. Is the fluid going to evolve to the regime with convection or is it going to remain still?

Drawing on the case of chemical kinetics, they answered that they believed that:

> the fluid will evolve and the reason for this is the existence of *fluctuations.* These spontaneous deviations from the mean are always present in a macroscopic system. They are known to be the primary cause of instabilities associated with phase transitions.[66]

In these comments, derived from the analogy with phase transition, and based on arguments appealing to the microscopic dynamics of fluids, Prigogine in fact concurred with one of the strong messages Ruelle wanted people to take away from the model he had proposed with Takens. In 1975, Ruelle indeed proclaimed that the view that noise was necessary in order to explain turbulent flows was misleading, since sensitive dependence on initial conditions was enough. For Prigogine as for Ruelle, although they argued the case differently, the onset of instability in fluid systems was a consequence of the exponential growth of initial disturbances that existed even in practically noise-free situations.

It is worth emphasizing that no mention of the Ruelle-Takens model was ever made in the published proceedings of the Brussels Conference. One therefore cannot

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[66] G. Nicolis, I. Prigogine, and P. Glansdorff, "On the Mechanism or Instabilities in Nonlinear Systems," 3. My emphasis.

see the views presented there as stemming from Ruelle and Takens's proposal, or even from the "dynamical systems approach" that was gaining ground for topics of instability in fluids. The push for interdisciplinarity came from a continuation of Prigogine's earlier concerns. The interdisciplinary character of the study of instabilities in fluids would, however, provide a fertile ground in which to plant Ruelle and Takens's model.

b) Rayleigh-Bénard as Phase Transition

(i) Phenomenological Analogy with Phase Transition

In the early 1970s, Jean-Pierre Boon was an experimenter collaborating with Prigogine's Brussels school, who would have an important indirect impact on the experimental study of the Rayleigh-Bénard problem. He was using light-scattering methods for the study of phase transitions in physical systems at their critical points, that is, when pressure and temperature are such that thermodynamic variables change continuously. At the time this was a booming field for two main reasons. The dearest to Boon was that new experimental techniques had recently come up using laser technology. The second reason was that the theoretical understanding of these phenomena had received a huge boost from the injection of renormalization group techniques in an article published in 1971 by Kenneth G. Wilson, a work for which he would receive the Nobel Prize in 1982. At the 1973 Brussels conference Boon characterized the analogy between phase transitions and fluid instability as follows.

> It is important to notice that the analogy between the phenomena considered herein [Rayleigh-Bénard] and phase transition phenomena is essentially *phenomenological*. . . . The analogy should be understood as a *translation*

> from the molecular description language used for phase transitions to the hydrodynamic mode jargon used for instability phenomena. [67]

Indeed, since Landau's analysis, studies of phase transitions aimed at exhibiting power laws, typically of the form $\propto (T - T_c)^\gamma$, where T was a parameter, often the temperature (with a critical value T_c at the transition). γ was called the critical exponent, which was the object of much theoretical and experimental scrutiny. In systems such as Rayleigh-Bénard, a critical exponent could also be calculated following Landau's theory for the onset of turbulence. He himself had predicted that for fluid instabilities $\gamma = \frac{1}{2}$.[68]

Thus, for Boon, much of what was done to study phase transitions could apply to fluid instabilities. Following Boon, it is however worthwhile to mention that the analogy was purely formal:

> at the level of microscopic analysis there is presently no evidence for an actual analogy between hydrodynamic or thermal instabilities and phase transitions for the very good reason that the microscopic mechanism governing the evolution of a system towards an instability point remains at present a totally open question.[69]

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[67] J.-P. Boon, "Light-Scattering from Nonequilibrium Fluid Systems," *Advances in Chemical Physics,* 32, ed. I. Prigogine and S. A. Rice, 87-99, 89. See also, P. A. Fleury and J.-P. Boon, "Laser Light-Scattering in Fluid Systems," *Advances in Chemical Physics,* 24 (1973): 1-93.

[68] L. D. Landau and E. Lifschitz, *Mécanique des fluides* (Moscow: Mir, 1971), 130. Since this critical exponent only depends on the first bifurcation, it could not be a test that could distinguish Landau's model for the onset of turbulence for Ruelle and Takens's.

[69] J.-P. Boon, "Light-Scattering," 89.

(ii) A Visit at Bell Labs: The Computer as Experimental Development

A few years earlier, in 1970, Jean-Pierre Boon visited Bell Laboratories, met staff member Günter Ahlers, and told him about Rayleigh-Bénard. Ahlers's experiments on Rayleigh-Bénard would inspire Martin to take up the study of the onset of turbulence. At the time, Ahlers was studying critical phenomena connected with the superfluid transition in liquid helium at about 2° Kelvin. Ahlers's recollections underscore the marginality of hydrodynamic research on Rayleigh-Bénard in 1970:

> It seems difficult to imagine from our present vantage point; but to my knowledge none of us [at Bell Labs] had ever heard about this phenomenon as an interesting physical system even though of course all of us were familiar with convection from everyday experience.[70]

Insisting on the "serendipitous aspects of Jean-Pierre's visit," Ahlers recalled that his apparatus was at the time ready and cold, so that he could perform some manipulations following Boon's suggestions. In his usual experiments, Ahlers constantly "was heating the [helium] sample from below as to *avoid* convection." Merely by increasing the temperature by a fraction of a degree, he could start studying Rayleigh-Bénard convection phenomena. Within "a day or two," he was "able to obtain heat-transport data which were a great deal more precise than previous results in the literature."[71]

This, however, was no coincidence. The material conditions for experimental work were by then being revolutionized by the introduction of new technologies in the

[70] G. Ahlers, "Over Two Decades of Pattern Formation, a Personal Perspective," *25 Years of Non-Equilibrium Statistical Mechanics: Proceedings of the XIIIth Sitges Conference, Held in Sitges, Barcelona, Spain, 13-17 June 1994,* Lecture Notes in Physics, 445, ed. J. J. Brey et al. (Berlin: Springer, 1995): 91-124, 94.

laboratory. Ahlers, and other physicists who like him had been studying critical phenomena (Gollub and Swinney, Bergé and Dubois), had at their disposal powerful tools which they brought to bear on fluid mechanics.

> Although the conventional tools of solid-state physics, such as high-resolution thermometry, lock-in amplifiers, light scattering, and others played an important role, I believe that the most important *experimental development* of the 1970's was the advent of the *computer* in the laboratory.[72]

To think of the computer as an experimental development may be surprising, but one should realize that this was not just a new machine coming into the laboratory. Early in 1971, Ahlers had a home-made data acquisition system, borrowed from his colleague J. E. Graebner, enabling him to collect time series of experimental data containing *several thousands of values.* Using numerical fast-Fourier transforms, he could likewise quickly transform time series into power spectra. One might recall that, as early as 1973, these spectra were clearly identified by David Ruelle as the experimental data one should consider in order to check his suggestions.[73]

Just as for the study of differential equations, the advent of the computer did not merely entail that one could do the same operation as before, only much faster. It meant that the very conceptualization of the problems one decided to tackle was to be totally rethought. These developments, Ahlers witnessed, "*revolutionized* the kind of project that could be tackled." Computers (data acquisition and analysis systems) "not

[71] G. Ahlers, "Over Two Decades," 94.
[72] G. Ahlers, "Over Two Decades," 96. My emphasis.
[73] D. Ruelle, "Some Comments on Chemical Oscillations," *Transactions of the New York Academy of Sciences,* series II, 35 (1973): 66-71; repr. *TSAC,* 109-115.

only provided a new tool, but they also gave us a *completely new perspective on what types of experiments to do.*"[74]

(iii) Physicists Take over Fluid Mechanics, Part I

In 1994, Ahlers also emphasized another advantage physicists had over the traditional researchers working on Rayleigh-Bénard. As opposed to applied mathematicians and engineers, physicists for the most part had not previously appreciated the interesting problems of hydrodynamic instability.

> When physicists finally learned about this fascinating area of study, I believe they soon began to play an important role. The experimentalists did not feel constrained by the practical needs of the engineer and felt free to concentrate on problems which were *simple enough to be amenable to theoretical analysis and quantitative experimental study.*[75]

Of course, this can only be a physicist's appreciation of the role physicists played. These relatively simple systems may not have presented much interest for the engineers, and their study could rightly be considered as rather sterile by them. On the other hand, this approach was hardly new in the field of hydrodynamic instability, as the study of stability theorists in Chapter VII plainly shows. The experience physicists working on critical phenomena had in comparing sophisticated experimental results with sophisticated theories, however, was a crucial difference with stability theorists. Indeed, the insistence on observable quantities, which was a trademark of physicists, would help to bring experimenters, theoreticians, and even mathematicians, closer to one another: an important feature of the history of chaos, as we may recall Pierre Bergé emphasizing.

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[74] G. Ahlers, "Over Two Decades," 96. My emphasis.

In 1970, however, Günter Ahlers did not become an instantaneous convert to the study of Rayleigh-Bénard. Perhaps still feeling that it was merely an interesting but marginal physical phenomenon which hardly deserved more than "a day or two," he did not publish his results.[76] At least one traditional fluid dynamist, namely Koschmieder, nevertheless was quite enthusiastic about Ahlers's results. In a review article on Bénard convection published in Prigogine's own *Advances in Chemical Physics,* Koschmieder thus concluded an otherwise bleak review:

> We end this article on an optimistic note by mentioning the absolutely new approach to convection experiments that has been introduced recently by Ahlers. . . . In such experiments we finally arrive at infinitesimal disturbances the theories have postulated all along.[77]

What then were Ahlers's results, so exciting for some, and not worth a publication in the opinion of their author? The physicist from Bell Labs simply measured how heat fluxes across his helium-filled cell evolved as the temperature gradient, or equivalently the Rayleigh number R, was increased above the critical value R_c where convection set in. By exploiting the experimental advantages of low-temperature techniques Ahlers could achieve "measurements of very high resolution

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[75] G. Ahlers, "Over Two Decades," 94.

[76] "Evidence of my prior rather casual attitude toward [the Rayleigh-Bénard problem] can be found in the fact that, except for a couple of talks at the January 1972 APS meeting in San Francisco, I had not really published my results." G. Ahlers, "Over Two Decades," 100-101. APS above stands for the American Physical Society. Short abstracts of his talks were however published: "Convective Heat Transport Between Horizontal Parallel Plates," *Bulletin of the American Physical Society,* 17 (1972): 59-60; G. Ahlers and J. E. Graebner, "Time Dependence in Convective Heat Transport Between Horizontal Parallel Plates," *Ibid.*: 61.

[77] E. L. Koschmieder, "Bénard Convection," 209. Similarly, having heard about it from Koschmieder, M. G. Velarde concluded his 1973 review with a mention of Ahlers's "impressive and most complete work": "Hydrodynamic Instabilities," 521.

and great accuracy."[78] Much like Krishnamurty, at about the same time, he measured the Nusselt number N and plotted it against R. He observed that N was independent of time for values of R up to $R_T \approx 2R_c$. At this point, however, a second transition took place and N suddenly became time-dependent, exhibiting a continuous power spectrum and showing the absence of oscillatory modes. Was this evidence for sudden transition to turbulence?

In line with his earlier experiments on phase transitions, Ahlers at first was not interested by this question. Instead, he focused on fitting a power law with his data. Beyond R_T the Nusselt number seemed to follow a similar law to the one Landau's theory gave: $N \propto \left[(R-R_T)/R_T\right]^{1/2}$.

(iv) The Topological Analogy

The analogy between phase transitions at critical points and hydrodynamic instabilities described by the tools of dynamical systems lay not only in the fact that similar experimental tools and techniques could be used to study both, but also in the very spirit of the theories. As I explain below, this analogy would greatly inspire the work of Pierre-Gilles de Gennes and his collaborators. It also was picked up by Thom

[78] G. Ahlers, "Low-Temperature Studies of the Rayleigh-Bénard Instability and Turbulence," *Physical Review Letters,* 33 (1974): 1185-1188, 1185. See his later studies of convection: R. P. Behringer and G. Ahlers, "Heat Transport and Critical Slowing Down Near the Rayleigh-Bénard Instability in Cylindrical Containers," *Physics Letters,* 62A (1977): 329-331; and G. Ahlers and R. P. Behringer, "Evolution of Turbulence from the Rayleigh-Bénard Instability," *Physical Review Letters,* 40 (1978): 712-716.

who interpreted Landau's theory in terms of catastrophes.[79] One could find "conceptual kinship" between the modeling practices coming out of Thom's school and the renormalization group approach that excited physicists in the early 1970s, as is witnessed by a course given by French physicist Gérard Toulouse, from the Laboratoire de physique des solides at Orsay:

> The renormalization group approach leads to a *topological* description of phenomena in an abstract space: the space of parameters. . . . From this point of view, the approach possesses a conceptual kinship with the mathematical theory of dynamical systems. . . . [Both] theories, characterized by their global approach and their insistence on universality properties, constitute a new framework for thought.[80]

Indeed the study of critical phenomena at phase transitions had already constituted a "vast domain of physics [which had] constituted itself 'horizontally'." Many similar phenomena concerning phase transitions in magnetic materials, superfluid helium, superconducting metals, etc., had made physicists suspect that there was an ensemble of analogies that could be rigorously codified. In October 1969, one such physicist, Cyril Domb, contended:

> Unifying features have been discovered which suggest that the critical behavior of a larger variety of theoretical models can be described by a simple type of equation of state. But the rigorous mathematical theory needed to make the above development "respectable" is still lacking.[81]

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[79] R. Thom, "Phase Transitions as Catastrophes," *Statistical Mechanics: New Concepts, New Problems, New Applications,* ed. S. A. Rice, et al. (Chicago: University of Chicago Press): 93-107.

[80] G. Toulouse and P. Pfeuty, *Introduction au groupe de renormalisation et à ses applications. Phénomènes critiques des transitions de phase et autres* (Grenoble: Presses universitaires de Grenoble, 1975), 8-9. My translation and emphasis. See however the English translation: *Introduction to the Renormalization Group and to Critical Phenomena* (London: John Wiley, 1977).

[81] Quoted in C. Domb, *The Critical Point: A Historical Introduction to the Modern Theory of Critical Phenomena* (London: Taylor & Francis, 1996), xi.

According to Domb, the adaptation by Kenneth G. Wilson, in 1971, of renormalization group methods, which had been introduced in the 1950s in order to account for the infinities that were plaguing quantum electrodynamics, to the study of critical phenomena did not make these analogies rigorous. But it had made them "respectable."[82] In his work, Wilson had built upon Leo Kadanoff's "hypothesis of universality" which allowed to establish correspondences between systems.[83] The term 'universality' would soon be applied to chaos, and the excitement that greeted Feigenbaum's theories in the late 1970s can be fully appreciated only in relation to the explicit connection he was drawing between the renormalization group and dynamical systems, a connection which had been suspected for a long time.[84]

But it was Gérard Toulouse who first noted "the great heuristic value" of the topological analogy between the two approaches. Citing Thom's *SSM,* Toulouse and Pfeuty wrote that in both cases:

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[82] For histories of the renormalization group in QED, see L. M. Brown, ed., *Renormalization: From Lorentz to Landau (and Beyond)* (New York: Springer); T. Y. Cao, *Conceptual Developments of 20th Century Field Theories* (Cambridge: Cambridge University Press, 1997); and S. S. Scweber, *QED and the Men who Made It: Dyson, Feynman, Scwhinger, and Tomonaga* (Princeton: Princeton University Press, 1994). The classic papers are: E. C. G. Stueckelberg and A. Pétermann, "La normalisation des constantes dans la théorie des quantas," *Helvetica Physica Acta,* 26 (1953): 499-520; M. Gell-Mann and F. E. Low, "Quantum Electrodynamics at Small Distances," *Physical Review,* 95 (1954): 1300-1312; N. N. Bogoliubov and D. V. Shirkov, *Introduction to the Theory of Quantized Fields* (New York: Interscience, 1959); and K. G. Wilson, "Renormalization Group and Critical Phenomena," *Physical Review,* 4B (1971): 3174-3205.

[83] L. P. Kadanoff, "Critical Behavior. Universality and Scaling," *Critical Phenomena: Proceedings of the International School of Physics Enrico Fermi,* Course LI, Lake Como, Italy, 27 July-8 August 1970, ed. M. S. Green (New York: Academic, 1971): 100-117.

[84] About universality in chaos, see of course P. Cvitanovic, ed., *Universality in Chaos,* 2nd ed. (Bristol: Adam Hilger, 1989).

> one is led to a global approach to phenomena, to an analogous classification of singularities, to a similar understanding of universality properties. This rapprochement may be noted, for it is perhaps indicative of a theoretical moment in the formation of a certain level of knowledge.[85]

The analogy with dynamical systems was however severely limited by the fact that, in the renormalization group approach, one was only interested in fixed points. Limit-cycles, much less strange attractors, did not have an immediate analogue in critical phenomena.

Therefore, Toulouse would be among the few French physicists who visited the IHÉS in the 1970s with the goal of working with Thom. He wrote Thom in September 1975 to ask whether he could spend a sabbatical year at the IHÉS. On February 11, 1976, Toulouse talked about "Critical Phenomena and the Renormalization Group." As he later wrote to Kuiper, Toulouse was enthusiastic about the result of his stay at the IHÉS.

> My frequenting your institute was very beneficial to me, by allowing all kinds of unfreezings [*déblocages*] of a mathematical or psychological nature. My progress in the theory of the classification of defects in ordered matter were greatly helped by discussions with René Thom and Louis Michel.[86]

In summary, the analogy between hydrodynamic instabilities and phase transition was a potent one. It took many forms. For Prigogine and his collaborators, variational methods offered the hope of unifying many fields of science, which directed his attention towards new developments in the study of nonlinear equations. For Boon and Ahlers, as well as for Swinney and Gollub, the phenomenological

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[85] G. Toulouse and P. Pfeuty, *Introduction au groupe de renormalisation,* 32.
[86] Lettres de Gérard Toulouse à René Thom (15/9/75); de René Thom à Gérard Toulouse (14/10/75); *Rapport scientifique* 1976; and lettre de Gérard Toulouse à Nicolaas Kuiper (20/9/76) from which is the above quote is extracted.

analogy served as a basis for applying their experimental setups to hydrodynamic phenomena and thus come up with data of greatly improved accuracy. Finally, for Toulouse, the topological analogy between renormalization group methods and the IHÉS modeling practices was the dawn of "a new framework for thought." Physicists' excitement about phase transitions had directed their attention to hydrodynamic phenomena. As it would turn out, while the experimental analogy had a considerable impact, theoretical analogies in the end proved disappointing.

c) Rayleigh-Bénard as a Test-Case for Theories of Turbulence

When one opens up the issue of the *Physical Review Letters* where Günter Ahlers first published his results in 1975, it is striking to note that it was placed just before the numerical computations of John McLaughlin and Paul Martin. In fact, this was not coincidental.

