successful effort, research programs that would prove congenial both to the forging of links with other groups working on hydrodynamic instabilities and phase transitions and the reception of the Ruelle-Takens model. This chapter will end by examining the role Ruelle himself and the IHÉS played in this story, and by looking at the way new experiments and syntheses made by physicists ended up in an adaptation of the IHÉS modeling practices that differed importantly from the original ones.

## 4. <u>HYDRODYNAMICAL INSTABILITIES AND TURBULENCE IN FRANCE, 1971-1975</u>

### a) Three conferences in France

The Dijon Symposium was not an isolated event. In the course of the summer 1975, three conferences took place at various places in France dealing in part with hydrodynamics and turbulence. A sign of renewed interest in classical physics, these meetings expressed a feeling that something new was happening with these topics. This clearly stated feeling seemed to be shared by many communities of scientists. And the ultimate goal of establishing channels of communication across disciplinary boundaries ran across all these meetings. In 1975, however, it might have been somewhat difficult to see what any of the three conferences might have had to do with the others.

On June 12-13, a workshop on "Turbulence and the Navier-Stokes Equations" was held at the University of Orsay. Organized by Roger Tenam, the Orsay workshop gathered mathematicians, physicists, hydrodynamicists, and in particular the stability

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[100] P. Bergé et al., *Order within Chaos*, xv.

theorists mentioned in Chapter VII, namely Joseph, Sattinger, and Iooss. A

mathematician specialized in the analysis of computer algorithms for the modeling of

hydrodynamic flows, Tenam thought that there had been "a strong renewed interest in

the mathematical aspects of Turbulence during recent years," as he wrote in his

preface. But he was not an unconditional promoter of the Ruelle-Takens model.

Indeed, that very same year, with the help of Ciprian Foias, he put forth views that

seemed "to redeem Leray's point of view on the occurrence of turbulence," showing

that if neither the Ruelle-Takens nor the Landau-Hopf pictures occur, then Leray's

scenario should occur.[101]

The Orsay workshop was organized by the Société Mathématique de France

(SMF), "in order to examine the present state of the subject. . . . This was the

opportunity of very stimulating interdisciplinary contacts." Although he was on the

organizing committee, Ruelle could not attend the Orsay Workshop, but nonetheless

contributed a piece in the published proceedings where, for the first time, he picked up

the Lorenz attractor. Among the other organizers, were C. Bardos (Nice), M. Craya

(Grenoble), U. Frisch (Nice), and J.-L. Lions (Collège de France, Paris). In addition,

session chairmen were P. Germain, G. Guiraud (Iooss's advisor), and J. Leray. Among

the lecturers, one notes Benoît Mandelbrot, and especially astrophysicist Michel

Hénon and plasma physicist Yves Pomeau who, on this occasion, studying the Lorenz

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[101] C. Foias and R. Tenam, "On the Stationary Statistical Solutions of the Navier-Stokes Equations and Turbulence," *Publications mathématiques d'Orsay*, N° 120-75-28 (1975). Jussieu Lib.

model, introduced a famous strange attractor that would soon be widely known as the

Hénon attractor.[102]

Between June 30 and July 4, the Société française de physique (SFP) held a

similar meeting in Dijon, mentioned previously in connection with the Paul Martin's

talk. Organized by A. Martinet, from the Solid State Physics group of Orsay, the

Dijon Symposium dealt with "Physical Hydrodynamics and Instabilities." Coupled

with a half-day on "Instabilities and Critical Phenomena," the Dijon Symposium

"provided," Martinet contended, "an occasion for exchange between the community of

physicists and that of fluid mechanists." Again, the "pluridisciplinary character" of the

topics raised was emphasized, as well as the objective of "assessing [*faire le point*] the

present state of knowledge on turbulence and the contribution of recent theories

describing instabilities."[103] Aside from Martin's talk, the rest of the Dijon Symposium

was devoted mainly to experimental studies of various aspects of turbulence and

instabilities in fluids.

Surprisingly perhaps, the Dijon Symposium had almost no overlap with the

Orsay Workshop. Only Auguste Craya, who was to die a few months later, had been

involved in both, and his prospect was bleak. "To speak about turbulence is difficult,"

he wrote.

> For more than a hundred years, the accumulated knowledge in this regard has
> above all been experimental. . . . Theoretical efforts on a few simple cases

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[102] R. Tenam, ed., *Turbulence and the Navier-Stokes Equations: Proceedings of the Conference held at the University of Paris-Sud, Orsay, June 12-13, 1975,* Lecture Notes in Mathematics, 565 (Berlin: Springer, 1976).
[103] Proceedings of the Dijon Colloque were published in the *Journal de physique, 37, Suppl.,* Colloque C1 ["Hydrodynamique physiques et instabilités"] (1976). Quotes are taken from Mrtinet's "Avant-propos."

should be pursued by using all the modern resources of mathematics and statistical mechanics; some abnegation will nonetheless be needed, for it does not seem that we are entitled to expect decisive breakthroughs to occur in the short term.[104]

The two conferences at Orsay and Dijon made blatant the chasm that separated

theoreticians from experimentalists in new studies of turbulence. As emphasized by

Paul Martin, the experimenters mainly came into this field by adapting the tools and

practices they had exploited with success for the study of critical phenomena, while

theoreticians at Orsay relied on techniques displaying a much wider array of

approaches.

Finally, in September, from the 14th to the 21st, an "International Conference

on Dynamical Systems in Mathematical Physics" was held at the University of

Rennes, in Brittany. Organized by G. Galleotti, M. Keane, and D. Ruelle, and

sponsored by the SMF, it welcomed a large delegation of scientists that had often

been associated with the IHÉS. The Rennes Conference was however much more

focused on statistical mechanics than turbulence.[105]

In all three conferences, a clear emphasis was put on interdisciplinarity.

Nonlinear phenomena, associated with macroscopic physics, were tackled by

mathematicians and physicists with a feeling that communication across disciplinary

boundaries was a necessary condition for achieving some progress. But tools used in

order to achieve an interdisciplinary understanding of turbulence were quite varied.

One of the few common concepts mentioned at all conferences were strange

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[104] A. Craya, "Turbulence," *Journal de physique,* 31, Colloque C1 (1975): 35-55.
Quote from pp. 35 and 54. Craya deceased on February 13, 1976.

attractors. Clearly something was going on. Can this have been a consequence of

Ruelle and Takens's model for the onset of turbulence and a growing recognition of

the usefulness of a "dynamical systems approach" for studying of nonlinear

phenomena? I think not.

## b)      An 'Action thématique programmée' on turbulence

In the prefaces of the Orsay Workshop and the Dijon Symposium, a common

acknowledgment indicates that there was a common force behind these two meetings

dealing with turbulence in an interdisciplinary fashion, but otherwise so different from

one another. The organizers of both conferences thanked the CNRS for having

sponsored their meetings as part of an "ATP" devoted to "Turbulence and

Instabilities." This particular program, in which Pierre-Gilles de Gennes was

importantly involved, provided the initial glue for the complex alliances that made it

possible for an important chaos constellation to emerge in France in the course of the

1970s—a constellation which would embrace the Ruelle-Takens model as a welcome

exemplar for their undertaking. In particular, it will become obvious that groups

working on liquid crystals important for drawing attention on the study of

hydrodynamic instabilities benefited from the possibilities offered by this ATP.

> *(i)      The VIth Plan, the CNRS, and the ATPs: Active Management of*
> *Scientific Research*

Here, ATP stood for "Action thématique programmée [programmed thematic action]."

They were a means established by the Centre national de la recherche scientifique

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[105] M. Keane, ed., *International Conference on Dynamical Systems in Mathematical*

(CNRS) in 1971, to give priority sponsorship to certain interdisciplinary domains of

research emphasized by the VIth Plan.[106] In 1970-1971, a group of physicists and

chemists had written a report on "The Study of Matter and Radiation" for the

Commission of the Plan. They expressed that research in physics (outside of nuclear

physics) and chemistry had been previously sacrificed to more pressing concerns.[107]

They pushed for great financial efforts to meet a few "objectives," especially

concerning material sciences (solids, liquids, gas, plasmas, etc.). For this, they

outlined vague research "programs" to be developed along a few "axes." One such

program was called "Fluid and Plasma Dynamics," described as "hydrodynamics,

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*Physics, Rennes 1975,* in *Astérisque,* 40 (Paris: SMF, 1976).

[106] Starting in 1946, the French Government had designed the Plan in order to cope with reconstruction after World War II and insure the economic and social development of the country. From 1953 on, the Plan included a chapter on scientific and technological research, which emphasized technological applications. In 1958, the Délégation générale à la recherche scientifique et technique (DGRST) was created in order to promote a coherent science policy at the highest level of the government and present its recommendation to the Commissariat général du Plan. On the history of the French Plan, H. Rousso, ed., *La planification en crises (1965-1985)* (Paris: Editions du CNRS, 1988); Richard F. Kuisel, *Le capitalisme et l'Etat en France. Modernisation et dirigisme au XXè siècle,* traduction française (Paris: Gallimard, 1984); P. Massé, *Le plan ou l'anti-hasard* (Paris: Gallimard, 1965). About science and technological policies, see, in particular, A. Prost, "Les origines de la politique de la recherche an France (1939-1958)," *Cahier pour l'histoire du CNRS,* 1 (1988): 41-62; J.-F. Picard, *La République des savants* (Paris: Fayard, 1990); R. Gilpin, *France in the Age of the Scientific State* (Princeton: Princeton University Press, 1968), chap. VIII; P. Papon, "Research Planning in French Science Policy: An Assessment," *Research Policy,* 2(1973): 226-245.

[107] "We must be conscious of the fact that the brutal freezing [*blocage*] of the last few years, above all the freezing in the authorizations of programs in light physics and chemistry has led university researchers to the brink of bankruptcy." DGRST, *Rapport de la Commission du 6e Plan, 1971-1974. Recherche,* tome 2 (Paris: La Documentation française, 1971), Chapitre I: "G.S. 1 - Etude de le matière et du rayonnement," 11-32. Fonds doc. CNRS. Quote on p. 16. The group insisted on the fact that many laboratories in "light physics and chemistry" were now in a "catastrophic situation" (p. 15).

physics of gases and plasmas, researches having controlled thermonuclear fusion in view." It included an ATP called "Instabilities and Random Phenomena in Liquids, Gases, and Plasmas," which was to receive 9 million francs out of a total of 15 MF devoted to the objective.[108]

The new orientations in science policy outlined by the VIth Plan in terms of objectives led the CNRS to introduce, in 1971, a new frame in which to push for the realization of these objectives: the "action thématiques programmées" (ATP). Since its inception, in 1939, the CNRS was supposed to "stimulate [*provoquer*], coordinate, and encourage researches in pure and applied science, . . . and especially to ease scientific researches and works concerning national defense and economy."[109] But it had found this role a hard one to assume.

At a symposium organized by these two agencies in 1975, Herbert Curien, an ex-General Director of the CNRS responsible for the creation of ATPs, explained that as far as management of research was concerned two attitudes were possible: the "passive" one, traditionally assumed by the CNRS, and the "active" one.

> The active way consists in sending out a few cubic decimeters to the outside . . . . by telling the laboratories: if you wish to do something in this domain, we are ready to help you, do you have offers to make to us? This is what we inaugurated with ATPs.[110]

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[108] DGRST, *Rapport de la Commission du 6e Plan, 1971-1974. Recherche*, 2, 14, and 26. For comparison the total annual budget of the IHÉS for 1971 was of the order of 2.5 MF.

[109] §3 of the article 1er of the décret creating the CNRS on October 19, 1939; quoted by A. Prost, "Les origines de la politique de recherche," 42.

[110] H. Curien, in *La Planification et l'administration de le recherche. Séminaire DGRST-CNRS, Gif-sur-Yvette, 3-4 juillet 1975*. Arch. CNRS, Fonds doc.

ATPs would provide the means for the CNRS to entice research groups in

tackling some of the Plan's objectives. It was a way for the administrators of the

CNRS to bypass the corporations of specialists in its different sections, so that a few

objectives would be tackled by loose interdisciplinary groups. With this goal in mind,

it appeared "that a certain rigidity of research structures and the traditional separation

[*cloisonnement*] of discipline required to complement the usual measures of financing

with more direct and selective modes of incitement."[111] In the eyes of the

administration, the promotion of interdisciplinary research themes imposed to

circumvent the corporate demands of specialists.[112]

The emphasis was put on the flexibility of the method, which was managed by

*ad hoc* committees, with members nominated by the administrators being in the

majority, and with other members delegated by the concerned sections of the National

Committee (regrouping representatives elected by the research personnel of the

CNRS). These committees received the task of designing specific ATPs, elaborating

general programs of research, and submitting them to the scientific community.[113] In

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[111] R. Chabbal and J. Gavoret, "Les actions thématiques programmées du CNRS en physique," typed manuscript (1971). CNRS Arch. G940035 LABOS, n° 16. See the published version, which does not contain the above quote, in *Courrier du CNRS,* 3 (January 1972): 37-40.
[112] J.-F. Picard, *La République des savants,* 255.
[113] The label ATP was indistinctly applied to the committees who worked on the "objectives" of the Plan, and to more specific actions undertaken under such "objectives." Typically, an ATP was to be in activity for about three years. In 1971, thirteen ATPs were organized with a total budget of about 15 MF. In 1974, the funds devoted to ATPs represented 10% of those affected to equipment by the CNRS, and 5% of the total funds spent by the CNRS, except for personnel. See R. Chabbal and J. Gavoret, "Les actions thématiques programmées"; CNRS, *Rapport d'activité 1971* (Paris: CNRS, 1971), 52-54; and Procès-verbal de la séance du Directoire (25 et 26/6/74). Point V. Arch. CNRS. G870168 SGCN n°1.

short, in 1971, the General director of the CNRS, Hubert Curien, described ATPs as

follows:

> ATPs, which we can represent to ourselves as "laboratories without walls,"
> were conceived in order to allow the better linkage of selected researches to a
> determinate finality, the anticipation over several years of the necessary funds
> for actions considered as having priority, [and] the orchestration, in a common
> effort of reflection and realization, of scientists eventually belonging to
> different disciplines.[114]

Reviewing the benefits derived from ATPs in 1974, Robert Chabbal, who

would become General Director of the CNRS in 1976, contended that they had

"encouraged researchers to change research subjects" and helped to organize

collective actions tightening links between laboratories. As a consequence,

"interdisciplinary themes have become more numerous and have expanded."[115]

Clearly, interdisciplinarity was seen as highly desirable.

Not before 1973 was a specific ATP undertaken to address the objective of the

Plan described above on "Instabilities and Random Phenomena in Liquids, Gases, and

Plasmas." However, one was organized on the theme: "Molecular Fluid" within the

objective "Materials."[116] Among the nominated members of the *ad hoc* committee was

Pierre-Gilles de Gennes, who was responsible for subcommittee A on the Physics of

Molecular Fluids.[117]

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[114] Quoted in R. Chabbal and J. Gavoret, "Les actions thématiques programmées," 40.
[115] Procès-verbal du Comité sectoriel II: Physique (19/9/74), 5. Arch. CNRS, G870168 SGCN n°4.
[116] CNRS, *Rapport d'activité 1971*, 54.
[117] Dossier général de l'ATP "Matériaux." Arch. CNRS, G950016 DPM n° 1.

*(ii)     Liquid Crystals: Analogies Get Real*

"The media have a tendency to present us (me and my colleagues of a similar type) as

jacks-of-all-trades [*touche-à-tout*], people able to pass through walls [*passe-*

*murailles*], jumping from one domain to another."[118] Indeed, the career of Pierre-

Gilles de Gennes was punctuated by a few spectacular changes of orientation.

