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

4. HYDRODYNAMICAL INSTABILITIES AND TURBULENCE IN FRANCE, 1971-1975

a) Three conferences in France

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

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

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.  

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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model, introduced a famous strange attractor that would soon be widely known as the Hénon attractor.\textsuperscript{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 "pluraldisciplinary 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."\textsuperscript{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


\textsuperscript{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 Martinet'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.\textsuperscript{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.\textsuperscript{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

attractors. Clearly something was going on. Can this have been a consequence of Ruelle and Takens's model for the onset of turbulence and a growing recognition of the usefulness of a "dynamical systems approach" for studying of nonlinear phenomena? I think not.

b) An 'Action thématique programmée' on turbulence

In the prefaces of the Orsay Workshop and the Dijon Symposium, a common acknowledgment indicates that there was a common force behind these two meetings dealing with turbulence in an interdisciplinary fashion, but otherwise so different from one another. The organizers of both conferences thanked the CNRS for having sponsored their meetings as part of an "ATP" devoted to "Turbulence and Instabilities." This particular program, in which Pierre-Gilles de Gennes was importantly involved, provided the initial glue for the complex alliances that made it possible for an important chaos constellation to emerge in France in the course of the 1970s—a constellation which would embrace the Ruelle-Takens model as a welcome exemplar for their undertaking. In particular, it will become obvious that groups working on liquid crystals important for drawing attention on the study of hydrodynamic instabilities benefited from the possibilities offered by this ATP.

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

Here, ATP stood for "Action thématique programmée [programmed thematic action]." They were a means established by the Centre national de la recherche scientifique

105 M. Keane, ed., International Conference on Dynamical Systems in Mathematical
(CNRS) in 1971, to give priority sponsorship to certain interdisciplinary domains of research emphasized by the VIth Plan. 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. 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.


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 la 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

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.
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."\textsuperscript{111} In the eyes of the administration, the promotion of interdisciplinary research themes imposed to circumvent the corporate demands of specialists.\textsuperscript{112}

The emphasis was put on the flexibility of the method, which was managed by \textit{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.\textsuperscript{113}

\textsuperscript{112} J.-F. Picard, \textit{La République des savants}, 255.
\textsuperscript{113} The label ATP was indistinctly applied to the committees who worked on the "objectives" of the Plan, and to more specific actions undertaken under such "objectives." Typically, an ATP was to be in activity for about three years. In 1971, thirteenth ATPs were organized with a total budget of about 15 MF. In 1974, the funds devoted to ATPs represented 10\% of those affected to equipment by the CNRS, and 5\% of the total funds spent by the CNRS, except for personnel. See R. Chabdal and J. Gavorot, "Les actions thématiques programmées"; CNRS, \textit{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.\textsuperscript{114}

Reviewing the benefits derived from ATPs in 1974, Robert Chabdal, 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."\textsuperscript{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."\textsuperscript{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.\textsuperscript{117}

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

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.\footnote{P. Pieranski and É. Guyon, "Cylindrical Couette Flow Instabilities in Nematic Liquid Crystals," \textit{Advances in Chemical Physics}, 32, ed. I. Prigogine and S. A. Rice: 151-161.} In the case of liquid crystals, indeed, the relation between phase transitions and hydrodynamic instability was more than a mere analogy.

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 micellar 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. 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.

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

On June 20, 1973, Jean Govoret, adoint 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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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 He₄, metallic, nematic [liquid crystal], dielectric, or magnetic liquids).

— Numerical simulation and analysis of models.

— Acoustic effects: interaction turbulence-sound; role of intermittency.\textsuperscript{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, \textit{the arbitrary and sterilizing character of such a rift . . . has become more and more obvious.}\textsuperscript{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, \textit{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 \textit{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

\textsuperscript{128} R. Chabba and J. Gayvoret, "Les ATP de physique," 47.
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 mechanists."\textsuperscript{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.\textsuperscript{131} Using computer simulations, however, he explicitly exhibited
the "nature of 'random' solutions to (deceptively simple) differential equations."\textsuperscript{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 mechanist's worldview which
conspicuously excluded physicists (see Fig. 17).\textsuperscript{133} Among the seminars presented at
Les Houches, let us note: Manuel G. Velarde's extensive review of the Rayleigh-