In May 1973, a meeting on critical phenomena was held at Temple University, which provided an opportunity for Ahlers, then spending a sabbatical year in Germany, to show some of his experimental results on the Rayleigh-Bénard convection to Paul Martin. "I think," Ahlers recalled, "that Paul's genuine interest in my results played an important role in convincing me that [this] study should be taken seriously."[87] As for Martin, he acknowledged that "our own interest in the Lorenz model was intensified when we were told of the experiments Ahlers had performed on heat flow in normal (that is, not superfluid) liquid helium as a function of the Rayleigh

[87] G. Ahlers, "Over Two Decades," 100.

number."[88] If we believe Jean-Pierre Eckmann's recollections to the effect that Martin initially was skeptical of the Ruelle-Takens model, we may better appreciate the mutual benefit that both physicists derived from the confrontation of their results.[89]

A student of Julian Schwinger, Paul C. Martin was a theoretical physicist who had worked on several problems, from quantum electrodynamics to statistical physics, *via* the many-body problem. In particular, he had been interested in critical phenomena and was mentioned in the acknowledgments of Wilson's paper on renormalization.[90] In 1971-1972, he had spent a sabbatical year at Orsay working with Cyrano da Daminicis in the solid-state group. Around this time, realizing the analogy with phase transitions, he got interested in turbulence. In 1972, as we saw in Chapter VII, he gave a talk on this topic at the IHÉS. An unpublished manuscript of his, dated 1972, had even been circulating, which was titled: "A Tentative Picture for Fully Developed Turbulence."[91]

A Harvard physicist, he had moreover had a chance to get introduced to the Lorenz model through the MIT applied mathematician Barry Saltzman. No doubt partly as a consequence of his meeting with Ruelle, he moved from the study of developed turbulence to that of the onset of turbulence. With the help of his graduate student McLaughlin, he attempted to build a "unified picture of turbulence," based on numerical computations involving the integration of as many as 39 first order

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[88] P. C. Martin, "Instabilities, Oscillations, and Chaos," C1-63.
[89] Interview of Jean-Pierre Eckman by the author (13 March, 1997).
[90] K. G. Wilson, "Renormalization Group," 3205.
[91] Mentioned in J.-P. Boon, "Light-Scattering," I have not had a chance to see it.

equations.[92] By building on the theoretical works of Ruelle, Takens, Lorenz, Joseph, Sattinger, and Busse, as well as the experimental results of Ahlers, Krishnamurty, Bergé, and Dubois, Martin and McLaughlin placed themselves in the position of being able to confront the many recent contributions to the Rayleigh-Bénard problem coming from such different horizons.

Retrospectively, their pair of articles were seen as being among the first hints for a confirmation of the Ruelle-Takens model. In 1975, Ruelle himself found it "excellent although perhaps over enthusiastic."[93] But they could not say more than that their model "seem[ed] to agree with the qualitative picture of the transition to turbulence suggested by Ruelle and Takens."[94] The agreement was at best "semiquantitative." When he spoke at the Dijon Symposium, during the summer of 1975, Paul Martin was even less optimistic. "It is far too early to claim that any of [these models] gives the essence of the phenomenon of turbulence—if indeed it is a single phenomenon and has an essence."[95] In short, he was arguing directly against Ruelle and Takens's claim to have found the "mechanism for the generation of turbulence."[96]

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92 J. B. McLaughlin and P. C. Martin, "Transition to Turbulence of a Statically Stressed Fluid," *Physical Review Letters,* 33 (1975): 1189-1192; "Transition to Turbulence in a Statically Stressed Fluid," *Physical Review A,* 12 (1975): 186-203.
93 D. Ruelle, "The Lorenz Attractor," 154.
94 J. B. McLaughlin and P. C. Martin, *Physical Review Letters,* 33, 1189.
95 P. C. Martin, "Instabilities," C1-57.
96 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 from the abstract on p. 167. In interview Jean-Pierre Eckmann claimed that Martin had embarked on his work with McLaughlin with the goal of showing the Ruelle-Takens model to be false.

At Dijon, Martin compared Landau's model for the onset of turbulence with three other alternative pictures. In the figure he introduced to illustrate these alternatives (Fig. 15), Martin drew an axis along which the Reynolds (or Rayleigh) number R increased. On this axis, he located the many bifurcations that might occur as the system was subjected to increasing external stress. Using the language of bifurcation theory, Martin described the behavior of the flow below and above the critical value R_c:

> Below R_c all orbits in phase space are attracted towards a single point. Above R_c they are attracted towards one of either two points which describe the rolls going either clockwise or counter-clockwise.[97]

As we saw above, Martin expressed words of caution with respect to both the Ruelle-Takens and the Landau-Hopf pictures. The most striking feature of Martin's reaction to the Ruelle-Takens model was that he chose to argue for or against it on totally different grounds than Ruelle and Takens. In general, his discussion of their assumption remained quite vague. Constantly mentioning "genericity" with the quotation marks, he never attempted to convince his audience on mathematical grounds. For him, the adoption of the Ruelle-Takens model did not entail a modification of the standard modeling practice of physicists. Even while mobilizing a wide variety of resources, Martin never turned to Smale's or Thom's writings and did not adopt a "dynamical systems approach." The Ruelle-Takens model was seen in terms of a precise prediction, namely that turbulence set in suddenly after the appearance of a limited number of oscillatory modes. This could be checked by

[97] P. C. Martin, "Instabilities," C1-60.

numerically integrating the Navier-Stokes equations, or through carefully designed laboratory experiments.

Moreover, as mentioned above, Martin's interest was not limited to the onset of turbulence, but extended to the domains of fully developed turbulence, where any dynamical systems analysis in terms of few degrees of freedom had no hope of being relevant. For these cases, Martin held on to the idea that renormalization group techniques, as they had been recently developed for the study of critical phenomena, might prove helpful, but he could do little more than raise a few open questions.

In conclusion, Martin's Dijon talk did provide a new framework within which to incorporate a wide variety of studies of the onset of turbulence. However, he had to acknowledge that the modeling practice he had mobilized for this study, namely analogies with phase transition and critical phenomena, had proved disappointing.

> While the experimental techniques that have been invaluable in understanding phase transitions promise to be very useful in the study of hydrodynamic phenomena, I suspect that the recent addition to our theoretical arsenal [coming from phase transition studies] may be less effective than many had hoped.[98]

Emphasizing the experimental techniques of phase transition, Martin showed the inadequacy of the corresponding theoretical framework, but he did not venture to make the subsequent move, which would be to replace it by concepts and practices coming from dynamical systems theory. The synthesis he had attempted was at best incomplete.

> The title of this talk [Instabilities, Oscillations, and Chaos] applies not only to the phenomena I have been discussing but to our understanding of these

[98] P. C. Martin, "Instabilities," C1-57.

phenomena. And lastly it applies to the talk itself which has now reached its final chaotic stage.[99]

The French audience Martin was addressing at the Dijon Symposium, however, was one that had been trying to attack hydrodynamic instability phenomena with the physicists' tools for some years already. One of the experimenters who talked about nonlinear effects at the Dijon Symposium, Monique Dubois, later fondly recalled Martin's visit to her laboratory, and the effect this had on her later work with Pierre Bergé. Among those present at Dijon, was Albert Libchaber, an experimenter from the École normale supérieure in Paris, who would, a few years later, provide careful evidence in favor of several "scenarios" for the onset of turbulence. His experiments played a major role in propelling chaos to the fore of physics. In the early 1980s, French physicists would be among the first to synthesize the results of chaos theory into a coherent, dynamical systems framework, as witnessed by the book of Pierre Bergé, Yves Pomeau and Christian Vidal, titled *Order within Chaos.* In this book, they noted that "the contributions [to the study of dynamical systems] originating in France occupy one of the most honorable places in this domain, theoretically as well as experimentally."[100]

The rest of this chapter will therefore be focused on France. It will examine how the audience for the Dijon Symposium was built as a result of a deliberate effort on the part of French science policy makers to promote interdisciplinary research in what they called "light physics [*physique légère*]", as opposed to "big" nuclear and particle physics emphasized earlier. We shall see how, out of that more or less

[99] P. C. Martin, "Instabilities," C1-66.

successful effort, research programs that would prove congenial both to the forging of links with other groups working on hydrodynamic instabilities and phase transitions and the reception of the Ruelle-Takens model. This chapter will end by examining the role Ruelle himself and the IHÉS played in this story, and by looking at the way new experiments and syntheses made by physicists ended up in an adaptation of the IHÉS modeling practices that differed importantly from the original ones.

4. HYDRODYNAMICAL INSTABILITIES AND TURBULENCE IN FRANCE, 1971-1975

a) Three conferences in France

The Dijon Symposium was not an isolated event. In the course of the summer 1975, three conferences took place at various places in France dealing in part with hydrodynamics and turbulence. A sign of renewed interest in classical physics, these meetings expressed a feeling that something new was happening with these topics. This clearly stated feeling seemed to be shared by many communities of scientists. And the ultimate goal of establishing channels of communication across disciplinary boundaries ran across all these meetings. In 1975, however, it might have been somewhat difficult to see what any of the three conferences might have had to do with the others.

On June 12-13, a workshop on "Turbulence and the Navier-Stokes Equations" was held at the University of Orsay. Organized by Roger Tenam, the Orsay workshop gathered mathematicians, physicists, hydrodynamicists, and in particular the stability

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[100] P. Bergé et al., *Order within Chaos,* xv.

theorists mentioned in Chapter VII, namely Joseph, Sattinger, and Iooss. A mathematician specialized in the analysis of computer algorithms for the modeling of hydrodynamic flows, Tenam thought that there had been "a strong renewed interest in the mathematical aspects of Turbulence during recent years," as he wrote in his preface. But he was not an unconditional promoter of the Ruelle-Takens model. Indeed, that very same year, with the help of Ciprian Foias, he put forth views that seemed "to redeem Leray's point of view on the occurrence of turbulence," showing that if neither the Ruelle-Takens nor the Landau-Hopf pictures occur, then Leray's scenario should occur.[101]

The Orsay workshop was organized by the Société Mathématique de France (SMF), "in order to examine the present state of the subject. . . . This was the opportunity of very stimulating interdisciplinary contacts." Although he was on the organizing committee, Ruelle could not attend the Orsay Workshop, but nonetheless contributed a piece in the published proceedings where, for the first time, he picked up the Lorenz attractor. Among the other organizers, were C. Bardos (Nice), M. Craya (Grenoble), U. Frisch (Nice), and J.-L. Lions (Collège de France, Paris). In addition, session chairmen were P. Germain, G. Guiraud (Iooss's advisor), and J. Leray. Among the lecturers, one notes Benoît Mandelbrot, and especially astrophysicist Michel Hénon and plasma physicist Yves Pomeau who, on this occasion, studying the Lorenz

[101] C. Foias and R. Tenam, "On the Stationary Statistical Solutions of the Navier-Stokes Equations and Turbulence," *Publications mathématiques d'Orsay,* N° 120-75-28 (1975). Jussieu Lib.

model, introduced a famous strange attractor that would soon be widely known as the Hénon attractor.[102]

Between June 30 and July 4, the Société française de physique (SFP) held a similar meeting in Dijon, mentioned previously in connection with the Paul Martin's talk. Organized by A. Martinet, from the Solid State Physics group of Orsay, the Dijon Symposium dealt with "Physical Hydrodynamics and Instabilities." Coupled with a half-day on "Instabilities and Critical Phenomena," the Dijon Symposium "provided," Martinet contended, "an occasion for exchange between the community of physicists and that of fluid mechanists." Again, the "pluridisciplinary character" of the topics raised was emphasized, as well as the objective of "assessing [*faire le point*] the present state of knowledge on turbulence and the contribution of recent theories describing instabilities."[103] Aside from Martin's talk, the rest of the Dijon Symposium was devoted mainly to experimental studies of various aspects of turbulence and instabilities in fluids.

Surprisingly perhaps, the Dijon Symposium had almost no overlap with the Orsay Workshop. Only Auguste Craya, who was to die a few months later, had been involved in both, and his prospect was bleak. "To speak about turbulence is difficult," he wrote.

> For more than a hundred years, the accumulated knowledge in this regard has above all been experimental. . . . Theoretical efforts on a few simple cases

[102] R. Tenam, ed., *Turbulence and the Navier-Stokes Equations: Proceedings of the Conference held at the University of Paris-Sud, Orsay, June 12-13, 1975,* Lecture Notes in Mathematics, 565 (Berlin: Springer, 1976).

[103] Proceedings of the Dijon Colloque were published in the *Journal de physique,* 37, *Suppl.,* Colloque C1 ["Hydrodynamique physiques et instabilités"] (1976). Quotes are taken from Mrtinet's "Avant-propos."

> should be pursued by using all the modern resources of mathematics and statistical mechanics; some abnegation will nonetheless be needed, for it does not seem that we are entitled to expect decisive breakthroughs to occur in the short term.[104]

The two conferences at Orsay and Dijon made blatant the chasm that separated theoreticians from experimentalists in new studies of turbulence. As emphasized by Paul Martin, the experimenters mainly came into this field by adapting the tools and practices they had exploited with success for the study of critical phenomena, while theoreticians at Orsay relied on techniques displaying a much wider array of approaches.

Finally, in September, from the 14th to the 21st, an "International Conference on Dynamical Systems in Mathematical Physics" was held at the University of Rennes, in Brittany. Organized by G. Galleotti, M. Keane, and D. Ruelle, and sponsored by the SMF, it welcomed a large delegation of scientists that had often been associated with the IHÉS. The Rennes Conference was however much more focused on statistical mechanics than turbulence.[105]

In all three conferences, a clear emphasis was put on interdisciplinarity. Nonlinear phenomena, associated with macroscopic physics, were tackled by mathematicians and physicists with a feeling that communication across disciplinary boundaries was a necessary condition for achieving some progress. But tools used in order to achieve an interdisciplinary understanding of turbulence were quite varied. One of the few common concepts mentioned at all conferences were strange

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[104] A. Craya, "Turbulence," *Journal de physique,* 31, Colloque C1 (1975): 35-55. Quote from pp. 35 and 54. Craya deceased on February 13, 1976.

attractors. Clearly something was going on. Can this have been a consequence of Ruelle and Takens's model for the onset of turbulence and a growing recognition of the usefulness of a "dynamical systems approach" for studying of nonlinear phenomena? I think not.

b) An 'Action thématique programmée' on turbulence

In the prefaces of the Orsay Workshop and the Dijon Symposium, a common acknowledgment indicates that there was a common force behind these two meetings dealing with turbulence in an interdisciplinary fashion, but otherwise so different from one another. The organizers of both conferences thanked the CNRS for having sponsored their meetings as part of an "ATP" devoted to "Turbulence and Instabilities." This particular program, in which Pierre-Gilles de Gennes was importantly involved, provided the initial glue for the complex alliances that made it possible for an important chaos constellation to emerge in France in the course of the 1970s—a constellation which would embrace the Ruelle-Takens model as a welcome exemplar for their undertaking. In particular, it will become obvious that groups working on liquid crystals important for drawing attention on the study of hydrodynamic instabilities benefited from the possibilities offered by this ATP.

(i) The VIth Plan, the CNRS, and the ATPs: Active Management of Scientific Research

Here, ATP stood for "Action thématique programmée [programmed thematic action]." They were a means established by the Centre national de la recherche scientifique

[105] M. Keane, ed., *International Conference on Dynamical Systems in Mathematical*

(CNRS) in 1971, to give priority sponsorship to certain interdisciplinary domains of research emphasized by the VIth Plan.[106] In 1970-1971, a group of physicists and chemists had written a report on "The Study of Matter and Radiation" for the Commission of the Plan. They expressed that research in physics (outside of nuclear physics) and chemistry had been previously sacrificed to more pressing concerns.[107] They pushed for great financial efforts to meet a few "objectives," especially concerning material sciences (solids, liquids, gas, plasmas, etc.). For this, they outlined vague research "programs" to be developed along a few "axes." One such program was called "Fluid and Plasma Dynamics," described as "hydrodynamics,

Physics, Rennes 1975, in *Astérisque,* 40 (Paris: SMF, 1976).

[106] Starting in 1946, the French Government had designed the Plan in order to cope with reconstruction after World War II and insure the economic and social development of the country. From 1953 on, the Plan included a chapter on scientific and technological research, which emphasized technological applications. In 1958, the Délégation générale à la recherche scientifique et technique (DGRST) was created in order to promote a coherent science policy at the highest level of the government and present its recommendation to the Commissariat général du Plan. On the history of the French Plan, H. Rousso, ed., *La planification en crises (1965-1985)* (Paris: Editions du CNRS, 1988); Richard F. Kuisel, *Le capitalisme et l'Etat en France. Modernisation et dirigisme au XXè siècle,* traduction française (Paris: Gallimard, 1984); P. Massé, *Le plan ou l'anti-hasard* (Paris: Gallimard, 1965). About science and technological policies, see, in particular, A. Prost, "Les origines de la politique de la recherche an France (1939-1958)," *Cahier pour l'histoire du CNRS,* 1 (1988): 41-62; J.-F. Picard, *La République des savants* (Paris: Fayard, 1990); R. Gilpin, *France in the Age of the Scientific State* (Princeton: Princeton University Press, 1968), chap. VIII; P. Papon, "Research Planning in French Science Policy: An Assessment," *Research Policy,* 2(1973): 226-245.

[107] "We must be conscious of the fact that the brutal freezing [*blocage*] of the last few years, above all the freezing in the authorizations of programs in light physics and chemistry has led university researchers to the brink of bankruptcy." DGRST, *Rapport de la Commission du 6e Plan, 1971-1974. Recherche,* tome 2 (Paris: La Documentation française, 1971), Chapitre I: "G.S. 1 - Etude de le matière et du rayonnement," 11-32. Fonds doc. CNRS. Quote on p. 16. The group insisted on the fact that many laboratories in "light physics and chemistry" were now in a "catastrophic situation" (p. 15).

physics of gases and plasmas, researches having controlled thermonuclear fusion in view." It included an ATP called "Instabilities and Random Phenomena in Liquids, Gases, and Plasmas," which was to receive 9 million francs out of a total of 15 MF devoted to the objective.[108]

The new orientations in science policy outlined by the VIth Plan in terms of objectives led the CNRS to introduce, in 1971, a new frame in which to push for the realization of these objectives: the "action thématiques programmées" (ATP). Since its inception, in 1939, the CNRS was supposed to "stimulate [*provoquer*], coordinate, and encourage researches in pure and applied science, . . . and especially to ease scientific researches and works concerning national defense and economy."[109] But it had found this role a hard one to assume.

At a symposium organized by these two agencies in 1975, Herbert Curien, an ex-General Director of the CNRS responsible for the creation of ATPs, explained that as far as management of research was concerned two attitudes were possible: the "passive" one, traditionally assumed by the CNRS, and the "active" one.