A student of the École normale, de Gennes entered the CEA at Saclay in 1955

and worked with André Herpin on a thesis devoted to the study of magnetic materials

irradiated by neutrons. In 1961 he became assistant professor [*maître de conférences*]

at the University of Orsay and joined the Laboratoire de physique des solides. Then,

he, a theoretician, had "the naive idea of directing an experimental group on

*superconducting* metals."[119] In 1968, however, de Gennes and his collaborators

learned from a Russian paper about a completely different subject: *liquid crystals.*

> For us, it was a revelation. There were holes in there, fascinating things that
> the Russians had let go! A true gold mine!... Within a few months, everybody
> in the teams around us had realized that we had to exploit the vein.[120]

Constituted of elongated molecules sensitive to electromagnetic fields, liquid crystals

could exhibit different phases: isotropic, nematic, or smectic. They therefore provided

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[118] P.-G. de Gennes and J. Badoz, *Les Objets fragiles* (Paris: Plon, 1994), 167.
[119] P.-G. de Gennes and J. Badoz, *Les Objets fragiles*, 167. On the career of de
Gennes, see his recollections in *Hommes de science: 28 portraits,* interviewed and
photographed by M. Schmidt (Paris: Hermann, 1990): 81-89; G. Deutscher "De la
physique des solides à celle de la matière 'molle'," *La Recherche,* 22 (1991): 1478-
1479; and a special issue of *Science et vie,* hors-série n°192 (September 1995),
devoted to him. On the Orsay group on solid-state physics, see J. Friedel, "Le
laboratoire de physique des solides d'Orsay," *Courrier du CNRS,* 3 (January 1972):
22-26.
[120] P.-G. de Gennes and J. Badoz, *Les Objets fragiles,* 166-167. See pp. 71-87 for a
popularization of the physics of liquid crystals.

an excellent field of study in order to tackle simultaneously problems related to phase

transitions and hydrodynamic instabilities. This fact made collaborators of de Gennes

eager to participate in the 1973 Brussels conference on hydrodynamic instabilities and

dissipative structures organized by Prigogine.[121] In the case of liquid crystals, indeed,

the relation between phase transitions and hydrodynamic instability was more than a

mere analogy.

One place where phase transitions and hydrodynamic instabilities merged

naturally was in the study of the Rayleigh-Bénard convection problem in liquid

crystals.[122] A group of young experimenters recruited by de Gennes (Élisabeth

Dubois-Violette, Étienne Guyon, and Pawel Pieranski) attacked the problem. The

results of their work were spectacular, and in 1991, de Gennes won the Nobel Prize in

part for his contribution to the physics of liquid crystals. They did not miss the

practical consequences of this research. In 1973, Jacques Friedel, director of the Orsay

Laboratory, already was writing: "It is possible that [liquid crystals] could be

technically used for the display of data."[123] Generally speaking, however, the study of

the Rayleigh-Bénard problem in liquid crystals, like Chandrasekhar's work, amounted

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[121] P. Pieranski and É. Guyon, "Cylindrical Couette Flow Instabilities in Nematic Liquid Crystals," *Advances in Chemical Physics,* 32, ed. I. Prigogine and S. A. Rice: 151-161.
[122] É. Dubois-Violette, É. Guyon, and P. Pieranski, "Cristaux liquides nématiques," *Fluid Dynamics,* ed. R. Balian and J.-L. Peube (London: Gordon and Breach, 1977): 603-619; P. Pieranski and É. Guyon, *Physical Review A,* 9 (1974): 404; P.-G. de Gennes, *The Physics of Liquid Crystals* (Oxfod: Clarendon, 1974); É. Guyon, "Instabilities in Nematic Liquid Crystals," *Fluctuations, Instabilities, and Phase Transitions,* ed. T. Riste (New York: Plenum, 1975): 295-311.
[123] J. Friedel, "Le Laboratoire de physique des solides," 24.

to introducing an additional complexity into the problem, and not a direct study of the

instability itself.

The VIth Plan, and the ATP programs put in place by the CNRS, proved to be

well designed resources for establishing the Orsay liquid crystals group. They allowed

a new research group, which could insert itself only with difficulty into the existing

structure of the CNRS, to be created and to get the necessary funding for their

activity. As the chairman of subcommittee A on "Molecular Fluids," de Gennes's role

was to promote the study of original molecular fluids showing order at an

intermediary level, notably: polymers, lubricants, and mesomorphics and micellary

phases (including liquid crystals). He was therefore in a good position to distribute

money to researches on liquid crystals and establish a network of research groups

working on these problems. In 1971, 1972, and 1973, this ATP funded about ten

projects, for a total of about 800,000 F, per annum.[124] Moreover this was the occasion

of building close links between physicists, chemists, and specialists in the study of

macromolecules, which would prove useful for de Gennes's later work on

polymers.[125]

*(iii)    Les Houches 1973: Physicists and Turbulence*

On June 20, 1973, Jean Govoret, adjoint to the scientific director of the CNRS, wrote

to members of the ATP Committee on "Physics of Molecular Fluids," that their

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[124] CNRS, *Rapports d'activité,* 1971, 1972, 1973. Arch. CNRS, Fonds doc.
[125] J.-F. Delpech et al., "Les ATP en physique de base et mathématiques," *Courrier du CNRS,* 25 (July 1977): 34-42.

meeting would be held after a summer school devoted to fluid mechanics in Les

Houches.

> In the future, it is specified that molecular hydrodynamics will be addressed in
> a new ATP, but within the objective "Turbulence," rather than within the
> objective "Materials."[126]

Indeed, in the spring of 1973, it was decided that a new ATP be set in motion,

directly addressing the objective stated of the Plan concerning instabilities in fluids

and plasmas. On May 23, about fifty participants gathered in Paris, coming from the

following disciplines: fluid mechanics, condensed matter physics, atmospheric and

astrophysical turbulence, plasma physics, and numerical analysis.

> Eight exposés, and the debates they spurred, showed the similarities and
> specificities of researches in these different domains. In particular, *the*
> *researches effectuated on turbulence in fluid mechanics did not appear as*
> *being located at the center of other sectors' concerns,* contrary to what might
> have been thought a priori.[127]

As an outcome of this meeting, an ATP called "Instabilities in Fluids and

Plasmas" was put in place, which in 1975 would sponsor both the Orsay Workshop

and the Dijon Symposium. Its goals were stated to be:

> —— Development of diagnostic methods for small scales and corresponding
> development of techniques for the treatment of data.

> —— Realization and analysis of unstable or turbulent flows in original systems
> (superfluid $He_4$, metallic, nematic [liquid crystal], dielectric, or magnetic
> liquids).

> —— Numerical simulation and analysis of models.

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[126] Lettre de J. Gavoret aux membres du Comité ATP "Physique des fluides
moléculaires" (20/6/73), avec une note: "Conseil d'Objectifs des ATP Matériaux.
Compte-rendu de la réunion du 13 mars 1973). Dossier général de l'ATP "Matériaux."
Arch. CNRS G950016 DPM n°1.
[127] R. Chabbal and J. Gavoret, "Les ATP de physique au CNRS," *Courier du CNRS,* 9
(July 1973): 46-49, 47.

——Acoustic effects: interaction turbulence-sound; role of intermittency.[128]

With these goals, the Les Houches summer school, in which, in July 1973,

about 35 participants gathered on the footsteps of Mont Blanc, fitted perfectly.

Founded in 1954 by Cecile DeWitt, the school addressed a topic of classical physics

for the first time. Its stated intent was to draw the physicists' attention back to

hydrodynamic phenomena.

> The extraordinary progress of microscopic physics since the beginning of the
> century has somewhat overshadowed the developments of the physics of
> continuum and in particular fluid mechanics. Thus, in most countries, this area
> has evolved more or less independently, its links with the other branches of
> physics getting looser and looser while its technical applications widened.
> However, *the arbitrary and sterilizing character of such a rift . . . has become*
> *more and more obvious.*[129]

The reasons for convergence, organizers Roger Balian and Jean-Laurent Peube

stated, were that physicists encountered flows in many situations (laboratory

experiments in fluids, plasmas or condensed matter, geophysics, meteorology, and

astrophysics). At the same time, they contended, "one of the toughest problems of

fluid dynamics, *the theory of turbulence, has much progressed in the last years,* owing

to the applications of methods similar to those currently used in field theory or in

statistical mechanics; some *analogies with the quite recent theory of phase transitions*

are stimulating the interest of theoretical physicists working in this area."

They stressed "the special character of this Summer School: their interest for

fluid mechanics had led to Les Houches participants from quite varied fields, several

of them being experienced specialists [in other fields]." With the publication of the

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[128] R. Chabbal and J. Gavoret, "Les ATP de physique," 47.
[129] R. Balian and J.-L. Peube, "Preface," *Fluid Dynamics,* vii. My emphasis.

courses and talks of the summer school they expressed high aims, namely to

"contribute to fill the gap, unfortunately too frequent in traditional teaching, between

physicists and fluid mechanicists."[130]

Once again, a striking emphasis was put on new developments of theories of

turbulence. At the same time, however, Ruelle and Takens's model was almost totally

neglected by the participants. This underscores the fact that things were moving in

fluid mechanics, even before the Ruelle-Takens model was mobilized as a way to

organize new theoretical and experimental findings in turbulence. Indeed, the only

mention to be found in the published proceedings of either this model or Edward

Lorenz's, lies hidden in the course on the statistical theory of turbulence by MIT

applied mathematician Steven A. Orszag. Even there, Orszag did not explain what

these models were.[131] Using computer simulations, however, he explicitly exhibited

the "nature of 'random' solutions to (deceptively simple) differential equations."[132]

Obviously, grouping such diverse scientists, the Les Houches proceedings

remained quite eclectic, and applied mathematician H. Keith Moffat, from Cambridge

University, presented a picture of the fluid mechanicist's worldview which

conspicuously excluded physicists (see Fig. 17).[133] Among the seminars presented at

Les Houches, let us note: Manuel G. Velarde's extensive review of the Rayleigh-

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[130] All quotes above are from R. Balian and J.-L. Peube, "Preface," *Fluid Dynamics*, vii-viii. My emphasis.
[131] S. A. Orszag, "Lectures on the Statistical Theory of Turbulence," *Fluid Dynamics*, ed. R. Balian and J.-L. Peube (London: Gordon and Breach, 1977): 235-374, 329.
[132] S. A. Orszag, "Lectures on the Statistical Theory," 249-250.
[133] H. Keith Moffat, "Six Lectures on General Fluid Dynamics and Two on Hydromagnetic Dynamo Theory," *Fluid Dynamics*, ed. R. Balian and J.-L. Peube: 149-233, 154.

Bénard problem, repeatedly mentioned above; a talk by the Orsay group on liquid

crystals; and a lively presentation by Benoît Mandelbrot, who introduced his notion of

fractional dimension.[134] Among the other subjects raised, were experimental and

numerical methods in fluid dynamics, electrohydrodynamics, critical fluctuations,

superfluid helium, stellar dynamics, and dynamical meteorology (in which, following

Lorenz, the question of the limits to predictibility owing to the nonlinear nature of

turbulence was raised).[135]

> *(iv)*    *De Gennes's Program: Let Physicists Take Over Fluid Mechanics, Part II*
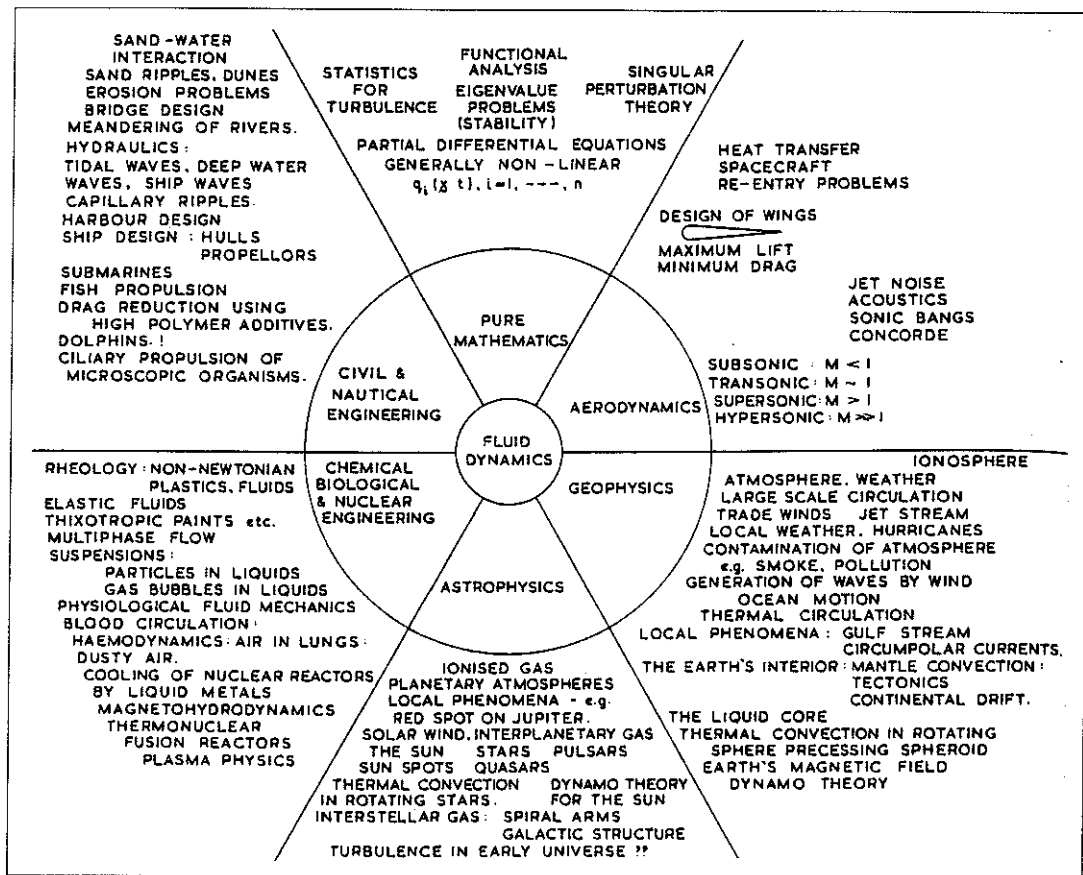
In the fall following the Les Houches summer school, Pierre-Gilles de Gennes, who

had participated, decided to devote his 1973-1974 course at the Collège de France to

"physical hydrodynamics."

> It is indeed important, at this time, to *tighten the contacts between [fluid] mechanists and physicists:* the latter can contribute to the study of flows in different ways: a) by experimental methods, notably for the measure of

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[134] B. Mandelbrot, "Physical Objects with Fractional Dimensions: Seacoasts, Galaxy Clusters, Turbulence and Soap," *Fluid Dynamics,* ed. R. Balian and J.-L. Peube: 555-578. At the same as he was giving his lectures at Les Houches in 1973 and at Orsay in 1975, Mandelbrot had been invited to speak at the Collège de France: "Nouvelles formes du hasard dans les sciences" on 13/1/73, mentioned in *Annuaire du Collège de France,* 73 (1973): 639; "1. Objets physiques de dimension fractionnaire: Côtes – Galaxie – Turbulence – Savon;" and "2. Géométrie de la turbulence et intensité des bouffées" on 22 and 29/1/74, mentioned in *Annuaire du Collège de France,* 74 (1974): 687. a series of lectures that would form the backbone of his famous book *Les Objets fractals,* published in 1975. See his recollections in *Mathematical People: Profiles and Interviews,* ed. D. J. Albers and G. L. Alexanderson (Boston: Birkhäuser, 1985): 207-225, 222.

[135] O. Talagrand, in collaboration with D. Anderson and M. Ghil, "Éléments de météorologie dynamique," *Fluid Dynamics,* ed. R. Balian and J.-L. Peube: 641-656, 655.

SAND -WATER
INTERACTION
SAND RIPPLES, DUNES
EROSION PROBLEMS
BRIDGE DESIGN
MEANDERING OF RIVERS.
HYDRAULICS :
TIDAL WAVES, DEEP WATER
WAVES, SHIP WAVES
CAPILLARY RIPPLES.
HARBOUR DESIGN
SHIP DESIGN : HULLS
PROPELLORS
SUBMARINES
FISH PROPULSION
DRAG REDUCTION USING
HIGH POLYMER ADDITIVES.
DOLPHINS. !
CILIARY PROPULSION OF
MICROSCOPIC ORGANISMS.

STATISTICS FOR TURBULENCE

FUNCTIONAL ANALYSIS
EIGENVALUE PROBLEMS (STABILITY)

SINGULAR PERTURBATION THEORY

PARTIAL DIFFERENTIAL EQUATIONS GENERALLY NON - LINEAR
$q_i(x\ t), i=l, ---, n$

HEAT TRANSFER
SPACECRAFT
RE-ENTRY PROBLEMS

DESIGN OF WINGS
MAXIMUM LIFT
MINIMUM DRAG

JET NOISE
ACOUSTICS
SONIC BANGS
CONCORDE

PURE MATHEMATICS

CIVIL & NAUTICAL ENGINEERING

AERODYNAMICS

SUBSONIC : M < 1
TRANSONIC : M ~ 1
SUPERSONIC : M > 1
HYPERSONIC : M ≫ 1

FLUID DYNAMICS

RHEOLOGY : NON-NEWTONIAN
PLASTICS, FLUIDS
ELASTIC FLUIDS
THIXOTROPIC PAINTS etc.
MULTIPHASE FLOW
SUSPENSIONS :
PARTICLES IN LIQUIDS
GAS BUBBLES IN LIQUIDS
PHYSIOLOGICAL FLUID MECHANICS
BLOOD CIRCULATION :
HAEMODYNAMICS - AIR IN LUNGS :
DUSTY AIR.
COOLING OF NUCLEAR REACTORS
BY LIQUID METALS
MAGNETOHYDRODYNAMICS
THERMONUCLEAR
FUSION REACTORS
PLASMA PHYSICS

CHEMICAL
BIOLOGICAL
& NUCLEAR
ENGINEERING

GEOPHYSICS

ASTROPHYSICS

IONISED GAS
PLANETARY ATMOSPHERES
LOCAL PHENOMENA - e.g.
RED SPOT ON JUPITER.
SOLAR WIND, INTERPLANETARY GAS
THE SUN    STARS    PULSARS
SUN SPOTS    QUASARS
THERMAL CONVECTION    DYNAMO THEORY
IN ROTATING STARS.    FOR THE SUN
INTERSTELLAR GAS : SPIRAL ARMS
GALACTIC STRUCTURE
TURBULENCE IN EARLY UNIVERSE ??