\textsuperscript{130} All quotes above are from R. Balian and J.-L. Peube, "Preface," \textit{Fluid Dynamics},
vii-viii. My emphasis.
\textsuperscript{131} S. A. Orszag, "Lectures on the Statistical Theory of Turbulence," \textit{Fluid Dynamics},
\textsuperscript{133} H. Keith Moffat, "Six Lectures on General Fluid Dynamics and Two on
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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velocities and correlation, ... c) by bringing in certain theoretical concepts stemming from different domains.\textsuperscript{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 hydrodynamists might

\textsuperscript{136} \textit{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," \textit{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

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.\footnote{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).} The following year, the phenomenon of turbulence more than ever became an
integral part of de Gennes's program.\footnote{Annuaire du Collège de France, 75 (1975): 81-82. His emphasis.} 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
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,
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M. Dubois and P. Bergé, "Mesure des vitesses de convections naturelle dans
l'instabilité de Rayleigh-Bénard" (7/6/74).} 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
example of how the Ruelle-Takens model would become a useful resource for a
guyanist'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.

\[140\] I thank Paul Manneville to have provided me a copy of his personal lecture notes
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.  

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). 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."\textsuperscript{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."\textsuperscript{144}

In 1974, the ATP on "Instabilities" had sponsored 13 projects for a total
amount of 1,460,000 F.\textsuperscript{145} True to the ATP's intent of sponsoring the acquisition of

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

\textsuperscript{144} Procès-verbal du Comité national du CNRS (Section 02: Physique théorique) (15,

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. A clear diversity of projects was funded by the ATP. In 1975, the ATP again served to sponsor 9 projects, for a total of 1,203,000 F. Among a similar diversity of projects, ranging from meteorology to plasma physics, from superfluidity to shock dynamics, a contract was given to Gruyan 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.  

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 préparatoires au 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. 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 suggested that in the future plasma/astrophysics and fluid mechanics/turbulence be distinguished.

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 group in 1976, he himself more or less dropped this topic from his future courses at the Collège de France.

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 SrTiO₃ 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." 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

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."\textsuperscript{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.\textsuperscript{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 $\equiv(\Delta T - \Delta T^*)^{-x}$ where $x$ is some fixed exponent inside
one universality class?"\textsuperscript{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

\textsuperscript{156} P.-G. de Gennes, "Phase Transitions and Turbulence," 3.
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. 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." 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. By 1975, therefore, it seems that a chaos constellation had already emerged. It had been built

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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 than "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 find 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.

163 Rapport scientifique 1974. Arch. IHÉS.
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.\footnote{D. Ruelle, "The Lorenz Attractor," 147. My emphasis.}

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."\footnote{D. Ruelle, "The Lorenz Attractor," 154.} 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.\footnote{D. Ruelle, "The Lorenz Attractor," 154-155.}

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
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

\(^{169}\) 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, Móser, Malgrange, etc. I suppose Smale is not available.\textsuperscript{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.\textsuperscript{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.\textsuperscript{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.\textsuperscript{173} This marked an important change of

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

\textsuperscript{171} Report by E. C. Zeeman of a conversation with M. Atiyah. \textit{Comité scientifique} (15/6/71). Arch. IHÉS.

\textsuperscript{172} Compte-rendu du Comité scientifique (14/4/72). Arch. IHÉS.

\textsuperscript{173} \textit{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."  

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. 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. For

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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.\textsuperscript{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.\textsuperscript{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 \textit{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.\textsuperscript{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,
(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

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 spermatozoaids. 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.

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.\textsuperscript{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."\textsuperscript{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."\textsuperscript{184} For Rayleigh numbers up to

\textsuperscript{182} Interview of P. Bergé and M. Dubois by the author (6/3/97).
\textsuperscript{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 \((R - R_*)^{0.60\pm0.02}\), 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.\(^{186}\)

\((ii)\) \textit{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


\(^{187}\) 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.

At the time, the Ruelle-Takens model was not explicitly discussed by Bergé and Dubois. They had been enrolled in the study of Rayleigh-Bénard by de Gennes, Velarde, and Pomeau because they had an original apparatus that allowed them to measure local quantities. But none of the above theoreticians were very keen, up to 1975, to take up a dynamical systems approach to the problem of the onset of turbulence in convection. Normand, Pomeau, and Velarde extended Busse's theory to include more and more modes. Striking tensions in the popular account by Velarde and Normand underscores that they had not adopted a dynamical systems approach.¹⁹⁰ When this is compared with Ruelle and Takens's attitude, the contrast is patent. While the former still dreamt of exact solutions, the latter expressed their agnosticism toward the fundamental Navier-Stokes equation itself.