> The active way consists in sending out a few cubic decimeters to the outside . . . by telling the laboratories: if you wish to do something in this domain, we are ready to help you, do you have offers to make to us? This is what we inaugurated with ATPs.[110]

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108 DGRST, *Rapport de la Commission du 6e Plan, 1971-1974. Recherche,* 2, 14, and 26. For comparison the total annual budget of the IHÉS for 1971 was of the order of 2.5 MF.
109 §3 of the article 1er of the décret creating the CNRS on October 19, 1939; quoted by A. Prost, "Les origines de la politique de recherche," 42.
110 H. Curien, in *La Planification et l'administration de le recherche. Séminaire DGRST-CNRS, Gif-sur-Yvette, 3-4 juillet 1975.* Arch. CNRS, Fonds doc.

ATPs would provide the means for the CNRS to entice research groups in tackling some of the Plan's objectives. It was a way for the administrators of the CNRS to bypass the corporations of specialists in its different sections, so that a few objectives would be tackled by loose interdisciplinary groups. With this goal in mind, it appeared "that a certain rigidity of research structures and the traditional separation [*cloisonnement*] of discipline required to complement the usual measures of financing with more direct and selective modes of incitement."[111] In the eyes of the administration, the promotion of interdisciplinary research themes imposed to circumvent the corporate demands of specialists.[112]

The emphasis was put on the flexibility of the method, which was managed by *ad hoc* committees, with members nominated by the administrators being in the majority, and with other members delegated by the concerned sections of the National Committee (regrouping representatives elected by the research personnel of the CNRS). These committees received the task of designing specific ATPs, elaborating general programs of research, and submitting them to the scientific community.[113] In

[111] R. Chabbal and J. Gavoret, "Les actions thématiques programmées du CNRS en physique," typed manuscript (1971). CNRS Arch. G940035 LABOS, n° 16. See the published version, which does not contain the above quote, in *Courrier du CNRS,* 3 (January 1972): 37-40.
[112] J.-F. Picard, *La République des savants,* 255.
[113] The label ATP was indistinctly applied to the committees who worked on the "objectives" of the Plan, and to more specific actions undertaken under such "objectives." Typically, an ATP was to be in activity for about three years. In 1971, thirteenth ATPs were organized with a total budget of about 15 MF. In 1974, the funds devoted to ATPs represented 10% of those affected to equipment by the CNRS, and 5% of the total funds spent by the CNRS, except for personnel. See R. Chabbal and J. Gavoret, "Les actions thématiques programmées"; CNRS, *Rapport d'activité 1971* (Paris: CNRS, 1971), 52-54; and Procès-verbal de la séance du Directoire (25 et 26/6/74). Point V. Arch. CNRS. G870168 SGCN n°1.

short, in 1971, the General director of the CNRS, Hubert Curien, described ATPs as follows:

> ATPs, which we can represent to ourselves as "laboratories without walls," were conceived in order to allow the better linkage of selected researches to a determinate finality, the anticipation over several years of the necessary funds for actions considered as having priority, [and] the orchestration, in a common effort of reflection and realization, of scientists eventually belonging to different disciplines.[114]

Reviewing the benefits derived from ATPs in 1974, Robert Chabbal, who would become General Director of the CNRS in 1976, contended that they had "encouraged researchers to change research subjects" and helped to organize collective actions tightening links between laboratories. As a consequence, "interdisciplinary themes have become more numerous and have expanded."[115] Clearly, interdisciplinarity was seen as highly desirable.

Not before 1973 was a specific ATP undertaken to address the objective of the Plan described above on "Instabilities and Random Phenomena in Liquids, Gases, and Plasmas." However, one was organized on the theme: "Molecular Fluid" within the objective "Materials."[116] Among the nominated members of the *ad hoc* committee was Pierre-Gilles de Gennes, who was responsible for subcommittee A on the Physics of Molecular Fluids.[117]

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[114] Quoted in R. Chabbal and J. Gavoret, "Les actions thématiques programmées," 40.
[115] Procès-verbal du Comité sectoriel II: Physique (19/9/74), 5. Arch. CNRS, G870168 SGCN n°4.
[116] CNRS, *Rapport d'activité 1971,* 54.
[117] Dossier général de l'ATP "Matériaux." Arch. CNRS, G950016 DPM n° 1.

(ii) Liquid Crystals: Analogies Get Real

"The media have a tendency to present us (me and my colleagues of a similar type) as jacks-of-all-trades [*touche-à-tout*], people able to pass through walls [*passe-murailles*], jumping from one domain to another."[118] Indeed, the career of Pierre-Gilles de Gennes was punctuated by a few spectacular changes of orientation.

A student of the École normale, de Gennes entered the CEA at Saclay in 1955 and worked with André Herpin on a thesis devoted to the study of magnetic materials irradiated by neutrons. In 1961 he became assistant professor [*maître de conférences*] at the University of Orsay and joined the Laboratoire de physique des solides. Then, he, a theoretician, had "the naive idea of directing an experimental group on *superconducting* metals."[119] In 1968, however, de Gennes and his collaborators learned from a Russian paper about a completely different subject: *liquid crystals.*

> For us, it was a revelation. There were holes in there, fascinating things that the Russians had let go! A true gold mine!... Within a few months, everybody in the teams around us had realized that we had to exploit the vein.[120]

Constituted of elongated molecules sensitive to electromagnetic fields, liquid crystals could exhibit different phases: isotropic, nematic, or smectic. They therefore provided

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[118] P.-G. de Gennes and J. Badoz, *Les Objets fragiles* (Paris: Plon, 1994), 167.
[119] P.-G. de Gennes and J. Badoz, *Les Objets fragiles*, 167. On the career of de Gennes, see his recollections in *Hommes de science: 28 portraits,* interviewed and photographed by M. Schmidt (Paris: Hermann, 1990): 81-89; G. Deutscher "De la physique des solides à celle de la matière 'molle'," *La Recherche,* 22 (1991): 1478-1479; and a special issue of *Science et vie,* hors-série n°192 (September 1995), devoted to him. On the Orsay group on solid-state physics, see J. Friedel, "Le laboratoire de physique des solides d'Orsay," *Courrier du CNRS,* 3 (January 1972): 22-26.
[120] P.-G. de Gennes and J. Badoz, *Les Objets fragiles*, 166-167. See pp. 71-87 for a popularization of the physics of liquid crystals.

an excellent field of study in order to tackle simultaneously problems related to phase transitions and hydrodynamic instabilities. This fact made collaborators of de Gennes eager to participate in the 1973 Brussels conference on hydrodynamic instabilities and dissipative structures organized by Prigogine.[121] In the case of liquid crystals, indeed, the relation between phase transitions and hydrodynamic instability was more than a mere analogy.

One place where phase transitions and hydrodynamic instabilities merged naturally was in the study of the Rayleigh-Bénard convection problem in liquid crystals.[122] A group of young experimenters recruited by de Gennes (Élisabeth Dubois-Violette, Étienne Guyon, and Pawel Pieranski) attacked the problem. The results of their work were spectacular, and in 1991, de Gennes won the Nobel Prize in part for his contribution to the physics of liquid crystals. They did not miss the practical consequences of this research. In 1973, Jacques Friedel, director of the Orsay Laboratory, already was writing: "It is possible that [liquid crystals] could be technically used for the display of data."[123] Generally speaking, however, the study of the Rayleigh-Bénard problem in liquid crystals, like Chandrasekhar's work, amounted

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[121] P. Pieranski and É. Guyon, "Cylindrical Couette Flow Instabilities in Nematic Liquid Crystals," *Advances in Chemical Physics,* 32, ed. I. Prigogine and S. A. Rice: 151-161.
[122] É. Dubois-Violette, É. Guyon, and P. Pieranski, "Cristaux liquides nématiques," *Fluid Dynamics,* ed. R. Balian and J.-L. Peube (London: Gordon and Breach, 1977): 603-619; P. Pieranski and É. Guyon, *Physical Review A,* 9 (1974): 404; P.-G. de Gennes, *The Physics of Liquid Crystals* (Oxfod: Clarendon, 1974); É. Guyon, "Instabilities in Nematic Liquid Crystals," *Fluctuations, Instabilities, and Phase Transitions,* ed. T. Riste (New York: Plenum, 1975): 295-311.
[123] J. Friedel, "Le Laboratoire de physique des solides," 24.

to introducing an additional complexity into the problem, and not a direct study of the instability itself.

The VIth Plan, and the ATP programs put in place by the CNRS, proved to be well designed resources for establishing the Orsay liquid crystals group. They allowed a new research group, which could insert itself only with difficulty into the existing structure of the CNRS, to be created and to get the necessary funding for their activity. As the chairman of subcommittee A on "Molecular Fluids," de Gennes's role was to promote the study of original molecular fluids showing order at an intermediary level, notably: polymers, lubricants, and mesomorphics and micellary phases (including liquid crystals). He was therefore in a good position to distribute money to researches on liquid crystals and establish a network of research groups working on these problems. In 1971, 1972, and 1973, this ATP funded about ten projects, for a total of about 800,000 F, per annum.[124] Moreover this was the occasion of building close links between physicists, chemists, and specialists in the study of macromolecules, which would prove useful for de Gennes's later work on polymers.[125]

(iii) Les Houches 1973: Physicists and Turbulence

On June 20, 1973, Jean Govoret, adjoint to the scientific director of the CNRS, wrote to members of the ATP Committee on "Physics of Molecular Fluids," that their

[124] CNRS, *Rapports d'activité,* 1971, 1972, 1973. Arch. CNRS, Fonds doc.

[125] J.-F. Delpech et al., "Les ATP en physique de base et mathématiques," *Courrier du CNRS,* 25 (July 1977): 34-42.

meeting would be held after a summer school devoted to fluid mechanics in Les Houches.

> In the future, it is specified that molecular hydrodynamics will be addressed in a new ATP, but within the objective "Turbulence," rather than within the objective "Materials."[126]

Indeed, in the spring of 1973, it was decided that a new ATP be set in motion, directly addressing the objective stated of the Plan concerning instabilities in fluids and plasmas. On May 23, about fifty participants gathered in Paris, coming from the following disciplines: fluid mechanics, condensed matter physics, atmospheric and astrophysical turbulence, plasma physics, and numerical analysis.

> Eight exposés, and the debates they spurred, showed the similarities and specificities of researches in these different domains. In particular, *the researches effectuated on turbulence in fluid mechanics did not appear as being located at the center of other sectors' concerns,* contrary to what might have been thought a priori.[127]

As an outcome of this meeting, an ATP called "Instabilities in Fluids and Plasmas" was put in place, which in 1975 would sponsor both the Orsay Workshop and the Dijon Symposium. Its goals were stated to be:

> —Development of diagnostic methods for small scales and corresponding development of techniques for the treatment of data.
>
> —Realization and analysis of unstable or turbulent flows in original systems (superfluid He_4, metallic, nematic [liquid crystal], dielectric, or magnetic liquids).
>
> —Numerical simulation and analysis of models.

[126] Lettre de J. Gavoret aux membres du Comité ATP "Physique des fluides moléculaires" (20/6/73), avec une note: "Conseil d'Objectifs des ATP Matériaux. Compte-rendu de la réunion du 13 mars 1973). Dossier général de l'ATP "Matériaux." Arch. CNRS G950016 DPM n°1.
[127] R. Chabbal and J. Gavoret, "Les ATP de physique au CNRS," *Courier du CNRS,* 9 (July 1973): 46-49, 47.

—Acoustic effects: interaction turbulence-sound; role of intermittency.[128]

With these goals, the Les Houches summer school, in which, in July 1973, about 35 participants gathered on the footsteps of Mont Blanc, fitted perfectly. Founded in 1954 by Cecile DeWitt, the school addressed a topic of classical physics for the first time. Its stated intent was to draw the physicists' attention back to hydrodynamic phenomena.

> The extraordinary progress of microscopic physics since the beginning of the century has somewhat overshadowed the developments of the physics of continuum and in particular fluid mechanics. Thus, in most countries, this area has evolved more or less independently, its links with the other branches of physics getting looser and looser while its technical applications widened. However, *the arbitrary and sterilizing character of such a rift . . . has become more and more obvious.*[129]

The reasons for convergence, organizers Roger Balian and Jean-Laurent Peube stated, were that physicists encountered flows in many situations (laboratory experiments in fluids, plasmas or condensed matter, geophysics, meteorology, and astrophysics). At the same time, they contended, "one of the toughest problems of fluid dynamics, *the theory of turbulence, has much progressed in the last years,* owing to the applications of methods similar to those currently used in field theory or in statistical mechanics; some *analogies with the quite recent theory of phase transitions* are stimulating the interest of theoretical physicists working in this area."

They stressed "the special character of this Summer School: their interest for fluid mechanics had led to Les Houches participants from quite varied fields, several of them being experienced specialists [in other fields]." With the publication of the

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[128] R. Chabbal and J. Gavoret, "Les ATP de physique," 47.
[129] R. Balian and J.-L. Peube, "Preface," *Fluid Dynamics,* vii. My emphasis.

courses and talks of the summer school they expressed high aims, namely to "contribute to fill the gap, unfortunately too frequent in traditional teaching, between physicists and fluid mechanicists."[130]

Once again, a striking emphasis was put on new developments of theories of turbulence. At the same time, however, Ruelle and Takens's model was almost totally neglected by the participants. This underscores the fact that things were moving in fluid mechanics, even before the Ruelle-Takens model was mobilized as a way to organize new theoretical and experimental findings in turbulence. Indeed, the only mention to be found in the published proceedings of either this model or Edward Lorenz's, lies hidden in the course on the statistical theory of turbulence by MIT applied mathematician Steven A. Orszag. Even there, Orszag did not explain what these models were.[131] Using computer simulations, however, he explicitly exhibited the "nature of 'random' solutions to (deceptively simple) differential equations."[132]

Obviously, grouping such diverse scientists, the Les Houches proceedings remained quite eclectic, and applied mathematician H. Keith Moffat, from Cambridge University, presented a picture of the fluid mechanicist's worldview which conspicuously excluded physicists (see Fig. 17).[133] Among the seminars presented at Les Houches, let us note: Manuel G. Velarde's extensive review of the Rayleigh-

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130 All quotes above are from R. Balian and J.-L. Peube, "Preface," *Fluid Dynamics,* vii-viii. My emphasis.
131 S. A. Orszag, "Lectures on the Statistical Theory of Turbulence," *Fluid Dynamics,* ed. R. Balian and J.-L. Peube (London: Gordon and Breach, 1977): 235-374, 329.
132 S. A. Orszag, "Lectures on the Statistical Theory," 249-250.
133 H. Keith Moffat, "Six Lectures on General Fluid Dynamics and Two on Hydromagnetic Dynamo Theory," *Fluid Dynamics,* ed. R. Balian and J.-L. Peube: 149-233, 154.

Bénard problem, repeatedly mentioned above; a talk by the Orsay group on liquid crystals; and a lively presentation by Benoît Mandelbrot, who introduced his notion of fractional dimension.[134] Among the other subjects raised, were experimental and numerical methods in fluid dynamics, electrohydrodynamics, critical fluctuations, superfluid helium, stellar dynamics, and dynamical meteorology (in which, following Lorenz, the question of the limits to predictibility owing to the nonlinear nature of turbulence was raised).[135]

(iv) De Gennes's Program: Let Physicists Take Over Fluid Mechanics, Part II

In the fall following the Les Houches summer school, Pierre-Gilles de Gennes, who had participated, decided to devote his 1973-1974 course at the Collège de France to "physical hydrodynamics."

> It is indeed important, at this time, to *tighten the contacts between [fluid] mechanists and physicists:* the latter can contribute to the study of flows in different ways: a) by experimental methods, notably for the measure of

[134] B. Mandelbrot, "Physical Objects with Fractional Dimensions: Seacoasts, Galaxy Clusters, Turbulence and Soap," *Fluid Dynamics,* ed. R. Balian and J.-L. Peube: 555-578. At the same as he was giving his lectures at Les Houches in 1973 and at Orsay in 1975, Mandelbrot had been invited to speak at the Collège de France: "Nouvelles formes du hasard dans les sciences" on 13/1/73, mentioned in *Annuaire du Collège de France,* 73 (1973): 639; "1. Objets physiques de dimension fractionnaire: Côtes – Galaxie – Turbulence – Savon;" and "2. Géométrie de la turbulence et intensité des bouffées" on 22 and 29/1/74, mentioned in *Annuaire du Collège de France,* 74 (1974): 687. a series of lectures that would form the backbone of his famous book *Les Objets fractals,* published in 1975. See his recollections in *Mathematical People: Profiles and Interviews,* ed. D. J. Albers and G. L. Alexanderson (Boston: Birkhäuser, 1985): 207-225, 222.

[135] O. Talagrand, in collaboration with D. Anderson and M. Ghil, "Éléments de météorologie dynamique," *Fluid Dynamics,* ed. R. Balian and J.-L. Peube: 641-656, 655.

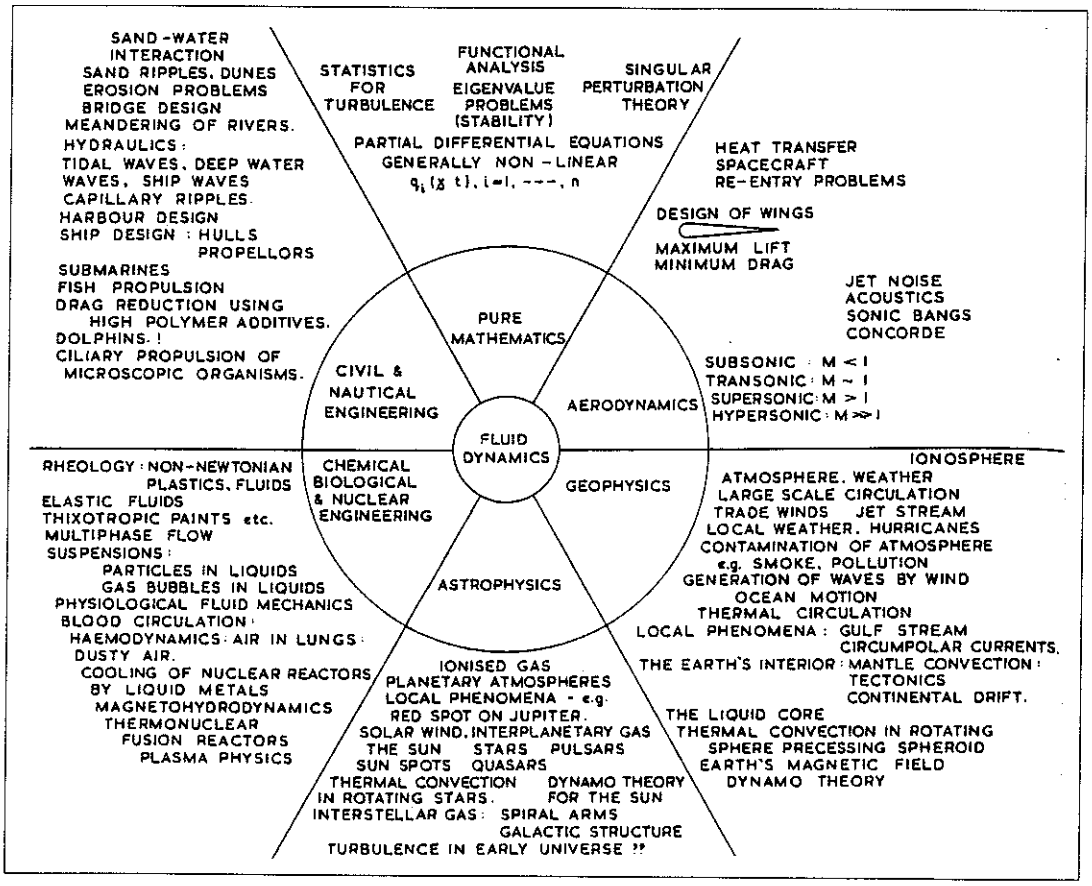

Figure 17: The Fluid Dynamicist's View of the World. Repr. with permission from H. K. Moffat, "Six Lectures on General Fluid Dynamics and Two on Hydromagnetic Dynamo Theory," Fluid Dynamics, ed. R. Balian and J.-L. Peube: 149-233, 154. Copyright © Gordon and Breach Publishing Co.