IONOSPHERE
ATMOSPHERE, WEATHER
LARGE SCALE CIRCULATION
TRADE WINDS    JET STREAM
LOCAL WEATHER, HURRICANES
CONTAMINATION OF ATMOSPHERE
e.g. SMOKE, POLLUTION
GENERATION OF WAVES BY WIND
OCEAN MOTION
THERMAL CIRCULATION
LOCAL PHENOMENA : GULF STREAM
CIRCUMPOLAR CURRENTS.
THE EARTH'S INTERIOR : MANTLE CONVECTION :
TECTONICS
CONTINENTAL DRIFT.
THE LIQUID CORE
THERMAL CONVECTION IN ROTATING
SPHERE PRECESSING SPHEROID
EARTH'S MAGNETIC FIELD
DYNAMO THEORY

**Figure 17:** The Fluid Dynamicist's View of the World. Repr. with permission from H. K. Moffat, "Six Lectures on General Fluid Dynamics and Two on Hydromagnetic Dynamo Theory," Fluid Dynamics, ed. R. Balian and J.-L. Peube: 149-233, 154. Copyright © Gordon and Breach Publishing Co.

velocities and correlation, . . . c) by bringing in certain theoretical concepts stemming from different domains.[136]

De Gennes's actions at the Collège de France, from 1971 to 1975, make explicit the

way in which, in order to promote the idea that physicists and hydrodynamicists might

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[136] *Annuaire du Collège de France,* 74 (1974): 77-79. My emphasis. Point (b) was dealing with the physicists' theories and experiments on liquid crystals and superfluidity. M. Dubois said that around 1975, the problem of the transition to turbulence "changed hands" from the hydrodynamists to physicists. "Modèles du chaos déterministe," *Modèles, modélisations, simulations (Histoire, Épistémologie, enjeux actuels),* symposium organized by Amy Dahan-Dalmedico (Paris, 22/4/97).

have something to share with one another, the Ruelle-Takens model could be

mobilized. In 1970, de Gennes was nominated to the Collège de France in

replacement of Jean Laval, whose chair of theoretical physics was renamed

"Condensed matter physics." His first course was titled "Broken Symmetries and

Phase Transitions." Among the phenomena studied was the instability of convective

flows, and in particular the Rayleigh-Bénard phenomenon. This however was a rather

marginal aspect of his outline. In the same year, de Gennes's invited Pierre Bergé, an

experimenter he knew from the CEA to speak about "Fluctuations in Liquids."[137]

While topics remained far from what chaos theory, links that would become important

for its future development were thereby being forged.

Around those years, after Kenneth Wilson's theory became fashionable, de

Gennes directed his attention to critical phenomena and phase transitions especially in

relation with liquid crystals. In his course at the Collège de France, de Gennes did not

come back to hydrodynamic phenomena *per se* until a couple of years later. But when

he did, he had formed an ambitious program.

De Gennes conceived his 1973-1974 course on "physical hydrodynamics" as

the continuation of the summer school at Les Houches, where, he wrote, "an excellent

session in hydrodynamics [had been] directed towards physicists." True to his focus

on liquid crystals, however, de Gennes raised one particular theme that was quite far

from the approach suggested three years earlier by Ruelle and Takens. Special

attention was paid to "making the link between molecular description and global

mechanical properties." Turbulence was not the goal of his inroad into fluid

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[137] On November 11, 1971. *Annuaire du Collège de France,* 72 (1972): 65-73.

mechanics. "Discussion was limited to viscous flows at small speed (. . . no

turbulence)." But again, Bergé's work assumed an important place assumed in the

design of de Gennes's course. "As an example, the locomotion of microorganisms

(bacteria, spermatozoids, etc.) was discussed," an experiment performed by Bergé and

Dubois at the CEA.[138]

The following year, the phenomenon of turbulence more than ever became an

integral part of de Gennes's program.[139] He wished to make an inventory of recent

optical methods for the study of flows and of the methods for the generation and

detection of turbulence. He noted that the work of Paul C. martin had indicated

"interesting analogies between developed turbulence and phase transitions." In

January 1975 de Gennes addressed the problem of the onset of turbulence at the

Collège de France. Thanks to Paul Manneville who kindly provided me a copy of the

personal notes he then took, we can have a glimpse at the way de Gennes tackled the

problem.[140] In particular, we can see how the Ruelle-Takens model got integrated to

de Gennes's view of the onset of turbulence—"an interesting pb [problem]," wrote

Manneville, who was a member of the Orsay group and a later contributor to the

chaos theory of turbulence. His lecture notes of de Gennes's course are a fascinating

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[138] *Annuaire du Collège de France,* 74 (1974): 77. See Service de physique du solide et de résonance magnétique, CEA, Saclay, "Une nouvelle technique pour mesurer les mouvements au sein des fluides," *1975 Images de la physique,* suppl. to *Courrier du CNRS,* 16 (1975): 72-76. Arch. CNRS, Fonds doc. Among the seminars of de Gennes, one may note P. Bergé, "Nouvelle méthode de vélocimétrie optique" (25/1/74); É. Guyon and P. Pieranski, "Comparaisons de diverses instabilités convectives" (1/2/74); M. Dubois and P. Bergé, "Mesure des vitesses de convections naturelle dans l'instabilité de Rayleigh-Bénard" (7/6/74).
[139] *Annuaire du Collège de France,* 75 (1975): 81-82. His emphasis.

example of how the Ruelle-Takens model would become a useful resource for a

physicist's approach to the problem of turbulence.

Like Martin and Ruelle, in the same year, de Gennes started with the situation

where turbulence appeared as the result of a "cascade of instabilities," using the

paradigmatic case of the resistance a sphere opposed to the flow of water. He

introduced Landau's model, noting the analogy with phase transitions. He emphasized

the possibility of computing critical exponents and, using Bergé and Dubois's

experiment (see below), mentioned more or less satisfactory experimental

confirmations for the Rayleigh-Bénard flow.

He then criticized Landau's scheme, pointing out that many variables of the

system (amplitude and phase of the disturbances) could not be specified unless "one

knows everything" about the system. "In fact, the image of reality is more

complicated." Before he extensively tackled the Rayleigh-Bénard problem, de Gennes

spent some time explaining the analogy between hydrodynamic instabilities and the

phase transition theory of Landau and Ginzburg. Turning to Rayleigh-Bénard *per se,*

he commented on Busse's theory and used the experiments of Krishnamurty, Willis

and Dearsdorff, and Ahlers, pointing out that these experiments had the inconvenience

that the flow could not be visualized, making difficult to tackle problems with

Rayleigh-Bénard systems related to their critical exponent and the spatial structure of

rolls. De Gennes also pointed out that liquid crystal systems might be advantageously

used to study the Rayleigh-Bénard instability.

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[140] I thank Paul Manneville to have provided me a copy of his personal lecture notes
taken for de Gennes's course in 1974-1975.

But it seems that de Gennes's lecture was designed with the goal of discussing the Ruelle-Takens model so that one could see how it might be called upon for the interpretations of recent experimental results. De Gennes's course is one the first accounts of the Ruelle-Takens model that took seriously the dynamical systems modeling practice of its authors. Using their analysis, de Gennes concluded, with them, that "the multiperiodic case is exceptional; the situation envisioned by Landau is not realized." De Gennes moreover emphasized that this entailed a new "operational definition of turbulence," which had two sides. Theoretically, for Ruelle, when the attractor was a fixed point or an orbit, the flow was said to be nonturbulent; if, on the contrary, it was attracted to a strange attractor, then it was turbulent. Most importantly, de Gennes drew the experimental consequences of this "operational definition."

- experimentally, if one measures a discreet spectrum: 1 freq. + harmonics [then the flow is] nonturbulent;

- [if one measures a] continuous spectrum [then it is] turbulent.

"How to differentiate experimentally the continuous spectra from a multiperiodic spectrum[?]" de Gennes asked. By the "clear dominance of a few peaks." With this, de Gennes had outlined an experimental program that could unambiguously differentiate the model proposed by Ruelle and Takens from Landau's. As opposed to the IHÉS scientists, in order to achieve this, he did not rely on mathematical arguments, which he had clearly presented, but on experiments. Only experiments could decide whether the model was valid or not! In view of the close association of Bergé and Dubois with

de Gennes, it is not surprising that they would soon endeavor to find these continuous

spectra in their Rayleigh-Bénard experimental setups.

De Gennes concluded his two-year series of lectures on hydrodynamics in the

following terms:

> At the end of two years devoted to hydrodynamics, we can establish this
> temporary assessment: on some points like rheology of polymers or the flow
> of liquid crystals, useful progress has been accomplished. The attention of
> physicists has been drawn to a certain number of open hydrodynamic
> problems. But exchanges with fluid mechanists have still remained too
> fragmentary; a long effort of cooperation and research still needs to be
> made.[141]

Indeed, the ambitious ATP of the CNRS devoted to the interdisciplinary attack on

phenomena of "Instabilities and Turbulence in Fluids and Plasmas" had meanwhile

turned sour.

*(v)       Disputes and Disappointment: Interdisciplinarity is not an Easy Task*

Set in motion in May 1973, the ATP "Instabilities and Turbulence" was examined by

the various concerned sections of the National Committee at the end of 1974.

Constituted of elected representatives from the various scientific sections of the

CNRS, the National Committee was intended as the place where intellectual decisions

were taken by the CNRS researchers themselves.

Not surprisingly, the ATP on "Instabilities" was favorably discussed by de

Gennes, one of its instigators, in Section 08 (solid-state physics).[142] Similarly, the

discussion that took place in Section 04 (Mechanics) in December 1974 was quite

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[141] *Annuaire du Collège de France,* 75 (1975): 82.
[142] Procès-verbal du Comité national du CNRS (Section 08: Physique des Solides)
(1/10/74). Arch. CNRS G870168 SGCN n°6.

positive. They even noted that the amounts allotted to this ATP were below the

envelop devoted to it by the CNRS. However, a concern was expressed to the effect

that a new program was to be written because the previous one was "too vast." The

committee emphasized that "the accent should be put on studies tending to bring new

and fundamental elements to the understanding of mechanisms."[143]

Representatives of Section 02 (theoretical physics) were much more critical of

the project. Examining the ATP President Meyer "notices the considerable amounts

allotted to *a subject of limited interest,* and the small control [they themselves] had on

these funds." It was stressed that the problem of turbulence had hardly evolved for the

last 25 years. Worries were expressed concerning the line of demarcation between

physicists and fluid mechanists. To these objections, Robert Chabbal, in charge of the

ATP program, declared: "If this ATP in no way contributes to favor theoretical

research [on this topic], then this is an argument to stop it." At the same time,

however, the section of the National Committee deplored what they called "the inertia

of researchers of their discipline vis-à-vis ATPs."[144]

In 1974, the ATP on "Instabilities" had sponsored 13 projects for a total

amount of 1,460,000 F.[145] True to the ATP's intent of sponsoring the acquisition of

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[143] Procès-verbal du Comité national du CNRS (Section 04: Mécanique) (2/12/74). Arch. CNRS G870168 SGCN n°6.

[144] Procès-verbal du Comité national du CNRS (Section 02: Physique théorique) (15, 16, et 17/10/74). My emphasis. Arch. CNRS G870168 SGCN n°6.

[145] Documents prépatoire du Directoire des 25 et 26 juin 1974, Tableau 7. Arch. CNRS, G870168 SGCN n°1. Note however that the *Rapport d'activité* 1973, p. 99, mentions 19 contracts for a total amount of 1,497,500 F. Fonds doc.

equipment, projects were mostly experimental.[146] A clear diversity of projects was

funded by the ATP. In 1975, the ATP again serve to sponsor 9 projects, for a total of

1,203,000 F.[147] Among a similar diversity of projects, ranging from meteorology to

plasma physics, from superfluidity to shock dynamics, a contract was given to Guyon

for a study of the "hydrodynamic instability in liquid crystals." Clearly, the committee

for this ATP seemed to have forgotten that it was supposed to have a "determinate

scientific finality."

When at the end of 1975 the different sections of the National Committee met,

the representatives for the Mechanics Section seemed to be the only ones who were

still happy with the ATP on instabilities. In other sections, a feeling of disappointment

was clearly expressed. In October, at the meeting of Section 08,

> M. de Gennes declares that he is not very satisfied with this ATP because it
> gathers: Classical Mechanicians, Physicists, Plasma Physicists, plus a few
> Astrophysicists. . . . There are too many different specialties and the pre-
> colloquium gave rise to violent disputes. In the future, heavier structures [*sic*]
> should not be renewed.[148]

Similarly, in Section 03 (electronics), the ATP was deemed a

"disappointment." It was an "enterprise lacking in maturity. . . . However, it was worth

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[146] They came from laboratories specializing in fluid dynamics at Nice (Frisch), Orsay
(Lelièvre), Grenoble (Craya), and Lyons (Comte-Bellot, Courseau), but also from
plasma physics (D. Grésillon at Paris, Mantei at Orsay) and astrophysics (Roddier at
Nice) laboratories. One may even note a contract with Quemada on biological
applications, and one with Valentin (Rouen), titled "Applications of laser-Doppler
anemometry to the measurement of velocities in plasma." A good source for the
content of these project is to be found in the proceedings of the Dijon Symposium,
cited above. *Journal de physique*, 37, *Suppl.*, Colloque C1 (1976).
[147] Documents prépatoire du Directoire des 30 juin, 1er et 2 juillet 1975, Tableau
n°123. Arch. CNRS, G870168 SGCN n°1. Note however that the *Rapport d'activité*
1974, p. 66, mentions 10 contracts for a total amount of 1,353,000 F. Fonds doc.

trying to do something in this domain." He moreover reported that there were

"internal quarrels" within the committee which led it to accept projects that were

criticized.[149] Finally, in Section 02, it was reported that the coordination of the

projects had remained difficult. "M. Winter regrets that he was too ambitious by

associating researchers that were too different." The fact that it would be stopped

could only bring a bettering of the situation. Meyer sugested that in the future

plasma/astrophysics and fluid mechanics/turbulence be distinguished.[150]

In these conditions, the ATP "Instabilities and Turbulence in Fluids and

Plasmas" was abandoned in 1976. The disagreements expressed in the ATP

Committee may be taken as a hint for the reasons why the Orsay Workshop and the

Dijon Symposium of the summer of 1975, albeit similar in intent, nonetheless looked

so different. Similarly, although a Seminar of Physical Hydrodynamics nonetheless

survived in de Gennes's goup in 1976, he himself more or less dropped this topic from

his future courses at the Collège de France..[151]

No matter how disappointing the ATP on "Instabilities and Turbulence"

proved to be in action, it had succeeded in attracting the attention of a few physicists

to problems of hydrodynamic instabilities. It had favored a recognition that the

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[148] Procès-verbal du Comité national du CNRS (Section 08: Physique des Solides) (6, 7, 8 et 9/10/75). Arch. CNRS G870168 SGCN n°6.

[149] Procès-verbal du Comité national du CNRS (Section 03: Électronique) (25, 26, 27 et 28/11/75). Arch. CNRS G870168 SGCN n°6.

[150] Procès-verbal du Comité national du CNRS (Section 02: Physique théorique) (9, 10 et 11/12/75). Arch. CNRS G870168 SGCN n°6.

[151] Among the talks presented, we may note: Pierre Bergé, "Instabilité de convection" (26/5/76), Yves Pomeau, "Les attracteurs étranges. Intermittence" (31/5/76), and one by Étienne Guyon on the same day, and finally, Monique Dubois, "Instrumentation nouvelle" (7/4/76). *Annuaire du Collège de France,* 76 (1976).