Briefly, before the late 1970s, Bergé and Dubois's work could not be seen as stemming from a dynamical systems approach.¹⁹¹ However, theirs was one of the experimental evidences that dynamical systems with a few degrees of freedom contained most of the mechanics of the transition to turbulence in confined geometry, findings that served to vindicate Ruelle's viewpoint. Moreover, it allowed contacts to be established between experimenters and theoreticians. On these bases, the chaos constellation was built. Among those mentioned above, Pomeau, more than anyone

¹⁹⁰ "Although exact solutions to [the Rayleigh-Bénard problem] are still lacking, substantial progress . . . has been made." M. G. Velarde and C. Normand, "Convection," 93.
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 repousoir]." 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.,
193 His more cited paper before 1975: Y. Pomeau and P. Résibois, "Time-Dependent
Correlation Functions and Mode-Mode Coupling Theories," Physics Reports, C19
Statistical Physics Conference in Chicago. 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'." 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.

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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.

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." 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.


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." 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 $r_\tau$, the curve had two intersection points with the diagonal, which collapsed into a single point for $r = r_\tau$, while "for $r > r_\tau$ the curve is lifted up and no longer crosses the [diagonal] so that a
'channel' appears between them" (Fig. 18).\textsuperscript{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."\textsuperscript{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."\textsuperscript{203} In their view, such generic properties of \textit{other} dynamical systems could provide an explanation for experimental behaviors which were assumed to stem from a similar, but unknown, dynamical system.

\textsuperscript{202} P. Bergé, M. Dubois, P. Manneville, and Y. Pomeau, "Intermittency in Rayleigh-Bénard," L-343. Their emphasis.
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

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.


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.\textsuperscript{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.\textsuperscript{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."\textsuperscript{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.

\textsuperscript{213} Interview of A. Libchaber (24/10/1993). For a discussion of changes in the practice of the definition of objects, see I. Stengers, \textit{Cosmopolitiques}, 5, 152.

\textsuperscript{214} A. Libchaber, "Hydrodynamique de l'hélium IV," C1-115.

\textsuperscript{215} Interview of A. Libchaber (24/10/93).
The upper part is the detail of the experimental system. The lower part is an enlarged view of the local probe seen both in a transverse cut and from the bottom. Dimensions are not conserved.

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

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. 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."\textsuperscript{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.\textsuperscript{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.\textsuperscript{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 Bergê's

\textsuperscript{217} A. Libchaber and J. Maurer, "Local Probe," L-372.
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 $f/2, f/4$ ..."\textsuperscript{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.\textsuperscript{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

\textsuperscript{220} J. Maurer and A. Libchaber, "Rayleigh-Bénard Experiments," L-422.
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. 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. Carefully noting a "demultiplication of frequencies," Libchaber and Maurer were drawn to conclude that this process "appeared as crucial for the germination of turbulence." 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. 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. 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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behavior had led Martin to consider this sequence of bifurcation as a possible scenario for the onset of turbulence (Figure 1).


$$\frac{R_{n-1} - R_n}{R_n - R_{n+1}} \xrightarrow{n \to \infty} \delta = 4.6692...;$$

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
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)"

228 Libchaber and Maurer, "Helium in a Small Box," 280.
229 Interview of A. Libchaber (10/24/93).
catastrophe theory."\textsuperscript{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."\textsuperscript{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 \textit{experiments} as a guide for which bifurcations from simple attractors to nontrivial ones might be the most relevant for physics (and chemistry).\textsuperscript{234} He thus introduced the notion of a \textit{scenario} to describe the \textit{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 \textit{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 \textit{never} knows

\textsuperscript{233} J.-P. Eckmann, "Roads to turbulence," 643.
\textsuperscript{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.\textsuperscript{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."\textsuperscript{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

\textsuperscript{235} J.-P. Eckmann, "Roads to turbulence," 646. My emphasis.
\textsuperscript{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.\textsuperscript{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!\textsuperscript{238} Chaos was born.