> velocities and correlation, . . . c) by bringing in certain theoretical concepts stemming from different domains.[136]

De Gennes's actions at the Collège de France, from 1971 to 1975, make explicit the way in which, in order to promote the idea that physicists and hydrodynamicists might

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[136] *Annuaire du Collège de France,* 74 (1974): 77-79. My emphasis. Point (b) was dealing with the physicists' theories and experiments on liquid crystals and superfluidity. M. Dubois said that around 1975, the problem of the transition to turbulence "changed hands" from the hydrodynamists to physicists. "Modèles du chaos déterministe," *Modèles, modélisations, simulations (Histoire, Épistémologie, enjeux actuels),* symposium organized by Amy Dahan-Dalmedico (Paris, 22/4/97).

have something to share with one another, the Ruelle-Takens model could be mobilized. In 1970, de Gennes was nominated to the Collège de France in replacement of Jean Laval, whose chair of theoretical physics was renamed "Condensed matter physics." His first course was titled "Broken Symmetries and Phase Transitions." Among the phenomena studied was the instability of convective flows, and in particular the Rayleigh-Bénard phenomenon. This however was a rather marginal aspect of his outline. In the same year, de Gennes's invited Pierre Bergé, an experimenter he knew from the CEA to speak about "Fluctuations in Liquids."[137] While topics remained far from what chaos theory, links that would become important for its future development were thereby being forged.

Around those years, after Kenneth Wilson's theory became fashionable, de Gennes directed his attention to critical phenomena and phase transitions especially in relation with liquid crystals. In his course at the Collège de France, de Gennes did not come back to hydrodynamic phenomena *per se* until a couple of years later. But when he did, he had formed an ambitious program.

De Gennes conceived his 1973-1974 course on "physical hydrodynamics" as the continuation of the summer school at Les Houches, where, he wrote, "an excellent session in hydrodynamics [had been] directed towards physicists." True to his focus on liquid crystals, however, de Gennes raised one particular theme that was quite far from the approach suggested three years earlier by Ruelle and Takens. Special attention was paid to "making the link between molecular description and global mechanical properties." Turbulence was not the goal of his inroad into fluid

[137] On November 11, 1971. *Annuaire du Collège de France,* 72 (1972): 65-73.

mechanics. "Discussion was limited to viscous flows at small speed (. . . no turbulence)." But again, Bergé's work assumed an important place assumed in the design of de Gennes's course. "As an example, the locomotion of microorganisms (bacteria, spermatozoids, etc.) was discussed," an experiment performed by Bergé and Dubois at the CEA.[138]

The following year, the phenomenon of turbulence more than ever became an integral part of de Gennes's program.[139] He wished to make an inventory of recent optical methods for the study of flows and of the methods for the generation and detection of turbulence. He noted that the work of Paul C. martin had indicated "interesting analogies between developed turbulence and phase transitions." In January 1975 de Gennes addressed the problem of the onset of turbulence at the Collège de France. Thanks to Paul Manneville who kindly provided me a copy of the personal notes he then took, we can have a glimpse at the way de Gennes tackled the problem.[140] In particular, we can see how the Ruelle-Takens model got integrated to de Gennes's view of the onset of turbulence—"an interesting pb [problem]," wrote Manneville, who was a member of the Orsay group and a later contributor to the chaos theory of turbulence. His lecture notes of de Gennes's course are a fascinating

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[138] *Annuaire du Collège de France,* 74 (1974): 77. See Service de physique du solide et de résonance magnétique, CEA, Saclay, "Une nouvelle technique pour mesurer les mouvements au sein des fluides," *1975 Images de la physique,* suppl. to *Courrier du CNRS,* 16 (1975): 72-76. Arch. CNRS, Fonds doc. Among the seminars of de Gennes, one may note P. Bergé, "Nouvelle méthode de vélocimétrie optique" (25/1/74); É. Guyon and P. Pieranski, "Comparaisons de diverses instabilités convectives" (1/2/74); M. Dubois and P. Bergé, "Mesure des vitesses de convections naturelle dans l'instabilité de Rayleigh-Bénard" (7/6/74).
[139] *Annuaire du Collège de France,* 75 (1975): 81-82. His emphasis.

example of how the Ruelle-Takens model would become a useful resource for a physicist's approach to the problem of turbulence.

Like Martin and Ruelle, in the same year, de Gennes started with the situation where turbulence appeared as the result of a "cascade of instabilities," using the paradigmatic case of the resistance a sphere opposed to the flow of water. He introduced Landau's model, noting the analogy with phase transitions. He emphasized the possibility of computing critical exponents and, using Bergé and Dubois's experiment (see below), mentioned more or less satisfactory experimental confirmations for the Rayleigh-Bénard flow.

He then criticized Landau's scheme, pointing out that many variables of the system (amplitude and phase of the disturbances) could not be specified unless "one knows everything" about the system. "In fact, the image of reality is more complicated." Before he extensively tackled the Rayleigh-Bénard problem, de Gennes spent some time explaining the analogy between hydrodynamic instabilities and the phase transition theory of Landau and Ginzburg. Turning to Rayleigh-Bénard *per se,* he commented on Busse's theory and used the experiments of Krishnamurty, Willis and Dearsdorff, and Ahlers, pointing out that these experiments had the inconvenience that the flow could not be visualized, making difficult to tackle problems with Rayleigh-Bénard systems related to their critical exponent and the spatial structure of rolls. De Gennes also pointed out that liquid crystal systems might be advantageously used to study the Rayleigh-Bénard instability.

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[140] I thank Paul Manneville to have provided me a copy of his personal lecture notes taken for de Gennes's course in 1974-1975.

But it seems that de Gennes's lecture was designed with the goal of discussing the Ruelle-Takens model so that one could see how it might be called upon for the interpretations of recent experimental results. De Gennes's course is one the first accounts of the Ruelle-Takens model that took seriously the dynamical systems modeling practice of its authors. Using their analysis, de Gennes concluded, with them, that "the multiperiodic case is exceptional; the situation envisioned by Landau is not realized." De Gennes moreover emphasized that this entailed a new "operational definition of turbulence," which had two sides. Theoretically, for Ruelle, when the attractor was a fixed point or an orbit, the flow was said to be nonturbulent; if, on the contrary, it was attracted to a strange attractor, then it was turbulent. Most importantly, de Gennes drew the experimental consequences of this "operational definition."

> - experimentally, if one measures a discreet spectrum: 1 freq. + harmonics [then the flow is] nonturbulent;
>
> - [if one measures a] continuous spectrum [then it is] turbulent.

"How to differentiate experimentally the continuous spectra from a multiperiodic spectrum[?]" de Gennes asked. By the "clear dominance of a few peaks." With this, de Gennes had outlined an experimental program that could unambiguously differentiate the model proposed by Ruelle and Takens from Landau's. As opposed to the IHÉS scientists, in order to achieve this, he did not rely on mathematical arguments, which he had clearly presented, but on experiments. Only experiments could decide whether the model was valid or not! In view of the close association of Bergé and Dubois with de Gennes, it is not surprising that they would soon endeavor to find these continuous spectra in their Rayleigh-Bénard experimental setups.

De Gennes concluded his two-year series of lectures on hydrodynamics in the following terms:

> At the end of two years devoted to hydrodynamics, we can establish this temporary assessment: on some points like rheology of polymers or the flow of liquid crystals, useful progress has been accomplished. The attention of physicists has been drawn to a certain number of open hydrodynamic problems. But exchanges with fluid mechanists have still remained too fragmentary; a long effort of cooperation and research still needs to be made.[141]

Indeed, the ambitious ATP of the CNRS devoted to the interdisciplinary attack on phenomena of "Instabilities and Turbulence in Fluids and Plasmas" had meanwhile turned sour.

(v) Disputes and Disappointment: Interdisciplinarity is not an Easy Task

Set in motion in May 1973, the ATP "Instabilities and Turbulence" was examined by the various concerned sections of the National Committee at the end of 1974. Constituted of elected representatives from the various scientific sections of the CNRS, the National Committee was intended as the place where intellectual decisions were taken by the CNRS researchers themselves.

Not surprisingly, the ATP on "Instabilities" was favorably discussed by de Gennes, one of its instigators, in Section 08 (solid-state physics).[142] Similarly, the discussion that took place in Section 04 (Mechanics) in December 1974 was quite

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[141] *Annuaire du Collège de France,* 75 (1975): 82.
[142] Procès-verbal du Comité national du CNRS (Section 08: Physique des Solides) (1/10/74). Arch. CNRS G870168 SGCN n°6.

positive. They even noted that the amounts allotted to this ATP were below the envelop devoted to it by the CNRS. However, a concern was expressed to the effect that a new program was to be written because the previous one was "too vast." The committee emphasized that "the accent should be put on studies tending to bring new and fundamental elements to the understanding of mechanisms."[143]

Representatives of Section 02 (theoretical physics) were much more critical of the project. Examining the ATP President Meyer "notices the considerable amounts allotted to *a subject of limited interest,* and the small control [they themselves] had on these funds." It was stressed that the problem of turbulence had hardly evolved for the last 25 years. Worries were expressed concerning the line of demarcation between physicists and fluid mechanists. To these objections, Robert Chabbal, in charge of the ATP program, declared: "If this ATP in no way contributes to favor theoretical research [on this topic], then this is an argument to stop it." At the same time, however, the section of the National Committee deplored what they called "the inertia of researchers of their discipline vis-à-vis ATPs."[144]

In 1974, the ATP on "Instabilities" had sponsored 13 projects for a total amount of 1,460,000 F.[145] True to the ATP's intent of sponsoring the acquisition of

[143] Procès-verbal du Comité national du CNRS (Section 04: Mécanique) (2/12/74). Arch. CNRS G870168 SGCN n°6.
[144] Procès-verbal du Comité national du CNRS (Section 02: Physique théorique) (15, 16, et 17/10/74). My emphasis. Arch. CNRS G870168 SGCN n°6.
[145] Documents prépatoire du Directoire des 25 et 26 juin 1974, Tableau 7. Arch. CNRS, G870168 SGCN n°1. Note however that the *Rapport d'activité* 1973, p. 99, mentions 19 contracts for a total amount of 1,497,500 F. Fonds doc.

equipment, projects were mostly experimental.[146] A clear diversity of projects was funded by the ATP. In 1975, the ATP again serve to sponsor 9 projects, for a total of 1,203,000 F.[147] Among a similar diversity of projects, ranging from meteorology to plasma physics, from superfluidity to shock dynamics, a contract was given to Guyon for a study of the "hydrodynamic instability in liquid crystals." Clearly, the committee for this ATP seemed to have forgotten that it was supposed to have a "determinate scientific finality."

When at the end of 1975 the different sections of the National Committee met, the representatives for the Mechanics Section seemed to be the only ones who were still happy with the ATP on instabilities. In other sections, a feeling of disappointment was clearly expressed. In October, at the meeting of Section 08,

> M. de Gennes declares that he is not very satisfied with this ATP because it gathers: Classical Mechanicians, Physicists, Plasma Physicists, plus a few Astrophysicists. . . . There are too many different specialties and the pre-colloquium gave rise to violent disputes. In the future, heavier structures [*sic*] should not be renewed.[148]

Similarly, in Section 03 (electronics), the ATP was deemed a "disappointment." It was an "enterprise lacking in maturity. . . . However, it was worth

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146 They came from laboratories specializing in fluid dynamics at Nice (Frisch), Orsay (Lelièvre), Grenoble (Craya), and Lyons (Comte-Bellot, Courseau), but also from plasma physics (D. Grésillon at Paris, Mantei at Orsay) and astrophysics (Roddier at Nice) laboratories. One may even note a contract with Quemada on biological applications, and one with Valentin (Rouen), titled "Applications of laser-Doppler anemometry to the measurement of velocities in plasma." A good source for the content of these project is to be found in the proceedings of the Dijon Symposium, cited above. *Journal de physique,* 37, *Suppl.,* Colloque C1 (1976).
147 Documents prépatoire du Directoire des 30 juin, 1er et 2 juillet 1975, Tableau n°123. Arch. CNRS, G870168 SGCN n°1. Note however that the *Rapport d'activité* 1974, p. 66, mentions 10 contracts for a total amount of 1,353,000 F. Fonds doc.

trying to do something in this domain." He moreover reported that there were "internal quarrels" within the committee which led it to accept projects that were criticized.[149] Finally, in Section 02, it was reported that the coordination of the projects had remained difficult. "M. Winter regrets that he was too ambitious by associating researchers that were too different." The fact that it would be stopped could only bring a bettering of the situation. Meyer sugested that in the future plasma/astrophysics and fluid mechanics/turbulence be distinguished.[150]

In these conditions, the ATP "Instabilities and Turbulence in Fluids and Plasmas" was abandoned in 1976. The disagreements expressed in the ATP Committee may be taken as a hint for the reasons why the Orsay Workshop and the Dijon Symposium of the summer of 1975, albeit similar in intent, nonetheless looked so different. Similarly, although a Seminar of Physical Hydrodynamics nonetheless survived in de Gennes's goup in 1976, he himself more or less dropped this topic from his future courses at the Collège de France..[151]

No matter how disappointing the ATP on "Instabilities and Turbulence" proved to be in action, it had succeeded in attracting the attention of a few physicists to problems of hydrodynamic instabilities. It had favored a recognition that the

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148 Procès-verbal du Comité national du CNRS (Section 08: Physique des Solides) (6, 7, 8 et 9/10/75). Arch. CNRS G870168 SGCN n°6.
149 Procès-verbal du Comité national du CNRS (Section 03: Électronique) (25, 26, 27 et 28/11/75). Arch. CNRS G870168 SGCN n°6.
150 Procès-verbal du Comité national du CNRS (Section 02: Physique théorique) (9, 10 et 11/12/75). Arch. CNRS G870168 SGCN n°6.
151 Among the talks presented, we may note: Pierre Bergé, "Instabilité de convection" (26/5/76), Yves Pomeau, "Les attracteurs étranges. Intermittence" (31/5/76), and one by Étienne Guyon on the same day, and finally, Monique Dubois, "Instrumentation nouvelle" (7/4/76). *Annuaire du Collège de France,* 76 (1976).

Ruelle-Takens model might be a useful resource for experimental physicists who had invested time and energy in the problem in order to account for their results by theoretical means. The ATP had moreover brought the Lorenz model to the fore. It had allowed a clear experimental program to test these models to be undertaken. The ATP and de Gennes's course at the Collège did not solve the turbulence problem but they put scientists coming from a variety of backgrounds in contact with one another. The focus on instabilities and turbulence in fluid has moreover strengthened the contacts of French physicists with other people who were concerned with hydrodynamic instabilities and the Rayleigh-Bénard problem in particular, as is witnessed by à conference held in Norway in 1975.

c) Geilo 1975: The Emergence of an International Community?

Throughout the 1970s, a series of six NATO Advanced Study Institutes on phase transitions and instabilities were held in Geilo, Norway. Mostly they were organized by Tormod Riste, from the Institutt for Atomenergi in Kjeller. When reflecting back on the accomplishment of this series of conference, Riste and his co-organizers wrote in 1981:

> Ten years ago, at the first Geilo school, the report of a central peak in the fluctuation spectrum of $SrTiO_3$ close to its 106 K structural phase demonstrated that the simple . . . theory of such transitions was incomplete. The missing ingredient was the essential nonlinearity of the system.[152]

From April 11 to 20, 1975, 70 physicists, most from Europe, gathered for the third Geilo Institute, which was devoted to "Fluctuations, Instabilities, and Phase

Transitions."[153] Experimenters who had recently adapted light-scattering methods from the study of phase transitions to that of hydrodynamic instabilities, in France and the US, as well as Günter Ahlers joined scientists from the Orsay liquid crystal group, and from Prigogine's School in order once again to study the "analogy" between phase transitions and instabilities in nonequilibrium systems. One of the main examples of course was "instabilities in hydrodynamic systems, for which one may draw on the very rich experimental material obtained by hydrodynamicists." Here again, the intent of drawing the physicists' attention to such phenomena was clearly stated:

> It is the hope of the program committee that this institute may have created an interest among the participants to make a wider use of the powerful tools of modern physics in such studies. . . . The full span of the program is introduced by de Gennes' lecture on phase transition and turbulence.[154]

Indeed, de Gennes explained in detail the analogy between phase transitions and, now, turbulence, and not only hydrodynamic instabilities. In a two-part talk, he introduced "the general ideas and the theoretical methods which have been most fruitful for phase transitions," as well as "salient facts about turbulence." Always with the same goal, he added: "I hope that [these views] can help to bridge the gap between the two schools of thought, [namely: "the fraction of this audience who works primarily on fluid mechanics" and the "physicists"]."[155] Like Gérard Toulouse, de

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[152] H. Z. Cummins, E. H. Hauge, J. G. Feder, R. Pynn, T. Riste, and H. Thomas, "Preface," *Nonlinear Phenomena at Phase Transitions and Instability,* ed. T. Riste (New York: Plenum, 1982), vi.
[153] T. Riste, ed., *Fluctuations, Instabilities, and Phase Transitions: NATO Advanced Study Institute, Geilo, Norway, April 1975* (New York: Plenum, 1975).
[154] T. Riste, "Preface," *Fluctuations, Instabilities, and Phase Transitions,* ed. T. Riste, v.
[155] P.-G. de Gennes, "Phase Transitions and Turbulence," *Fluctuations, Instabilities, and Phase Transitions,* ed. T. Riste: 1-18, 1.