Ruelle-Takens model might be a useful resource for experimental physicists who had

invested time and energy in the problem in order to account for their results by

theoretical means. The ATP had moreover brought the Lorenz model to the fore. It

had allowed a clear experimental program to test these models to be undertaken. The

ATP and de Gennes's course at the Collège did not solve the turbulence problem but

they put scientists coming from a variety of backgrounds in contact with one another.

The focus on instabilities and turbulence in fluid has moreover strengthened the

contacts of French physicists with other people who were concerned with

hydrodynamic instabilities and the Rayleigh-Bénard problem in particular, as is

witnessed by a conference held in Norway in 1975.

**c)      Geilo 1975: The Emergence of an International Community?**

Throughout the 1970s, a series of six NATO Advanced Study Institutes on phase

transitions and instabilities were held in Geilo, Norway. Mostly they were organized

by Tormod Riste, from the Institutt for Atomenergi in Kjeller. When reflecting back

on the accomplishment of this series of conference, Riste and his co-organizers wrote

in 1981:

> Ten years ago, at the first Geilo school, the report of a central peak in the
> fluctuation spectrum of $SrTiO_3$ close to its 106 K structural phase
> demonstrated that the simple . . . theory of such transitions was incomplete.
> The missing ingredient was the essential nonlinearity of the system.[152]

From April 11 to 20, 1975, 70 physicists, most from Europe, gathered for the

third Geilo Institute, which was devoted to "Fluctuations, Instabilities, and Phase

Transitions."[153] Experimenters who had recently adapted light-scattering methods

from the study of phase transitions to that of hydrodynamic instabilities, in France and

the US, as well as Günter Ahlers joined scientists from the Orsay liquid crystal group,

and from Prigogine's School in order once again to study the "analogy" between phase

transitions and instabilities in nonequilibrium systems. One of the main examples of

course was "instabilities in hydrodynamic systems, for which one may draw on the

very rich experimental material obtained by hydrodynamicists." Here again, the intent

of drawing the physicists' attention to such phenomena was clearly stated:

> It is the hope of the program committee that this institute may have created an
> interest among the participants to make a wider use of the powerful tools of
> modern physics in such studies. . . . The full span of the program is introduced
> by de Gennes' lecture on phase transition and turbulence.[154]

Indeed, de Gennes explained in detail the analogy between phase transitions

and, now, turbulence, and not only hydrodynamic instabilities. In a two-part talk, he

introduced "the general ideas and the theoretical methods which have been most

fruitful for phase transitions," as well as "salient facts about turbulence." Always with

the same goal, he added: "I hope that [these views] can help to bridge the gap between

the two schools of thought, [namely: "the fraction of this audience who works

primarily on fluid mechanics" and the "physicists"]."[155] Like Gérard Toulouse, de

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[152] H. Z. Cummins, E. H. Hauge, J. G. Feder, R. Pynn, T. Riste, and H. Thomas,
"Preface," *Nonlinear Phenomena at Phase Transitions and Instability,* ed. T. Riste
(New York: Plenum, 1982), vi.

[153] T. Riste, ed., *Fluctuations, Instabilities, and Phase Transitions: NATO Advanced
Study Institute, Geilo, Norway, April 1975* (New York: Plenum, 1975).

[154] T. Riste, "Preface," *Fluctuations, Instabilities, and Phase Transitions,* ed. T. Riste,
v.

[155] P.-G. de Gennes, "Phase Transitions and Turbulence," *Fluctuations, Instabilities,
and Phase Transitions,* ed. T. Riste: 1-18, 1.

Gennes emphasized universality as a way to bring forth the analogy: "the details of

the atomic (or molecular) structure become unimportant: the behavior near [the

critical temperature] is to a certain extent *universal.*"[156]

In this lecture, de Gennes emphasized even more than at the Collège the

importance of the Ruelle-Takens model. "A precise definition of turbulence is not

easy to find," he acknowledged, "but the following features seem to be essential:" (a)

rapid and not uniform flows; and (b) "*stochastic character,* which is not due to

external noise sources, but which is an intrinsic consequence of the non linearity in

the hydrodynamic equations." As we have seen, this was an essential conclusion at

which both Prigogine and Ruelle had arrived. De Gennes went on, citing Lorenz's

model as an example:

> The stochastic character already appears in nonlinear systems with a *small*
> number of degrees of freedom, where numerical studies are relatively easy to
> perform, and can in fact be found in the literature of very different fields—
> celestial mechanics, accelerator physics, electronics, population dynamics, etc.
> . . . Whenever the trajectories are truly non periodic as they seem to be [in the
> Lorenz system] *we shall call the flow turbulent*—following a definition
> expressed (more rigorously) by Ruelle and Takens.[157]

But de Gennes's goals remained those of a physicist studying phase transitions, and

not those of a mathematician attempting a classification of dynamical systems: "is

there a scaling law of the form $\cong (\Delta T - \Delta T^*)^{-x}$ where $x$ is some fixed exponent inside

one universality class?"[158]

As emphasized above by Martin in particular, the most important consequence

of the analogy between phase transitions and turbulence was *not* theoretical, but rather

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[156] P.-G. de Gennes, "Phase Transitions and Turbulence," 3.
[157] P.-G. de Gennes, "Phase Transitions and Turbulence," 9-10. My emphasis.

experimental. The Geilo Institute provided an occasion for Jerry Gollub, Günter

Ahlers, Pierre Bergé, and Monique Dubois to meet and compare their experimental

results. While all computed critical exponents, they realized that by using light-

scattering methods they could achieve local measurements of great accuracy. This

enabled them to go beyond the "linear domain," corresponding to Rayleigh numbers

slightly above the critical value and which seemed to be well understood theoretically,

and to attack the "nonlinear domain" where classic theories were much vaguer.[159] This

was the domain where the Ruelle-Takens provided clues to the theoreticians about

how to tackle it and to the experimenters about what to look for.


## 5.      EPILOGUE: BEYOND RUELLE-TAKENS

On February 6, 1975, Ruelle went to address physicists of the CEA, at Saclay, a

research center south of Paris which was hardly more than ten minutes away from the

IHÉS by car. He spoke on "The Problem of Turbulence."[160] Having met Swinney and

Gollub in 1974, Ruelle was now talking to their closest colleagues in France: the

experimenters Pierre Bergé and Monique Dubois who after having studied

fluctuations at critical points had started to tackle the study of convection.[161] By 1975,

therefore, it seems that a chaos constellation had already emerged. It had been built

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[158] P.-G. de Gennes, "Phase Transitions and Turbulence," 10-11.
[159] G. Ahlers, "The Rayleigh-Bénard Instability at Helium Temperature;" J. P. Gollub and M. H. Freilich, "Critical Exponents and Generalized Potential for the Taylor Instability;" and P. Bergé, "Rayleigh-Bénard Instability: experimental Findings Obtained by Light Scattering and Other Optical Methods," *Fluctuations, Instabilities, and Phase Transitions,* ed. T. Riste: 181-193; 195-203; and 323-352.
[160] *Rapport scientifique 1975. Voyages et publications,* 5. Arch. IHÉS.
[161] Ruelle's visit to Swinney and Gollub is briefly mentioned in J. Gleick, *Chaos,* 131, and 150.

upon foundations that had to do with common concerns for instabilities in fluid, for which the models of Ruelle-Takens and Lorenz, but also analogies and tools coming from phase transitions, were useful resources for the development of a common language. However, this constellation was still far from having synthesized these loose analogies into a rigorous, or at least respectable, theory, to use Domb's categories. The theoretical analogies with phase transitions had proved disappointing, but in 1975 a "dynamical systems approach" had hardly been uniformly adopted by all members of the chaos constellation.

From 1975 to the early 1980s, a few French physicists, in contact with other scientists in the United States and Europe, would build a more complete deterministic theory of the onset of turbulence, and provide firm experimental bases for it. Being general, this theory would not have the achieved character of many traditional physical theories, but being general, it would be successfully transferred to many other phenomena, starting with oscillating chemical reactions which had been a focus of Prigogine's School. By the early 1980s, chaos theory had been established as a major field of research in physics. The common theoretical language and the modeling practices that were found to be best suited for this endeavor would be greatly inspired by those of dynamical systems theorists, such those at the IHÉS.

The process by which these practices and language came to occupy the foreground certainly deserves more space that can be devoted to it here. In the following, this story will be concluded by providing snapshots of a few career trajectories. A few French physicists would play an important role in making the

switch from phase transition analogies to dynamical systems. In particular, Monique

Dubois and Pierre Bergé, Yves Pomeau, and Albert Libchaber all used the Rayleigh-

Bénard system as one of their main objects of study. In a large part, it finally was as a

consequence of their work that a language and modeling practice derived from

dynamical systems theory were adapted to the study of a wide array of physical

systems.

This "epilogue" first rules out the view that Ruelle and the IHÉS played a first-

rank role in that process, mainly because their programs remained little affected by

experiments. Then it will provide a few snapshots of French physicists' careers in the

later part of the decade in order to show how, even if they adopted many of the tools

from dynamical systems theory, physicists however considerably adapted the

modeling practices that were examined in previous Chapters.

### a)     Ruelle and the IHÉS, 1970-1977

The following pays attention to the evolution of Ruelle's involvement in the emerging

field of chaos. This will underscore the fact that the physicists' reception of his model

had little to do with his own concerns. By bringing the same kind of attention to the

IHÉS, it will moreover be shown that, if the programs pushed forward by the Institute

in the 1970s definitely moved towards the study of turbulence and dynamical systems,

they remained within a mathematical tradition close to earlier concerns of the IHÉS

but which had little to do with the new emphasis put on experimental and numerical

results that so much shaped the outcome of chaos theory.

Significantly, throughout the early 1970s, Ruelle mainly chose to address

physicists, rather than mathematicians. But of course, he tried to convince those he

felt the closest to: that is, axiomatic quantum field theorists and mathematical

physicists who tackled the rigorous foundations of the statistical mechanics of

equilibrium. These were mathematical physicists who remained much more aloof

from experimental work than those, mentioned above, studying phase transitions and

critical phenomena.

At first, Ruelle's talks on turbulence and dissipative systems were mainly

given abroad. The first talk he gave in France outside of the IHÉS, on "Differential

dynamical systems and the problem of turbulence" was on December 19, 1973—three

and a half years after having written his paper with Takens! It was directed to the

physicists of the École polytechnique.[162] Prior to this, Ruelle had given many lectures

dealing with turbulence, but mainly outside of France where his professional

connections were the strongest. When speaking in France, he above all addressed

issues of statistical mechanics.[163]

### (i)      Ruelle's Picks up Lorenz

In 1975, a few months after having been to the CEA and right after a stay at Stanford

and the IAS at Princeton, Ruelle started to consider the Lorenz attractor. By then,

turbulence and dynamical systems definitely were high on his agenda. At La Jolla,

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[162] *Rapport scientifique. Voyages et conférences du directeur et des professeurs permanents de l'IHÉS en 1973,* 3. Arch. IHÉS. There might be a bias in my information, collected from the Scientific Report of the IHÉS, which emphasized "trips" made by its permanent faculty members, rather then "talks." The perception I present above nevertheless is consistent with other sources.

California, on July 17, 1971, David Ruelle and Edward Lorenz had participated to the

same session of a conference devoted to "statistical models and turbulence."

Apparently, although both of their papers published in the proceedings raised the issue

of sensitivity to initial conditions, they did not found much to exchange with one

another. In the proceedings of the 1975 Orsay Workshop, however, an article of

Ruelle's appeared under the title: "The Lorenz Attractor and the Turbulence Problem."

This was the text of a talk he had delivered at a symposium on "Quantum Dynamics

Models and Mathematics," at Bielefeld held in September 1975.[164]

This talk really marks the full-fledged return of Ruelle to the general study of

turbulence and dissipative systems. He gave a straightforward physical interpretation

of the model he had proposed with Takens a few years earlier: "putting a nonlinear

coupling between 4 or more oscillators can produce a 'turbulent' time evolution with

sensitive dependence on initial conditions."[165] This very property—sensitive

dependence on initial conditions—was emphasized by Ruelle as a general explanation

for "erratic, chaotic, or turbulent behaviors," a phrase he used many times in his

lecture.

Following private communications by Lanford and Bowen about Lanford's

and Guckenheimer's studies of the Lorenz system, Ruelle now decided to address this

model, albeit reinterpreting it as a model for the onset of turbulence, which it had not

been explicitly at the time it was proposed by Lorenz.

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[163] *Rapport scientifique 1974.* Arch. IHÉS.
[164] D. Ruelle, "The Lorenz Attractor and the Problem of Turbulence," *Turbulence and the Navier-Stokes Equations,* ed. R. Tenam (Berlin: Springer, 1976): 146-158.
[165] D. Ruelle, "The Lorenz Attractor," 147.

> Lorenz' work . . . is the first attempt at interpreting turbulence by solutions of differential equations which appear chaotic, and have sensitive dependence on initial conditions. The ideas of Lorenz and those of Takens and myself have recently received support from the theoretical work of McLaughlin and Martin and the experimental work of Gollub and Swinney. It can be hoped that more experimental results on the onset of turbulence in various systems will become available in the next few years; *their theoretical interpretation will constitute a worthy challenge for the mathematical physicist.*[166]

Mentioning Li and Yorke's preprint as being relevant to this study, Ruelle had put in place similar connections to those Martin had mobilized. Ruelle however remained much more skeptical of the traditional ways hydrodynamicists tackled the problem of the onset of turbulence. "I think it would be a miracle if the usual procedure of imposing stationarity, [and] truncating the resulting system of equations, would lead to results much related to physics."[167] His conclusion again was a condemnation of traditional views of the onset of turbulence:

> let me express my feeling that, after decades of misconceptions, we are beginning to have correct ideas on the time-dependence in turbulence near its onset.

Referring to a figure of Feynman's showing spatial features of turbulence, also used by Martin and de Gennes, Ruelle emphasized that there much work remained to be done.[168]

Although he had proposed a new model for turbulence, which was gaining ground, and although his concept of a 'strange attractor' was starting to be widely used, Ruelle appears, in 1975, just as much a follower of the interdisciplinary bandwagon of turbulence as any. Clearly, he had expressed views for the future

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[166] D. Ruelle, "The Lorenz Attractor," 147. My emphasis.
[167] D. Ruelle, "The Lorenz Attractor," 154.
[168] D. Ruelle, "The Lorenz Attractor," 154-155.

development of the field that seem to have been later realized, partly because of his

own work. But Ruelle did not have, at the time, a synthetic view comparable to

Martin's; and he still did not consider the theoretical work done on turbulence by non-

dynamical-systems specialists as being well oriented. He was not on the same

wavelength as most physicists who tackled the turbulence problem. That it was so is

made even more manifest by the directions the IHÉS chose to pursue in the course of

the 1970s.

*(ii)    IHÉS: 'Foreign in View of Some Frenchmen'*

By hiring David Sullivan in 1974, the IHÉS had oriented his domains of research still

more in the direction of dynamical systems theory. In fact, Sullivan was the first

specialist in this field to be appointed to the IHÉS. In his own words, he focused on

the "study of geometrical properties of spaces—in particular manifolds—in view of

understanding their geometrical and topological forms (flows, foliations, measures

and general dynamical phenomena)."[169]

Throughout the early 1970s, the Scientific Committee of the IHÉS had wished

to attract a new permanent faculty member. In June 1971, before he officially became

director, Nicolaas Kuiper wrote down general remarks about the choice of new

permanent members in mathematics.

> They should be extremely creative, original, and independent, as proved by
> their work and in particular, their theorems. . . . Their interest should be wider
> than just one restricted field of mathematics. . . . They should be powerful

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[169] Annexe to Lettre de N. Kuiper au Secrétaire d'État aux Universités (26/2/75):
"Orientations scientifiques pour la période 1976-1980 (VIIème Plan-Recherche)."
Arch. IHÉS.

mathematicians and stimulating to others. . . . To some extent the interests of the permanent members should cover different parts of mathematics.

Looking at the present composition of the faculty of the IHÉS, Kuiper wrote:

> I wonder whether further study should be made into the possibility of finding a permanent member in [analysis]: P. Lax, Möser, Malgrange, etc. I suppose Smale is not available.[170]

Indeed Smale had been considered. But Atiyah's judgment solicited by Zeeman did

not make this prospect seem likely:

> It is possibly unrealistic to consider Smale because he would not want to emigrate from America, for financial and other reasons. In spirit he is very close to Thom with a similar style and some overlap of interest. He is probably the most original mathematician on the list, with tremendous drive, and the ability of rushing where angels fear to tread.[171]

In 1971, however, the Scientific Committee decided, under Léon Motchane's lead, to

offer positions to Bombieri, Langlands, and Armand Borel, who all turned them

down. The next year, while it seemed clear that they had to start considering other

candidates, David Sullivan's name was for the first time proposed by Deligne.[172] An

offer was made to him in November 1973.