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.\textsuperscript{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

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

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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'." 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

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." 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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7. **COMPLEMENT TO CHAPTER VIII: DOCUMENT**

Research Program Presented by Nicolaas Kuiper to the Volkswagen Foundation (1976).

In the general framework of dynamical systems and singularities, a certain number of research projects have been and will be in activity at the IHÉS. Some of these projects are described below.

(a) **Turbulence and differentiable dynamical systems.**

Turbulence in fluid dynamics is a phenomenon of considerable practical importance, which is however very poorly understood at a fundamental level. One point of view which is gaining acceptance is that "turbulent" solutions of the time evolution equations of fluid dynamics are solutions with an apparently chaotic asymptotic behavior and sensitive dependence on initial conditions. One of the basic papers on the subject was written at the IHÉS (D. Ruelle and F. Takens. On the Nature of turbulence. Commun. math. Phys. 20, 167-192 (1971)). Further work on turbulent solutions of differential equations at the IHÉS was done by O. Lanford and J. Curry, making use in particular of the HP 9830 A calculator of the Institute. Currently, S. Newhouse and D. Ruelle are engaged in further mathematical study of differential equations and diffeomorphisms exhibiting turbulent behavior.

(b) **Differentiable dynamical systems.**

This is a vast and important subject which has been rejuvenated by S. Smale and his school - among others. The IHÉS has played an important role in the development of this area of research, where R. Thom has been an active source of inspiration. Among the
past visitors one can mention: S. Smale, C.C. Pugh, M. Shub, R. Bowen, R.F. Williams, J. Frank, J. Robbin, J. Palis, F. Takens, etc. These have worked mainly on systems satisfying Smale’s Axiom A (or related hyperbolicity conditions), obtaining in particular important results on structural stability and bifurcations of these systems. In this line of research we have at present S. Newhouse as visitor.

Another direction of research in differentiable dynamical systems is the study of irrational rotations on the circle (or flows on tori). Spectacular results there have been obtained recently by M. Hermann (Ecole Polytechnique and IHÉS). P. Deligne has become interested in this work. This work is related to that of C. Siegel, J. Moser and Russmann.

(c) Foliations.

The coming of D. Sullivan to the IHÉS as permanent member has promoted a considerable development of the subject. He has led a very active seminar, developed the concept of "geometric current" (with D. Ruelle), and obtained new and beautiful results (in particular collaborating with R. Edwards and K. Millett, as well as D. Epstein). Let us mention as an example Sullivan’s construction of a flow on a compact five-manifold so that every orbit is periodic but the length of orbits is unbounded.

(d) Relation between qualitative dynamics and algebraic singularities, Catastrophes.

This is the subject of bifurcation theory, which aims to describe how a stable régime of a dynamical system can be destroyed and transformed discontinuously into another one. After the scheme of "elementary catastrophe theory", associated to singularities of functions, one feels a more complete theory is needed, involving bifurcations of a more general type than those of a gradient system.
This involves considering topological nature of attractors, and how topologically
different attractors may nevertheless be considered as "thermodynamically" alike (a study
in which Robert Williams has given very interesting results) the qualitative use of
bifurcations in the interpretation of natural phenomena (known as Thom's catastrophe
theory) will be continued, with particular reference to applied mechanics and theory of
elasticity (Thompson-Hunt), and applications ranging from [2] organic chemistry (change
of forms of molecules) to biophysics (discontinuous behavior of membranes), to geology
(plate tectonics), and to social sciences (in the spirit of C. Zeeman's models).

(e) Singularities of polynomial mappings.

These occur in various domains of mathematics and science. Therefore their
analytic, differential and topological properties are important to know. Much has been
done and many interesting results have been obtained in this subject in the last couple of
years, but even so it remains a wide open field. Going into details, in some domains of
application we mention that with the help of specific singularities one has constructed
objects such as exotic differential structures, group actions on manifolds, exotic piecewise
linear structures, and knots. Actually, a lack of examples is an obstacle in the theory of
four-manifolds and constructions with singularities are attempted. On this and related
programs have worked and will work at the IHÉS: A'Campo, Brieskorn, Ehlers, H. King,
Kirby, Lojasiewicz, Looyenga, Moisheson, Pinkham, Sebastiani, Siebenman (Orsay) and
Siersma.