Gennes emphasized universality as a way to bring forth the analogy: "the details of the atomic (or molecular) structure become unimportant: the behavior near [the critical temperature] is to a certain extent *universal.*"[156]

In this lecture, de Gennes emphasized even more than at the Collège the importance of the Ruelle-Takens model. "A precise definition of turbulence is not easy to find," he acknowledged, "but the following features seem to be essential:" (a) rapid and not uniform flows; and (b) "*stochastic character,* which is not due to external noise sources, but which is an intrinsic consequence of the non linearity in the hydrodynamic equations." As we have seen, this was an essential conclusion at which both Prigogine and Ruelle had arrived. De Gennes went on, citing Lorenz's model as an example:

> The stochastic character already appears in nonlinear systems with a *small* number of degrees of freedom, where numerical studies are relatively easy to perform, and can in fact be found in the literature of very different fields—celestial mechanics, accelerator physics, electronics, population dynamics, etc. . . . Whenever the trajectories are truly non periodic as they seem to be [in the Lorenz system] *we shall call the flow turbulent*—following a definition expressed (more rigorously) by Ruelle and Takens.[157]

But de Gennes's goals remained those of a physicist studying phase transitions, and not those of a mathematician attempting a classification of dynamical systems: "is there a scaling law of the form $\cong(\Delta T - \Delta T^{*})^{-x}$ where x is some fixed exponent inside one universality class?"[158]

As emphasized above by Martin in particular, the most important consequence of the analogy between phase transitions and turbulence was *not* theoretical, but rather

156 P.-G. de Gennes, "Phase Transitions and Turbulence," 3.
157 P.-G. de Gennes, "Phase Transitions and Turbulence," 9-10. My emphasis.

experimental. The Geilo Institute provided an occasion for Jerry Gollub, Günter Ahlers, Pierre Bergé, and Monique Dubois to meet and compare their experimental results. While all computed critical exponents, they realized that by using light-scattering methods they could achieve local measurements of great accuracy. This enabled them to go beyond the "linear domain," corresponding to Rayleigh numbers slightly above the critical value and which seemed to be well understood theoretically, and to attack the "nonlinear domain" where classic theories were much vaguer.[159] This was the domain where the Ruelle-Takens provided clues to the theoreticians about how to tackle it and to the experimenters about what to look for.

5. EPILOGUE: BEYOND RUELLE-TAKENS

On February 6, 1975, Ruelle went to address physicists of the CEA, at Saclay, a research center south of Paris which was hardly more than ten minutes away from the IHÉS by car. He spoke on "The Problem of Turbulence."[160] Having met Swinney and Gollub in 1974, Ruelle was now talking to their closest colleagues in France: the experimenters Pierre Bergé and Monique Dubois who after having studied fluctuations at critical points had started to tackle the study of convection.[161] By 1975, therefore, it seems that a chaos constellation had already emerged. It had been built

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[158] P.-G. de Gennes, "Phase Transitions and Turbulence," 10-11.
[159] G. Ahlers, "The Rayleigh-Bénard Instability at Helium Temperature;" J. P. Gollub and M. H. Freilich, "Critical Exponents and Generalized Potential for the Taylor Instability;" and P. Bergé, "Rayleigh-Bénard Instability: experimental Findings Obtained by Light Scattering and Other Optical Methods," *Fluctuations, Instabilities, and Phase Transitions,* ed. T. Riste: 181-193; 195-203; and 323-352.
[160] *Rapport scientifique 1975. Voyages et publications,* 5. Arch. IHÉS.
[161] Ruelle's visit to Swinney and Gollub is briefly mentioned in J. Gleick, *Chaos,* 131, and 150.

upon foundations that had to do with common concerns for instabilities in fluid, for which the models of Ruelle-Takens and Lorenz, but also analogies and tools coming from phase transitions, were useful resources for the development of a common language. However, this constellation was still far from having synthesized these loose analogies into a rigorous, or at least respectable, theory, to use Domb's categories. The theoretical analogies with phase transitions had proved disappointing, but in 1975 a "dynamical systems approach" had hardly been uniformly adopted by all members of the chaos constellation.

From 1975 to the early 1980s, a few French physicists, in contact with other scientists in the United States and Europe, would build a more complete deterministic theory of the onset of turbulence, and provide firm experimental bases for it. Being general, this theory would not have the achieved character of many traditional physical theories, but being general, it would be successfully transferred to many other phenomena, starting with oscillating chemical reactions which had been a focus of Prigogine's School. By the early 1980s, chaos theory had been established as a major field of research in physics. The common theoretical language and the modeling practices that were found to be best suited for this endeavor would be greatly inspired by those of dynamical systems theorists, such those at the IHÉS.

The process by which these practices and language came to occupy the foreground certainly deserves more space that can be devoted to it here. In the following, this story will be concluded by providing snapshots of a few career trajectories. A few French physicists would play an important role in making the switch from phase transition analogies to dynamical systems. In particular, Monique Dubois and Pierre Bergé, Yves Pomeau, and Albert Libchaber all used the Rayleigh-Bénard system as one of their main objects of study. In a large part, it finally was as a consequence of their work that a language and modeling practice derived from dynamical systems theory were adapted to the study of a wide array of physical systems.

This "epilogue" first rules out the view that Ruelle and the IHÉS played a first-rank role in that process, mainly because their programs remained little affected by experiments. Then it will provide a few snapshots of French physicists' careers in the later part of the decade in order to show how, even if they adopted many of the tools from dynamical systems theory, physicists however considerably adapted the modeling practices that were examined in previous Chapters.

a) Ruelle and the IHÉS, 1970-1977

The following pays attention to the evolution of Ruelle's involvement in the emerging field of chaos. This will underscore the fact that the physicists' reception of his model had little to do with his own concerns. By bringing the same kind of attention to the IHÉS, it will moreover be shown that, if the programs pushed forward by the Institute in the 1970s definitely moved towards the study of turbulence and dynamical systems, they remained within a mathematical tradition close to earlier concerns of the IHÉS but which had little to do with the new emphasis put on experimental and numerical results that so much shaped the outcome of chaos theory.

Significantly, throughout the early 1970s, Ruelle mainly chose to address physicists, rather than mathematicians. But of course, he tried to convince those he felt the closest to: that is, axiomatic quantum field theorists and mathematical physicists who tackled the rigorous foundations of the statistical mechanics of equilibrium. These were mathematical physicists who remained much more aloof from experimental work than those, mentioned above, studying phase transitions and critical phenomena.

At first, Ruelle's talks on turbulence and dissipative systems were mainly given abroad. The first talk he gave in France outside of the IHÉS, on "Differential dynamical systems and the problem of turbulence" was on December 19, 1973—three and a half years after having written his paper with Takens! It was directed to the physicists of the École polytechnique.[162] Prior to this, Ruelle had given many lectures dealing with turbulence, but mainly outside of France where his professional connections were the strongest. When speaking in France, he above all addressed issues of statistical mechanics.[163]

(i) Ruelle's Picks up Lorenz

In 1975, a few months after having been to the CEA and right after a stay at Stanford and the IAS at Princeton, Ruelle started to consider the Lorenz attractor. By then, turbulence and dynamical systems definitely were high on his agenda. At La Jolla,

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162 *Rapport scientifique. Voyages et conférences du directeur et des professeurs permanents de l'IHÉS en 1973,* 3. Arch. IHÉS. There might be a bias in my information, collected from the Scientific Report of the IHÉS, which emphasized "trips" made by its permanent faculty members, rather then "talks." The perception I present above nevertheless is consistent with other sources.

California, on July 17, 1971, David Ruelle and Edward Lorenz had participated to the same session of a conference devoted to "statistical models and turbulence." Apparently, although both of their papers published in the proceedings raised the issue of sensitivity to initial conditions, they did not found much to exchange with one another. In the proceedings of the 1975 Orsay Workshop, however, an article of Ruelle's appeared under the title: "The Lorenz Attractor and the Turbulence Problem." This was the text of a talk he had delivered at a symposium on "Quantum Dynamics Models and Mathematics," at Bielefeld held in September 1975.[164]

This talk really marks the full-fledged return of Ruelle to the general study of turbulence and dissipative systems. He gave a straightforward physical interpretation of the model he had proposed with Takens a few years earlier: "putting a nonlinear coupling between 4 or more oscillators can produce a 'turbulent' time evolution with sensitive dependence on initial conditions."[165] This very property—sensitive dependence on initial conditions—was emphasized by Ruelle as a general explanation for "erratic, chaotic, or turbulent behaviors," a phrase he used many times in his lecture.

Following private communications by Lanford and Bowen about Lanford's and Guckenheimer's studies of the Lorenz system, Ruelle now decided to address this model, albeit reinterpreting it as a model for the onset of turbulence, which it had not been explicitly at the time it was proposed by Lorenz.

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163 *Rapport scientifique 1974.* Arch. IHÉS.
164 D. Ruelle, "The Lorenz Attractor and the Problem of Turbulence," *Turbulence and the Navier-Stokes Equations,* ed. R. Tenam (Berlin: Springer, 1976): 146-158.
165 D. Ruelle, "The Lorenz Attractor," 147.

> Lorenz' work . . . is the first attempt at interpreting turbulence by solutions of differential equations which appear chaotic, and have sensitive dependence on initial conditions. The ideas of Lorenz and those of Takens and myself have recently received support from the theoretical work of McLaughlin and Martin and the experimental work of Gollub and Swinney. It can be hoped that more experimental results on the onset of turbulence in various systems will become available in the next few years; *their theoretical interpretation will constitute a worthy challenge for the mathematical physicist.*[166]

Mentioning Li and Yorke's preprint as being relevant to this study, Ruelle had put in place similar connections to those Martin had mobilized. Ruelle however remained much more skeptical of the traditional ways hydrodynamicists tackled the problem of the onset of turbulence. "I think it would be a miracle if the usual procedure of imposing stationarity, [and] truncating the resulting system of equations, would lead to results much related to physics."[167] His conclusion again was a condemnation of traditional views of the onset of turbulence:

> let me express my feeling that, after decades of misconceptions, we are beginning to have correct ideas on the time-dependence in turbulence near its onset.

Referring to a figure of Feynman's showing spatial features of turbulence, also used by Martin and de Gennes, Ruelle emphasized that there much work remained to be done.[168]

Although he had proposed a new model for turbulence, which was gaining ground, and although his concept of a 'strange attractor' was starting to be widely used, Ruelle appears, in 1975, just as much a follower of the interdisciplinary bandwagon of turbulence as any. Clearly, he had expressed views for the future

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[166] D. Ruelle, "The Lorenz Attractor," 147. My emphasis.
[167] D. Ruelle, "The Lorenz Attractor," 154.
[168] D. Ruelle, "The Lorenz Attractor," 154-155.

development of the field that seem to have been later realized, partly because of his own work. But Ruelle did not have, at the time, a synthetic view comparable to Martin's; and he still did not consider the theoretical work done on turbulence by non-dynamical-systems specialists as being well oriented. He was not on the same wavelength as most physicists who tackled the turbulence problem. That it was so is made even more manifest by the directions the IHÉS chose to pursue in the course of the 1970s.

(ii) IHÉS: 'Foreign in View of Some Frenchmen'

By hiring David Sullivan in 1974, the IHÉS had oriented his domains of research still more in the direction of dynamical systems theory. In fact, Sullivan was the first specialist in this field to be appointed to the IHÉS. In his own words, he focused on the "study of geometrical properties of spaces—in particular manifolds—in view of understanding their geometrical and topological forms (flows, foliations, measures and general dynamical phenomena)."[169]

Throughout the early 1970s, the Scientific Committee of the IHÉS had wished to attract a new permanent faculty member. In June 1971, before he officially became director, Nicolaas Kuiper wrote down general remarks about the choice of new permanent members in mathematics.

> They should be extremely creative, original, and independent, as proved by their work and in particular, their theorems. . . . Their interest should be wider than just one restricted field of mathematics. . . . They should be powerful

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169 Annexe to Lettre de N. Kuiper au Secrétaire d'État aux Universités (26/2/75): "Orientations scientifiques pour la période 1976-1980 (VIIème Plan-Recherche)." Arch. IHÉS.

> mathematicians and stimulating to others. . . . To some extent the interests of the permanent members should cover different parts of mathematics.

Looking at the present composition of the faculty of the IHÉS, Kuiper wrote:

> I wonder whether further study should be made into the possibility of finding a permanent member in [analysis]: P. Lax, Möser, Malgrange, etc. I suppose Smale is not available.[170]

Indeed Smale had been considered. But Atiyah's judgment solicited by Zeeman did not make this prospect seem likely:

> It is possibly unrealistic to consider Smale because he would not want to emigrate from America, for financial and other reasons. In spirit he is very close to Thom with a similar style and some overlap of interest. He is probably the most original mathematician on the list, with tremendous drive, and the ability of rushing where angels fear to tread.[171]

In 1971, however, the Scientific Committee decided, under Léon Motchane's lead, to offer positions to Bombieri, Langlands, and Armand Borel, who all turned them down. The next year, while it seemed clear that they had to start considering other candidates, David Sullivan's name was for the first time proposed by Deligne.[172] An offer was made to him in November 1973.

At the same time, the Scientific Committee resolved to build a program for 1974-1975 centered around "attractors and structure." The theme would be "Dynamical systems, turbulence, statistical mechanics" and "attractors" would be represented by Bowen, Sattinger, and Mather.[173] This marked an important change of

[170] N. Kuiper, *General Remarks on the Choice of New Permanent Members (Mathematics).* "To be considered as informative, not my final judgement." *Comité scientifique* (15/6/71). Arch. IHÉS.
[171] Report by E. C. Zeeman of a conversation with M. Atiyah. *Comité scientifique* (15/6/71). Arch. IHÉS.
[172] Compte-rendu du Comité scientifique (14/4/72). Arch. IHÉS.
[173] Comité scientifique (16/11/73), Compte-rendu (dated 26/11/73). Arch. IHÉS.

the attitude, since until then, as mentioned in Chapter VII above, little had been coherently organized to promote these subjects at the IHÉS. This exactly coincided with Ruelle's definite change in orientation. Outlining in February 1975 the scientific orientations of the IHÉS for 1976-1980, Kuiper selected the qualitative theory of differential equations as the main point needing emphasis. With three of its permanent professors (Thom, Ruelle, and Sullivan) listing this topic among the research themes they planned to pursue in the following years, this emphasis, Kuiper wrote, "can only improve the already very satisfying spirit of cooperation which animates our researches and strengthen the cohesion of fundamental researches in mathematics and theoretical physics."[174] The way Ruelle then described his research agenda is enlightening:

> The study of dissipative systems (for example turbulence); statistical mechanics of equilibrium (in particular quantum). Between these two kinds of questions, there exist unexpected mathematical relations.

Around 1974, that, in liaison with Orsay and the CNRS, the IHÉS acquired its first calculating machine with a curve plotter.[175] It was met with "great success," but, strikingly, only Pierre Cartier, a long-term visitor paid by the CNRS who studied group theory, mentioned numerical calculations among his research concerns.[176] For

[174] Lettre de N. Kuiper au Secrétaire d'État aux Universités (26/2/75): "Orientations scientifiques pour la période 1976-1980 (VIIème Plan-Recherche)." With an annex listing the permanent professors' future research themes. Arch. IHÉS.
[175] Comité scientifique (19/4/74). Arch. IHÉS.
[176] Annexe to Lettre de N. Kuiper au Secrétaire d'État aux Universités (26/2/75), 2. Arch. IHÉS.

specialists in the theory of dynamical systems, numerical studies would of course have represented a drastic change of modeling and theoretical practices.[177]

In June 1976, Kuiper presented the Volkswagen Foundation in Hanover, with a research program to be sponsored by them for the following three years.[178] Kuiper's own program illustrates the fact that the problems of dynamical systems theory that the members of the IHÉS then thought of as being the most promising remained in direct line with their earlier work.

> During this period [1 January 1977 to 1 January 1980], we shall concentrate our interest on *Dynamical systems, their global structures and singularities.* Many new problems, concepts, and methods will have to be elaborated before it finds itself in the subsequent state of a science or theory. The state of synthesis of parallel and isolated problems with a very developed mathematical apparatus and technique is in this domain not yet achieved.[179]

The program concocted by Kuiper, which I have reproduced in the Complement to Chapter VIII below, focused on five areas: (a) Turbulence and differentiable dynamical systems, (b) Differentiable dynamical systems,

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[177] An important exception to the above statement was Oscar E. Lanford who did use the IHÉS computer to study the Lorenz attractor.
[178] Early in 1976, this Foundation had just started to sponsor the research program on the closely related theme of "Synergetics" of Hermann Haken, from the Institut für Theoretische Physik at the University of Stuttgart. Lettre de H. Plate à Nicolaas Kuiper (21/3/79). Arch. IHÉS. As a sample of Haken's work in the mid-1970s, see H. Haken, "Analogy Between Higher Instabilities in Fluids and Lasers," *Physics Letters,* 53A (1975): 77-78; and H. Haken, *Synergetics – An Introduction: Noneequilibrium Phase Transition and Self-Organization in Physics, Chemistry, and Biology* (Berlin: Springer, 1977).
[179] Lettre de N. Kuiper à Frau Dr. Zarnitz (4/6/76). My translation from German. Also Gastforschungsprogramm 1977-79: Dynamische Systeme, ihre globale Strukturen und Singulritäten. Antrag für eine Verlängerung der Bewilligung in 1980-81 (1/6/79). Pressemitteilung (Projektdarstellung). Mathematik Stipendum-Programm an das Institut des Hautes Etudes Scienfiques "Dynamiche Systeme, ihre globalen Strukturen und Singularitäten" (1977-81) (30/1/80). Sclussbericht des dreijährigen

(c) Foliations, (d) Relation between qualitative dynamics and algebraic singularities, Catastrophes, and (e) Singularities of polynomial mappings. This obviously was a vast program, but it succeeded in grouping together more than half of the permanent members and long-term visitors of the IHÉS. A striking feature of Kuiper's program was that, although mentioning applications, it stayed mainly focused on the mathematical theory of dynamical systems, as well as on the implications it might have on other fields of mathematics. Nowhere were experimental researches on chaotic behavior in turbulent systems even mentioned. This program was meant as a continuation of the theoretical research and modeling practices developed at the IHÉS for many years.

Therefore, while in the 1970s, research on dynamical systems definitely became one of the main activities at the IHÉS, the fascinating convergence of fields that became a trademark of chaos at this very moment remained a marginal aspect of the research conducted and presented at the IHÉS. Significantly, one may note that, in the 1970s, the word 'chaos' rarely surfaced in the official records of the IHÉS. Dynamical systems, catastrophe theory, and singularities remained the preferred labels for the fields in question. Many important later contributions to the mathematical theory of dynamical systems would therefore be worked out at the IHÉS, often sponsored by the Volkswagen Foundation, but this Institute contributed little to the formation of what I have called the chaos constellation, even in France. For the IHÉS, dynamical systems theory by and large remained a branch of

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Gastforschungsprogramm "Dynamiche Systeme, ihre globalen Strukturen und Singularitäten" 1977, 1978, 1979 (9/5/81). Arch. IHÉS.

mathematics. More than ever, the Institut des hautes études scientifiques of Bures-sur-Yvette remained, in Sullivan's words, "'foreign' in the view of some Frenchmen."[180] Indeed, many of the Frenchmen working on chaos were busy elsewhere.

b) Bergé-Dubois: Laser Velocimetry

During the 1970s, as we have seen above, the experiments that Pierre Bergé and Monique Dubois performed at the CEA, Saclay, were closely linked with de Gennes's concerns with hydrodynamic instability. They even provided some of the matter for his course at the Collège de France. Working on the Rayleigh-Bénard system with new techniques combining laser technology and the computer, these two experimenters would later become closely associated with the chaos constellation.