At the same time, the Scientific Committee resolved to build a program for

1974-1975 centered around "attractors and structure." The theme would be

"Dynamical systems, turbulence, statistical mechanics" and "attractors" would be

represented by Bowen, Sattinger, and Mather.[173] This marked an important change of

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[170] N. Kuiper, *General Remarks on the Choice of New Permanent Members (Mathematics)*. "To be considered as informative, not my final judgement." *Comité scientifique* (15/6/71). Arch. IHÉS.

[171] Report by E. C. Zeeman of a conversation with M. Atiyah. *Comité scientifique* (15/6/71). Arch. IHÉS.

[172] Compte-rendu du Comité scientifique (14/4/72). Arch. IHÉS.

[173] Comité scientifique (16/11/73), Compte-rendu (dated 26/11/73). Arch. IHÉS.

the attitude, since until then, as mentioned in Chapter VII above, little had been

coherently organized to promote these subjects at the IHÉS. This exactly coincided

with Ruelle's definite change in orientation. Outlining in February 1975 the scientific

orientations of the IHÉS for 1976-1980, Kuiper selected the qualitative theory of

differential equations as the main point needing emphasis. With three of its permanent

professors (Thom, Ruelle, and Sullivan) listing this topic among the research themes

they planned to pursue in the following years, this emphasis, Kuiper wrote, "can only

improve the already very satisfying spirit of cooperation which animates our

researches and strengthen the cohesion of fundamental researches in mathematics and

theoretical physics."[174] The way Ruelle then described his research agenda is

enlightening:

> The study of dissipative systems (for example turbulence); statistical
> mechanics of equilibrium (in particular quantum). Between these two kinds of
> questions, there exist unexpected mathematical relations.

Around 1974, that, in liaison with Orsay and the CNRS, the IHÉS acquired its

first calculating machine with a curve plotter.[175] It was met with "great success," but,

strikingly, only Pierre Cartier, a long-term visitor paid by the CNRS who studied

group theory, mentioned numerical calculations among his research concerns.[176] For

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[174] Lettre de N. Kuiper au Secrétaire d'État aux Universités (26/2/75): "Orientations scientifiques pour la période 1976-1980 (VIIème Plan-Recherche)." With an annex listing the permanent professors' future research themes. Arch. IHÉS.
[175] Comité scientifique (19/4/74). Arch. IHÉS.
[176] Annexe to Lettre de N. Kuiper au Secrétaire d'État aux Universités (26/2/75), 2. Arch. IHÉS.

specialists in the theory of dynamical systems, numerical studies would of course

have represented a drastic change of modeling and theoretical practices.[177]

In June 1976, Kuiper presented the Volkswagen Foundation in Hanover, with

a research program to be sponsored by them for the following three years.[178] Kuiper's

own program illustrates the fact that the problems of dynamical systems theory that

the members of the IHÉS then thought of as being the most promising remained in

direct line with their earlier work.

> During this period [1 January 1977 to 1 January 1980], we shall concentrate our interest on *Dynamical systems, their global structures and singularities.* Many new problems, concepts, and methods will have to be elaborated before it finds itself in the subsequent state of a science or theory. The state of synthesis of parallel and isolated problems with a very developed mathematical apparatus and technique is in this domain not yet achieved.[179]

The program concocted by Kuiper, which I have reproduced in the

Complement to Chapter VIII below, focused on five areas: (a) Turbulence and

differentiable dynamical systems, (b) Differentiable dynamical systems,

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[177] An important exception to the above statement was Oscar E. Lanford who did use the IHÉS computer to study the Lorenz attractor.

[178] Early in 1976, this Foundation had just started to sponsor the research program on the closely related theme of "Synergetics" of Hermann Haken, from the Institut für Theoretische Physik at the University of Stuttgart. Lettre de H. Plate à Nicolaas Kuiper (21/3/79). Arch. IHÉS. As a sample of Haken's work in the mid-1970s, see H. Haken, "Analogy Between Higher Instabilities in Fluids and Lasers," *Physics Letters,* 53A (1975): 77-78; and H. Haken, *Synergetics – An Introduction: Noneequilibrium Phase Transition and Self-Organization in Physics, Chemistry, and Biology* (Berlin: Springer, 1977).

[179] Lettre de N. Kuiper à Frau Dr. Zarnitz (4/6/76). My translation from German. Also Gastforschungsprogramm 1977-79: Dynamische Systeme, ihre globale Strukturen und Singulritäten. Antrag für eine Verlängerung der Bewilligung in 1980-81 (1/6/79). Pressemitteilung (Projektdarstellung). Mathematik Stipendum-Programm an das Institut des Hautes Etudes Scienfiques "Dynamiche Systeme, ihre globalen Strukturen und Singularitäten" (1977-81) (30/1/80). Sclussbericht des dreijährigen

(c) Foliations, (d) Relation between qualitative dynamics and algebraic singularities,

Catastrophes, and (e) Singularities of polynomial mappings. This obviously was a vast

program, but it succeeded in grouping together more than half of the permanent

members and long-term visitors of the IHÉS. A striking feature of Kuiper's program

was that, although mentioning applications, it stayed mainly focused on the

mathematical theory of dynamical systems, as well as on the implications it might

have on other fields of mathematics. Nowhere were experimental researches on

chaotic behavior in turbulent systems even mentioned. This program was meant as a

continuation of the theoretical research and modeling practices developed at the IHÉS

for many years.

Therefore, while in the 1970s, research on dynamical systems definitely

became one of the main activities at the IHÉS, the fascinating convergence of fields

that became a trademark of chaos at this very moment remained a marginal aspect of

the research conducted and presented at the IHÉS. Significantly, one may note that, in

the 1970s, the word 'chaos' rarely surfaced in the official records of the IHÉS.

Dynamical systems, catastrophe theory, and singularities remained the preferred

labels for the fields in question. Many important later contributions to the

mathematical theory of dynamical systems would therefore be worked out at the

IHÉS, often sponsored by the Volkswagen Foundation, but this Institute contributed

little to the formation of what I have called the chaos constellation, even in France.

For the IHÉS, dynamical systems theory by and large remained a branch of

Gastforschungsprogramm "Dynamiche Systeme, ihre globalen Strukturen und
Singularitäten" 1977, 1978, 1979 (9/5/81). Arch. IHÉS.

mathematics. More than ever, the Institut des hautes études scientifiques of Bures-sur-

Yvette remained, in Sullivan's words, "'foreign' in the view of some Frenchmen."[180]

Indeed, many of the Frenchmen working on chaos were busy elsewhere.

**b)     Bergé-Dubois: Laser Velocimetry**

During the 1970s, as we have seen above, the experiments that Pierre Bergé and

Monique Dubois performed at the CEA, Saclay, were closely linked with de Gennes's

concerns with hydrodynamic instability. They even provided some of the matter for

his course at the Collège de France. Working on the Rayleigh-Bénard system with

new techniques combining laser technology and the computer, these two

experimenters would later become closely associated with the chaos constellation.

*(i)     A Simple and Easy to Implement Technique*

In 1974, Dubois and Bergé had become quite enthusiastic about their experimental

setup, which in their view, provided simple and easy-to-implement experimental

techniques for the measurement of local velocity flows in fluids.[181] They first applied

them to the measurement of the speed of particles in a fluid, like spermatozoids. But

around 1972, they turned to the study of the velocity field of the fluid itself for which

they provided local, non-intrusive measurements. In view of difficulties in getting

quantitative observations out of fluid dynamical systems, this represented a real

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[180] *Notes de séances manuscrites. Comité scientifique* (13/5/77). Arch. IHÉS.
[181] A picture is repr. in Service de physique du solide, "Une nouvelle technique," 75.
P. Bergé, "Ordre et convection dans les fluides," *La Recherche,* 6(62) (1975): 1070-
1073.

experimental breakthrough, since velocity fields were exactly what existing theories could best calculate.

Dubois entered the CEA as an engineer in 1954, and Bergé as a physicist in 1956. Up until 1968, they worked with solids irradiated by neutrons, just like de Gennes during his Ph.D. Around that time, however, a new tool, the laser, made its appearance in their laboratory, which, they say, had a profound impact on their experimental work.[182] They started to study fluctuations in liquids at their critical point, and critical exponents. Four to five years later, following advice from de Gennes and Yves Pomeau, a theoretical physicist studying plasmas in the laboratory next door(see below), they turned to the study of Rayleigh-Bénard systems.

They got in contact with Manuel Velarde, who provided them with a good entry into the literature, and found that, although this was a system that had been well studied experimentally, "very little has been done about velocity measurements."[183] Their Rayleigh-Bénard cell was a rectangular box of $4.5 \times 100 \times 60$ mm$^3$ filled with silicon oil. By including graphite or aluminum powder in the fluid, they were able to observe the structure of the rolls directly. Their laser interferometer provided them with precise measurements of various components of the velocity field. "To our knowledge this is the first report on the measurement of local convective velocities near the threshold of the Rayleigh-Bénard convection."[184] For Rayleigh numbers up to

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[182] Interview of P. Bergé and M. Dubois by the author (6/3/97).
[183] P. Bergé and M. Dubois, "Convective Velocity Field in the Rayleigh Instability: Experimental Results," *Physical Review Letters,* 32 (1974): 1041-1044, 1041.
[184] P. Bergé and M. Dubois, "Convective Velocity Field," 1044.

about 2.5 times the critical value, their observation seemed to confirm the theory for

the spatial dependence of the rolls.

Coming from critical phenomena, and in close contact with de Gennes, they

obviously endeavored to verify Landau's power law. Much to their surprise they found

that their observations disagreed with Landau's prediction, since they found the

maximum of the vertical velocity component to be proportional to $(R - R_c)^{0.60\pm0.02}$,

where the critical exponent was expected to be 0.5. This led them, and Velarde, to

wonder whether the classic Boussinesq approximation was valid.[185] Later

experiments, however, in which they replaced their side walls made of glass with

copper showed that the lower thermal conductivity of glass was responsible for this

deviation from Landau's theory.[186]

### (ii)    Mere Confirmation of Theory?

At the time, Yves Pomeau and Manuel Velarde—soon with the help of Pomeau's

"only true graduate student," Christiane Normand—started to adapt Busse's theory in

order to be able to account for Bergé and Dubois's results.[187] Bergé and Dubois were

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[185] M. G. Velarde, "Hydrodynamical Instability," Notes added in Proof, 526. See R. Peréz-Cordon and M. G. Velarde, "On the (Non Linear) Foundations of Boussinesq Approximation Applicable to a Thin Layer of Fluid," *Journal de physique,* 36 (1975): 591-601.

[186] P. Bergé, "Rayleigh-Bénard Instability: Experimental Findings," 340-341; P. Bergé, "Aspects expérimentaux," 30-31. See the concluding paper on critical effects in Rayleigh-Bénard: J. E. Wesfreid, Y. Pomeau, M. Dubois, C. Normand, and P. Bergé, "Critical Effects in Rayleigh-Bénard Convection," *Journal de physique— Lettres,* 39 (1978): 725-731.

[187] M. G. Velarde announced a coming monograph in collaboration with Y. Pomeau in "Hydrodynamical Instability," 522. They would finally publish a long review article: C. Normand, Y. Pomeau, and M. G. Velarde, "Convective Instability: A Physicist's

soon led to distinguish two "domains." The "linear domain" was where velocity fields

exhibited a perfect sinusoidal variation across the cell. For this domain, which

corresponded to simple rolls in the fluid, the observation matched the theory.

> Morose spirits [*esprits chagrins*] will say that . . . nothing allowed us to doubt
> that conveniently adapted hydrodynamic equations would not pertain to this
> problem: this is, in any case, what one is tempted to say after the fact. . . . On
> our part, we think that we needed to attempt the experiment and that *the very*
> *positive collaboration between theoretical and experimental physicists that it*
> *caused would be a sufficient reason to justify it.*

Noting that convection cells were applied to many natural phenomena, Bergé

tried to convince his audience that physicists should look anew into mundane

phenomena, a call which may remind us of some of Thom's or Mandelbrot's.

> We think that it is good that, from time to time, physicists get out of their
> generally artificial world in order to study seriously the natural phenomena
> that surround them.[188]

When they got into the "nonlinear domain," at a Rayleigh number above $3R_c$,

however, Bergé and Dubois's results remained unexplained. Despite the fact that

Normand had "tried one's best to perform many theoretical computations," the fit

between theory and experiment remained shaky. The picture they provided "was

somewhat speculative, as more experimental evidence as well as theoretical analysis

are needed to reach definitive conclusion."[189] But this seemed to worked These

experimental results may have been somewhat disappointing since they only seemed

to confirm the theory.

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Approach," *Review of Modern Physics*, 49 (1977): 581-624. It was P. Manneville who
told me in interview that Normand was Pomeau's "only true graduate student."
[188] P. Bergé, "Aspects expérimentaux," C1-33. My emphasis.
[189] P. Bergé, "Rayleigh-Bénard Instability: Experimental Findings," 346, and 351.

At the time, the Ruelle-Takens model was not explicitly discussed by Bergé

and Dubois. They had been enrolled in the study of Rayleigh-Bénard by de Gennes,

Velarde, and Pomeau because they had an original apparatus that allowed them to

measure local quantities. But none of the above theoreticians were very keen, up to

1975, to take up a dynamical systems approach to the problem of the onset of

turbulence in convection. Normand, Pomeau, and Velarde extended Busse's theory to

include more and more modes. Striking tensions in the popular account by Velarde

and Normand underscores that they had not adopted a dynamical systems approach.[190]

When this is compared with Ruelle and Takens's attitude, the contrast is patent. While

the former still dreamt of exact solutions, the latter expressed their agnosticism toward

the fundamental Navier-Stokes equation itself.

Briefly, before the late 1970s, Bergé and Dubois's work could not be seen as

stemming from a dynamical systems approach.[191] However, theirs was one of the

experimental evidences that dynamical systems with a few degrees of freedom

contained most of the mechanics of the transition to turbulence in confined geometry,

findings that served to vindicate Ruelle's viewpoint. Moreover, it allowed contacts to

be established between experimenters and theoreticians. On these bases, the chaos

constellation was built. Among those mentioned above, Pomeau, more than anyone

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[190] "Although exact solutions to [the Rayleigh-Bénard problem] are still lacking, substantial progress . . . has been made. " M. G. Velarde and C. Normand, "Convection," 93.

[191] The first really 'chaotic' paper of Bergé and Dubois was: M. Dubois and P. Bergé, "Experimental Evidences for the Oscillators in a Convective Biperiodic Regime," *Physical Review Letters,* 76A (1980): 53-56.

else, was responsible for the translation of the dynamical systems approach into a

language that physicists could understand.

### c)      Pomeau: Interdisciplinarity in Action

#### (i)      A New Scientific Community?

A graduate from the École Normale, where he studied in 1961-1965, Yves Pomeau

likes to recall the impression that Rocard's course on mechanical vibration left on him:

this was the "first interesting professor" he had.[192] For someone who had an interest in

mathematics in view of its applications, the Bourbakist influence was a "gigantic

repellent [*gigantesque repoussoir*]." Pomeau became a theoretical physicist.

He worked on a Ph.D. at Orsay on plasma physics, and soon after was

appointed to the CEA. This subject led him to take a look at phenomena of

hydrodynamic instabilities in plasmas. He had started to collaborate with the Belgian

physicist Paul Résibois, who belonged to Prigogine's school. Pomeau frequented the

Brussels school and befriended Velarde.[193] This was the start of a long collaborative

.effort.

Contrary to most physicists, Pomeau was attracted by Thom's ideas on

catastrophe theory, having been impressed by his, and Smale's, talks at the 1971

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[192] Interview of Yves Pomeau by the author (4/8/97). See his preface in P. Bergé, ed.,
*Le Chaos. Théorie et expériences* (Paris: Eyrolles and CEA, 1988).
[193] His more cited paper before 1975: Y. Pomeau and P. Résidois, "Time-Dependent
Correlation Functions and Mode-Mode Coupling Theories," *Physics Reports*, C19
(1970): 63-139.

