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"

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[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,
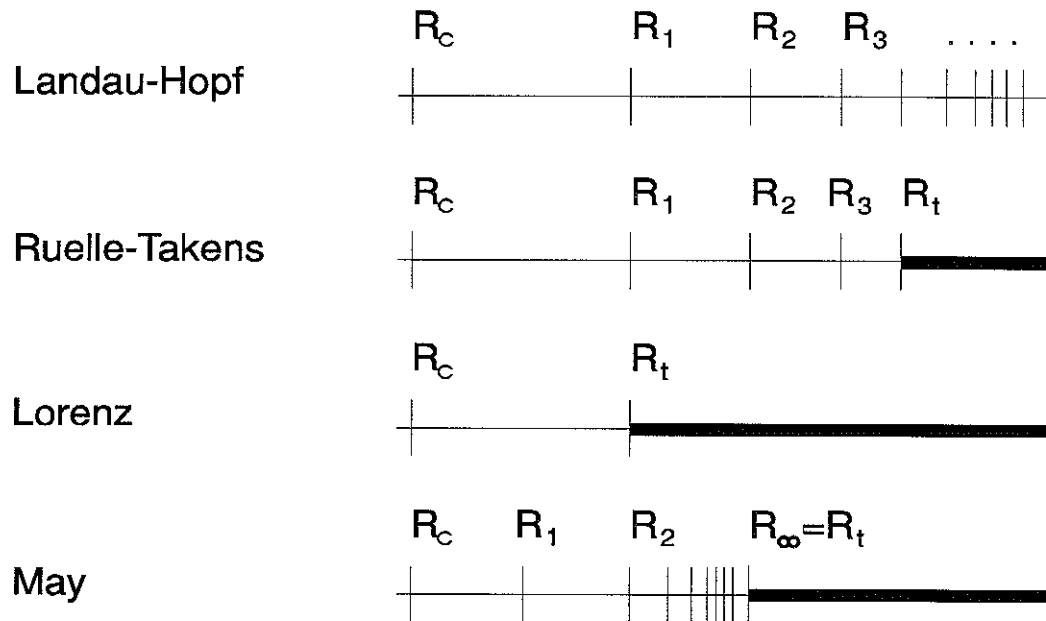Pioneer in Avation and Pathfinder in Space* (Boston: Little, Brown, 1967), 139.

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

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[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

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[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
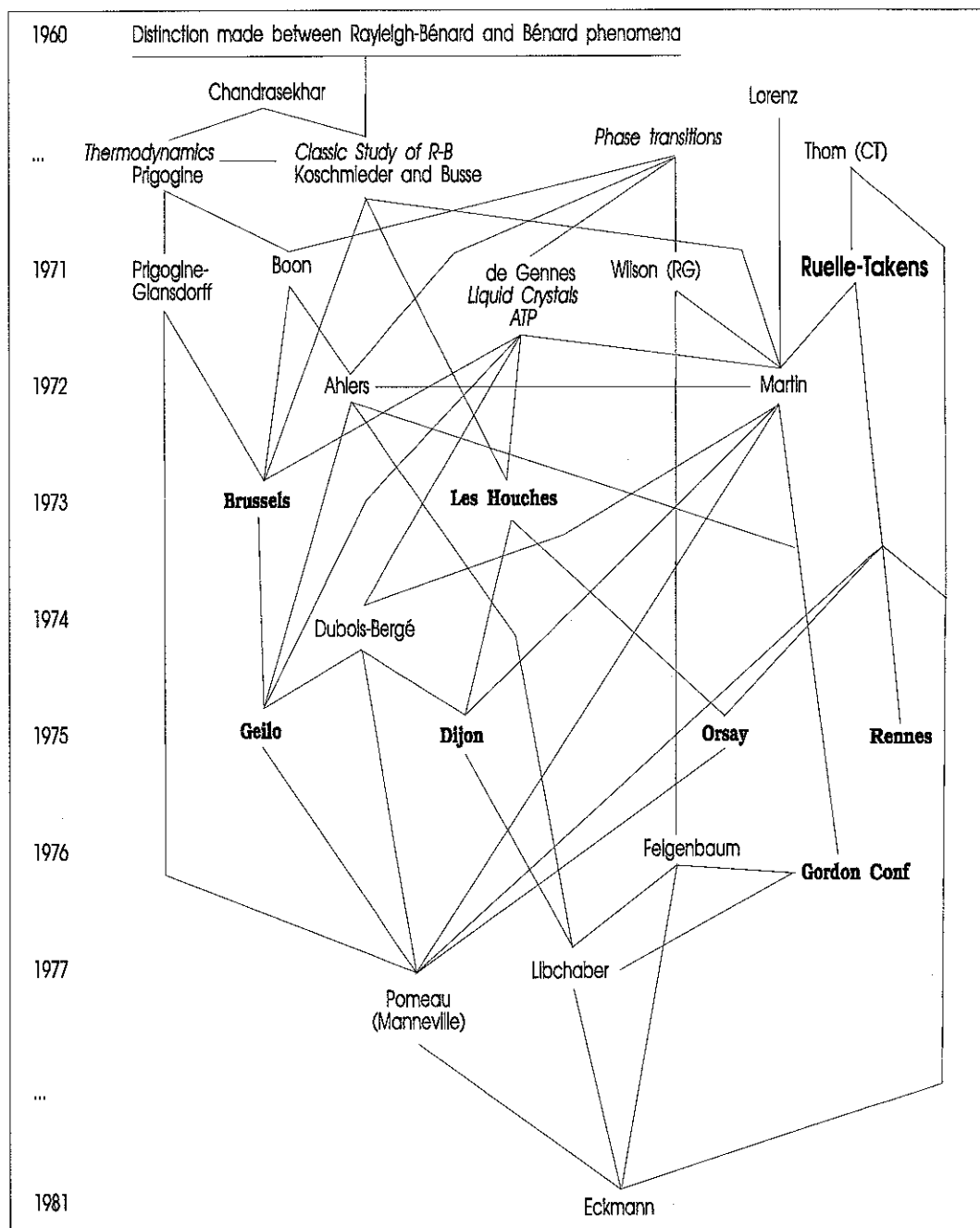
story.

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

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[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:*

$$Ra = \frac{g\alpha d^3 \Delta T}{\kappa \nu};$$

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[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 ~$0.74R_c$, the third at
> ~$0.85R_c$. 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:

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[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: $Pr = v/\kappa$, where v 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

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[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

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[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

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[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

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[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

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[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

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[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

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[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

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[99] P. C. Martin, "Instabilities," C1-66.