(i) A Simple and Easy to Implement Technique

In 1974, Dubois and Bergé had become quite enthusiastic about their experimental setup, which in their view, provided simple and easy-to-implement experimental techniques for the measurement of local velocity flows in fluids.[181] They first applied them to the measurement of the speed of particles in a fluid, like spermatozoids. But around 1972, they turned to the study of the velocity field of the fluid itself for which they provided local, non-intrusive measurements. In view of difficulties in getting quantitative observations out of fluid dynamical systems, this represented a real

[180] *Notes de séances manuscrites. Comité scientifique* (13/5/77). Arch. IHÉS.
[181] A picture is repr. in Service de physique du solide, "Une nouvelle technique," 75. P. Bergé, "Ordre et convection dans les fluides," *La Recherche,* 6(62) (1975): 1070-1073.

experimental breakthrough, since velocity fields were exactly what existing theories could best calculate.

Dubois entered the CEA as an engineer in 1954, and Bergé as a physicist in 1956. Up until 1968, they worked with solids irradiated by neutrons, just like de Gennes during his Ph.D. Around that time, however, a new tool, the laser, made its appearance in their laboratory, which, they say, had a profound impact on their experimental work.[182] They started to study fluctuations in liquids at their critical point, and critical exponents. Four to five years later, following advice from de Gennes and Yves Pomeau, a theoretical physicist studying plasmas in the laboratory next door(see below), they turned to the study of Rayleigh-Bénard systems.

They got in contact with Manuel Velarde, who provided them with a good entry into the literature, and found that, although this was a system that had been well studied experimentally, "very little has been done about velocity measurements."[183] Their Rayleigh-Bénard cell was a rectangular box of $4.5 \times 100 \times 60$ mm^3 filled with silicon oil. By including graphite or aluminum powder in the fluid, they were able to observe the structure of the rolls directly. Their laser interferometer provided them with precise measurements of various components of the velocity field. "To our knowledge this is the first report on the measurement of local convective velocities near the threshold of the Rayleigh-Bénard convection."[184] For Rayleigh numbers up to

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[182] Interview of P. Bergé and M. Dubois by the author (6/3/97).
[183] P. Bergé and M. Dubois, "Convective Velocity Field in the Rayleigh Instability: Experimental Results," *Physical Review Letters,* 32 (1974): 1041-1044, 1041.
[184] P. Bergé and M. Dubois, "Convective Velocity Field," 1044.

about 2.5 times the critical value, their observation seemed to confirm the theory for the spatial dependence of the rolls.

Coming from critical phenomena, and in close contact with de Gennes, they obviously endeavored to verify Landau's power law. Much to their surprise they found that their observations disagreed with Landau's prediction, since they found the maximum of the vertical velocity component to be proportional to $(R-R_c)^{0.60\pm0.02}$, where the critical exponent was expected to be 0.5. This led them, and Velarde, to wonder whether the classic Boussinesq approximation was valid.[185] Later experiments, however, in which they replaced their side walls made of glass with copper showed that the lower thermal conductivity of glass was responsible for this deviation from Landau's theory.[186]

(ii) Mere Confirmation of Theory?

At the time, Yves Pomeau and Manuel Velarde—soon with the help of Pomeau's "only true graduate student," Christiane Normand—started to adapt Busse's theory in order to be able to account for Bergé and Dubois's results.[187] Bergé and Dubois were

[185] M. G. Velarde, "Hydrodynamical Instability," Notes added in Proof, 526. See R. Peréz-Cordon and M. G. Velarde, "On the (Non Linear) Foundations of Boussinesq Approximation Applicable to a Thin Layer of Fluid," *Journal de physique,* 36 (1975): 591-601.

[186] P. Bergé, "Rayleigh-Bénard Instability: Experimental Findings," 340-341; P. Bergé, "Aspects expérimentaux," 30-31. See the concluding paper on critical effects in Rayleigh-Bénard: J. E. Wesfreid, Y. Pomeau, M. Dubois, C. Normand, and P. Bergé, "Critical Effects in Rayleigh-Bénard Convection," *Journal de physique—Lettres,* 39 (1978): 725-731.

[187] M. G. Velarde announced a coming monograph in collaboration with Y. Pomeau in "Hydrodynamical Instability," 522. They would finally publish a long review article: C. Normand, Y. Pomeau, and M. G. Velarde, "Convective Instability: A Physicist's

soon led to distinguish two "domains." The "linear domain" was where velocity fields exhibited a perfect sinusoidal variation across the cell. For this domain, which corresponded to simple rolls in the fluid, the observation matched the theory.

> Morose spirits [*esprits chagrins*] will say that . . . nothing allowed us to doubt that conveniently adapted hydrodynamic equations would not pertain to this problem: this is, in any case, what one is tempted to say after the fact. . . . On our part, we think that we needed to attempt the experiment and that *the very positive collaboration between theoretical and experimental physicists that it caused would be a sufficient reason to justify it.*

Noting that convection cells were applied to many natural phenomena, Bergé tried to convince his audience that physicists should look anew into mundane phenomena, a call which may remind us of some of Thom's or Mandelbrot's.

> We think that it is good that, from time to time, physicists get out of their generally artificial world in order to study seriously the natural phenomena that surround them.[188]

When they got into the "nonlinear domain," at a Rayleigh number above $3R_c$, however, Bergé and Dubois's results remained unexplained. Despite the fact that Normand had "tried one's best to perform many theoretical computations," the fit between theory and experiment remained shaky. The picture they provided "was somewhat speculative, as more experimental evidence as well as theoretical analysis are needed to reach definitive conclusion."[189] But this seemed to worked These experimental results may have been somewhat disappointing since they only seemed to confirm the theory.

Approach," *Review of Modern Physics,* 49 (1977): 581-624. It was P. Manneville who told me in interview that Normand was Pomeau's "only true graduate student."

[188] P. Bergé, "Aspects expérimentaux," C1-33. My emphasis.

[189] P. Bergé, "Rayleigh-Bénard Instability: Experimental Findings," 346, and 351.

At the time, the Ruelle-Takens model was not explicitly discussed by Bergé and Dubois. They had been enrolled in the study of Rayleigh-Bénard by de Gennes, Velarde, and Pomeau because they had an original apparatus that allowed them to measure local quantities. But none of the above theoreticians were very keen, up to 1975, to take up a dynamical systems approach to the problem of the onset of turbulence in convection. Normand, Pomeau, and Velarde extended Busse's theory to include more and more modes. Striking tensions in the popular account by Velarde and Normand underscores that they had not adopted a dynamical systems approach.[190] When this is compared with Ruelle and Takens's attitude, the contrast is patent. While the former still dreamt of exact solutions, the latter expressed their agnosticism toward the fundamental Navier-Stokes equation itself.

Briefly, before the late 1970s, Bergé and Dubois's work could not be seen as stemming from a dynamical systems approach.[191] However, theirs was one of the experimental evidences that dynamical systems with a few degrees of freedom contained most of the mechanics of the transition to turbulence in confined geometry, findings that served to vindicate Ruelle's viewpoint. Moreover, it allowed contacts to be established between experimenters and theoreticians. On these bases, the chaos constellation was built. Among those mentioned above, Pomeau, more than anyone

[190] "Although exact solutions to [the Rayleigh-Bénard problem] are still lacking, substantial progress . . . has been made. " M. G. Velarde and C. Normand, "Convection," 93.

[191] The first really 'chaotic' paper of Bergé and Dubois was: M. Dubois and P. Bergé, "Experimental Evidences for the Oscillators in a Convective Biperiodic Regime," *Physical Review Letters,*76A (1980): 53-56.

else, was responsible for the translation of the dynamical systems approach into a language that physicists could understand.

c) Pomeau: Interdisciplinarity in Action

(i) A New Scientific Community?

A graduate from the École Normale, where he studied in 1961-1965, Yves Pomeau likes to recall the impression that Rocard's course on mechanical vibration left on him: this was the "first interesting professor" he had.[192] For someone who had an interest in mathematics in view of its applications, the Bourbakist influence was a "gigantic repellent [*gigantesque repoussoir*]." Pomeau became a theoretical physicist.

He worked on a Ph.D. at Orsay on plasma physics, and soon after was appointed to the CEA. This subject led him to take a look at phenomena of hydrodynamic instabilities in plasmas. He had started to collaborate with the Belgian physicist Paul Résibois, who belonged to Prigogine's school. Pomeau frequented the Brussels school and befriended Velarde.[193] This was the start of a long collaborative effort.

Contrary to most physicists, Pomeau was attracted by Thom's ideas on catastrophe theory, having been impressed by his, and Smale's, talks at the 1971

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[192] Interview of Yves Pomeau by the author (4/8/97). See his preface in P. Bergé, ed., *Le Chaos. Théorie et expériences* (Paris: Eyrolles and CEA, 1988).
[193] His more cited paper before 1975: Y. Pomeau and P. Résidois, "Time-Dependent Correlation Functions and Mode-Mode Coupling Theories," *Physics Reports,* C19 (1970): 63-139.

Statistical Physics Conference in Chicago.[194] In 1975, Pomeau invited Ruelle to speak at the CEA. By then he was teaching, with Annie Gervois, a course on hydrodynamics at Saclay, in which the Ruelle-Takens model was discussed. Impressed by dynamical systems theory, Pomeau studied the classics: Poincaré, Birkhoff, Smale, Arnol'd and Avez, etc. For Pomeau, it was clearly Ruelle and Takens's paper which had set everything in motion. "After the article of Ruelle and Takens, there has been recently much interest in the problem of the 'onset of turbulence'."[195] Following Martin, Pomeau saw three cases as possibly describing the onset of turbulence: Lorenz, Ruelle-Takens and the successive bifurcation picture as described by Li and Yorke among others. In collaboration with scientists coming from very different backgrounds, Pomeau embarked on many projects aiming at a better understanding of systems—mathematical, numerical, and experimental—which exhibited a sensitive dependence on initial conditions.[196]

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[194] S. A. Rice, K. T. Freed, and J. C. Light, eds. *Statistical Mechanics: New Concepts, New Problems, New Applications: Proceedings of the Sixth International Union of Pure and Applied Physics Conference on Statistical Mechanics, Chicago, March 1971* (Chicago: University of Chicago Press, 1972).

[195] Y. Pomeau, "Turbulence: Determinism and Chaos," *Problems of Stellar Convection: Proceedings of the Colloquium Nr. 38 of the International Astronomical Union, Held in Nice, August 16-20,1976,* ed. E. A. Spiegel and J.-P. Zahn (Berlin: Springer, 1977): 337-348, 337. See also P. Bergé, Y. Pomeau and M. Dubois-Gance, *Des rythmes au chaos* (Paris: Odile Jacob, 1994), 229.

[196] M. Hénon and Y. Pomeau, "Two Strange Attractors with a Simple Structure," *Turbulence and Navier-Stokes Equations,* ed. R. Tenam (Berlin: Springer): 29-67. J. L. Ibañez and Y. Pomeau, "A Simple Case of Non-Periodic (Strange) Attractor," *Journal of Non-Equilibrium Thermodynamics,* 3 (1978): 135-152 [preprint already written in 1975]; B. Derrida, A. Gervois, and Y. Pomeau, "Itération d'endomorphismes de la droite réelle et représentation des nombres," *CRAS A,* 285 (1977): 43-46; "Iteration of Endomorphisms on the Real Axis and Representation of Numbers," *Annales de l'Institut Henri-Poincaré,* 29 (1978): 305-356; partly repr. *Chaos,* ed. Hao B.-L., 1984 ed., 252-266; "Universal Metric Properties of Bifurcations

Despite the number of collaborators Pomeau had in very different fields, it seems difficult to see in this the emergence of a new scientific community, working on similar topics, with some shared research goals, outlets for their publications, and regular meetings. Most of Pomeau's collaborators indeed worked in different laboratories of the CEA at Saclay, but he appears to have been one of the only links between them. As Paul Manneville admitted, it was not easy to collaborate with Pomeau, despite his charming personality. He abruptly came to your office and threw at you lots of incomprehensible ideas without providing the references.[197]

By 1976, Yves Pomeau had arrived at the same kind of coherent picture of chaotic behavior and turbulence as Ruelle or Martin. As opposed to Martin, however Pomeau adopted a more mathematical language, inspired by Thom's catastrophe theory, Smale's dynamical systems theory, but also abstract ergodic theory. As opposed to Ruelle, Pomeau understood that merely to proclaim, as Thom had done with catastrophe theory, that chaos was a revolutionary mathematical way of modeling natural phenomena, was not enough. One needed to get one's hands dirty, to compare the results of theory with experiments, to collaborate with people coming from previously widely separated disciplines, and to build a common language. Just to insist on making people learn a new mathematical theory would not do it.

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of Endomorphisms," *Journal of Physics A,* 12 (1979): 269-296; C. Laj, D. Nordemann, and Y. Pomeau, "Correlation Function Analysis of Geomagnetic Field Reversals," *Journal of Geophysical Research,* 84B (1979): 4511-4515. See also M. Hénon, "A Two-Dimensional Mapping with a Strange Attractor," *Communications in Mathematical Physics,* 50 (1976): 69-77; repr. *Univ. Chaos,* 341-349; *Chaos II,* 235-243. For Pomeau's work on intermittency, see below.

[197] Interview of Paul Manneville by the author (23/5/97).

(ii) Intermittency: Translation of Dynamical Systems Modeling Practices

In 1976, Bergé and Dubois observed a singular phenomenon with their Rayleigh-Bénard system: "the velocity amplitude show[ed] intermittent periodic oscillations versus time." Even if the mechanism responsible for this phenomenon remained unclear, they believed that it represented "the most important step in the transition to turbulence."[198] At about the same time, Pomeau, who was studying the Lorenz model on an analogous computer, noticed a similar kind of intermittent flashes on his oscilloscope. He asked Paul Manneville to take a look at this and soon they came up with still another series of bifurcations that could be found in the onset of turbulence. Manneville's office at the CEA being just a few doors down from Bergé and Dubois's laboratory, he was given long printouts of experimental time series to analyze. A truly exciting collaboration again took place. With their help, this type of intermittent behavior was then also observed in oscillating chemical reactions.[199]

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[198] P. Bergé and M. Dubois, "Time Dependent Velocity in Rayleigh-Bénard Convection: A Transition to Turbulence," *Optics Communications,* 19 (1976): 129-133.

[199] The literature about intermittency is huge: see, e.g., Y. Pomeau and P. Manneville, "Intermittency: A Generic Phenomenon at The Onset of Turbulence," *Intrinsic Stochasticity in Plasmas,* ed. G. Laval and D. Grésillon (Orsay: Éditions de physique Courtaboeuf, 1979): 330-340; Y. Pomeau and P. Manneville, "Intermittent Transition to Turbulence in Dissipative Dynamical Systems," *Communications in Mathematical Physics,* 74 (1980): 189-197; repr. *Univ. Chaos,* 327-335; *Chaos II,* 355-363; P. Bergé, M. Dubois, P. Manneville, and Y. Pomeau, "Intermittency in Rayleigh-Bénard Convection," *Journal de physiques – Lettres,* 41 (1980): L341-L345; repr. *Univ. Chaos,* 149-153; Y. Pomeau J.-C. Roux, A. Rossi, S. Bachelart, and C. Vidal, "Intermittent Behaviour in the Belousov-Zhabotinski Reaction," *Journal de physique – Lettres,* 42 (1981): L271-L273; repr. *Univ. Chaos,* 167-169. P. Manneville and Y. Pomeau, "Different Ways to Turbulence in Dissipative Dynamical Systems." *Physica,* 1D (1980): 219-226.

A closer look at the intermittent model for the onset of turbulence provides a better appreciation of the modeling practices which became trademarks of chaos theory as conceptualized by physicists. Here, Pomeau and his collaborators were faced with similar phenomenological observations in the laboratory and on the computer: time series exhibiting apparently regular, periodic behavior randomly and abruptly disrupted by sudden bursts of erratic behavior. These observations were however made on systems which *a priori* had little to do with one another. Was there a common cause for these behaviors and, if yes, what could it be?

In trying to answer these questions, a dynamical systems approach proved to be most useful. But at the same time, Pomeau and Manneville did not wholly adopt the modeling practice of Ruelle and Takens, but transformed it in order to fit better with physicists' concerns. Indeed, they contended: "the general framework of these theories based on genericity arguments [Lorenz's and Ruelle's] is *sufficiently versatile* to allow for different possible transitions."[200] Thus, as Martin had earlier, they disputed the fact that the Ruelle-Takens model had to be the only way to turbulence.

In order to account for the similarity of patterns, Pomeau and Manneville considered a Poincaré map of the Lorenz model, in the form $y_{n+1} = f(y_n, r)$, where r was a parameter of the model associated with the Rayleigh number. They then considered the case where for r slightly below a critical value r_T, the curve had two intersection points with the diagonal, which collapsed into a single point for $r = r_T$, while "for $r > r_T$ the curve is lifted up and no longer crosses the [diagonal] so that a 'channel' appears between them" (Fig. 18).[201] Using a desktop computer, they were able explicitly to extract this picture from the Lorenz model. This simple abstract picture, Pomeau and Manneville contended, was "displaying generic features susceptible of *explaining* the experimental observations."[202] When the system passed through the "channel," it exhibited a behavior that seemed almost regular. Leaving the "channel," it explored chaotically other regions of phase space until it found itself again trapped into one such "channel."

Pomeau and Manneville's modeling practice lay in between the mathematical arguments used by the IHÉS 'applied topologists' and the traditional practice of physicists, which aimed at finding solutions to fundamental laws. The latter attitude could not be adopted here, they claimed. "A detailed quantitative interpretation is clearly out of reach, even from the simplified point of view of dynamical systems." Even if realistic dynamical systems relevant to the experiment could not be constructed: "Anyway, this unknown realistic dynamical system should share some generic properties with already well studied models."[203] In their view, such generic properties of *other* dynamical systems could provide an explanation for experimental behaviors which were assumed to stem from a similar, but unknown, dynamical system.

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200 P. Bergé, M. Dubois, P. Manneville, and Y. Pomeau, "Intermittency in Rayleigh-Bénard," L-341. My emphasis.
201 Y. Pomeau and P. Manneville, "Intermittent Transition," *Univ. Chaos,* 328.
202 P. Bergé, M. Dubois, P. Manneville, and Y. Pomeau, "Intermittency in Rayleigh-Bénard," L-343. Their emphasis.
203 P. Bergé, M. Dubois, P. Manneville, and Y. Pomeau, "Intermittency in Rayleigh-Bénard," L-342.