Statistical Physics Conference in Chicago.[194] In 1975, Pomeau invited Ruelle to speak

at the CEA. By then he was teaching, with Annie Gervois, a course on hydrodynamics

at Saclay, in which the Ruelle-Takens model was discussed. Impressed by dynamical

systems theory, Pomeau studied the classics: Poincaré, Birkhoff, Smale, Arnol'd and

Avez, etc. For Pomeau, it was clearly Ruelle and Takens's paper which had set

everything in motion. "After the article of Ruelle and Takens, there has been recently

much interest in the problem of the 'onset of turbulence'."[195] Following Martin,

Pomeau saw three cases as possibly describing the onset of turbulence: Lorenz,

Ruelle-Takens and the successive bifurcation picture as described by Li and Yorke

among others. In collaboration with scientists coming from very different

backgrounds, Pomeau embarked on many projects aiming at a better understanding of

systems—mathematical, numerical, and experimental—which exhibited a sensitive

dependence on initial conditions. [196]

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[194] S. A. Rice, K. T. Freed, and J. C. Light, eds. *Statistical Mechanics: New Concepts, New Problems, New Applications: Proceedings of the Sixth International Union of Pure and Applied Physics Conference on Statistical Mechanics, Chicago, March 1971* (Chicago: University of Chicago Press, 1972).

[195] Y. Pomeau, "Turbulence: Determinism and Chaos," *Problems of Stellar Convection: Proceedings of the Colloquium Nr. 38 of the International Astronomical Union, Held in Nice, August 16-20, 1976,* ed. E. A. Spiegel and J.-P. Zahn (Berlin: Springer, 1977): 337-348, 337. See also P. Bergé, Y. Pomeau and M. Dubois-Gance, *Des rythmes au chaos* (Paris: Odile Jacob, 1994), 229.

[196] M. Hénon and Y. Pomeau, "Two Strange Attractors with a Simple Structure," *Turbulence and Navier-Stokes Equations,* ed. R. Tenam (Berlin: Springer): 29-67. J. L. Ibañez and Y. Pomeau, "A Simple Case of Non-Periodic (Strange) Attractor," *Journal of Non-Equilibrium Thermodynamics,* 3 (1978): 135-152 [preprint already written in 1975]; B. Derrida, A. Gervois, and Y. Pomeau, "Itération d'endomorphismes de la droite réelle et représentation des nombres," *CRAS A,* 285 (1977): 43-46; "Iteration of Endomorphisms on the Real Axis and Representation of Numbers," *Annales de l'Institut Henri-Poincaré,* 29 (1978): 305-356; partly repr. *Chaos,* ed. Hao B.-L., 1984 ed., 252-266; "Universal Metric Properties of Bifurcations

Despite the number of collaborators Pomeau had in very different fields, it

seems difficult to see in this the emergence of a new scientific community, working

on similar topics, with some shared research goals, outlets for their publications, and

regular meetings. Most of Pomeau's collaborators indeed worked in different

laboratories of the CEA at Saclay, but he appears to have been one of the only links

between them. As Paul Manneville admitted, it was not easy to collaborate with

Pomeau, despite his charming personality. He abruptly came to your office and threw

at you lots of incomprehensible ideas without providing the references.[197]

By 1976, Yves Pomeau had arrived at the same kind of coherent picture of

chaotic behavior and turbulence as Ruelle or Martin. As opposed to Martin, however

Pomeau adopted a more mathematical language, inspired by Thom's catastrophe

theory, Smale's dynamical systems theory, but also abstract ergodic theory. As

opposed to Ruelle, Pomeau understood that merely to proclaim, as Thom had done

with catastrophe theory, that chaos was a revolutionary mathematical way of

modeling natural phenomena, was not enough. One needed to get one's hands dirty, to

compare the results of theory with experiments, to collaborate with people coming

from previously widely separated disciplines, and to build a common language. Just to

insist on making people learn a new mathematical theory would not do it.

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of Endomorphisms," *Journal of Physics A,* 12 (1979): 269-296; C. Laj, D.
Nordemann, and Y. Pomeau, "Correlation Function Analysis of Geomagnetic Field
Reversals," *Journal of Geophysical Research,* 84B (1979): 4511-4515. See also M.
Hénon, "A Two-Dimensional Mapping with a Strange Attractor," *Communications in
Mathematical Physics,* 50 (1976): 69-77; repr. *Univ. Chaos,* 341-349; *Chaos II,* 235-
243. For Pomeau's work on intermittency, see below.
[197] Interview of Paul Manneville by the author (23/5/97).

  *(ii) Intermittency: Translation of Dynamical Systems Modeling Practices*

In 1976, Bergé and Dubois observed a singular phenomenon with their Rayleigh-

Bénard system: "the velocity amplitude show[ed] intermittent periodic oscillations

versus time." Even if the mechanism responsible for this phenomenon remained

unclear, they believed that it represented "the most important step in the transition to

turbulence."[198] At about the same time, Pomeau, who was studying the Lorenz model

on an analogous computer, noticed a similar kind of intermittent flashes on his

oscilloscope. He asked Paul Manneville to take a look at this and soon they came up

with still another series of bifurcations that could be found in the onset of turbulence.

Manneville's office at the CEA being just a few doors down from Bergé and Dubois's

laboratory, he was given long printouts of experimental time series to analyze. A truly

exciting collaboration again took place. With their help, this type of intermittent

behavior was then also observed in oscillating chemical reactions.[199]

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[198] P. Bergé and M. Dubois, "Time Dependent Velocity in Rayleigh-Bénard
Convection: A Transition to Turbulence," *Optics Communications,* 19 (1976): 129-
133.

[199] The literature about intermittency is huge: see, e.g., Y. Pomeau and P. Manneville,
"Intermittency: A Generic Phenomenon at The Onset of Turbulence," *Intrinsic
Stochasticity in Plasmas,* ed. G. Laval and D. Grésillon (Orsay: Éditions de physique
Courtaboeuf, 1979): 330-340; Y. Pomeau and P. Manneville, "Intermittent Transition
to Turbulence in Dissipative Dynamical Systems," *Communications in Mathematical
Physics,* 74 (1980): 189-197; repr. *Univ. Chaos,* 327-335; *Chaos II,* 355-363; P.
Bergé, M. Dubois, P. Manneville, and Y. Pomeau, "Intermittency in Rayleigh-Bénard
Convection," *Journal de physiques – Lettres,* 41 (1980): L341-L345; repr. *Univ.
Chaos,* 149-153; Y. Pomeau J.-C. Roux, A. Rossi, S. Bachelart, and C. Vidal,
"Intermittent Behaviour in the Belousov-Zhabotinski Reaction," *Journal de physique
– Lettres,* 42 (1981): L271-L273; repr. *Univ. Chaos,* 167-169. P. Manneville and Y.
Pomeau, "Different Ways to Turbulence in Dissipative Dynamical Systems." *Physica,*
1D (1980): 219-226.

A closer look at the intermittent model for the onset of turbulence provides a

better appreciation of the modeling practices which became trademarks of chaos

theory as conceptualized by physicists. Here, Pomeau and his collaborators were

faced with similar phenomenological observations in the laboratory and on the

computer: time series exhibiting apparently regular, periodic behavior randomly and

abruptly disrupted by sudden bursts of erratic behavior. These observations were

however made on systems which *a priori* had little to do with one another. Was there

a common cause for these behaviors and, if yes, what could it be?

In trying to answer these questions, a dynamical systems approach proved to

be most useful. But at the same time, Pomeau and Manneville did not wholly adopt

the modeling practice of Ruelle and Takens, but transformed it in order to fit better

with physicists' concerns. Indeed, they contended: "the general framework of these

theories based on genericity arguments [Lorenz's and Ruelle's] is *sufficiently versatile*

to allow for different possible transitions."[200] Thus, as Martin had earlier, they

disputed the fact that the Ruelle-Takens model had to be the only way to turbulence.

In order to account for the similarity of patterns, Pomeau and Manneville

considered a Poincaré map of the Lorenz model, in the form $y_{n+1} = f(y_n, r)$, where $r$

was a parameter of the model associated with the Rayleigh number. They then

considered the case where for $r$ slightly below a critical value $r_T$, the curve had two

intersection points with the diagonal, which collapsed into a single point for $r = r_T$,

while "for $r > r_T$ the curve is lifted up and no longer crosses the [diagonal] so that a

'channel' appears between them" (Fig. 18).[201] Using a desktop computer, they were able explicitly to extract this picture from the Lorenz model. This simple abstract picture, Pomeau and Manneville contended, was "displaying generic features susceptible of *explaining* the experimental observations."[202] When the system passed through the "channel," it exhibited a behavior that seemed almost regular. Leaving the "channel," it explored chaotically other regions of phase space until it found itself again trapped into one such "channel."

Pomeau and Manneville's modeling practice lay in between the mathematical arguments used by the IHÉS 'applied topologists' and the traditional practice of physicists, which aimed at finding solutions to fundamental laws. The latter attitude could not be adopted here, they claimed. "A detailed quantitative interpretation is clearly out of reach, even from the simplified point of view of dynamical systems." Even if realistic dynamical systems relevant to the experiment could not be constructed: "Anyway, this unknown realistic dynamical system should share some generic properties with already well studied models."[203] In their view, such generic properties of *other* dynamical systems could provide an explanation for experimental behaviors which were assumed to stem from a similar, but unknown, dynamical system.
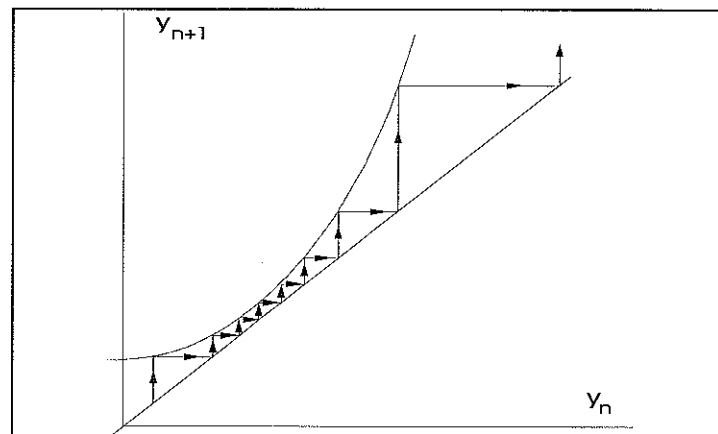
[200] P. Bergé, M. Dubois, P. Manneville, and Y. Pomeau, "Intermittency in Rayleigh-Bénard," L-341. My emphasis.

[201] Y. Pomeau and P. Manneville, "Intermittent Transition," *Univ. Chaos,* 328.

[202] P. Bergé, M. Dubois, P. Manneville, and Y. Pomeau, "Intermittency in Rayleigh-Bénard," L-343. Their emphasis.

[203] P. Bergé, M. Dubois, P. Manneville, and Y. Pomeau, "Intermittency in Rayleigh-Bénard," L-342.

**Figure 18:** Poincaré Map from the Lorenz Model, with $r$ slightly above $r_T$. The motion through the channel corresponds to the laminar phase of the movement. The slow drift is quite imperceptible on the time record. Redrawn from Y. Pomeau and P. Manneville, "Intermittent Transition," *Univ. Chaos*, 330, Fig. 4.

While eschewing traditional modeling practices, Pomeau and Manneville

likewise neglected to ground their model on rigorous mathematical proofs.

> The reader must be warned that the discussion is made *in physical terms*. No proof is given. Many of them are certainly very difficult and require advanced mathematics. . . . We try to present our point of view *as intuitively as possible*, and have avoided almost completely any standard analytical formalism which is *useless* for this sort of problem.[204]

They plainly admitted that they had "*guessed*" from numerical computations.[205] The

intermittent picture for the onset of turbulence would nonetheless strike physicists

"because of its esthetic and conceptual beauty."[206]

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[204] Y. Pomeau and P. Manneville, "Intermittency," 331.

[205] Y. Pomeau and P. Manneville, "Intermittency," 339.

[206] J.-P. Eckmann, "Roads to Turbulence in Dissipative Dynamical Systems," *Reviews of Modern Physics*, 53 (1981): 643-654, 650; repr. *Univ. Chaos*, 94-105.

When compared with the modeling practices of Ruelle, Smale, Thom, and Zeeman, discussed in previous chapters, the practice adopted by Pomeau and Manneville for intermittency reveals many similarities and some crucial differences. Like most of the above, the pair of CEA physicists started with phenomenological similarities that they wished to explain with topological, rather than reductionist, arguments. The link with the substratum was postulated but was not a concern of their practice. Bifurcations were again interpreted as the source for changes in behaviors. However, unlike the others, Pomeau and Manneville relied on specific computer models, whose generic properties, they assumed, could be transferred to a "realistic," but unknown, dynamical system. Rigorous mathematical proofs were neither the starting point, nor the goal of their study. The mathematics of dynamical systems theory, and the important concept of genericity, were loosely used in order to infer that phenomena observed in numerical studies could account for experimental data.

Dynamical systems theory thus provided theoretical physicists, like Pomeau and Manneville, with concepts and techniques that could be profitably used in order to make sense of experimental observations, even while avoiding too much mathematical technicality. They realized that even very abstract mathematical constructs might be useful in trying to understand nonlinear phenomena. The experimental work that Libchaber undertook in 1977 would only serve to further this feeling.

### d)    Libchaber: Helium in a Small Box

During the late 1970s, some of the physics staff of the École normale supérieure of

Paris were stunned by the new tabletop experiment of their colleague Albert

Libchaber.[207] With the help of the engineer Jean Maurer, he built a tiny cavity whose

volume was less than a few cubic millimeters and filled it with non-superfluid liquid

helium. They then heated it slightly at the bottom and observed changes in

temperature: a classic Rayleigh-Bénard experiment.

For some physicists and mathematicians his results came as a revelation: it

was "a kind of miracle, not like the usual connection between theory and

experiment."[208] For Leo Kadanoff, it was "an experience like no other experience I

can describe, the best thing that can happen to a scientist, realizing that something

that's happened in his or her mind exactly corresponds to something that happens in

nature."[209] By and large, Libchaber and Maurer's observation of the cascade of

bifurcations conjectured by Los Alamos physicist Mitchell Feigenbaum was

responsible for the chaos fashion of in the following decade. Ten years later, when

James Gleick wrote his book, this experiment was to take a prominent position. There

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[207] Libchaber's experiment was recounted by J. Gleick, *Chaos,* 189-211. Many details on Libchaber's career and ideas, as well as the circumstances surrounding this experiment are based on an interview that I conducted with him on October 24, 1993 in Princeton, who then told me that this was , "the first [*sic*] experiment in classical physics at the École.".
[208] Jerry Gollub quoted by J. Gleick, *Chaos,* 209.
[209] Leo Kadanoff quoted by J. Gleick, *Chaos,* 189.

Libchaber became *the* experimenter of chaos.[210] Since this story is well known, I want

only to underscore how Libchaber's experiment served to vindicate a dynamical

systems approach to the study of turbulence.

*(i)       Bolometers: A Local Probe*

A frequent visitor of Bell Labs, Libchaber had been in contact with Günter Ahlers,

who convinced him of the interest of Rayleigh-Bénard systems. Libchaber's

experiment was therefore very similar to Ahlers's, his principal innovation being the

local probe–a resistor sensitive to heat, or *bolometer*–that enabled him to measure

locally the heat flow in the liquid.[211] This was a crucial difference. As opposed to

Ahlers's global measurements, Libchaber's gave local information about the fluid

flow. He could thus hope to observe phenomena similar to the ones exhibited by

Bergé and Dubois.


For some time, Libchaber had been interested in superconductivity and

superfluidity, through which he was introduced to the experimental manipulation of

liquid helium. At the Dijon Symposium in 1975, he had presented a talk on turbulence

in superfluid helium.[212] The experimental cavity he then used was similar to the one

he would later use to study Rayleigh-Bénard, which underscores the transfer of

[210] I suppose that this was because, contrary to other experimenters mentioned above, Libchaber had a strong interest for philosophy, and Goethe and D'Arcy Thompson in particular.

[211] A. Libchaber and J. Maurer, "Local Probe in a Rayleigh-Bénard Experiment in Liquid Helium," *Journal de physique – Lettres,* 39 (1978): L369-L372.