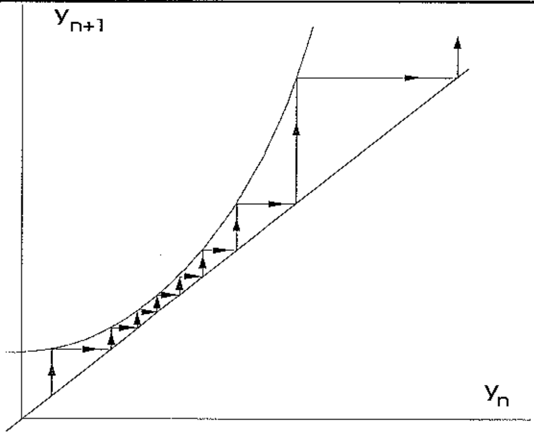

Figure 18: Poincaré Map from the Lorenz Model, with r slightly above r_T. The motion through the channel corresponds to the laminar phase of the movement. The slow drift is quite imperceptible on the time record. Redrawn from Y. Pomeau and P. Manneville, "Intermittent Transition," *Univ. Chaos,* 330, Fig. 4.

While eschewing traditional modeling practices, Pomeau and Manneville likewise neglected to ground their model on rigorous mathematical proofs.

> The reader must be warned that the discussion is made *in physical terms.* No proof is given. Many of them are certainly very difficult and require advanced mathematics. . . . We try to present our point of view *as intuitively as possible,* and have avoided almost completely any standard analytical formalism which is *useless* for this sort of problem.[204]

They plainly admitted that they had "*guessed*" from numerical computations.[205] The intermittent picture for the onset of turbulence would nonetheless strike physicists "because of its esthetic and conceptual beauty."[206]

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204 Y. Pomeau and P. Manneville, "Intermittency," 331.
205 Y. Pomeau and P. Manneville, "Intermittency," 339.
206 J.-P. Eckmann, "Roads to Turbulence in Dissipative Dynamical Systems," *Reviews of Modern Physics,* 53 (1981): 643-654, 650; repr. *Univ. Chaos,* 94-105.

When compared with the modeling practices of Ruelle, Smale, Thom, and Zeeman, discussed in previous chapters, the practice adopted by Pomeau and Manneville for intermittency reveals many similarities and some crucial differences. Like most of the above, the pair of CEA physicists started with phenomenological similarities that they wished to explain with topological, rather than reductionist, arguments. The link with the substratum was postulated but was not a concern of their practice. Bifurcations were again interpreted as the source for changes in behaviors. However, unlike the others, Pomeau and Manneville relied on specific computer models, whose generic properties, they assumed, could be transferred to a "realistic," but unknown, dynamical system. Rigorous mathematical proofs were neither the starting point, nor the goal of their study. The mathematics of dynamical systems theory, and the important concept of genericity, were loosely used in order to infer that phenomena observed in numerical studies could account for experimental data.

Dynamical systems theory thus provided theoretical physicists, like Pomeau and Manneville, with concepts and techniques that could be profitably used in order to make sense of experimental observations, even while avoiding too much mathematical technicality. They realized that even very abstract mathematical constructs might be useful in trying to understand nonlinear phenomena. The experimental work that Libchaber undertook in 1977 would only serve to further this feeling.

d) Libchaber: Helium in a Small Box

During the late 1970s, some of the physics staff of the École normale supérieure of Paris were stunned by the new tabletop experiment of their colleague Albert Libchaber.[207] With the help of the engineer Jean Maurer, he built a tiny cavity whose volume was less than a few cubic millimeters and filled it with non-superfluid liquid helium. They then heated it slightly at the bottom and observed changes in temperature: a classic Rayleigh-Bénard experiment.

For some physicists and mathematicians his results came as a revelation: it was "a kind of miracle, not like the usual connection between theory and experiment."[208] For Leo Kadanoff, it was "an experience like no other experience I can describe, the best thing that can happen to a scientist, realizing that something that's happened in his or her mind exactly corresponds to something that happens in nature."[209] By and large, Libchaber and Maurer's observation of the cascade of bifurcations conjectured by Los Alamos physicist Mitchell Feigenbaum was responsible for the chaos fashion of in the following decade. Ten years later, when James Gleick wrote his book, this experiment was to take a prominent position. There

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[207] Libchaber's experiment was recounted by J. Gleick, *Chaos,* 189-211. Many details on Libchaber's career and ideas, as well as the circumstances surrounding this experiment are based on an interview that I conducted with him on October 24, 1993 in Princeton, who then told me that this was , "the first [*sic*] experiment in classical physics at the École.".
[208] Jerry Gollub quoted by J. Gleick, *Chaos,* 209.
[209] Leo Kadanoff quoted by J. Gleick, *Chaos,* 189.

Libchaber became *the* experimenter of chaos.[210] Since this story is well known, I want only to underscore how Libchaber's experiment served to vindicate a dynamical systems approach to the study of turbulence.

(i) Bolometers: A Local Probe

A frequent visitor of Bell Labs, Libchaber had been in contact with Günter Ahlers, who convinced him of the interest of Rayleigh-Bénard systems. Libchaber's experiment was therefore very similar to Ahlers's, his principal innovation being the local probe–a resistor sensitive to heat, or *bolometer*–that enabled him to measure locally the heat flow in the liquid.[211] This was a crucial difference. As opposed to Ahlers's global measurements, Libchaber's gave local information about the fluid flow. He could thus hope to observe phenomena similar to the ones exhibited by Bergé and Dubois.

For some time, Libchaber had been interested in superconductivity and superfluidity, through which he was introduced to the experimental manipulation of liquid helium. At the Dijon Symposium in 1975, he had presented a talk on turbulence in superfluid helium.[212] The experimental cavity he then used was similar to the one he would later use to study Rayleigh-Bénard, which underscores the transfer of

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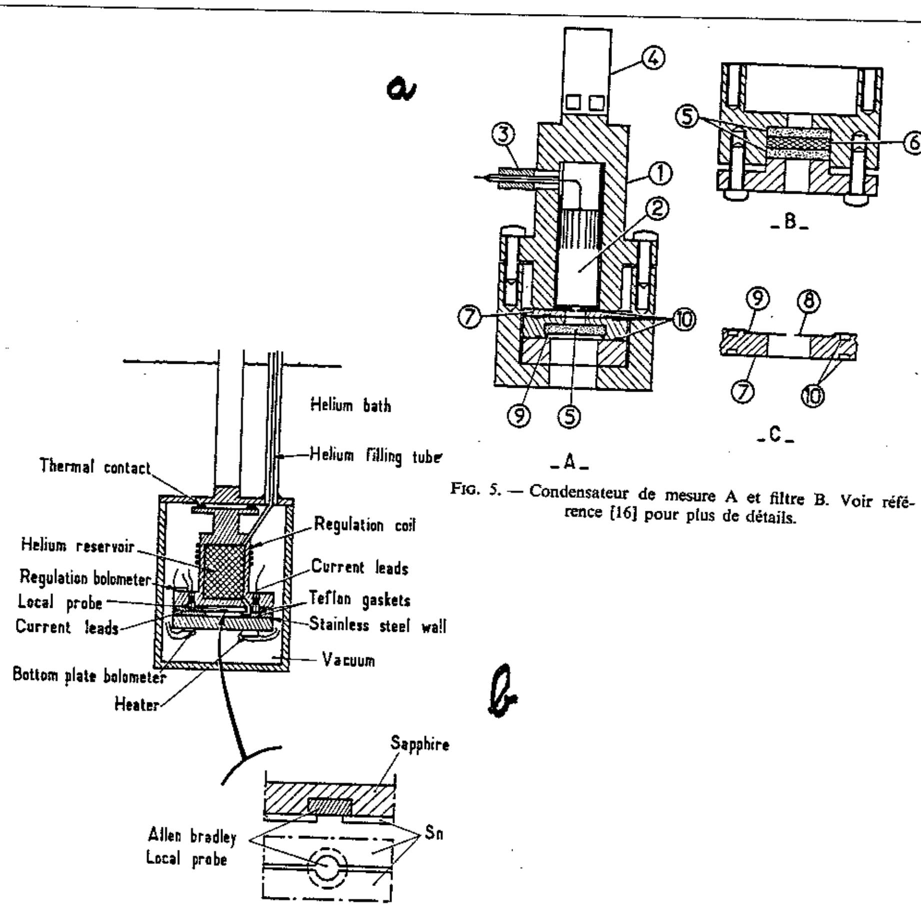
[210] I suppose that this was because, contrary to other experimenters mentioned above, Libchaber had a strong interest for philosophy, and Goethe and D'Arcy Thompson in particular.
[211] A. Libchaber and J. Maurer, "Local Probe in a Rayleigh-Bénard Experiment in Liquid Helium," *Journal de physique – Lettres,* 39 (1978): L369-L372.
[212] A. Libchaber, "Hydrodynamique de l'hélium IV superfluide," *Journal de physique,* 37, Colloque C1 (1976): 111-116.

experimental techniques from the study of phase transition to that of turbulence (Fig. 19). At the conceptual level, it was precisely on problems close to dynamics that he focused his attention: "the dynamics in the vortices within superconductors, and the equivalent within superfluids." By working in a very small geometry of the order of a micron, it was possible to isolate a small number of these "objects," as he insists on calling them.[213] Considering "a kind of *polymer* of quantized vortices of the superfluid," Libchaber's study of turbulence in superfluid had considerable bearing on the concerns expressed in the ATP on instability.[214]

Being microscopic, these vortices were quite hard to study individually. He considered the problem a bit, but then turned to classical, macroscopic vortices in non-superfluid liquid helium. It is in this spirit that he undertook the experiment. He wanted "to see a macroscopic *object,* i.e. one or two convection rolls, and to study their dynamics."[215] His motivation was conceptual rather than strictly theoretical, in the sense that at the beginning, he was not aware of the theories of Lorenz, Ruelle, or Feigenbaum. Nevertheless, Libchaber always thought a lot about his experiments before he started building systems. This Rayleigh-Bénard experimental design deliberately conceived in such a way as to catch glimpses of emergent dynamical structures.

[213] Interview of A. Libchaber (24/10/1993). For a discussion of changes in the practice of the definition of objects, see I. Stengers, *Cosmopolitiques,* 5, 152.
[214] A. Libchaber, "Hydrodynamique de l'hélium IV," C1-115.
[215] Interview of A. Libchaber (24/10/93).



The upper part is the detail of the experimental system. The lower part is an enlarged view of the local probe seen both in a transverse cut and from the bottom. Dimensions are not conserved.

Figure 19: Libchaber's Experimental Apparatuses for the Study of Superfluid Helium, and for the Study of the Onset of Turbulence. Repr. with permission from (a) A. Libchaber, "Hydrodynamique de l'hélium IV," C1-114. Copyright © Les Éditions de Physique. (b) A. Libchaber and J. Maurer, "Helium in a Small Box." Copyright © Plenum Publisihers.

So Libchaber's interest differed from that of other physicists then experimenting with convection (Ahlers, Bergé, Gollub, etc.). The latter focused on the onset of turbulence from the point of view of a phase transition. On the other hand, Libchaber's concerns with dynamics triggered, first, the question of how to observe the relevant objects of study. And then, how should these effects be described physically and mathematically? Naturally following Landau's theory, Libchaber's ingenious bolometers gave time series easily amenable to a Fourier analysis of frequencies. The way he would study these frequencies was shaped by his interest in the dynamics of macroscopic objects emerging in fluids.

(ii) Experiment and Observations

Libchaber and Maurer's first article offer clear evidence of their concerns at the time of the experiment. They showed the "extraordinary dependence of the results on the aspect ratio," the radius of the cavity divided by its height. The diameter of their cylindrical cell was fixed at 25 mm. They studied the behavior of their system for heights varying from about 1 mm to 6 mm and for Rayleigh numbers R ranging from just above the critical value to about $15R_c$. For large heights they observed a sharp spectral line together with up to ten harmonics that appeared past a certain value of the Rayleigh number, in accordance with Landau's theory. For smaller heights, however, the behavior was totally different. No well defined frequencies and a continuum of noise in the low frequencies. This extreme dependence on the aspect ratio came as surprise to other physicists.[216] For him, this was not so surprising since the objects that could form in the cavity had to be different depending on the aspect ratio. For Libchaber, the next step was to study more carefully the geometry of the

[216] Interview of A. Libchaber (24/10/93).

shapes they were just starting to perceive, before attempting a "theoretical analysis of the data."[217]

In August 1979, for their second paper, Libchaber and Maurer changed the geometry of their cavity from a cylindrical to parallelepipedic cell, and placed two bolometers.[218] For the first time, they referred to the theoretical work of Fritz Busse, and the word 'bifurcation' entered their vocabulary. However, their interest partly lay elsewhere. For a rectangular cell, it seemed possible to define more clearly the rolls geometry: in the simplest case, their data seemed to be consistent with the hypothesis that two transverse rolls were formed, in agreement with previous studies.[219] The power spectrum of the temperature field again exhibited well-defined frequencies. They thus focused on the dynamics of these frequencies.

In particular, a phenomenon caught their eye. In addition to the one clearly defined frequency, they now were able to exhibit a second one above a second threshold. Libchaber and Maurer noticed that "as one keeps increasing the temperature difference, all the combination frequencies, $f = mf_1 + nf_2$, m and n [positive or negative] integer, appear as one Fourier analyses the data." All of these combination of frequencies rapidly produced a situation that seemed quite noisy, the ratio of the two frequencies being approximately irrational. Citing Gollub and Bergé's

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[217] A. Libchaber and J. Maurer, "Local Probe," L-372.
[218] J. Maurer and A. Libchaber, "Rayleigh-Bénard Experiment in Liquid Helium: Frequency-Locking and the Onset of Turbulence," *Journal de Physique – Lettres,* 40 (1979): L419-L423.
[219] K. Stork and V. Muller, "Convection in Boxes: Experiments," *Journal of Fluid Mechanics,* 54 (1972): 599-611.

work, they had clearly started to focus on quantities bearing on the Ruelle-Takens model, which, however, they did not mention.

What interested in the first place Libchaber and Maurer was to take these frequencies as legitimate objects susceptible of a dynamical analysis. These concerns made them notice in passing two other aspects of their data, which assumed more importance in their future investigations. They were: that "more than two frequencies are never measured until we reach the turbulent regime, in confirmation with other observations;" and "that the onset of turbulence starts from this locking state with parametric amplification of frequencies *f*/2, *f*/4 ..."[220]

While it would be an exaggeration to say that Maurer and Libchaber's next three papers would principally deal with a careful investigation of these observations, their scientific worth would later be recognized mainly on the basis of the resonance that these observations would find in theoretical works on dynamical systems.[221] Indeed after an extensive collaboration with theoreticians, these observations would be more or less reinterpreted as: a contradiction of Landau's picture and an indication for the validity of the Ruelle-Takens scenario; and the first experimental observation of the Feigenbaum cascade of bifurcations. Libchaber's later observations would

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[220] J. Maurer and A. Libchaber, "Rayleigh-Bénard Experiments," L-422.

[221] A. Libchaber and J. Maurer, "Une expérience de Rayleigh-Bénard de géométrie réduite: Multiplication, accrochage et démultiplication de fréquences," *Journal de physique, Supplément,* Colloque C3, 41 (1980): C3-51 to C3-56; J. Maurer and A. Libchaber, "Effect of the Prandtl number on the onset of turbulence in liquid ^{4}He," *Journal de physique – Lettres,* 41 (1980): L515-L518; A. Libchaber and J. Maurer, "A Rayleigh-Bénard Experiment: Helium in a Small Box," *Nonlinear Phenomena at Phase Transitions and Instabilities: Proceedings of the NATO Advanced Study Institute [Geilo, Norway: March 1981],* ed. T. Riste (New York: Plenum, 1982): 259-286.

moreover enable him to compare "universal" features of Feigenbaum's model with experiment, and to exhibit Pomeau and Manneville's intermittent road to turbulence.

(iii) Feigenbaum: Surprise and Excitement

By 1980, Albert Libchaber, as previous experimenters, had found and mastered the direct relevance of Busse's theory. He noticed the "qualitative" accordance with the theory.[222] But as soon as the second frequency appeared, he "had no physical model to interpret this second oscillatory mode." Libchaber's interaction with the theoreticians Pomeau, Ruelle, Eckmann, and Feigenbaum, would redirect his experiment towards new goals.

Libchaber had met Feigenbaum at the Gordon Research Conference on "Dynamical Instability and Fluctuations in Classical and Quantum Systems," organized by Paul Martin in July 1976.[223] Carefully noting a "demultiplication of frequencies," Libchaber and Maurer were drawn to conclude that this process "appeared as crucial for the germination of turbulence."[224] Not much later, they identified the phenomenon with the frequency-doubling cascade that Feigenbaum was studying as a purely mathematical phenomenon.

To go into much details about the history of discrete iteration of mappings of the type $x_{i+1} = F_\mu(x_i)$ would take us away from the main focus of this chapter.[225] Let

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222 A.Libchaber and J. Maurer, "Une expérience de Rayleigh-Bénard."
223 List of participants provided to me by Paul Martin.
224 A. Libchaber and J. Maurer, "Une expérience de Rayleigh-Bénard," C3-56.
225 Important parts of this story are moreover already well known. This is one of the main theme of J. Gleick, *Chaos,* esp. the chapters titled "Life's Ups and Downs" and "Universality," 57-80 and 155-187. Another accessible introduction is Leo P. Kadanoff, "Roads to Chaos," *Physics Today* (December 1983), 46-53. See also C.

me just recall that, in 1974-1975, unexpectedly complex behavior in these kinds of problems had led May, Li, and Yorke to embrace the word 'chaos' to describe it.[226] As we have seen, in the later 1970s, this became a favorite field of study at the IHÉS.

Since the work of Smale, these maps had been considered as dynamical systems as well, and one could study the bifurcations as μ increased. As it turned out, for a large class of nonlinear functions F_μ, chaotic behavior could be obtained by varying the parameter μ. Following a pattern similar to the Ruelle-Takens model, for small values of μ, there was a fixed point x_0 defined as an attractor, such that $F_\mu(x_0)= x_0$, and as μ was increased, instead of a Hopf bifurcation, this system went through a pitchfork bifurcation giving rise to a pair of attractors, the system oscillating between both of them. This doubling process was repeated at an accelerating cadence–there were 4, then 8, then 16 values in the cycle, etc.–until the situation became nonperiodic. The main difference with the Ruelle-Takens scheme was that aperiodicity was reached after an infinite number of bifurcations, instead of only three. It is not like Laudau's either, because aperiodicity was eventually achieved. This

Mira, "Some Historical Aspects Concerning the Theory of Dynamic Systems," *Dynamical Systems: A Renewal of Mechanism,* ed. S. Diner, D. Fargue, and G. Lochak (Singapore: World Scientific, 1986): 250-262; "Some Historical Aspects of Nonlinear Dynamics, Possible Trends for the Future," *Visions of Nonlinear Science in the 21th Century, Seville, June 26, 1996.* A mathematical survey can be found in P. Collet and J.-P. Eckmann, *Iterated Maps on the Interval as Dynamical Systems* (Boston: Birkhäuser, 1980).

[226] R. May, "Biological Populations with Nonoverlapping Generations, Stable Points, Stable Cycles, and Chaos," *Science,* 186 (1974): 645-647; "Simple Mathematical Models with Complicated Dynamics," *Nature,* 261 (1976): 459-467; *Univ. Chaos,* 85-73; *Chaos II,* 151-159; and T.-Y. Li and J. A. Yorke, "Period Three Implies Chaos," *American Mathematical Monthly,* 82 (1975): 985-992.

behavior had led Martin to consider this sequence of bifurcation as a possible scenario for the onset of turbulence (Figure 1).

May, and others after him, had noted certain qualitative properties that did not seem to rely on the precise form of F_μ. In a series of article starting in 1978, Los Alamos theoretical physicist Mitchell J. Feigenbaum explored the problem, using renormalization group methods and a pocket calculator. He showed that it was possible to extract from these generic qualitative properties universal quantities–numbers![227]

In the words of Libchaber, one "essential prediction" of the theory was the Rayleigh numbers at which the frequency doubling occurred should be such that:[228]

$$\frac{R_{n-1} - R_n}{R_n - R_{n+1}} \xrightarrow[n\to\infty]{} \delta = 4.6692...;$$

where R_n was the Rayleigh number at which occurred the bifurcation producing 2^n frequencies, and δ was a universal constant.

Thus were Libchaber's intriguing observations explained. While he had been lacking theoretical guidelines to account for them, Feigenbaum's theory helped Libchaber articulate his observation. His experimental result for δ was "quite bad," but not in contradiction with Feigenbaum's theory: $\delta = 3.5 \pm 1.5$. Everything thence

[227] M. J. Feigenbaum, "Quantitative Universality for a Class of Nonlinear Transformations," *Journal of Statistical Physics,* 19 (1978): 25-52; repr. *Chaos II,* 160-187; "The Onset Spectrum of Turbulence," *Physics Letters,* 74A (1979): 375-378; "The Universal Metric Properties of Nonlinear Transformations," *Journal of Statistical Physics,* 21 (1979): 669-706; "The Transition to Aperiodic Behavior in Turbulent Systems," *Communications in Mathematical Physics,* 77 (1980): 65-86; and "Universal Behavior in Nonlinear Systems," *Los Alamos Science,* 1: 4-27; repr. *Univ. Chaos,* 49-84.

went fairly quickly. People and preprints went back and forth over the Atlantic, so that pairs of articles could refer to one another. "At some point, you don't know anymore who's doing what. It becomes a highly interactive milieu."[229] The final success was total, the excitement general. Still, a puzzling question remained, that was the foremost cause of the initial surprise among scientists. These simple iterations of functions, these nice mathematical games, what had they to do with real fluid flows? At this moment, Eckmann was already working on the integration in a global framework of these new approaches to turbulence.

e) Eckmann's Synthesis: The 'Dynamical Systems Approach'?

The time for synthesis had come. Jean-Pierre Eckmann, from Geneva, the son of a famous Swiss mathematician, had been in contact with Thom and Ruelle in the "stimulating atmosphere at Bures-sur-Yvette."[230] A frequent visitor there, he was well positioned to achieve this synthesis, having been one of the crucial personal link between Libchaber and Feigenbaum.[231]

In October 1981, Eckmann published an article that presented the general philosophy and theory behind this new "approach to the understanding of irregular (or nearly irregular) phenomena, which has been relatively successful recently," adding in a footnote: "this approach can be viewed as a concretization of Thom's (1972)

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[228] Libchaber and Maurer, "Helium in a Small Box," 280.
[229] Interview of A. Libchaber (10/24/93).
[230] P. Collet and J.-P. Eckmann, *Iterated Maps on the Interval,* vii. It was Eckmann who wrote Ruelle's Lausanne lecture note in 1970.
[231] G. B. Lubkin, "Period-Doubling Route to Chaos Shows Universality," *Physics Today,* 34-3 (March 1981): 17-19.

catastrophe theory."[232] Although it hardly contain anything new, after more than a decade of work on instabilities in fluids, this was one of the first syntheses to come out, which was explicitly based on dynamical-systems modeling practices. "In order to describe our main topic, we need an adequate language for describing deterministic evolution equations."[233] This language would be that on dynamical systems theory.

Acknowledging that an aim "clearly felt throughout the literature on dynamical systems," was far from being achieved, Eckmann turned to *experiments* as a guide for which bifurcations from simple attractors to nontrivial ones might be the most relevant for physics (and chemistry).[234] He thus introduced the notion of a *scenario* to describe the *most probable* sequences of bifurcations. Three roads to turbulence deserved the label: the Ruelle-Takens scheme, Feigenbaum's cascade, and the Pomeau-Manneville intermittent behavior. There were no guarantee at all that this list was exhaustive.

"We are going to look at the nature of the prediction which can be made with the help of scenarios, since this may be a *somewhat unfamiliar way of reasoning*." Eckmann characterized scenarios as "if. . ., then . . ." statements, "i.e., if certain things happen as the parameter is varied, then certain other things are likely to happen as the parameter is varied further." The mathematical definition of "likely," clearly linked with genericity, depended on the scenario.

> But what does likely means in a physical context? I do not intend to go to any philosophical depth but, rather, take a pragmatic stand. (1) One *never* knows

[232] J.-P. Eckmann, "Roads to turbulence in dissipative dynamical systems," *Reviews of Modern Physics,* 53 (1981): 643-654, 643.
[233] J.-P. Eckmann, "Roads to turbulence," 643.
[234] J.-P. Eckmann, "Roads to turbulence," 645.

> exactly which equation . . . is relevant for the description of the system. (2) When an experiment is repeated, the equation may have slightly changed (e.g. the gravitational effects change on the earth by the motion of the moon). (3) The equation under investigation is one among several, all of which are very close to each other. (4) If among these there are many which satisfy the scenario, then we will say that if we perform an actual experiment, it will be probable that the conclusions of the scenario applies.[235]

In general, the scenarios described only the tiniest part of the phase space. "Therefore, *several scenarios may evolve concurrently in different regions of phase space*. There is thus no contradiction if several scenarios occur in a given physical system, depending on how the initial state is prepared." While the "then" part of a scenario was likely to happen if the "if" part was satisfied, there was no attempt in the theory to say how probable the hypothesis was. "*A scenario does not describe its domain of applicability.*"[236]

The theory was "completely general," but these restrictions transformed it into a patchwork that was full of holes. Moreover there was no way to tell where the holes were located and how much surface they occupied. Since the theory was a local description of different scenarios, its predictive power was limited to those patches of known scenarios. Once one had recognized the patch corresponding to the situation, the succession of events to follow was likely to be known.

Inspired by Thom and Ruelle's modeling practices, this type of mathematization was of an original type. It underscored that "new types of questions" could be raised. The validity of the answers one provided to these questions eschewed

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[235] J.-P. Eckmann, "Roads to turbulence," 646. My emphasis.
[236] J.-P. Eckmann, "Roads to turbulence," 646. His emphasis.

reliance on fundamental laws. This general method was briefly summarized by Eckmann and his collaborators as such:

> Physical models of hydrodynamics or of other dissipative dynamical systems tend to be very complicated. In addition, the laws describing such systems are only known *approximately.* One is thus faced with the problem of isolating and if possible answering *new types of questions which are more or less independent of detailed knowledge of the dynamics* of any given physical system. Such questions the have answers which are *universal.*[237]

On the basis of a mathematical failure, a fruitful modeling practice was thus constructed by physicists for physicists. Indeed, mathematicians had not succeeded in classifying generic bifurcations of dynamical systems. Nevertheless, Eckmann's scheme emphasized the benefits of a dynamical systems approach coupled with experimental results, indicating which bifurcations were the likeliest to occur. Eckmann explained the way to use results of dynamical system theory. Turbulence, with its baggage of bifurcations, cascades, and strange attractors, became a standard exemplar used to describe many other systems from chemical reactions to menstrual cycles, as well as clarinets![238] Chaos was born.

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[237] P. Collet, J.-P. Eckmann, and H. Koch, "Period Doubling Bifurcations for Families of Maps on $\mathbf{R}^n$," *Journal of Statistical Physics,* 25 (1981): 1-14, 1; repr. *Univ. Chaos,* 353-366. My emphasis.

[238] Just to list a few places where this modeling practice was applied, see, e.g., I. R. Epstein, "Oscillations and Chaos in Chemical Systems," *Physica D,* 7 (1983): 47-56; L. Glass and M. C. Mackay, *From Clocks to Chaos: The Rhythms of Life* (Princeton University Press, 1988); and C. Maganza, "Du silence au chaos acoustique: les 'bifurcations' d'une clarinette," *La Recherche,* 17(173) (1985): 100-103.

In 1982, a proof of Feigenbaum's conjectures was provided by Oscar E. Lanford, from Berkeley. Symbolically, the proof turned out to be computer-assisted.[239]

6. CONCLUSION

Partly as a result of Ruelle and Takens's proposal, a "dynamical systems approach" was indeed adopted by physicists. But this approach was characterized by modeling practices which differed significantly from Ruelle and Takens's. Indeed, a decade of work on various models for the onset of turbulence, in Rayleigh-Bénard systems especially, had shown that mathematical arguments alone, based on the notion of structural stability and genericity, could be misleading. Other "scenarios" existed besides Ruelle and Takens's. But, where mathematicians faced tremendous difficulties, namely for the classification of bifurcations in dynamical systems, experiments, it was realized, offered an original means to determine which scenarios were more relevant than others.

a) The Triumph of 'Light' Physics

In the above, I displayed how misleading is the view that Ruelle and Takens's model was a mathematical theory which gained credence from experimental evidence. By focusing on Rayleigh-Bénard convection as boundary system, I showed that the interest in hydrodynamic instabilities hardly stemmed from Ruelle and Takens's proposal. On the contrary, many groups of scientists focused on these problems as a

[239] O. E. Lanford, "A Computer-Assisted Proof of the Feigenbaum Conjectures," *Bulletin of the American Mathematical Society,* 81 (1982): 427-434; repr. *Univ.*

way to explore the ramification of the theoretical tools already enabling them to tackle nonlinear phenomena. To start with, by the late 1960s and early 1970s, hydrodynamicists, by undertaking both experimental and theoretical studies of the Rayleigh-Bénard system, had already begun to disentangle the problems raised by this system. They clearly distinguished between Bénard's hexagonal cells and the rolls triggered by Rayleigh's instability. They moreover discussed the perturbations affecting these rolls. All of these developments took place independently of Ruelle and Takens's proposal. And indeed, we can even see that, when he picked up dynamical systems again, in 1974-75, Ruelle joined, rather than propelled, a bandwagon that already was in motion.

At the same time, the Rayleigh-Bénard system was singled out by various groups of scientists, which saw in it helpful analogies with other phenomena such as dissipative structures in chemical kinetics and phase transitions. Indeed, Prigogine's school and people working on phase transitions considered that their methods were "universal" enough to provide accounts for hydrodynamic instabilities. Moreover, the study of such systems could offer useful resources, since they were well studied and often made use of recent mathematical theories of qualitative dynamics. Thus the analogy could not only help understand hydrodynamic instabilities with methods developed in order to deal with nonlinear phenomena in physics and chemistry, it could moreover provide new resources to think about these phenomena. Constant contacts among these different groups generated enough excitement, so that the study

Chaos, 245-252.

of these phenomena became quite fashionable even before anything from the dynamical systems theory developed by Smale entered the scene.

But theoretical analogies proved to be disappointing as a way to tackle the turbulence problem. On the other hand, experimental techniques common to the study of phase transitions at a critical point were very successfully transferred to the study of hydrodynamic instabilities. These techniques could be used to provide measurements of greater accuracy than earlier ones, and most importantly, measurements of local quantities to compare with hydrodynamic theories.

In the process, the study of fluid dynamics witnessed a striking influx of physicists. Once again in its long history, rather mundane phenomena of fluid mechanics thereby became a central object of inquiry for physicists and mathematicians. As Bergé contended: "It is a domain in which, with relatively modest material tools, but a good dose of imagination, one can contribute: it illustrates, as it were, the triumph of 'light physics'."[240] At the same time, this field was profoundly transformed, not only by the adoption of a dynamical systems approach, but mostly as a consequence of the physicists' bringing a wide array of theoretical and experimental tools to bear on the study of fluids.

In France, the case I studied the most above, it became clear that this also was the result of a political desire. While atomic and nuclear physics had been emphasized before, it became urgent, in the eyes of the French science policy makers of the early 1970s, to revive the study of "light" physics and chemistry. The VIth Plan underscored the need and outlined means to achieve this goal. One consequence of this political orientation was that an interdisciplinary study of fluid instabilities was valued, especially since energetic groups studying liquid crystals had made the analogy between phase transitions and hydrodynamic instabilities real, since both occurred in their systems.

Out of these efforts, a chaos constellation emerged among French physicists. In the above, the work of a few experimenters and theoreticians provided concrete examples of how something approaching a "dynamical systems approach" was finally adopted. But in the process, the modeling practices of 'applied topologists' or of Ruelle and Takens hardly was wholly picked up by physicists. On the contrary, by looking at the IHÉS research program in 1970s, it has been argued that the Institute indeed seemed to have remain somewhat remote from the physicists' main considerations.

b) Experiment-Based Topology?

Physicists actively adapted the language and the (modeling and theoretical) practices coming out of dynamical systems theory. The traditional view has it that, in the 1970s, it was finally shown that dynamical systems theory could be usefully "applied" to real systems, offer explanations for turbulent behaviors, and even make predictions. This was a different type of prediction, to be sure, emphasizing qualitative behavior and, especially, the very limits of the predictions due to the property of sensitive dependence on initial conditions. But, as the above chapter indicates, we may completely reverse this view. Experiments in fluid mechanics provided some

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[240] P. Bergé, Y. Pomeau, and C. Vidal, *Order within Chaos,* 267.

justification for an approach that, on a purely mathematical level, still was quite incomplete. They offered a basis for achieving some classification of "generic" bifurcations in dynamical systems, where rigorous arguments had failed. In this way, the modeling practice of many chaologists became what we might call an experiment-based topological modeling practice, which used the objects of dynamical systems and bifurcation theory as an important part of their mathematical arsenal, but eschewed the most grandiose claims of the IHÉS applied topologists discussed in previous chapters.

As a testimony for this, Libchaber explained that his work "surely a theoretical breakthrough. I say theoretical, it wasn't experimental. . . . My essential contribution was to show that this mathematical game existed in nature." If there was an element of surprise for this experimenter, it was universality. It really struck him as something grandiose that made him realize that he *was "playing with mathematics."*[241] For many, his experiment really *showed* the genericity of the Feigenbaum cascade scenario. Mathematical games could help understand nature, but experiments, and only experiments, could decide which of those games were relevant to the study of nature.

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241 Interview of A. Libchaber .

7. COMPLEMENT TO CHAPTER VIII: DOCUMENT

Research Program Presented by Nicolaas Kuiper to the Volkswagen Foundation (1976).

In the general framework of dynamical systems and singularities, a certain number of research projects have been and will be in activity at the IHÉS. Some of these projects are described below.

(a) Turbulence and differentiable dynamical systems.

Turbulence in fluid dynamics is a phenomenon of considerable practical importance, which is however very poorly understood at a fundamental level. One point of view which is gaining acceptance is that "turbulent" solutions of the time evolution equations of fluid dynamics are solutions with an apparently chaotic asymptotic behavior and sensitive dependence on initial conditions. One of the basic papers on the subject was written at the IHÉS (D. Ruelle and F. Takens. On the Nature of turbulence. Commun. math. Phys. 20, 167-192 (1971)). Further work on turbulent solutions of differential equations at the IHÉS was done by O. Lanford and J. Curry, making use in particular of the HP 9830 A calculator of the Institute. Currently, S. Newhouse and D. Ruelle are engaged in further mathematical study of differential equations and diffeomorphisms exhibiting turbulent behavior.

(b) Differentiable dynamical systems.

This is a vast and important subject which has been rejuvenated by S. Smale and his school - among others. The IHÉS has played an important role in the development of this area of research, where R. Thom has been an active source of inspiration. Among the past visitors one can mention : S. Smale, C.C. Pugh, M. Shub, R. Bowen, R.F. Williams, J. Frank, J. Robbin, J. Palis, F. Takens, etc. These have worked mainly on systems satisfying Smale's Axiom A (or related hyperbolicity conditions), obtaining in particular important results on structural stability and bifurcations of these systems. In this line of research we have at present S. Newhouse as visitor.

Another direction of research in differentiable dynamical systems is the study of irrational rotations on the circle (or flows on tori). Spectacular results there have been obtained recently by M. Hermann (Ecole Polytechnique and IHÉS). P. Deligne has become interested in this work. This work is related to that of C. Siegel, J. Moser and Russmann.

(c) Foliations.

The coming of D. Sullivan to the IHÉS as permanent member has promoted a considerable development of the subject. He has led a very active seminar, developed the concept of "geometric current" (with D. Ruelle), and obtained new and beautiful results (in particular collaborating with R. Edwards and K. Millett, as well as D. Epstein). Let us mention as an example Sullivan's construction of a flow on a compact five-manifold so that every orbit is periodic but the length of orbits is unbounded.

(d) Relation between qualitative dynamics and algebraic singularities, Catastrophes.

This is the subject of bifurcation theory, which aims to describe how a stable régime of a dynamical system can be destroyed and transformed discontinuously into another one. After the scheme of "elementary catastrophe theory", associated to singularities of functions, one feels a more complete theory is needed, involving bifurcations of a more general type than those of a gradient system.

This involves considering topological nature of attractors, and how topologically different attractors may nevertheless be considered as "thermodynamically" alike (a study in which Robert Williams has given very interesting results) the qualitative use of bifurcations in the interpretation of natural phenomena (known as Thom's catastrophe theory) will be continued, with particular reference to applied mechanics and theory of elasticity (Thompson-Hunt), and applications ranging from **[2]** organic chemistry (change of forms of molecules) to biophysics (discontinuous behavior of membranes), to geology (plate tectonics), and to social sciences (in the spirit of C. Zeeman's models).

(e) Singularities of polynomial mappings.

These occur in various domains of mathematics and science. Therefore their analytic, differential and topological properties are important to know. Much has been done and many interesting results have been obtained in this subject in the last couple of years, but even so it remains a wide open field. Going into details, in some domains of application we mention that with the help of specific singularities one has constructed objects such as exotic differential structures, group actions on manifolds, exotic piecewise linear structures, and knots. Actually, a lack of examples is an obstacle in the theory of four-manifolds and constructions with singularities are attempted. On this and related programs have worked and will work at the IHÉS : A'Campo, Brieskorn, Ehlers, H. King, Kirby, Lojasiewicz, Looyenga, Moisheson, Pinkham, Sebastiani, Siebenman (Orsay) and Siersma.