[212] A. Libchaber, "Hydrodynamique de l'hélium IV superfluide," *Journal de physique,* 37, Colloque C1 (1976): 111-116.

experimental techniques from the study of phase transition to that of turbulence (Fig. 19). At the conceptual level, it was precisely on problems close to dynamics that he focused his attention: "the dynamics in the vortices within superconductors, and the equivalent within superfluids." By working in a very small geometry of the order of a micron, it was possible to isolate a small number of these "objects," as he insists on calling them.[213] Considering "a kind of *polymer* of quantized vortices of the superfluid," Libchaber's study of turbulence in superfluid had considerable bearing on the concerns expressed in the ATP on instability.[214]
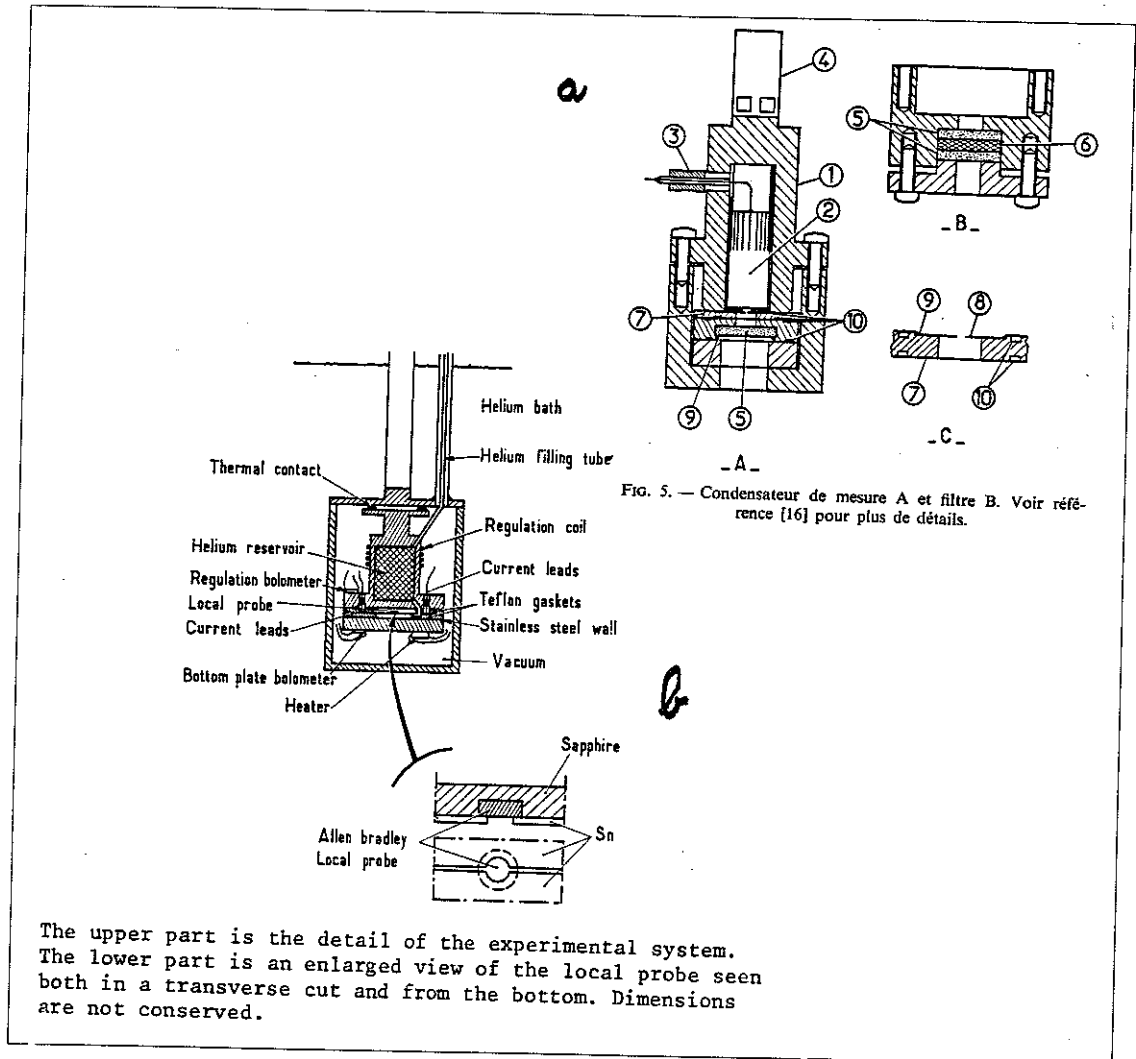
Being microscopic, these vortices were quite hard to study individually. He considered the problem a bit, but then turned to classical, macroscopic vortices in non-superfluid liquid helium. It is in this spirit that he undertook the experiment. He wanted "to see a macroscopic *object,* i.e. one or two convection rolls, and to study their dynamics."[215] His motivation was conceptual rather than strictly theoretical, in the sense that at the beginning, he was not aware of the theories of Lorenz, Ruelle, or Feigenbaum. Nevertheless, Libchaber always thought a lot about his experiments before he started building systems. This Rayleigh-Bénard experimental design deliberately conceived in such a way as to catch glimpses of emergent dynamical structures.

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[213] Interview of A. Libchaber (24/10/1993). For a discussion of changes in the practice of the definition of objects, see I. Stengers, *Cosmopolitiques,* 5, 152.
[214] A. Libchaber, "Hydrodynamique de l'hélium IV," C1-115.
[215] Interview of A. Libchaber (24/10/93).

*a*

Helium bath

Thermal contact —Helium filling tube

Regulation coil

Helium reservoir

Regulation bolometer

Local probe

Current leads

Current leads

Bottom plate bolometer

Heater

Current leads

Teflon gaskets

Stainless steel wall

Vacuum

_A_

_B_

_C_

FIG. 5. — Condensateur de mesure A et filtre B. Voir réfé-rence [16] pour plus de détails.

*b*

Sapphire

Sn

Allen bradley
Local probe

The upper part is the detail of the experimental system.
The lower part is an enlarged view of the local probe seen
both in a transverse cut and from the bottom. Dimensions
are not conserved.

**Figure 19:** Libchaber's Experimental Apparatuses for the Study of Superfluid Helium, and for the Study of the Onset of Turbulence. Repr. with permission from (a) A. Libchaber, "Hydrodynamique de l'hélium IV," C1-114. Copyright © Les Éditions de Physique. (b) A. Libchaber and J. Maurer, "Helium in a Small Box." Copyright © Plenum Publisihers.

So Libchaber's interest differed from that of other physicists then experimenting with convection (Ahlers, Bergé, Gollub, etc.). The latter focused on the onset of turbulence from the point of view of a phase transition. On the other hand, Libchaber's concerns with dynamics triggered, first, the question of how to observe

the relevant objects of study. And then, how should these effects be described

physically and mathematically? Naturally following Landau's theory, Libchaber's

ingenious bolometers gave time series easily amenable to a Fourier analysis of

frequencies. The way he would study these frequencies was shaped by his interest in

the dynamics of macroscopic objects emerging in fluids.

### (ii)    Experiment and Observations

Libchaber and Maurer's first article offer clear evidence of their concerns at the time

of the experiment. They showed the "extraordinary dependence of the results on the

aspect ratio," the radius of the cavity divided by its height. The diameter of their

cylindrical cell was fixed at 25 mm. They studied the behavior of their system for

heights varying from about 1 mm to 6 mm and for Rayleigh numbers $R$ ranging from

just above the critical value to about $15R_c$. For large heights they observed a sharp

spectral line together with up to ten harmonics that appeared past a certain value of

the Rayleigh number, in accordance with Landau's theory. For smaller heights,

however, the behavior was totally different. No well defined frequencies and a

continuum of noise in the low frequencies. This extreme dependence on the aspect

ratio came as surprise to other physicists.[216] For him, this was not so surprising since

the objects that could form in the cavity had to be different depending on the aspect

ratio. For Libchaber, the next step was to study more carefully the geometry of the

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[216] Interview of A. Libchaber (24/10/93).

shapes they were just starting to perceive, before attempting a "theoretical analysis of the data."[217]

In August 1979, for their second paper, Libchaber and Maurer changed the geometry of their cavity from a cylindrical to parallelepipedic cell, and placed two bolometers.[218] For the first time, they referred to the theoretical work of Fritz Busse, and the word 'bifurcation' entered their vocabulary. However, their interest partly lay elsewhere. For a rectangular cell, it seemed possible to define more clearly the rolls geometry: in the simplest case, their data seemed to be consistent with the hypothesis that two transverse rolls were formed, in agreement with previous studies.[219] The power spectrum of the temperature field again exhibited well-defined frequencies. They thus focused on the dynamics of these frequencies.

In particular, a phenomenon caught their eye. In addition to the one clearly defined frequency, they now were able to exhibit a second one above a second threshold. Libchaber and Maurer noticed that "as one keeps increasing the temperature difference, all the combination frequencies, $f = mf_1 + nf_2$, $m$ and $n$ [positive or negative] integer, appear as one Fourier analyses the data." All of these combination of frequencies rapidly produced a situation that seemed quite noisy, the ratio of the two frequencies being approximately irrational. Citing Gollub and Bergé's

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[217] A. Libchaber and J. Maurer, "Local Probe," L-372.

[218] J. Maurer and A. Libchaber, "Rayleigh-Bénard Experiment in Liquid Helium: Frequency-Locking and the Onset of Turbulence," *Journal de Physique -- Lettres,* 40 (1979): L419-L423.

[219] K. Stork and V. Muller, "Convection in Boxes: Experiments," *Journal of Fluid Mechanics,* 54 (1972): 599-611.

work, they had clearly started to focus on quantities bearing on the Ruelle-Takens

model, which, however, they did not mention.

What interested in the first place Libchaber and Maurer was to take these

frequencies as legitimate objects susceptible of a dynamical analysis. These concerns

made them notice in passing two other aspects of their data, which assumed more

importance in their future investigations. They were: that "more than two frequencies

are never measured until we reach the turbulent regime, in confirmation with other

observations;" and "that the onset of turbulence starts from this locking state with

parametric amplification of frequencies $f/2$, $f/4$ ..."[220]

While it would be an exaggeration to say that Maurer and Libchaber's next

three papers would principally deal with a careful investigation of these observations,

their scientific worth would later be recognized mainly on the basis of the resonance

that these observations would find in theoretical works on dynamical systems.[221]

Indeed after an extensive collaboration with theoreticians, these observations would

be more or less reinterpreted as: a contradiction of Landau's picture and an indication

for the validity of the Ruelle-Takens scenario; and the first experimental observation

of the Feigenbaum cascade of bifurcations. Libchaber's later observations would

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[220] J. Maurer and A. Libchaber, "Rayleigh-Bénard Experiments," L-422.
[221] A. Libchaber and J. Maurer, "Une expérience de Rayleigh-Bénard de géométrie réduite: Multiplication, accrochage et démultiplication de fréquences," *Journal de physique, Supplément,* Colloque C3, 41 (1980): C3-51 to C3-56; J. Maurer and A. Libchaber, "Effect of the Prandtl number on the onset of turbulence in liquid $^4$He," *Journal de physique – Lettres,* 41 (1980): L515-L518; A. Libchaber and J. Maurer, "A Rayleigh-Bénard Experiment: Helium in a Small Box," *Nonlinear Phenomena at Phase Transitions and Instabilities: Proceedings of the NATO Advanced Study Institute [Geilo, Norway: March 1981],* ed. T. Riste (New York: Plenum, 1982): 259-286.

moreover enable him to compare "universal" features of Feigenbaum's model with

experiment, and to exhibit Pomeau and Manneville's intermittent road to turbulence.

### (iii)    Feigenbaum: Surprise and Excitement

By 1980, Albert Libchaber, as previous experimenters, had found and mastered the

direct relevance of Busse's theory. He noticed the "qualitative" accordance with the

theory.[222] But as soon as the second frequency appeared, he "had no physical model to

interpret this second oscillatory mode." Libchaber's interaction with the theoreticians

Pomeau, Ruelle, Eckmann, and Feigenbaum, would redirect his experiment towards

new goals.

Libchaber had met Feigenbaum at the Gordon Research Conference on

"Dynamical Instability and Fluctuations in Classical and Quantum Systems,"

organized by Paul Martin in July 1976.[223] Carefully noting a "demultiplication of

frequencies," Libchaber and Maurer were drawn to conclude that this process

"appeared as crucial for the germination of turbulence."[224] Not much later, they

identified the phenomenon with the frequency-doubling cascade that Feigenbaum was

studying as a purely mathematical phenomenon.

To go into much details about the history of discrete iteration of mappings of

the type $x_{i+1} = F_\mu(x_i)$ would take us away from the main focus of this chapter.[225] Let

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[222] A.Libchaber and J. Maurer, "Une expérience de Rayleigh-Bénard."
[223] List of participants provided to me by Paul Martin.
[224] A. Libchaber and J. Maurer, "Une expérience de Rayleigh-Bénard," C3-56.
[225] Important parts of this story are moreover already well known. This is one of the main theme of J. Gleick, *Chaos,* esp. the chapters titled "Life's Ups and Downs" and "Universality," 57-80 and 155-187. Another accessible introduction is Leo P. Kadanoff, "Roads to Chaos," *Physics Today* (December 1983), 46-53. See also C.

me just recall that, in 1974-1975, unexpectedly complex behavior in these kinds of

problems had led May, Li, and Yorke to embrace the word 'chaos' to describe it.[226] As

we have seen, in the later 1970s, this became a favorite field of study at the IHÉS.

Since the work of Smale, these maps had been considered as dynamical

systems as well, and one could study the bifurcations as $\mu$ increased. As it turned out,

for a large class of nonlinear functions $F_\mu$, chaotic behavior could be obtained by

varying the parameter $\mu$. Following a pattern similar to the Ruelle-Takens model, for

small values of $\mu$, there was a fixed point $x_0$ defined as an attractor, such that

$F_\mu(x_0)= x_0$, and as $\mu$ was increased, instead of a Hopf bifurcation, this system went

through a pitchfork bifurcation giving rise to a pair of attractors, the system oscillating

between both of them. This doubling process was repeated at an accelerating

cadence–there were 4, then 8, then 16 values in the cycle, etc.–until the situation

became nonperiodic. The main difference with the Ruelle-Takens scheme was that

aperiodicity was reached after an infinite number of bifurcations, instead of only

three. It is not like Laudau's either, because aperiodicity was eventually achieved. This

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Mira, "Some Historical Aspects Concerning the Theory of Dynamic Systems,"
*Dynamical Systems: A Renewal of Mechanism*, ed. S. Diner, D. Fargue, and G.
Lochak (Singapore: World Scientific, 1986): 250-262; "Some Historical Aspects of
Nonlinear Dynamics, Possible Trends for the Future," *Visions of Nonlinear Science in
the 21th Century, Seville, June 26, 1996.* A mathematical survey can be found in P.
Collet and J.-P. Eckmann, *Iterated Maps on the Interval as Dynamical Systems*
(Boston: Birkhäuser, 1980).

[226] R. May, "Biological Populations with Nonoverlapping Generations, Stable Points,
Stable Cycles, and Chaos," *Science,* 186 (1974): 645-647; "Simple Mathematical
Models with Complicated Dynamics," *Nature,* 261 (1976): 459-467; *Univ. Chaos,* 85-
73; *Chaos II,* 151-159; and T.-Y. Li and J. A. Yorke, "Period Three Implies Chaos,"
*American Mathematical Monthly,* 82 (1975): 985-992.

behavior had led Martin to consider this sequence of bifurcation as a possible scenario

for the onset of turbulence (Figure 1).

May, and others after him, had noted certain qualitative properties that did not

seem to rely on the precise form of $F_\mu$. In a series of article starting in 1978, Los

Alamos theoretical physicist Mitchell J. Feigenbaum explored the problem, using

renormalization group methods and a pocket calculator. He showed that it was

possible to extract from these generic qualitative properties universal quantities–

numbers![227]

In the words of Libchaber, one "essential prediction" of the theory was the

Rayleigh numbers at which the frequency doubling occurred should be such that:[228]

$$\frac{R_{n-1} - R_n}{R_n - R_{n+1}} \xrightarrow{n \to \infty} \delta = 4.6692...;$$

where $R_n$ was the Rayleigh number at which occurred the bifurcation producing $2^n$

frequencies, and $\delta$ was a universal constant.

Thus were Libchaber's intriguing observations explained. While he had been

lacking theoretical guidelines to account for them, Feigenbaum's theory helped

Libchaber articulate his observation. His experimental result for $\delta$ was "quite bad,"

but not in contradiction with Feigenbaum's theory: $\delta = 3.5 \pm 1.5$. Everything thence

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[227] M. J. Feigenbaum, "Quantitative Universality for a Class of Nonlinear
Transformations," *Journal of Statistical Physics*, 19 (1978): 25-52; repr. *Chaos II*,
160-187; "The Onset Spectrum of Turbulence," *Physics Letters*, 74A (1979): 375-378;
"The Universal Metric Properties of Nonlinear Transformations," *Journal of
Statistical Physics*, 21 (1979): 669-706; "The Transition to Aperiodic Behavior in
Turbulent Systems," *Communications in Mathematical Physics*, 77 (1980): 65-86; and
"Universal Behavior in Nonlinear Systems," *Los Alamos Science*, 1: 4-27; repr. *Univ.
Chaos*, 49-84.

went fairly quickly. People and preprints went back and forth over the Atlantic, so that

pairs of articles could refer to one another. "At some point, you don't know anymore

who's doing what. It becomes a highly interactive milieu."[229] The final success was

total, the excitement general. Still, a puzzling question remained, that was the

foremost cause of the initial surprise among scientists. These simple iterations of

functions, these nice mathematical games, what had they to do with real fluid flows?

At this moment, Eckmann was already working on the integration in a global

framework of these new approaches to turbulence.

### e)      Eckmann's Synthesis: The 'Dynamical Systems Approach'?

The time for synthesis had come. Jean-Pierre Eckmann, from Geneva, the son of a

famous Swiss mathematician, had been in contact with Thom and Ruelle in the

"stimulating atmosphere at Bures-sur-Yvette."[230] A frequent visitor there, he was well

positioned to achieve this synthesis, having been one of the crucial personal link

between Libchaber and Feigenbaum.[231]

In October 1981, Eckmann published an article that presented the general

philosophy and theory behind this new "approach to the understanding of irregular (or

nearly irregular) phenomena, which has been relatively successful recently," adding in

a footnote: "this approach can be viewed as a concretization of Thom's (1972)

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[228] Libchaber and Maurer, "Helium in a Small Box," 280.

[229] Interview of A. Libchaber (10/24/93).

[230] P. Collet and J.-P. Eckmann, *Iterated Maps on the Interval,* vii. It was Eckmann who wrote Ruelle's Lausanne lecture note in 1970.

[231] G. B. Lubkin, "Period-Doubling Route to Chaos Shows Universality," *Physics Today,* 34-3 (March 1981): 17-19.

catastrophe theory."[232] Although it hardly contain anything new, after more than a

decade of work on instabilities in fluids, this was one of the first syntheses to come

out, which was explicitly based on dynamical-systems modeling practices. "In order

to describe our main topic, we need an adequate language for describing deterministic

evolution equations."[233] This language would be that on dynamical systems theory.

Acknowledging that an aim "clearly felt throughout the literature on

dynamical systems," was far from being achieved, Eckmann turned to *experiments* as

a guide for which bifurcations from simple attractors to nontrivial ones might be the

most relevant for physics (and chemistry).[234] He thus introduced the notion of a

*scenario* to describe the *most probable* sequences of bifurcations. Three roads to

turbulence deserved the label: the Ruelle-Takens scheme, Feigenbaum's cascade, and

the Pomeau-Manneville intermittent behavior. There were no guarantee at all that this

list was exhaustive.

"We are going to look at the nature of the prediction which can be made with

the help of scenarios, since this may be a *somewhat unfamiliar way of reasoning.*"

Eckmann characterized scenarios as "if. . ., then . . ." statements, "i.e., if certain things

happen as the parameter is varied, then certain other things are likely to happen as the

parameter is varied further." The mathematical definition of "likely," clearly linked

with genericity, depended on the scenario.

> But what does likely means in a physical context? I do not intend to go to any
> philosophical depth but, rather, take a pragmatic stand. (1) One *never* knows

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[232] J.-P. Eckmann, "Roads to turbulence in dissipative dynamical systems," *Reviews of Modern Physics,* 53 (1981): 643-654, 643.
[233] J.-P. Eckmann, "Roads to turbulence," 643.
[234] J.-P. Eckmann, "Roads to turbulence," 645.

exactly which equation . . . is relevant for the description of the system. (2) When an experiment is repeated, the equation may have slightly changed (e.g. the gravitational effects change on the earth by the motion of the moon). (3) The equation under investigation is one among several, all of which are very close to each other. (4) If among these there are many which satisfy the scenario, then we will say that if we perform an actual experiment, it will be probable that the conclusions of the scenario applies.[235]

In general, the scenarios described only the tiniest part of the phase space.

"Therefore, *several scenarios may evolve concurrently in different regions of phase*

*space.* There is thus no contradiction if several scenarios occur in a given physical

system, depending on how the initial state is prepared." While the "then" part of a

scenario was likely to happen if the "if" part was satisfied, there was no attempt in the

theory to say how probable the hypothesis was. "*A scenario does not describe its*

*domain of applicability.*"[236]

The theory was "completely general," but these restrictions transformed it into

a patchwork that was full of holes. Moreover there was no way to tell where the holes

were located and how much surface they occupied. Since the theory was a local

description of different scenarios, its predictive power was limited to those patches of

known scenarios. Once one had recognized the patch corresponding to the situation,

the succession of events to follow was likely to be known.

Inspired by Thom and Ruelle's modeling practices, this type of

mathematization was of an original type. It underscored that "new types of questions"

could be raised. The validity of the answers one provided to these questions eschewed

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[235] J.-P. Eckmann, "Roads to turbulence," 646. My emphasis.
[236] J.-P. Eckmann, "Roads to turbulence," 646. His emphasis.

reliance on fundamental laws. This general method was briefly summarized by

Eckmann and his collaborators as such:

> Physical models of hydrodynamics or of other dissipative dynamical systems tend to be very complicated. In addition, the laws describing such systems are only known *approximately*. One is thus faced with the problem of isolating and if possible answering *new types of questions which are more or less independent of detailed knowledge of the dynamics* of any given physical system. Such questions the have answers which are *universal*.[237]

On the basis of a mathematical failure, a fruitful modeling practice was thus

constructed by physicists for physicists. Indeed, mathematicians had not succeeded in

classifying generic bifurcations of dynamical systems. Nevertheless, Eckmann's

scheme emphasized the benefits of a dynamical systems approach coupled with

experimental results, indicating which bifurcations were the likeliest to occur.

Eckmann explained the way to use results of dynamical system theory. Turbulence,

with its baggage of bifurcations, cascades, and strange attractors, became a standard

exemplar used to describe many other systems from chemical reactions to menstrual

cycles, as well as clarinets![238] Chaos was born.

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[237] P. Collet, J.-P. Eckmann, and H. Koch, "Period Doubling Bifurcations for Families of Maps on $\mathbf{R}^n$," *Journal of Statistical Physics*, 25 (1981): 1-14, 1; repr. *Univ. Chaos*, 353-366. My emphasis.

[238] Just to list a few places where this modeling practice was applied, see, e.g., I. R. Epstein, "Oscillations and Chaos in Chemical Systems," *Physica D*, 7 (1983): 47-56; L. Glass and M. C. Mackay, *From Clocks to Chaos: The Rhythms of Life* (Princeton University Press, 1988); and C. Maganza, "Du silence au chaos acoustique: les 'bifurcations' d'une clarinette," *La Recherche*, 17(173) (1985): 100-103.

In 1982, a proof of Feigenbaum's conjectures was provided by Oscar E.

Lanford, from Berkeley. Symbolically, the proof turned out to be computer-

assisted.[239]

## 6.    CONCLUSION

Partly as a result of Ruelle and Takens's proposal, a "dynamical systems approach"

was indeed adopted by physicists. But this approach was characterized by modeling

practices which differed significantly from Ruelle and Takens's. Indeed, a decade of

work on various models for the onset of turbulence, in Rayleigh-Bénard systems

especially, had shown that mathematical arguments alone, based on the notion of

structural stability and genericity, could be misleading. Other "scenarios" existed

besides Ruelle and Takens's. But, where mathematicians faced tremendous

difficulties, namely for the classification of bifurcations in dynamical systems,

experiments, it was realized, offered an original means to determine which scenarios

were more relevant than others.

## a)    The Triumph of 'Light' Physics

In the above, I displayed how misleading is the view that Ruelle and Takens's model

was a mathematical theory which gained credence from experimental evidence. By

focusing on Rayleigh-Bénard convection as boundary system, I showed that the

interest in hydrodynamic instabilities hardly stemmed from Ruelle and Takens's

proposal. On the contrary, many groups of scientists focused on these problems as a

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[239] O. E. Lanford, "A Computer-Assisted Proof of the Feigenbaum Conjectures," *Bulletin of the American Mathematical Society,* 81 (1982): 427-434; repr. *Univ.*

way to explore the ramification of the theoretical tools already enabling them to tackle

nonlinear phenomena. To start with, by the late 1960s and early 1970s,

hydrodynamicists, by undertaking both experimental and theoretical studies of the

Rayleigh-Bénard system, had already begun to disentangle the problems raised by this

system. They clearly distinguished between Bénard's hexagonal cells and the rolls

triggered by Rayleigh's instability. They moreover discussed the perturbations

affecting these rolls. All of these developments took place independently of Ruelle

and Takens's proposal. And indeed, we can even see that, when he picked up

dynamical systems again, in 1974-75, Ruelle joined, rather than propelled, a

bandwagon that already was in motion.

At the same time, the Rayleigh-Bénard system was singled out by various

groups of scientists, which saw in it helpful analogies with other phenomena such as

dissipative structures in chemical kinetics and phase transitions. Indeed, Prigogine's

school and people working on phase transitions considered that their methods were

"universal" enough to provide accounts for hydrodynamic instabilities. Moreover, the

study of such systems could offer useful resources, since they were well studied and

often made use of recent mathematical theories of qualitative dynamics. Thus the

analogy could not only help understand hydrodynamic instabilities with methods

developed in order to deal with nonlinear phenomena in physics and chemistry, it

could moreover provide new resources to think about these phenomena. Constant

contacts among these different groups generated enough excitement, so that the study

*Chaos*, 245-252.

of these phenomena became quite fashionable even before anything from the

dynamical systems theory developed by Smale entered the scene.

But theoretical analogies proved to be disappointing as a way to tackle the

turbulence problem. On the other hand, experimental techniques common to the study

of phase transitions at a critical point were very successfully transferred to the study

of hydrodynamic instabilities. These techniques could be used to provide

measurements of greater accuracy than earlier ones, and most importantly,

measurements of local quantities to compare with hydrodynamic theories.

In the process, the study of fluid dynamics witnessed a striking influx of

physicists. Once again in its long history, rather mundane phenomena of fluid

mechanics thereby became a central object of inquiry for physicists and

mathematicians. As Bergé contended: "It is a domain in which, with relatively modest

material tools, but a good dose of imagination, one can contribute: it illustrates, as it

were, the triumph of 'light physics'."[240] At the same time, this field was profoundly

transformed, not only by the adoption of a dynamical systems approach, but mostly as

a consequence of the physicists' bringing a wide array of theoretical and experimental

tools to bear on the study of fluids.

In France, the case I studied the most above, it became clear that this also was

the result of a political desire. While atomic and nuclear physics had been emphasized

before, it became urgent, in the eyes of the French science policy makers of the early

1970s, to revive the study of "light" physics and chemistry. The VIth Plan

underscored the need and outlined means to achieve this goal. One consequence of

this political orientation was that an interdisciplinary study of fluid instabilities was

valued, especially since energetic groups studying liquid crystals had made the

analogy between phase transitions and hydrodynamic instabilities real, since both

occurred in their systems.

Out of these efforts, a chaos constellation emerged among French physicists.

In the above, the work of a few experimenters and theoreticians provided concrete

examples of how something approaching a "dynamical systems approach" was finally

adopted. But in the process, the modeling practices of 'applied topologists' or of

Ruelle and Takens hardly was wholly picked up by physicists. On the contrary, by

looking at the IHÉS research program in 1970s, it has been argued that the Institute

indeed seemed to have remain somewhat remote from the physicists' main

considerations.

## b)    Experiment-Based Topology?

Physicists actively adapted the language and the (modeling and theoretical) practices

coming out of dynamical systems theory. The traditional view has it that, in the 1970s,

it was finally shown that dynamical systems theory could be usefully "applied" to real

systems, offer explanations for turbulent behaviors, and even make predictions. This

was a different type of prediction, to be sure, emphasizing qualitative behavior and,

especially, the very limits of the predictions due to the property of sensitive

dependence on initial conditions. But, as the above chapter indicates, we may

completely reverse this view. Experiments in fluid mechanics provided some

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[240] P. Bergé, Y. Pomeau, and C. Vidal, *Order within Chaos*, 267.

justification for an approach that, on a purely mathematical level, still was quite

incomplete. They offered a basis for achieving some classification of "generic"

bifurcations in dynamical systems, where rigorous arguments had failed. In this way,

the modeling practice of many chaologists became what we might call an experiment-

based topological modeling practice, which used the objects of dynamical systems and

bifurcation theory as an important part of their mathematical arsenal, but eschewed

the most grandiose claims of the IHÉS applied topologists discussed in previous

chapters.

As a testimony for this, Libchaber explained that his work "surely a theoretical

breakthrough. I say theoretical, it wasn't experimental. . . . My essential contribution

was to show that this mathematical game existed in nature." If there was an element of

surprise for this experimenter, it was universality. It really struck him as something

grandiose that made him realize that he was *"playing with mathematics."*[241] For many,

his experiment really *showed* the genericity of the Feigenbaum cascade scenario.

Mathematical games could help understand nature, but experiments, and only

experiments, could decide which of those games were relevant to the study of nature.

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[241] Interview of A. Libchaber .

## 7.    <u>COMPLEMENT TO CHAPTER VIII: DOCUMENT</u>

**Research Program Presented by Nicolaas Kuiper to the Volkswagen Foundation (1976).**

In the general framework of dynamical systems and singularities, a certain number of research projects have been and will be in activity at the IHÉS. Some of these projects are described below.

(a) <u>Turbulence and differentiable dynamical systems</u>.

Turbulence in fluid dynamics is a phenomenon of considerable practical importance, which is however very poorly understood at a fundamental level. One point of view which is gaining acceptance is that "turbulent" solutions of the time evolution equations of fluid dynamics are solutions with an apparently chaotic asymptotic behavior and sensitive dependence on initial conditions. One of the basic papers on the subject was written at the IHÉS (D. Ruelle and F. Takens. On the Nature of turbulence. Commun. math. Phys. 20, 167-192 (1971)). Further work on turbulent solutions of differential equations at the IHÉS was done by O. Lanford and J. Curry, making use in particular of the HP 9830 A calculator of the Institute. Currently, S. Newhouse and D. Ruelle are engaged in further mathematical study of differential equations and diffeomorphisms exhibiting turbulent behavior.

(b)  <u>Differentiable dynamical systems</u>.

This is a vast and important subject which has been rejuvenated by S. Smale and his school - among others. The IHÉS has played an important role in the development of this area of research, where R. Thom has been an active source of inspiration. Among the

past visitors one can mention : S. Smale, C.C. Pugh, M. Shub, R. Bowen, R.F. Williams, J. Frank, J. Robbin, J. Palis, F. Takens, etc. These have worked mainly on systems satisfying Smale's Axiom A (or related hyperbolicity conditions), obtaining in particular important results on structural stability and bifurcations of these systems. In this line of research we have at present S. Newhouse as visitor.

Another direction of research in differentiable dynamical systems is the study of irrational rotations on the circle (or flows on tori). Spectacular results there have been obtained recently by M. Hermann (Ecole Polytechnique and IHÉS). P. Deligne has become interested in this work. This work is related to that of C. Siegel, J. Moser and Russmann.

(c) Foliations.

The coming of D. Sullivan to the IHÉS as permanent member has promoted a considerable development of the subject. He has led a very active seminar, developed the concept of "geometric current" (with D. Ruelle), and obtained new and beautiful results (in particular collaborating with R. Edwards and K. Millett, as well as D. Epstein). Let us mention as an example Sullivan's construction of a flow on a compact five-manifold so that every orbit is periodic but the length of orbits is unbounded.

(d) Relation between qualitative dynamics and algebraic singularities, Catastrophes.

This is the subject of bifurcation theory, which aims to describe how a stable régime of a dynamical system can be destroyed and transformed discontinuously into another one. After the scheme of "elementary catastrophe theory", associated to singularities of functions, one feels a more complete theory is needed, involving bifurcations of a more general type than those of a gradient system.

This involves considering topological nature of attractors, and how topologically different attractors may nevertheless be considered as "thermodynamically" alike (a study in which Robert Williams has given very interesting results) the qualitative use of bifurcations in the interpretation of natural phenomena (known as Thom's catastrophe theory) will be continued, with particular reference to applied mechanics and theory of elasticity (Thompson-Hunt), and applications ranging from [2] organic chemistry (change of forms of molecules) to biophysics (discontinuous behavior of membranes), to geology (plate tectonics), and to social sciences (in the spirit of C. Zeeman's models).

(e) Singularities of polynomial mappings.

These occur in various domains of mathematics and science. Therefore their analytic, differential and topological properties are important to know. Much has been done and many interesting results have been obtained in this subject in the last couple of years, but even so it remains a wide open field. Going into details, in some domains of application we mention that with the help of specific singularities one has constructed objects such as exotic differential structures, group actions on manifolds, exotic piecewise linear structures, and knots. Actually, a lack of examples is an obstacle in the theory of four-manifolds and constructions with singularities are attempted. On this and related programs have worked and will work at the IHÉS : A'Campo, Brieskorn, Ehlers, H. King, Kirby, Lojasiewicz, Looyenga, Moisheson, Pinkham, Sebastiani, Siebenman (Orsay) and Siersma.

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